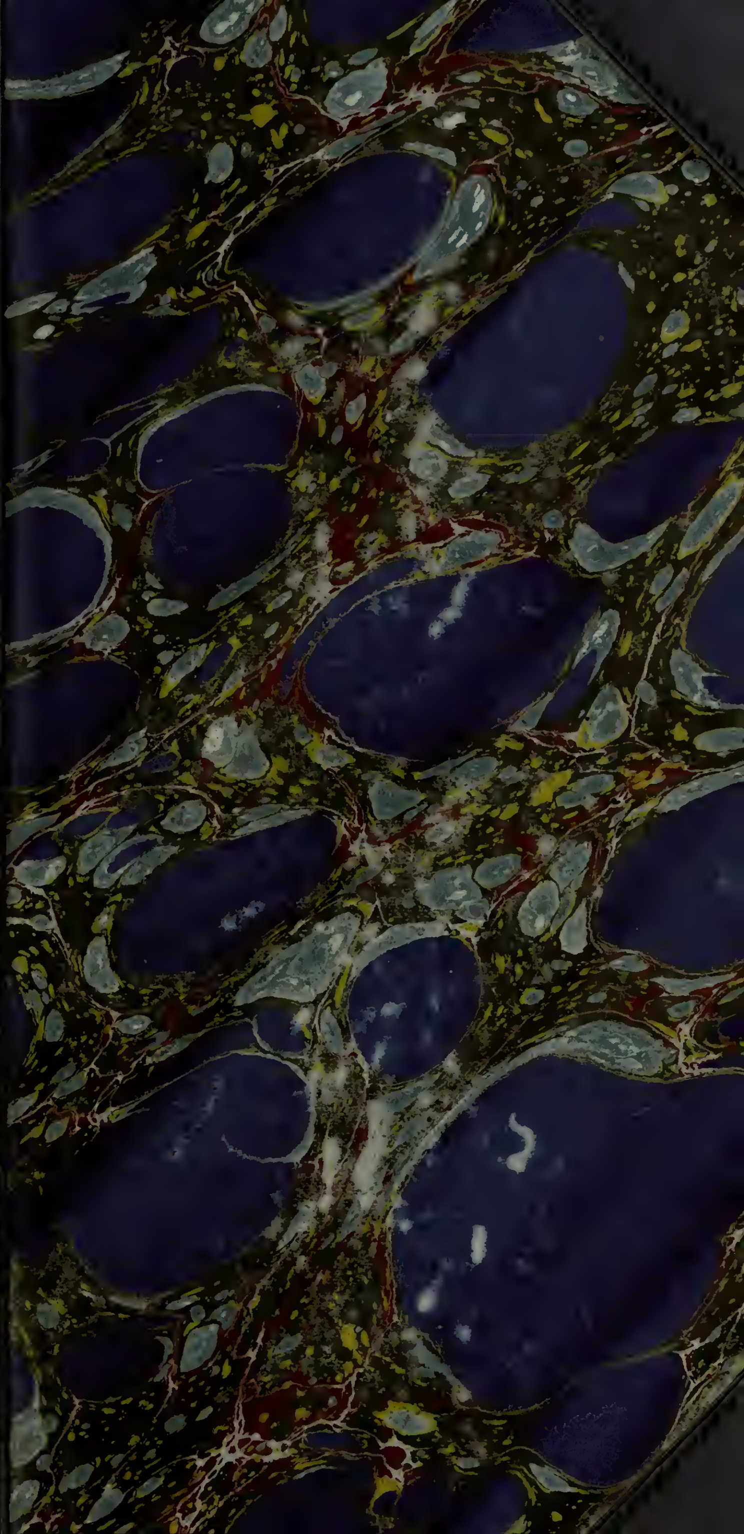

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CONDUCTED BY

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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.

VOL. XXVII.

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

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NUMBER CLXXXIII.—SUPPLEMENT TO VOL. XXVII.

PLATES.

ERRATA AND ADDENDA.

In the Meteorological Table for March (May Number), the average temperature observed at Sandwick Manse, Orkney, is stated to be 48·40 instead of 38·72.

Page 126, paragraph 2, line 2. On the *left*-hand side of equation (9.), for x read y.
... 286, line 2 from bottom, *for* p^m *read* p^n.
... 287, ... 13, the last + ought to be —.
... ... 20, $\frac{n-1}{2}$ is an exponent.
... 288, ... 9, *for* $z-y$ *read* $\overline{z-y}$.
... 363, ... 2, the exponent should be n instead of $n+1$.
... 397, ... 18, *for* venal *read* renal.

THE
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JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1845.

I. *On the Magnetic Relations and Characters of the Metals.* *By* Michael Faraday, *D.C.L., F.R.S., Foreign Associate of the Acad. Sciences, &c.*[*]

In two former communications to the Philosophical Magazine[†] respecting the magnetic characters of metals generally and the temperatures at which they respectively lost or gained the powers of magnetic induction, I said that iron and nickel were the only metals which had this power, and that I could not find it in pure cobalt. In this I was in error; cobalt has this property in common with iron and nickel, as others have said. I have sought for the piece of cobalt on which I experimented and believed to be pure, but cannot find it, and am now unable to ascertain the cause of my error, though not too late to correct it.

By favour of Dr. Percy and Mr. Askin I have recently experimented with two fine pieces of pure cobalt prepared by the latter, both being well-fused and perfectly clean masses. This metal becomes strongly magnetic by induction, either of a magnet or an electro-current, and can easily be made to lift more than its own weight. Like soft iron and nickel, it does not retain its magnetism when the inducing influence is removed.

It was to me a point of great interest to ascertain whether, and at what temperature, cobalt would lose this power and become as the unmagnetic metals. To my surprise I found this to be very high, not merely much higher than with nickel, but far above that required for iron or steel, and nearly approaching the temperature of melted copper. That for iron is a moderate red heat, and that for nickel the temperature of boiling oil only. As the temperature rises, the magnetic force of the cobalt continues, apparently undiminished, to

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[*] Communicated by the Author.

[†] 1836, vol. viii. p. 177, and 1839, vol. xiv. p. 161.

a certain degree of heat, and then suddenly ceases; and it comes on as suddenly in descending from still higher temperatures.

The oxide of cobalt procured by burning the metal cobalt on charcoal by a jet of oxygen was obtained as a fused globule and was not magnetic.

The oxide of nickel being heated and cooled is not magnetic, but whilst heating care must be taken that it is not reduced. If heated in the flame of a spirit-lamp, especially at the lower part, it will often appear to be not magnetic, and then as the temperature falls will become magnetic; but this is caused by the reduction of a little of the oxide at the edges or elsewhere in the mass to the metallic state, and its exhibition of the properties of the *metal* at temperatures above and below the magnetic point.

Manganese.—A piece of manganese prepared by Mr. Thomson and considered pure was put into my hands. It contained a trace of iron; it was very slightly magnetic, and probably only in consequence of the little iron present. Before a jet of oxygen or charcoal, it burnt with sparkles somewhat in the manner of iron, and produced an oxide which could be obtained either as a porous white mass or as a dense compact brown mass, translucent in small slices; but in neither state was the oxide magnetic.

I then cooled* manganese, chromium and many other metals and bodies to the lowest temperature which I could obtain by a mixture of æther and solid carbonic acid placed *in vacuo*†, the temperature being then not higher than 156° F. below 0°, but not one of them assumed the magnetic state. The following is a list of the substances:—

Platinum.
Gold.
Silver.
Palladium.
Copper.
Tin.
Lead.
Cadmium.
Zinc.
Rhodium.
Manganese.
Chromium.
Titanium.

Iridium and Osmium.
Antimony.
Arsenic.
Bismuth.
Fusible metal.
Speculum metal.
Plumbago.
Gas-retort carbon.
Kish.
Orpiment.
Realgar.
Sulphuret of antimony.
... bismuth.

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* Phil. Mag., 1839. vol. xiv. p. 162.

† Philosophical Transactions, 1845, pp. 157, 158.

Sulphuret of copper.
... iron.
... lead.
... silver.
... tin.
Carbonate of iron, native.
Prussian blue.
Crystallized sulphate of iron.
Calomel.
Chloride of silver.
... lead.
Protoxide of arsenic.
... antimony.
... lead.
Oxide of bismuth.
... tin (native).
... manganese (native).

Thus it appears that only iron, nickel and cobalt are magnetic, or can be made magnetic amongst metals after the manner of iron; but the addition of cobalt, and still more, the very high temperature required to take away this property from it, increases the probability[*] that all the metals form a common class in respect of this property; and that it is only a difference of temperature which distinguishes these three from the rest, just as it also in a similar respect distinguishes them from each other.

In connexion with the effect of heat it may be remarked (and perhaps has been remarked already), that, assuming an elevated temperature for the internal parts of the earth, then it is evident that at a very moderate distance from the surface, as compared with the earth's diameter, the substances composing the earth must be destitute of such magnetic power as is possessed by a magnet; and, at a distance somewhat greater, none of them can retain even that power which soft iron has of becoming magnetic by induction. In such case, whether the earth be considered as magnetic of itself, like a loadstone, or rendered magnetic by induction under the influence of external magnetic masses, as the sun for instance; still it can only be in its crust that the magnetic power could be developed. Assuming with Ampère, that the magnetism of the earth is due to electric currents circulating around it parallel to the equator, then of course the above observations regarding the effects of heat would not apply.

Royal Institution, June 7, 1845.

II. *On the Products of the Distillation of Benzoate of Copper.*

By JOHN STENHOUSE, *Ph.D.*[†]

NEAR the conclusion of Dr. Ettling's very able and elaborate paper on the Essential Oil of *Spirea Ulmaria* and Salicylous Acid, which appeared in Liebig's *Annalen* for 1840, it is shortly stated, that by the destructive distillation of

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[*] Phil. Mag., 1836, vol. viii. p. 177.

[†] Communicated by the Author.

salicylite and benzoate of copper, Dr. Ettling had obtained among other products two neutral crystalline bodies—that from benzoate of copper having a smell very much resembling the odour of the geranium. Being quite unaware that Dr. Ettling had, subsequently to the publication of that notice, analysed the compound from the benzoate of copper, I was induced a few months ago also to prepare a quantity of that substance and to subject it to examination. Having mentioned this circumstance to Dr. Ettling, he very kindly furnished me with the subjoined account of his experiments, and at the same time requested me to prosecute the examination of the subject.

"A quantity of benzoate of copper consisting of a mass of small, slightly soluble needles of a greenish-blue colour, obtained by mixing hot solutions of benzoate of potash and sulphate of copper, was heated in a retort placed in an air-bath to about the temperature of 220° R. The retort was furnished with a tubulated receiver and a pneumatic apparatus to collect any gases which might be evolved. The salt became first brown and then of a reddish copper colour, giving off at the same time an oily matter, a portion of which crystallized in the neck of the retort, while a light oil passed into the receiver, where on cooling it also became filled with crystals. A gas passed into the pneumatic apparatus, which, as it rendered lime-water turbid and was absorbed by potash, was evidently carbonic acid. A second portion of gas was also evolved, which burned with a blue flame and was not absorbed by potash. It was not more closely examined. The mass of salt in the retort diminished to about half its bulk, and when still more highly heated gave out an oily liquid, which on cooling crystallized in needles. If the distillation was carried still further, the retort was found to contain a little of a brownish-coloured salt of copper mixed with a quantity of metallic copper.

"The solid portion yielded by the distillation had a greenish colour, was hard, brittle, melted readily, and it, as well as the more fluid portion, had very much the smell of naphthaline, though also faintly reminding one of the odour of red geranium. When the products of the distillation were heated with a weak solution of potash, a portion of them dissolved, forming a yellow solution, while another swam on the surface of the liquid as a colourless oil, which on cooling became a crystalline mass resembling paraffine. This crystalline portion was repeatedly treated with alkaline leys, by which operation its bulk was exceedingly diminished, and probably a considerable quantity of it decomposed. The portion insoluble in alkalies dissolved very readily in hot alcohol, and crystallized on cooling in long snow-white crystals. These were melted and subjected to analysis in the usual way.

"I. 0·581 grm. gave 1·675 carbonic acid and 0·265 water.

"II. 0·5655 gave 1·633 carbonic acid and 0·258 water.

	I.	II.
Carbon .	78·55	78·76
Hydrogen	5·06	5·06
Oxygen .	16·39	16·18
	100·00	100·00

"When that portion of the distillate which dissolved in the potash-ley was boiled in a retort, water passed over mixed with a few drops of an oily matter, which was not more closely examined. What remained in the retort, on being saturated with sulphuric acid, became a mass of shining crystals, which closely resembled benzoic acid. When the brownish-red coloured salt which remained in the retort was treated with sulphuretted hydrogen gas, it became very hot and was covered with a sublimate of white crystalline needles, while hydrogen gas was given off and sulphuret of copper remained. This sublimate was soluble both in alcohol and water, and crystallized on cooling in long shining needles, having a strong acid reaction and an appearance quite different from that of benzoic acid. The acid can also be extracted from the copper salt by means of ammonia. It readily crystallizes when the ammonia is saturated with acetic acid, and the copper is precipitated by sulphuretted hydrogen."

The quantity of the neutral crystalline compound which I subjected to examination was obtained from benzoate of copper prepared exactly in the way already described by Dr. Ettling. The dried salt was distilled in a copper retort over the naked fire. The amount of the neutral body which the salt yielded was pretty considerable; it came over in oily drops which condensed into a pasty crystalline mass as the receiver cooled. The products of the distillation were benzoic acid, the neutral crystalline body, and a small quantity of a heavy oil, in appearance and smell much resembling benzone. The pasty mass was first strongly pressed between folds of blotting-paper to remove adhering oil. The crystals were then repeatedly agitated with a hot solution of carbonate of soda in great excess, which removed the benzoic acid, the amount of which considerably exceeded that of the neutral body. It is improper to employ a solution of a caustic alkali to remove the benzoic acid, as such a proceeding would certainly destroy a large proportion of the neutral compound. The crystals were next collected on a filter and washed with cold water till every trace of alkali was removed. They were then dissolved either in hot alcohol or in æther, in both of which liquids they are exceedingly soluble, and were purified by repeated crystallizations. They crystallize out of æther in large four-sided oblique rhombic prisms. The crystals are hard and brittle, and grate between the teeth like sugar. They have a faint aromatic smell, somewhat resembling that of the geranium, but when heated approaching that of lemons. When slowly deposited from an æthereal solution, the crystals may be readily obtained an inch in length. Their melting-point is 70° C. After they had been kept melted for some time, to drive off any adhering æther, they were dried *in vacuo* over sulphuric acid and subjected to analysis.

I. 0·3508 grm. substance burned with chromate of lead, gave 1·0242 carbonic acid and 0·161 water.

II. 0·3902 gave 1·1437 carbonic acid and 0·182 water.

Atoms.			Calculated.	1.	2.
Carbon .	14	1050·00	80·00	79·62	80·01
Hydrogen	5	63·39	4·76	5·09	5·18
Oxygen .	2	200·00	15·24	15·29	14·81
		1313·39	100·00	100·00	100·00

It is evident from these analyses that this compound contains only one atom of oxygen less than benzoic acid, the formula of which is $C_{14} H_5 O_3$, and consequently it is isomeric with benzile. It differs very essentially from benzile, however, for when it is heated with solid potash it is immediately converted with evolution of hydrogen into benzoate of potash; while benzile, when similarly treated, yields, as is well known, benzilate of that alkali. When treated with an alcoholic solution of potash, it quickly forms benzoate of potash without any change of colour, while at the same time a little benzoic æther is also produced. When it is boiled with an aqueous solution of potash or soda, it is also converted into benzoic acid, though more slowly; and a portion of the compound is apt to distil over undecomposed. A quantity of the acid obtained by decomposing the neutral compound with an alkali in the way just described was purified by repeated crystallizations and subjected to analysis.

0·3807 grm. substance, dried at 100° C., gave 0·9575 carbonic acid and 0·1698 water.

	Atoms.		Calculated.	Found.
Carbon .	14	1050·00	68·85	68·67
Hydrogen	6	74·87	4·92	4·95
Oxygen .	4	400·00	26·23	26·38
		1524·00	100·00	100·00

The silver salt was prepared by double decomposition. The appearance of its crystals was exactly that of benzoate of silver.

I. 0·4287 grm. salt dried at 100° C., gave 0·202 metallic silver = 50·67 per cent. oxide.

II. 0·6175 salt gave 0·289 silver = 50·33 per cent. oxide.

The calculated quantity of oxide of silver in the benzoate, with the atomic weight of carbon at 75, is 50·68 per cent.

The neutral compound dissolves in sulphuric acid with the assistance of heat; the solution has a greenish-yellow colour. Nitric acid also readily attacks it and converts it into benzoic acid.

Action of Chlorine on the neutral body.

The compound was kept melted in a tube, and a stream of dry chlorine gas was sent through it for six days till it was completely saturated. It soon assumed a deep yellow colour, and retained a semifluid consistence at an ordinary temperature, thus evidently consisting of a mixture of a liquid and a solid crystalline substance. When cooled down to nearly 32° F. it became quite solid. Its smell was exceedingly pungent and offensive, bringing tears into the eyes very readily. The pasty mass was gently heated for some time to remove adhering chlorine, and then dried *in vacuo* over sulphuric acid and quicklime.

I. 0·3765 grm. substance, burned with hydrate of lime to determine the amount of chlorine, gave 0·4515 chloride of silver = 29·59 per cent. chlorine.

II. 0·3144, burned with chromate of lead, gave 0·616 carbonic acid and 0·0916 water = 53·43 per cent. C. and 3·23 H.

As the substance analysed was evidently a mixture of two compounds, I proceeded to separate them by pressing the pasty mass between folds of blotting-paper, which absorbed and removed the greater portion of the fluid body. The solid portion was then repeatedly dissolved in anhydrous æther, in which it was very soluble. On the evaporation of the æther, it was deposited in large flat shining crystals, which had a very faint smell, not at all disagreeable, and somewhat resembling that of the solid chloride of carbon, though not so strong. When sublimed, it formed flat four-sided prisms, which were iridescent like the crystals of chlorate of potash. Its melting-point was very high, being 87° C. It was kept melted for some time to drive off any adhering æther, then dried under the air-pump and subjected to analysis.

I. 0·316 grm. gave 0·7658 carbonic acid and 0·114 water.
II. 0·321 gave 0·7755 carbonic acid and 0·1116 water.
III. 0·3805 gave 0·9183 carbonic acid and 0·1408 water.
IV. 0·2122 gave 0·1380 chloride of silver = 16·04 chlorine.
V. 0·2505 gave 0·1675 chloride of silver = 16·49 chlorine.
VI. 0·177 gave 0·1155 = 16·09 chlorine.

	I.	II.	III.
Carbon .	66·09	65·88	65·87
Hydrogen	4·00	3·86	4·11
Chlorine .	16·20	16·20	16·20
Oxygen .	13·71	14·06	13·82
	100·00	100·00	100·00

The substance employed for these analyses was prepared at three different times. I am sorry to say, however, that I have been unable to deduce any probable formula from these analyses.

When this crystalline compound is heated in an alcoholic solution of potash, benzoate of potash, chloride of potassium with a little benzoic æther are immediately formed, the colour of the solution becoming at the same time of an inky blackness. When this alkaline solution is treated with an excess of muriatic acid, the benzoic acid precipitates on the cooling of the liquid. The benzoic acid is mixed with a good deal of a dark-coloured tenacious semifluid resin, which has a strong empyreumatic smell, exactly similar to that of creosote; so that chloride of potassium and benzoic acid are not the only products of this decomposition. The benzoic acid was purified by repeated crystallizations and subjected to analysis.

0·1572 grm. gave 0·3986 carbonic acid and 0·0734 water.

	Found.	Calculated.
Carbon .	69·15	68·85
Hydrogen	5·19	4·92
Oxygen .	25·66	26·23
	100·00	100·00

The liquid chlorine compound already mentioned can readily be dissolved out of the bibulous papers by treating them with æther. The liquid compound however always retained a considerable quantity of the crystals dissolved in it, just as oleine does margarine or stearine in the fats, so that I was quite unable to purify it. This is much to be regretted, as it appeared to be much the more interesting of the two, as it is that which possesses the very pungent smell already mentioned.

When the liquid compound was treated with an alcoholic solution of potash, it was also resolved into benzoate of potash, chloride of potassium, and a similar resinous matter, the quantity of which appeared to be even greater than that yielded by the solid compound.

Neutral benzoate of lead, formed by adding a solution of benzoate of soda to acetate of lead, was also subjected to destructive distillation. A small quantity of benzoic acid came over mixed with a little of an aromatic oil, the smell of which was quite different from that yielded by benzoate of copper. I was unable, however, to detect the presence of any neutral crystalline body.

Glasgow, May 12, 1845.

III. *On the Aberration of Light. By* G. G. Stokes, *M.A., Fellow of Pembroke College, Cambridge*[*].

THE general explanation of the phænomenon of aberration is so simple, and the coincidence of the value of the velocity of light thence deduced with that derived from observations of eclipses of Jupiter's satellites so remarkable, as to leave no doubt on the mind as to the truth of that explanation. But when we examine the cause of the phænomenon more closely, it is far from being so simple as it appears at first sight. On the theory of emissions, indeed, there is little difficulty; and it would seem that the more particular explanation of the cause of aberration usually given, which depends on the consideration of the motion of a telescope as light passes from its object-glass to its cross wires, has reference especially to this theory; for it does not apply to the theory of undulations, unless we make the rather startling hypothesis, that the luminiferous æther passes freely through the sides of the telescope and through the earth itself. The undulatory theory of light, however, explains so simply and so beautifully the most complicated phænomena, that we are naturally led to regard aberration as a phænomenon unexplained by it, but not incompatible with it.

The object of the present communication is to attempt an explanation of the cause of aberration which shall be in accordance with the theory of undulations. I shall suppose that the earth and planets carry a portion of the æther along with them so that the æther close to their surfaces is at rest relatively to those surfaces, while its velocity alters as we recede

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[*] Communicated by the Author.

from the surface, till, at no great distance, it is at rest in space. According to the undulatory theory, the direction in which a heavenly body is seen is normal to the fronts of the waves which have emanated from it, and which have reached the neighbourhood of the observer, the æther near him being supposed to be at rest relatively to him. If the æther in space were at rest, the front of a wave of light at any instant being given, its front at any future time could be found by the method explained in Airy's Tracts. If the æther were in motion, and the velocity of propagation of light were infinitely small, the wave's front would be displaced as a surface of particles of the æther. Neither of these suppositions is however true, for the æther moves while light is propagated through it. In the following investigation I suppose that the displacements of a wave's front in an elementary portion of time due to the two causes just considered take place independently.

Let u, v, w be the resolved parts along the rectangular axes of x, y, z, of the velocity of the particle of æther whose co-ordinates are x, y, z, and let V be the velocity of light supposing the æther at rest. In consequence of the distance of the heavenly bodies, it will be quite unnecessary to consider any waves but those which are plane, except in so far as they are distorted by the motion of the æther. Let the axis of z be taken in, or nearly in the direction of propagation of the wave considered, so that the equation to the wave's front at any time will be

$$z = \mathrm{C} + \mathrm{V}t + \zeta, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

C being a constant, t the time, and ζ a small quantity, a function of x, y and t. Since u, v, w and ζ are of the order of the aberration, their squares and products may be neglected.

Denoting by α, β, γ the angles which the normal to the wave's front at the point (x, y, z) makes with the axes, we have, to the first order of approximation,

$$\cos\alpha = -\frac{d\zeta}{dx}, \quad \cos\beta = -\frac{d\zeta}{dy}, \quad \cos\gamma = 1\,; \quad . \quad . \quad (2.)$$

and if we take a length V dt along this normal, the co-ordinates of its extremity will be

$$x - \frac{d\zeta}{dx}\mathrm{V}\,dt, \quad y - \frac{d\zeta}{dy}\mathrm{V}\,dt, \quad z + \mathrm{V}\,dt.$$

If the æther were at rest, the locus of these extremities would be the wave's front at the time $t + dt$, but since it is in motion, the co-ordinates of those extremities must be further increased by $u\,dt$, $v\,dt$, $w\,dt$. Denoting then by x', y', z' the co-ordinates of the point of the wave's front at the time $t + dt$, which corresponds to the point (x, y, z) in its front at the time t, we have

$$x' = x + \left(u - V\frac{d\zeta}{dx}\right)dt, \quad y' = y + \left(v - V\frac{d\zeta}{dy}\right)dt,$$
$$z' = z + (w + V)\,dt;$$

and eliminating x, y and z from these equations and (1.), and denoting ζ by $f(x, y, t)$, we have for the equation to the wave's front at the time $t + dt$,

$$z' - (w + V)\,dt = C + Vt$$
$$+ f\left\{x' - \left(u - V\frac{d\zeta}{dx}\right)dt, \quad y' - \left(v - V\frac{d\zeta}{dy}\right)dt, \ t\right\},$$

or, expanding, neglecting dt^2 and the square of the aberration, and suppressing the accents of x, y and z,

$$z = C + Vt + \zeta + (w + V)\,dt. \quad . \quad . \quad . \quad (3.)$$

But from the definition of ζ it follows that the equation to the wave's front at the time $t + dt$ will be got from (1.) by putting $t + dt$ for t, and we have therefore for this equation,

$$z = C + Vt + \zeta + \left(V + \frac{d\zeta}{dt}\right)dt. \quad . \quad . \quad . \quad (4.)$$

Comparing the identical equations (3.) and (4.), we have

$$\frac{d\zeta}{dt} = w.$$

This equation gives $\zeta = \int w\,dt$: but in the small term ζ we may replace $\int w\,dt$ by $\frac{1}{V}\int w\,dz$: this comes to taking the approximate value of z given by the equation $z = C + Vt$, instead of t, for the parameter of the system of surfaces formed by the wave's front in its successive positions. Hence equation (1.) becomes

$$z = C + Vt + \frac{1}{V}\int w\,dz.$$

Combining the value of ζ just found with equations (2.), we get, to a first approximation,

$$\alpha - \frac{\pi}{2} = \frac{1}{V}\int \frac{dw}{dx}\,dz, \quad \beta - \frac{\pi}{2} = \frac{1}{V}\int \frac{dw}{dy}\,dz, \qquad (5.)$$

equations which might very easily be proved directly in a more geometrical manner.

If random values are assigned to u, v and w, the law of aberration resulting from these equations will be a complicated one; but if u, v and w are such that $u\,dx+v\,dy+w\,dz$ is an exact differential, we have

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

whence, denoting by the suffixes 1, 2 the values of the variables belonging to the first and second limits respectively, we obtain

$$\alpha_2-\alpha_1=\frac{u_2-u_1}{V}, \quad \beta_2-\beta_1=\frac{v_2-v_1}{V}. \quad . \quad . \quad (6.)$$

If the motion of the æther be such that $u\,dx+v\,dy+w\,dz$ is an exact differential for one system of rectangular axes, it is easy to prove, by the transformation of co-ordinates, that it is an exact differential for any other system. Hence the formulæ (6.) will hold good, not merely for light propagated in the direction first considered, but for light propagated in any direction, the direction of propagation being taken in each case for the axis of z. If we assume that $u\,dx+v\,dy+w\,dz$ is an exact differential for that part of the motion of the æther which is due to the motions of translation of the earth and planets, it does not therefore follow that the same is true for that part which depends on their motions of rotation. Moreover, the diurnal aberration is too small to be detected by observation, or at least to be measured with any accuracy, and I shall therefore neglect it.

It is not difficult to show that the formulæ (6.) lead to the known law of aberration. In applying them to the case of a star, if we begin the integrations in equations (5.) at a point situated at such a distance from the earth that the motion of the æther, and consequently the resulting change in the direction of the light, is insensible, we shall have $u_1=0$, $v_1=0$; and if, moreover, we take the plane xz to pass through the direction of the earth's motion, we shall have

$$v_2=0, \quad \beta_2-\beta_1=0,$$

and

$$\alpha_2-\alpha_1=\frac{u_2}{V};$$

that is, the star will appear to be displaced towards the direction in which the earth is moving, through an angle equal to the ratio of the velocity of the earth to that of light, multiplied by the sine of the angle between the direction of the earth's motion and the line joining the earth and the star.

In considering the effect of aberration on a planet, it will be convenient to divide the integrations in equation (5.) into three parts, first integrating from the point considered on the surface of the planet to a distance at which the motion of the æther may be neglected, then to a point near the earth where we may still neglect the motion of the æther, and lastly to the point of the earth's surface at which the planet is viewed. For the first part we shall have $u_2 = 0$, $v_2 = 0$, and u_1, v_1 will be the resolved parts of the planet's velocity. The increments of α and β for the first interval will be, therefore, $-\frac{u_1}{V}$, $-\frac{v_1}{V}$. For the second interval α and β will remain constant, while for the third their increments will be $\frac{u_2}{V}$, $\frac{v_2}{V}$, just as in the case of a star, u_2 and v_2 being now the resolved parts of the earth's velocity.

Fig. 1.

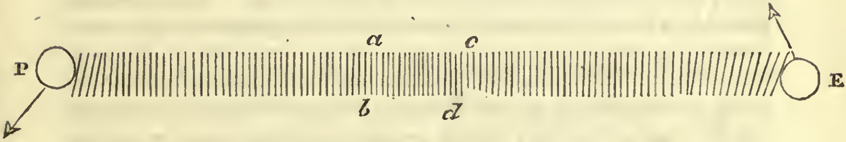

Fig. 2.

Fig. 1 represents what is conceived to take place. P is the planet in the position it had when the light quitted it; E the earth in the position it has when the light reaches it. The lines *a b*, *c d*, &c. represent a small portion of a wave of light in its successive positions. The arrows represent the directions in which P and E may be conceived to move. The breadth *a b* is supposed to be comparable to the breadth of a telescope. In fig. 2, *p m n e* represents an orthogonal trajectory to the surfaces *a b*, *c d*, &c.; *p* is the point of the planet from which the light starts, *e* the point of the earth which it reaches. The trajectory *p m n e* may be considered a straight line, except near the ends *p* and *e*, where it will be a little curved, as from *p* to *m* and from *e* to *n*. The curvature at *e* will have the same effect on the apparent position of the planet as it would have on that of a star in the same direction: as to the curvature at *p*, if we draw *p q* perpendicular to *m n* produced, the curvature will have the effect of causing *p* to be seen as if it were at *q*. Now the angle between the tangents at *p* and *m* being that through which a star in the direction of *e* is displaced by aberration to an observer at *p*, and the di-

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* The lines towards P in fig. 1. should lean in the opposite direction.

stance $p\,m$ being by hypothesis small (two or three radii of the planet suppose), it follows that the angle peq is extremely small, and may be neglected. Hence a planet will appear to be displaced from the position which it had when the light left it, just as a star in the same direction is displaced. But besides this, the planet has moved from P while the light has been travelling to E. These two considerations combined lead to the formula for aberration, which is applicable to the planets, as is shown in treatises on astronomy. The same reasoning which applies to a planet will apply equally to the sun, the moon, or a comet.

To give an idea of the sort of magnitudes neglected in neglecting $p\,q$, suppose $p\,m$ equal to the diameter of P, and suppose the curvature from p to m uniform. Let r be the radius of P, v its velocity, and R the distance P E. The greatest possible value of the angle between the tangents at p and m is $\frac{v}{\mathrm{V}}$. In this case we should have $\angle p\,e\,q = \frac{v\,r}{\mathrm{V\,R}} = \frac{v}{\mathrm{V}}\mathrm{D}$, D being the semidiameter of P as seen from E. Hence the angle $p\,e\,q$ must be very much greater for the moon than for any other body of the solar system; for in the case of the planets the value of v is in no instance double its value for the earth or moon, while their discs are very small compared with that of the moon; and in the case of the sun, although its disc is about as large as that of the moon, its velocity round the centre of gravity of the solar system is very small. It would indeed be more correct to suppose the sun's centre absolutely at rest, since all our measurements are referred to it, and not to the centre of gravity of the solar system. Taking then the case of the moon, and supposing $\frac{v}{\mathrm{V}} = \frac{20''}{180^\circ}\pi$, $\mathrm{D} = 15'$, we find that the angle $p\,e\,q$ is about $\frac{1}{11}$th of a second, an insensible quantity.

If we suppose the whole solar system to be moving in space with a velocity comparable with that of the earth round the sun, it follows from the linearity of the equations employed, that we may consider this motion separately. It is easy to show, that as far as regards this motion, the sun, moon, and planets will come into the positions in which they are seen just at the instant that the light from them reaches the earth. With respect to the stars also, that part of the aberration which varies with the time of year, the only part which can be observed, will not be affected. If we suppose the æther which fills the portion of space occupied by the solar system to be moving in a current, with a velocity comparable with that of the earth in its orbit, the result will still be the same. For if we suppose a velocity equal and opposite to that of the æther to be impressed, both on the æther and on the bodies of the solar system, the case is reduced to that of the solar system moving through the æther supposed to be at rest.

IV. *On the Structure of Electro-precipitated Metals.*

By Warren De la Rue, *Esq.**

THE following observations, being the result of an extensive series of experiments on the practical application of the processes of electro-metallurgy, will, I am induced to believe, be acceptable to the Chemical Society.

The various appearances of the metallic deposit are familiar to all manipulators in electro-metallurgy, and are distinguished by the names crystalline, lesser crystalline, malleable, sandy and spongy; the latter being produced by an excess of power in the battery, the first by too small a power in relation to the strength of the solution operated on. All these deposits are however merely modifications of each other, they are essentially crystalline, and even the malleable, or in other words the most cohesive, is very inferior in strength to metals wrought by the processes in ordinary use.

The malleable is that deposit usually required; yet, even with all the art of a practised electro-metallurgist, it is difficult for a lengthened period to obtain it, inasmuch as the power of the battery, the temperature of the air, and consequently the conducting power of the fluids composing the circuit are constantly changing their relation to the strength of the electrolyte to be decomposed. There are other causes presently to be considered which also play an important part in producing these difficulties, and which we shall better understand by considering what effect the form of the matrix and the nature of its original surface have on the resulting precipitate.

It is well known to persons conversant with the precipitation of metals from their respective solutions by means of voltaic electricity, that these solutions become exhausted of the metal at the cathode to such an extent, that if we place the cathode on the surface of the liquid all action after a short time ceases: the exhausted liquid being specifically lighter, no mechanical transfer of fresh liquid takes place†, and conse-

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

† Professor Daniell and Dr. Miller, in a paper on the electrolysis of secondary compounds, have entered into the investigation of these phænomena.

quently the process is stopped. This exhaustion of the electrolyte is the primary cause of the difficulties of the process.

If, for example, an attempt be made to produce a solid medal by depositing between the respective matrixes of the obverse and reverse, the attempt would undoubtedly fail; the opposite and approaching deposits never join and form one solid piece. The cause of this is sufficiently obvious, and can be easily traced to the exhaustion of the electrolyte; for as the opposite deposits approach they render the cavity smaller and smaller, and at last it becomes so reduced that its capillarity interferes with the renewal of the liquid, and eventually the action ceases, a cavity containing the exhausted liquid remains, and no true juncture takes place.

The following experiment proves, in a striking manner, that the preceding is not merely a conjectural case. I prepared a matrix by cutting angular lines about one-twentieth of an inch deep in a metallic plate, the angle of the opening being about 35 degrees. On this was deposited copper to about the eighth of an inch in thickness; the deposit was what would be termed exceedingly good, and, to a practised eye, uniform, there being but a slight indication of the original lines at the back of the cast; yet the copper could be broken with great facility through the centre of each line, in a plane bisecting the angle, presenting to the eye a very smooth and uniform fracture, and giving the idea of its having been rather purposely cut through than broken at random. Here we have an example of the interstitial space being not only left between the walls of the original groove, but existing through the additional eighth of an inch of deposited metal, from the obvious cause of the incapability of the included liquid acting the part of a cathode. Plate I. fig. 1 is a diagram of the section through the matrix and deposited copper; A being the matrix, B the deposited metal.

A matrix from a wood engraving presents a good example of the effect which the form of the mould has on the resulting deposited counterpart: in it we have a vast assemblage of minute grooves, the reverse of the lines in the engraving; these grooves are never perfectly filled, from the cause before stated, and the deposit is consequently cut up into an assemblage of minute pieces, joined *apparently though not in reality*, and nicely held together by the intricacy of their interlacings. The sky, from its consisting of a number of straight lines in the engraving, is especially tender and troublesome to obtain perfect. Though I have chosen the copy of a wood engraving as an example, from its presenting an extreme case of difficulty, yet the same effect is produced under other circumstances; for instance, in the silver coating of a cylindrical vessel having a flat bottom; in it there will infallibly exist a fissure at the angle formed by the cylinder and the bottom. Hence we see the necessity of *rounding off* as much as possible all angles where we intend to deposit metals with the aid of electricity.

The surface of the mould must consequently have an obvious effect on the deposit, inasmuch as no surface can be perfect in its polish; hence it must necessarily follow that the more gradual the undulations and depressions are, the more cohesive will be the deposit; a wax matrix is perhaps one of the best substances to deposit on, and a polytype matrix, obtained by pressing a woodcut into a melted metallic alloy when near its point of solidification, one of the worst, for we have on it a copy of all the pores of the wood; besides, the alloy being crystalline, its surface is, when microscopically examined, exceedingly rough. All abrupt deviations from the original plane, though small, have an injurious effect on the resulting deposit, and this increases with the depth; but deep or shallow, the result is the same, and differs only in amount.

I have stated that all electro-metallic deposits are essentially crystalline in their structure; this is readily proved by submitting them to the test of microscopic investigation, and however carefully they be prepared, we shall perceive that they consist of an assemblage of minute crystals more or less perfectly formed. In Plate I. fig. 5 is represented a portion of a beautiful deposit on a polished piece of metal after five minutes' action, the magnifying power employed being 300 times. In it the crystalline structure is rendered apparent, and we see moreover, here and there, groups of crystals starting up perpendicularly from the surface to a considerable height. Such being the character of the first layers, the crystals, as in the crystallization of a salt from its solution, increase in size, and could we even obtain a perfect surface to commence on, we must have, from the very nature of the process, the production of inequalities, to which the shooting of these crystals above the general surface mainly contributes; these irregularities, as before explained, can never perfectly be filled up, and a porous structure must result; add to this, that the crystallization likewise spreads laterally from the summit of one group of crystals to that of those in the neighbourhood, and it will be readily perceived that spaces are here and there enclosed, which can only become filled to the extent that the included liquid is capable of yielding metal. Hence the microscope reveals a structure such as I have delineated in fig. 2, which is a section of electro-deposited copper magnified 100 times linear; the section is a cutting made with sharp scissors through a plate of about the fortieth of an inch in thickness, having the character of the lesser crystalline, and appeared to the unassisted eye perfectly polished and compact. Fig. 3 is a representation of the back of the same specimen, showing clearly its crystalline structure; this is likewise magnified 100 times. Even the most compact specimens present, under a sufficient magnifying power (see fig. 5), the same porous appearance; the pores are smaller, it is true, but greater in number, and the character of the deposit is the same. I here again repeat that the electro-metallic deposit is essentially crystalline in structure; in fact it is but a tissue of crystals *interlacing* but *not adhering*. We may diminish the power of the battery with respect to the quantity of metallic salt present in the electrolyte, so as to obtain, by these favourable circumstances, large and well-formed crystals,—we may go on increasing the power, and produce the crystals more and more hurriedly, and consequently smaller and smaller and less perfectly formed, but we ultimately reach a point in the quantity of electricity transmitted, that, if we increase it, the electrolyte cannot be renewed with sufficient rapidity at the surface of the cathode, and we have larger spaces left unfilled, thus producing the sandy deposit. Lastly, we may increase it to so great an extent that the metal assumes the form of the spongy deposit; but still all are crystalline.

The ridges or lines which are frequently seen at the back of electro-casts, placed vertically in the trough, more especially in those where there are sharp angles jutting out from the matrix, are produced by these points impeding the upward flow of partially exhausted liquid, and causing it to run in little detached streams; where these exist *there* the deposit is formed less quickly than in those parts where the liquid is stronger, consequently we have a hollow or groove produced, which remains permanent. To produce an interchange of the fluid in the precipitating troughs, a constant stream of fresh liquid was caused to run in at the bottom whilst the weak fluid overflowed at the top: the resulting current in meeting the upward flow of the fluid at the surface of the electro-cast produced a series of vortices, and changed the vertical ridges into a series of curiously-curved lines, which were equally objectionable; hence this scheme had to be rejected.

Fig. 4 is a magnified representation of a curious electro-deposit of silver, kindly furnished me by Mr. Napier, who has named it "silver sponge," from its resemblance to that substance. Under the microscope it is a most splendid arborescent assemblage of crystals, and presents us with an extreme case of crystallization in electro-metallic deposits. The drawing is made 200 times the natural size.

To return to an electro-copy of a wood engraving: it is found by examination under the microscope, and would be anticipated from the preceding remarks, that each line has a space in its centre; they are in fact exceedingly hollow, and present much the appearance of the hollow crystallized cakes of sugar made by confectioners, and hence we see the advantage of thoroughly tinning it at the back as soon as we have removed the cast from its matrix; the tin insinuates itself into a great number of the pores and binds the whole firmly together. With the help of a little chloride of zinc the tinning is effected very readily, and should be done without disturbing the structure by filing.

There is one phænomenon, connected with the employment of electro-casts, exceedingly curious—it is their uselessness for printing with vermilion ink (sulphuret of mercury), which is not the case with respect to ordinary engraved copper blocks. When an electro-cast is inked over with vermilion printing ink, and a few impressions have been taken, the vermilion is blackened, and as the process is continued the copper begins to get white, and at last so much mercury is precipitated on the surface as to prevent the adherence of the ink. I am inclined to think that the porous and divided nature of the electro-cast is the sole cause of its decomposing the vermilion, and that the purity of the copper is not concerned in it.

In most of the preceding remarks I have alluded to deposits of copper, but I wish it to be understood that they apply equally to gold, silver or other metals; and such being the nature of precipitated metals, I consider that though the electro-metallurgic processes are a valuable addition to the arts, there are uses to which they should not be applied; as, for example, the coating of one metal with another when it is intended to protect the covered metal from the action of certain fluids, at all events wihout the precaution in all possible cases of subsequently partially fusing the coating metal.

The production of ornamental pieces of plate, and the silvering or gilding of such objects as are not subject to much friction, is not open to the same objections. Platinum and palladium also, if ever obtained by the electro-metallurgic process in plates or other forms, should subsequently be subjected to the process of welding and hammering. It has been proposed to copper the bottoms of ships by this process; apart from the great difficulty of effecting this on a large scale, the copper in my opinion would be far too friable for such a use.

Copper duplicates of steel or copper engravings, it is well known, are not nearly so durable as the original plates, yet in some cases they may be advantageously employed,—not however where very large numbers are likely to be required, as, for example, in the printing of bank notes. I was once consulted as to the practicability of coating the interior of the air-pumps and the valve facings of large marine engines by the electro-metallurgic process, and I include this as one of the improper applications of the art. In conclusion, I again repeat, that the processes of electro-metallurgy are a valuable acquisition to the arts, yet it is necessary to bear in mind its defects, in order that we may not apply it to purposes for which it is unfitted.

V. *On the true Composition of Chlorindatmit.*

By AUGUST WILHELM HOFMANN, *Ph.D.*[*]

IN a preceding memoir on the Metamorphoses of Indigo, I have described under the name of trichloraniline a body which may be regarded as aniline, in which three equivalents of hydrogen are replaced by an equal number of chlorine:

$$\text{Aniline} \quad . \quad . \quad . \quad = C_{12}\,H_7\,N$$

$$\text{Trichloraniline} \quad . \quad = C_{12}\left\{\begin{matrix} H_4 \\ Cl_3 \end{matrix}\right\}N.$$

I had already, a year and a half ago, observed the formation of this body by the action of chlorine on aniline[†], but at that time the quantity obtained was so small that I was obliged to content myself with a conjectural opinion of its constitution. More lately I found that the same substance was produced by the treatment of chloraniline with chlorine. Even by the last-mentioned way the quantity obtained was but small. With all my efforts I could not obtain more than was just sufficient for one combustion, which unfortunately gave somewhat too much carbon[‡]. Although myself thoroughly convinced of the exactness of the given formula,—the method of formation of this body, the complete analogy with tribromaniline and chlorodibromaniline, independently of the analysis adduced, being sufficient evidences,—I thought it however desirable to place the composition of trichloraniline beyond every doubt by the production of better numbers.

At first I thought of obtaining these numbers by a repeti-

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[*] Communicated by the Chemical Society; having been read February 17, 1845.

[†] Liebig's *Ann.* vol. xlvii. p. 68, and Phil. Mag. S. 3. vol. xxvi. p. 199.

[‡] See the preceding Memoir.

tion of the preparation and analysis, until I found the object could be attained by a much more convenient way.

For this purpose I must here mention some results obtained by Erdmann in his excellent investigation on indigo.

If indigo suspended in water be treated with chlorine, it is transformed, as is known, into a reddish yellow mass, which is a mixture of different substances. On submitting this to distillation with water, chlorisatine and dichlorisatine remain in the retort, whilst another body volatilizes with the watery vapour, which according to Erdmann contained no nitrogen. He called this substance chlorindoptene*, and gave for it the following formula:

$$C_{32}\, H_8\, Cl_8\, O_4.$$

By later experiments Erdmann found however that on distilling chlorindoptene with potash or carbonate of potash it is decomposed into a neutral body which passes over, and an acid which remains in the retort united to the potash. The latter is chlorindoptenic acid (chlorophenisic acid of Laurent); the former Erdmann called chlorindatmit, and gave it the formula $C_{12}\, H_4\, Cl_3\, O_2$.

Whence comes this body? In what relation does it stand to the other members of the indigo or phenyle series? Among the many derivatives of indigo made known to us by the investigations of Erdmann and Laurent it stands quite isolated.

On comparing the properties which Erdmann described as belonging to chlorindatmit with those I had observed in trichloraniline, it appeared to me in the highest degree probable that these bodies were identical. The properties of both bodies correspond exactly, and the method of formation is extremely alike. The production of trichloraniline, as well as that of chlorindatmit, is accompanied by the formation of chlorindoptenic acid. Erdmann had determined the carbon, hydrogen and chlorine in chlorindatmit. His numbers correspond completely with the composition of trichloraniline. For comparison I placed together the theoretical numbers of this body and the analytical results of Erdmann.

Chlorindatmit.		Trichloraniline.	
Carbon . .	=36·42†	Carbon . .	=36·66
Hydrogen .	= 2·23	Hydrogen .	= 2·03
Chlorine . .	=53·58	Chlorine . .	=54·09
Loss . . .	= 7·79	Nitrogen . .	= 7·22
	100·00		100·00

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* *Journ. für Prakt. Chem.*, vol. xix. p. 334.

† The equivalent of carbon is here taken as 75.

Erdmann did not look for nitrogen in chlorindatmit as he had found none in chlorindoptene. Did chlorindoptene consist of equal equivalents of chlorindoptenic acid and chlorindatmit, such a compound (assuming chlorindatmit to be an azotized body) would contain 3·61 per cent. nitrogen, a quantity which could not have been overlooked. It is, however, not composed of a like quantity of each element, but the chlorindoptenic acid predominates very much, so that the amount of nitrogen is much reduced, and may be easily overlooked. On the other hand, the proportion of carbon and hydrogen cannot essentially alter, as chlorindoptenic acid and chlorindatmit contain nearly equal quantities.

To determine this question by experiment, I treated with chlorine, according to Erdmann's method, a quarter of a pound of reduced and again oxidized indigo suspended in water. Although the operation was conducted in sunshine, yet it was only after three or four days that the indigo was converted into the brownish-yellow body. The whole mass, together with the fluid, was poured into a retort and distilled over an open fire. In the receiver a yellow crystalline substance condensed and swam on the surface of the water, which had also passed over and contained hydrochloric acid. The distilled product was saturated with potash ley, which communicates a dark colour to the fluid from the solution of a portion of the crystals, and the whole was again submitted to distillation. By gently warming the retort, a vapour was evolved which condensed in the neck in the form of fine white crystals like hairs, about an inch long, which, on bringing the fluid to the boiling-point, passed over with the watery vapours as an oil and again crystallized in the receiver. The crystals were separated by filtration and dried. The investigation was confined to the single question, whether chlorindatmit so prepared contained nitrogen or not?

On passing the vapour of chlorindatmit over fused potassium a large quantity of cyanide of potassium was formed, which evolved freely hydrocyanic acid on the addition of an acid. A mixture of a proto- and persalt of iron produced a gray-brown precipitate, which assumed the pure colour of Berlin blue by the addition of a little hydrochloric acid. Further, on heating with the soda-lime mixture a quantity of ammonia was given off. Chlorindatmit therefore contains nitrogen.

From the preceding facts several conclusions may be drawn.

1st. The body produced by the action of chlorine on aniline and chloraniline has in reality the following composition:

$$C_{12}\left\{\begin{matrix} H_4 \\ Cl_3 \end{matrix}\right\}N.$$

2nd. The chlorindatmit of Erdmann is nothing but trichloraniline.

3rd. The name chlorindoptene merely denotes an indefinite mixture of chlorindoptenic acid and trichloraniline.

The formation of trichloraniline by the action of chlorine on indigo is not at all surprising. The transformation of indigo into nitropicric and chlorindoptenic acids, and into aniline, proves sufficiently that there are present in this body the conditions necessary for the production of all the members of the phenyle series. The action of potash and of chlorine on indigo is in this respect essentially the same. The only difference is, that in the first instance a pure product of oxidation is obtained, whilst in the second a number of the hydrogen equivalents of this product is replaced by a corresponding number of the oxidizing agent, viz. chlorine. In the one case aniline is formed, in the other trichloraniline. The latest investigations of Cahours*, in which he succeeded in obtaining salicylic acid by treating indigo with hydrate of potash, show that out of this latter substance hydrate of phenyle can likewise be produced, the compound free from chlorine, which corresponds to chlorindoptenic acid.

In an analogous manner Erdmann† obtained, by the action of bromine on indigo, bromisatine, dibromisatine and bromindoptene. This last substance, distilled with potash, is decomposed into bromindoptenic acid and bromindatmit. Erdmann did not analyse bromindatmit; but it is scarcely necessary to remark that this body is nothing but tribromaniline (Fritzsche's bromaniloid),

$$C_{12}\left\{\begin{matrix} H_4 \\ Br_3 \end{matrix}\right\}N.$$

VI. *Analyses of Farm-Yard Manure, and of Coal-Gas.*
By THOMAS RICHARDSON, *Esq.*‡

THE accompanying analysis of farm-yard manure is part of an investigation in which I am at present engaged, and the results show, rather unexpectedly, that this manure has a somewhat similar composition in different localities.

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* *Compt. Rend.*

† *Journ. für Prakt. Chem.*, vol. xix. p. 358.

‡ Communicated by the Chemical Society; having been read February 17, 1845.

(See Boussingault's *Economie Rurale.*) The methods of analysis were those in general use. The manure was an average sample, and taken just previous to its being applied to the soil.

Farm-Yard Manure.

1. *Fresh.*

Water	64·96	
Organic matter . . .	24·71	
Inorganic salts, &c. .	10·33	100·00

2. *Dried at* 212°.

Carbon	37·40	
Hydrogen	5·27	
Oxygen	25·52	
Nitrogen	1·76	
Ashes	30·05	100·00

3. *Inorganic Matter.*

A. *Portion Soluble in Water.*

Potash	3·22	
Soda	2·73	
Lime	0·34	
Magnesia	0·26	
Sulphuric acid . . .	3·27	
Chlorine	3·15	
Silica	0·04	13·01

B. *Portion partially Soluble in Muriatic Acid.*

Silica	27·01	
Phosphate of lime . .	7·11	
Phosphate of magnesia.	2·26	
Phosphate of iron . .	4·68	
Phosphate of manganese	trace	
Phosphate of alumina ?.	trace	
Carbonate of lime . .	9·34	
Carbonate of magnesia .	1·63	
Sand	30·99	
Carbon	·83	
Alkali and loss . . .	3·14	86·99
		100·00

The following analyses are of the coal-gas which is supplied to the town of Newcastle-on-Tyne. It contained in 100 parts,—

	I.	II.
Olefiant gas	10·19	9·25
Carburetted hydrogen	31·35	36·05
Hydrogen	28·80	30·17
Carbonic oxide	16·28	11·42
Azote	13·35	14·01
Carbonic acid	a trace	a trace
Atmospheric air	0·48	0·50
Naphtha vapour	a trace	a trace
Ammonia	a trace	——
	100·35	101·40

VII. *Contributions to Actino-Chemistry. By* Robert Hunt, *Esq., Secretary to the Royal Cornwall Polytechnic Society, &c.**

MANY of the changes produced upon photographic preparations, by the influence of the solar rays, are of a remarkable character, and few of them in the present state of our knowledge can be satisfactorily explained. In some instances it would appear that new properties are imparted to bodies by exposure to sunshine; in others, that radiation has the power of disturbing the known chemical forces, and apparently establishing a new order of affinities; whilst in all we are forced to recognise the operations of a principle, the nature of which is involved in the most perplexing uncertainty. In the hope of being enabled to follow out some of these phænomena, under circumstances more favourable for investigation than those which offer themselves in the ordinary method of pursuing photographic experiments with the chemical compounds spread upon paper, where we have organic matter interfering with the results, I have been led to examine with attention the changes which take place under more simple conditions. It must be evident that a subject so extensive as this new branch of inquiry, embracing all material elements and their combinations, will not readily admit of satisfactory generalization; in this paper, indeed, I am scarcely able to do more than record a few facts, under particular heads, without any attempt at systematic arrangement.

To prevent any misinterpretation of terms, or confusion of ideas, it will be necessary to make a few preliminary remarks. The researches of Sir John Herschel and others most distinctly prove that the Light, Heat and Chemical Power of the solar rays are three distinct classes of phæno-

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* Communicated by the Chemical Society; having been read March 3, 1845.

mena. It has indeed been suggested they may be *distinct principles*, and a considerable amount of experimental evidence has been brought forward in support of this position. Without entering into the argument in this place, I desire simply to impress strongly the necessity of considering the *three effects* of luminous agency and colour, of calorific action, and of what has been called photographic influence, as phænomena depending upon three *things* (whether these be independent principles, or only modifications of one great immaterial element, not being here considered), to which it is necessary to give distinctive appellations. Light and heat are terms about which, in their familiar acceptation, there can be no mistake; but it is to be regretted that some confused terms have had the sanction of some eminent experimentalists, leading, as it appears to me, to a sad complexity of ideas. An "*invisible chemical coloration*" is the hypothesis of one philosopher and "*invisible light*" the epithet of another, introduced in memoirs of great interest, in which the changes produced by the dark chemical rays, as they have been called, are sought to be explained. In support of this view it has been suggested that rays of light may exist which do not produce any excitement of the optic nerves of man, and it has been assumed that these rays may still be sufficiently powerful to produce vision in the night-roving animals; howbeit, of this there is not the slightest proof, and all the phænomena of vision in the cat, owl, and the like, in comparative, not absolute darkness, may be physiologically explained. However, in this paper LIGHT will be used to distinguish those radiations which produce vision and colour, HEAT, those affecting any thermic phænomena; whilst those radiations on which certain chemical changes are supposed to depend will be distinguished by the epithet of ACTINIC.

At the Meeting of the British Association at York, it was proposed by Sir John Herschel that all those phænomena which exhibit change of condition under the influence of the solar rays should be distinguished, as forming a peculiar province of chemistry, by the term ACTINO-CHEMISTRY, and this was generally approved by the Chemical Section. ACTINISM it is proposed shall in future be used to express that principle or modification upon which these phænomena depend. ACTINICITY will distinguish this power in action, and ACTINIZED I shall use to signify any substance which has been exposed to *Actinic* influence. In the progress of the examination of these physico-chemical phænomena other epithets must of necessity be formed; for example, it will be convenient to speak of media which obstruct the passage of actinism, like the quadri-sulphuret of lime and the solution of the bichromate of potash, as ADIACTINIC, whilst media admitting the free permeation of this power, as colourless glass, or a solution of the ammoniuret of copper, would be distinguished as DIACTINIC*. Any instrument used for measuring the amount of chemical disturbance effected by the solar rays, would be called, as was proposed by Sir John Herschel, an ACTINOGRAPH. With these preliminary observations I do not think any difficulty will arise from the terms made use of in this paper.

INFLUENCE OF THE SOLAR RAYS ON PRECIPITATION.

In 1832 Sir John Herschel communicated the remarkable fact, that when a solution of platinum in nitro-muriatic acid, which has been neutralized by the addition of lime, and has been well-cleared by filtration, is mixed with lime-water in the dark, no precipitation, or scarcely any, takes place, but when (being thoroughly cleared of any sediment) this mixture is exposed to sunshine it instantly becomes milky, and a white or yellowish-white precipitate speedily falls.

By exposing this mixture behind coloured media, Sir John Herschel found that the effect was due to the influence of the most refrangible rays. I have placed this mixture in small glass tubes, and so arranged them that they were individually exposed to a separate ray of the spectrum. After an exposure of one hour the following results were obtained, the precipitates having been carefully washed and dried in the tubes in which they were formed.

Most refrangible rays beyond the visible spectrum	0·07 gr.
Violet rays	1·05 ...
Indigo rays	0·60 ...
Blue rays	0·45 ...
Green rays	0·10 ...
Yellow and orange rays	
Red rays	0·05 ...

It is a fact worthy of especial notice, that this precipitation is so dependent upon the amount of sunshine, that precipitates obtained in the same time, being carefully weighed off, will show the relative amounts of actinic influence to which they have been exposed.

Manganate of Potash.—A solution of this body having been made in the dark, was placed in two glass vessels and set aside. After having been kept in darkness for two hours, the

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* These terms are formed from those proposed by M. Melloni in his "New Nomenclature for the Science of Calorific Radiations," a slight liberty being taken with them for the sake of euphony.

solutions remained as clear as at first. One of the vessels with its contents was then removed into the sunshine, when the solution immediately became cloudy, and was very speedily decomposed, the precipitate falling heavily. By experiments with the spectrum, I have since found that the precipitation is due almost entirely to the more refrangible rays. I have not been enabled to decide with that degree of accuracy I could desire, in which ray the maximum effect is produced. The precipitates formed in the blue, indigo and violet rays were nearly of the same weight, but it did appear that the precipitation was most *speedily* produced by the mean *blue* ray.

If we dissolve the brown precipitate from the mineral chameleon in a solution of cyanide of potassium, we have a clear fluid. Place one portion in darkness, and expose another to good sunshine, the solution preserved in the dark will remain quite clear for many days, whereas that exposed to actinic influence throws down a brown precipitate after a few hours' exposure. If the solution is washed over paper, we procure by exposure good negative images of leaves or any other body superposed.

A few grains of sulphate of the protoxide of iron were dissolved in rain water. If kept in perfect darkness, the solution remains clear for a long time; it becomes, however, eventually cloudy and coloured from the formation of some peroxide of iron, even in tubes hermetically sealed. A few minutes' exposure to sunshine is sufficient to produce this change, and the oxide formed, instead of floating in the fluid, and as in the former case rendering it opake, falls speedily to the bottom.

Some years since, at the Meeting of the British Association at Plymouth, I published an account of an exceedingly sensitive "photographic" process, the agents employed being iodide of silver and the ferro-cyanide of potassium. I have since then made some experiments with the hope of ascertaining the rationale of the chemical changes which take place. This has not been done in anything like a satisfactory manner; they have, however, led me to observe a curious effect produced in mixed solutions of iodide of potassium and ferrocyanide of potassium under the influence of sunshine. If a mixture of these salts is kept in the dark, no change takes place for a long period, but if exposed to sunshine, the colour of the solution is much deepened, becoming of a bright golden yellow, and a light brown powder is precipitated. I have reserved the examination of this powder for some future period: I believe it to be a combination of iron and iodine, and I have good reason for believing that a very remarkable change takes place in the arrangement of the elements of the salts employed.

100 grains of the protosulphate of iron were dissolved in 2 oz. of distilled water; 30 grs. of the bichromate of potash were dissolved in 4 oz. of distilled water; 1 oz. of the iron solution was weighed into two large test-tubes, and 6 drachms of the solution of the bichromate of potash added to each, which formed a fine red-brown clear solution. One tube was kept from all light, and the other exposed to good sunshine for an hour; in both tubes there was a precipitation of the chromate of iron, described by Dr. Thomson in his memoir on the Salts of Chromium, published in the Philosophical Transactions. The solutions, which were perfectly clear, were poured off from the precipitates; these were washed and weighed. The precipitate formed in the dark was found to weigh three grains more than that formed in the sunshine. This result was different from what my previous experiments had led me to expect, and it was several times repeated with the most scrupulous care. In every instance the precipitation which took place during *the first exposure* to sunshine was less than that formed in darkness.

The clear actinized solution poured off from the chromate of iron, being again exposed to solar influence, the other being carefully guarded from the slightest radiation, it was found that the precipitation went on much quicker in sunshine than in the dark; the side of the tube facing the sun was always thickly coated with chromate of iron, whilst the other side of it was perfectly free from any precipitation. Dr. Thomson has shown that this precipitation is produced by heat; it became interesting to know what part solar heat had in the phænomena. As I experienced some difficulty in arranging in a satisfactory manner many tubes along the prismatic spectrum, I used only three. One was placed in the mean indigo ray, another in the mean yellow ray, and the third in the least refrangible red ray. After an exposure of four hours, the precipitation formed in the tubes in the different rays was as follows:—

Indigo ray	5·25
Yellow ray	0·50
Red ray	2·15

Similar results were obtained by placing the solutions behind blue, yellow and red glasses, proving that although the calorific rays were not without action, the principal effect was due to the chemical rays. The half grain formed in the yellow ray I still regard as due to the thermic or actinic power of the beam, as behind a deep-coloured yellow solution (sulphate of potash and chromium) no precipitate was formed in eight hours.

I have made one or two other experiments (particularly one with a mixture of the bichromate of potash and the sulphate of copper) in which precipitation appears retarded by solar agency. I am rather inclined to think that it will eventually be proved that the electric energy of the different bodies in relation to each other will greatly modify the results we shall obtain in these experiments, and I intend, if possible, to investigate this part of the subject with care. Since Becquerel has shown (*Annales de Chimie*, November 1843) that the electrical excitation produced by the solar rays is different for each ray, and that this power is at its maximum in the yellow ray and also in the violet ray, the minimum force being between the green and blue rays; and since the mean maximum of chemical energy has been shown to manifest itself in the violet ray, and its minimum invariably to reside in the yellow ray; and as it is known that the negative and positive currents or sparks produce opposite effects, may we not reasonably conclude that the solar radiations are in some remarkable manner involved in the production of electrical phænomena? To say, as has been said, that electricity is the agent producing the effects I have been considering, is rushing much too hastily to a conclusion; indeed, cases will be found in which, at the maximum of electrical power in the spectrum, electrical phænomena which would occur in darkness or in light, apart from heat or actinism, are entirely prevented.

Colour of Precipitates.

If a solution of bichromate of potash is exposed to sunshine, it acquires a property of precipitating several metals as chromates, differing many shades in colour from the colours produced by a similar solution prepared and kept in the dark. If the actinized solution be poured into a solution of nitrate of silver, the chromate of silver formed is of a much more beautiful colour than that given by a solution which has not been exposed to the sun. A like effect will take place in precipitating chromate of mercury with actinized and non-actinized solutions of the chrome salt.

Solutions of sulphate of iron exposed to sunshine yield a prussian blue with the ferro-prussiate of potash, of a far more beautiful colour than that produced by a solution which has not been so exposed. If solutions of both the salts are kept for many hours in good sunshine, the colour of the resulting prussian blue is still improved.

I have long noticed in the process of darkening photographic papers and Daguerreotype plates, that the colours produced varied in richness of tint and in shade with the brightness of the sun and the clearness of the atmosphere at the time of exposure. In some positive processes, in which a darkened paper was bleached under the influence of actinic radiations (if this expression be permitted), the effects of colour were often very striking and beautiful, whereas in winter, or when a slight haze existed, a dull dun-brown almost invariably resulted. It appears that dyers and the manufacturers of some of the fine pigments are not unacquainted with these facts; they are certainly curious, and deserve attentive investigation at the present time. I regret that I am only in a position to record the facts I have noticed, not having as yet determined even the influence of the various independent rays upon these coloured precipitations.

ACTINIC INFLUENCE ON CHEMICAL COMBINATION.

Two phials were filled with a solution of acetate of silver and carefully corked. One was exposed for an hour to good sunshine, whilst the other was carefully kept in the dark. At the end of this time, a solution of the protosulphate of iron having been made in the dark, ten drops of it were added to each solution of silver. The one which had been exposed gave *immediately* a copious precipitate of silver, whereas the other was only rendered slightly turbid, and was some minutes before it precipitated. After having stood eight or ten minutes no difference could be detected in the quantity of silver precipitated in either phial.

Acetate of mercury was used in the place of the acetate of silver, and the difference between the actinized solution and the other, on the addition of the iron salt, was very striking.

The two salts, acetates of silver and mercury, were mixed (they had been used, combined rather successfully, in a photographic process of some interest). One portion was exposed in a large test-tube carefully corked, and another portion was protected from all light in a bottle. The exposure in this case was from two to three hours, but during that time there was not more than half an hour's good sunshine. By the light of a taper an equal quantity of the sulphate of iron was added to each. In about three minutes the solution which had been exposed appeared a little disturbed, small specks were seen to form in various parts of the fluid, and these rapidly increasing in size and assuming star-like shapes, fell heavily. At the expiration of an hour a dark and bulky precipitation was formed, but in the unexposed solution the precipitate was but little and of a light gray colour. In about two or three hours a coating of white metal was formed in two well-defined stripes along the tube which had been under solar influence; one on the side directly facing the sun, and the other on the other side of the tube, but along a line, upon which I found by subsequent experiment the rays were concentrated, by the form and refractive power of the media—glass and metallic solution—through which they had to pass. That these lines were due to the action of the solar rays was proved by placing a piece of blackened paper around a tube, as marked in the figure, which effectually prevented the metallic deposit over the space it covered. This deposit is somewhat capricious in its formation. The experiment has been often repeated, but although the precipitation was invariably as described, it frequently happened that no metal was deposited along the glass.

Sulphate of iron in solution was found to acquire the same property by exposure as the other salts above-named. 30 grs. of this protosalt were dissolved in an ounce and a half of water. It was then divided into two portions, one of which was exposed for an hour to sunshine. 100 grs. of each solution were carefully weighed into test-tubes, and the same weight of a solution of nitrate of silver made in the dark, was added to each. As quickly as possible the precipitates were collected, washed, dried and weighed. The precipitate produced immediately by the iron solution which had been actinized weighed 2·8 grains, whereas the precipitate by the unexposed solution weighed 0·7 grain. These results are the mean of six experiments.

Two test-tubes had 120 grains of an actinized solution of nitrate of silver weighed into them, and into two others was put the same quantity of a like solution which had not been actinized. Four other tubes held the same weights of solutions of the protosulphate of iron, two of them actinized, the others not so. They were mixed in the following order, and the precipitates collected immediately from each were as stated:—

1. Actinized silver with actinized iron	0·5 gr.
2. Unactinized silver with unactinized iron	0·7 ...
3. Actinized silver with unactinized iron	1·0 ...
4. Unactinized silver with actinized iron	1·5 ...

Considerable difficulty arises from the length of time which must necessarily elapse before the precipitates can be removed from the solutions. In all cases I find that after some little time the conditions required to effect a precipitation are established, and in both kinds of solutions it then proceeds without any apparent difference. It would appear from the above results, which are the mean of many experiments, that when both solutions are brought into the same actinic condition they do not precipitate more freely than those do which have been kept in the dark. It appears to be necessary that the actinic states should be dissimilar to ensure the production of these curious phænomena, which evidently point to some law of chemical action which has not yet been made the subject of study.

Bichromate of potash, it is well known, is decomposed by the agency of the solar rays when in contact with organic matter. A solution of this salt spread upon paper forms a photographic agent of some interest. The paper, which is of a fine yellow colour when first prepared, becomes brown upon exposure to the sunshine, from the chromic acid of the salt being acted upon by the organic matter of the paper. A similar change takes place, to a certain extent, in solutions of this salt in distilled water, when exposed in clear glass vessels, for some time, to bright sunshine. The evidence we have of this is the gradual formation of minute bubbles of air, which are redissolved, and the progressive increase of free chromic acid in the solution, which may be detected by any of the usual methods.

A combination of the bichromate of potash and of sulphate of copper, either on paper or in solution, exhibits the above change very clearly, in the gradual formation of a chromate of copper of a peculiar character, which, although brown at first, becomes nearly white by prolonged exposure to solar influence; indeed, upon paper the whiteness is quite complete. Some very remarkable changes have been detected in combinations of these salts, which will be made the subject of a future communication.

ACTINIC INFLUENCE ON ELECTRO-CHEMICAL ACTION.

Some years since, when endeavouring to discover the chemical action exerted by the sun's rays on the iodide of silver, I was induced to form some small galvanic arrangements, a description of which and the results of my experiments, I published in the Philosophical Magazine for October 1840. As these experiments bear strongly on my present researches, I venture to extract a passage or two from that paper.

" In a watch-glass, or any capsule, place a little solution of silver; in another, some solution of any hydriodic salt; connect the two with a filament of cotton, and make up an electric circuit with a piece of platina wire; expose this little arrangement to the light, and in a very short time it will be seen that iodine is liberated in one vessel, and the yellow iodide of silver formed in the other, which blackens as quickly as it is formed.

"Place a similar arrangement to the above (31.) in the dark, iodine is slowly liberated. *No iodide of silver formed; but around the wire a beautiful crystallization of metallic silver.*

"A piece of platina wire was sealed into two glass tubes; these when filled, the one with hydriodate of potash in solution, and the other with a solution of the nitrate of silver, were reversed into two watch-glasses containing the same solutions, the glasses being connected by a piece of cotton. A few hours of daylight occasioned the hydriodic solution in the tube to become quite brown with liberated iodine; a small portion of iodide of silver was formed along the cotton, and at the end dipping in the salt of silver. *During the night the hydriodic liquid became again colourless and transparent, and the dark salt along the cotton became yellow as at first.*"

I have resumed this inquiry under modified conditions, but the results have been invariably of a similar character to the above. I shall for the present merely describe two sets of experiments, from which I draw these conclusions:—

1st. That electro-metallic precipitation is prevented by the influence of the sun's rays.

2nd. That light is not the retarding agent, but that the exercise of electrical force is negatived by the direct influence of actinism.

I placed in a test-tube a strong solution of nitrate of silver; in another tube, being closed at one end with a thin piece of bladder, I placed a solution of iodide of potassium; this was supported in the solution of nitrate of silver by being fixed in a cork, and a piece of platina wire was carried from one solution into the other. An arrangement of this kind was kept in the dark; iodine was liberated in the inner tube, and a crystalline arrangement of metallic silver was formed around the platina wire in the outer one. Another was placed in the sunshine; iodine was liberated in the dark, but no silver was deposited.

Having exposed the above solution to the sunshine of July during a long day, the tube was placed in a dark cupboard, but the actinic influence which had been exerted on the solution of silver had produced a permanent change in its condition; after several days no trace of any metallic deposit could be detected, but the whole of the iodine again entered into combination, whereas this was not the case in the unexposed glasses.

In the inner tube I placed the solution of silver, and in the outer one the hydriodate of potash. One arrangement was kept in the dark, the other was exposed to good sunshine. In both instances the liberated iodine gave an intense yellow to the solution, and in both cases the quantity of metallic silver deposited was precisely the same.

This yellow fluid being analysed by the prism was found to obstruct all the rays above the green, whilst it permitted the permeation of the yellow and orange rays in great quantity and power. It is therefore evident that the luminous rays of the solar spectrum have no power in retarding electro-chemical action.

I submit these details of experiments in a new and important branch of science to the Chemical Society, knowing that they form but a very imperfect account of an extensive series of phænomena, but I do so with the hope of being enabled eventually to render my contributions more comprehensive.

VIII. *Experiments on the Electric Discharge of the Jar.* (*Extract of a letter from* M. Matteucci *to* M. Arago.)*

HAVING lately had occasion, in one of my lectures on electricity, to employ the new electrical machine of Armstrong, I thought it would be interesting to repeat the experiments of Colladon with this machine. The large quantity of electricity which is developed with this machine led me to hope that, even with an ordinary galvanometer, we might obtain indications of current by making a communication between *the insulated boiler* and the conductor furnished with points and *also insulated, against which* the jet of vapour is directed. It was, indeed, with an ordinary galvanometer, the wire of which makes 200 turns, and which is furnished with a somewhat imperfect static arrangement, that I obtained, operating in the manner I have stated, a fixed deviation in the direction given by the conditions of the experiment. This direction took place the contrary way, on reversing the position of the extremities of the galvanometer, being always directed from the extremity of the wire turned toward the vapour to that of the boiler. I have tried some experiments with a view of comparing the intensity of the current with the tension of the vapour in the boiler; and I have operated from two atmospheres up to five successively. The fixed deviation increased from 3 degrees up to 10 and 27 degrees, going from the pressure of two atmospheres to four. It appeared to me that the deviation was sensibly the same for pressures exceeding four atmospheres. On holding one of the extremities of the wire

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* From the *Comptes Rendus*, April 14, 1845.

with an insulating handle, at a certain distance from the conductor or the boiler, whilst the other extremity of the wire was in contact with the boiler or the conductor, there was always a constant deviation in the needle, but it was less, however, than that which existed when there was no interruption in the circuit. In this case, even at the distance of several centimetres, there was a series of sparks which appeared without interruption. I wished however to assure myself of it by one of the very ingenious means discovered by Mr. Wheatstone. This was a revolving disc, upon which I traced black stripes. The space of time which elapsed in passing from one stripe to the other was 0·00009 of a second: the disc appeared immoveable. It is proved therefore that the light was not continuous, and that it was a series of sparks or of successive discharges which traversed the circuit, giving to this circuit itself the properties of a conductor traversed by a voltaic current. The enormous quantity of electricity which is produced by this machine in all circumstances of the atmosphere, allowed of my making a series of experiments which complete the identification of the electric current properly so called with the discharge of the jar. I prepared a demi-right angle of copper wire perfectly similar to the moveable conductor of Ampère. The two small cups filled with mercury, into which the points of the moveable conductor are plunged, are fixed on a column of resin. The moveable conductor is supported by a silk thread without torsion. I fixed upon a resin foot a copper wire, which was consequently parallel to the longest side of the right angle, to which it could thus be brought near or removed from it at will. The whole apparatus was covered with a bell glass to prevent the effect of the agitation of the air. It is not difficult to conceive the arrangements of the experiment so as to cause the discharge of a battery to pass into the two conductors, either in the same direction, or in the opposite one; I shall therefore not stop to describe them. I began by passing the discharge into one only of the conductors, keeping the other either insulated or in communication with the ground: one while the discharge passed through the moveable conductor, at another by the fixed conductor. When the two conductors are at the distance of fifteen to twenty millimetres, employing the discharge of a battery of nine jars, each of which had a surface of $0^{m.c.}{\cdot}12$, no movement was observed in the moveable conductor, even when examined with the telescope of the cathetometer. At a distance less than fifteen millimetres between the two conductors, the moveable wire was in all cases seen to be slightly attracted by the fixed conductor. This same attraction is manifested in a much more evident manner when the fixed conductor or the moveable conductor is in communication with the conductor of the electrical machine. Lastly, if the fixed conductor be electrified with sparks, the moveable conductor is successively attracted and repelled. These phænomena are very easily explained by the ordinary attractions and repulsions of electrified bodies, in presence of bodies in the natural state. I only wished to try these experiments with my apparatus, in order to see what part it might have in the phænomena which I am about to describe. The two conductors are arranged in such a manner as that the discharge should penetrate them in opposite directions. The distance between the two conductors was from ten to fifteen up to thirty millimetres. I began by observing the moveable conductor with a telescope; but this was useless, for the movements of repulsion which take place in the moveable conductor at the moment of the discharge are so great that they may be observed with the unassisted eye. On passing the current along the two conductors in the same direction, the moveable conductor was also seen, and in a very distinct manner, to fall upon the fixed conductor at the moment of the discharge. Thus we may admit, without any kind of doubt, that the fundamental law of Ampère, of the attraction of currents of the same direction, and of the repulsion of currents of opposite direction, holds good in regard to the discharge of the jar under the same circumstances.

I shall lastly add, that I repeated my experiments on the induction of the discharge of the jar (*Annales de Chimie et de Physique*, 3rd series, t. iv. Feb. 1842.), by passing the discharge of the battery through the wire of a plane spiral in presence of a similar spiral, the two extremities of which are united with the ends of the wire of a galvanometer. At whatever distance the two spirals are, the current of induction, which continually diminishes in proportion as the distance increases, always passes in the same direction as the current of the jar. cesses of magnetizing to ascertain the direction of the current I lay a stress on this result, because, when employing the pro- of induction, we sometimes find this result, sometimes the contrary, according to the distance of the two spirals and the tension of the discharge. When the current of induction is made to act on another spiral, whose extremities communicate with the galvanometer, the current of induction of the second order which is obtained passes in the contrary direction to the inductor current. These are the results described in my memoir already cited,—results which I have just now had occasion to verify.

IX. *On Algebraical Couples.* *By* ARTHUR CAYLEY, *Esq., B.A., F.C.P.S., Fellow of Trinity College, Cambridge**.

IT is worth while, in connection with the theory of quaternions and the researches of Mr. Graves (Phil. Mag., No. 173), to investigate the properties of a couple $\iota x + jy$, in which ι, j are symbols such that

$$\begin{aligned} \iota^2 &= \alpha\iota + \beta j \\ \iota j &= \alpha^1\iota + \beta^1 j \\ j\iota &= \gamma\iota + \delta j \\ j^2 &= \gamma^1\iota + \delta^1 j. \end{aligned}$$

If $\overline{\iota x + jy}\ \overline{\iota x_1 + jy_1} = \iota X + jY$, then

$$\begin{aligned} X &= \alpha x x_1 + \alpha^1 x y_1 + \gamma x_1 y + \gamma^1 y y_1, \\ Y &= \beta x x_1 + \beta^1 x y_1 + \delta x_1 y + \delta^1 y y_1. \end{aligned}$$

Imagine the constants $\alpha, \beta \ldots$ so determined that $\iota x + jy$ may have a modulus of the form $K(x + \lambda y)(x + \mu y)$; there results one of the four following essentially independent systems:—

A.
$$\begin{aligned} \iota^2 &= \frac{1}{\lambda\mu}(\delta\lambda\mu + \gamma\overline{\lambda + \mu})\iota - \frac{\gamma}{\lambda\mu}j \\ \iota j &= j\iota = \gamma\iota + \delta j \\ j^2 &= -\lambda\mu\delta i + (\gamma + \overline{\lambda + \mu}\delta)j \end{aligned}$$

$$\begin{cases} X + \lambda Y = \dfrac{1}{\lambda}(\gamma + \lambda\delta)(x + \lambda y)(x_1 + \lambda y_1) \\[2ex] X + \mu Y = \dfrac{1}{\mu}(\gamma + \mu\delta)(x + \mu y)(x + \mu y_1). \end{cases}$$

The couple may be said to have the *two linear moduli*,

$$\frac{1}{\lambda}(\gamma + \lambda\delta)(x + \lambda y) : \frac{1}{\mu}(\gamma + \mu\delta)(x + \mu y);$$

as well as the quadratic one,

$$\frac{1}{\lambda\mu}\overline{\gamma + \lambda\delta}\ \overline{\gamma + \mu\delta}\ \overline{x + \lambda y}\ \overline{x + \mu y},$$

the product of these, which is the modulus, and the only modulus in the remaining systems.

B.
$$\begin{aligned} \iota^2 &= -\delta\iota + \frac{1}{\lambda\mu}(\gamma + \delta\overline{\lambda + \mu})j \\ \iota j &= j\iota = \gamma\iota + \delta j \\ j^2 &= (\overline{\lambda + \mu}\gamma + \lambda\mu\delta)\iota - \gamma j. \end{aligned}$$

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* Communicated by the Author.

$$\begin{cases} X+\lambda Y=\dfrac{1}{\mu}(\gamma+\lambda\delta)(x+\mu y)(x_1+\mu y_1) \\ X+\mu Y=\dfrac{1}{\lambda}(\gamma+\mu\delta)(x+\lambda y)(x_1+\lambda y_1). \end{cases}$$

C. $$\iota^2=\frac{1}{\lambda\mu}(\delta\lambda\mu+\gamma(\overline{\lambda+\mu}))\,\iota-\frac{\gamma}{\lambda\mu}j$$

$$\iota j=\left(\frac{\mu^2+\mu\lambda+\lambda^2}{\mu\lambda}\gamma+\overline{\mu+\lambda}\,\delta\right)\iota+\left(-\delta-\frac{\lambda+\mu}{\lambda\mu}\gamma\right)j$$

$$j\iota=\gamma\iota+\delta j$$

$$j^2=(\overline{\lambda+\mu}\,\gamma+\lambda\mu\delta)\,\iota-\gamma j$$

$$\begin{cases} X+\lambda Y=\dfrac{1}{\lambda}(\gamma+\lambda\delta)(x+\lambda y)(x_1+\mu y_1) \\ X+\mu Y=\dfrac{1}{\mu}(\gamma+\mu\delta)(x+\mu y)(x_1+\lambda y_1). \end{cases}$$

D. $$\iota^2=-\delta i+\frac{1}{\lambda\mu}(\gamma+\delta\overline{\lambda+\mu})j$$

$$\iota j=(-\gamma-\overline{\lambda+\mu}\,\delta)\,\iota+\left(\frac{\lambda+\mu}{\lambda\mu}\gamma+\frac{\lambda^2+\lambda\mu+\mu^2}{\lambda\mu}\delta\right)j$$

$$j\iota=\gamma\iota+\delta j$$

$$j^2=-\lambda\mu\delta i+(\gamma+\overline{\lambda+\mu}\,\delta)j$$

$$\begin{cases} X+\lambda Y=\dfrac{1}{\mu}(\gamma+\lambda\delta)(x+\mu y)(x_1+\lambda y_1) \\ X+\mu Y=\dfrac{1}{\lambda}(\gamma+\mu\delta)(x+\lambda y)(x_1+\mu y_1). \end{cases}$$

The formulæ are much simpler and not essentially less general, if $\mu=-\lambda$. They thus become

A′. $$\iota^2=\delta\iota+\frac{\gamma}{\lambda^2}j$$

$$\iota j=j\iota=\gamma\iota+\delta j$$

$$j^2=\lambda^2\delta\iota+\gamma j$$

$$X\pm\lambda Y=\pm\frac{1}{\lambda}(\gamma\pm\lambda\delta).(x\pm\lambda y)(x_1\pm\lambda y_1).$$

(Two linear moduli.)

B′. $$\iota^2=-\delta\iota-\frac{\gamma}{\lambda^2}j$$

$$\iota j=j\iota=\gamma\iota+\delta j$$

$$j^2=-\lambda^2\delta\iota-\gamma j$$

$$X\pm\lambda Y=\mp\frac{1}{\lambda}(\gamma\pm\lambda\delta)(x\mp\lambda y)(x_1\mp\lambda y_1).$$

C′.
$$\begin{aligned}\iota^2 &= -\delta\iota + \frac{\gamma}{\lambda^2}j\\ \iota j &= -\gamma\iota - \delta j\\ j\iota &= \gamma\iota + \delta j\\ j^2 &= -\lambda^2\delta\iota - \gamma j\end{aligned}$$

$$X \pm \lambda Y = \pm\frac{1}{\lambda}(\gamma \pm \lambda\delta)(x \pm \lambda y)(x_1 \mp \lambda y_1).$$

D′.
$$\begin{aligned}\iota^2 &= -\delta\iota - \frac{\gamma}{\lambda^2}j\\ \iota j &= -\gamma\iota - \delta j\\ j\iota &= \gamma\iota + \delta j\\ j^2 &= \lambda^2\delta\iota + \gamma j\end{aligned}$$

$$X \pm \lambda Y = \mp\frac{1}{\lambda}(\gamma \pm \lambda\delta.)(x \mp \lambda y)(x_1 \pm \lambda y).$$

There is a system more general than (A.) having a *single* linear modulus $q(\theta X + Y)$: this is

E.
$$\begin{aligned}\iota^2 &= \alpha(\iota - \theta j) + \theta^2 qj\\ \iota j &= \alpha'(\iota - \theta j) + \theta qj\\ j\iota &= \gamma(\iota - \theta j) + \theta qj\\ j^2 &= \gamma'(\iota - \theta j.) + qj\end{aligned}$$

$$\theta X + Y = q(\theta x + y)(\theta x_1 + y_1);$$

or, without real loss of generality,

E′.
$$\begin{aligned}\iota^2 &= \alpha i\\ \iota j &= \alpha'\iota\\ j\iota &= \gamma\iota\\ j^2 &= \gamma'\iota + qj.\end{aligned}$$

$$Y = qyy_1.$$

To complete the theory of this system, one may add the identical equation

$$X + \frac{1}{\theta - qM}Y = \frac{q(\theta^2 - M\alpha)}{\theta - qM}\left(x + \frac{\alpha' - \theta\gamma}{\alpha - \theta\alpha'}y\right)\left(x_1 + \frac{\alpha' - \theta\gamma'}{\alpha - \theta\gamma}y_1\right);$$

where
$$M = \frac{\theta(\gamma - \theta\gamma') - (\alpha - \theta\alpha')}{\alpha'\gamma - \alpha\gamma'}.$$

By determining the constants, so that $\frac{1}{\theta - qM} = \frac{\alpha' - \theta\gamma}{\alpha - \theta\alpha'} = \frac{\alpha' - \theta\gamma'}{\alpha - \theta\gamma}$, the system would reduce itself to the form A.

Cambridge, April 23, 1845.

X. *On a Phænomenon in which the distant objects were seen inverted.* *By* Edward Joseph Lowe, *Esq.*[*]

On Thursday, April 17, 1845, at 2^h 20^m p.m., a curious and very unusual phænomenon was observed near Radford, which is situated about a mile W. of Nottingham.

A group of trees at the distance of two miles, and a hill (behind the trees) at the distance of four miles at the most remote portion from the place of observation, were seen inverted in the air and of a gray colour; the inverted phænomenon did not touch the objects of which they were the image, but appeared about 30′ elevated above them. There was a haze between the objects and the phænomenon; also the summits of the inverted landscape did not end in blue sky, but were observed also to end in haze. The breadth of the hill was 20°, which was inverted of an equal size, and appeared somewhat to resemble Pendle Hill in Lancashire. The phænomenon was seen for five minutes, and when left still visible; returning an hour afterwards it had vanished. No clouds were visible at the time, and the wind was very brisk.

The morning had been cloudless until 10^h a.m., then few cumuli clouds of small size floated over, increasing in abundance until 1^h p.m., when they began to disappear below the S.E. horizon; at 2^h p.m. sky almost cloudless and the weather warm, soon after perfectly cloudless; at 3^h p.m. cirrostrati formed themselves on N.W. horizon; these increased rapidly, and in an hour covered the sky, which remained overcast for the rest of the day. The barometer fell from 30·596 in. at 9^h a.m. to 30·560 in. at 9^h p.m. (attached thermometer 9^h a.m., 64°; 9^h p.m., 66°); the minimum temperature was 36°, and the maximum temperature 58°·7; the direction of the wind and clouds was from the N.E., and the distant prospect rather hazy. There is a valley of some considerable size in the direction of the phænomenon, but not much water; the principal portion is the river Leen, which is about seven yards wide, and does in some measure take its course in a direction from the observer to the phænomenon.

XI. *On a Magnificent Meteor seen in Nottinghamshire.* *By* Edward Joseph Lowe, *Esq.*[*]

On Thursday, April 24, 1845, a blue meteor, of a most unusual size and brilliancy, was seen near High Field House (lat. 52° 57′ 30″; long. 1° 11′ W.) in the above county.

The weather had been for the five days prior to the 24th hot, and almost cloudless; for the mean of clouds for that

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[*] Communicated by the Author.

period only amounted to $\frac{2 \cdot 4}{10}$. The temperature had reached its greatest of heat on the 24th at 4^{h} p.m., viz. $+70^{\circ} \cdot 1$, at which time the hygrometer was $+59^{\circ} \cdot 2$; the wind nearly calm and veering to E., and in the evening to S. The barometer had been gradually falling from the morning of the 21st, and at 6^{h} p.m. a heavy thunder-storm passed over High Field House from the S. moving to the N.; the lightning was most vivid at $6^{h}\ 15^{m}$ p.m. In the morning a faint solar halo was formed, and in the evening an arc of a solar iris, very sensibly prismatic, was visible. At $9^{h}\ 35^{m}$ (mean time), the night, which was very dark, suddenly became light as day, and the objects near and distant were visible as plainly as in broad daylight: immediately a magnificent meteor, of a blue colour, was seen traversing the interval from the zenith, through the stars 21, 30, 40 and 41 of the constellation of Leo Minor, and the stars 95, 96, x, 59, τ and 75 of the constellation of Leo Major (a distance of 30°), which it accomplished in little less than three seconds of time: it exploded very near the star ϕ, Leonis Majoris, and, after falling in small fragments of light for the space of 1°, became suddenly extinguished. Its apparent size was very nearly equal to the disc of the moon, and perfectly round in form; but its brilliancy very far surpassed that luminary, and its intensity could not possibly have been less than three times as light as our satellite. No train of light was left behind the meteor, as is seen with the caudate meteors. It appeared of no considerable height above the surface of our earth. There were no clouds visible at the time; but a few cumuli appeared soon after, and the moon rose of a red colour.

Should any one have noticed this phænomenon in the azimuth of the meteor, a comparison of remarks would prove both interesting and important, for the height of the meteor above the surface of our earth might be ascertained.

XII. *Reduction of the Four Forms of* ω *in* Jacobi's *General Transformation of an Elliptic Function to one form only. By the* Rev. Brice Bronwin*.

THE constants in Jacobi's transformation of an elliptic function are all expressed by the two series of quantities—

$$\sin^2 am\,(4\,\omega),\ \sin^2 am\,(8\,\omega) \ldots\ldots \sin^2 am\,(2\,n-2)\,\omega,$$
$$\sin^2 co\,am\,(4\,\omega),\ \sin^2 co\,am\,(8\,\omega) \ldots\ldots \sin^2 co\,am\,(2\,n-2)\,\omega.$$

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* Communicated by the Author.

But
$$\sin am\,(2n\omega - 2\omega) = \mp \sin am\,(2\omega),$$
$$\sin am\,(2n\omega - 6\omega) = \mp \sin am\,(6\omega), \text{ \&c.}$$
and
$$\begin{aligned}\sin co\,am\,(2n\omega - 2\omega) &= \sin am\,(\mathrm{K} + 2\omega - 2n\omega)\\ &= \pm \sin am(\mathrm{K} + 2\omega) = \mp \sin am\,(-\mathrm{K} - 2\omega)\\ &= \mp \sin am\,(\mathrm{K} - 2\omega - 2\mathrm{K}) = \pm \sin am(\mathrm{K} - 2\omega)\\ &= \pm \sin co\,am\,(2\omega),\ \sin co\,am\,(2n\omega - 6\omega) = \pm \sin co\,am\,(6\omega),\\ &\text{\&c.}\end{aligned}$$

These constants therefore may be all expressed by the two series

$$\sin^2 am\,(2\omega),\ \sin^2 am\,(4\omega) \ldots\ldots \sin^2 am\,(n-1)\,\omega,$$
$$\sin^2 co\,am\,(2\omega),\ \sin^2 co\,am\,(4\omega) \ldots\ldots \sin^2 co\,am\,(n-1)\,\omega.$$

Consequently the 4ω in this theory may be everywhere replaced by 2ω; and this reduction Jacobi has himself partially made.

But
$$\sin^2 co\,am\,(2\omega) = \frac{\cos^2 am\,(2\omega)}{\Delta^2 am\,(2\omega)} = \frac{1 - \sin^2 am\,(2\omega)}{1 - k^2 \sin^2 am\,(2\omega)},$$
$$\sin^2 co\,am\,(4\omega) = \frac{1 - \sin^2 am\,(4\omega)}{1 - k^2 \sin^2 am\,(4\omega)}, \text{ \&c.}$$

And $\sin^2 am\,(4\omega)$, $\sin^2 am\,(6\omega)$, &c. may all be expressed by functions of $\sin^2 am\,(2\omega)$. Therefore all the constants in this theory may be expressed by functions of $\sin^2 am\,(2\omega)$.

Now $\sin^2 am\,(2\omega) = \sin^2 am\,(2\omega - 2\mathrm{K}) = \sin^2 am\,(2\omega')$, if $\omega' = \omega - \mathrm{K}$. Let
$$\omega = \frac{2r\mathrm{K} + 2r'\mathrm{K}'\sqrt{-1}}{n};$$
and make $n = 2p - 1$, $r - p = r_1$; we have
$$\omega' = \frac{(2r - 2p + 1)\mathrm{K} + 2r'\mathrm{K}'\sqrt{-1}}{n} = \frac{(2r_1 + 1)\mathrm{K} + 2r'\mathrm{K}'\sqrt{-1}}{n}.$$
Hence the second form of ω reduces to the first.

Again,
$$\sin^2 am\,(2\omega) = \sin^2 am\,(2\omega - 2\mathrm{K} - 2\mathrm{K}'\sqrt{-1}) = \sin^2 am(2\omega'),$$
if $\omega' = \omega - \mathrm{K} - \mathrm{K}'\sqrt{-1}$. Let
$$\omega = \frac{2r\mathrm{K} + (2r' + 1)\mathrm{K}'\sqrt{-1}}{n},$$
and make $r_1' = r' - p + 1$, r_1 and p remaining as before; we have in this case,
$$\omega' = \frac{(2r - 2p + 1)\mathrm{K} + (2r' - 2p + 2)\mathrm{K}'\sqrt{-1}}{n}$$
$$= \frac{(2r_1 + 1)\mathrm{K} + 2r_1'\mathrm{K}'\sqrt{-1}}{n}.$$

The third form, therefore, reduces to the first.

Also,

$$\sin^2 am\,(2\,\omega) = \sin^2 am\,(2\,\omega - 2\,\mathrm{K}'\sqrt{-1}) = \sin^2 am\,(2\,\omega'),$$

if $\omega' = \omega - \mathrm{K}\sqrt{-1}$. Let

$$\omega = \frac{(2\,r+1)\,\mathrm{K} + (2r'+1)\,\mathrm{K}'\sqrt{-1}}{n}.$$

Then
$$\omega' = \frac{(2\,r+1)\,\mathrm{K} + (2\,r' - 2\,p + 2)\,\mathrm{K}'\sqrt{-1}}{n}$$
$$= \frac{(2r+1)\,\mathrm{K} + 2r_1{}'\mathrm{K}'\sqrt{-1}}{n},$$

which reduces the fourth form to the first. For, from what has been demonstrated, it follows that

$$\sin^2 am\,(2\,m\,\omega) = f\{\sin^2 am\,(2\,\omega)\} = f\{\sin^2 am\,(2\,\omega')\}$$
$$= \sin^2 am\,(2\,m\,\omega')\sin^2 co\;am\,(2\,m\,\omega) = f_1\,\{\sin^2 am\,(2\,\omega)\}$$
$$= f_1\,\{\sin^2 am\,(2\,\omega')\} = \sin^2 co\,am\,(2\,m\,\omega'),$$

where m is any integer, f and f_1 denote certain functions, ω stands for one of the last three forms of this quantity, and ω' for the first form. To these we may add

$$\cos^2 am\,(2\,m\,\omega) = 1 - \sin^2 am\,(2\,m\,\omega) = 1 - \sin^2 am\,(2\,m\,\omega')$$
$$= \cos^2 am\,(2\,m\,\omega').$$

Jacobi's transformation reduced is

$$\int_0^y \frac{d\,y}{\sqrt{(1-y^2)\,(1-\lambda^2 y^2)}} = \frac{1}{\mathrm{M}}\int_0^x \frac{d\,x}{\sqrt{(1-x^2)\,(1-k^2x^2)}}, \quad (a.)$$

$$\left.\begin{array}{l} y = \dfrac{x}{\mathrm{M}}\,\dfrac{1-\dfrac{x}{\mathrm{s}^2 a\,(2\,\omega)}}{1-k^2\,x^2\,\mathrm{s}^2\,a\,(2\,\omega)}\cdot\dfrac{1-\dfrac{x}{\mathrm{s}^2\,a\,(4\,\omega)}}{1-k^2\,x^2\,\mathrm{s}^2\,a\,(4\,\omega)} \\ \qquad\cdots\cdots\cdots\dfrac{1-\dfrac{x^2}{\mathrm{s}^2\,a\,(n-1)\,\omega}}{1-k^2x^2\mathrm{s}^2a(n-1)\omega} \end{array}\right\} \quad . \quad . \quad (1.)$$

$$\mathrm{M} = \left\{\frac{\sin co\;am\,(2\,\omega)\sin co\;am\,(4\,\omega)\ldots\sin co\;am\,(n-1)\,\omega}{\sin am\;(2\,\omega)\sin am\,(4\,\omega)\ldots\sin am\,(n-1)\,\omega}\right\}^2,$$

$$\lambda = k^n\,\{\sin co\;am(2\,\omega)\sin co\;am\,(4\,\omega)\ldots\sin co\;am(n-1)\omega\}^4,$$

where ω may have any of the four forms. Let ω again denote any of the last three forms, ω' the first. In (1.) and in the expressions of M and λ, we may change ω into ω', and the values of all the coefficients and all the constants will remain unchanged. There is then but one transformation.

We easily transform (1.) into

$$y = \frac{\mathrm{s}\,a\,.\,u\,\mathrm{s}\,a\,(u+2\,\omega')\ldots\ldots\mathrm{s}\,a\,(u+(2\,n-2)\,\omega')}{\mathrm{s}\,a\,.\,\omega'\,\mathrm{s}\,a\,(3\,\omega')\ldots\ldots\,\mathrm{s}\,a\,(2\,n-1)\,\omega'}. \quad . \quad (2.)$$

Make $u=0,\ 2\omega',\ 4\omega'$, &c.; and we have $y=0$. Again, make $u=\omega',\ 3\omega',\ 5\omega'$, &c.; and we have $y=\pm 1$. For the other forms of ω we have

$$y=\frac{\mathrm{s}a\,.\,u\,\mathrm{s}a\,(u+2\,\omega)\,\ldots\ldots\,\mathrm{s}a\,(u+(2\,n-2)\,\omega)}{\mathrm{s}a(\mathrm{K}-2\,\omega)\,\mathrm{s}a\,(\mathrm{K}-4\,\omega)\,\ldots\ldots\,\mathrm{s}a(\mathrm{K}-(2\,n-2)\,\omega)}\,. \quad (3.)$$

When $u=\mathrm{K},\ \mathrm{K}-2\,\omega$, &c., this gives $y=\pm 1$. But which of these values falls between $u=0,\ u=2\,\omega$, or between $u=0,\ u=2\,\omega'$, we cannot tell. We ought, however, in good factorial formulæ to know. And when $u=\omega',\ 3\,\omega'$, &c., this must give $y=\pm 1$; because (3.) must give the same value of y that (2.) does. But when we put for ω' its value in ω, this does not appear; nor can we reduce the result, except for the second form of ω, without eliminating ω, which would be equivalent to reducing (3.) to (2.) But all these things ought to be apparent in the face of factorial formulæ. Other faults might be pointed out; and we might point out similar faults in the expression of the value of $\sqrt{1-y^2}$. But I shall not dwell upon the subject. If the last three forms of ω do not render the values of y erroneous, they render the formulæ faulty. They are unnatural; they reduce to the first form, and there is but one transformation.

If Mr. Cayley had proved in his paper of November 1844 all that he wished, it would not follow that the different forms of ω would give really distinct transformations. But his formula (6.) is not Jacobi's. To agree with his, the first member should be $\phi_1\left(\frac{u}{\mathrm{M}}\right)$, the denominator of the second

$$\phi\,(\mathrm{K}-2\,\omega)\;\phi\,(\mathrm{K}-4\,\omega)\,\ldots\,\phi\,(\mathrm{K}-(2\,n-2)\,\omega).$$

He might however, by a suitable modification of (5.), have arrived at Jacobi's result. But his proof of the possibility of

$$m\,\mathrm{K}+m'\,\mathrm{K}'\sqrt{-1}+r\,\theta=\mu\mathrm{H}+\mu'\,\mathrm{H}'\sqrt{-1}$$

is not to me satisfactory. It is unnecessary to state my objections, because he has not shown the possibility of

$$2\,m\,\mathrm{K}+(2\,m'+1)\,\mathrm{K}'\sqrt{-1}+2\,r\,\theta=2\mu\,\mathrm{H}+(2\,\mu'+1)\mathrm{H}'\sqrt{-1}.$$

The omission of this appears to me very strange. Of course the omission renders his paper perfectly nugatory. For the possibility of this equation does not follow from that of the former, supposing that to be fairly proved. If he had shown the possibility of this last, I believe he would have found it necessary to modify a little the form of some of the quantities a, b, &c. Moreover, if he had transformed the factorial values of $f(u)$, $\mathrm{F}(u)$ (I use here Abel's symbols), he might have found it necessary to modify them still further. In a complete proof, such as Mr. Cayley professes his to be, all these things ought to have been done. And I heartily wish he had done them, and done them rightly, for the sake of the result.

Mr. Cayley thinks that his paper just referred to will lead to the complete determination of H and H′. I think so too; and I say that $H = \frac{\omega'}{M}$, and that this follows from (2.) of this paper. But from the last three forms of ω I cannot determine H. And if I could, it is easy to see from (3.) that they would give different values to this quantity; another proof of the faulty nature of these forms. For the transformation being but one, H ought to have but one value. I suppose the different forms of ω derived from one another, as in this paper.

With regard to Mr. Cayley's last paper, I have to observe that I had made trial of the form $\omega = \frac{K'\sqrt{-1}}{3}$, and did not make it to be a transformation. But on going over the subject again, I find I somewhere made a mistake. It is complementary however, and makes no part of the direct transformation, although somehow strangely derived from it, without the process by which the complementary is derived from the direct one. I pass over the rest of this paper, because it would be an endless and useless task to discuss every minute particular.

Gunthwaite Hall, May 13, 1845.

XIII. *On* Fresnel's *Theory of Diffraction. By* R. Moon, *M.A., Fellow of Queen's College, Cambridge, and of the Cambridge Philosophical Society.*

[Continued from vol. xxvi. p. 94.]

In a paper which appeared in a recent Number of this Journal (see vol. xxvi. p. 89), I endeavoured to point out some remarkable errors in Fresnel's investigation of the fringes produced by an opake body illuminated from a single point; and in particular I endeavoured to stigmatize an erroneous principle of approximation adopted by Fresnel, and which I then stated, and now assert to be such, as to vitiate *every* investigation of that ingenious person in this department of optics. I thence proceeded to show that Fresnel's mode of investigation, when properly conducted, leads to conclusions very different from what he supposes, and such in fact as are completely at variance with the observed phænomena which he wholly fails to account for; and I further drew the conclusion, that the principle of small waves emanating from the general front (certainly one of the most absurd and chimerical ever proposed) is untrue, and that the whole structure founded upon it as a necessary consequence falls to the ground. But let not the scope of these observations be misunderstood: the principle in question has been represented by some leading writers on this subject as so completely part and parcel of the undulatory theory*, that the student who is little conversant with these subjects might suppose, that by rejecting it we reject the theory *in toto*; but such is by no means the case.

The principle is quite collateral to the theory. With the investigations into the phænomena of polarized light it has absolutely no connection whatever. Rejecting it as I do, I still admit and profess my belief in the truth of the undulatory theory of reflexion and refraction, of the explanations of the interesting experiments of the two mirrors and the prism of small refracting angle, and of that admirable portion of the theory which relates to the phænomena observed in the shadows of narrow fibres and the colours of Newton's rings. But if there be any soundness in the principles I have laid down, the remaining portion of the theory of diffraction, the whole theory of apertures, and of the shadows of extended bodies must be entirely rejected.

Having thus explained my position, I shall proceed to make some further observations on the subject of diffraction, partly with a view to a more complete exposure of the extraordinary

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* One can hardly resist a smile at the extravagant admiration which some of these gentlemen have displayed towards this monstrous hypothesis. Even were it true that Fresnel's deductions from his principle were correct, that circumstance would only establish its truth, as a curious, indeed, but isolated and useless fact. To call it an *explanation* were an abuse of terms: for a phænomenon can only be said to be explained when its occurrence is traced to agents whose existence is known or probable, and the mode of whose operation is understood. Will any one pretend to say that he can understand how a wave should at every point of its progress push out other waves from every point of its front? The mind of an angel could not compass such an idea. And if it be admitted that the truth of the proposition cannot be seen *à priori*, will any be bold enough to assert that they anticipate the time when such a property of luminous waves shall be proved? It is just as likely as that we should be able to prove the existence of the solid epicycles dreamt of by the ancient philosophers. Let me once for all make a remark which applies as much to Fresnel's theory of double refraction and to the labours of others in the same department as to the matter in hand. It is not by a system of hap-hazard conjectures that we are to expect to arrive at truth. After all that has been said and written about the "Inductive Philosophy," one might have expected that by this time its merits were pretty well established, and that we should not now have to restrain the vain efforts of men seeking for light upon any other principles; but in fact, in this case of Fresnel's theory, the whole scientific world seems to have lost sight of these principles.

fallacies which have been worked up into the received theory, but chiefly with the intention of giving a clear view of the entire subject; and whilst I disclaim the idea of endeavouring to build up a settled theory of my own in the place of that which I assume myself to have destroyed, I propose to suggest various considerations which from time to time have occurred to me, and which I have some hope may lead, in the hands of those who have more time and opportunity for such researches than are likely to fall to my lot, to the ultimate and complete elucidation of the subject.

Recurring to my former example, suppose a series of concentric, spherical, diverging waves to be diffracted by an opake body indefinitely extended in all directions but one. If we suppose that no reflexion takes place near the edge of the diffracting body, and that no vibration is communicated through it to the æther beyond, or, admitting the possibility of either or both of these circumstances occurring, if we suppose it to be ascertained, by experiment or otherwise, that the phænomena are in no degree attributable thereto, it is evident that each wave of the series must prolong itself *continuously* within the shadow. On a former occasion (see vol. xxiv. p. 81 of this Journal) I endeavoured to point out how, upon a particular assumption as to the nature of the incident waves, this would naturally occur upon simple mechanical principles. But I conceive the same would be true, whatever the nature of the incident wave might be. For experiment assures us, that light actually penetrates within the geometrical shadow of the diffracting body, and upon the assumptions we have above made, this can only occur through an innate power in diffracted waves of diffusing themselves laterally; and this being the case, it seems impossible that such diffusion should operate itself otherwise than continuously. If we were to lay aside the consideration of any disturbing effect which the forces residing in the particles of the diffracting body might produce in the diffracted waves, perhaps this position might be stated absolutely; as would be the case if Fresnel's conclusion, that the diffraction is independent of the nature of the diffracting body and the form of the edge were to be relied on; and at any rate it is very difficult to conceive how these forces should tend to produce discontinuity.

Assuming then that each extended wave is continuous up to the point where it finally terminates, it is evident that no interference can occur, except through the intersection of waves *nearly parallel* to each other, which would imply, that after diffraction the waves vary from the spherical form; and that this change of form is effected differently in different waves, and this even when the light is what is commonly called homogeneous, or of one colour. If this view of the subject be correct, we have only to suppose the waves composing light of one colour to be propagated in the same ever-recurring cycle, the successive members of such cycle being so related, that whilst they possess the property when undiffracted of traversing transparent media with the same velocity, yet they differ from one another so far in the nature of their vibration (or, assuming in order to fix our ideas a specific hypothesis as to the nature of the undulations, the relative condensations and rarefactions of their several parts), as that after diffraction they change their forms in different degrees; and if we further suppose that this relative change of form is so adjusted as that each wave of the cycle is intersected by its immediate consecutive very soon after passing the diffracting body, that the loci of such intersections are nearly coincident and form a hyperbolic line of sensible breadth, and that the loci of the intersections of each wave, with its second, third, &c. consecutives, form other such lines respectively, laying without the former and each other, and the known phænomena are at once accounted for*.

Of the remarkable view of the constitution of light which this mode of considering the subject, if well-founded, unfolds to us, it is not my present purpose to speak; I shall rather address myself to the discussion of two objections which may be urged against it.

The first is, that the lines of interference actually start immediately from the edge of the diffracting body, whereas upon the principle above explained, they can only occur at a finite distance from it. To this I shall only reply, that assuming the objection to be founded in fact, of which I am disposed to think there is no irrefragable proof, it in reality amounts to very little, for though the lines of interference according to the above theory do really commence at a finite distance from the edge of the body, it is perfectly possible, and in fact highly probable, that such distance should be of insensible magnitude.

The second objection to which I propose to advert may to some appear to have more weight, and I freely admit that, *primâ facie*, it is deserving of some consideration. It is this: the change in the form of the waves after diffraction, to which we have ascribed the interference in the above example, and which occurs as well in that part of each wave without the geo-

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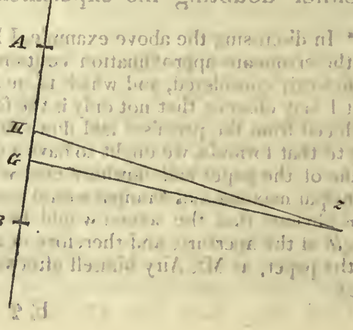
* The reader who may be desirous of seeing this mode of explanation more elaborated, will find a somewhat more detailed account of it in the volume of this Journal last referred to.

metrical shadow, as in the part within it (since the interference with which we are now dealing occurs without the shadow), can only take place through a change in the velocity of transmission; whereas it has generally been considered that *in vacuo* all waves are transmitted with the same uniform velocity. But though disposed to consider it as approximately true, that *in vacuo* all continuous waves are propagated with the same uniform velocity, I am by no means inclined to admit that the remarkable case of broken waves is amenable to the same rule. I have elsewhere observed in reference to this subject (see the paper last referred to), that "the present is a case of wave-motion altogether peculiar, and one of which no example has been hitherto subjected to investigation. All cases of wave-motion hitherto investigated algebraically (I might have added 'or otherwise,' unless Fresnel's *luminous* theory be thought an exception to the remark) resolve themselves into the simple case of the propagation in the direction of the axis of a cylindrical tube, of a wave whose front is perpendicular to its edge. The motion of a wave after diffraction may be assimilated to motion along a tube of which part of the side has been cut away." Now, though perfectly true that in the former case the wave is propagated with the same uniform velocity, it seems very inconceivable, I might say perfectly incredible, that in the latter the wave, attenuated by its lateral extension (for it *must* extend itself laterally), should present an even front with the undiffracted wave, and so far, therefore, from the change of form of the waves being contrary to received principles, it is just what in the nature of things must take place.

But that no one may suppose Fresnel's theory to be free from an objection, real or imaginary, which presses upon mine, I shall proceed to show, that even according to it the form of the wave is changed after diffraction. This would readily appear from a consideration of the formula which Fresnel gives for the disturbance in the example above discussed, but it is manifested with singular clearness in the following investigation, taken substantially from Mr. Airy's tract on the Undulatory Theory, p. 270 *et seq.*



A B a section of the diffracting body by the plane of the paper which likewise passes through the aperture A B, which is to be considered as a parallelogram of very small breadth. [Mr. Airy does not explicitly state this fact, but the investigation assumes it.] Take A H = H B, and let z be the point whose illumination is to be considered.

$$\mathrm{H\,G} = z, \quad \mathrm{H\,Z} = c, \quad \mathrm{G\,H\,Z} = \theta, \quad \mathrm{A\,B} = 2b.$$

Considering each small element dz of A B as the origin of a secondary wave, we may represent the vibration at z by

$$\int \frac{dz}{\mathrm{ZG}} \sin \frac{2\pi}{\lambda}(vt - \mathrm{Z\,G})$$

$$= \int \frac{dz}{c} \sin \frac{2\pi}{\lambda}(vt - c - z\cos\theta)$$

$$= \frac{1}{c}\,\frac{\lambda}{2\pi\cos\theta} \cdot \frac{\cos 2\pi}{\lambda}(vt - c - z\cos\theta) \qquad \begin{pmatrix} z = +b \\ z = -b \end{pmatrix}$$

$$= \frac{1}{c} \cdot \frac{\lambda}{2\pi\cos\theta}\left(\cos\frac{2\pi}{\lambda}(vt - c - b\cos\theta) - \cos\frac{2\pi}{\lambda}(vt - c + b\cos\theta)\right)$$

$$= \frac{\lambda}{\pi c\cos\theta} \sin\frac{2\pi}{\lambda} b\cos\theta \,.\, \sin\frac{2\pi}{\lambda}(vt - c).$$

Now if this really represented the intensity at z, as Mr. Airy would have us believe, it would follow, that after entering the aperture the waves cease to be plane, and assume a spherical form, having the middle point of the aperture for their centre. Hence it is plain, that if the variation in velocity of the diffracted waves be an objection, Fresnel's theory is as much open to it as mine*.

The considerations with reference to the change in the condition of the diffracted waves which I have above endeavoured to unfold, were suggested to me at a time when I looked up to Fresnel and his theory with some portion of that respect and reverence which certain teachers of the University of Cambridge have so sedulously endeavoured to inculcate. Neither doubting his experimental results, nor indeed his

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* In discussing the above example, I have not thought proper to advert to the erroneous approximation contained in it, which I have elsewhere sufficiently considered, and which is such as to render it totally valueless. But I may observe, that not only is the formula for the intensity incorrectly deduced from the premises laid down, but it is itself *untrue*. For, according to that formula, we ought to have a series of maxima and minima in the plane of the paper and nowhere else, whereas it is very certain that the principal maxima and minima would *not* occur in the plane of the paper. It is obvious that the screen would be crossed by bands parallel to the *length* of the aperture, and therefore in a plane *perpendicular* to the plane of the paper, as Mr. Airy himself afterwards admits (see art. 78 of his treatise).

theory, so far as it went (which in the way of *explanation* was not a great way), I was led to the above conclusions while in search of a more intelligible mode of accounting for the phænomena than Fresnel's appeared to be, and which I naturally sought in the only direction in which, assuming the trustworthiness of his experiments, it could be found, namely by consideration of the effect of the diffraction of the waves themselves abstracted from any specific action of the diffracting body. But I must confess that since that time my faith in Fresnel's merits as an experimentalist, as well as a theorist, have been greatly shaken, and that I do not feel the degree of confidence which I once entertained as to the truth of the position which he assumes himself to have established by experiment, "that diffraction is independent of the nature of the diffracting body and of the form of its edge." But if this be not the case, Dr. Young's theory of diffraction is at once revived; and thus this point of fact forms an *experimentum crucis* as to whether his theory or mine is to be received. If Fresnel's conclusion be contrary to the fact, and the diffraction *is* dependent on the nature of the diffracting body, my theory cannot well be true and his cannot well be false, and *vice versâ*. But whatever be the conclusion we may ultimately come to upon this point, and to experimentalists I leave it, the above suggestions may not be without their use; first, as directing attention to the nature of the motion of discontinuous waves, a subject which has never hitherto been discussed in any intelligible manner; and secondly, as thereby affording a clearer and more certain elucidation than has ever yet been given of that remarkable phænomenon usually referred to, by saying that "light will not penetrate round a corner."

I hope to have an opportunity at an early period of entering upon the consideration of Fresnel's theory of polarized light and of double refraction.

10 Maddox Street, Bond Street,
June 13, 1845.

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxvi. p. 539.]

May 22, 1845. "MEMOIR on the Rotation of Crops, and on the Quantity of Inorganic Matters abstracted from the Soil by various plants under different circumstances." By Charles Daubeny, M.D., F.R.S., Professor of Rural Economy, &c. in the University of Oxford.

The author was first led to undertake the researches of which an account is given in the present memoir, by the expectation of verifying the theory of DeCandolle, in which the deterioration experienced by most crops on their repetition was attributed to the deleterious influence of their root-excretions. For this purpose he set apart, ten years ago, a number of plots of ground in the Botanic Garden at Oxford, uniform as to quality and richness, one-half of which was planted each year, up to the present time, with the same species of crop, and the other half with the same kinds, succeeding each other in such a manner that no one plot should receive the same crop twice during the time of the continuance of the experiments, or at least not within a short period of one another. The difference in the produce obtained in the two crops, under these circumstances, would, the author conceived, represent the degree of influence ascribable to the root-excretions.

The results obtained during the first few years from these experiments, as well as from the researches which had, in the mean time, been communicated to the world by M. Braconnot and others on the same subject, led him in a great measure to abandon this theory, and to seek for some other mode of explaining the falling off of crops on repetition. In order to clear up the matter, he determined to ascertain, for a series of years, not only the amount of crop which would be obtained from each of the plants tried under these two systems, but also the quantity of inorganic matters extracted in each case from the soil, and the chemical constitution of the latter, which had furnished these ingredients. The chemical examination of the crops, however, on account of the labour it involved, was confined to six out of the number of the plants cultivated; and of these, three samples were analysed, the first being the permanent one, viz. that cultivated for nine or ten successive years in the same plot of ground; the second, the shifting one, obtained from a plot which had borne different crops in the preceding years; the third, the standard, derived from a sample of average quality, grown under natural circumstances, either in the Botanic Garden itself, or in the neighbourhood of Oxford. These analyses were performed by Mr. Way, formerly assistant to Professor Graham, of University College, London, and now attached to the Agricultural College near Cirencester.

The examination of the soils was carried on in two ways; the first, with the view of estimating the entire amount of their available ingredients; and the second, with that of ascertaining the quantity in a state to be taken up at once by plants, the available ingredients being those which are soluble in muriatic acid; the active ones, those which are taken up by water impregnated with carbonic acid gas. This portion of the investigation was conducted in part by the author, and in part by Mr. Way, and has reference to three subjects; first, to the amount of produce obtained from the deficient crops; secondly, to their chemical constitution; and thirdly, to the nature of the soil in which the crops were severally grown.

The plants experimented upon were spurge, potatoes, barley, turnips, hemp, flax, beans, tobacco, poppies, buckwheat, clover, oats, beet, mint, endive, and parsley. The only crop which seemed to show the influence of root-excretions was *Euphorbia Lathyris*, which would not grow in the same ground three years successively, although the soil was found afterwards fitted for rearing several other species of plants. In the remaining cases, there was in general a marked difference between the permanent and the shifting crop, to the disadvantage of the former; and where exceptions occurred to this rule, they seemed capable of being accounted for by accidental causes. The amount of each year's crop is given in a tabular form, and their differences illustrated by diagrams showing the relation between the two crops of each vegetable.

The second part of the memoir commences with an account of the method of analysis pursued by the author for determining the nature and proportions of the ingredients present in the ashes of the crops submitted to examination. This method was, in general, similar to that recommended by Will and Fresenius in their paper published in the Memoirs of the Chemical Society *; but in determining the amount of phosphoric acid, the following mode was adopted in preference to the one therein given.

As the phosphoric acid would seize upon the iron in preference to any other base, the amount of peroxide of iron present in the ash was first determined by precipitating it from a muriatic solution by means of acetate of ammonia. The weight of the precipitate gives that of phosphate of iron, from which that of the peroxide of iron may be readily calculated. This being ascertained, he proceeds to determine the phosphoric acid by operating on a fresh portion of the solution of the ash, into which a certain known weight of iron dissolved in muriatic acid is introduced, in quantity more than sufficient to unite with the whole of the phosphoric acid present. This done, acetate of ammonia is added and the mixture boiled, when all the peroxide of iron, whether combined with phosphoric acid or not, is thrown down. From the weight of the precipitate, that of the phosphoric acid present may be calculated, as both the amount of peroxide of iron present in the ash, and that which was added subsequently, are known.

A report is then given of the analysis of the ashes of barley, of the tubers of potatoes, of the bulbs of turnips, of hemp, of flax, and of beans, all cultivated in the Botanic Garden; and from the data thus obtained, the quantity of inorganic matters abstracted from the soil in ten years by the above crops is deduced: and a table is given showing the relation between the permanent and shifting crops, with respect to their produce, the amount of inorganic matters, that of alkali, and that of phosphates, contained in them.

In the third part of his paper, the author considers the chemical composition of the soil in which the above-mentioned crops were grown. He states, in the first place, the method he adopted for determining the amount of phosphoric acid present in the soil.

An analysis is then given of the soil taken from a portion of the garden contiguous to that in which the experiments were carried on, and from one of the plots of the garden itself, and from these data

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* See Phil. Mag. S. 3. vol. xxv. p. 500.

a calculation is made, that the ground at present contains enough phosphoric acid for nineteen crops of barley of the same amount as the average of those of the permanent crops, and of the same quality as that obtained in 1844. It was also found, that there was a supply of potass sufficient for fifteen crops of barley; of soda, for forty-five; and of magnesia, for thirty-four. When, however, we examine how much of these ingredients is taken up by water containing carbonic acid, the proportion of each is found to be much smaller; and a striking difference exists, in this respect, between the soil which had been recently manured and that which had been drawn upon by a succession of crops. In the first case, the quantity of alkaline sulphate obtained in the pound was 3·4 grs.; in the latter it varied from 0·7 to 0·07; and of phosphate, the quantity in the former was about 0·3, whilst in the latter it varied from 0·18 to 0·05.

From these facts the author concludes, first, that the falling off of a crop after repetition depends, in some degree, on the less ready supply of certain of the inorganic ingredients which it requires for its constitution; but that two crops equally well supplied by the soil with these ingredients may take up different quantities of them, according as their own development is more or less favoured by the presence of organic matter in the soil in a state of decomposition.

Secondly, that it is very possible that a field may be unproductive, although possessing abundance of all the ingredients required by the crop, owing to their not being in a sufficiently soluble form, and therefore not directly available for the purposes of vegetation: so that, in such a case, the agriculturist has his choice of three methods; the first, that of imparting to the soil, by the aid of manure, a sufficient quantity of these ingredients in a state to be immediately taken up; the second, that of waiting until the action of decomposing agents disengages a fresh portion of those ingredients from the soil (as by letting the land remain fallow); and the third, that of accelerating this decomposition by mechanical and chemical means.

Thirdly, that it is probable that in most districts a sufficient supply of phosphoric acid and of alkali for the purposes of agriculture lies locked up within the bowels of the earth, which might be set at liberty and rendered available by the application of the artificial means above alluded to.

Fourthly, that the aim of nature seems to be to bring into this soluble, and therefore available condition, these inorganic substances by animal and vegetable decomposition, and therefore that we are counteracting her beneficial efforts when we waste the products of this decomposition by a want of due care in the preservation of the various excrementitious matters at our disposal.

Fifthly, that although we cannot deny that plants possess the power of substituting certain mineral ingredients for others, yet that the limits of this faculty are still imperfectly known, and the degree in which their healthy condition is affected by the change is still a matter for further investigation.

Lastly, that the composition of various plants, as given in this paper, differs so widely from that reported by Sprengel and others, that we are supplied with an additional argument in favour of the importance of having the subject of ash-analysis taken up by a public body, such as the Royal Agricultural Society of England, possessed of competent means and facilities for deciding between the conflicting authorities, and supplying us with a more secure basis for future calculations.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxvi. p. 612.]

April 20, 1844.—On the Transport of Erratic Blocks. By W. Hopkins, M.A., F.R.S. &c.

The principal object of this paper is to investigate the transporting power of currents of water in general, and to explain in particular the nature of those which would arise from the instantaneous or paroxysmal elevation of any considerable extent of the earth's surface lying beneath the surface of the sea. The author has termed them *elevation currents*. The immediate effect of an elevation like that just supposed, would be the elevation to a nearly equal height, of the surface of the superincumbent water, whence a great wave would diverge in all directions. Such a wave would be attended by a *current* in the direction of the wave's propagation, and has thence been called a *wave of translation*. When such a wave proceeds along a uniform canal, Mr. Russell has established experimentally the following facts:—

1. Every particle in the same transverse section of the canal has the same motion.

2. The velocity with which the wave is propagated is equal to that due to half the height of the crest of the wave above the bottom of the canal.

From these data the author has calculated the velocity of the currents which would necessarily attend these waves of elevation. It depends principally on the height of the elevation and the depth of the sea, while the time during which the current will flow, depends principally on the extent of the elevated area and the depth of the sea. Thus if the depth of the sea should be 300 feet, and the height of the crest of the wave above the even surface of the sea (which may be considered as approximately the same as the elevation of the suddenly raised area) should be 50 feet, the wave would be propagated with a velocity of upwards of 70 miles an hour, and the attendant current would be upwards of 10 miles an hour. Also, if the elevated area were circular, the width of the wave would exceed the radius of the circle. The wave would have the essential character of a tidal wave termed a *bore*, except that it diverges in all directions, instead of proceeding along a confined channel.

The author next proceeds to calculate the motive power of currents of water. Let v be the velocity of the current, ρ_1 the density of the water, and S the area of a plane surface on which the current acts, and so placed as to make an angle θ with the direction of the current; then if R denote the whole normal action of the current on we have

$$R = \frac{v^2}{2} \rho_1 S \sin^2 \theta,$$

provided θ do not deviate too much from 90°. When $\theta = 90°$ the truth of this formula has been proved by numerous experiments, for all velocities up to 11 or 12 miles an hour, and may be assumed to hold, at least approximately, for still greater velocities. It has also been proved experimentally to be approximately true for any value of θ not differing by more than 45° from a right angle, as is the case in the applications made of the formula.

The velocity of the current just sufficient to move a block will depend on the volume, the specific gravity, and the form of the block. If the block *slide*, much will depend on the nature of the surface over which it is transported, and thus a very uncertain element will be introduced into the calculations. This uncertainty, however, will be in a great degree removed if we calculate the force sufficient to make the block *roll*. Each block would present a separate problem if it were required to find accurately the current necessary to move it, but as great accuracy is not necessary in the cases here contemplated, it is sufficient to make the calculations for a few determinate and simple forms as those to which more irregular forms may be referred with a sufficient approximation to accuracy. Thus the author has considered the cases of blocks whose sections perpendicular to their length are squares, pentagons, hexagons, &c., and has calculated their dimensions, that a current of about 10 miles an hour might just be sufficient to make them move by rolling. Assuming the specific gravity of the blocks to be 2·5, we have the following results:—

1. A *parallelopiped*.

Side of the square section perpendicular to its length = 2·73 feet.

2. A *pentagonal prism*.

Side of the pentagonal section perpendicular to its length = 2·27 feet.

3. A *hexagonal prism*.

Side of hexagonal section perpendicular to its length = 2·3 feet.

When the motion takes place, as here supposed, in a direction perpendicular to the length of the block, the efficiency of the current to move it will evidently be independent of the length of the block. If we suppose the length of the parallelopiped to be equal to the side of a section of it taken as above, it becomes a cube; and if we take the lengths of the blocks in the other two cases to be equal to twice the length of the sides of their sections respectively, their lengths will not much exceed their heights. Then the weights of the blocks will be $1\frac{1}{2}$ ton in the first, nearly 3 tons in the second, and upwards of 4 tons in the third case. Again, if the block be an oblate spheroid resting with its axis vertical, and the polar axis $= \frac{3}{4}$ths of the equatorial diameter, the current of about 10 miles an hour will just make it roll if its height be about 2 feet, and its weight about 4 tons. If the polar axis = $\frac{5}{6}$ths of the equatorial diameter, the block will be just moved, provided its height be $3\frac{1}{3}$ feet and its weight 14 or 15 tons.

In this part of the investigation it is shown that the power of rapid currents to transport blocks of enormous magnitude is perfectly consistent with the almost inappreciable power of currents of which the velocity does not exceed, for instance, 2 miles an hour; for it is shown that *the weight of a block of given form and specific gravity, which may thus be moved, varies as the 6th power of the velocity of the current.* Thus if a current of 10 miles an hour will just move a block of a certain form, whose weight is 5 tons, a current of 15 miles an hour would move a block of similar form of upwards of 55 tons. A current of 20 miles an hour would, according to the same law, move a block of 320 tons, while a current of 2 miles an hour would scarcely move a small pebble.

In the previous calculations the relation between the magnitude of the block and the velocity of the current has been determined on the supposition that the current, at the instant it acquires its greatest velocity, shall just be able to move the block, which would again be left at rest without being moved through any sensible space. If the velocity be greater or the mass smaller, the block will be transported to a distance which the author has calculated. Let

v_2 be the velocity of a current just sufficient to move an assigned block;

v_1 the velocity of the transporting current acting on the above block, v_1 being greater than v_2;

l the breadth of the great wave of translation producing the current;

h the height of the highest point of the wave above the level of the ocean;

H the depth of the ocean;

s the space through which the block is transported by the wave.

The following Table gives corresponding values of these quantities. The last column gives the corresponding value of the space (s_0) through which a particle of the water, or any body floating in the water, will be carried by the wave. The expressions for s and s_0 are

$$s = \frac{1}{2} \cdot \frac{(v_1 - v_2)^2}{V v_1} l,$$

$$s_0 = \frac{1}{2} \cdot \frac{v_1}{V} \cdot l,$$

V being much greater than v_1. [See opposite page.]

In estimating the magnitude of a block which may be moved by a given current, the transport is supposed to take place over a horizontal surface sufficiently hard and even for the block to roll upon it without impediment. In other states of the surface the transport might be more or less impeded. The constant action of denuding causes would be highly favourable to the transport by the successive removal of local impediments. The author conceives that the objection to this mode of transport, founded on inequalities of surface

H.	h.	V.	v_1.	v_2.	s.	s_0.
feet.	feet.	miles.	miles.	miles.		
200	50	62	12	5	$\frac{l}{30}$ nearly	$\frac{l}{10}$ nearly
				10	$\frac{l}{372}$ ...	
300	50	73	10·4	5	$\frac{l}{52}$ ...	$\frac{l}{14}$ ...
300	100	77	19·4	5	$\frac{l}{14}$ ...	$\frac{l}{8}$ ...
				10	$\frac{l}{34}$ ...	
400	100	86	17	5	$\frac{l}{20}$ ...	$\frac{l}{10}$ ...
				10	$\frac{l}{60}$ ...	
450	150	95	23·5	10	$\frac{l}{24}$ ...	$\frac{l}{8}$ ...
600	150	106	20·5	10	$\frac{l}{39}$ ...	$\frac{l}{10}$ ...
800	200	140	28	10	$\frac{l}{25}$ ...	$\frac{l}{10}$ ...

which now exist between the original site of a block and its present position, have been far too much insisted on by some geologists, for, he contends, such inequalities could not generally exist under the continued action of denuding causes, among the most powerful of which may be reckoned the transporting currents themselves.

It should be remarked, that it appears from the values of s given in the preceding table, that the space through which any considerable block could be moved by a single wave of elevation, is only equal to a small fraction of the breadth of the wave. Consequently, if such a block has been moved by this agency to a considerable distance from its original site, the transport must have been effected by a repetition of transporting waves; and, therefore, since a wave of considerable height can only be produced by a *sudden* elevation, this theory of transport is ultimately associated with the theory which attributes the more marked phænomena of geological elevation to a repetition of *paroxysmal* movements.

The author concludes with some general observations on the evidence by which we may hope to distinguish between the effects of the three different agencies to which the transport of blocks may be attributed—glaciers, floating ice, and currents of water. Large angular blocks in the immediate neighbourhood of glacial mountains (such as the alpine blocks) may doubtless, in many cases, be referred to glaciers, while the transport of similar blocks to great distances may be referred to floating ice. Smooth rounded blocks of smaller dimensions, especially when spread out with other detrital matter in layers of considerable horizontal extent, the author would refer to the action of aqueous currents.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxvi. p. 361.]

February 14, 1845.—*Extracts from the Report of the Council of the Astronomical Society to the Annual General Meeting.*

To her Majesty's Government we have to express our obligations for printing in our Memoirs, at the public expense, the late Professor Henderson's right ascensions of principal fixed stars, deduced from observations made at the Cape of Good Hope in 1832 and 1833. It has been a fixed rule with the Council not to print in the Memoirs the official transactions of public observatories; and they are happy to say that administrations of all parties, and also the Directors of the East India Company, have always recognised the reasonableness of this rule, the instant the grounds on which it was made were brought before them.

The Council have received from Mr. Baily's executors the manuscript details of the Cavendish experiment.

In the month of March last, Dr. Lee requested that the Society would accept the perpetual advowson of the Vicarage of Stone, near Hartwell. The grant is now enrolled according to the statute. This is the second gift of an advowson from Dr. Lee, to whom the Society is under many other obligations, and who has shown an incessant interest in its welfare, not only by munificent contributions to its funds, its library, and its collection of instruments, but by active personal services extending over many years.

Among the losses by death, your Council have to regret Captain Basil Hall, Professor Henderson, and Mr. J. Frodsham.

Memoir of Professor Henderson.

At none of its former anniversary meetings has this Society had cause to deplore heavier losses than those it has sustained in the past year. The death of its president, Mr. Baily, was followed, at a short interval, by that of Professor Henderson of Edinburgh; an astronomer of first-rate merit, and one who, for many years, has been conspicuously distinguished among us by the frequency and importance of his contributions to our publications. His services to the cause for which we are associated have been, indeed, of no ordinary kind; and, although prematurely terminated, have entitled him to a high place among the most deserving of our members. It becomes, therefore, a duty we owe to his memory to recapitulate in this Report his principal claims to our gratitude, and to place on record a few particulars of his personal history; as well to testify our respect for his eminent merits, as to gratify, however imperfectly, the desire which will be felt to know something of the life and character of one whose labours will henceforth form part of the annals of astronomy.

Thomas Henderson was born at Dundee on the 28th of December, 1798. His father was a tradesman in respectable circumstances, who died early in life, leaving to the care of his widow a family consisting of two sons and three daughters. The eldest son, John, was bred to the legal profession; and, after practising for some time as a writer in Dundee, went to Edinburgh, where he studied for the bar, and was rapidly rising to distinction as an advocate, when he died suddenly, at the age of thirty-eight, of aneurism of the heart. Thomas, the other son, and the youngest of the family, was destined also for the profession of the law, and had the advantage of receiving the best education which could be obtained in a town which has long been distinguished in Scotland for the excellence of its public schools. After the usual preliminary instruction, he was sent, at the age of nine, to the grammar-school, where he pursued the usual course of classical study during four years, and was distinguished by his diligence and quickness of apprehension, being generally the dux of his class. In 1811, he proceeded to the Academy, where he continued two years longer. The Dundee Academy was at that time under the very able rectorship of Mr. Duncan, now Professor of Mathematics in the University of St. Andrew's. The course of instruction included elementary mathematics, natural philosophy, and chemistry; and young Henderson passed through the complete course with the same distinction which had marked his progress at the grammar-school. Professor Duncan, in a letter to the writer of this notice, bears the following testimony to the merits of his former pupil. "The two Hendersons," he says, "were the best scholars I had in the whole period of my incumbency. You are aware, I suppose, that John became distinguished at the bar, and was only prevented, by an early death, from rising to great eminence. Thomas, the future astronomer, was remarkable for every thing that was good,—the diligence and success with which he prepared his lessons, the exactness with which he performed the exercises, the propriety and modesty of his demeanour."

At the age of fifteen he was placed in the office of Mr. Small, a writer (or solicitor) in Dundee, with whom his brother had entered into partnership. In this situation he remained six years; and it appears to have been during this period that he began to devote his leisure hours to the study of astronomy, though the particular circumstance, or accident (if, indeed, there were any other cause than the promptings of a naturally active and inquisitive mind), which first gave this direction to his inquiries is not known. During his attendance at the Academy, and even at an earlier date, he had evinced a remarkable predilection for works relating to geography and chronology; and his taste for these studies was often gratified at the expense of his health, for he was naturally of a weakly constitution, and subject to some disorder of the eyes, which at times rendered him nearly blind. From these subjects to astronomy the transition is easy and natural; and, although he received no aid from the lessons of a master, and had no encouragement from example, yet, in a town which could boast of being the birth-place of Ivory, and in which Dr. Small, the expositor of Kepler, had so long resided, it may be supposed there would prevail, among the better-informed classes of the inhabitants, some general sentiment of respect for proficiency in astronomy and mathematics, which might not be without its influence on a mind possessing a peculiar aptitude for such studies. However this may be, the fact deserves to be recorded, as an instance of what may be done under circumstances apparently the most adverse, that it was while employed as an attorney's clerk in a provincial town, that he laid the foundations of that extensive acquaintance with astronomy for which he became afterwards so distinguished.

Having gone through a six years' apprenticeship, Mr. Henderson, at the age of twenty-one, repaired to Edinburgh for the purpose of completing his legal education and obtaining professional employment. He first obtained a situation in the law office of a Writer to the Signet, where his intelligence and abilities were remarked by Mr. (now Sir James) Gibson Craig, who became his steady patron and friend, and by whose recommendation he was appointed secretary, or Advocate's clerk, to the celebrated John Clerk, afterwards one of the judges of the Supreme Court of Scotland, under the title of Lord Eldin. On Lord Eldin's retirement from the bench, he was for some time private secretary to the Earl of Lauderdale; an office which he relinquished for the more profitable appointment of secretary to the Lord Advocate (Jeffrey). In these successive employments, he passed the twelve years from 1819 to 1831; and it may be remarked that, although of a subordinate nature, they were such as would only be entrusted to a person of acknowledged abilities, and one whose character for fidelity, discretion, and general intelligence, was already established. It is probable that they allowed him considerable intervals of leisure, but in no other respect were they favourable for scientific pursuits; and, indeed, it may be regarded as one of the most remarkable features in his history, that while engaged in the discharge of multifarious and active duties, in a line of life so foreign to astronomy and physical science of any kind, he should not only retain his tastes for the liberal studies he had commenced at the Dundee Academy, but find leisure to make so many new and important acquisitions.

Soon after he became resident in Edinburgh his astronomical acquirements procured him introductions to Professors Leslie and Wallace, Captain Basil Hall, and other distinguished persons, by whom his talents were quickly appreciated, and who afforded him every encouragement to persevere in his scientific pursuits. At that time the small observatory on the Calton Hill, belonging to the Astronomical Institution of Edinburgh, was placed under the charge of Professor Wallace, who, finding in Mr. Henderson a person to whose hands the instruments could be safely entrusted, allowed him free access to them, and thereby gave him an opportunity of acquiring a practical knowledge of a subject which he had already become familiar with from study and books. The instruments, it is true, were not of first-rate excellence,—a clock and transit of 30 inches focal length, with an altitude and azimuth instrument by Troughton, formed the equipment of the observatory; —but to a young astronomer, who had no other access to astronomical apparatus, such an opportunity was invaluable; and there is little doubt that this circumstance had considerable influence on his future history.

The opportunity of making observations, however, did not in any degree withdraw him from the less inviting parts of the science. From the outset of his career he perceived that, in order to be an astronomer, something more is necessary than mere expertness in the use of instruments. He accordingly continued to direct his main attention to the reduction of observations; and, at an early period, acquired a great knowledge of methods, and great facility in calculating eclipses, occultations, cometary orbits, and, generally, in all the computations and reductions which are subservient to practical astronomy.

Mr. Henderson first brought himself into notice as an astronomer in 1824, by communicating in that year to Dr. Young, then Secretary to the Board of Longitude, a method of computing an observed occultation of a fixed star by the moon, of which that accomplished philosopher thought so highly, that he caused it to be published, under the title of an improvement on his own method, in the Nautical Almanac for 1827 and the four following years; accompanied in some of the last of those years by a second method also proposed by Mr. Henderson. These methods were also published in the London Quarterly Journal of Science; and he received for them the thanks of the Board of Longitude. About the same time, or shortly after, he began to contribute to the Quarterly Journal of Science various useful papers and notices; among which may be mentioned, in particular, elements for computing the eclipses of the sun, and lunar occultations of the planets and satellites for the years 1826, 1827, and 1828; and lists of the principal lunar occultations for the years 1826, 1827, 1828 and 1829. These lists, it is presumed, were the cause of several valuable observations of the phænomena being made, which, but for them, would probably have been neglected.

In 1827 he communicated a paper to the Royal Society of London, "On the Difference of Meridians of the Royal Observatories of London and Paris," which is published in the Philosophical Transactions for that year, and which furnishes a remarkable instance of the value of that habit of scrutinising calculation for which he was particularly distinguished. In the copy of the observations officially furnished from the Royal Observatory to Sir John Herschel, with a view to his operations in 1825 for determining the difference of longitude between Greenwich and Paris by means of fire signals, there was an error of a second in one of the numbers, which had the effect of causing some irregularity in the results of the different days' work; but as the discrepancies were small, they had been ascribed to errors of observation. Mr. Henderson, remarking the irregularity, was led to recalculate the original data, and thereby detected the error; and not content with this, he submitted the entire process to a new calculation. His result differed immaterially from that which had been previously obtained; but the correction of the error, by rendering the single results more consistent, gave a greatly increased confidence to the general conclusion; and, as was said of it by Sir John Herschel himself, "had the effect of raising a result liable to much doubt, from the discordance of the individual days' observations, to the rank of a standard scientific *datum*, and thus conferring on a national operation all the importance it ought to possess."

Mr. Henderson's connexion with this Society began by his undertaking, upon the request of the Council, to compute an ephemeris of the occultations of Aldebaran by the moon, in the year 1829, for ten different observatories in Europe. In this undertaking he was associated with Mr. Maclear, and the ephemeris, purporting to be their joint production, was read at the December meeting in 1828, and published in No. 15 of the Monthly Notices. His first contribution to the Memoirs (published in vol. iv.) contained observations of transits of the moon, and stars nearly in the same parallel of declination, over the meridian, made at the Calton Hill Observatory in 1828, from which and corresponding observations made at Greenwich he computed the difference of meridians. This paper deserves notice, as showing that he had already adopted the practice of estimating and allowing for the weights of the results and determining their probable errors, according to the methods in use among the German astronomers, but of which the examples were not, as yet, frequent in this country. The method of determining differences of longitude by means of observations of moon-culminating stars, then recently proposed by Nicolai, had been strongly recommended by Mr. Baily in a paper published in vol. ii. of the Memoirs, where all the requisite rules and formulæ were given for the computation. Mr. Henderson entered into these views with his accustomed energy, and not only embraced every opportunity of putting the method in practice by computing corresponding observations, but was at much pains to promote such observations by preparing lists of moon-culminating stars for the use of observers. A list of this kind was prepared by him at the request of the Council for the use of the Arctic expedition under the command of Captain (Sir John) Ross in 1830.

Another subject on which his talents for computation were frequently exercised in furtherance of the views of the Council, was the calculation of the lunar occultations of fixed stars and planets. Observations of these phænomena, interesting both to practical and physical astronomy, were at that time much encouraged by the Society on account of their use in determining longitudes; and, as such phænomena will seldom be observed unless they have been predicted, it was desirable to ascertain the times of their occurrence by a previous calculation. In this case also Mr. Henderson rendered most efficient aid by contributing, for several years, monthly lists of the principal lunar occultations computed for the meridian of Greenwich.

The promptitude and accuracy with which these calculations were made induced the Council to request him to communicate his methods to the Society; and he accordingly drew up a set of Practical Rules for the Approximate Prediction of Occultations, which are published in vol. iv. of the Memoirs. These services were duly acknowledged in our Annual Reports; and at the anniversary meeting in 1830 the thanks of the Society were voted to him "for the very valuable assistance he had rendered to the cause of astronomy in his various computations presented to the Society."

The amount of Mr. Henderson's contributions to astronomy from 1825 to 1830, consisting of observations, tables, remarks, methods, and calculations of various kinds, published in the Quarterly Journal of Science, the Nautical Almanac, and the Notices and Memoirs of this Society, would have done credit to a professed computer; but in order rightly to appreciate his zeal, it must be remembered that he was all this while occupied with professional duties of a kind which would be found by most persons to be sufficiently engrossing. His disinterestedness was no less remarkable than his scientific ardour; for though in the receipt of very moderate emoluments, he declined all remuneration for his calculations; nor would this feature of his character be fully appreciated unless it were told at the same time that a considerable part of his income was appropriated by him to the support of his mother and sisters.

Mr. Henderson's official duties, while connected with the Earl of Lauderdale and the Lord Advocate, brought him for some months in each year to London, upon which occasions he became personally acquainted with the principal astronomers of the metropolis, and had an opportunity, particularly at the observatory of Sir James South, which was freely thrown open to him, of seeing and handling instruments of the first class. His various useful contributions to astronomy had already acquired for him a considerable reputation; and the high opinion which had been formed of his talents was increased by observation of the worth and unaffected simplicity of his character, and the range and extraordinary accuracy of his information on all astronomical subjects. Accordingly, a prospect of attaching him to an office by which his services would be secured for the exclusive benefit of astronomy, afforded satisfaction to all those who took an active interest in the progress of our science.

The death of Dr. Robert Blair, in December 1828, having caused a vacancy in the professorship of Practical Astronomy in the University of Edinburgh, Mr. Henderson's qualifications for that office were represented to the Government (the patrons of the appointment) by Dr. Thomas Young, and energetically urged by some other astronomers, particularly by Captain Basil Hall. From the correspondence which passed with the Secretary of State, it appears that the Government upon that occasion postponed the filling up of the vacancy in order that opportunity might be given to consider upon what footing the office, which had hitherto been a sinecure, could be placed with the greatest prospect of advantage to science. In the summer of 1829 another astronomical appointment became vacant by the lamented death of

Dr. Young. This very eminent person had received valuable assistance from Mr. Henderson in the computations required for the Nautical Almanac, and had formed the most favourable opinion of his talents. About a fortnight before his death he placed a memorandum in the hands of the late Professor Rigaud of Oxford, to be made use of in case of his decease; and when the anticipated event took place, the memorandum was found to contain a request that it might be stated to the Board of Admiralty that he knew no person more competent to be his successor in the superintendence of the Almanac than Mr. Henderson. Professor Rigaud lost no time in making known in the proper quarter the recommendation of his deceased friend, which he also supported with the weight of his own influence; but in consequence of other contemplated arrangements it was unsuccessful, and the superintendence of the Nautical Almanac was, upon that occasion, committed to Mr. Pond, then Astronomer Royal. Mr. Pond was also well aware of the efficiency and value of Mr. Henderson's aid, and immediately preferred a request to him to continue the same computations which he had been accustomed to supply to Dr. Young; making offer at the same time of remuneration, and sufficient employment to occupy a great portion of his time. But although Mr. Henderson shortly after supplied Mr. Pond with some calculations on which he had previously been engaged, this offer appears to have been declined, and for two years longer he continued to follow his professional occupation.

His character as an astronomer, however, was now fully established; and accordingly, on the death of Mr. Fallows in 1831, he was regarded as one of the persons best qualified to undertake the direction and management of the observatory established by Government, and then recently completed, at the Cape of Good Hope. Through the intervention of Captain Beaufort his qualifications were a second time brought under the notice of the Lords Commissioners of the Admiralty, and on this occasion successfully: but the idea of leaving his country was distasteful to him; and he accepted the office with some reluctance, and only in deference to the advice of his friends. The warrant of his appointment is dated in October 1831, and a few months after he embarked for the colony.

Mr. Henderson arrived at the Cape in April 1832, and forthwith commenced his observations. The principal instruments were a ten-foot transit by Dollond, and a six-foot mural circle by Jones; and his only assistant was Lieutenant Meadows, who had been sent out in the previous year. Few examples are upon record of more zealous and successful exertion than that which is furnished by him during his residence of thirteen months at the Cape. The results of his own personal exertions during that short interval comprehend the determination of the latitude and longitude of his station; the positions of stars near the South Pole for determining the polar positions of his instruments; the amount of refraction near the horizon; observations of the moon and stars for determining the moon's horizontal parallax; of Mars for determining the parallax of that planet, and thence that of the sun; of eclipses of Jupiter's satellites; occultations of fixed stars by the moon; a transit of Mercury; places of Encke's and Biela's comets; and finally, between 5000 and 6000 observations of declination. So large a mass of work would have been sufficiently creditable under the most advantageous circumstances; but when we call to mind that he had to contend with all the difficulties incidental to a new and imperfectly organized establishment, where no assistance was to be obtained from artists, and with a notoriously unmanageable instrument, it will be easily admitted that it would be difficult to overrate the zeal, perseverance and skill, with which he laboured to discharge the duties which had been intrusted to him. But the merit of accumulating so large and valuable a mass of observations, great as it must be allowed to be, is completely thrown into the shade by comparison with that which is due to the persevering industry with which he laboured in their reduction, and in deducing from them the results we shall presently have occasion to mention.

When Mr. Henderson accepted the appointment to the Cape Observatory, it was no doubt anticipated (probably even by himself) that his residence there would be of some considerable duration. These anticipations were not realized, for in May 1833 he resigned the office, and shortly after returned to Europe. The reasons which induced him to take this step were fully stated by him in his letter of resignation addressed to the Secretary of the Admiralty. After briefly alluding to the exertions necessary to carry their lordships' instructions into effect, he went on to say, that not only did the state of his health render him unable much longer to support the requisite labour, but that the observatory, considered as a place of residence, laboured under so many disadvantages, and required a mode of life so different from that to which he had been accustomed, that he found it impracticable to remain longer; and being thus unable to perform the duties, he felt it to be incumbent on him instantly to resign the situation. But with that regard for the interests of science, and that kindness of disposition so eminently characteristic of him, he added, that on his return home he would proceed, with the sanction of their lordships, to the task of reducing the observations and of extracting from them the results they were intended to afford; expressed his sense of the efficient aid he had received from his assistant, Lieutenant Meadows; modestly proffered such assistance as his experience might enable him to give to his successor; and concluded by recommending the state of the observatory to their lordships' consideration.

Had his state of health permitted him to continue his observations satisfactorily, there can be little doubt that the inconveniences of which he complained would in due time have been removed, or at least have become supportable; but he had been made aware before he left England, that his physicians already apprehended the germs of that disease which eventually proved fatal to him, and under the depressing influence of this knowledge, aggravated by separation from all his friends, and his family to whom he was tenderly attached —and by his complete isolation—his spirits gave way, and he became apprehensive that he would be unable to maintain the observatory, from which so much was expected, in a state of the requisite efficiency. Impressed with this idea he took the resolution to give in his resignation, in order that it might have the contingent benefit of another appointment; and thus an act which, hastily judged, might appear to be an abandonment of his post, was the simple and natural result of a high, it may be an exaggerated, sense of public duty. The sacrifice involved in his resignation was of no small importance to him in a pecuniary point of view; for all he had to fall back upon was a pension of 100*l.* a-year, to which he had become entitled upon the retirement of Lord Eldin. But no one was ever less influenced by considerations of personal or pecuniary advantage, and under any circumstances he would have disdained the emoluments of office without the most punctilious discharge of its duties.

On his return to this country in 1833, Mr. Henderson took up his abode in Edinburgh, and being now without official engagements, he began the task of reducing the rich store of observations he had brought with him from the Cape. The first result of this self-imposed labour was the determination of an important astronomical element—the sun's parallax—from a comparison of observations of the declinations of Mars near opposition, made at Greenwich, Cambridge and Altona, with the corresponding observations at the Cape. Previous to his departure for that station he had expressed a wish that a selection should be made of such stars as could conveniently be observed with Mars at the opposition in November 1832, with a view to the determination of the parallax, and that a list of them should be circulated among different astronomers in various parts of the world, for the purpose of obtaining corresponding observations. Accordingly, Mr. Sheepshanks having furnished the apparent places of Mars during the requisite period, and Mr. Baily having selected the stars to be observed, the Council caused the list to be printed and circulated with directions as to the mode in which the observations should be made. In consequence of these preparations four sets of corresponding observations were obtained, from each of which Mr. Henderson deduced a value of the parallax. The mean of the whole gave a parallax of $9''\cdot028$; a result which is known from the more certain method of the transits of Venus to be somewhat too large, as was the case also in Lacaille's attempt at the Cape in 1751, to determine the solar parallax by the same method. The determination, he remarks, is chiefly valuable as showing the probable accuracy of the method, and the limits within which this important datum in physical astronomy may be determined independently of the rare phænomena of the transits of Venus.

Another paper of a more elaborate kind followed soon after, containing an investigation of the anomalies of the six-foot mural circle in the Cape Observatory. When this instrument was first set up there were found to be considersble discrepancies in the reading of the different microscopes, a circumstance which occasioned great perplexity to Mr. Fallows; and although that astronomer ascertained that the mean of the six readings might be depended upon, he did not succeed in arriving at any satisfactory conclusion respecting the cause and the laws of the irregularities. Aware of these anomalies, which indeed were confirmed by his first observations, Mr. Henderson a few weeks after his arrival at the Cape, undertook a laborious examination of the state of the instrument by means of a series of readings of each of the six microscopes at every tenth degree of the limb; and in April and May 1833, shortly before his departure from the Cape, he repeated the experiment on a more extended scale, by examining the division corresponding to every fifth degree of the circle, and also the divisions immediately before and after it. On the results of this last experiment, which, however, were found to be identical with those of the former one, he grounded the investigation which forms the subject of the paper;—an investigation which exhibits in a very advantageous point of view his sagacity, patience and laborious accuracy, and is an admirable model of a carefully conducted experimental inquiry. The result was, that in order to explain the observed anomalies it was necessary to suppose the figure of the instrument to be an oval of small eccentricity, that the pivots of the axis were not exactly circular, and that the whole instrument frequently changed its position upon the pier from the defective bearing of one of the pivots. But the most important conclusion deduced from the investigation,—as it involved no less important a question than the trustworthiness of the *whole* of his Cape observations,—was that the mean of the readings of the six equidistant microscopes was affected only to a very small extent (if affected at all) by these imperfections, and that the probable error of the instrument is not greater than the errors of the best instruments of similar construction hitherto made. From a similar investigation, founded on a less complete examination of the instrument by Mr. Fallows, Mr. Sheepshanks and Mr. Airy, in a paper which is printed in vol. v. of the Memoirs, had previously arrived at a similar conclusion*.

While thus busily employed with the reduction of his own observations, on the results of which, he was well aware, his reputation as an astronomer would essentially depend, Mr. Henderson's assistance was still, as it had been in the early part of his career, freely extended to others whenever an opportunity occurred of promoting the cause of astronomy. Thus, at the request of Mr. Baily, he undertook the reduction of Captain Foster's observations of the comet of 1830, made at Ascension Island, the results of which, together with an ephemeris for facilitating the calculation of observations of the comet made in the southern hemisphere, are published in vol. viii. of the Memoirs. Fortunately, however (even though thus turned to

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* It may not be without interest to state that, when this instrument was sent back to England some years afterwards, and examined at the Royal Observatory, it was found that, owing probably to some oversight in the construction, the large steel collar carried by the conical axis was quite loose. It is well, perhaps, the discovery was not made sooner, or the instrument would probably have been condemned, and the observations been lost.

account), the interval of leisure was of no long duration; and he was soon called upon for the discharge of more active duties. In 1834 an agreement was concluded between the government and the members of the Astronomical Institution of Edinburgh, whereby the latter gave up to the University the use of their observatory on the Calton Hill, which the former undertook to convert into a public establishment, by furnishing it with suitable instruments, and making provision for an observer and assistant. It was then resolved to fill up the office of Professor of Practical Astronomy, which had remained vacant since 1828, and to combine with it the direction and superintendence of the observatory; and the Secretary of State did this Society the honour to request that a deputation from the Council would confer and advise with him respecting the person whom it might be proper to appoint to the situation. In consequence of this request, a deputation waited upon Lord Melbourne, and, in the strongest terms, recommended Mr. Henderson, whose appointment accordingly followed immediately. The royal commission, nominating him Professor of Practical Astronomy and His Majesty's Astronomer for Scotland, which was dated the 18th of August of that year, required him to take upon himself the care and custody of the instruments within the observatory, and "to apply himself with diligence and zeal to making astronomical observations at the said observatory, for the extension and improvement of astronomy, geography, and navigation, and other branches of science connected therewith."

Mr. Henderson was now placed in a situation suited in every respect to his tastes, habits, and pursuits; and, as he was still young, those who were best acquainted with the extent of his knowledge, his industrious habits, and his facility and accuracy in all practical matters, formed the highest expectations of the value of his future services to astronomy. Nor were their expectations disappointed. The annals of the Edinburgh Observatory, his Catalogue of Southern Stars, his investigations of annual parallax, and other deductions from his Cape observations, besides various contributions relative to subjects of a less important, but always of an interesting nature, amply justify the recommendation of our Council; while their very excellence increases our present regrets that a career so auspiciously begun has been so prematurely brought to a close.

As soon as Mr. Henderson had got his observatory into working order, and had established a regular routine of duty, he resumed the reduction of his Cape observations,—an occupation which engrossed the greater part of his leisure time during the remainder of his life, and of which the fruits are a series of papers communicated to the Society and published in our Memoirs, all more or less interesting, and some of them of the first order of excellence.

The next result of these reductions, and, indeed, one of the most intrinsically important of the whole, was a Catalogue of the declinations of 172 principal fixed stars, chiefly in the southern hemisphere, which was read to the Society at the April meeting in 1837. Owing to various causes of delay, the reduction of the right ascensions of the same stars was only completed, and the results given to the Society, in the course of the last year. They have, however, already been printed, and will appear in our forthcoming volume. Although the number of stars contained in this Catalogue is not large, it acquires importance and value both on account of the still relatively defective state of our knowledge of the absolute positions of the southern stars, and from the circumstance that it is the first which has been deduced from observations made in the southern hemisphere with instruments equal to those of the best European observatories. Indeed, if we except Mr. Johnson's excellent Catalogue of Stars observed at St. Helena, it must be regarded as the first in which the places of stars not visible in our latitudes have been determined with the precision and certainty now aimed at in astronomy. It was principally with a view to the determination of the positions of the principal southern stars, for the aid of navigation, that the establishment of the Cape Observatory had been urged upon and undertaken by the government, and this catalogue forms one of the most important instalments astronomy has yet received from it.

Although the determination of the absolute places of the stars formed the principal business of the observatory, Mr. Henderson was too zealous an astronomer to omit taking advantage of the circumstances in which he was placed to investigate various important points which can only be determined by a comparison of observations made at places remote from each other, or which acquire a special interest from the position of the observer. A point of this latter kind is the amount of refraction in the southern hemisphere. In an interesting paper on this subject, printed in vol. x. of the Memoirs, he has given the results of a series of observations made at the Cape, of stars having a greater zenith distance than 85°, both north and south, and compared the refractions thence deduced with the tables of Bessel and Ivory. As tending to throw light on the important subject of horizontal refractions, these results are valuable with reference to general physics as well as to practical astronomy.

Another important determination, also prompted by the locality, was that of the moon's horizontal parallax. It is well known that the determination of this element was one of the principal objects of Lacaille's voyage to the Cape; his purpose being to obtain observations of the moon's declination corresponding to others made at different European observatories. A similar investigation was proposed for Mr. Henderson, or rather several stars had been marked in the Berlin Ephemeris and Nautical Almanac as favourably situated for having their declinations observed along with the moon's in both hemispheres, by which means the moon's apparent declinations are obtained free from the effects of errors in the assumed declinations of the stars. But the number of corresponding observations of this kind which could be made, being found to be too small to permit the element to be determined from them with sufficient accuracy, Mr. Henderson had recourse, whenever there was a deficiency of moon-culminating stars, to such of the principal stars as were observed, and could be used for comparison. The investigation, like all others which he undertook, is conducted in the most careful manner; and the result, although, in consequence of the mode of proceeding adopted, it is dependent on the accuracy of the determination of the relative declinations of the principal stars, is probably the best determination of the constant we are in possession of.

It would almost seem as if, in these investigations, Mr. Henderson had it in view to repeat the labours of Lacaille in his memorable residence at the Cape about the middle of the last century. In another and more important feature he may be considered as an imitator of that great astronomer. Lacaille had remarked that, in the *then* advanced state of astronomy no one could any longer be believed on his mere word; and that, in order to employ with confidence an observed position, it was necessary to have all the details of the observation and all the elements of reduction. No astronomer was ever more careful of the observance of this important maxim than Mr. Henderson. From the reading of the circle to the final result of the investigation, every thing necessary for the complete understanding of the process or verification of the work is carefully set forth and explained; and, accordingly, all his investigations and results are characterized by an air of truthfulness which it would be in vain to look for where a different practice is followed.

But of all the results which he deduced from this persevering examination of his Cape observations, the one which will be considered as the most interesting is the annual parallax of the double star α Centauri, amounting to about a second of space. This binary system was recognised to be double about a century ago; and, on comparing the observations of Lacaille with those of the present time, it is found to have an annual proper motion of about $3''\cdot6$. Being situated within about 30° of the South Pole, it is always above the horizon at the Cape, and consequently favourably placed for being accurately observed at all times. The intrinsic brightness of the two component stars is also remarkable; and from this, and their large proper motion—circumstances indicative of proximity to our system—Mr. Henderson was led to suspect they might have a sensible parallax. He was not, however, aware of their large proper motion till he was about to leave the Cape; and the observations from which the result was first deduced, were accordingly not made with especial reference to the question of parallax, but for the purpose of determining their mean positions. The first indications of parallax were detected upon a comparison of their declinations with those of such of the standard stars as were observed on the same day throughout the year; but he deferred the announcement of the discovery until he had completed the reduction of the right ascensions, and obtained, by this means, a further test of the accuracy of his conclusion. A similar investigation of these latter observations confirmed the previous deduction; and, in a paper read to the Society at the January meeting of 1839, he announced a parallax of the double star amounting to about a second of space. With his habitual caution and accuracy, he applied every means of testing the result the observations afforded, deducing the parallax separately from the observations of right ascension, from the direct observations of declination, and from the reflected observations of declination, and this in respect of each of the two stars; and though the resulting parallax differed somewhat in the different cases, the general agreement was satisfactory, and the amount much too large to be ascribable to probable errors of observation. But notwithstanding the corroboration which the results of these different modes of deduction afforded each other, if the question of the parallax of this binary star had been allowed to rest in the state in which it was placed in Mr. Henderson's first paper, astronomers would probably have agreed in thinking his investigation only went the length of establishing a strong probability in favour of its existence. Mr. Main, after an elaborate comparison of the result with the individual observations from which it was deduced, observes, "For the present it must be considered that the star well deserves a rigorous examination by all the methods which the author himself has so well pointed out; and that, in the event of a parallax at all comparable with that assigned by Mr. Henderson being found, he will deserve the merit of its discovery, and the warmest thanks of astronomers, as an extender of the knowledge we possess of our connexion with the sidereal system."—*Memoirs*, vol. xii. p. 36. The result, however, was far too curious to be allowed long to remain without an attempt being made to confirm it. No sooner was Mr. Henderson's paper communicated to Mr. Maclear, than that energetic astronomer undertook a series of observations of the double altitudes of the two stars with the mural circle, with an express view to the question. They extend over seventeen months, from March 26, 1839, to August 12, 1840; and, as the mural circle at the Cape was changed shortly after their commencement, they have the further advantage of having been made, for the greater part, with a different instrument. These observations being transmitted to Mr. Henderson, he immediately undertook to reduce them; and, in an elaborate paper inserted in vol. xii. of the Memoirs, he deduced the parallax for each of the two stars both from the direct and reflected observations. The results entirely confirmed his former deductions, but, as was to be expected from the greater number of observations, they were more consistent. The mean gave, as before, a parallax of about 1″; whence it is inferred that this system, the nearest, perhaps, of the stellar bodies, is separated from us by an interval exceeding 200,000 times the distance of the sun from the earth.

Looking to the history of this interesting subject—to the consistent results given by the Dublin circle, which have been disproved by other instruments—and considering that the present determination has as yet been only partially confirmed by the observations of right ascension, the cautious astronomer will, perhaps, be inclined to maintain some reserve until the star shall have been examined with an instrument affording the means of exact micrometrical measurement, or, at least, until a longer series of meridian observations shall have been made; for, as has been remarked by Sir John Herschel, "It is only on a very long series of observations of absolute places, affected, as they are, by instrumental error and uncertainty of refraction, that any conclusion of this kind can rest with security." But whatever the ultimate decision of astronomers may be with respect to the parallax in question, there will be but one opinion as to the merit of Mr. Henderson's investigations, and the interest which attaches to the subject: and, in the meantime, it will be remembered that this is the first determination of the parallax of a star which has been confirmed by a different observer using a different instrument; and that if future observations shall continue to give similar results, which now seems a reasonable anticipation, to Mr. Henderson will indisputably belong the enviable distinction of having been the first who succeeded in an inquiry so often, but fruitlessly, attempted by astronomers, namely, the discovery of a fixed star whose distance from our system is capable of measurement and expression. For these researches he was proposed to the Council at the meeting in November last, as deserving the Society's gold medal for the present year, but his untimely death prevented the question from being entertained.

Another attempt made by him to determine the annual parallax was less successful. Sirius, the brightest star in the heavens, might be supposed to be one of the least remote; and some astronomers—Cassini, Lacaille, and Piazzi—had assigned it a parallax of several seconds. The Cape observations being well adapted for the investigation of the question, Mr. Henderson undertook the examination of those made by himself, and also a series by Mr. Maclear, in order to ascertain if they indicated any sensible parallax; but in this case the result amounted only to about a quarter of a second,—a quantity not exceeding, perhaps, the probable error of the determination.

Mr. Henderson's labours on the subject of parallax did not terminate even here. In a letter to Mr. Main, which was read at the December meeting of 1842, he gave the results of an investigation of the parallaxes, as they appear from his Cape observations, of twenty stars which had been observed sufficiently often to reduce the errors of observation within reasonable limits. They comprehend the greater number of the stars situated in the same region of the heavens as α Centauri, which have been indicated by Sir John Herschel as deserving of investigation for parallax, and which, on that account, are at present objects of particular attention at the Cape Observatory. In a few instances considerable parallaxes appear; in the greater number of cases the results are so small as to afford little hope of a measurable parallax being found, and in some they are negative. The mean of the twenty parallaxes is $+0''\cdot29$, a result which, on the whole, may be considered as affording encouragement to continue the research.

It is matter of much regret that the reduction of the entire series of Mr. Henderson's Cape observations—the rich store from which the above interesting results have been extracted—has not been completed by him; and that the observations must consequently be deprived of the advantage of his final revision. From the examination of his papers it appears that the number of positions determined both with the mural circle and transit instrument, from April 10, 1832, to May 24, 1833, is between 5000 and 6000. The transit observations (made chiefly by Lieut. Meadows) are found to be carefully written out, with the column headed "correction of instrument" filled up for the whole period; and that headed "correction of clock" from the commencement to October 31, 1832. The circle observations, which were all made by himself, are in a similar state of forwardness; and it may be inferred from the appearance of the papers, that if he had lived a few months longer, and the work had proceeded at the ordinary rate, the whole would have been ready for the press.

A statement of the results deduced by Mr. Henderson from his Cape observations could not be properly concluded without an expression of admiration for that disinterested zeal which led him to undertake and execute so great an amount of extra-official labour. The observations, it will be remembered, were made at a public establishment, and their reduction was matter of public concern and importance; and after his resignation of the appointment, the public had no claim on his services more than on those of any other individual. But, as already remarked, considerations of this kind never, in any degree, influenced his conduct; and, during ten long years, he gratuitously devoted to this purpose all the time he could spare from his official duties. In the same circumstances almost any other person would have claimed, and undoubtedly have obtained, both assistance and remuneration.

In addition to the results now mentioned, he rendered still another important service to the astronomy of the southern heavens, in superintending the reduction of the stars observed at the Cape by Lacaille. This work, which he undertook at the instance of Mr. Baily for the British Association, was announced as completed in our Annual Report for 1843; but it has not yet been published.

Mr. Henderson's labours in the Edinburgh Observatory are well-known to astronomers from the five volumes of observations which have been published for 1834–1839. A sixth volume is understood to be left nearly ready for publication; and the observations for the remaining years will, no doubt, still be rendered available to science. The published volumes are prefaced by an Introduction, containing a minute and most lucid description of the instruments and methods used in the observations and reductions, with every detail and explanation which can contribute to give authenticity and value to the work. His first year's observations having been referred to this Society by the Home Secretary, a committee of the Council reported them to be of first-rate excellence, and recommended their publication as matter of scientific importance. This report has been ratified by all astronomers; and, so far as they have yet been published, the Edinburgh observations not only do credit to the astronomer and his assistant, Mr. Wallace, but have conferred on the Observatory a high reputation among the similar institutions of Europe.

Although the periodical publication of the Edinburgh observations rendered it, in a great measure, unnecessary for him to have recourse to other channels of publicity, he continued, from time to time, to communicate to this Society various observations of phænomena, either of an occasional nature or having an immediate interest, and notices likely to be useful to practical astronomers. Among these may be mentioned observations of the planets near their oppositions and inferior conjunctions, of moon-culminating stars, of the annular eclipse of the sun on May 15, 1836; and, more especially, elements of cometary orbits. Indeed, if he had no other claim on our regard, his care in disseminating the earliest information respecting the orbits of newly-discovered comets would alone have entitled him to the applause due to a useful labourer in the cause of astronomy. Various observations and notices of the kind now referred to were also communicated by him to Professor Schumacher, and published in the *Astronomische Nachrichten,* whereby they obtained immediate circulation over the continent.

His attention at Edinburgh was chiefly directed to planetary observations, and the formation of an extensive catalogue of zodiacal stars. He had proceeded far in the latter work; and in one of his last letters, written to an intimate friend, he spoke of being engaged in fixing the positions of some of those stars in Argelander's recent catalogue, which, though visible to the naked eye, had escaped the notice of all former astronomers.

In recounting Mr. Henderson's labours in the Edinburgh Observatory, we should fail to do justice to his zeal if we omitted all allusion to other avocations which occasionally made large demands on his time. His office of professor, though he gave no lectures in his own department, entailed on him various duties. During one whole session (1835-1836) he delivered the mathematical lectures for Professor Wallace, then incapacitated by illness; and in the last year he undertook, for some time, a similar duty for Professor Forbes, in the class of Natural Philosophy. In short, he was one whose general talents and habits of business, coupled with extreme benevolence and great good sense, rendered him a most useful coadjutor; and, accordingly, the largest share of any public business, with which he happened to be connected, was sure to fall into his hands.

Having none of the accessory advantages of birth, fortune, or early introduction, Mr. Henderson had to rely on his own energy and talents alone in conquering his way to fame; and he was only beginning to enjoy his well-earned reputation, both in his own country and abroad. He was admitted a Fellow of this Society in 1832, and of the Royal Society in 1840, and he had been a member of the Royal Society of Edinburgh since 1834. It does not appear that he was a member of any foreign society, but he was in frequent correspondence with Schumacher, Bessel, Encke, and other distinguished astronomers on the continent, by whom his talents and opinion on astronomical subjects were held in high estimation.

In 1836 he married Miss Adie, eldest daughter of the well-known optician and ingenious inventor of the sympiesometer. The death of this lady in 1842, a few weeks after the birth of their only child, produced an effect on his sensitive temperament from which he never completely recovered. In the summer of that year he was gratified by an event which afforded him at the time the liveliest pleasure, and ever after formed a bright spot in his memory. This was the visit to Edinburgh of Professor Bessel, whom he had always been accustomed to regard as his master in science; and for whose character and writings he entertained an unbounded admiration. In company with the great astronomer, and his countryman and colleague, the celebrated mathematician Jacobi, he made a short excursion to the Highlands; and his friends well remember the delight with which he used to recount the incidents of that journey, and relate anecdotes of his illustrious companions.

Although his constitution was never robust, and he was occasionally subject to low spirits, during the influence of which he would express misgivings as to his hold on life, his health did not undergo any visible change till the autumn of 1844, when he was suddenly seized with an illness of so alarming a kind, that, happening at the time to be on a visit to a friend, some days elapsed before he could be removed to his own house. From this attack he partially recovered, and hopes were entertained that he would soon be enabled to resume his usual duties; but a relapse having occurred, he expired suddenly on the 23rd of November, a few weeks before he would have completed his forty-sixth year. The disease was then ascertained to be hypertrophy of the heart; and there can be little doubt that, in the state of health induced by this organic disorder, the fatigue of the nightly observations, and of climbing the steep hill on the summit of which the observatory is built, had been extremely prejudicial to him, and contributed to accelerate its fatal termination.

The character of Mr. Henderson as an astronomer stands high, and his name will go down to posterity as an accurate observer, an industrious computer, a skilful manipulator, and an improver of methods in that department to which he devoted himself. Endowed by nature with perceptive powers of great acuteness, and accustomed by his early professional training to examine and sift the evidence of every fact or statement presented to his mind, and to keep it before him until he had obtained a clear conception of it in all its bearings, every acquisition he made was perfect; and all his knowledge was stored up in a memory of unusually retentive powers, in so orderly a manner, as to be always available at the moment it was wanted. A sharp eye, and habits of order, regularity, and attention, enabled him to become an excellent observer; but the services for which he will be remembered consist not so much in numerous and accurate observations, as in the use which he made of them and the manner in which he worked out their results. At the outset of his career he was led (probably by the commendation of them in our Memoirs) to study attentively the methods of the German astronomers, particularly those of Bessel and Struve, upon whose model he formed his practice, and from which he never departed. All his memoirs and investigations are characterised by the excellences of those illustrious masters. Every observation is scrupulously discussed, and its results drawn out in the most concise and serviceable form. His processes are fully explained; his formulæ of reduction carefully chosen; no labour is evaded; and no circumstance which can affect the accuracy of the final result is passed unnoticed. Nor is the manner in which his results are set forth and communicated inferior to the skill displayed in deducing them. His descriptions of instruments, methods, and details of practice, are stamped by a simplicity, neatness, and precision, which show at once the correctness of his taste and his mastery over his subject. His introductions to the Edinburgh observations may be cited as admirable specimens of astronomical writing. Nothing, indeed, is more remarkable in all his compositions than good taste, and the entire absence of every trace of exaggeration of his own merits or affectation of singularity. Discerning clearly his object, he pursues it in a direct undeviating course; never stepping aside for the sake of display, yet skilfully availing himself of every means of reaching it, which science had placed at his disposal. To represent him as profoundly skilled in the higher departments of physical astronomy would be to make a pretension, from which the modesty of his nature would have recoiled; but he had a sufficiently accurate knowledge of the analytical processes by which the phænomena of the universe are deduced from, or connected with, the theory of gravitation, to appreciate correctly their bearings on the practical branches, to which he directed his attention. To a very considerable knowledge of mathematics he added great powers of calculation—powers which he never abused; for though less liable than most men to make mistakes, he, in no case, allowed himself to dispense with the most scrupulous revision of his work. One of his most distinguishing qualities was sound judgment. He never attempted any thing to which his powers were not fully equal; and, as a consequence of this, whatever he did he did well.

Few men have been more conversant with the modern history of astronomy, especially that branch of it with which he was more immediately connected. It may be said, without any exaggeration, that he knew, and could name the author of every invention or contrivance, whether mathematical or mechanical, bearing on his professional duties.

In his private character, and the relations of domestic life, Mr. Henderson was distinguished by great warmth of affection and amiability of disposition. Naturally modest and retiring he shrunk from the most distant contact with obtrusiveness and ostentation; but he knew well enough his position, and was by no means deficient in spirit to defend it. His disposition was cheerful and social; and when released from official duties he took great pleasure in the society of his friends, to whom he was endeared by the worth of his character, the unaffected simplicity of his manners, his kind-hearted benevolence, and enthusiastic admiration of whatever is noble and excellent. His reputation for astronomical knowledge, and his readiness to assist in its promotion, involved him in an extensive correspondence; but, however occupied he might be, when reference was made to him on any point connected with his professional pursuits, he was always to be depended upon for a prompt answer, either containing (as was most frequently the case) a full exposition of the matter in hand, or a frank acknowledgment that he had no information to give. He was disinterested and generous to a degree hardly consistent with his circumstances. Notwithstanding his slender appointments he had already collected an extensive and valuable astronomical library; but the proceeds of its sale were the principal part of the heritage he left for his orphan daughter.

The loss of so excellent a member, at an age when his knowledge and faculties had only attained to maturity, and when past triumphs justified the brightest anticipations for the future, cannot be regarded by the Society with feelings of ordinary regret. Let us hope there will never be wanting among us examples of similar and equally successful devotion.

METEOROLOGICAL OBSERVATIONS FOR MAY 1845.

Chiswick.—May 1, 2. Very fine. 3. Fine, with clouds. 4. Cloudy and cool. 5. Fine: dense clouds: clear. 6. Cloudy: rain. 7. Cloudy: showers. 8. Rain. 9. Cloudy: clear. 10. Foggy: cloudy: clear at night. 11. Cloudy. 12. Rain: showery. 13. Cloudy and fine. 14. Fine. 15. Overcast: fine. 16. Cloudy. 17. Overcast: slight frost at night. 18. Cloudy: showery. 19. Cloudy and cold. 20. Cloudy: rain. 21. Heavy rain. 22. Cloudy: very clear. 23. Overcast: fine: heavy rain. 24. Hazy clouds: heavy rain at night. 25. Rain. 26. Overcast: heavy rain. 27. Very fine. 28. Hazy and damp. 29. Thick haze: rain. 30. Fine. 31. Very fine: cloudless: overcast at night.—Mean temperature of the month 5°·3 below the average.

Boston.—May 1. Fine. 2. Fine: thunder and lightning P.M., with rain. 3. Cloudy: thunder and lightning P.M., with rain. 4. Fine: rain early A.M. 5. Fine: rain early A.M.: rain P.M. 6. Rain. 7. Cloudy: rain early A.M.: rain P.M. 8. Fine: rain A.M. 9. Cloudy: rain early A.M. 10. Rain. 11. Cloudy. 12. Fine: rain A.M. and P.M. 13. Fine: rain P.M. 14, 15. Fine. 16. Cloudy. 17. Cloudy: rain A.M. 18. Windy. 19. Fine: rain early A.M. 20. Fine: rain P.M. 21. Cloudy. 22. Fine. 23. Cloudy: rain P.M. 24, 25. Cloudy: rain early A.M. 26. Cloudy: rain early A.M.: rain P.M. 27. Fine. 28. Fine: rain early A.M.: rain P.M. 29. Cloudy: rain P.M. 30, 31. Fine.

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

Applegarth Manse, Dumfries-shire.—May 1. Heavy showers. 2, 3. Heavy showers, with hail. 4. Sunshine and showers. 5. Fine. 6. Moist: light drops. 7—9. Slight showers. 10. Fine. 11. Rain P.M.: hoar-frost A.M. 12. Showers. 13. Fair and fine. 14. Rain nearly all day. 15—17. Fair: fine: bracing air. 18. Fair, but threatening. 19. Fair, but threatening: fine. 20, 21. Fair, but threatening: droughty. 22—24. Fair, but threatening. 25. Fair, but threatening: few drops of rain. 26. Fair, but threatening: very droughty. 27—30. Fair, but threatening. 31. Fine: warm.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. 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.	29·905	29·829	29·27	29·42	29·40	29·23	29·20	65	48	60	57½	49	52	48	w.	w.	ssw.	sw.	·01	......	......	......
2.	29·957	29·821	29·22	29·48	29·66	29·34	29·52	64	36	57	55½	46½	49	44	sw.	calm	w.	w.	......	......	......	......
3.	29·989	29·878	29·40	29·60	29·66	29·50	29·61	59	37	50	55½	45	48	42½	w.	calm	w.	w.	·01	·05	......	·21
4.	29·895	29·791	29·34	29·66	29·78	29·76	29·86	60	36	49	56	39	48½	43	nw.	calm	w.	nnw.	......	·22	......	......
5.	29·885	29·822	29·40	29·78	29·80	29·94	29·94	58	27	50·5	55	34	46	40½	nw.	calm	nne.	se.	·03	·03	......	·22
● 6.	29·836	29·681	29·30	29·72	29·77	29·94	30·08	55	37	47	53	36½	47½	42	w.	calm	ne.	ne.	·48	·09	......	......
7.	29·649	29·589	29·24	29·75	29·58	29·98	29·84	50	37	46	49	41½	48½	44	nw.	calm	n.	nne.	·20	·11	......	......
8.	29·457	29·407	29·00	29·45	29·40	29·62	29·62	52	34	50	52	39½	45	43	sw.	calm	ne.sw.	e.	·01	·35	0·31	·10
9.	29·490	29·483	29·08	29·41	29·41	29·69	29·70	59	33	48	55	37	47	43	sw.	calm	e.	e.	......	·04	......	·09
10.	29·597	29·457	29·08	29·45	29·60	29·67	29·73	58	36	49	55	39	47	44	n.	calm	ne.	e.	·04	·09	......	·10
11.	29·849	29·805	29·40	29·69	29·60	29·68	29·50	58	42	50	59	34	50	46	n.	calm	s.	sse.	·08	·01	......	......
12.	29·859	29·737	29·24	29·59	29·80	29·66	29·89	58	42	54	57½	42	49	46	n.	calm	n.	nnw.	·09	......	......	·08
13.	30·171	29·943	29·58	30·00	30·18	30·13	30·26	61	44	53·5	59½	42	49½	45½	ne.	n.	n.	s.	·03	·20	......	......
☽ 14.	30·302	30·247	29·80	30·27	30·20	30·24	30·23	61	36	55	55½	39	49½	48	n.	nw.	w.	wnw.	......	·04	......	......
15.	30·278	30·274	29·72	30·21	30·22	30·24	30·26	63	48	56·5	65	47½	50	48	n.	calm	w.	wnw.	......	......	......	......
16.	30·288	30·234	29·77	30·22	30·13	30·17	30·10	63	45	55	64	48	49½	46½	n.	calm	w.	nw.	......	......	0·30	·06
17.	30·138	30·115	29·53	30·05	30·08	30·16	30·15	57	31	52·5	61	50	48	46	n.	calm	w.	nw.	·06	......	......	·10
18.	30·056	29·851	29·54	29·97	29·82	30·05	29·96	59	42	54·5	64½	41½	48½	46	w.	n.	nw.	n.	·07	·06	......	......
19.	29·814	29·806	29·30	29·80	29·85	29·98	30·03	59	38	51	60	48	47	46	n.	n.	ne.	ne.	......	·04	......	......
20.	29·849	29·841	29·34	29·89	29·80	30·05	30·01	59	41	54	57½	43½	47	46	n.	n.	ene.	nne.	·27	......	......	......
○ 21.	29·764	29·554	29·34	29·79	29·78	30·03	30·00	56	42	48·5	61	44½	50	48	nw.	calm	ne.	ne.	·26	·09	......	......
22.	29·789	29·694	29·29	29·80	29·81	30·00	30·00	59	41	56	59	46	50	45	n.	ne.	ne.	n.	·02	......	......	......
23.	29·834	29·798	29·35	29·85	29·84	30·00	30·00	60	44	51·5	64	44½	47	46½	nw.	n.	ne.	ne.	·16	......	......	·06
24.	29·822	29·803	29·25	29·88	29·83	30·09	30·06	61	45	53	56	44½	49½	46½	w.	calm	ne.	ne.	·54	·46	0·00	......
25.	29·788	29·685	29·36	29·81	29·78	30·02	29·99	57	46	50	55½	44½	48½	46	e.	calm	ne.	ne.	·10	·02	......	......
26.	29·685	29·610	29·15	29·68	29·61	29·98	29·98	59	36	54	52½	43½	50	46½	s.	calm	nne.	ne.	·27	·53	......	......
27.	29·778	29·758	29·33	29·78	29·95	30·11	30·19	67	48	60	54	45½	49	45	ne.	s.	ne.	ne.	·08	·08	......	......
☾ 28.	29·790	29·772	29·40	30·00	29·95	30·23	30·20	67	49	57	57	43½	49	46	ne.	e.	ne.	ne.	·01	·02	......	......
29.	29·692	29·682	29·35	29·89	29·84	30·14	30·14	55	48	50	58	42½	52	46	ne.	e.	ne.	ne.	·07	·80	......	......
30.	30·045	30·751	29·35	29·91	30·00	30·14	30·14	64	39	58	62	45	52	46	n.	ne.	ene.	......	......	·04	0·02	......
31.	30·181	30·166	29·64	30·08	30·00	30·14	30·07	69	43	55	63½	44	49½	45½	sw.	calm	se.	......	......	......	......	......
Mean.	29·885	29·836	29·36	29·802	29·813	29·932	29·943	59·74	40·35	52·7	57·7	2·9	48·80	45·32					2·89	3·37	0·63	1·02

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XV. *Experiments and Observations on some Cases of Lines in the Prismatic Spectrum produced by the passage of Light through Coloured Vapours and Gases, and from certain Coloured Flames. By* W. A. Miller, *M.D., F.R.S., Professor of Chemistry in King's College, London*[*].

[With two coloured Plates.]

THE inquiry into the cause of the dark lines in the solar spectrum is one involved in so much difficulty and obscurity, and the interest which attaches to its solution, in connexion with some of the most refined questions relating to the nature of light, is so great, that I trust I shall be pardoned for bringing forward some additional facts bearing upon the subject, however little I may have to say in explanation of them. These facts may, however, at a future period, assist us in arriving at some general conclusion.

In examining the prismatic spectrum of light that had been transmitted through the deep red vapours of the nitrous acid (NO_4), Sir D. Brewster made the remarkable discovery, that the absorption of the rays by this medium differs from that of coloured liquids and solids in general, in the production of a multitude of lines crossing the spectrum in the same direction as those observed by Fraunhofer; these lines being broadest, darkest, and most abundant towards the more refrangible extremity of the spectrum. He found them to occur equally, whether solar or artificial light were employed. Shortly after, Prof. Miller of Cambridge, and the late Prof. Daniell, of King's College, extended this inquiry to other coloured vapours, viz. to chlorine, iodine, bromine, euchlorine and indigo. Between the spectra of iodine and bromine they discovered a very exact parallelism, the coloured image in each case being

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[*] Read (June 21, 1845) at the Meeting of the British Association held at Cambridge, and communicated by the Author.

crossed through a great portion of its extent by numerous equidistant lines, corresponding both in number and position in the two substances.

This remarkable similarity of action upon light, between bodies whose chemical properties are in many respects so closely allied, induced me, as the subject appeared to have been completely laid aside by the philosophers to whom these observations are due, to examine a variety of other coloured compounds. I was in hopes of discovering, amongst other bodies, and particularly amongst compounds of similar nature and properties, a correspondence, if not in the colorific position, at least in the general arrangement of the lines (if any were produced), which are rendered visible by a prismatic analysis of the light that has been transmitted through the different coloured media. This hope, however, I have not been able to realize to any considerable extent. But though the results obtained are many of them negative, as the cases in which no lines occur are much more numerous than those in which they are produced, I have met with some facts which I believe are not generally known, and which may, therefore, possess sufficient interest to warrant my submitting them to the consideration of men of science. The mere enumeration of the substances in which I have failed to discover them may, besides saving others some fruitless labour, be of some value.

My observations may be referred principally to two heads, those which relate to the effect of coloured gases and vapours upon transmitted light, and those which arise from experiments on the spectra produced by flames of different colours.

Before detailing the mode in which the following experiments were conducted, I will briefly state the principal results which I have obtained from the first series.

1. In no case where *colourless gases* are employed, have I detected any additional lines. Fourteen different substances were employed, varying in density from that of hydrogen, 0·0691, to that of æther, 2·586, and hydriodic acid, 4·388. Of these bodies three were simple, viz. oxygen, hydrogen and nitrogen, and eleven were compounds, varying greatly in their mode of condensation, as in binoxide of nitrogen, where the elements unite without condensation; protoxide of nitrogen, where three volumes of the gases become two; and ammonia, in which two volumes are condensed into one; in chemical properties they differed not less widely. Five of these gases were acid, the carbonic, hydrosulphuric, hydrochloric, hydriodic, and sulphurous acid. One ammonia is alkaline, one cyano-

gen is usually considered an organic radicle, one a compound of such a radicle as æther; the remaining one, chloride of sulphur, possesses properties referable to none of these heads.

2. The mere presence of colour is no indication of the existence of lines. Vapours which the eye cannot distinguish from each other will be found in one case, as with bromine, to give numerous lines, in another, as the chloride of tungsten, to give none at all.

3. The probable position of the lines cannot be inferred from the colour of the gas: with the green perchloride of manganese the lines are most abundant in the green, whilst with the red vapours of nitrous acid they increase in number and density as we advance towards the blue end of the spectrum.

4. Simple bodies, as well as compounds, may produce lines; and two simple bodies which singly do not produce them, may in their compounds occasion them abundantly. Neither oxygen, nitrogen, nor chlorine, when uncombined, occasions lines; but some of the oxides, both of nitrogen and of chlorine, exhibit the phænomena in a most striking manner. There are, however, oxides, both of nitrogen and of chlorine, which do not occasion the appearance of lines.

5. We find also that lines may exist in the vapour of simple substances, as in iodine, which disappear in their compounds; this is exemplified in the hydriodic acid, a combination of equal volumes of gaseous iodine and hydrogen, united without condensation.

6. Sometimes the same lines are produced by different degrees of oxidation of the same substances, a remarkable instance of which is furnished in the oxides of chlorine. Here chemical considerations of interest may assist in explaining the cause.

7. The lines are increased in number and density by increasing the depth of the coloured stratum through which the light is transmitted, or by any cause which increases the intensity of the colour; proving that more lines exist than our instruments or eyes are capable of discerning when the vapour is dilute. This is especially exemplified with iodine and bromine.

8. These lines occur both with polarized and non-polarized light. There is little doubt, although I have not yet made the experiment, that the lines thus produced, as with the ordinary solar lines, indicate an absence of chemical influence, as well as of the luminiferous portion of the ray.

In all the experiments on transmitted light, the luminous source employed was, unless otherwise specified, the diffused light of day. The different spectra represented in Plate II. were obtained by receiving the rays upon a prism of Munich flint glass, mounted upon Fraunhofer's plan and adjusted to an achromatic telescope, placed at 20 feet from a vertical slit in a brass plate, one-fortieth of an inch in width and about 4 inches long. Half of this slit admitted diffused daylight in its ordinary state, the other half of the aperture was covered by the tube or other vessel containing the gas or coloured vapour. The two spectra were thus placed in juxtaposition, and the lines of Fraunhofer could, in most cases, be traced across the vapour-spectrum, furnishing exact points of comparison between the two.

By increasing the depth of the coloured stratum, we increase the strength and number of the lines visible; new and fainter lines being rendered discernible by rendering the colour of the gas more intense. A smaller quantity of some bodies is capable of rendering these lines visible than is required to produce the same effect with others; *e.g.* a quantity of iodine vapour, just sufficient to give a violet tinge to the air in the tube, renders the lines distinctly perceptible, whilst with a tube of bromine, the vapour of which is as full a red as that of nitrous acid, the lines, though visible, are very indistinctly defined. I have, therefore, in experimenting on permanent gases and vapours of liquids volatile at ordinary temperatures, been careful not to decide on the absence of lines unless the light had been transmitted through a body of vapour at least 9 inches deep. It is possible that in one or two instances of substances which require high temperatures for their volatilization, the employment of a stratum of greater depth might have revealed spectral lines, as, with the exception of sulphur, the experiments were, on account of the difficulty of uniformly heating larger vessels, performed in tubes three-fourths of an inch internal diameter. These exceptions, however, if they exist, will I believe be very few, as in no case, by using thicker strata, have I found lines, when in such tubes all indications of their existence were wanting. The vessels I have found convenient in operating on the larger quantities of gases are rectangular boxes of colourless plate-glass, usually cemented with marine glue.

In connexion with the effect of varying the depth of the coloured stratum, I may mention, that if we increase the intensity of the colour, *e.g.* with nitrous acid by the application of heat, the lines throughout the whole spectrum become much darker, and where they are few in number, more sharply defined; but where they are numerous, they increase to such an extent as altogether to arrest the passage of the light, and the space from F to G, which in fig. 4 represents several well-marked groups, becomes one continuous band of shadow. On allowing the tube to cool, the lines in this part resume their former distinctness. Analogous phænomena are observed in the case of iodine; this vapour acts principally on the green portions of the spectrum, and if the vapour be at all dense, the orange and yellow spaces appear crossed by the equidistant lines (Plate II. fig. 2) which characterise this vapour; and they gradually are lost in the green, which seems blotted out by a uniform shadow. As the tube is allowed to cool, and the mass of vapour becomes rarer and its hue fainter and fainter, the lines are seen by degrees extending into the green, and at length the whole shaded portion is filled with them: they do not disappear in this part until those in the orange and yellow are gone, and until the last tinge of violet in the tube has completely passed away. In most instances, where vapours, and not gases, are the subjects of experiment, it will be found advantageous to exhaust the air and seal the tube by the flame of a blowpipe. The vapour of indigo and of several other bodies is thus experimented on without difficulty.

I need not advert to the remarkable but now well-ascertained fact, that in the light of the afternoon and evening, lines become visible which at other times are not readily detected, and that those of the red and orange portions of the spectrum, in particular, become much more distinctly marked; but a singular appearance accidentally presented itself to me the other day. I was examining the spectrum of the diffused daylight towards the evening when a violent thunder-shower came on; lines not before visible were distinctly apparent, and a group in the brightest part of the spectrum between D and E, though nearer to the former line, became very evident, increasing in distinctness with the violence of the shower; as the rain passed away they again faded and disappeared. I have had subsequent opportunities on several occasions of confirming the accuracy of this observation.

In fig. 1, I have represented approximatively some of the principal groups of lines that I have observed in daylight after transmission through different vapours. No pretensions to accuracy are made in these sketches; they are simply intended to convey an idea of the general position and grouping of the lines.

The two first series, those of bromine and iodine, are produced by simple bodies; the remaining ones by compounds containing only two elements, it is true, but the arrangement of the molecules of which, it is all but certain, is far more complicated than such an apparent simplicity might warrant us in expecting.

In fig. 1 some of the principal solar lines are depicted as fixed points of comparison.

Fig. 2 gives the general appearance of the lines presented by iodine; the specific gravity of this vapour is 8·707; the lines in this case are most distinct in the orange and yellow portions, and in the green becoming so numerous as to obliterate it: these lines are not sharply defined, but shadow off into the lighter spaces very gradually*. In the more refrangible part of the spectrum I did not discern them.

Fig. 3. Bromine shows lines corresponding to those of iodine; they are less distinct, however, and can scarcely be discerned in the orange portions; the violet end of the spectrum is entirely intercepted. Specific gravity 5·390.

Fig. 4. Some of the principal lines in the *peroxide of nitrogen*, NO_4, specific gravity 3·183, the gas is of a deep orange: in this gas one vol. of nitrogen and two of oxygen are condensed into one.

Fig. 5 gives some of the groups exhibited by the *peroxide of chlorine*, Cl_2O_8; these are principally confined to the blue and indigo portions, the colour of the gas being a bright orange. No correspondence between the position of the principal groups in these two gases is observable.

It appeared to me of great interest now to ascertain what influence the successive steps of oxidation of the same body might have upon the spectrum; and two classes of substances presented themselves as well-adapted to this inquiry, viz. the oxides of chlorine and of nitrogen.

Thanks to the researches of many eminent chemists, and especially of Millon and Pelouze in the last few years, we are now in possession of means for preparing and insulating these compounds of chlorine in a state of purity. Finding the lines (fig. 5) in the gas extricated from chlorate of potash on the addition of sulphuric acid, I next prepared some pure chlorous acid from chlorite of lead (PbO, ClO_3); this salt, on the addition of nitric acid, furnished chlorous acid (ClO_3) in a state of purity. In this gas 2 vols. of chlorine and 3 vols. of oxygen are condensed into 3 vols., and it has a specific gravity of 2·646; and on proceeding with prismatic analysis of the transmitted light, the very same series of lines was pro-

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* I am informed by Prof. Miller, that with instruments of high magnifying power and accuracy of definition, these bands are seen distinctly to be composed of very fine lines, increasing in number and strength towards the central or darkest portion of the band.

duced. Chlorous acid, in very small quantity, is capable of tinging a very large bulk of water of a bright yellow colour. As the gas had so marked an effect on the spectrum, I was induced to try this solution, but failed entirely in discovering lines; the liquid arrested the whole of the more refrangible rays entirely, no light passing beyond the midspace between E and F.

I now prepared some pure *peroxide of chlorine* ($Cl_2 O_8$), freeing it from chlorine and chlorous acid, by which it is always accompanied, by passing the gas through tubes surrounded with a mixture of ice and salt; the peroxide alone condenses at this temperature, and the deep red liquid thus obtained was allowed to fall into a tube and evaporate; the same series of lines were produced. In this gas, which has a specific gravity of 2·325, 1 vol. of chlorine and 2 vols. of oxygen are condensed into the space of 2 vols.

Euchlorine was next tried; the gas was disengaged from a mixture of hydrochloric acid and chlorate of potash, condensing the compound as before by a freezing mixture: to this compound Millon assigns the curious formula of $Cl_3 O_{13}$; here too I found the same series of lines. Millon's theory of the composition of these bodies is, that the peroxide of chlorine is a compound of *chloric and chlorous acids* in single equivalents, as it is immediately resolved into these two acids by the action of alkalies; and he considers the euchlorine, or chloro-chloric acid, as he proposes to call it, a compound of 1 eq. $Cl O_3$ with 2 ClO_5, the action of acids, immediately resolving it into those acids in the proportions just mentioned. Certainly the occurrence of similar lines in all three, although the condensation varies, does not militate against this view, but increases its probability.

Millon's new compound, chloro-perchloric acid, which results from the action of solar light on chlorous acid, I have not hitherto obtained in a state of sufficient purity to pronounce upon.

It is remarkable that the *hypochlorous acid* ($Cl_2 O_2$), though of a bright greenish-yellow, furnishes no such lines; the density of this gas is 5·881, 2 vols. of chlorine and 1 of oxygen being condensed into 1. It is an interesting fact, that the colourless *protoxide of nitrogen*, in which 2 vols. of nitrogen and 1 vol. of oxygen are condensed into 2, having a specific gravity of 1·524, has also no sensible effect upon the spectrum.

In the series of oxides of nitrogen, the *binoxide*, NO_2, in which 1 vol. of nitrogen is united with 1 vol. of oxygen without condensation, also a colourless gas, has no effect on the spectrum. No oxide corresponding to this in the chlorine series is as yet known.

Hyponitrous acid I have not isolated in sufficient purity to furnish trustworthy results.

Fig. 6 represents the grouping of these lines as seen in daylight transmitted through the vapours of perchloride of manganese, which is readily prepared by adding a few crystals of permanganate of potash to oil of vitriol in a test-tube, and dropping in a fragment of fused chloride of sodium, a green vapour ($Mn_2 Cl_7$) is immediately liberated, which is however quickly decomposed by the moisture of the air. It will be remarked, that in this vapour, the colour of which is green, the lines are most abundant in the green part of the spectrum, the green tint of the gas being due principally to the mixture of the blue and yellow rays.

The *perfluoride of manganese*, which corresponds in composition to the perchloride, does not yield any lines that I could perceive, but the moment a fragment of common salt is dropped into the mixture from which the fluoride is being disengaged, the lines of the perchloride make their appearance. Both the perchloride and perfluoride are decomposed by water, and on dropping in a little of this fluid into the tube of perchloride, all the lines instantaneously disappear. The purple vapour of the permanganic acid, in which the number of equivalents of oxygen corresponds to that of chlorine and fluorine in these compounds, is equally inactive with the fluoride.

I have examined very many other substances which yield vapours of different colours, but have not succeeded in finding any which give distinct lines besides those above mentioned, with the telescope which I employed. It was not, however, an instrument of very high power.

Simple bodies:—

Chlorine.—Greenish-yellow colour; specific gravity 2·47. This is the more remarkable, as bromine and iodine both give them.

Sulphur.—Yellow; specific gravity 6·654.

Selenium.—Yellow.

Compounds:—

Selenious acid.—Yellow ($Se O_2$).

Hypochlorous acid.—Yellow; specific gravity 2·993 (Cl_2O_2).

Oxychloride of chromium.—($Cr\ Cl\ O_2$); red vapours*.

Oxychloride of tungsten.—($W ClO_2$); red vapours.

Terchloride of tungsten.—($W Cl_3$); red.

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* Prof. Miller informs me, that by means of an instrument of superior quality, he has succeeded in satisfying himself that lines are produced by this vapour.

Sesquichloride of iron.—($Fe_2 Cl_3$); reddish-brown.

Perfluoride of manganese ($Mn_2 Fl_7$).—Greenish-yellow. This is remarkable, as the perchloride gives a very distinct series.

Permanganic acid ($Mn_2 O_7$).—Purple. This is beautifully shown by allowing a drop of water to fall into a tube full of the perchloride of manganese, when all the lines immediately disappear.

Indigo.—Splendid crimson vapour ($C_{16} H_5 NO_2$).

Alizarine from madder.—Orange vapour ($C_{37} H_{12} O_{10}$). Both these substances require careful management in the sublimation, as they are readily charred, and a small portion is always decomposed, whatever the amount of care employed.

In making experiments on coloured flames, I generally used an alcoholic solution of the compound on which I was experimenting. A common cotton wick supported in a small glass tube furnished the lamp whose flame I wished to examine. This little lamp was placed opposite the fissure, in a tin box, the side of which next the slit was permanently open, whilst the opposite side was furnished with a door opening outwards and upwards, so that by a string it could be raised to admit the light of day, and would afterwards close by its own weight when allowed to fall back. Fraunhofer's lines (Plate III. fig. 7) thus again served as points of comparison; the comparison, though not rigidly accurate, being still very nearly so, and perfectly sufficient for my purpose.

Fig. 8 in Plate III. exhibits the remarkable spectrum given by a solution of *chloride of copper*. Several intervals of absolute darkness here occur, interrupted by bright lines of great intensity, particularly in the green and blue spaces. The general hue of the diffused light is a bluish-green.

Fig. 9 represents the spectrum from the green light of *boracic acid*, in which, besides the bright streak at D, five well-marked bright bands occur in the yellow and green, and a narrow line in the indigo.

Fig. 10 is the spectrum from *nitrate of strontia*, the red and orange portions of which are particularly developed, and are crossed by three very strong black lines; a bright line will be remarked in the blue and another in the indigo.

Fig. 11 is that from an alcoholic solution of *chloride of calcium*; besides the lines in the orange, there is a very brilliant yellow streak at D, and two bright bands in the yellow: in addition to these is a bright streak in the indigo.

Fig. 12 represents the spectrum of a flame coloured by chloride of barium. It is in its features intermediate between that of strontia and lime; the most remarkable character is in a bright streak in the orange, another in the yellow, and one in the indigo.

Even *chloride of sodium*, though the intensity of the light is accumulated in the bright band near D, shows a marked tendency to the occurrence of bands in other parts; a distinct, though faint light extending far into the indigo, when a bright streak terminates it almost suddenly.

Chloride of manganese gives indications of the occurrence of bands, though but feebly. I have also tried solutions of the chlorides of iron, zinc, cobalt, nickel, mercury and magnesia: in all a feeble band occurs in the green portion, as represented in the three last spectra; this is probably connected with the disengagement of chlorine from the salts during combustion; and the bichloride of mercury gives a bright streak near G in the indigo.

The combustion of phosphorus in the open air gives a pure spectrum without lines. I tried it also in chlorine, but did not obtain any very clear spectrum, as the deposition of chloride on the jar quickly rendered it opake.

Another source of light was furnished by the ignition of different substances in the oxyhydrogen jet: the light thrown directly upon the prism was too intense for the eye to bear, I therefore received it on a sheet of white paper, and employed the diffused light so obtained. In the spectrum from charcoal nearly midway between D and E, a very short brilliant streak was visible, and the same streak appeared when the flame was thrown upon a fragment of dried *alum*.

Lime and strontia gave results similar to those furnished by the solution of their salts when used to tinge the flame of alcohol, but not quite so well marked. Baryta melted too rapidly to furnish any satisfactory result.

Zinc, *iron*, *steel* and *platinum* gave brilliant spectra, but no lines except that at D were visible. *Copper*, *lead* and *antimony* melted without giving light sufficient to make any satisfactory observation.

It may be interesting to remark, in connexion with the speculations on the absorptive action of the sun's atmosphere, that if solar light be transmitted through a flame exhibiting well-marked black lines, these lines reappear in the compound spectrum, provided the light of day be not too intense compared with that of the coloured flame: this may be seen in the red light of the nitrate of strontia, and less perfectly in the green of the chloride of copper. It would therefore appear that luminous atmospheres exist in which not only certain rays are wanting, but which exercise a positive absorptive influence upon other lights.

It was my intention to have instituted a comparative series of experiments with the light obtained by ignition of the metals by the voltaic battery, but this point has already, I find, engaged the attention of my friend and colleague Professor Wheatstone; and he has made it the subject of an investigation, characterized by his usual ingenuity and accuracy.

XVI. *On Imaginary Zeros, and the Theory of Conjugate Points.* By J. R. YOUNG, *Professor of Mathematics in Belfast College**.

IN a paper read at the last meeting of the British Association, the writer of these remarks had occasion to notice the errors frequently committed by neglecting the algebraic sign of zero in the extreme or limiting cases of certain infinite series; and to show that this neglect had led Abel and other distinguished analysts to conclusions incompatible with the general laws of algebra.

For example: Abel says, in reference to the series

$$\frac{1}{2}\varphi = \sin\varphi - \frac{1}{2}\sin 2\varphi + \frac{1}{3}\sin 3\varphi - \ldots,$$

that "lorsque $\varphi = \pi$ ou $-\pi$, la serie se réduit à zéro, comme on voit aisément. Il suit de là, que la fonction

$$\sin\varphi - \frac{1}{2}\sin 2\varphi + \frac{1}{3}\sin 3\varphi - \ldots,$$

a la propriété remarquable, pour les valeurs $\varphi = \pi$, et $\varphi = -\pi$ d'être discontinue." (*Œuvres*, tom. i. p. 90.)

But this conclusion is erroneous; for it is overlooked that when φ becomes π, $\sin\varphi$ vanishes positively, $\sin 2\varphi$ negatively, $\sin 3\varphi$ positively, and so on; and that when φ becomes $-\pi$, the signs of the several vanishing terms are the opposites of these; so that, the continuity being still preserved, we have, in these extreme cases,

$$\frac{1}{2}\pi = \left(1 + \frac{1}{2} + \frac{1}{3} + \ldots\right) \times 0$$

and

$$-\frac{1}{2}\pi = \left(1 + \frac{1}{2} + \frac{1}{3} + \ldots\right) \times -0;$$

from either of which we learn that

$$\frac{\frac{1}{2}\pi}{0} = \infty = 1 + \frac{1}{2} + \frac{1}{3} + \ldots,$$

a result that we know to be true from other principles.

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* Read at the Meeting of the British Association at Cambridge, June 1845, and communicated by the author.

As it is thus important to inquire whether the sign of an evanescent quantity be plus or minus, may it not be of equal importance to ascertain, in certain investigations of analysis, whether that sign be real or imaginary? That attention to this is of consequence in the application of algebra to geometry will appear by taking the extreme case of the ellipse, when by the continual shortening of the minor diameter, the curve ultimately degenerates into a finite straight line—the major diameter: the equation to this ultimate form of the ellipse is

$$y = \frac{0}{a}\sqrt{a^2 - x^2}.$$

Between the limits $x = -a$ and $x = +a$, the evanescent quantity on the right is *real*, implying that every point on the axis of x, which does not lie without these limits, is a real point of the locus. But if x exceed a, in either the positive or the negative direction, the evanescent quantity is *imaginary*; and consequently, in accordance with the ordinary interpretation, the corresponding points on the axis of x are beyond the bounds of the locus.

If the curve had been an hyperbola instead of an ellipse, the equation, in the extreme case, would have represented two infinite straight lines *in directum* with the axis of abscissas, but separated from each other by an interval equal to the fixed principal diameter.

But it is in the theory of conjugate points that the imaginary zero is in a more especial manner an item of importance; and it is from the disregard that has hitherto been paid to it, that the discrepancies and contradictions, to be found in the most recent expositions of that theory, chiefly arise.

That such contradictions really exist may be easily shown: thus Mr. Walton, in a very ingenious and instructive paper on this subject in the Cambridge Mathematical Journal, vol. ii. p. 164, says, in reference to the doctrine almost universally taught, "Several writers on the calculus have erroneously supposed $\frac{dy}{dx}$, at a conjugate point, is necessarily imaginary." And the late distinguished Mr. Gregory, in his valuable Examples, states that "it may happen that $\frac{dy}{dx}$ and any number of the differential coefficients are possible at a conjugate point" (p. 162). In the preceding page we find it affirmed that, "If $\frac{dy}{dx}$ is found to have two or more equal and possible values, there are two or more branches of the curve touching each other in one point, which is called a point of *osculation*."

And yet at page 170 it is said, "The curve whose equation is

$$(c^2y-x^3)^2 = (x-b)^5(x-a)^6, \quad a<b$$

has a conjugate point whose co-ordinates are $x=a$, $y=\frac{a^3}{c^2}$, but the differential coefficients are *possible* till we come to the third." Now these differential coefficients, thus affirmed to be possible, give each a pair of *equal* values; so that the point under consideration ought to be pronounced, not a conjugate point, but a point of osculation*.

Again, the most prevalent doctrine respecting conjugate points is, that every such point may be correctly regarded as an evanescent oval; and simultaneously with this view it is also maintained that the existence of the point is indicated by $\frac{dy}{dx}$ becoming imaginary (*Lacroix*, English translation, p. 99). But in Mr. Walton's paper, before referred to, it is affirmed that, according to the theory of the evanescent oval, "$\frac{dy}{dx}$ must remain perfectly indeterminate;" while Prof. De Morgan declares (Differential and Integral Calculus, p. 382) that "it is useless to attempt a test of a conjugate point by the differential calculus."

It is clear, from the above contradictory views, that the theory of conjugate points is still in an unsettled state; but before attempting to reconcile these contradictions—for after all they are more apparent than real—it may be well to offer a remark or two on the principle of continuity in reference to inquiries of this kind.

It appears to me that this important principle universally prevails in all the general expressions of analysis; and that it is incorrect to affirm that it suffers any breach or interruption in any instance whatever. No doubt a continuous series of real values, furnished by a general analytical expression, may terminate, and a new series—a series of imaginary values—be originated, both series conforming equally to the law implied in the general form; but no instance can be adduced in which a continuous series of imaginary values becomes interrupted by the interposition of a single real value, and is then again resumed; although such a remarkable interruption is frequently said to happen at a conjugate point, and analogous circumstances are often said to occur in other analytical inquiries, as for instance by Abel, at the place already quoted, and again

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* In reality the osculation will be that of two imaginary, not of two real branches of the curve, since the equal values alluded to are imaginary, as will be seen hereafter.

in the foot note at p. 71 of his first volume, as also at p. 268 of the second volume.

It is a valuable peculiarity of algebra, that it supplies, by indications of its own, cautionary information which, if attended to, would prevent hasty interpretations of this kind. The symbol $\frac{0}{0}$, as is well known, is often such an indication; and, as Lagrange has observed, is the form which analysis assumes to preclude a violation of its own laws.

The hitherto neglected symbol $0\sqrt{-1}$, is a symbol of like character; its office being to apprise us that whenever we arrive at it, by passing over a continuous series of values for the quantity that has thus become zero, the corresponding continuous series of values of the function itself is an imaginary series, with which imaginary series the value arrived at necessarily unites, and must thus be itself considered as imaginary. It is possible that if this imaginary zero be arrived at through a descending series of values, it may appear as a real zero when reached through an ascending series, and *vice versâ*. But this implies no contradiction, nor would it lead to the conclusion that the same point would, in such a case, be both real and imaginary; the proper inference would be that the imaginary point is superposed upon the real point, or that the imaginary branch of the curve passes through a real point of the locus. If, however, the zero continue to be imaginary, in whichever way we arrive at it, then the values of the function continue to be imaginary after the passage through this zero, as well as before. *At* the passage a real point no doubt exists —not however to interrupt the continuous series of imaginary values adverted to—it exists independently of that series, and free from all connexion with it; inasmuch as the co-ordinates of the point satisfy the equation though the symbol $0\sqrt{-1}$, implying such connexion, be suppressed; the co-ordinates of this *conjugate point* will therefore be determined by finding the co-ordinates of the *imaginary point* situated upon it, and then suppressing the symbol $0\sqrt{-1}$.

In like manner there is no interruption of continuity in those curves that have what have been called *punctuated branches*; the continuous branches, whether real or imaginary, exist independently of the assemblage of real points constituting the punctuated branch; and, as among these no continuity ever existed, it would be wrong to speak of any *interruption* of continuity in reference to them.

From the foregoing considerations it is easy to infer plain and unequivocal rules for the determination of conjugate points, keeping in mind the additional principle, that when y becomes imaginary, for a particular value of x, the coefficients $\frac{dy}{dx}$, $\frac{d^2y}{dx^2}$, $\frac{d^3y}{dx^3}$, &c. must also be imaginary for the same value of x, inasmuch as differentiation does not interfere with the radicals which affect x in the original function y. We thus obtain the following criteria:—

1. Let the equation be solved for one of the variables; then if any pair of values for x and y which satisfy the resulting explicit equation, differ from real values only by the symbol $0\sqrt{-1}$, in whichever direction we arrive at the zero, those real values will be the co-ordinates of a conjugate point.

2. If it be inconvenient to solve the equation for one of the variables, we may take a differential coefficient of any order whatever; then, if any pair of values for x and y render this coefficient imaginary, and at the same time satisfy the rational equation of the curve, those values will be the co-ordinates of a conjugate point; it being observed, as before, that if the imaginary value differ from a real value only by $0\sqrt{-1}$, this zero must remain imaginary in whichever direction we arrive at it. In the contrary case, a limit or a cusp will be implied, and not a detached point.

Let us apply these principles to the example already quoted from Mr. Gregory, viz.

$$(c^2y-x^3)^2=(x-b)^5(x-a)^6,\ a<b;$$

solving this for y, we have

$$c^2y=(x-b)^{\frac{5}{2}}(x-a)^3+x^3,$$

which, for $x=a$, gives $y=\frac{a^3}{c^2}\pm 0\sqrt{-m^5}$ (m being put for $b-a$); and as the zero remains imaginary, whether $x=a$ be arrived at from a succeeding or a preceding value of x, we conclude at once that the co-ordinates $x=a$, $y=\frac{a^3}{c^2}$, belong to a conjugate point.

If differentiation be employed, we find the imaginary to be zero, both in $\frac{dy}{dx}$ and in $\frac{d^2y}{dx^2}$, but to be finite in $\frac{d^3y}{dx^3}$. We may therefore conclude that in the imaginary curve through the conjugate point, y, $\frac{dy}{dx}$ and $\frac{d^2y}{dx^2}$ each give *equal* imaginary values, while the imaginary values of $\frac{d^3y}{dx^3}$ are unequal at that point: the imaginary curve, according to the inference of Mr. Gregory, "has contact of the second order with the plane of the axes*."

I have employed the term *imaginary* in the foregoing observations in accordance with the usual phraseology; but the views of Mr. Warren and Dr. Peacock, in reference to the geometrical interpretation of the roots of unity—and which views Mr. Gregory and Mr. Walton have done so much to illustrate and confirm—will necessarily lead to the abandonment of this expression in the application of analysis to curves†.

From what has now been shown, it appears that it is *not* "useless to attempt a test of a conjugate point by the differential calculus:" since this point has the same situation as another which unites with the continuous series of points forming an imaginary curve, or a curve out of the plane of the coordinates, and to any continuous series of values the calculus is, of course, always applicable. But we may connect the isolated point with a continuous series of *real* values, by regarding it as the extreme case of a series of diminishing ovals. By confining our attention to this latter continuous series, the values of $\frac{dy}{dx}$ at the point become innumerable, and all real; by confining our attention to the former continuous series, the values of $\frac{dy}{dx}$ become imaginary; but if we consider the point in its detached state, unconnected with either series, then, of course, to attempt to apply the calculus would be absurd.

The equation

$$(x-a)^2+(y-b)^2=0$$

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* In strictness it has contact of the second order with a certain curve in this plane; the curve, namely, of which every point, indifferently, is represented by the co-ordinates of the conjugate point, $x=a, y=\frac{a^3}{c^2}$, when a is supposed to vary; in other words, the curve whose equation is $y=\frac{x^3}{c^2}$; for $y, \frac{dy}{dx}$ and $\frac{d^2y}{dx^2}$, when a is put for x, will, with the exception of the imaginary zero, be the same in both curves. When the imaginary curve is viewed as a real curve out of the plane of co-ordinates, as Mr. Gregory views it, I must confess that I do not see the grounds of the inference, quoted above, as to the order of contact with this plane.

† The writer of these remarks is anxious here to express his conversion to these views of Dr. Peacock; and he wishes to do so thus publicly, because upon a former occasion (Mathematical Dissertations, p. 30) he ventured to state his misgivings as to their accuracy, although he took care to do so with that becoming hesitation and respect which he felt to be due towards so high an authority.

furnishes a good illustration of these remarks. By solving it for one of the variables, we have

$$y = b \pm \sqrt{-(x-a)^2} = b \pm (x-a)\sqrt{-1},$$

which for $x = a$, however this value of x be reached, becomes $y = b \pm 0\sqrt{-1}$; therefore $x = a$, $y = b$ are the co-ordinates of a conjugate point.

Again, by differentiating the equation, we have

$$\frac{dy}{dx} = -\frac{x-a}{y-b} = \frac{-(x-a)}{(x-a)\sqrt{-1}} = \sqrt{-1}:$$

this, for $x = a$, as indeed for every value of x, is imaginary; and as this value of x, conjointly with $y = b$, satisfies the rational equation proposed, a conjugate point is thus indicated.

Lastly, by regarding the equation as the extreme case of

$$(x-a)^2 + (y-b)^2 = r^2,$$

we have

$$\frac{dy}{dx} = -\frac{x-a}{y-b} = \frac{-(x-a)}{\sqrt{r^2-(x-a)^2}}.$$

For $x = a$, simply, this no longer becomes a vanishing fraction; the numerator only becomes zero: in order that the denominator may vanish also, we must introduce the additional hypothesis $r = 0$; thus numerator and denominator vanish in consequence of two distinct hypotheses, so that in this case the fraction is necessarily indeterminate; hence when the circle degenerates into a point, the values of $\frac{dy}{dx}$ are unlimited; and conversely, when these values are unlimited, the circle degenerates into a point—the same point that was indicated by the preceding methods.

Belfast, June 9, 1845.

XVII. *On Styrole, and some of the Products of its decomposition.*

By Dr. John Blyth *and* Dr. Aug. Wilhelm Hofmann*.

THE class of bodies comprehended under the general name of Balsams has not been forgotten in that active investigation of organic bodies to which chemists have with predilection devoted themselves, since the discovery of easier and securer methods of investigation have removed the chief difficulties. An examination of these substances was indeed highly desirable, as the knowledge of them derived from older works no longer corresponded to the present state of the science. The earlier investigations of these substances were purely

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* Communicated by the Chemical Society; having been read April 7, 1845.

qualitative; everything crystalline and which united with bases was benzoic acid; those, on the other hand, which did not enter into combination were described as camphor. If, on distillation, a volatile fluid body was obtained, it was considered sufficient to state that the substance contained also a volatile oil.

Since the discovery of the potash apparatus has introduced an entirely new mode of investigation, our knowledge on these points has considerably augmented. The labours of Fremy, Plantamour, Simon and Deville, are rich in interesting results.

Along with the proper balsams of Peru, Tolu, &c., there is also known in commerce a material whose extraction is .130 uncertain, called liquid storax (*Styrax liquidus*), which, from its properties, is closely allied to this class of substances. Fluid storax has already been the subject of several investigations. The earliest known to us is that of Bouillon-Lagrange[*]. It contains no results at all deserving of notice. He considered the crystalline acid present in storax as benzoic acid. More lately, Bonastre[†] communicated observations on fluid storax. He investigated a crystalline deposit which had been formed in an alcoholic solution of the balsam long kept. These crystals, which Bonastre took at first for benzoic acid, were insoluble in cold and hot water, but soluble in alcohol; the solution had no acid reaction. To this indifferent crystallizable body he gave the name styracine. He found the same substance, along with many others, again in an investigation of the American Copal[‡], a balsam which in many points resembles storax.

The most complete investigation of fluid storax we owe more lately to Edward Simon[§]. This chemist first showed that the acid found in the balsam, and which had been hitherto taken for benzoic acid, possessed all the properties of cinnamic acid. An analysis of the silver salt made by Marchand[||] put this beyond a doubt. Simon gave also some fuller details on the substance named styracine by Bonastre, and finally he examined the oil obtained from fluid storax by distillation with water, and which in its properties was quite different from that prepared by Bonastre from the Copal balsam. To this oil from storax he gave the name styrole.

The investigation of this chemist is rich in interesting observations; on the other hand it contains in proportion but

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[*] *Ann. de Ch. et de Phys.*, 1st ser., t. xxvi.

[†] *Journ. de Pharm.*, vol. xiii. p. 149. 1827.

[‡] *Journ. de Pharm.*, vol. xvii. p. 338. 1831; and *Mag. für Pharm.*, von Geiger und Liebig, vol. xxxvi. p. 90.

[§] Liebig's *Annal.*, vol. xxxi. p. 265.

[||] *Journ. für Prakt. Chem.*, vol. xvi. p. 60.

few analytical determinations. From the analyses of Marchand, Simon gives the per-centage composition of styrole and styracine, without however at all attempting to assign rational formulæ. The determination of such formulæ was however of much interest, as only by these could the relations of the compounds to other groups be shown. To fill up this blank, we undertook, at the request of Prof. Liebig, to make some inquiries in this direction: these were performed in the laboratory of Giessen, and, we need scarcely add, that during their course we were assisted by his advice.

We commence with the description of the oil of storax.

Styrole.

1. *Preparation.*—The material which we employed for the preparation of styrole was the common commercial "liquid storax." It possesses a dark gray colour, and at the temperature of summer is of a soft consistency, which allows it to be drawn into threads. At a low temperature (0° C.) it is hard and brittle. It has a highly aromatic odour, and is so rich in oil, that it can be pressed out with the fingers. Fluid storax oil can be obtained by distilling the balsam with water; but, as the latter contains also free cinnamic acid, it is more advantageous to add an alkali. For this purpose we followed Simon in employing carbonate of soda, in his proportions of 14 lbs. of crystallized soda to 20 of storax. We found that the half was quite sufficient.

The distillation was conducted in a copper retort with a gentle heat; the watery vapour loaded with oil was directed through a serpentine tube. In this way a milky-coloured water distilled over, on the surface of which the oil collected in a transparent, slightly yellow layer. We have submitted to distillation in the above method about 70 lbs. of storax. The material we employed was not all received at the same time, and gave very different amounts of oil. In the first operation we obtained 12 oz. of styrole out of 41 lbs. of storax. A later preparation, in which 27 lbs. of the balsam were employed, gave scarcely more than 3 oz. This difference arises, as Simon has already remarked, from the age of the balsam, as the styrole experiences in time a peculiar change. To this point we shall return later.

The volatile storax oil, as it is obtained by distillation, contains a small quantity of water, from which it can be easily freed by remaining over fused chloride of calcium. The fluid deprived in this way of water can be regarded as pure styrole, although it still possesses a shade of yellow. To obtain it quite colourless, it must again be submitted to distillation.

In this operation a peculiar phænomenon is observed, which was first indicated by Simon. When styrole is heated in a tubulated retort in which a thermometer is placed, a quantity of vapour is evolved at 100° to 120° C. At 145¾° the fluid begins to boil, and there distils over a clear, oily and beautifully iridescent fluid. For some time the thermometer remains stationary at the above temperature, then begins suddenly to rise so fast, that it must quickly be removed from the retort. The portion left behind has now completely changed its character to a thick, tenacious substance, through which the bubbles formed at the bottom of the vessel force their way with much difficulty to the surface. From this period scarcely any more oil distils over, and, on allowing the retort to cool, its contents become a solid transparent glass. The quantity of this solid residue is variable: it amounts sometimes to a third of the oil employed. We shall return to this point afterwards. The product of the distillation is perfectly pure styrole.

2. *Properties of Styrole.*—This body is in the form of a colourless, transparent fluid, of a burning taste and peculiar aromatic odour, resembling a mixture of benzole and naphthaline, which adheres tenaciously to linen. At a temperature of 20° C. it neither solidifies nor loses any of its extreme mobility. Exposed however to a mixture of solid carbonic acid and æther, it solidifies instantaneously into a beautiful white crystalline mass. It is in the highest degree volatile, and evaporates at all temperatures; the oily spots made by it on paper disappear after a few seconds. The boiling-point lies, as already stated, about 145¾° C. This same temperature was found in several trials. A wick dipped in styrole burns with a brilliant, smoky flame. Its vapour can be passed without decomposition through a red-hot tube.

Styrole is lighter than water: its specific gravity at the ordinary temperature of summer is 0·924. It disperses and refracts the rays of light in the highest degree. Its coefficient of refraction is for the red rays 1·532.

It mixes in all proportions with æther and absolute alcohol. Spirit of wine dissolves less according to the proportion of water it contains. It is soluble in pyroxylic spirit, acetone, bisulphuret of carbon, fat and æthereal oils. Water dissolves only a small proportion, but still sufficient to give it the burning taste and odour of the oil. The amount of water which can become mixed with the oil is equally small.

Styrole shows no reaction on vegetable colours. Like the æthereal oils, it dissolves by the aid of heat a large proportion of sulphur, which, on cooling, is deposited in the form of large prisms. Phosphorus is likewise soluble in hot styrole, from which it also crystallizes. Caoutchouc swells up by the aid of heat in this substance; only a small portion however is dissolved.

3. *Composition of Styrole.*—We have already stated that on the occasion of Simon's investigation, Marchand analysed styrole. From his experiment it appears that this body consists of carbon and hydrogen, in the proportion of two of the former to one of the latter*. We have also analysed styrole, and our results confirm those of Marchand.

On combustion with oxide of copper, styrole prepared at two different periods gave the following numbers:—

I. 0·2722 grm. of styrole gave 0·9168 grm. of carbonic acid and 0·1935 grm. of water.

II. 0·4640 grm. of styrole gave 1·5745 grm. of carbonic acid and 0·3290 grm. of water.

Corresponding in the 100 parts to—

	I.	II.
Carbon . . .	91·86	92·54
Hydrogen . .	7·89	7·87

Theory requires—

			Mean of experiments.
2 equivalents Carbon†	= 150·0	92·30	92·20
1 ... Hydrogen	= 12·5	7·70	7·88
	162·5	100·00	100·08

The above experiments leave no doubt as to the relative amount of the carbon and hydrogen, but give no idea of the absolute number of equivalents of the elements in the equivalent of styrole; or, in other words, lead to no conclusion as to the formula of the compound. This point it was however very desirable to ascertain, as we already possess two bodies, benzole and cinnamole, which have the same composition in the hundred parts.

The determination of a chemical formula for styrole was attended with some difficulty. Like benzole and cinnamole, this body formed no direct compounds, if we do not consider here the products obtained by the action of chlorine and bromine; in the case of the former substances, however, their

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* The volatile oil which Bonastre (*Journ. de Pharm.*, vol. xviii. p. 344) obtained from the Copal balsam is certainly a totally different carbo-hydrogen from styrole. Henry and J. Plisson (same Journal, p. 451) found its composition to be—
Carbon = 89·25
Hydrogen = 10·46
Oxygen = 00·29
100·
This oil is also distinguished from styrole by becoming solid at 0° C.

† Carbon = 75. Hydrogen = 12·5.

formulæ were in a certain measure given by their origin from benzoic and cinnamic acids, and could be controlled by a determination of the specific gravity of their vapours. The origin of styrole, on the other hand, is quite obscure, and the determination of the specific gravity of its vapour was no longer to be thought of after we had observed the peculiar metamorphosis produced by the action of heat.

There remained therefore no other way to arrive at a proper formula than by studying the products of its decomposition. As we had already some information from Simon on this point, it appeared desirable to follow the same way. In the course of this memoir it will be seen that we were led to adopt $C_{16}\ H_8$ as the formula of styrole.

4. *Products of the Decomposition of Styrole. Action of Nitric Acid.*—In its physical properties styrole is closely allied to benzole and to benzoëne (toluine of Berzelius), lately discovered by Deville* as a product of the dry distillation of Tolu balsam. The interesting results obtained by Mitscherlich, and later by Deville, by the action of nitric acid on the above-named bodies, and particularly the remarkable transformation of nitrobenzide into aniline, observed a short time ago by Zinin†, indicated to us a method which promised to lead to a proper formula of styrole. Were a compound analogous to nitrobenzide to be obtained, or a base to be formed whose atomic weight could be easily determined, such data would at once lead to a knowledge of the proper constitution of styrole.

In the memoir already referred to, Simon has communicated some experiments on the action of nitric acid on styrole. He obtained a peculiar neutral body, which he named *nitrostyrole*, without however ascertaining its composition. Along with this body he observed the formation of benzoic and hydrocyanic acids.

Nitrostyrole.

The preparation of this substance is attended with difficulty. Notwithstanding a great number of experiments, we have not been able to find out a method to procure at will a considerable quantity of this body. According to Simon, storax oil must be distilled with nitric acid (but of what concentration is not stated) till it has become brown, and is completely converted into a resinous mass. After freeing it from nitric acid by washing, this resin is to be distilled with water, when the nitrostyrole volatilizes with the watery vapour.

By following the directions given by Simon, and employing

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* *Ann. de Ch. et de Ph.*, 3rd ser., vol. iii. p. 151; and *Ann. der Chem. und Pharm.*, vol. xliv. p. 304.

† *Ann. der Chem. und Pharm.*, vol. xliv. p. 286.

in the distillation ordinary nitric acid, we obtained indeed the peculiar substance named by him nitrostyrole, but in quantity bearing no proportion to the amount of oil employed. The oil is very slowly attacked by common nitric acid. On heating the mixture to boiling the styrole becomes of a yellow colour, but at the same time a large quantity of undecomposed yellowish oil distils over. Only after being five or six times poured back into the retort does it begin to change its character, by becoming heavier, more tenacious, and finally sinking to the bottom of the vessel. At this period the oil-drops which pass over with the water smell no longer of styrole, but possess a peculiar sharp odour of cinnamon, and violently attack the eyes. If the styrole has become quite brown and sunk to the bottom, it solidifies on the cooling of the retort into a resinous mass. The watery fluid above the resin, on the other hand, deposits a quantity of crystalline plates. On removing the crystals from the retort, washing the resin with water to free it from the nitric acid, and again heating to boiling the resin with water, the greater portion becomes dissolved, and there distils over a volatile matter possessing in the highest degree the odour of cinnamon. After a short time these oil-drops solidify in the retort. On allowing the retort to cool when no more of this substance passes over, the whole fluid becomes a crystalline mass. By long-continued distillation, a portion of this crystalline matter, which is, however, much less volatile than the nitrostyrole, also passes over into the receiver.

As already stated, the quantity of the volatile oil obtained was exceedingly small. Only after repeated operations, and at the sacrifice of several ounces of styrole, was a quantity prepared sufficient for analysis. It was separated by filtration from the fluid which distilled over with it, and after washing with water, which dissolves but a small proportion, again distilled with the vapour of water, and finally dissolved in boiling alcohol. On cooling, large beautiful prisms* crystallized from the solution, possessing the characteristic odour of cinnamon, and a sweet but highly burning taste.

The following results were obtained on combustion with oxide of copper, after the substance had been dried over sulphuric acid for several days:—

I. 0·2662 grm. of the crystals gave 0·6250 grm. of carbonic acid and 0·1190 grm. of water.

II. 0·1630 grm. of crystals gave 0·3885 grm. of carbonic acid and 0·0790 grm. of water.

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* In the memoir of Simon exact measurements of these crystals are given from G. Rose.

III. 0·2787 grm. of crystals gave 0·1255 grm. of water.

IV. The nitrogen was determined by the method of Dumas, by burning the substance in an atmosphere of carbonic acid. 0·5088 grm. of crystals gave 48· cub. cent. of moist nitrogen at 19° C. and 327‴ barom.

These numbers correspond to—

	I.	II.	III.	IV.
Carbon . .	64·04	64·50		
Hydrogen .	4·96	5·38	5·00	
Nitrogen . .	......	......	......	10·30

When we consider that the method of Dumas always gives an excess of nitrogen, we can from the above numbers deduce the following formula, $C_{16}H_7NO_4$, giving per cent.,—

				Mean of experiments.
16 equiv.	Carbon . .	1200·00	64·36	64·27
7 ...	Hydrogen . .	87·50	4·69	5·11
1 ...	Nitrogen .	177·04	9·50	10·30
4 ...	Oxygen . .	400·00	21·45	21·32
		1864·54	100·00	101·00

According to this formula, which will be further confirmed by the analysis of the bromine and chlorine compounds, the body investigated is analogous to nitrobenzide, which it resembles in many respects, and fully deserves the name of *nitrostyrole*, proposed by Simon.

If from this formula we deduce the composition of styrole, it must be expressed by $C_{16}H_8$, and the formation of nitrostyrole is shown in the following manner:—

$$\underbrace{C_{16}H_8}_{\text{Styrole.}} + NO_5, HO = \underbrace{C_{16}H_7NO_4}_{\text{Nitrostyrole.}} + 2HO.$$

As to the properties of nitrostyrole, we have but little to add to the statements of Simon. This body is particularly characterized by its odour, which strongly irritates the eyes to tears, and by the manner in which it affects the skin, long contact with which produces a painful burning, and finally blisters.

On distilling a mixture of nitrostyrole and an alcoholic solution of potash, after most of the spirits have passed over, there are obtained red oil-drops, which are not nitrostyrole. From want of materials we have not investigated this body: it is probably a compound analogous to the *azobenzide* of Mitscherlich. An exact study of this reaction will certainly lead to interesting results, but there will be some difficulty in obtaining a quantity of substance necessary for such a purpose.

We further endeavoured to transform nitrostyrole into a base by the action of sulphuret of ammonium. This body would have had the composition $C_{16}H_9N$, and by treatment with oxidizing means would probably give compounds of the indigo series, perhaps *isatine.* We have hitherto, however, not succeeded in preparing it.

We have already stated, that by the treatment of styrole with nitric acid, there passes over, towards the end of the distillation, an oil which possesses in the highest degree the odour of nitrostyrole. This oil is nothing but a solution of nitrostyrole in several other fluid bodies, probably undecomposed styrole and some oil of bitter almonds. On exposing this mixture to a very low temperature (−20° C.), it solidifies nearly completely into a crystalline mass, which can be separated from the adhering fluid oil by pressing in filtering paper: it is pure nitrostyrole.

Benzoic Acid.

The crystals which separate on the cooling of the watery fluid during the preparation of the resin for the formation of nitrostyrole, consist, according to the length of the distillation or the concentration of the nitric acid employed, either of benzoic or nitrobenzoic acid. In general we obtained a mixture of both.

To be quite certain on this point, we treated, in the repeated distillations which we undertook for the preparation of nitrostyrole, storax oil sometimes with quite dilute nitric acid.

By continuing the distillation for a considerable time, there passed over with the watery vapour a substance which condensed in the neck of the retort and in the receiver into a crystalline mass. This was dissolved in weak potash ley and boiled till all odour of nitrostyrole had disappeared. Acids produced in this alkaline solution a white crystalline precipitate, which was separated by filtration, washed, and dissolved in boiling water. From this solution large crystalline plates separate, which are identical with those deposited from the liquid above the resin (in the preparation of nitrostyrole), and which in every respect resembled benzoic acid. To remove every doubt, we formed benzole from it, and submitted the acid itself to analysis.

The following results were obtained by combustion with oxide of copper:—

0·3260 grm. gave 0·8230 grm. of carbonic acid and 0·1545 grm. of water.

The amount per cent. calculated from these numbers corresponds exactly to the formula $C_{14}H_6O_4$, as is shown by the following statement:—

		Theory.		Experiment.
14 equiv.	Carbon . .	1050·00	68·85	68·85
6 ...	Hydrogen .	75·00	4·92	5·25
4 ...	Oxygen .	400·00	26·23	25·90
1 ...	Benzoic acid =	1525·00	100·00	100·00

The transformation of styrole into benzoic acid is shown by the following equation:—

$$\underbrace{C_{16}H_8}_{\text{Styrole.}} + 10O = \underbrace{C_{14}H_6O_4}_{\text{Benzoic acid.}} + 2CO_2 + 2HO.$$

This equation merely represents the ultimate products of decomposition. It is, however, probable that an intermediate product precedes the formation of the benzoic acid. We have already mentioned a heavy oily liquid, which during the action of nitric acid on styrole condenses in the receiver. It contains, as stated, a large quantity of nitrostyrole; and also another fluid body, which we are inclined to consider as oil of bitter almonds. At least, at certain periods of distillation, the products acquired the strongest odour of that oil.

As the small quantity of the mixture we possessed excluded the possibility of a separation by distillation, we thought it might be possible to prove the presence of oil of bitter almonds by the formation of benzoine. For this purpose the mixture was treated, according to Zinin's method, with an alcoholic solution of potash and some cyanide of potassium*. We have, however, not been able to observe any crystals of benzoine, consequently the settlement of this point must be reserved for further investigation. The formation of oil of bitter almonds under the circumstances already mentioned would beside not have been very extraordinary. The investigations of Dumas and Peligot†, which were confirmed by Plantamour‡ and Simon§, have shown that cinnamic acid, treated by oxidizing means, produces hydruret of benzoyle before passing into benzoic acid. According to very recent observations of Cahours‖, oil of anise, before being finally changed into anisinic acid, is first converted into hydruret of anisyle, which stands in the same relation to the last acid as hydruret of benzoyle to benzoic acid.

Nitrobenzoic Acid.

If strong nitric acid, such as the commercial, has been employed in distillation with styrole, there are obtained, by the

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* *Ann. der Chem. und Pharm.* vol. xxxiv. p. 186.

† Ibid. vol. xiv. p. 59.

‡ Ibid. vol. xxx. p. 349.

§ Ibid. vol. xxxi. p. 271.

‖ *Compt. Rend.* p. xix. 795.

solution of the residue, crystals which possess all the properties of nitrobenzoic acid discovered by Mulder*. We have made several analyses of this acid and its silver salt, which, however, we consider we may pass over, as, after proving the formation of benzoic acid, the appearance of nitrobenzoic acid was easily explained.

Simon states that hydrocyanic acid is one of the products of the action of nitric acid on styrole: we have not observed the formation of this acid.

It has been already stated that the amount of nitrostyrole obtained was exceedingly small. We have modified the process for forming it in several ways, in order to obtain, if possible, this interesting body in larger quantities.

The analogy which exists between nitrostyrole and nitrobenzide, both in their mode of formation and properties, appeared to indicate the way to a more advantageous method of preparation. For this purpose we dropped styrole into fuming nitric acid, which formed with it a deep red solution, with great evolution of heat. Here also, even by the employment of artificial cold, the formation of red vapours could not be avoided. From the nitric acid solution water precipitated a yellow resinous substance of soft consistency, which possessed in the highest degree the characteristic smell of nitrostyrole. After washing with water till the greater part of the nitric acid was removed, the yellow mass was submitted to distillation with water. A quantity of nitrostyrole volatilized with the watery vapour, greater than was obtained in the former method, but still out of all proportion to the amount of oil employed. We were equally unfortunate in the treatment with fuming nitric acid, which had been previously saturated with deutoxide of nitrogen.

If a current of nitrous acid (NO_3) be passed through styrole when warm, a violent reaction takes place with the evolution of vapours, which possess the irritating odour of nitrostyrole; at the same time there is formed a peculiar odourless crystalline substance, insoluble in water, alcohol and æther. This compound we have not studied.

In this reaction, which is certainly very complicated, nitrostyrole is likewise formed in only very small quantities.

Action of Chromic Acid on Styrole.

On submitting to distillation a mixture of styrole, bichromate of potash, sulphuric acid and water, the greater portion of the styrole passes with the watery vapour undecomposed into the receiver. A reaction takes place only after the contents of

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* *Ann. der Chem. und Pharm.* vol. xxxiv. p. 297.

the retort have commenced to become solid; by continuing the distillation large crystals of benzoic acid collect in the receiver.

Action of fuming Sulphuric Acid on Styrole.

On mixing fuming Nordhausen sulphuric acid with styrole, a violent reaction takes place with the evolution of heat; the oil becomes tenacious, and assumes a dark colour. The addition of water produces the separation of a brown resinous mass. The fluid filtered from this substance gave, when treated with an excess of carbonate of baryta, a soluble baryta salt, which, however, could not be obtained in a crystalline form. The physical properties of these products, probably analogous to Mitscherlich's sulphobenzide and hyposulphobenzidic acid, are far from inviting to a nearer investigation.

Action of Bromine on Styrole.

Bromostyrole.—By the addition of bromine to the volatile oil of storax it becomes heated to boiling, and the evolution of hydrobromic acid always takes place. The oil is by this reaction completely transformed into a crystalline mass of a peculiar odour.

The formation of hydrobromic acid can be completely avoided by placing the flask in water and adding the bromine by drops. By waiting each time till the heat evolved has been removed, the product obtained is uniform.

The crystalline mass is insoluble in water, but communicates to it a highly peculiar penetrating odour and taste, which resemble a mixture of oil of citron and juniper. It is, on the other hand, very soluble in alcohol and æther. We have in vain endeavoured, by gentle evaporation of the æthereal solution, to obtain well-formed crystals. The boiling saturated alcoholic solution on cooling deposits the compound in the form of an oil, which covers the bottom of the vessel, and not unfrequently remains long fluid under the temperature at which it solidifies, until shaken, when it becomes solid. It fuses under boiling water. It is decomposed by an alcoholic solution of potash, bromide of potassium, and another compound containing bromine being formed.

The products submitted to analysis were of different preparations. Nos. I. and II. were formed directly by the treatment of styrole with an excess of bromine. The somewhat yellow substance was washed with spirits of wine till it appeared white, and after solution in the same menstruum was again precipitated by the addition of water, and washed with the same fluid till all trace of hydrobromic acid, which forms in considerable quantity during the preparation, was removed. The substances employed in Nos. III. and IV. were made with great care; merely traces of hydrobromic acid were formed, which were also removed in the same manner.

The combustions, which were all made with chromate of lead, gave the following results:—

I. 0·3950 grm. of substance gave 0·5150 grm. of carbonic acid and 0·1155 of water.

II. 0·3095 grm. of substance gave 0·4085 grm. of carbonic acid and 0·0920 grm. of water.

III. 0·3650 grm. of substance gave 0·4085 grm. of carbonic acid and 0·1047 grm. of water.

IV. To determine the bromine, the substance was carefully mixed with caustic lime and heated to redness in a combustion-tube. The heated mass was then dissolved in nitric acid, filtered, and precipitated by nitrate of silver. Treated in this way there was obtained from 0·2668 grm. of substance 0·3805 grm. of bromide of silver.

These numbers correspond per cent. to—

	I.	II.	III.	IV.
Carbon .	35·85	35·99	36·23	
Hydrogen	3·23	3·27	3·18	
Bromine .	...	...	...	59·83

From which the following formula is deduced,

$$C_{16}\,H_8\,Br_2;$$

giving per cent. the following numbers:—

16 equivts.	Carbon	=1200·00	36·85
8 ...	Hydrogen	= 100·00	3·07
2 ...	Bromine	=1956·61	60·06
		3256·61	100·00*

Action of Chlorine on Styrole.

Chlorostyrole.—By the treatment with chlorine there is formed a compound analogous to the bromostyrole. The chlorine compound is fluid. It is extremely difficult to prepare the chlorine compound pure. A stream of the gas passed through styrole is rapidly absorbed, without any evolution of hydrochloric acid, if the current be not too rapid, and the temperature kept low, at the same time that the influence of the direct rays of the sun be avoided. By long treatment the oil changes into a thick fluid, which is chlorostyrole. As soon as the evolution of hydrochloric acid indicates that no more styrole exists free, the action must be interrupted. The evolution

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* The loss of carbon in Nos. I. and II. arose evidently from a mixture of another product richer in bromine. The evolution of hydrobromic acid in the preparation can only take place from such a cause.

of hydrochloric acid indicates that the decomposition is proceeding further, by the formation of products of substitution which contain fewer hydrogen equivalents than exist in styrole; whereas chlorostyrole is a simple compound of chlorine and styrole, or at least may be considered as such.

We have frequently prepared and analysed the first product of the action of chlorine on styrole. The analyses proved however to us that only on one occasion were we fortunate enough to have hit the proper moment when the action of chlorine must be interrupted. All the other combustions, which we pass over, gave a constantly varying, but always too small a proportion of carbon.

Burned with chromate of lead the following results were obtained:—

0·3553 grm. of chlorostyrole gave 0·7200 grm. of carbonic acid and 0·1505 of water.

From which the following formula and theoretical numbers are deduced, $C_{16}\,H_8\,Cl_2$.

		Theory.		Experiment.
16 equivts.	Carbon . .	1200	54·91	55·26
8 ...	Hydrogen .	100	4·57	4·70
2 ...	Chlorine . .	885·3	40·52	
1 ...	Chlorostyrole	2185·3	100·	

By heating chlorostyrole it is decomposed into a new oily product with evolution of hydrochloric acid. This product, which is probably $C_{16}\left\{\begin{matrix}H_7\\Cl\end{matrix}\right\}$, is obtained in more considerable quantities when chlorostyrole is distilled with caustic lime. We have not examined it more closely. The formula of styrole derived from nitrostyrole is thus completely supported by the analysis of the bromine and chlorine compounds.

By the continued action of chlorine for several days on styrole in direct sunlight, that oil is changed into a thick viscid fluid, with the evolution of a very large quantity of hydrochloric acid. This substance is not even the last product of the action, as by further continuance hydrochloric acid is still given off.

The same fact is observed on submitting styrole to the action of chlorate of potash and hydrochloric acid.

Action of Heat on Styrole.

Metastyrole.—The remarkable change experienced by styrole under the influence of heat has been already mentioned in the preceding pages. We stated that in rectifying the oil a considerable portion of pure unchanged styrole passed over at first, but at a certain point the contents of the retort became thick, and on cooling solidified into a firm transparent vitreous mass.

On first observing this phænomenon we thought it arose from the conversion into resin by the action of the air of a portion of the raw oil which had been standing in a bottle only half-filled, and that this substance being held in solution by the remaining fluid oil, was separated on distilling the latter from it. A similar idea seems also to have occurred to Simon, from his bestowing the name of oxide of styrole on the residue found in the retort on submitting styrole to distillation.

We soon convinced ourselves that this idea was erroneous by distilling a second time a portion of newly-rectified oil. A very considerable residue of solid matter, though not quite so great as in the first distillation, was observed to remain in the retort. The same amount was further found in a third, fourth and every succeeding distillation. The rapidity with which styrole changes into the solid substance, indicated sufficiently that it could not be from combination with oxygen. This fact was besides established by direct experiment. Indeed so little attraction has styrole for oxygen, that it can be left exposed to air for weeks together without any change in its colour. A portion of oil was confined for several months over mercury in a tube filled with oxygen gas without the least diminution in the volume of the latter. We soon recognised that this metamorphosis of styrole takes place without loss of or addition to any one of its elements, and solely through a change in the molecular structure of this body produced by the action of heat. Analysis as well as synthesis has equally proved that styrole and the vitreous mass (for which we propose the name of metastyrole) possess the same constitution per cent.

Metastyrole, when prepared from colourless oil, is equally transparent, and possesses the same powers of refraction as the latter body*. On the other hand, it completely loses the characteristic odour and taste which distinguish styrole. Metastyrole is totally inodorous and tasteless. At the ordinary temperature this body is hard and can be cut with a knife. By heat it becomes soft, and can be drawn out into threads, which strikingly resemble spun glass. It is insoluble in water and alcohol, as well in the cold as when heated. In boiling æther a small portion is dissolved, which, on leaving the solution to spontaneous evaporation, becomes attached to the sides

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* There is scarcely an organic body which refracts the rays of light so strongly as metastyrole. It is not improbable that it might be applied to several optical purposes.

of the vessel in a thin almost transparent layer, that can be separated from the glass, and presents a most deceptive likeness to the thin membrane lining the shell of an egg. The portion which has not been dissolved in the æther swells to six or seven times its original volume. This mass contains, when dried at the ordinary temperature, a large quantity of æther, which is driven off only by heat. Turpentine oil also dissolves traces of metastyrole; sulphuric acid is in the cold without action, but on the application of heat carbonizes the metastyrole, with evolution of sulphurous acid. The action of nitric acid will be presently mentioned.

To obtain metastyrole completely pure, and in a condition fit for analysis, advantage was taken of its relations to æther. The vitreous mass was boiled in æther till it was completely converted into the swollen gelatinous matter from which the excess of æther was decanted; this contains the unchanged styrole that had remained in the resinous mass; and after heating the residue in a water-bath, to expel the æther, a completely inodorous and tasteless white spongy mass was left behind, which could be easily reduced to powder. This powder was again boiled in alcohol, dried, and burned with chromate of lead.

0·2955 grm. gave 0·9970 grm. of carbonic acid and 0·2130 grm. of water. Corresponding in 100 parts to—

Carbon	92·05
Hydrogen	8·00

Styrole contains—

Carbon	92·30
Hydrogen	7·70

This combustion is sufficiently decisive, but to remove all doubt the following experiment was made.

A portion of styrole was confined in a strong glass tube, hermetically sealed by the blowpipe lamp, and placed in an oil-bath, whose temperature was not allowed to rise above 200° C. At the end of half an hour *the styrole had completely changed into metastyrole.* The same result was obtained by sealing a portion of it in small glass bulbs, in which scarcely a trace of air was left, and submitting them to the temperature of boiling water. On the second day the styrole had become thick, and on the third solid.

A bulb of the same size was exposed to the heat of the sun in the hot summer months. The same transformation likewise took place; but at this temperature it required three weeks to produce the same effect as takes place almost immediately at that of 200° C. Intensity and duration * of action are seen here, as everywhere else in nature, to be equivalent.

Light appears, however, to play a part in the transformation of styrole; at least a small portion sent to Professor Liebig five years ago by E. Simon, and which had been kept in a dark press, is still as fluid as if newly distilled.

This metamorphosis of styrole by heat is so much the more extraordinary, as this agent generally acts in a contrary direction, either to liquify or volatilize. The white of egg alone, in certain points, offers some analogy; and these two bodies, at a higher temperature, exhibit likewise, though in a distant degree, the same behaviour.

L. Gmelin † has shown that the coagulated white of egg, heated in a Papin's digester with water to 200° C., becomes again fluid, and forms with the water a yellow liquid. This observation has been lately confirmed by Wöhler and Vögel ‡.

If metastyrole is heated in a small retort over a spirit-lamp, it becomes again fluid, and a colourless oil distils over, which is pure styrole. By a carefully conducted distillation there remains scarcely any residue in the retort.

It was possible that the oil obtained by distilling metastyrole might be another body of the same composition as styrole and metastyrole, but possessing different properties. The regenerated oil forms instantaneously with bromine the characteristic smelling bromine compound. Besides this reaction, which, as will be seen immediately, is not decisive, the action of heat on the regenerated oil is quite conclusive. Heated to 200° C., in a tube hermetically sealed, it is converted into metastyrole, which by a higher temperature is again in its turn reconverted into styrole.

After having ascertained that styrole and metastyrole had the same composition per cent., and that the difference in their physical properties could only arise from a dissimilar arrangement of their molecules, it was of some interest to obtain an explanation of this arrangement. As the specific weight of the vapours of both bodies could not be compared with each other, there remained no other resource but to study more closely the products of decomposition of metastyrole. As bromine and chlorine act only very slowly on metastyrole, sulphuric acid carbonizes it, and by fusing with potash a sim-

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* The varying amount of styrole obtained from the different sorts of storax is probably owing to the same metamorphosis produced in the course of time.

† *Handbuch*, 3rd edition, vol. ii. p. 1053.

‡ *Ann. der Chem. und Pharm.*, vol. xli. p. 238.

ple conversion into styrole follows; nitric acid was selected as the decomposing agent.

Action of Nitric Acid on Metastyrole.

Nitrometastyrole.—Common nitric acid, either warm or cold, attacks metastyrole but little. Even strong fuming nitric acid acts but feebly in the cold, but on the application of heat dissolves it quickly with the evolution of red fumes. If a sufficient quantity of nitric acid has not been employed, the new product is precipitated on cooling in the form of a slimy substance. On the other hand, if the acid has been in excess, the fluid remains quite clear, from which, on the addition of water, a white curdy mass, which sometimes has a shade of yellow, is precipitated. This is washed with water to remove all trace of nitric acid, and afterwards boiled in alcohol to get rid of any trace of benzoic acid which may have been formed from styrole still remaining in the pores of the metastyrole.

The body thus obtained, and for which we propose the name of nitrometastyrole, is when dry in the form of a white or yellowish, completely amorphous powder, insoluble in æther, alcohol and water, but soluble in fuming nitric and in sulphuric acid. By the application of a gentle heat it burns with a slight deflagration, like many bodies which possess the elements of hyponitric acid, and with a strong odour of oil of bitter almonds. As the physical properties of this body do not indicate any certainty of its purity, it was burnt to ascertain this point as nearly as possible. After some analyses it was recognised that the new product of decomposition retained traces of undecomposed metastyrole, from the nitric acid solution becoming very turbid on cooling. This fact was proved by dissolving the substance in concentrated sulphuric acid, when the undecomposed metastyrole remains undissolved. On the other hand, by boiling the solution of metastyrole too long in the nitric acid, a mixture was obtained, on precipitating with water, consisting for the most part of nitrometastyrole, and of a second product containing less carbon and more nitrogen.

Nitrometastyrole is obtained pure by dissolving metastyrole in a quantity of acid sufficient to hold it in solution on cooling. Nitric acid must be added to the hot solution as long as it is observed to become turbid on pouring a few drops out into a watch-glass.

The analysis of the substance prepared in this way gave the following results:—

I. 0·3235 grm. of nitrometastyrole gave 0·7230 grm. of carbonic acid and 0·1325 grm. of water.

II. 0·2880 grm. of substance gave 0·6535 grm. of carbonic acid and 0·1330 grm. of water.

III. 0·2865 grm. of substance gave 0·1170 grm. of water.

IV. 0·3142 grm. of substance gave 0·1278 grm. of water.

V. The nitrogen was determined by the method of Dumas, by burning the substance in an atmosphere of carbonic acid, and calculated from its volume.

0·6125 grm. of nitrometastyrole gave 53 cubic centimetres of nitrogen at 329''' barometer and 9° C. thermometer. In per cent.

	I.	II.	III.	IV.	V.
Carbon . .	60·95	61·69			
Hydrogen . .	4·55	5·26	4·53	4·51	
Nitrogen . .					10·06

These numbers correspond to the formula $C_{14}H_6NO_4$, as is shown by the comparison of the numbers found and calculated.

		Theory.		Mean of experiments.
14 equiv.	Carbon . .	= 1050·00	61·69	61·32
6 ...	Hydrogen .	= 75·00	4·40	4·71
1 ...	Nitrogen . .	= 177·04	10·40	10·06
4 ...	Oxygen . .	= 400·00	23·51	
1 ...	Nitrometastyrole	= 1702·04	100·00	

Taking the most probable supposition, that nitrometastyrole stands in the same relation to metastyrole as nitrostyrole to styrole, or nitrobenzide to benzole, that is, that one equivalent of hydrogen in the carbo-hydrogen is replaced by one equivalent of hyponitric acid, the formula for metastyrole will then be $C_{14}H_7$, from which it is seen, that by the metamorphosis of the body, $C_{16}H_8$, the number of equivalents of carbon and hydrogen in the new compound has diminished by one-eighth.

The formula of nitrometastyrole differs from that of anthranilic acid and protonitrobenzoene by containing one equivalent less of hydrogen. The formula of the two latter bodies is $C_{14}H_7NO_4$. Late investigations* have shown that protonitrobenzoene, as well as anthranilic acid, when distilled with lime, is decomposed into carbonic acid and aniline. As nitrometastyrole is a body completely analogous to nitrobenzoene, we expected to form under similar circumstances a base with the formula $C_{12}H_6N$. The experiment did not however turn out to our expectation. On distilling a mixture of lime and nitrometastyrole, a very complicated reaction follows, a large quantity of carbon is deposited, ammonia is evolved, and at

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* New Modes of Formation of Aniline, by Dr. J. S. Muspratt and Dr. A. W. Hofmann.—Philosophical Magazine, vol. xxvi. p. 581.

the same time a proportionately small quantity of a brown coloured oil passes over. Hydrochloric acid dissolves a large portion of this oil, from which it is again separated by the addition of alkalies. This oil is nothing but *aniline*, which is at once recognised by the dark violet colour it gives with a solution of chloride of lime, and the yellow colour its solution in acids communicates to pine-wood. The formation from nitrometastyrole of this base, which contains more hydrogen than the original substance, is certainly the result of a very complicated reaction, which cannot be exhibited in any equation.

The following is the series of bodies investigated in this memoir:—

Styrole	$C_{16}\ H_8$
Bromostyrole	$C_{16}\ H_8\ Br_2$
Chlorostyrole	$C_{16}\ H_8\ Cl_2$
Nitrostyrole	$C_{16}\left\{\begin{matrix}H_7\\ NO_4\end{matrix}\right\}$
Hydruret of benzoyle(?)	$C_{14}\ H_5\ O_2,\ H$
Benzoic acid	$C_{14}\ H_5\ O_3,\ HO$
Nitrobenzoic acid	$C_{14}\left\{\begin{matrix}H_4\\ NO_4\end{matrix}\right\}O_3,\ HO$
Metastyrole	$C_{14}\ H_7$
Nitrometastyrole	$C_{14}\left\{\begin{matrix}H_6\\ NO_4\end{matrix}\right\}.$

The experiments detailed in the preceding pages have proved that the formula of the carbo-hydrogen derived from the storax must be expressed by $C_{16}\ H_8$. This is, however, likewise the formula of the carbo-hydrogen analogous to benzole, and which is derived from hydrated cinnamic acid by the separation of two equivalents of carbonic acid.

Are Styrole and Cinnamole identical?

This question must necessarily present itself when we consider that storax contains large quantities of cinnamic acid, and that this balsam, according to the views of later celebrated pharmacologists (as Th. W. Martius*), is obtained not by incision but by a kind of distillation.

How easily in this operation a portion of the cinnamic acid might be converted into cinnamole and carbonic acid, we know from the fact of benzoic acid, when passed in vapour through a red-hot tube, being decomposed with ease into carbonic acid and benzole.

The details given us by different chemists on cinnamole

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* *Grundriss der Pharmakognosie des Pflanzenreichs*, p. 346.

differ exceedingly from each other. Simon and Marchand prepared this substance by distilling cinnamic acid and caustic lime. Herzog[*] followed the same course; he obtained a yellowish fluid, which on rectifying gave a light colourless oil, whilst a yellowish-brown, heavy liquid remained in the retort. Cinnamole (named by Marchand and Simon cinnamomine) which has been prepared in this way boils at 89° C. and has a specific gravity of 0·88.

According to Mitscherlich[†] no product can be obtained in this way possessing a constant boiling-point. He considers the liquid which distils over on rectification as a mixture of different carbo-hydrogens of similar composition, and suspects that it contains benzole.

Finally, Gerhardt and Cahours[‡] have stated that a mixture of 1 part cinnamic acid and 4 parts baryta, when distilled, gave a product having a constant boiling-point.

The formula $C_{16}H_8$ was controlled by a determination of the specific gravity of the vapour, and by the analysis of a bromine compound.

The properties described by the last two chemists as belonging to cinnamole (cinnamene), correspond completely with those of styrole. Cinnamole boils, according to Gerhardt and Cahours, at 140° C. It forms with chlorine a fluid, and with bromine a crystalline compound, the latter having the formula $C_{16}H_8Br_2$. With nitric acid it gives rise to a product which appears to be benzoic acid. In one point, however, they differ. Cinnamole does not possess the property of forming by heat an isomeric solid compound. Gerhardt and Cahours do not mention in their memoir anything of this phænomenon, which, if it existed, could not have escaped their observation in the determination of the specific gravity of the vapour. This determination could not in reality have been made at the temperature mentioned. Styrole was exposed for three or four hours in an oil-bath to a temperature of 182° C. (the same temperature by which Gerhardt and Cahours made the determination of the specific gravity of the vapour), without, however, being altogether volatilized. There remained constantly a considerable portion of metastyrole behind.

To obtain accurate results on this point, we prepared cinnamole by means of lime and baryta. With respect to the action of the lime, we obtained the same results as Mitscherlich. The distilled product is a mixture of different bodies.

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[*] *Arch. der Pharm.*, 2 ser. vol. xx. p. 167.

[†] *Monatsbericht der Ber. Acad. u. Lehrb.* 4th edit. vol. i. p. 179.

[‡] *Ann. de Chim. et de Phys.*, 3rd ser. vol. i. p. 60, and *Ann. der Chem. u. Pharm.* vol. xxxviii. p. 96.

His suspicion of the presence of benzole we found to be also correct. The body described by Herzog under the name of cinnamole was evidently benzole mixed with traces of other materials. The boiling-point and specific gravity given by him are in themselves sufficient evidence. By distilling a considerable portion of cinnamic acid with lime we easily obtained proof of this identity in the following manner:—The rectified product was submitted again to an interrupted distillation. The oil which passes over under 100° C. was collected by itself, and after treatment with fuming nitric acid, was then exposed to the action of a reducing agent. Aniline was thus obtained, which is easily recognised by its characteristic properties.

It was probable that the portion of fluid which boils at a higher temperature contained cinnamole. We endeavoured by interrupted distillations to separate it, but could not obtain any liquid having a constant boiling-point. The thermometer rose at last above 200°. The portion distilled at 140° formed with bromine a white crystalline mass, which in appearance and smell possessed the most striking similarity to bromostyrole. This crystalline compound was probably *bromocinnamole*, as no styrole could be supposed to exist in it, no trace of residue being left in repeated distillations.

Cinnamole obtained, according to Gerhardt and Cahours' plan, by distilling cinnamic acid with baryta, proved, after rectification, a colourless fluid, which in point of smell was very similar to styrole, though the former was rather more agreeable than the latter. With bromine it forms a crystalline compound having very much the appearance of bromostyrole. Unfortunately a sufficient quantity was not obtained to determine the boiling-point.

As a further point of comparison, a portion of the product (prepared from lime and cinnamic acid), distilled at 140°, was sealed up in a strong tube. Another tube was filled with the oil made according to the plan of Gerhardt and Cahours, and several others with styrole. All these tubes were placed in an oil-bath heated to 200° C. After half an hour the bath was allowed to cool and the tubes removed, when the styrole was found to have become solid, whilst the substances prepared from cinnamic acid by lime and baryta had lost nothing of their fluidity.

This experiment appears at first sight to negative the idea of the identity of the two substances. When, however, it is considered that the product obtained on distilling cinnamic acid and lime contains other bodies besides cinnamole, and that the substance prepared from baryta and the same acid was in too small a quantity to be more closely investigated, it is evident that the solution of this question must be reserved for future investigations.

Should these prove in an unequivocal way the difference of the two bodies, it would be of great interest to ascertain from what material styrole has its origin. For this purpose it would be in the first place particularly important to gain some information about the source from which *Styrax liquidus* is obtained. The only authentic information we possess on this point is given by Martius*, namely, that it is introduced through Suez and Trieste, is named by the East Indians *Cotter Mija*, and is produced from a tree called *Rosa Mallas*. From this tree two products are, according to Martius, obtained, the one, the common *Styrax liquidus*, through a sort of distillation; the other a balsam, which is very rarely to be met with in commerce. An investigation of the latter would be of great interest.

Postscript.

The preceding memoir was finished in its principal points when the investigation of the products of distillation of dragon's blood by Glenard and Boudault† appeared.

A short notice of this subject had been earlier laid before the French Academy‡. What was then stated, however, did not lead us to suspect in the slightest degree that the products obtained by distillation from dragon's blood could stand in any relation to our investigation of fluid storax. Their late memoir, which contains many essential corrections of the former communication, showed us at first sight the connection between it and our investigation. Messrs. Glenard and Boudault having communicated some striking results, not easily to be understood, it appears to us not to be without interest to conclude this memoir with a few remarks on the investigation of the French chemists, as our experiments furnish a key to their difficulties.

Glenard and Boudault, by distilling dragon's blood, obtained, in addition to water and some acetone and benzoic acid, a reddish-brown oily fluid, which is a mixture of different bodies. From this they isolated two carbo-hydrogens, which they named dracyle and draconyle.

For their preparation the raw oil was submitted anew to distillation, and the product, distilled at 180° C., was collected by itself; it contains dracyle and draconyle. To separate them,

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* *Briefliche Mittheilung.*

† *Journ. de Pharm. et de Chem.* 3me ser. vol. vi. p. 250.

‡ Ibid. vol. i. p. 274, and *Ann. der Chem. u. Pharm.* vol. xlviii. p. 343.

the mixture must be several times distilled at a low temperature, never reaching the boiling-point. What passes over is a colourless fluid, and consists of dracyle, with a small quantity of draconyle; the solid residue in the retort is entirely draconyle.

To free the dracyle from the draconyle, "*which passes over dissolved in the vapour of the former*," it must, acccording to Glenard and Boudault, be several times distilled over hydrate of potash. The potash appears here to form no combination with the draconyle, "*but only to modify it in such a way that it is no longer soluble in dracyle.*" The draconyle remaining in the retort is to be washed with alcohol to remove the dracyle and then heated to 150° C., by which the last traces of dracyle are driven off.

Pure draconyle is, according to these chemists, a colourless mass of a shining appearance; insoluble in water, alcohol, æther and potash-ley, but soluble by heat in fat and æthereal oils.

Draconyle is not volatile, but by aid of another carbo-hydrogen it can be distilled. (Il n'est point volatile; toutefois, il peut distiller à la faveur d'un autre hydrogène carboné).

From the analysis of Glenard and Boudault, draconyle contains two equivalents of carbon to one of hydrogen. The formula found for the substance obtained by the action of nitric acid on draconyle, and for which they proposed the name nitrodraconyle, was $C_{14} H_6 NO_4$. This gives for draconyle $C_{14} H_7$. That substance sealed up in a tube and heated to a high temperature is converted into a yellow fluid, which boils at 140° C., and has the same composition as draconyle itself. Thus far are the details given by Glenard and Boudault. On comparing the results of these chemists with ours, it is at once seen that *metastyrole and draconyle are identical.* A satisfactory explanation is immediately found for some things in their memoir otherwise incomprehensible. We cannot, for instance, understand how a body with the properties given it, and totally devoid of volatility, can distil by the aid of another carbo-hydrogen. In boracic acid, indeed, we possess a body in itself completely fixed, but which volatilizes in not inconsiderable quantities with the vapour of boiling water; still this quantity is but a trace in comparison with the amount of the watery vapour. Our experiments however explain quite easily this difficulty. *Draconyle (metastyrole) exists in the corresponding product of the distillation of dragon's blood, not as such, but in the form of styrole.* After the distillation only is this styrole changed into metastyrole (draconyle). It is not easily understood why the mixture must be repeatedly distilled to obtain draconyle, and why the distilled dracyle obstinately retains a small portion of draconyle (that is, in this case styrole), which can only be removed by continued heating with hydrate of potash. The hydrate of potash is here really unessential, the separation being produced by the action of the heat alone. The most certain and complete mode of separation would be to seal up the mixture in a strong tube of glass, and heat it for an hour at 200° C. in an oil-bath. On opening the tube, the dracyle could be directly distilled from the metastyrole (draconyle).

Although, on making a comparison of the two bodies, there can be no doubt of their identity, it appeared desirable to substantiate the assertion by direct experiment. The point would be settled could there be obtained from dragon's blood styrole or any of its compounds. We are indebted to Dr. J. S. Muspratt for a quantity of the oil collected at 180° C. in the rectification of the raw product. On submitting this to a new distillation, dracyle alone at first passes over, then a mixture of dracyle and styrole, and finally almost pure styrole shortly before the metastyrole (draconyle) is formed in the retort. With bromine it forms a crystalline body, possessing all the properties of bromostyrole. Much weight however was not to be attached to this reaction, as it was found that dracyle also formed with bromine a compound which crystallized in beautiful needles. To decide the point, a quantity of the fluid was sealed in a strong tube and heated in an oil-bath to 200° C. On withdrawing it at the end of an hour, the contents were not quite solid, but the original light, moveable fluid was converted into a jelly of tenacious consistency, which, on allowing the tube to cool, could scarcely be moved. The styrole had evidently contained a small quantity of dracyle.

The appearance of styrole in the products of distillation of storax and dragon's blood brings these two substances into close relation with each other; probably they both contain the same, or a similar principle, which by distillation is decomposed in a similar manner. Its isolation would be of great interest. For this purpose however the analysis of the resins must be conducted in a different manner than has hitherto been the case. Cinnamic acid has not as yet been observed in dragon's blood.

XVIII. *Note on the useful applications of the Refuse Lime of Gas-works.* *By* Thomas Graham, *Esq., F.R.S.*[*]

I HAD lately occasion to examine the lime as removed from a dry lime purifier. The gas before reaching the latter had been washed with dilute sulphuric acid, which accounts

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[*] Communicated by the Chemical Society; having been read April 7, 1845.

for the absence of ammonia and cyanogen compounds. The lime had not been exposed more than a few hours to the air before it was operated upon. Still, to my surprise, it did not blacken an acid salt of lead, and contained no sulphuret of calcium. It was not dried, but analysed in a damp state, exactly as it is sent out of the works to be used as manure.

Composition of Gas-Lime.

Hyposulphite of lime . . .	12·30
Sulphite of lime	14·57
Sulphate of lime	2·80
Carbonate of lime	14·48
Hydrate of lime	17·72
Sulphur	5·14
Sand	·71
Water combined	8·49
Water (free)	23·79
	100·

With no more than a trace of ammonia and cyanogen.

The lime in the porous condition in which it is taken from the dry-lime purifiers, absorbs oxygen so rapidly from the air as to heat, and hence the state of oxidation in which the sulphur is found. If the lime be very damp, or diffused through a quantity of water, as it comes from the wet-lime purifiers, then the absorption of oxygen is much slower. The fluid portion then contains in solution the bisulphuret of calcium of Herschel, which may be crystallized from it; and at first very little else.

After the first rapid absorption of oxygen, the further oxidation of the gas-lime is decidedly slow. A specimen kept in an open vessel, and repeatedly moistened and rubbed to powder when it dried, was found after three months' exposure to retain 7 per cent. of sulphurous acid, besides all the free sulphur originally present. The hyposulphurous acid had entirely disappeared. Hence, if added to soil as manure, gas-lime must be powerfully deoxidizing, a property which will generally impair its utility.

It appears advisable, where the refuse-lime does not possess any value from ammonia, to dry it strongly, or roast it. It would thereafter consist of nearly equal weights of sulphate and carbonate of lime, and be in the condition most valuable as a manure.

Refuse-lime, such as was examined, may be recommended as a convenient and most œconomical source of the hyposulphites. The lime, after being taken from the purifiers, should be exposed to air for two or three days, till it loses all smell of sulphuretted hydrogen. The highly soluble hyposulphite of lime may then be dissolved out by little more than an equal weight of cold water. The solution may be evaporated at 120°, and the hyposulphite of lime crystallized out; or, the solution, by adding carbonate of soda, converted into hyposulphite of soda, which is a more stable salt, may be evaporated at a higher temperature, and crystallizes more easily.

From the refuse-lime, one-sixth of its weight of crystallized hyposulphite of lime has been obtained in a state of purity by a single crystallization. When the gas is washed with sulphuric acid to remove ammonia, before being conducted into the lime-purifier, it yields the refuse-lime more suitable for this purpose. The preparation of the hyposulphites in quantity is becoming the more important, as besides their use in electro-plating and photography, they are likely to be applied largely to the extraction of chloride and bromide of silver from silver ores.

XIX. *Examination of Babingtonite.* By Robert D. Thomson, *M.D., Lecturer on Practical Chemistry in the University of Glasgow*[*].

SYNONYMS.—Axotomous Augite spar (Haidinger); hemidomatischer Korazit (Breithaupt).

History.—The name Babingtonite was given in 1824, by M. Levy, to a mineral occurring crystallized on felspar from Arendal in Norway, in honour of Dr. Babington, one of the founders of the Geological Society of London[†]. Hitherto this mineral has been of such rare occurrence, that no analysis of it has been made. A specimen having lately come into my possession, I have taken the opportunity of subjecting it to a careful examination.

Crystalline Form.—The crystals examined appear to agree exactly with those described by Levy. They occur distributed over the surface of albite and associated with dark green amphibole. The crystals are sometimes six-sided, at other times eight-sided, doubly oblique prisms.

The *colour* of the crystals is black, and the *lustre* splendent and vitreous.

The *hardness* is 5·25.

The *specific gravity* is 3·355. It is stated by Breithaupt to be 3·406, a very close approximation; and in Alger's edition of Phillips's Mineralogy, the specific gravity is given as 3·5

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[*] Communicated by the Author.

[†] Annals of Philosophy, Second Series, vol. vii. p. 275.

without any authority. Kobell fixes it at 3·4, on what authority is not mentioned.

Before the blowpipe the crystals fuse *per se* into a black enamel.

With *carbonate of soda* they fuse into a green opake bead in the reducing flame.

With *borax* in excess a clear pale yellow bead becoming violet, and with microcosmic salt in excess a clear pale amber bead are formed. These results are similar to those obtained by Mr. Children, and his conclusions respecting the nature of the constituents of the mineral, by testing, are corroborated by analysis.

The following analysis was made with the careful assistance of my friend and pupil, William Parry, Esq., late of the 4th King's Own Regiment.

20·87 grains lost by a red heat 0·26 grs. water,
19·62 grains gave 9·42 grs. silica,
3·38 ... protoxide of iron,
2·17 ... red oxide of manganese,
or 2·02 ... protoxide of manganese,
1·29 ... alumina,
5·34 ... carbonate of lime,
or 2·93 ... lime,
0·44 ... magnesia.

These numbers correspond with the following per-centage composition:—

	I. Composition by expt.	II. Atomic weights of the constituents.	III. No. of atoms.	IV. No. of atoms reduced to lowest terms.
Silica	47·46	2·	23·73	26·36
Protoxide of iron . .	16·81	4·5	3·75	4·16
Protoxide of manganese	10·16	4·5	2·25	2·5
Alumina	6·48	2·25	2·88	3·2
Lime	14·74	3·5	4·21	4·6
Magnesia	2·21	2·5	0·90	1·0
Water	1·24	1·125	1·10	1·22
	99·10			

It is necessary to observe that the manganese contained a minute portion of iron.

The first column gives the results of experiment; the second the atomic weights of the constituents; the third column is the product of the division of the first by the second column; and the fourth exhibits the product of the division of the numbers in the third column by the lowest term in that column.

The following Table expresses the theoretical composition of the mineral as deduced from the third and fourth columns of the preceding table:—

	By calculation.	No. of atoms.	Atomic weight.
Silica	47·24	23	46
Protoxide of iron . .	18·45	4	18
Protoxide of manganese	9·24	2	9
Alumina	6·94	3	6·75
Lime	14·39	4	14·
Magnesia	2·59	1	2·5
Water	1·15	1	1·125
	100·	38	97·375

The formula to express the composition of babingtonite, as deduced from these calculations, is as follows:—

$MgO\,SiO, + 4CaO\,SiO + 4FeO\,(SiO)_2 + 2MnO\,(SiO)_2 + 3AlO\,(SiO)_2 + HO$;

or it consists of two simple silicates and three bisilicates.

Babingtonite agrees with the amphibole class in possessing a complicated constitution. It contains nearly the same amount of silica, but much less magnesia and a greater amount of manganese. Its specific gravity is also much higher than that of amphibole. My analysis approaches one by Bonsdorff, of a black hornblende from Nordmark and Pargas, the magnesia being replaced by manganese in babingtonite.

XX. *On the Resolution of Equations of the Fifth Degree.* By James Cockle, *Master of Arts, of Trinity College, Cambridge; of the Middle Temple, Special Pleader*[*].

1. IN the equation of the fifth degree in y,

$$y^5+p_1y^4+p_2y^3+p_3y^2+p_4y+p_5=0. \quad . \quad . \quad (1.)$$

Let
$$p_3=\frac{p_1}{5}\left(\frac{2p_1^{\,2}}{5}+3\,\mathrm{A}\right). \quad . \quad . \quad . \quad (2.)$$

and
$$p_4=\frac{p_1}{5^2}\left(\frac{p_1^{\,3}}{5}+3p_1\,\mathrm{A}+\mathrm{A}^2\right), \quad . \quad . \quad . \quad (3.)$$

where
$$\mathrm{A}=p_2-\frac{2p_1^{\,2}}{5}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

then, if $v-\frac{p_1}{5}$ be substituted for y in (1.), we obtain an equation in v of the form of De Moivre. Denoting by x the root of the general equation of the fifth degree, let

$$y=\Sigma_2(\Lambda'x^{\lambda'})+\Sigma_4(\mathrm{M}'x^{\mu'})+\mathrm{N}'x^{\nu'}+\mathrm{P}'x^{\pi'}+\mathrm{Q}'x^{\chi'}+\Sigma_2(\mathrm{R}'x^{\rho'}), \quad (5.)$$

the suffixes denoting the number of quantities included under each Σ, and the expression for y consisting, consequently, of

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[*] Communicated by T. S. Davies, Esq., F.R.S., F.S.A.

eleven terms, so that, by the notation of p. 114 of vol. i.* of the Mathematician, (2.) and (3.), respectively, become

$$f^3(10) = 0 \quad . \quad . \quad . \quad (6.), \qquad \text{and } f^4(10) = 0 \quad . \quad . \quad . \quad (7.)$$

Now I have shown, at paragraph 6 of that page, how, by means of a quadratic, which we shall here represent by

$$\theta_{10}{}^2(z_{10}) = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (a.)$$

and four *base*† quadratics which may, respectively, be denoted by

$$J_9^{(2)} = 0 \,.\, (b.), \quad J_8^{(2)} = 0 \,.\, (c.), \quad J_7^{(2)} = 0 \,.\, (d.), \quad J_6^{(2)} = 0 \,.\, (e.)$$

(6.) may be reduced to

$$h_1{}^3 + f^3(4) = 0; \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

and also, how, by means of a quadratic,

$$\theta_3{}^2(z_5) = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (f.)$$

a 'base' quadratic, $J_4^{(2)} = 0, \quad . \quad . \quad . \quad . \quad (g.)$

and a cubic, $\theta^3(z_3) = 0, \quad . \quad . \quad . \quad . \quad (h.)$

(8.) is ultimately reduced to the base cubic,

$$h_1{}^3 + h_2{}^3 = J_2^{(3)} = 0; \quad . \quad . \quad . \quad . \quad . \quad (i.)$$

whence, by eliminating z_2 between this last equation and (7.) (which we may now reduce to $f^4(2) = 0$), we arrive at a final biquadratic,

$$C_1 = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (j.)$$

for determining z_1; and z_2, &c. can be found by means of the other equations distinguished by letters; and, consequently, Λ', Λ'', &c. are known.

2. In this investigation, if we suppose that

$$x = Q\,x^q + \psi(x), \quad . \quad . \quad . \quad . \quad (9.)$$

then our object is *not* to satisfy (6.) *independently* of Q, so as to have that quantity at our disposal for the purpose of satisfying (7.)‡, but to reduce (6.) to a linear form, and then eliminate between (6.) and (7.).

3. In general, after effecting our fundamental reduction

$$f^a(b) = \Sigma(h^a), \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

we may group any two of the quantities h^a together, as in the above instance, and make their sum equal to zero; or should it in any case be necessary, we may increase the number of disposable quantities, and, possibly, obtain a (transcendental?) result, by grouping two or more terms of the right-hand side of (10.) together, and making their sums equal to arbitrary

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* The 2nd page of No. iii. (July 1844).

† The word "base" is used to indicate that they degenerate to linear equations.

‡ See Phil. Mag., S. 3. vol. viii. p. 538, and vol. ix. p. 28.

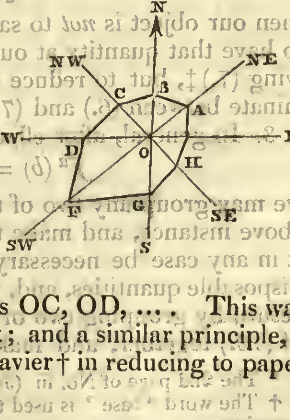
quantities, taking care that the sum of the arbitrary quantities shall vanish; or we may change the right-hand side of (10.) into $\chi(h)$, χ being an arbitrary function.

4. The values of α to be selected in the transformations of quadratics and biquadratics, which I gave at p. 384 of the last volume of this work, are, respectively, 1 and −1.

Cambridge, July 1, 1845.

XXI. *On a certain Method of representing by Diagrams the Results of Observations.* *By the* Rev. S. Earnshaw, *M.A.*, *Cambridge**.

IN attending the proceedings of the Physical Section of the British Association at the late meeting in this place, I was struck on more than one occasion with a defect in the method which had been employed by some of the experimentalists in representing their results on paper. My remarks refer to those cases in which polar co-ordinates were used, as was done in two very interesting and important papers; one "On the quantity of rain which had fallen with different winds at Toomavara," and the other "On Shooting Stars." In the former paper, the observer (the Rev. Thomas Knox) reduced his observations to a pictorial state, by drawing upon paper from a fixed point several lines to represent as many directions of the wind; and this done, he set off upon each of these lines, measuring from the fixed point, a length proportional to the quantity of rain which had fallen while the wind was in that particular direction, and within certain limits on each side of it. Perhaps this will be plainer by a figure.



From O draw eight lines, making angles of 45° with each other, to represent the directions of the wind according to the letters placed at their extremities. From O set off OB to represent the quantity of rain which fell, not only when the wind was in the north, but also when it was in any direction between the points N.N.E. and N.N.W. On a similar plan set off the other portions OC, OD, … . This was the method employed by Mr. Knox; and a similar principle, I believe, was made use of by M. Gravier† in reducing to paper

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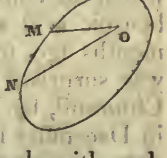
* Communicated by the Author.

† A notice of M. Gravier's researches on Shooting Stars will be found among the miscellaneous articles of the present Number.—Ed.

the numerical laws of the falling stars which appeared in the various quarters of the heavens. The remarks, therefore, which I have to offer apply equally to both those papers; and indeed to all others in which the method of polar co-ordinates is employed. In forming a diagram in the manner just described, three objects seem to be proposed; to discover the mean result, by drawing a curve line through the points A, B, C, D, ...; from the inspection of such a curve to conjecture the general laws of the phænomenon; and to be able to interpolate results. These objects are gained in perfection when rectangular co-ordinates are used, but not so when polar co-ordinates are employed. Now when the experiments have a relation to directions estimated about a point, and to magnitudes depending upon those directions, polar co-ordinates have a natural claim to be employed: but when used, as Mr. Knox and M. Gravier used them, there is a peculiar disadvantage, which it is desirable to remove, especially as the remedy is extremely simple. In Mr. Knox's diagram, if a radius-vector be drawn at random, it will represent the mean quantity of rain which fell while the wind's direction was within an angular distance of $22\frac{1}{2}°$ on each side of that radius-vector. It is therefore impossible to see by inspection how much rain fell while the wind's direction lay within two proposed directions, unless they happen to include exactly an angle of 45°. It must be acknowledged that this is a great defect. The remedy is as follows:—Instead of the lines, set off from the fixed point, being taken proportional to the quantities to be recorded, they should be taken proportional to *the square roots* of those quantities. If this be done, the figure, drawn through the points so obtained, has this property,—the *area* included between *any two* radii-vectores represents to the eye the whole result corresponding to *all* directions included between those two radii. For example; in Mr. Knox's experiments, OA, OB, OC, OD ..., being set off proportional to the square roots of the corresponding quantities of rain; and a curve (as in the annexed diagram) being drawn through the points A, B, C, D ...; if we now draw *any* two radii, OM, ON, making any angle with each other, the area MON will represent the quantity of rain which fell when the direction of the wind lay between MO and NO. In M. Gravier's experiments, where number of stars takes the place of inches of rain, the area MON represents the number of stars which fell in that portion of the heavens which is included between two planes, one of which passes through MO and the observer's zenith, and the other



through NO and the zenith. [Perhaps I may be allowed to suggest, that it would be an advantage to some of M. Gravier's results, were they reduced so as to have reference either to the pole of the earth, or to the magnetic pole as a zenith point.]

Through the medium of your valuble Journal, I desire to obtain for this communication the attention of men of science who are engaged in inferring from observations the general laws of phænomena. The advantage of the method I have proposed will be immediately seen by every person who will give it a trial; I shall not, therefore, occupy your space by any further statement of its superiority, but conclude with remarking, that unless the plan I have proposed be followed, the method of polar co-ordinates is inferior to that of rectangular co-ordinates, and is deprived of some of the advantages which it can be made to possess.

Cambridge, June 27, 1845.

XXII. *On the Action of Chlorine on Cinnamic and Benzoic Acids.* By JOHN STENHOUSE, *Ph.D.*[*]

IN a short paper "On the Action of Peroxide of Lead on Cinnamic Acid," published in vol. xix. of the Philosophical Magazine so long ago as the year 1841, I have incidentally mentioned that "when cinnamic acid is boiled with a solution of hypochlorite of lime, it is converted into benzoic acid with the formation of an oily liquid, which however is not oil of bitter almonds, as its taste and smell are quite different and much more aromatic." The extremely small quantity in which I at that time obtained this oily liquid unfortunately prevented me from being able to subject it to examination. As I was subsequently led to doubt if the acid into which the cinnamic acid had been decomposed was really the benzoic acid as I then imagined, I was induced, nearly six months ago, to resume the subject. While thus engaged, I was surprised to find the following passage in a paper on Chloranil, by Dr. Hofmann, read before the Chemical Society in December last:—"Neither phloridzine, phloretine, cumarine, nor cinnamic acid are transformable into chloranil. The first three furnish, by the action of chlorate of potash, yellow resinous bodies insoluble in water but soluble in alcohol, while the last is converted into a *colourless oil*, to which I shall recur in a future paper[†]."

From the perusal of the above extract I was immediately led to suspect that Dr. Hofmann had, by a different process,

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[*] Communicated by the Author.

[†] See Phil. Mag. S. 3. vol. xxvi. p. 205.

obtained the oil to which I have already referred; and on repeating his experiment I was soon convinced that the oils produced by acting on cinnamic acid either with hypochlorite of lime or with chlorate of potash and muriatic acid are identical. I may mention that the cinnamic acid on which I operated was prepared from liquid storax by the following process:—The storax was boiled for a couple of days with a very strong alkaline lye. The alkaline liquor when cold was diluted with a considerable quantity of water, which precipitated most of the resin which it held in solution, and it was then decomposed by an excess of muriatic acid. The cinnamic acid precipitated in a very impure state, being mixed with a large quantity of resin.

If the alkaline liquor is neutralized while hot, this resinous matter melts and aggregates into large masses, which inclose much cinnamic acid, which is not easily separated from them, as they are not readily acted on by water. The cinnamic acid was purified by repeated crystallizations; the resin of the storax which had escaped decomposition during the first operation was again digested with an alkali, and the process above described repeated so long as it continued to yield any cinnamic acid. Cinnamic acid is usually prepared from balsam of Peru by means of an alcoholic solution of potash; balsam of Peru however is very difficult to decompose, does not yield quite so much cinnamic acid, and is more than twice the price of liquid storax, which is therefore by far the more œconomical source of that acid.

Action of Hypochlorite of Lime on Cinnamic Acid.

When a quantity of cinnamic acid is dissolved in a saturated solution of hypochlorite of lime and subjected to distillation, a violent effervescence ensues, owing to the escape of carbonic acid gas, and a quantity of oil mixed with some acid and water passes over into the receiver. This oil is heavier than water, and has a very peculiar aromatic smell, intermediate between that of bitter almonds and *Spirea ulmaria*, though not exactly like either of them. Its taste is sharp and burning, pretty closely resembling that of cress seed. The oil was first washed with water to remove adhering acid. It was then left to stand for some days over a mixture of fused chloride of calcium and fragments of unslaked lime, to free it from moisture and muriatic acid; when poured off this mixture, the oil was strongly alkaline. It was then very cautiously distilled: the first small portion which came over was colourless and neutral, but what followed became more and more acid, the last portions exceedingly so, evolving fumes of muriatic acid and assuming a deep yellow colour. This operation was several times repeated with similar results. In this respect the oil very closely resembles chlorbenzine, which also cannot be distilled without being more or less decomposed. As the oil was always partially decomposed when distilled off fused chloride of calcium, another portion of it was repeatedly rectified with the vapour of water. The oil then came over quite colourless and neutral. This operation also freed it from a quantity of resinous matter which very readily forms in it. The purified oil when freed from water as well as possible was still more completely dried by being kept for some weeks over sulphuric acid *in vacuo*. When heated it readily catches fire and burns with a green-coloured flame, and emits fumes of muriatic acid gas. When heated with caustic potash, it is partially decomposed with the formation of chloride of potassium. When the oil is treated with strong nitric acid, it is readily attacked with copious evolution of deutoxide of nitrogen; and on the cooling of the liquid the oil is converted into a crystalline mass. The acid which is formed contains nitrogen and crystallizes readily; it is very soluble both in water and in alcohol; it forms soluble salts with the alkalies, and when neutralized it causes no precipitate in solutions of lime or silver salts. I have subjected portions of the chlorine oil, prepared at different times, to numerous analyses, from the results of which I am induced to believe that it is a carburetted hydrogen in which variable quantities of the hydrogen are replaced by chlorine. As the oil is a neutral body, I have been unable to determine its atomic weight, but I expect that the examination of the acid which it forms with nitric acid will throw some light upon this point. I am at present occupied with this subject, the results of which I expect to communicate very soon. It has been already stated that the chlorine oil may also be readily procured by digesting cinnamic acid with chlorate of potash and muriatic acid. It is also invariably formed when a stream of chlorine gas is passed through a hot solution of cinnamic acid; so that there are three different methods by which it can be procured. The production of this oil forms an excellent test for the presence of cinnamic acid.

When either salicine or phloridzine are digested with a solution of hypochlorite of lime, carbonate of lime and resinous substances also united to lime are produced, but neither chloranil nor any oily or crystalline compounds.

Examination of the Acids formed by the Action of Hypochlorite of Lime on Cinnamic and Benzoic Acids.

After the mixture of hypochlorite of lime and cinnamic acid had ceased to yield oil to any considerable extent, it was transferred into a flask and boiled with a large additional quantity of a solution of bleaching powder, so as to ensure the complete decomposition of the cinnamic acid. The boiling was continued till every trace of the oil had been removed. A sediment slowly formed in the liquor consisting of carbonate of lime and resinous matter united to lime: the liquid was filtered in order to separate this precipitate: the clear liquid contained a soluble salt of lime united to an organic crystallizable acid. The lime-salt is exceedingly soluble, but does not crystallize, and forms, when concentrated, a shining transparent pellicle on the surface of the liquor. The salt was decomposed by an excess of muriatic acid, and digested till all the chlorine was driven off. As the liquor cooled, the acid precipitated in white voluminous flocks; these were collected and washed with cold water to remove adhering muriatic acid. The crystals usually contain a little adhering resinous matter, from which they may be easily purified by repeated crystallizations. When the acid is heated in a quantity of water too small for its complete solution, it melts into an oily liquid, and in this respect differs from benzoic acid and resembles cinnamic acid: cold water dissolves very little of it, but it is tolerably soluble in boiling water, especially when the boiling is continued for some time. It is much less soluble than benzoic acid, however, and a good deal of it remains dissolved in the mother liquors. The acid dissolves very readily, both in alcohol and in æther. Water precipitates it from its alcoholic solution. When left to spontaneous evaporation, it readily crystallizes in small needles, which cross each other and form stars; they have a silky lustre. Its smell is disagreeable when it is impure, a little resembling that of naphthaline, but it diminishes as the acid grows purer, and at last wholly ceases: its taste is sharp and bitter, and affects the throat like benzoic acid. When gently heated, it melts and sublimes in crystals; when very highly heated, it catches fire and burns with a greenish flame, emitting much smoke. This led me to suspect that the acid contained chlorine, and on neutralizing a little of it with potash, treating the salt destructively, neutralizing it with nitric acid and testing it with nitrate of silver, I obtained a copious precipitate of chloride of silver. The acid itself produces no precipitate in a solution of nitrate of silver, but when it is neutralized by an alkali, a white flocculent precipitate appears. This precipitate is pretty soluble in boiling water, though much less so than benzoate of silver, but I was unable to obtain it in crystals. The acid gives no precipitate with acetate of lead, but a pretty dense precipitate with the subacetate. Several portions of the purified acid prepared at different times by treating cinnamic acid with hypochlorite of lime, were dried at 212° and subjected to analysis.

(1.) 0·2011 grm. substance burned with chromate of lead gave 0·5085 carbonic acid and 0·0905 water.

Found.	
Carbon .	68·96
Hydrogen	4·99
Oxygen .	26·05
	100·00

Almost pure benzoic acid.

Benzoic acid calculated.		
14 Carbon.	1050	68·85
6 Hydrogen	75	4·91
4 Oxygen .	400	26·24
	1525	

(2.) *a.* 0·2455 gave 0·573 CO_2 and 0·095 HO.

b. 0·2332 gave 0·086 Ag Cl.

Found.	
Carbon .	63·65
Hydrogen	4·29
Chlorine .	9·09
Oxygen .	22·97
	100·00

Acid between benzoic acid and the acid with one equivalent of chlorine.

Formula of acid with 1 equiv. of chlorine.

$$C_{14}\genfrac{}{}{0pt}{}{H_4}{Cl}O_3, HO.$$

14 Carbon .	1050·0	53·70
5 Hydrogen	62·5	3·19
1 Chlorine	442·6	22·63
4 Oxygen .	400·0	20·48
	1955·1	100·00

(3.) 0·2495 gave 0·471 CO_2 and 0·067 HO.

Found.	
Carbon .	51·48
Hydrogen	2·98
Cl and O	45·54
	100·00

Acid with between one and two equivalents of chlorine.

Formula of acid with 2 equiv. of chlorine.

$$C_{14}\genfrac{}{}{0pt}{}{H_3}{Cl_2}O_3, HO.$$

14 Carbon .	1050·0	44·02
4 Hydrogen	50·0	2·09
2 Chlorine	885·2	37·01
4 Oxygen .	400·0	16·88
		100·00

(4.) I. 0·3293 gave 0·5423 CO_2 and 0·0667 HO.

II. 0·2892 gave 0·477 CO_2 and 0·062 HO.

	I.	II.
C . . .	44·91	44·98
H . . .	2·28	2·38
Cl and O	52·81	52·64
	100·00	100·00

Corresponding to the acid with two equivalents of chlorine.

Formula of acid with two equiv. of chlorine.

		Calculated.
14 C	1050·0	44·02
4 H	50·0	2·09
2 Cl	885·2	37·01
4 O	400·0	16·88
		100·00

(5.) *a.* 0·393 silver salt gave 0·449 CO^2 and 0·064 HO.
b. 0·5025 gave 0·268 Ag Cl.

	Found.	Formula of silver salt with 1 at. chlorine.			
C . .	31·16	14 at.	Carbon	1050·	31·87
H . .	1·80	4 ...	Hydrogen	50·	1·31
AgO .	43·14	1 ...	Chlorine . .	442·6	13·43
Cl & O	23·90	3 ...	Oxygen.	300·0	9·13
	100·00	1 ...	AgO	1451·6	44·06
				3294·2	100·00

Corresponding very nearly with the silver salt with one equivalent of chlorine.

The first portion of the acid analysed was treated for a short time only with a comparatively small quantity of hypochlorite of lime; it resembled benzoic acid, and differed very considerably in its properties from the other portions of acid above described. In fact, as is evident from the results of its analysis, it was nearly pure benzoic acid, and contained scarcely a trace of chlorine, as I ascertained by direct experiment. The 2nd, 3rd, 4th and 5th portions of acid had been repeatedly treated with large quantities of hypochlorite of lime and muriatic acid, some of them five and six times successively; and their analysis showed that their carbon, hydrogen and oxygen had diminished, and the amount of chlorine they contained increased in a corresponding proportion, thus forming a series of chlorinated acids in which one and two atoms of hydrogen are replaced by similar equivalents of chlorine. It is evident, as has been already observed, that the acid, the analysis of which stands first in order, is almost pure benzoic acid, the hydrogen of which has been scarcely at all attacked by the chlorine. The action of chlorine on cinnamic acid, therefore, is undoubtedly at first confined to converting it into the oily chlorine compound so often mentioned, and into benzoic acid; so that if its action could be interrupted exactly when this had been effected, these would be the only products. This precise period it is extremely difficult to hit, as the chlorine proceeds immediately to attack the benzoic acid and to replace one, two, and as we will by and by see, so many as three equivalents of its hydrogen, thus forming a series of three distinct chlorinated acids. It is extremely difficult to obtain any one of these acids quite free from admixture, either of undecomposed benzoic acid on the one hand, or of more highly chlorinated acids on the other. The way in which this is most nearly accomplished, is by means of the silver salts, which are much less soluble than the benzoate of silver, and which consequently remains dissolved in their mother liquors when pretty dilute. The action of a mixture of chlorate of potash and muriatic acid on cinnamic acid is precisely similar to that of hypochlorite of lime, viz. the production of the oily compound, and subsequently of some of the series of chlorinated acids. As stated in a previous part of this paper, it is not necessary to have the chlorine in a nascent state, in order to effect the decomposition of the cinnamic acid. For if a stream of chlorine gas is sent through a hot solution of cinnamic acid, the chlorine oil and benzoic acid are immediately formed; and if the action of the chlorine is continued, the hydrogen of the benzoic acid is replaced just as in the cases already detailed.

There is a striking analogy, therefore, between the action of chlorine and of nitric acid upon cinnamic acid, the first effect of nitric acid upon cinnamic acid being confined to converting it into benzoic acid and oil of bitter almonds; but if the action of the nitric acid is pushed still further, the benzoic acid is also decomposed and converted into nitro-benzoic acid.

From what we have already stated, it was to be expected that in order to obtain these chlorinated acids, it would not be necessary to employ cinnamic acid, but that they would also be formed by acting directly upon benzoic acid itself, which is a much more easily procurable substance than cinnamic acid, and therefore a much more convenient source of these acids. This I found to be the case. Of course the benzoic acid gave no oil, but only the chlorinated acids.

The following are some of the results of chlorine on benzoic acid applied in the three several ways already described:—

A. Chlorobenzoic acids produced from benzoic acid by means of hypochlorite of lime and muriatic acid.

I. *a.* 0·6092 substance burned with chromate of lead gave 0·990 CO_2 and 0·114 HO.

b. 0·4375 burned with hydrate of lime gave 0·6175 chloride of silver.

c. 0·3845 of the silver salt gave 0·186 chloride of silver.

d. 0·3425 of the silver salt gave 0·3525 carbonic acid and 0·034 water.

II. Another portion of benzoic acid treated still more frequently with hypochlorite of lime than the preceding.

0·3435 substance gave 0·5095 CO_2 and 0·610 HO.

III. Acid which had been still longer treated than No. II.

0·3572 substance gave 0·510 CO_2 and 0·051 HO.

B. Chlorobenzoic acid obtained by acting on benzoic acid by a mixture of chlorate of potash and muriatic acid.

IV. 0·2066 substance gave 0·324 CO_2 and 0·036 HO.

C. V. Acid obtained by means of chlorine gas sent through a hot solution of benzoic acid for about 56 hours.

a. 0·3334 substance, 0·6675 carbonic acid, and 0·097 HO.

b. 0·2068 substance, 0·4165 CO_2, and 0·0665 HO.

c. 0·3522 substance gave with hydrate of lime 0·3125 chloride of silver.

d. 0·8098 of the silver salt gave 0·9395 CO_2 and 0·1095 HO.

e. 0·414 silver salt gave 0·223 chloride of silver.

VI. Acid through a solution of which a stream of chlorine gas was passed for seventy hours.

a. 0·4158 substance gave 0·8053 CO_2 and 0·119 HO.

b. 0·336 of the silver salt gave 0·1775 chloride of silver.

The following are the determinations per cent.

	I.	II.	III.	IV.	V.			VI.
	a and *b*				*a.*	*b.*	*c.*	
C	44·32	40·45	38·93	42·77	54·60	54·92		52·82
H	2·07	1·97	1·58	1·93	3·23	3·57		3·18
Cl	34·82	57·58	59·49	55·30	42·17	41·51	21·29	44·00
O	18·79							
	100·00	100·00	100·00	100·00	100·00	100·00	100·00	100·00

Silver salts.

	I.	V.	VI.
	c and *d*.	*d* and *e*.	
C . .	28·06	31·64	
H . .	1·10	1·50	
AgO .	39·13	43·57	42·73
Cl and O	31·71	23·29	
	100·00	100·00	

It will be observed by reference to the formulæ given in a previous part of the paper, that the acid A. obtained by hypochlorite of lime corresponds to the acid with two equivalents of chlorine. No. II., which was treated with still more hypochlorite of lime, gave a mixture containing acids with two and three equivalents of chlorine; and No. III., in which the treatment was carried still further, corresponds to an acid with nearly three equivalents of chlorine.

	Calculated.
14 Carbon . .	37·29
3 Hydrogen .	1·33
3 Chlorine . .	47·16
4 Oxygen . .	14·22
	100·00

B. The acids obtained by means of chlorate of potash is a mixture of acids containing two and three equivalents of chlorine.

C. marked V. is an acid containing one equivalent of chlorine; and the silver salt marked VI. is a mixture of acids containing one and two equivalents of chlorine.

Though several attempts were made, I did not succeed in obtaining any of the salts of these acids in distinct crystals. When boiled with a mixture of muriatic acid and alcohol, each of these acids formed an æther, which in its smell and other properties closely resembled that of benzoic acid.

It is stated in most systems of chemistry, that chlorine has no action on solutions of benzoic acid, but from the facts already adduced, it is evident that this opinion is very incorrect. It would be unjust to conclude without referring to the experiments of M. Herzog, detailed in Berzelius's Report for 1842, p. 107, Paris edition. M. Herzog says, "that when dry benzoic acid is exposed to the action of chlorine under the influence of solar light, the gas is absorbed and the acid is transformed into a reddish humid mass, having a disagreeable and fishy smell. When this is treated with carbonate of soda, it forms a brownish-red solution, while a resinous body having the smell of benzoin remains undissolved. When the alkaline solution is neutralized with nitric acid, a crystalline acid containing chlorine precipitates." The amount of the chlorine contained in it he did not determine. M. Hertzog also mentions that cinnamic acid is decomposed by chlorine in a similar way, yielding a chlorinated cinnamic acid. He makes no reference to the formation of any oil, nor does he appear to have attempted an analysis of any of these compounds whatever.

Glasgow, June 9, 1845.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Anniversary Meeting, Nov. 30, 1844.

THE Marquis of Northampton in the Chair.

The noble President stated that the two Royal Medals had been adjudged by the Council to Mr. Boole, for his important papers on a General Method of Analysis*; to Dr. Andrews of Belfast, for his valuable paper on the Thermal Changes accompanying Basic Substitutions†; and the Copley Medal to Signor Matteucci of Pisa, for his discoveries in the magnetic electricity of animal nature.

The President then presented the Medals; after which he proceeded to state that the Society had lost some distinguished mem-

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* An abstract of this paper will be found in vol. xxiv. p. 459.—Ed.

† Abstracts of Dr. Andrews's researches will be found in vol. xix. p. 183; vol. xxiv. p. 457; vol. xxv. p. 93, of this Journal.—Ed.

bers; among whom were Sir Henry Halford, who long presided over the College of Physicians; and Dr. Hope, who for many years was Professor of Chemistry in the University of Edinburgh. "We have still more immediate reason," his Lordship added, "to lament the decease of one of our ablest and most zealous colleagues, the late Mr. Baily, who had always taken an active share in the business as well as in the scientific pursuits of the Royal Society*. We have also to deplore the death, at a venerable age, of Dr. Dalton of Manchester, whose eminent discoveries have so largely contributed to our chemical knowledge and to the scientific reputation of England."

The Address contains obituary notices of deceased Fellows of the Society, from which we select the following:—

DR. THOMAS CHARLES HOPE was the son of Dr. John Hope, Professor of Botany in the University of Edinburgh, and was born at Edinburgh on the 21st of July 1766.

His devotion to chemical science and his recognition as a chemist date from an early period of his life; for he was, on the death of Dr. Irvine, appointed Lecturer on Chemistry at Glasgow on the 10th of October 1787, while yet in his twenty-first year. He was further, in 1789, appointed Professor of Medicine in the same university, conjointly with his uncle Dr. Stevenson. It does not appear, however, that he had actually delivered lectures on either subject in Glasgow until 1793; he most probably passed the interval in studying at home and abroad, for we are informed that he returned from France in 1791, and he thereafter continued to lecture at Glasgow until 1795. At this period, he received the distinguished compliment of being recommended by Dr. Black as his assistant and successor in the chemical chair at Edinburgh. Accordingly, in 1795, Dr. Hope entered on his new duties by delivering a course conjointly with Dr. Black, whose decaying powers permitted him only to deliver the lectures on Caloric. In this, as well as in the courses of chemistry which he delivered in Glasgow, Dr. Hope taught the then recent doctrines of Lavoisier, which had not yet entirely overthrown the doctrine of phlogiston, and had not previously been publicly taught by any professor in Britain.

Dr. Hope's exertions during his residence at Glasgow had not been limited to writing and improving his lectures. On the 4th of November 1793, he read to the Royal Society of Edinburgh his well-known paper, "On a mineral from Strontian," in which he pointed out the existence of an undescribed earth, distinct from barytes, with which it had been confounded, and to which he gave the name of *Strontites*.

In 1803, in the 6th volume of Nicholson's Journal, a brief notice was published of the instrument with which Dr. Hope employed a solution of sulphuret of potassium for eudiometrical purposes; and

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* The admirable memoir of our much-valued friend and correspondent the late Mr. Francis Baily, from the pen of Sir J. Herschel, and read by him at a special meeting of the Astronomical Society, is printed entire in our 26th volume, p. 38.—ED.

in 1804, he laid before the Royal Society of Edinburgh the careful and elegant experiments, by which he demonstrated that the proposition laid down by Dr. Croune in 1683 regarding the expansion of water by cold is really correct; and that the phenomena observed do not depend, as Hooke and others had maintained, on changes occurring in the capacity of the vessel in which the experiment is conducted. From these researches, Dr. Hope concluded that the maximum density of water is at 39½° or 40° Fahr.; a result confirmed by the more recent and very accurate experiments of Hällström, who estimates its point of greatest density at 39·39° Fahr. This, his favourite subject, received a still further elucidation from him at a later period of his life. In 1839 he read to the Royal Society of Edinburgh a paper, in which he showed that this law held true with regard to water only in its pure state; and that when it contains saline matters in solution, as in sea-water, it follows the ordinary law of regularly progressive expansion by heat and contraction by cold.

The last communication which Dr. Hope made to the chemical world was in 1843, when he read two papers on the colouring matter of the *Camellia japonica*, *Magnolia grandiflora*, and *Chrysanthemum leucanthemum*. In these communications, he asserted the existence of three new proximate organic bodies, to which he gave the names of *Camelline*, *Magnoline* and *Leucanthemine*, but which he did not obtain in a separate form.

Dr. Hope's claims on the gratitude of the chemical world are not to be estimated by his merits as an observer. He seems, from the first, to have resolved to devote himself rather to the business of teaching than of research. He spared no pains to make his lectures attractive to the student; his style of writing was elegant and concise, and his delivery easy but impressive. The great charm of his lectures, however, consisted in the elegant experiments with which he illustrated them: these were so well-devised, so skilfully arranged and so neatly performed, that a failure on his lecture-table was an occurrence almost unknown. One other circumstance which contributed to his success as a lecturer was the excellent health which he enjoyed. During a period of nearly sixty years, in which he was engaged in studying or teaching medicine and chemistry, he was not more than six days in all prevented by illness from discharging his duties. He retained his vigour and his faculties with little diminution till within a short period of his death; but in 1843, finding himself unequal to the duties of his class, he arranged with his colleague Dr. Traill to deliver the lectures for him, and shortly after resigned the appointment into the hands of the patrons.

Dr. Gregory was appointed to the vacant chair, and before he had entered on its duties his veteran predecessor died at Edinburgh on the 13th of June 1844, in the 78th year of his age.

John Dalton was born on the 5th of September, 1766, at Eaglesfield, near Cockermouth, in the Cumberland Lake district. He passed some years as teacher of mathematics in a school at Kendal, and removed in 1793 to Manchester, where he continued to reside during the whole of his after-life.

It was doubtless his long residence among the lakes and mountains of Cumberland, and his consequent early familiarity with the ever-varying conditions of the atmosphere observable in that district, that gave the first impulse to his genius, and materially influenced his subsequent scientific career. His earliest important publication was a Treatise on Meteorology, which furnished a clear compendium of all the facts then ascertained, and made known various original views, especially on the altitude of the Aurora Borealis. From observing and recording the sensible atmospheric changes, the phænomena of dew, of clouds and of temperature, he was naturally impelled to inquire into the constitution of the atmosphere, and more generally of mixed elastic fluids, and into the theory of evaporation and the laws of heat. On these questions he made public, through the Transactions of the Manchester Society, a series of experimental memoirs, of which it is impossible to over-estimate the importance. His first conception of the mutual relations of mixed gases was, that each gas stood in the relation of a vacuum to the particles of all other gases; but in his New System of Chemical Philosophy he subsequently relaxed the strictness of this original proposition, by conceding that the particles presented some mechanical impediment to commingling. He ascertained the form of the vapours of water and some other liquids at different temperatures, and dispelled by these experiments and others of equal importance the obscurity in which the theory of vaporization had been left by De Luc and Saussure. He first showed that a given space, whether void or filled with any gas, in contact with water, contains precisely the same amount of aqueous vapour, and thus established the non-existence of chemical affinity between the gas and the steam of water. It is impossible not to be impressed with the beautiful simplicity of the instruments by which these important results were wrought out. Four barometric tubes, filled with mercury, over which were admitted small columns of water, alcohol, ether and sulphuret of carbon, were the means employed for the admeasurement of the comparative forms of the vapours at different atmospheric or artificial temperatures. Among these successive memoirs is one of great merit on the heat evolved during the entrance of air into a vacuum. He showed the inadequacy of the thermometer to serve as a measure of this evanescent elevation of temperature, and by an ingenious contrivance obtained a much closer approximation to the true heat.

The first part of his New System of Chemical Philosophy will probably constitute the most durable monument of his scientific genius; in this small volume are condensed the results of many years' patient thinking and of much laborious research; those larger portions, which are devoted to the measure of temperature and the theory of specific heat, may still be studied with advantage, though they were deemed by Dr. Dalton himself to have been in great part superseded by the labours of MM. Petit and Dulong. The short concluding chapter contains the first announcement of the atomic doctrine of chemical combination.

He has often expressly stated that the tables of chemical equivalents constructed by Wenzel and Richter first suggested to him the conception that chemical combination must have place between the ultimate particles or indivisible atoms of bodies. The tabulated differences of weight of the different bases required to neutralize a given weight of acid would, on this hypothesis, represent the respective weights of their ultimate atoms. Further evidence of more decisive character presented itself in the instances in which one body combines with another in more than one proportion. The successive combining quantities were ascertained to be represented by numbers that were simple multiples of the smallest or lowest quantity. Dr. Dalton's earliest illustration of his law of multiple proportions was derived from the gaseous compounds of oxygen and nitrogen. Dr. Wollaston afterwards discovered other examples of the law in the tartrates and oxalates, and M. Gay Lussac's precise experiments on gaseous combination completed the chain of evidence. All the phænomena of inorganic chemistry have been since shown to be in strict accordance with the atomic hypothesis, which has banished the uncertainty of conflicting results, by enabling the experimentalist to anticipate and correct his analyses, and has thus raised chemistry, as respects numerical precision, almost to the rank of a mathematical science.

It would be inconsistent with the principles of logical induction to claim for the atomic doctrine higher rank than that of the most convenient form of expressing and recording chemical phænomena, and of the most probable hypothesis that has been hitherto proposed for interpreting chemical combination. In the field of organic analysis, which has of late years been laboured with signal success, rules of combination seem to obtain which are difficultly reconcileable with the doctrine of Dalton. It is scarcely possible to conceive the mechanical juxtaposition of so large a number of elementary atoms as would appear to constitute one compound organic atom; there are consequently many among the cultivators of this branch of chemical science who refuse to accept the atomic hypothesis as now constituting a sufficient generalization of established facts. Yet even in the chemistry of vegetable substances, the remarkable changes discovered by Mitscherlich, which he conceives best explained by the union of propejacent atoms of hydrogen and oxygen, and their elimination in the form of water, minister strong support to the theory of atomic combination. Without venturing to anticipate the future destinies of the hypothesis of Dalton, it is sufficient for the fame of its author to acknowledge the mighty impulse it has given to the progress of chemical knowledge.

Dr. Dalton died on the 27th of July, 1844.

THOMAS HENDERSON, Professor of Practical Astronomy in the University of Edinburgh, and Her Majesty's Astronomer for Scotland*.

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* A full memoir of Professor Henderson, from the Proceedings of the Astronomical Society, will be found at p. 60 of the present volume.—ED.

William Allen was the son of Mr. Job Allen, a silk-manufacturer of Spitalfields, and was born in London on the 29th of August 1770. His father being of the Society of Friends, he was strictly educated in the principles of that religious community, of which he continued till his death an exemplary and distinguished member. The early preference evinced by him for chemistry induced his father to place him in an establishment of high repute, of which, some years after, he became the proprietor. But happily for suffering humanity, neither commercial gains nor the love of science itself could hold exclusive possession of his mind. From an early period of his life he co-operated with Clarkson, Wilberforce and other philanthropists in their efforts for the abolition of the African slave-trade, which were happily at length successful.

Mr. Allen's connexion with the schools of chemistry and natural philosophy in Guy's Hospital commenced in 1803, and continued till 1826: Mr. Allen's ability and fitness as a teacher of science were there amply attested.

In 1804, Mr. Allen read his first course of lectures on natural philosophy in the Royal Institution. The valuable researches on carbon, carbonic acid, and the changes effected in atmospheric air by respiration, made by Mr. Allen in conjunction with Mr. Pepys, are too well known to require remark. The results are to be found in our Transactions for 1807, 1808, 1809. Mr. Allen was in 1807 admitted a Fellow of the Royal Society. He also became a member of other scientific bodies of this country and of the continent.

It was, however, to other objects of public utility that the greater part of Mr. Allen's life was devoted. The education of the poor on christian principles, and the circulation of the holy scriptures, were among the first to which he directed his efforts; and wherever he went the moral improvement of his fellow-creatures occupied his attention. He was at all times ready to cooperate with the good of every creed whose aim was the happiness of mankind, and probably no man of his generation lived to see nobler fruits of his labours*.

Having been one of the founders of the British and Foreign School Society, he was, in 1808, elected its first treasurer, an office which he held until his death. To this institution he liberally contributed not only his time, but also large pecuniary assistance. At Lindfield, in Sussex, he expended large sums in building schools of industry, and laboured assiduously by other plans for bettering the condition of the poor. Among these the system of allotments of land found him a zealous patron.

In 1818–19 Mr. Allen visited Russia, where, in the reign of the Emperor Alexander, he, in conjunction with two friends, compiled a volume of scriptural selections, for the instruction of youth in the

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* [Among the laudable objects to which Mr. Allen's efforts were devoted, and of which he lived to see the fruits, the abolition of capital punishments was one of the most important, although it has not been mentioned in the notices which have been published of his useful life.

For several years a committee, of which the writer of this note was a member, for assisting Sir S. Romilly and others in the pursuit of this object, held its meetings regularly at Mr. Allen's house, in Plough Court.—R.T.]

military and other schools of that empire. Mr. Allen made several other journeys for benevolent objects to the continent, and he greatly exerted himself in behalf of the Greeks and of the Waldenses of Piedmont.

The source of Mr. Allen's extraordinary influence was to be found in that singleness of purpose, which, breaking down the obstacles of party, gave him access to those in power, and carried him as by a royal road to the confidence and favour of princes. At home we find him enjoying the marked consideration of their late Royal Highnesses the Dukes of Kent and Sussex, the former appointing him one of his executors; and abroad he was in frequent communication with the Emperor Alexander.

Mr. Allen possessed in an uncommon degree the power of giving his mind to a great variety of subjects, and this, together with his activity, economy of time and early habits, enabled him to accomplish a prodigious amount of good. His correspondence alone was a labour from which most men would have shrunk.

In addition to subscriptions and donations to public charities, the casual calls on Mr. Allen's benevolence were very heavy; he also gave cheerfully, and there are perhaps few great works which he did not at some period of his life assist. The extent of Mr. Allen's charity seems to have been widely known, for we find the Emperor Alexander urging it as a reason for his undertaking the supply of drugs for the Russian armies, an offer which he gratefully but firmly refused, and to his honour be it spoken, he resisted a temptation the value of which it would be difficult to estimate. At the end of a long life he could say that he had never compromised his public usefulness for private interests.

During the last fourteen years of his active life, Mr. Allen had passed about half his time at Lindfield, diffusing happiness and doing good to those about him. The review of a well-spent life gave comfort to the evening of his days, and that gospel which had been his rule through life was his stay in death, sustaining his mind in serenity and peace.

He died at Lindfield on the 30th of December, 1843, aged 73.

The following Noblemen and Gentlemen were elected Officers and Council for the ensuing year, viz.—

President.—The Marquis of Northampton. *Treasurer.*—Sir John William Lubbock, Bart., M.A. *Secretaries.*—Peter Mark Roget, M.D., Samuel Hunter Christie, Esq., M.A. *Foreign Secretary.*—John Frederic Daniell, Esq., D.C.L. (since deceased) *Other Members of the Council.*—John Bostock, M.D.; William Bowman, Esq.; Isambard Kingdom Brunel, Esq.; Rev. William Buckland, D.D.; Sir William Burnett, M.D., K.C.H.; George Dollond, Esq.; Very Rev. Dean of Ely; Thomas Graham, Esq., M.A.; Roderick Impey Murchison, Esq.; Richard Owen, Esq.; Sir James Clark Ross, Capt. R.N.; John Forbes Royle, M.D.; William Sharpey, M.D.; John Taylor, Esq.; Rev. Robert Walker, M.A.; Lord Wrottesley.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 79.]

February 14, 1845.—*Extracts from the Report of the Council of the Astronomical Society to the Annual General Meeting.*

The Royal Observatory must occupy a very prominent place in this year's report.

The reduction of the Greenwich planetary observations from 1750 to 1830 was suggested by the British Association at the [first] Cambridge meeting. On the motion of the Board of Visitors of the Royal Observatory, Her Majesty's government undertook to defray the expense of printing, and committed the work to the charge of the Astronomer Royal. All is now done and printed in 671 large quarto pages, except the introduction. There are five sections: I. contains the investigation of clock errors and rates, and computation of mean time, all by stars. II. Investigation (by stars) of index errors of quadrants and circles, and zenith points of circles (the *Tabulæ Regiomontanæ* are the basis of these two sections). III. Geocentric places of the planets inferred from the original observations, and corrected by the elements obtained in I. and II. IV. Computation of the tabular geocentric places of the planets, each from the best existing theory applying to that planet (the four small planets excepted). V. A comparative view of the observed and tabular geocentric places, and an exhibition of the equations which this gives for the heliocentric errors of each planet.

The reduction of the Greenwich lunar observations from 1750 to 1830 was also suggested by the British Association, and has been carried on under the superintendence of the Astronomer Royal at the expense of the government. The reductions being now very nearly completed, the Board of Visitors has recommended to the government to print them with considerable detail, and with actual correction of the elements of the tables. This last is rendered practicable by the deduced results having been uniformly compared with those of Plana's lunar theory (with some emendations). It is understood that an adequate sum is to be inserted in next year's estimates.

It will be perceived that the Royal Observatory is making up its ledger; and future astronomers, who will nearly as soon publish unmade as unreduced observations, will be surprised at the uniform credit which it has maintained during the long period in which it has never investigated the state of its own accounts. The truth is that it has always led the world; and it is not fair to demand of the highest why it is not yet more high. We may now confidently expect new lunar tables, and considerable emendations of the planetary ones. The astronomical world will not fail to bear in mind what it owes to the present Astronomer Royal, who, when at the head of the Cambridge Observatory, first presented a complete volume of reduced observations as part of the regular business of the institution. If, as may reasonably be assumed, the impulse given to astronomy in England by the young exertions of this Society was one of the causes of the foundation of an observatory at Cambridge, we form no trivial wish when we hope that the consequences of the future may equal in importance those of the past.

It is probably known to most of the Fellows, that the government, on the representation of the Astronomer Royal to his Board of Visitors, has sanctioned the erection of an altitude and azimuth instrument for extra-meridional observations of the moon. For more than a century all fundamental observations of stars and planets have been made in the meridian; and if sufficient frequency of observation could be ensured, there is no reason to suppose that any other species of observation could be required. In the case of the moon, however, there is a necessary loss of the meridian observation during a part of the month, and occasional occurrence of clouds at the time of otherwise visible transit; to which it must be added, that theoretical knowledge of our satellite is so advanced as to make it difficult to carry it further by such sets of observations as can be procured with meridional instruments. The Astronomer Royal has therefore determined to commence the task of following the moon through her daily course to an extent which will supply, or more than supply, the failure of meridian opportunities; and your Council feels a sanguine presentiment that this abandonment of the meridian will be an epoch in the history of astronomy. The altitude and azimuth instrument is constructed on the same general principles as the ordnance zenith sector described by Mr. Airy at the meeting of May 13, 1842. It consists of very few parts, and these cast, and therefore rather massive; and so important has it been considered to unite the small parts, that the microscopes are cast in the same piece with the rest, and are bored afterwards. The instrument turns on pivots above and below. The revolving azimuthal frame consists only of four casts—namely, the lower end, with a pivot and four microscopes; the two sides, of which one has four microscopes; and the top, with its pivot. The moving vertical circle consists of two parts only; one having one pivot, the graduated circle, and the two ends of the telescope, the other having the other pivot. The extreme firmness which is required in the construction of a *theodolite* (as it may very properly be called), which shall give results comparable to those of the *fixed instruments*, is of course accompanied by considerable loss of manual adjustment, and consequent increase of arithmetical reduction. Those who have any idea of the very serious labour involved in the contemplated class of observations, as compared with that required in meridional work, will feel that the present Astronomer Royal has dictated to his successors the motto by which Ptolemy described Hipparchus, φιλόπονος καὶ φιλαλήθης; and they will also feel that he has adopted it for his own.

Your Council cannot but feel it to be a matter of congratulation, that the principal public observers of Great Britain have, to no small extent, begun to bear in mind that different observatories, situated at no great distance from each other, should aim at diversified plans of action and independent objects of investigation. While all have recorded the places of the bodies of the solar system, and have lent the assurance of number of observations, and consequent power of detection, to the *data* on which future planetary tables will be constructed, each one has tried, or is trying, to make the greatest progress of some one branch of astronomy peculiarly its own work. Thus, while Greenwich has constantly devoted its peculiar attention, pursuant to the will of its founder, to the moon, the large equatoreal at Cambridge has suggested the researches of the Plumian Professor; the Radcliffe observer at Oxford has devoted his special attention to the circumpolar stars, and will soon be furnished with a new mode of action by the possession of the splendid heliometer which is in preparation; and our lamented colleague at Edinburgh has left the materials for a catalogue of zodiacal stars. Differences of locality, of instruments, of mode of government, of taste and reading in the directors of different observatories, will originate differences of plan; it may be permitted to a body so closely connected with the common pursuit as your Council, and so deeply interested in the astronomical welfare of each and all of these institutions, to hope that these differences of plan will one day arise out of a matured system of co-operation, in which foreign observatories will be combined with our own. Much advantage has arisen in this country from the division of labour, which has thrown the observation of double stars and nebulæ upon the amateur astronomer; and more will always be gained, the larger the amount of strength and the wider the range of researches which are thus judiciously subdivided.

The benefit of co-operation has been lately seen in the junction of the observatories of Pulkowa, Altona, and Greenwich, for the determination of their differences of longitude. Our associate, M. Struve, had connected Pulkowa and Altona; and, having strongly represented to his government the propriety of taking Greenwich as the zero point for all longitudes, it was resolved to connect the two last-named observatories. A portable transit was erected during the last summer in a temporary observatory on the grounds of the Royal Observatory, and forty-two box chronometers were carried backwards and forwards by the steam-boats, eight times each way. At first M. Otto Struve observed at Greenwich, and M. Döllen at Altona, for two voyages: the observers were then reversed for four voyages, and again resumed their old stations for two more; by which arrangement it was hoped to eliminate both personal equation and its gradual changes. The clock in the temporary observatory was regularly compared with the Greenwich transit clock, so that the ordinary observations of the Observatory will contribute to the result: to this end all necessary observations for personal equation were made. The result is not yet completely calculated.

While the preceding operation was in progress, another of a similar kind was undertaken by the Astronomer Royal and Mr. Sheepshanks, for the connexion of Valentia in Ireland (the western point of Europe) and Greenwich; to which was added, the incidental determination of the longitude of Liverpool Observatory and Kingstown Harbour. Valentia is nearly in the latitude of Greenwich, and it will probably be the extremity of an arc of parallel extending across the south of Russia. Mr. Sheepshanks (and afterwards Mr. Hind) was stationed with a portable transit instrument at Kingstown, and thirty pocket chronometers were carried backwards and forwards, eight times each way. The mode of carriage was as follows:—

The chronometers being in two well-padded cases, each containing fifteen, these cases were inserted in boxes which were screwed to the railway carriages and steam-boats (the owners of which gave every possible facility): these boxes were not disturbed throughout the whole operation, each one forming, in fact, a part of a carriage or steam-boat; all had similar locks, and each person employed was furnished with a key. An assistant took the chronometers from Greenwich to the Euston Square station; Mr. Hartnup received them at Liverpool and transferred them to the steam-boat; and Mr. Sheepshanks or Mr. Hind received them at Kingstown. In a similar manner, when a portable transit was erected at a station at Valentia, under the charge of Lieut. Gossett, R.E., the chronometers were carried backwards and forwards, ten times each way, in boxes fixed upon the mail coaches as far as Tralee, and afterwards in an express car, furnished by M. Bianconi.

Several parts of the disputed boundary between the United States and British North America are defined astronomically; and, to prepare for the proper execution of this part, two officers of engineers, Captain Robinson and Lieutenant Pipon, employed under the British Commissioner, Lieut.-Colonel Estcourt, were for some time stationed at the Royal Observatory. There was one point which was not necessarily astronomical, the drawing a straight line of between 60 and 70 miles to connect two defined points. As it appeared almost impossible to effect this by survey, from the difficult character of the country, Mr. Airy recommended that the azimuths at the two ends should be computed from observed latitudes and difference of longitudes, and that two parties should cut through the woods in the assigned directions, one from each terminal station. The two parties thus cutting, independently of each other, drew lines which met within 300 feet.

We have yet to acknowledge one more obligation to the Astronomer Royal, namely, the recent publication of a Catalogue of the Places of 1439 Stars reduced to the 1st of January 1840. This work contains the mean places of stars deduced from all the observations made at Greenwich in the years 1836 to 1841 inclusively. The place of the equinox is that resulting from observations during the same period. The year which corresponds to the mean of the observations of each element is also given; and the annual precession for 1840, with the proper motion for those stars in which account of proper motion has been taken. This necessary information with respect to the mean date was first given, we believe, in the Cambridge Catalogue; and we trust that every future catalogue will also contain it. It is otherwise impossible to investigate proper motion, or the changes in proper motion, with the nicety which the present state of practical astronomy authorises us to apply to these delicate researches. The nomenclature is taken from Baily's 'Flamsteed,' the magnitudes from Argelander's *Uranometria*; and there are columns of references for those stars which occur in the catalogues of Hevelius, Bradley, Mayer, Piazzi, the Astronomical Society, Groombridge, Pond, Argelander, Cambridge (first), Johnson and Taylor. This brief notice needs no further comment here; for it would only be a waste of time to add one word describing the excellence of the instruments employed, the finish and perfection of the reductions, the care of the editor or the utility of the final results. We congratulate all cultivators of astronomy on this noble addition to its *Fundamenta,* the influence of which will be instantly felt in every working observatory, and directly or indirectly throughout every department of the science.

In the Cambridge Observatory Professor Challis has confined himself in a great degree, so far as the meridian instruments are concerned, to the planets and those double stars which have been observed with the Northumberland equatoreal. A first series of observations of double stars is in preparation, and a Second Cambridge Catalogue, in continuation of the one which was inserted by Mr. Airy in our Transactions. The observations of the various recent comets with the equatoreal above-mentioned will be valuable additions to the several yearly volumes.

Your Council have great satisfaction in directing the attention of the Society to the Observatory recently established at Liverpool by the corporation. In accordance with the advice of the Astronomer Royal, the astronomical portion consists of a transit-room and a dome for a large equatoreal. An adjoining apartment is appropriated to the chronometers which are brought there for trial or rating, and to the meteorological instruments; the rest of the building forms a comfortable house for the observer. A transit of five feet focal length and four inches aperture by Simms, a sidereal clock and a mean time clock by Molyneux, and a standard barometer (Newman's construction) by Adie of Liverpool, have been for some months in use, to the perfect satisfaction of Mr. Hartnup, the director. The telescope of the transit is a particularly fine one, and the mounting, which was directed by the Astronomer Royal, is the strongest and stiffest perhaps in existence. With the two clocks (which are within hearing of each other, and which are regularly compared whenever time is got or chronometer errors ascertained) it forms a perfectly efficient apparatus for getting and keeping the time, and is adequate to the most delicate determinations of right ascension. When the equatoreal is completed (the object-glass will have an aperture of eight inches, the mounting is to be under Mr. Airy's superintendence), we may expect most valuable assistance from the Liverpool Observatory in the extra-meridian branch of practical astronomy. But the principal and most interesting object of this establishment is, that of *giving true time to the great port of Liverpool*; an object which is of high national importance, and which has hitherto been almost unaccountably neglected. The observatory is admirably situated for this purpose, on the brink of the Mersey at the entrance to the Waterloo Dock: the horizon is good, and infinitely better than could have been hoped for in the heart of a busy manufacturing town. A ball similar to that at Greenwich is let fall every day except Sunday, precisely at one P.M. Greenwich time, and the whole arrangement is so complete, and the longitude so well known, that the dropping of the balls at the two observatories may be considered to be simultaneous. It is evident that an observatory furnishing exact time will be of the greatest utility to all makers of *good* chronometers, and a hindrance to the venders of those which are indifferent. The ship-owners may, too, if they please, enjoy an advantage hitherto belonging solely to the Admiralty, that of having their chronometers tried and rated by a competent and disinterested party previous to purchase; an advantage which will be thought almost inappreciable by persons fully aware of the dependence of modern navigation on the goodness of timekeepers; and also, what indifferent watches are now disposed of to the unwary by ignorant and unscrupulous dealers. Mr. Hartnup's aid was most efficient in the measurement of the Valentia arc of longitude; and we trust that is only the first of many services to be rendered to science by this zealous and intelligent observer.

The magnificent telescope erected by the Earl of Rosse, which has attracted so much of public attention, is nearly in working order; nothing being now incomplete except some of the gallery machinery. It may reasonably be predicted that the energy which has called this instrument into existence will not quail before the more easy and pleasant task of using it. There is nothing on which it is so difficult to speculate as the probable results of an increase of optical power.

Passing to another extreme, your Council desire to notice the remarkable catalogue published by our associate, M. Argelander, of stars observed with the naked eye. The author is the Bayer of our generation, and his appreciation of magnitudes will probably come into universal use. He has re-opened the road in which the astronomer may make himself useful without any instruments at all. His announcement of nearly thirty stars, which, though visible to the naked eye, are not to be found in any catalogue, startling as it may seem at first, will not perhaps surprise those who remember how various the objects of different catalogues have been. It is most desirable that these stars should be observed with a view to the verification of the fact. If only half the number of new visible stars should be substantiated, it will be a useful lesson in any point of view. It is to be remembered that we are not positively to assume that these stars have been neglected by preceding observers: there is much reason to suppose that such bodies have before now disappeared. There may be *re-appearances*, and there may be *new appearances*. Variable stars, with periods of several hundred years, or even less, might easily give rise to what M. Argelander has observed.

While commemorating the results of the past year, we ought not to forget those who are working in distant lands, under circumstances which prevent our receiving immediate information of their proceedings. Mr. Maclear's verification of Lacaille's arc at the Cape of Good Hope is an operation in which English and French astronomers have each their peculiar interest; and some little account of this arduous and toilsome undertaking will doubtless be interesting to the Society.

With the aid of his assistants and a military detachment placed under his command, Mr. Maclear began, on the 1st of September, 1840, the measurement of a base line, eight miles in length, on the plains of Zwartland. Its direction is about west by south to east by north. The measurement of the first 1600 feet was repeated, and the difference of the results was found to be inappreciable, though the operation was rendered most harassing by the excessive heat. The measurement of the whole line occupied six months, not more than from 500 to 750 feet being completed in one day. Mr. Maclear thinks that there is just ground for believing that the entire base, 42,818 feet in length, is scarcely erroneous to the amount of half an inch, and that the error is probably much less than this.

Thenceforward the trigonometrical survey continued to occupy a large share of Mr. Maclear's attention. In December 1842 he proceeded in H.M.S. Arrow to take the zenith sector up to the Oliphant's River, which was then intended to be the northern limit of the arc, though it was afterwards deemed advisable to extend the triangulation both north and south of Lacaille's original arc; northward to Lily Fontein on the Kamies Berg, which has been accomplished; and southward to Cape l'Agulhas, which is approaching to completion.

This labour has subjected the observers to the greatest privations, to intense heat on the arid Karroos in summer, and to extreme cold in the winter, when they had to ascend such commanding heights as the Cedar Berg, the Snew Cop, the Winter Hock, and the Worcester Range, leaving their wagon or horses far below them, and being consequently reduced to sleep under the open sky, amidst snow and sleet, waiting from day to day, till the weather would admit of flashing their heliotropes towards the concerted points. Mr. Maclear speaks in the highest terms of the zeal of his assistants during the whole of these laborious and trying operations.

The East India Company has lately presented the Society with two valuable volumes, printed by the order of the Madras government. The first, containing the meteorological observations made at the Madras Observatory for twenty years (1822–1843), will find those who can more appropriately discuss its merits than your Council. The second touches us more nearly. It is a Catalogue of 11,015 stars, made from the five volumes of Madras Observations, and includes all of the Astronomical Society's Catalogue and of Piazzi's, which are visible at Madras, together with 3445 southern stars, selected with reference to the Paramatta Catalogue, all reduced to January 1, 1835, about the middle period of the observations. A systematic error of considerable magnitude in the divisions of the mural circle was discovered in 1840, and its amount ascertained for each single division; and every place has been corrected for the error incident to the division in which it was observed. The proper motions of stars which appear to exceed a quarter of a second of space have been tabulated. Of the merits of this Catalogue it would be impossible to give any opinion at present; but your Council need not say that, from what is known of the zeal and industry of Mr. Taylor, they are well prepared to believe that it will soon be characterised as a valuable addition to sidereal astronomy, and an indispensable aid to the southern astronomer.

The progress of Sir John Herschel's reductions of his southern observations must interest the Fellows of this Society. From the state in which they now are, it is not very improbable that the Catalogue may appear in a year from this time. The constant attention which the completion of this great undertaking requires, has prevented Sir John Herschel from taking that share in the business of the Society, from which, previously to his departure for the Cape of Good Hope, its affairs derived so much benefit. The Council hope that the impediment will speedily cease to exist, not more for the sake of the Society than of its distinguished ex-president; for it must be admitted that, in travelling through the numerical reduction of observations, the astronomer finds very few of the flowers with which the love of science may strew the more intellectual part of the road.

By the death of Mr. Baily, the superintendence of the standard measure of length, and of the astronomical catalogues in progress at the expense of the government and of the British Association, pass into other hands. The standard scale has been undertaken by Mr. Sheepshanks, who has obtained from the Council the use of a vaulted room in the basement story in which to make the comparisons.

The Nautical Almanac for 1848, the fifteenth of the new series, appeared in the month of December last. For several years past, the advance which it was judged necessary to provide, namely, four complete years, has been gained; and it must not be forgotten that, when Lieut. Stratford was placed in charge of this national work, there was hardly a year in advance. On looking at the steady and effective manner in which this arduous undertaking has been executed, your Council feel that their praise is needless. With regard, however, to the obligations under which Mr. Stratford has laid astronomers, in matters unconnected with the immediate routine of the ephemeris, they can hardly be fully appreciated, and certainly not duly acknowledged, till after the next appearance of Halley's comet. There is also a stability given to the ordinary mode of conducting the Nautical Almanac, by the formation of a regular board of computers; a thing which did not exist when the present Superintendent came into office, and without which it would have been difficult to have fully carried out the recommendations of the Nautical Almanac Committee.

Since the last anniversary of the Society three comets have been discovered. The first was found by M. Mauvais at the Royal Observatory of Paris on July 7; and, two nights afterwards, it was detected at Berlin. The elements differ materially from those of any comet whose orbit has been previously calculated.

The second comet was discovered at Rome at the observatory of the Collegio Romano, by our Associate, Signor Francesco de Vico, on the night of August 22. Considerable interest is attached to this comet, its period of revolution appearing to be only five and a half years. At the latter end of September it was easily seen with a small telescope; but, after that time, it became rapidly fainter, and, during the first week in December, was visible only with instruments of very great power. Another revolution of the comet will be completed in the early part of the year 1850.

A third comet was discovered on the evening of December 28, by M. d'Arrest, of the University of Berlin. It is still visible. The elements somewhat resemble those of the second comet of 1793, for which Burckhardt computed an elliptical orbit of about twelve years' period; but the perihelion distance of the comet in 1793 was 1·4, while that of the present comet is only 0·9, a difference far too large to admit of the identity of the two bodies, except in the case of enormous perturbations.

Your Council will conclude this Report with one remark. It is obvious, from what has been stated, that this is a period of great activity, and that all parts of practical astronomy are in full cultivation. It may, perhaps, be rather a proof of this than the contrary, that the number of communications forwarded to the Society to be read at the ordinary meetings has materially decreased within the last few years. We may easily suppose that, as the work of the Observatory becomes more arduous, its superintendent has less time for the consideration of points which are not immediately connected with annual duties; and it is certain that nothing diminishes the occasional power of contributing to Transactions in the private astronomer, so much as self-devotion to some great branch of the subject, pointed out by personal taste or strong inducing circumstances. But, nevertheless, it ought to be remembered that the Memoirs of the Society are not merely the depository of facts, processes, or reasonings, which it is right to preserve. Though they may finally become nothing more, if it be right to apply such words to so important a function, yet it must be remembered that, on its first appearance, each volume is the impulse and stimulus of its day, the suggester as well as the receptacle of thought, the cause of future as well as the proof of past progress. The ordinary meetings of the Society must dwindle, to the serious diminution of its utility, unless they be nourished by communications of interest. And even granting that the magnitude of the objects on which astronomers are engaged at any one time, renders it almost impossible for them to supply the Memoirs at the equable rate hitherto maintained, it must not be forgotten that there are many points connected with the theory and practice, and, above all, with the history of astronomy, which are constantly arising out of every research, directly or collaterally. Those to whom such things present themselves, are especially requested by the Council to bear in mind that any interesting information, however inadequate to be the basis of a formal paper, finds its immediate use at our meetings, and its record in our Monthly Notices.

There is much reason for regret in the circumstance that the scientific body of our day has almost entirely lost the character of the collector of suggestions worthy of consideration, hints of useful tendency, and valuable but isolated information. If the minutes of the Royal Society in the time which preceded the publication of the *Principia* should now and then excite a smile, it will be checked by the obvious consideration that we owe much of what has been since done to the currency which the disposition to communicate, and the facilities for doing it, afforded to mere suggestions. Much might, perhaps, be done by encouraging the slighter and less elaborate form of communication, which it is one object of the Monthly Notices to perpetuate; and your Council hope that those Fellows who have shown themselves capable of greater things, will not forget that the small matter which, thinking only of themselves, they might be inclined to throw on one side, may possibly meet the difficulty, or advance the object, of some other member of the Society. At the least, it will add attraction to the proceedings of the ordinary meeting, which it is so important to the welfare of the Society, and, through it, of astronomy, to render both instructive and interesting.

XXIV. *Intelligence and Miscellaneous Articles.*

RESEARCHES ON SHOOTING STARS. BY M. COULVIER-GRAVIER.

UP to the present time, says the author of this memoir, shooting stars have not been the object of observations regularly followed up and prolonged for a sufficient time, to enable us to arrive at any general law. In truth, it has been imagined that determinate periods were observable when these meteors showed themselves infinitely more numerous than at ordinary times; but the periodical returns to which it has been thought they might be subjected, begin to appear problematical, and perhaps they would never have been admitted if the investigation had been commenced by seeking to ascertain the appearances for each night of the year. Such a task, indeed, would have been very arduous, and it is this doubtless that has discouraged observers. For my own part, having been engaged since 1829 in this class of researches, to which I first devoted my attention with a particular object, I have since pursued them for their own sake, and from the year 1841 I have kept regular registers of my observations. I have found it desirable, for this purpose, to take an associate in my labours, M. Chartiaux, who observes one half of the heaven while I am employed on the other. I myself note down each appearance, both those which my assistant announces aloud and those which I myself see. In this manner it is impossible to take one observation twice over, which is otherwise almost inevitable when several persons being engaged in observing at the same time, each notes down what he perceives in the portion of the heaven which is assigned to him. I may perhaps explain thus the very extraordinary numbers noted by four persons who made observations simultaneously on the same spot.

Observers have generally chosen their time for making these researches. For our part, a clouded sky alone interrupts our observations, to which we return, at any hour in the night, as soon as the state of the sky permits. My registers give the following results: from the month of July 1841 to the month of February 1845, 5302 shooting stars observed in 1054 hours. I have grouped, in the different tables contained in my memoir, these observations in such a manner as to be able to deduce from them results relatively to the greater or less frequency of these meteors, according to the hours, months and years. It will be conceived that, on this latter point, I do not yet pretend to draw any conclusions from observations which do not extend in a regular manner over more than four years; but as to the horary and mensual variations, I think that I have already arrived at two general laws. Thus, in each month comprised between the winter and summer solstices, the mean number of falling stars, for one hour, is very sensibly the same; and this is also the case during the six other months; with this difference, however, that for the latter, the mean is nearly double what it is for the others, and the change takes place, so to speak, without transition. With regard to the horary variations, on the contrary, there is a gradual change, and from six o'clock in the evening, which is the hour of the minimum, the number of the appearances goes on increasing continually up to six in the morning, which is the moment of the maximum.

In the second part the author treats of the directions of shooting stars. The following is the distribution of 1000 of these meteors relatively to the sixteen angular spaces into which he divides the horizon, proceeding in the direction north, east, south, west:—74, 90, 82, 91, 114, 86, 70, 79, 63, 34, 29, 28, 33, 28, 35, 64; which shows that many more stars come from the east than from the west, and nearly as many from the north as from the south.

The author thinks that the difference between the number of stars observed in the two directions, east and west, is attributable to the twofold motion of the earth.

There are mensual variations which it is difficult to determine. In winter, the influence of the south is the greatest possible; in summer the influence of the north is very perceptible. With regard to the influence of the east, it is the weakest in summer, and the strongest in spring and in autumn.

The horary variations are more marked. Those from the north are most numerous towards midnight, and least so in the morning. With respect to the east, there are most in the morning and fewest in the evening. From the south there are often the most in the morning; lastly, from the west there are most in the evening.

In the third part of his memoir, the author treats first of the magnitude of shooting stars; he gives the name *globe filant* to every meteor which presents a sensible disc, and reserves the name of shooting stars for the meteors which have an appearance analogous to that of the fixed stars and the planets. He calls them of the first magnitude when they appear equal to Venus or Jupiter; of the second magnitude when they resemble fixed stars of the first magnitude; and thus with the rest. Among 5302 meteors, the author counted eight shooting globes and eighty shooting stars of the first magnitude; whence it follows that, if no obstacle prevented, an observer would see one shooting globe a week, and one falling star of the first magnitude each night of eleven hours.

In general the falling stars have the same tint as the fixed stars. Sometimes this colour passes into yellow, then into a bluish or a greenish tint, in proportion as the meteor approaches the horizon.

Among all these meteors there are some red ones, which travel slowly, and have a globular form, like a billiard-ball coloured red. The author thinks that these are of a particular character. Lastly, he distinguishes others which are extinguished at their highest point of brilliancy, as if they were plunged into water.

With respect to the trains which some stars leave behind them, these cannot be compared to smoke, but rather to a shower of sparks like that of rockets. The train begins and ends always with the star producing it, but it continues one or two seconds after the disappearance of that star. Sometimes the star breaks up into fragments, which form a continuation of the train, and which vanish almost as soon. No star has ever caused any audible noise, whether remaining simple, or producing a train, or separating into fragments.

In general the course of shooting stars is rectilinear, or rather in an arc of a large circle. The author has seen fifteen whose course was curvilinear.

At the conclusion of his investigation the author has given a catalogue of the most remarkable shooting stars, with the indication of the characters which they have presented. Before proceeding to the theoretical part of the subject he announces some historical researches relative to it.—*Comptes Rendus,* May 5th and June 2nd, 1845.

ON THE SUPPOSED OCCURRENCE OF PHOSPHORIC ACID IN ROCKS OF IGNEOUS ORIGIN. BY PROF. C. KERSTEN.

In the 'Philosophical Magazine,' vol. xxiv. p. 467, is an abstract of a paper by Dr. Fownes, on the occurrence of phosphoric acid in rocks of igneous origin. Dr. Fownes asserts that he found small quantities of phosphoric acid in combination with alumina, &c. in all these rocks, and supposes that it is probably to this presence of phosphoric acid that many soils which have originated by the decomposition of those rocks owe their fertility. He found phosphoric acid in a porcelain clay from Dartmoor, in the gray vesicular lava from the Rhine, and in the white trachyte from the Drachenfels on the Rhine, in tolerable quantity in the two latter rocks; further, in several basalts, in porphyritic lava inclosing crystals of hornblende from Vesuvius, and in volcanic tufa from the same locality in considerable quantity. As these observations of Dr. Fownes appeared of some importance in a geognostical and agricultural point of view, and, as far as I am aware, no phosphoric acid had hitherto been detected in the above-mentioned rocks, I was induced to repeat Dr. Fownes's experiments.

The following rocks were examined for phosphoric acid:—1st, porcelain earth from Aue in Saxony; 2nd, porcelain earth from Bayonne; 3rd, gray vesicular lava from Niedermennig on the Rhine; 4th, white trachyte from the Drachenfels; 5th, basalt from the Meissner in Hessia; 6th, Trass from Vesuvius.

The following was the mode of examination:—The finely-pulverized rocks were fused with 4 times the quantity of chemically-pure soda, the fused masses digested in water, and the liquids filtered from the insoluble residues neutralized with nitric acid. These neutralized liquids were now evaporated to dryness, and after treatment with water and separation of the silicic acid, tested with nitrate of silver and the other known reagents for phosphoric acid.

In all the above rocks, however, *not the least trace of phosphoric acid* could be detected; consequently the above-mentioned observations of Dr. Fownes are founded on some error*. In the gray vesicular lava from Niedermennig there was found a quantity of *chlorine*; and on boiling it in the pulverized state with water and evaporating the liquid, small quantities of *chloride of sodium* were obtained; at the same time with this a brown organic substance was extracted, which coloured the water brownish on evaporation and was carbonified on ignition.—*Journ. für Prakt. Chem.*, March 31, 1845.

EXAMINATION OF SOME NATIVE AND ARTIFICIAL COMPOUNDS OF PHOSPHORIC ACID. BY C. RAMMELSBERG.

Wagnerite (Pleuroklase).

We are indebted for the only analysis of this exceedingly rare mineral to Fuchs, who obtained in the year 1821 the following results: phosphoric acid 41·73 per cent., magnesia 46·66, peroxide of iron 5·0, peroxide of manganese 0·5, hydrofluoric acid 6·5. Fuchs decomposed the mineral with sulphuric acid, and separated the sulphate from the phosphate of magnesia by water. The phosphate of magnesia was decomposed by boiling it with a solution of potash. The phosphoric acid and fluorine were not directly estimated. If his method of analysis be minutely examined, the results are seen to be only approximative, and a repetition of the analysis would be very desirable. In the specimens selected by the author for analysis, there were large, opake, reddish, and small transparent, wine-yellow crystals of wagnerite, imbedded in a green aluminous mass and accompanied with quartz, calcareous spar and bitter spar. Its specific gravity, according to Fuchs, is 3·13. The author found it 3·068 at a temperature of 57° F. in the transparent parts, and 2·985 in the opake portions.

Three analyses were made by methods differing as widely as possible.

1. The substance reduced to a fine powder was treated with sulphuric acid in a platinum crucible. This, even in the cold, caused the evolution of fumes of fluoride of silicium from the admixture of

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* The observations of Dr. Fownes have, however, been fully confirmed by W. Sullivan, Esq., as will appear from a communication to be inserted in our next.—Ed. Phil. Mag.

silicic acid (quartz). The mixture was heated until the whole of the fluorine and the greater part of the sulphuric acid were expelled: it was then moistened with water, next mixed with four parts of carbonate of soda, evaporated, and finally heated to redness for half an hour. The perfectly fused mass was exhausted with water, and after repeatedly evaporating the alkaline solution, each time adding carbonate of ammonia and redissolving, the phosphoric acid was precipitated by chloride of calcium. The exhausted residue, after solution in muriatic acid, yielded, on the addition of ammonia, a precipitate of oxide of iron, with a small quantity of alumina and magnesia, and some phosphoric acid; it was therefore again fused with carbonate of soda. The separation of the bases, and also the separation of the lime and magnesia from the ammoniacal liquid, was accomplished by the ordinary methods.

2. The mass treated with sulphuric acid as above was finally retained at a temperature below that of redness, until fumes of sulphuric acid ceased to escape, and was then exhausted with water. According to the statement of Fuchs, sulphate of magnesia (with peroxide of iron) only should have been thus dissolved; and by ascertaining the amount of sulphuric acid we might expect to obtain the equivalent amount of fluorine. But this cannot be accomplished, partly because the mineral always contains a little silicic acid which escapes in the form of fluoride of silicium, and partly because the water removes phosphate of alumina as well as the sulphate. The portion insoluble in water is likewise decomposed in this case by heating it to redness with carbonate of soda, although by so doing the whole of the phosphoric acid cannot be removed, and the precipitate obtained on the addition of ammonia must be digested and boiled with solution of potash for a considerable time.

3. The more transparent parts of the mineral, being the most recent and the purest, were decomposed by a mixture of an equal weight of hydrate of potash and three times their weight of carbonate of soda. The perfectly fused mass when treated with water yielded an alkaline solution, containing phosphoric acid and fluorine, and an insoluble residue, consisting of magnesia and the other bases. *a*. The liquid was evaporated to dryness in a water-bath in a platinum capsule, treated with water, separated by filtration from a small residue of phosphate of magnesia, hydrochloric acid then added so as to produce a slightly acid reaction, the carbonic acid expelled by a very gentle heat, and then precipitated by ammonia and chloride of calcium in a closed vessel. The precipitate, consisting of phosphate of lime and fluoride of calcium, was decomposed by sulphuric acid and alcohol, and thus the lime was estimated; the alcoholic liquid was slightly evaporated, then mixed with ammonia and chloride of calcium, and the quantity of phosphoric acid in the phosphate of lime ascertained by decomposing it with sulphuric acid. *b*. The residue of the exhaustion of the fused mass, when acted upon by hydrochloric acid, left a residue consisting of silicic acid. The precipitate thrown down from this solution by ammonia also contained some phosphoric acid, it was therefore again heated with carbonate of soda. It con-

tained moreover peroxide of iron, lime and magnesia; a portion of the two latter still remained to be estimated in the ammoniacal filtered liquor. The three above analyses, the third of which yielded the best results, afforded the following proportions:—

	I.	II.	III.
Silicic acid			2·68
Phosphoric acid	41·89	40·23	39·56
Magnesia	42·04	38·49	45·07
Lime	1·65	4·40	2·32
Protoxide of iron ..	2·72	3·31	4·47
Alumina	0·55	0·96	
Fluorine			9·12
			103·22

The silicic acid is merely mixed with the wagnerite in the form of quartz, it must therefore be deducted. The same holds good with the alumina and small quantities of the remaining bases, which are combined with carbonic acid, forming calcareous and bitter spar, and cannot be completely removed in selecting the fragments. Their amount is however very inconsiderable. If we deduct the 2·68 per cent. of silica there remain 40·61 per cent. phosphoric acid, 46·27 magnesia, 4·59 protoxide of iron, 2·38 lime, 9·36 fluorine; results which do not differ to any extent from those of Fuchs except in the proportion of fluorine. If we calculate from the third analysis, we find that the quantities of protoxide of iron and lime, which nearly replace one part of magnesia, amount to one equivalent for 4·43 of the latter. Now 9·36 fluorine to form MgFl require 6·34 Mg=10·34 magnesia; consequently 40·36 per cent. of magnesia remain to combine with the 40·61 per cent. phosphoric acid. But as the above experiment could hardly yield the entire amount of phosphoric acid and of fluorine, we have no hesitation in regarding the magnesian phosphate as $3MgO, P_2O_5$, assigning to wagnerite the formula $MgFl + 3MgO, P_2O_5$. This formula requires 43·32 per cent. of phosphoric acid and 11·35 per cent. of fluorine, whilst the formula $MgFl + 4MgO, P_2O_5$ requires only 38·5 phosphoric acid, thus less than the experiment yields, and 55·73 magnesia; it is therefore incorrect.

In the specimens of the wagnerite selected for analysis, there were reddish, dull, and soft portions, the examination of which showed that they were probably decomposed wagnerite; they consisted principally of silicic acid. On analysis they yielded 93·81 per cent. silicic acid, 1·87 phosphoric acid, 1·41 alumina and peroxide of iron, 2·58 lime, and 1·49 magnesia.—*Poggendorff's Ann.* lxiv. p. 251.

[To be continued.]

ON THE STRUCTURE AND RELATIONS OF CORNULITES AND OTHER SILURIAN FOSSILS. BY MR. J. W. SALTER.

The singular fossil named *Cornulites serpularius* is well known as characteristic of the Silurian rocks of Gothland, Britain and North America, and its affinities have been the subject of much conjecture, Mr. Salter's investigations proved that it differed essentially from any crinoidal animal or coral, and was most probably an ancient form of the *Serpulinæ.* From a similarity of structure in the Tentaculites, another abundant fossil of the Silurian rocks, Mr. Salter is disposed to refer it to the same group with Cornulites.—*Proc. Brit. Assoc. Cambridge*, June 19.

ERUPTION OF VESUVIUS.

Letters from Naples announce, that the expected eruption of Mount Vesuvius has commenced. At their date, the volcano had been for several days throwing up flames and stones, by the mouth of the cone formed some months ago in the midst of the crater; and the burning lava had opened a wide passage below, and was flowing down the side of the mountain.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1845.

Chiswick.—June 1. Overcast and fine. 2. Very fine: sultry. 3. Sultry: rain at night. 4. Showery. 5. Slight rain: cloudy: boisterous at night. 6. Cloudy and windy. 7. Very fine: showery, and boisterous at night. 8. Boisterous: clear and fine. 9. Very fine. 10—12. Very fine: sultry: clear and fine at nights. 13. Foggy: sultry. 14, 15. Very hot and sultry. 16. Overcast. 17. Sultry. 18. Rain: clear at night. 19. Hazy: very fine. 20. Cloudless: very fine. 21—23. Very fine. 24. Uniformly overcast: heavy rain: clear. 25. Cold and dry: cloudy: rain. 26. Cloudy: fine. 27. Rain. 28. Overcast: heavy showers: clear. 29. Very fine. 30. Slight rain: very fine.—Mean temperature of the month 1½° above the average.

Boston.—June 1. Rain: rain early A.M.: rain A.M. 2. Fine. 3. Fine: 2 o'clock P.M. thermometer 72°: rain P.M. 4. Fine: thunder and lightning, with rain P.M. 5. Cloudy: rain early A.M. 6. Windy. 7. Fine: rain P.M. 8. Cloudy. 9. Fine. 10. Fine: 3 o'clock P.M. thermometer 74°. 11. Fine: 1 o'clock P.M. thermometer 75°. 12. Fine: 4 o'clock P.M. thermometer 76°. 13. Fine: 1 o'clock P.M. thermometer 78°. 14. Fine: 12 o'clock noon thermometer 78°. 15. Cloudy. 16. Cloudy: 1 o'clock P.M. thermometer 78°. 17. Fine. 18. Cloudy: rain early A.M.: rain A.M. and P.M. 19. Fine. 20. Cloudy. 21. Fine. 22. Cloudy. 23. Fine. 24. Fine: rain, with thunder and lightning P.M. 25. Cloudy. 26. Fine. 27. Cloudy: rain A.M. and P.M. 28. Cloudy: rain A.M.: thunder and lightning 1 P.M. 29. Fine. 30. Fine: rain early A.M.—N.B. The warmest June since June 1826.

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

Applegarth Manse, Dumfries-shire.—June 1. Fine soft rain. 2. Very fine: fair. 3. Fine rain. 4. Showers all day. 5. Rain continued. 6. Rain. 7. Soft showers. 8. Heavy rain A.M.: fair P.M. 9. Showers. 10. Fair and growing. 11—14. Very fine summer day. 15. Very fine summer day: thunder: a few drops of rain. 16. Showery all day. 17. Slight showers. 18. Slight showers P.M. 19, 20. Fair and fine. 21. Fair and fine: a few drops: rain. 22. Fair and fine: very dry. 23. Fine: one slight shower. 24. Rain, very heavy. 25. Showers. 26. Fair and fine. 27. Rain: wind: thunder. 28. Fair and clear. 29. Fair A.M.: shower P.M. 30. Showers.

Mean temperature of the month	56°·55
Mean temperature of June 1844	55 ·10

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. 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.	30·133	30·042	29·54	29·94	29·84	29·90	29·81	72	43	56	57	46½	62	52	sw.	calm	s.	sw.	......	·11	......	......
2.	29·951	29·761	29·37	29·75	29·62	29·64	29·51	81	46	63	60	52	59	54	s.	w.	s.	s.	......	·04	......	......
3.	29·620	29·367	28·93	29·40	29·02	29·25	29·14	81	44	68	60	50	59	50	sw.	calm	s.	......	·06	......	......	......
4.	29·607	29·516	28·79	29·15	29·29	29·08	29·17	70	49	59	57	44	50	49	sw.	w.	sw.	wsw.	·05	·04	......	·62
● 5.	29·597	29·543	28·94	29·23	29·07	29·32	29·17	68	55	58	59½	48½	55	53	sw.	sw.	sw.	se.	·08	·12	......	......
6.	29·800	29·575	28·83	29·03	29·38	29·10	29·23	67	50	62·5	58	52	54½	57	sw.	w.	sw.	ssw.	......	......	......	·14
7.	29·864	29·736	29·30	29·61	29·52	29·54	29·72	67	47	65	62	49½	57½	53	s.	w.	ne.	s.	·25	......	......	......
8.	30·200	29·807	29·13	29·49	29·89	29·74	29·82	68	41	56	61	49½	56	48	sw.	w.	wnw.	w.	......	·24	1·50	......
9.	30·365	30·335	29·66	30·09	30·01	29·94	30·00	72	40	62	57	47	53	50	sw.	w.	sw.	wsw.	......	......	......	·48
10.	30·365	30·269	29·68	30·19	30·18	30·19	30·11	76	46	67	64	53	54	52	e.	calm	sw.	se.	......	......	......	......
11.	30·235	30·151	29·50	30·14	30·14	30·06	30·13	78	50	73	70	54	58	55	ne.	calm	sw.	sw.	......	......	......	......
12.	30·165	30·141	29·46	30·10	30·10	30·10	30·03	85	54	70	75	52	63½	57	e.	calm	sw.	sw.	......	......	......	......
☽ 13.	30·157	30·106	29·49	30·15	30·13	30·09	30·20	84	57	71·5	75½	52½	54	50	ne.	calm	sw.	w.	......	......	......	......
14.	30·120	30·050	29·45	30·09	30·02	30·21	30·15	82	53	76	75½	53	60	53	e.	e.	s & se.	w.	......	......	......	......
15.	30·038	29·905	29·46	29·95	29·82	30·03	29·89	84	60	67	69	59½	55	52	se.	calm	s.	e.	......	......	......	......
16.	29·844	29·794	29·22	29·73	29·69	29·83	29·78	75	50	67	62	58	53½	52½	s.	calm	sse.	ese.	......	......	0·13	·12
17.	29·814	29·771	29·19	29·74	29·74	29·86	29·88	80	59	75·5	63	53½	55	52	ne.	calm	se.	e.	·19	......	......	......
18.	29·857	29·739	29·16	29·71	29·65	29·80	29·76	70	44	64	63½	54	58	54	ne.	calm	wnw.	nw.	·05	·12	......	·22
○ 19.	30·039	29·944	29·39	29·79	29·95	29·80	30·00	78	48	65	61	50	54	52	s.	w.	wnw.	wnw.	......	·41	......	......
20.	30·117	30·083	29·58	30·05	30·03	30·06	29·95	74	50	60	68	50	54½	53	ne.	calm	w.	sw.	......	......	......	......
21.	30·082	29·991	29·59	30·00	29·97	29·97	30·00	80	51	65	67	53½	52	51	e.	calm	w.	w.	......	......	......	·05
22.	30·014	29·935	29·44	29·97	29·94	29·93	29·92	73	48	64	62½	48	52	51	ne.	calm	wnw.	sw.	......	......	......	......
23.	30·126	30·094	29·60	30·00	29·97	30·00	30·00	73	51	62	63	47	52½	51	sw.	nw.	wnw.	sw.	......	......	......	·07
24.	30·034	29·797	29·47	29·77	29·60	29·80	29·66	74	52	67	56	48½	57	51	sw.	nw.	wnw.	w.	·21	......	0·15	·09
25.	29·808	29·798	29·26	29·58	29·64	29·66	29·65	74	52	56·5	62	48	54	50	sw.	w.	w.	w.	·09	·20	......	......
☾ 26.	29·856	29·776	29·30	29·69	29·73	29·74	29·80	74	49	61	61½	43½	48	48	w.	w.	nw.	nw.	......	......	......	·30
27.	29·760	29·462	29·29	29·60	29·27	29·74	29·69	66	56	57	60½	41½	50	49	sw.	calm	e.	ne.	·15	......	......	·17
28.	29·751	29·230	28·75	29·18	29·51	29·64	29·62	69	43	61	58	47	51½	45½	w.	calm	n.	n.	·16	·33	......	·07
29.	29·937	29·897	29·39	29·68	29·63	29·58	29·71	72	51	63	60	45	49½	50	w.	calm	w&se.	w.	·04	·39	......	......
30.	29·856	29·838	29·33	29·58	29·59	29·68	29·65	70	53	64	62	51	54	49	w.	w.	sw.	e.	·03	·04	1·70	·07
Mean.	29·970	29·848	29·31	29·746	29·731	29·776	29·771	74·56	49·73	64·2	63·0	50·0	54·86	51·46					1·36	2·04	3·48	2·40

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[THIRD SERIES.]

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XXV. *On the Presence of Phosphoric Acid in Rocks and Minerals.* By William Sullivan, *Esq.*[*]

IN a recent paper in the Philosophical Transactions, Dr. Fownes showed the presence of phosphoric acid in several igneous rocks, and having made some investigations on the same point, I may perhaps be allowed to state them here. About two years ago, Prof. Liebig mentioned to me the importance of examining different rocks in order to ascertain, if possible, the presence of phosphoric acid in them, as he considered very naturally that it must be universally present in all soils and in the rocks from which they are derived, as we invariably find it in the ashes of plants. In the winter of 1843–44, I made a number of analyses in order to see how far this idea could be borne out, and also to show that impure limestones were better adapted for agricultural purposes than those usually preferred. To these analyses I have since considerably added; but the only publicity I gave the matter up to the present time, was a short notice which was published in a Cork newspaper about a year and a half ago, intended principally for the information of the agriculturists of that locality.

The ordinary method employed for the detection of phosphoric acid I found not to answer where the quantity of that substance is very minute; and besides, it is likely to give erroneous results where large quantities of iron, alumina and lime are present: indeed, it is chiefly owing to phosphoric acid being thrown down along with these bases in the analyses of minerals containing this acid only in minute quantities, that its presence in most substances of mineral origin has hitherto escaped detection.

The mode which I adopted is capable of giving the most

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[*] Communicated by the Author.

accurate results,—in fact the most minute quantities possible may be detected with certainty. The following is the method:—

When the mineral was limestone, or a substance containing a large quantity of lime, I took about four ounces of it broken into fragments, introduced it into a covered crucible and exposed it for three or four hours to a strong white heat; the mass thus treated was then digested in water, when I suspected the substance contained potash or soda, in order to dissolve out these bodies, and strong hydrochloric acid poured on the remainder (if I did not wish to examine for potash, the ignited mass was directly treated with the hydrochloric acid); the whole was then evaporated to dryness, the dried mass moistened with a few drops of hydrochloric acid, and then treated with water and filtered to separate undissolved silica; ammonia was then added to the solution, the precipitate collected, well-washed and dissolved in hydrochloric acid; if the mineral contained no iron, or but little of that substance, a few drops of perchloride of iron were added to the solution and then excess of acetate of potash; if phosphoric acid was present in the most minute quantity, a precipitate, at first light brown, but gradually darkening as it falls to the bottom of the glass, of phosphate iron, was obtained, as this substance is totally insoluble in acetate of potash. If the precipitate do not immediately make its appearance, it should be allowed to stand for some time, when it will be perceptible. It sometimes happens, when the quantity of the phosphoric acid is extremely minute, that it does not deposit itself for twelve hours; and in one or two instances it did not occur until after twenty-four hours. When the precipitate obtained was large, which was usually the case from the large quantity of the substance employed in the analyses, I invariably verified the result by re-dissolving the precipitate thus obtained and testing with sulphate of magnesia and ammonia and nitrate of silver, which always afforded well-known results where phosphoric acid was present.

The substances which did not contain lime enough to be treated in the preceding manner were reduced to powder and fused with a mixture of carbonates of potash and soda, or where it was of importance to ascertain the presence of potash, with caustic barytes, and I then proceeded as in the other case.

It may not be unimportant to mention here, that potash, or rather potash and soda, appear to be as universally present as phosphoric acid: indeed, in none of the minerals which I examined was it totally wanting, though there was frequently but a mere trace.

The following is a list of the minerals which I examined for phosphoric acid. I have also noticed those in which potash and soda occur, that is, those rocks in which these substances had not hitherto been noticed.

Minerals.	Phosphoric acid.	Alkalies.
Nine varieties of *carboniferous limestone* from the neighbourhood of Cork.	In all I could detect its presence, and in one it occurred in sufficient quantity to be determined.	Potash was found present in all, but in such small quantity that I could not determine it. From the same canse I could not determine whether soda was present.
Muschelkalk and *dolomite* from the same formation, from near Fulda in Hesse-Casel.	Present, but apparently more abundant, in the dolomite.	Minute traces.
Roofing-slate used at Giessen.	Considerable precipitate, but *wavellite* is found in some localities in the same rock.	Large precipitate, with chloride of platinum and a small trace of soda.
Flags used in the west of Ireland, supposed to be *millstone grit.*	Traces, but very minute.	Trace of potash, but could not ascertain if soda was present.
Slate from Corrigaline, near Cork.	Precipitate large enough to examine quantitatively. [This is the best locality known, I believe, for wavellite.]	Decided precipitate with chloride of platinum. Sufficient soda to produce a precipitate with antimoniate of potash.
Old red sandstone, Cork.	Trace in one specimen, but a decided precipitate was obtained from three ounces of another.	Could not detect soda, but detected a trace of potash.

Minerals.	Phosphoric acid.
Diorite containing augite, from the valley of the Lahn, Nassau.	Obtained sufficient from two ounces to test with nitrate of silver.
Clinkstone, from Rhöngebirge, Bavaria.	Abundant traces.
Phonolitic Tufa, Rhöngebirge, Bavaria.	Considerable precipitate.
Hornblende in crystals, from Rhöngebirge.	Minute traces.
Olivine, in a state of decomposition, from the Vogelsgebirge, Hesse-Darmstadt.	Sufficient to apply the test of nitrate of silver; but three ounces were employed.
Augite, crystalline, Rhöngebirge.	Abundant traces.
Compact basalt, from Giessen.	Considerable traces.
Trap rock of the nature of greenstone, from the north of Ireland.	Only minute traces in one specimen, but in considerable quantity in another.
Basalts, Giant's Causeway.	Enough to produce a precipitate with sulphate of ammonia and magnesia from two ounces.
Pumice stone, from the Lake of Laach, near Andernach, on the Rhine.	Considerable precipitate from three ounces.

Minerals.	Phosphoric acid.
Sodalite, Vesuvius.	Trace; had not enough for a second analysis.
Variety of *hypersthene*, occurring in the diorite on the Lahn, Nassau.	Distinct traces.
Lepidolite, locality unknown.	I obtained so large a precipitate, that I think it must form an important constituent of this mineral.
Obsidian, from Lipari.	Present in considerable quantity.
Mica, Spessart, Bavaria.	Minute traces, but more abundant in another specimen from Russia.
Granite, Odenwald, Germany.	Enough to give all the usual tests from about 1½ ounce.
Chlorite slate, locality unknown.	Sufficient from two ounces to give a precipitate with nitrate of silver.
Augite porphyry, locality unknown.	Considerable traces.
Mica slate, locality unknown.	Enough for nitrate of silver test.
Gneiss, Odenwald, Germany.	Considerable traces from two ounces.
Two specimens of Scotch *granite*, localities unknown.	Large precipitate from two ounces.
Tincal, or native borax, from East Indies.	The quantity of phosphoric acid in this substance was very considerable, being equal in one instance to 2·13 per cent. I think most native borax will be found to contain phosphate of soda.
Datholite.	In one specimen of this mineral from Andreasberg in the Harz, the quantity of this acid present was sufficiently large to be estimated. I also detected its presence in a specimen from North America, but in neither case had I enough of the mineral to make another analysis.

It is very curious that such a large quantity of phosphoric acid should in these two cases be associated with boracic acid, which is of a decided volcanic origin. It would be very interesting if the substances occurring with the native boracic acid in Tuscany, and also the other minerals containing boracic acid, were examined for phosphoric acid. The quantitative determination of the phosphoric acid in rocks would, in my opinion, be of little importance, as phosphoric acid, although a constant, is a very variable constituent. This, however, does not apply to the simple minerals, such as olivine, of which it forms very probably a constant constituent.

Dublin, July 17, 1845.

XXVI. *Some Observations on the Theory and Application of Electricity.* *By* F. C. HENRICI[*].

1. IT is usually considered, that in consequence of the supposed repulsion exerted between the particles of elec-

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[*] From Poggendorff's *Annalen*, vol. lxiv. p. 345.

tricity of the same kind, the free electricity of any conductor is diffused over its surface and forms a layer of inconsiderable thickness. But, on the one hand, if the electricity were thus forced towards the surface, there must be something which forces, consequently electricity, in the interior of the conductor, and again the repelled particles must exert an equal but opposing action, inasmuch as they cannot escape into any surrounding non-conductor. Thus an equilibrium would be formed between all the electrical particles, which could only consist in an equality of their mutual pressure. In fact, we only find this condition in aëriform bodies which are confined in any given space (as in porous bodies, which have no attraction for them); moreover, in a solid body heated throughout and surrounded by a bad conductor of heat, we find no trace of a distribution of this heat over the surface.

It is difficult to understand what is the nature of the repulsion supposed to exist between the particles of like electricities. When repulsion occurs between two such particles, both must evidently separate from one another, consequently a fluid consisting of such particles must be expansible, and *vice versâ* compressible. The mathematical theory of electricity hitherto adopted requires, however, that the electricities should be incompressible fluids. This assumption is however by no means supported by analogy. In ponderable bodies we find a repulsion of the ultimate molecules in gases only, *i. e.* in remarkably compressible bodies alone; in liquids, properly so called, which are always to a certain extent compressible, we find an attraction of their particles.

The ordinary view of the electrical principle is consequently not capable of explaining the action of electrified bodies at a distance, which is produced as if their free electricity existed only at the surface, and altogether we must consider it merely of use as a means of handling electrical phænomena, and as enabling us to treat the same mathematically.

2. The current from an electrical machine produces galvanic (magnetic, thermal, chemical, &c.) effects, just as the current from a galvanic series. We can, without difficulty, reduce two currents, one originating in each manner, to exactly the same galvanic action. In this case the electrical particles must have the same quantity of motion in both.

But what a marked difference exists in their tension ! and yet, according to the dualistic hypothesis, the separation and reunion of the electricities in the conducting circles of each must take place with equal energy if the above-mentioned conditions are to be fulfilled. Hence it appears natural that there should exist different kinds of electricity, just as there are different kinds of light, heat and sound. Electrical phænomena would then also be undulatory. We certainly do not readily see how the positive and negative could in this way be accounted for; but such a difficulty cannot justify us in rejecting an explanation in other respects so accordant with reason. Viewed in this light, the increased effect produced by the simultaneous action of several sources of electricity (the voltaic pile, the electrical battery) would be considered as caused in the same way, as by the simultaneous action of many sources of the same kind of light an increased illumination occurs, and in the simultaneous emission of several sounds an increase in the sound conveyed to the ear is produced. The action of the condenser would then be intelligible as converting electricity of less into that of higher tension.

I may be permitted here to draw attention to the facts which electrical phænomena seem to point out, that an essentially different mode of propagation of the electricity occurs in the so-called non-conductors to what takes place in the conductors. Thus it appears that in the former, when they allow the electricity to be diffused to any extent, as the air, its propagation follows the same laws as light and radiant heat; this is especially pointed out by the late experiments of Petrina, who found that electricity, when propagated in a rectilinear direction in the atmosphere, in passing by a screen (a metallic conducting plate connected with the earth), suffers diffraction, just as rays of light and heat do. Hence any substance would be so much a better or worse conductor than another as it was suitable to either one or the other method of propagation of the electricity; with imperfect conductors both must occur to a certain extent. In other respects the non-conductors of electricity can only be compared within narrow limits with transparent bodies, because in the former the diffusion of the electricity will always be proportionally very limited; the so-called non-conductors of electricity will always be comparatively opake substances when considered electrically. It must also be admitted that they may be compelled by a sufficiently strong electrical power to propagate the electricity by *conduction* through more or less limited spaces (electric sparks, lightning). It moreover deserves to be noticed, that there is no conduction of heat and light in the same sense as occurs with electricity. We are unacquainted with any diffusion of light within bodies which can be compared to the conduction of electricity, and the diffusion of heat within bodies not only obtains with infinitely less rapidity, but also according to entirely different laws from the conduction of electricity (the same holds good with sound).

Magnetism exhibits the remarkable property of being retained or absorbed by no other bodies than the few magnetic metals, whilst it is diffused through these comparatively slowly, and in a manner more analogous to heat. Consequently there is no conduction of magnetism at all resembling the conduction of electricity.

If the appearances of light, heat, electricity and magnetism, are phænomena of motion, we must arrive at the conclusion that their exceedingly rapid mode of propagation essentially depends upon motion of an imponderable matter (æther), their slow diffusion being essentially dependent on the movements of the atoms of the bodies. An analogy between magnetism and heat consists in the rays emanating from the former principle being absorbed by the magnetic metals, these thus becoming magnetized, just as bodies become warmed by the absorption of heat; but there is this difference between the two agents, viz. that there are but few bodies capable of absorbing the magnetic rays, whilst there is scarcely one which is not able to absorb those of heat. Consequently nearly all bodies may be regarded as completely pervious to the former rays.

3. If we place two equal-sized plates of copper and zinc upon one another, and after contact separate them, when tested by a delicate electrometer they are both electrical, this occurring only after contact. From this it has been concluded that the dissimilar electricities evolved by the contact of heterogeneous metals neutralize one another at those parts where they are in contact, and that a comparatively small portion of them is set in motion. This retention in very good conductors is unintelligible, if no other forces were exerted: these effects however are evidently those of condensation, as Péclet long since proved by experiment; he showed that all smooth metallic surfaces, when in contact, even without intervening layers of varnish, act as condensers. In fact, in the present case, the same effect would be evident if the contact of the zinc with the copper plate occurred at a few physical points only (probably when their surfaces are perfectly smooth, and they are lightly placed upon one another, there is no real contact): this agrees with what we observe, that the effect is weaker in proportion as the surfaces in action are less. Thus the experiment by no means justifies the supposition, that the electricities set free by the contact of heterogeneous metals neutralize one another at the place of contact; it is rather natural to suppose that they possess freedom of motion. The consequence of this will be, that from the almost perfect conduction exerted at the place of contact, they will for the greater part combine again, and that a very small portion of them only

is diffused over the two conductors lying in contact. This latter portion however may be further carried away by conduction, and it is evident that by contact compensation must ensue for that which is thus carried away (perhaps absorbed by a condenser, or in some other way).

If the conduction be now completed in a manner requiring no further action by contact, an electric current is produced, the power of which must evidently be proportional to the perfection of the whole conduction, because in any given time a certain quantity only of electricity can escape at the point of contact. This quantity of electricity however always amounts to a very small portion only of that originally produced by the contact, as even the best conduction presents a remarkably greater resistance than the point of contact itself.

Thus the internal condition of an open galvanic series* cannot be compared with a charged electrical jar, because in this the electricities conveyed to it from without are artificially separated from one another by an intervening non-conductor and prevented from neutralizing each other. But even here a certain amount of electricity, although it is but small, and proportional to the conducting power of the separating non-conductor, must evidently pass through this to produce neutralization. The discharge of the jar ensues when any better conduction than by means of the non-conductor is presented to the electricities, as for instance, by the air, or still more the earth (which affords in the case of Volta's pile very imperfect conduction). Consequently, an electric jar, if no discharge occurred through the air, would almost entirely part with the electricity communicated to it by rapid discharge through the completed circuit, whilst of the electricity produced by contact a small quantity only can ever be collected and made effective at one time.

From these considerations, it cannot be allowed that the electricity from contact in the experiments we have investigated is capable of being rendered perceptible to the electroscope without condensation, because, as we have stated, even between unvarnished and very small metallic surfaces condensatory effects occur. I have succeeded, by the following method of proceeding, in making the electricity of contact unequivocally perceptible without condensation. A strip of silver was fastened to one end of a strip of zinc, the whole was then bent into the form of a U; between the two arms a narrow strip of gold leaf was suspended, so that it could be moved without

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* By an open but complete series, as is well known, we understand one in which, if we adopt the contact theory, the series is complete, as expressed by the formula, " Copper—zinc—liquid—copper."

difficulty to and fro. By carefully moving this gold leaf, it was very easy to place it in such a position that it should be attracted by the strip of zinc only when negatively, and the strip of silver only when positively electrified. As it was electrified by induction, on removing the electrifying bodies (sealing-wax and glass gently excited by friction) the opposite attraction to that produced by the approximation of them took place if an attraction had been thus produced. In my opinion we have here no ambiguity. There is scarcely occasion to remark, that the apparatus was enclosed in a glass vessel. As the normal attraction ensued only when the gold leaf was in a certain position (*i. e.* exactly midway between the arms of the compound strip), the small intensity of the electricity of contact is perfectly intelligible. Thus if the gold leaf be approximated to either arm, its free electricity will overpower the weak electricity of contact of this arm, and act upon it by induction, whence attraction between the two must always ensue, as the experiment indicates. I have some hopes of being able to obtain a comparative admeasurement of the intensity of the electrical effects of contact by this method.

4. It is well known that free electricity appears in many chemical processes. Thus the fumes of burning potassium are positive, whilst those of burning phosphorus are negative*. Are we hence to conclude that the atoms of the potash contain positive electricity and those of the phosphoric acid negative electricity in excess? Certainly not. We must merely consider that if the fumes of potassium appear positively electrical, the burning potassium itself must possess *negative* electricity; the conclusion that the potassium contained preponderance of negative electricity would thus be arrived at. However, the phosphorus, which by combustion must possess *positive* electricity, as the phosphoric acid appears negative, might be regarded as a body containing positive electricity predominantly. There is no doubt that the electricity which appears in these experiments is first produced in the act of combustion; perhaps it arises from the violent separation of the burnt particles from those still unburnt. Besides, this spontaneously disappears in a very short time, even when it is not purposely annihilated, and the precipitated fumes then indicate no further trace of free electricity; the chemical attraction, therefore, of the potash for the phosphoric acid cannot depend upon the action of free opposite electricities with which their atoms are said to be endowed. Neither can we suppose that any latent electricity exists in them, for such would be unable (if so capable *per se*) to exert any outward action. If, nevertheless, we were to attribute free electricity to the atoms of bodies, and

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* Berzelius, *Lehrbuch der Chemie*, 5te auf. S. 103.

to deduce hence their chemical attraction, we must evidently admit the existence of a great difference in the quantity of their peculiar electricities in the various atoms of bodies; the most positive bodies must contain the largest amount of positive, and the negative bodies the largest amount of negative electricity. But how can electro-chemical attraction occur, when two positive or negative bodies act on each other? and yet such bodies combine with each other in the most varied proportions.

Thus it is vain to endeavour to find a physical basis for the electro-chemical views. But there exists between the electrical relations and the general chemical character of a body, a parallelism which cannot be mistaken (as I have already endeavoured to prove elsewhere), and I am still of the opinion that it deserves attentive consideration.

5. The compound galvanic series presents a peculiarity which, in my opinion, deserves to be considered more carefully.

If n represents the cells of like constitution, the electromotive force of which is expressed by E, their resistance being $=r$, when united with an homologous metal, and provided with a common conductor whose resistance is $=r'$ (previously to which the circuit is not perfectly complete), the intensity of the current (Q) in the latter is

$$Q=\frac{nE}{r+nr'}.$$

When the elements are arranged in the form of a pile, the intensity of the current Q' is

$$Q'=\frac{nE}{nr+r'}.$$

When r' is $=r$, we have $Q=Q'=\dfrac{nE}{(n+1)r}$,

and it is then a matter of indifference in what manner the elements are combined as regards the effects. If we now examine the intensity of the current in the elements of the compound series, theory gives the equation

$$q=\frac{E}{r+nr'};$$

we thus arrive at the remarkable conclusion, that when r' is $=r$, the intensity of the currents in the elementary series is only $\dfrac{1}{n}$ of that contained in the pile series; in other words, the same mechanical (or other) effect of the conductor closing the circuit, produces under these circumstances in the pile a consumption of matter n times greater than occurs in the compound series formed of the same elements. Thus, were it possible, by mechanical arrangements, to render the resistance r' of the conductor completing the circuit $=r$, the expenditure of these arrangements arising from the galvanic consumption of matter in using 1000 elementary series combined in the manner of the compound series, would amount to $\frac{1}{1000}$th only of that caused by their arrangement in the form of a pile. Consequently, in the former combination, the polarization would also be remarkably slight; we might also thus avoid the use of constant series, and in the selection of a liquid might merely take into consideration, in addition to its conducting power, its chemical relation to the two metals, so as to avoid chemical action between them.

If n be very considerable, so that r in nr' may be disregarded, we have

$$Q = \frac{E}{r'} \text{ and } q=0;$$

in a very large (strictly considered infinite) number of elementary series; the greatest possible effect of the conductor closing the circuit is thus obtained, considering r' as one, without any galvanic consumption of matter, for then no electric current exists in the elementary series. Perhaps we can arrive still nearer the mark, as in a very large number of elementary series, not only the thickness of the smooth metallic plates, between which the liquid is enclosed, but also the thickness of the layers of the liquid is reduced to a minimum: thus the whole apparatus may be confined in a comparatively limited space.

XXVII. *Contributions to the Knowledge of Conjugate Compounds. By* Dr. H. KOLBE, *Chemical Assistant in the University of Marburg*[*].

THE examination of the products arising from the decomposition of sulphuret of carbon by means of chlorine has led to the discovery of an interesting series of bodies, which belong, almost without exception, to the class of the so-called conjugate compounds[†], and among them the substitution of chlorine by hydrogen is particularly observed, which will probably throw considerable light on the constitution of certain organic combinations. These compounds, produced almost by the direct combination of their elements, have such a strong similarity to those considered to belong to organic chemistry, that it is impossible here to draw a distinct line between organic and inorganic bodies.

I have already[‡] shown that chlorine and sulphuret of carbon carefully dried are decomposed into a mixture of chloride of sulphur and perchloride of carbon; this takes place not

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[*] Communicated by the Chemical Society; having been read April 7, 1845.

[†] "Corps copulés" of Gerhardt.

[‡] Liebig's *Annal.* vol. xlv. p. 41.

merely at a red heat but at ordinary temperatures, in daylight as well as in the dark. This perchloride of carbon is identical with Regnault's "*ether chlorhydrique de l'esprit de bois perchlorure*," and may be obtained with ease from the chloride of carbon. Perchloride of carbon does not enter into direct combinations, but it unites with sulphurous acid when both are in a nascent state, and forms conjugate, neutral, indifferent bodies, which Berzelius and Marcet discovered when they digested sulphuret of carbon with moistened chlorine or nitric acid*. I call this white, volatile, crystallized compound sulphite of perchloride of carbon, $C\,Cl_2\,SO_2$. It may be obtained in large quantities by digesting peroxide of manganese and hydrochloric acid for several days in a close vessel with sulphuret of carbon, and after the decomposition is complete distilling the mixture. Its formation is explained by the following formula,—

$$\left.\begin{matrix} C\,S_2 \\ 4Cl \end{matrix}\right\} \begin{matrix} C\,Cl_2 \\ 2S\,Cl \end{matrix} + 2HO = \left\{\begin{matrix} C\,Cl_2\,SO_2 \\ 2H\,Cl \\ S. \end{matrix}\right.$$

Sulphite of perchloride of carbon suspended in water is reduced by chloride of tin, sulphuretted hydrogen, or sulphurous acid, and converted with the loss of one equivalent of chlorine into sulphite of chloride of carbon, $C\,Cl\,SO_2$, a colourless, inodorous, and very unstable combination, soluble in water and alcohol. The solution in water is decomposed upon exposure to the air, combining with one equivalent of oxygen, forming oxide of chloride of carbon and sulphurous acid. Chlorine occasions a precipitate of regenerated sulphite of perchloride of carbon. Bromine also produces a similar insoluble combination, which will be the subject of a future examination. Iodine occasions no precipitate. The behaviour of sulphite of perchloride of carbon towards the caustic alkalies gives rise to a series of conjugate hyposulphates, which have the greatest similarity to one another, although their conjunctives, by which they are alone distinguished, are bodies of quite a different chemical nature. The acids thus produced do not show the slightest similarity to sulphurous acid; the chemical properties of the latter are upon formation of the conjoined bodies completely changed, and they have obtained properties and a power of combination which place them by the side of the strongest inorganic acids. The first of these combinations is hyposulphate of chloride of carbon, $HO + C_2\,Cl_3,\ S_2\,O_5$. It contains as a conjunct perchloride of carbon, and is formed together with chloride of potassium when the sulphite of perchloride of carbon is dissolved in caustic potash. The potash salt crystallizes after evaporation of the neutral

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* Berzelius's Chemistry, i. p. 622.

solution in large rhombic tables, which effloresce upon exposure to the air. The crystals contain one atom of water; the effloresced salt is free from water, and is composed of $KO + C_2\,Cl_3,\,S_2\,O_5$. This decomposition is thus explained—

$$\left.\begin{array}{l} 2C\,Cl_2\,SO_2 \\ 2KO \end{array}\right\} \begin{array}{l} KO + C_2\,Cl_3,\,S_2\,O_5 \\ K\,Cl. \end{array}$$

The hydrous hyposulphate of chloride of carbon, which is obtained by precipitation of the baryta salt with sulphuric acid, crystallizes upon evaporation in small prisms, not permanent in the air, which may be heated to 150° without decomposition. They are not oxidized by boiling with nitric, nitromuriatic or chromic acids; nitric and hydrochloric acid may be distilled off from them without any change. The salts of hyposulphate of chloride of carbon are soluble in water and in alcohol, and easily crystallizable. They are decomposed by heat into sulphurous acid, chloro-carbonic oxide gas, and the metallic chloride, which last remains free from sulphuric acid, and is colourless. The barytic salt is formed like the alkaline salt, by solution of the sulphurous perchloride of carbon in baryta water; it crystallizes upon evaporation in large four-sided tables; at 100° C. it is composed of $BaO + C_2\,Cl_3,\,S_2\,O_5 + aq.$ The remaining salts are formed by neutralising the free acid with a basic carbonate. The ammoniacal salt crystallizes most readily in fine large prisms.

Hyposulphate of chloride of formyle, $HO + C_2\,HCl_2,\,S_2\,O_5$, is the second in this class of coupled acids. It is formed by the substitution of one equivalent of hydrogen for one equivalent of chlorine of the hyposulphate of chloride of carbon, when zinc is dissolved by hyposulphate of chloride of carbon, which takes place without the evolution of hydrogen, but with the formation of hydrochloric acid,

$$\left.\begin{array}{l} HO + C_2\,Cl_3,\,S_2\,O_5 \\ 2Zn \end{array}\right\} \begin{array}{l} ZnO + C_2\,HCl_2,\,S_2\,O_5 \\ Zn\,Cl. \end{array}$$

The potash salt, $KO + C_2\,HCl_2,\,S_2\,O_5$, is also obtained by boiling caustic potash with sulphite of chloride of carbon, which combines with one atom of water,

$$\left.\begin{array}{r} 2C\,Cl\,SO_2 \\ KO \\ HO \end{array}\right\} KO + C_2,\,HCl_2,\,S_2\,O_5.$$

This salt is easily soluble in water and hot alcohol, difficultly soluble in cold alcohol; crystallizes from a hot, saturated alcoholic solution in small, shining nacreous leaves; dried at 100° it is anhydrous, and is resolved at 250° C. into hydrochloric acid, sulphurous acid, carbonic acid and oxide of carbon; chloride of potassium slightly coloured by charcoal remains behind. The free acid may be obtained by precipitating the alcoholic solution of the potash salt with sulphuric acid. The filtered acid solution may be evaporated, and the excess of sulphuric acid precipitated either by carbonate of lead or baryta water. The hydrate of the hyposulphate of chloride of formyle forms a thick liquid, which crystallizes, *in vacuo* over sulphuric acid, in small scales, and is similar both in its stability and power of combination to the hyposulphate of chloride of carbon. The silver salt, $Ag\,O + C_2\,HCl_2, S_2\,O_5$, is very sensitive both to light and heat; it can only be obtained in a crystallized state in a vacuum and in the dark; when dry it will bear a temperature of 100° C. without change.

Hyposulphate of chlor-elayle, $HO + 2C_2H_2\,Cl, S_2O_5$, contains chloride of elayle as a conjunct, or another isomeric compound. It is formed by the substitution of two equivalents of hydrogen for two equivalents of chlorine of the hyposulphate of chloride of carbon, when a solution of hyposulphate of chloride of carbon and potash is acidified by sulphuric acid, and decomposed by a galvanic current. Two pairs of the Bunsen carbon and zinc series are required for it; two amalgamated zinc plates were the electrodes.

The average power of the current, as measured by Weber's galvanometer, was 82°, equal to a deflection of the needle of 55°. The hydrogen in the nascent state unites with the chlorine of the hyposulphate of chloride of carbon at the cathode, and forms muriatic acid; the equivalents of chlorine are substituted by equivalents of hydrogen. This exchange only proceeds in the formation of the hyposulphate of chlor-elayle, as long as the fluid which is to be decomposed contains an excess of acid,

$$\begin{array}{l} KO + C_2\,Cl_3\,S_2\,O_5 \\ 4HO \end{array} \left\{ \begin{array}{l} KO + C_2\,H_2\,Cl, S_2\,O_5 \\ 2HCl \\ 2O. \end{array} \right.$$

The oxygen is not set free by the above decomposition, but unites with the zinc of the anode; but if the fluid is so far saturated with sulphate of zinc, that the reduced zinc is deposited on the cathode, then it must be precipitated with carbonate of potash, and the filtered fluid being again rendered acid, must be once more exposed to decomposition by the current. This operation is to be repeated as long as traces of chlorohyposulphate of formyle are perceived, which will be known when the potash salt of the hyposulphate of chlor-elayle, to be extracted out of the fluid precipitated by carbonate of potash and evaporated to dryness, gives out muriatic acid at a red heat. It is impossible to separate both the salts when they occur mixed.

The potash salt of the hyposulphate of chlor-elayle is very much like the salt of hyposulphate of chloro-formyle; it crystallizes out of hot alcohol in small needles, and differs, as before stated, principally by its behaviour at a red heat. It is precipitated by diluted sulphuric acid and chloride of calcium, which remains of a dark black by the separation of carbon; in fine, the hyposulphate of chlor-elayle is quite like the hyposulphate of chloro-formyle. If a platinum anode be used in the above decomposition of the potash-hyposulphate of chloride of carbon, then the free oxygen acts on the compound and oxidizes it. There goes off at the anode a mixture of chlorine, oxygen and carbonic acid, and after the decomposition is finished there is contained in the fluid only hypochlorate of potash and bisulphate of potash. Under the same conditions the solution of chloride of potassium changes into hypochlorite of potash, and ultimately into chlorate of potash; hypochlorite of potash is only formed in the presence of free acid. Iodide of potassium likewise becomes iodate of potash; I could not produce a *fluorate* of potash from the decomposition of fluoride of potassium.

Hyposulphate of methyle = $HO + C_2\,H_3, S_2\,O_5$. This compound concludes the series of conjugate hyposulphites. It contains as conjoined a carbo-hydrogen corresponding to the bichloride of carbon, which possesses the same composition as the radical of methyle. It may be produced in the same manner as the above acid, by means of the galvanic current, from hyposulphate of chloride of carbon and potash when the solution contains free alkali. The decomposition takes place as represented by the following formula:—

$$\begin{matrix} KO + C_2\,Cl_3, S_2\,O_5 \\ 6HO \end{matrix} \left\{ \begin{matrix} KO + C_2\,H_3 \\ 3H\,Cl \\ 6O \end{matrix} \right. = S_2\,O_5.$$

The uncombined hyposulphate of methyle exhibits also the greatest resemblance to the hyposulphate of chloro-formyle, and is produced like it from the alcoholic solution of the pottash salt by precipitating with sulphuric acid. The concentrated acid crystallizes *in vacuo* over sulphuric acid in delicate, easily soluble laminæ, which may be heated to 130° C. without decomposition. It cannot be oxidized by nitric or by chromic acid, and forms only insoluble crystalline salts.

The potash salt, $KO + C_2\,H_3, S_2\,O_5$, at 100° C. is dissolved with difficulty in cold absolute alcohol; and the hot saturated solution crystallizes during the cooling in soft, silky fibres, which form so close a texture, that the mother-water appears gelatinous. On being heated a liquor separates, which has a neutral reaction, and there is also obtained a fetid, volatile product, not further examined: there remains bisulphate of potash mixed with charcoal.

The lead salt, $PbO + C_2 H_3, S_2 O_5 + aq$, crystallizes by spontaneous evaporation in large prisms: it has a feeble acid reaction, a sweetish taste, and when dried at 100° C. is anhydrous. By boiling with oxide of lead a basic salt is produced, $PbO + C_2 H_3 S_2 O_5 + 2PbO$, which becomes dull by exposure to the air from the separation of carbonate of lead, and in a vacuum dries up into an amorphous saline mass.

The silver salt, $AgO + C_2 H_3, S_2 O_5$, admits of evaporation by heat, and only becomes slowly blackened by exposure to the light. It crystallizes in small, thin, anhydrous laminæ, and by a red heat leaves pure metallic silver. All the four conjugate hyposulphates agree further in not uniting with oxide of ethyle to form an æther. In respect to its composition, the hyposulphate of chloro-carbon is ranked with the oxalate of chloro-carbon, or the combination which is known under the name of chlor-acetic acid.

Oxalate of chloro-carbon, $HO + C_2 Cl_3 C_2 O_3$.—As respects the chemical properties of this compound, I have but little to add to the minute investigations of Dumas, as I found that Melsen's statement as to the reproduction of acetic acid by means of the potassium amalgam was perfectly corroborated. Respecting its formation, I may mention that it can be also obtained from the oxidation of chloral by fuming nitric acid, or with a mixture of chlorate of potash and muriatic acid,—

$$C_4 Cl_3 HO_2 + O_2 = HO + C_2 Cl_3 C_2 O_3.$$

Further, chloral is to oxalate of chloro-carbon what aldehyde is to acetic acid. This body, about the constitution of which there have already been so many opinions expressed, is also to be considered as a conjoined compound of formylic acid with perchloride of carbon, $C_2 Cl_3 O_2 HO_2$, an opinion which not only shows the oxidation to form oxalate of chloro-carbon, but also explains simply and sufficiently the origin of aldehyde and the known decomposition into perchloride of formyle and formic acid.

Perchloride of carbon and oxalic acid can unite to form oxalate of chloro-carbon, if they are brought together at the moment of their production. This takes place if chloride of carbon (C Cl) is exposed, under a layer of water, with chlorine to the sunlight. One part of the perchloride of carbon produced is decomposed in the nascent state by water to form muriatic acid and oxalic acid; the latter of which unites with another part of the perchloride of carbon, thus—

$$\left.\begin{array}{l} 2C_2 Cl_3 \\ 4HO \end{array}\right\} \begin{array}{c} HO + C\,2\,Cl\,3\,C_2 O_3 \\ 3H\,Cl \end{array}$$

The oxalate of chloro-carbon remains dissolved in water, and may be ultimately separated from the muriatic acid mixed with it and crystallized, by evaporation *in vacuo* over lime and sulphuric acid. The silver salt of this acid does not contain any hydrogen: it detonates by heating, and becomes black by exposure in the air. 0·396 grm. left by a red heat 0·210 grm. chloride of silver.

0·658 grm. burned with oxide of copper, produced 0·214 grm. of carbonic acid. These numbers correspond in 100 parts to 39·9 of silver and 8·9 of carbon. The formula $AgO + C_2Cl_3C_2O_3$ requires 40·0 silver and 8·9 of carbon.

If we admit these simple facts, then it follows that the acetic acid, which has been until now only known as a product of the oxidation of organic matters, can be also formed synthetically from its elements. Sulphuret of carbon, chloride of carbon, and oxalate of chloro-carbon are the members which in conjunction with the elements of water constitute the means of transition from carbon to acetic acid.

The oxalate of chloro-carbon can also be changed like the hyposulphate of chloro-carbon by the galvanic current, alkali being present, into acetic acid. I have likewise obtained certain signs of the existence of the corresponding members of the hyposulphate of chloro-formyle and hyposulphate of chlor-elayle, which lie between the oxalate of chloro-carbon and acetic acid. All these facts seem to speak in its favour, and make it at the same time highly probable that the acetic acid is a conjoined compound, or, in other words, methyl-oxalic acid, $HO + C_2H_3, C_2O_3$.

I shall communicate these observations in a future paper after the examination is terminated.

It follows from the above, that without doing homage to the new theory of substitution, we may assume, as an undoubted fact, a reciprocal exchange of hydrogen and chlorine. Methyle or perchloride of carbon may be conjoined with hyposulphuric acid, the character of the compound will be little changed by it; but nevertheless, there is no reason for supposing hyposulphate of methyle to be hyposulphate of chloro-carbon, in which hydrogen takes the part of chlorine. The assertion reversed would be equally true. It is a constant fact that several, perhaps, isomorphic combinations *can* be represented as conjuncts of one and the same acid, without an essential change of the acid properties of the conjoined body. The organic bases containing chlorine, which have been newly discovered by Hofmann*, may perhaps be elucidated in a similar point of view.

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* [Phil. Mag. S. 3. vol. xxvi. p. 385.]

XXVIII. *On Toluidine, a new Organic Base.* *By* JAMES SHERIDAN MUSPRATT, *Ph.D.*, *and* AUGUSTUS WILLIAM HOFMANN, *Ph.D.*[*]

THE artificial formation of different compounds hitherto considered as exclusively the products of the vital process has been during the last ten years among the most interesting results furnished by the study of the metamorphoses of organic bodies. We have found in *cyanic acid* and *ammonia* the compounds by whose union *urea* is produced; by the oxidation of *uric acid* with pure oxide of lead *allantoin* formed, a crystallizable matter existing in the allantoic fluid of the cow; and *salicine* and *fusel oil*, when properly treated with substances rich in oxygen, furnish us the acids produced in the process of the vegetation of *Spiræa Ulmaria* and *Valeriana officinalis*; and, lastly, the composition and properties of the volatile oil of *Gaultheria procumbens* were only necessary to be known for its artificial production to succeed immediately in the hands of the chemist.

Still the greater part of the researches made during the last ten years in organic chemistry have been of a purely *analytical* nature. Although the metamorphoses of a considerable number of organic bodies have been studied, still this has not been with the view of obtaining certain compounds which suggested themselves to the theoretical inquirer, but rather with the intention of drawing conclusions as to the composition and properties of the body which was the starting-point of the investigation from the properties and nature of the products of its decomposition.

From such inquiries we have reaped a rich harvest of experience, they have made us somewhat better acquainted with the transformations which an organic substance suffers under the influence of the most different agents, and have thus qualified us for attempting the formation of a given compound in one or the other way. But few such *synthetical* experiments have as yet been performed; it is likely that a great number will be made without success, which may be probably owing to suppositions contrary to nature; but it cannot be doubted that we shall proceed in this direction with greater certainty when a number of even unsuccessful trials of this kind has been undertaken.

The artificial production of bodies occurring in nature presents at first a purely theoretical interest, but all will agree that such endeavours may become of the highest practical importance when they consider those compounds which now

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[*] Communicated by the Chemical Society; having been read April 7, 1845.

take a high place in medicine, the arts and commerce. The valerianic acid is already so often employed in medicine, that its production upon a large scale from fusel oil cannot long be postponed, and this becomes the more feasible, as we can obtain the latter in such large quantities as a secondary product in rectifying distilleries. Of what influence would be the invention of a process for procuring the medicinal vegetable alkaloids in a simple artificial way? If a chemist should succeed in transforming in an easy manner *naphthaline* into *quinine*, we would justly revere him as one of the noblest benefactors of our race.

Such a transformation has not as yet succeeded, but this does in nowise show its impossibility. We have become acquainted in the last ten years with a remarkable series of artificial organic bases, and, with the exception of urea, which in many respects differs from the other organic bases, there is *none* which has been met with in nature, but there are many among them bearing the greatest similarity with the natural ones in properties and composition.

The artificial bases which we now possess have been obtained in very different ways. The first bases procured by Liebig*, *melamine*, *ammeline* and *ammelide*, were produced by the decomposition of sulphocyanide of ammonium by heat; and others, such as *aniline*† and *chinoline*‡, were formed by fusing alkalies with organic matters, or by distillation only, as the first mentioned and *lophine*, recently discovered by Laurent§. Basic bodies have further been produced by the action of ammonia upon organic compounds. To these belong the *Thiosinnamine* of Will‖, and *amarine* obtained by Laurent** from the hydruret of benzoyle, and another highly remarkable base newly prepared by Fownes†† from the so-called artificial oil of ants. Lastly, chemists have succeeded in replacing the sulphur in sulphuretted compounds by oxygen, and thus obtaining new bodies possessing basic properties. We see examples of this kind in *sinnamine* formed by Varrentrapp and Will from the *thiosinnamine*; and Simon's *sinapoline*, obtained by the desulphuration of oil of mustard.

These modes, however, for the formation of organic bases are only applicable in a very few cases, as the bodies from which they were derived were themselves more or less insu-

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* *Annal. der Chem. und Pharm.*, vol. x. p. 1.

† Fritzsche, *Annal. der Chem. und Pharm.*, vol. xxxvi. p. 84.

‡ Gerhardt, *Annal. der Chem. und Pharm.*, vol. xlii. p. 310.

§ *Compt. Rend.* vol. xviii. p. 1016.

‖ *Annal. der Chem. und Pharm.*, vol. lii p. 8.

** *Compt. Rend.*, vol. xix. p. 353.

†† Phil. Trans., 1845. [Phil. Mag. S. 3. vol. xxvi. p. 254.]

lated and peculiar, and therefore till chemists succeed in discovering more compounds of the same classes, the formation of basic bodies in this manner must be limited.

It is quite different with another method, by which chemists have also succeeded of late years in procuring basic bodies. Zinin* was the first who conceived the happy idea of investigating the products produced by the action of sulphuret of ammonium upon nitrogenous compounds, which latter were procured by treating various carbo-hydrogens with nitric acid. Zinin investigated in this point of view Laurent's *nitronaphthalase* obtained by the action of nitric acid upon naphthaline, and *nitrobenzide*, discovered by Mitscherlich, and he arrived at the remarkable result that these bodies lose their whole oxygen under the influence of sulphuretted hydrogen, and in assuming hydrogen pass into combinations, presenting in every respect the characters of true organic bases; the two bases formed by Zinin in the manner described, are the *naphthalidam*, for which Berzelius† has lately proposed the more appropriate name of naphtalidine, and *benzidam*, afterwards recognised as identical with aniline. The following comparison of formulæ shows how nearly the original compounds are connected with the required products:—

$$\text{Nitronaphthalase } C_{20}\,H_7\,NO_4 = C_{12}\left\{\begin{matrix}H_7\\NO_4\end{matrix}\right.$$

$$\text{Naphtalidine . . } C_{20}\,H_9\,N = C_{12}\left\{\begin{matrix}H_7\\Ad\end{matrix}\right.$$

$$\text{Nitrobenzide . } C_{12}\,H_5\,NO_4 = C_{12}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right.$$

$$\text{Aniline . . . . } C_{12}\,H_7\,N = C_{12}\left\{\begin{matrix}H_5\\Ad\end{matrix}\right.$$

By this comparison the transformation occurring through sulphuret of ammonium may be considered as ultimately a substitution of the elements of amidogen for those of peroxide of nitrogen, independently of these combinations existing or not in the compounds. Zinin's discovery is very remarkable in its consequences, and will, without doubt, become of vast importance for the group of alkaloids. If we consider how large is the number of carbo-hydrogens already known, all of which change their composition when acted upon by nitric acid, giving compounds corresponding to nitronaphthalase and nitrobenzide; then, supposing these products to suffer also an analogous decomposition with sulphuret of ammonium, there will be no limit to the production of new bases, and we may naturally infer that even those occurring in nature might be

* *Annal. der Chem. und Pharm.*, vol. xliv. p. 283.

† *Jahresbericht*, xxii. p. 545.

produced if we only succeed in obtaining the appropriate carbo-hydrogen.

A greater correspondence as to properties and chemical character cannot be imagined than that subsisting between aniline and the two following bases: *nicotine* contained in the fresh leaves of the tobacco plant, and *coniine* found by Geiger in all parts of the hemlock (*Conium maculatum*).

According to the analysis of Ortigosa and Belard, lately corrected by Melsens*, nicotine is represented by the formula

$$C_{10}H_7N,$$

and coniine, by Ortigosa's† formula, which has yet to be confirmed, by

$$C_{16}H_{16}N\ (?).$$

Now if we could succeed in obtaining the carbo-hydrogens

$$C_{10}H_6$$
$$C_{16}H_{15},$$

there would be no difficulty in procuring, in an artificial way, nicotine and coniine, *i. e.* by treating the product of the action of fuming nitric acid upon the above hydrocarbons with sulphuret of ammonium. We should have—

$$C_{10}H_6 \text{ unknown.}$$
$$C_{10}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}\text{unknown.}$$
$$C_{10}\left\{\begin{matrix}H_5\\NH_2\end{matrix}\right\}\text{nicotine.}$$
$$C_{16}H_{15} \text{ unknown.}$$
$$C_{16}\left\{\begin{matrix}H_{14}\\NO_4\end{matrix}\right\}\text{unknown.}$$
$$C_{16}\left\{\begin{matrix}H_{14}\\NH_2\end{matrix}\right\}=\text{coniine.}$$

The hypothetical hydro-carbons above cited have not hitherto been obtained; but when we consider how many decompositions yield carbo-hydrogens, and also that the destructive distillation of organic matters promises to furnish us with an inexhaustible supply of these compounds; and as we recognise daily, produced in these processes of transformation, new bodies thoroughly analogous to those looked for, we need not despair of obtaining those sought; and thus, by the destruction of organic compounds, open a new source for the formation of products generated in other ways, and which are produced in the vegetation of plants.

Considerations such as these have led us to undertake

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* *Ann. der Chem. und Pharm.*, vol. xlix. p. 353.

† Ibid. vol. xlii. p. 313.

a series of investigations concerning the action of sulphuret of ammonium upon various bodies containing, like nitronaphthalase and nitrobenzide, the elements of peroxide of nitrogen.

There occurred in this inquiry some questions the answers to which appeared of interest. Above all, it was necessary to decide whether the transformations suffered by nitronaphthalase and nitrobenzide were identical with those of all similarly constituted bodies. Secondly, it was requisite to ascertain how hydrocarbons behave in which more than one equivalent of hydrogen is replaced by peroxide of nitrogen. Here very different results might be imagined. Lastly, it was of the greatest value to study the properties of those bodies resulting from treating with sulphuret of ammonium the products of the action of nitric acid upon *oxides of hydrocarbons.*

The following is the first of a series of investigations made with regard to the preceding statements. The experiments were performed in the Giessen laboratory, and therefore we cannot refrain from expressing our greatest thankfulness to Professor Liebig for his kind assistance and counsel during the course of them. We commenced by a careful repetition of Zinin's experiments upon the formation of *naphthalidine*, and have procured this body possessing all the properties ascribed to it by that chemist. As to the production of aniline from nitrobenzide, every information has already been given upon that subject*; in fact, we must confirm Zinin's statements in every respect. From among the numerous carbo-hydrogens offering themselves for an investigation we selected two, which, from their particular analogy with benzole, seemed most appropriate for carrying out our object. These are *toluole* and *cumole.* We shall in this treatise communicate the experiments upon toluole.

Preparation of Toluole.

Toluole was some years ago recognised by Deville† among the products of the distillation of Tolu balsam, and this chemist described it under the name benzoene, which indeed is not happily chosen. Berzelius‡ has given to this body the name *toluine* or toluole, which last we shall retain in our communications. We have proceeded in the preparation of toluole according to the directions of Deville, which we willingly confirm. Eighteen to twenty pounds of the balsam of Tolu

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[*] Vide Hofmann's investigation upon Coal-gas Naphtha, Philosophical Magazine, vol. xxiv. pp. 115, 193, 261.

[†] *Ann. de Chim. et Phys.* 3 sér. vol. iii. p. 168.

[‡] *Jahresbericht,* vol. xxii. p. 354.

were submitted to distillation in glass retorts, partly over an open fire and partly on a sand-bath. There passes over at first aqueous vapour, and when the temperature is sufficiently elevated, a large quantity of benzoic acid (with a slight admixture of cinnamic acid), which solidifies immediately in the recipient into a white crystalline mass. As soon as the fluid distilling over no longer solidified the receiver was changed, and then there condensed a yellow liquid*, which is, according to Deville, a mixture of *toluole* and *benzoic æther*. When it had cooled—a quantity of benzoic acid previously dissolved was deposited—we redistilled and only collected the portions passing over from 130°—140° C. This distillate is deprived of the last traces of benzoic acid by being repeatedly distilled with pieces of solid potash. The fluid obtained in this way possessed nearly all the properties ascribed by Deville to toluole. The product, however, in this manner is extremely small when compared with the quantity of Tolu balsam employed. We have, on account of this, endeavoured to discover another source for getting the toluole in larger quantities, but without success; we therefore deem it necessary to mention this, as it may spare perhaps other vain attempts.

Cahours† has occasionally found in his investigation upon gaultheria oil that the salicylate of the oxide of methyle, when distilled with caustic barytes, furnishes anisole, the same product that the anisic acid isomeric with the former yields under the same circumstances.

$$\underbrace{C_{16}\,H_8\,O_6}_{\substack{\text{Salicylate of the}\\\text{oxide of methyle.}}} + 2BaO = \underbrace{C_{14}\,H_8\,O_2}_{\substack{\text{Anisole.}\\\text{Dracole.}}} + 2(BaO, CO_2).$$

This unexpected transformation seemed to point out a new source for the production of manifold hydrurets of carbon, by treating in a similar manner the various compound æthers.

Anisole differs only from toluole in two equivalents of oxygen.

$$\begin{array}{ll}\text{Anisole} & C_{14}\,H_8\,O_2\\ \text{Toluole} & C_{14}\,H_8\end{array}$$

The question now presented itself as to whether toluole could not be produced by the action of caustic barytes or caustic lime upon an æther containing two equivalents less of oxygen than the salicylate of the oxide of methyle? Such an æther is the benzoate of the oxide of methyle:—

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* The crystalline mass collected in the first receiver contains likewise a large quantity of the same liquid, which can be separated by mere distillation with water.

† *Ann. de Chim. et de Phys.* 3 sér. vol. x. p. 353.

$$\underbrace{C_{16}\,H_8\,O_4}_{\substack{\text{Benzoate of the}\\ \text{oxide of methyle.}}} + 2BaO = \underbrace{C_{14}\,H_8}_{\substack{\text{Toluole.}\\ \text{Benzoene.}}} + 2(BaO,\,CO_2)?$$

Deville has given benzoate of the oxide of ethyle as one of the products of distillation of Tolu balsam; but if we consider how minute is the difference per cent. between the ethyle and methyle compounds of benzoic acid, as also that Deville did not either analyse the alcohol from the former, or change it into acetic acid, we may readily infer that his compound is not benzoic æther, but benzoate of methyle, which by the temperature acquired in the distillation is partly converted into *toluole* and carbonic acid. Considerations of this kind prompted us to study the action of caustic lime upon benzoate of the oxide of methyle. We passed the vapour of this body over incandescent lime previously strewn in a combustion tube, and obtained an oil which was not dissolved by potash ley. From its comportment and analysis it proved to be benzole and not toluole. It solidified at 0° C. into a crystalline mass resembling loaf-sugar, and could be converted readily into aniline. The transformation which the benzoate of the oxide of methyle undergoes is therefore of a different kind, and there is probably generated olefiant gas, or an isomeric compound, which we did not however collect.

$$\underbrace{C_{16}\,H_8\,O_4}_{\text{Benzoate of methyle.}} + 2CaO = \underbrace{C_{12}\,H_6}_{\text{Benzole.}} + \underbrace{C_2\,H_2}_{\text{Olefiant gas.}} + 2(CaO,\,CO_2)\,?$$

The metamorphosis observed in the salicylate of the oxide of methyle appears therefore to be peculiar only to the æthers of those acids whose hydrates contain six atoms of oxygen.

Preparation of Toluidine.

We acted upon toluole with concentrated nitric acid, and on the addition of water procured an oily fluid perfectly analogous to nitrobenzide. This substance is nitrotoluide, and when it is dissolved in alcohol, saturated with ammoniacal gas, treated with sulphuretted hydrogen, and then allowed to repose in a corked flask for some days, a beautiful crystallization of sulphur appears, and the liquid loses the smell of sulphuretted hydrogen. It must now be saturated anew with hydrosulphuric acid, and this process repeated until that gas no longer disappears. This method is very convenient, but tedious, as to obtain a sufficient quantity of the base for an investigation two or three months are required. To hasten the process, it is necessary, after each treatment with sulphuretted hydrogen, to distil the whole liquid, whereby the deposition of sulphur is quickened, although some of the gas escapes decomposition. After the separation of the sulphur the residue in the retort must be mixed with the distillate, the whole treated again with hydrosulphuric acid, and distilled. This process must be repeated five or six times successively. The decomposition of nitrotoluide by sulphuret of ammonium, as well as the bodies similarly constituted, takes place with great difficulty. Even after a very long exposure to the influence of sulphuretted hydrogen and endless distillations, a small quantity of the nitrotoluide remains intact, which is readily seen by adding water to the treated menstruum, the hydrochloric acid in excess filtering off the sulphur, and agitating the filtrate with æther. On evaporating the æthereal extract, undecomposed nitrotoluide will remain. The dark red acid solution remaining, after draining off the æthereal solution of nitrotoluide, on being evaporated to about one-third of its original volume, in order to expel the alcohol, and then submitted to distillation with sticks of potash, gives off in addition to aqueous vapour and ammonia, a colourless or slightly yellowish oil, which sinks in the receiver and solidifies after some time into a crystalline mass. This body is *a new organic base*, for which we propose the name *toluidine*. Its purification is very simple. The distillate, which was still slightly alcoholic, contained some of the base in solution; we therefore supersaturated the whole with oxalic acid, evaporated to dryness in a water-bath, treated the residue with boiling alcohol, and filtered. The oxalate of toluidine percolates through, while the oxalate of ammonia, which is insoluble in this menstruum, remains. When the filtrate cools, the oxalate of the new base separates almost entirely in fine white needles. These were edulcorated, then dissolved in boiling water and decomposed by a strong potash ley. The base immediately separated in the form of colourless oil-drops, which after some time rose to the surface as an homogeneous layer, and in cooling solidified into a radiated crystalline mass. This was ejected, thrown upon a filter, washed with distilled water until the liquid percolating possessed no more an alkaline reaction, and then desiccated between folds of bibulous paper. The dry crystalline mass required only to be again rectified in order to obtain it chemically pure. During this last process it first melts and then there distils over a beautiful fluid dispersing light in an eminent degree, and which, after some seconds, solidifies into a colourless transparent crystalline mass.

Composition of Toluidine.

The perfect analogy subsisting between benzole and toluole, so evident in every point of view, left in our minds no doubt as to the composition of the new base. We have notwithstanding submitted it to elementary analysis, but have omitted the determination of the nitrogen.

By a combustion with oxide of copper, 0·1955 grm. toluidine gave 0·5630 grm. carbonic acid and 0·1515 grm. water.

Centesimally,—

Carbon	78·53
Hydrogen . . .	8·61

Agreeing with the formula $C_{14}\ H_9\ N$; which we append in a calculated form.

		Theory.		Found.
14 equiv.	Carbon .	1050·00	78·38	78·53
9 ...	Hydrogen	212·50	8·39	8·61
1 ...	Nitrogen .	177·00	13·23	12·86
		1339·5	100·00	100·00

In order to control this formula, we prepared the double compound of the base with chloride of platinum. On igniting this fine crystalline compound, 0·4337 grm. of it gave 0·1372 grm. = 31·36 platinum.

Atomic weight from experiment	1359·50
Atomic weight theoretical . .	1339·50

The formation of toluidine from nitrotoluide is perfectly analogous to that of aniline from nitrobenzide:

$$\underbrace{C_{14}\ H_7\ NO_4}_{\text{Nitrotoluide.}}+4\ HS=\underbrace{C_{14}\ H_9\ N}_{\text{Toluidine.}}+4\ HO+6\ S$$

Properties of Toluidine.

The new base, as previously stated, is obtained in the form of a colourless crystalline mass. From a hot saturated solution in aqueous alcohol it shoots, upon cooling, into large broad plates, which fill the whole menstruum. It dissolves in æther, pyroxylic spirit, acetone, sulphuret of carbon, the fatty and essential oils with the same facility as in alcohol. Water also dissolves a small quantity of toluidine, particularly with heat, and after some time it separates in extremely small leafy crystals, exhibiting a splendid iridescence. On agitating with æther the base is extracted from the water.

Toluidine possesses a vinous aromatic smell and burning taste. The remarkable correspondence as to smell and taste between benzole and toluole, nitrobenzide and nitrotoluide, is also recognised between toluidine and aniline. These bases could not possibly be distinguished one from the other by smell. Toluidine has no action upon curcuma paper, but it colours green the pigment of dahlias. Reddened litmus paper is faintly blued. Toluidine is heavier than water. It evaporates at all temperatures, and when a glass rod moistened with hydrochloric acid is held over it white fumes are perceptible; this also takes place with nitric acid, but in a less degree. When pressed with the hand between paper it leaves a slight stain, which is however very fugitive. The fusing-point of this base lies at 40° C., the boiling-point exactly at 198° C.

Some years ago Professor Kopp * drew attention to the remarkable relation subsisting between the boiling-points of bodies whose composition differs in a constant number of equivalents of the same elements. He found, for instance, that the difference in the boiling-points of two compounds which differ by two equivalents of carbon and two equivalents of hydrogen ($C_2 H_2$) is invariably 19° C. Kopp showed this at first by comparing the boiling-points of a vast number of ethyle and methyle compounds, and then the numerous hydrated acids differing in $C_2 H_2$. Since that time this concurrence between a series of newly-discovered æthers and acids has been perfectly confirmed, but it seems to take place only in bodies of an analogous character, as remarked by Kopp and subsequently by Fehling †.

We possess in benzole and toluole and their derivatives also two series of analogous bodies, whose composition is distinguished by the constant difference $C_2 H_2$. It was of interest to compare the boiling-points of these bodies in the sense referred to.

Boiling-point.	Difference.
Benzole 86° C.—Mitscherlich . . .	22°
Toluole, 108°.—Deville	
Nitrobenzide 213°.—Mitscherlich . .	12°
Nitrotoluide 225°.—Deville	
Aniline 182°.—A. W. Hofmann . .	16°
Toluidine 198°.—Muspratt & Hofmann	

This comparison shows undoubtedly that Kopp's rule holds good with regard to the above-cited bodies.

The differences evidently arise from mistakes in observation. As for the boiling-points of nitrobenzide and nitrotoluide, the somewhat considerable variation can by nowise surprise, knowing as we do with what readiness bodies of this nature undergo decomposition when submitted to distillation.

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* *Ann. der Chem. und Pharm.*, vol. xlii. p. 79. [Phil. Mag. S. 3. vol. xx, p. 187.]

† Ibid., vol. liii. p. 410.

It is very probable that the boiling-point of nitrotoluide is higher, and as for toluole its boiling-point by Deville also does not appear exact.

We will prove in a future paper that dracyle, a hydrocarbon obtained in the destructive distillation of dragon's blood by Bondault and Glénard*, is *identical* with toluole. Glénard and Bondault give as the boiling-point of their liquid 106° C. According to this determination the difference between it and benzole would only be 20°.

From the preceding remarks an uncertainty is removed which has reigned with regard to the boiling-point of aniline, which according to Fritzsche is 228° C.; his product being obtained from indigo. The foregoing statement refers to aniline (cyanole) procured from coal-gas naphtha. It is therefore perfectly clear that there must have been some error in Fritzsche's determination.

Toluidine shares with aniline, and in general with the bases obtained in a similar manner, the property in an acid solution of imparting an intense yellow colour to fir-wood and the pith of the elder-tree. But it does not afford the beautiful reaction with hypochlorite of lime which characterizes aniline; an aqueous solution of toluidine when mixed with liquid chloride of lime gives a pinkish hue. It also differs from aniline in its behaviour towards fuming nitric acid and chromic acid. Toluidine gives with *nitric acid* a fine scarlet colour, whereas aniline assumes an indigo blue tinge.

With *chromic acid* it gives a reddish brown precipitate, which appears to be chromate of toluidine. It does not inflame when brought into contact with crystals of chromic acid.

The following will suffice as to the behaviour of toluidine towards the solutions of the metallic oxides.

Sulphate and chloride of copper give greenish precipitates, possessing a crystalline aspect. *Chloride of iron* deposits hydrated peroxide of iron when heated with toluidine.

Toluidine gives with nitrate of silver a white crystalline precipitate, evidently a double salt, which easily blackens. This base gives with chloride of platinum and chloride of palladium beautiful orange-yellow crystalline precipitates, of which the latter is somewhat lighter than the former.

Toluidine furnishes no particular appearances with other reagents.

Compounds of Toluidine.

The same extraordinary capacity for crystallizing which we have in the salts of aniline also distinguishes those of toluidine.

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* *Journal de Pharm. et de Chem.*, 3 sér. vol. vi. p. 250.

An alcoholic solution of this base solidifies almost immediately when mixed with most acids, and in order to obtain the salts in good crystals it is only necessary to employ water or spirits of wine as the solvent. The compounds are inodorous, and with the exception of the palladium and platinum salt colourless. When they are exposed, particularly in a moist state, to the air, they acquire a pinkish hue like the aniline compounds. They are decomposed by the caustic or carbonated alkalies and ammonia with the greatest facility; the toluidine separating in crystalline grains.

The constitution of the salts of toluidine resembles that of the aniline and ammoniacal salts.

Sulphate of toluidine, $SO_3 + C_{14} H_9 N, HO$.—Toluidine was dissolved in æther and then a few drops of sulphuric acid added, when immediately a shining white crystalline precipitate appeared, which was collected on a filter and edulcorated with more of the solvent. This salt is difficultly soluble in alcohol, but more readily dissolved by æther.

Analysis.

0·4102 grm. of sulphate of toluidine gave 0·3015 grm. of sulphate of barytes=25·22 per cent. sulphuric acid.

This determination agrees with the formula

$$SO_3 + C_{14} H_9 N, HO,$$

as the subjoined comparison shows.

	Theory.		Found.
1 equiv. Sulphuric acid	500·00	25·60	25·22
1 ... Toluidine . .	1339·50	68·67	
1 ... Water . . .	112·50	5·73	
	1952·00	100·00	

Binoxalate of toluidine, $2 C_2 O_3 + C_{14} H_9 N, HO + 2$ aq. —The salt obtained on mixing a solution of toluidine in alcohol with an excess of oxalic acid is a binoxalate. It appears in silky needles, which are sparingly soluble in cold water and alcohol and quite insoluble in æther. This salt is more readily dissolved by spirits of wine and boiling water. The solution has a strong acid reaction and a saline unpleasant styptic after-taste. The combustion of this compound with chromate of lead afforded the following numbers :—

0·3975 grm. of the oxalate gave 0·7575 grm. carbonic acid and 0·2120 grm. of water.

Centesimally,—

Carbon	51·99
Hydrogen . . .	5·90

The formula of the acid salt is in perfect accordance with the above results, $2\ C_2\ O_3 + C_{14}\ H_9\ N,\ HO + 2$ aq, which we append with the calculated composition.

	Theory.		Found.
18 equiv. Carbon . . .	1350	52·37	51·99
12 ... Hydrogen . .	150	5·82	5·90
1 ... Nitrogen . . .	177	6·97	
9 ... Oxygen . . .	900	34·84	
	2577	100·00	

In order to verify the formula, we also determined the oxalic acid of the salt by dissolving a portion in boiling water, supersaturating with ammonia and adding chloride of calcium.

0·4835 grm. of binoxalate gave 0·2330 grm. of carbonate of lime = 34·33 per cent. anhydrous oxalic acid.

	Theory.		Found.
2 equiv. Oxalic acid .	900·00	34·92	34·33
1 ... Toluidine . .	1339·50	52·01	
3 ... Water . . .	337·50	13·07	
	2577·00	100·00	

This compound possesses therefore the same constitution as the binoxalate of ammonia. Napththalidine and chloraniline form salts of the same composition.

Hydrochlorate of toluidine, $Cl\ H,\ C_{14}\ H_9\ N$.—It is obtained by evaporating a solution of toluidine in muriatic acid. Upon cooling this salt deposits in scaly crystals, which in a pure state are white, but when exposed to the air assume a yellowish colour. They dissolve copiously in water and alcohol, sparingly in æther. The solution has an acid reaction. When the crystals are slightly heated in a tube they sublime like sal-ammoniac.

Analysis of the salt.

0·3590 grm. of hydrochlorate of toluidine gave 0·3568 grm. of chloride of silver = 25·29 per cent. hydrochloric acid, agreeing perfectly with the formula $ClH,\ C_{14}\ H_9\ N$, which is specified in the following calculated per cents.:—

1 equiv. Hydrochloric acid	455·15	25·36	25·29
1 ... Toluidine . . .	1339·50	74·64	
	1794·65	100·00	

Platinchloride of Toluidine, $ClH,\ C_{14}\ H_9\ N + Pt\ Cl_2$.—It is obtained as an orange-yellow spangular mass when toluidine, hydrochloric acid and chloride of platinum are mixed together. The crystals were washed with a mixture of alcohol and æther and then desiccated in a water-bath. We have already alluded to the determination of the platinum in this salt when speaking of the atomic weight. The analysis gave 31·36 per cent. of platinum.

The formula $ClH, C_{14} H_9 N + Pt Cl_2$ agrees with 31·51 per cent. platinum.

We have not examined the other salts of toluidine, and shall therefore only remark that the nitrate and phosphate are obtained in crystals on bringing the respective acids in contact with toluidine.

Products of the Decomposition of Toluidine.

We should have been exceedingly glad to have submitted toluidine to the action of different agents, but the difficulty of obtaining this body in sufficient quantity prevented us from satisfying our wishes. We can therefore only give a few reactions, which we had the opportunity of remarking during the course of the investigation. When the vapour of toluidine is passed over fused *potassium*, an energetic reaction ensues, and there is formed a quantity of *cyanide of potassium*, as was readily ascertained by the residue giving off hydrocyanic acid when treated with a mineral acid. With concentrated *nitric acid* the base is decomposed, and a large quantity of nitrous acid eliminated. On mixing the solution with water, sulphur-yellow flakes are deposited, which dissolve in alkalies with a brownish-red colour, and reappear on the addition of an acid. This behaviour is analogous to that of nitrophenisic acid, but we are not in a position at present to communicate anything more definite concerning this substance. When this base is treated with a solution of bromine a violent action ensues, accompanied by an elevation of temperature and formation of hydrobromic acid. When the product is heated in a test-tube, there sublimes in the upper part white, shining needles, which are soluble in alcohol and æther, but insoluble in water. They do not possess basic properties, and behave in every respect like tribromaniline; it is highly probable that their composition is also similar. We have not yet procured a sufficient quantity of these crystals in order to verify by experiment the annexed formula,—

$$C_{14}\left\{\begin{matrix}H_6\\Br_3\end{matrix}\right\}N.$$

The effect of chlorine upon toluidine has not been at all studied, but it is highly probable that in this reaction there would be formed collaterally with trichlorotoluidine derivative products of substitution of anisole.

We possess in the indigo, salicyle and phenyle families, well-examined groups of organic bodies, which are in the most manifold manner connected with each other. There appears to be parallel with these another extended series of corresponding compounds, which differ only in $C_2 H_2$, but as yet there are many interruptions, so that the group of indigo is entirely wanting.

In toluidine we have described one member which hitherto was unknown; the others requisite will also certainly be discovered before long.

The following is a view of these two series of compounds:—

Indigo, Salicyle and Phenyle series.		Anisyle series, &c.	
Indigo	$C_{16} H_5 NO_2$		
Isatine	$C_{16} H_5 NO_4$		
Chlorisatine	$C_{16}\left\{\begin{matrix}H_4\\Cl\end{matrix}\right\}NO_4$		
Dichlorisatine	$C_{16}\left\{\begin{matrix}H_3\\Cl_2\end{matrix}\right\}NO_4$		
Salicylic acid	$C_{14} H_6 O_6$	Anisic acid	$C_{16} H_8 O_6$
Bromosalicyle	$C_{14}\left\{\begin{matrix}H_5\\Br\end{matrix}\right\}O_6$	Bromanisic acid	$C_{16}\left\{\begin{matrix}H_7\\Br\end{matrix}\right\}O_6$
Dibromosalicyle	$C_{14}\left\{\begin{matrix}H_4\\Br_2\end{matrix}\right\}O_6$		
Tribromosalicyle	$C_{14}\left\{\begin{matrix}H_3\\Br_3\end{matrix}\right\}O_6$		
Nitrosalicylic acid Indigotic acid	$C_{14}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}O_6$	Nitranisic acid	$C_{16}\left\{\begin{matrix}H_7\\NO_4\end{matrix}\right\}O_6$
Wanting		Bromonitrasinc	$C_{16}\left\{\begin{matrix}H_6\\Br\\NO_4\end{matrix}\right\}$
Phenole Hydrate of phenyle	$C_{12} H_6 O_2$	Anisole	$C_{14} H_8 O_2$
Bromophenole Bromophenasic acid	$C_{12}\left\{\begin{matrix}H_5\\Br\end{matrix}\right\}O_2$	Bromanisole	$C_{14}\left\{\begin{matrix}H_7\\Br\end{matrix}\right\}O_2$ (?)
Dichlorophenole Chlorophenissic acid	$C_{12}\left\{\begin{matrix}H_4\\Cl_2\end{matrix}\right\}O_2$	Dibromanisole	$C_{14}\left\{\begin{matrix}H_6\\Br_2\end{matrix}\right\}O_2$
Trichlorophenole Chlorophenissic acid	$C_{12}\left\{\begin{matrix}H_3\\Cl_3\end{matrix}\right\}O_2$		
Pentachlorophenole Chlorophenusic acid	$C_{12}\left\{\begin{matrix}H\\Cl_5\end{matrix}\right\}O_2$		
Dinitrophenole Nitrophenesic acid	$C_{12}\left\{\begin{matrix}H_4\\2NO_4\end{matrix}\right\}O_2$	Dinitranisole	$C_{14}\left\{\begin{matrix}H_6\\2NO_4\end{matrix}\right\}O_2$
Trinitrophenolc Carbazotic acid Nitrophenisic acid	$C_{12}\left\{\begin{matrix}H_3\\3NO_4\end{matrix}\right\}O_2$		
Anthranilic acid	$C_{14} H_7 NO_4$		
Salicylous acid	$C_{14} H_6 O_4$		
Benzoic acid	$C_{14} H_6 O_4$		
Nitrobenzoic acid	$C_{14}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}O_4$		
Benzole	$C_{12} H_6$	Toluole	$C_{14} H_8$
Sulphobenzide	$C_{12}\left\{\begin{matrix}H_5\\SO_2\end{matrix}\right\}$	Sulphotoluide	(?)
Hyposulphobenzidic acid	$C_{12} H_5 S_2 O_5$	Hyposulphotoluidic acid.	$C_{14} H_7 S_2 O_5$

Indigo, Salicyle and Phenyle series.		Anisyle series, &c.	
Chlorbenzole	$C_{12}\left\{\begin{matrix}H_3\\Cl_3\end{matrix}\right\}+3ClH$	Products of the action of Chlorine upon Toluole.	$\left\{\begin{array}{l}C_{14}\left\{\begin{matrix}H_7\\Cl\end{matrix}\right\}\\C_{14}\left\{\begin{matrix}H_5\\Cl_3\end{matrix}\right\}+ClH\\C_{14}\left\{\begin{matrix}H_3\\Cl_3\end{matrix}\right\}+2ClH\\C_{14}\left\{\begin{matrix}H_3\\Cl_5\end{matrix}\right\}+3ClH\\C_{14}\left\{\begin{matrix}H_2\\Cl_6\end{matrix}\right\}\end{array}\right.$
Chlorbenzide	$C_{12}\left\{\begin{matrix}H_3\\Cl_3\end{matrix}\right\}$		
Nitrobenzide	$C_{12}\left\{\begin{matrix}H_5\\NO_4\end{matrix}\right\}$	Nitrotoluide	$C_{14}\left\{\begin{matrix}H_7\\NO_4\end{matrix}\right\}$
Binitrobenzide	$C_{12}\left\{\begin{matrix}H_4\\2NO_4\end{matrix}\right\}$	Binitrotoluide	$C_{14}\left\{\begin{matrix}H_6\\2NO_4\end{matrix}\right\}$
Azobenzide	$C_{12}H_5N$		
Aniline	$C_{12}H_7N$	Toluidine	$C_{14}H_9N$
Bromaniline	$C_{12}\left\{\begin{matrix}H_6\\Br\end{matrix}\right\}N$		
Dibromaniline	$C_{12}\left\{\begin{matrix}H_5\\Br_2\end{matrix}\right\}N$		
Tribromaniline	$C_{12}\left\{\begin{matrix}H_4\\Br_3\end{matrix}\right\}N$		
Chlorodibromaniline	$C_{12}\left\{\begin{matrix}H_4\\Cl_2\\Br\end{matrix}\right\}N$		
Nitraniline	$C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N$		

We have given at the close of the table a body which has not yet been described. This is the *nitraniline*,—

$$C_{12}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}N,$$

aniline, in which *one equivalent of hydrogen* is replaced by the elements of *peroxide of nitrogen*. In continuing our researches on the substitution products of aniline, and upon which one of us has lately published *, we tried to produce the above compound in a variety of ways, but we have only arrived at a satisfactory result by acting upon binitrobenzide with sulphuret of ammonium.

The new body in which the electro-positive properties of aniline are preserved, crystallizes, affords crystallizable compounds possessing the same constitution as those of aniline, and undergoes distillation without being decomposed. Chemistry has no analogue to nitraniline. It is the first body in which basic properties are recognisable in spite of the entrance of peroxide of nitrogen, and does on this account therefore deserve the attention of chemists.

We shall shortly communicate to the Society our results upon nitraniline. In concluding this treatise we shall draw

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* Hofmann, Metamorphoses of Indigo. Phil. Mag., vol. xxvi. p. 385, 502.

attention to a remarkable way for obtaining aniline, and which we discovered during our experiments.

Chemists are well acquainted with the interesting compound Mitscherlich described under the name of azobenzide in one of his beautiful papers upon the derivatives of benzoic acid. He obtained this body by distilling nitrobenzide with alcohol and potash, together with other products which were not further investigated. Azobenzide differs only from aniline in two equivalents of hydrogen.

Aniline . . . $C_{12}H_7N$
Azobenzide . . $C_{12}H_5N$.

In order to see if a closer relation did not subsist between these two bodies, we prepared large quantities of azobenzide, following Mitscherlich's directions throughout, and obtained this body, possessing all the properties ascribed to it by that chemist.

On more closely examining the secondary products formed during this process, we found that besides other substances there is likewise produced a very large quantity of *aniline*, and the residue contains a considerable quantity of *oxalic acid*.

These observations appear to shed more light upon the manner in which the azobenzide is formed, *i. e.* if we assume that the oxalic acid is produced from the alcohol at the expense of the oxygen of the nitrobenzide: 2 equivalents of nitrobenzide and 1 equivalent of alcohol contain the elements of 1 equivalent of azobenzide, 1 equivalent of aniline, 2 equivalents of oxalic acid, and 2 equivalents of water, as shown in the subjoined equation,—

$$2\,\underbrace{(C_{12}H_5NO_4)}_{\text{Nitrobenzide.}} + \underbrace{C_4H_5O\,HO}_{\text{Alcohol.}} =$$

$$\underbrace{C_{12}H_5N}_{\text{Azobenzide.}} + \underbrace{C_{12}H_7N}_{\text{Aniline.}} + 2\,\underbrace{(C_2O_3, HO)}_{\text{Oxalic acid.}} + 2\,HO.$$

We are far from supposing that the above equation represents the true decomposition, as other products are formed; and further, the aniline distils over long before a trace of oxalic acid is detectable in the retort. The true transformation can only be given when all the products of this complex reaction are investigated. We are at present engaged upon this subject, and expect shortly to solve the problem.

XXIX. *Observations connected with the Discovery of the Composition of Water.* By Sir DAVID BREWSTER, *K.H., F.R.S., and V.P.R.S. Edin.*

To Richard Taylor, Esq.

MY DEAR SIR,

I OBSERVE in the Number of the British Quarterly Review just published, a paragraph containing some very unfounded and unjust assertions respecting the discussion which took place some years ago on the subject of the discovery of the composition of water. The following is the paragraph in which these assertions are contained:—

"Sir David Brewster afterwards took up the subject (Edin. Review, No. 142), and *endeavoured to mediate* between the contending parties, but to little purpose. For *he chose* to consider it as *a question of national honour*, involving the *rival* claims of Cavendish *the Englishman* and Watt *the Scotchman*, and whilst he *was willing* to divide the merit between them, *assigned to his countryman the lion's share*."

Without questioning the author's courtesy in attaching my name to an anonymous review, when his object was to injure my character and wound my feelings, I have no scruple in giving the most pointed contradiction to every statement and every *insinuation* contained in the preceding paragraph. I use the word *insinuation*, because it is impossible to read the paragraph in question without perceiving, in the language employed, even if that language had been the vehicle of truth, the reviewer's desire to infuse into his statement a bitter personality. But the assertions of the British Quarterly reviewer are equally offensive with the language which conveys them. He charges me, by name, with having decided a great scientific question, interesting to the whole civilised world, *from motives of national feeling*,—with sacrificing by a temporising verdict *Cavendish the Englishman* to *Watt the Scotchman*,—and, under the pretence of *dividing the merit*, with *assigning to my countryman the lion's share*.

Were I to seek an apology for these unfounded aspersions, I should conjecture that the author had not read the review which he thus misrepresents, for the very reverse of every statement which he makes is true. In place of dividing the merit of the discovery between the Englishman and the Scotchman, and giving the lion's share to my countryman, *I have given the whole merit of the discovery to Cavendish the Englishman*, and have reserved only for Watt the Scotchman, the merit of the previous hypothesis,—a merit freely given him by Cavendish himself, and one which no other person ever claimed.

In *proof* of these assertions, I refer to the following passages in the Edinburgh Review.

"Mr. Watt felt a just indignation at the idea suggested by Deluc, of his not claiming *the merit which belonged to his hypothesis*; but we are sure that his astonishment would be equal to ours, were he to learn that that hypothesis had been made to supersede and cast into oblivion *the grand experimental discovery of Cavendish*. The great merits of these two great men are fortunately not in collision. Mr. Watt will for ever enjoy the honour of that singular sagacity which presented to him the *hypothesis* of the composition of water; and Cavendish will never lose *the glory which belongs to him, of having given that hypothesis, whether he was cognisant of it or not, the force and stability of truth*." If the reviewer should perceive any ambiguity in these expressions when standing alone, he will find their meaning amply explained in *previous* parts of the Review, and especially in the following expression:—"The glory of having discovered the composition of water, that is, of having established it as a physical truth, &c. &c." I have likewise stated, that "Mr. Watt never pretended to have discovered the composition of water;" and a little further on I add, that "as Mr. Watt never wrote another word on the subject, nor made a single experiment after his paper (the paper containing his hypothesis) was printed, *how is it possible to identify this hypothesis with the discovery of the composition of water*?"—No. 142, p. 496, Jan. 1840.

Now this decision, which in the Review I have characterized as "deliberately and impartially formed," is precisely, and in terms, the very same as that of Dr. Black. "This idea," says he, "of the nature of water was suggested by Mr. Watt; Mr. Cavendish, however, was the first *who gave it solid foundation and credibility*."—p. 494.

Having thus repelled the charges of the reviewer, I may now state the reasons why I might have *taken up the subject* as the British Quarterly reviewer expresses it, even if I had not been compelled to discuss it in a review of the Life and Inventions of Mr. Watt. In the year 1819, immediately after Mr. Watt's death, his friends submitted to Dr. Hope and myself his claims to the discovery of the composition of water. After a careful examination of the documents on which these claims were founded, we came to the decision that the merit of the discovery belonged to Mr. Cavendish. This decision, however, was not satisfactory to Mr. Watt's friends, and we accordingly find a detailed statement of his claims in the biographical sketch of him published in the Supplement to the *Encyclopædia Britannica*. This statement was reprinted by me in the life of Mr. Watt in the Edinburgh Encyclopædia, and upon that statement I made the following observations:—

"We have copied the preceding statement as that of Mr. Watt's friends, but a regard for the reputation of Mr. Cavendish, independent of higher motives, compels us to acknowledge that the statement is partial, and the argument not well-founded. We are not able, at present, to refer to the original documents, but we had occasion, some years ago, along with a distinguished chemist (Dr. Hope), to examine them with minute attention; and it was then our decided conviction, *that the merit of the discovery of the composition of water belonged to Mr. Cavendish*."—Edin. Encycl., vol. xviii. p. 786.

These observations will, I trust, convince every candid reader that the reviewer's charges against me are utterly groundless. Although Mr. Watt was my countryman, and my personal friend and correspondent; although I cherished for him the warmest affection, and admired him as one of the greatest and best men of his age, I have at three different times of my life come to the decision that he was not, and that Cavendish was the discoverer of the composition of water. Had I been disposed to sacrifice truth, under the influence of national and personal feelings, I might have found a safe shelter behind the broad shield of M. Arago and Lord Brougham's authority, and would thus have avoided the annoyance of rebutting the calumnies of the British Quarterly reviewer.

I am, my dear Sir,
Ever most faithfully yours,
D. Brewster.

St. Leonard's College, St. Andrews,
August 8, 1845.

XXX. *Observations on Ozone as compared to Chlorine.* *By* Professor Schœnbein *of Basle.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Many properties, and the most essential ones belonging to ozone, are so similar to those possessed by chlorine, that I was led to suspect ozone to be a simple halogenous body like chlorine*. My more recent researches having rendered it highly probable, if not altogether certain, that ozone is a peculiar compound of hydrogen and oxygen, I can no longer maintain my former view, and we must place ozone far away from chlorine, in case we regard, according to the established

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* The nature and properties of ozone have been described in vol. xvii. p. 293, vol. xx. p. 64, and vol. xxiv. p. 466 of the present series.—Ed.

theory of the day, the latter body as an elementary one. The elementary nature of chlorine having, however, by no means been proved by any decisive fact, and it being well known that all the reactions produced by that body can be accounted for by the theory of Berthollet, just as well as by the hypothesis set up by your illustrious countryman, it seems to me that the existence of ozone ought to induce chemists to reconsider the two once-rival theories, and make them the subject of a very close examination.

It was chiefly from reasons of theoretical simplicity and analogies, that chemists were determined to give up the old theory and adopt the new one, cyanogen forming with mercury a compound similar to that produced by chlorine with the same metal; cyanogen constituting with oxygen and hydrogen two acids bearing some analogy to the chloric and hydrochloric acids, the readily oxidable carbon remaining, even at the highest temperature, inactive towards chlorine, the muriatic acid of Berthollet having never been isolated, and the views of Davy offering a very great facility in explaining the reactions of chlorine; these were the reasons which induced chemists to abandon the old theory and admit the present one. In doing so they sacrificed a sum of analogies much larger than that on account of which the new hypothesis was adopted. The numerous class of muriatic salts that bear so close an analogy to what we call oxy-salts, could no longer be paralleled to the latter; nay, on account of the striking resemblance existing between those two classes of salts, chemists saw themselves, as it were, forced to change the views they had formerly taken of the oxy-salts and oxy-acids, and imagine a great number of compound halogenous bodies being, as to their chemical character, similar to chlorine and cyanogen. But up to this present moment not one of those imagined halogenous bodies has yet been produced in an isolated state, any more than the anhydrous muriatic acid of Berthollet. It appears, therefore, that there are at least as many arbitrary and hypothetical notions mixed up with the modern views as there were in the old theory, and that in this respect Davy's idea does not offer peculiar advantages over that of Berthollet.

Before entering into an appreciation of both rival theories, I shall take the liberty to point out once more the very great similarity existing between the properties of ozone and chlorine.

1. Though we are not yet acquainted with ozone in its pure condition, we nevertheless know that at the common temperature it exists in a gaseous state like chlorine.

2. Ozone when concentrated has an odour very similar to that of chlorine, bromine and iodine.

3. Ozone, like the three last-named bodies, enjoys the power of negatively polarizing gold and platinum.

4. Ozone, like chlorine, is absorbed at the common temperature by most metallic substances.

5. Ozone, like chlorine, is readily taken up by phosphorus at the common temperature, causing at the same time a disengagement of light and some heat.

6. Ozone, like chlorine, is absorbed by iodine.

7. Ozone, like chlorine, very easily unites with olefiant gas, producing a compound which seems to be analogous to the chloride of hydrocarbon.

8. Ozone, like chlorine, has the power of decomposing sulphuretted hydrogen, and other compounds of hydrogen similarly constituted.

9. Ozone, like chlorine, transforms sulphurous acid into sulphuric acid.

10. Ozone, like chlorine, decomposes the iodide of potassium and the ferro-cyanuret of the same metal.

11. Ozone, like chlorine, being placed in contact with the protoxides of a number of metals, converts them into the corresponding peroxides. The protoxides of lead, manganese and nickel, for instance, are transformed into the peroxides both by ozone and chlorine.

12. Ozone, like chlorine, discharges the colour of many sulphurets, for instance, that of the sulphurets of lead and antimony.

13. Ozone, like chlorine, destroys completely, and with great energy, all the vegetable colouring matters.

14. Ozone, like chlorine, is readily taken up by a variety of other organic substances, for instance, by the ligneous ones, albumen, fibrine, caseous matter, &c.

15. Ozone, to finish the comparison, being inhaled, causes catarrhal affections, very similar to those produced by chlorine.

Now, as there can be hardly any doubt that the reactions just mentioned are due to part of the oxygen contained in ozone, and as there is not one single fact known which is irreconcilable to Berthollet's theory, we must think it at least possible that the chemical effects produced by chlorine may also depend upon the oxygen supposed to be contained in oxymuriatic acid.

In a theoretical point of view it is a most important, and, I may add, by far too little an appreciated fact, that free or isolated oxygen happens to be much less apt to unite with oxidable substances than oxygen in a certain state of combination. In proof of the correctness of my assertion, I need only call to mind certain acids and peroxides. Half the oxygen contained in chromic acid exhibits such a high degree of chemical excitement, that at the common temperature it sets on fire, not only æther and alcohol, but also ligneous substances, as paper and wood; whilst under the same circumstances, free oxygen does not act at all, or at any rate very slowly upon the above substances. Nitric acid offers a similar instance; and the peroxides of lead and manganese being put in contact with a solution of indigo, destroy, like chlorine, that colouring matter; and the same peroxides, when brought into the presence of a solution of iodide of potassium, eliminate iodine. But the most striking instance of the exalted oxidifying power of latent oxygen we have in ozone itself. The affinities of free oxygen are generally called forth by the agency of heat, so that at the common temperature even potassium is not acted upon by oxygen, if the latter happens to be completely anhydrous.

Starting from the hypothesis of Berthollet, according to which chlorine is composed of muriatic acid and oxygen, we can easily conceive why chlorine at the common temperature acts upon so great a number of oxidable substances. The oxygen being latent in chlorine, is in a state of great chemical excitement, similar to that in which the same element exists in ozone, chromic acid, nitric acid, the metallic peroxides, &c.; and in which state oxygen is so very apt to unite, even at the common temperature, with most of the oxidable matters. If these substances happen to be of such a kind as to combine readily in their oxidized condition with the muriatic acid of the older chemists, which is the case with hydrogen, phosphorus, sulphur, and the greater part of the metals, that circumstance must prove an additional reason why, in most instances, oxymuriatic acid acts at the common temperature upon those bodies. There are two facts that have, as already observed, much contributed to set aside Berthollet's views, and gain to oxymuriatic acid the reputation of being a simple body. Those facts are, the chemical inactivity exhibited by chlorine towards carbon, and the discovery of cyanogen. As to the difficulty arising out of the first fact, it is easily removed by admitting that anhydrous muriatic acid unites only in one proportion with carbonic acid to form what Davy called phosgene gas, and that there does not exist a compound consisting of muriatic acid and carbonic oxide. These suppositions, taken together with Berthollet's view, according to which muriatic acid cannot be obtained in an isolated state, satisfactorily account for the inactivity alluded to. Giving anhydrous muriatic acid the sign $\overline{\mathrm{M}}$, we must consider chlorine as $\overline{M}+O$. Now C with $\overline{M}+O$ might possibly produce $CO+\overline{M}$, but no such compound existing, according to our supposition, chlorine does not act upon carbon. C with $2\overline{M}+O$ could form $CO_2+2\overline{M}$, but no such compound likewise existing, only $CO_2+\overline{M}$ could be produced; but we easily see that in such a case $\overline{M}$ would consequently be set at liberty, that $\overline{M}$, however, not being able to exist in an isolated state, chlorine must prove chemically inactive towards carbon.

As to cyanogen, it certainly cannot be denied, that with regard to some of its chemical bearings, that compound exhibits a close analogy to chlorine; and we can easily conceive how such a fact could induce Berzelius to adopt the new doctrine, after having so ably, and with so much tenacity, defended the old theory.

But in paralleling cyanogen to chlorine, chemists were forced to admit, that in some cases a compound body is capable of acting the part of an elementary substance; and indeed a very extensive use has lately been made of such an admission to account for a great number of facts belonging to organic chemistry, though not one single organic element has been produced in an isolated state, for as far as I know, nobody has as yet seen either ethyle, formyle, benzyle, or any other yle*. In order to establish an analogy between cyanogen and chlorine, according to the principles of Berthollet's theory, we must admit that the former contains oxygen; but cyanogen yielding nothing but nitrogen and carbon, from whence shall we take oxygen? Starting from Berzelius's hypothesis, according to which nitrogen consists of one equivalent of an elementary substance called Nitricum and one equivalent of oxygen, we can conceive how cyanogen might be a compound similar to oxymuriatic acid. C_2N (by N we note the nitricum of Berzelius) is to be considered as a body analogous to $\overline{M}$, and as $\overline{M}+O$ is $=$ Cl, so is $C_2N+O=$ Cy. In combining cyanogen with potassium, the latter takes up the oxygen of the cyanogen, and C_2N combines with the oxide just in the same way as, agreeably to the old theory, the oxygen of the oxymuriatic acid unites with the metal, and the muriatic acid with the oxide. In decomposing cyanide of mercury, the oxygen of the oxide of that metal unites with C_2N to form $C_2N+O=$ Cy. In exposing cyanogen to the action of intense heat, it is resolved into carbon and $N+O$ or nitrogen. I need not say, that on this hypothesis what is called anhydrous hydrocyanic acid must be considered as C_2N+HO, as, according to the old doctrine, anhydrous hydrochloric acid

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* Prof. Schœnbein forgets that kakodyle has been isolated.—Ed.

must be taken for $\overline{M}+HO$. This view of the nature of cyanogen offers, in my opinion, the advantage of assigning a similar constitution to two bodies, being, as to their chemical bearings, so very like each other, whilst the established theory is forced, in spite of that similarity, to consider one of those bodies as an elementary substance, and the other as a compound consisting of principles which differ from chlorine as much as night from day.

If we compare the compounds which oxygen forms with elementary bodies with the compounds which chlorine produces with the same substances, we cannot but be struck by the similarity existing between both series as regards the ratio in which the equivalents of their constituent parts are united. If we except the bromides, iodides, and, to a certain extent, the sulphurets, there are, to my knowledge, no other sets of compounds that bear, with respect to the relation alluded to, so close an analogy to each other as the oxides and chlorides do. That similarity loses, however, its peculiar character so soon as we admit the correctness of the old views. According to them, it is, as it were, a matter of course that chlorine unites, for instance, with iron in the same definite proportions as oxygen combines with that metal; for Cl being $\overline{M}+O$, protochloride of iron is $=\overline{M}+FeO$, and the perchloride $3\overline{M}+Fe_2O_3$.

It is a very remarkable exception to that rule according to which no elementary substance forms a real chemical compound with water, that chlorine and bromine produce hydrates. If we take chlorine and bromine for compound substances, their capability of chemically uniting with water loses its exceptional character and comes under the general rule.

Admitting with Berthollet that chlorine is muriatic acid united to oxygen, and considering ozone as a compound analogous to chlorine, we are obliged to parallel water to muriatic acid, and admit that with regard to oxygen, water acts the same part as muriatic acid does. Water uniting so intimately with the stronger bases that these compounds cannot be decomposed by any degree of heat, and the same substance combining so readily with many other basic oxides, water may be considered as a sort of acid; and that view has very often been taken of the chemical nature of the compound mentioned. We may therefore be allowed to consider water as an acid and arrange it beside muriatic acid. As to ozone, it appears not unlikely that it is an isomeric modification of peroxide of hydrogen, *i. e.* composed of one equivalent of water and one equivalent of oxygen. Now chlorine being considered as a compound consisting of one equivalent of muriatic acid and one equivalent of oxygen, it is very easy to parallel to each other the reactions exerted both by ozone and chlorine upon metallic substances. Let us take potassium for an instance: ozone is readily taken up by that metal, hydrate of potash being produced, as potassium is transformed by chlorine into a muriate of potash; in one case the metal unites with the oxygen of ozone, in the other with that of chlorine; and in one instance water unites with the base, and in the other muriatic acid. The latter being a strong acid completely neutralizes the basic properties of potash, whilst water, on account of its being a feeble acid, is not able to do so. In a similar manner all the other chemical reactions exhibited by ozone and chlorine, may very easily be compared to each other. The greater energy of action exhibited by chlorine is not very difficult to be accounted for. Muriatic acid has a very powerful affinity for metallic oxides, whilst that of water for the same substances happens to be much weaker.

A fact which merits attention is the formation of ozone and chlorine being effected by electrical means. If electrical sparks are caused to pass through a mixture of aqueous vapour and oxygen, ozone is generated; and on making electrical sparks act upon a mixture of anhydrous hydrochloric acid and oxygen, chlorine is produced. As to the generation of chlorine effected by electrical means, the old theory accounts for it by admitting that electricity has the power of determining free oxygen to combine with muriatic acid and eliminate water. Regarding the formation of ozone under the circumstances above mentioned, we cannot explain it in any other way than by saying, that electricity causes the direct union between water and oxygen. The similarity existing between the before-mentioned ways of producing ozone and chlorine is at any rate a very remarkable fact, establishing another analogy between both bodies.

My experiments have shown, that in electrolysing water ozone is disengaged at the positive electrode as a secondary product. Now by subjecting hydrochloric acid to the action of a current, chlorine happens to be set free at the same electrode. According to the old theory this chlorine must also be considered as a secondary product of electrolysis; and in this respect another striking analogy is exhibited between chlorine and ozone.

The question is, whether phosphorus acts upon a mixture of muriatic acid gas and oxygen in the same way as that substance does upon a mixture of aqueous vapour and oxygen. I have not yet tried the experiment, though it is worth while to do so.

The electro-chemical system, as established by Berzelius, places oxygen at the head of what are called the electro-negative elements, and puts chlorine in a positive relation to that body. It is however a well-known fact, that in a great number of cases, chlorine, with respect to its electro-negative activity, very much surpasses oxygen. Chlorine is capable of expelling oxygen from a great number of oxides; chlorine eliminates bromine and iodine from the bromides and iodides, whilst pure oxygen seems not to act upon those compounds. In spite of these exalted electro-negative powers possessed by chlorine, it is apt to produce with oxygen a series of acids in which chlorine is said to act the part of an electro-positive principle. I ask, how is it possible that in some cases chlorine proves to be a more electro-negative body, and in another instance a more positive one than oxygen? It seems to me that the facts alluded to are very little in accordance with the principles of the electro-chemical system; nay, that they imply a direct contradiction to them, provided chlorine be taken for a simple substance. But if we consider it as oxymuriatic acid, all the anomalies spoken of disappear, and we can easily account for the reactions produced by chlorine. The oxygen disengaged, for instance, during the reaction of chlorine upon potash, comes from the oxygenized muriatic acid, and not from the potash; and the decomposition of the former is effected by the strong affinity which potash has for muriatic acid. Those who maintain that there is some truth in the principles of the electro-chemical system, must, I think, be inclined to readopt the views of Berthollet, in order to save their endangered theory.

Before concluding, I shall take the liberty to add some general considerations regarding the subject I have treated of. By regarding chlorine as an elementary substance, one of the greatest theoretical changes that ever took place in the history of chemistry was effected. Indeed, by Davy's theory, oxygen lost, if I may say so, its royal dignity amongst the elements; and at any rate a very powerful rival was set up in chlorine. Oxygen was no more the body exclusively enjoying the privilege of being the generator of acids, the supporter of combustion, &c.; that privilege was also claimed for the upstart. But if there should be found good reasons for giving up Davy's hypothesis and readopting the old doctrine, we could hardly help restoring oxygen to its ancient dignities, and considering it again as an agent that has not its equal amongst the rest of the elementary bodies. To speak without metaphor, oxygen would become again one of the centres of chemistry, as it formerly was, and as most likely hydrogen may be. Indeed both substances bear so much the stamp of simplicity, they exhibit so decided a chemical antagonism towards each other, both of them, conjointly with their remarkable product "water," act throughout the domain of our science so extensive and important a part, that we can hardly help suspecting them to be active in some way or other in most, if not in all chemical reactions, and seeing in oxygen and hydrogen the hinges upon which the whole chemical world turns. The theory of Davy, seducing and plausible as it appears at first sight, has possibly proved a check, rather than a spur, to the development of chemistry, on account of its having changed, perhaps, the true point of view from which oxygen ought to be looked at. For if oxygen should happen to act that all-important part which Lavoisier and the chemists of the last century assigned to that element, it is not difficult to see that the views of Davy are calculated to retard the progress of theoretical chemistry rather than to accelerate it.

I need not say, that the considerations I have taken the liberty to submit to you have been entered into with the view only of drawing the attention of philosophers towards a subject which seems to me to be of considerable theoretical importance, and worthy of our study.

C. F. SCHŒNBEIN.

XXXI. *On the Existence of an Equivalent Relation between Heat and the ordinary Forms of Mechanical Power.* By JAMES P. JOULE, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE principal part of this letter was brought under the notice of the British Association at its last meeting at Cambridge. I have hitherto hesitated to give it further publication, not because I was in any degree doubtful of the conclusions at which I had arrived, but because I intended to make a slight alteration in the apparatus calculated to give still greater precision to the experiments. Being unable, however, just at present to spare the time necessary to fulfil this design, and being at the same time most anxious to convince the scientific world of the truth of the positions I have maintained, I hope you will do me the favour of publishing this letter in your excellent Magazine.

The apparatus exhibited before the Association consisted of a brass paddle-wheel working *horizontally* in a can of water. Motion could be communicated to this paddle by means of weights, pulleys, &c., exactly in the manner described in a previous paper *.

The paddle moved with great resistance in the can of water, so that the weights (each of four pounds) descended at the slow rate of about one foot per second. The height of the pulleys from the ground was twelve yards, and consequently, when the weights had descended through that distance, they had to be wound up again in order to renew the motion of the paddle. After this operation had been repeated sixteen times, the increase of the temperature of the water was ascertained by means of a very sensible and accurate thermometer.

A series of nine experiments was performed in the above manner, and nine experiments were made in order to eliminate the cooling or heating effects of the atmosphere. After reducing the result to the capacity for heat of a pound of water, it appeared that for each degree of heat evolved by the friction of water, a mechanical power equal to that which can raise a weight of 890 lbs. to the height of one foot, had been expended.

The equivalents I have already obtained are,—1st, 823 lbs., derived from magneto-electrical experiments †; 2nd, 795 lbs., deduced from the cold produced by the rarefaction of air ‡; and 3rd, 774 lbs. from experiments (hitherto unpublished) on the motion of water through narrow tubes. This last class of experiments being similar to that with the paddle-wheel, we may take the mean of 774 and 890, or 832 lbs., as the equivalent derived from the friction of water. In such delicate experiments, where one hardly ever collects more than half a degree of heat, greater accordance of the results with one another than that above exhibited could hardly have been expected. I may therefore conclude that the existence of an equivalent relation between heat and the ordinary forms of mechanical power is proved; and assume 817 lbs., the mean of the results of three distinct classes of experiments, as the equivalent, until still more accurate experiments shall have been made.

Any of your readers who are so fortunate as to reside amid the romantic scenery of Wales or Scotland, could, I doubt not, confirm my experiments by trying the temperature of the water at the top and at the bottom of a cascade. If my views be correct, a fall of 817 feet will of course generate one de-

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* Phil. Mag. vol. xxiii. p. 436. The paddle-wheel used by Rennie in his experiments on the friction of water (Phil. Trans. 1831, plate xi. fig. 1) was somewhat similar to mine. I employed, however, a greater number of "floats," and also a corresponding number of stationary floats, in order to prevent the rotatory motion of the water in the can.

† Phil. Mag. vol. xxiii. pp. 263, 347. ‡ Phil. Mag. May 1845. p. 369.

gree of heat; and the temperature of the river Niagara will be raised about one-fifth of a degree by its fall of 160 feet.

Admitting the correctness of the equivalent I have named, it is obvious that the *vis viva* of the particles of a pound of water at (say) 51°, is equal to the *vis viva* possessed by a pound of water at 50° plus the *vis viva* which would be acquired by a weight of 817 lbs. after falling through the perpendicular height of one foot.

Assuming that the expansion of elastic fluids on the removal of pressure is owing to the centrifugal force of revolving atmospheres of electricity, we can easily estimate the absolute quantity of heat in matter. For in an elastic fluid the pressure will be proportional to the square of the velocity of the revolving atmospheres; and the *vis viva* of the atmospheres will also be proportional to the square of their velocity; consequently the pressure will be proportional to the *vis viva*. Now the ratio of the pressures of elastic fluids at the temperatures 32° and 33° is 480 : 481, consequently the zero of temperature must be 480° below the freezing-point of water.

We see then what an enormous quantity of *vis viva* exists in matter. A single pound of water at 60° must possess $480° + 28° = 508°$ of heat, in other words, it must possess a *vis viva* equal to that acquired by a weight of 415036 lbs. after falling through the perpendicular height of one foot. The velocity with which the atmospheres of electricity must revolve in order to present this enormous amount of *vis viva*, must of course be prodigious, and equal probably to the velocity of light in the planetary space, or to that of an electric discharge as determined by the experiments of Wheatstone.

I remain, Gentlemen,
Yours respectfully,
JAMES P. JOULE.

Oak Field, near Manchester,
August 6, 1845.

XXXII. *An Examination of* Dr. Dalton's *New Method of Measuring the Water of Crystallization contained in different varieties of Salts.* By SAMUEL HOLKER, *Bury*[*].

THE subject of this paper was suggested to me by my friend Mr. Davies, Professor of Chemistry at the Royal Medical School, with whom I have for some time devoted myself to the study of chemistry. I may observe as a rather remarkable circumstance, that an inquiry of so much interest and importance, commenced by Dr. Dalton, and displaying

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[*] Read at the Manchester Literary and Philosophical Society, February 20, 1844; and communicated by the Author.

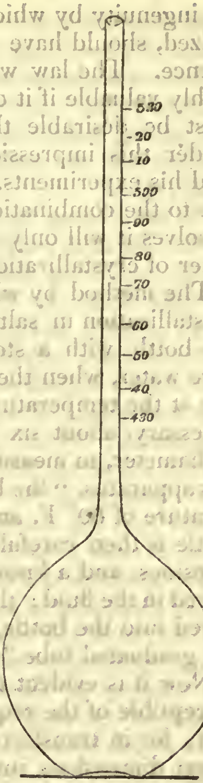
the ingenuity by which all his researches have been characterized, should have excited so little attention among men of science. The law which he attempted to establish would be highly valuable if it could be confirmed, and if not correct it must be desirable that its fallacy should be demonstrated. Under this impression I have undertaken to repeat and extend his experiments. The law which Dr. Dalton gives relative to the combination of salts and water is, that when a salt dissolves it will only increase the volume in proportion to the water of crystallization it contains.

The method by which Dr. Dalton ascertains the water of crystallization in salts is as follows:—"I have," he observes, "a bottle with a stopper which just contains 572 grains of pure water, when the stopper is put in and wiped clean and dry at the temperature of 60° F.; a graduated tube or jar is necessary, about six inches long and one quarter of an inch in diameter, to measure exactly to a grain of water." In using this apparatus, "the bottle is first filled with water at the temperature of 60° F. and wiped clean and dry; the water in the bottle is then carefully transferred into a vessel of larger dimensions, and a known weight of the salt to be tried is dissolved in the fluid; the solution of salt is then carefully transferred into the bottle, and the spare liquor, if any, is put into the graduated tube."

Now it is evident that this apparatus of Dr. Dalton is not susceptible of the requisite precision and nicety of manipulation; for in transferring the solution of salt from the vessel of larger dimensions into the bottle and graduated tube, there will be a loss owing to the cohesion of the water to the glass, so that the error will be very considerable when a sparingly soluble salt is used.

In prosecuting a research of so much interest and importance, I found it necessary to use much more delicate apparatus than that employed by Dr. Dalton, as it was desirable to obtain for the establishment or subversion of a law as accurate results as possible. In order to effect this purpose, I procured a tube about six inches long and a quarter of an inch in diameter, with a bulb blown at one end, which when full up to a certain mark on the stem, held exactly 430 grains of pure water. From this mark upwards the tube was divided into 100 grain measures, so that when full up to the highest mark it held exactly 530 grains of pure water.

In order to experiment with this apparatus, I first filled the bulb and stem to the lowest mark with water, and afterwards added a known weight of salt, carefully noting the increase of bulk.



On repeating Dr. Dalton's experiments with this apparatus, there appeared to be considerable discrepancy in the results; indeed they were so much at variance, that I was induced to suspect at once that the law, which his ingenuity had suggested, would not bear the test of practical application; for according to his results, "if the salt was anhydrous it would all go into the bottle, exactly filling it to a grain, showing that the salt enters into the pores or interstices of the water. If the salt contained water, the quantity of water was measured in all cases whatever by the narrow tube, showing that the solid matter had in reality entered into the pores of the water. Thus if the sulphate of magnesia be made anhydrous and then dissolved, the solution of the sulphate would exactly fill the bottle as the water did before;" or in other words, when a salt, whether hydrous or anhydrous, is dissolved in water, the acid and base of that salt will occasion in all cases whatever a condensation exactly equal to the bulk of that acid and base."

Dr. Dalton here recommends that the sulphate of magnesia be made anhydrous and then dissolved; and with this salt, as well as with the carbonate of soda, our experiments, both on the hydrous salts and when made anhydrous, very nearly agreed, though in the experiments on several of the other salts tried in the same manner the results varied.

But Dr. Dalton seems to have overlooked the fact, that the law in order to be correct must apply, not only to the hydrous salts made anhydrous, but to the naturally anhydrous salts, as sulphate of potash, nitrate of potash, &c.; and in no part of his paper can I find that he has tried any of this variety.

In ascertaining whether the really anhydrous salts dissolved in water without increasing the bulk, I was almost as much surprised with the results of my experiments as Dr. Dalton was with his; for I found that when a salt, whether hydrous or anhydrous, dissolved in water, there was sometimes a condensation and sometimes an expansion of the whole volume of the salt and water, according to the nature of the salt used.

And that when a class of isomorphous salts dissolved, they occasioned an increase or diminution of volume in a multiple ratio of the condensation or expansion occasioned by the several atoms of those salts. And that when a class of isomorphous salts made anhydrous, combined with the requisite quantity of water of crystallization, the condensation then occasioned was also in a multiple ratio.

What I mean by condensation is, that when the joint bulk of the salt and water is less than the sum of their bulks severally, and when there is a greater volume than the original volume of the salt and water, there is an expansion.

In making an experiment with one of the anhydrous salts, "for instance nitrate of potash," I took a certain quantity of the nitrate well-dried and in fine powder, and dissolved it in the 430 grains of water contained in the bulb and stem of the apparatus; I then suffered it to remain about half an hour partly immersed in a vessel of water, with a piece of glass over the opening of the tube to prevent loss by evaporation, in order that the solution might absorb the requisite quantity of heat to raise it to the temperature of 60° F. at which the room was; as there is a certain quantity of heat rendered latent by a salt dissolving in water, so that if the condensation occasioned by the cold was not counterbalanced by the addition of the requisite quantity of heat, the experiment would be incorrect. In making an experiment with a hydrous salt, I proceeded much in the same way as with the anhydrous, except that it was dried by exposure to a temperature of about 60° F., for if the heat was raised higher, as in the case of the anhydrous salt, some of the water of crystallization might probably have been driven off. As by this mode of experimenting I could only obtain the increase of volume when a certain quantity of salt was dissolved in the 430 grains of water taken as a standard, and finding that most of my experiments, both on the hydrous and anhydrous salts, did not correspond to the law which Dr. Dalton had given, the only ones that did approximate being the sulphate of magnesia and carbonate of soda, as stated before; I next proceeded to determine if the increase of volume had any relation to the specific gravities of the different substances, or if, when a known weight of salt was dissolved in water, the increase of volume was in proportion to the volume, as indicated by the specific gravity, and if so, the salt would dissolve without either expansion or contraction.

In trying if there was any relation of this kind, it was important to obtain the specific gravities of the different salts as near the truth as possible. The way in which I found them was as follows:—I have a bottle with a stopper, which when full contains exactly 500 grains of pure water at the temperature of 60° F. I balance it on a pair of scales, then take a solution of the salt made by heating water with more than it can dissolve, allow it to cool to the required temperature, so that it may be perfectly saturated, and fill the bottle exactly half-full of the solution. The bottle being half-full is then balanced on a pair of scales, and filled up to the stopper with the salt well-dried and in fine powder: it is then carefully weighed again, and this weight, minus the weight of the former additions, is divided by 250, which gives the specific gravity of the salt. The reason I divide by 250, is because the bottle will hold 500 grains volume of pure water; therefore if it be half-full of solution and half-full of salt, 250 will equal the volume of that salt compared to an equal volume of water; so that if a saturated solution, alcohol, æther, or any other fluid, be poured into the bottle till half-full, there will be a bulk which will equal 250 grains of pure water.

On referring to Dr. Ure's Dictionary of Chemistry, I found several of the specific gravities of the salts used in my experiments, and these agreed almost exactly with the results as determined by the above method, though I afterwards found on repeating the experiments, some of the others, especially the hydrous salts, to be incorrect. Thus, according to my experiments, I found the specific gravity of nitrate of potash to equal 2·075, while the specific gravity of the same salt, according to Dr. Ure, was 2·073, and several of the others agreed almost as closely; I therefore inferred that the preceding method was correct.

It is important to use a saturated solution of the substance, the specific gravity of which is to be taken; for as I afterwards found, there was generally a certain condensation or expansion when a salt dissolved. For if pure water be employed, or any fluid that will partially dissolve the substance when put into the bottle, there will be an erroneous result of the experiment, amounting in cases where there is a condensation to a greater specific gravity than the truth, and where there is an expansion to a less. Thus on dissolving 100 grains nitrate of potash in 500 grains water, I found the specific gravity of the solution to = 1·1100, while the calculated specific gravity equalled 1·0944, for 100 grains nitrate, specific gravity 2·074, equal 48·21 grains of pure water bulk for bulk, and 500 grains of water equal 500. Therefore

$\underline{100 + 500 = 600}$ for the weight,
which divided by $48{\cdot}21 + 500 = 548{\cdot}21$ for the bulk of water, gives 1·0944 as the specific gravity of the solution. But since when 100 grains of nitrate of potash are dissolved in 500 grains

water, there is a decrease of volume amounting to 6·22 grains by measure; therefore the preceding way of calculating the specific gravity, when the quantity of water and nitrate are known, will be incorrect, as there is no allowance made for the condensation which takes place when the nitrate dissolves.

The following formula will give the solution to this calculation. Let $x =$ the specific gravity of the solution; $W =$ the weight of salt; $w =$ the weight of water; $V =$ the volume of salt; $v =$ the volume of water; and $C =$ the volume of condensation. Then $x = \frac{W + w}{V + v - C} =$ the specific gravity of the solution.

Thus if 100 grains nitrate of potash, specific gravity 2·074, be dissolved in 500 grains water, required the specific gravity of that solution. Now it is known by experiment, that when 100 grains of the nitrate dissolve in 500 grains water, there is a condensation of 6·22 grains by measure. Therefore

$$x = \frac{100 + 500}{48{\cdot}21 + 500 - 6{\cdot}22} = 1{\cdot}107,$$ the required specific gravity; and as this calculated specific gravity differs only $\frac{3}{1000}$ parts from that obtained by experiment, I think it goes far to prove that there is generally a certain condensation or expansion when a salt dissolves, though not in the proportion Dr. Dalton has given.

In order to find the amount of condensation or expansion when a salt dissolves, the specific gravity is first ascertained as accurately as possible, and after this the increase of volume, when a certain quantity is dissolved in the water contained in the bulb and stem of the apparatus. From these two known quantities, the increase of volume more than that occasioned by the volume of the salt, or, as I call it, the expansion or the decrease of bulk less than that indicated by the volume or the condensation, may be calculated. Thus if 100 grains of nitrate of potash, specific gravity 2·074, be dissolved in 430 grains of water, and give an increase of bulk from the lowest mark of 41·99 grains, the condensation in this case will equal 6·22 grains by measure, for 100 grains of the nitrate = 48·21 grains of water bulk for bulk: and as there are only 41·99 grains indicated by the narrow tube for 100 grains nitrate of potash, there is a condensation of 6·22 grains measure: or 100 grains of the nitrate combine with 430 grains of water, the bulk of the whole becoming 471·99 grains measure; and as the whole volume, if there had been no condensation, would have been equal to 478·21 grains measure, according to the specific gravity, therefore there must have been 6·22 grains, by measure, decrease of volume.

There is here inserted a table showing the results of most of the experiments on the salts both hydrous and anhydrous, together with the specific gravities as determined by the above method.

It may perhaps not be out of place to state, that I have carefully repeated most of the experiments since this paper was first read, and have inserted those which appear to be most accurate; so that a slight difference will exist in the results as then copied by Dr. Playfair and several other gentlemen, and those of the experiments more recently conducted.

Isomorphous Salts.

Dissolved in 430 grains of water at the temperature of 60° F. maintained throughout the experiment.

	Atom.	Class.	Increase of volume. grs. measure.		Condensation.	Specific gravity.	
	100	Chromate of potash	26·50 26·00 26·25	26·25	11·09	2·675 2·680 2·680	=2·678
30=7·00 30=7·00 30=7·33	88	Sulphate of potash .	20·53 20·53 21·50	20·85	12·03	2·670 2·680 2·680	=2·676
	102	Nitrate of potash ...	43·00 42·50 43·00	42·83	6·35	2·075 2·073 2·073	=2·074
	86	Nitrate of soda......	32·00 31·75 32·00	31·91	6·38	2·250 2·240 2·250	=2·246
	60	Chloride of sodium .	19·00 19·50 19·50	19·33	8·51	2·155 2·156 2·155	=2·155
	76	Chlor. of potassium	32·00 32·50 32·00	32·16	7·46	1·910 1·945 1·900	=1·918
	123	Sulph. of magnesia .	68·00 67·00 66·00	67·00	6·87	1·664 1·665 1·666	=1·665
100=45·5 100=46·0 100=45·5	143	Sulphate of zinc	65·00 65·78 65·00	65·26	7·10	1·980 1·970 1·980	=1·976
	60	Anhyd. sulph. magn.	4·50 4·00 4·00	4·16	62·84		
	80	Anhyd. sulph. zinc .	2·50 3·00 3·00	2·83	62·43		
	102	Nitrate of potash ...	43·00 42·50 43·00	42·83	6·35	2·073 2·075 2·073	=2·074
25=10·00 25=10·00 25=10·25	124	Chlorate of potash .	49·60 49·60 50·84	50·01	3·36	2·320 2·320 2·330	=2·323

Hydrous and Anhydrous Salts.

	Atom.	Class.	Increase of volume. grrs. measure.		Condensation.	Specific gravity.	
	166	Nitrate of lead	27·00 26·50 27·25	26·91	10·73	4·42 4·40 4·42	=4·41
	54	Muriate of ammonia	38·34 39·25 39·00	38·86	Expansion. 3·34	1·53 1·52 1·52	=1·52
100=49·00 100=50·00 100=49·00	130	Sulphate of iron ...	63·70 65·00 63·70	64·13	Condensation. 6·10	1·840 1·864 1·850	=1·851
100=65·00 100=65·00 100=64·00	144	Carbonate of soda ..	93·60 93·60 92·16	93·12	5·84	1·460 1·440 1·465	=1·455

102 grains nitrate of potash dissolved at 150° F. gave an increase of 45·00 grains by measure.

60 grains chloride of sodium dissolved at 150° F. gave an increase of 21·50 grains by measure.

100 = 56 = 123 grains sulphate of magnesia dissolved at 150° F. gave an increase of 68·88 grains by measure.

100 = 47 = 143 grains sulphate of zinc dissolved at 150° F. gave an increase of 67·21 grains by measure.

54 grains muriate of ammonia dissolved at 150° F. gave an increase of 41·00 grains by measure.

At 60° *Fahrenheit.*

430 grains measure of saturated solution of nitrate of soda diluted with 60 grains of water occasioned a condensation of 1·00 grain measure.

430 grains measure of saturated solution of nitrate of potash diluted with 60 grains of water occasioned a condensation of 0·5 grain measure.

430 grains measure of saturated solution of muriate of ammonia diluted with 60 grains of water occasioned a condensation of 0·25 grain measure.

430 grains measure of saturated solution of sulphate of magnesia diluted with 60 grains of water occasioned a condensation of 1·00 grain measure.

430 grains measure of saturated solution of sulphate of zinc diluted with 60 grains of water occasioned a condensation of 1·00 grain measure.

I have here repeated the experiments as much as possible in the atomic numbers of the various salts, in order that the results may be affected as little as possible by the influence of water in producing a greater or less expansion or contraction out of the proportion to that occasioned by the other salts. I am perfectly aware that by obtaining data on which allowances for the quantity of water might be based, the multiple relation which appears to exist in some of the salts might be brought to coincide much more perfectly than it does at present; but as this paper is designed only as an introduction to a research which at a future time I hope to complete in a more satisfactory manner, I have merely stated the direct results of experiments without obtaining data requisite for allowances, which hereafter I might find to be incorrect.

To ensure accuracy in the above experiments, it was necessary to obtain the various kinds of salts perfectly free from impurity, and having analysed some of them, they were found to contain the same quantity of acid, base and water, as indicated by Dr. Turner's Chemistry; and most of the others being tried with the several tests for impurity in such salts, were found to contain no appreciable quantity. The way I calculate the amount of condensation occasioned by a salt made anhydrous combining with the requisite portion of water of crystallization, is as follows:—

Let x = the amount of condensation occasioned by the anhydrous salt.

W = the weight of crystallized salt.

V = the volume of crystallized salt.

C = the condensation resulting from the solution of the crystallized salt.

B = the weight of the water of crystallization.

E = the increase of volume that the salt deprived of its water of crystallization occasions in solution.

A = the weight of anhydrous salt.

Then $(W-B)=A$; therefore $x=(V-C-E)$ = the condensation produced by A. To give an example:—If 123 grains of sulphate of magnesia, specific gravity 1·665, condense in solution 6·87 grains by measure, the water of crystallization being 63 grains; and 60 grains anhydrous sulphate increased 4·16 grains by measure, required the amount of condensation occasioned by the anhydrous sulphate.

Then $A=(123-63)=60$, therefore $x=(73{\cdot}87-6{\cdot}87-4{\cdot}16)$ $=62{\cdot}84$ grains measure, the amount of condensation produced by the 60 grains of anhydrous sulphate combining with the requisite portion of water of crystallization to form 123 grains of crystalline salt.

I have drawn out a table, partly theoretical, of most of the isomorphous salts used in the experiments, being chiefly directed by the composition to show the multiple relation that appears to exist in each order.

The discrepancies that occur in this proportion may be very reasonably accounted for, as they are not so great as to excite a doubt as to the accuracy of the law, by supposing that the isomorphous salt may contain a small quantity of another salt of the same class combined with it; and if so, the separation of these by crystallization will be exceedingly difficult, if not impossible. The ordinary reagents do not show one of two isomorphous salts in the same solution when the other is present in great excess, though the greatest source of error appears to be in the quantity of water influencing the condensation produced by each salt which is not dissolved in atomic proportion.

On continuing this research on the increase or diminution of volume, I found that a chloride, iodide, or sulphuret of a metal, or by the other nomenclature, the hydrochlorate, &c. of the oxide of a metal, when dissolved in water occasioned a condensation, while the hydrochlorate of ammonia caused an expansion; and if this line of demarcation could have been established, it would have given rise to one of the most important laws in chemical science. For when hydrochloric acid is added to the oxide of a metal, "for instance soda," we do not know whether, in the presence of water, a chloride of the metal is formed or the muriate of soda; though the muriate of ammonia is considered by some to be a chloride of ammonium, yet the theory now generally adopted is, that when muriatic acid unites with ammonia, the muriate of ammonia is formed; and when the same acid unites with a metallic oxide, it forms a chloride; and as the hydrochlorates expand while the chlorides contract, we may infer that a salt, the acid of which contains hydrogen, expands in solution, while the haloid salts contract; the combination of the hydracids with ammonia thus forming muriates, &c., and of the same acids with metallic oxides, forming chlorides, iodides, &c.

If this most important law could have been established, we should then have a ready and easy way of distinguishing between the hydrochlorates and chlorides in solution, and not only of distinguishing between these two varieties; for if the muriates expand while the chlorides contract, the various combinations of chlorine and muriatic acid following the same order, we should then, perhaps, have had an opportunity of distinguishing the nature of bleaching powder and the like compounds, and thence inferring if chlorine can combine at all with the oxide of a metal!

But unfortunately for this law, on proceeding with the experiments, I found that several of the ammoniacal salts did not correspond with it, so that this law, like the one devised by the superior skill and ingenuity of Dr. Dalton, could not be practically established.

I think Dr. Dalton's law relating to the water of crystallization would have been almost as interesting as the one showing the difference between the muriates and chlorides, if they could have been both established, as then we should have had a much more convenient way of measuring the water of crystallization than making an analysis, which, perhaps, would be very complicated, and therefore much more liable to error.

From the experiments contained in this paper, I think the following conclusions may be drawn:—

First, that when a salt dissolves in water, there is sometimes a condensation and sometimes an expansion of the original volume of the salt and water.

Secondly, that the application of heat and the quantity of water make a certain difference in the amount of condensation or expansion resulting from the solution of a salt.

Thirdly, that in the case of the isomorphous salts especially, when a class of them dissolve, they occasion an expansion or condensation in a multiple ratio of the increase or diminution of volume occasioned by the several atoms of those salts; and that when a class of hydrous isomorphous salts made anhydrous combine with their requisite portion of water of crystallization, the condensation then occasioned is also in a multiple ratio.

Manchester, June 1845.

XXXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Feb. 26, 1845. A PAPER was read by Mr. Lyell, "On the Miocene Tertiary Strata of Maryland and Virginia, and North and South Carolina."

These rocks of the middle tertiary period are chiefly exhibited between the hill country and the Atlantic, and form a band of low and nearly level country, almost 150 miles wide, and not 100 feet high. They are assumed to belong to this period, because they are seen resting on the eocene deposits, and exhibit about the same proportion of recent species. The United States miocene beds consist chiefly of incoherent sand and clay, and the sandy beds, otherwise barren, have often been fertilized by the use of shell marl. In the suburbs of Richmond, Virginia, there is however a remarkable bed of siliceous sand, derived from the cases of infusorial animalcules. The paper was accompanied by comparative tables and lists of the fossils.

A paper, also by Mr. Lyell, "On the White Limestone and other Eocene Tertiary Formations of Virginia, South Carolina and Georgia."

The eocene beds extend chiefly to the south of the miocenes described in the foregoing paper, and are very widely spread in the Southern States on the shores of the Atlantic. The mineral character of the beds in the north is so like that of the cretaceous series, that were it not for the fossils, they might readily be mistaken; but towards the south a new mineral type is put on, and the rocks consist of highly calcareous white marl and white limestone. In point of fact, there seems to be as great a chasm between the cretaceous rocks and the tertiaries in America as in Europe.

A second part of Mr. Lyell's paper gave an account of a series of rocks, called in America the Burr-stone, a siliceous rock, containing fossil sponges, and belonging, it would seem, to the upper division of the eocene period.

March 12.—A communication was read by Prof. Sedgwick, "On the Comparative Classification of the Fossiliferous Slates of North Wales, with the corresponding deposits of Cumberland, Westmoreland and Lancashire."

The object of the author in this memoir was to give a general account of the Silurian rocks of the lake district of the North of England, comparing them with those of North Wales, so far as he had hitherto investigated the subject. In both there appears to be a series extending through the various members of the Silurian rocks. In the lake district, the Lower Silurian rocks are imperfectly seen, and are not more than 300 or 400 feet thick, the Ash Gill beds being the highest; but the Upper Silurians are admirably shown, and contain characteristic fossils. Of these latter, the Coniston limestone and the Coniston flags form an important group as much as 1200 or 1400 feet thick, and correspond with the Denbigh flags of North Wales and the Wenlock shale and limestone of the Silurian system. The Ireleth slate and grits succeed and occupy a considerable space, and must be of very great thickness. These higher beds in Cumberland abound with *Terebratula Navicula*, but above them are remarkable bands with *Asterias*, while the upper portion is full of fossils, the prevailing type of which is Upper Ludlow.

April 2.—A paper was read by Mr. Austen, "On an Aërolite said to have fallen near Lymington, Hants."

A paper was also read by Capt. Bayfield, R.N., "On the Junction of the Transition and Primary Rocks of Canada and Labrador."

April 16.—A paper was read by Mr. Macintosh, "On the Supposed Evidence of the former existence of Glaciers in North Wales."

Mr. Macintosh combated the opinion of Dr. Buckland as to the origin of scratches and grooves on various rocks, referring these appearances, in many cases, to structural phænomena.

April 30.—A paper was read by Mr. Murchison, "On the Palæozoic Deposits of Scandinavia and the Baltic provinces of Russia, and their relations to *Azoic* or more ancient crystalline rocks, with an account of some great features of dislocation and metamorphism along their northern frontiers."

In this paper Mr. Murchison gave a general outline of the result of his recent examination of Northern Europe, and the conclusions to which he has arrived, chiefly with reference to the classification of a large tract of country before undescribed on a large and comprehensive scale.

May 14.—An extract was first read from a letter by Dr. A. Gesner, "On the Gypsiferous Red Sandstone of Nova Scotia."

A paper was read by Mr. Austen, "On the Coal Beds of Lower Normandy."

The chief object of the author was to describe the actual geological position of these small basins, and suggest that they might rather be of the Permian than the true Carboniferous period.

Dr. Mantell read a paper, entitled "Notes of a Microscopical Examination of the Chalk and Flint of the South-east of England, with remarks on the Animalcules of certain Tertiary and Modern Deposits."

This paper is published entire in Number 103 of the Annals of Natural History for August, p. 73.

A paper was read by Mr. Bowerbank, "On some specimens of Pterodactyl recently found in the Lower Chalk of Kent."

May 28.—A communication was read, "On the Geology of Lycia." By Prof. E. Forbes and Lieut. Spratt, R.N.

The authors stated, that the rock forming the greater part of Lycia consists of the *scaglia,* or Apennine limestone, a series not very distinctly defined, and that near the river Xanthus another rock of greenish sandstone, whose age was not determined, rested conformably on the scaglia. In other places, true tertiary beds, both marine and freshwater, overlie the scaglia; and of these the marine are the most ancient, and, from the fossils which occur in the different beds, they are found to be all of the same age. The authors then described the districts in which the tertiary marine beds appear, some of them being from 2000 to 3000 feet, and others at a still greater elevation above the sea-level. The freshwater tertiaries of Lycia are much more extensive than the marine beds, and extend over the district at heights of 200 or 300 feet above the plain. They consist of marls, capped by flat tables of conglomerate limestone. The relative age of these tertiary beds is determined by the presence of both marine and freshwater strata in the two great valleys of the Xanthus, the former being identified with the Bordeaux miocenes, and the latter therefore being much newer than the eocene freshwater tertiaries of Smyrna. A considerable mass of travertine is found in the great plains of Pamphylia, and it forms cliffs of considerable height, through which the rivers pour. Certain recent changes of level were also noticed, which had attracted the attention of Sir C. Fellows. In conclusion, the authors consider that the scaglia, the formation of most ancient date, was deposited as fine sediment in a deep sea, and was in progress during the whole of the secondary, including the cretaceous, epoch; the evidence of this consisting in the remarkable mixture of fossils observable in Mount Lebanon and elsewhere, and the great thickness, the extent, and the conformable superposition of the different beds. The sandy beds resting on the scaglia seem to have been more recent than the miocene marine strata, the presence of which marks a great change in elevation. This change was more than paralleled by a converse one of depression, producing lakes in which the freshwater tertiary beds were deposited, and which have been since drained by changes in level still going on.

A short notice was read, being the translation of a memoir by the

Baron Leopold von Buch, "On a new family of Crinoidal Animals, called *Cystideæ*."

The stony cases of these animals differ from Encrinites chiefly in the absence of arms and the presence of ovarial apertures in the plates. They are found abundantly in the lower beds of the Silurian series, chiefly in Scandinavia.

A paper was read, "On the Relation of the New Red Sandstone to the Carboniferous Strata in Lancashire and Cheshire." By E. W. Binney, Esq.

The author endeavoured to show that the Lancashire coal-field, although of great thickness, does not exhibit a passage upwards into the new red sandstone, but that it is a more perfect series than that in the west of Yorkshire and Derbyshire. He also supposes that the coal-measures are generally thrown down by the various faults, the dislocation being of some extent; that these measures continue unaltered beneath the upper beds; and finally, that the lower portions of the new red sandstone are but imperfectly exhibited in the coal-field in question.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 153.]

Address of the President (G. B. Airy, *Esq., Astronomer Royal*) *on the award of the Medal to* Captain Smyth.

Before I proceed to my immediate object, you will allow me, gentlemen, to express my regret that the duty of addressing you should have fallen upon me. The place which I reluctantly occupy ought, in the ordinary course, to have been filled this day by one to whom this Society mainly owes its existence and prosperity, and with whose scientific life every portion of our history is indissolubly interwoven. I will not, however, enlarge further on this irretrievable loss, but will briefly remind you that every member of the Society, who has been proud of his connexion with it while its interests were successfully supported by Mr. Baily, is bound in honour and conscience to give his best efforts for its continued prosperity, by the means which have hitherto proved so successful: individual and independent efforts tempered by a love of unanimity, and zealous industry guided by prudent forethought.

It is the duty of your president, gentlemen, to announce the decision of the Council as to the award of the Medal which, by the laws of the Society, is at their disposal on the present occasion: and I have to inform you that the Council have awarded the Medal to Captain W. H. Smyth, R.N., Foreign Secretary of this Society, for his Bedford Catalogue, forming the second part of the work entitled Celestial Cycle, which has been published by that gentleman within a few months.

The Bedford Catalogue contains the most interesting double and multiple stars of which the primaries are in Piazzi's Catalogue, and a selection of clusters and nebulæ from Messier's Catalogue (*Conn. des Temps*, 1784) and from the papers of the two Herschels; comprising 170 nebulæ and clusters, 580 double stars, 20 binary systems, and 80 triple and multiple stars. The magnitude, colours, &c. of the stars are carefully noted; there are numerous comparisons with the results (published and unpublished) of other observers; and the proper motion of the primary of each system is given with care. This scientific information is accompanied with much antiquarian research and literary history, and the work is likely to prove extremely attractive to the general reader. But the value of the work, in reference to the Medal of this Society, is derived almost entirely from its measures of double and multiple stars, and to these I shall confine my further remarks.

The subject of the labours for which this Medal is awarded is interesting; and the circumstances under which the Medal is awarded are peculiar. It is incumbent on me to make a few remarks upon the subject, and to explain the circumstances to which I allude; but, after the Report which has so long engaged your attention, it is necessary for me to be brief.

The astronomy of double stars may be stated to have commenced with Mr. W. Herschel's paper in the Philosophical Transactions, 1782. It is, therefore, essentially a modern science. But it is worth while to remark that it was not then begun with the views which have since become the principal motives for pursuing it. It was begun with the intention of discovering whether the observations of stars presumed to be at very different distances, but nearly in the same position as seen from the earth, would exhibit any indication of the earth's annual parallax. No such indication was discovered; but an unexpected and perhaps a more important result was in no long time deduced from the observations. The relative places of the components of a double star were found to change, and the change had no respect to the position of the earth in its orbit, but went on from year to year. In several instances the change altered its character so completely, that it could be represented in no way but by supposing that one star revolves round the other. And from that time the observations have been pursued almost exclusively with the view of tracing the orbits of binary stars.

One step of great importance has been made. Methods have been successfully introduced for the investigation of the elements of the orbits of double stars, on the supposition that the law of gravitation applies to them. And these methods have been applied to many stars, and from the elements so computed ephemerides have been prepared, by means of which the theoretical position of the double stars, computed on the same law of gravitation, may be compared with the position which shall be observed in the heavens.

To complete this outline of the progress of double-star astronomy, it appears only necessary to add, that it is believed that not more than one or two stars have completed their revolutions since they were first observed, and that there is no sufficient evidence that the same orbit has been retraced in successive revolutions.

Had I attempted, gentlemen, to enter more in detail into the history of this science, I should have done little more than weary you with the repetition of the same name. A Herschel was the projector of the science; the same Herschel established it to the utmost extent to which a most powerful intellect using the most powerful instruments in the world could carry it; another Herschel improved the accuracy of the observations, increased their number, and fixed the positions of many stars for an epoch sufficiently distant from the former to give accurate measures of their motions; and, finally, introduced that method of determining the elements of their orbits which is yet probably the best that exists. A Struve has filled volumes with the records of the observations made with the magnificent instruments at his command. Compared with these, the additions made by others to the theory or to the observations appear small. Yet it would be unjust to omit mention of the labours of Savary on the theory, and of those of South, Dawes, Bessel, and Mädler on the observations. To these names we can now add that of one whose labours place him in a higher position, the name of Smyth.

I may perhaps for a moment quit the scientific part of this notice to remark that this science is in its origin and principal advances essentially English, and that by far the greater part of the work done upon it has been done by private and not by official observers. The former class is one of whom our country has good reason to be proud. I say advisedly that, since the time of Tycho, no country has witnessed efforts directed with such force, such judgement, and such perseverance, as those of Herschel and Groombridge in sidereal observation, and those of Baily in astronomical literature and in observations of a different class. It has been the pride of our men of business to show that in them at least the effect of the cares incidental to their position has been not to degrade but to sharpen the intellect; not to render it insensible to everything but gain, but to show that honourable gain is only a means to an end, and that that end is the very highest cultivation of the mind.

Although the instance before us is in some degree different, its general character is the same. An officer, whose rank has been derived, in the first instance, from the honourable profession of arms; whose European reputation has been founded upon his services, first as a volunteer and afterwards in official employment, in the scientific and useful task of maritime survey; employs the leisure hours of his riper years upon the furtherance of the astronomy of double stars, devotes to that object his fortune and his energies with a perseverance scarcely inferior to those of the persons to whom I have already alluded, and finally produces an extensive catalogue of double stars possessing, as we believe, the highest claims to the favourable reception of the scientific world. I cannot forbear to add that the results of this labour have been published in a form which cannot fail to fix the attention and to direct the studies of many other able men of the same class: but I add also that this circumstance ought to have no influence, and has had no influence, in deciding your Council on the award of this Medal.

I might offer you my reasons, gentlemen, for believing that observations, like those of double stars, requiring little calculation, but demanding peremptorily the most complete devotion of time and energies when favourable states of the air occur, are likely always to be better carried on by private observers than by official astronomers. I might state, that the regular observer, when wearied with five hours' calculation in the forenoon, is not likely to seize upon that precious sky which sometimes suddenly presents itself in the evening, and to continue his observations till dawning day terminates them. I might, on the other hand, explain that private observers can seldom undertake the masses of calculation which are incidental to meridional observations. But I shall remark no further on this than to observe that, in the instance of the double-star observations before us, as well as in many of the others, discretion has been shown in the selection of the subject as well as perseverance and skill in the pursuit of it.

Gentlemen, the position of the person by whom this work has been executed is thus far important, that it is necessary for us to distinguish between the work executed in the discharge of official duty, and that which is presented by the gratuitous labour and expense of a private individual upon whom we have no claim of any kind. The former circumstance may frequently prevent us from even taking into consideration the merits of some important works; the latter will render similar works admissible for our judgement as to the propriety of awarding to them our Medal. But when I have said *admissible* for our judgement, I have said all. No claim whatever is established by this gratuitous character of the work. The claim must be founded only upon the value of the work with reference to the present wants of science; and to this point I shall now advert.

It has happened, gentlemen, that I have twice had the honour to deliver addresses from this chair, in which allusion has necessarily been made to the nature of the nebulæ, and of those apparently nebuloid bodies, the comets. I have endeavoured to explain my idea, that it is in the examination of these that the cosmogonic mysteries of the world are to be read on the large scale, as in geology on the small scale. The investigation of the motion of double stars appears to me likely to give us an insight into laws equally grand, but of very different character. It is here alone that we can see the mechanics of the universe on a grand scale. The radius of the orbit of Uranus is small in comparison with the distance of the two stars of 61 Cygni, and probably *very* small in comparison with the distance between the components of some binary stars whose parallaxes have not yet been ascertained. The law of gravitation seems to be failing even at the distance of Uranus. May it not, perhaps, fail more completely, or may not a different law almost completely prevail over it, at distances like those of the double stars? Whether this question is to be answered affirmatively or negatively, and whatever may be the modification which the law may require, this appears certain, that it is only in the observation of double stars that we can find an answer to the question.

If now we inquire what is the quality of our observations upon which the correctness of our answer will mainly depend, we find that it is *accuracy*; comprising under this word the two different steps, of exactness in making observations, and certainty in transmitting them to the reader. And upon the importance of this accuracy, as contrasted with number or variety, I cannot express my opinion with sufficient strength. It is matter of history that the establishment of the laws of Kepler, of the system of Copernicus, and finally, of the theory of gravitation, depended upon a discordance between the theory of that period and the observations amounting to eight minutes of arc. This was then a small quantity but certainly appreciable, if the best instruments and best methods of the time were used. Now, gentlemen, am I wrong in saying that the establishment of a cosmical theory, infinitely more comprehensive and more important than that of gravitation, may depend upon the certainty of a measure to the tenth part of a second? I say, that it is more than possible; that it is highly probable; and that there is fair probability of its occurring within our time. Kepler on a similar occasion put to himself the question, "Is it likely that an observer so accurate as Tycho can have been in error by eight minutes?" and boldly answered to himself, "It is impossible, and, therefore, a new theory must be formed." When the question shall be put regarding the measures of the Bedford Catalogue, made at a critical time, and on which a future theory may hinge, Can these numbers be trusted with certainty to one or two-tenths of a second? shall we be able to answer, Without doubt they can? This leads me to a very important part of my present remarks.

The Catalogue of the Celestial Cycle, as exhibited to the public, contains simply the result for each star of all the observations made on that star. In some cases results are given for more than one epoch; but in all cases, every result is given without exhibition of the individual observations from which it was deduced. This form of publication is open to a very grave objection, and one which was seriously discussed by your Council. In a remark above I spoke of the accuracy of published observations as depending on two circumstances; exactness in making the observations, and certainty in transmitting them to the reader. With regard to the exactness of the observations, we had the evidence of a member of the Council who had compared unpublished observations made under the most unquestionably favourable circumstances with individual observations made by Captain Smyth; and we had also the indirect evidence derived from the progressive changes in the relative positions of some of the stars. These kinds of evidence probably had their weight with members of the Council; but to me, I must aver, they were entirely unnecessary. My confidence in the exactness of the observations is purely personal. Knowing the attention which has been given to the adjustments, the intentness of the observer upon his work, the nerve which is made steady rather than disturbed by the anxiety to procure a good observation, and the general skill in the management of the instruments, I can truly say, that if an accurate observation were required, I would desire that it should be made by Captain Smyth. Yet I could wish that I had the means of exhibiting to the world the grounds of my general confidence in the skill of the observer. Still more, I wish that I could produce sufficient means for estimating numerically the probable error of the observations, as affected not only by personal sources of error, but also by the causes of error which no skill can overcome. These remarks apply to the probable exactness of the observation only. With regard to the certainty of transmission to the reader, there is no sufficient security. The fairness of apportionment of weights, the correctness of means of separate results, the correctness of the printing; for all these there is no security. Let it not be supposed that these remarks are answered by referring me to the circumstances, that the computation is easy, that it has been performed by the astronomer himself or immediately under his eye, and that he has himself superintended the printing. I know by experience that errors are more likely to occur in easy than in difficult computation; that the principal person usually performs calculations and reads proof sheets with less accuracy than comparatively illiterate assistants; and that, after all his care in passing the sheets through the press, errors will creep in over which he has no control whatever. Had the manuscripts of the observations and of the calculations in this instance been placed at our command, my remarks would have been completely answered. In the case of Lord Wrottesley's Catalogue, to which the Medal of this Society was awarded, and in that of Groombridge's Catalogue, the printing of which was placed by the Admiralty under my superintendence, the original observations and the intermediate calculations were placed at the command of this Society; and the repeated references which already it has been found necessary to make to the latter manuscripts prove the propriety of this caution. In the instance of the Bedford Catalogue we have no such power of referring to the originals. Feeling these things, gentlemen, and impressed with a sense of the responsibility to you and to the world of science which is implied by my position in this chair, I deem it my duty to state to you, that I for one have hesitated in assenting to this award except in the hope that the manuscripts relating to these observations would at some time be placed in our hands. And I am fully persuaded that it is the general feeling of the Council that the reasons upon which this Medal is now presented are such as have never before been used to justify our awards, and are not likely to be used again. I trust that the value of the Medal will be greatly enhanced to Captain Smyth by this consideration. I trust that he will perceive that, where direct evidence was wanting, this Council have not refused to give to the world their most solemn assurance of the value of the Bedford Catalogue, relying only, until further security shall be given to them, upon their personal appreciation of the instrumental skill, the editorial care, and the general exactness and fidelity of the observer.

(*The President then, delivering the Medal to* Captain Smyth, *addressed him in the following terms*):—

Captain Smyth,—In the name of the Council of the Royal Astronomical Society, I present to you this Medal; and I beg leave to convey with it the expression of my own opinion that never was a Medal more worthily earned. Permit me, Sir, at the same time to remark, that the character of the Council is most deeply pledged in this award, and that I trust that, at no distant period, it will be redeemed by such communication of the details of the observations as will enable the Council to refer other inquirers to publications that are within the reach of all for a sufficient justification of this judgement.

The following Fellows were elected Officers and Council for the ensuing year, viz.—

President.—Captain W. H. Smyth, R.N., K.S.F., D.C.L., F.R.S. —*Vice-Presidents.* George Biddell Airy, Esq., M.A., F.R.S., *Astronomer Royal*; Samuel H. Christie, Esq., M.A., F.R.S.; Bryan Donkin, Esq., F.R.S.; Thomas Galloway, Esq., M.A., F.R.S.—*Treasurer.* George Bishop, Esq. —*Secretaries.* Rev. Robert Main, M.A.; William Rutherford, Esq.—*Foreign Secretary.* Rev. Richard Sheepshanks, M.A., F.R.S. — *Council.* George Dollond, Esq., F.R.S.; Solomon M. Drach, Esq.; Lieut.-Col. George Everest, F.R.S.; Rev. George Fisher, M.A., F.R.S.; Manuel J. Johnson, Esq., M.A.; John Lee, Esq., LL.D., F.R.S.; Edward Riddle, Esq.; Richard W. Rothman, Esq.; Lieutenant William S. Stratford, R.N., F.R.S.; The Right Hon. Lord Wrottesley, M.A., F.R.S.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 60.]

October 28, 1844.—On the Foundation of Algebra, No. IV.—On Triple Algebra. By Augustus De Morgan, Esq., of Trinity College.

The extensions which have successively been made in algebraical interpretation have been consequences of efforts to interpret symbols which *presented themselves* as necessary parts of the algebraical language which is suggested by arithmetic. The now well-known signification of $a+b\sqrt{-1}$ did not yield any new imaginary or unexplained quantities: and accordingly no effort (within the author's knowledge) was made to produce an algebra which should require three dimensions of space for its interpretation, until Sir William Rowan Hamilton wrote a paper (the first part of which appeared in the Philosophical Magazine* before the present one was begun) on a System of Quaternions. This system, as the name imports, involves four distinct species of units, one of which may by analogy be called *real*, the three others being *imaginaries*, as distinct from one another as the imaginary of ordinary algebra is from the real. These imaginaries are not deductions, but inventions; *their laws of action on each other are assigned*: this idea Mr. De Morgan desires to acknowledge as entirely borrowed from Sir William Hamilton.

Sir William Hamilton has rejected the idea of producing a triple algebra, apparently on account of the impossibility of forming one in which such a symbol as $a\xi+b\eta+c\zeta$ represents a line of the length $\sqrt{(a^2+b^2+c^2)}$. Mr. De Morgan does not admit the necessity of

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* Vol. xxv. pp. 10, 241.

having a symmetrical function of *a*, *b*, *c*, and, throwing away this stipulation, points out a variety of triple systems, partially or wholly interpreted.

Sir William Hamilton's quaternion algebra is not entirely the same in its symbolical rules as the ordinary algebra: differing in that the equation $AB=BA$ is discarded and $AB=-BA$ supplies its place. Those of Mr. De Morgan's system, which are imperfect, all give $AB=BA$, but none of them give $A(BC)=(AB)C$, except in particular cases.

Mr. De Morgan gives systems of triple algebra, which he distinguishes into quadratic, cubic, and biquadratic, according as the invented imaginary units represent square roots, cube roots, or square and fourth roots, of the negative real unit. It would not be easy in an abstract to give any account of these, but among them are found,—

1. An imperfect quadratic system, strongly resembling the common double algebra, and which would, but for its imperfect character, be at once recognised as the proper and natural extension of the interpretation of imaginary quantities to three dimensions of space: the ultimate symbol for a line is $l\,(\cos\theta+\sin\theta\,\sqrt{\ }_{\omega}{-1})$.

2. An imperfect quadratic system, very like the former one, except in having a peculiar inversion in the operation of multiplication, and a somewhat remarkable mode of representing what would by analogy be called arithmetical multipliers.

3. A perfect quadratic system, the interpretation of which has considerable resemblance to that of the first-mentioned system, and is completely attainable, though not of great interest.

4. Three perfect cubic systems, each irreconcileable with the others, though closely connected with them. Each system presents a triple trigonometry, the cosine and *two sines* of which are each a function of two angles; but these can be easily expressed as functions of common circular and hyperbolic sines and cosines. The interpretations of these systems are very imperfect, and appear to present great difficulty, but their symbolical character is unimpeachable.

5. A perfect biquadratic system, which is of a redundant character, that is, its fundamental form represents a line drawn in space from a given origin, with a symbol to spare, which may represent the time of drawing it, its density, its tendency to a given position, &c. at pleasure. Many interpretations are attainable, but Mr. De Morgan does not pretend to say that he knows the one which ought to be adopted. It is singular that every attempt to reduce this algebra, by assigning a condition among the subsidiary symbols of its fundamental form, leads to an imperfect algebra. The system first mentioned in this abstract is one such result, and fails in its rules of multiplication, as before mentioned. Another is obtained, which is perfect as to its rules of multiplication, &c., but fails in its rules of addition.

Mr. De Morgan concludes by giving some formulæ which may be useful to those who would try to interpret algebra of three dimensions by the use of solid angles in the place of plane ones.

December 9, 1844.—On the Values of the Sine and Cosine of an Infinite Angle. By the Rev. S. Earnshaw, of St. John's College.

It has been usual with mathematicians to write zero as an equivalent for both $\sin x$ and $\cos x$ when x becomes infinite. The object of this paper is to examine into the propriety of this usage. The inquiry derives importance from its bearing on the general correctness of Fourier's theorem for the transformation of functions, and from its affecting the truth of many remarkable results in definite integrals. Certain principles also which have been assumed and acted on by Poisson, Fourier, Cauchy and others, in treating of periodic infinite series, are examined, and shown to be untenable: for example, it is shown, that as $1-x$ approaches zero, $1-x+x^2-x^3+\ldots$ ad inf. does not approach $1-1+1-1+\ldots$ ad inf. as its limit; that this last series has not a unique value, and that its value is not $\frac{1}{2}$, as has generally been argued. It is also remarked that every series of the form $a_1x^\alpha+a_2x^\beta+\ldots a_nx^\nu+\ldots$ is discontinuous in those terms which are at an infinite distance from the first, unless the coefficients tend to zero as n and ν tend to ∞. The truth of this depends on a circumstance which does not seem to have been remarked before, viz. that however small $1-x$ may be, a value of ν can always be found so large that $(1-x)\nu$ may be finite, and therefore x^ν, which is equal to $(1-\overline{1-x})^\nu$, is not equal to 1 in the limit, but to $e^{\text{lim. of } (1-x)^\nu}$.

It is lastly proved that $\sin\infty$ and $\cos\infty$ are not equivalent to zero, whether we regard them as the results of integration between limits, or as the limiting cases of more general forms.

February 10, 1845.—On the Connexion between the Sciences of Mechanics and Geometry. By the Rev. H. Goodwin, of Caius College.

This paper contains an attempt to determine the ground of the truth of the elementary propositions of mechanics. The remarkable analogy between mechanics and geometry suggests the thought, that perhaps there may be something more than analogy, that in fact the basis of the two may be the same. The author endeavours to show that this is really the case; the ground of the reasoning is, that force is a *physical* expression of the two ideas of *magnitude* and *direction*, of which a straight line is the *geometrical* expression, and therefore that propositions which are true for one event are true for the other. Hence it is argued, that inasmuch as the giving two sides of a triangle gives the third, so that the third may be considered as the *resultant* of the two already given, so if the two sides represent *forces*, the third will still represent the *resultant* of the two forces already given.

Reasoning of this kind does not, of course, admit of a very demonstrative character *primâ facie*; it is the author's design rather to point out a path to the truth, than to assert that he has cleared away every difficulty.

The subject is further elucidated by the application of the remarkable symbol $e^{\theta\sqrt{-1}}$, a symbol which in geometry serves to indicate the direction in which a line is drawn with respect to a given fixed line; the same symbol is perfectly applicable as a sign of affection for forces, and hence the conclusion is strengthened that the ground of truth in the two sciences is the same.

The reasoning of this paper extends not only to forces, but also to velocities and moments, and to all expressions of whatever kind of the pure ideas of *magnitude* and *direction.*

If the author's reasoning be sound, the elementary propositions of mechanics are *necessary truths* in as strict, in fact, in exactly the same, sense as the elementary propositions of geometry; and to a mind which dwells upon them, the truths of the one science ought to appear in as axiomatic a light as those of the other.

XXXIV. *Intelligence and Miscellaneous Articles.*

ADDITIONAL NOTE TO MR. SULLIVAN'S PAPER AT p. 161 OF THE PRESENT NUMBER.

SINCE writing the preceding paper, a short notice on the same subject has been published by Prof. Kersten*, in which he speaks of having detected chlorine in a specimen of lava from Niedermennig. I am glad that I can add a few experiments on the same point. In nearly all the basalts, clinkstone, one specimen of granite, diorite, and particularly carboniferous slate, from which I never found it absent, I could detect chlorine by the same means as that employed by Kersten. Fluorine appears to be equally diffused, as I could detect its presence in nearly all the minerals which I analysed, but particularly in diorite from the Lahn, and the carboniferous slate from the south of Ireland. The following is the method which I adopted for the detection of the fluorine, particularly in bodies undecomposable by means of sulphuric acid, recommended by Rose. The mineral, finely pulverised, was ignited in a platinum crucible with from three to four times its weight of carbonate of soda; the ignited mass moistened with water, and the soluble portion filtered from the insoluble. Care should be taken to employ as little water as possible. The solution thus obtained is to be poured into a platinum capsule, and very carefully supersaturated with hydrochloric acid. In stirring the solution no glass should be employed, but either platinum or silver. The acid should be allowed to rest for some time until all the carbonic acid has escaped. When no more is evolved, the liquor should be supersaturated with ammonia, and the whole poured into a flask provided with a cork. Chloride of calcium is then to be

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* Page 155 of the present volume.

added, which throws down fluoride of calcium if a fluoride of any metal be present. Care should be taken to prevent access of air as much as possible, otherwise carbonate of lime would also be thrown down. The whole is then to be filtered, and the precipitate gently heated with strong sulphuric acid in a platinum crucible covered with a glass plate, having a thin coating of wax with a few letters or words etched on the wax, so as to leave the glass bare at a few points. The precipitate can thus be proved beyond doubt to be fluoride of calcium, by the glass being attacked by the evolved hydrofluoric acid.

Dublin, August 12.

ON THE NEW METAL RUTHENIUM.

M. Claus states, that after an uninterrupted labour of two years' duration, he has succeeded in obtaining the above metal, which he had previously discovered, in a state of purity and by a simple process from the residues of platina.

This metal has, however, as yet been obtained only in the state of a gray powder, which is much lighter than iridium; and its simple and double chlorides strongly resemble those of this metal. The chloride of potassium and iridium so strongly resembles that of potassium and ruthenium, that M. Berzelius, to whom M. Claus sent a portion, stated it to be the salt of iridium; but some days after he withdrew this opinion, and considered this salt as that of a metal which was unknown to him.

The characters of this metal are so distinct, that no doubt can be entertained as to its being different from all previously known. Its highest chloride has a fine orange colour; and when ammonia is added to its aqueous solution, a black oxide is precipitated, whereas the solutions of the other platina metals are not precipitated at all by ammonia at common temperatures. The solution of the chloride is not altered by hydrosulphuric acid (H^2S) till the action has been long continued, and then a brown precipitate is at first obtained, which afterwards becomes a black sulphuret, and the solution assumes a magnificent azure-blue colour. These changes depend upon the precipitation of a small portion of metallic sulphuret and the conversion of the chloride into a higher one, which has a blue colour, and which is not decomposed by the hydrosulphuric acid. If a plate of zinc be put into the solution of the orange chloride, acidified by a little hydrochloric acid, a black metallic powder is precipitated after some time, and the solution becomes of a deep indigo-blue colour; eventually the whole of the metal is precipitated, and the solution becomes colourless.

The metal thus obtained, and all its combinations, when mixed with a large quantity of nitre and heated strongly to redness, give a blackish-green mass, which, when dissolved in water, yields a solution of a fine orange-red colour. This solution of the potash salt of the metallic acid blackens organic bodies, and is decomposed by the addition of organic substances, such as alcohol, and also by the action of acids, &c.; and there is precipitated a velvet-black compound of the oxide and potash; this is soluble in boiling hydrochloric acid, and yields an orange-coloured solution of the chloride. This chloride is very easily decomposed, especially when dissolved in water; it loses chlorine and becomes nearly black, and sometimes of a cherry-red, and precipitates an insoluble black powder. In this state of decomposition it possesses an almost incredible degree of colouring power, so that a few hundredths of a grain render half a pound of water almost opake.

If the chloride be precipitated by ammonia, and the black precipitate obtained is dissolved in hydrochloric acid, and the solution evaporated to dryness, a dirty green residue is formed, this, when dissolved in a large quantity of water, gives an opake solution, which is black with a tint of cherry-red; this, with the addition of a little nitric acid, yields, when evaporated, extremely fine tints of colour; it becomes first violet, azure-blue, red, yellow, and lastly green by evaporation to dryness; this last property is also possessed by the sesquichloride of iridium. But its other properties, its action with nitre and hydrosulphuric acid, are so characteristic, that the existence of ruthenium as a peculiar metal is evident to the sight. The tendency of this metal to combine with the alkalies is so great, that when fused with potash it dissolves perfectly, and yields with water an orange-yellow solution, which is the potash salt of the metallic acid.

On this account this metal cannot be reduced in the same manner as the chlorides of the other metals of platina, by mixing them with soda and heating them to strong redness. If a mixture of the chloride of this metal with soda be heated to redness, the greater part of the mass dissolves in water with an orange-red colour.

M. Claus states that he names this metal *ruthenium*, because it is found in small quantity in the white substance mentioned by Osann, consisting chiefly of silicic and titanic acid, peroxide of iron and zirconia; and considered by him as a peculiar metallic oxide, which he named oxide of ruthenium. The discovery of the new metal escaped Osann, because he repeatedly treated his impure oxide of ruthenium with hydrochloric acid, without examining the solution, and considered the insoluble residue as the new oxide. M. Claus further states, that he has also obtained Osann's oxide of ruthenium, possessing all the properties which this chemist assigns to it; from this oxide M. Claus procured, by means of hydrochloric acid, a large quantity of his oxide of ruthenium with a little peroxide of iron.

Having determined the properties of this metal by operating on small quantities of it, the author states that he found it easy to obtain it from the platina residue by means of a single fusion with nitre; the treatment with hydrochloric acid being difficult, and occupying much time.—*Journ. de Pharm. et de Ch.*, Juin 1845.

ON OSMIUM AND ITS COMPOUNDS.

M. Claus states, that from fifteen pounds of platina residue he obtained nearly four ounces of metallic osmium. During these experiments he states that he discovered previously to M. Fremy the osmite of potash, but it was obtained by a different process. It is a very beautiful salt, which crystallizes in regular octahedrons of a black, garnet, or rose-red colour. The colour depends upon the quickness of their formation. When formed slowly by gradual cooling in solutions which are not too much saturated, they are black, the edges being transparent and of a garnet-red colour: if they are quickly deposited from a saturated solution, they are garnet-red; and if the separation of the salt be effected by agitation, the crystals are pulverulent and of a rose-red colour. When reduced to powder, the salt is almost white; it is $KO+OsO^3+Aq^2$. It dissolves slowly in water, imparting to it the same colour as manganesiate of potash. The solution is partially decomposed by evaporation; free osmic acid, OsO^4, is disengaged, and black oxide of osmium, OsO^2+Aq, is precipitated; the liquor becomes strongly alkaline, and a part of the salt crystallizes without decomposition; the decomposition occasioned by evaporation may be prevented by adding great excess of potash to the solution of the salt. Acids readily decompose the solution of osmite of potash, yielding oxide of osmium and osmic acid: $2(OsO^3)$ produce OsO^2+OsO^4; this fact the author also states that he observed previously to M. Fremy. The oxide has the remarkable property of decomposing with slight detonation when it is heated in a glass tube into metallic osmium and osmic acid; $2(OsO^2)$ yield $Os+OsO^4$. Osmite of barytes, $BaO+OsO^3+Aq$, forms fine black crystals with a diamond-like splendour; this salt is prepared by mixing a solution of osmic acid with excess of barytes water; the yellow liquor deposits crystals at the expiration of a month; a large quantity of the salts of iridium was also obtained among other products of the first fusion with nitre; among these were three ounces of a salt which the author supposed to be a salt of iridium, but which he afterwards found to be chloride of potassium and ruthenium.

The author then attempted to obtain this new metal from the residue which had been once heated to redness with nitre, and treated with water and acid; the following process succeeded, and one ounce and a half of the chloride of potassium and ruthenium was obtained by it:—equal parts of the residue and of nitre were heated to whiteness for two hours in a Hessian crucible; the calcined mass was taken by an iron spatula from the crucible while red-hot, and reduced when cooled to coarse powder. This was treated with distilled water, and the mixture allowed to become clear by standing; this solution was of a fine deep yellow colour, and water was added as long as it continued to dissolve. This solution contains rutheniate, chromate, and silicate of potash, without any trace of rhodium or iridium, and only a slight one of osmiate of potash. Nitric acid is to be cautiously added to the solution till it ceases to be alkaline; this precipitates a compound of oxide of ruthenium and potash, and a small quantity of silicic acid, in the state of a velvet-black powder, and chromate of potash remain in solution. After washing the compound of potash and oxide of ruthenium in hydrochloric acid, the solution is to be evaporated until the silicic acid gelatinizes; it is then to be diluted with water and filtered. The solution must not be evaporated to dryness in order to separate the silica more perfectly, because in that case the chloride of ruthenium is converted into an insoluble sub-chloride.

The filtered solution is of a fine yellow colour, and is to be reduced to a small quantity and mixed with a concentrated solution of chloride of potassium, there then separates the salt $KCl^2 + RuCl^4$, in reddish-brown crystals. The liquor poured off from the crystals furnishes afterwards much of this salt by evaporation; by repeated crystallizations this salt is obtained in great purity.—*Journ. de Pharm. et de Ch.*, Juin 1845.

OBSERVATIONS ON THE FOREGOING MEMOIRS. BY M. FREMY.

The author remarks, that M. Claus alludes to some compounds of osmium which he had obtained before him (M. Fremy). He further observes, that before his paper on metallic acids appeared in the *Annales de Chimie,* he had inserted in the *Comptes Rendus de l'Académie des Sciences* and in the *Journal de Pharmacie,* extracts from his researches on osmium, in which he stated the properties and composition of the osmites. M. Fremy therefore contends for the priority of his experiments; he also observes that M. Claus represents the osmites by OsO^3, MO, which is the composition stated in his memoir.—*Journ. de Pharm. et de Ch.*, Juin 1845.

EXAMINATION OF SOME NATIVE AND ARTIFICIAL COMPOUNDS OF PHOSPHORIC ACID. BY C. RAMMELSBERG.

[Continued from p. 158.]

Phosphate of Magnesia.

According to Graham's analysis, the salt which separates on mixing dilute solutions of sulphate of magnesia and phosphate of soda contains 15 eq. of water, 8 of which are given off at 212°. The author examined the gelatinous precipitate which is formed in concentrated solutions of these salts, and which differs from the former crystallized salt in containing less water. After a portion had been washed in the cold and dried in the air, it lost 1·558, by remaining for some time over sulphuric acid it lost 0·113, and on heating to redness 0·451; in all $0{\cdot}567 = 36{\cdot}2$ per cent. The estimation of the magnesia showed that the salt was $2MgO, P_2O_5$, and that the amount of water it contained was almost accurately 7 eq. $= 35{\cdot}85$ per cent.; consequently this salt has the composition of that crystallized and dried at 212°. If the liquid which has been separated from the crystals by filtration is allowed to stand, crystals of $2MgO, P_2O_5 + 15HO$ form in it; if it be heated, it becomes turbid, and a flocculent substance is deposited, probably the tribasic compound.

To test the accuracy of Riffault's statement, that the phosphate of magnesia is decomposed by ebullition into an insoluble basic and a soluble acid salt, a portion of the precipitate we have described, still moist, was repeatedly boiled with water, the whole was filtered and the residue washed with hot water. The filtrate was acid, contained a considerable quantity of phosphoric acid, but little magnesia, and did not yield any crystalline salt. The phosphate of magnesia is thus decomposed by boiling with water into a basic salt and free acid. The former when dried forms a white powder, and only differs from the bibasic salt in being coloured yellow by salts of silver, after having been heated to redness. 1·087 grm. of the substance dried in the air lost by exposure to red heat 0·267; the residue, after being heated with carbonate of soda and washed, yielded 0·37 magnesia; consequently the salt is $= 3MgO, P_2O_5 + 5HO$, and should yield, according to calculation, 34·76 per cent. magnesia, 40·02 phosphoric acid, and 25·22 water.

Lazulite and Blue spar.

The only complete analyses, and these were published twenty-six years ago, are of the lazulite from Rädelgraben, near Werfen, and of blue spar from Krieglach in Styria.

	Lazulite according to Fuchs.	Blue spar according to Brandes.
Silica	2·10	6·50
Phosphoric acid ..	41·81	43·32
Alumina	35·73	34·50
Magnesia	9·34	13·56
Protoxide of iron ..	2·64	0·80
Lime		0·42
Water	6·06	0·50
	97·68	99·60

The author analysed a dark blue lazulite from the Fischbacher Alp in the province of Gratz, and a bright blue spar from Krieglach in Styria. The specific gravity of the lazulite was 3·106–3·123, Fuchs found 3·057; that of the blue spar 3·021, according to Brandes 3·001; according to Klaproth 3·046. On account of the great difficulties presented in the analysis of these substances, the author, even by repeating them several times, could not obtain results which agreed perfectly. The mineral was heated to redness, to estimate the quantity of water it contained: this caused the colour to change from yellow to brown, resulting from the oxidation of the protoxide of iron. The substance in fine powder was fused with carbonate of soda and the mass decomposed by muriatic acid. After separating the silica, the acid solution was digested and boiled with concentrated solution of potash in a platinum dish, which effected the solution of the greater part of the alumina and phosphoric acid. The phosphate of alumina was then thrown down by ammonia, from the liquid acidified with muriatic acid; its composition was ascertained either by fusion with carbonate of soda and silica, or by decomposing it by sulphuric acid and sulphate of soda. The phosphoric acid remaining in the liquid was precipitated by chloride of calcium. The portion undissolved by the potash, which contained the oxide of iron, lime and magnesia, with some alumina and phosphoric acid, was dissolved in muriatic acid and precipitated by ammonia.

This threw down everything except the greater part of the lime and magnesia, which was separated from the filtered solution in the ordinary way. The precipitate was heated to redness with carbonate of soda, a little caustic potash being added, then exhausted with water, and the solution treated in the same manner as the potash solution obtained at first, to which it was added, whilst the residue, dissolved in muriatic acid, yielded the amount of iron, lime, and magnesia. The following are the results of five analyses of lazulite and three analyses of blue spar:—

Lazulite.

	I.	II.	III.	IV.	V.
Silica	0·53	4·44	4·44	1·76	8·04
Phosphoric acid......	42·19	41·58	44·91	40·60 (46·10*)	43·26 (49·8)
Alumina...............	29·42	31·63	26·39	32·10	24·76
Magnesia	10·61	8·60	10·69	9·37	9·81
Protoxide of iron ...	10·55	6·69	6·18	9·37	7·21
Lime	1·11	1·38	2·03	0·77	1·11
Water..................	5·59	5·68	5·36	6·03	5·81
	100·	100·	100·	100·	100·

Blue spar.

	I.	II.	III.
Silica.............	6·64	6·64	12·56
Phosphoric acid....	38·05 (43·05)	44·21	40·12 (42·77)
Alumina	33·93	28·05	24·12
Magnesia	12·04	11·39	10·67
Protoxide of iron ..	1·54	1·77	2·56
Lime.............	1·32	1·54	4·67
Water	6·48	6·40	5·30
	100·	100·	100·

These analyses differ principally in the quantity of phosphoric acid and alumina they contain, and the amount of the latter is much too small, which is dependent on the method used in its separation. The silica exists accidentally in the mineral in the form of quartz, its amount must therefore be deducted. Now if we calculate the oxygen in the quantities ascertained, it is found that in most of the experiments the amount of oxygen in the alumina is twice as great as that in the bases (RO). That of the phosphoric acid is apparently twice as great as that of the alumina, and that of the water is equal to that of the bases (RO). The quantities of oxygen in $RO : Al_2O_3 : P_2O_5 : HO$ are in the proportion of 3 : 6 : 12·5 : 3. The most probable expression of lazulite and blue spar then is, $2R_3O^3P_2O_5 + 4Al_2O_3, 3P_2O_5 + 6HO$; a formula the first member of which corresponds with the ordinary degree of saturation, the second with a

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* The figures within the brackets indicate the quantities of phosphoric acid found, those in the diagram itself are calculated.

wavellite free from water and fluorine; consequently lazulite and blue-spar are the same mineral, and are only distinguished by the former containing a larger portion of protoxide of iron, which is isomorphous with the magnesia, hence also its darker colour. But as the protophosphate of iron is colourless, and the double salt only yields a blue colour with basic phosphate of iron, it is probable that lazulite also contains some peroxide of iron.

Amblygonite.

This mineral, which was discovered by Breithaupt, and is found only in the granite of Penig (Chursdorf and Arnsdorf) and Zinnwald, is one of the rarest, and from its chemical composition, one of the most remarkable minerals. Berzelius found 11 per cent. of lithia, as well as phosphoric acid and fluorine. According to Plattner it also contains soda, tinging the flame of the blowpipe yellow, so that the reaction of the lithia is barely perceptible. Its specific gravity, according to Breithaupt, is 3·11. After making many fruitless experiments the author adopted the following method of analysis:—The mineral, in fine powder, is heated with sulphuric acid in a platinum vessel until the fluorine is driven off, the solution is diluted with water and precipitated by ammonia; the alumina and part of the phosphoric acid are thus thrown down, and the precipitate may be decomposed by fusion with either carbonate of soda and silica, or bisulphate of potash. The ammoniacal filtrate, which contains the alkalies and the remainder of the phosphoric acid, is decomposed with chloride of calcium to separate the latter, and the excess of lime is afterwards removed by oxalic acid. The alkaline sulphates remaining after evaporation and heating to redness, are estimated by first precipitating the potash by chloride of platinum; the weights of the sulphate of lithia and soda are next estimated, and finally the quantity of acid they contain. To ascertain the amount of fluorine, the powder of the mineral was distilled with silica and sulphuric acid, and the fluoride of silicium conducted into water, so that none of it could escape. The fluid was supersaturated in a platinum dish with pure carbonate of soda, boiled, and finally, after the escape of the carbonic acid, precipitated by ammonia and chloride of calcium. The analysis yielded the following results:—

	I.	II.	III.	IV.	V.
Phosphoric acid .	48·00	47·15			
Alumina	36·26	38·43	36·62	36·89	
Lithia	6·33	7·03			
Soda..........	5·48	3·29			
Potash		0·43			
Fluorine					8·11

We cannot regard amblygonite as a compound of fluoride of lithium and sodium with phosphate of alumina, because the amount of fluorine is not sufficient to saturate the metallic bases of the alkalies; consequently it must consist of phosphate of alumina and lithia (soda), and fluoride of aluminium (sodium). Three-fourths of the fluorine belong to the aluminium, one-fourth to the lithium and sodium, thus in the proportion of 5 equiv. to 2 eq. In the remaining double phosphate the quantity of oxygen in the alumina is half as great as that in the phosphoric acid, that of the alkalies amounts to a third part of the former. Whilst the alumina of the phosphate contains five times as much oxygen as the aluminium of the fluoride which is converted into alumina, we thus obtain

$$(5R_5 O\, 3P_2 O_5 + 5Al_2 O_5\, 3P_2 O_5) + (RFl + 2Al\, 3Fl)$$

as the formula for amblygonite.

It has been previously mentioned, that ammonia throws down phosphate of alumina from the solution of the amblygonite in sulphuric acid. The quantity in one experiment amounted to 68·33 per cent. of the mineral, and consisted of 31·71 phosphoric acid and 36·62 alumina, consequently it was $5Al_2O_3\, 3P_2O_5$, *i.e.* the same phosphate as the formula requires.—*Poggendorff's Ann.* lxiv. p. 251.

[To be continued.]

THE GREAT SYMMETRICAL BAROMETRIC WAVE.

In the report on Atmospheric Waves which I had the honour to present to the British Association for the Advancement of Science at its last meeting, there is one point of great interest, and which it is very desirable to investigate further; I allude to the recurrence of the great symmetrical barometric wave, an engraving of which is inserted in the Report of the Thirteenth Meeting of the Association. This wave, extending over thirteen days, was first observed in November 1842; the apex passed London about noon of the 18th. Its return in 1843 occurred about noon of the 14th, with a rise and fall of nearly the same period; and in 1844 a nearly similar rise and fall of the barometer took place,—the epoch of the transit of the crest, October 27, about noon. These almost regular barometric oscillations render the barometric movements of the months of October and November highly interesting. It is accordingly proposed that meteorological observations, on a similar plan, should be made as extensively as possible, with a view to observe this particular wave, should it again return; and meteorologists are invited to direct their particular attention to the oscillations of the barometer during these months.

Times of Observation.—The following hours are the most suitable for the object now in view:—3 a.m., 9 a.m., 3 p.m. and 9 p.m.; these hours divide the day into four equal parts; they have been recommended by the Royal Society as *meteorological* hours, and are the hours at which observations are made daily, by direction and under the superintendence of the Honourable the Corporation of the Trinity House, which have been most advantageously used in the examination of atmospheric waves.

In cases, however, in which the observation at 3 a.m. may be inconvenient or impracticable, it will be important to substitute for it *two* observations, one at midnight and the other at 6 in the morning, so that the hours of observation will in such cases be 6 a.m., 9 a.m., 3 p.m., 9 p.m. and midnight.

To individuals who cannot command these hours, it is recommended that observations should be made *as near them as possible*; these will still be valuable, although not to so great an extent as those made at the regular hours. In these cases, however, it will be absolutely necessary to substitute *two* readings for every one of the regular hours omitted—one previous to, the other succeeding the hour so omitted; and these should, if possible, include an equal interval both before and after such hour. In all cases the *exact* hour and minute of mean time at the place of observation should be inserted in its appropriate column in the form for recording the observations.

At the regular hours of observation, or any others that the observer may fix upon, in accordance with the foregoing instructions, it will be necessary to observe,—

1st. The barometer, with its attached thermometer, and enter in the form the *actual* height observed with the temperature of the mercury.

2nd. The external and dry thermometer.

3rd. The wet bulb thermometer.

These observations are particularly essential in order to separate the pressure of the vapour from the aggregate pressure, as measured by the mercurial column.

4th. The direction and force of the wind.

These are important to determine the connexion between the undulatory and molecular motion of the wave.

5th. The character of the weather at the times of observation; which may be recorded by Capt. Beaufort's symbols.

It is proposed to commence the observations on the 1st of October next, and continue them *daily*, until the end of November, unless it should be found that at that time the wave is not completed, in which case it will be requisite to continue them a few days longer.

It will be necessary on returning the form (to be forwarded to observers) when filled, to accompany it with the following data for reduction. A blank is left for this purpose on the back of the form.

The geographical co-ordinates of the place of observation, viz. latitude and longitude.

The altitude of the cistern of the barometer above the level of the sea, *exactly*, if not, as near as it can be obtained.

The internal diameter of the tube of the barometer.

The capacity, neutral point, and temperature.

These are usually engraved on the instrument.

If the coefficients of the diurnal and annual oscillations have been determined for the place of observation, include them.

Those sets of observations which may be reduced by the observers should be accompanied with the original observations, and a reference to the tables used in their reduction, also the data above mentioned.

Gentlemen, on applying to me, may be furnished with copies of these instructions and forms for recording the observations; and all observations that may be made in accordance therewith and forwarded to me, will be carefully examined and reported on at the next meeting of the British Association.

N.B. Observations will be made at several Lighthouses by direction of the Honourable the Corporation of the Trinity House, and Captain Beaufort has most kindly undertaken to obtain observations from several of our surveying vessels.

2 Sidney Place, Cambridge Road,
Bethnal Green.

W. R. Birt.

METEOROLOGICAL OBSERVATIONS FOR JULY 1845.

Chiswick.—July 1. Rain and boisterous, with heavy rain in forenoon: overcast. 2. Overcast: rain. 3. Sultry: thunder and rain: clear at night. 4, 5. Very fine. 6. Sultry, with slight dry haze: lightning at night. 7, 8. Very fine. 9. Cloudy: rain. 10. Rain: overcast. 11. Rain: cloudy: 1 P.M. thunder and excessively heavy rain commenced. 12. Fine. 13. Slight rain: overcast. 14. Very fine: rain. 15. Showery: fine. 16. Very fine: rain. 17. Showery. 18, 19. Very fine. 20. Cloudy: rain. 21. Fine. 22. Very fine: rain. 23. Drizzly. 24. Overcast. 25, 26. Foggy: overcast. 27, 28. Cloudy: rain. 29. Heavy clouds: clear. 30. Densely clouded: rain. 31. Heavy showers.—Mean temperature of the month 1½° below the average.

Boston.—July 1. Cloudy: rain early A.M.: rain A.M. and stormy P.M. 2. Cloudy. 3. Cloudy: rain early A.M.: rain with thunder and lightning A.M. 4. Cloudy: rain, with lightning P.M. 5. Fine. 6. Fine: lightning at night. 7. Fine: 3 o'clock P.M. thermometer 81°. 8. Fine. 9. Fine: rain P.M. 10. Cloudy. 11. Rain. 12, 13. Cloudy. 14. Cloudy: rain early A.M.: rain P.M. 15, 16. Fine. 17. Rain. 18. Fine. 19. Cloudy. 20. Cloudy: rain P.M. 21—27. Cloudy. 28. Fine: rain P.M., with thunder and lightning. 29. Rain: rain early A.M: rain P.M. 30. Fine: rain P.M. 31. Fine: rain, with thunder and lightning P.M.

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

Applegarth Manse, Dumfries-shire.—July 1. Very heavy rain. 2. Very beautiful day. 3. Showers: thunder. 4. Fine. 5. Very fine. 6. Showers: thunder. 7. Showers A.M.: fine P.M. 8, 9. Showers. 10. Fair and fine. 11. Showers. 12. Fair and clear. 13. Wet. 14. Heavy showers. 15. Fair and fine. 16. Heavy showers. 17. Fair and fine. 18, 19. Fair, but cloudy. 20, 21. Fair, but clear. 22, 23. Fair, but cloudy. 24, 25. Fair and fine. 26. Showers. 27. Fair A.M.: showers P.M. 28. Fine: thunder. 29. Showers P.M. 30, 31. Showers.

Mean temperature of the month	56°·2
Mean temperature of July 1844	56 ·9
Mean temperature of July for twenty-three years	58 ·1
Rain in July	2 ·18 inches.
Mean rain in July for eighteen years	3 ·91 „

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

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.			Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. July.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.									
1.	29·665	29·572	29·09	29·16	29·10	29·46	29·21	71	52	58	57	51	53	51		sw.	s.	e–sw.	ne.	·13	·05	......	......
2.	29·824	29·776	29·26	29·55	29·70	29·45	29·80	65	56	61	64	50	49½	52½		s.	calm	nw.	sw.	·06	·16	......	·46
3.	29·828	29·640	29·19	29·60	29·40	29·88	29·65	84	52	71	65	49	57	52		s.	calm	ne.	e.	·01	·12	......	......
● 4.	30·109	30·033	29·48	29·68	29·94	29·34	29·87	75	50	62·5	62½	50½	58	50		s.	w.	sse.	wsw.	......	·20	......	·24
5.	30·196	30·161	29·69	30·04	30·10	30·03	30·19	77	56	68	66	50	58½	55		s.	w.	ese.	s.	......	·27	......	·06
6.	30·067	29·990	29·58	30·07	29·90	30·21	30·16	81	60	69	68½	52	61	52		e.	se.	se.	e.	......	......	......	......
7.	29·960	29·943	29·34	29·79	29·81	30·00	29·84	84	56	72	68	56½	52	52		se.	w.	s.	s.	·01	......	......	·06
8.	29·959	29·940	29·32	29·71	29·64	29·77	29·60	78	54	66	66	55	60	53½		sw.	w.	s.	se.	·01	......	0·82	·03
9.	30·001	29·978	29·36	29·66	29·66	29·57	29·57	71	53	66	66	54½	58	53		sw.	w.	sw.	wsw.	·15	......	......	......
10.	29·818	29·721	29·22	29·56	29·52	29·60	29·67	71	55	61	67	54	55	50		sw.	w.	s.	n.	·02	·06	......	·10
11.	29·722	29·533	29·04	29·53	29·66	29·72	29·78	70	46	64	65	47	52½	47		sw.	w.	nne.	nw.	·71	·10	......	·10
☽ 12.	29·946	29·907	29·40	29·78	29·81	29·83	29·87	64	48	56	63	44½	55	50		nw.	w.	nnw.	nw.	·07	·08	......	·05
13.	29·940	29·811	29·49	29·79	29·69	29·84	29·80	71	55	64	56½	46½	51½	50		sw.	calm	se.	nw.	·01	......	......	·09
14.	30·035	29·884	29·35	29·80	29·96	29·84	30·03	71	49	61	63½	52½	51½	49½		w.	w.	nw.	nw.	·04	·06	......	·04
15.	30·069	30·059	29·61	30·00	29·94	30·10	30·09	71	46	60	63½	44	49	49		n.	nw.	nw.	n.	·03	·06	......	·15
16.	30·045	30·005	29·56	29·93	29·82	30·02	29·94	71	54	58·5	62	41½	49½	50		sw.	calm	sw.	sw.	·07	......	0·60	......
17.	30·029	29·974	29·47	29·77	29·84	29·92	29·94	75	54	58	62½	51½	54	53		sw.	calm	w.	se.	·02	·09	......	......
18.	30·091	30·066	29·55	29·94	30·00	30·08	30·17	76	48	64	68	52½	58	53½		sw.	w.	e.	e.	......	......	......	......
○ 19.	30·058	30·020	29·58	30·03	30·05	30·22	30·22	71	52	58	65	51½	58	53		e.	w.	ene.	se.	......	......	......	......
20.	30·041	29·955	29·60	30·08	30·04	30·26	30·27	71	53	57	62½	51½	54	52		e.	n.	n.	n.	·24	......	......	......
21.	29·924	29·908	29·50	30·02	30·04	30·27	30·25	73	57	61	64	54	54	50		e.	calm	ene.	ne.	......	·12	......	......
22.	29·935	29·866	29·49	30·00	30·00	30·25	30·20	71	54	57	60½	51½	51½	52		ne.	e.	ene.	n.	·05	......	......	......
23.	29·910	29·905	29·48	29·98	29·98	30·15	30·10	58	53	57	58	50	52	51		ne.	calm	ne.	nw.	·05	......	......	......
24.	29·924	29·905	29·48	29·92	29·90	30·03	29·92	60	55	57	60½	51½	55	52		ne.	calm	w.	w.	......	......	0·25	......
25.	29·932	29·931	29·50	29·83	29·80	29·85	29·78	64	57	61	65½	54½	56	53½		ne.	calm	s.	sw.	......	......	......	......
☾ 26.	29·929	29·921	29·43	29·78	29·68	29·66	29·51	71	52	64	63½	50	58	57		se.	calm	sw.	s.	......	......	......	......
27.	29·869	29·774	29·27	29·48	29·54	29·40	29·42	67	48	63	64	55½	60	52		w.	calm	sw.	w.	·07	......	......	·16
28.	29·746	29·577	29·22	29·55	29·53	29·55	29·65	72	44	63·5	63½	45½	55	51		nw.	calm	w.	......	·05	......	......	......
29.	29·794	29·559	29·10	29·58	29·60	29·74	29·72	65	43	53	65	41½	56	52		w.	calm	sw.	......	......	·78	......	.15
30.	29·790	29·577	29·24	29·50	29·30	29·55	29·60	64	45	62	62½	40½	53	51		sw.	calm	se.	se.	·33	·19	0·51	......
31.	29·587	29·545	29·09	29·28	29·29	29·62	29·65	69	50	61	61½	51	56	54½		sw.	w.	se.	e.	·18	·17	......	......
Mean.	29·927	29·852	29·38	29·464	29·749	29·845	29·853	71·03	51·84	61·7	63·5	48·4	54·88	51·74						2·31	2·51	2·18	1·69

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XXXV. *An Account of the Fossil Trees found at St. Helen's.*

By Edward William Binney *and* Robert Harkness*.

[With two Plates.]

ABOUT the beginning of August 1843, owing to the excavations made in the White Grit Quarry at Scotch-row, near St. Helen's, three almost perfect specimens of fossil trees were discovered.

At the Cork meeting of the British Association, Mr. Binney gave a verbal account of the fossils, and in October of the same year read a paper upon them before the Manchester Geological Society, which was afterwards published in the Philosophical Magazine for March of last year.

The fossils, then laid bare only on their western side, were found standing almost upright in a thick deposit of indurated gray silty clay, locally known by the name of "warren," which lies immediately upon the white sandstone, which in this locality is about ten yards thick, and which it is necessary to remove before the white sandstone can be worked. The whole thickness of the clay, as seen at this period, was about seven yards, and the bases of the fossils were on an average about eight feet above the white sandstone; above the clay there occurred a deposit of till, about seven feet in thickness, which appeared to cut off the upper portion of the trees; all of which stood in a line running nearly north and south, and at right angles to the strata, which dip at an angle of about 23° to the E.S.E.

The geological position of the deposit in which the fossil trees are met with, is in the lower part of the middle Lancashire coal-field, about 119 yards above the Rushey-park seam, the last thick coal in the series, and between the seams known in the neighbourhood of St. Helen's by the names of the Sir

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* Communicated by the Authors.

Roger and Yard mines. We are enabled, by the kindness of our friend Mr. William Hawkshead Talbot, to give the following section showing the position of the fossils:—

	yds.	ft.	in.
Coal	0	2	0
Dirt	0	1	0
Coal	0	0	9
Dirt	0	0	6
Coal	0	1	0
Warren containing the trees .	17	0	0
Stone, "white grit"	16	1	0
Coal and dirt	1	0	0

The thickness of the warren and white grit, as shown in this section, differs from that obtained in the quarry; but this circumstance is easily accounted for, when we consider that in the one case the section is from the deep, whilst in the other, at the quarry, it is taken at the outcrop.

On the discovery of the trees, hundreds of people visited them, and the novelty of the occurrence, as well as their very perfect state of preservation, had so far excited the ignorant cupidity of many, that although every care was taken by the proprietor of the quarry to preserve them, yet still individuals found means to obtain portions of the roots, by placing ladders against the perpendicular wall of rock beneath them at night, and by this means mutilated them very considerably.

The tree on the north side of the quarry was about four feet in length, cylindrical, decorticated, and about a foot in thickness, not possessing any well-defined seams, but with ribs and furrows, proving it to be a Sigillaria. At its base there were no distinct roots, but in the clay underneath it were numerous impressions, which were evidently those of roots, and from which there emanated distinct and easily traced fibrils or rootlets.

The one occupying a central position in the quarry had been removed, and was lying in a fragmentary state in an adjoining quarry. Its diameter was about fifteen inches, and in form it appeared to have been nearly cylindrical. The external surface presented all the scars, ribs and furrows which commonly appertain to *Sigillaria reniformis*, so much so as to induce the authors to believe that the individual belonged to that species. From information received from the workmen, this specimen does not appear to have had any traces of roots.

The individual situated on the south-west side of the quarry was by far the largest and most perfect specimen, being originally nine feet in height; two feet had however been removed from it when the authors first saw it, so that when measured by them it was found to be only seven feet high. It differed from the others in form, being tapering instead of cylindrical. Fully one-half the tree was encased in the matrix, but sufficient was visible to give a correct idea of the nature of the stem, the upper portion of which was covered by pretty regular ribs, which were about two inches in breadth and separated from each other by shallow and narrow furrows, partially filled with coaly matter. The surface of the ribs was covered with irregular longitudinal lines, and when viewed through a lens showed distinct impressions of longitudinal fibres: no traces of scars were however visible.

The trunk was for the most part decorticated, but in some places small patches of coaly bark were observable. The ribs and furrows became more irregular and indistinct as they approached the base, and in the lower parts of the tree wholly disappeared. At this part the stem presented a rugged and gnarled appearance, much resembling the base of large and old trees when deprived of their bark.

At the base of the stem were found four roots, which seemed to emanate from the tree in pairs. The two which issued immediately from the most prominent portion of the base of the tree, as seen when in part encased in its matrix, had been in a great measure removed, only about eight inches remaining; but the two others were more perfect and larger, the one being about fourteen inches and the other about two feet in length. The workmen however affirmed that the roots were originally considerably larger, and that they had extended fully nine feet from the stem. All the roots were covered with bark, which adhered to the matrix in which they were imbedded, so that when cleared they always appeared decorticated. The surface of the roots, when at any distance from the stem, was covered with the corrugated lines and areolæ, which are characteristic of Stigmaria. From each of these areolæ there issued a long, generally compressed, black fibril, having in some cases traces of a small internal cylinder. These fibrils were met with running in all directions through the indurated blue clay, and some of which when measured exceeded three feet in length.

Such is a description of the trees as seen in August 1843; they remained in the same position from this period, protected, by a perpendicular face of rock sufficiently high to prevent access by ordinary ladders, from the vandalism which previously injured them; and by their mineral nature, which differed from the substance in which they were imbedded, being a hard fine-grained grayish-brown sandstone, from the effects of the atmosphere until the month of May 1845. During this interval they were visited at different times by the authors, who were desirous of being present when the whole of the larger tree should be uncovered, in order to examine the opposite side. This occurrence took place on the 21st of May last, intimation having previously been given to Dr. Buckland and other geologists, that on that day the tree would be entirely uncovered, in order to afford them an opportunity of seeing the connection which subsists between Sigillaria and Stigmaria, as shown by this plant. Circumstances having, however, interfered to prevent several from availing themselves of the opportunity, the only individuals who were present when the tree was laid bare, with the exception of the workmen, were the authors of this account. The appearances on the eastern side of the tree were very similar to those on the western: the furrows and ribs were to a great extent the same, and at once identified the tree with Sigillaria, although no distinct scars were noticeable. Larger portions of the bark occurred on this side than on the one laid bare two years previous. One of these near the top, in thickness from one-third to half an inch, was eight inches square, and another near the base was of still larger size. These patches of bark were marked by longitudinal fibrous lines, easily distinguishable to the eye, and similar to those which occurred upon the stem when decorticated, but of greater size. The stem contained no internal cylinder, and all its tissues seemed to have been removed before the mineral matter of which it was composed had filled up the internal portion of the plant. A considerable portion had disappeared from the upper part of the stem since it was first seen in August 1843, the eastern side only measuring three feet seven inches in height, and its circumference at the top was six feet eleven inches, about the middle eight feet five inches, and at the base nine feet nine inches, showing correctly the tapering form of the stem, which at its lower part presented the same gnarled appearance as seen on the opposite side two years previously.

At the base of the stem the tree appeared to increase in thickness very rapidly, and on removing the matrix on the north-eastern side, a bulging form was in part visible; an appearance owing to the roots diverging from the stem, which when a greater portion of the gray clay had been removed, became observable. On this side there seemed at first to be two enormously thick roots, each occupying about a quarter of the base of the tree; but on the removal of more of the matrix, each of these two roots again divided themselves equally, and gave rise to two others similar in size: and when more of the clay had been removed, each of these secondary roots in their turn became dichotomously divided, and gave origin to two other equal and similar roots.

To enter into a more detailed description of these roots and their sizes :—The main roots at their commencement measured each three feet two inches across, and, after running about one foot three inches, gave birth to the secondary ones, which were about two feet four inches in breadth at their commencement; these, at the distance of about seven inches from their origin, again bifurcated and produced two tertiary roots, each of which was about one foot four inches across. By this regular mode of dividing each of the main roots produced four distinct and separate ones, each at its commencement of a compressed oval form. The tertiary roots struck down into the clay in an almost perpendicular direction from one foot ten inches to about two feet, after which they took a nearly horizontal direction, without taking the dip of the strata into consideration, or else they would have appeared to run upwards. In order to give a more correct idea of the roots, the following description of the drawings has been added :—In Plate V. fig. 1 A represents a fragment of a root on the northern side which had been injured in making the excavation; its inclination was about 30°. The one marked B ran at its commencement more on the level than the preceding one, and swelled out in a singular manner for some distance, dipping at an angle of 20°; it soon however struck downwards at a much steeper angle, viz. 50°, and after proceeding in that direction about fourteen inches, it took a horizontal course and became a flattened Stigmaria. The one marked C dipped downwards at an angle of 58°; that marked D at 80°, and the ones marked E and F at a similar angle, and G at 50° before they took horizontal directions. The root H, like A, was a mere fragment and dipped at an angle of 35°. All the roots taper much as they proceed downwards, assuming more and more the Stigmaria character, until they run through the strata in a horizontal direction, when they become possessed of all the appearances of flattened specimens of that plant. In order to show how quickly the roots diminish in size, it may be stated that the one marked G at the point of its first bifurcation measured one foot three inches across, and at the distance of three feet below the same root only measured five inches in breadth. This root was four feet six inches long from its origin, at the stem, to its lowest extremity traced, from whence a cylindrical rootlet, about an inch in diameter, having a thick coaly envelope, and sending out side shoots took its rise, an appendage not observed on any other of the five roots, which were followed about four feet, when they entered masses of ironstone, which prevented the successful use of the pickaxe, so that none of them were traced to their termination.

All the roots were surrounded throughout their whole length with a coaly bark, but like that which covered the stem, it adhered so firmly to the matrix, that in general, when the roots were laid bare, they presented a decorticated appearance. The upper surface of the decorticated roots was marked with irregular and interrupted ribs and furrows, which diverged on each side of lines parallel to the longitudinal axis of the root, as observed in the fossil trees of Dixon Fold. Several slight depressions traversed the upper sides of the roots in a diagonal direction to the ribs and furrows, these resembled impressions produced by long grass-like fibres having laid upon the roots; circular impressions occurred similar to those seen on the large stems of Stigmaria. From each of these impressions issued the long fibrous appendages which have been termed leaves. These fibrils were met with in the matrix all round the roots, but they occurred much more abundantly on the under than on the upper sides, and were in general less frequent on the higher parts. These appendages were cylindrical, and when not compressed, about a quarter of an inch in diameter; their length must have been very great, for at the distance of three or four feet from the root, they were not found to decrease in thickness: they occur abundantly throughout the whole depth of the warren, which is seventeen yards in thickness, so that it is not improbable that these appendages may be at least equal in length to those described by Mr. Steinhauer. These appendages were covered by a carbonaceous envelope, and some of them appeared to afford traces of a small internal cylinder.

The deposit all around the tree abounded in various coal plants; these, for the most part, consisted of leaves of the *Pecopteris nervosa*, sometimes adhering to their rachis, Calamites, Asterophyllites, Lepidostrobus, Lepidodendron, species of Neuropteris and Cyclopteris. The Calamites appeared in great abundance, and in several instances specimens were obtained which appeared to have their bases in contact with the main roots, before they assume the Stigmaria character, in such a manner as almost to induce the observers to believe that they had some intimate connection with the tree.

In removing the encasing matter from the eastern side of the large tree, another was discovered about five feet distant, and due east from the large tree; occurring further on the dip than the other, it appeared rather lower in position, but in reality its base was about one foot six inches higher before the strata were inclined. Of this tree only one foot seven inches of the stem remained, which at its base measured four feet seven inches in circumference. As in the large tree, the whole of the bark was converted into coal, about one-third of an inch thick, which fortunately adhered to the stem instead of the matrix much better than in any of the other trees, and therefore showed the ribs and furrows of Sigillaria in a much more perfect and distinct manner than the other individuals. This tree, which was cylindrical in shape, was of a softer nature than the others, being composed of the indurated gray silty clay which constituted the matrix. Its base was a little concave, and from it sprung four slightly compressed roots, measuring one foot six inches across at their origin. On the under side of each root in the middle, appeared a singular suture which ran under the base of the tree, and there uniting formed a regular cross. The roots struck downwards almost perpendicularly into the strata, and the whole tree bore great resemblance to the molar teeth of a human being. The external surface of the roots was covered with a coaly envelope much thinner than that which occurred upon the stem, and was marked in a similar manner to the surfaces of the roots of the large plant, except that it showed the raised circular markings of Stigmaria and the diagonal depressions previously alluded to running across their upper surfaces in a more prominent degree. The roots all proceeded downwards to the depth of about one foot six inches, tapering in thickness until they came nearly to a point; afterwards they took a horizontal direction, and could be traced as thin black flattened carbonaceous matter, sending forth radicles like those described previously in all directions, being in fact Stigmaria. In all respects this stem and root and the stem and root of the large individual already described resemble each other, except in the number of the roots, appearing to be individuals of the same species, differing only in age (see Plate V. fig. 2).

No person, even on a casual view, could fail to be struck with the great regularity and uniformity of the roots of the larger tree when taken collectively. This great regularity resulted from the spaces which intervened between each of the roots, and which were of so uniform a nature that they could scarce have been produced by accident. Of these spaces which ran up to the base of the stem, there were four principal ones, each resembling the other in size and form, and separated from each other by the main roots, which were more than three feet across. The boundaries of these intervals, which were the sides of the main roots, gave them a rounded form at their upper extremity, beneath which, at the distance of about nine inches, they measured nearly a foot across. As the roots proceeded downwards, they increased in width, and the spaces became narrower in consequence, until the roots again rapidly narrowed and finally terminated. The secondary roots also gave rise to secondary spaces; these commenced about one foot three inches below the upper part of the principal spaces; they were likewise rounded at their upper extremities, but continued to increase in width as they descended. These spaces were separated from each other by the thickness of the secondary roots, which measured two feet four inches. At the distance of about seven inches below the higher extremities of these secondary spaces, originated the spaces caused by the bifurcation of the tertiary roots; these were separated from each other at their origin by one foot four inches, the breadth of the tertiary roots. In form these spaces resembled those produced by the secondary roots, except increasing more rapidly in width as they descended. This uniformity and regularity in the roots of this plant will be more manifest by referring to Plate VI., which would represent the roots if compressed; where A shows the main roots, A′ the principal areas, B the secondary roots, and B′ the secondary areas, C the tertiary roots, and C′ the tertiary areas. This peculiar regular mode of rooting is most probably characteristic of this genus of plants, and the various degrees of dichotomization appear to result from a difference of age in the different individuals. Thus, in the small specimen found about five feet east of the large one, there is seen only the four main roots, which had not commenced bifurcating at their base.

In the trees found at Dixon Fold, the same regular appearances are presented as in the large individual; and a Stigmaria in the museum of the Leeds Philosophical Society affords evidence of a similar regularity.

Such is the description of a tree having for its base and roots a fossil the most common in the coal formation, and concerning which various incorrect opinions have been propagated, resulting in part from an examination of imperfect specimens, and also from authors refusing to receive correct descriptions, which did not altogether coincide with their preconceived ideas of the natural affinities of this fossil. Stigmaria has, since the commencement of geology as a science, never ceased to exercise the ingenuity of fossil botanists; but of all the accounts we possess concerning this fossil, the one published in the year 1818 in the Transactions of the American Philosophical Society stands pre-eminent, both for its fullness and its remarkable accuracy; and although some of the deductions are incorrect, still the great care and correctness of Mr. Steinhauer, as manifest in this account, cannot be too highly eulogized, since every discovery connected with this fossil renders the value of the description more appreciable.

The authors of the Fossil Flora have represented, in the 31st plate of their valuable work, what they term a "nearly perfect specimen" of Stigmaria. This specimen, which was obtained from the roof of one of the Newcastle coal seams, shows only the under side; the central part, like the large tree already described, was concave, "the whole surface being covered with wrinkles, which, when attentively examined, were seen to be caused by depressed semicircular spots, compactly arranged in a spiral manner, in the centre of which is a roundish scar, to which a little fine coaly matter adheres." From this centre the arms radiate, bifurcating as they increase in length, and becoming covered with the characteristic tubercles of Stigmaria, having fibrils attached as they receded from the centre. Two other individuals occurred in the same roof, but the authors state "that the upper part of the centre itself was too much damaged to have its structure made out." From an examination of these specimens they were induced to conclude "that Stigmaria was a prostrate land-plant, the branches of which radiated regularly from a common centre, and finally became forked." What the nature of this centre was, the authors declare to be difficult to conjecture, but they conceive it to have been dome-shaped, a form which probably resulted from the plant growing upon a hillock. The succulent nature of Stigmaria they infer from its commonly occurring in a compressed state, and also from the eccentricity of its woody axis, when this is traceable, which also shows it to have been of a dicotyledonous nature.

From the great regularity of the tubercles which cover the radiating portions of the plant, the authors are led to suppose that they were the base to which leaves were attached, and hence they reject the conclusion of Steinhauer, that they were the parts from whence the roots emanated. The same circumstance which led them to infer the succulency of the radiating parts, viz. their compressed state, also induced them to suppose that what they termed leaves were of a similar character. From their examination of the whole characters, the authors came to the conclusion that the Stigmariæ were allied to the Euphorbiaceæ or Cacti.

The reliance on the part of these authors on the perfection of their specimen, which appears to have been sufficiently imperfect entirely to mislead them, has been the cause of many errors having been propagated respecting Stigmaria. In the first place, there appears to have been no evidence upon which they were able to form any opinion as to the nature of the roots of their creeping plant, but on the contrary, the concave base from whence the roots ought to have issued possessed similar characters with the radiating stems; and with regard to the leaves, these appear to have radiated from the so-called stems, not only striking upwards and sideways, but also *downwards*,—rather an extraordinary direction for the leaves of a creeping plant to take. Then as regards the supposed dome-shaped summit, which they state to be much damaged, they do not appear to have looked after any portion of it in the roof from whence the plant fell. Altogether these plants appear to have borne great resemblance to the base and roots of the large tree at St. Helen's, which would most probably have presented a perfect similarity, had the circumstances under which it was first discovered been the same, viz. by excavating under its base, when the lower portion would most probably have separated from the matrix, by one of the joints which run through the plant from the strata, and have fallen on the floor a perfect duplicate of the Jarrow fossil.

Dr. Buckland, in his Bridgewater Treatise, takes for granted the dome-shaped summit, which was merely conjectured by the authors of the Fossil Flora; but the absence of large roots appears to have been a difficulty, and he is led to trail in swamps or float in shallow lakes his Stigmaria, "like the modern Stratiotes and Isoetes." Indeed, when we consider that fossil botany has been termed the romance of natural history, we need not express any surprise at the numerous and vague opinions which have floated along with this plant.

Such are the two opinions which have been most currently adopted, and which are still most generally entertained concerning the nature of Stigmaria, which our description has shown to be merely the root of Sigillaria. Perhaps it may be urged that there is scarcely sufficient proof to show that the larger tree was a Sigillaria, for although possessed of ribs and furrows, it wanted the characteristic scars of this genus. These scars are however in general obliterated near the base of all large specimens, an effect the result of age, which also causes several of the ribs, each of which is covered with a series of scars, to unite and form themselves into one broad rib. These scars also undergo considerable change in form as the plant increases in age, first narrowing and lengthening considerably, with only a small circular space in their centre covered with a coaly envelope: this circular space gradually disappears as the tree increases in size, and the elongated lines become less distinct, and ultimately in old specimens all traces of it are lost; the large ribs themselves at the base becoming imperfect, as seen in the large individual.

The interior of the large specimen did not afford any traces of the small internal cylinder eccentrically situated, so common to uncompressed Sigillaria; nor by fracture did it show anything which would lead to a knowledge of its internal structure. Fortunately for fossil botany, a specimen of *Sigillaria elegans* from Autun, with internal structure, fell into the hands of the illustrious French botanist, Adolphe Brongniart. From an examination of this individual, he was induced to refer Sigillaria to the Cycadeous gymnosperms, and the internal cylinder which had hitherto been considered as pith, he found to consist of both pith and woody fibre, separated from each other by reticulated vascular tissue. Between this cylinder and the cuticle there was an immense development of cellular tissue, through which the bundles of vessels ran which communicated with the scars on the exterior of the plant. In his observations on the internal structure of this fossil, this botanist enters into an examination of the characters of Stigmaria, and suggests, that in all probability it is merely the root of Sigillaria, and that in its internal arrangement it bears great resemblance to the roots of the *Zamia pungens*. With regard to the regular order of the areolæ, which induced the authors of the Fossil Flora to suppose that Stigmaria could not have been roots, Brongniart states that a similar arrangement is met with in the roots of some aquatic plants.

In the year 1839, Mr. Binney, in company with several gentlemen, found some upright specimens of *Sigillaria reniformis* resting upon a small seam of coal exposed in cutting the tunnel at Clay Cross, on the North Midland Railway near Chesterfield. These were followed downwards until they became Stigmaria, but there was some difficulty of proving the absolute insertion of one plant into the other, so that in this case some doubt existed.

From the present time, by the discovery of the fossil tree at St. Helen's, the question as to the relation which existed between Sigillaria and Stigmaria must be considered as settled in favour of the opinion of the one being the stem and the other the root of the same plant, justifying the conclusion formed by one of the authors of this account in 1839, and the inferences afterwards drawn from an examination of their internal structure by Brongniart.

What may have been the foliage of Sigillaria, the authors, from actual observation, are not prepared to say; but they consider it not improbable that *Pecopteris nervosa*, from its great abundance and its peculiar character, may have been intimately connected with this plant. This peculiar character consists in the absence of any midrib in the leaflets, and in having several veins originating from the rachis, circumstances very different from those which prevail in true ferns, more particularly in the Pecopteris, and which appear to connect it with Odontopteris, a form very nearly allied to Otopteris, a fossil of decided cycadeous type, occurring along with other cycadeous forms in the Yorkshire oolite*.

The roots of Sigillaria are more abundant than any other remains which occur in the coal formation, and the smaller fibrils abound to such a degree in coal floors, that it is scarcely possible to obtain a hand specimen of one of them without meeting with some trace of the rootlets of Stigmaria; and in most of the floors these roots are met with unassociated with any other description of plant.

This great abundance of the roots in floors, and the very frequent occurrence of stems of Sigillaria in various states in the coal deposits, would almost justify the supposition that this plant had, by its destruction, formed the chief portion of our coal seams, an opinion which is further confirmed when we remember that such portions of coal as show traces of internal organization have decidedly a gymnospermous character.

The upright position of many of these stems also seems to lead to the conclusion, that these trees have not been transported by the agency of water, but rather that they have perished on the spot where they originally grew, most probably by some such cause as a subsidence of the locality, to which circumstance it is probable that we owe the formation of all our coal deposits with floors containing Stigmaria.

The external character, the internal structure, and all the circumstances under which Sigillaria occur, render the supposition that this plant was of a partially aquatic nature, and required a watery habitat, exceedingly probable. Its roots most commonly occur in a deposit which was originally soft mud, through which the rootlets struck for a considerable distance in all directions: the quincunx order of the arrangement of the scars which cover the main roots also justifies the conclusion as to the nature of the habitat in which it grew and flourished during the epoch when plants were gradually purifying the atmosphere and rendering it fit for the respiration of vertebrated animals.

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* The authors are unable to conceive on what grounds Lindley and Hutton have referred this specimen to Pecopteris, having only the adherence of its base to the rachis to identify it with this genus, and wanting entirely the distinct midrib from whence the veins originate.

XXXVI. *On the Constitution of Bebeerine.* *By* Douglas Maclagan, *M.D., F.R.S.E., and* Thomas G. Tilley, *Esq., Professor of Chemistry in the Queen's College, Birmingham**.

BEBEERINE is the name given by Dr. Rodie of Demerara to an organic base, of which he pointed out the existence, in the bark of a tree of that colony known by the names of *Bebeeru* or *Green heart*. This tree has recently been examined botanically by Sir Robert Schomburgk, and found to be a species of *Nectandra*, which in compliment to Dr. Rodie he has named *N. Rodiei* (Hooker's London Journal of Botany, December 1844).

The general properties of the alkali, and the preparation of its sulphate for medicinal use, were further described by one of us in a paper read before the Royal Society of Edinburgh in April 1843 (Transactions, vol. xv. part iii.). In this memoir an account was given of the preparation of bebeerine, in what was regarded as a state of chemical purity, and of its separation by means of æther from what was believed to be another alkaline matter, sipeerine; but as the substance is neither crystallizable nor colourless, it could not decidedly be affirmed to be chemically pure until its constitution had been determined by ultimate analysis. This desideratum it is proposed to supply in the present paper.

The chief difficulty in preparing the pure alkali arose from the troublesome process required to free it from adhering tannin, considerable loss being experienced in the methods then adopted. This has subsequently been accomplished with greater facility by the following process, which may be applicable to the purification of other organic bases.

The sulphate prepared for medicinal use, which is always more or less impure, is precipitated by ammonia; and the alkaline matter thus separated, is, after washing with water, triturated with about an equal weight of freshly precipitated and moist hydrated oxide of lead. The magma thus formed is then dried over the water-bath, and the alkali is taken up by absolute alcohol. On distilling off the spirit, the organic base is left in the form of a transparent orange-yellow resinous mass, as described in the paper above quoted. This, on being dried, pulverized, and treated with successive portions of pure æther, is in great part dissolved in this fluid. The dissolved portion, on distilling off the æther, is obtained in the form of a translucent, amorphous, but homogeneous resinous-looking substance, of a pale yellow colour, and possessed of all the pro-

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* Communicated by the Authors.

perties of an organic alkali. This is pure bebeerine, as described in April 1843.

Bebeerine thus obtained does not crystallize. It is very soluble in alcohol, less so in æther, and very sparingly in water. Heated, it fuses; and the heat being continued it swells up, giving off vapours of a strong peculiar odour, and burns without residue. Subjected to the action of oxidizing agents, it gives with bichromate of potash and sulphuric acid a black, and with nitric acid a yellow resin. It forms with acids salts which are all uncrystallizable; with perchloride of gold, mercury, copper, iron and platinum, it gives precipitates which are soluble to a certain extent in hot water and alcohol, but which on the solution cooling are not deposited from it in a crystalline form*.

To ascertain the composition of the base, two portions of it, prepared at separate times, were subjected to combustion.

(1.) 0·300 grms. substance gave 0·179 water and 0·788 carbonic acid.

(2.) 0·3505 substance gave 0·1965 water and 0·922 carbonic acid.

(3.) 0·350 substance gave 0·2095 water and 0·914 carbonic acid.

Nitrogen was estimated by Will and Varrentrapp's method.

(1.) 0·4030 substance gave 0·3490 ammonio-chloride of platinum.

(2.) 0·417 substance gave 0·1175 platinum.

These numbers lead to the following per-centage proportions, the atomic weight of carbon being taken at 75·85.

	I.	II.	III.	Mean.
Carbon . .	72·22	71·75	71·80	71·92
Hydrogen .	6·62	6·22	6·64	6·49
Nitrogen . .	5·49	4·02		4·75
Oxygen . .	15·67	18·01		16·84
	100·00	100·00		100·00

The combination with chloride of platinum was made use of to determine the atomic weight of the base.

(1.) 0·6380 salt gave 0·1215 platinum = 19·04 per cent.

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* When precipitated by ammonia from its solutoin in acids, it is obtained in the form of a very pale yellow powder. This was examined with a view to determine whether or not it was a hydrate of the alkali. It was washed, dried *in vacuo* over sulphuric acid, and heated in an oil-bath. At 308° Fahr. it began to soften, and at 370° it fused into a transparent brown resinous mass. It did not sensibly lose weight during the process, and had undergone no change in properties from exposure to this temperature, as it retained its perfect solubility in acids, and formed with them the usual bright yellow solution.

(2.) 0·6250 salt gave 0·1200 platinum = 19·28 per cent.

(3.) 0·5335 gave 0·1080 platinum = 20·20 per cent.

The platinum salt was burned with chromate of lead.

0·4005 salt gave 0·152 water and 0·6232 carbonic acid.

For nitrogen.—0·430 salt gave 0·177 ammonio-chloride of platinum.

Chlorine was estimated by heating with lime, dissolving in nitric acid, and precipitating by nitrate of silver.

0·420 salt gave 0·3730 chloride of silver.

These numbers lead to the following formulæ:—

	I.	II.	III.
Carbon . . .	42·69		
Hydrogen . .	4·21		
Nitrogen . . .	2·61		
Oxygen . . .	10·36		
Platinum . .	19·04	19·24	20·24
Chlorine . . .	21·09		
	100·00		

	I.	II.	III.	Atoms.		Calculated per cent.
Carbon .	42·69			35 =	2654·76	42·43
Hydrogen	4·21			42	262·07	4·18
Nitrogen .	2·61			2	177·04	2·83
Oxygen .	10·36			6	600·00	9·59
Platinum	19·04	19·24	20·24	1	1233·30	19·73
Chlorine .	21·09			6	1328·00	21·23
	100·00				6255·17	99·99

The atomic weight calculated from the quantity of platinum found, is—

I. 6476·0

II. 6423·4

III. 6092·2

$$\frac{18991{\cdot}6}{3} = 6330{\cdot}2 \left\{ \begin{array}{l} \text{mean found atomic weight of pla-} \\ \text{tinum salt.} \end{array} \right.$$

Referring to our previous numbers, we find—

	Atoms.		Calculated per cent.	Mean found.
Carbon .	35 =	2654·75	72·11	71·92
Hydrogen	40	249·59	6·77	6·49
Nitrogen .	2	177·04	4·82	4·75
Oxygen .	6	600·00	16·30	16·84
		3681·38	100·00	100·00

On comparing these calculated and found numbers, they will be seen to agree, and on subtracting the weight of one atom of chloride of platinum and hydrochloric acid from the atomic weight of the platinum salt of bebeerine, we find

$PtCl_4$	2118·30	6330·20	1 atom platinum salt
HCl	455·13	2573·43	1 atom $PtCl_4 + HCl$
	2573·43	3756·77	atomic weight of bebeerine

which is a close approximation to the found numbers. The rational formula for bebeerine then is

$$C_{35}H_{40}N_2O_6.$$

It appears to have been sufficiently established by the experience of medical practitioners, that bebeerine possesses marked properties as a remedy for periodic diseases (Maclagan, Edinburgh Medical and Surgical Journal, April 1845). On comparing its composition with that of other vegetable alkalies possessed of similar remedial powers, such as quinine and cinchonine, it is not a little remarkable that it differs from them in atomic constitution, and that it is isomeric with morphia which acts as a pure narcotic. The atomic constitution of morphia, calculated from the formulæ deduced by Liebig and Regnault from their analyses, agrees perfectly with that given above for bebeerine. The composition of the two bases is, in fact, identical.

This is the first instance of isomerism occurring in substances belonging to the class of organic bases.

One very interesting conclusion regarding the action of medicinal substances appears to be deducible from these results, that similarity of physiological properties does not depend upon similarity in the proportions of their constituents. It seems probable that the mode in which their atoms are grouped has an important share in modifying their physiological actions; for in the present instance we have the same number of atoms both in morphia and bebeerine, but in the one producing a white crystalline body of narcotic qualities, in the other a yellow amorphous resinous-looking substance, acting as an anti-periodic. The difference in their physical properties in fact proves that their elements are differently arranged.

We have not been enabled to examine *sipeerine*, the substance left behind when pure bebeerine is dissolved out by means of æther. It occurs in very minute quantity, and requires a very tedious repetition of the treatment with æther to free it from the last traces of bebeerine. There seems, from some trials which we have made, good ground for believing that it is a distinct alkali, but it has not been obtained in sufficient quantity to enable it to be subjected to ultimate analysis.

XXXVII. *On the Spheroidal Condition of Liquids.*
By W. G. Armstrong, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

At the recent meeting of the British Association at Cambridge, I had the pleasure of witnessing M. Boutigny's pleasing experiments on the spheroidal condition of liquids; and since my return to Newcastle I have exhibited the same experiments to the Philosophical Society of this town, accompanied by explanatory observations, the substance of which I take the liberty of communicating to you for insertion in the Philosophical Magazine. This I am induced to do, not only because I conceive that observations upon a subject so much before the public as this is at present, are likely to be interesting to a considerable portion of your readers, but also because the explanation of the phænomena is still in some measure a matter of controversy.

Although M. Boutigny's experiments embody no new discovery, yet they have the no small merit of showing in a striking and prominent manner, effects with which most persons are in some degree acquainted, but which fail to excite attention in consequence of the unattractive form in which they are commonly observed.

The fundamental experiment of the series consists in dropping water upon a metallic plate or dish to which heat is applied. If the dish be heated to 212° or upwards, the water immediately boils and is dissipated in vapour, provided the temperature of the dish do not exceed a certain limit; but if it do exceed that limit, then the condition of the liquid is entirely changed and the following effects may be observed:—

1st. The ebullition of the water, instead of being accelerated, is altogether prevented, and the evaporation is greatly retarded.

2nd. The water assumes a rounded or "*spheroidal*" form, like a globule of mercury.

3rd. The water does not touch the dish, being separated from it by a thin interposing stratum of vapour.

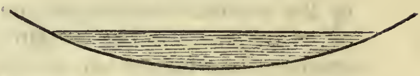
4th. The temperature of the water never rises above 205° (96 centigrade), being 7° below the boiling-point.

A tolerably correct idea may be formed of the spheroidal condition of water by reference to the annexed diagrams. Fig. 1 represents a dish containing water in the spheroidal form, and, by way of contrast, the diagram fig. 2 represents a similar dish containing water which is not in the spheroidal form.

If instead of water we use a liquid that boils at a lower temperature than water, then similar effects are obtained at a reduced temperature of the dish. Thus if the dish be not sufficiently hot to sustain water in the spheroidal state, it will still be sufficiently hot to sustain spirit of wine in that state; and again, when it is too cool for spirit of wine, it may still have sufficient heat to produce the effect with æther. On the other hand, if the boiling-point of the liquid be higher than that of water, then it is necessary to increase the temperature of the dish, in order to produce the spheroidal state. But whatever the boiling-point of the liquid may be, the maximum temperature it attains in the spheroidal form is always several degrees below the temperature at which ebullition takes place.

Fig. 1.

Fig. 2.



It is obvious, therefore, that the cause which limits the temperature of the liquid spheroid, as well as that which occasions its separation from the heating surface, has an intimate connection with the boiling-point of the liquid. This circumstance affords a strong presumption that the effects are in both cases in some way or other attributable to evaporation; and I shall now endeavour to explain the phænomena in conformity with this view of the subject.

It is easy to conceive that the radiant heat of a red-hot dish, into which water is poured, will be sufficiently powerful to vaporize the inferior surface of the water *before* it comes into actual contact with the dish; and it is obvious that the vapour, resulting from such an action of the heat, must necessarily find a place between the water and the dish, so as to prevent their nearer approximation. The weight of the liquid will tend to displace the interposing steam, but the continued evaporation will maintain the separating stratum so long as the dish remains sufficiently hot to vaporize the water without actual contact.

The absence of ebullition, which also characterizes the spheroidal state of liquids, is plainly attributable to the steam being enabled to escape from beneath the fluid without rising upwards through the mass; and the rounded form which the liquid assumes is simply owing to there being no adhesion between the fluid and the dish to prevent the particles from obeying the influence of their mutual attraction.

With regard to the fact that liquids in the spheroidal state never attain, by several degrees, the temperature at which, under ordinary conditions, they would boil, the reason of this appears to me to be clearly explainable as follows:—

Suppose heat to be applied to the dish represented in fig. 2, then the particles constituting the inferior surface of the liquid, being in contact with the dish, would abstract *from the dish* the heat which is taken up in the process of evaporation. But when the liquid is in the spheroidal state, as represented in fig. 1, then the particles constituting the inferior surface of the fluid being separated from the heated dish, abstract the heat required to convert them into vapour, not from the dish, as in the other case, but *from the rest of the liquid*, with which alone they are in contact. In other words, the cooling influence of evaporation is in the one case exercised upon *the dish*, and in the other case upon *the liquid*. Hence, therefore, a liquid in the spheroidal state never can attain the temperature at which it boils, because the more we increase the heating action of the dish, the more we also increase the cooling action of evaporation.

This explanation is at variance with that propounded by M. Boutigny, who accounts for the comparatively low temperature of the liquid spheroid, by ascribing to it a power of almost entirely reflecting heat from its inferior surface; but this hypothesis affords no explanation of the relationship which unquestionably exists between the boiling-point of the liquid and its maximum temperature in the spheroidal state. Besides, if reflexion of heat prevented increase of temperature, it would also prevent evaporation, which it clearly does not; for, when the dish is highly heated, the water evaporates with considerable rapidity, although its sensible heat remains constant at 7° below the boiling-point.

It is said, and I have no doubt of the fact, that when a liquid heated to the boiling-point is poured into a dish sufficiently hot to produce the spheroidal state, the temperature of the liquid immediately recedes to the point which it would have attained if the liquid had been placed in the dish without being previously heated. Now this effect is fully accounted for by the cooling action of evaporation operating in the manner I have described, but it is perfectly inexplicable on the hypothesis of reflexion of heat.

I repeated M. Boutigny's beautiful experiment of plunging a mass of red-hot platina into a glass of cold water, in which the metal remains incandescent for several seconds after the immersion, and is seen invested with an atmosphere of steam. In this experiment there is no ebullition of the water, notwithstanding that the steam has no outlet such as it has when the liquid is in the ordinary spheroidal state. The reason of this I rendered apparent by first heating the water to the boiling-point, and then immersing the red-hot platina, which in that case produced a rapid ebullition during the time that the metal remained incandescent and separated from the water by the usual atmosphere of steam. The absence of ebullition in M. Boutigny's experiment is obviously therefore simply owing to the steam being condensed by the coldness of the water, as soon as it recedes beyond the influence of the heated platina.

It is instructive also to observe the different effect produced by the partial and the total immersion of the red-hot platina in water heated to 205°, which it will be recollected is the maximum temperature that water acquires in the spheroidal state. If the heated metal be only partially immersed, there is no ebullition, and the temperature of the liquid remains stationary; but if the platina be wholly sunk in the water, then the temperature rises and ebullition takes place. The reason of this difference is very evident: when the platina is not entirely covered by the water, the steam it produces has an outlet, and the conditions resemble those of water in the spheroidal state; but when the platina is submerged, the heated steam is compelled to bubble up through the water, and thereby not only produces ebullition, but also imparts in its passage additional heat to the liquid.

I easily succeeded in showing M. Boutigny's remarkable experiment of freezing water in a red-hot platina capsule by the agency of sulphurous acid, which boils at a temperature of 18° below the freezing-point of water. This singular experiment is entirely dependent upon the peculiar mode of evaporation, which is incident to liquids in the spheroidal form. When the acid is poured into the red-hot vessel, its inferior surface is instantly vaporized, and the resulting stratum of interposing vapour prevents the liquid from touching the metal, and causes it to pass into the spheroidal form. In this condition the acid consumes by evaporation part of its own sensible heat, and its temperature in consequence suffers a further depression of about 5°. The acid is thus maintained at an extremely low temperature in the heated capsule, so that when water is also poured into the red-hot vessel, it is immediately congealed by the intense cold of the contiguous liquid.

M. Boutigny attempts to account for the explosion of steam-boilers by reference to the spheroidal condition of water. He supposes that when water is admitted into an over-heated boiler, it assumes the spheroidal state; and that afterwards, on the partial cooling of the boiler, it is brought into contact with the heated metal and causes so sudden a production of steam as to burst the boiler.

I feel, however, great difficulty in conceiving the possibility of water within a steam-boiler existing in the spheroidal condition. The coarse rough surface which the interior of the boiler presents to the water is extremely unfavourable to the spheroidal state; and there is every reason to believe that the separation could not exist in part of the boiler without existing throughout the whole. But be this as it may, the excessive heating of the boiler is the real cause of the mischief; and unless it can be shown that the spheroidal state of the water is *the cause* and not *the effect* of the boiler becoming red-hot, the explanation appears to me to amount to nothing.

M. Boutigny, with more reason I think, also conceives that the spheroidal condition of water has an important bearing upon the art of tempering steel. Whatever promotes contact between the heated steel and the water into which it is plunged, must accelerate the cooling and increase the hardness of the steel. Now it has been shown that the higher the boiling-point of the liquid, the higher is the temperature of the metal necessary to preserve separation; and hence probably the reason why artificers can harden steel more effectively in salt and water than in water alone, because the addition of salt raises the boiling-point of the water.

The remainder of M. Boutigny's experiments do not involve any other principles than those I have already touched upon, and I shall not, therefore, trouble your readers by any observations upon them.

Newcastle-upon-Tyne, W. G. ARMSTRONG.
August 9, 1845.

XXXVIII. *On Currents of Electricity produced by the Vibration of Wires and Metallic Rods.* *By* WILLIAM SULLIVAN, *Esq.*[*]

IN reading over a paper of Seebeck[†], in which he observed that when a compound bar of bismuth and antimony was heated at the junction of the two metals, a peculiar sound was heard to accompany the deflection of the galvanometer-needle, I thought at the time that if a similar sound could be produced by vibration, a current might in all probability be developed. Although I soon found that the sound so produced had nothing whatever to do with electricity, I did not give up the idea that electricity was more intimately connected with

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[*] Communicated by the Author.

[†] Ueber die Magnetische Polarisation der Metalle und Erze durch Temperatur—Differenz in Poggendorff's *Annal.*, vol. vi. p. 269.

molecular motion than was generally supposed. In order to test the accuracy of my views, I took a flat piece of board about two feet long and morticed two uprights, each three inches high, into it about eighteen inches apart. I then took a piece of No. 15 brass wire and soldered it to a similar piece of iron wire, and stretched it tight over the uprights so as to produce, when set in vibration, a musical note; the ends of this compound wire were then connected with an exceedingly delicate galvanometer, similar to that used by Melloni in his researches on radiant heat; when this was set in rapid vibration, a slight deviation of the needle took place, the direction of the current being from the iron to the brass, as in a similar thermo-electric circle; this was not, however, always the case, but seemed to depend upon the kind of vibration, though I could not discover how. That this effect did not depend on any heat produced is quite certain, as the current ceased the moment the vibration ceased. I could not always produce a deviation, and very often, when there was a current produced, it was scarcely appreciable until I increased the oscillation of the galvanometer-needle by frequently breaking contact. On one or two occasions I noticed a deviation equal to that produced by heating the junction of the two wires to dull redness. From some of my experiments, it would appear that when the wire vibrated continuously from end to end, the greatest effect was obtained; but when from some cause a nodal point was produced in the centre of the wire, and the two halves of the wire vibrated in opposite directions, no effect was produced. This point indeed was the cause of all my difficulties, as I found it almost impossible to detect when this took place. As the amount of electricity developed by such an arrangement as that just described must naturally be very small, I thought it would be well to increase the masses set in vibration: for this purpose I took a bar of antimony, five inches long, three-eighths of an inch wide, and a quarter of an inch thick, and soldered it to a similar bar of bismuth; to the ends of this compound bar copper wires were attached, which communicated with a galvanometer. When this was set in vibration, by striking it with a piece of iron, as a file, suspended from a piece of string, results similar to the former were obtained: at times considerable deviations were thus obtained, whilst very often no deviation whatever could be observed; the whole effect seeming to depend on the manner in which the vibrations are propagated along the wire. The results were on the whole much more satisfactory with this arrangement than with the former. I could also detect the nature of the vibrations occurring in such a bar much better than with wires in the beautiful manner pointed out by Strehlke*, that is, by covering small drops of water with lycopodium, and placing these globules on the bar, when the directions of the vibrations will be shown by the motion of the particles of lycopodium.

I could obtain no satisfactory results in my attempts to increase the deviation of the galvanometer-needle produced by a current flowing along a conducting wire by setting the latter vibrating, as was naturally to be expected from the nature of the current produced. In some cases the deviation was materially increased, whilst in others it appeared to be *very much diminished*. It is unnecessary for me to say, that whenever a current was observed to be produced in the vibrating wire, it acted on the magnetic needle as all conductors under similar circumstances. Indeed, there must be some intimate connection between magnetism and vibration, from the fact that if a bar of steel be struck with a hammer, so as to occasion a ringing sound, the bar will become magnetic; whilst if struck with anything soft, the effect so produced is very trifling; and also when a magnet is allowed to fall on any hard body which causes it to vibrate rapidly, it will be much more injured than if no such vibration be produced.

I do not think two metals necessary for the production of electricity by vibration; for when I tried the effects of vibration on a bar composed of crystalline hard iron and soft fibrous iron, I obtained results which certainly lead me to think that the arrangement of the particles has much more to do with the matter than their nature. Such an arrangement certainly gives decisive thermo-electric effects, as does also a wire composed of crystalline copper and fibrous copper. The subject however is not decided, as it would require much more delicate apparatus than I at present possess.

Just as I was about to publish these results, I read an account of the experiments of Ermann, which were communi cated to the last meeting of the British Association, from which he obtained some very beautiful results so closely connected with my own that I was induced to repeat all my experiments again; but my results did not differ from those which I obtained at first, although the galvanometer which I used on the second occasion was not very sensible, at least for such purposes. One of Ermann's results certainly coincides in a very remarkable manner with mine, namely, "that the tribo-thermical effect is instantaneous, and that the deviation vanishes quite as instantaneously as it commenced; the return of the needle to its primitive station being one of the most striking features of the phænomena." To these facts he adds, that the

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* Poggendorff's *Annal.*, vol. xl. p. 146.

results seem more likely to be effected by *a vibratory motion of molecules* than by a continuous effluence of caloric fluid. In fact, vibratory motion is the sole cause of the phænomena he observed, and not the heat produced by the friction.

There are a number of points of great importance in this inquiry which demand further investigation, but which at present I cannot follow out from want of proper apparatus; but I hope it will meet with the attention from men of science which it deserves; indeed it cannot be in better hands than in those of Ermann, who, I hope, will prosecute the subject with vigour.

The effect of vibratory motion on bodies has hitherto attracted little attention, strange to say, although it must produce some very curious effects on them. Its effects on chemical combination will, I am sure, very shortly yield some extremely beautiful results. I have been myself trying some experiments of this kind latterly, and shall, I hope, be able in a short time to communicate my results to this Journal. One experiment however deserves to be mentioned here. While in Giessen I saw the curious result obtained by Drs. Hofmann and Blyth, by heating styrole in a closed tube to a temperature of 200° centigrade, by which that substance was converted into a solid vitreous mass having exactly the same composition as the styrole, to which they gave the name of metastyrole. Some time after I thought I might be able to produce a similar effect, by subjecting a portion of it inclosed in a tube to rapid vibration for several days. For this purpose I prepared a considerable portion of styrole and inclosed it in a long narrow glass tube, and caused it to vibrate by the friction of a moist cloth attached to a little mechanism set in motion by a spring for thirty hours, when nearly the whole of the styrole become solid: when the solid mass was again distilled, liquid styrole was obtained. The tube containing the substance unfortunately broke just as I was about to repeat the experiment, but I hope to be able to repeat it in a short time.

Dublin, August 12, 1845.

XXXIX. *On Brown Iron Ore. By* Lieut.-Col. P. I. YORKE[*].

SOME observations having led me to believe that the analysis generally assigned in systems of mineralogy to the crystallized hydro-oxide of iron really belonged to another substance, I undertook the following experiments to clear up the point.

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[*] Communicated by the Chemical Society; having been read March 17, 1845.

When I made the experiments I had not seen the second edition of Dana's Mineralogy*, or the analyses of Kobell quoted therein, or I should probably have saved myself the labour. Although my results accord with those of Kobell, yet as it does not appear that any distinctly crystallized specimen has been analysed, and as the most interesting problem in mineralogy is the connection between crystalline form and composition, it may not be deemed superfluous if I submit my results to the Chemical Society.

A. *Crystallized Brown Iron Ore.*

The specimen was obtained from Restoomel mine near Lostwithiel. The specific gravity of the mineral, taken on a crystal weighing 2·504 grs., I found to be 4·37; when taken from the powdered mineral, in the manner described by G. Rose, it came out 4·22 to 4·24.

I annex figures of the forms of two crystals, and the measurements I have observed on them, together with those of similar angles obtained by Phillips and Levy. Levy makes the primary form a right rhombic prism of 95°·14, the ratio of whose terminal edges is to that of the lateral edges as 10 to 9†; and I have followed this author in the choice of the planes assumed as primary.

Fig. 1. agrees perfectly with the view that the primary form is a right rhombic prism; but with the crystal fig. 2. I met with a difficulty, inasmuch as I obtained as the inclination of g, on a plane apparently corresponding to h, fig. 1, the angles 115° 40′, and with g' on the other side 114° nearly. I believe this is owing to there being in reality two very narrow planes, i i', as I have represented them in fig. 2, meeting at a very obtuse angle, and to my catching the reflexion from one plane only. Could it be from a cause of this kind that G. Rose has referred this substance to his fifth system of crystallization?‡ I hope an experienced crystallographer may be induced to examine this crystal.

Composition.—Some preliminary experiments satisfied me that the substance consisted of red oxide of iron, oxide of manganese and water, with small but variable quantities of silica. I could not detect any phosphoric acid or alumina.

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* A System of Mineralogy, by James Dana, 2nd edit., New York and London, 1844.

† *Déscription d'une Collection de Minéraux formée par M. H. Heuland,* par A. Levy. Londres, 1837. vol. iii. p. 155.

‡ *Elémens de Crystallographie,* par G. Rose, traduit par V. Regnault, p. 261 (18).

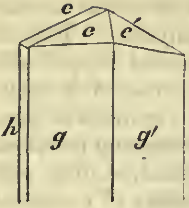

Fig. 1.

	W. Phillips[*].	P. I. Yorke.
g on *g'* . .	130° 40'	130° 27'.
g on *h* . .	117° 50'	114° 46'.
c on *h* . .	121° 45'	121° 34'.
g on *e* . .	129° 30'	129° 26'.

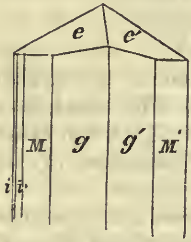

Fig. 2.

	Levy[†].	P. I. Yorke.
M on M' . .	95° 14'	96° 0'
g on *i'* . . .		115° 42'?
g on *i* . . .		114°?

The most brilliant and perfect planes are *g g'*.

Quantitative Analysis.

1. 100 parts lost by ignition 10, and left undissolved, when acted on by muriatic acid, 2·61 of silica.

2. 12·16 grs. of picked crystals free from quartz yielded—

		In 100 parts.
Silica	0·035	0·28
Water	1·225	10·07
Red oxide of iron . . .	10·89	89·55
Oxide of manganese . .	0·02	0·16
	12·17	100·06
Excess . .	0·01	

The silica obtained is probably an admixture only; abstracting the silica, the following gives the result of experiment compared with theory:—

	Atomic Weight.	In 100 parts. Theory.	Experiment.	
Fe_2O_3 =	80 =	89·89	89·81	89·97
			Mn_2O_3 0·16	
HO =	9 =	10·11		10·10

This formula (Fe_2O_3+HO) agrees with that obtained by Kobell from substances designated as nadeleisenerz, goethite and lepidokrokite[‡]: the first is a name given by Breithaupt to the crystals found in the neighbourhood of Bristol, which have been described by W. Phillips; the specimen examined

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[*] Crystals from the neighbourhood of Bristol.

[†] Crystals from Botallack, Cornwall.

[‡] *Annales des Mines*, 3d Ser. vol. xi. p. 493.

by Kobell was from Oberkirchen in Oldenbourg; it was crystallized, but the crystals were not measurable. This is undoubtedly the same species that I have just described. Under the second name is included a substance of the same composition apparently as the first, but from the observations of Rose it seems doubtful whether it has the same crystalline form*. It is under the name of goethite, however, that our mineral of Lostwithiel, &c. appears in Dana's work. I may now remark, that as the composition of this substance is perfectly analogous with that of manganite ($Mn_2 O_3 + HO$), so are the crystals plesiomorphous; the primary form of manganite being a right rhombic prism of 99° 40′, and the ratio of its terminal to its lateral edges is as 25 to 21 (Levy). The secondary planes also, which appear on the figures 1 and 2, are found on the crystals of manganite, as may be seen by examining the figures of that mineral in the works of Phillips and Levy.

Kobell comes to the conclusion that there are in nature two hydrates of the red oxide of iron, viz. $Fe_2 O_3 + HO$, and $2 Fe_2 O_3 + 3 HO$: the first very rare, the second common. My observations lead me to believe that the first is by no means uncommon in England, as appears by the following examination of brown iron ore from different localities.

α. Compact brown iron ore accompanied by very minute crystals from the Forest of Dean. Specific gravity obtained from a fragment 4·11. It contained 1 per cent. of silica, and deducting this lost 9·8 per cent. by ignition.

β. Fibrous brown iron ore, part of a stalactite, from the same locality as the last; sp. gr. 4·22: loss by ignition 9·41 in 100.

γ. Fibrous brown iron ore from Cornwall; sp. gr. 4·19; loss by ignition 10·24 in 100.

These specimens, then, all appear to belong to the same species, $Fe_2O_3 + HO$.

B.

I then examined a specimen, also from Cornwall, which was partly compact and breaking into smooth splintery fragments, and partly earthy. The sp. gr. taken on a fragment = 3·71, taken from the powdered mineral = 3·98. 8 grs. dissolved in muriatic acid and examined for alumina gave a barely perceptible precipitate of that substance; I could not detect any phosphoric acid, but obtained a trace of a white malleable metal, whose sulphuret was soluble in hydrosulphuret of ammonia, and which appeared to be tin. It lost 14 per cent. by ignition. The following was the result of analysis made on 10·99 grs.:—

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* According to Levy the forms are not incompatible.—Vol. iii. p. 161.

		In 100.
Silica	0·26	2·42
Water	1·57	14·28
Sesquioxide of iron	9·03	82·16
... of manganese	0·124	1·13
Trace of alumina, tin, loss	0·006	
	10·99	99·99

Deducting the silica and using the same atomic numbers as before, $2Fe_2O_3+3HO$ would contain in 100,—

	Theory.		Experiment.	
Peroxide of iron.	85·57		84·32	85·47
		Ox. mang.	1·15	
Water	14·43			14·53
	100·			100·

This, then, is the substance called limonite by Beudant, and the analysis of D'Aubuisson commonly attached to the crystallized mineral first described refers to this species.

C.

I have also examined a specimen of fibrous brown iron ore, which was ticketed "Brauner Glasskopf" from Wansiedel. This specimen was covered externally by a blackish, brilliant substance about one-tenth of an inch thick, which separated from the body of the specimen on hammering it, and broke off in small, brilliant, concretionary masses, which might at first sight be mistaken for crystals. The powder of this substance was of a purplish red. It lost 5·3 per cent. of its weight by ignition.

19·835 grs. of the body of the specimen which had a fibrous structure, separated from the external coating, yielded on analysis—

		In 100.
Silica	0·29	1·46
Water	2·46	12·42
Red oxide of iron	17·02	85·80
⅔ Oxide of manganese	0·26	1·30
Excess	0·20	
	19·835	100·98

This analysis agrees with the hypothesis that the substance is composed of single equivalents of the two mineral compounds just described, but it would be altogether premature from this single result to assume the existence of such a definite compound.

Indeed these experiments seem to me to lead rather to the notion that all the brown iron ores having a crystalline structure, including the fibrous varieties, belong to the species first described, and are composed of 1 atom of peroxide of iron and 1 atom of water.

D.

A solution of persulphate of iron was precipitated when boiling by a solution of carbonate of soda; the precipitate was washed with large quantities of hot distilled water and then dried at 212°. It was of an orange-brown colour, and had a specific gravity of 3·77.

To ascertain the degree of purity of this substance, 20 grs. were dissolved in muriatic acid, and chloride of barium added; this gave a precipitate which, when ignited, weighed ·3 gr. = ·1 sulphuric acid. 20 grs. were also dissolved in muriatic acid, and precipitated by caustic ammonia; the liquid filtered, evaporated to dryness, and the residue ignited, weighed ·298 gr., consisting of sulphate of soda mixed with the oxides of manganese and iron. From these experiments it would appear that the substance cannot contain more than about 1 per cent. of impurity derived from its mode of preparation. When ignited in a platina crucible it lost 14·05 per cent. by one experiment, and 14·15 by a second; it was therefore $2Fe_2O_3+3HO$.

A portion of this substance contained in a test-tube was placed along with a thermometer in a bath of fusible metal; it began to give off water at about 260°; it was kept at a little below 400° till no more water was given off; the remainder then lost by ignition 10 per cent.; it was therefore converted into Fe_2O_3+HO at that temperature. When the temperature of the bath was raised to 500°, or a little higher, more water was given off, and by weighing it appeared that nine-tenths of the quantity of water contained in the substance could be driven off at that temperature, but not the whole.

A portion of the native sesquihydrate (B) behaved much in the same manner, and appeared to be converted into Fe_2O_3+HO at a temperature between 380° and 400°.

The native protohydrate (A B) heated to 512° did not lose more than $\frac{3}{10}$ths per cent., this was probably hygrometric. It begins to give off its water however at a temperature considerably below red heat, which is visible by daylight.

XL. *Observations on the Action of Animal Charcoal.*

By Robert Warington, *Esq.*[*]

ABOUT twelve months since a friend requested that I would undertake the performance of an experiment for

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[*] Communicated by the Chemical Society; having been read March 17, 1845.

him, which, if successful, he considered might prove of importance. The idea had occurred to him in passing one of the large breweries, that, from the greatly increased demand for pale ale, such as is exported to India, as a beverage for home use, it would be practicable to discharge the colour from the ordinary beer by artificial means, and thus obtain the desired object. What rendered this, he said, the more important, was the difficulty he had heard expressed by persons interested in the matter, in preparing an article sufficiently pale for the purpose of sale, the malt employed requiring to be made expressly for this quality of beer. To effect this desideratum, he considered that all that would be required would be to pass the fluid through a medium of animal charcoal, and that the ordinary amber colour of ale would be thus partially removed. I undertook to make his experiment for him, at the same time telling him that I thought the beer would be rendered very flat and the flavour much injured, but certainly not anticipating what followed. The method adopted, as presenting the least objectionable points to the successful attainment of the desired object, was to pass the cold ale through a stratum of animal charcoal placed on a paper filter, and to repass the filtrate through the same medium until the required effect was obtained; the ale operated upon was high-coloured, and had a bitter taste and the fine aroma of the hop. The colour was rapidly removed, but the ale was found on tasting extremely vapid and flat, and the whole of the bitter flavour was found to have been also abstracted. Not being prepared for this result, I was surprised at the rapidity of the action, and resolved immediately to carry on the investigation by substituting other and stronger bitters in the place of the hop. For this purpose a quantity of the ale which had previously had the flavour of the hop removed by charcoal was prepared, one portion of which was boiled with bruised gentian root and another with the raspings of quassia wood, but on passing these decoctions when cold through the charcoal filter as before, the whole of the intense bitter flavour which they had imbibed rapidly disappeared.

It now occurred to me that this property of animal charcoal might be made of considerable utility to the chemist, as, from all our information up to the present time, it does not affect the active alkaline organic principles, and therefore should prove the means of separating the bitter of the hop and other materials from that of strychnia or morphia as contained in nux vomica or opium, it being frequently stated that some persons, vendors or makers of this common beverage, are in the habit of adding these and other materials in small quantities to their beer to give it a more intoxicating power. I therefore digested some nux vomica in coarse powder with a part of the ale prepared for the previous experiments, and on passing this when cold a great number of times through the charcoal the bitter flavour still remained, and hence I concluded at the time that my hypothesis was correct. Other engagements broke in on my experiments at this time, and prevented my prosecuting the investigation further.

About ten days since, however, I was recalled to this subject by some other experiments, and tried the effect of animal charcoal on the decoctions of some other astringent bitter substances, as oak bark, Peruvian bark (*Cinchona cordifolia*), and others, all of which had their flavour entirely removed. It may be as well to mention here, that in this second and also in a subsequent series of experiments the animal charcoal was added to the warm solution of the material under trial, and they were digested together on a sand-bath until the full effect had taken place, after which the fluid was passed through a filter. The charcoal used was the ordinary bone-black of the sugar-refiner purified by digestion with hydrochloric acid, washed as long as any saline matter was abstracted, and then dried and heated to low redness. I then tested the extent of this extraordinary power of charcoal by submitting a decoction of aloes to its action, with the same results, the whole of the bitter flavour being removed.

I had proceeded thus far when I was referred by my friend Mr. Henry to a work published at Breslau in 1842, by MM. A. Duflos and A. Hirsch, entitled *Ökonomische Chemie*, where, under the article Beer at page 65, I found nearly all that I had observed described in a few words; namely, that the bitter of the hop, and of all other vegetables used as substitutes for it, as gentian, wormwood, &c., and even aloes, was removed by animal charcoal, but that the bitter of nux vomica, which is often made use of in England, was not affected.

On looking over my experiments, however, my attention was arrested by finding that the decoction of the bark of the *Cinchona cordifolia* had had the whole of its bitter removed. Now, on the grounds stated in the previous part of this paper regarding the active principle, this should not have been the case. I therefore took a small quantity of disulphate of quina, dissolved it by heat in distilled water, and then added the charcoal, and discovered that the whole of the bitter was removed in less than a minute. A solution of acetate of morphia was also submitted to the action of charcoal with a like result. A watery solution of strychnia was similarly affected, and this made it necessary to repeat my former experiment on nux vomica, which had been made on a cold solution and simply filtering it through the charcoal; it was now digested with the aid of heat, and the whole of its bitter flavour quickly disappeared. The statement of Duflos and Hirsch, on this point, is not therefore entirely correct.

To try the extent to which this abstracting power proceeds, 2 grains of disulphate of quina were dissolved in 2 ounces of distilled water, and animal charcoal gradually added in small quantities to the warm solution until the whole of its bitter flavour was removed; it was found that 12 grains of the charcoal had been required to effect this.

It becomes a matter of surprise that this power of animal charcoal has, as far as I am aware, never been observed before, notwithstanding the frequent and extensive employment which is made of this agent in the preparation of the vegetable alkalies, their salts, and a great variety of other substances of an organic nature, for which it is directed to be used for the purposes of discoloration in all our chemical works and pharmacopœias. Numerous analyses of organic substances, in which it has been employed, will also be open to error on the same grounds.

Before leaving this branch of the subject, I should refer to a paper, and I believe the only one that bears particularly on any of the previous statements, by Dr. Louis Hopff, in the 17th volume of the *Journal de Pharmacie*, p. 172, "On the property of Charcoal in destroying the bitter flavour of many bodies;" in which he gives a series of comparative trials with wood and animal charcoal at a temperature between 78° and 86° Fahr. on solutions of a specified strength, of the extracts of bitter herbs, woods, roots, barks, leaves, flowers and fruits.

I annex a few examples from this paper of substances which have formed the subject of experiment in the present communication, as I believe that the great discrepancies which will be noticed arise from the charcoal employed by Dr. Hopff not having been well prepared.

	After twenty-four hours.	After forty-eight hours.	After boiling.
Gentian ...	Had lost a little of its bitter.	Purely bitter.	Had lost very little of its bitter.
Quassia ...	Perhaps lost a little of its bitter.	As before.	Lost very little of its bitter.
Aloes	Perhaps a little changed.	A little changed.	As before.
Nux vomica	Very little changed.	Little changed, still very bitter.	Sensibly less bitter.

In a note the author says that the bitter of nux vomica disappeared entirely on agitating its solution with a great excess of animal charcoal.

By analogical reasoning I was led to try the effect of this agent on salts of inorganic constituents, and here a similar action was exerted. A large quantity of sulphate of magnesia was removed from its solution in water, as also of chloride of barium, sulphate of soda and other salts. The fact of sulphate of lime being removed from its solution has I find been already noticed by Payen and Graham.

On laying the preceding experiments before the Society, I stated that I proposed to carry on the investigation, but I have since met with a paper by M. Chevallier[*], "On the action of Animal Charcoal on Metallic Salts," in which he also proposes investigating its action on the *organic salts*. Had I been aware of the existence of this paper at the time, I certainly should not have brought my own experiments forward, and shall not continue the investigation until M. Chevallier has closed his researches. I can only express my regret that I should even thus far have anticipated his results.

XLI. *On the Application of Photography to Registering the Thermometer and Barometer.* By Henry Hennessy[†].

THE delicacy required in an instrument for registering continuously the indications of the thermometer or barometer is such that contrivances of a merely mechanical nature could never fulfil such a purpose. As I am not aware that the photographic process has been ever applied to effect this desirable object, I here present some account of my attempts at showing the practicability of such an application of that process[‡]. It occurred to me a few months since, that the shadow of the mercury, or whatever fluid may be used to indicate by its oscillations physical changes going on in the atmosphere, could be received upon a sheet of prepared photographic paper or a Daguerreotype plate. In proceeding to make the necessary experiments, I found that the common mercurial thermometer was not adapted for my purpose. No method in my power could prevent the light from travelling as it were around the column of mercury, and thus preventing the formation of a distinct shadow. It appears that the best method for obviating such an inconvenience would be to have the sides of the thermometer tube made of some opake sub-

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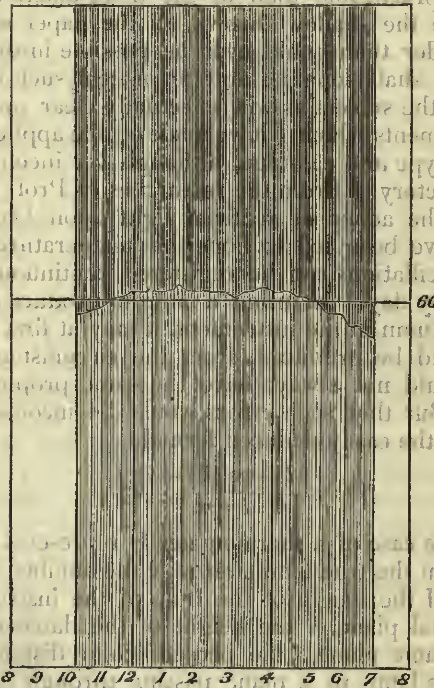
[*] *Comptes Rendus*, vol. xix. p. 1279. [†] Communicated by the Author.

[‡] [We are informed by Mr. Hunt, that many years since, Mr. Thomas Jordan tried a great many experiments on using photographic papers to register meteorological instruments, and published his results in the Reports of the Polytechnic Society.—Ed.]

stance, while the front and back should each consist of a thin film of glass equal in breadth to the bore of the tube. In the absence of such an instrument I used a spirit thermometer, the tube of which had a large bore and a small external diameter. This was inserted in a slit made in the side of a flat box, and the portions at each side of the spirit column were blackened. A sheet of photographic paper, prepared with bromide of potassium and nitrate of silver, was placed on a plate of glass which was drawn by clockwork through the box. The plate was at a distance of nearly ·1 of an inch from the column of spirit in the thermometer, and five feet from an open window. The window faced towards the east, but the direct rays of the sun did not fall on the apparatus during the experiment. As the plate passed before the thermometer, a beam of light was thrown upon it more or less high, according to the height of the spirit in the tube or the temperature of the surrounding air. A shadow bounded by a defined curve was thus formed, as is shown in the figure.



The imperfections of my apparatus having rendered the motion of the plate carrying the paper rather irregular, the shadow was not perfectly continuous; but, as can be perceived, a series of white spaces were interposed between each of the dark portions. The translucency of the coloured spirit also permitted some light to act upon the lower part of the sheet, making faint prolongations of the dark spaces.

In my first experiment the temperature was registered in this way for only five hours; in my second, the result of which is exhibited in the diagram, the operation was carried on for nine hours, or from ten in the morning until seven in the afternoon. During the course of the last experiment, the sky was occasionally much obscured so as to lessen the intensity of the light, and both experiments were performed during the present month.

When results so satisfactory have been obtained with such imperfect apparatus as that which I have used, it is superfluous to anticipate what would take place under more favourable circumstances. The application of photography to register the height of the mercury in the barometer ought to be still more practicable than in the thermometer, as in the former case the shadow thrown on the paper would be so much broader than could ever be possible in the latter. I trust that I shall soon be able to present such facts on this portion of the subject as will effectually clear up all doubts. The experiments which I have made on the application of the Daguerreotype and calotype, although yet incomplete, have been satisfactory. From the researches of Prof. Draper and others on the action of artificial light upon Daguerreotype plates, I have been led to hope that temperatures and barometrical oscillations can be registered continuously at night as well as by day. In using a lamp to produce a shadow of the fluid column in the instrument, it may at first appear that errors would be produced, from the circumstance that the shadow would not always have the same proportion to the column. But that such errors would be inconsiderable appears from the easily deduced formula

$$\Theta = \frac{\theta l - m(\mathrm{H} - h)}{l + m};$$

where in the case of a thermometer Θ represents the number of degrees in the true temperature, θ the number recorded on the plate, H the height of the bulb of the instrument above an horizontal plane, h the height of the flame of the lamp above the same plane, l the perpendicular distance from the flame of the lamp to a plane passing through the mercurial column parallel to the plate, and m the distance from the mercurial column to the plate. A similar formula would give the true height of the mercury in the case of a barometer. If circumstances should render the correction obtained by the foregoing formula of any importance, tables could be easily calculated to facilitate its practical application.

Dublin, August 14, 1845.

XLII. *Contributions to Actino-Chemistry.—The Chemical Changes produced by the Solar Rays on some Photographic Preparations examined.* By ROBERT HUNT, *Keeper of Mining Records, Museum of Œconomic Geology**.

1. BEING desirous of ascertaining, with more correctness than has hitherto been done, the nature of the changes produced by solar radiations upon the various preparations used for photographic purposes, I instituted, a short time since, a series of experiments with this object in view. Commencing with the salts of silver most commonly used in this new and beautiful art, it is my intention to proceed with all organic as well as inorganic bodies, which the researches of late years have shown us undergo a chemical change by exposure to sunshine. This inquiry must necessarily extend itself over a considerable period, but by confining myself strictly to the examination of one compound at a time, I hope to be enabled gradually to place the entire subject in a more satisfactory light than it is at present. The results I have already obtained are in the highest degree satisfactory; and as they have reference particularly and solely to the oxides, nitrate and chloride of silver, I see no good reason for withholding their publication. As the examination of the other preparations is completed, I shall, from time to time, forward my results to the editors of the Philosophical Magazine.

2. During the past five or six years, the attention of some of the most eminent philosophers of Europe has been turned to the subject of photography. The result has been the discovery of a great number of most interesting processes; and in some few cases the chemistry of the changes produced by ACTINIC power has been examined and explained. This has, however, so rarely been the case, that I shall offer no apology for proceeding anew over the entire subject; and I hope in every instance, where I do not acknowledge the previous labours of other inquirers, that the omission will be set down to its true cause—my ignorance of those labours—and not attributed to any desire on my part to arrogate to myself the merit of any discovery which is fairly due to another.

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* Communicated by the Author.

Oxides of Silver.

3. To a weak solution of nitrate of silver in distilled water a very dilute solution of pure baryta was added, and the resulting precipitated oxide of silver received upon plates of glass. When the whole of the oxide had fallen down, the fluid was removed very slowly by means of a small glass siphon, so that the powder on the glass plates might not be disturbed. Without being removed from the vessel in which the precipitation was effected, the oxide was dried at a very moderate heat, and there resulted exceedingly thin films of the oxide of silver on the glass, quite free from all organic matter, every precaution being taken to prevent its presence.

4. These plates were exposed to full sunshine for periods varying from one to twenty-four hours, one-half of them being covered with an opake body. The brown oxide of silver (protoxide) was gradually darkened, so that after half an hour's exposure, the exposed part was a much deeper brown than that which had been covered. This darkening process was continued until a perfect black was produced, after which, by the prolonged action of sunshine, a very remarkable whitening of the oxide takes place, and proceeds slowly until it is of a very fine olive colour.

5. On immersing one of these plates in strong ammonia, all the oxide which had been kept from the solar action was dissolved off, whereas the exposed part did not appear to be acted on. Another plate, immersed in a solution of twenty drops of nitric acid in 300 drops of water, was acted on in precisely the same manner; the oxide being entirely removed from the unexposed portion of the glass plate, whilst the changed, or actinized part, appeared to remain untouched. From this we might consequently infer that the oxide of silver had been reduced to the metallic state. Moderately strong nitric acid, dissolving the whole from the glass, seemed to render this in every way probable.

6. Metallic silver, in however fine a state of division it may be, conducts electricity; consequently it was expected that the changed oxide would conduct the current of a voltaic battery. One of the glass plates covered with oxide was made part of the circuit, between a single pair and a galvanometer of great delicacy, but not the slightest indication of any conducting power could be detected.

7. Films of oxide of silver were darkened all over by long exposure to sunshine. One was placed in ammonia and another in very dilute nitric acid. It was now discovered that the ammonia did dissolve a portion of the actinized oxide; it

became of a pale brown colour, and was found to hold silver in solution. The dilute nitric acid also dissolved off some oxide from the plate, as was shown by its becoming milky on the addition of muriatic acid. On adding some potash to the ammonia, and dissipating the ammonia by heat, a black precipitate was formed. The precipitate from nitric acid by potash was dark brown. The general characters of this oxide were the same as those of the suboxide of silver examined by M. Wöhler.

8. After having removed as much as possible from the glass plate by solution in ammonia, there was still a film upon the surface of the glass, which upon drying became nearly black, appearing of a fine olive-green colour when looked through. It was now ascertained that this film conducted an electric current, and on pouring over the plates nitric acid diluted with an equal quantity of water, the films were dissolved off with the formation of nitrous acid fumes.

9. These results prove to us that the influence of the chemical rays (actinism) on the protoxide of silver, converts it into a suboxide and into metal. I have not been able by the longest exposure to sunshine to reduce all the suboxide to the state of metal, unless organic matter was present.

10. Pure protoxide of silver being spread whilst moist upon paper, by means of a camel-hair pencil, and carefully dried, passes in the course of a few hours, if the sun shines strongly, into a perfect black, the whole of which dissolves off in ammonia. This salt precipitated by potash, the ammonia being dissipated by heat, gave upon reduction the exact formula of Wöhler's oxide ($Ag2O$).

11. By continuing the exposure of these papers for a few days, their colours were changed from black to an olive. Ammonia dissolved nothing, and diluted nitric acid was equally inactive. In moderately strong nitric acid, however, the paper was almost immediately rendered white with the formation of nitrous acid fumes. Any analytical results from preparations on paper are so liable to error, from the inconstant character of the composition of the paper itself, that they are scarcely to be depended upon. But by very carefully drying two weighed pieces of paper until they lost no further weight, and even heating them up to a point but a little below that at which the paper would scorch, placing them both in warm tubes and hermetically sealing them, exposing one to the actinic influence whilst the other was preserved in the dark, opening the tubes under the same circumstances, burning the papers at the same temperature, and weighing the ashes against each other, I succeeded in reducing the sources of

error considerably; and the result was, in several experiments, that the actinized oxide lost in weight exactly (or *very nearly*) the weight of the oxygen of the oxide employed.

12. Several trials were made to ascertain if the films of oxide of silver on the glass plates lost weight upon darkening; but, although some evidence was obtained that they did so, it must be admitted that it was not at all satisfactorily proved to be the case by this means. The films seldom exceeded a grain in weight, and the pieces of glass upon which they were spread generally weighed 200 grains; it will therefore be easily conceived how difficult a thing it was after an exposure of some hours to sunshine, and under all the varying conditions of temperature and atmospheric moisture, to decide with anything like accuracy the loss by weight of oxygen of the oxide employed; the more so as evidence was obtained to show that the finely divided metal had the property of condensing water within its pores with some rapidity. I cannot refrain from venturing to express my opinion, that the silver reduced by actinic power is different in many respects from silver brought to an equally fine state of division by mechanical means.

13. The oxide of silver, as is well known, is soluble in ammonia; by exposing such a solution to the action of the atmosphere, a pellicle forms upon its surface, which Faraday considers a peculiar oxide. If this solution is spread over paper and dried, I believe a similar oxide of silver is formed. This paper darkens by exposure to light with some rapidity, as does also the film formed on the surface of the ammonia. After darkening, it will be found that the darkened parts are no longer soluble in ammonia, or in diluted nitric acid, but that moderately strong nitric acid dissolves it with the formation of nitrous acid vapours. In this case, consequently, we may conclude that the oxide is reduced to the metallic state at once by the action of the sun's rays.

14. Perfectly pure silver, as is well known, does not combine with oxygen by exposure to the air; but silver reduced as above by solar action, becomes oxidized by exposure, and after a short time ammonia will dissolve off a considerable portion.

Nitrate of Silver.

15. Nitrate of silver does not change by the action of light, unless organic matter is present, for which it is so delicate a test, that Dr. John Davy recommended it as the most useful for ascertaining the presence of minute quantities in a solution. It has been stated that the "nitric acid appears to be the agent which destroys the animal texture, and the black stain

is owing to the separation of oxide of silver*;" and this opinion has been very generally entertained. I am satisfied, however, that something more than this takes place, and that an actual combination of the organic body with the silver ensues; and I hope to have an opportunity of examining some of the very complicated phænomena which have presented themselves to my notice, during investigations which have been made incidentally to the present inquiry, into the influence of organic matter upon the salts of silver and gold, during exposure to light and heat.

16. Count Rumford appears to have been the first who noticed that carbon had the property of reducing the salts of gold and silver from their solutions, at a temperature below that of boiling water in *the dark*; and that the same precipitate of pure metal took place by exposure to the sun's rays. He inferred from this, that the "chemical properties attributed to light" were due alone to the heating powers of the sun's rays. The error of this has long been known, as experiments with the prismatic spectrum have shown that the rays in which the calorific power exists at its maximum have little effect in producing any such reduction. The precipitation of the silver from the nitrate in solution takes place very speedily under the influence of the solar rays which have been deprived of their heat by being passed through plates of alum or rock-salt, and when the temperature of the solution is kept at 32° F.

17. Nitrate of silver dissolved in distilled water was spread on paper; it was allowed to darken to a chocolate-brown: ammonia now dissolved off the darkened surface, proving it to be in the condition of an oxide. The exposure being continued for some hours during very bright sunshine, a surface of a peculiar russet colour, and of a dirty appearance, resulted. Ammonia did not now possess the power of altering the colour; it removed some oxide from the paper, which arose from undecomposed nitrate of silver, protected by the superficial film from the action of the rays of the sun. But if the darkened paper was previously soaked in distilled water, until all the free nitrate was dissolved out of it, neither ammonia nor dilute nitric acid separated any oxide, clearly showing that the darkened surface was metallic silver.

18. Some nitrate of silver in distilled water was boiled with a grain or two of animal gelatine for some hours in the dark; the solution became coloured a fine brown, but even after standing for many hours no precipitation took place. Potash precipitated the oxide of a black colour, which ammonia dis-

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* Turner's Elements of Chemistry.

solved. The solution being of a brown colour, I am led to infer that this is a suboxide of silver (7.).

19. A similar solution of nitrate of silver and gelatine was put into a stoppered bottle and exposed to sunshine; it speedily darkened, and in about two hours the solution was so opake that the sun could scarcely be seen through it. Its colour was a grayish-green, which it retained during an exposure to very bright sunshine for five days. It was expected that an increase in the acid reaction of the solution might be detected during the progress of the precipitation. Such can hardly be said to have been the case: the solution was repeatedly tested with litmus paper, but the change of colour was very nearly as decided at first as after the entire length of the exposure. Does the nitric acid enter into combination with the organic compound, or is it itself decomposed by the combined action of the solar rays and the organic matter?

20. It was with great difficulty that the precipitated matter could be separated from the solution; this was at length effected by subsidence, and it was found to be entirely soluble in ammonia, giving a reddish-brown solution. The bottle in which the solution had been placed was coated with a thin film of an olive-coloured matter, transmitting green light, upon which neither ammonia nor very diluted nitric acid acted. By nitric acid diluted with an equal quantity of water it was very speedily dissolved, the bottle becoming filled with nitrous acid fumes. It is apparent from this experiment, that the first action of the light is to liberate the protoxide of silver from its combined acid, then to set free another proportional of oxygen, and that eventually the suboxide is entirely reduced to metallic silver. The nitric acid and the organic compound are evidently thrown into some new conditions by the agency of the sun's rays, the examination of which I reserve for the present.

Chloride of Silver.

21. Perfectly pure chloride of silver was prepared by dissolving crystallized nitrate of silver in distilled water which had been boiled with a little nitric acid, to ensure the absence of any organic matter: it was then precipitated by pure muriatic acid, well-washed with distilled water, separated by subsidence, and dried on glass in a sand bath. In diffused light this chloride changes colour very slowly, and eventually assumes only a light lead colour. In bright sunshine it darkens much more rapidly, and becomes brown by about one hour's exposure.

22. Under the influence of the prismatic spectrum, two very

decided colours are produced on the pure chloride of silver,—a blue or fine rich lead colour, by the space between the most refrangible green, and the extreme edge of the violet rays, beyond which for some distance the same colour is produced, but gradually lowered, until it becomes a mere tint of a lilac hue, at a distance beyond the visible spectrum, equal to about one-third its length. Now, supposing we divide the visible group of rays into 100 parts, this tinting goes on over a space equal in extent to thirty-four of such parts. Over the space covered by the ordinary and the extreme red rays, a very decided red impression is slowly made. By combining the influence of the red and the blue rays, the chloride with tolerable quickness becomes of a fine brown colour. I find it would be quite practicable to arrange a scale of colours with which the actinized chloride of silver might be compared, and thus enable us to ascertain with tolerable correctness the relative quantities of heat and chemical power existing in the solar rays at any time. By means of an arrangement similar to that used by Mr. Shaw and Dr. Percy, by which the chloride is restored, after darkening, to its original condition by the action of chlorine, a very accurate and interesting series of observations might be made*.

23. As it appeared probable that artificial heat would produce similar effects to those described as the result of exposing chloride of silver to the thermic rays, many experiments were made with this view. In no instance however would artificial heat directly applied or reflected from a metallic surface produce any change in the colour of the chloride of silver, short of the point at which it is converted into horn silver, and then the colour is but very slightly altered; and even if the heat is incautiously allowed to become too great, it only passes into a lead colour, this change arising from the reduction of some portions of the chloride to the metallic state. From this we may infer that the solar and terrestrial heat have properties distinct from each other, or perhaps that solar heat has, of itself, in its ordinary state, no particular power in producing a change in the chemical condition of bodies, but that it modifies the action of actinic force, in a similar manner to which light has been shown to do so (see the published experiments of Herschel†, Becquerel‡, Draper and the author§).

24. Mr. Fox Talbot, and, following him, most other inquirers, have stated that in the preparation of the ordinary chloridated photographic papers, it was necessary, to ensure sensibility to solar action, that the chloride of silver should have

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* Philosophical Magazine, vol. xxv. p. 450.

† Philosophical Transactions, 1841. ‡ *Annalès de Chimie*, 1843.

§ Philosophical Magazine for 1840, 1842, 1843, 1844 and 1845.

some free nitrate of silver in mixture with it. It was important to ascertain, if possible, the cause of this. Three pieces of glass covered with films of chloride of silver, by subsidence, in the manner recommended by Sir John Herschel, were selected; one was exposed quite dry, another moistened with distilled water, and the third with a solution of nitrate of silver. The action was most rapid on that wetted with the nitrated solution, the next in order was the moist one. In an hour they were however darkened to as nearly as possible the same shade of colour.

25. The glass plate over which the wash of nitrate of silver had been applied, was soaked in distilled water to remove any of that salt that might remain undecomposed. The three pieces of glass with their darkened films were then boiled separately in nitric acid diluted with double its quantity of water. The colours of the plates were but very slightly changed. The liquids were then examined for silver, and in that alone in which the plate on which the nitrate had been spread had been, was any detected. It was evident that the nitric acid had either dissolved off oxide of silver, or oxidized metallic silver and then dissolved it. The films on the other plates were not changed in colour by the application of either ammonia or hyposulphite of soda.

26. Three similar plates of glass covered with films of chloride of silver were placed in jars of pure hydrogen and nitrogen gases and in atmospheric air, and then exposed to bright diffused daylight. The object of this was to ascertain if the nitrogen of the acid, or the hydrogen of the water, were likely to assist the change in any way. The chloride in the nitrogen and the atmospheric air darkened equally in equal times. The first action of the hydrogen on the chloride of silver was to reduce it to the state of metal over the surface. But as soon as the chloride began to darken, this bright metallic film was removed, and the whole darkened equally, but to a deeper tint than either of the others. These plates, as in the former case, were boiled in diluted acid, from which experiment I ascertained that the hydrogen plate alone gave any evidence of the presence of oxide of silver or revived metal.

27. It may appear that this metal was produced by the hydrogen independent of the actinic power, but I find when chloride of silver is plunged into hydrogen gas in bright sunshine, that no reduction of the chloride due directly to the hydrogen takes place, yet most distinct evidence of the presence of metallic silver after darkening has been obtained. We thus learn that the advantage of the nitrate of silver over the chloride is, that it is more readily reduced to the metallic

state (17.) than the chloride; and that the well-known accelerating power of moisture is due to the decomposition of water, and the influence of the nascent hydrogen, in reducing the argentine salt.

28. The principal point was now to ascertain the condition of the chloride of silver after it had been thoroughly darkened by exposure. Many experiments were tried, in various ways, which need not now be detailed, as they were in varying degrees successful; to one alone which gave very satisfactory results I shall at present confine myself. Pure chloride of silver was prepared with great care, as before stated (21.), well-washed with boiling distilled water, until neither nitrate of silver nor muriatic acid produced any precipitate, and then dried. Five grains of the salt were put into a long test-tube full of distilled water, and placed in the sunshine to darken, the powder being frequently moved that every part might be acted upon by the sun's rays. It was found, even after an exposure of a few minutes, that the water contained chlorine; it became opake on the addition of nitrate of silver, and this very gradually increased as the chloride darkened. The darkening was continued for several hours, after which the solution was filtered to free it from chloride of silver, and nitrate of silver added to the filtered fluid; chloride of silver was precipitated, which, when collected and dried, weighed 1·4 grain on one occasion, 1 grain on another, and 1·5 grain on a third trial. It is very difficult to ensure the darkening of every portion of the chloride of silver, it adheres in little masses; these are darkened on their outer surface, and the interior is then protected from the solar influence; this was overcome as much as possible by repeatedly breaking down those small particles with a glass rod. On drying the darkened chloride and weighing, it was found that the loss of weight was scarcely a quarter of a grain. From this it is evident that the chlorine liberated by the sun's rays must have been partly replaced by some other body; and it becomes highly probable that the darkened chloride is in the state of an oxychloride. No oxide of silver could be detected by digestion in diluted nitric acid.

29. The exposure in the water was in another case continued for many days, but no greater degree of darkening occurred; but a curious fact was now noticed. It was found that during the night nearly all the chlorine which had been liberated during the day was recombined, and that the darkened powder became much lighter. A similar effect to this, but a more striking one, has been noticed by Dr. Percy and Mr. Shaw, when chloride of silver is darkened by light in an at-

mosphere of chlorine. Some other salts of silver exhibit this curious phænomenon in a striking manner, as will be shown in future communications.

30. Pure chloride of silver was dried on a glass plate and heated nearly to the point of fusion; it was then placed in an atmosphere of chlorine and darkened in very bright sunshine. No loss of weight could be detected by the most careful manipulation and with a very delicate balance. There is no very satisfactory way of explaining this, except we suppose that the liberated chlorine is retained by cohesive force in connection with the finely divided particles of what we must regard as a subchloride of silver, unless we return to the old theory of the compound nature of chlorine.

31. In these experiments the presence of organic matter has been carefully avoided. It now became necessary to inquire into the condition of the chloride of silver darkening by the solar rays on paper. Bath post paper, highly glazed, was covered with chloride of silver in the usual way, all free nitrate of silver being washed off. The prepared paper was then exposed to light for forty-eight hours, in which time it darkened and became of a fine olive-brown. The paper was now cut in pieces; some parts were immersed in very dilute nitric acid, which produced no change; others in ammonia, which had not the slightest effect upon them; therefore it was evident that no oxide of silver was present. On putting fragments of the paper into nitric acid diluted with equal parts of water, all the darkened portion was rapidly dissolved off, and the paper was left of a lilac colour. Hence we have very satisfactory proof that metallic silver is eventually formed on the surface of the chloridated photographic papers, and that the under sensitive surface is preserved in the condition of a subchloride of silver by the opacity of the superficial coat.

32. From other experiments which do not properly belong to this paper, I am inclined to believe that the first action of the solar rays (I avoid using the term light, considering these phænomena as the result of a modified principle) is to liberate one-half of the combined chlorine, which is very readily, moisture being present, replaced by oxygen. By the continued action of the exciting cause, the other proportional of the combined gaseous element is in like manner set free and replaced. We have seen (3, 4, 5, &c.) that oxide of silver is in a short time decomposed under Actinic power, and hence we have eventually, nearly pure metallic silver in a state of extremely fine division. It will be seen that organic matter plays an equally important part with the chloride of silver as it does with the nitrate of that metal when under the influence

of that mysterious power which appears to emanate from the solar orb.

33. Admitting the imperfect character of the results which I have endeavoured to record as faithfully as possible, I cannot but regard them as important, and they appear to point to some general law, which is probably applicable to those phænomena which Moser has described, and which are in all probability acting a grand part in the mutations of the universe. The same power which we detect changing the film of chloride of silver on the photographic paper, has been in action for countless ages on the earth's surface; and by pursuing with due care the investigation, we may be enabled to proceed, step by step, into the great laboratory of nature, and discover the various causes which have been in operation on the consolidated masses of this globe, and which are producing multifarious chemical changes, to the excitation of which are due the great magnetic phænomena which are exciting so much the attention of philosophers.

September 11, 1845.

XLIII. *Proof of* Fermat's *Undemonstrated Theorem, that $x^n + y^n = z^n$ is only possible in whole numbers when $n = 1$ or 2. By* S. M. Drach, *Esq., F.R.A.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

We may evidently regard x, y and z as mutually prime, for a divisor common to two must enter the third, if an integer solution be possible, ∴ two are odd and one even.

Case 1. $n = 2m + 1$.

By the rules of this analysis (Euler's Algebra, pt. 2. ch. xii.), it appears that if

$$Z = a^m p^n + \Sigma \left(\frac{n.(n-1)\,..\,n-2i+1}{1.2\,..\,2i} = A_i \right) p^{n-2i} q^{2i} a^{m-i} c^i,$$

and

$$Y = c^m q^m + \Sigma\, A_i\, q^{n-2i} p^{2i} c^{m-i} a^i,$$

that

$$a Z^2 - c Y^2 = (a p^2 - c q^2)^n.$$

Put therefore $a = z$, $Z = z^m$, $c = y$, $Y = y^m \therefore x = zp^2 - cq^2$,

and $$z^m = z^m p^m + \Sigma\, A_i\, p^{n-2i} q^{2i} z^{m-i} y^i, \text{ or}$$

$$1 = p^n \left(1 + \,..\, \Sigma\, A_i \left(\frac{q^2 y}{p^2 z} \right)^i \right);$$

similarly, the equation in Y gives

$$1 = q^n \left(1 + \Sigma A_i \left(\frac{p^2 z}{q^2 y}\right)^i\right).$$

If $n = 1$, $p = 1 = q$; if $n > 1$, $A_i > 1$, and as x is positive, $p^2 z > q^2 y$; therefore p^n is multiplied by a series of positive terms $= \Sigma A_i \times$ a quantity less than unity; whilst the factor of q^n is similar, but $= \Sigma A_i \times$ quantity > 1, $\therefore p^n > q^n$, in order that the respective products shall $= 1$.

Combining the last found equations, we obtain by the binomial expansion,

$$2 z^{\frac{n}{2}} = (p\sqrt{z} + q\sqrt{y})^n + (p\sqrt{z} - q\sqrt{y})^n,$$

$$2 y^{\frac{n}{2}} = (p\sqrt{z} + q\sqrt{y})^n - (p\sqrt{z} - q\sqrt{y})^n;$$

whence

$$\frac{p\sqrt{z}}{q\sqrt{y}}\left(\overline{z^{\frac{n}{2}} + y^{\frac{n}{2}}}\Big|^{\frac{1}{n}} - \overline{z^{\frac{n}{2}} - y^{\frac{n}{2}}}\Big|^{\frac{1}{n}}\right) = \overline{z^{\frac{n}{2}} + y^{\frac{n}{2}}}\Big|^{\frac{1}{n}} + \overline{z^{\frac{n}{2}} + y^{\frac{n}{2}}}\Big|^{\frac{1}{n}};$$

the difference of these quantities is therefore zero. But putting the coefficient of the ith term in the binomial development $= B_i$, so that

$$B_i = \left(\frac{1}{n}\cdot\frac{1-n}{2n}\cdot\frac{1-2n}{3n}\cdot\frac{1+4n-2in}{2in-3n}\cdot\frac{1+3n-2in}{2in-2n} = B_{i-1}\right)$$

$$\times \frac{1-\overline{2i-2}.n}{2in-n}\cdot\frac{1-\overline{2i-1}.n}{2in},$$

the difference is expressible by

$$0 = \Sigma\left(1 - \frac{p}{q}\left(\frac{y}{z}\right)\frac{n-1}{2}\cdot\frac{1-2in}{2in+n}\right) B_i \left(\frac{y}{z}\right)^{in}.$$

When $n=1$, $p=q$, $n-1=0$, the equation is satisfied; when n exceeds unity, we have $1 - 2in =$ a negative quantity, and consequently the factor within ( ) is positive, as is also $(y \div z)^{in}$; but then $B_i = B_{i-1} \times$ the product of two *negative* fractions, and is consequently of the same sign as B_{i-1} or B_{i-2} ... or B_1, but B_1 is negative, $\therefore 0 =$ a *series of exclusively negative quantities*, which is impossible, unless a factor common to all $= 0$, which is here $1 - n$, as the case of $y = 0$ is of course excepted. Consequently $n = 1$ is the only *odd* possible value of this exponent.

Case 2. $n = 2m$, or $x^{2m} = z^{2m} - y^{2m}$.

As the sum of two squares is never of the form $4i + 2$ or $4i + 3$, it follows that x must be even and y odd; also z is odd. Moreover, m must be odd, as we know that the sum of two biquadrates cannot be a square, much less a biquadrate (unless in the excepted case of $y = 0$). Put therefore by the ordinary rule, $x = 2pq$, $z = p^2 + q^2$, $y = p^2 - q^2$. Hence p and q are prime to each other, and

$$\frac{p^2}{q^2} = \frac{z^m + y^m}{z^m - y^m} = 1 + \frac{2y^m}{z^m - y^m}$$

$$= \frac{z+y}{z-y} \cdot \frac{z^{m-1} - yz^{m-2} + \dots y^{m-1} \text{ (m odd terms)}}{z^{m-1} + yz^{m-2} + \dots y^{m-1} \text{ (m odd terms)}}.$$

The second fraction is not divisible by $(z - y) \div (z + y)$ for the remains $y^{m-1} \div (z \mp y)$, which is not an integer. Likewise

$$\frac{p^2}{q^2} = \left(\frac{z+y}{z-y}\right)^2 \cdot \frac{z^{m-2} - 2yz^{m-3} + \dots + \overline{m-1}\,y^{m-2} + (my^{m-1} \div \overline{z+y})}{z^{m-2} + 2yz^{m-3} + \dots + \overline{m-1}\,y^{m-2} + (\overline{2-m}\,.\,y^{m-1} \div \overline{z-y})};$$

and as $z \pm y$ is even whilst m and $m-2$ are odd, there would still remain a fractional $\frac{1}{2}$. Further, if $z \mp y = 4i$, $z \pm y$ is of the form $4j + 2$, so that one term will have $\frac{1}{4}$ and the other $\frac{2}{4}$ for a remainder.

Hence, as $p^2 q^{-2}$ = a quantity $\div$ by $(z+y) \div (z-y)$ to the first power only, it follows that $(z+y) \div (z-y)$, or their product $z^2 - y^2$, must be a square. Putting therefore $z = r^2 + s^2$, $y = r^2 - s^2$,

$$\therefore \left(\frac{z}{y}\right)^m = \left(\frac{r^2+s^2}{r^2-s^2}\right)^m = \frac{p^2+q^2}{p^2-q^2}.$$

But p, q are real and prime to each other, so are r and s, $\therefore$ as we cannot suppose $p^2 - q^2 = (r^2 + s^2)^{-m}$ without introducing the imaginary sign of $\sqrt{-1}$ in the factorial result, $p^2 - q^2 = (r^2 - s^2)^m$, $\therefore p + q = (r \pm s)^m$, and $p - q = (r \mp s)^m$. Hence

$$p^2 + q^2 = \frac{1}{2}(r+s)^{2m} + \frac{1}{2}(r-s)^{2m} = (r^2 + s^2)^m,$$

or

$$r^{2m} + 2m\,.\,\frac{2m-1}{2}\,r^{2m-2}s^2$$

$$+ \frac{2m\,.\,2m-1\,..\,2m-2i+1}{1\,.\,2\,..\,2i}\,r^{2m-2i}s^{2i}\,..\,+ s^{2m}$$

$$= r^{2m} + m\,.\,r^{2m-2}s^2 + \frac{m\,.\,m-1\,..\,m-i+1}{1\,.\,2\,..\,i}\,r^{2m-2i}s^{2i}\,..\,+ s^{2m}.$$

Setting out from either end, the extreme quantities are equal, and have the same coefficient, *unity*; but the upper coefficient afterwards more and more exceeds the corresponding under one, for at the ith term they equal their immediate predecessors multiplied by

$$\frac{2m-2i+2}{2i-1}\cdot\frac{2m-2i+1}{2i}=\frac{m-i+1}{i}\left(\frac{2m}{2i-1}-1\right)$$

and $\frac{m-i+1}{i}$.

Now m equals or exceeds $2i-1$, $\therefore$ the former fraction is to the latter always in the proportion of $1+\alpha:1$, α being positive; consequently the upper coefficient is continually greater than the under one, as far as the mid-term, when the inverse march begins, but the upper one still retaining its greater magnitude. Hence the difference, which ought to be zero, is the sum of *positive multiples of real squares*, a series which cannot $=0$, unless a common factor in the coefficient vanishes; the one for $i=1$ is $2m(2m-1)=2m$, whence $m=0$ or 1, and as $m(m-1)$ enters all the terms, these are the only possible values (the case of r or $s=0$ is of course excepted). But m cannot $=0$, for then $x^0+y^0=z^0$, $\therefore m=1$ or $n=2$ is the only possible *even* value of the exponent.

Hence I consider Fermat's proposition to be correct.

London, August 18, 1845. S. M. DRACH.

[In publishing this asserted proof of Fermat's theorem, we are actuated by a hope that, even should it be found unsatisfactory, the method may contain useful suggestions.—ED. PHIL. MAG.]

XLIV. *On an Œconomical Method of procuring Phosphate of Lime and Magnesia from Urine for Agricultural Purposes.* *By* JOHN STENHOUSE, *Ph.D.**

THERE are few points in agricultural chemistry which are at present believed to be so fully established, as that the earthy and alkaline phosphates are indispensable to the perfect development of those plants and seeds which constitute the food of man and of the inferior animals. This consideration has led to many attempts to procure more abundant supplies of phosphoric acid, as a deficiency of that element, it is evident, must ever present a serious impediment to the progress of agricultural improvement. The chief sources from which phosphoric acid has been hitherto obtained, have been bones, guano, and the urine of men and animals. The usual ways of employing urine have been, either to apply it directly to the fields in its liquid state, or by neutralizing it with sulphuric acid, and evaporating it to dryness, till it forms a mass of salts, which renders it easily transportable. In the liquid state, from its great bulk, urine is an inconvenient article to collect and

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* Communicated by the Author.

transport to any distance, and any attempt to reduce it to the solid state by evaporation, involves so large an expense for fuel as to render it in most cases quite unadvisable.

The method by which I purpose to procure phosphoric acid from urine is not by evaporation, but by precipitating it in the state of the insoluble phosphate of lime or bone-earth. This is easily and cheaply effected, by merely adding a slight excess of lime-water, or still better, of milk of lime, to the urine so long as it causes a precipitate. This precipitate, when produced by means of lime-water, has a bulky gelatinous appearance, very much resembling alumina, and, as will be seen by the subjoined analysis, consists of basic phosphate of lime with a little magnesia and some organic matter. After standing for some hours, the precipitate subsides pretty readily, and the greater portion of the supernatant water may be easily drawn off by a siphon. The remainder may then be removed by collecting the precipitate on any suitable filtering apparatus. On drying the precipitate, which, as we have already mentioned, is very voluminous, it shrinks exceedingly. For the purpose of obtaining this precipitate, it is by no means necessary that the urine should be in a concentrated state; I have been able to procure it quite readily from urine diluted with ten times, or even more, of its weight of water. It is in this circumstance that the chief advantage of the proposed method consists, as urine can thus be made available as a source of phosphoric acid, even in the very diluted state in which it usually passes into the common sewers, when it has hitherto been regarded as unfit for any useful purpose. When the precipitate is destructively heated, it blackens, owing to the considerable quantity of organic matter it contains; and it emits at the same time a disagreeable ammoniacal smell, so that it is by no means destitute of nitrogen. The organic matter I believe to consist chiefly of mucus which has been carried down by the lime. A quantity of this precipitate dried at 212° F. was subjected to analysis. It gave—

Lime	44·96
Magnesia	1·32
Phosphoric acid	40·18
Loss by ignition, organic matter and water	13·54
	100·00

As I have already mentioned, the precipitate when heated to 212° F. gives off an ammoniacal smell, and consequently, when it is meant to be employed as a manure, should be dried at a very gentle heat. The quantity of nitrogen contained in the precipitate, when dried at an ordinary temperature, was more than I expected. One portion dried in this way gave by Will's method 1·91 per cent. nitrogen, or nearly two per cent.; a second quantity, dried at 212° F., gave only 0·88 nitrogen. Another portion, air-dried, when heated to redness, gave only 41·19 per cent. of fixed residue.

One pound of urine precipitated with lime-water gave, when heated to redness, 19·92 grains of phosphate of lime and magnesia. A second pound of more highly concentrated urine, similarly treated, gave 32·38 grs. of the same substances. Both quantities were urine in its natural state. It is clear that these results can only be regarded as approximative, as urine varies so much in concentration in every individual, according to the circumstances in which it is produced.

The amount of phosphate of lime obtained from a pound of urine is, it must be confessed, by no means considerable. When we reflect, however, what an immense quantity of dilute urine may be readily obtained from the common sewers of some of our large towns, it is clear that this is a source which might be made to furnish the fields with large quantities of the phosphates which are at present passing into the rivers, and which thus, for a long time at least, remain unavailing for agricultural purposes. In conclusion, I shall shortly describe the way in which I believe the operation of precipitating the phosphates may be most conveniently conducted on a large scale. The urine, in the state in which it passes into the sewers, may be collected in any suitable pond or tank, and the lime-water, or what is still better, the milk of lime, may be prepared in a second tank of much smaller dimensions situated on a higher level. The milk of lime should then be run into the first tank containing the urine, and the liquids well-mixed by agitation so long as a precipitate falls. I greatly prefer employing a slight or even a considerable excess of milk of lime, rather than lime-water; for though the phosphoric acid in the urine is completely precipitated by either reagent, lime-water produces a gelatinous precipitate which does not subside so readily and is difficult to filter, while milk of lime throws down a flocculent precipitate which is much more manageable. When the mixture of the lime and urine has been left to repose for some hours, the precipitate subsides pretty fully, so that three-fourths of the water may be readily drawn off by means of a siphon. By removing a plug at the bottom of the tank, the remainder of the water may be run off through any suitably constructed filtering apparatus, and the phosphate of lime and magnesia obtained as a bulky gelatinous mud.

The mass may be rendered perfectly dry, either by exposing it in shallow vessels to the rays of the sun, or to a current of dry or hot air. When dry it forms a friable mass, which, when slightly rubbed, crumbles into a very fine powder. The quantity of lime required to precipitate the phosphoric acid from urine is by no means great, and the only difficulty experienced in the whole process is in the filtration, which proceeds much more slowly than could be wished, though I have no doubt that a little practice would suggest the means of greatly diminishing, if not of wholly obviating this impediment.

Glasgow, August 26, 1845.

P.S. Since the above was written, I have ascertained that the difficulty in filtering and drying the precipitate, to which reference has more than once been made, may be greatly obviated by intimately mixing a small quantity of finely-powdered wood-charcoal with the precipitate, after the great bulk of the water has been drawn off by means of a siphon or otherwise. The quantity of charcoal which is necessary for this purpose, is by no means considerable; it has the effect of rendering the precipitate tolerably porous, and thus greatly facilitating its being filtered and dried. If the charcoal-powder, before being mixed with the precipitate, has had a considerable quantity of putrid urine filtered through it, it becomes strongly charged with ammonia, and is thus rendered much more valuable as a manure. The urine which has been used to impregnate the charcoal can, of course, be run into the tank, and the phosphoric acid it contains be precipitated with lime in the way already described.

J. S.

XLV. *Supplementary Remarks on Elimination, and on the Theory of Equations.* By James Cockle, *M.A., of Trinity College, Cambridge; Special Pleader*[*].

5. In the development of $f^3(10.)$[†], let the coefficient of $z_p z_q z_r$ be denoted by $(p\,q\,r)$, with the exception of that of z^3, which we shall represent by a_1, and let

$$\tau_{p,q} = a_1(1\,p\,q) - \frac{2}{3}(1^2 p)(1^2 q), \quad . \quad . \quad . \quad . \quad (11.)$$

$$\tau_{p,q,r} = a_1^2(p\,q\,r) - \frac{2}{9}(1^2 p)(1^2 q)(1^2 r), \quad . \quad . \quad (12.)$$

subject to the condition that, when m of the quantities p, q, r become equal, the last terms of (11.) and (12.) are to be divided by $m(m-1)\,.\,.\,2\,.\,1$, then, if

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[*] Communicated by T. S. Davies, Esq., F.R.S., F.S.A., &c.

[†] See my paper in this (August) Number of the Phil. Mag. p. 126, line 3.

$$a_1^2 f^3(10.) = h_1^3 + a_1 z_1 f^2(9.) + f^3(9.), \quad . \quad . \quad (13.)$$

we have

$$f^2(9.) = \Sigma(\tau_{p,q}\, z_p\, z_q), \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

and

$$f^3(9.) = \Sigma(\tau_{p,q,r}\, z_p\, z_q\, z_r). \quad . \quad . \quad . \quad . \quad (15.)$$

6. Next, suppose J_r to be free from $z_1, z_2, .. z_{r-1}$, then it is allowable at line 9 of p. 126 of my last paper, to change the suffixes of J from 9, 8, 7, 6, into 2, 4, 6, 8, respectively, for we may group as we please the squares into which $f^2(9.)$ is decomposed. This gives us after reduction $J_2^{(2)} =$

$$0 = \gamma_2 z_2 + \gamma_3 z_3 + \cdots + \gamma_9 z_9 + \gamma_0, \quad . \quad . \quad . \quad (16.)$$

where

$$\gamma_2 = \pm \sqrt{\tau_{2^2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

and

$$\gamma_3 = \frac{1}{2} \cdot \frac{\tau_{2,3}}{\gamma_2} \pm \sqrt{\tau_{3^2} - \frac{1}{4} \cdot \frac{\tau_{2,3}{}^2}{\gamma_2{}^2}}; \quad . \quad . \quad (18.)$$

and if δ, ε, ζ, η be the coefficients, taken in the usual order, of the terms which (after substituting for z_3 in $f^3(9.)$ its value derived from (16.)) contain z_2^3, then, a_2 being the coefficient of z_2^3 in the result,

$$a_2 = \delta - \varepsilon \frac{\gamma_2}{\gamma_3} + \zeta \left(\frac{\gamma_2}{\gamma_3}\right)^2 - \eta \left(\frac{\gamma_2}{\gamma_3}\right)^3 \quad . \quad . \quad . \quad (19.)$$

The above is general, whatever be the value or form of $f^3(10.)$, and is applicable to the annihilation of the 2nd, 4th and 6th terms of the equation of the 6th degree; but, in case the order in which the squares into which $f^2(9.)$ is decomposed are to be grouped should in any instance become material, the last paragraph will have to be modified accordingly.

7. (*Supplement to p.* 384 *of the last volume.*)—Let $n = 4$, and add a term, $N\phi(x')$, to the right-hand side of (3.); then a product similar to (4.) may be obtained by means of the principle, that "ϕ_1, ϕ_2, .. . ϕ_{n-1} being linear and homogeneous functions of x_1, x_2, x_n, and $\phi^{(r)}$ denoting the result of substituting $x^{(r)}$ for x in ϕ; then, when π is composed of symmetric functions of x, &c., $\pi^{\backprime\backprime}$ $(=\Sigma\{\phi_1\,\phi_2 \cdot\cdot\, \phi'_{n-2}\,\phi''_{n-1}\})$ consists of symmetric functions of $x_1 \,..\, x'_1 \,...\, x''_n$:" so that, when $x'' = x'$, $\pi^{\backprime\backprime} = \pi''$. By means of the obvious extensions of this principle, which is a branch of one of numerous classes of the same kind as that used in paragraph 3, we may add to (3.) as many terms as we please, for any values of n. These remarks are made with reference to paragraphs 5 and 6. It is possible that in all these investigations we may be able to derive some aid from the quaternion theory, a different distribution of α, &c.; and it will be borne in mind that a want of symmetry in the

coefficients of an equation sometimes disappears in its roots, as is the case with the equation

$$w^2 + y_1 w + \frac{1}{4}\{y_1^2 - (y_2 + y_3)^2\} = 0,$$

which has *one* root symmetric with respect to y.

Birchington, near Margate,
August 29, 1845.

XLVI. *On the Action of Bleaching Powder on the Salts of Copper and Lead.* By Walter Crum, *Esq.*, *F.R.S.*[*]

In February 1843 I read to the Philosophical Society of Glasgow an account of a rose-coloured oxide of copper which I had obtained by the action of bleaching powder and lime upon nitrate of copper. Although I had then made numerous analyses of this substance, prepared under a variety of circumstances, I had been unable to obtain from it the full amount of oxygen which a definite compound must contain, and delayed therefore to make it further known until I should have the opportunity of producing it in a purer form. In the mean time the rose-coloured substance was observed, and correctly described, by Krüger of Berlin, as a combination of the oxide of copper, or, as he calls it, cupric acid, with lime. Having completed my experiments on this subject as far as my leisure will permit, I shall now state the results I have obtained.

When the hydrated oxide of copper is added to a solution of bleaching powder it changes colour and becomes brown. Oxygen gas is then plentifully disengaged, and the effervescence continues till the whole of the hypochlorite of lime is decomposed. The brown precipitate suffers no change during this decomposition. When separated from the soluble matters, it is found to contain no chlorine and no excess of oxygen. It is anhydrous oxide of copper. Hypochlorite of soda produces the same effect.

If we add nitrate of copper to a solution of bleaching powder containing a considerable quantity of lime, and previously cooled to below the freezing-point of water, a bluish-green precipitate is formed. When the precipitate subsides, we find the solution of a fine blue colour and containing copper; but in what state I have not examined. As the heat advances to the ordinary temperature, the copper in solution, as well as

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

the precipitate, changes colour, and both at last become an insoluble purplish black powder. Oxygen gas is disengaged during the latter part of this process, and continues for some time to prevent the precipitate from subsiding; but after twenty or twenty-four hours the evolution of gas nearly ceases, the particles having united into larger grains sink to the bottom of the vessel into moderate bulk, and may then readily be separated from the soluble matters, by repeated mixing with cold lime-water and drawing off the clear liquid with a syphon. The precipitate thus obtained is, as I have said, nearly black; but by triturating upon a piece of glass it is seen that its real colour is rose-red.

Exposed to the action of boiling water oxygen gas is disengaged from this substance, and brown anhydrous oxide of copper is left behind. Acids dissolve it, with the liberation of oxygen gas, mixed with the carbonic acid taken down by the lime. The solution in nitric acid gives no precipitate with nitrate of silver. Exposed to the air the substance is speedily changed into the green carbonate. In attempting to press and then to dry it *in vacuo* over sulphuric acid, a large proportion was changed into the brown oxide mixed with carbonate. It can only therefore be examined in the moist state and newly prepared. I shall describe the process by which I have obtained the best results.

Twenty grains of black oxide of copper, prepared by calcining the nitrate, are dissolved with the assistance of heat in 70 grs. of nitric acid, specific gravity 1·35. Fifty grains of fresh hydrate of lime, sifted through fine calico, are mixed with 1 lb. solution of bleaching powder of specific gravity 1·06, and added to the solution of copper. When the precipitate becomes granular, as already described, it is quickly washed, and decanted until the lime water comes off nearly pure. The precipitate is then put into a wide tube over mercury, an excess of sulphuric acid added to it; and, after pouring out as much as possible of the solution which is thus formed, caustic soda is added to absorb the carbonic acid. In six experiments made in this way, 20 grs. of oxide of copper produced a compound which yielded of oxygen gas, after the necessary corrections,—

1·875
1·886
1·748
1·915
1·795
1·747

Mean . . 1·828 grain.

By calculation 20 grains CuO changed into Cu_2O_3 ought to yield, by Berzelius's numbers, 1·98 grain of oxygen. A nearer approximation than in the foregoing results is scarcely to be expected; for although there was no perceptible disengagement of gas during the washing of the precipitate in these experiments, it is certain that oxygen always escapes during the time so employed.

The quantity of lime necessary to the production and stability of this oxide is not more than 1 equivalent after saturation of the nitric acid: 1 atom of lime to 3 of copper gave only 0·558 gr. of oxygen gas instead of the mean quantity of 1·828; 2 atoms to 3 of copper yielded 1·295. I conceive the rose-coloured powder then to be a compound of an oxide of copper with lime, in which the copper exists in the state of sesquioxide, Cu_2O_3.

I have not succeeded in producing this oxide by means of the hypochlorites of potash or soda, even with the alkali in great excess; but by adding caustic soda to a solution of hypochlorite of lime, and afterwards nitrate of copper, we obtain the calcareous compound (lime being precipitated along with the copper) in a state of division so fine as to show the rose colour as soon as it is formed. This method, however, does not serve for the purposes of analysis, for the powder never becomes granular, and remains therefore too bulky to be washed.

It will now be observed that the dehydrating action of the hypochlorites upon oxide of copper must depend upon the momentary formation of a sesquioxide in which the oxygen has replaced the previously combined water.

The solution of bleaching powder in which the sesquioxide has been formed is of a fine but very pale pink colour, and contains so small a proportion of the colouring ingredient, that the nature of that body can scarcely be discovered by analytical means. The second washing of the oxide is colourless; but if a very minute portion of sulphate of manganese be added, the pink colour is restored. When manganate of potash is dropped into nitric acid, the well-known red colour of hypermanganic acid is produced. In lime water the colour is bluish green; but in bleaching liquor, even with excess of lime, we have the peculiar amethystine colour of the solution in which the sesquioxide of copper has been produced. Bleaching powder has long been said to contain manganese, and to this I at first attributed the pink colour of the original solution; but I afterwards found that it could be reproduced from pure Irish limestone which I employed. Even marble gives a pink solution in the same circumstances.

The vessel in which the sesquioxide has been produced, is lined with a beautiful rose-coloured deposit, which remains attached to the glass when the other matters are washed out; but it fades away in a few hours, particularly when exposed to light, and cannot even be long preserved in the solution in which it forms. Dissolved in dilute nitric acid, copper is found in the solution and no manganese. There can be no doubt that, like the precipitate, it is the sesquioxide of copper in combination with lime.

The red oxide of iron has also the power of decomposing the hypochlorites. This fact, as well as the formation of a superoxide of copper, was observed many years ago by Mr. Mercer of Oakenshaw, and stated by him to the British Association in 1842, in a paper containing some interesting speculations on these and other weak affinities which give rise to many of the phænomena of catalysis.

When a solution of bleaching powder is mixed with nitrate of copper, a light bluish-green powder precipitates, the bulkiness of which renders it somewhat difficult to wash. This powder is very slightly soluble in water, and scarcely changes colour in boiling. Heated in a glass tube over a spirit-lamp, chloride of copper sublimes into a cooler part of the tube, and water escapes. The residue consists of black oxide of copper mixed with a quantity of chloride, which may be separated from the oxide by washing. Professor Graham, who suggested to me this experiment, remarked the analogous effect of boiling water in separating water from a hydrate. It proved to be a hydrated oxichloride of copper, the substance known by the name of Brunswick green, and formed in a variety of other circumstances. Analysis gave me nearer $3\,CuO, CuCl$ than $4\,CuO, CuCl$; but the presence of carbonate in the specimen left me in doubt upon this point, and I could not resume the inquiry. In this reaction the whole of the hypochlorous acid is set free.

$$4(CuO\ NO_5) + 3(CaO\ ClO,\ CaCl)$$
$$= 4(CaO\ NO_5) + 3\,CuO,\ CuCl + 2\,CaCl + 3\,ClO.$$

Peroxide of lead is often produced by passing a stream of chlorine through a solution of sugar of lead. The chloride which accompanies it in this way may be also converted into peroxide by employing a solution of bleaching powder instead of chlorine. The peroxide produced by these means has a light brown colour, although washed with dilute nitric acid and boiling water. I have succeeded, by the following process, in forming a compound, nearly colourless, of *peroxide of lead and lime*. Dissolve in water 1 lb. of nitrate of lead, and add it, along with 3 equivs. of lime, to 16 pounds of a solution of bleaching powder of sp. gr. 1·08. Heat the mixture gradually to 160° Fahr. and stir it frequently during five hours. Pour off the clear liquid, add 16 pounds more of the same solution, and continue the heat three hours longer. The combination is obtained white with only a slight brown tinge. It is quite insoluble in water, and when dried does not alter in the air. Nitric acid, by dissolving the lime, leaves the peroxide of a deep black colour, and therefore much deeper than that obtained by any of the processes usually employed.

I have had no means of determining the proportion of lime contained in this plumbate. With less than two equivalents to one of oxide the compound is not white; and an excess of lime cannot afterwards be dissolved away by an acid without discolouring the salt.

I found it convenient in these experiments to prepare a quantity of cream of lime, by dropping newly-burnt lime into boiling water, stirring up, allowing the sand and the grosser parts to subside, and pouring off the superstratum. When this again had subsided for some time, the water was poured away, and the cream of lime which remained corked up in small bottles for use. By this means I had always at hand a quick lime, whose equivalent I knew, free from sand and free from carbonate. Marble, of course, answers the best for this purpose.

Manganese, again, appears in the nitric acid which has been employed to decompose the plumbate, in the state of a pink-coloured hypermanganic acid. When this solution is poured off and more water and nitric acid added to the peroxide that is left, a small quantity of sulphate of manganese restores the colour. Peroxide of lead, prepared by the same or by other means, when dried, does not yield the pink colour without the application of heat.

XLVII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 226.]

April 11, 1845. ON the Longitude of Paramatta. By C. Rumker, Esq. Communicated by Dr. Lee.

The object of this communication is to show that the method of determining the longitude by means of moon-culminating stars can be extended to large meridional differences. The observations of the moon and moon-culminating stars made at Paramatta are compared with those made at Greenwich, Paris, Abo, and Konigsberg, and the results have proved satisfactory. The correction to the assumed east longitude, $10^h\,4^m\,6^s{\cdot}25$, from the mean of all the observations, is $+\,0^s{\cdot}967$; whence the resulting longitude is $10^h\,4^m\,7^s{\cdot}217$.

On the Use of a new Micrometer, and its Application to the Determination of the Parallax of Mars at his ensuing Opposition. By M. Boguslawski. Communicated, with a Letter to the President, from Sir John Herschel.

This communication is introduced by the following letter from Sir John Herschel:—

"Collingwood, March 30, 1845.

"My dear Sir,—I request permission to lay before the Astronomical Society the accompanying communication from M. Boguslawski, just received, in which he describes a new micrometer, and urges its application to the determination of the parallax of Mars at his ensuing opposition.

"M. Boguslawski, as will be observed, requests me to take on myself the sole charge of directing and arranging the observations he recommends. My engagements utterly preclude the possibility of my undertaking this duty, or any part of it. Nevertheless, being willing and desirous, so far as I am able, to forward this or any other well-recommended scientific object, I have considered it, on the whole, as my duty to forward his letter where it is sure to receive every due consideration; leaving it to the Council, if it should seem of sufficient importance, to make any arrangements they may see fit for meeting the end in view.

"I ought to add that I have myself had in use, for some time during my observations at the Cape, a process of micrometrical measurement by means of the transit of stars over the moveable wire of a position micrometer, set at a given angle of inclination to the parallel, which I have designated as *the method of oblique transits* for the purpose of determining the distances of double stars, which appears to me nearly or quite identical in principle with this of M. Boguslawski.

"An account of this method, and the results obtained by its use, have been for some time written for the press, and will form part of my forthcoming observations in the southern hemisphere.—I remain, my dear Sir, &c.

"J. F. W. Herschel."

The author states that he intends shortly to publish a detailed account of the construction and use of this micrometer, for observing differences of right ascensions and declinations of stars. Its principal peculiarity consists in this, that its scale is not dependent upon the individual telescope employed, and is therefore independent of the optical power of it, except only as far as the distinctness and precision of the images are concerned. An account of some observations made with it will be found in some of the forthcoming numbers of the *Astronomische Nachrichten*. It consists simply of one *wire*, *thread*, or *lamina*, which is placed in the common focus of the object-glass and eye-piece of a telescope, as a diameter across the field of view, in such a manner as to turn round the centre in every direction, and to make with the declination circle any given angle. The author then proceeds to show, that if τ°, τ', and T, be the times of passage across the wire of three stars (the first two being known, and the third being unknown or its position required), whose right ascensions are α°, α', and A, and declinations δ°, δ', and D, for any given position of the wire; and if θ°, θ', and Θ, be the corresponding times of transit of the same three objects for any other position transverse to the former, then

$$\mathrm{A}-\alpha^\circ=\mathrm{T}-\tau^\circ+[(\alpha'-\alpha^\circ)-(\tau'-\tau^\circ).]\frac{(\Theta-\theta^\circ)-(\mathrm{T}-\tau^\circ)}{(\theta'-\theta^\circ)-(\tau'-\tau^\circ)},$$

or
$$=\Theta-\theta^\circ+[(\alpha'-\alpha^\circ)-(\theta'-\theta^\circ).]\frac{(\mathrm{T}-\tau^\circ)-(\Theta-\theta^\circ)}{(\tau'-\tau^\circ)-(\theta'-\theta^\circ)},$$

and
$$\mathrm{D}-\delta^\circ=\frac{\delta'-\delta^\circ}{e}.\frac{(\Theta-\theta^\circ)-(\mathrm{T}-\tau^\circ)}{(\theta'-\theta^\circ)-(\tau'-\tau^\circ)},$$

where
$$e=\frac{\cos.\frac{1}{2}(\delta'+\delta^\circ)}{\cos.\frac{1}{2}(\mathrm{D}+\delta^\circ)},$$
and is very nearly equal to 1.

When the unknown object is a planet, since its position is determined by observations made at two different times, T and Θ, a correction will be necessary for its motion during the interval of time $\Theta-\mathrm{T}$; and the author determines the corrections to be applied to the times Θ and T, to reduce them to the same epoch.

The author considers this micrometer, both from its simplicity and from the precision of its results, to be exceedingly well adapted for observations of Mars and neighbouring stars, for the purpose of determining its parallax; and he gives the following formulæ for the value of the parallax:—

Let ψ_W and ψ_E be the latitudes of two observatories favourably situated for determining the parallax by observations of right ascensions, or differing considerably in longitude, ψ_W being the latitude of the western and ψ_E of the eastern station. Let also h_W and h_E be the corresponding hour-angles of Mars; $\mathrm{A_W}$ and $\mathrm{A_E}$ the observed right ascensions, reduced to the same instant; and D the approximate declination; then

The horizontal parallax of Mars

$$=\frac{(\mathrm{A_E}-\mathrm{A_W})\cos\mathrm{D}}{\cos\psi_\mathrm{W}\sin h_\mathrm{W}-\cos\psi_\mathrm{E}\sin h_\mathrm{E}}.$$

Again, if $\mathrm{D_N}$ and $\mathrm{D_S}$ denote the declinations observed at two stations differing very considerably in latitude (N denoting the northerly and S the southerly station, and the rest of the notation being suitably changed),

The horizontal parallax

$$=\frac{(\mathrm{D_S}-\mathrm{D_N})\,\mathrm{Sec.\,D}}{\sin\psi_\mathrm{N}-\sin\psi_\mathrm{S}+\tan\mathrm{D}\cos\psi_\mathrm{S}\cos h_\mathrm{S}-\tan\mathrm{D}\cos\psi_\mathrm{N}\cos h_\mathrm{N}}.$$

This communication is accompanied by a list of stars selected by the author as favourably situated for observation at the ensuing opposition of Mars; the list includes many of those set down for a similar purpose in the Nautical Almanac for 1845.

May 9.—The following communications were read:—

I. Extract of a Letter from the President to the Secretary.

"I forward you a new epoch of γ Virginis for this apparition, thinking it may be welcome to such of our Fellows as are following up the orbit of that remarkable system, which promises, comparatively, to be to double stars what Halley's comet is among that class of bodies. I made the observations in Dr. Lee's Observatory, at Hartwell House, with my former instruments, which are always obligingly kept in readiness for my immediate use. The mean micrometrical readings gave 174° 36′·6 for the position, which, corrected to the meridian, and brought to proportional parts, afford

Position 185° 23′·3 (w. 6); Distance 2″·10 (w. 4); Epoch 1845·34.

"The measures made on the 2nd and 3rd inst. merit considerable confidence, for, previously to my placing the micrometer on the objects, it was steadily viewed in an unilluminated field with a negative eye-piece magnifying 240 times, under which the minute companion following nearly on the parallel was distinctly seen. On this occasion, the wire-micrometer was under a positive eye-piece of 340 times during three sets of the measures, and once under one of 600; and the rock-crystal was applied with its power of 145, doubled by the interposition of the achromatic lens described in my 'Cycle.'"

II. On a new construction of the Divided Eye-glass Double-Image Micrometer. By G. B. Airy, Esq., Astronomer Royal.

The author remarks, that the convenience and accuracy of the double-image micrometer are so great that it is highly desirable to remove, if possible, any of the inconveniences to which it is subject. The form in which he has employed the instrument is that of a four-glass eye-piece, the second lens (as measured from the object-glass) being divided, and one or both segments being moveable. The second lens must therefore be placed in such a position that every pencil of rays coming from the object-glass is equally divided upon the two segments,—a condition which is satisfied when the distance of the second lens from the first is (sensibly) the same as the focal length of the first lens. When this adjustment to focal length is perfectly made, and when also the observer has the power of securing the equal division of light upon the two segments for one pencil (and therefore for all), the use of this eye-piece is extremely convenient. Without any effort in placing the eye in a definite position, a single image, in whatever part of the field, is broken up into two of equal intensity.

The most important defect of the micrometer is this, that, unless certain conditions are respected, the images will be coloured in all parts of the field of view except the centre. This colour is not produced, like the colour in an achromatic object-glass, from the want of convergence of all the rays in one pencil to the same focal distance. It arises from a lateral chromatic disturbance of the focus of each pencil; and it may be represented most conveniently by tracing the course of the axis of each pencil, considered as a single ray subject to chromatic separation, through all the lenses to the eye, when, if they enter the pupil in any relative direction, except that of parallelism, they will produce the sensation of chromatic separation in the direction of the radius of the field of view. In this micrometer eye-piece such a defect is wholly inadmissible, and therefore the first condition is, that the eye-piece shall be (without reference to the fault described, and without regard to the separation of images) achromatic.

The theory of achromatic eye-pieces was given by the author in the Cambridge Transactions, vol. ii., and the first divided eye-glass micrometers which he constructed were made of four-glass eye-pieces in which the equation of achromatism was satisfied, subject to the condition already mentioned, that the distance between the first and second lens should be equal to the focal length of the first lens. But in the use of these a new inconvenience soon manifested itself. The separation of the images is produced by so moving a segment of the divided lens that the pencils of light are received upon a part of the segment where the surfaces are inclined, so that there is introduced in this part of the eye-piece a prismatic refraction and consequently a prismatic dispersion. In general, the dispersion thus produced will not be so modified by the passage of the rays through the two remaining lenses that the rays of all colours (as separated by the motion of the segment of the second lens) will enter the eye in a state of parallelism; and therefore the separated images will appear coloured from this cause without respect to the part of the field of view in which they are seen.

This failing is, perhaps, more important than the other, for it affects the estimation of the scale of the micrometer as well as the ordinary use of the micrometer. The only practicable method of ascertaining the scale of the divisions is to place a wire in the meridional direction across the centre of the field; to make two images of a single star by separation through a known number of divisions in an equatorial direction, and to observe the transit of these two images across the wire. If one or both images are coloured, so as to present the appearance of a spectrum in the direction of separation, these transits are uncertain, and the estimation of the scale-divisions derived from them will be uncertain.

It was not till long after this inconvenience had been perceived that it occurred to the author that it was possible to ascertain, in an algebraic form, the theoretical equation upon which the removal of this chromatic dispersion would depend, and thereby to discover whether the two equations (of achromatism in the ordinary case of no separation of images, and achromatism as regards only the dispersion produced by the separation of images) could be satisfied simultaneously, and without disturbing the assumption that the distance between the first and second lenses should be equal to the focal length of the third lens. On forming the equations, and substituting trial numbers, he had the gratification of discovering that numbers could be found for the distances and focal lengths which are very favourable as regards the breadth of the pencil of rays where it meets each lens, and which leave the focal length of the first lens perfectly arbitrary, so that the power of the eye-piece may be changed, preserving all its properties undisturbed, by merely changing the first lens. This is the "new construction" proposed as the object of the paper.

The algebraic investigation is then given in detail; and the author, after the reading of the paper was concluded, gave an oral explanation, illustrated by diagrams, of the construction and the theory upon which it depends.

June 13.—The following communications were read:—

I. On the Ephemerides of Jarchi, the Chaldean Months, and the derivation of Orion and his Dogs. By S. M. Drach, Esq.

The author suggests, on the authority of Weidler's *Historia Astronomiæ*, that the first astronomical tables and ephemerides published in Europe are those of Rabbi Solomon Jarchi, although still inedited. Jarchi is supposed by Dr. Jost to have lived from the years 1040 to 1105 (*Gesch. der Israel.*, vol. vi. p. 243).

The author then proceeds to the consideration of the passage of the book of Job, contained in the thirty-first and thirty-second verses of the thirty-eighth chapter: "Canst thou bind the sweet influences of Pleiades, or loose the bands of Orion? Canst thou bring forth Mazzaroth in his season? or canst thou guide Arcturus with his sons?" And he cites the opinion of the Rabbinical commentators for the meaning of each of the astronomical words contained in it.

He then gives from Buxtorf's Lexicon the meanings of the names of the Jewish months, and concludes that their calendar is founded on the agricultural occupations of the Chaldean shepherds, who were employed during winter in preparing the fleeces of their flocks for clothing, &c.

He thinks it not impossible that the numerical values of the names of the months indicate the number of days wherein celestial phænomena recur, mystically decomposed for priestly purposes; and he gives instances in which certain combinations of their values represent pretty nearly the synodical periods of some of the planets.

There is some reason for supposing that Orion, Sirius, and Procyon, may be regarded as Hellenized Egyptian names by dropping the final syllables, and that they may thus be referred to the Chaldean dialect.

II. Extracts from a Letter to the President, dated Picket Berg, Cape of Good Hope, January 1, 1845, from C. Piazzi Smyth, Esq. Communicated by Captain Smyth.

Great Comet of 1844–45.

"Another splendid comet is at this moment astounding the southern hemisphere and displaying the most interesting series of phænomena, which are passing away unrecorded, because the Institution is still without sufficient means.

"The apparition of this comet, 1844–45, is just such an opportunity, such a crucial test of his theory, as Bessel is so ardently desirous of beholding, provided, however, as he would of course be, with a sufficient apparatus.

"During the first nine days of its appearance to this end of the colony generally, this comet was only seen once at the Royal Observatory, on account of the clouds which form so frequently to the west, because Table Mountain is there; and, now the comet is going southward so very fast, that it will probably soon be out of reach of the equatoreal mounting of the telescope. This state of things has already supervened in the case of Mauvais' comet, which has been observed since October, and is likely to be visible for a long time to come; but from the middle of December its south declination has been so great as to place it beyond the range of the instrument at the best times for observation. Now, therefore, a polar axis mounting is being hastily run up."

Meteorology.—"A large mountain is too generally associated in idea with constant clouds and storms; but two years' experience on great elevations at this end of the colony shows decidedly that the highest position is by no means, on that account solely, liable to most mist, or vexed with most wind; such effects depending more on the proximity of the sea, and the configuration of the neighbouring country.

"Winterhock-berg, 6500 feet high and 60 miles from the sea, had little wind and less mist; but one of its off-sets contains a *kloof*, or cleft, which serves as a vent for the wind which has been blowing up the Worcester and Tulbagh valleys; and, in this kloof, 1200 feet high, there is generally abundance of both wind and cloud,—the latter of that sort which is forming at the same time on the top of Table Mountain, 3600 feet high.

"Kamiesberg, 5500 feet high, a cold mountain, isolated in extensive warm plains, had little wind; but Klyp Rug-berg, only 1000 feet high, situated on the edge of a steppe dividing a cold region from a hot one, was subject to wind, violent and continual beyond example.

"In the plains there generally prevails during summer a certain degree of haziness and indistinctness in the atmosphere, highly prejudicial both to the definition and the intensity of any distant illuminated object. A residence on a high mountain shows that there is spread at such times over the whole lower country a stratum of dry fog to the depth of 3000 or 4000 feet, not affecting the dew-point, of a smoky colour, but so rare as to be unsuspected by a person immersed in it. When he is above its level the peaks of distant mountains in a similar condition appear well-defined, and of an intense blue or purple colour, while all below is indistinct, dancing, and boiling, and overcome by a general murkish gray tint. This peculiar fog may last in summer two or three months; in the winter, only as many days, and it is then more rare and attains a greater height. It is usually formed gradually, and is at length precipitated or dissipated by rain; but it is sometimes very sudden, and apparently capricious both in appearance and disappearance; no other important changes being evident at the time.

"What this fog is seems a question very difficult to solve. An infinite number of substances on the surface of the earth are constantly giving out their peculiar vapours; even metals do so, a fact implied by their having a smell: for to be perceived in that manner, particles must be flying off from them as from camphor. How high do such vapours ascend in the atmosphere, how long do they remain suspended, and are they at times precipitated, or are they decomposed?

"Smoke appears to play a part, though perhaps a very small one, in the business. From the universal practice all over the colony of 'burning the land,' or the bush, there is seldom a week passes without there being numerous fires burning in one's neighbourhood. In clear, calm weather the smoke from one of these has been seen rising up, cumuliform, 3000 or 4000 feet in height, and then forming a level stratum sometimes fifty miles in length, and this has continued several days, the fire still burning and the calm still enduring. While the dry fog prevails, a smoke from the plains has been seen to rise up 4000 feet and be lost in the upper layers of this haze, while at the same time smoke from the top of a neighbouring mountain 6000 feet high, has only risen up about 200 feet, and has then slanted off *downwards* and mingled with the same.

"In clear weather, on Winterhock-berg the theodolite telescope (aperture 2 inches, power 25) has shown a pile of rough stones (12 feet high, 12 feet broad at the base, and 3 at the top) on the Cedar mountains, nearly seventy miles distant, so clearly, that an object one-third the breadth of the top would have been abundantly visible. From the same station a man was seen on Picket Berg, 45 miles off; and on this, one of Hottentot's Holland Sneew Kop, the spars at the Lion's Rump signal station (40 miles off), have been most clearly visible. And what is very deserving of notice is, that some of the very best definition have been witnessed on these mountains in the middle of summer with a light northerly air; while at the same time, in the level country below, a strong south-east wind has been blowing with its usual accompaniment of large, faint, woolly blotches in the place of what ought to have appeared, viz. stellar points."

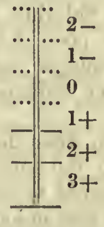
Magnitudes of Stars.—"Bessel's recommendation of steel specula reminds me of a series of astronomical phænomena that are passing unrecorded, viz. the changes in the magnitude of some stars.

"In March 1843, Mr. Mackay wrote from Calcutta to Sir John Herschel that η Argus had become as bright as Canopus, from which Sir John concluded that η was making another step forward, and would in time rival Sirius and the planets. But, after having for a few days in the beginning of the above-mentioned month approximated to Canopus, η dwindled down again very rapidly. But Sir John was not far out, for, although the star may not be advancing by successive steps, it seems to be doing the same by undulations; for, in March 1843, it did not go down to the point whence it started, and it is now again on the increase. It is, and has been for a month, brighter than Canopus, half-way indeed between him and Sirius, and is very red.

"Not having been able yet to get my tourmalines applied to a telescope, I cannot report on their actual sufficiency for measuring the magnitudes of stars; the principle, however, seems to answer, inasmuch as a mode of observation is produced, in which the only variable element is angular motion.

"Numerical measurement of the periodical and secular changes in the *colour* of stars would seem to be as important as that of their *lustre*. My plan for doing it cannot be put into action without a clock-moved telescope. The principle is merely this:—a prism is to be introduced into the tube of a telescope so as to make the linear spectrum of a star, instead of the round white image to be viewed by the eye-piece. The mean yellow ray is to be brought by mechanical adjustments to a fixed point in the field, and, the spectrum being divided into a number of spaces of certain angular extent, the brightness of each is to be measured separately, as in the case of the *magnitudes* of the stars.

"White stars of all sizes will, of course, show the same relative intensities in the different divisions of the spectrum; while coloured stars will exhibit a preponderance in the part which answers to their colour. To eliminate the differences of the effects of the atmosphere, and to determine what is white light, a method must be pursued similar to that in the discussion of the measurement of *magnitudes*; and the *colour* of the star will ultimately be expressed by numbers, without attempting to determine by what particular designation such a tint would be conceived by a person of ordinary eyesight.



"Let this be a spectrum divided into equal angular spaces of 2″ each, the one marked 0 being the yellow ray; by means of a perforated plate in the focus of the telescope, each of these portions is to be brought singly and successively into view, and have its brightness separately measured as a single star; the several portions of colour will then be represented thus:—

	−2	−1	0	+1	+2	+3
Sirius	20	40	70	50	30	10
η Argus	30	80	40	20	10	0
Canopus	10	20	35	25	15	5

"The method is undoubtedly rough, and inapplicable to the smaller magnitudes of stars visible in any telescope, but if a measurement is obtained in numbers (free from theoretical objection), and capable of having the probable error computed, it should be adopted until something better can be advanced.

"It will not do for any one to tell the public, even in the case of the magnitudes of stars, that 'he has tried all instrumental methods, and found all to be inferior to estimation by the naked eye;' because, so long as such a method of observation is the only one followed, so long will the assertion remain mere rhetoric,—a consumption of time without producing its equivalent of useful effect. Let us rather remember the primary and aphorismal foundations of practical science, such as:—

"'Science begins with the observation of common facts, but, even at this stage, requires that the observations be precise.'

"'Facts are the materials of science, but all facts involve ideas, and since in observing facts we cannot exclude ideas, we must take care that for the purposes of science, the ideas be clear and vigorously applied. Therefore, facts for the purposes of material science, should involve *conceptions* of the intellect only, and not *emotions*, and must be observed with reference to our most exact conceptions, number, place, figure, motion, force, &c.'"

XLVIII. *Intelligence and Miscellaneous Articles.*

ON OXIDATION BY MEANS OF CYANOGEN.

BY M. P. C. BOUDAULT.

THE author observes that chlorine, bromine and iodine are not the only bodies which possess oxidizing power by indirect action; and he shows that cyanogen, which has been proved to act in many cases as an elementary body, is also susceptible of effecting oxidizement.

The instability of cyanogen, when dissolved in water, always appeared to offer an obstacle to the author's proposed researches on this subject; it therefore occurred to him to replace cyanogen by a compound, in which this body was in loose combination, or, so to express it, in a mobile condition, that it might readily act upon oxygenated compounds, and separate their oxygen; and these conditions appeared to be fulfilled by the ferridcyanide of potassium, or red prussiate of potash. This salt, when heated out of the contact of air, is decomposed into nitrogen, cyanogen, ferrocyanide of potassium, cyanide of potassium and carburet of iron; but when decomposed in air it yields peroxide of iron and more cyanogen. It appeared therefore to the author that this compound might yield a portion of its cyanogen by certain acid reactions; and this was found to be the case.

When the equivalent of potassium, which chlorine removes from ferrocyanide of potassium to convert it into ferridcyanide, is added to it, no reaction takes place between them, even in a month, at ordinary temperatures; but when boiled and concentrated, peroxide of iron is precipitated, cyanide and ferrocyanide of potassium are formed, but no oxygen is obtained.

The author concluded that the decomposition of ferridcyanide into ferrocyanide, would afford cyanogen which would act as if it were free; and he found that when he heated oxide of lead in a mixture of ferridcyanide and potash, he procured binoxide of lead, and the solution contained ferrocyanide of potassium only. Having thus proved that the ferridcyanide might be substituted for cyanogen as an oxidizing agent, experiments were performed on the superoxidizement of various oxides.—*Journ. de Pharm. et de Ch.*, Juin 1845.

SUPEROXIDIZEMENT OF METALLIC OXIDES BY MEANS OF THE FERRIDCYANIDE OF POTASSIUM. BY M. BOUDAULT.

The author remarks, that metallic oxides in general have a tendency to pass to a higher degree of oxidizement; and some of them undergo it with great readiness, under the oxidizing influence of cyanogen, either at common temperatures or when boiling, according as the oxide is more or less superoxidizable.

Oxide of Manganese.—When protoxide of manganese, which has been precipitated without the contact of air, or a salt containing this oxide, is added to a mixed solution of cold ferridcyanide of potassium and potash, there are immediately obtained peroxide of manganese and ferrocyanide of potassium: with a large quantity of potash, crystallized peroxide of manganese was formed. If, instead of adding an excess of ferridcyanide, an excess of a salt of manganese be employed with a large quantity of potash, a reddish-brown precipitate may be obtained, which is probably the manganoso-manganic oxide.

Oxide of Nickel and Oxide of Cobalt.—The protoxide of nickel, when added to an alkaline solution of the red salt, does not appear to be superoxidized; and if it be boiled for some time decomposition occurs unconnected with oxidizement.

The same is the case with oxide of cobalt; there occurs, nevertheless, a moment in which the oxide of cobalt seems to be superoxidized, but it is probably reduced by the ferrocyanide which is formed.

Oxide of Lead.—The superoxidation of lead is the most marked of any oxide; a salt of oxide of lead, or the oxide itself, when dissolved in potash, is converted into binoxide, and almost always precipitated in the crystalline state; this latter fact, however, appears to depend upon the quantity of potash employed. On some occasions the author obtained minium by employing excess of the salt of lead, and heating the liquor but slightly.

Oxide of Copper.—The ferridcyanide of copper, or the oxide, are superoxidized by the mixed solution of ferridcyanide of potassium and potash; there are obtained a black precipitate, and ferrocyanide as usual.

Oxide of Tin.—The salts of protoxide of tin effect a reduction of the ferridcyanide, which indicates superoxidizement; but the author could not ascertain the nature of the oxide which is precipitated.

This oxide may probably be the stannate of protoxide of tin, discovered by M. Fremy (SnO, Sn^3O^6).

Oxide of Chromium, when dissolved in potash and heated with the ferridcyanide to the boiling-point, is converted into chromate of potash; and if sufficient oxide be used, all the ferridcyanide is reduced to ferrocyanide; this is evidently an example of very energetic oxidation, for hitherto chromate of potash has not been procured in the moist way.

Oxides of Gold and Silver are not superoxidized by the ferridcyanide; in a cold solution no effect is produced, but at a temperature near that of ebullition, peroxide of iron is precipitated; and the solution contains ferrocyanide of potassium, and cyanide of gold dissolved in the cyanide of potassium.

The salts of silver exhibit similar phænomena, viz. the formation of ferrocyanide, precipitation of peroxide of iron; and the solution contains ferrocyanide of potassium, which being precipitated by alcohol, cyanide of silver and cyanide of potassium were obtained. These facts are not included in simple oxidizement, they are true decompositions.

Sulphur may also be oxidized by this same influence; it becomes sulphuric acid.

Acids and Salts.—Certain mineral acids may be oxidized, whether free or combined; phosphorous acid and the soluble hypophosphites are converted into phosphates; sulphurous acid yields sulphuric acid, the sulphites are converted into sulphates. Oxalic acid, and oxalates added to the alkaline solution, yield carbonates almost instantaneously.

M. Boudault gives the following explanation of the phænomena which he has described:—the ferridcyanide of potassium is considered by him as a double salt, that is to say, as a compound of percyanide of iron and cyanide of potassium; this percyanide of iron, therefore, does not exist in a free state, and being a very unstable salt, it is retained in the red prussiate by the cyanide of potassium. As soon as a reaction takes place in the presence of this salt, it has a tendency to be decomposed, and the author gives the following as explanatory of his views:—

$$Cy^3Fe^2 + Cy^3K^3 = Cy^2Fe^2 + Cy^3K^3 + Cy.$$

On adding potash and oxide of lead, we have

$$\begin{aligned} &Cy^2Fe^2Cy^3K^3 + Cy + KO + PbO \\ &= Cy^2Fe^2Cy^3K^3 + CyK + PbO^2 \\ &= 2CyFeCy^2K^2 + PbO^2. \end{aligned}$$

By this formula we find two equivalents of ferrocyanide of potassium and binoxide of lead, which is what we obtain by the reaction which it explains.

The author considers the ferridcyanide of potassium, under the circumstances described, as ferrocyanide, minus potassium, and consequently as possessing an equivalent of free cyanogen, capable of oxyg replacing ~~cyanogen~~. It is therefore demonstrated, M. Boudault oncludes, that cyanogen possesses the additional analogy with chlorine, iodine and bromine, of being like them an energetic oxidizer.—*Journ. de Pharm. et de Ch.*, Juin 1845.

PHOSPHORIC ACID FOUND IN ALMOST ALL ROCKS*.

The following extracts from a lecture by Dr. R. D. Thomson in the forthcoming number of the Quarterly Agricultural Journal of Scotland, taken in conjunction with the paper printed in the Philosophical Magazine on the Inorganic Constituents of Lichens, render it obvious that few rocks will be found destitute of phosphoric acid†.

Greywacke Soil.—By explaining geologically the dependence of the soil upon rocks, and tracing the production of various soils from their respective rocks, not only may much interest, but much instruction be derived in the chemical studies of the agriculturist. During the last winter many careful analyses of rocks have been made. These I do not intend to describe at present; but, as an example of the importance of such analyses, I may notice those of the greywacke and the superimposed soil at Balkerr in Wigtonshire. The experiments were conducted by my pupil, Mr. James M'Bryde, under my immediate direction. The analyses of the rock and soil are placed in parallel columns for the sake of comparison. Specific gravity of the rock 2·76.

Decomposed soil.	Soil.	Greywacke.	Partially undecomposed soil.	Soil.	Greywacke.
Fibre	1·01		Gravel	6·39	
Organic matter and some water of undecomposed soil	12·51		Stones	19·16	
			Silica	38·79	72·18
			Alumina	8·30	11·68
Phosphate of iron	2·49	0·94	Magnesia	0·69	2·30
Peroxide of iron	7·60	9·94	Lime	0·55	0·96
Carbonate of lime	·74		Potash and soda		0·95
Magnesia	1·72		Water		2·40
Chloride of sodium Sulphate of lime	0·05	trace.			

The important fact that phosphoric acid is contained in greywacke is demonstrated by this analysis. In another examination we found a somewhat larger amount, and the variation may be considerable in different localities. A comparison of the preceding columns enables us to deduce the effects of cultivation. It is obvious that phosphoric acid and carbonate of lime have been added artificially. Phosphoric acid has been long known to exist in volcanic rocks (Boussingault's *Economie Rurale*, i. 561), and the fact has been confirmed by Mr. Fownes, but I am not aware that its occurrence in primary rocks was pointed out until the experiments made here in 1843, upon lichens, demonstrated the fact‡. I have since analysed all the primary rocks,

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* Communicated by Dr. Thomson.

† See also the very complete researches of Mr. Sullivan on this subject in our last number.—EDS.

‡ See the Journal of Agriculture for January 1845, No. 8.

and have never found it absent. Indeed the former experiments have demonstrated, that on those rocks where we find lichens growing, we may infer the presence of phosphoric acid and of various salts; and the same inference applies to all localities where marshy vegetation exists.

Greenstone Soil.—The following analyses were made by Mr. James Paterson, Barnego, near Denny. The greenstone upon which the soil is superincumbent, consists of hornblende in small crystals, with portions of quartz and some particles of calcareous spar. There is probably also felspar, but, owing to the minute nature of the ingredients of which the rock consists, it is difficult to discriminate the substances with the eye. The analysis of the rock and soil has been repeated each twice:—

	Soil.		Greenstone. Specific gravity 2·91.	
	I.	II.	I.	II.
Silica	50·00	49·70	50·20	51·20
Phosphate of iron	6·10	6·66	2·00	2·04
Peroxide of iron	14·04	14·16	26·40	20·66
Alumina	5·60	5·83		6·30
Lime	2·64	3·50	9·12	8·76
Magnesia and some potash	4·20	4·31	10·40	9·66
Soluble salts	1·63			
Organic matter	10·69			
Water	5·00		2·00	1·50
	100·00		100·12	100·12

The large amount of magnesia in the rock obviously belongs to the hornblende, the predominating constituent of the rock.

Limestones.—The experiments made in the Glasgow laboratory have proved the existence of phosphoric acid in the Irish limestone in minute quantity, but in a somewhat greater amount than in the English chalk, thus establishing chemically the parallelism of these two rocks in a geological point of view. The determination of this fact was conducted by Mr. John Thomson, M.A., in a most satisfactory manner. Limestone from Ireland, which is employed abundantly at Glasgow and on the Ayrshire and Wigtonshire coasts, was examined and compared with chalk from Cromer on the coast of Norfolk. The results were as follows:—

Phosphoric acid in Cromer chalk	0·77 per 1000 grains.
... ... Irish limestone	0·905

Scottish Magnesian Limestone.—I requested my pupil, Mr. James C. Stevenson of Jarrow, Shields, to examine the magnesian limestone of Sunderland for phosphoric acid, but we could not detect a trace of it in 1500 grains. We then turned our attention to the magnesian limestone of Berwickshire, as found on the Tweed between Kelso and Coldstream, and formerly burned at Hadden, and which I had found some years ago to possess the following composition:—

	1.	2.	3.
Specific gravity	2·723		
Carbonate of lime	49·600		
Carbonate of magnesia	44·000		
Silica	4·000		
Phosphate of iron		0·74	0·727
Peroxide of iron	1·200		
Alumina	1·000		

We found it to contain a decided amount of phosphoric acid, as exhibited in two trials made by Mr. J. C. Stevenson.

A similar limestone exists in Stirlingshire, at Boquhan, and is believed to extend to the south-west, although I have not had an opportunity of tracing it. For the opportunity of examining this limestone, I am indebted to the attention of William Murray, Esq. of Polmaise. The specific gravity of this limestone varied from 2·753 to 2·830 and 2·833.

The following are analyses of four different specimens:—

	1.	2.	3.	4.
Carbonate of lime	53·09	56·07	53·06	54·00
Carbonate of magnesia	43·49	39·34	43·11	
Phosphate and peroxide of iron	0·96	0·74	2·34	
Silica and alumina	2·46	3·85	1·49	

The first analysis was made by myself; No. 2, by Mr. Archibald Sinclair, Hillhead, Stirling; No. 3, by Mr. John Thomson, jun.; and No. 4 by one of my assistants.

By three analyses the composition of this limestone was ascertained to be, stating it in another form,—

	1.	2.	3.
Lime	29·73	27·886	
Magnesia	22·50		20·465
Phosphate of iron	0·96	0·431	0·279
Silica and alumina	2·46	0·862	2·958
Carbonic acid and water	44·35	47·721	46·500

EXAMINATION OF SOME NATIVE AND ARTIFICIAL COMPOUNDS OF PHOSPHORIC ACID. BY C. RAMMELSBERG.

[Continued from p. 237.]

Phosphate of Alumina and Lithia.

If, after washing the phosphate of alumina obtained from a solution of alum by precipitation with phosphate of soda, we dissolve it in solution of potash until it is saturated, and then add a solution of chloride of lithium, a voluminous precipitate is formed, which, according to Berzelius, contains phosphoric acid, alumina and lithia. When washed with cold water and dried, it forms a white, very loose powder, which is readily soluble in acids, and on being heated parts with a large quantity of water. For analysis it was heated to redness, the residue dissolved in muriatic acid, and the alumina as well as the greater part of the phosphoric acid precipitated by ammonia, after which, what remained, as also of lithia, were estimated in the manner previously described. In another experiment, the mass, after having been heated to redness, was fused with silica and silicate of soda, the lithia being left out of consideration. The results were as follows:—

	I.	II.	III.		
Phosphoric acid . .	22·81	22·73	22·61	1	= 24·36
Alumina	35·98	38·11		2	= 35·08
Lithia	7·35			2	= 9·84
Water	32·09	32·10		10	= 30·72

which leads to the following formula,

$$2LiO_3 P_2O_5 + 6Al_2O_3, P_2O_5 + 30HO.$$

Phosphate of Alumina.

But little is at present known concerning the composition of the artificial compounds of phosphoric acid and alumina. Two principal salts are distinguished—1, *neutral*, formed by the precipitation of a neutral aluminous salt by $2NaO, P_2O_5$, to which, consequently, the composition $2Al_2O_3, 3P_2O_5$ is assigned; and 2, a *basic* salt, which is formed by dissolving the former in an acid and precipitating it by excess of ammonia. This is usually regarded as $4Al_2O_3, 3P_2O_5$, although we are unacquainted with any analysis of it. We know that a compound of phosphoric acid and alumina is not decomposed by fusion with carbonate of potash, because a large portion of it is dissolved out with the excess of alkali when it is washed with water. Berzelius, in analysing wavellite, made use of a method in which the decomposition was effected by heating it to redness with a mixture of an alkaline carbonate and silica. Fuchs also used basic silicate of potash with the same view. But Berzelius's method is somewhat tedious, and the addition of silica always causes a portion of it to remain in the alkaline liquid, and on evaporating it and adding carbonate of ammonia it cannot be entirely removed. This is more completely effected by using a solution of oxide of zinc in carbonate of ammonia, but the subsequent separation of the zinc renders the process still more complex. The object is attained more rapidly by dissolving the substance in a sufficient quantity of sulphuric acid which has been previously diluted with an equal bulk of water, and then adding a proper quantity of potash, which is also dissolved by heat. The whole is then mixed with a considerable quantity of alcohol of 0·863 sp. gr., and set aside for some hours. Alum and sulphate of potash then separate, whilst the sulphuric and phosphoric acids remain in solution. It is filtered, washed with alcohol, the residue dissolved in water and the alumina precipitated by ammonia. The alkaline filtrate is treated with water, evaporated until the alcohol is removed, supersaturated with ammonia (whereby a few millegrammes of a precipitate are produced, arising from a trace of alumina) and precipitated with chloride of calcium. The precipitated liquid should be filtered with exclusion of the air, as it constantly contains sulphate of lime;

it is washed for a short time only, dissolved in muriatic acid, and some alcohol added to the water with which it is diluted. On filtration, the sulphate of lime remains, and ammonia precipitates pure phosphate of lime only.

a. So-called neutral Phosphate of Alumina.—If a solution of alum is mixed with one of ordinary phosphate of soda until the latter produces no further precipitate, the filtered liquid, which, although containing excess of the alkaline precipitant, reacts strongly acid, is free from alumina, but contains a considerable amount of phosphoric acid. It might thus be readily imagined that the decomposition which ensues here is similar to that in the case of silver, where, as we know, $(2NaO, HO)P_2O_5$ throws down $3AgO, P_2O_5$. The precipitate, when carefully washed and dried in the air, forms a white, very loose and hygroscopic powder, which after having been dried in a stove, loses 39·17 per cent. of water at a red heat. When dried *in vacuo* over sulphuric acid, it lost in four experiments 30·63–33·05 per cent. The results of its further analysis were,—

		With silica and sulph. potash.	With silica and carb. soda.	With sulphuric acid and sulph. potash.	
Phosphoric acid	35·07	37·55	41·85	38·62	36·92
Alumina......		26·34	26·14		24·50
Water........	39·17		33·05		

The quantities of oxygen in the alumina and phosphoric acid in all these analyses approximate to the proportion of 3 : 5; the compound is certainly $= Al_2O_3, P_2O_5$, but the quantity of water it contains varies, inasmuch as the salt dried in the air contains 9 eq., that over sulphuric acid sometimes 8, sometimes 7 and 6 eq. It is probable that the latter number of equivalents of water belong to a definite hydrate, and the intermediate grades correspond to a mixture of salts containing 6 and 9 eq. of water.

b. So-called basic Phosphate of Alumina.—To procure this, the previously described salt, whilst still moist, was dissolved in hydrochloric acid and precipitated with ammonia. The precipitate is still more transparent and gelatinous than the former, and is washed with greater difficulty.

	The salt dried in the air yielded—		The salt dried at 212° in a water-bath gave,—		
	With silica and carb. soda.		With silica and carb. soda.	With sulphuric acid and sulph. potash.	
Phosphoric acid	34·57	3 = 36·81		37·04	3 = 38·61
Alumina.........	36·78	4 = 35·34	37·27	37·36	4 = 37·06
Water...(28·92)	29·04	18 = 27·85	23·79		15 = 24·33

The compound is consequently $4Al_2O_3, 3P_2O_5$, and contains when dried in the air 18 eq. of water, at 212° however only 15 eq.

Vauquelin has stated, that when phosphate of alumina is digested with a solution of carbonate of potash, it is converted into pure alumina. In repeating this experiment, allowing the digestion to continue for fourteen days and then boiling the mixture, it was found that a part (one-fourth) only of the acid of the supposed neutral phosphate of alumina was removed during its conversion into the so-called basic salt. Of the methods recommended for analysing an aluminous precipitate, especially after it has been heated to redness, the author found that of Otto, by means of a magnesian salt, the best. It is not however applicable to quantitative analysis, as it always yields too little.

Vivianite.

We have already several analyses of this mineral, which however differ considerably, and do not give its real composition, as all the iron it contains is estimated as protoxide, although both the proto- and peroxide enter into its composition. They are as follows:—

	From the Isle of France (Laugier).	Bodenmais (Vogel).	From St. Agnes in Cornwall (Stromeyer).	From New Jersey. (Thomson).	From Kertsch in Crimea (Segeth).
Phosphoric acid	21	26·4	31·1825	26·06	24·95
Protoxide of iron	45	41·0	41·2266	46·31	48·79
Water	34	31·0	27·4834	27·14	26·26
	100	98·4	99·8934	99·51	100·

The author analysed a variety of vivianite, which was indistinctly crystallized in the form of needles; it came from the Mulica hills in New Jersey (and was probably the same as that analysed by Thomson, and which he called Mullicite);—and also the well known crystalline variety from Bodenmais.

1. *Vivianite from New Jersey.*—*a.* The solution of the mineral in muriatic acid was precipitated by ammonia and hydrosulphuret of ammonia; the sulphuret of iron was dissolved in nitro-muriatic acid and precipitated by ammonia; the phosphoric acid was thrown down from the former solution by chloride of calcium. In a second experiment (*b*) the fossil was heated to redness in dry hydrogen gas, and the water which escaped was estimated.

The muriatic solution was evaporated to dryness, then fused with carbonate of soda, to estimate the oxide of iron. Finally (*c*), a portion of the mineral was boiled with dilute muriatic acid and a weighed portion of copper, until the whole of the iron was reduced to the state of protoxide.

2. *Vivianite from Bodenmais.*—The mineral was at first treated as above with copper, the solution freed from copper by sulphuretted hydrogen, evaporated to dryness, the residue heated to redness with carbonate of soda, washed with water, and thus the oxide of iron and phosphoric acid estimated in the usual way. If we first of all suppose the whole of the iron to exist in the form of peroxide, the analyses yield,—

	From New Jersey.		From Bodenmais.
	a.	*b.*	
Phosphoric acid......	28·40		29·01
Peroxide of iron	49·74	49·36	51·21
Water		27·49	

But the peroxide of iron estimated in *c* amounts in the vivianite of New Jersey barely to 12·06 per cent., in that of Bodenmais to 11·60 per cent., so that the remainder must be considered as protoxide of iron. The mean quantities of these results calculated with this view yield as follows:—28·60 per cent. phosphoric acid, 34·82 per cent. protoxide of iron, 11·91 per cent. peroxide of iron, and 27·49 of water.

Vivianite is isomorphous with cobalt bloom, the composition of which, according to Kersten, is = $3CO, 2AsO_5 + 8HO$. Vivianite is likewise in its origin $3FeO, P_2O_5 + 8HO$; but we ordinarily find it only in pseudomorphous crystals, which nevertheless possess all the characters of genuine crystals, for instance in their perfect cleavage. One portion of the protoxide has become more highly oxidized, and thus the blue colour of the mineral is produced, which is originally white, as is shown by the impure earthy varieties. Exactly the same takes place with the artificial protophosphate of iron, which passes more rapidly into the double salt of the protoxide from its aggregated condition, and which exhibits the composition of vivianite, except that it contains more peroxide. The formula of vivianite should thus clearly illustrate what change has taken place in this oxidation; and this is exactly the case if the mineral contains 3 eq. of peroxide of iron, 18 eq. of protoxide of iron, 8 eq. of phosphoric acid, and 56 eq. of water. The quantity of water is the same in both phosphates, and agrees exactly with that contained in the artificial salt.

Bearing these facts in mind, we arrive at

$$6(3FeO, P_2O_5 + 8HO) + (3Fe_2O_3, 2P_2O_5 + 8HO)$$

as the formula for vivianite; and calculation according to this requires 29·10 per cent. phosphoric acid, 33·00 protoxide of iron, 12·22 peroxide of iron, and 25·68 water. Now as the artificially prepared compound merely differs from vivianite in containing 2 eq. only of the proto-salt, and perhaps this alone yields a blue compound, which also exists in vivianite, its formula may be constructed thus:

$$4(3FeO, P_2O_5 + 8HO) + \\ [2(Fe_3O_3P_2O_5 + 8HO) + (3Fe_2O_3, 2P_2O_5 + 8HO)]$$

Thus we at once see how the perphosphate of iron, $3Fe_2O_3, 2P_2O_5 + 8HO$, is formed from $3FeO\,2P_2O_5 + 8HO$. 2 eq. of the latter have exchanged half the water for 3 eq. of oxygen, for

$$2(3FeO, P_2O_5 + 8HO) = 6Fe\,6O + 2P_2O_5 + 16HO$$
$$3Fe_2O_3, 2P_2O_5 + 8HO = 6Fe\,9O + 2P_2O_5 + 8HO.$$

Phosphate of the Proto- and Peroxide of Iron.

It is well known that phosphate of soda, $(2NaO, HO)P_2O_5$, throws down a white precipitate, which subsequently becomes blue, from a solution of the protosalts of iron. If 1 eq. of phosphate of soda is used to 2 of crystallized protosulphate of iron, the liquid separated from the precipitate is colourless, but strongly acid, whilst the salt of iron has a slightly acid reaction, and the salt of soda itself is powerfully alkaline. It contains a considerable quantity of phosphoric acid, as well as a small quantity of protoxide of iron; hence the precipitate cannot, as has usually been considered, consist of $2FeO, P_2O_5$, but of $3FeO, P_2O_5$, and is thus analogous to the silver salt. The precipitate quickly becomes blue on the filter; when dried in the air it forms a bright blue powder, which is perfectly undistinguishable from blue iron ore. In the water-bath it loses a portion of its water and becomes green. In its analysis, a method exactly similar to that used for vivianite was adopted, and gave the following results:—

	I.	II.		
P_2O_5 ..	32·55	29·85	31·20	4 = 29·83
FeO ..	20·63	21·95	21·29	6 = 22·55
Fe_2O_3 ..	25·58	25·58	25·28	3 = 25·06
HO ..			21·93	24 = 22·56

This compound may be expressed by the formula

$$2(3FeO, P_2O_5 + 8HO) + (3Fe_2O_3, 2P_2O_5 + 8HO).$$

Perphosphate of Iron.

If a neutral solution of a persalt of iron, as ammonio-sulphate of iron, be precipitated with phosphate of soda (2NaO, HO) P_2O_5, the filtered solution has an acid reaction, although it contains an excess of phosphate of soda; hence the precipitate does not correspond in composition to the phosphate of soda, but must be a basic compound. It yielded on analysis,—

P_2O_5	36·50	1 = 38·10
Fe_2O_3	41·90	1 = 42·69
HO	20·30	4 = 19·21

A compound possessing the same composition is obtained by adding phosphate of soda by drops to a solution of protosulphate of iron, until the precipitate ceases to be redissolved. The solution when filtered becomes turbid by exposure to the air and deposits a yellowish-white granular precipitate, which when washed is identical with the above salt.

Basic Perphosphate of Iron.

By the action of alkalies a portion of the acid is removed from the perphosphate of iron, and two basic salts are formed. A. If the salt be dissolved in a small quantity of muriatic acid, excess of ammonia be then added and heat applied, a bright brown basic salt is formed. Its formula is $3Fe_2O_3, 2P_2O_5 + 16HO$, or $2(Fe_2O_3, P_2O_5 + 5HO) + Fe_2O_3\ 6HO$. B. If the perphosphate of iron formed in this manner, or by precipitation, be digested for a considerable time with dilute solution of potash, the whole of the phosphoric acid is not removed from the oxide of iron. This substance is a very basic salt of definite composition, which suffers no further decomposition from an excess of alkali. It consists of—

P_2O_5.....	5·71	1 = 5·68
Fe_2O_3.....	94·29	19 = 94·32

and is consequently $15Fe_2O_3, P_2O_5$.

Phosphate of Lime.

In the above experiments it very frequently happened that a liquid rendered acid by muriatic acid, and containing soda as well as phosphoric acid, was digested for a considerable time with a mixture of chloride of calcium and ammonia in excess, for the purpose of precipitating the phosphoric acid. The precipitate was heated to redness, dissolved in hydrochloric acid, and decomposed by sulphuric acid and alcohol. The phosphoric acid in an average of fourteen experiments amounted to 45·71 per cent., consequently the compound consisted of $3CaO, P_2O_5$. If, however, a pure solution of $2NaO, P_2O_5$ be precipitated by a mixture of chloride of calcium and ammonia, we obtain $2CaO, P_2O_5$. According to Berzelius, the same precipitate is formed by adding the salt of soda to the solution of chloride of calcium by drops. If, however, the method of proceeding be reversed, and the addition of the precipitant be continued as long as the liquid has an alkaline reaction, or the lime salt be added to a solution of phosphate of ammonia containing free ammonia, $8CaO, 3P_2O_5$ is formed. $2CaO, P_2O_5$ is crystalline, both the basic salts are gelatinous, and $5CaO, P_2O_5$ is mucous, resembling in this respect precipitated fluoride of calcium.—*Poggend. Annal.*, vol. lxiv. p. 251–271, and 405–425.

ANALYSIS OF TAURINE.

Professor Redtenbacher of Vienna, formerly a pupil of Liebig, has subjected taurine (the asparagine biliaire of Gmelin) to a new analysis, and has found it to contain 26 per cent. of sulphur; this substance, on account of the regularity of its crystalline form, is one of the most beautiful presented by organic chemistry. The chemists who have analysed taurine, overlooked the presence of sulphur, and gave as its formula, $C^4H^7NO^{10}$.—*Journ. de Pharm. et de Ch.*, Juin 1845.

DOUBLE SALT OF [BI]CHLORIDE OF MERCURY AND ACETATE OF COPPER.

M. Wöhler formed this double salt by mixing cold saturated solutions of its component salts.

The compound is gradually deposited in concentric hemispheric crystals of a deep blue colour and great beauty. It is nearly insoluble in cold water; in boiling water it is converted into a bright green powder, and bichloride of mercury remains in solution.

According to repeated analyses performed by M. Hütteroth, and which agreed satisfactorily, the blue compound is formed of two equivalents of chloride and one equivalent of anhydrous basic acetate of copper, or in 100 parts, of—

[Bi]chloride of mercury	67·65
Acetate of copper	32·35
	100·

Journ. de Pharm. et de Ch., Juin 1845.

RAIN DURING THE PRESENT YEAR.

Mr. Luke Howard has furnished us with the following monthly amounts of rain for this year up to the end of August inclusive:—

January	0·83	4·69 first four months.
February	0·65	
March	1·85	
April	1·36	
May........	2·00	13·40 last four months.
June	3·09	
July........	3·95	
August......	4·36	18·09

18·09 in the eight months,

the usual average of a year being 26 inches.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1845.

Chiswick.—August 1. Fine: cloudy: overcast. 2. Very heavy rain. 3. Cloudy: showery; partially overcast. 4. Cloudy: fine: clear. 5. Showery. 6. Very fine. 7. Rain: thunder-showers in afternoon: clear. 8. Fine. 9. Rain: boisterous, with rain. 10. Overcast. 11. Heavy showers: thunder. 12. Overcast. 13. Slight rain: cloudy: showers. 14. Densely clouded. 15. Cloudy: clear and fine. 16, 17. Cloudy. 18. Very fine: rain. 19. Foggy: heavy rain. 20. Very clear: cloudy. 21. Very fine. 22. Cloudless and very fine. 23. Overcast: rain at night. 24. Very fine. 25. Foggy: very fine: rain at night. 26, 27. Fine. 28. Clear: cloudy. 29, 30. Bright sun and very fine. 31. Slight haze: very fine: overcast.—Mean temperature of the month 2° below the average.

Boston.—Aug. 1. Windy: rain yesterday (rain P.M.). 2. Fine: rain A.M. and P.M. 3. Cloudy. 4. Rain. 5. Cloudy: rain A.M. and P.M. 6. Cloudy. 7. Cloudy: hail and rain, with thunder. 8. Cloudy. 9. Cloudy: rain early A.M. 10. Cloudy: rain early A.M.: rain A.M. 11. Cloudy: rain P.M. 12. Cloudy: rain early A.M. 13. Cloudy. 14. Cloudy: rain A.M. and P.M. 15. Cloudy: rain early A.M.: rain P.M. 16—18. Cloudy. 19. Rain: stormy P.M.: rain A.M. and P.M. 20. Stormy. 21. Fine: rain P.M. 22. Fine. 23. Fine: rain P.M. 24. Fine. 25. Fine: rain P.M. 26. Fine: rain P.M., with thunder and lightning. 27. Fine. 28. Cloudy. 29, 30. Foggy. 31. Fine.—Not so much rain in one month since August 1831, nor so cold an August since August 1833.

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

Applegarth Manse, Dumfries-shire.—Aug. 1—3. Showery. 4. Rain heavy A.M. 5, 6. Slight showers. 7. Fine and fair: a few drops. 8. Fine and fair. 9. Rain all day. 10. Slight shower. 11. Fine: a few drops. 12—16. Fine. 17. Rain from 11 A.M. 18. Shower. 19. Showers. 20. Fair, but cloudy. 21. Fair and fine. 22. Fair, but cloudy: stormy and rain P.M. 23. Heavy showers. 24. Fine after one shower A.M. 25. Heavy rain: thunder. 26. Fine: one shower. 27. Fair and fine. 28. Very fine and clear. 29. Fine, but cloudy. 30, 31. Very fine.

Mean temperature of the month .... 56°·4
Mean temperature of Aug. 1844 .... 54 ·6
Mean temperature for 23 years .... 57 ·0

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. Aug.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	29·732	29·578	29·14	29·35	29·38	29·63	29·53	68	52	61	64	51½	59	52	sw.	sw.	se.	e.	·06	·08	......	......
2.	29·518	29·340	29·00	29·35	29·29	29·51	29·47	64	48	64	63	49	57	55	sw.	calm	se.	e.	·61	·03	......	......
● 3.	29·764	29·630	29·07	29·21	29·32	29·29	29·26	69	54	59	58	51	53	51½	sw.	w.	se.	s.	·10	·37	......	·42
4.	29·694	29·681	29·17	29·42	29·52	29·26	29·50	71	55	60	63	54	54	53	sw.	w.	se.	w.	......	·02	......	·30
5.	29·672	29·597	29·20	29·58	29·65	29·67	29·78	75	55	63	66	46½	54	55	s.	calm	s.	......	·05	·38	......	·13
6.	29·824	29·778	29·27	29·66	29·60	29·75	29·72	73	50	59	64½	48½	56½	53½	w.	w.	ssw.	e.	·12	·77	......	......
7.	29·786	29·728	29·17	29·59	29·62	29·72	29·74	69	47	64	67	52	57	54	sw.	w.	wnw.	n.	·24	......	......	......
8.	29·831	29·820	29·29	29·66	29·61	29·68	29·60	71	55	61·5	64	53½	52	52½	sw.	w.	wnw.	nnw.	·26	·20	1·10	·62
9.	29·628	29·427	28·97	29·40	29·35	29·48	29·64	68	52	61·5	58½	52½	58	52½	sw.	sw.	w.	e.	·01	·35	......	·35
☽ 10.	29·577	29·526	28·92	29·35	29·48	29·73	29·76	68	53	57	63	52	57	53	sw.	sw.	n.	nne.	·05	·58	......	......
11.	29·760	29·623	29·10	29·58	29·72	29·84	29·93	70	53	60	67	50½	53½	50½	sw.	nw.	nne.	n.	·36	·17	......	·11
12.	29·929	29·826	29·35	29·80	29·89	29·96	30·00	63	49	56	62	53½	51½	50	nw.	nw.	nne.	nw.	·01	·53	......	......
13.	30·036	30·003	29·50	29·92	29·91	29·98	29·95	66	50	56	63	48	52½	52½	nw.	nw.	nw.	wnw.	·07	......	......	......
14.	29·952	29·819	29·40	29·83	29·73	29·88	29·85	62	52	56	63½	51½	49	49	nw.	calm	wnw.	wnw.	......	......	......	·14
15.	29·800	29·728	29·26	29·72	29·80	29·90	29·95	62	44	56	62	48½	52	48½	nw.	nw.	n.	n.	......	·10	......	·06
16.	29·911	29·837	29·48	29·82	29·80	29·93	29·89	61	48	54·5	60	44	49	48	nw.	nw.	n.	nw.	......	·06	0·28	......
○ 17.	29·901	29·727	29·44	29·75	29·57	29·80	29·73	69	43	57	56½	41	52	50½	sw.	calm	sw.	nne.	......	......	......	......
18.	29·762	29·594	29·22	29·51	29·50	29·66	29·65	71	52	57·5	64	53½	52½	52	sw.	w.	sw.	ese.	·28	......	......	......
19.	29·371	29·111	28·94	29·39	29·28	29·60	29·53	67	49	58	59	49½	51½	51	sw.	calm	sse.	ne.	·42	·02	......	......
20.	29·765	29·524	28·90	29·20	29·44	29·42	29·53	66	45	58	56½	50½	53	50	sw.	w.	w.	n.	......	·84	......	·38
21.	30·060	29·894	29·30	29·68	29·86	29·73	29·88	68	39	58	59½	46½	50½	49	w.	w.	w.	nw.	·04	......	......	......
22.	30·207	30·184	29·65	29·98	29·88	29·95	29·76	72	49	58	62	41	55	53½	w.	w.	sw.	se.	......	·09	......	......
23.	30·139	30·004	29·62	29·71	29·70	29·60	29·63	70	50	58	64	54	58½	52	sw.	w.	ssw.	wsw.	·07	......	......	·21
☾ 24.	29·979	29·975	29·44	29·78	29·81	29·80	29·85	72	44	61	62½	46½	51	47	w.	w.	sw.	se.	......	·09	0·38	·05
25.	30·004	29·824	29·45	29·67	29·50	29·69	29·57	73	50	59·5	59½	53	54	52½	sw.	w.	e & s.	sw.	·04	......	......	·08
26.	30·020	29·850	29·30	29·62	29·80	29·49	29·79	71	46	59	62½	51½	53	53	w.	w.	w.	n.	......	·09	......	·74
27.	30·182	30·080	29·60	30·03	30·16	30·12	30·22	71	51	59	65	49	53½	50	nw.	w.	w.	s.	......	·08	......	·36
28.	30·296	30·217	29·81	30·20	30·22	30·21	30·25	66	47	58	67½	47½	61	55	ne.	calm	nw.	wsw.	......	......	......	......
29.	30·303	30·271	29·90	30·25	30·27	30·27	30·27	69	42	52·5	68½	55½	61	57	ne.	calm	w.	sw.	......	......	......	......
30.	30·295	30·262	29·90	30·30	30·29	30·21	30·25	72	47	56	70½	56	57	55	ne.	calm	w.	w.	......	......	1·33	·18
31.	30·288	30·266	29·83	30·28	30·28	30·32	30·34	75	54	63	73	54	57	53	ne.	calm	w.	w.	......	......	......	......
Mean.	29·902	29·797	29·34	29·696	29·620	29·776	29·800	69·42	49·19	58·7	63·2	50·1	54·37	51·96					2·79	4·85	3·09	4·13

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AND
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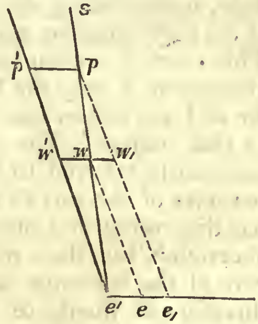
[THIRD SERIES.]

NOVEMBER 1845.

XLIX. *A Theoretical Explanation of the Aberration of Light. By the* Rev. J. CHALLIS, *M.A., Plumian Professor of Astronomy in the University of Cambridge**.

AT the meeting of the British Association held at Cambridge last June, I brought forward a theory of the aberration of light, the same in principle as that which forms the subject of this communication, but requiring some elucidation with respect to its applicability on the undulatory hypothesis of light. My present object is to show how it applies on that hypothesis, and for this purpose I proceed to state, first, the general principle of the theory.

Fig. 1.



Let s and w (fig. 1) be simultaneous positions of two visible objects, one of which, s, is fixed in space while the other is carried by the earth's motion with the eye of the spectator. For instance, s may be a star, and w the wire of a telescope. Let the straight line joining the positions s and w be produced to meet the straight line $e_{,}\,e\,e'$, in which the spectator's eye is moving in the point e'. Take $e'e$ to $e'w$ in the ratio of the earth's velocity to the velocity of light; and let e and w be simultaneous positions of the eye and the object w. Now the light which started from w in the direction we' at the instant the eye was at e, arrives, in company with light from s, at e' when the eye comes to the same point; and the eye receives the impression that w and s are in the *same* direction, because it receives light from each object proceeding in the common

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* Communicated by the Author.

direction swe'. But at this instant the object w is at w' (ww' being taken equal and parallel to ee'), and the directions of the objects from e' are really separated by the angle $se'w'$.

Let us now consider two objects which both partake of the earth's motion, and let p, $w_\prime$ and $e_\prime$ be simultaneous positions of the objects and the spectator's eye in the same straight line. Draw pp' and $w_\prime w'$ parallel and equal to $e_\prime e'$, and join $e'w'p'$. Then, since $w_\prime w$ is to wp in the ratio of the earth's velocity to the velocity of light, it follows that the object at $w_\prime$ and the light which started from p in the direction pw when the eye was at $e_\prime$, arrive at w at the same instant. Hence the eye at e' receives at the same time light from p and w coming in the common direction pwe', and consequently sees the two objects in the same direction when they are really at p' and w', and therefore really in the same direction from e'.

It thus appears to be a necessary consequence of the earth's motion and the temporaneous and rectilinear transmission of light, without making any hypothesis about the nature of light or the manner in which the eye receives impressions, that the directions of two objects, one of which partakes of the earth's motion and the other is fixed in space, are separated by a certain angle when they appear to be coincident, while two objects, both of which partake of the earth's motion, are really in the same direction when they are seen in the same direction. The angle of separation, it is plain from the figure, is equal to the ratio of the earth's velocity to the velocity of light, multiplied by the sine of the angle which the direction of the earth's motion makes with that in which the light comes. This result is a complete explanation of the phænomenon of aberration, if only the following remark be added, which, as far as I am aware, has not been before made with reference to this subject. The visual direction of a celestial object is necessarily referred to the visual direction of an object which partakes of the earth's motion, and astronomical observation has discovered that these directions are relatively affected by aberration, but does not determine whether the star or the wire of the telescope is seen out of its true place. We are therefore at liberty to suppose, as the foregoing theory requires, that the apparent place of the *wire* is affected by aberration. It is clear that observations of terrestrial objects alone could not detect aberration (its maximum amount being very small), simply for the reason that two objects partaking of the earth's motion are really in the same direction when they appear to be so, though they may not be seen in their true direction. On this account geodetical observations are unaffected by the aberration of light.

It will be seen that in the above explanation it is assumed that light travels from the object to the eye in a straight line. According to the emission theory of light such is the case, and if this view of the nature of light be adopted, nothing further need be said. The same would be true on the undulatory hypothesis, if we might suppose the æther to be absolutely at rest. But it is impossible to conceive that the earth can move through space without communicating some motion to the æther which surrounds and pervades it. It is therefore necessary, if we adopt the undulatory theory, to inquire how far the preceding explanation will be modified by taking account of the motion of the æther. And here I may remark, that the theory I am explaining is widely different from that which Mr. Stokes has proposed in the July number of this Journal. According to Mr. Stokes's views, the phænomenon of aberration is entirely owing to the motion which the earth impresses on the æther, and which at the earth's surface he supposes to be equal to the earth's motion. On the contrary, I have to show that the amount of aberration will be the same *whatever* be the motion of the æther, and if this cannot be shown, the undulatory theory, and not the foregoing explanation, is at fault. I am, however, so fully persuaded of the truth of the undulatory theory, that I have no doubt this proposition admits of proof, and the following I consider to be satisfactory.

Let, as before, e and w (fig. 2) be simultaneous positions of the eye of the spectator and a terrestrial object, as the wire of a telescope, and let luminous waves from a distant fixed object, as a star, proceed in the direction $s w e'$, accompanied, after reaching w, by waves from w, so that the eye at e' receives the

Fig. 2.

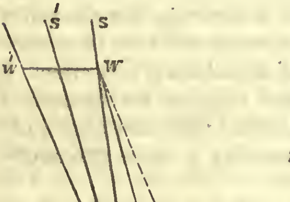
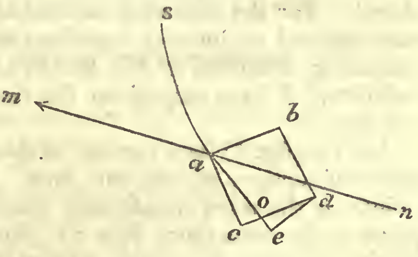


Fig. 3.



impression that w and s are in the same direction. Suppose the æther which is situated between e' and w to be moving with a uniform velocity, and let $n\,e'$ represent in magnitude and direction the space through which it is carried while light travels from w to e'. The line $n\,e'$ may be of any magnitude less than $e'\,e$, and in any direction not necessarily in the plane $e\,w\,w'\,e'$. Join $w\,n$. Now the motion of the luminous waves from w to e' is compounded of two motions represented by $w\,n$ and $n\,e'$, the former of which is due to the motion of propagation of the wave through the æther, and the other to the motion of the æther itself. It is well known that the motion of propagation is in the direction of a normal to the front of the wave, and that the front of a given wave continues parallel to a given plane so long as it is propagated through fluid at rest, or through fluid moving with a *uniform velocity in a given direction.* Also the normal to the front of a wave is in the direction of vision. Hence if $e'\,s'$ be drawn parallel to $n\,w$, this will be the common direction in which the objects w and s will be seen. Consequently, if the front of the wave retained its parallelism the whole distance from the object to the eye, the *true* direction of the celestial object s from e' would be $e'\,s'$, when the true direction of the terrestrial object w from e' is $e'\,w'$. The aberration would consequently be an angle $s'\,e'\,w'$, different from the angle $w\,e'\,w'$, which is known from observation to be the actual angle of aberration.

It is not, however, true, as we have supposed above, that the front of a wave continues parallel to itself in passing through the æther put in motion by the earth's motion. For evidently the wave is propagated through portions of the æther moving with *different velocities in different directions*, and the effect this circumstance has in altering the direction of the normal to the front of the wave must now be considered. For the following method of calculating this effect, I am indebted to the very ingenious and original mathematical reasoning contained in Mr. Stokes's communication above alluded to. I have only given the reasoning a more geometrical form.

Let $s\,a$ (fig. 3) be a portion of the path in space of a given point of a wave of constant form, and let a and b be two points of the wave indefinitely near each other in the same phase at the same time. Join $a\,b$. Since the velocity impressed on the æther by the earth's motion is very small compared to the velocity of light, $a\,b$ must be nearly perpendicular to $s\,a$. Let $V_{\prime}$ and $V_{\prime}'$ represent the velocity of the æther at a and b, so far as it is due to the earth's motion, and let that at a take place in the direction $n\,a\,m$. Also let V represent the uniform velocity of propagation of the wave through the æther. Then, since the point a with regard to the direction of motion of the æther is *in advance* of the point b, the velocity of the æther is *less* at a than at b. For we are here considering a position in that part of the æther which, with regard to the direction of the earth's motion, is in advance of the earth's centre; and it is plain that if we trace at any time a line of motion in that part, beginning at the earth's surface and proceeding in the direction of the motion of the particles through which it passes, the velocity will be less the further we advance along this line. Let the velocities $V_{\prime}$ and $V_{\prime}'$ continue uniform during the small time δt, and let the small straight lines $a\,e$, $b\,d$ be respectively the spaces through which the points a and b of the waves are carried by the composition of these velocities with the uniform velocity V. Join $d\,e$. Draw $a\,c$ equal and parallel to $b\,d$ and join $c\,d$. It is easy to see that as the motion of propagation is less opposed by the motion of the æther at a than at b, $a\,e$ is greater than $b\,d$, and inclined *towards* $b\,d$. The general effect of the æther's motion is, therefore, to throw the normal to the front of the wave more and more in the direction towards which the æther is moving; and this effect would be similarly found to take place if we considered a position on the other side of the earth's centre.

I proceed now to calculate the amount of deviation of the normal, and to determine the planes in which it takes place. For this purpose let us resolve the velocity $V_{\prime}$ into w along $a\,c$, u along $a\,b$, which we may suppose to be in a given direction perpendicular to $a\,c$, and v along a straight line through a perpendicular to $a\,b$ and $a\,c$, and let us first consider the effect of u and w, abstracting from v. The point e will thus be in the same plane as $a\,b\,d\,c$, and if u' be the resolved part of $V_{\prime}'$ in $a\,b$, we have $c\,d - d\,e$, which is very nearly $c\,o$, equal to $(u'-u)\,\delta t$. Hence if $a\,c = \delta s$, it follows that the angle $c\,a\,o = \dfrac{(u'-u)\,\delta t}{\delta s}$. But $V = \dfrac{\delta s}{\delta t}$ very nearly, neglecting the ratio of $V_{\prime}$ to V. Hence the angle $c\,a\,o = \dfrac{u'-u}{V}$, which is the angle of deviation of the normal in the plane $c\,a\,b$. The sum of all such angles for the whole course of the wave may be found by taking, since δs is perfectly arbitrary, $a\,c$ of such a length that u' at c is the same as u' at b; or, which is the same thing, supposing $u' - u = \dfrac{d\,u}{d\,s}\,d\,s$. Hence if $u_{\prime}$ be the value of u at the earth's surface, and u_0 at any distant point of the course, the whole deviation in the plane of u and $w = \dfrac{u_{\prime} - u_0}{V}$. At a distance much less than that of the moon from the earth, u_0 must be quite insensible. Hence for any celestial body the deviation $= \frac{u_{\prime}}{\mathrm{V}}$. So the deviation of the normal in the plane of v and w is $\frac{v_{\prime}}{\mathrm{V}}$. The resulting deviation is therefore $\frac{\sqrt{u_{\prime}^2 + v_{\prime}^2}}{\mathrm{V}}$ in a plane containing the direction of the motion of the æther at the earth's surface and the direction of the course of the wave; that is, in fig. 3, the plane $n\,a\,c$, and in fig. 2 the plane $w\,e'\,n$. Now since $n\,e'$ in fig. 2 represents the velocity of the æther at the earth's surface, $e'f$ drawn perpendicular to $w\,n$ represents $\sqrt{u_{\prime}^2 + v_{\prime}^2}$, so that the angle $e'\,w\,f$, which is the same as $s\,e'\,s'$, is equal to $\frac{\sqrt{u_{\prime}^2 + v_{\prime}^2}}{\mathrm{V}}$. This then is the *aberration* arising from the different states of motion of the parts of the æther through which the wave is propagated, in consequence of which, when the normal to the front of the wave is in the direction $e'\,s'$, the object is really in the direction $e'\,s$. Thus the actual angle of separation between the directions of the two objects s and w is $s\,e'\,w'$ when they are *seen* in the same direction $e'\,s'$. This angle is the same that we found without considering the motion of the æther, and it therefore follows that the amount and law of aberration are the same whatever motion the earth impresses on the æther.

The foregoing mathematical reasoning does not appear in any respect wanting in generality, and is equally true whether $u\,dx + v\,dy + w\,dz$ be or be not an exact differential. It may, however, be remarked, that as the angle $c\,a\,o$ was found to be equal to $\frac{du}{ds}\delta t$, so we might find the angle $e\,d\,o$ equal to $\frac{dw}{dx}\delta t$, the axis of x being supposed parallel to $a\,b$. But if the form of the wave alters in no respect, the angle $e\,d\,o$ is equal to the angle $c\,a\,o$, and consequently $\frac{du}{ds} = \frac{dw}{dx}$. So $\frac{dv}{ds} = \frac{dw}{dy}$. It would seem therefore that the motion of the æther must be such as to satisfy these equations, at least approximately, the reasoning being only approximate. Now it happens that whenever the motion of an elastic fluid is such that the terms involving the squares of the velocity may be neglected (and the case before us is one of this kind), we have for determining the motion the approximate equations following:

$$\frac{d\,\mathrm{P}}{dx} = \mathrm{X} - \frac{du}{dt}; \quad \frac{d\,\mathrm{P}}{dy} = \mathrm{Y} - \frac{dv}{dt}; \quad \frac{d\,\mathrm{P}}{dz} = \mathrm{Z} - \frac{dw}{dt}.$$

Hence, supposing no force impressed, we obtain approximately,

$$\frac{d\,.}{dt}\left(\frac{du}{dy} - \frac{dv}{dx}\right) = 0; \quad \frac{d\,.}{dt}\left(\frac{du}{dz} - \frac{dw}{dx}\right) = 0;$$

$$\frac{d\,.}{dt}\left(\frac{dv}{dz} - \frac{dw}{dy}\right) = 0.$$

These equations are true, not because the functions in brackets do not contain t, but because approximately $\frac{du}{dy} = \frac{dv}{dx}$, $\frac{du}{dz} = \frac{dw}{dx}$, and $\frac{dv}{dz} = \frac{dw}{dy}$. Thus the corresponding equations above are accounted for without affecting the generality of the previous reasoning.

Cambridge Observatory, Sept. 29, 1845.

L. *On the Allotropism of Chlorine as connected with the Theory of Substitutions.* By John William Draper, *M.D., Professor of Chemistry in the University of New York**.

[With a Plate.]

THE researches of M. Dumas on chemical types have shown that between chlorine and hydrogen remarkable relations exist, indicating that the electrical characters of elementary atoms are not essential, but rather incidental properties.

The extension of these researches has given much weight to the opinion that the electro-chemical theory may be regarded as failing to account for the replacement of such a body as hydrogen, by chlorine, bromine, oxygen, &c.

I do not know that as yet any direct evidence has been offered that the electrical character of an atom is not an essential quality, but one that changes with circumstances. It appears to be rather a matter of inference than of absolute demonstration.

It is the object of this memoir to furnish such direct evidence, and to show that chlorine, the substance which has given rise to the discussions connected with the theory of substitutions, under the very circumstances contemplated, has its electro-chemical relations changed.

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* Communicated by the Author.

More than two years ago I brought before the British Association some of the facts. They were subsequently published in the Philosophical Magazine, in a memoir on "Tithonized Chlorine," July 1844). The connexion of these experiments with the discussion between the theory of substitutions and the electro-chemical theory is obvious.

Very recently M. Berzelius has published an important paper on the Allotropism of Simple Bodies *, the object of which is to point out that many of those bodies can assume different qualities by being subjected to certain modes of treatment. Thus carbon furnishes three forms—charcoal, plumbago and diamond.

To a certain extent these views coincide with those which have offered themselves to me from the study of the properties of chlorine. They are not, however, altogether the same. M. Berzelius infers that elementary bodies can, as has been said, assume under varying circumstances different qualities. The idea which it is attempted to communicate in this memoir is simply this; that a given substance, such as chlorine, can pass from a state of high activity, in which it possesses all its well-known properties, to a state of complete inactivity, in which even its most energetic affinities disappear; and that between these extremes there are innumerable intermediate points. Between the two views there is therefore this essential difference; from the former it does not appear what the nature of the newly-assumed properties may be; from the latter they must obviously be of the same character, and differ only in intensity or degree, diminishing from stage to stage until complete inactivity results.

In the case of chlorine, the same activity which is communicated by the indigo rays can also be communicated by a high temperature, or by the action of platina. The term "tithonized chlorine," which I formerly used, is therefore too restricted, and indeed in this view of the case improper. The simple appellations active and passive, are perhaps the best, and I shall therefore employ them.

The points which this memoir is intended to establish are,—

I. That chlorine gas can exist under two forms. In the same way that metallic iron can exist as active or passive iron, chlorine can assume the active or passive state.

II. Having established the fact of the allotropism of chlorine, I shall then show its connexion with the Theory of Substitutions of M. Dumas, and how the most remarkable points in that theory may be easily accounted for.

The time perhaps has not yet arrived for offering a com-

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* A translation of this paper was published in Taylor's Scientific Memoirs, part xiv., 1845.—Ed.

plete mechanical explanation of the assumption of an active or passive state. It may be remarked, that a very trivial modification of our admitted views of the relation between atoms and their properties is all that is required to give a consistent explanation of every one of these facts. Instead of regarding the specific qualities of an atom as appertaining equally to the whole of it in the aggregate, we have merely to assume that there is a relation between its properties and its sides; and that any force which can make it change its position upon its axis will throw it from the active to the passive state. But this is nothing more than the well-known idea of the polarity of atoms.

Phænomena of the Decomposition of Water by Chlorine in the Rays of the Sun.

From the various facts which might be employed, as offering the means of establishing the allotropism of chlorine, I shall select those which arise from an examination of the phænomena of the decomposition of an aqueous solution of chlorine by the rays of the sun.

For many years it has been known that an aqueous solution of chlorine undergoes decomposition by the aid of the solar rays. Several of the most remarkable phænomena connected with this decomposition appear to have been overlooked. Among such may be mentioned the singular fact, that chlorine, which has thus been influenced by the sun, has obtained the quality of effecting this decomposition subsequently, to a measured extent, even in the dark. Not to anticipate what I shall have to offer on this point, I shall now proceed in the first place to establish the various facts connected with the decomposition in question.

Having provided a number of small glass vessels, consisting of a bulb and neck, of the capacity of from 1·5 to 2·0 cubic inches, I filled them with a solution of chlorine in recently boiled water, and inverted them in small glass bottles containing the same solution, as shown in Plate IV. fig. 1. With these bulbs the following experiments were made:—

I. An aqueous solution of chlorine does not decompose in the dark.

One of the bulbs was shut up in a dark closet, and kept there for a week, being examined from time to time. No decomposition was perceptible, for no gas collected in the upper part of the bulb.

II. A solution of chlorine decomposes in the light.

One of the bulbs was placed in a beam of the sun reflected into the room by a heliostat. For sixteen minutes no change was perceptible, then small bubbles of gas made their appearance; they increased in quantity for a time, but finally the speed of decomposition became uniform. On analysis by detonation with hydrogen, after washing out any chlorine contained in it, this gas was found to contain 97 per cent. of oxygen.

III. The rapidity of this decomposition depends on the quantity of the rays and on the temperature.

In various repetitions of these experiments on different days, I soon convinced myself that the rate of evolution of oxygen depended on the quantity of the rays. Among other proofs I may mention this: after ascertaining the rate of decomposition in the reflected beam, if the bulb be set in the direct sunshine, the bubbles increase in number, the total quantity of oxygen evolved becoming greater in the same space of time; an effect obviously due to the difference of intensity of the reflected and incident beams. When a certain point is gained, apparently no further increase of effect takes place on increasing the brilliancy of the light, as I found by employing a convex lens.

With respect to the influence of temperature.—If, while one of the bulbs is actively evolving gas in the sun's rays, it be warmed by the application of a spirit-lamp, the amount of gas thrown off becomes very much greater: a difference of a few degrees produces a striking effect. As an illustration of this I placed in the sunshine two bulbs which were nearly alike, except that one of them was painted black with India ink on that portion which was furthest from the sun. The rays coming through the transparent part had access to the solution, and then impinging on the dark side raised its temperature. On measuring the quantity of gas collected, it was found,—

In the transparent bulb . . .	3·46
In the half-blackened bulb . .	6·19

IV. The decomposition of water, once begun in the sunbeams, goes on afterwards in the dark.

1st. This very important fact may be established in a variety of ways. Thus, if a bulb be removed from the sunshine whilst it is actively evolving gas, and be placed in the dark after the gas has been turned out of it, a slow evolution continuously goes on; the gas collecting in the upper part of the bulb.

2nd. A bulb A, fig. 2, having a neck *b*, the end of which was bent at *c* upwards at an angle of about 45°, was employed. After exposure to the sun, by inverting the bulb and with one finger closing the extremity *c*, the gas disengaged could be transferred to a graduated vessel and measured. I satisfied myself, by several variations of this arrangement, that the small quantity of water introduced from time to time when the gas bubble escaped from the end of the tube *c*, exerted no essential influence on the phænomenon. The following Table shows the amount of gas evolved in the dark during the periods indicated.

The bulb having been exposed to the sunshine, in ten minutes the evolution of gas commenced, and in an hour ·107 cubic inch having collected, this was cast away and the arrangement placed in the dark. To prevent the undue escape of the chlorine, a piece of flat glass, *d*, was laid on the open end of the tube *c*. In each successive hour the quantity of gas given in the following Table was then evolved:—

1st hour . . .	·0162
2nd	·0159
3rd	·0086
4th	·0060
5th	·0038
6th	·0031

and for four days afterwards gas was collecting in the bulb in diminished quantities.

V. This evolution of gas in the dark is not merely a gradual escape of oxygen originally formed whilst the solution was exposed to the sun, but is traceable to an influence continuously exerted by the chlorine, arising in properties it has acquired during its exposure to the rays.

If a bulb which has been exposed to the sun be raised by a spirit-lamp to such a temperature that its gaseous constituents are rapidly evolved, its extremity dipping beneath some of the same solution in the bottle, after allowing a sufficient space of time for the disengaged chlorine to be re-dissolved, and the oxygen be turned out of the bulb, it will be found, on keeping the arrangement in the dark, that oxygen will slowly disengage as before.

Now there is every reason to believe that any small amount of oxygen dissolved in the liquid would be expelled with the chlorine at a high temperature. We therefore have to infer that the chlorine, after this treatment, still retains the quality of causing the decomposition steadily to go forward.

The oxygen which thus accumulates in the course of time in the dark, after an exposure to the sun, does not arise from any portion of that gas held in a state of temporary solution, nor from peroxide of hydrogen, nor from chlorous acid in the liquid undergoing gradual decomposition. From any of these states a high temperature would disengage it.

VI. The evolution of gas is not of the nature of a fermentation, for when it once sets in the molecular motion is not propagated from particle to particle, but only affects those which were originally exposed to the rays.

Let a bulb be filled with chlorine-water which has been exposed to the sun, and in a second bulb place a quantity of the same liquid equal to about one-third of its capacity. Fill the remaining two-thirds with chlorine water which has been made and kept in the dark, and after keeping both bulbs in obscurity for some days, measure the volumes of gas they contain. If the qualities of chlorine which has been changed by exposure were communicable by contact or close proximity from atom to atom, we might expect that both the bulbs would yield the same quantity of gas, but this is very far from being the case; and in such an experiment I found that the bulb containing the mixture gave only one-fourteenth of the gas contained in the other.

VII. The quantity of gas which thus collects in the dark, depends on the intensity of the original disturbance, which in its turn depends on the time of exposure to the rays, to their intensity, and other such conditions. In other words, the rays are perfectly definite in their action, a long exposure giving a larger amount of subsequent decomposition, and a short exposure a lesser amount.

On exposing a bulb filled with chlorine-water to the rays until bubbles of gas began to appear, and a second one until the decomposition had been actively going on for a quarter of an hour, and then transferring both to the dark, and measuring the oxygen which collected at the end of a day, I found in the former one-twelfth of what was contained in the latter.

VIII. In a given quantity of chlorine-water, the decomposition in the dark corresponding to a given exposure to the light having been performed, and the proper quantity of oxygen evolved, and the phænomenon ended, it can be re-established from time to time as long as any chlorine is found in the liquid, by a renewed exposure to the sun.

In a glass vessel, like fig. 3, which indeed was nothing more than one of Liebig's drying apparatus, I placed a sufficient quantity of chlorine-water to fill the larger vessel, and also the vertical tubes half-full. After exposing this to the light for a certain time, until decomposition had fairly set in, I placed it in the dark, and found that for several days it gave off gas, the quantity continually diminishing. Finally no more gas was evolved; but the liquid still contained free chlorine, as was shown by its colour. I therefore again exposed it to the sun, and repeating the former observation, found that it evolved gas for several days in the dark. A third exposure was followed by the same result.

The form of this vessel renders it very convenient for these experiments, because when sufficient gas has collected for the purpose of observation, it is easily removed by inclining the instrument, without the necessity of introducing fresh quantities of liquid.

Having found, as has been said, that the rapidity of the decomposition depended to a certain extent on the temperature, it seemed desirable to determine whether heat alone could bring about the change.

IX. The decomposition of water by chlorine is not brought about by mere elevation of temperature when the liquid is set in the sunbeam; although heat accelerates, it does not give rise to the phænomenon.

1st. I raised, by a spirit-lamp, the temperature of one of the bulbs nearly to its boiling-point, or until so much gas was given off that the liquid was expelled from the tube to the bottle beneath. If, at this temperature, which probably was higher than 200° Fahr., chlorine had been able to decompose water, an equivalent quantity of oxygen would have been produced; but on allowing the apparatus to cool, all the gas was re-absorbed, with the exception of a small bubble amounting in volume to $\frac{1}{1087}$ of the water. This bubble, which was left after the chlorine was recondensed, I found in three different experiments contained 32, 33, and 36 per cent. of oxygen, the remainder being nitrogen; but this being nearly the constitution of the gas which is dissolved in ordinary water, the source from which the small bubble came was inferred to be the water used in these experiments.

2nd. One of the bulbs was painted black all over with India ink; its temperature now rose much higher than in former experiments when it was set in the sun, but not a bubble of oxygen appeared.

X. When chlorine-water has been exposed to the sun, the oxygen accumulated in it is readily expelled by raising the temperature.

Having exposed one of the bulbs used in the last experiment until it was actively evolving gas, I raised its temperature with the spirit-lamp until the bulb was full of gas; but on cooling this gas did not all condense, as, in the last instance, a large quantity remained behind: this was oxygen.

These ninth and tenth facts are of further interest as bearing upon a question which has been much discussed by chemists, —the nature of the bleaching compounds of chlorine. The chloride of lime, and other such substances, probably have the same theoretical constitution as chlorine-water. Berzelius and Balard suppose, that in this solution chlorous or hypochlorous acids exist. It might be inquired, if this be the condition of things, why does not an exposure to heat alone evolve oxygen? for chlorous acid is exceedingly liable to decomposition by slight elevation of temperature; and we should be justified in inferring, that if any of this acid is to be found in chlorine-water, it would be decomposed at the boiling-point. M. Millon adopts the view, that the bleaching compounds are metallic chlorides, analogous to the corresponding peroxides. But the ninth fact seems incompatible with this view. If chlorine water is analogous to peroxide of hydrogen, and the last be what its name imports, and not merely oxygenated water, it is difficult to understand why, when chlorine water is boiled, oxygen is not given off. If the atom of chlorine and the atom of oxygen in this body are placed under the same relations to the atom of hydrogen, it seems necessary that the chlorine atom at 212° Fahr. should expel the oxygen atom and chlorohydric acid form. It is probable, indeed, that the two oxygen atoms in peroxide of hydrogen are related to their hydrogen atom with different degrees of affinity, and that one of them is retained far more loosely than the other. But this would correspond with our ideas of oxygenized water and not peroxide of hydrogen, and leads us to the conclusion that the solution employed in this memoir is strictly a solution of chlorine in water.

XI. The decomposition of chlorine-water when placed in the sunbeam does not begin at once, but a certain space of time intervenes, during which the chlorine is undergoing its specific change.

I need quote no further instance of the truth of this than the experiment given in support of the second fact. This is the same phænomenon which takes place when chlorine and hydrogen are exposed together; they do not begin to unite at once, but a certain space of time elapses, during which the preliminary tithonization is taking place, and when that is over union begins (Phil. Mag., July 1844).

On the Relations of Chlorine and Hydrogen.

We have thus traced the cause of the decomposition of water, in the case before us, to a change impressed upon the chlorine by exposure to the rays of the sun. In this decomposition three elementary bodies are involved, chlorine, oxygen and hydrogen.

We can therefore reduce the problem under discussion to simpler conditions, and study the relations of each of these substances to each other and to the solar rays successively.

When a mixture of oxygen and hydrogen gases, in the proportion to form water, is exposed to the most brilliant radiation converged upon it by convex lenses, union does not ensue; the reason being, as I have formerly shown, that those gases are perfectly transparent to the rays, and do not possess either real or ideal coloration.

For the same cause, water exposed alone for any length of time to the sun, or to the influence of a large convex lens, does not decompose. It is transparent and cannot absorb any of the rays.

But, as is well known, a mixture of chlorine and hydrogen unites under the same circumstances with an explosion. I have formerly proved that this depends on the absorption of the indigo rays, for in the indigo space the action goes on with the greatest rapidity.

If, therefore, this phænomenon is due to absorption taking place by the mixture, it is easy to determine the function discharged by each of its ingredients.

I transmitted a ray of light through hydrogen gas contained in a tube seven inches long, the ends of which were terminated by pieces of flat glass, and then dispersing the ray by a flint-glass prism, received the resulting spectrum on a Daguerreotype plate. Simultaneously, by the side of it, I received the spectrum of a ray which had not gone through hydrogen, but through a similar tube filled with atmospheric air. On comparing the resulting impressions together, I could find no difference between them.

I therefore infer that hydrogen gas does not exert any absorptive action on the solar rays.

In one of the foregoing tubes I placed dry chlorine gas, the other containing atmospheric air as before; and receiving the two spectra side by side on the same Daguerreotype plate, I found that a powerful absorption had been exercised by the chlorine. All the tithonic rays between the fixed line H and the violet termination of the spectrum were removed, and no impression corresponding to their place was left upon the plate. On repeating this experiment, so as to determine with precision the rays which had been absorbed, I found that chlorine absorbs all the rays of the spectrum included between the fixed line *i* and the violet termination, and is probably effected by all those waves whose lengths are between 0·00001587 and 0·00001287 of a Paris inch, and inasmuch as it absorbs photic rays included between the same limits, it is to their absorption that its yellow colour is due.

In the Philosophical Magazine the same result was established by me in another way. I found that a ray which had passed through a given thickness of a mixture of equal volumes of chlorine and hydrogen, lost by absorption just half as much of its original intensity as when it passed through the same thickness of pure chlorine gas, a result which obviously leads to the conclusion, that when chlorine and hydrogen unite under the influence of the sun, they discharge functions which are different, the chlorine an active and the hydrogen a passive function. The primary action or disturbance takes place upon the chlorine, and a disposition is communicated to it, enabling it to unite readily with the hydrogen.

By arranging a series of tubes containing a mixture of these gases in the spectrum, it was found that the gases situated in the indigo space went into union first.

These various experiments enabling us thus to trace to the chlorine the source of disturbance, I have next to remark, that chlorine, which has been exposed to the rays of the sun, has gained thereby a tendency to unite with hydrogen, which is not possessed by chlorine which has been made and kept in the dark.

In proof of this I may cite an experiment from the Philosophical Magazine (July 1844). "In two similar white glass tubes place equal volumes of chlorine, which has been made from peroxide of manganese and muriatic acid by lamplight, and carefully screened from access of daylight. Expose one of the tubes to the full sunbeams for some minutes, or if the light be feeble, for a quarter of an hour: the chlorine which is in it becomes tithonized. Keep the other tube during this time carefully in a dark place; and now, by lamplight, add to both equal volumes of hydrogen gas. These processes are best carried on in a small porcelain or earthenware trough, filled with a saturated solution of common salt, which dissolves chlorine slowly; and to avoid explosions, operate on limited quantities of the gases. Tubes that are eight inches long and half an inch in diameter will answer very well. The tubes now contain the same gaseous mixture, and differ only in the circumstance that one is tithonized and the other not. Place them therefore side by side before a window, through which the entrance of daylight can be regulated by opening the shutter; and now, if this part of the process be conducted properly, it will be seen that the tithonized chlorine commences to unite with the hydrogen, and the salt water rises in that tube. But the untithonized chlorine shows no disposition to unite with its hydrogen, and the liquid in its tube remains motionless for a long time. Finally, as it becomes slowly tithonized by the action of the daylight impinging on it, union at last takes place. From this, therefore, we perceive that chlorine which has been exposed to the sun will unite promptly and energetically with hydrogen; but chlorine which has been made and kept in the dark shows no such property.

This form of experiment may be supposed imperfect, since the chlorine is in a moist condition and confined by water. I have therefore made the following variation.

I took a tube A, fig. 4, six inches long and half an inch in diameter, closed at one end and open at the other, and cemented its open end on a piece of flat plate glass M N, one inch wide and two long, ground on both sides, and having a hole, *p*, one-sixth of an inch in diameter perforated through it. This hole was not in the centre of the glass, but on one side, as shown in the figure. The interior of this tube was perfectly clean and dry.

A second tube B, consisting, as is shown in fig. 4, of two portions, a wide portion B, and a narrower tube *c*, was cemented on another piece of ground plate glass, similar to the foregoing in all respects. The tube *c* was open at its lower extremity, and the entire capacity of B and *c* conjointly was adjusted so as to be equal to the capacity of A.

Next I filled A with dry chlorine and B *c* with dry hydrogen, and kept them from mixing until the proper time by operating in the following way:—

I placed the ground glasses face to face, as shown in fig. 5, with a small quantity of soft tallow between them, arranging them in such a way that the aperture which led to the interior of A was open.

Through this aperture dry chlorine was conveyed. It was generated by a mixture of peroxide of manganese and chlorohydric acid in the flask D, fig. 6, and passed along a tube E filled with chloride of calcium. A slender glass tube *f* conveyed it to the bottom of A, which was then filled by displacing the atmospheric air. When A was supposed to be full of chlorine, it was slowly lowered so as to bring the tube out of the aperture, and as soon as it was disengaged the glass plates were moved in such a manner by sliding them on one another, that the aperture leading into A was shut, but that leading to B was open. The vessel A was thus filled with dry chlorine and securely closed.

In the next place I filled B with dry hydrogen, which was done as follows:—To a bottle G, fig. 7, containing dilute sulphuric acid and zinc, a drying tube, K, of chloride of calcium was adjusted, and at its upper end a cork *h* arranged, so as to receive tightly the tube *c*. In a short time, therefore, B became full of dry hydrogen, the surplus escaping through the open aperture *p*. The two ground glass plates were now moved on one another in such a manner that they mutually closed each other. The vessel A was therefore filled with dry chlorine, and the vessel B *c* with an equal volume of dry hydrogen, without communicating for the present with one another.

I had provided two sets of these tubes as nearly alike as they could be made, and operated with them in the following way:—

In a dark room I filled the tube A of each of them with dry chlorine, in the manner just described, and confined it by sliding the plates. One of the tubes was retained in the dark room and kept carefully screened from the light, but the other was set for half an hour in the sunbeams. The chlorine which was in it underwent the specific change which it is the object of this paper to describe.

After restoring this tube to the dark room, and waiting a few minutes for it to gain the same temperature as the other, the tubes B *c* of each set were filled with dry hydrogen in the manner described.

In each instance, as soon as the plates were moved on each other so as to confine the hydrogen, and were released from the cork *h* of the drying tube K, fig. 7, their lower extremity was dipped beneath the surface of some water contained in the saucer P, fig. 8; the two sets of tubes being held steadily in a proper position by the aid of a wooden frame Q R, Q R.

The two sets of tubes now differed from one another in nothing but the circumstance that the chlorine of one had been exposed to the sun and that of the other had not.

The gases were now brought in contact. This was easily done by sliding each pair of ground glasses until their apertures coincided, as shown in fig. 9. The hydrogen now rose through the hole into the upper vessel, the chlorine descending through it, mutual and perfect diffusion of the two gases rapidly taking place. This was done by lamplight in the dark room. And now it could be ascertained that the gases were at the same temperature in the different tubes, and that the experiment had thus far been carried on successfully, by the water retaining its level at the same point in the tube *c* of both sets. If that which had been in the sunshine was warmer than the other, as soon as the apertures coincided a bubble of gas would have escaped through the water, or at all events the level would have changed.

It remained now to open the shutter of the dark room, the tubes having been previously set in such a position that the light would fall equally on both. As soon as this was done, the chlorine which had been exposed to the sun united at once with its hydrogen, and the water rose in the tube *c*. But in the other, which had not been exposed to the sun, no movement took place until the gases had had time to be affected by the light coming through the open shutter.

When care has been taken to have the gases made quite dry, and, owing to the narrowness of the tube *c*, no aqueous vapour has had time to contaminate the gas in B, so that no water is present to condense the chlorohydric acid as it forms, a little delay may be occasioned in the liquid rising in the tube the chlorine of which was exposed to the sun. But after a time a mist arises in the neighbourhood of the water in the narrow tube, due to the chlorohydric acid condensing, and then the process goes forward with regularity.

It appears, therefore, that chlorine, by exposure to the sun, contracts a tendency to unite with hydrogen, which is not possessed by chlorine which has been kept in the dark.

On the Allotropism of Chlorine, or its Active and Passive States.

In what then does this remarkable change, impressed by indigo rays upon chlorine, consist? This is the question which immediately arises from the phænomena we have had under consideration.

To this I answer, that when chlorine has been thus influenced, its electro-negative properties are exalted, and it has passed from an inactive to an active state.

It is now fully established that a great number of the elementary bodies undergo similar modifications. Many of these can exist in no less than three different states, and these peculiarities are impressed on the compounds to which they give rise. To these peculiarities Berzelius has recently directed the attention of chemical philosophers, in his paper on the Allotropism of Simple Bodies, and its Relation with certain cases of Isomerism in their Combinations. He shows, that of the elementary bodies now known, many undoubtedly exist in several allotropic states, and infers that all are liable to analogous modifications. He indicates that the isomerism of compound bodies is due, sometimes to the different modes in which the atoms of which their constituent molecules consist are grouped, and sometimes to the different allotropic states in which one or other of those elements is found. Thus, as M. Millon has remarked, the intrinsic difference between carburetted hydrogen gas (CH) and otto of roses (CH), which are isomeric bodies, may perhaps consist in this,—that in the former the carbon is under the form of common charcoal, and in the latter under the form of diamond.

The following instances from Berzelius may serve as examples of these allotropic states.

Carbon is known under three forms, charcoal, plumbago, and diamond; they differ in specific gravity, in specific heat, and in their conducting power as respects caloric and electricity. In their relations to light the one perfectly absorbs it, the second reflects it like a metal, the third transmits it like glass. In their relations with oxygen they also differ surprisingly; there are varieties of charcoal that spontaneously take fire in the air, but the diamond can only be burnt with difficulty at a high temperature in pure oxygen gas. The second and third varieties do not belong to the same crystalline form.

Silicium exists also under two forms. In its first it burns with facility in the air under a slight elevation of temperature; but if it be previously exposed to a strong red heat, it changes into the second variety and becomes incombustible, so that it will not oxidize when placed with nitrate of potash in the hottest part of the blowpipe flame. As is well known, there are two forms of silicic acid, one soluble in water and hydrochloric acid, but passing into the insoluble state by being previously made red-hot. The silicium, therefore, carries in its combinations the same properties that it exhibits in the free state.

In the same manner it might be shown that sulphur, selenium, phosphorus, titanium, chromium, uranium, tin, iridium, osmium, copper, nickel, cobalt, and a variety of other bodies exist under several different forms, with distinctive properties that are often well-marked. In several of them the influence of this allotropic condition is plainly carried into the compounds, as is well shown in the two varieties of arsenic which give rise to the two arsenious acids.

The passage from one allotropic state to another takes place commonly through the agency of apparently very trivial causes, such as slight elevation of temperature, and the contact of certain bodies. Thus iron, which is so easily oxidized under ordinary circumstances, appears to lose its affinity for oxygen after it has been touched under the surface of nitric acid by a piece of platina. It then puts on the attributes of a noble metal and simulates the properties of platina and gold.

This remarkable instance of the passage from an active to a passive state, as Berzelius remarks, may lead to a conjecture respecting the true condition of certain gases. No one can reflect on the inactivity of nitrogen gas under ordinary circumstances, contrasted with its equally extraordinary activity as a constituent of organic bodies, without being struck with the apparent connexion of that phænomenon with those of allotropism. And though Berzelius, with his customary caution, merely insinuates that nitrogen can exist under two forms, the facts which are here developed in relation to chlorine seem to show that that opinion rests on something more solid than conjecture. The habitudes of many of the gaseous bodies strengthen this conclusion. Oxygen gas refuses to unite when mixed with hydrogen, precisely in the manner of chlorine; and it requires a certain modification to be made in the electro-negative element before water or chlorohydric acid can result.

Just, therefore, in the same manner that so many elementary bodies can put on under the influence of external causes an active or passive condition, I infer as the final result of the experiments brought forward in this memoir, that chlorine is one of those allotropic bodies having a double form of existence; that, as commonly prepared, it is in its passive state, but that on exposure to the indigo rays or other causes, it changes and assumes the active form; and that in this latter state its affinity for hydrogen becomes so great that it decomposes water without difficulty, as in the experiment which this memoir is designed to illustrate.

On the Relation of the preceding Conclusions with the Theory of Substitutions.

Having thus explained the facts which appear to indicate the allotropism of chlorine, I shall now offer some considerations on its connexion with the theory of substitutions of M. Dumas.

Admitting the fact that the electro-negative qualities of chlorine are exalted upon its exposure to the indigo rays, and that the resulting effect is not a temporary thing, but one which lasts for a considerable period of time, as appears to be proved in the Philosophical Magazine (July 1844), we can give a very plain and simple account of the decomposition of water by this gaseous substance under the influence of sunshine.

Upon the same principle that a mixture of chlorine and hydrogen may be kept in the dark without union for a long time, so may a solution of chlorine in water be preserved. The chlorine is in an inactive state.

But if anything is done to make the chlorine take on its other form and pass to the active condition; if, for example, it be set in the sunshine, its affinity for hydrogen is exhibited, and decomposition is the result.

The qualities thus communicated to the chlorine not being of a transient kind, but remaining for a length of time, we see how it is that after an exposure to the sun decomposition is subsequently carried forward in the dark.

If the power which chlorine assumes, of uniting with hydrogen and carbon, depends on a change in its electrical relations, a passage from the passive to the active state, we might expect that those various causes, which in the case of other elementary bodies bring about analogous changes, and throw them from one allotropic condition to another, would here also exercise a perceptible action. Among such causes we may enumerate the action of a high temperature, and the contact or presence of other bodies.

It may be remarked, in the instances to which Berzelius has referred, that exposure to a high temperature is one of the most frequent causes of allotropic change. In the case of chlorine the remark holds good, for, as is well known, when a mixture of chlorine and hydrogen is passed through a red-hot tube, chlorohydric acid forms with rapidity. The high temperature therefore impresses upon chlorine the same tendency to unite with hydrogen which is communicated by the solar rays.

But the contact of other bodies frequently determines in a given substance an allotropic change. Thus, when a piece of iron is placed in nitric acid in contact with platina, the iron becomes less electro-positive, or, what is the same thing, more electro-negative than it was before, and the acid can no longer oxidize it. The contact of the very same substance, platina, determines an analogous change in chlorine, giving it at once the capacity of uniting with hydrogen. The porous condition of spongy platina is not essential to the result, for clean platina-foil exhibits the same phænomenon.

In the case of iron, the action of a high temperature on the contact of platina throws the metal from the active to the passive state; in the case of chlorine, the same causes apparently produce the opposite result, throwing the gas from the passive to the active state. But the difference is rather in appearance than in reality. In both cases it amounts to the same thing, and is an exaltation of the electro-negative qualities of either substance respectively.

The same causes therefore which produce allotropic changes in other bodies produce analogous changes in chlorine.

Now among the physical facts connected with the theory of types and substitutions, two are prominent:—1st, the union of chlorine with hydrogen, giving rise to the removal of that hydrogen as chlorohydric acid; 2nd, the subsequent function discharged by the chlorine which has entered as an integrant portion of the molecule, and occupies the place of the hydrogen removed. This function is in many instances that of the hydrogen itself, and it is this fact which is the remarkable point in the phænomena of substitution, that an intensely electro-negative body can act the part of a positive body. It is this fact which is leading chemists to the conclusion, that the properties of compound bodies arise as much from the mode of grouping of their constituent atoms, as from the qualities of those atoms themselves.

But if it be admitted that the experiments related in this memoir establish the allotropism of chlorine, then it is plain that a very different, and perhaps satisfactory account, of the phænomena of substitution may be given.

As has already been said, no difficulty can arise in accounting for the removal of hydrogen from organic bodies, or for the first fact just alluded to. This removal will ensue whenever processes are resorted to which bring the chlorine into an active state. When we expose acetic acid and chlorine to the sun, the latter becomes active, gains the quality of uniting with hydrogen, and chloracetic acid forms. Probably the same change could be brought about by the aid of spongy platina and heat. Upon the second fact, the similarity of function discharged by the chlorine which has replaced the hydrogen atoms with the function of those atoms themselves, a flood of light is thrown by other phænomena of allotropism. If a piece of iron be dipped in hydrated nitric acid, though it may be acted on for a few moments, it rapidly becomes passive; and so with the chlorine atoms which have substituted the hydrogen. In the circumstances under which they are placed they rapidly revert from the active to the passive state. They no longer are endued with an intense electro-negative quality, they have assumed the condition of inactivity. The fact that chlorine in chloracetic acid simulates the functions of hydrogen in acetic acid, is not more remarkable than that iron touched by platina under nitric acid simulates the properties of that noble metal.

Do not therefore these considerations seem to point out, that, if we admit the fact that simple substances can exist in different states, in a passive and active form, the phænomena of substitution are deprived of much of their singularity?

Thus, to recall once more the same example to which I have before referred, and which has been so well illustrated by the researches of M. Dumas; the transmutation of acetic into chloracetic acid exhibits a double phænomenon, — 1st, the existence of active chlorine expressed by the removal of hydrogen, activity having been communicated by the rays of the sun, or by some other appropriate method; 2nd, the existence of passive chlorine in the particles of chloracetic acid.

I consider, that were no other instances known, the two cases cited by Berzelius of the double forms of silicic acid and arsenious acid establish the fact, that a given allotropic condition may be continued by an elementary atom when it goes into union with other bodies. And I regard the various cases in which hydrogen is replaced by iodine, bromine, &c., in which in the resulting compound those energetic electro-negative elements fail to give any expression of their presence and activity, as analogous to other common and too much overlooked facts. Chlorine which is in the dark may be kept in contact with hydrogen without exhibiting any of its latent energies. Touched by an indigo ray it instantly assumes the active state, and a violent explosion is the result.

To use therefore the same nomenclature to which Berzelius has resorted in the case of other allotropisms, we may designate the ordinary form of chlorine, procured by the action of chlorohydric acid on peroxide of manganese, as Cl β, and admit that this passes into the condition Cl α by the action of the solar rays, contact of platina, or a high temperature; and that in any case of substitution, the hydrogen is removed under the condition Cl α, and the resulting compound molecule contains Cl β; the assumption of the passive state disguising the presence of the electro-negative atom.

The explanation here given of the phænomena of substitution involves the position, that chlorine, when brought in relation with carbon, under certain circumstances, is thrown into the passive state,—the state Cl β. We naturally look for direct evidence that this is the case. It seems to me that there are many well-known chemical facts which tend to establish this passive condition. In the first case to which we turn, the chlorides of carbon, the inactive state is established in a striking manner. The affinity which exists between chlorine and carbon is apparently feeble, yet when these bodies have once united, the chlorine is brought into such a condition that it has lost the quality of being detected by the ordinary tests which determine its presence. How strongly does this contrast with the case of chlorohydric acid! a feeble affinity unites carbon and chlorine, and intense affinity unites hydrogen and chlorine; yet in the former case the chlorine is undiscoverable by the most common tests, in the latter it yields to them all. And the causes are obvious; in the one case it is in the passive, in the other in the active condition.

I have hitherto spoken of the active and passive states as though they were fixed points in elementary bodies, and as though the transition from one to the other was abrupt and sudden. I have done this that the views here offered might be unembarrassed and distinct; but there are many facts which serve to show that the passage from a state of complete activity to a state of complete inactivity takes place through gradual steps. Thus, in carbon itself, there are undoubtedly many intermediate stages between the almost spontaneously inflammable varieties and diamond, which is, under common circumstances, incombustible. Berzelius admits three allotropic conditions of this body, $C\alpha$, $C\beta$, $C\gamma$. Between the first and last term of this series, it is probable that several intermediate bodies besides plumbago might be found, their existence establishing the gradual passage from one to the other state.

For similar reasons, in this memoir the illustrations and arguments given have for the most part been restricted to one substance, chlorine. It need scarcely be pointed out, in conclusion, that if the views here offered are true, very much of this reasoning may be transferred to other bodies, as oxygen, nitrogen, hydrogen, sulphur, &c. When oxygen and hydrogen are mixed, there is no disposition exhibited by them to unite; and this does not arise from their happening to have the gaseous form. As in the instance we have been considering, if they be exposed to a high temperature, or to the influence of platina, the active condition is assumed with promptitude, and union takes place.

The power which carbon possesses of throwing bodies into a completely passive state, is far from being limited to chlorine; it re-appears in the case of sulphur. The sulphuret of carbon yields to none of the tests to which we commonly resort for determining the presence of sulphur, for the simple reason that its sulphur is in an inactive state. This substance, moreover, serves to illustrate what has been said of the gradual passage of bodies from a state of complete activity to one of complete inactivity. Berzelius recognises for it three different allotropic states, an alpha, beta, and gamma condition. In none of these is it in that condition of absolute inactivity which it assumes in the sulphuret of carbon*.

In offering these experiments and arguments to the consideration of chemists, I am fully aware of the magnitude of the change which would be impressed on the science generally,

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* For these examples, the chloride and sulphuret of carbon, I am indebted to M. Millon's paper, Remarks on the Elements which compose Organic Substances, and on their Mode of Combination, *Comptes Rendus*, t. xix. p. 799. That chemist, however, gives a very different explanation of the phænomena involved.

and especially on several of our modern theories, by their reception. The long-established idea of the immutability of the properties of elementary bodies would to a certain extent be sacrificed; and it is probable that before these results are conceded, more cogent evidence of the main principle will be required. In the mean time, however, it is plain that the admission of these doctrines throws much light on theories now extensively attracting the attention of men of science, and for that reason they commend themselves to our consideration. I have offered no opinion here on the atomic mechanism which is involved in these changes from an active to a passive state, though it is impossible to deal with these things without the reflection arising in our minds, that here we are on the brink of an extensive system of evidence connected with the polarity of atoms,—an idea, which, under a variety of forms, is now occurring in every department of natural philosophy.

University of New York, July 29, 1845.

LI. *On the Diffusion of Gases.*

By Thomas Starkie Thomson, *Esq.*

To Richard Taylor, Esq.

Dear Sir,

On perusing lately the highly interesting volume on Natural Magic by your distinguished co-editor, Sir David Brewster, I was much struck by his description of natural phænomena which had for many years past occupied my consideration.

As one fact which was new to me bears directly upon the subject of my previous communications to you on the Diffusion of Gases, and will, I think, tend further to elucidate the matter, I may be allowed to offer some observations upon it, and shall introduce them by the following quotation from the learned author.

"The great audibility of sounds during the night is a phænomenon of considerable interest, and one which had been observed even by the ancients. In crowded cities, or in their vicinity, the effect was generally ascribed to the rest of animated beings, while in localities where such an explanation was inapplicable, it was supposed to arise from a favourable direction of the prevailing wind. Baron Humboldt was particularly struck with this phænomenon when he first heard the rushing of the great cataracts of the Orinoco in the plain which surrounds the Mission of the Apures. These sounds he regarded as three times louder during the night than during the day.

"Some authors ascribed this fact to the cessation of the humming of insects, the singing of birds, and the action of the wind on the leaves of trees; but M. Humboldt justly maintains that this cannot be the cause of it on the Orinoco, where the buzz of insects is much louder in the night than in the day, and where the breeze never rises till after sunset. Hence he was led to ascribe the phænomenon to the perfect transparency and uniform density of the air, which can exist only at night after the heat of the ground has been uniformly diffused through the atmosphere. When the rays of the sun have been beating on the ground during the day, currents of hot air of different temperatures, and consequently of different densities, are constantly ascending from the ground and mixing with the cold air above. The air thus ceases to be a homogeneous medium; and every person must have observed the effects of it upon objects seen through it, which are very indistinctly visible, and have a tremulous motion as if they were dancing in the air. The same effect is perceived when we look at objects through spirits and water that are not perfectly mixed, or when we view distant objects over a red-hot poker or over a flame. In all these cases the light suffers refraction in passing from a medium of one density into a medium of a different density; and the refracted rays are constantly changing their direction as the different currents rise in succession. Analogous effects are produced when sound passes through a mixed medium, whether it consists of two different media, or of one medium where portions of it have different densities. As sound moves with different velocities through media of different densities, the wave which produces the sound will be partly reflected in passing from one medium to the other, and the direction of the transmission wave changed; and hence in passing through such media, different portions of the wave will reach the ear at different times, and thus destroy the sharpness and distinctness of the sound. This may be proved by many striking facts. If we put a bell in a receiver containing a mixture of hydrogen gas and atmospheric air, the sound of the bell can scarcely be heard."

With all due deference to so great an authority as Sir David Brewster, I cannot admit the analogy between the non-existence of sound in a mixture of hydrogen gas and atmospheric air, and the interruptions to hearing and vision produced by the refraction of sound and light "in passing from a medium of one density into a medium of a different density." Surely when hydrogen gas and atmospheric air are mixed in a receiver, there cannot exist media of different densities? In accordance with the law of the diffusion of gases, the arrangement of the molecules of the three gases concerned must be perfectly symmetrical. I see in this fact a confirmation of an idea which I believe I was the first to announce, of the existence of an attractive force between mixed gases of a double character; in one sense a *chemical* force, because it exists only between the particles of *dissimilar* gases; in another sense a *mechanical* force, because it obeys a mechanical law,—the law of diffusion as ascertained by Prof. Graham. In atmospheric air, which is a *mixture*, not a *chemical combination*, of oxygen and nitrogen, the chemical force is so feeble as not to interfere with the communication of sound, as it would take place in a homogeneous medium. But in a mixture of oxygen, nitrogen and hydrogen, the conditions are changed; the superior chemical affinity of oxygen for hydrogen disturbs the mechanical equilibrium of the system; the molecules cease to vibrate, as in a homogeneous medium; and sound ceases to be communicated, as I infer from the fact announced by Sir David Brewster.

I cannot better conclude these remarks than by adding to the interesting facts previously quoted, the explanation of a similar phænomenon which I have never seen noticed before. In Lancashire, where I passed the early years of my life, I soon found that it was, among the farmers and country people, a traditional observation, that when the distant hills appeared unusually distinct, it was a sign of approaching rain. This I attribute to the homogeneity of the atmosphere under such circumstances, with respect to aqueous vapour.

The whole intervening space between the eye and the distant object is saturated with moisture, and consequently vision is more perfect, as there are no series of media of different densities to interfere with the direct transmission of light.

I am, Sir,
Your obedient Servant,
Richmond, October 1, 1845. THOMAS STARKIE THOMSON.

LII. *On* Grotthus's *Theory of Molecular Decomposition and Recomposition.* *By* W. R. GROVE, *Esq., F.R.S., &c., and Professor of Natural Philosophy in the London Institution.*

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

DEAR SIR,

THE great mystery of voltaism, or the phænomenon of two constituents of a chemical compound being eliminated at distant points without any apparent disturbance in the intervening liquid, is accounted for, as you are well aware, by

Grotthus, on the supposition of a series of molecular decompositions and recompositions. This theory has become almost universally adopted by electro-chemists; I have frequently availed myself of it in my lectures, and, though with some doubt and hesitation, have applied it to explain the action of the gas battery. I have ever been anxious, if possible, to account for new phænomena by generally received theories, one main object of a theory being to collate facts in a manner which may render them conventionally intelligible, while plurality of theories introduces anarchy into science.

It has lately, however, struck me very forcibly that the gas battery presents a strong objection to the theory of Grotthus, or in other words, that it places us in a dilemma in which we must either abandon this theory or the generally received views (I think I may say established laws) of chemical affinity. My difficulty is this. In *a single pair* of the gas battery, we must, according to the theory of Grotthus, suppose that oxygen and hydrogen tear asunder oxygen and hydrogen already united; for whether we suppose the action to begin at the hydrogen extremity of the electrolyte, or at the oxygen, or at both simultaneously, the force which produces the composition of water is regarded by the theory sufficiently strong to overcome the force by which its constituents are already united, *i. e.* a force is equal and unequal at the same time.

The gas battery also presents cases where, according to this theory, a more feeble affinity overcomes a more powerful one, as when water is the electrolyte and deutoxide of nitrogen and oxygen the gases: we have indeed some instances in chemistry where the order of affinities is reversed, as in the decomposition of water by iron, and of oxide of iron by hydrogen, supposing the oxide to be the same in each case, which is much doubted; but we have no instance, as far as I am aware, of the divellent and quiescent affinities being identical, as for instance, the affinity of oxygen for hydrogen decomposing water, that of oxygen for iron decomposing oxide of iron. Where water is decomposed at the electrodes by a series of the gas battery, the same difficulty does not arise; as, abandoning all theory as to the transfer in the cells of the battery, there is nothing inconsistent in the fact, that the reduplicated force of a series of affinities should overcome an equal or stronger affinity taken singly, just as we decompose potash by a series of affinities of zinc for oxygen; but in the single cell there is no such exaltation of intensity.

It may be said that the pulverulent platinum exalts the chemical energies of the gases, but this I think is inconsistent with all we know of the catalytic action of platinum; all the observed facts go to prove that platinum reduces the gases to a state analogous to the nascent state, *i. e.* one at which their specific chemical energies are at the highest, but it does not change their specific energies; thus platinum enables gaseous oxygen to combine with gaseous hydrogen, but it does not give to oxygen the affinities of chlorine, or to hydrogen those of potassium. Even the granting this hypothesis, however, does not extricate us, as whatever specific power the platinum may possess in respect to the gas, it must also possess in respect to the electrolyte; it can scarcely be supposed to assist combination and yet not to prevent decomposition of the same molecule; it is an equation upon any view.

Nor can it be supposed that the action in the liquid of the gas battery cells differs from other cases of electrolysis; the line of discharge in the gas battery affects the magnet, and indeed forming portion of the voltaic circuit, it would be contrary to all analogy to suppose it exceptional in respect of its mode of action. An hypothesis might be framed which would regard the action of the gas battery as resulting from the formation of a soluble peroxide of hydrogen and perhydruret of oxygen; but if so, a similar hypothesis must be extended to all cases of electrolysis; and this view presents many difficulties. Perhaps some of your correspondents may be able to solve the theorem; for if not reconciled with facts, however ingenious and useful the theory of Grotthus be, it is a theory, while the gas battery is a fact, and in case of collision, it is needless to say which must go to the wall.

I remain, my dear Sir,
Yours very truly,
W. R. Grove.

LIII. *On the Proportion of Nitrogen contained in Alimentary Substances taken from both the Organic Kingdoms as a comparative measure of their Nutritive Power. By* Dr. J. Schlossberger *and* Alexander Kemp, *Assistant Chemical Teachers in the University of Edinburgh*[*].

THE distinction between the elements of the reproductive and those of the respiratory functions, is probably one of the most fertile ideas for which physiology is indebted to modern chemistry; even if we do not admit that division in its full extent, nevertheless we must allow it to be a beautifully conceived idea, and one founded on a great amount of observation. It may be assumed with safety that no other classification of the substances comprised under the vague designation

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[*] Communicated by the Authors.

of alimentary bodies, has in an equal degree represented the essential differences in their chemical composition and their physiological effects, or has presented to the eye of the mind the important part which these substances perform, as the above-mentioned distinction, which is also identical with that of *azotised* and *non-azotised* bodies.

In order to give some indication of the state of confusion which existed, even in this most elementary proposition of dietetics, previous to that distinction being made, it may not be more than necessary to mention the idea of that classical author, Dr. Prout*, that the nutritive power is in direct proportion to the quantity of carbon. At the present day not a single fact is known which can support the idea of the animal body being able to form azotised from non-azotised substances, —possibly, under the influence of ammonia or of nitrogen from the atmosphere; on the contrary, all experiments, as well as daily observation, seem to prove the absolute necessity of azotised food for the preservation of the individual. Chemistry has likewise demonstrated the presence of a more or less high, but constant proportion of nitrogen in all the tissues and fluids of the animal body, while in all those substances which, according to our modern ideas, are the most nutritive, namely, the *proteine* compounds, the carbon is present in medium quantity. Those substances esteemed by Prout the most nourishing, because richest in carbon, as the fats and oils, must be altogether excluded from the list of reproductive bodies, except in so far as fatty matter is necessary to the formation of animal cells. The chemical physiologist could make use of the theory of Prout, in measuring the fitness of the bodies necessary to respiration, if along with the carbon could be taken into account the quantity of combustible or unoxidized hydrogen contained in them; but for those substances, which in the strict sense of the word are capable of being transformed into blood and animal tissue, according to our present knowledge, the capability for these purposes may be estimated relatively by the amount of nitrogen. This has been already done to a certain extent by the researches of several chemists and physiologists, but, so far as we are aware, it has been confined to vegetables; and it therefore appeared to us not to be without interest to make use of the same principle in extending the investigation to the various alimentary substances taken from the animal kingdom, and so to give to the physiologist a basis founded on facts in a de-

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* See Mayo's Outlines of Human Physiology, p. 206, second edition. London, 1829.

partment which has hitherto been so defective in the mysterious doctrine of nutrition.

Already Boussingault and Liebig have demonstrated, that in general the amount of proteine compound, and therefore that of nitrogen, is in a direct ratio to the phosphates; this proposition has as yet been extended only to vegetable matters, but will most probably preserve its value when applied likewise to those derived from animals. In so far as the nitrogen may be taken as an indication of the quantity of these salts, we could not find any observations as to how far it might be also applied to the gelatigenous compounds; and we have alluded to this as a very interesting field for future research, particularly with regard to the phosphate of lime, which seems so universal and so necessary to the whole animal œconomy.

It seems to be at present a proper time to overcome an objection, which, if not alluded to, might have been made against our attempt to determine the nutritive power of animal aliments; it is with regard to the delicate question of the use of animal gelatine, which seemed to Mulder, and likewise to us, as not at all decided by the experiments lately made at Paris. It is a fact sufficiently proved by the experiments of Magendie, Tiedemann and Gmelin, that any substance, even the most nourishing, if very simple and used without admixture, cannot sustain animal life for any length of time; and if it was proved at Paris that dogs fed exclusively on gelatine perish, it is far from being just to conclude that the substance is not nutritive, as we should be compelled to apply the same rule to albumen and fibrine, by the exclusive use of which an animal would no less speedily perish. At all events gelatine has a high value as nutriment, if even that value arises alone from its being useful in the formation of gelatigenous tissues; in addition, the gelatigenous tissues, as well as those containing proteine, in regard to their formation and chemical constitution, seem to lie in close relation, although that relation is at present not perfectly understood[*]. The experience of our best physicians at the bedside of the patient tends to prove that during convalescence a well-prepared gelatinous diet, but not exclusively gelatinous, is highly nutritious.

All organic alimentary substances, as presented to us by nature, are mixtures, but seem to be pretty constant in their composition, although variously modified by our different modes of cooking them. Of such natural mixtures, the greater part taken from vegetable bodies have already had the

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[*] See Mulder's Chemistry of Animal and Vegetable Physiology, translated by Fromberg, 319 and following pages.

proportion of nitrogen contained in them determined by previous observers; and we believe we shall best introduce the subject by a concise representation of that which has been done in regard to the vegetable part of our aliments, which will likewise afford the best means of comparing the results of our experiments on animal substances.

The following table shows the amount of nitrogen contained in 100 parts of the dry vegetable bodies named in it:—

Rice	1·39	Boussingault, *Economie Rurale*. Paris, 1844, tome 2^{me}, p. 438.
Potatoes	1·5	
Turnips	1·7	
Rye	1·7	
Oats	2·2	
Wheat	2·0 to 2·3	
Carrots	2·4	
Barley	2·0	
Maize	2·0	
Peas	3·8	
Lentils	4·4	
Haricots	4·5	
Beans	5·1	
White bread	2·27	R. D. Thomson, in the London and Edinburgh Philosophical Magazine for November 1843.
Brown bread	2·63	
Glasgow unfermented bread	2·14	
Essex flour	2·17	
Canada flour	2·21	
Lothian flour	1·96	
United States flour	1·82	
Agaricus deliciosus	4·6	Schlossberger and Dœpping, Liebig's *Annalen* for October 1844, Chemical Gazette, July 15, 1845.
... russula	4·2	
... cantharellus	3·2	

Boussingault has calculated from the results of his experiments, that which he calls the equivalent nutritive powers of these substances for the domestic herbivorous animals, and Thomson has also given one for those bodies which he examined. We omit these tables, as we have the intention of constructing a table of our own for all the substances which are used as food by man, and which have been hitherto examined in this respect.

We may now proceed to the relation of our experiments. All the substances made the subject of observation were first carefully dried at 212° F., and then analysed according to the method of Varrentrapp and Will, slightly modified to overcome some practical difficulties.

Intermediate between animal and vegetable aliment, as daily observation teaches and chemical analysis confirms, is to be found the most general food of the young of the class Mammalia, namely, milk.

I. Cow's milk. 0·404 gramme* of the dried residue, obtained by evaporating the fresh-drawn milk in the water-bath and subsequent desiccation at 212° F., yielded 0·241 grm. of the ammonio-chloride of platinum, corresponding to 3·78 per cent. of nitrogen.

II. 0·438 grm. human milk, treated as in the first experiment, gave 0·11 grm. ammonio-chloride of platinum, equal to 1·59 per cent. of nitrogen.

Human milk is, according to most analyses, one of the poorest in caseine, and in this respect is very far inferior to that of the cow, but in consequence of this may probably be more easily digested. The amount of nitrogen in milk, which corresponds to that of caseine, comes much nearer to the proportion contained in vegetables than any other kind of aliment from the animal kingdom.

Of the substances obtained from milk and used as food, we have examined only that of cheese, selecting four of the kinds most commonly used in Britain.

III. 0·485 grm. of Dunlop cheese gave 0·461 grm. of ammonio-chloride of platinum, equal to 6·03 per cent. of nitrogen.

IV. 0·475 grm. of Gouda cheese gave 0·532 grm. of ammonio-chloride of platinum, equal to 7·11 per cent. of nitrogen.

V. 0·444 grm. of Cheshire cheese gave 0·471 grm. of ammonio-chloride of platinum, equal to 6·75 per cent. of nitrogen.

VI. 0·477 grm. of double Gloucester cheese gave 0·525 grm. of ammonio-chloride of platinum, equal to 6·98 per cent. of nitrogen.

VII. 0·557 grm. of a very old double Gloucester cheese, abounding in mites and mould, in the condition in which it is sought after to gratify the depraved taste of the epicure, gave 0·463 grm. of ammonio-chloride of platinum, equal to 5·27 per cent. of nitrogen.

We add here, as being similar in composition, the yolk of the egg of the common fowl.

VIII. 0·526 grm. of well-dried yolk gave 0·387 grm. of ammonio-chloride of platinum, equal to 4·86 per cent. of nitrogen.

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* The balance used in these analyses having been made by Deleuil of Paris, and being only furnished with French weights, will account for the introduction of the word here, and in the other analyses throughout the paper. The French gramme is equal to 15·444 English grains.

Somewhat more nutritive than milk, and more nearly allied to cheese, is a series of bodies obtained from the lower orders of the animal kingdon. We give here, as examples, the oyster and one or two others.

IX. 0·418 grm. of the oyster, *Ostrea edulis*, gave 0·346 grm. of ammonio-chloride of platinum, equal to 5·25 per cent. of nitrogen.

As this seemed to us a very low result, we repeated our analysis, which, however, confirmed our first experiment; in this case 0·354 grm. giving 0·283 grm. of ammonio-chloride of platinum, equal to 5·07 per cent. of nitrogen.

X. 0·354 grm. of the yellow matter (liver and bile) from the crabfish, *Cancer communis*, gave 0·418 grm. of ammonio-chloride of platinum, equal to 7·52 per cent. of nitrogen.

XI. 0·377 grm. of the common mussel, *Mytilus edulis*, gave 0·498 grm. of ammonio-chloride of platinum, equal to 8·41 per cent. of nitrogen.

XII. 0·308 grm. of the above animal, previously boiled, gave 0·510 grm. of ammonio-chloride of platinum, equal to 10·51 per cent. of nitrogen.

We find that many of the organs of the higher animals, for example, the liver of the ox, nearly agree in the amount of nitrogen with those substances we have just treated of.

XIII. 0·432 grm. of ox liver gave 0·726 grm. of ammonio-chloride of platinum, equal to 10·66 per cent. of nitrogen.

XIV. 0·419 grm. of the liver of the pigeon gave 0·778 grm. of ammonio-chloride of platinum, equal to 11·80 per cent. of nitrogen.

For the reason just mentioned, we here likewise notice the analysis of the muscles of some kinds of fish, which, from being quite saturated with oil, give a low amount of nitrogen; for instance, that of the eel containing 6·91 per cent., and pork ham, from the large quantity of salt, giving only 8·57 per cent.; but we shall return to these more in detail. The connecting link between the two extremities of the nutritive scale of animal aliments we have formed, appears to be the dried extract of meat, which is sold under the name of portable soup, or *bouillon*.

XV. 0·441 grm. of portable soup, of an excellent quality, gave 0·845 grm. of ammonio-chloride of platinum, equal to 12·16 per cent. of nitrogen. Osmazome is therefore a body rich in nitrogen, probably from containing oxide of proteine, and perhaps also kreatine.

We shall give our results on the different kinds of flesh, including that of the fishes also, with which we begin.

Common eel, *Anguilla vulgaris*.—The flesh of this fish is poorer in nitrogen than any of the others which we have examined, in consequence of the large quantity of fat or oil it contains; indeed, during the whole desiccation it was floating in a liquid fat.

XVI. 0·345 grm. of raw eel's flesh gave 0·379 grm. of ammonio-chloride of platinum, equal to 6·91 per cent. of nitrogen.

XVII. 0·306 grm. of boiled eel's flesh gave 0·329 grm. of ammonio-chloride of platinum, equal to 6·82 per cent. of nitrogen.

XVIII. 0·293 grm. of eel's flesh, which had been previously washed with distilled water and afterwards boiled in alcohol as long as any matter separated, yielded 0·667 grm. of ammonio-chloride of platinum, equal to 14·45 per cent. of nitrogen, a quantity as great as that from the higher animals.

XIX. 0·274 grm. of the flesh of the salmon, *Salmo Fario*, gave 0·533 grm. of ammonio-chloride of platinum, equal to 12·35 per cent. of nitrogen.

XX. 0·286 grm. of boiled salmon gave 0·437 grm. of ammonio-chloride of platinum, equal to 9·70 per cent. of nitrogen.

XXI. 0·319 grm. of the purified muscular fibre of the salmon gave 0·785 grm. of ammonio-chloride of platinum, equal to 15·62 per cent. of nitrogen.

XXII. 0·271 grm. of the raw herring, *Clupea Harengus*, gave 0·590 grm. of ammonio-chloride of platinum, equal to 14·48 per cent. of nitrogen.

XXIII. 0·314 grm. of boiled herring gave 0·636 grm. of ammonio-chloride of platinum, equal to 12·85 per cent. of nitrogen.

XXIV. 0·350 grm. of purified muscle of the herring gave 0·802 grm. of ammonio-chloride of platinum, equal to 14·54 per cent. of nitrogen.

XXV. 0·4065 grm. of the milt of the herring gave 0·940 grm. of ammonio-chloride of platinum, equal to 14·69 per cent. of nitrogen. This substance is therefore as nutritive as the muscular parts of the fish, containing the same amount of nitrogen.

XXVI. 0·316 grm. of the flesh of the haddock, *Æglefinus communis*, gave 0·729 grm. of ammonio-chloride of platinum, equal to 14·64 per cent. of nitrogen.

XXVII. 0·331 grm. of boiled haddock gave 0·677 grm. of ammonio-chloride of platinum, equal to 12·98 per cent. of nitrogen. Whereas in the case of the herring, salmon, haddock, and eel, the proportion of nitrogen was considerably diminished by boiling the flesh for about half an hour, the reverse of this occurred in some of the other cases; for example, in the mussel, the flesh of the ox and calf. These are mere observations of facts, arising probably from accidental causes; the modification which meat undergoes by boiling not being sufficiently understood by chemists at present, although Mulder has proved the formation of oxide of proteine by it; from the observations just made, no general conclusion can be drawn, because they do not seem to coincide, at least at first sight.

XXVIII. 0·271 grm. of the purified muscle of the haddock gave 0·671 grm. of ammonio-chloride of platinum, equal to 15·72 per cent. of nitrogen.

XXIX. 0·348 grm. of the flesh of the flounder, *Platessa Flessus*, gave 0·783 grm. of ammonio-chloride of platinum, equal to 14·28 per cent. of nitrogen.

XXX. 0·342 grm. of boiled flounder gave 0·818 grm. of ammonio-chloride of platinum, equal to 15·18 per cent. of nitrogen.

XXXI. 0·301 grm. of purified muscle of the flounder gave 0·745 grm. of ammonio-chloride of platinum, equal to 15·71 per cent. of nitrogen.

We have selected the skate, *Raia Batis*, as a fair example of the cartilaginous order of fishes.

XXXII. 0·415 grm. of the flesh of the skate gave 1·066 grm. of ammonio-chloride of platinum, equal to 15·39 per cent. of nitrogen.

XXXIII. 0·402 grm. of boiled skate gave 0·964 grm. of ammonio-chloride of platinum, equal to 15·22 per cent. of nitrogen.

XXXIV. 0·407 grm. of the boiled muscle taken from the claw of the crabfish gave 0·877 grm. of ammonio-chloride of platinum, equal to 13·66 per cent. of nitrogen. From this analysis we are led to believe that the muscles of the *Crustacea* are as rich in nitrogen as those of much more highly organised animals.

XXXV. 0·299 grm. of the flesh of the pigeon gave 0·570 grm. of ammonio-chloride of platinum, equal to 12·10 per cent. of nitrogen. This is a surprisingly low number, more especially as the muscle of this bird was nearly free of fat.

XXXVI. 0·334 grm. of boiled pigeon gave 0·649 grm. of ammonio-chloride of platinum, equal to 12·33 per cent. of nitrogen.

XXXVII. 0·166 grm. of the purified muscle of the pigeon gave 0·344 grm. of ammonio-chloride of platinum, equal to 13·15 per cent. of nitrogen. The fibre used in this experiment most tenaciously retained a small quantity of the colouring matter of the blood. We have already given the analysis of the liver of this bird (experiment XIV.).

XXXVIII. 0·347 grm. of lamb gave 0·725 grm. of ammonio-chloride of platinum, equal to 13·26 per cent. of nitrogen.

XXXIX. 0·320 grm. of the purified fibre of lamb gave 0·734 grm. of ammonio-chloride of platinum, equal to 14·56 per cent. of nitrogen.

XL. 0·336 grm. of mutton gave 0·651 grm. of ammonio-chloride of platinum, equal to 12·30 per cent. of nitrogen. In this case it was found extremely difficult to separate mechanically the whole of the fat: it is necessary to notice this, as the numbers are somewhat lower than might have been expected.

XLI. 0·341 grm. of boiled mutton gave 0·728 grm. of ammonio-chloride of platinum, equal to 13·55 per cent. of nitrogen.

XLII. 0·335 grm. of the purified fibre from mutton gave 0·779 grm. of ammonio-chloride of platinum, equal to 14·76 per cent. of nitrogen.

XLIII. 0·318 grm. of veal gave 0·696 grm. of ammonio-chloride of platinum, equal to 13·89 per cent. of nitrogen.

XLIV. 0·379 grm. of boiled veal gave 0·866 grm. of ammonio-chloride of platinum, equal to 14·50 per cent. of nitrogen.

XLV. 0·214 grm. of purified fibre from veal gave 0·532 grm. of ammonio-chloride of platinum, equal to 15·78 per cent. of nitrogen.

XLVI. 0·306 grm. of ox-beef gave 0·675 grm. of ammonio-chloride of platinum, equal to 14·00 per cent. of nitrogen.

XLVII. Last experiment repeated. 0·292 grm. gave 0·633 grm. of ammonio-chloride of platinum, equal to 13·73 per cent. of nitrogen.

XLVIII. 0·331 grm. of boiled beef gave 0·781 grm. of ammonio-chloride of platinum, equal to 14·98 per cent. of nitrogen.

XLIX. 0·392 grm. of purified fibre of beef gave 0·919 grm. of ammonio-chloride of platinum, equal to 14·88 per cent. of nitrogen. The liver of the ox gave 10·66 per cent. of nitrogen (see XIII.).

L. 0·216 grm. of the lungs of the ox gave 0·504 grm. of ammonio-chloride of platinum, equal to 14·81 per cent. of nitrogen.

LI. 0·359 grm. of pork-ham gave 0·485 grm. of ammonio-chloride of platinum, equal to 8·57 per cent. of nitrogen.

LII. 0·395 grm. of the boiled pork-ham gave 0·777 grm.

of ammonio-chloride of platinum, equal to 12·48 per cent. of nitrogen.

LIII. 0·384 grm. of the purified fibre of pork-ham gave 0·860 grm. of ammonio-chloride of platinum, equal to 14·21 per cent. of nitrogen.

We regret that at the time when these experiments were made, we were unable to procure fresh pork, but we thought that an examination of the salted and smoked substance would not be devoid of interest.

From these results we see that there is no appreciable change produced in the composition of the fibre by the preparation and length of time it had been kept; if however we take equal weights of the prepared ham and of the fresh flesh of the sow, we shall of course find a considerable difference in the amount of nitrogen from the large quantity of salt which is present in the prepared ham. We conclude this account of our experiments by giving the analysis of the white of the egg of the barn-door fowl.

LIV. 0·369 grm. of the white of the egg gave 0·781 grm. of ammonio-chloride of platinum, equal to 13·44 per cent. of nitrogen. The quantity of nitrogen in pure albumen, as determined by Mulder, is 15·8 per cent.

We take the liberty to add to these experiments the following remarks:—The proportion of nitrogen in purified muscular fibre seems to be identical, from whatever part of the animal kingdom it may be obtained; and the differences given by the results of analysis may be fully explained by the difficulty, or even impossibility, of analysing it in an equally pure or impure condition, as obtained from different animals, in which it is always mixed with cellular tissue, minute vessels and nerves. Moreover, it is extremely difficult to get rid of traces of fat and hematine.

That the chemical properties of muscular fibre in the whole animal kingdom are identical, one of us endeavoured to prove in a former research (Schlossberger, *Vergleichende Untersuchungen uber das Fleisch verschiedener Thiere.* Stuttgardt, 1840). In contradiction to a very generally-received opinion, it appears to us that the muscles of fish are as rich in nitrogen as those of higher animals; at first sight, however, owing to the presence of a greater quantity of water, and in some fishes, as the eel, to a difficultly separable fat, its amount appears very much lower. Further, as the proximate principles are essentially the same in both classes of animals, it seems to us that they should be equally nutritive, although this proposition is also directly opposed to another very general prejudice. According to our scale, the oyster does not seem nearly so nourishing as it is generally reputed to be by common opinion, although it is possible that the proteine compounds in the lower classes of animals may be found to be much richer in phosphorus, in sulphur, and in phosphates than in the higher. Should this prove to be the case, we can see, that although the proportion of nitrogen may not be so great, nevertheless that they might act more powerfully as *stimulants*, which, as regards the oyster, is believed by some of our best physicians. We intend in a future series of experiments to direct our attention to this part of the subject.

In order that our views on this subject should not be misunderstood, it is necessary for us to state, that we do not consider the proportion of nitrogen, taken alone, to be an absolute measure of the nutritive power of our aliments; but as there is a total want of any positive data in regard to this subject, and as so many different and contradictory opinions are given in the works on dietetics*, it must be granted that any attempt to fix a standard for comparison is not without interest.

With respect to the capability for nutrition, we are far from denying that the physical condition, the state of admixture, the peculiar kind of proteine compound, the amount of water and other inorganic matters, of fat, and lastly, the effects of cookery, must necessarily have a very great influence on the physiological effects of our aliments.

There is one consideration in particular which requires to be noticed, namely the distinction between the absolute amount of nutritive matter and that portion of it which is in such a state as to be easily digested and assimilated by the system, in the same way as a soil produced from minerals abounding in alkaline salts is not always the most fertile as regards plants with a predominance of alkaline bases, but the fertility of which depends on the amount of these bodies contained in a state in which they can be taken up and made use of by the plant. Thus an aliment abounding in nutritive matter may be inferior to one with a much smaller quantity as regards the nourishing effects produced by it; if in the first of these cases only a part, in the second the whole may be easily absorbed and assimilated. Here we must rely upon the experiments of the physiologist as to the degree of digestibility of different substances; and there already exists an excellent basis in the researches of Beaumont and of Blondlot. This is one of the questions, in which, by the co-operation alone of the physiologist and chemist, any progress can be expected to be made

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* See all works from Plenk's *Bromatologia* to the latest, as Paris's Treatise on Diet, as well as our standard works on Materia Medica, and we think it will be allowed that we have not asserted too much.

in the elucidation of this point. If the work were once fairly begun by the physiologist, chemistry would not be long in rendering that assistance which would then be found necessary; for example, in the various experiments which might be made in artificial digestion, from the results of which as one element in the calculation taken along with the proportion of nitrogen as the other, it would at once be possible to determine scientifically the real nutritive value of the different kinds of aliment; the importance to our dietetics of this method of determination, which at present exists only in idea, would then not only be felt by the patient, but by the whole human race.

Table of the comparative proportion of nutriment in our organic aliments. If we assume the amount of nitrogen in human milk, perfectly dried at 212° F., to be represented by 100, we can then express the degree of nutritive power of the other alimentary substances by the following numbers:—

Vegetable.	
Rice	81
Potatoes	84
Turnips	106
Rye	106
Maize	100 to 125
Barley	125
Unfermented bread of Glasgow	134
Oats	138
White bread	142
Wheat	119 to 144
Carrots	150
Brown bread	166
Agaricus cantharellus	201
Peas	239
Agaricus russula	264
Lentils	276
Haricot beans	283
Agaricus deliciosus	289
Beans	320
Animal.	
Human milk	100
Cow's milk	237
Oyster	305
Yolk of egg	305
Cheese	331 to 447
Eel, raw	434
... boiled	428
Liver of crabfish	471
Mussel, raw	528
... boiled	660
Ox-liver, raw	570
Pork-ham, raw	539
... boiled	807
Salmon, raw	776
... boiled	610
Liver of pigeon	742
Portable soup	764
White of egg	845
Crabfish, boiled	859
Skate, raw	859
... boiled	956
Herring, raw	910
... boiled	808
... milt of	924
Haddock, raw	920
... boiled	816
Flounder, raw	898
... boiled	954
Pigeon, raw	756
... boiled	827
Lamb, raw	833
Mutton, raw	773
... boiled	852

Animal (continued).

Veal, raw	873
... boiled	911
Beef, raw	880
... boiled	942
Ox-lung	931

Purified muscular fibre from various animals.

Fibre of eel	908
... salmon	982
... herring	914
... haddock	988
... flounder	988
... pigeon	775
... lamb	916
Fibre of sheep	928
... calf	993
... ox	935
... sow	893

Proximate principles of animals, calculated from the quantity of nitrogen as determined by Mulder.

Pure proteine	1006
... albumen	996
... fibrine	999
... caseine	1003
... gelatine	1128
... chondrine	910

LIV. *On the Evaluation of the Sums of Neutral Series.* By J. R. YOUNG, *Professor of Mathematics in Belfast College**.

THE attention of several analysts has been called of late to the series $1-1+1-1+1$ ......, and the long-prevailing doctrine that the sum of this series to infinity is necessarily $\frac{1}{2}$, is beginning to be questioned, and the true character of it to be recognised. There is danger however of mistake arising in another direction, for it has been affirmed that this series cannot be $\frac{1}{2}$, even when regarded as the limit of the converging cases of the general series $1-x+x^2-x^3+x^4\dots$ It is my wish in the present communication to point out the error of this statement, and to notice the circumstances which appear to have led to it.

And in the first place I would observe, that when the series in question is intended to represent that which the general converging series becomes in the extreme case of $x=1$, it is considered as identical with $1-1+1^2-1^3+1^4\dots\dots 1^{\infty'}$, or rather,—indicating more explicitly the connexion of this extreme case with the general series of converging cases that it terminates,—it is considered as identical with

$$1-\left(1-\frac{1}{\infty}\right)+\left(1-\frac{1}{\infty}\right)^2-\left(1-\frac{1}{\infty}\right)^3+\dots\left(1-\frac{1}{\infty}\right)^{\infty'}.$$

Again, the general expression for n terms of the series $1-x$

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* Communicated by the Author.

$+ x^2 - x^3 +$ &c. is

$$\frac{1}{1+x} - \frac{(-x)^{n+1}}{1+x}, \quad \ldots\ldots\ldots \quad [1.]$$

the accuracy of which expression, under all circumstances as to the values of x and n, this latter being a whole number, is universally admitted. It is also allowed, when n is infinite, that all the *convergent* cases of the series, short of the extreme case $x = 1$, are comprised in the single fraction $\frac{1}{1+x}$. Now although I hold it to be an axiom, that if the expression suffice for all cases, short of the extreme case, it must of necessity suffice for that too, yet it may be well to show the entire consistency of this truth with the general expression [1.] for $n =$ infinite.

In order to this, let $1 - \frac{1}{k}$ be put for x; then it is plain that we shall commit no error if for $n + 1$ we write $k \, . \, \infty'$; so that the sum of the infinite series

$$1 - \left(1 - \frac{1}{k}\right) + \left(1 - \frac{1}{k}\right)^2 - \left(1 - \frac{1}{k}\right)^3 + \ldots\ldots \left(1 - \frac{1}{k}\right)^{\infty'}$$

will be correctly expressed by

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

whatever be the value of k.

Take now the extreme case, $k = \infty$, which in fact is that of $x = 1$, and we have

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

But it is well known that $\left(1 - \frac{1}{\infty}\right)^{\infty} = \frac{1}{e}$, and that $\left(\frac{1}{e}\right)^{\infty'} = 0$, consequently

$$S = \frac{1}{1 + \left(1 - \frac{1}{\infty}\right)} - \frac{0}{1 + \left(1 - \frac{1}{\infty}\right)} = \frac{1}{2}.$$

The extreme case of the general series $1 - x + x^2 - x^3 +$ &c., which we have here been considering, the case namely of $x = 1$, has been considered exclusively in reference to its connexion with the *convergent* cases of that series; but the same extreme case may be regarded as limiting the *divergent* cases, and therefore—its connexion exclusively with these cases being symbolized—as identical with

$$1-\left(1+\frac{1}{\infty}\right)+\left(1+\frac{1}{\infty}\right)^2-\left(1+\frac{1}{\infty}\right)^3+\ldots\ldots\left(1+\frac{1}{\infty}\right)^{\infty'};$$

so that reasoning as before, and recollecting that $\left(1+\frac{1}{\infty}\right)^\infty = e$, and that $e^{\infty'}$ is infinite, we infer that the limiting case of the series of divergent cases is, like every one of the cases which precedes it, infinite.

But the series $1-1+1-1+$ &c. is frequently met with in analysis uncontrolled by any law binding it in connexion with a continuous series of either convergent or divergent cases, and where, in consequence, it would be unwarrantable to replace it by either of the forms employed above, since in these the fact of such connexion is, as an essential condition, impressed upon each; and it is from overlooking this circumstance that the series $1-1+1-1+$ &c. is so often erroneously assumed to be the representative of $\frac{1}{2}$. As an illustration, suppose the summation of the infinite series

$$\frac{2}{3\cdot5}-\frac{3}{5\cdot7}+\frac{4}{7\cdot9}-\frac{5}{9\cdot11}+\text{\&c.}$$

were required: the true value would be obtained by subtracting the lower of the following series from the upper, and then dividing the remainder by 2:

$$\frac{2}{3}-\frac{3}{5}+\frac{4}{7}-\frac{5}{9}+\text{\&c.}$$

$$\frac{2}{5}-\frac{3}{7}+\frac{4}{9}-\text{\&c.}$$

The remainder adverted to is evidently $\frac{2}{3}$ *minus* the series

$$1-1+1-1+1-1+\text{\&c. } \textit{to infinity} \quad . \quad . \quad [2.]$$

$\pm\frac{\infty}{2\infty+1}$, seeing that the lower series must always project one term to the right beyond the upper. Now if we replace the series [2.] by $\frac{1}{2}$, twice the sum of the proposed series will be ambiguously $\frac{2}{3}-\frac{1}{2}\pm\frac{1}{2}$; which is absurd.

It is plain that the sum of the series [2.] is to a certain extent indeterminate, being indifferently 1 or 0; and on the choice of these will obviously depend the sign with which $\frac{\infty}{2\infty+1}$, or, which is the same thing, $\frac{1}{2}$ is to be taken:—it will be *minus* for the first mentioned value of [2.], and *plus* for the other, so that the sum of the proposed series is $\frac{1}{2}\left(\frac{2}{3}-\frac{1}{2}\right)=\frac{1}{12}$.

The term *neutral series*, a term I believe first adopted by Hutton, should I think be in strictness confined to the forms which arise, as in this example, independently of all connection with, and therefore uncontrolled by, the laws which govern general algebraic series.

It may not be superfluous to remark, in passing, that the series here discussed is very intimately connected with the doctrine of definite integrals, a doctrine into which a good deal of error will be found to have crept. A very able contributor to this Journal, Mr. R. Moon, Fellow of Queen's College, Cambridge, has, I see, recently turned his attention to this important topic (Phil. Mag. vol. xxvi. p. 483). It will be seen however, from the present communication, that I have been constrained to differ from him, as to the general theory of the series $1-1+1-1+$ &c., regarding that series as strictly and exclusively $\frac{1}{2}$, when it is the limit of the converging cases of $1-x+x^2-x^3+$ &c. I will merely add further, that I invited the attention of analysts to this subject, in connexion with non-converging series generally, in a paper read before the British Association at the York meeting in 1844, a short abstract of which appears in the volume of published reports: the same paper was also read at the Royal Irish Academy in January 1845, and is published at length in the Proceedings.

It is perhaps scarcely necessary to state, that although in the preceding reasoning I have considered $1-1+1-1+$ &c., when really a limit, only in connexion with $1-x+x^2-x^3+$ &c., yet the same reasoning applies when it is viewed in connexion with $1-x^2+x^3-x^5+$ &c., or in general with $1-x^\alpha+x^\beta-x^\gamma+$ &c.; and, when the series is strictly neutral and isolated, that I regard the assumption, which would connect it with any particular case of this last general algebraic form, to be no more unwarrantable than the assumption ordinarily made of its connexion with $1-x+x^2-x^3+$ &c. It is this assumption which will be found to vitiate several important and generally received results of analysis, for it is only when the assumed connexion actually exists, that the unique value, usually attributed to the series, ought to be admitted, and then it may be admitted with perfect confidence. This I now proceed to exemplify.

[To be continued.]

LV. *On the Oil produced by the Action of Chlorine on Cinnamic Acid.* By JOHN STENHOUSE, *Ph.D.**

IN a paper on the Action of Chlorine on Cinnamic and Benzoic Acids, published in the August number of this Magazine, I described the formation and some of the properties of a remarkable oily compound which is invariably formed whenever cinnamic acid is heated in contact with either chlorine or hypochlorous acid. I shall now proceed to detail some additional observations which I have been able subsequently to make upon the oil; but as the mode of its formation has been pretty fully described in the previous paper, I do not think it necessary to repeat it here.

The oil is heavier than water and has a very peculiar aromatic smell, which reminds one at once of the oil of bitter almonds and of that of *Spiræa Ulmaria*, though it is not exactly like either of these bodies. When strongly heated, it readily catches fire, burns with a green-coloured flame, and emits fumes of muriatic acid gas. When boiled with potash lye, it is partially decomposed with the formation of chloride of potassium. When some bits of sodium are put into it, bubbles of a gas, apparently hydrogen, are given off, the oil becoming hot and inflaming with explosion. Ammonia does not appear to act upon it, either in the liquid or gaseous state. Sulphuric acid does not dissolve it in the cold, but with the assistance of heat first reddens and then chars it. A portion of the oil, which had been rendered anhydrous by being allowed to stand for some days over a mixture of quicklime and fused chloride of calcium, was then very cautiously distilled. The first portion which came over was colourless and neutral, but what followed became more and more acid, the last portions exceedingly so, evolving fumes of muriatic acid and assuming a deep yellow colour. The first portion of the oil which had been dried in this way was subjected to analysis. The chlorine contained in the oil was determined by passing it very slowly over a large quantity of red-hot quicklime in a very long tube. Unless great care is taken to conduct this operation very slowly, the oil passes over the hot lime without giving up all its chlorine. When a condensing apparatus is attached to the open end of the combustion-tube containing the lime, an agree-

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* Communicated by the Author.

ably smelling oily liquid collects in it. Its smell resembles that of the other benzyle compounds, and it crystallizes on cooling. Its quantity however is extremely small.

Per cent.
1. 0·5533 oil distilled over quicklime gave 0·5515 Cl Ag = 24·58 Cl
2. 0·259 grm. gave 0·259 Cl Ag = 24·67 Cl
Mean ...... 24·62 Cl

a. 0·3201 grm. burned with chromate of lead gave 0·1395 HO and 0·793 CO_2.

b. 0·2569 burned with chromate of lead gave 0·1145 HO and 0·632 carbonic acid.

	a.	*b.*
Carbon . .	67·56	67·09
Hydrogen .	4·84	4·95
Chlorine .	24·62	24·62
	97·02	96·66

As the oil was always partially decomposed when distilled off fused chloride of calcium, another portion of it was repeatedly rectified with the vapour of water. The oil then came over quite colourless and neutral. This operation also freed it from a quantity of resinous matter which very readily forms in it. The purified oil, when freed from water as well as possible, was still more completely dried by being kept for some weeks over sulphuric acid *in vacuo*. The sulphuric acid on standing smelt strongly of oil of bitter almonds.

0·2781 oil well-washed with water and dried under the air-pump, gave 0·702 carbonic acid and 0·134 HO.

0·2602 oil gave 0·2245 Cl Ag = 24·17 Cl per cent.

Carbon . .	68·84
Hydrogen .	5·35
Chlorine .	24·17

A second quantity of the oil was prepared in a similar way, and also dried *in vacuo* over sulphuric acid.

a. 0·4149 oil gave 0·3065 Cl Ag = 18·22 Cl per cent. } Mean.
b. 0·256 oil gave 0·192 Cl Ag = 18·50 Cl per cent. } 18·36
c. 0·4193 gave 0·202 HO and 1·0835 carbonic acid.
d. 0·357 gave 0·1795 HO and 0·9242 carbonic acid.

	c.	*d.*
Carbon . .	70·47	70·62
Hydrogen .	5·35	5·57
Chlorine .	18·36	18·36

When the chlorinated oil is treated with strong nitric acid, it is readily attacked with copious evolution of deutoxide of nitrogen; and at the same time a very pungent disagreeable vapour, which affects the eyes strongly, is given off. When the liquid is concentrated and allowed to cool, the oil is found to be converted into a crystalline mass. The crystals were purified by being repeatedly crystallized; they had a strong acid reaction and were very soluble, both in hot water and in alcohol, out of which liquids they crystallized very readily. When dried at 212° F. and subjected to analysis, 0·2128 grm. substance gave 0·388 CO_2 and 0·061 HO.

	Calculated numbers.		
	Atoms.	per cent.	Found.
Carbon .	14	50·25	49·72
Hydrogen	5	2·99	3·18
Nitrogen .	1	8·47	
Oxygen .	8	38·29	
		100·00	

The silver salt was prepared by neutralizing the acid with ammonia and precipitating the solution with nitrate of silver. The salt formed a rather bulky precipitate.

a. 0·2225 of the silver salt gave 0·0844 metallic silver = 38·83 per cent. oxide; the calculated quantity is 39·12 per cent. oxide.

b. 0·2985 of the silver salt gave 0·329 carbonic acid and 0·041 water.

	Found.	Calculated.
Carbon	30·06	30·60
Hydrogen . .	1·52	1·45
Oxide of silver .	38·83	39·12

It is evident from the results of these analyses that the acid and the salt have precisely the composition of nitrobenzoic acid and its silver salt, with the character of which substances they exactly correspond. I am induced to believe, therefore, that the oil is essentially a carburetted hydrogen belonging to the benzoic acid series, in which variable quantities of the hydrogen are replaced by chlorine; and that the small quantity of oxygen it contains, amounting in some instances to little more than two per cent., is the result of foreign admixture. The most probable source of this impurity is a little oil of bitter almonds, for, so far as we know, oil of bitter almonds is always generated whenever cinnamic acid is treated with an oxidizing agent; at least it is so when cinnamic acid is boiled with peroxide of lead, with chromic acid, or with sulphuric acid and peroxide of manganese, or any other hyperoxide. This view of the constitution of the chlorinated oil is strongly confirmed by the effect of nitric acid upon it, which, as we have seen, converts it into nitrobenzoic acid. Now nitric acid, as is well known, also converts oil of bitter almonds into nitrobenzoic acid, so that the result obtained is exactly what might have been expected from the action of nitric acid on a carburetted hydrogen of the benzoic acid series containing a little oil of bitter almonds.

In conclusion, I may mention that the production of this oil, from its striking and peculiar properties, furnishes an excellent test for detecting the presence of cinnamic acid.

A quantity of the chlorinated oil was also treated for some days, both exposed to the light of the sun and in diffused light, with dry chlorine, in the hope that perhaps a crystalline compound might be produced. In this however I was unsuccessful; much muriatic acid was given off and the liquid became tolerably viscid, but no crystals were formed.

LVI. *Note on the Existence of Phosphoric Acid in the Deep-Well Water of the London Basin. By* Thomas Graham, *Esq., F.R.S.**

THIS water is obtained on piercing the London clay, which forms an impervious bed, generally exceeding 200 feet in thickness, and flows from fissures in the subjacent chalk. It is always highly soft and alkaline, and remarkable for the predominance of soda salts over earthy salts among its solid constituents. I have never found it to contain a sensible quantity of potash, although salts of the vegetable alkali appear among the constituents of the water of the deep Artesian well of Grenelle.

When evaporated considerably, a small deposit takes place in the London deep-well water, which consists chiefly of carbonate and phosphate of lime. The remaining liquid gives with nitrate of silver a precipitate of chloride and carbonate of silver, which is white without any shade of yellow; but if a portion of the water, amounting to an ounce or two, be evaporated to dryness in a platinum capsule, without removing the precipitate, and the heat afterwards continued so as to raise the temperature of the resulting dry saline matter to low redness, then, on redissolving by distilled water, and adding nitrate of silver, a precipitate is obtained, in which the yellow colour of the phosphate of silver is very perceptible. The earthy phosphate is decomposed by ignition with the alkaline belonging to the water, and the soluble phosphate of soda is produced.

The following are the results of the analysis of the water from the deep well in the Brewery of Messrs. Combe and

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* Communicated by the Chemical Society; having been read May 5, 1845.

Delafield, Long Acre. An imperial gallon of the water contained 56·45 grains of solid matter, 100 parts of which gave—

Carbonate of soda	20·70
Sulphate of soda	42·94
Chloride of sodium	22·58
Carbonate of lime	10·96
Carbonate of magnesia	1·92
Phosphate of lime	0·34
Phosphate of iron	0·43
Silica	0·79
	100·66

The growth of green confervæ in this water is extremely rapid, and occasions inconvenience when the water is kept in open tanks. It is a subject perhaps worthy of inquiry, whether the value of some waters for irrigation may not depend upon their containing phosphoric acid, this constituent having hitherto been generally overlooked in waters.

LVII. *On a Crystallized Alloy of Zinc, Iron, Lead and Copper. By* Warren De la Rue, *Esq.*[*]

THE alloy in question was obtained from the worn-out amalgamated zinc plates used in the voltaic battery after the mercury had been recovered by distillation. As the sulphate of zinc, resulting from the solution of the zinc in the battery, is exceedingly pure, it follows that the residue of the plates contains, besides the mercury used for amalgamation, most of the impurities contained originally in the whole plates, and the metal obtained therefrom is consequently much inferior in quality to the original rolled zinc.

Zinc in a fit state for rolling is obtained by running off the fluid portion from a mass of cast zinc which has been allowed to cool down to a certain point after fusion, the metal left behind being less pure than that which flows off. The manufacturer who furnishes me with rolled zinc and takes back the zinc obtained from the worn-out plates, informed me that the latter is unprofitable to re-work from its leaving an unusually large residue in refining; this statement induced me to investigate the composition of this residue, and I proceeded in the following manner.

About 22 pounds of worn-out plates being introduced into an iron pot, and the mercury distilled off by heating it to redness, the fused metal was fined by adding a quantity of tallow and

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[*] Communicated by the Chemical Society; having been read May 5, 1845.

stirring well; after the oxide had been withdrawn the metal was removed to another vessel to cool; it weighed about 15 lbs. When partially solidified the fluid portion was drained off, leaving a considerable portion of the metal in a spongy state, and occupying the same bulk as the whole mass; one part of zinc plate residues usually gives the following results:—

Zinc	·673
Mercury	·043
Dross and loss . .	·284
	1·000

this average being taken on a quantity of old plates amounting to 2½ hundred weight.

The spongy residue, examined with the microscope under a power of twenty to fifty times linear, proved to be composed of minute, perfectly clean and well-formed crystals of the form of right rhombic prisms; their breadth varying from the $\frac{1}{600}$th to $\frac{1}{300}$th of an inch; the angles of the base are of about 119° and 61° respectively.

A selected portion, free from scoria, weighing 5 grammes, was subjected to analysis, and found to be an alloy of zinc, iron, lead and copper. After solution in nitric acid the lead was separated as sulphate, which weighed ·441 grm.

The copper being precipitated by sulphuretted hydrogen and then converted into oxide weighed ·09 gramme. The iron being thrown down as succinate from the neutralized solution, and after the removal of the soluble salts, washed with ammonia, and ignited, there resulted ·1847 gramme of peroxide of iron.

The zinc, precipitated by carbonate of soda, gave when ignited 5·6213 grammes of oxide of zinc.

These results give the following composition of the alloy:—

	Grammes.		Per cent.
Zinc	4·500	=	90·
Iron	·128	=	2·56
Lead	·300	=	6·
Copper	·072	=	1·44
	5·000		100·00

These quantities agree very exactly with the following formula:—

	Per cent. Calculated.	Per cent. Found.
240 Zn =	89·99	90·
8 Fe =	2·52	2·56
5 Pb =	6·02	6·
4 Cu =	1·47	1·44

It is possible that these substances may be combined into proximate constituents, and the alloy be a compound of these, but we have no data for so grouping them. On re-melting, however, a portion of these crystals, the mass separated into two portions,—a metal considerably more fluid, and a compound more infusible than the crystals; these I have not examined.

From the preceding analysis, it will be seen that a portion of alloy amounting to but 10 per cent. is capable of retaining in combination 90 per cent. of zinc: this will account for the large portion of difficultly fusible metal in a pasty state which frequently rises to the top of the melting-pot in fusing commercial zinc, and which is probably an alloy of zinc combined with but a small portion of other metals.

LVIII. *Some Experiments on Ozone.*

By A. W. Williamson, *Esq.*[*]

IT has been satisfactorily proved by Marignac[†], that the phænomena attributed to ozone have no connexion with the presence of nitrogen. He finds that the ozone odour may be developed in liquids free from nitrogen as well as in those which contain that element. He decomposed by the voltaic current a portion of water from which atmospheric air was carefully excluded, and found that the peculiar smell of ozone was given off, as abundantly after a continuance of the action for several days, when a quarter of the liquid had disappeared in the form of gas, as at the beginning of the decomposition. Marignac confirms Schönbein's statement, that the odorous matter in air acted upon by phosphorus, is identical with that present in the oxygen set free by the electrolytical decomposition of water; and indeed recommends, as the most convenient way of preparing ozone, to pass atmospheric air over phosphorus. The substance made use of in his researches was thus prepared.

The object of the experiments I am about to describe was to obtain some explanation of the phænomena which gave rise to the supposition of the existence of such a body as ozone. The poles of a Bunsen's battery, consisting of four elements, were plunged into sulphuric acid diluted with three volumes of water. The hydrogen from the copper plate at which it was evolved was allowed to escape into the atmosphere. The oxygen which was evolved upon a plate of platinum was col-

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

[†] *Comptes Rendus à l'Académie*, Mars 1845.

lected in a small tubulated glass bell placed over it, and two-thirds sunk in the liquid. Hence it was conveyed into a tube filled with chloride of calcium, through which it passed without any diminution of its peculiar smell. The oxygen thus dried was conducted into a glass tube filled with copper turnings and heated to redness. At the further end of this tube, which was kept cool, globules of water soon made their appearance, which on being removed were speedily reproduced, and continued increasing so long as the process lasted. The oxygen, on entering the tube full of copper, being quite dry, it thus appears that water is formed by the reducing action of the heated metal.

In the manner of performing this experiment, there are two circumstances which take from the result the certainty of correctness necessary for drawing conclusions. These circumstances are the following:—

1st. The copper turnings having, by heating to redness in the air, been covered with a coating of oxide, were reduced by means of a current of hydrogen gas, and although the excess of that gas had been removed as far as possible by a current of dry atmospheric air, yet it was possible that some hydrogen might adhere to the porous copper, which would account for the formation of water on oxygen being brought to the hydrogen at a red heat.

2nd. Hydrogen being somewhat soluble in water, the oxygen may in passing through the liquid have taken up some traces of that element, and on coming in contact with the heated metal the two gases would combine.

To obviate the first of these objections, the oxidated copper was reduced by a current of dry carbonic oxide gas, and to remove the possibility of free hydrogen being carried over with the oxygen, the latter was passed through a tube filled with spongy platinum, and then again over chloride of calcium. It was, however, found that the substance producing the peculiar odour was either decomposed or absorbed by the platinum, none of it passing over.

The difficulty was at length avoided by evolving the oxygen from a liquid in the decomposition of which no hydrogen is set free. Oxide of copper dissolved in sulphuric acid was decomposed instead of water. The oxygen then evolved possessed precisely the same odour as that from acidulated water, and after careful drying was conducted into the heated tube containing copper turnings reduced by carbonic oxide.

Water speedily appeared at the cool end of the tube, and the quantity continued increasing as long as the process lasted.

In subsequent experiments, oxygen prepared in the manner just described was dried and passed through a small glass tube, heated to redness by a spirit-lamp; on coming out of which it had completely lost all odour. At the end of this tube a chloride of calcium tube was then fixed, which had been accurately weighed.

After the oxygen was conducted for a short time through the heated tube, the chloride of calcium tube was found to have increased perceptibly in weight.

Water through which oxygen charged with ozone was allowed to pass assumed the odour of it. When this solution of ozone was added to a solution of iodide of potassium and starch, a pale blue colour was produced; ferro-cyanide of potassium containing ozone gave a blue precipitate with a proto-salt of iron. Lime water formed with the solution of ozone a heavy and apparently crystalline precipitate. Baryta water behaved in a similar manner. The liquid after the precipitation gave no reaction with iodide of potassium and starch; but on an acid being added, a blue colour immediately appeared. The odour did not reappear.

Oxygen charged with ozone was next passed through a tube surrounded by a frigoric mixture consisting of chloride of calcium and snow; but nothing perceptible was deposited.

The following experiments were made for the purpose of ascertaining whether, as has been assumed, the substance causing the ozone odour is also produced by the action of phosphorus on atmospheric air.

Through a glass tube filled with pieces of phosphorus a current of moist atmospheric air was driven by means of a gasometer; it assumed the peculiar odour so well known to accompany phosphorus. Water, through which this air was then allowed to pass for a considerable time, remained inodorous, gave not the slightest reaction with iodide of potassium and starch, left ferro-cyanide of potassium unchanged, and gave all the reactions of a dilute solution of phosphoric acid.

On repeating this experiment it was performed in a somewhat different manner. The air which had passed over phosphorus was allowed to pass directly into the iodide and starch, and a deep blue reaction soon ensued in that part of the liquid on which the bubbles in passing through first acted; and was increased, in a greater proportion to the air passing through, when a rapid current of air was driven over the phosphorus instead of a slow one. On again, as in the former instance, passing the air through water and then treating the latter with iodide of potassium and starch, no reaction was obtained. We have here an evident difference between the reactions of the substance contained in the electrolytic oxygen and that produced by the action of phosphorus on atmospheric air. To what then is to be attributed the reaction produced by the direct action of air passed over phosphorus on iodide of potassium and starch? The following experiment was made. In a long and wide tube loose pieces of asbestos and pieces of phosphorus were placed alternately. By heating the tube thus filled the phosphorus was partly melted into the asbestos, and partly sublimed upon it; thus exposing a far greater surface than in the former experiments. After the tube had completely cooled, atmospheric air was driven through it into the iodide and starch, but not the slightest reaction was produced.

This result plainly indicates the cause of the reaction in the preceding instance. The phosphorus was there unable for want of sufficient surface to absorb all the oxygen of the air passing over it. A mixture of phosphoric acid and oxygen therefore went over, and by their simultaneous action on the iodide of potassium set iodine free. As soon as a sufficient surface was given to the phosphorus all oxygen was absorbed, phosphoric acid was alone carried over by the nitrogen, and the iodide was decomposed in a different manner.

Lime water, through which the air thus treated with phosphorus was passed, deposited a voluminous sediment, consisting of phosphate of lime. This liquid cleared by a few drops of acid remained afterwards unclouded on being heated with chloride of mercury: it consequently contained no phosphorous acid. With iodide and starch it gave no reaction, either alone, or after an acid was added to it.

These experiments appear to me to show,—

I. That the peculiar properties belonging to the oxygen set free by the agency of the electric current are produced by the admixture of a *peroxide or acid of hydrogen*;

II. That by the action of phosphorus on atmospheric air the same substance is *not* produced.

That this compound of hydrogen and oxygen is not identical with Thénard's peroxide, would appear from the fact of its being volatile and odorous, properties which the other does not possess. I have repeatedly prepared Thénard's peroxide of hydrogen by decomposing peroxide of barium with different acids, for the purpose of satisfying myself whether it ever possesses odour, but have always obtained a negative result. The oxygen disengaged from it by the action of peroxide of manganese is as inodorous as the liquid itself. The oxygen obtained by the electrolytical decomposition of an aqueous solution of peroxide of hydrogen possesses the same odour, only apparently in a somewhat greater degree, as that from water.

The bleaching action which the substance contained in electrolytical oxygen produces when dry on litmus paper, is a fact which of itself indicates that it must be a peroxide. It is well known that chlorine does not possess that property, but only such combinations of oxygen as give up this element with great ease, as for instance hypochlorous acid.

I cannot conclude this notice without expressing the obligation I am under to Professors Liebig and Buff for their kind direction and assistance during these experiments.

LIX. *Some Researches upon the Connexion between the Rotation of the Earth and the Geological Changes of its Surface.* By Henry Hennessy, *Esq.*[*]

It is now generally acknowledged that the agency of modern causes in producing geological changes on the surface of the earth is of no inconsiderable importance. Although the amount of matter which in the course of centuries may change its position on the surface of the earth, by the action of elevating and degrading forces, is thus locally important to the earth's surface, it must appear insignificant when compared with the mass of the entire globe. From this circumstance, it may at first appear futile to examine how the rotation of the earth could be affected by the mechanical action of the changes in position of such comparatively minute portions of its mass. It may be said that observation has not yet disclosed any irregularity in the period of the earth's rotation, but this could occur when the causes producing such an irregularity would be counteracted by others having a contrary tendency.

It is here proposed to examine how certain changes of position, with respect to the earth's centre of masses on its surface, would affect its rotation. Let ω represent the earth's angular velocity of rotation, M its mass, v the sum of the velocities impressed on it, by which both its rotation on its axis and its translation through space are produced, h a perpendicular from its centre to a plane passing through the centre of the impressed forces, and I the moment of inertia of the whole mass. Then

$$\omega = \frac{\mathrm{M}vh}{\mathrm{I}} \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

From this well-known formula, it is evident that any change in the value of I must produce a corresponding change in the value of ω. If θ represent the arc through which a point on the surface of the earth is carried during a certain time t, and if t' represent t when ω becomes ω' by a change of I into I′, then

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[*] Communicated by the Author.

$$\omega = \frac{\theta}{t}, \quad \omega' = \frac{\theta}{t'}, \quad \omega' = \omega \frac{\mathrm{I}}{\mathrm{I}'}.$$

Hence

$$t' = t \frac{\mathrm{I}'}{\mathrm{I}} \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

It is thus apparent that when the moment of inertia of the earth changes by a known quantity, the change in sidereal time can be calculated.

This fact furnishes a method for ascertaining whether, during certain defined intervals of time, an equilibrium has been maintained between the forces which tend to elevate or to depress the external crust of the earth. If any unknown *residual phænomena* exist on either side, their existence will be made known by finding the difference between the total mechanical actions exerted by the known elevatory and degrading forces.

The earth may be considered as a spheroid surrounded by a thin shell, whose external surface is covered with protuberances and depressions. A change in the moment of inertia of this shell produces a change in the moment of inertia of the whole earth, which is compounded of that moment, and of the moment of inertia of the internal spheroid. In finding both of those moments of inertia, the following general method is employed.

It is well known that if a solid of revolution be supposed to consist of an infinite number of plates perpendicular to the axis, and each plate of an infinite number of concentric rings, the moment of inertia of any zone will in general be expressed by

$$\int_{l'}^{l} \int_{0}^{y} 2\pi\rho r^3\, dr\, dx; \quad . \quad . \quad . \quad . \quad (a.)$$

π being the ratio of the diameter of a circle to its circumference, r the radius of one of the rings, ρ its density, x the abscissa, and y the ordinate of the generating curve. The distance of the extremities of the zone from the origin of the co-ordinates, are expressed respectively by l and l'. When the body is heterogeneous, it will be found more convenient to transform the rectangular into polar co-ordinates. Let R represent the radius vector of a point in the ring, θ the angle comprised between the plane of R, y and the plane of x, y, and Ψ the angle comprised between R and the plane of y, z. As R is the same for all points in the ring, the plane of R, y and the plane of x, y coincide, and $\theta = 0$. If the origin of the co-ordinates be at the extremity of the axis of x, and if the distance from the point where the radius vector of the ring touches the axis be called A′, then $x = A' (1 - \sin \Psi)$, $R = \sqrt{r^2 + a^2 \sin^2 \Psi}$. Thus, if ρ be given as a function of a or R, it can be expressed as a function of r and Ψ, or $\phi (r, \Psi)$. y, a function of x, can also be expressed as a function of Ψ or $F(\Psi)$, and (a) then becomes

$$-\int_{\Psi_2}^{\Psi_1} \int_0^{F\Psi} 2\pi A' \phi(r, \Psi)\, r^3 \cos \Psi \, dr\, d\Psi. \quad . \quad (3.)$$

In the case of a spheroid, where A′ is the semi-polar axis, and the plane of y, z coincides with the plane of the equator, Ψ is evidently the latitude of the ring, and $y = a(1+e)\cos\Psi$, e representing the ellipticity of the generating curve. If the earth be supposed to consist of an infinite number of spheroidal shells, it has been shown that the density of any shell which will agree best with the known ellipticity of the earth and its mean density, is represented by the formula*

$$\rho = \frac{A \sin q a}{a},$$

where ρ represents the density of the shell, a its semi-polar axis, and A and q constants. The value assigned to q is expressed by $q = \frac{5\pi}{6p}$, p representing the semi-polar axis of the entire spheroid. If I_1 represent the moment of inertia of the internal spheroid, then

$$a = \frac{r}{(1+e)\cos\Psi}, \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

and

$$I_1 = -\int_{\Psi_2}^{\Psi_1} \int_0^{b\cos\Psi} 2\pi A p r^2 \cos^2\Psi . (1+e) \sin\left(\frac{5\pi r}{6p(1+e)\cos\Psi}\right) dr\, d\Psi,$$

b representing the semi-equatorial axis of the entire spheroid. But†

$$e = \frac{5m}{2} \cdot \left\{ \frac{\left(1 - \frac{qa}{\tan qa}\right)^2}{2 - q^2a^2 - \frac{qa}{\tan qa} - \frac{q^2a^2}{\tan^2 qa}} \right\} \left\{ \frac{1 - \frac{3}{q^2a^2} + \frac{3}{qa \tan qa}}{1 - \frac{qa}{\tan qa}} \right\}.$$

To completely eliminate e and a between this equation and (4.), is a step which must be at present considered impossible. An approximate solution could be obtained, but the resulting expression would be so long and complicated as to be entirely useless. It is evident, however, that when e is small, its equality in every shell can be assumed without material error. Let e therefore be equal to the ellipticity of the external shell, so that

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* Airy on the Figure of the Earth.

† Ibid.

$$a = \frac{r\,p}{b\cos\Psi}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

then

$$\mathrm{I}_1 = -\int_{\Psi_2}^{\Psi_1}\int_0^{b\cos\Psi} 2\pi \mathrm{A} b r^2 \sin\left(\frac{5\pi r}{6b\cos\Psi}\right)\cos^2\Psi\, dr\, d\Psi; \quad (6.)$$

$$= -2\pi\mathrm{A}\left\{\frac{5}{3}\pi\sin\frac{5}{6}\pi - 2 - \left(\frac{25}{36}\pi^2 - 2\right)\cos\frac{5}{6}\pi\right\}\frac{216}{125\pi^3}\int_{\Psi_2}^{\Psi_1} b^4\cos^5\Psi\, d\Psi.$$

When $\Psi_1 = \frac{\pi}{2}$, and $\Psi_2 = -\frac{\pi}{2}$, or when the above is integrated throughout the whole mass of the spheroid, it becomes

$$\mathrm{I}_1 = \frac{6912\mathrm{A}}{1875\pi^3}\left\{\left(\frac{25}{36}\pi^2 - 2\right)\cos\frac{5}{6}\pi + 2 - \frac{5}{3}\pi\sin\frac{5}{6}\pi\right\} b^4\pi. \quad . \quad . \quad (7.)$$

When a becomes p and ρ becomes ρ', the density at the surface of the spheroid

$$\rho' = \frac{\mathrm{A}}{p}\sin\frac{5}{6}\pi, \qquad \mathrm{A} = \frac{\rho' p}{\sin\frac{5}{6}\pi}.$$

Substituting this value of A in (7.), and remembering that $\mathrm{M}_1 = \frac{8}{3}\pi\rho' b^2 p$, M_1 being the mass of the spheroid, we shall obtain

$$\mathrm{I}_1 = \frac{864}{625\pi^3}\left\{\left(\frac{25}{36}\pi^2 - 2\right)\cot\frac{5}{6}\pi + \frac{2}{\sin\frac{5}{6}\pi} - \frac{5}{3}\pi\right\}\mathrm{M}b^2. \quad . \quad . \quad (8.)$$

If the spheroid were homogeneous, we should have, after making the necessary substitutions in (3.) or (6.), and integrating throughout the whole mass of the body,

$$\mathrm{I}_2 = -\int_{-\frac{\pi}{2}}^{+\frac{\pi}{2}}\int_0^{b\cos\Psi} 2\pi p\rho r^3\cos\Psi\, dr\, d\Psi = \frac{8}{15}\pi\rho a b^4 = \frac{2}{5}\mathrm{M}_2 b^2, (9.)$$

a result easily obtained by the ordinary methods.

In finding the moment of inertia of the external shell so as to be able to appreciate the changes which it may undergo, it should be remembered that as geological changes have not the same magnitude or importance at every latitude, a change in the moment of inertia of the whole shell can be found only by considering the changes of the moments of inertia of its different parts.

Let the shell be supposed therefore to consist of a series of zones, on each of which geological changes occur in a comparatively uniform manner, whatever may be the nature or extent of the changes on any of the others. Let ξ_1, ξ_2, ξ_3, &c. represent the moments of inertia of the zones in the northern hemisphere, and ζ_1, ζ_2, ζ_3, &c. the moments of inertia of the zones of the southern hemisphere, ξ_1 and ζ_1 being those of the zones in the immediate vicinity of the equator. Then J the moment of inertia of the entire shell will be obtained by the equation

$$J=\xi_1+\xi_2+\xi_3+\&c.+\zeta_1+\zeta_2+\zeta_3+\&c.=I_2\,; \qquad (10.)$$

the external shell being so thin that the variation in its density may be neglected, and its moment of inertia found as if it were homogeneous. Let the zone ξ_1 be included between the latitude ψ_1 and the equator, the zone ξ_2 between ψ_1 and ψ_2, and so on to the zone ξ_n, included between ψ_{n-1} and ψ_n, or $\frac{\pi}{2}\cdot$ Similarly, let ζ_1, ζ_2 and ζ_n, be included respectively between the equator and θ_1, θ and θ_2, θ_{m-1} and θ_m.

The values of the moments of inertia of the zones will be then found by the equations which follow, where A_1, B_1, A_2, B_2, &c. represent the semi-polar and semi-equatorial axes of the imaginary spheroidal shells which correspond to each zone of the northern hemisphere; and A'_1, B'_1, A'_2, B'_2 have a similar meaning with respect to the zones of the southern hemisphere.

$$\left.\begin{aligned}
\xi_1&=\int_0^{\psi_1}\frac{\pi}{4}\rho A_1 B^4{}_1\cos^5\Psi d\Psi\\
\xi_2&=\int_{\psi_1}^{\psi_2}\frac{\pi}{4}\rho A_2 B^4{}_2\cos^5\Psi d\Psi\\
&\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\\
\xi_n&=\int_{\psi_{n-1}}^{\frac{\pi}{2}}\frac{\pi}{4}\rho A_n B^4{}_n\cos^5\Psi d\Psi.
\end{aligned}\right\}(11.)
\qquad
\left.\begin{aligned}
\zeta_1&=\int_0^{\theta_1}\frac{\pi}{4}\rho A'_1 B'^4{}_1\cos^5\Psi d\Psi\\
\zeta_2&=\int_{\psi_1}^{\psi_2}\frac{\pi}{4}\rho A'_2 B'^4{}_2\cos^5\Psi d\Psi\\
&\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\\
\zeta_m&=\int_{\psi_{n-1}}^{\frac{\pi}{2}}\frac{\pi}{4}\rho A'_n B'^4{}_n\cos^5\Psi d\Psi
\end{aligned}\right\}(12.)$$

The values of A_1, B_1, &c.; A'_1, B'_1, &c., must be found for each zone by the following method.

Let T represent the thickness in feet or miles of a zone of the external shell, Θ the mean latitude of the zone, α the polar semi-axis of the earth, β its equatorial semi-axis, D the depth of the lower surface of the shell below the surface of the sea, R_θ the radius of the earth at Θ, and R the radius of the internal spheroid at the same latitude. Then

$$R=R_\theta-(D-T), \qquad R_\theta=\sqrt{(\alpha^2\sin^2\Theta+\beta^2\cos^2\Theta)}.$$

Let γ correspond to α, η to β, as D − T, or G does to R_θ.

$$\text{Then}\quad \alpha\pm\gamma=\frac{\alpha\,(R_\theta\pm G)}{R_\theta}, \qquad \beta\pm\eta=\beta\,\frac{(R_\theta\pm G)}{R_\theta};$$

or,
$$\left.\begin{aligned} A &= \frac{\alpha\left(\sqrt{(\alpha^2 \sin^2 \Theta + \beta^2 \cos^2 \Theta)} \pm G\right)}{\sqrt{(\alpha^2 \sin^2 \Theta + \beta^2 \cos^2 \Theta)}}, \\ B &= \frac{\beta\left(\sqrt{(\alpha^2 \sin^2 \Theta + \beta^2 \cos^2 \Theta)} \pm G\right)}{\sqrt{(\alpha^2 \sin^2 \Theta + \beta^2 \cos^2 \Theta)}}; \end{aligned}\right\} \quad (13.)$$

A and B representing the general values of the polar and equatorial semi-axes of the shell corresponding to the zone of the shell whose thickness is T.

When investigating the moment of inertia of the shell for any zone, it will be found most convenient to consider the centres of gravity of the parts of the shell, or the surface passing through the centres of gravity of all its parts, as equidistant from its interior surface.

No matter how irregular any zone of the shell may be, its thickness can be considered without much chance of error, as the distance between two imaginary surfaces each of which is equidistant from the surface passing through the centres of gravity of all the parts of the shell. Let the internal surface of the shell be at the mean depth of the sea, the position of its external surface being determined by the distances of the centres of gravity of its parts. Let the entire shell be supposed to consist of an infinite number of pyramidal frusta, which if prolonged would form pyramids meeting at the earth's centre. The height and dimensions of the base of each frustum being infinitely small compared to the height of its entire pyramid, we can without sensible error consider it as a parallelopiped. Let H represent the mean height of the land above the level of the sea, and D the mean depth of the ocean. The height of a parallelopiped whose external surface is on dry land is then H + D, and the distance of its centre of gravity from each of the surfaces of the shell $\frac{1}{2}(H+D)$. Similarly, $\frac{1}{2}D$ represents the distance of the centre of gravity of a parallelopiped whose upper surface forms a part of the surface of the ocean, from the interior surface of the shell. Let Δs represent an indefinitely small portion of the surface of the land, $\Delta s'$ a similar portion of the surface of the sea, δ_1 the density of the land, and δ_2 the density of the ocean. The equation for the value of C_1, the thickness of the shell, will be—

$$C_1 = \frac{H\Sigma(H+D)\Delta s\,\delta_1}{\{(H+D)\delta_1\Sigma\Delta s + \delta_2 D\Sigma\Delta s'\}} + \frac{D(\delta_2\Sigma\Delta s' + \delta_1\Sigma\Delta s)}{2(\Sigma\Delta s + \Sigma\Delta s)}$$

$$= \frac{H s(D+H)\delta_1}{\{\delta_1(H+D)s + \delta_2 D s'\}} + \frac{D(\delta_2 s' + \delta_1 s)}{2(s'+s)} \quad . \quad . \quad . \quad . \quad (14.)$$

In an examination of the equilibrium of the degrading and elevatory forces on the surface of the earth, it is indifferent whether we examine the action of one or both as affecting the earth's rotation, we can therefore examine the change in C_1 either when it is diminished or increased: it will be found most convenient to examine its change in the former case. Of the two principal causes which operate in diminishing C_1 or degrading the level of the land, a knowledge of the intensity of one will suffice for our purposes. We shall therefore proceed to find an equation expressing merely the diminution of C_1 from the transportation of solid matter from the high levels of the parallelopipeds to their lower levels. This course is adopted from the conviction, that however little is our present knowledge of the degradation of land, an accurate estimate of its annual amount can be obtained with more certainty than a knowledge of the subsidences of portions of the earth's crust produced by causes comparatively hidden.

Let h_1, h_2, h_3, &c. be the heights of any places above the level of the internal surface of the shell, from which are transported in the same time the masses p_1, p_2, p_3, &c., then by the theory of moments, the result will be in effect the same as if the entire mass $p_1 + p_2 + p_3 + \ldots$, or P were carried from the distance

$$U_1 = \frac{h_1 p_1 + h_2 p_2 + h_3 p_3 + \ldots}{P} \quad \ldots \ldots \ldots \quad (15.)$$

Similarly, if i_1, i_2, i_3, &c. represent the heights of any places above the same level to which any masses q_1, q_2, q_3, &c. are transported, the resulting mechanical action will be in effect the same as if the whole mass $q_1 + q_2 + q_3 + \ldots$, or P were removed to the distance from the internal surface of the shell expressed by the formula

$$U_2 = \frac{i_1 q_1 + i_2 q_2 + i_3 q_3 + \ldots}{Q} \quad \ldots \ldots \ldots \quad (16.)$$

After the removal of P from U_1 to U_2, let C_1 become C_2, then C_2 will evidently be expressed by the formula

$$C_2 = C_1 - \frac{P}{\mu}(U_1 - U_2); \quad \ldots \ldots \ldots \quad (17.)$$

where $\mu = \delta_1 (H + D) s + \delta_2 D s'$, the mass of the zone of the shell. By means of the equation (14.), we can calculate the thickness of the shell when P is at U_1; and its thickness when P is at U_2, can be obtained by (17.). We can then find G, which being used in the equations (13.), will serve to point out any particular values of A and B. The same process being performed for every zone, the value of J in (10.) will be obtained, and by the final substitution in (2.) of $I_1 + J$ for I, and $I_1 + J'$ for I' (J' being the value of J when C_1 changes to

C_2), we can ascertain the actual change in sidereal time produced by the removal of masses on the surface of the earth from high to low levels. It is evident, that when calculating the value of J' by means of C_2, the masses of the zones of the shell must not be changed from what they were when obtained from C_1.

To show that the general equations which have been obtained in the preceding part of this memoir can be applied to the determination of geological questions, the following numerical application of them has been made. This problem would be solved if it could be shown that an appreciable change in the length of the day would be produced by an amount of degradation of the earth's surface, such as would be within the limits of what geological considerations render probable for the interval of time between the periods when the data may have been obtained for determining the change in time.

Let τ represent any appreciable change in the length of the day. Then in the equation $t' = t\,\frac{I'}{I}$, t' will become $t \pm \tau$, the upper sign being taken when $I' > I$, the lower when $I' < I$, and the former notation being used. Hence $\tau = t\left(1 - \frac{I'}{I}\right)$ in the case now under consideration. But

$$I' = I_1 + I'_2 - I_3, \quad I = I_1 + I_2 - I_3,$$

I_1 representing the moment of inertia of the internal heterogeneous spheroid, $I'_2 - I_3$ the moment of inertia of the external shell with the mean thickness C_2, and $I_2 - I_3$ the moment of inertia of the shell with the thickness C_1. The value of τ will therefore be represented by the formula

$$\tau = t\left(\frac{I_2 - I'_2}{I_1 + I_2 - I_3}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

If C_1, C_2, and D the mean depth of the sea, be supposed the same at every latitude, and if we substitute the values of I_1, I_2, I'_2 and I_3, found upon these suppositions in the above equations, we shall have, when we solve it with respect to C_2,

$$C_2 = D - \beta + \sqrt{\frac{1}{k_1}\left\{k(\alpha - D + C_1)(\beta - D + C_1)^4 - \frac{\tau}{t}\left[K(\alpha - D)\right.\right.}$$
$$\left.\left.(\beta - D)^4 + k((\alpha - D + C_1)(\beta - D + C_1)^4 - (\alpha - D)(\beta - D)^4)\right]\right\}.$$

But $$C_2 = C_1 - \frac{PV}{\mu},$$

V representing $U_1 - U_2$, or the mean vertical space through which the mass P is transported. Eliminating C_2 between the last two equations, and we shall obtain

$$P=\frac{SC_1}{V}\left\{\beta-D+C_1-\sqrt{(\beta-D+C_1)^2-\frac{\tau}{t}\left[\frac{K}{k_1}(\alpha-D)\right.}\right.$$
$$\left.\left.(\beta-D)^4+(\beta-D+C_1)^2-\frac{(\alpha-D)(\beta-D)^4}{(\alpha-D+C_1)(\beta-D+C_1)^2}\right]\right\}, \quad (19.)$$

substituting for μ its value SC_1, S being the surface of the land.

In the above formula, and the first of the two equations expressing the value of C_2, we have used for brevity the letters k, k_1 and K, for the following expressions:—

$$k=\frac{8}{15}\pi,\ k_1=\frac{8}{15}\pi(\alpha-D+C_1)(\beta-D+C_1)^2,$$

$$K=\frac{6912}{1875\pi^2}\left\{\left(\frac{25}{36}\pi^2-2\right)\cot\frac{5}{6}\pi+\frac{2}{\sin\frac{5}{6}\pi}-\frac{5}{3}\pi\right\}.$$

If we assume that the mean height of the land above the level of the sea is equal to the mean depth of the ocean, and for the value of the latter two miles, the surface of the sea twice that of the dry land, and the density of the water half that of the land, the value of C_1 will evidently be two miles. A value of V not very inconsistent with observation would be half a mile; those of α and β are obtained from Mr. Airy's treatise on the Figure of the Earth. We have made $\tau=\frac{1}{1000}$ of a second, as it has been shown that if the length of the day had varied since the time of Hipparchus by the one-three-thousandth part of a second, the value of the secular equation of the moon would be changed by more than 4″. The sidereal day being represented by t, it follows that $\frac{\tau}{t}=\frac{1}{86400000}$. The value of P calculated from these data will be less than five feet multiplied into the area of all the dry land.

The amount of denudation of the earth's surface represented by the above quantity appears to be within the limits which can be assigned by geological observations.

Dublin, July 9, 1845.

LX. *On a Peculiar Method of obtaining the Sesqui-ferrocyanide of Potassium.* *By* Prof. SCHŒNBEIN, *of Basle*[*].

MY experiments on ozone have shown that this substance, like chlorine, is capable of transforming the yellow ferrocyanide of potassium into the red one. The other day

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[*] Communicated by the Author.

I made use of this property with the view of preparing a comparatively considerable quantity of the last-named salt. To arrive at this end, I put into each of twelve large balloons filled with atmospheric air a piece of pure phosphorus, an inch long, in such a manner that half of the bulk of the pieces of phosphorus was immersed in water, whilst the other half was exposed to the action of the ambient air. I left the whole to itself at a temperature of 70° F. for about twelve hours, after which time the atmospheres of the vessels appeared to be so much charged with ozone, that their smell resembled that of chlorine; paste of starch containing some iodide of potassium became instantaneously black in them, litmus paper was bleached within a few minutes, &c.

Things being in this state, the acid was carefully removed from the balloons by rinsing them with distilled water, and then an aqueous solution of the yellow prussiate was poured into one of the vessels. I need not say, that in shaking the solution with the ozonized atmosphere of the balloon, the odoriferous principle disappeared almost instantaneously. After the ozone had been entirely taken up, the solution was transferred to a second balloon and shaken; from thence poured into a third vessel, and so on until ozone acted no further upon the liquid, or what comes to the same thing, until the solution no longer yielded prussian blue with nitrate of peroxide of iron. The solution, on being now sufficiently evaporated, yielded most beautiful crystals of the red cyanide. It is worth while to state here, that under the circumstances mentioned so much ozone had been developed, that the ozonized air of two balloons was sufficient to transform completely 5 grammes of the yellow crystallized salt into the sesqui-cyanide. The formation of this compound depending upon the elimination of part of the potassium contained in the yellow prussiate, it follows that besides the red cyanide another alkaline substance must be formed. What that compound is, I am not yet prepared to say, but I have reasons which make me inclined to believe that ozone combines with some potassium, changing the latter into a peculiar peroxide of that metal soluble in water and able to co-exist with the red cyanide. Those reasons are, that the solution of the yellow cyanide, after having been decomposed by ozone, exhibits a much stronger alkaline reaction than the yellow salt does; and that the said solution, by being acidulated with dilute sulphuric acid, colours deeply the paste of starch containing iodide of potassium. It is a remarkable fact, which I ascertained many years ago, that peroxide of lead or of manganese acts in the same manner upon a solution of the yellow cyanide as ozone does. That circumstance seems to speak in favour of the supposition of ozone and peroxide of lead being similarly constituted.

Before concluding this notice, I must not omit to remark, that according to my experiments the methods which yield ozone more abundantly than any other hitherto made use of, consists in exposing clean phosphorus to the action of stagnant air and water at a temperature of 70°–80° F. M. Marignac and some other chemists, with a view of producing ozone as abundantly and easily as possible, caused a continuous current of air to pass over phosphorus; but how imperfect that method is, appears from the fact, that the distinguished chemist of Geneva procured with his apparatus, working night and day for four weeks, no more ozone than was just sufficient to decompose 2 grammes of iodide of potassium. It seems that the presence of some ozone in the atmospheric air favours very much the formation of that substance, and probably accounts for so little ozone being obtained by Marignac's method.

LXI. *On the Nature of Ozone.*

By Prof. C. F. SCHŒNBEIN *of Basle*[*].

THE properties of ozone, and the circumstances under which that remarkable substance is produced, render it highly probable that ozone is a peculiar peroxide of hydrogen. MM. De la Rive and Marignac have lately ascertained the highly interesting fact, that dry oxygen, on being exposed to the action of electrical sparks, yields ozone; and from this fact the philosophers of Geneva have drawn the conclusion, that ozone is nothing but oxygen brought into a peculiar condition by the agency of electricity. Simple and well-founded as that inference seems to be, it is nevertheless so extraordinary that I cannot help making a remark or two on the subject.

It has very often been asserted that electricity is capable of modifying the chemical properties of elementary substances, for instance, the degree of the oxidability of metals, &c. As far as I know, there is however not one single fact known that proves the correctness of the assertion in a satisfactory manner, for what is called the passive state of some metals brought about by voltaic currents, or the protection of metals effected by voltaic means, are phænomena referable to other than electrical causes. As far as our knowledge goes regarding the influence exerted by electricity upon simple bodies, it does not, to my apprehension, entitle us to admit that their chemical properties are in the least altered by electricity; on the contrary,

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[*] Communicated by the Author.

the present state of science renders it rather more than probable, that that agent, powerful as it is in other respects, is incapable of causing in a direct manner any material change in the chemical bearings of elementary bodies.

If the conclusion drawn by the distinguished philosophers of Geneva should happen to be correct, the transformation of common oxygen into ozone by electrical action would be a case quite unique, and on that very account of the utmost scientific importance. Such a fact would indeed almost imply the possibility of transforming one elementary body into another by the agency of electricity, for in many respects ozone differs as materially from oxygen as any two heterogeneous bodies can possibly do. It is true, modern chemistry has made us acquainted with a number of facts which prove that under given circumstances the same elementary substance may exist in different conditions. Those peculiar states have, however, as far as I know, not yet been observed in any gaseous elementary body*; and it is further known that the allotropical modifications of simple bodies have as yet been brought about by the agency of heat only, and in no case by that of electricity. It is said that chlorine, after having been exposed to the action of solar light, enjoys the power of combining with hydrogen in the dark, and at the common temperature. Supposing Dr. Draper's observation fully correct, the elementary nature of chlorine is not half so certain as that of oxygen; and then we must bear in mind that the modification which chlorine undergoes under the influence of light is insignificant if compared to that which oxygen is said to suffer when exposed to the action of electricity.

If ozone be nothing but oxygen modified by electrical influence, it follows that a given volume of absolutely pure, *i. e.* anhydrous oxygen, when sufficiently long acted upon by electrical sparks, must be entirely transformed into ozone, *i. e.* changed in such a way as to be readily and completely taken up by a solution of iodide of potassium or of the yellow ferrocyanide of potassium. An experiment of that kind has not yet been made; but if any chemist succeeds in transforming one cubic inch only of what is called completely dry oxygen into ozone, we must admit the correctness of the conclusion of the philosophers of Geneva, extraordinary as it may appear to us. Before such a decisive result is obtained, I continue to consider ozone as a peculiar peroxide of hydrogen; and the principal reasons which determine me to maintain that view are the following:—

1. Ozone is such a powerfully-smelling substance, that in-

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* See Prof. Draper's paper in the present Number, p. 327.—Ed.

finitely small quantities are capable of affecting the olfactory nerves.

2. Quantities of ozone, by far too minute to be ascertained by weight, still perceptibly colour the test-paste.

Hence it follows that a quantity of aqueous vapour, so small as cannot be ascertained even by our most delicate hygroscopical tests, is sufficiently large to produce with oxygen so much ozone, that the presence of the latter is detected both by the nose and the test-paste. MM. De la Rive's and Marignac's conclusion is founded upon the assumption that the oxygen with which they made their experiments was absolutely dry; and if chemists admit that moist oxygen, after having passed through strong sulphuric acid, does not contain any trace of aqueous vapour, this inference is drawn from the fact, that in oxygen treated in the manner indicated, no water can be detected by any hygroscopical means hitherto known. It is however very possible that minute quantities of vapour escape the hygroscopic action of sulphuric acid; it is very possible also that oxygen obtained even from melted chlorate of potash still contains some traces of water, and that those minute quantities of vapour are large enough to produce ozone sufficient to colour the test-paste and affect the sense of smelling. Such a conjecture seems indeed to be far less bold and more in accordance with the present state of chemical science, than the view which MM. De la Rive and Marignac have taken of the subject. If my opinion should happen to be correct, the very property exhibited by oxygen, of producing traces of ozone when the former happens to be exposed to electrical action, would prove the most delicate means of detecting the minutest quantities of vapour contained in that gas.

In forming a judgement respecting the nature of ozone, I think one thing ought not to be left entirely out of consideration, namely the fact, that in spite of some material differences of properties which exist between ozone and Thenard's peroxide of hydrogen, both bodies exhibit many chemical bearings very similar to each other. Both substances possess bleaching powers, both of them transform a number of metallic protoxides into peroxides, both produce sulphuric acid if added to sulphurous acid, both are destroyed by a great number of organic matters, &c.

I myself and M. Marignac have ascertained that ozone is destroyed when exposed to a certain degree of temperature. Now that chemist must admit that heat has the power to annihilate the peculiar condition which oxygen is said to assume when placed under electrical influence. It appears to me that the fact alluded to is more satisfactorily accounted for by admitting that the destruction of ozone depends upon its decomposition, than by ascribing it to a change of state of oxygen by heat. M. Marignac is inclined to consider electricity as the engendering cause of the ozone produced by means of phosphorus, and thinks the electricity necessary for the production of the odoriferous principle to be developed in consequence of the chemical action taking place between the oxygen of the atmosphere and phosphorus. On this point I made many experiments some years ago, because I thought it possible that electricity had something to do with the chemical generation of ozone. I did not however succeed in tracing any sign of electricity developed under the circumstances mentioned. Now as the most powerful discharges effected by our electrical arrangements produce quantities of ozone immeasurably minute, when compared to those which a small piece of phosphorus is able to generate at the same time, I doubt as yet very much the conclusion that electricity has something to do with the generation of chemical ozone; and I doubt it so much the more as I am not convinced at all that any chemical action is accompanied by a development of electricity.

Before concluding, I take the liberty to suggest a simple method, by means of which the question regarding the nature of ozone may be decided in a satisfactory manner. From the notice above given, On a Peculiar Method of preparing the Red Ferrocyanide of Potassium, it appears that under favourable circumstances so much ozone was formed in two balloons only as to enable me to transform by that odoriferous principle 5 grammes of the yellow prussiate into the red one. This fact proves, that in the case mentioned a quantity of ozone had been produced sufficient to be ascertained by weight. Supposing the atmospheres of a number of balloons charged with ozone as strongly as possible, let those atmospheres pass through tubes containing either chloride of calcium or asbestos drenched with strong sulphuric acid, so as to deprive the ozonized air of its vapour as completely as possible. Cause the dried atmospheres to go through a narrow tube, heated so as to destroy entirely the ozone passing through it. Now if the view I have taken of the nature of ozone be correct, under the circumstances mentioned, that odoriferous substance will be decomposed into water and oxygen; and having in our balloons a measurable quantity of ozone, it follows that the quantity of water resulting from the decomposition of the ozone must also be large enough to allow of being collected by hygroscopic substances and ascertained by weight.

Basle, Aug. 20, 1845.

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 56.]

May 29, 1845. "ON the Ashes of Wheat." By William Sharp, Esq., F.R.S.

The experiments recorded in this paper were undertaken principally with the ultimate view of ascertaining with exactness what quantity of inorganic matter is removed from the soil by the seeds of a crop of wheat. The author first inquires what is the average amount of the inorganic or incombustible portion of a given quantity of wheat; a question to which no satisfactory answer has yet been given. The result of the author's experiments is, that wheat yields, by slow combustion, a residue of from 1½ to 1¾ per cent. He then proceeds to determine by experiment the degree in which this result is influenced by previous drying at different temperatures, varying from 230° to 260° Fahr., and finds that a heat of 245° is not sufficient to expel all the moisture contained in wheat; for while the loss of weight is then about 8 per cent, by a heat of 260°, the amount of this loss is 10 per cent. When the heat is so great as to occasion decomposition, the saline matter contained in the wheat fuses, and a portion of the carbon becomes so entangled or firmly adherent to it, as to be incapable of separation by burning. Hence he recommends, in order to obtain greater uniformity in the results, that the wheat subjected to these experiments should be dried at a low temperature, such as that of a room in summer, and be allowed to remain a few days under its influence. The author tried the effect of the addition of nitric acid, with a view to save time by accelerating the combustion; but found that the results could not be relied upon when this plan was adopted, and he was therefore obliged to relinquish it. He next directed his inquiries to the ascertaining whether the quantity of inorganic matter was in proportion to the specific gravity of the grain, that is, to its weight per bushel; and this he found in general to be the case. The conclusion he deduces from this investigation is, that the mean amount of inorganic matter removed from the soil by the grain of a crop of wheat is exactly one pound per acre.

"On Benzoline, a new organic Salt-base obtained from Oil of Bitter Almonds." By George Fownes, Esq., F.R.S.

Pure oil of bitter almonds is converted, by the action of a strong solution of ammonia, into a solid white substance having a crystalline form, and which was termed by M. Laurent *hydrobenzamide*. The author found that this substance, by the further action of alkalies, became harder and less fusible than before, and not differing in chemical composition from the original substance, but exhibiting the properties of an organic salt-base. To this substance the author gives the name of *benzoline*. He finds that the salts which it forms by combination with acids are, in general, remarkable for their sparing solubility; and that many of them, as the hydrochlorate, the nitrate and the sulphate, are crystallizable. Of the properties of these salts the author gives a detailed account.

June 5.—"Electro-Physiological Researches." Memoir First. By Professor Carlo Matteucci. Communicated by Michael Faraday, Esq., D.C.L., F.R.S.

The author describes several arrangements by which he was enabled to make new experiments in confirmation of the laws of muscular currents, of which he has given an account in his recent work, entitled "Traité des Phénomènes Electro-Physiologiques des Animaux." He finds that, in these experiments, the employment of a galvanometer is unnecessary, as the sensibility of the electroscopic frog of Galvani gives sufficient indications of the electric current without the use of that instrument.

The general results obtained from these experiments are the following.

In the first place, the intensity and duration of the muscular current is independent of the nature of the gas in which the muscular pile is immersed. Secondly, it is altogether independent of the cerebro-spinal portion of the nervous system. Thirdly, the circumstances which exercise a marked influence on its intensity are the conditions of the respiratory and circulatory systems. Fourthly, those poisons which seem to act directly on the nervous system, such as hydrocyanic acid, morphia and strychnine, have no influence on the nervous current. Fifthly, sulphuretted hydrogen has a decided influence in diminishing the intensity of the muscular current. Sixthly, the intensity of this current in frogs varies according to the temperature in which the frogs have been kept for a certain time during life; a result which, of course, is not obtainable with animals which do not take the temperature of the surrounding medium. Lastly, the intensity of the muscular current in animals increases in proportion to the rank they occupy in the scale of beings; and on the other hand, its duration after death is exactly in an inverse ratio to its original intensity. The author concludes by stating his belief, that the property of the muscles immediately connected with their electric currents, is identical with that which was long ago denominated by Haller *irritability*, but which is at present more usually designated by the term *contractility*. He ascribes the development of this muscular electricity to the chemical actions which are attendant on the process of nutrition of the muscles, and result from the contact of arterial blood with the muscular fibre. He conceives that in the natural state of the muscle, the two electricities thus evolved neutralise each other at the same points at which they are generated; while in the muscular pile contrived by the author, a portion of this electricity is put into circulation in the same manner as happens in a pile composed of acid and alkali separated from one another by a simply conducting body.

June 19.—"On the Connexion between the Winds of the St. Lawrence and the Movements of the Barometer." By William Kelly, M.D., Surgeon R.N., attached to the Naval Surveying Party on the River St. Lawrence. Communicated by Captain Beaufort, R.N., F.R.S.

The author adduces a great number of observations which are in opposition to the generally received opinion, that the mercury in the barometer has always a tendency to fall when the wind is strong. During a period of fifteen years passed in the Gulf and River St. Lawrence, he found that the barometer as frequently rises as falls under the prevalence of a strong wind; and that the winds often blow with greater force with a rising than with a falling barometer. He gives a circumstantial account of the progress and course of various gales which fell under his observation during that period, and from which he infers the existence of a steady connexion between the prevailing winds of this region and the movements of the barometer, and enters into an inquiry into the mode in which that instrument is affected by them. The extensive valley of the St. Lawrence is bounded at its lower part, for a distance of nearly 500 miles, by ranges of hills, rising on each side to a considerable elevation. Within this space, the ordinary winds follow the course of the river; and in almost every instance when they approach from windward, the barometer rises with them; and when, on the other hand, the wind approaches from leeward, the barometer not only falls before the arrival of the wind, but continues to fall until it has subsided.

An appendix is subjoined containing extracts from the tabular register of the barometer and winds at various points in the valley of the River St. Lawrence, during the years 1834 and 1835, accompanied by remarks on different points deserving notice in particular cases.

"On the Elliptic Polarization of Light by reflexion from Metallic Surfaces." By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

In a former paper, published in the Philosophical Transactions for 1843, the author gave an account of the observations he had made on the phænomena of elliptic polarization by reflexion from certain metallic surfaces, but with reference only to one class of comparative results. He has since pursued the inquiry into other relations besides those at first contemplated, and the present paper is devoted to the details of these new observations, obtained by varying the inclination of the incident rays, and the position of the plane of analysation, and by employing different metals as the reflecting surfaces. By the application of the undulatory theory of light to the circumstances of the experiments and the resulting phænomena, the law of metallic retardation is made the subject of analytic investigation. A polariscope of peculiar construction, of which a description is given at the conclusion of the paper, was employed in the experiments; and tables are subjoined of the numerical results of the observations.

"On the Gas Voltaic Battery. Voltaic Action of Phosphorus, Sulphur, and Hydrocarbons." By William Robert Grove, Esq., M.A., F.R.S., V.P.R.I., Professor of Experimental Philosophy at the London Institution.

The author, referring to a paper of his published in the Philosophical Transactions for 1843, states, that in repeating and verifying some of the experiments therein contained, he was led to those which form the subject of the present memoir. With the form of gas battery last described in that paper, by which the interfering action of the external air is excluded, he finds that deutoxide of nitrogen associated with oxygen gives a continuous voltaic current; and that the volumes respectively absorbed by the electrolyte are as four to one, indicating the formation of hyponitrous acid.

Passing to the more immediate object of the present paper, he states that having observed nitrogen procured by the combustion of phosphorus to give rise, in the gas battery, to a temporary voltaic current, he was led to believe that phosphorus, although an insoluble non-conductor, might, by means of the gas battery, be made the excitant of a continuous voltaic current, analogous to the zinc element of an ordinary voltaic combination. This expectation was verified by experiments, a series of which is given; phosphorus being suspended in various gases and voltaically associated with oxygen. The experiments were continued during several months, and the results indicated the same consumption of phosphorus with reference to the oxygen, as would occur by the formation of phosphorous acid; the phosphorus being thus burned by oxygen at a distance. Phosphorus and iodine, both non-conducting solids, being each suspended in nitrogen in the associated tubes of a gas battery, give a continuous voltaic current, and are consumed in equivalent ratios. Sulphur, suspended in nitrogen and associated with oxygen, gives a voltaic current when fused. Other volatile electro-positive bodies, such as camphor, essential oils, æther and alcohol, when placed in nitrogen and associated with oxygen, gave a continuous voltaic current.

The author observes that the gas battery which in his former experiments introduced gases, by the present experiments renders solid and liquid insoluble non-conductors the exciting constituents of voltaic combinations, and enables us to ascertain their electro-chemical relations: it also introduces the galvanometer as a test of vaporization.

A new form of gas battery is described, in which an indefinite number of cells are charged by the hydrogen evolved from a single piece of zinc; the oxygen of the atmosphere supplying the electro-negative element. The charge of the battery is self-sustained, in a manner somewhat similar to the Doebereiner light apparatus.

"The Blood-Corpuscle considered in its different phases of development in the Animal Series." By Thomas Wharton Jones, Esq., F.R.S., Lecturer on Anatomy, Physiology and Pathology, at the Charing Cross Hospital.

This paper is divided into three parts: the first relating to the blood-corpuscles of the Vertebrata; the second to those of the Invertebrata; and the last to a comparison between the two. He first describes the microscopic appearances of these corpuscles in different classes of vertebrate animals, beginning with the skate and the frog, and proceeding to birds and mammifera; first in their early embryonic state, and next in the subsequent periods of their growth. He finds in oviparous vertebrata generally, four principal forms of corpuscles. These he distinguishes as the phases, first of the *granule blood-cell*, which he describes as a cell filled with granules, disclosing by the solvent action of dilute acetic acid on these granules a vesicular, or as the author terms it, a "*cellæform*," nucleus. These granule cells appear under two stages of development, namely, the coarsely granulous stage and the finely granulous stage. The second phase is that of the *nucleolated blood-cell*, oval in shape, containing a vesicular (or "cellæform") nucleus, and red-coloured matter. These cells likewise appear under two stages of development; colourless in the first and coloured in the second, in which last stage it constitutes the *red corpuscle*. In the early mammiferous embryo, he finds, in addition to the former, a third phase, that of *free vesicular nucleus*, exhibiting, like the nucleolated cell, the colourless and the coloured stages.

On examining the corpuscles of the lymph of vertebrate animals, the author finds them in all the classes to be identical in structure with their blood-corpuscles, and differing only in the inferior degree of coloration attending their last stage. In the oviparous classes, he observes that the nucleolated are more numerous than the granule cells, while in the mammifera the latter are predominant, which is the reverse of the proportion in which they exist in the blood of these animals. He finds that some of the nucleolated cells of the contents of the thoracic duct exhibit a marked degree of coloration, and have an oval shape; thus offering a resemblance to the blood of the early embryonic state.

The blood-corpuscles of all the invertebrate animals in which the author examined them, present the same phases of granule and nucleolated cells as in the higher classes, except that in the last stage of the latter phase the coloration is very slight, but the vesicular nucleus is frequently distinctly coloured. As in the higher classes, corpuscles exist in different states of transition from the granular to the nucleolated form of cell. In some of the invertebrata, corpuscles are found which appear to be the nuclei of some of the nucleolated cells become free; and these the author considers to be abortions, rather than examples, of cells having attained their third phase of free cells. Corpuscles are also met with in these animals, in greater or less abundance, belonging to the lowest forms of organic elements, namely, elementary granules.

The comparison which the author institutes between the blood-corpuscles of the vertebrate and invertebrate divisions of the animal kingdom, tends to show that they in all cases pass through similar phases of development, except with respect to the last, or coloured stage of the nucleolated cell, which they do not attain in the lower classes of animals. He finds that the blood-corpuscles of the crab, according to an analysis made by Professor Graham, contain a sensible quantity of iron, perhaps as much as red corpuscles. He considers the corpuscles of the blood of the invertebrata, inasfar as relates to the absence of nucleated cells, as resembling those of the lymph of vertebrate animals.

"On some peculiar modifications of the Force of Cohesion, with reference to the forms and structure of Clouds, Films and Membranes." By William Addison, Esq., F.L.S.

In the course of his researches into the nature and origin of tubercles in the lungs, and into the physical properties of the secretions of the human structure, the author had occasion to notice various fibrous and membranous forms arising solely from physical agencies. On pursuing the inquiry, he observed a class of phænomena indicating some peculiar modifications of the force of cohesion. Thus he found that transparent liquid streams are visible when vapours or fluids mingle with one another, and also when solids are dissolving in a fluid; and that opake deposits, assuming the appearance of clouds, form on those streams, rendering visible the lines of junction of two fluid or gaseous surfaces. A great number of experiments are described illustrating these general facts, which are afterwards applied to explain various phænomena of clouds, films and membranous formations, resulting from the operation of the different cohesive forces which are called into action, under a diversity of circumstances, when liquid or gaseous bodies are brought into mutual contact.

"On the formation or secretion of Alkaline and Earthy Bodies by Animals." By Robert Rigg, Esq., F.R.S.

From the results of a comparison made by the author of the weight of the ashes obtained from a quantity of bread, equal to that which was the sole food of two mice during thirty-eight days, with the weight of the ashes of their excretions during the same period, it appeared that the latter exceeded the former in the proportion of 1334 to 934. The amount of soluble salts was also found to be greater in the latter than in the former. From these data, the author infers that both alkaline and earthy bodies are formed out of their elements by the animal system, and found in their excretions.

"An Account of the Observation of the total Eclipse of the Sun on the 21st of December 1843." By Lieutenant J. O. E. Ludlow, E.I.C. Engineers, Superintendent of the Magnetic Observatory at Madras. Communicated by J. C. Melville, Esq., F.R.S., Secretary to the Hon. the Court of Directors of the East India Company.

The author reports the results of his observation of the eclipse, which was unfortunately much interrupted by the passage of clouds; and he has also taken the opportunity of making observations on the Magnetic Dip and Horizontal Intensity at certain places on his journey.

"On the Barometrical Variation as affected by the Moon's Declination." By Luke Howard, Esq., F.R.S.

In this paper, which is a continuation of that which was published in the Society's Transactions for 1841 (p. 277), and in which the average results of the author's observations extended only from the year 1815 to 1832, similar records of calculated averages are given down to the year 1841; and a comparison drawn with those of the former period. The author places the whole of these data in the hands of the Royal Society, for the purpose of being made the basis of future inquiry, in what he terms "the nascent science of Meteorology."

"Contributions to the Chemistry of the Urine; on the variations in the Alkaline and Earthy Phosphates in the healthy state, and on the Alkalescence of the Urine from fixed Alkali." By Henry Bence Jones, M.A. Cantab., Licentiate of the Royal College of Physicians.

The author, having observed that in some states of disease there occurs in the urine a great excess of the earthy phosphates, was induced to investigate the subject; and as a preliminary inquiry, to ascertain the variations in the amount of these phosphates at different times in the same person in a state of health, and to trace the causes which determine an excess or a deficiency of these salts in the urine; noting, at the same time, the variations in the quantity of the alkaline phosphates contained in it, with a view of discovering whether these variations are influenced by the same, or by different causes. The principal results to which his experiments have conducted him are the following. The quantity of the earthy phosphates in the urine voided soon after taking food is considerably greater than in that voided at other times; and this happens whether the meal consists of animal food or of bread only. After long fasting, the proportion of earthy phosphates is considerably diminished. On the other hand, the alkaline phosphates are present in greatest quantity when the food consists of bread alone: when meat alone is taken, the deficiency in those salts is still more marked than the excess in the former case. Exercise occasions no change in the quantity of the earthy phosphates, but causes an increase of nearly one-third in the amount of alkaline phosphates; but its influence is, on the whole, less than that of diet. The earthy phosphates are increased in quantity by chloride of calcium, sulphate of magnesia, and calcined magnesia taken into the stomach.

The author next examines the conditions in which the urine is alkalescent, and which he considers to be of two kinds; the one, long known as *ammoniacal*, and arising from the presence of carbonate of ammonia; and the other, which has not hitherto been distinctly recognised, arising from fixed alkali, and appearing most frequently in urine secreted during a period of from two to four hours after breakfast, in persons suffering only from defective digestion. Under these circumstances, it may be, when voided, either turbid from amorphous sediment, or clear and alkaline when tested, or free from deposit and slightly acid. If in either of these last cases it be heated, an amorphous precipitate falls down, which is soluble in dilute hydrochloric acid, or in a solution of biphosphate of soda. Healthy urine may at any time be made to yield a precipitate of earthy phosphates by heat, even though it be acid, by having a portion of this acid neutralised by any alkali, or by phosphate of soda, the fluid becoming more acid when boiled. A solution of earthy phosphates in biphosphate of soda also gives a precipitate on boiling, if some of its acid reaction is removed by any alkali. The fluid when boiled becomes more acid to test-paper, indicating the formation of a more basic earthy phosphate. A result precisely similar is obtained when common phosphate of soda, phosphate of lime, and a little biphosphate of soda exist together in solution; and by varying the quantities of each of these substances, the various phænomena which the urine occasionally presents may be imitated. The time at which the alkalescence of the urine from fixed alkali generally occurs, indicates the existence of some alkaline phosphate, or of some carbonated alkali in the food.

"On the Nerves of the Uterus." By Thomas S. Beck, Esq. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

The object of the author in this communication is to record the results of his dissections of the nerves of the uterus, both in the unimpregnated and gravid states, with a view to determine if any changes are observable in them in these two conditions. He enters minutely into the anatomical details of the formation of the great splanchnic nerve, the composition of the semilunar ganglion, and the distribution of the branches proceeding from it to the different abdominal viscera. His conclusions are, that while the ovaria derive their nerves from the renal, the fallopian tubes from the hypogastric, and the bladder, rectum and vagina from the pelvic plexus, the nerves supplying the uterus are continuations of the hypogastric plexus, and that they undergo, during pregnancy, no further change, either in size or position, except that which is the simple consequence of the enlargement of the organ over which they are distributed, and that they undergo no other change during a second pregnancy. He thinks it probable, moreover, that the vessels of the uterus do not decrease in size after parturition, but are only contracted in their cavity. He notices several points relating to these subjects, which are still open to further investigation. The paper is accompanied by highly finished drawings of the appearance of the dissected parts.

"On a Peculiar Source of Deterioration of the Magnetic Powers of Steel Bars." By William Sturgeon, Esq. Communicated by S. Hunter Christie, Esq., Sec. R.S.

The author concludes, from various experiments on the changes in the magnetic force of steel magnets produced by subjecting them to blows with a wooden mallet, or other modes of creating tremors or vibrations among their particles, that the most apparently trifling mechanical agitation is sufficient to occasion a considerable diminution of magnetic power; that this loss, when it has taken place from such a cause, is permanent; and that in every case, after reaching a certain point, it attains its maximum, a fact which implies, in every magnet, the possession of a specific retentive force, of which it cannot be deprived by any further mechanical commotion of its particles. The more highly a bar is magnetized, the more it becomes susceptible of a loss of power by agitation.

"An Account of a Protracting Pocket Sextant." By Colonel Bainbridge. Communicated by P. M. Roget, M.D., Sec. R.S.

The object of the invention here described is to facilitate and expedite the making of sketches and surveys for military purposes on actual service, and by serving at once as a sextant and protractor, so that as soon as an angle is taken, it may at once be laid down on paper by employing the two legs of the instrument as rulers. A description referring to a diagram is given of this portable sextant.

"Observations on the Growth and Development of the Epidermis." By Erasmus Wilson, Esq., F.R.S., Lecturer on Anatomy and Physiology in the Middlesex Hospital.

The author adduces evidence derived from his microscopic observations, in confirmation of the commonly received doctrine respecting the origin of the cells of the epidermis and epithelium generally, from the materials furnished by the liquor sanguinis or plasma of the blood; which fluid, passing by endosmosis through the walls of the capillary vessels and peripheral boundary of the surface, developes granules by a vital process, analogous to coagulation. On a careful examination of the inner surface of the epidermis with the aid of the microscope, he finds it to be composed of four kinds of elements, arranged in such a manner as to constitute an irregular plane, similar to a tesselated or mosaic pavement. These elements are,—1. *Granules*, which the author terms *primitive*, of a globular form, solid and apparently homogeneous, and measuring about 1-20,000th part of an inch in diameter. 2. *Aggregated granules*, having about double the diameter of the former and apparently composed of as many of these as can be aggregated together without leaving an unoccupied space in the centre of the mass. 3. *Nucleated granules* measuring in diameter from the 6000th to the 4000th part of an inch, each being composed of an aggregated granule as a nucleus, enveloped by a single layer of aggregated granules, giving to the whole mass an oval or circular, and at the same time flattened shape. Their constituent granules have acquired, during this aggregation, greater density, and are separated from each other by distinct interstitial spaces filled with a transparent homogeneous substance. 4. *Nucleolo-nucleolated* cells pervading the deep stratum of the epidermis, and of which the longer diameter measures from the 3000th to the 2500th part of an inch. These cells, which constitute the principal portion, and may be regarded as the chief constituent of the epidermis, are formed from the nucleolated granules, on the exterior of which there is superposed a transparent layer, bounded by a well-defined outline, by the dark interstitial substance of the wall of the cell; the nucleolated granule being the nucleus, and the aggregated granule the nucleolus of these primitive cells of the epidermis. The author is of opinion that the nuclei, up to a certain point, grow with the cells, by the separation of the original granules from the deposition between them of interstitial matter, and also by the cleavage of the latter and the consequent multiplication of the granules. This peripheral growth of the cells is totally different from the mode of growth described by Schwann, and explains the disappearance of the nucleus in the scales of the epidermis. The observations of the author lead him to believe that the same process of development and of growth is followed in the epithelium as in the epidermis; and he offers evidence, showing that similar arrangements take place in the cells of melanosis, in the pigment cells of the choroid membrane of the eye, and in those of the skin of the negro.

"On the Temperature of Man." By John Davy, M.D., F.R.S. L. & E.

Having in a former paper shown that, contrary to a commonly received opinion, the temperature of the human body, as measured by a thermometer placed under the tongue, is not a constant one, the author has resumed the inquiry, and gives, in the present paper, the results of numerous observations made with a particular instrument constructed for the purpose, admitting of minute accuracy (each degree of the scale being divided into ten parts), and when used with the precautions pointed out, affording satisfactory indications in many problems which may be proposed relative to the temperature of man, &c., and confines himself to a small number, offering the information he brings forward only as a preliminary contribution in aid of their solution.

The paper is divided into seven sections.

The first treats of variations of temperature during the twenty-four hours. The author finds from his observations, that the temperature is highest in the morning, on rising after sleep; that it continues high, but fluctuating, till the evening; and that it is lowest about midnight, ranging on an average from 98°·7 to 97°·9.

The second, of variations during the different seasons. These, he finds, bear some relation to the temperature of the air, but less than might be expected; which he attributes to the majority of the observations having been made within doors, under circumstances peculiarly favourable to uniformity.

The third, of the influence of active exercise on the temperature. The effect of this, when not carried to the length of exhausting fatigue, he finds to be elevating; and that the augmentation is, within a certain limit, proportional to the degree of muscular exertion.

The fourth, of passive, such as carriage exercise. The effect of this in a cool air, contrary to that of quick walking or riding, would appear to be lowering.

The fifth, of abstinence from all exercise in a cold atmosphere. This he finds to be depressing in a still greater degree; sitting in a cold church has occasioned a reduction of temperature from 1° to 2°, the air of the church being from 42° to 32°.

The sixth, of sustained attention or exertion of mind. This would appear to have the effect of raising the temperature, but in a much less degree than bodily exercise.

The seventh, of taking food. It would appear that a light meal, such as breakfast, alters very little the temperature, whilst a heavy meal, such as dinner with wine, tends to lower it.

The conclusion drawn by the author from his observations, considered in their greatest generality, is, that the temperature of man is constantly fluctuating within a certain limit; regularly during the twenty-four hours; and irregularly according to the operation of certain disturbing circumstances.

Should multiplied observations give similar results, he infers that they will admit of many applications, both as regards the regulation of clothing, the warming of apartments, and possibly the prevention and cure of diseases,—conducive alike to increase of comforts and health.

Tables are appended, containing a series of observations extending through eight months, in which not only the temperature of the body is noticed, but also the frequency of the pulse and of respiration, and the temperature of the air.

"On Ozone." By C. F. Schœnbein, Professor of Chemistry at Basle, in a letter to Michael Faraday, Esq., D.C.L., F.R.S. Communicated by Michael Faraday, Esq.

The author finds that the peculiar substance he has denominated *ozone*, and which, reverting to the opinion he originally entertained, he now believes to be a compound of oxygen and hydrogen, is obtained readily and in great abundance by placing phosphorus in immediate contact with water and atmospheric air at a temperature of about 30° Cent., but that none is produced when water is absent. Heat was found to effect the decomposition of ozone. He infers, both from his own experiments and those of M. Marignac, that the presence of nitrogen, instead of being essential to the formation of ozone, as he formerly believed, does not in reality contribute in any way to the production of that substance.

"On the Theory of Vision," in a letter to S. Hunter Christie, Esq., Sec. R.S. By William Ford Stevenson, Esq., F.R.S.

The author adduces two experiments, of placing before the eye an object, the ends of which are marked, in a vertical position, as "clearly demonstrating that objects are not presented to the mind as they are found upon the retina, but in the actual position in which they are placed before the spectator."

"On the Compounds of Tin and Iodine." By Thomas H. Henry, Esq. Communicated by Richard Phillips, Esq., F.R.S.

Different properties have been assigned by different authors (as Sir Humphry Davy, Gay-Lussac, Boullay and Rammelsberg) to a combination of tin with iodine. With a view to explain these discordances, the author instituted the series of experiments detailed in this paper, and which have led him to the conclusion that the substance obtained by heating tin with twice its weight of iodine is a mixture of two salts, differing from each other in their composition. One of these is soluble in water to a slight extent without suffering decomposition, while the other is immediately decomposed on coming into contact with water; the former being the real proto-iodide described by Boullay, and the latter being a biniodide, a salt of which no particular description had hitherto been given, but which was probably the compound noticed by Sir Humphry Davy as being of a brilliant orange colour. The author found that this biniodide sublimes at a temperature of 356° F., while the proto-iodide, if protected from the contact of air, may be heated to redness without subliming. The author did not succeed in obtaining a combination of tin and iodine corresponding to the sesquioxide, although Boullay supposes that such was the composition of some yellow crystals which were formed by the mixture of solutions of proto-chloride of tin and of iodide of potassium. A more detailed account of the properties of the iodides of tin is reserved for a future communication.

Supplement to a Paper "On the Nervous Ganglia of the Uterus." By Robert Lee, M.D., F.R.S., Fellow of the Royal College of Physicians.

The author is confirmed in his views regarding the arrangement of the nervous filaments distributed to the uterus, as described in his papers printed in the Philosophical Transactions for 1841 and 1842, by his recent dissection of a gravid uterus at the full period, and which he considers as demonstrative of the accuracy of all the statements which are contained in those communications.

LXIII. *Intelligence and Miscellaneous Articles.*

ON THE RED COLOUR OF LITHARGE. BY M. LEBLANC.

THE author remarks, that M. Fournet is of opinion that red litharge contains more oxygen than yellow litharge, and that it owes its colour to a certain proportion of minium; and several specimens yielded him unquestionable traces of it. M. Thenard and most other chemists attribute the colour of litharge to the same cause.

The author, without denying that litharge slowly cooled may in certain circumstances absorb oxygen and give rise to minium, and which is stated to be a fact, is of opinion that another cause must be assigned for the production of red litharge; and his grounds for this opinion are the following:—1st, the red litharge which he examined yielded no oxygen by heat; 2nd, the same litharge, carefully examined by means of pure nitric acid, gave no binoxide of lead, whereas a trace of minium, added to yellow litharge, without even modifying its tint, was discoverable by this test; 3rd, red litharge, heated to a temperature at which it disengaged no oxygen, and quickly thrown into water, became yellow. Experience shows that these variations of structure and colour in litharge, dependent upon the temperature at which it is produced, are not occasioned by changes of chemical composition, but by isomeric or dimorphous modifications; the differences are also connected with the different densities of the specimens.

The author concludes from his experiments, that oxygen may be dissolved by litharge in fusion, as it dissolves in silver, without forming a superoxidized compound; he is further of opinion that the differences between specimens of litharge are those of structure, colour, and density, without any variation in chemical composition; and these differences are produceable at pleasure in the dry way, according to the circumstances of temperature and rapidity of cooling.—*Journ. de Pharm. et de Ch.*, Septembre 1845.

ANALYSIS OF THE SILICATE OF MANGANESE FROM ALGIERS. BY M. EBELMEN.

This mineral is found in veins in the primary formation of the environs of Algiers, and it accompanies an ore of manganese which is now worked. It consists of two distinct portions, one lamellated in three directions and of a rose colour; the other, on the surface exposed to the air, is black and earthy, and about one-third of an inch thick; it is, however, easy to ascertain the junction of these two substances; it is very evident that the black portion is the rose-coloured mineral altered.

Red Mineral.—The density of this is 3·559; it scratches glass; its texture is sometimes laminated and sometimes granular; it fuses before the blowpipe, but not very readily; it does not effervesce with acid; boiling hydrochloric acid acts slowly and with difficulty upon it, and deposits silica; the solution was found to contain protoxide of iron and of manganese, with lime and magnesia.

In order to analyse this substance, a portion of it was fused with carbonate of soda; the fused mass was treated with hydrochloric acid, and the silica separated by the usual process; this was entirely dissolved, after being weighed, in solution of potash; the iron and manganese were precipitated together from the solution by means of hydrosulphate of ammonia; the sulphurets were dissolved in concentrated hydrochloric acid, to which was added a small quantity of nitric acid to peroxidize the iron; the peroxide of iron was then separated from the oxide of manganese by succinate of ammonia.

The liquor from which the iron and manganese were separated was saturated with an acid, boiled and filtered to separate the sulphur, then saturated with ammonia, and precipitated successively by oxalate and phosphate of ammonia; the lime was estimated in the state of sulphate. The results of the analyses were as follows:—

Silica.	45·49
Protoxide of manganese.	39·46
Protoxide of iron	6·42
Lime	4·66
Magnesia	2·60
	98·63

This rose-coloured substance is therefore bisilicate of manganese (rhodonite of Beudant), as indicated by its external characters. A very considerable portion of protoxide of manganese is replaced by isomorphous bases; the formula of this mineral is (Mn . Fe . Ca Mg .) S^2.

Black Substance on the Surface of the Silicate.—This substance is friable; heated in a tube it yields water. Hydrochloric acid attacks it readily, with disengagement of chlorine, and leaves a rose-red residue.

To analyse this substance, it was acted upon by hydrochloric acid, and the chlorine evolved was received in a very clear solution of sulphurous acid mixed with chloride of barium; the boiling was discontinued soon after the decoloration of the substance, and the complete expulsion of the chlorine from the receiver, in order that the red matter should be as little acted upon as possible. The solution of sulphurous acid boiled and filtered yielded sulphate of barytes, the weight of which served to calculate the oxygen contained in the mineral above the protoxide.

The liquor derived from treating the black mineral with hydrochloric acid was filtered, then evaporated to dryness to separate some flocculi of silica, and again filtered; the manganese, iron and lime were separated by the means already described; no magnesia was found.

The residue unattacked by the hydrochloric acid was calcined and weighed, the gelatinous silica was then separated by liquid potash; the portion insoluble in the alkali consisted of small rose-coloured grains, which, examined by the glass, appeared to be perfectly similar to the bisilicate from the centre of the specimen. In fact, by acting upon this matter by carbonate of soda in the platina crucible, there was separated by acid 0·48 of its weight of gelatinous silica, a quantity very near that obtained by the analyses of the bisilicate.

To estimate the water of the black mineral, a portion of it was heated in hydrogen, by employing a platina crucible with a tubulated cover; a greenish substance was obtained which was acted upon by hydrochloric acid without evolving any chlorine. The loss of weight of the mineral heated in the hydrogen consisted of,—1st, the water; 2nd, the oxygen above that which corresponded to protoxide, which was known by the weight of the sulphate of barytes, so that the difference of the loss in the current of hydrogen would give the water.

The results of the analyses were—

Water		10·14
Oxygen		8·94
Protoxide of manganese		43·00
Peroxide of iron		6·60
Lime		1·32
Insoluble residue	Gelatinous silica	2·40
	Bisilicate unacted upon	27·20
		99·60

When it is considered that the rose-coloured silicate is acted upon by boiling in hydrochloric acid, it will be admitted as probable, that the small portion of gelatinous silica occurring in the analyses is derived from a portion of the bisilicate. Admitting this supposition, and subtracting from the quantity of protoxide of manganese found that which corresponds to the gelatinous silica, and which is 2·10, the remainder, 40·90, contains a quantity of oxygen equal to that occurring in the sulphurous acid. It may also be concluded that the black substance is a mixture of hydrates of peroxide of manganese and iron, with a certain proportion of unaltered bisilicate.

If also this composition be compared with that of the rose-coloured matter, it will be seen,—1st, that the silica and the magnesia have completely disappeared during decomposition, as well as the greater part of the lime; 2nd, that the protoxides of iron and of manganese are converted into hydrates of peroxides, and occur in the black substance in the same relation as in the unaltered mineral.—*Ann. des Mines,* tome vii.

ON THE ARTIFICIAL PRODUCTION OF DIAPHANOUS QUARTZ.

BY M. EBELMEN.

When either of the two silicic æthers which I have recently described[*] is exposed to the continued action of a moist atmosphere, the liquid finally solidifies to a transparent mass. This product, very delicate and fragile in the first days after its solidification, contracts more and more under the influence of the moist air still remaining diaphanous. Two or three months are requisite in operating on 5 or 6 grms. of æther, for the substance to cease to lose in weight and for its molecular movement to terminate. The substance prepared as above is hard, faintly scratches glass, and possesses great cohesion; its lustre, its fracture and transparency are perfectly comparable to the most beautiful rock crystal. Its density is 1·77. It is a hydrate, which contains twice as much oxygen in the silica as in the water, the formula of which is consequently $(SiO)^2 HO$.

It is essential, in order that the product may not become fissured during the contraction it experiences before attaining the definite formula $(SiO)^2 HO$, not to allow the moist air to enter except by an aperture of very small diameter. During the whole of the experiment the flask containing the silicic æther exhales an alcoholic odour, which persists a long time after the solidification, proving that only a portion of the organic matter had separated from the silica when the solidification took place. The contraction proceeds the more slowly the less easily the moist air is renewed in the apparatus, and this slowness appears indispensable to the success of the operation.

From the properties of the hydrate of silica, we may be allowed to hope that it may be turned to advantage in the construction of optical instruments. It is my intention to make some experiments in this direction.—*Comptes Rendus*, Aug. 25, 1845.

ON THE ARTIFICIAL PRODUCTION OF HYDROPHANE.

BY M. EBELMEN.

By slightly modifying the process for the preparation of hydrate of silica, described in the preceding paper, there is obtained, instead of a diaphanous product, an opake substance, which becomes entirely transparent when placed in water, in a word, true hydrophane. It suffices for this purpose that the silicic æther employed retain a little chloride of silicium, which happens when an excess of alcohol has not been employed in the preparation of the æther. On exposing this æther, still somewhat acid, to the action of moist air, there is obtained a solid mass, which is at first transparent, but ends by becoming opake after some weeks' exposure to the air. The translucidity of this substance is so much the less the greater the proportion of the chloride of silicium in the æther. Some of the fragments have the semi-transparency of opal. They all become entirely transparent in water.

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[*] See Phil. Mag. vol. xxv. p. 397.

We are therefore able artificially to reproduce hydrophane, this rare variety of quartz, and the curious property of which had struck mineralogists.

A very small proportion of foreign substances suffices in fact to modify the translucidity and the appearance of the hydrate of silica; thus a small flask containing silicic æther having been closed by mistake with a cork, which belonged to a bottle full of creosote, the æther, on becoming coagulated under the action of moist air, yielded a slightly yellowish silica, and merely translucid like chalcedony; the product thus obtained was not hydrophanous.

I propose to continue and to vary these experiments, which appear to me of great importance and of considerable interest.—*Comptes Rendus*, Sept. 1.

IMPROVEMENTS IN PHOTOGRAPHY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

From the many able papers which occasionally appear in your Journal on photography, by Sir J. Herschel, Messrs. Talbot, Hunt and others, I am induced to believe that any facts bearing on that most interesting subject will be welcome. I therefore hasten to communicate them for the information of your numerous readers.

My attention has lately been directed to ascertain what other substances besides chlorine and bromine, separately and combined, possessed the property of accelerating the action of light on a Daguerrean or iodized plate; and after many trials, I found that the vapour of ammonia possesses this singular quality in a very remarkable degree. I first employed it with iodine alone, by simply iodizing a plate to a full yellow colour, and then exposed it for a few seconds to the vapour of ammonia in an exceedingly attenuated state by adding a few drops of strong ammonia to a little water, just sufficient to recognise it by its odour; thus prepared it was placed in the camera, and produced a perfect impression of a building in half a minute in moderate sunshine; and several other experiments satisfied me that ammoniacal vapour has a very quickening action on iodine alone. My next step was to ascertain how it would operate with bromine; expecting it would either destroy or accelerate its action, I was rejoiced to find that it had the latter effect, and that plates prepared in the usual manner, with iodine and simple bromine water, are rendered infinitely more sensitive by exposing them a few seconds to this vapour than they are without it. Having found that I could obtain a perfect impression in sunshine instantaneously, and that five or ten seconds only were sufficient in a moderate light, I indulge the hope that, with its assistance, moving objects may therefore be taken with facility. I should inform you that I have employed this substance in a variety of shapes, by exposing the plates to its influence previous to placing them in the camera, or by allowing it to be evolved in it during the operation, or just before it is used, and in each case have found it equally efficacious. What is remarkable also, the accelerating influence of the ammonia seems to be retained in the camera for a considerable time, notwithstanding its volatility; in fact I have sometimes thought its presence in the operating room alone had an accelerating influence, and am persuaded it will be highly advantageous in a room where bromine or iodine is evolved, whose presence is known to suspend the action of light altogether; this vapour, on the contrary, neutralizes them, and instead of retarding the process, hastens it.

I have not had time to institute further experiments, which I think the subject well worthy of. My object now is simply to direct the attention of photographists and the scientific world to the fact; and I shall be much mistaken if this compound of hydrogen and nitrogen does not prove a valuable adjunct to the photographic art.

I am, Gentlemen,
26 Tavistock Place, Tavistock Sq., Your most obedient Servant,
October 20, 1845. W. H. Hewett.

P.S. I should add that my experiments were made with two meniscus lenses of small aperture in front, and worked at the chemical, not the visual, focus with achromatic glasses. I have no doubt much more satisfactory results may be obtained.

W. H. H.

ON THE LIGHTING OF MINES BY MEANS OF THE ELECTRIC LAMP. (Letter of M. De la Rive to M. Boussingault.)

I see, by the *Compte Rendu des Séances de l'Académie*, that you are engaged in seeking means to employ the voltaic pile in lighting mines. This subject has also occupied my attention for a long time. I have made several attempts, which have not been all equally fortunate; but I have lately had more success, and am now engaged in maturing a process which I think is both œconomical and convenient. The pile which I employ is formed of several concentric cylinders, of *copper* or *platinum*, separated from one another by porous cylinders, so as to form four or five pairs in a series; the positive metal is a liquid amalgam of zinc, or still better, an amalgam of potassium; the liquid is a solution of sulphate of copper, in case the negative metal is copper, and of chloride of platinum when it is platinum.

One of the chief difficulties is to maintain the light constant. In this I have not yet perfectly succeeded; but I have gained much by employing small hollow thin cylinders of coke, analogous to those employed in Bunsen's battery (except that their dimensions are much less), and by arranging these cylinders as the wick of a lamp. A ring or thick metallic disc, of the same diameter as the cylinder of coke, is arranged above the latter, so that the electric current escapes between the two. Care must be taken that the current pass from the coke cylinder, which is below, to the metallic conductor, which is above, in order that the particles of carbon, transported from below up, fall by their own weight. The whole, that is to say, the coke cylinder and the metallic adjustments which support it, as well as the ring or disc which serves as conductor, is placed in a small glass balloon hermetically closed. It is not necessary to make a vacuum in this, because the little oxygen which it contains is very quickly absorbed by the incandescent coke; but care must be taken to intercept entirely all communication with the external air. With regard to the battery, it is adjusted on the outside of the balloon to two metallic stems, which communicate, one with the coke cylinder, the other with the metallic conductor. It can be changed or charged anew without altering anything in the internal arrangement.

According to the strength of the battery, it is well to employ two points or two cylinders of coke, rather than a single one to a metallic conductor. The preparation of the coke is also very important; I have made several experiments on this subject, but I am not as yet completely satisfied.—*Comptes rendus*, Sept. 15th.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1845.

Chiswick.—September 1. Thick haze: fine. 2, 3. Overcast. 4. Dense dark clouds: clear. 5. Cold and overcast. 6. Hazy: fine. 7. Cloudless and very fine. 8. Foggy: clear and fine. 9. Foggy: clear. 10. Foggy: uniformly overcast: slight rain. 11. Overcast throughout. 12. Slight haze: clear. 13. Thick fog: very fine. 14. Densely overcast: showery. 15. Hazy: very heavy rain: clear at night. 16. Thickly overcast: heavy rain: boisterous at night. 17. Overcast: rain: boisterous throughout. 18. Very boisterous, with showers: clear at night. 19. Boisterous. 20. Fine: rain at night. 21. Densely overcast: rain. 22. Clear: partially overcast and fine. 23. Cloudy: frosty at night. 24. Clear and frosty: very fine, with bright sun: clear. 25. Densely overcast: drizzly. 26. Slight haze; cloudless and fine. 27. Uniformly overcast: slight drizzle: rain at night. 28. Cloudy throughout: clear. 29. Densely overcast: rain. 30. Cloudy and fine.—Mean temperature of the month $4\frac{6}{10}$° below the average.

Boston.—Sept. 1—5. Cloudy. 6—9. Fine. 10, 11. Cloudy. 12. Fine. 13. Foggy. 14. Rain: rain early A.M. 15. Cloudy: rain, with thunder and lightning P.M. 16. Fine: rain early A.M. 17. Rain: rain early A.M. 18. Cloudy. 19. Windy. 20. Fine. 21. Cloudy: rain early A.M.: rain P.M. 22. Cloudy: rain early A.M. 23, 24. Fine. 25. Cloudy. 26. Fine. 27. Cloudy: rain A.M. 28. Cloudy. 29, 30. Fine.

Sandwick Manse, Orkney.—Sept. 1. Haze: clear. 2. Clear. 3. Cloudy*: clear. 4. Cloudy. 5. Clear. 6. Fog: cloudy. 7. Bright: cloudy. 8. Rain. 9. Damp. 10. Fine: clear. 11. Bright: clear. 12. Cloudy. 13. Bright: cloudy. 14. Damp: clear. 15. Cloudy: clear. 16. Bright: clear. 17. Bright: cloudy. 18. Rain: cloudy. 19. Bright: clear. 20. Bright: cloudy. 21, 22. Showers: cloudy. 23. Bright: cloudy. 24. Cloudy. 25. Rain: clear: aurora borealis. 26, 27. Showers. 28. Hail-showers: showers: sleet. 29. Showers. 30. Cloudy: showers.

Applegarth Manse, Dumfries-shire.—Sept. 1—4. Fine, though cloudy. 5. Fine, but clear. 6. Fair, but cloudy. 7. Fair and clear: raw frost. 8. Fair, but cloudy. 9. Rain all day. 10. Fair, but cloudy. 11. Fair and fine. 12. Very fine. 13. Moist and cloudy. 14. Showers. 15. Hoar-frost: shower A.M. 16. Fair, but cloudy. 17. Heavy rain. 18. Very heavy rain. 19, 20. Rain P.M. 21. Fair and drying. 22, 23. Very fine: hoar-frost. 24. Wet A.M.: cleared up. 25, 26. Fair and clear. 27, 28. Heavy rain. 29, 30. Showers P.M.

Mean temperature of the month	52°·4
Mean temperature of Sept. 1844	54 ·0
Mean temperature of Sept. for 23 years	53 ·0

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* This morning there was a fall of ashes over all Orkney apparently volcanic, and supposed to be wafted from Iceland.

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. Sept.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
● 1.	30·278	30·200	29·79	30·27	30·19	30·27	30·27	66	50	59	69½	54½	54½	51½	ne.	ne.	w.	nw.	......	......	......	·05
2.	30·179	30·168	29·79	30·16	30·18	30·27	30·26	64	47	59	66	56½	52½	52	ne.	calm	ne.	nw.	......	......	......	......
3.	30·201	30·185	29·79	30·20	30·14	30·27	30·22	63	41	57	64	43½	53	51½	ne.	calm	ne.	nw.	......	......	......	......
4.	30·205	30·180	29·80	30·17	30·17	30·26	30·22	64	43	54	61	50	54	52	ne.	calm	ne.	......	......	......	......	......
5.	30·140	30·103	29·78	30·16	30·16	30·24	30·24	60	50	54·5	62	47½	53	49	ne.	calm	e.	......	·01	......	......	......
6.	30·149	30·131	29·75	30·19	30·19	30·24	30·28	63	38	59	58½	40½	52	51	ne.	e.	sw.	s.	......	......	......	......
7.	30·137	30·122	29·75	30·20	30·10	30·23	30·13	69	40	60	62	42½	56½	50	ne.	e.	e.	......	......	......	0·10	......
8.	30·115	30·098	29·70	30·04	30·01	29·93	29·88	71	37	57	62	48½	52	56	ne.	calm	s.	sw.	......	......	......	·12
☽ 9.	30·102	30·002	29·63	29·90	29·85	29·74	30·00	78	40	58	59	51	61	51	sw.	calm	s.	n.	......	......	......	......
10.	30·085	29·971	29·57	30·10	30·11	30·23	30·24	70	53	62	59	51½	53	48	ne.	ne.	ne.	ese.	·01	......	......	......
11.	30·038	29·966	29·55	30·05	29·95	30·13	30·08	61	54	56	60	47	51½	50	ne.	calm	ne.	se.	......	......	......	·26
12.	29·955	29·934	29·45	29·93	29·91	30·08	30·03	70	42	61	65	49	53½	49	ne.	calm	nw.	se.	......	......	......	......
13.	29·930	29·750	29·50	29·80	29·55	29·85	29·61	71	53	52·5	58½	51½	53	54	se.	calm	s.	se.	·04	......	......	......
14.	29·599	29·470	29·05	29·38	29·31	29·38	29·36	71	39	57	57	53	55½	51	sw.	calm	wsw.	nw.	·02	·06	0·15	......
○ 15.	29·611	29·353	28·93	29·35	29·38	29·40	29·40	57	43	52	61	36	52½	50	ne.	calm	s.	sw.	·45	·07	......	......
16.	29·699	29·487	29·19	29·44	29·32	29·44	29·41	64	54	54	60½	40	56½	49½	se.	calm	w.	......	·47	·24	......	·21
17.	29·476	29·397	28·90	29·14	28·97	29·34	29·26	64	55	60	60	52	56	52	sw.	calm	e to s.	e.	·19	·34	......	......
18.	29·498	29·271	28·78	28·94	29·04	29·14	29·25	65	48	60	58	52½	52	49½	sw.	calm	e.	nne.	·07	·04	......	·26
19.	29·996	29·681	29·00	29·37	29·72	29·46	29·77	65	34	56	58	47	47	44½	sw.	w.	wnw.	wnw.	......	......	......	......
20.	29·982	29·690	29·50	29·74	29·49	29·78	29·68	62	52	52	58½	37½	48½	48	s.	calm	s to se.	nw.	·29	......	......	......
21.	29·538	29·315	29·09	29·34	29·30	29·43	29·62	65	49	55	59½	51	45½	44½	s.	calm	wnw.	nnw.	·12	·09	1·58	·12
22.	29·946	29·682	29·65	29·58	29·88	29·68	29·85	64	42	51	56	43½	45	44	w.	w.	wnw.	nw.	......	·39	......	·08
☾ 23.	30·223	30·011	29·66	30·08	30·18	30·06	30·15	54	30	48	53½	31½	44	50½	ne.	calm	se.	w.	......	......	......	......
24.	30·242	30·016	29·85	30·10	29·83	29·99	29·68	62	37	42·5	55	32	51	50	e.	calm	sw.	s.	......	......	......	......
25.	29·852	29·692	29·43	29·49	29·48	29·38	29·39	62	38	51	58½	47½	50½	48	s.	calm	w.	ssw.	·01	......	......	·31
26.	29·945	29·740	29·30	29·53	29·68	29·47	29·58	65	40	49	56½	44½	53½	48	w.	calm	s.	w.	......	......	......	......
27.	29·914	29·790	29·45	29·40	29·45	29·12	29·08	60	48	50	60	45½	52	50	sw.	calm	s.	w.	·02	......	......	·08
28.	29·937	29·854	29·33	29·50	29·60	29·24	29·37	63	36	56	54½	44½	48½	47	w.	calm	sw.	w.	......	·03	......	·46
29.	29·949	29·753	29·47	29·60	29·45	29·43	29·32	62	48	48·5	56½	44½	46½	46	sw.	calm	sw.	ssw.	·06	......	0·92	·35
30.	29·800	29·704	29·22	29·40	29·42	29·27	29·23	62	41	50	55	46	49½	45	sw.	calm	sw.	sw.	·01	......	......	·52
Mean.	29·957	29·823	29·45	29·751	29·737	29·758	29·762	64·57	40·63	54·7	59·4	46·0	51·78	49·41					1·77	1·26	2·75	2·82

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[THIRD SERIES.]

DECEMBER 1845.

LXIV. *On the Comets which have been discovered since July 1844.* *By* J. R. HIND, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE prepared, at the request of Mr. Bishop, a few remarks on the comets which have recently appeared, and I forward them in the hope that they may be found useful to those who desire information on the subject without possessing the various astronomical publications in which it is generally to be found.

I also avail myself of the present opportunity to inclose you some results which I have lately obtained for early comets, by means of the Chinese Astronomical Annals.

I have the honour to be, Gentlemen,
Your most obedient Servant,
J. R. HIND.

On the night of July 7, 1844, M. Mauvais discovered a telescopic comet in the constellation Hercules: it was detected at Berlin by M. d'Arrest on the 9th. During the month of August this comet was easily observable; it presented a fine round nebulosity with a lucid point about the centre, and was distinctly seen in the strong twilight. It was followed by European astronomers until the second week in September, when it became invisible from proximity to the sun. Within ten days after perihelion passage (October 17), Mr. Mann rediscovered it at the Royal Observatory, Cape of Good Hope, with the assistance of an ephemeris calculated by M. Nicolai, and forwarded to the colony by the Astronomer Royal. From this time until March 10, Mr. Mann observed it most indefatigably; and he has furnished us with a series of measures which will prove of the greatest value in final researches on the elements. At the latter end of January, Professor Encke with the Berlin refractor, Mr. Lassell at Liverpool, and many others again observed the comet. I caught sight of it on February 2, when it was seen without much difficulty in Mr. Bishop's eleven-foot refractor: the state of the atmosphere near the horizon prevented our taking observations at an earlier date.

The parabolic elements of this comet have been computed by many astronomers. M. Nicolai has based his calculations on four normal positions, three before and one after the perihelion passage; his results are almost identical with my own, obtained from three observations, July 8, November 2, and February 8.

	Nicolai.	Hind.
Per. passage……… (T)	1844. Oct. 17·37544. Berlin mean time.	1844. Oct. 17·33613. Greenwich mean time.
Long. of perihelion (π)	180° 23′ 55″·2	180° 24′ 9″·6
Ascending node … (☊)	31 39 4·9	31 39 5·4
Inclination ……… (i)	48 36 23·7	48 36 21·7
Log. least distance (q)	9·9321208.	9·9321180.
	Motion retrograde.	Motion retrograde.

Our longitudes are referred to the mean equinox of 1845, January 1, and the perihelion is reckoned in the manner most usual among astronomers, viz. *on the ecliptic to the node and thence on the orbit.* The elements require very small corrections, and these will be best applied when we are in possession of the reduced Cape observations. M. Nicolai suggests that the slight differences between his calculations and the normal positions may arise from eccentricity in the orbit.

First or Periodical Comet of De Vico.

Before the last comet had disappeared in the sun's rays, another was found by Father De Vico, at the Observatory of the Collegio Romano at Rome. It was discovered on August 22 in the constellation Cetus. Mr. Mehrop of Hamburg, and Mr. Hamilton Smith of America made independent discoveries of this comet. Astronomers very soon found that a parabola would not satisfy the observations, and M. Faye of the Royal Observatory, Paris, calculated the elements by Gauss's general method, from which it was evident that the true path of the comet was elliptical, and that the period of revolution could not be more than six years. The discovery of another periodical comet so soon after astronomers had recognised that of Faye, among the few which revolve in less than eight years, is as singular as it is important. I subjoin the ellipsis deduced by various computers for this interesting comet.

Faye.	Nicolai.	Goldschmidt.	Hind.
T Sept. 2·483952. Mean time at Paris.	Sept. 2·51316. Mean time at Berlin.	Sept. 2·49152. Mean time at Berlin.	Sept. 2·50362. Mean time at Greenwich.
π ...... 342° 31′ 15″·18	342° 31′ 5″·50	342° 29′ 44″·9	342° 32′ 40″·1
☊...... 63 49 30·64	63 48 48·90	63 48 55·2	63 52 24·1
i ...... 2 54 45·04	2 54 45·80	2 55 1·9	2 54 27·14
φ ...... 38 6 57·68	38 6 31·85	38 12 53·0	37 59 59·65
Log. α 0·4912864.	0·4911552.	0·4929151.	0·4893706.
Period 1993 days.	1992 days.	2004 days.	1980 days.
Motion direct.			

Faye and Nicolai reckon their longitudes from the mean equinox at the commencement of 1845. Goldschmidt counts from mean equinox of 1844, Sept. 21·5, and I have referred my node and perihelion to the apparent equinox of Sept. 30.

The first comet of De Vico was not generally observed after the end of November. With the great equatoreal at Cambridge, it was observed until December 7; I observed it on December 6 with Mr. Bishop's refractor, and perceived it for the last time on the 30th of December. The comet had a bright nucleus, and, during the month of September, a short fan-like tail.

Dr. Brünnow of Berlin writes me that he is making a complete investigation on the elements of this comet, taking account of the higher powers of the masses in the calculation of planetary perturbations. This is precisely the discussion most desirable for comets of short period, as a basis on which future investigations may be erected. We shall probably learn more respecting the resisting medium brought to light by Encke, after the completion of another revolution of De Vico's comet.

Great Comet of 1844–45.

About the middle of December 1844, a very fine comet became visible in the southern hemisphere, in the constellation Grus. It was seen at the Cape of Good Hope on December 18; Mr. Caldecott saw it at Trevandrum on December 30; and it was observed by Mr. Taylor at the Madras Observatory on January 5. The tail was from 7° to 10° in length, the nucleus distinct and resembling a star of the fifth magnitude. On December 28, the tail was found to be slightly curved towards the north. Mr. Maclear considered the general outline "more sharp and clear than the great comet of March 1843."

This comet was found by M. Colla at Parma on February 5, and by Dr. Peters and Mr. Cooper at Naples on February 7. I saw it with great difficulty on March 3, and Professor Challis observed it until the 11th of that month with the great equatoreal at Cambridge. When I found the comet on March 3, it was barely perceptible in a dark field, and was observable with extreme difficulty. A faint oval nebulosity was all that could be distinguished in the large refractor. At the Cape of Good Hope, Mr. Mann observed the comet until the 12th of March, and in addition to measures for position, he has furnished us with some interesting observations on the angle of position of the tail and anterior luminous matter. With regard to the latter phænomenon, we have the following remarks in the Monthly Notices of the Royal Astronomical Society for May:—"On January 11, a faint ray of luminous matter, about $1\frac{1}{4}°$ in length, was seen to extend from the anterior portion of the comet's head in a direction opposite to that of the tail. The breadth of this ray near the head was about $2'$, increasing slightly towards the extremity. Its borders were comparatively well-defined, and the light gradually diminished in intensity from that portion nearest the comet's head until it became insensible. A dark space seemed to separate the head of the comet and ray." It is further remarked, that on the 27th of January, the northern borders of the tail and anterior ray appeared distinct and sharply defined, but the light was fainter on the southern border of the tail, while the corresponding border of the ray could no longer be traced. The anterior ray extended about $5'$ from the head of the comet, but the actual connexion of the two could not be traced. Mr. Waterston, who observed at Bombay, first noticed this singular appendage on the 16th of January, and traced it for 3°, the edges being well-defined and parallel. On January 31 the ray was still seen, but very indistinctly. A similar appearance was noticed in the comet of 1824.

The Cape observations do not yet admit of complete reduction, the places of the comparison stars being generally undetermined. I have reduced Mr. Caldecott's observations and compared them with an ephemeris computed from a set of parabolic elements which I obtained in March last. The final positions, ephemeris, and comparison of observed and calculated places will be found in the *Astronomische Nachrichten*; I shall merely transcribe the elements.

T 1844. December 13·68294. Greenwich mean time.

$$\left.\begin{array}{ll} \pi \ldots\ldots & 296^{\circ}\ 0'\ 32'' \\ ☊ \ \ldots & 118\ 23\ 24 \end{array}\right\}\text{Mean eq. 1845·0.}$$

i 45 36 34
Log. q 9·4001230.
Motion direct.

When Mr. Mann's observations are finally reduced, a more complete investigation may be made, and perhaps we shall then learn something respecting the nature of the conic section described by the comet. From some sextant observations taken in Ceylon, and the positions furnished by later observations in Europe, I found an elliptical orbit, as published in the *Astronomische Nachrichten* of Prof. Schumacher, and also in the *Comptes Rendus* of the Royal Academy of Sciences at Paris. The period in this ellipse was about forty-eight years, but on receiving Mr. Caldecott's observations, it became evident that the earlier observation employed was much in error, and that the probable orbit would be hyperbolic. It is somewhat singular, that about the same time M. Clausen, misled by an erroneous observation at Rome, found an ellipse of thirty-three years' period for De Vico's second comet; thus affording in a short time a double proof of the necessity of *extreme accuracy in cometary observations, and likewise of the advantage arising from the publication of the original measures, in cases where there is no check to be expected from corresponding observations*: by original measures may be understood the mean differences in right ascension and declination of the comet and comparison star, which affords the practical astronomer an important means of checking the deduced place of the comet, and likewise the accuracy of the star's assumed position.

Comet of d'Arrest.

On the 28th of December 1844, M. d'Arrest perceived a nebulosity in Cygnus, which proved to be a telescopic comet. Cloudy weather prevented general observation until the second week in January, when it was seen at most of the observatories. About the middle of February the comet was within 20,000,000 miles from the earth: at this time it was distinctly visible with the smallest optical aid; I saw it frequently with a telescope one foot in length and about one inch aperture. The exact determination of the elements was attended with more than usual difficulty, from the comet being so nearly in opposition. M. Sievers of Altona computed the orbit by Gauss's general method, and obtained hyperbolic elements:

nevertheless a parabola satisfies the observations very well. The following are the later determinations of the orbit:—

Nicolai.	Götze.	Hind.
T Jan. 8·19615. Mean time at Berlin.	Jan. 8·193091. Mean time at Berlin.	Jan. 8·165484. Mean time at Greenwich.
π...... 91° 19′ 57″·2	91° 19′ 39″·07	91° 20′ 22″·1
☊ ... 336 44 30·5	336 44 30·33	336 44 12·9
i ...... 46 50 30·5	46 50 30·08	46 50 39·0
Log. *q* 9·9567518.	9·9567392.	9·9567652.
Motion direct.		

All the longitudes are referred to the mean equinox of 1845·0.

The comet was observed generally until the middle of March, but it was seen at Berlin with the great refractor until the 30th of the same month. Its appearance was that of a large bright nebula, not quite circular, with a bright nucleus situated eccentrically, though very slightly so.

Second Comet of De Vico.

On the night of February 25, 1845, another comet was found at Rome by De Vico, in the constellation Ursa Major. When first observed in this country, it was brighter than the last comet, of a circular form, with a very fine lucid spot or condensation of the nebulous matter in the centre. We saw it at this Observatory until April 17; at Cambridge it was observed several days longer. The elements have been deduced by many computers, but I shall only transcribe those which depend on the widest extent of observation. M. Faye's excellent orbit was communicated to me by letter, the others are extracted from Professor Schumacher's *Astronomische Nachrichten.*

Faye.	Götze.	Hind.
T April 21·03748. Mean time at Paris.	April 21·078454. Mean time at Berlin.	April 21·03290. Mean time at Greenwich.
π...... 192° 33′ 18″·6	192° 34′ 13″·8	192° 33′ 14″·2
☊ ... 347 6 45·2	347 6 59·3	347 6 34·7
i ...... 56 23 36·3	56 22 50·6	56 24 5·6
Log. *q* 0·0985330.	0·0984859.	0·0985420.
Motion direct.		

The longitudes are counted from mean equinox 1845·0.

The Great Comet of June 1845. Colla's *Comet.*

On the 2nd of June M. Colla detected a fine comet in the constellation Perseus. During the first half of the month it was distinctly visible to the naked eye. The nucleus was very bright, equal to a star of the third magnitude, and a tail 2° in length was seen until moonlight interfered. On June 9, with a low power on Mr. Bishop's refractor, the tail was evidently bifid; one well-defined branch extended to a distance of about 2½° from the nucleus, the other train was much shorter and very ill-defined. The tail was far brighter on the preceding than on the following side. In about a week from this time, a coma formed round the nucleus, and the tail assumed a fan-like form, somewhat diminishing in length. Prof. Santini observed the comet at Padua until the end of June, but it was not generally seen after the 20th.

The most rigorous investigation on the elements of this comet is one recently published by M. d'Arrest. From fifty-two right ascensions and fifty-four declinations he deduced seven normal positions between June 7 and 17. From these, by the method of equations of condition with least squares, M. d'Arrest computed the most probable parabolic elements, and by a direct solution of the equations (a term depending on a small variation in the eccentricity being introduced), he found that the resulting elements were hyperbolic, with an eccentricity = 1·0025942. It appears, however, that the result of my calculations relating to the comet of 1596, undertaken at the suggestion of Prof. Schumacher, induced M. d'Arrest to ascertain how the observations would be represented by an ellipse, with a period of 249 years; and he finds elements agreeing in the most satisfactory manner with his normal places. It is therefore extremely probable that the comet of 1596, observed by Tycho, is identical with the great comet of June 1845.

M. d'Arrest's elliptical elements are as follows:—

T 1845. June 5·71064. Mean time at Berlin.

$$\begin{array}{rl} \pi & 262^\circ\ 2'\ 55''\!\cdot\!9 \\ \Omega & 337\ 48\ 56\cdot1 \end{array}\Bigg\}\ \text{Mean eq. 1845·0.}$$

$$\begin{array}{rl} i & 48\ 41\ 58\cdot7 \\ \sin^{-1} e = \varphi = & 81\ 50\ 22\cdot3 \\ \text{Log. } a = & 1\cdot598394. \end{array}$$

Motion retrograde.

The details of my investigation on the comet of 1596 will be found in the *Astronomische Nachrichten*. The tempestuous state of the weather only permitted Tycho to observe the comet on three days, but the observations were made at very convenient intervals for the determination of the elements. The copy of Tycho's observations, from which I calculated the comet's positions, was transcribed from the *originals* by Prof. Schumacher: it differs in no material points from the published details in the Cometography of M. Pingré.

Mr. Bishop's Observatory, Regent's Park,
November 3, 1845.
J. R. HIND.

LXV. *On the Elements of several Comets not previously computed.* By J. R. HIND, *Esq.*

IN the appendix to the *Connaissance des Tems* for 1846, M. Edouard Biot has given a translation of several Chinese catalogues of comets and extraordinary stars: these records have enabled me to calculate the elements of five comets whose orbits were previously unknown. From the nature of Chinese observations no great dependence can be placed on elements deduced from them, nevertheless it is probable that the results will be sufficiently near in the generality of cases, to show whether the comets are identical with any that have been observed and computed in after times.

Prof. Schumacher has published the elements of the comets in 568, 574 and 1385, in his *Astronomische Nachrichten.* The orbits of the comets in 574 and 770 are extremely uncertain, the others are, perhaps, tolerable approximations. I attempted the elements of a comet in the year 12 B.C., but could arrive at no satisfactory conclusions, owing to the vagueness of the Chinese description.

In the present paper all dates are given in the *Julian style*, unless otherwise mentioned.

Second Comet of 568.—In the third year of the period *Thien-ho*, on day *Ki-ouei* of the seventh moon (September 3), an extraordinary star was seen in the stellar division *Fang* (commencing at π Scorpii) and in the division *Sin* (commencing at σ Scorpii): it became gradually larger and moved towards the east. At the end of September it entered the group of stars surrounding α Ophiuchi and α Herculis; it passed by the star on the right of *Ho-ku* (γ Aquilæ). On day *Kouei-ouei* (September 27) it passed over the *Hou-koua* (κ, β, γ, δ Delphini), entering the stellar division *Tchi* (commencing at α Pegasi): it passed over *Li-koung* (λ, μ Pegasi). On day *Jin-yn*, ninth moon (October 16), the comet entered the stellar division *Koei* (commencing at ζ Andromedæ) and gradually diminished. On day *Jin-su* (November 5) it arrived on the north of *Leou* (determining star β Arietis) about 1° and disappeared. It was visible during sixty-nine days. M. Biot, in addition to the above, has the following extract:—"June 28 (under *Fei-ti* of *Tchin*, period *Kouang-ta*, second year, sixth moon, day *Jin-*

tseu), a strange star was seen to the east of the constellation *Ti* (commencing at *α* Libræ)." But it is to be observed, that in 568 there was no day *Jin-tseu* in the sixth moon: the date given by M. Biot falls in the fifth moon. The next day, *Jin-tseu*, answered to August 27 in the seventh moon; and I am strongly inclined to think that this is the date intended, for the following reasons:—1st. Ma-tuon-lin says the comet observed on the day *Jin-tseu* was situated to the east of *Ti* (Libra); and this position agrees very well with that which the second comet of 568 would have occupied on that day, for on September 3 it was in Scorpio, and moving in the order of signs: it would therefore pass through Libra previous to September 3. 2nd. The Chinese state that the comet was seen in all sixty-nine days, and that it disappeared on the day *Jin-su* of the ninth moon, or on November 5: this fixes the day of its discovery on August 27, or on day *Jin-tseu* of the seventh moon, which is therefore very probably the date intended in the extract given above.

The following is my parabolic orbit for this comet:—

Perihelion passage 568. August 28·27. Greenwich mean time. *Julian style.*

	°	′	
Longitude of the perihelion on the orbit...	316	47	Equinox of 568.
Longitude of ascending node	294	36	
Inclination to the ecliptic	4	2	
Perihelion distance	0·889 log. = 9·9491.		

Motion direct.

This comet made a near approach to the earth at the end of September.

The Comet of 574 was discovered in the third year of the period *Kien-te*, second moon, day *Wou-ou* (April 4): it was of a bluish colour and appeared to the south-east of the constellation Auriga; it was about 3° in length, and moved slowly to the east. On day *Jin-tchin*, fourth moon (May 8), it entered into *Wen-tchang* (*θ*, *υ*, *φ* Ursæ Majoris). On day *Ting-wei* (May 23) the comet entered the square of *Pe-teou* (*α*, *β*, *γ*, *δ* Ursæ Majoris); it afterwards left it and became gradually fainter. The comet was seen in all ninety-three days (M. Biot thinks we should read sixty-three days). From these positions I have deduced the following elements, which are very uncertain:—

Perihelion passage 574. April 7·28. Greenwich mean time. *Julian style.*

	°	″	
Longitude of perihelion	143	39	Equinox of 574.
Longitude of ascending node ...	128	17	
Inclination	46	31	
Log. perihelion distance.........	9·9836.		

Motion direct.

Ma-tuon-lin has some additional particulars relating to a comet in this year (no doubt the same as the above). On day *Y-mao* of fourth moon (May 31), it was on the confines of *Tse-kong* (the tail of Draco): it pointed to *Ou-ti-tso* (β Leonis), and moved slowly towards the south-east. The colour was a pale red. On day *Kia-tse*, fifth moon (June 9), it stopped to the north of *Chang-tai* (ι, κ Ursæ Majoris) and disappeared.

Comet of 770.—In the fifth year of the period *Ta-li*, fourth moon, day *Ki-ouei* (May 26), a comet appeared in the constellation Auriga: it had a luminous train 30° in length. On day *Ki-mao*, fifth moon (June 15), the comet was seen in the north. On day *Kouei-ouei* (June 19) it moved towards the east; it approached the star in the middle of *Pa-ko* (δ, ξ, *h*, *k*, *i* Aurigæ and the small neighbouring stars of Camelopardalis). On day *Kouei-mao*, sixth moon (July 9), it was at 2° of *San-koung* (stars in Asterion, under η Ursæ Majoris). On the 25th of the same month it disappeared.

My rough parabolic elements are:—

Perihelion passage 770. June 6·6. Greenwich mean time. *Julian style.*

Longitude of perihelion	2°	Equinox of 770.
Longitude of ascending node ...	89	
Inclination of the orbit	60	
Log. distance in perihelion	9·7801	

Motion retrograde.

These elements very much resemble those computed by Pingré for a comet in 1299, and, with the exception of the perihelion distance, they bear considerable analogy to those of the comet which passed the perihelion in December 1818.

The Comet of 1385 was discovered in China by the astronomers of the *Ming* dynasty, on October 23 (period *Houng-wou*, eighteenth year, ninth moon, day *Wou-yn*): it was then on the borders of *Thai-wei* (a constellation of stars in Leo and Virgo, surrounding β Leonis); it touched β Virginis, and left *Thai-wei* by what the Chinese termed the "great gate," *i. e.* between β and η Virginis. On day *Y-yeou* (October 30) it entered the division *Y* (commencing at α Crateris): the tail was about 10° long. On day *Keng-yn* of tenth moon (Nov. 4), the comet touched the group *Kiun-men* (ξ Hydræ, &c.): the tail swept *Thien-miao* (the stars in Pixis Nautica). From this description I have deduced an orbit, which may perhaps serve to recognise the comet if it should reappear.

Perihelion passage 1385. Oct. 16·26. Greenwich mean time. *Julian style.*

Longitude of perihelion	101°	47″	Equinox of 1385.
Longitude of ascending node ...	268	31	
Inclination to ecliptic	52	15	
Log. perihelion distance	9·8886		

Motion retrograde.

The Comet of 1433 was observed both in Europe and China. In the eighth year of the period *Siouen-te*, eighth (intercalary) moon, day *Jin-tseu*, a comet was seen near *Thien-tsiang* (θ, ι, κ Bootis): the tail was about 10° in length. On day *Ki-sse* (October 2) it entered *Kouan-so* (Corona Borealis): it swept the stars δ, μ, ι, ψ, φ Bootis. On day *Ki-mao* (October 12) it was seen again, entering the group of stars surrounding α Ophiuchi and α Herculis (a Chinese constellation): it swept the star *Tsin* (κ Herculis). The comet was seen twenty-four days. The elements which I have computed from this description, though doubtless very uncertain, have a striking similarity to those of a comet discovered by M. Montaigne in 1780 and calculated by Olbers. I place the two orbits in juxtaposition for the sake of easy comparison.

	Comet of 1433.		*Second Comet of* 1780.
Perihelion passage.	Nov. 5·19. Greenwich mean time. *Julian style.*		Dec. 1·85139. Paris mean time. *New style.*
Long. of perihelion	262°	Equinox of 1433.	$246^{\circ}\ 52'$
Long. of asc^ds^. node	110		121 1
Inclination	77 or 76°		$72\ 3\ 30''$
Log. perihelion distance	9·5166 or $q = 0·329$.		$q = 0·51528$.
	Motion retrograde.		Retrograde.

It is extremely desirable that we should extract as much as possible from the annals of the Chinese astronomers; in those cases where the probable identity of comets seen at intervals of many hundred years can be brought to light by the aid of their records, we in some measure anticipate what would otherwise be the work of centuries to come. It is true that these bodies can now be observed with the greatest accuracy, and that the researches of modern geometers furnish us with ready means of ascertaining the period of revolution when the observations can be implicitly depended upon; still the Chinese records enable us to search for past returns of a comet, whose periodicity is suspected, and thus render it possible to carry back its history to a more remote period than any European observations we possess. In addition to those comets already computed by Burckhardt, Pingré and myself, M. Biot's excellent memoir exhibits the path of several others, perhaps in sufficient detail to allow of rough approximations to their elements.

Mr. Bishop's Observatory, Regent's Park,
November 3, 1845.

J. R. Hind.

LXVI. *Observations on a paper by* Prof. Faraday *concerning Electric Conduction and the Nature of Matter. By* Richard Laming, *Esq.*

To Richard Taylor, Esq.

Sir,

In February of last year a letter from Professor Faraday was published by you, expressing an opinion that the immaterial centres of force of Boscovich* have a greater claim to be regarded as true than the solid atoms of Newton; and in which he represents his preference to result from the contemplation of certain *facts* relating to electrical conduction and insulation. In going over his arguments I have not arrived at the same conclusion, owing to a difficulty in admitting an assumption which I find mixed up with the facts. It will be better to give Mr. Faraday's meaning in his own words: he says, "The view of the atomic constitution of matter which I think is most prevalent, is that which considers the atom as something material having a certain volume, upon which those powers were impressed at the creation, which have given it, from that time to the present, the capability of constituting, when many atoms are congregated together into groups, the different substances whose effects and properties we observe. These, though grouped and held together by their powers, do not touch each other, but have intervening space, otherwise pressure or cold could not make a body contract into a smaller bulk, nor heat or tension make it larger; in liquids these atoms or particles are free to move about one another, and in vapours or gases they are also present, but removed very much further apart, though still related to each other by their powers. . . . If the view of the constitution of matter already referred to be assumed to be correct, and I may be allowed to speak of the particles of matter and of the space between them as two different things, then space must be taken as the only continuous part, for the particles are considered as separated by space from each other." All this may be accepted as descriptive of matter as it is regarded by the theory of solidity; and with this admission we proceed at once to the main question, namely, to which of the two parts of a body does its electrical conducting property belong; does it appertain to the centres of force themselves, or to the spaces which envelope them? I agree with Mr. Faraday that the conducting property does not belong to space, because if it did, it would follow that as all bodies indiscriminately are re-

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[* See Priestley's "History of Vision," &c., and "Disquisitions on Matter and Spirit," vol. i. p. 34, &c.—Ed.]

plete with spaces, so all bodies, without exception, would be conductors, however much their natures might otherwise differ. This conclusion is manifestly inevitable, for to deny it would be to impute different qualities to different parts of space. Not so inevitable, however, is the next conclusion which I have to notice: "Metal is a conductor; but," says Mr. Faraday, "how can this be except space be a conductor? for it is the only continuous part of the metal; space therefore (holding the theory of solid atoms) must be a conductor, or else the metals could not conduct." That gentleman therefore sees no alternative but in believing that "the reasoning ends in a subversion of the theory (of solid atoms) altogether."

This second conclusion would be as unavoidable as the preceding one, provided we knew, *as a fact*, that absolute continuity is necessary to electrical conduction; Mr. Faraday has assumed this to be the case, but I am not aware that anything has ever been observed in the nature of matter from which it can be inferred. By the old maxim, that *matter cannot act where it is not*, absolute continuity is made necessary to conduction; but this maxim is itself an assumption needing the evidence of facts, and therefore it may not be adduced as a proof of any other assumption.

The chief cause why I have thought it necessary thus to attempt the vindication of the theory of atoms from a charge of inconsistency, is because I think there is a way of looking at that theory, by which the conducting and insulating properties of bodies appear more intelligible than on other doctrines; and this without involving any other assumption (beyond the mere existence of the atoms) than the hypothesis that *different sorts of atoms are naturally associated with unequal quantities of electricity*. The degree of probability attaching to this hypothesis may be estimated by reflecting that if we had to assume its untruthfulness, that is to say, that all atoms, however unlike in their natures, had equal quantities of electricity naturally combined with them, the assumption might be deemed much more questionable. I will now beg permission to give an outline of the view I have taken of the theory of atoms, so far as it relates to electrical conduction and insulation; and as it is simple, it may be done in a few words.

In his "Researches," Mr. Faraday has adduced an experiment to prove that the quantity of electricity naturally combined with matter is enormously great; after making every allowance for the difficulty of a correct estimation, we may safely infer the quantity to be great enough to apportion to each atom of matter sufficient electricity to envelope it. In the second place, we know, by facts, that the force by which electricity is attracted by matter is greater at shorter distances; from whence it follows that the electricity around an atom of matter will approach to it as near as possible, thus forming a sphere of which the atom is the centre. Again, by the theory of solidity, a mass of electrical matter, or electricity, may be regarded as composed of electrical atoms, just as a mass of ordinary matter contains ordinary atoms; and thus the sphere of electricity which surrounds an ordinary atom will consist of a number of electrical atoms arranged in concentric strata. The number of electrical atoms belonging to a given ordinary atom may be assumed to be such as to complete its external spherical stratum, or, on the contrary, it may be such as to leave that external spherical stratum more or less imperfect. Now, in the former of these two cases, where the sphere of electricity has an unbroken surface, we have all the conditions necessary for electrical insulation; and in the latter case, where the surface of the sphere of electricity is broken, we have all the conditions essential to electrical conduction.

To explain this more fully we may begin with conductors. Imagine a line of ordinary atoms, each of which *naturally* has on the bounding surface of its mass of electricity *ten* electrical atoms; and place the ends of this line in connexion with two bodies oppositely electrified. Thereupon so many of the electrical atoms of the plus charge as can find room will place themselves in the broken electrical stratum of the first ordinary atom of the conductor; because when there *a certain part* of each electrical atom then in that stratum, in the aggregate amounting to *ten* whole atoms, will be attracted by the said ordinary atom. This manifestly is required by the law of force, which is greater at the lesser distance; and because the aggregate of all the parts is at a less distance than are any *ten* whole atoms in the same stratum. At the other end of the conducting line the negative body will be appropriating to itself the *ten* superficial electrical atoms naturally belonging to the most proximate ordinary atom of the conductor; because this atom, in its turn, can make an equal demand upon the next in succession, that one on its neighbour, and so onward continually to the plus end of the line, *without at any time the attractive force being called upon to act at an increased distance.* The first *ten* plus atoms being thus virtually discharged into the minus body, a second similar number will then be transferred in like manner, and after that a third, and thus the conducting action will be repeated as many times as is requisite to convey the whole electrical charge of the plus body into that which was equally minus. According to this explanation of electrical conduction, that phænomenon essentially depends on the facility possessed by an ordinary atom to receive on the surface of its mass of electricity a surplus of electrical atoms *at an equal distance from the common centre, as are already some of its own.*

In the case of insulators, we have ordinary atoms with perfect external spherical strata; a condition obviously incapable of conducting electricity, because no surplus number of electrical atoms, presented by a plus body, can possibly arrive so near to the first ordinary atom of a line as are already its own most distant electrical atoms.

It is not my intention, Sir, to enlarge here on this view of matter and of electrical action, because to trace the branches of the subject would exceed the limits that could be allowed in your valuable periodical. I hope soon to lay before the public my electrical opinions in a detailed form; but, before concluding my letter, I will just allude to what appears to me to be an aptitude in the theory of atoms to meet the exigences of electrical excitation and chemical affinity; which I think may both be shown to proceed from one and the same cause, the difference in result depending only upon unequal susceptibilities to locomotion. To have the most simple case, imagine two dissimilar ordinary atoms to be placed in contiguity; the atoms, being dissimilar, have unequal quantities of electricity around them, forming spheres of unequal radii. On the surface of each electrical sphere let there be one electrical atom, necessary to make up the complement of its mass of electricity. It is manifest that, on the reciprocal approach of the electrical spheres, the two external electrical atoms will place themselves between them, both in a line at right angles to the common axis, and in this position *one-half* of each external electrical atom be attracted towards each ordinary atom. If the ordinary atoms be now forcibly separated, *both* of the external electrical atoms will attach themselves to the sphere of least radius; because the electrical attraction acts with the greater force at the lesser distance, and because also an atom is by its nature indivisible.

I am, Sir, yours, &c.,

RICHARD LAMING.

Clichy-la-Garenne, near Paris, Oct. 4, 1845.

LXVII. *On the Transformation of Elliptic Functions.* By Arthur Cayley, *Esq., M.A., F.C.P.S., Fellow of Trinity College, Cambridge**.

IN a former paper I gave a proof of Jacobi's theorem, which I suggested would lead to the resolution of the very important problem of finding the relation between the complete functions. This is in fact effected by the formulæ there given, but there is an apparent indeterminateness in them, the cause of which it is necessary to explain, and which I shall now show to be inherent in the problem. For the sake of supplying an omission, for the detection of which I am indebted to Mr. Bronwin, I will first recapitulate the steps of the demonstration.

If $\frac{1}{2}\omega$, $\frac{1}{2}\upsilon$† be the complete functions corresponding to ϕx, then this function is expressible in the form

$$\phi x = x\,\Pi\left(1+\frac{x}{m\omega+n\upsilon}\right)\div\Pi\left(1+\frac{x}{m+\frac{1}{2}\omega+n+\frac{1}{2}\upsilon}\right).$$

Let p be any prime number, μ, ν integers not divisible by p, and

$$\theta=\frac{\mu\omega+\nu\upsilon}{p}.$$

The function

$$\phi_1 x=\phi x.\frac{\phi(x+2\theta)}{\phi 2\theta}\,\frac{\phi(x+4\theta)}{\phi 4\theta}\cdots\frac{\phi(x+2(p-1)\theta)}{\phi(2(p-1)\theta)}$$

is always reducible to the form

$$x\,\Pi\left(1+\frac{x}{m'\omega'+n'\upsilon'}\right)\div\Pi\left(1+\frac{x}{m'+\frac{1}{2}\omega'+n'+\frac{1}{2}\upsilon'}\right).$$

Or $\phi_1 x$ is an inverse function, the complete functions of which are $\frac{1}{2}\omega'$, $\frac{1}{2}\upsilon'$. And where ω', υ' are connected with ω, υ by the equations

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

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

α, β, α', β' being any integers subject to the conditions that α, β' are odd and α', β even; also

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* Communicated by the Author.

† Analogous to the K, K′ of M. Jacobi.

$$\alpha\beta' - \alpha'\beta = p,$$
$$\mu\beta' - \nu\alpha' = l'p,$$
$$\mu\beta - \nu\alpha = lp,$$

l, l' being any integers whatever. In fact, to prove this, we have only to consider the general form of a factor in the numerator of $\phi_1 x$. Omitting a constant factor, this is

$$\left(1 + \frac{x}{m\omega + n\upsilon + 2r\theta}\right) \quad [r < p];$$

and it is to be shown that we can always satisfy the equation

$$m\omega + n\upsilon + 2r\theta = m'\omega' + n'\upsilon',$$

or the equations

$$pm + 2r\mu = m'\alpha + n'\alpha',$$
$$pn + 2r\nu = m'\beta + n'\beta';$$

and also that to each set of values of m, n, r, there is a unique set of values of m', n', and *vice versâ*. This is done in the paper referred to. Moreover, with the suppositions just made as to the numbers α, β' being odd and α', β even, it is obvious that m' is odd or even, according as m is, and n' according as n is, which shows that we can likewise satisfy

$$\overline{m + \frac{1}{2}}\,\omega + \overline{n + \frac{1}{2}}\,\upsilon + 2r\theta = \overline{m' + \frac{1}{2}}\,\omega' + \overline{n' + \frac{1}{2}}\,\upsilon';$$

and thus the denominator of $\phi_1 x$ is also reducible to the required form.

Now proceeding to the immediate object of this paper, α, β, α', β', and consequently ω', υ' are to a certain extent indeterminate. Let A, B, A′, B′ be a particular set of values of α, β, α', β', and O, P the corresponding values of ω', υ'. We have evidently A, B′ odd and A′, B even. Also

$$\mathrm{A}\,\mathrm{B}' - \mathrm{A}'\,\mathrm{B} = p,$$
$$\mu\,\mathrm{B}' - \nu\,\mathrm{A}' = \mathrm{L}'p,$$
$$\mu\,\mathrm{B} - \nu\,\mathrm{A} = \mathrm{L}p,$$

$$\mathrm{O} = \frac{1}{p}(\mathrm{A}\,\omega + \mathrm{B}\,\upsilon),$$

$$\mathrm{U} = \frac{1}{p}(\mathrm{A}'\,\omega + \mathrm{B}'\,\upsilon).$$

By eliminating ω, υ from these equations and the former system, it is easy to obtain

$$\omega' = a\,\mathrm{O} + b\,\mathrm{U},$$
$$\upsilon' = a'\,\mathrm{O} + b'\,\mathrm{U},$$

where

$$a = \frac{1}{p}(\alpha\,\mathrm{B}' - \beta\,\mathrm{A}'), \qquad b = -\frac{1}{p}(\alpha\,\mathrm{B} - \beta\,\mathrm{A}),$$

$$a' = \frac{1}{p}(\alpha'\,\mathrm{B}' - \beta'\,\mathrm{A}'), \qquad b' = -\frac{1}{p}(\alpha'\,\mathrm{B} - \beta'\,\mathrm{A}).$$

The coefficients a, b, a', b' are integers, as is obvious from the equation $\mu\,(\alpha\,\mathrm{B}' - \beta\,\mathrm{A}') = p\,(\mathrm{L}'\,\alpha - l\,\mathrm{A}')$, and the others analogous to it; moreover, a, b' are odd and a', b are even, and

$$a\,b' - a'\,b = \frac{1}{p^2}\,(\mathrm{A}\,\mathrm{B}' - \mathrm{A}'\,\mathrm{B})\,(\alpha\,\beta' - \alpha'\,\beta);$$

i. e.

$$a\,b' - a'\,b = 1.$$

Hence the theorem,—"The general values ω', υ' of the complete functions are linearly connected with the particular system of values O, U by the equations, $\omega' = a\,\mathrm{O} + b\,\mathrm{U}$, $\upsilon' = a'\,\mathrm{O} + b'\,\mathrm{U}$, in which a, b' are odd integers and a', b even ones, satisfying the condition $a\,b' - a'\,b = 1$."

With this relation between O, U and ω', υ', it is easy to show that the function $\phi_1\,x$ is precisely the same, whether O, U or ω', υ' be taken for the complete functions. In fact, stating the proposition relatively to $\phi\,x$, we have,—"The inverse function $\phi\,x$ is not altered by the change of ω, υ into ω', υ', where $\omega' = \alpha\,\omega + \beta\,\upsilon$, $\upsilon' = \alpha'\,\omega + \beta'\,\upsilon$, and α, β, α', β' satisfy the conditions that α, β' are odd, α', β even, and $\alpha\,\beta' - \alpha'\,\beta = 1$." This is immediately shown by writing

$$m\,\omega + n\,\upsilon = m'\,\omega' + n'\,\upsilon',$$

or

$$m = m'\,\alpha + n'\,\alpha',$$
$$n = m'\,\beta + n'\,\beta'.$$

It is obvious that to each set of values of m, n there is a unique set of values of m', n', and *vice versâ*: also that odd or even values of m, m' or n, n' always correspond to each. It is, in fact, the preceding reasoning applied to the case of $p = 1$.

Hence finally the theorem,—"The only conditions for determining ω', υ' are the equations

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

where α, β' are odd and α', β even, and

$$\alpha\,\beta' - \alpha'\,\beta = p, \quad \mu\,\beta' - \nu\,\alpha' = l'\,p, \quad \mu\,\beta - \nu\,\alpha = l\,p,$$

l and l' arbitrary integers: and it is absolutely indifferent what system of values is adopted for ω', υ', the value of $\phi_1\,x$ is precisely the same."

We derive from the above the somewhat singular conclusion, that the complete functions are not absolutely determinate functions of the modulus; notwithstanding that they are given by the apparently determinate conditions,

$$\frac{1}{2}\omega = \int_0^{\frac{1}{c}} \frac{dx}{\sqrt{(1-c^2x^2)(1+e^2x^2)}},$$

$$\frac{1}{2}\upsilon = \int_0^{\frac{1}{e}} \frac{dx}{\sqrt{(1+c^2x^2)(1-e^2x^2)}}.$$

In fact definite integrals are in many cases really indeterminate, and acquire different values according as we consider the variable to pass through real values, or through imaginary ones. Where the limits are real, it is tacitly supposed that the variable passes through a succession of real values, and thus ω, υ may be considered as completely determined by these equations, but only in consequence of this tacit supposition. If c and e are imaginary, there is absolutely no system of values to be selected for ω, υ in preference to any other system. The only remaining difficulty is to show from the integral itself, independently of the theory of elliptic functions, that such integrals contain an indeterminateness of two arbitrary integers; and this difficulty is equally great in the simplest cases. Why, *à priori*, do the functions

$$\sin^{-1} x = \int_0 \frac{dx}{\sqrt{1-x^2}}, \text{ or } \log x = \int_0 \frac{dx}{x}$$

contain a single indeterminate integer?

Obs. I am of course aware, that in treating of the properties of such products as $\Pi\left(1+\frac{x}{m\omega+n\upsilon}\right)$, it is absolutely necessary to pay attention to the relations between the infinite limiting values of m and n; and that this introduces certain exponential factors, to which no allusion has been made. But these factors always disappear from the quotient of two such products, and to have made mention of them would only have been embarrassing the demonstration without necessity.

LXVIII. *On the Diurnal Changes of the Aqueous Portion of the Atmosphere, and their Effects on the Barometer.* By THOMAS HOPKINS, *Esq.*[*]

IT is admitted by meteorologists, that the various quantities of aqueous vapour which exist in the atmosphere during the different hours of the day, contribute to the production of

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[*] Communicated by the Author; having been read at the Manchester Literary and Philosophical Society, October 7, 1845.

the variable atmospheric pressure, and the semi-diurnal fluctuations of the barometer. The vapour is, at a certain hour in the morning, at its minimum quantity, from which it increases during the day up to its maximum; after that it declines, and its variable pressure is exerted on the mercury of the barometer, and affects the height of the column. This takes place in a less or greater degree in all latitudes, though to the greatest extent near the equator.

The quantity of vapour existing in the atmosphere in each hour of the day is ascertained from the dew-point, or point of condensation; it having been found that each particular quantity of vapour diffused through the air has its separate dew-point. The dew-point is therefore taken as the measure of the quantity of aqueous matter existing in the atmosphere, and of the vapour pressure, at every period of time. This pressure, thus ascertained, being deducted from the whole atmospheric pressure, furnishes the amount of the gaseous pressure, as given in our Meteorological Registers and Tables.

But, is the dew-point a correct measure of the quantity of aqueous matter that passes into and remains in the atmosphere during the different times of the day? On the answer to this question it depends whether the hourly vapour and gaseous pressures on the barometer are, or are not, correctly given in our registers. If the dew-point be a true measure, then the pressure arising from aqueous matter may be taken to be such as is stated in those registers, and so far all the reasonings respecting the causes of the diurnal fluctuations of the barometer may be correct; but if the dew-point is a fallacious measure of that pressure, then the alleged facts may be unfounded, and the conclusions drawn from them erroneous.

There is reason to believe that in certain parts of the world, and for considerable periods of time, the dew-point may be a correct indicator of the pressure of aqueous matter, but in other parts it may not; and in order that we may trace this difference, in different times and places, we will inquire what are the relative quantities of vapour that hourly pass into the atmosphere, in some of those parts from which we have been furnished with accounts, and endeavour to learn whether those quantities are such as to accord with the dew-points.

Kaemtz, a German meteorologist, in his Course of Meteorology, has furnished tables of the hourly vapour pressure in different places, deduced, in the usual way, from the dew-point, and among them of that which is found to be the mean of the year at Appenrade, in Denmark, from seven in the morning to eleven in the evening. At seven, the pressure, in French measure, is 8 millimetres ·119, from which it increases until one in the afternoon, when it reaches 9·511. From this time it diminishes, and at 11 P.M. is only 7·863.

The same writer has given the vapour pressure on the coasts of the Baltic, at Trapstow near the Rya, for the months of July and August. It appears that in those parts the minimum pressure for July is 10·05 at two o'clock in the morning, and the maximum is 11·41 at two o'clock in the afternoon. For August, the minimum is 11·18 at three o'clock in the morning; and there are two risings, the first until ten o'clock, when it is at 12·05; from this time it falls till two, and then suddenly rises until three o'clock, from which time it falls for the rest of the day. From these statements we find that there is, on the coasts of the Baltic, particularly in August, in the middle of the day, a material departure from a single rise and a single fall in the vapour pressure.

There are also tables for Zurich, and other places in its neighbourhood. At Zurich, in the month of June, the minimum pressure is 10·56 at 4 A.M., from which hour it rises until 8 A.M. After this it falls a little, and irregularly fluctuates until 8 P.M., when it reaches 11·34, having fluctuated greatly during twelve hours, namely, from 8 A.M. to 8 P.M., and ranged 0·78.

In September, at the same place, the minimum was at 5 A.M., and there were two risings, with an intervening fall. The first rise was up to twelve o'clock,—four hours later than the first in June; and the advance above the minimum was 1·73, making a greater range than that of June by 0·95. Here, too, the disturbance in the middle of the day is very palpable. These parts of the world are at comparatively low levels,—the first named being near the sea, and the last (at Zurich) an inland situation, which, though considerably above the sea, is not on a mountain.

When these observations were made at Zurich in the month of June, others were made on the adjoining mountain, called the Righi, 1402 metres above the Lake of Zurich. On the Righi, the minimum pressure was at 5 A.M., an hour later than that on the plain, being then 6·27, from which it rose until noon, and reached 7·54, making a range of 1·27. From this hour the pressure declined until five the next morning.

On the Faulhorn, a mountain in the same locality, but higher than the Righi by 870 metres, observations were made in September, at the same time as others were made at Zurich; and on the mountain the minimum pressure was 3·40, and occurred at 6 A.M., an hour later than at Zurich. From this time it rose until three in the afternoon, when it reached 5·07, making the range in the day so much as 1·67. It is thus shown that the range of vapour pressure was greatest, not where the temperature was the most raised, and where evaporation must have been the greatest, but in the latest and coolest month, and on the highest mountain! And in September the pressure increased to the latest period of the day, not near the surface, the source of evaporation, but on the high mountain. These irregularities show that some cause was in operation, which determined the vapour that had been produced by evaporation from the surface of the earth in the warm and comparatively dry month of June to continue increasing at the low level up to eight in the evening, but to accumulate only to a moderate extent, whilst on the mountain it accumulated to a much greater extent, but not later than until noon. In the cooler month of September, however, the vapour accumulated to about an equal extent, and about the same times, on the low level and on the high mountain, presenting a great difference between the action of the vapour in June and in September. The absolute pressure of the vapour, it will be recollected, is greater in the lower than in the higher strata; but the increase of that pressure is greater in the higher part in the dry and warm month of June, while it is only equal in the moist and cool month of September, showing that it was not merely expansion and diffusion of the vapour produced by evaporation that were in operation, but that some other cause was at work, which made the vapour accumulate on the mountain more than on the plain in June, but not in September.

In high latitudes the pressure of the vapour is the least in winter, and the most in summer. In Halle, in Prussian Saxony, for instance, it is 4·509 in January, and in July 11·626, almost three times the amount; and the same kind of difference between winter and summer is found in other northern parts. Generally it may be said to be the least in winter and in cold climates, and the most in summer and in warm climates.

When the dew-point, contiguous to the surface of the earth, is the nearest to the temperature, which is, say, at four or five in the morning, both the temperature and the dew-point are the lowest. From this time the temperature rises more than the dew-point, until the former reaches the highest point for the day. There is consequently in the lower part of the atmosphere an increasing difference occurring between the temperature and the dew-point up to the time of the highest temperature. But this does not take place in the same degree in the higher strata, as in them the dew-point progressively approximates to the temperature, until at some height the two become the same. In the forenoon, therefore, the lower air has its temperature removed progressively further from the dew-point, but when it ascends, it approaches the dew-point of the higher strata, until at last, at some height, condensation takes place and cloud is formed. When this occurs, the vapour that is in the air below the cloud, being partially relieved from incumbent vapour pressure, ascends more freely from the lower to the higher regions, where the cloud is forming. Thus it is the rise of temperature near the surface that increases evaporation and raises the dew-point, and the vapour produced by this evaporation expands and forces its way upwards by its own laws of expansion and diffusion. But in ascending it cools by expansion 1° for, say, every 500 yards, whilst it has to pass through the gaseous atmosphere, a medium which is made colder by its own law of cooling, 5° for every 500 yards of elevation; therefore, as the vapour ascends, it must at some height reach a temperature low enough to condense a part of it and form cloud. On the formation of the cloud taking place, a part of the vapour that is in the atmosphere is converted into globules of liquid (water), and the pressure of this condensed vapour on that immediately below it nearly ceases: for these globules of water, unlike the vapour from which they have been formed, do not rest upon or float in the *vapour* atmosphere alone, but also on the *gaseous* portion of the atmosphere, which, from its superior quantity and density, will sustain the greater part of the weight of this floating water. The lower vapour, relieved from a portion of that which previously pressed on it, expands upwards more rapidly, and ascends sometimes so freely as to prevent such an accumulation as shall further raise the dew-point, although evaporation continues active below. Indeed the pressure from above may be so far removed by cloud formation, and the ascent of the vapour be rendered so free and rapid as to lower the dew-point, as took place both at Zurich and on the coast of the Baltic. The processes which have been here described may be traced by attending at the same time to the dew-point and the heights of the ordinary and the wet-bulb thermometers. These are exhibited in the Plymouth registers and diagrams, presented to the British Association by Mr. S. Harris.

By reference to these, it may be seen that at Plymouth the difference between the dry and wet-bulb thermometers is, at five in the morning, say about 1° of Fahrenheit. This difference increases until one in the afternoon, when it is, say, 4°; evaporation must therefore have gone on with increasing activity during this time; and at three o'clock, that is two hours after the time of highest temperature, the difference between the two thermometers is greater than it was at eleven o'clock, two hours before the highest temperature! Evaporation must therefore have been more energetic, and must have continued to throw into the atmosphere more vapour from eleven to three than it had done four hours earlier! Now, if increase of vapour pressure always accompanied increase of vapour, the increase of pressure at Plymouth must have continued up to three o'clock! If however we look at the curve or line of the dew-point, which represents vapour pressure in the diagram, we find that it did not rise after eleven o'clock, but continued stationary from that hour until 4 P.M.! It is therefore apparent, that at Plymouth the quantity of vapour which by evaporation passed into the atmosphere in the middle of the day, to add to the general atmospheric pressure, in some form, was not indicated by the dew-point. And analogy authorises us to infer, that in other parts of the world, the state of the dew-point during the same portion of the day does not express the quantity of vapour that has passed into the atmosphere, and which must have added to its general pressure on the barometer.

In the Toronto registers, reported to the British Association at York in 1844 by Col. Sabine*, the state of the wet-bulb thermometer is not given. But we may assume that if it had been given, it would have shown the same features as those we have in the Plymouth registers and diagrams. In this report it is, however, stated that Mr. Caldecott has transmitted to England five years of hourly observations with the wet and dry-bulb thermometers at Trevandrum, near Cape Comorin, where a large quantity of vapour generally exists in the atmosphere. It appears from these accounts that the minimum and maximum pressures of the atmospheric vapour are there found to occur within three hours of each other,—the minimum coinciding with the coldest hour, 6 A.M., and the maximum occurring so early as at nine in the forenoon! Now, it is very desirable that it should be ascertained whether evaporation did or did not go on freely from the wet-bulb thermometer from six in the morning, not only until nine in the morning, but until two in the afternoon, the time of the highest temperature. Although the dew-point ceased to rise at nine, it is to be presumed, reasoning from analogy, that energetic evaporation continued through the middle of the day, and it probably was (as at Plymouth) more active between nine and two in the day, than it had been in any part of the time between six and nine in the morning. And the vapour which was thus produced at Trevandrum between

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* This report was inserted in the February Number of this Journal for the present year.—ED.

nine and two, or still later in the day, may have ascended and formed cloud, which cloud must have added to the general weight of the atmosphere. Had we accounts of the state of the wet and dry-bulb thermometers, and of the dew-points at different heights, there is little room to doubt that we might trace the ascent of the vapour at Trevandrum until we found it collected and floating in the atmosphere as a cloud, and in that form adding to the general weight of the atmosphere.

Colonel Sabine says that the maximum of vapour pressure occurring at Trevandrum at 9 A.M. may be a consequence of the sea-breeze blowing at that time. I have however shown that the daily sea-breeze is itself produced by the diurnal cloud formation; the sea-breeze is only another effect arising from the same cause. The sea-breeze blows towards the part, because the atmosphere has there been made lighter than in adjoining parts by the heating power of condensing vapour. The wind too that comes from the sea, particularly in the fine season, when the diurnal disturbance of the barometer is the greatest, comes more fully loaded with vapour after nine o'clock than was the air over the land before that time, and ought to increase the vapour pressure after that hour, instead of stopping the increase. If all the vapour that arose had to come from the same land surface of the locality, it might be supposed that evaporation could not continue to supply an adequate quantity to raise the dew-point after nine; but when the sea-breeze sets in, a current of air comes from an extensive sea surface, and brings with it the vapour which had been evaporated from that surface, not only up to nine o'clock, but until ten, twelve, or two o'clock, or still later: the tendency of the sea-breeze is therefore not to reduce, but to increase the supply of vapour. It may also be remarked, that whilst the maximum of vapour pressure is said to occur at Trevandrum at nine o'clock, the sea-breeze does not set in at Bombay until about eleven or half-past eleven. Supposing both these places affected alike by the sea-breeze, the cause of the stoppage of increase of vapour pressure, whatever that cause may be, must have been in operation two hours before the sea-breeze commenced blowing.

Formation of cloud is a cause sufficiently powerful in its operation to prevent the dew-point rising at Trevandrum after 9 A.M., as the vapour produced after that hour may be equal only to that which is consumed in cloud formation; and we are authorised to conclude that it is to that formation we are to attribute the stoppage of the dew-point at Plymouth at eleven, and at Trevandrum at nine o'clock, instead of having it rising with the temperature during the hottest portion of the day in both places. And in the more northern or drier climates, if we do not always trace the same stoppage, it is to be attributed to the absence of daily cloud formation. In a very dry and cold climate there is not in the course of the day sufficient water evaporated to produce a daily thick cloud, and therefore small vapour pressure goes on increasing with the temperature up to the hottest period. Under these circumstances, the vapour pressure, when exhibited in a diagram, forms a regular curve, having one rise and one fall in the twenty-four hours; but where much vapour exists, and much more is produced daily, the dew-point does not at all times indicate the pressure which results from evaporation, because the rise of the dew-point is stopped at certain periods, not by a cessation of the production of vapour, but through its ascent in the atmosphere and conversion into a floating cloud. Boiling water in the open air does not rise above 212°, yet heat continues to pass into it from the fire that is under the water. The reason that the temperature of the water does not rise higher is, that as much heat passes from the water into the air as from the fire into the water. In like manner, evaporation of water may continue to throw vapour into the air without the quantity in the air increasing, because condensation may convert vapour into water as fast as evaporation furnishes it. But neither the fire nor the vapour is annihilated,—the fire passes into the atmosphere and the vapour becomes cloud, and we may trace both of them in their new state of existence, and mark the effects they produce.

Taking the period of a year, in all places the average daily march of the temperature shows a single rise from about six in the morning till one or two in the day; and evaporation, as shown by the wet-bulb thermometer, increases with the rise of temperature. If the whole weight of the vapour thus produced were to be registered and exhibited in the form of a curve, that curve would be the same in form as the curve of temperature, having one rise and one fall. But in the actual curve or line of the dew-point there is frequently found to be a fall where there should be a rise. At Zurich and near the Baltic the departure from the regular curve is considerable; in Plymouth the line is level from eleven to four; and in Trevandrum, if a curve were formed, the line would cease to rise at nine o'clock, five hours before the hottest period! At Trevandrum the minimum and maximum of the dew-point occurred within three hours, whilst on the Faulhorn they were nine hours asunder; at Zurich, in June, they were sixteen hours asunder; and in other parts similar anomalies occur. These irregularities may be accounted for on the supposition that condensation of vapour produces them, because that process is very irregular in its action; but if this supposition is admitted to be true, it will follow that the dew-point is not a correct measure of the daily addition that is made to the weight of the atmosphere in the middle of the day by the vapour that has been thrown into it, and therefore it does not present the means of ascertaining the separate gaseous pressure. For the same operation that keeps down the dew-point in the middle of the day, creates cloud that floats in and rests upon the whole mass of the atmosphere; and the gaseous portion of that atmosphere must then press on the surface of the earth, not only with its own weight, but with the additional weight of nearly the whole of the cloud that is then floating in it. And if the curve of gaseous pressure, as commonly given, does not show a rise resulting from this additional pressure, it is because the whole atmosphere is at the same time made lighter by the heat which has been liberated by condensation of vapour.

LXIX. *Account of a remarkable difference between the Rays of Incandescent Lime and those emitted by an Electric Spark. By* JOHN W. DRAPER, *M.D., Professor of Chemistry in the University of New York*[*].

SOME years ago M. Becquerel discovered that the rays of an electric spark, if they were transmitted through a screen of glass, could not excite the phosphorescence of sulphuret of lime.

To make this experiment, wash a metallic plate over with gum-water. Dust upon it, from a fine sieve, a quantity of Canton's phosphorus (oyster-shells calcined with sulphur), and allow the plate to dry. An uniform surface is thus obtained, suitable for these purposes. Place before that surface a piece of glass and a piece of polished quartz, and discharge a Leyden phial a few inches off, so that the rays of its spark may fall on the plate. It will be found that under the quartz the phosphorus will shine as much as on the spaces that have not been covered; but under the glass it will remain almost entirely dark.

Last winter I observed the curious fact, that when this experiment is made with a piece of lime incandescing on a stream of oxygen, directed through the flame of a spirit-lamp, the glass, so far from being unable to transmit the rays, appears to be as transparent to them as quartz or atmospheric air.

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[*] Communicated by the Author.

A screen of glass is opake to the phosphoric rays of an electric spark, but is quite transparent to those of incandescent lime.

It might be supposed that the very brief duration of an electric spark has something to do with the phænomenon; but the voltaic arc passing between charcoal points gives the same results. I caused its rays to impinge on a plate for thirty seconds, and observed the obstructing effect of glass in a very satisfactory manner. That a certain portion of the rays does pass through, may be shown by continuing the light for a few minutes, when the phosphorus will begin to shine under the glass.

I came to the conclusion, also, that the transient duration of the light had nothing to do with the phænomenon, because the lime light occasions phosphorescence in glass in the space of a single second; but in that time the rays from a voltaic arc could not traverse a piece of glass so as to produce a sensible effect, the phosphorus beneath it appearing quite dark; and yet this light is incomparably brighter than the lime light.

The blue light which is emitted when a platina wire, in connexion with one pole of the voltaic battery, is brought down upon some mercury in connexion with the other, and the green light which is obtained when two copper wires are the medium of discharge, appear to produce the same effect as charcoal points.

It is therefore neither the colour nor the duration of the light that determines this result. It seems to depend on a peculiarity of the electric discharge.

Some time ago I determined the refrangibility of the rays of an electric spark which excite phosphorescence in sulphuret of lime; they are found at the violet extremity of the spectrum. I have made attempts to ascertain the position of the active rays of incandescent lime. They cannot pass through the blue solution of ammonio-sulphate of copper; but through the red solution of sulphocyanide of iron, and also a strong solution of bichromate of potash, they pass; in the latter case almost as copiously as through atmospheric air.

The phosphoric rays of an electric spark are in the violet space, but those of incandescent lime are at the other extremity of the spectrum.

An Argand lamp, when made to burn very brightly, emits phosphoric rays which traverse glass. As has been proved long ago, the sun rays possess the same property.

Thus, therefore, the rays of incandescent lime, of an oil lamp, and of the sun, can excite phosphorescence through glass; and differ from those of an electric spark, or galvanic discharge, in which that peculiarity is deficient.

I have also remarked some curious cases of spectral appearances,—they are analogous to those instances to which I first drew public attention in 1840, and which, at a later date, M. Moser brought before the British Association. They are interesting, as affording an ocular proof of "secondary radiation." The following experiments may serve as illustrations:—

Place a key, or any other opake object, before a sensitive phosphorescent surface, and having made that surface glow intensely, by a galvanic discharge, between charcoal points, continued for two or three minutes, on removing the key an image of it will of course be seen. This image in a short time will disappear. Then shut the plate in a dark place, where no light can have access to it in the daytime. If in a day or two the surface be carefully inspected, in the dark, no trace of anything will be visible upon it; but if it be laid on a piece of warm iron, a spectral image of the key is suddenly evolved.

It is still more curious, that a number of these latent images may co-exist on the same surface. Provide a phosphorescent surface, on which the latent image of a key, impressed a day or two before by the galvanic discharge, is known to exist. Take some other object, as a metal ring, and setting it before the surface, discharge at a short distance a Leyden jar. The phosphorus shines all over, save on those portions shaded by the ring; it exhibits therefore an image of that body. This image soon fades away, and totally disappears. Set the plate now upon a piece of warm iron; it soon begins to glow, and the image of the ring is first reproduced; and as it declines away, the spectral form of the key gradually unfolds itself, and after a time it totally vanishes.

A series of spectral images may thus exist together on a phosphorescent surface, and after remaining there latent for a length of time, they will come forth in their proper order on raising the temperature of the surface.

The idea that phosphorescence is merely the light of electric discharge from particle to particle seems to me wholly incompatible with such results.

LXX. *On the Evaluation of the Sums of Neutral and Periodic Series.* By J. R. Young, *Professor of Mathematics in Belfast College.*

[Continued from p. 366.]

METHODS for the evaluation of what have improperly been called neutral series—those series namely which limit the convergent cases of the general forms considered above—were proposed by Leibnitz and Daniel Bernoulli: these methods have been the subject of a good deal of controversy, which, as they are really true, would, no doubt, have been spared if they had been based upon firm mathematical principles. The considerations offered in the preceding part of this paper will conduct to a very easy and satisfactory proof of Bernoulli's rule.

Let the terms of the limiting series in question be represented by t_1, t_2, t_3, &c. These are such, that if we take the first term, the sum of the first two, the sum of the first three, and so on, we shall find that, after a certain term, the former sums will recur; so that the several results always return in a certain definite cycle or period. Suppose this period of results to be

$$s_1, s_2, s_3, \ldots \ldots s_m,$$

without stipulating anything as to the order in which these m values succeed one another: let the sum of n terms of the series be represented by S_n, and put

$$\begin{aligned}
&S_n = s_1,\\
&S_n + t_{n+1} = s_2,\\
&S_n + t_{n+1} + t_{n+2} = s_3,\\
&\vdots\\
&S_n + t_{n+1} + t_{n+2} + \ldots . t_{n+m-1} = s_m.
\end{aligned}$$

Now whichever of the individual results, in the entire period of results, s_1 or s_2, &c. be considered to represent, it is plain that n may be chosen so as to render each of these equations untrue; but notwithstanding this, it is equally plain that the sum of them all will be perfectly accurate, whatever be n; that is,

$$mS_n + (m-1)t_{n+1} + (m-2)t_{n+2} + \ldots t_{n+m-1} = s_1 + s_2 + s_3 + \ldots s_m,$$

$$\therefore S_n + \frac{m-1}{m}t_{n+1} + \frac{m-2}{m}t_{n+2} + \ldots \frac{t_{n+m-1}}{m} = \frac{s_1 + s_2 + s_3 + \ldots s_m}{m},$$

a result which shows that whatever value we give to n, the left-hand member of this equation is a constant quantity. But if the series we are now discussing be the limiting case of a general converging series proceeding according to the ascending powers of x, then, as already shown in the preceding part of this paper, when n is infinite, t_{n+1}, t_{n+2}, &c. are each zero. Consequently

$$S_\infty = \frac{s_1 + s_2 + s_3 + \ldots s_m}{m},$$

which is Bernoulli's rule.

This rule cannot, as usually supposed, apply to series that are strictly neutral; that is, not connected with more general forms by the principle of continuity: nor can it apply even where such connexion exists, unless, in virtue of that connexion, the terms t_{n+1}, t_{n+2}, &c. become zero. It cannot apply therefore to the series

$$\cos x - \cos 2x + \cos 3x - \cos 4x + \&c.$$

when it passes into

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

nor to

$$a + b\cos x - c\cos 2x + a\cos 3x + b\cos 4x - c\cos 5x + \&c.,$$

in which $c = a + b$, when it passes into

$$a + b - c + a + b - c + \&c.;$$

and this consideration alone is sufficient to show the fallacy of asserting the general value of the first of these series to be $\frac{1}{2}$.

Let us now examine the series

$$\frac{1}{2} + A\cos\theta + A^2\cos 2\theta + A^3\cos 3\theta + \ldots A^n\cos n\theta,$$

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

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

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

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

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

It may be proper to remark here that [1.] is affirmed to be the true value of the series in the case of $A = 1$, solely in consequence of that case coming under the control of the general law of continuity which governs all the cases furnished by the continuous variation of A, $\cos\theta$ being fixed. It is not allowable simultaneously with this continuous variation to introduce considerations connected with the variation of $\cos\theta$;

for the quantities themselves, and all their changes, are essentially independent of one another. Each value of $\cos\theta$, as A passes through its changes, gives rise to a distinct series of cases; and we are not to assume the law of continuity in passing from one of these distinct series to another, through the variation of $\cos\theta$: if we wish to consider the continuous results of this latter variation, A must be regarded as fixed.

I make these observations in case it might be imagined that there was anything anomalous in the circumstance that [1.] gives 0 for the limiting value of the series in every case of $\cos\theta$, except the single case of $\cos\theta = 1$, when the value is

$$\frac{1-A^2}{2(1-A)^2} = \frac{1+A}{2(1-A)} = \infty, \text{ when } A = 1.$$

It thus, I think, appears that Mr. De Morgan is not in error, as Mr. Moon supposes, in affirming [1.] to be the limit of the proposed series when A = 1. The real error, so frequently committed in analysis, consists in confounding

$$\frac{1}{2} + \cos\theta + \cos 2\theta + \cos 3\theta + \&c. \textit{ ad infinitum}$$

with the limit of

$$\frac{1}{2} + A\cos\theta + A^2\cos 2\theta + A^3\cos 3\theta + \&c. \textit{ ad infinitum},$$

and calling [1.], when A = 1, the sum of the former; and this belongs to the same class of errors as those discussed in the preceding part of this communication. I reserve a more detailed examination of the influence of them for a future occasion.

Poisson, in the 12th volume of the *Journal de l'Ecole Polytechnique*, a source which I have not as yet been able to consult, has, I believe, entered upon an examination of the series which form the subject of the present paper; and I think he connects the extreme case of each with the continuous series of cases as I have here done. But, from all that I can collect respecting his memoir, he falls into the errors here noticed, and which it has been my principal purpose to point out and correct, by showing the essential distinction between an isolated series and a series connected to a chain of others by the bond of continuity. The symbol $\frac{1}{\infty}$ prevents the condition implied in this connexion from being obliterated in the extreme case; and I think that this symbol, together with the symbol $0\sqrt{-1}$, employed for an analogous purpose in my former paper, in the August Number of this Journal, might, with propriety, be called *symbols of continuity*. By disregarding the influence of these, in extreme cases, we disconnect those cases from the others, remove from them a condition which they must otherwise have obeyed, and regard them as isolated and independent. For example, the isolated case of the general series discussed above, which arises from putting $A = 1$, is

$$\frac{1}{2} + \cos\theta + \cos 2\theta + \cos 3\theta + \dots \cos n\theta,$$

of which the correct sum is the expression [2.] above, for [1.] vanishes, that is, the sum is

$$\frac{\cos(n+1)\theta - \cos n\theta}{2(1-\cos\theta)};$$

and it is the error committed in confounding this isolated and independent case with that which is really the limit of the general series, and therefore under the control of the law of continuity:—it is this error (and errors such as this) that has led to the additional error of supposing $\sin\infty = 0$ and $\cos\infty = 0$, since it was found that the sums of the two series, supposed to be identical, would really differ in the case of $n = \infty$, unless the sine and cosine of an infinite arc were made equal to zero.

In some instances $\cos\infty$ is assumed to be 1 instead of 0, an assumption which, like that just noticed, seems to be necessary in order to prevent contradiction in the results of certain definite integrations; which integrations however will be found to involve the same error as that noticed above in reference to series; the error, namely, of bringing an isolated expression under the dominion of an arbitrary law, and then reasoning upon it in reference to that law. Although, as stated above, it is not my present intention to consider at any length the influence of this error upon the existing theory of definite integrals, yet it may be proper to adduce an example of it. It is easily proved that

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

from which it certainly follows, though the inference is denied by Mr. Moon, that in the limit, when $a = 0$, the true values of these integrals are 0 and 1. Yet it is not true, as Poisson and his followers affirm, that, in virtue of this,

$$\int_0^\infty \cos x\,dx = 0, \quad \int_0^\infty \sin x\,dx = 1;$$

inasmuch as *these* and the *limits* of the foregoing general forms are very different things. I ventured a conjecture above, that Poisson countenances this important error in his memoirs on series and definite integrals in the Journal of the Polytechnic School. I am confirmed in this conjecture from the manner in which that distinguished analyst treats cases of the kind here discussed in his *Théorie de la Chaleur*: thus, in reference to the isolated case of the general series above, when $A = 1$, he says, "Elle est de l'espèce des séries périodiques qui ne sont ni convergentes ni divergentes, mais qu'on peut néanmoins employer en les considérant comme les limites de séries convergentes, c'est-à-dire en multipliant leurs termes par les puissances ascendantes d'une quantité infiniment peu différente de l'unité."—*Théorie de la Chaleur*, p. 119. And again, "L'intégrale d'une quantité périodique qui s'étend à l'infini, doit toujours être considérée comme la limite d'une autre intégral dont les élémens décroissent à mesure que la variable augmente, et sont nuls quand la variable est infinie; observation semblable à celle que nous avons déjà fait rélativement aux séries infinies de quantités périodiques."—*Ib.*, p. 207. And this is the erroneous principle to which the mistakes animadverted upon in Mr. Moon's paper are all to be traced, and which will be found to affect the accuracy of many other received results in the doctrine of series and of definite integrals.

It may perhaps seem that the principle here deprecated, and which consists in bringing an isolated and independent case under the control of a law which governs a continuous series of cases, with the view of effecting a particular purpose,—it may perhaps seem that this principle is virtually sanctioned by myself at p. 363 of the present paper, where I give to the infinite exponent in the expression for S the peculiar form $k.\infty'$ —to the exclusion of other forms for infinity that may appear to be equally admissible—merely, it may seem, to effect a particular purpose. But it is not so. Any form for the exponent in question, other than that I have given to it, would be erroneous; as I shall show in a subsequent number of this Journal, if I be indulged with a brief space for that purpose.

LXXI. *On the Application of Voltaic Ignition to Lighting Mines.* *By* W. R. Grove, *Esq., M.A., F.R.S., Professor of Experimental Philosophy in the London Institution.*

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

Dear Sir,

In the *Comptes Rendus* of the Paris Academy of Sciences for the 1st and 15th of September last, are communications by M. Boussingault and M. De la Rive, on the employment of the voltaic disruptive discharge for the illumination of mines.

M. Boussingault is inclined to believe that some of the accidents in mines have occurred from draughts or currents of inflammable gas, and not from the carelessness of the workmen in the use of the safety-lamp, to which they are generally attributed; he considers that the voltaic arc, being independent of atmospheric air or other supporter of combustion, in the usual sense of the word, might be rendered practically available. M. De la Rive states that he has been occupied with the subject, and proposes a cylinder of close-textured charcoal, similar to that of Bunsen, with a metallic ring or plate above it; the carbon being rendered the positive terminal of a voltaic pile, the particles transferred from it to the disc fall down again by their own gravity, and a tolerably constant light is obtained; the vessel containing the electrodes is hermetically sealed, and the oxygen being soon exhausted by the ignited charcoal, the ignition proceeds in the residual nitrogen. M. De la Rive appears, however, to have met with but partial success, and says there are still many difficulties to contend with*.

Four or five years ago, soon after publishing the nitric acid battery, I was naturally struck by the facility and constancy with which the voltaic arc could be obtained by that combination, as compared with any previous one, and made several attempts to reduce it to a practical form for the purposes of illumination, but my success was very limited. By attending to certain precautions, which I will not stop to describe, I could occasionally keep up a steady voltaic light in attenuated nitrogen for four or five hours, but it was never sure; from some unseen imperfection in the charcoal, or other cause, it would become suddenly extinct; the glass also in which it was ignited became gradually dimmed by a deposition of condensed carbon vapour; it was costly, from the number of series, and consequently of equivalents of zinc and acid consumed; too bulky for portable purposes, and from the intensity of the heat, unless the recipient was very large, the collar of leathers and joints, into which the wires were sealed or cemented, were destroyed; and when ground plates were employed, the grease was liquefied. M. De la Rive does not state his method of hermetically sealing the vessel he employs; this I found one of the most difficult parts of the process. Not being able satisfactorily to overcome these difficulties, I abandoned it for the time, and made some experiments on another method of voltaic illumination, which appeared to me more

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* A translation of M. De la Rive's paper will be found at p. 406 of the present volume.—Ed.

applicable to lighting mines; their publication was postponed, and I had nearly forgotten them, until reminded by the papers above-mentioned.

I substituted the voltaic ignition of a platina wire for the disruptive discharge. Any one who has seen the common lecture-table experiment of igniting a platina wire by the voltaic current nearly to the point of fusion, will have no doubt of the brilliancy of the light emitted; although inferior to that of the voltaic arc, yet it is too intense for the naked eye to support, and amply sufficient for the miner to work by. My plan was then to ignite a coil of platinum wire as near to the point of fusion as was practicable, in a closed vessel of atmospheric air, or other gas, and the following was one of the apparatus which I used for this purpose, and by the light of which I have experimented and read for hours:—A coil of platinum wire is attached to two copper wires, the lower parts of which, or those most distant from the platinum, are well-varnished; these are fixed erect in a glass of distilled water, and another cylindrical glass closed at the upper end is inverted over them, so that its open mouth rests on the bottom of the former glass; the projecting ends of the copper wires are connected with a voltaic battery (two or three pairs of the nitric acid combination), and the ignited wire now gives a steady light, which continues without any alteration or inconvenience as long as the battery continues constant, the length of time being of course dependent upon the quantity of the electrolyte in the battery cells. Instead of making the wires pass through water, they may be fixed to metallic caps well-luted to the necks of a glass globe.

The spirals of the helix should be as nearly approximated as possible, as each aids by its heat that of its neighbour, or rather diminishes the cooling effect of the gaseous atmosphere; the wire should not be too fine, as it would not then become fully ignited; nor too large, as it would not offer sufficient resistance, and would consume too rapidly the battery constituents; for the same reason, *i. e.* increased resistance, it should be as long as the battery is capable of igniting to a full incandescence.

The helix form offers the advantages, that the cooling effect being lessened, a much longer wire can be ignited by the same battery; by this increased length of wire, the battery fuel is œconomised, while a greater light is afforded; by the increased heat, the resistance is still further increased, and the consumption still further diminished, so that, contrary to the usual result, the increment of consumption decreases with the exaltation of effect produced. The very necessity of inclosing the coil in a glass recipient also augments the heat, the light, and the resistance; if I remember rightly, Mr. Faraday first proposed inclosing wire in a tube for the purpose of being able to ignite a longer portion of it. Lastly, only two or three cells are required (one indeed might be sometimes sufficient), and the whole apparatus thus becomes portable and œconomical. The light is perfectly constant, subject to no fluctuation or interruption, and the heat is not so excessive as to destroy the apparatus.

As the effect of different gases on radiant heat is an important element in the practical application of the above, I had commenced some experiments on this subject, and the following I find in my note-book as the effect of four different gases. A voltameter was interposed in the circuit in order to furnish a better test of the amount of ignition than the eye, as, according to the position of Davy, that a wire becomes a worse conductor in proportion to its increase of temperature, the amount of gas in the voltameter should be, as indeed in these experiments it turned out to be, in inverse proportion to the degree of ignition. As a further test, the increased volume of the gas by expansion was noted, though the apparatus was not constructed for showing this increase with delicate accuracy.

Platina wire ignited by a given constant voltaic battery in	Effects to the eye.	Voltameter gave at the rate of one cub. in. in	Expansion of volume.
Hydrogen	Not visible even in the dark ...	″ 19	35 to 43
Carbonic acid...	Cherry-red by daylight	21·5	35 ... 43
Oxygen	Incandescent by daylight	23·5	35 ... 45
Nitrogen.........	Same	24	35 ... 45
Atmospheric air	Same	24	35 ... 45

I had intended to have carried these experiments further with other gases*, and also with condensed and rarefied air, but was interrupted; and as it may be some time before I may be able to renew them, I think I cannot do better than submit these experiments, with your permission, to the readers

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* I have some doubt whether the different gases do not exercise a specific action on the ignited wire, somewhat in the nature of catalysis; if a wire be brought by the voltaic current to a white heat in atmospheric air, and a vessel of hydrogen inverted over it, the light is as suddenly extinguished as the flame of a candle would be.

of the Philosophical Magazine, while the attention of scientific men is directed to this subject; actual practice can alone test their efficacy.

I remain, dear Sir, yours truly,
W. R. GROVE.

LXXII. *Reply to* Sir David Brewster's "*Observations on the Discovery of the Composition of Water.*" *By* A BRITISH QUARTERLY REVIEWER.

To Richard Taylor, Esq.

SIR,

WILL you allow us space enough in your excellent Journal to reply to a strongly-worded complaint by Sir David Brewster, which appeared in your Number for September last, against a passage concerning himself in the British Quarterly Review for August 1845? The passage offensive to Sir David is quoted below. It occurs in a brief discussion of the relative merits of Watt and Cavendish as discoverers of the composition of water, in an article on "Lord Brougham's Lives of Men of Letters and Science who flourished in the reign of George III."

"Sir David Brewster afterwards took up the subject (Edinburgh Review, No. 142), and endeavoured to mediate between the contending parties, but to little purpose. For he chose to consider it a question of national honour, involving the rival claims of Cavendish the Englishman and Watt the Scotchman; and whilst he was willing to divide the merit between them, assigned to his countryman the lion's share."

Sir David declares, in reference to the paragraph we have quoted, that it contains "very unfounded and unjust assertions;" that our object was to injure his character and wound his feelings; that it manifestly was "the reviewer's desire to infuse into his statements a bitter personality;" and he further speaks of our having occasioned him "the annoyance of rebutting the calumnies of the British Quarterly reviewer."

The special charges which Sir David Brewster brings against us we shall allude to immediately. In reference to the general one, of our remarks having been pervaded by an *animus* against him, we beg to assure Sir David that we had not, in the passage he complains of, the remotest intention of injuring his character or wounding his feelings: that we were actuated by no animosity towards him, and were guiltless of the slightest desire to vex or annoy him by aspersion, calumny, or insinuation. Sir David Brewster we know only as the most distinguished original observer in natural philosophy which Great Britain possesses; and he is a philosopher whom we delight to honour. As such we freely subjected his opinions to criticism, as in similar circumstances we should feel ourselves at liberty to do again; but we shall have little difficulty in satisfying Sir David himself, that in so doing, we did not in the least overstep the bounds which are conceded to impartial reviewers.

The special charges brought against us are,—

1st. That we declare Sir David Brewster to have been actuated by unworthy motives in deciding between the claims of Watt and Cavendish as discoverers of the composition of water.

2ndly. That we represent him as having preferred Watt where in truth he preferred Cavendish.

The first charge is contained in the following statement: "The assertions of the British Quarterly reviewer are equally offensive with the language which conveys them. He charges me, by name, with having decided a great scientific question, interesting to the whole civilized world, *from motives of national feeling*,—with sacrificing by a temporising verdict *Cavendish the Englishman* to *Watt the Scotchman*,—and, under the pretence of *dividing the merit*, with *assigning to my countryman the lion's share*." The italics are Sir David Brewster's. In reply, we must be allowed to say, that we brought no such charges against Sir David as he accuses us of having made. Our statement was to the effect, that his attempt to mediate between the partizans of Watt and Cavendish was to a great extent unsuccessful, because he made a national—a Scotch-and-English question, of the dispute as to the discovery of the composition of water; whilst he assigned the chief merit to his countryman Watt. That he made a national question of it, Sir David Brewster will not deny. Here are his own words. We quote from his article on Watt, in the Edinburgh Review, No. 142, p. 496:—"We cannot conclude this part of our subject without adverting to the discussion of it which emanated from the chair of the British Association at Birmingham. We regret that such a meeting should have become the arena of a controversy involving feelings so deeply personal, *as well as national*." The italics are our own. Here Sir David Brewster surely declares that he considers the question a "national" one. In what sense he considers it such will appear from a further statement: "Scotland will not soon forget the attempt which was lately made, by one of the Council of the British Association, to rob our illustrious countryman Dr.

Black of his grand discovery of latent heat; and it is an unfortunate circumstance that the President should have been specially requested by the same Council to draw up a discourse, in which *another of our countrymen* should have been deprived of an honour so justly due to him." That we were not singular in drawing the conclusion we published, will appear from the fact that Mr. Harcourt speaks of the writer of the passages we have quoted as "a northern critic who views this question of individual justice as one of national honour*." We are at a loss, indeed, to understand how any one who reads the passages we have selected, can differ from us in opinion, and are surprised that Sir David Brewster should judge us so harshly, because we have said that "he chose to consider it a national question," which it seems to us he certainly did.

Whether Sir David Brewster acted judiciously in thus appealing to the feelings of our scientific men as Englishmen and Scotchmen, is a matter of opinion. We think that he acted unwisely in so doing, the more so, that the great advocate of Cavendish, viz. Harcourt, was an Englishman, and many members of the Council of the British Association were Cavendish's countrymen also, and could not but find something unpleasant in the passages which we have quoted. We believe, accordingly, as we have implied in the paragraph to which Sir David Brewster objects, that he greatly injured the effect of his statements by his allusions to nationality. Sir David, however, need not attribute to us, on that account, any concealed purpose of insinuating against him a charge of dishonesty, or partiality, in adjudicating between Watt and Cavendish. We can only emphatically say, that we had no intention of attributing any such motive to him.

Sir David Brewster's reply to the second point is as follows:—"In place of dividing the merit of the discovery between the Englishman and the Scotchman, and giving the lion's share to my countryman, I have given the whole merit of the discovery to Cavendish the Englishman, and have reserved only for Watt the Scotchman the merit of the previous hypothesis." In reference to this, we have only to say, that Sir David, in rebutting our charge, takes for granted that our estimate of "the lion's share" is the same as his own; in other words, not to ring the changes on that phrase, he conceives that we are at one with him as to what the merits of the question between Watt and Cavendish are, whilst we have totally misstated his views regarding them. Accordingly, he is at a loss

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* Report of the Ninth Meeting of the British Association. Postscript to the President's Address, p. 23.

to decide whether we never read his article, and are therefore ignorant of his opinions; or having read it, did not understand it; or both read and understood it, but purposely misrepresented it. He inclines to the last view. We beg to assure Sir David Brewster that we spent several days in studying his article, and made ourselves master of his views. We fear, however, that he laid down our review too speedily on coming to the passage which offended him. Whether so or not, if he will now do us the justice to peruse it, he will find that, holding the views which we distinctly stated we did, we could not but consider Watt as having received the "lion's share" in Sir David Brewster's division of the merit between him and Cavendish. In so doing, however, we did not call in question the perfect honesty and sincerity of Sir David's opinions.

According to Sir David Brewster, "The great merits of these two great men are fortunately not in collision. Mr. Watt will for ever enjoy the honour of that singular sagacity which presented to him the *hypothesis* of the composition of water; and Cavendish will never lose *the glory which belongs to him of having given that hypothesis, whether he was cognizant of it or not, the force and stability of truth.*"—*Edinburgh Review,* No. 142, p. 496.

According to our view, and we are not singular in holding it, this mode of stating the question robs Cavendish of half, and that the best half, of his merit. The conviction that water is a compound of hydrogen and oxygen, we believe to have been an inference on the part both of Cavendish and of Watt. The former inferred it from his own experiments, the latter from Priestley's repetition of those of Cavendish. For Cavendish we claim the merit both of making the necessary experiments and of understanding and explaining their meaning. According to Sir David Brewster, Watt showed what the meaning of the experiments would be, and Cavendish made them. To us, the man who explains the nature of a phænomenon appears much more praiseworthy than he who merely observes it. We accordingly think that Watt gets the lion's share of merit, if he be counted the interpreter, whilst Cavendish is reputed only the observer of the phænomena in question. We did not, however, cast any moral censure on Sir David Brewster for holding his view, nor did we misrepresent his opinions. He accuses us of mistaking his estimation of merit, taking for granted that we accept *his* standard of excellence, whereas, like all other critics, we were stating our view of it according to *our own* standard of merit; and what that was we stated distinctly.

In conclusion, we have only further to observe, that Sir David Brewster has found ground of offence in several phrases, which we beg to assure him were used most innocently, and contain no such covert insinuations as he seems to think are lurking within them. The passage which has given him so much offence was intended to express the following and no other truths:—"Sir David Brewster afterwards published on the subject, and tried to mediate between the contending parties, but ineffectually; for he discussed it as a question of national honour affecting the claims of Cavendish the Englishman and Watt the Scotchman, and in the adjudication of merit he placed his countryman highest."

We have not sought to reply to the charge of discourtesy preferred against us by Sir David Brewster, for naming him as the author of an unacknowledged Review article. We did it inconsiderately; but if we have satisfied Sir David, that it was with no unkindly intention we referred to him as the writer of a paper so notoriously and so evidently his, we trust he will forgive this transgression of

A British Quarterly Reviewer.

November 1, 1845.

LXXIII. *Remarks on* Mr. Williamson's *experiments regarding Ozone.* *By* Prof. C. F. Schœnbein *of Basle*[*].

It is rather amusing to see the variety of opinions which have been given out respecting the nature of the odoriferous matter disengaged by phosphorus in moist atmospheric air. To some, that principle is either nitrous or nitric acid according to others, it must be considered as phosphorous or hypophosphorous acid. M. De la Rive and Marignac think it oxygen modified by the electricity which they suppose to be disengaged during the chemical action of phosphorus upon atmospheric air: oxide of arsenic, or arsenious acid resulting from an arseniferous phosphorus, was once mentioned as likely to be ozone, and some went so far as to declare that substance to be nothing but organic matter changed by phosphorus into ozone. I myself was inclined for a time to consider ozone as a constituent part of nitrogen. Mr. Williamson[†] has lately augmented this list of opinions by one which has certainly the merit of being the most original of all;

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[*] Communicated by the Author.

[†] Mr. Williamson's paper will be found at p. 372 of the present volume.—Ed.

for if I understand it correctly, it broadly denies that any peculiar substance at all is formed during the action of phosphorus upon moist air. That gentleman asserts, at least, in distinct terms, that under the circumstances mentioned no matter is produced which bears the slightest relation to that disengaged at the positive electrode during the electrolysis of water. Mr. Williamson made two experiments, on the results of which he founds his opinion: in one of them he passed a current of air over a piece of phosphorus and conveyed it into the test-paste; in the other he followed the same method, with the difference, however, that the phosphorus employed by him had previously been brought into a state of minute mechanical division by the means of asbestus. In the first case the paste was coloured, but not in the second; and from those results he drew the inference that the production of colour was due to the action of phosphoric acid and free oxygen upon the iodide of potassium, and that no ozone was formed by phosphorus. Though this conclusion implied the most direct contradiction to my own statements, and those made by Marignac, Mr. Williamson did not think it worth his while to repeat the experiments which I have so circumstantially described in my little work On the Chemical Production of Ozone, and satisfy himself whether, by employing another method of experimenting, he might not obtain results a little different from those he had come to. In my opinion, common fairness required him to do so before he had so broadly negatived the correctness of what I myself and others had stated as well-ascertained facts. But whatever the reasons were which prevented him from repeating my experiments, I take the liberty to tell him most distinctly, that his conclusion, according to which the odoriferous principle disengaged by phosphorus from moist atmospheric air has nothing to do with the ozone developed at the positive pole during the electrolysis of water, is entirely unfounded; and I continue, notwithstanding his results, to maintain that the very same substance is produced in the action of phosphorus upon air which is disengaged either at the positive electrode during the electrolysis of water, or at the points out of which electricity happens to pass into the atmospheric air. My reasons for being so positive on this point are founded on the following facts:—

1. Chemical ozone (and I mean by that the odoriferous principle produced by phosphorus placed in moist atmospheric air) destroys, with great energy, all vegetable colouring matters at common temperatures.

2. Chemical ozone oxidizes at common temperatures most

metallic substances to the highest degree of which they are capable: even silver is so acted upon.

3. Chemical ozone transforms a series of metallic protoxides into peroxides, for instance, those of lead, manganese, cobalt, nickel, silver.

4. Chemical ozone, at common temperature, converts iodine into iodic acid, phosphorus into phosphoric acid, sulphurous acid into sulphuric acid, hyponitric acid into nitric acid.

5. Chemical ozone changes iodide of potassium into iodate of potash, a great number of metallic sulphurets into sulphates, for instance, sulphuret of lead into the sulphate.

6. Chemical ozone destroys instantaneously sulphuretted, seleniuretted, phosphoretted and ioduretted hydrogen.

7. Chemical ozone transforms the yellow prussiate of potash into the red one.

8. Chemical ozone, as such, is insoluble in water.

9. Chemical ozone when heated to a certain temperature is destroyed.

10. Chemical ozone possesses the power of polarizing negatively gold and platinum.

The odoriferous principle disengaged at the positive electrode during the electrolysis of water has, according to my experiments, exactly the same properties; and I have not yet been able to detect the slightest difference between chemical and voltaic ozone. Hence I conclude, of course, that these substances are identical. If Mr. Williamson will take the trouble of putting a piece of clean phosphorus into a balloon filled with common air and containing some water, in such a way that the phosphorus is partly immersed in the fluid and partly in contact with the ambient air, and leaving the whole to itself at a temperature of 70° to 80° F., after a few hours he will find in his balloon an atmosphere which will serve to convince him of the correctness of the statements above made. Mr. Williamson will allow me to tell him why the negative result of what he calls his decisive experiment proves nothing against the correctness of my statements; and indeed his very mode of experimenting could lead to no other than a negative result. If phosphorus in a state of rather minute mechanical division be shaken with voltaic ozone, the latter is instantaneously taken up or destroyed, just as if it had been treated with iron filings or any other readily oxidizable body. I need hardly state, that an atmosphere of chemical ozone does not in the least differ from one of voltaic ozone, *i. e.* is destroyed by being put in contact with phosphorus. Now Mr. Williamson making use in his decisive experiment of phosphorus exhibiting a very large surface, if compared to the volume of air passing at a time over that phosphorus, it is manifest that under those circumstances, ozone, immediately after its having been formed, must have been taken up again by the substance under whose influence it was produced. Ozone disappears also, only a little more slowly, if into a bottle containing ozonized air a common piece of phosphorus is placed, and that bottle happens to be closed air-tight. In that case ozone is destroyed and re-formed as long as there are free oxygen, water or aqueous vapour and phosphorus in contact with one another. The last trace of oxygen being transformed into ozone, and nothing left but nitrogen and ozone, the latter will soon be entirely absorbed by phosphorus, and the bottle contain nothing but nitrogen and the phosphatic acid.

One word or two more, and I shall have done. Mr. Williamson, who is a beginner in chemistry, will not, I am sure, take it ill, if I, being his senior, take the liberty of advising him to be on future occasions a little more circumspect and reserved before he broadly denies the correctness of statements of others,—of statements which have been the result of long, laborious, and I add, of conscientious researches; of statements, the truth or groundlessness of which can be so easily ascertained if there is no disinclination to do so.

Basle, Aug. 20th, 1845.

LXXIV. *On Atomic Volume and Specific Gravity.* *By* LYON PLAYFAIR, *Esq., Ph.D. and* J. P. JOULE, *Esq.*[*]

SECTION I.

THE discovery of Gay-Lussac, that gaseous bodies combine in equal or in multiple volumes, and that the resulting compounds stand in a similar simple relation to their constituents, is one of the most important discoveries ever made in physical science. Its utility has been diminished by its supposed inapplicability to liquid and solid bodies, as its own exactitude at different temperatures is entirely owing to the equal expansibility of the same volumes of different gases by equal increments of heat.

In its most simple form, therefore, it was *à priori* improbable that the law of Gay-Lussac should apply to the liquid and solid forms of matter. But, as the larger number of substances are either liquid or solid, and incapable of passing into

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

the gaseous state, even at very high temperatures, the importance of discovering the law which governs the volumes of these forms of matter, has long been recognised, and for some time past has much engaged the attention of philosophers.

The first chemist who drew attention to this subject was Dr. Thomson, who published a Table*, in the year 1831, of the specific volumes of certain of the metals, obtained by dividing their atomic weights by their specific gravities. In this table a remarkable coincidence of volume is observed in several of the metals most nearly allied in chemical characters.

More recently the subject has been examined in detail by Kopp, Schröder and Persoz, whose researches have thrown considerable light on this obscure department of physics.

Kopp† drew attention to the circumstance, that in many cases isomorphous bodies possess the same atomic volume, the law being correct when the isomorphism is strictly accurate, but approximating only when this is not the case. He admits also that perfect equality of the volume exists only at particular temperatures, on account of the unequal expansion of isomorphous crystals.

Schröder‡ made the interesting observation, that the remainder is the same when the primitive volume of the corresponding member of a series of analogous compounds is subtracted from them; thus AO, BO and CO leave a constant remainder when the known volumes of A, B and C are subtracted respectively from the known atomic volumes of the compounds.

Kopp§ confirms this discovery to a certain extent, believing, however, that the primitive volumes A, B and C must be assumed in certain classes of salts to be different when in combination with O from their volumes when isolated. He also announces the discovery‖ of a great regularity in the physical properties of analogous organic compounds, so much so, that the study of the physical characters of the compounds of one body enables us to predicate those of the corresponding compounds of another substance.

The discoveries of Schröder and Kopp, with regard to the atomic volumes of liquid and solid bodies, do not, except in a very few instances, indicate an approach to a simple multiple ratio of volumes, and are therefore only in a small de-

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* Chemistry of Inorganic Bodies, vol. i. p. 14.

† Poggendorff's *Annalen*, vol. xlvii.; and *Annalen der Chemie*, vol. xxxvi. p. 1.

‡ Poggendorff's *Annalen*, vol. l. p. 554.

§ *Ueber das Specifische Gewicht der Chemischen Verbindungen.* Frankfort, 1841.

‖ *Annalen der Chemie*, vol. xli. p. 79.

gree connected with the law of gaseous volume. We therefore thought it desirable to enter into a series of inquiries on this most important subject, and we have now the honour to lay before the Society the First Part of these researches.

Hitherto the inquiry has been principally confined to solid bodies, on the just ground that their diminished rate of expansion offers less difficulty to the discovery of the law regulating volumes. But there is an objection to the use of solids, which to a certain extent counterbalances this advantage, viz. that they do not present matter in a perfectly uniform condition, free from cohesion. On consideration, therefore, we were led to believe this objection to be so powerful, that we conceived it to be preferable, so to separate the particles of the body under examination, as to destroy their cohesion, without at the same time altering their chemical properties. Solution in water was the obvious means of effecting this purpose, according to the notions generally entertained of solution, and it was therefore resolved to experiment principally upon soluble bodies of well-known and defined constitution. At the same time, it was necessary to examine the relation of the solid volume to the volume of the body when in solution, so as to indicate the connection between the solid and the liquid atom.

The specific gravities of salts are little known, and even when recorded are described so differently by different observers, that it was necessary to determine the specific gravity in each of the cases upon which the experiments were instituted. Hitherto the volumes of solids had always been referred to an equal volume of water; in other words, the *solid* form of matter had been referred to its *liquid* form. This difference of conditions was no small impediment to the discovery of a law which might be modified for each form of matter*. By determining the volume of the substance in solution, we compared it in its liquid state to the liquid form of matter in which it was dissolved; and by contrasting the volume of the solids with each other, and also with their volume when ren-

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* Before leaving the notice of the labours of those who have preceded us in inquiring into the nature of specific gravity, we must not omit to notice the speculations of the ingenious Persoz, who (in vol. xl. of the *Ann. de Ch. et de Phys.*, p. 119) drew attention to the equality in volume of isomorphous bodies, and even of some which were not isomorphous. Persoz also believes that the volumes of all bodies are multiples of 56, or half the atomic weight of water; but this idea does not agree with recorded observations, and is directly contradicted by accurate estimations of specific gravities.—See the work of M. Persoz, *Introduction à l'Etude de la Chimie Moléculaire*, page 834 *et seq.*

dered liquid by water, we conceived that we might be placed in more favourable conditions for elucidating a law.

Bishop Watson* was the first chemist who endeavoured to estimate the increase of volume when salts dissolve in water; for, although both Gassendus† and the Abbé Nollet ‡ had written, and Ellis § had experimented upon the same subject, they had arrived at conclusions entirely erroneous, which were removed by Watson's more accurate experiments. Watson's apparatus was rude enough, being a matrass capable of holding 67 ounces of water, into which he projected 24 pennyweights of each of the salts upon which he experimented, and noted the rise in the neck of the matrass. He completely exploded, however, the idea that saline substances dissolve in water without increasing its bulk.

Between the time of Bishop Watson, whose investigations on this subject are most profound, when we consider the period at which he wrote, and that of Dalton, there were no labourers in this field to whom we need draw especial attention. In the year 1840, Dalton || made the interesting discovery, that sugar and certain salts on being dissolved in water increase its bulk only by the amount of water pre-existing in them. He generalized this observation by asserting that all hydrated salts dissolve in water, increasing its bulk merely by their amount of water of hydration, while anhydrous salts do not at all increase the bulk of the water in which they are dissolved.

But it must not be forgotten, that when Dalton published this paper, he was much enfeebled by illness, and on this account it does not derogate from the acuteness of the philosopher, that Mr. Holker was unable to confirm Dalton's results in repeating the experiments in 1843**. He did so, however, in the case of sulphate of magnesia, and approximatively in that of one or two other salts. As Mr. Holker's paper has not been published, we are unable to state his claims in the progress of this subject; but we believe that an attempt was made to show a multiple relation in the increments of *isomorphous* salts, although his experiments were conducted

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* Philosophical Transactions, 1770.

† *Gass. Phys.* lib. i. sect. 1. cap. 3.

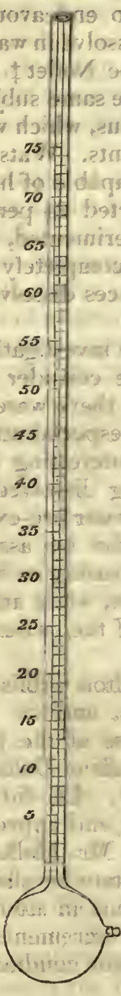
‡ *Leçons de Physique,* vol. iv.

§ Berlin Memoirs, 1750.

|| "On the quantity of Acids, Bases and Waters in Salts, and a new mode of measuring them," read to the Manchester Literary and Philosophical Society, 6th October 1840, and published as a pamphlet.

** Paper read to the Manchester Literary and Philosophical Society, but not published. [The paper here referred to will be found at p. 207 of the present volume of this Journal.—Ed.]

without reference to the density or temperature of the solution on which he operated.



Volumenometer for Salts in Solution.
One-half the size of the Instrument, used with 1000 grains of water.

In the experiments about to be described, the apparatus for estimating the volume of bodies when dissolved, consisted of a glass bulb, to which a stem was attached. The bulbs varied in capacity from 1000 to 4000 grains of water, and the diameter of the stem was from one-eighth to one-sixteenth of an inch, according to the character of the experiment. In the bulbs employed for ordinary purposes, each grain of water occupied about a quarter of an inch in the stem, and as the graduation was made in grains of water at 60°, the experiment could be made to the tenth of a grain of increase in volume. In every case care was taken that the salts used were rigidly pure, and in their proper state of hydration. The distilled water employed to dissolve them was deprived of air by long-continued boiling, and preserved for use in stoppered bottles. The salt was introduced by a tubulure in the side of the bulb in the following manner. The bulb was filled with water until it reached a fixed point in the stem, when it was reclined and the stopper removed. A weighed quantity of salt was then introduced by a dry funnel, and the stopper re-inserted, care being taken that no air was admitted during the operation; the increase in the stem, after the salt was dissolved, gave the volume of the quantity of salt used in the experiment. It was found by repeated trials that no loss of volume or error was occasioned by the moistening of the tube during the time it was in the reclining posture, for the precaution was always taken to moisten the walls of the tube previous to the experiment. It is evident that the volume occupied by a salt in solution must be modified by the position of the point of its maximum density. Despretz has shown* that the temperature at which solutions are most dense becomes lower in proportion to the quantity of matter held in solution. It is also known, from the experiments of Dalton and others, that from the point of maximum density to about 30° above or below it,

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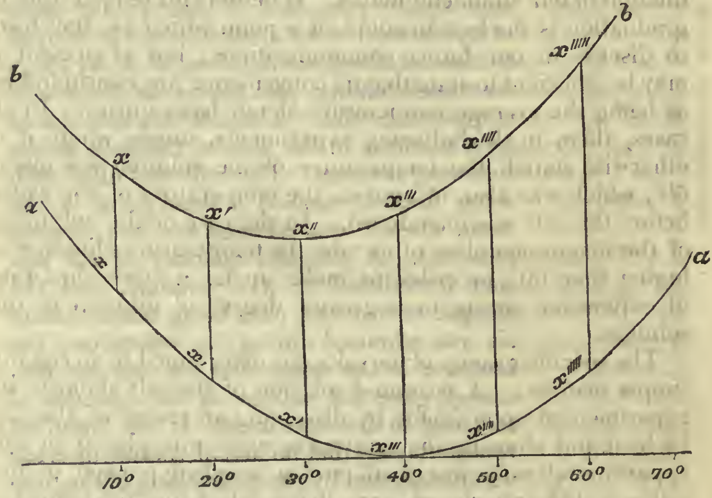
* *Annales de Chimie*, vol. lxx. 1839, p. 81.

water and dilute solutions expand according to the square of the temperature from that of greatest density. From Despretz's table of the expansion of water, it appears that the law is not true as far as 212°. As far however as it does hold good, it is evident, from the properties of the parabola, that the volume occupied by a salt in solution will increase in arithmetical progression with the temperature at which the experiment is made.

For instance, let $a\,a$ in the following diagram be a parabola representing the expansion of water, and let $b\,b$ be a similar parabola representing the expansion of a solution. Let the latter parabola have its vertex or point of greatest density opposite 30°, while the former parabola has its vertex at 40°; then $x\,x$, $x'\,x'$, $x''\,x''$, &c., quantities which increase in arithmetical progression, will represent the volumes occupied by the salt in solution at the temperature of 10°, 20°, 30°, &c.



In a similar manner it may be shown that the volume occupied by each equivalent of a salt in solution at any given temperature will increase with the density of the solution. In order to ascertain the amount of influence exercised by a change in the position of the point of maximum density, we have made a series of experiments on the expansion of water and of solutions by heat, which we propose to lay before the Society in a succeeding memoir. In order, however, to render evident the augmentation of volume caused by increased density, we have constructed the following table of the volumes occupied by 172 grains, or one equivalent of sugar, in solutions of different degrees of density.

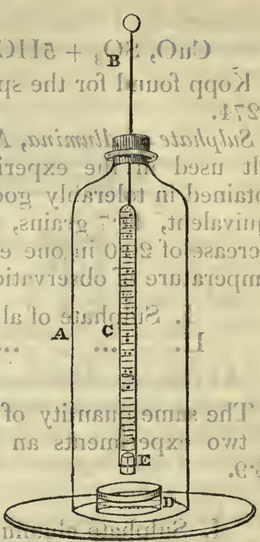
TABLE I.

Ratio of the quantity of sugar to the quantity of water in which it was dissolved.	Temperature.	Volume in grain measures of water.
1 : 120	60°	99·00
1 : 10	52	105·09
1 : 1	52	107·01
3 : 1	52	108·06

As the rate of expansion of dilute solutions is so near that of water, it was in most cases sufficient, for a very close approximation to absolute accuracy, to take the observation within a few degrees below 60°, the temperature of the graduation of our volumenometers. Whether this temperature of graduation is the best to adopt, is a point which we shall have to discuss in our future communications; but at present it may be sufficient to state that its convenience was considerable, as being the average temperature of our laboratories. In all cases, then, in the following experiments, unless where it is otherwise stated, the temperature of the solution was about 60°, which was also, of course, the temperature of the water before the salt was introduced. In the case of the sulphates of the magnesian class of metals, the temperature chosen was higher than 60°, in order to make up for a diminished rate of expansion, owing to a greater degree of dilution in the solution.

The specific gravity of the salts was determined in an equally simple manner. A saturated solution of the salt about to be experimented upon (made by dissolving an excess of the salt by heat and allowing the solution to cool) was placed in the apparatus already described, and a weighed portion of the salt was then introduced, care being taken that the temperature did not vary during the experiment. As the new portion of salt could not dissolve, the increase in the stem indicated the volume due to the quantity of salt introduced, and afforded data for calculating the specific gravity. In many cases oil of turpentine was used instead of the saline solution.

It was frequently desirable, especially in the case of hydrated salts rendered anhydrous, to avoid the use of water, and in the case of organic compounds, also of turpentine; and to meet such cases we constructed the following simple apparatus, which we believe to possess various advantages.



A is the receiver of an air-pump, furnished at the top with a collar and sliding rod. B C is a small graduated tube filled with the substance, the volume of which is to be determined; it is closed with a stopper E, perforated with a hole of dimensions so small as to prevent any of the salt from falling out. D is a cup of mercury placed immediately below the graduated tube C. The sketch indicates the position of the apparatus on an air-pump when the experiment is about to be performed. The receiver is then exhausted as thoroughly as possible, and the indication of the siphon-gauge is accurately noted. The graduated tube is then lowered by means of the sliding rod until it touches the bottom of the cup containing the mercury, which, after the admission of air, flows into the tube until it is filled. The whole contents of the tube are then thrown into water, and the salt is washed away by decantation. The mercury is dried by bibulous paper, and restored to the tube. If the temperature be different from that which it possessed in the first part of the experiment, it is restored to the original temperature, or a correction is made for the difference. It is now obvious that the space in the tube unoccupied by the mercury is that which was formerly filled with the salt. To this, however, must be added a slight correction for the imperfect nature of the vacuum, which is not Torricellian,—a correction which need not exceed $\frac{1}{200}$th of the volume observed. With these preliminary descriptions and observations, we now proceed to describe the details of our experiments, throwing them into various classified groups of salts, for the purpose of easy reference.

The first group described is remarkable for containing a large amount of water of hydration.

Sulphate of Copper, $CuO, SO_3 + 5HO = 124{\cdot}88$.—The third part of an equivalent of this salt, 41·62 grains, dissolved in 3140 grains of water at 32°, with an increase of 13·15, but dissolved in water at 90°, with an increase of 15·0.

$CuO, SO_3 + 5HO$, vol. in solution 45·0.

Half an equivalent of this salt, 62·44 grains, being immersed in a saturated solution, occupied the volume of 27·7 water-grain measures.

	Sp. vol.		Sp. gr.
$CuO, SO_3 + 5HO$, vol. of salt	55·4	...	2·254

Kopp found for the specific gravity of this salt the number 2·274.

Sulphate of Alumina, $Al_2O_3, 3SO_3 + 18HO = 333·7$.—The salt used in the experiments was carefully prepared, and obtained in tolerably good crystals. The eighth part of an equivalent, 41·7 grains, dissolved in 1000 of water, with an increase of 20·0 in one experiment, and 19·9 in another, the temperature of observation being 51°.

I. Sulphate of alumina, vol. in solution	160·0
II.	159·2
Mean . .	159·6

The same quantity of salt thrown into turpentine caused in two experiments an increase of 25·0, and in a third of 24·9.

			Sp. gr.
I. Sulphate alumina, vol. of salt	200·0	...	1·668
II.	199·2	...	1·675
Mean . .	199·6	...	1·671

Sulphate of Soda, $NaO, SO_3 + 10HO = 161·48$.—Sulphate of soda, crystallized out of a strong warm solution, carries down 10 atoms of water. Of this salt, about one-fourth of an equivalent (40·4 grains), on being dissolved in 1000 grains of water, caused in two experiments an increase of 23·0, at a temperature of 59°; and in a third experiment of 22·8 at the same temperature.

I. II. $NaO, SO_3 + 10HO$, vol. in solution =	91·8
III.	91·2
Mean . .	91·5

The same quantity of the salt being immersed in a saturated solution occasioned an increase of 27·8; and on a second experiment, of 27·2 at a temperature of 62°.

			Sp. gr.
I. $NaO, SO_3 + 10HO$, vol. of salt	111·1	...	1·453
II.	108·7	...	1·485
Mean . .	109·9	...	1·469

When sulphate of soda crystallizes from a weak cold solution, it carries down a quantity of water, corresponding to eleven equivalents. In two experiments, the volume in solution of salt procured in this way was 98; but we apprehend that the water is merely mechanical, for reasons which will be seen hereafter, as the volume of the salt itself, by a mean of several experiments, came out to 119·5, whereas had this eleventh atom of water been combined it should have been 121.

Biborate of Soda, $NaO, 2BO_3 + 10HO = 191{\cdot}23$.—On dissolving 40 grains of this salt in 1000 of water, the increase was 19·2 at a temperature of 55°.

$NaO, 2BO_3 + 10HO$, vol. in solution 91·7.

Half an equivalent, or 95·61 grains, on being placed in a saturated solution, occasioned an increase of 55·5; and 47·8 grains caused an increase of 27·5; both experiments being made at a temperature of 55°.

			Sp. gr.
I.	$NaO, 2BO_3 + 10HO$, vol. of salt	111	1·722
II.	... ...	110	1·738
	Mean . .	110·5	1·730

Chloride of Strontium, $Sr, Cl + 6HO = 133{\cdot}32$.—There are two hydrates of chloride of strontium, the one with nine, and the other with six equivalents of water. To determine which of these hydrates was under examination, 4·324 grammes were heated to redness with a loss of 1·75 gramme = 40·47 per cent., showing that the hydrate was that with six equivalents of water, which gives by calculation 40·50 per cent.

On dissolving 40 grains of this salt in 1000 of water, the increase occasioned at a temperature of 56° was 16·0; a second experiment, in which the same quantities were used, gave exactly the same result.

I. II. $Sr Cl + 6HO$, vol. in solution 53·3.

The same quantity of salt (40 grains), immersed in a saturated solution, caused an increase of 20·0 at a temperature of 57°; and on a second experiment of 19·7.

			Sp. gr.
I.	$Sr Cl + 6HO$, vol. of salt	66·6	2·000
II.	... ...	65·6	2·030
	Mean . .	66·1	2·015

Chloride of Calcium, $Ca Cl + 6HO = 109{\cdot}92$.—On dissolving 55 grains, or the half of an equivalent of this salt, in 1000 grains of water, an increase of 28·0 was obtained at the temperature of 70°; and in a second experiment 27·6 at 60°.

I.	$Ca Cl + 6HO$, vol. in solution	56·0
II.	... ...	55·2
	Mean . .	55·6

The same quantity of salt thrown into turpentine caused an increase of 32·7 and 32·8 in two experiments.

			Sp. gr.
I.	$Ca Cl + 6HO$, vol. of salt	65·4	1·682
II.	... ...	65·6	1·677
	Mean . .	65·5	1·680

Chloride of Magnesium, $\mathrm{Mg\,Cl} + 6\mathrm{HO} = 102{\cdot}16$.—Millon has lately described this salt as containing $6\frac{1}{2}$ atoms of water; but, as we have not been successful enough to obtain this hydrate, we retain the old formula. 25·54 grains, or the fourth of an equivalent, dissolved in 1000 grains of water at 53°, with an increase of 14·0.

$\mathrm{Mg\,Cl} + 6\mathrm{HO}$, vol. in solution = 56·0.

The same quantity, 25·54 grains, gave, in four experiments, respectively, 16·5; 16·0; 16·4; 16·5.

			Sp. gr.
I.	$\mathrm{Mg\,Cl} + 6\mathrm{HO}$ volume of salt	66·0 ...	1·548
II.	... ...	64·0 ...	1·595
III.	... ...	65·6 ...	1·557
IV.	... ...	66·0 ...	1·548
	Mean . .	65·4 ...	1·562

The salts now examined are not calculated, on account of the deliquescent character of several of them, to produce absolutely accurate experimental results; but, notwithstanding this circumstance, the determination of their volumes is sufficiently uniform to indicate the theory. The actual volume observed for each of the salts in solution, when divided by 9, the atomic volume of water, yields as the quotient the same number as that representing the atoms of water in the salt. Hence it is quite certain that the salts now described dissolve in water without adding to its bulk more than is due to the liquefaction of the water in chemical combination with them.

The volumes of the salts in their solid state possess a number considerably higher than that representing the liquid volume, but affect a divisor, which is the same for all the salts, allowing for errors of experiments, or for alterations caused by incidental circumstances. This divisor is a number either equal or approximating to 11. When the volumes of the salts in the solid state are divided by this number, the quotient represents the number of atoms of water attached to the salt. The most natural view of this circumstance is to suppose that water in combination as a solid with a salt possesses a higher volume than liquid water, just as in the case of ice. If this view be correct, the atomic volume of the salts described is the same in the state of a solid as when in solution, the only difference being, that in the one case the volume is expressed by liquid, in the other by solid water. In this case, however, water in combination with a salt does not possess the same volume as ice, which, according to our experiments, detailed in another part of this paper, has a volume of 9·8, and not of a number approaching to 11.

Yet there is nothing extravagant in the idea that water combined with a salt may have a volume different from that of ice. Indeed, we are inclined to be of the opinion that ice itself represents nearly the mean of the volume of water uncombined and that of combined water. Be this as it may, it will be observed, as we proceed, that the number 11 is the best exponent of one class of our experiments on specific gravity; and therefore, without resting its claims to acceptance entirely on the present experiments, we assume it in the following tables as the theoretical result for each class of salts. With these views we tabulate the experiments which have been already detailed.

TABLE II.—Showing the volumes occupied by certain salts containing a large amount of hydrate water.

Designation.			Volume in solution.			Volume as salt.				
Name.	Formula.	Atomic Weight.	Volume in solution.	9, or volume of water as unity.	Volume by calculation.	Volume of salt by experiment.	11, an assumed number as unity.	Volume by calculation.	Specific gravity by calculation.	Specific gravity by experiment.
Sulphate of copper......	CuO, SO_3+5HO	124·88	45·	5	45	55·4	5	55	2·270	2·254
Sulphate of alumina	$Al_2O_3, 3SO_3+18HO$	333·7	159·6	18	162	199·6	18	198	1·685	1·671
Sulphate of soda.........	NaO, SO_3+10HO	161·48	91·6	10	90	109·9	10	110	1·468	1·469
Biborate of soda	$NaO, 2BO_3+10HO$	191·23	91·7	10	90	110·5	10	110	1·738	1·730
Chloride of strontium .	$SrCl+6HO$	133·33	53·3	6	54	66·1	6	66	2·020	2·015
Chloride of calcium	$CaCl+6HO$	109·92	55·6	6	54	65·5	6	66	1·665	1·680
Chloride of magnesium.	$MgCl+6HO$	102·16	56·0	6	54	65·4	6	66	1·548	1·562

There are some salts which do not take up any space in solution, except that due to their water, but which assume a volume due to one of their constituents on becoming solid; the potash and ammonia alums are examples of this class.

Sulphate of Alumina and Potash, $Al_2O_3\ 3SO_3 + KO\ SO_3 + 24HO = 474·95$,—59 grains of alum dissolved in 1000 grains of water, gave the increase of 27·0 in one experiment and 27·1 in another, both at the temperature of 60°.

I. Alum, volume in solution 217·3
II. ... ... 218·1
Mean . . . 217·7

On throwing 59·37 grains into a saturated solution, an increase of 34·4 was obtained in the first experiment; of 34·7 in a second; of 34·3 in a third; and of 34·2 in a fourth, all at a temperature about 60°.

			Sp. gr.
I.	Alum, vol. of salt	275·2 ...	1·726
II.	... ...	277·6 ...	1·711
III.	... ...	274·4 ...	1·730
IV.	... ...	273·6 ...	1·735
	Mean .	275·2 ...	1·726

Sulphate of Alumina and Ammonia, $Al_2O_3, 3SO_3 + NH_4O, SO_3 + 24HO = 454·26$.—20 grains of this salt dissolved in 4100 grains of water with an increase of 10·0 at 58°.

Ammoniacal alum, vol. in solution 227·1

The eighth part of an equivalent, 56·78 grains, immersed in turpentine, caused a rise in the stem of 34·9 in one experiment and 35·0 in another, the temperature being 60°.

			Sp. gr.
I.	Ammonia alum, vol. of salt	279·2 ...	1·627
II.	... ...	280·0 ...	1·623
	Mean . .	279·6 ...	1·625

Chrome Alum, $Cr_2O_3, 3SO_3 + KO, SO_3 + 24HO = 504·1$.—On dissolving 32 grains of this salt in 4100 grains of water, an increase of 13·7 was effected at 37°.

Chrome alum, vol. in solution 215·8

In two experiments 63 grains of this salt thrown into turpentine caused an increase of 34·5.

		Sp. gr.
Chrome alum, volume of salt	276 ...	1·826

Iron Ammonia-alum, $Fe_2O_3, 3SO_3 + NH_4O, SO_3 + 24HO = 481·03$.—On dissolving 30·06 grains in 1000 grains of water, an increase of 14·3 was obtained at a temperature of 37°.

Iron alum, vol. in solution 228

The eighth part of an equivalent, 60·13 grains, produced an increase of 35·0 measures when thrown into turpentine.

		Sp. gr.
Ammoniacal iron alum, vol. of salt	280·0 ...	1·718

Pyrophosphate of Soda, $2NaO, PO_5 + 10HO = 224·15$.—The eighth part of an equivalent of the crystallized pyrophosphate, 28 grains, dissolved in 1000 grains of water with an increase of 11·2 in one experiment and of 11·3 in another, the temperature in both cases being 58°.

Pyrophosphate of soda, vol. in solution 89·6
... ... 90·4
Mean . . 90·0

On immersing 56·04 grains of the salt in a saturated solution, an increase of 30·5 was obtained in two experiments.

		Sp. gr.
Pyrophosphate of soda, vol. of salt	122·0 ...	1·836

By tabulating the results thus obtained we find the following relationship between the class of alums.

TABLE III.—Showing the volumes of certain Alums*.

Designation.			Volume in solution.			Volume in state of salt.				
Name.	Formula.	Atomic weight.	Vol. in solution by experiment.	9, or vol. of water as unity.	Vol. by theory.	Vol. of salt by experiment.	11, taken as unity.	Vol. of salt by theory.	Specific gravity by theory.	Specific gravity by experiment.
Potash alum ...	$\left\{ \begin{matrix} Al_2O_3, 3SO_3, KO, SO_3 \\ +24HO \end{matrix} \right\}$	474·95	217·7	24	216	275·2	25	275	1·727	1·726
Ammonia alum.	$\left\{ \begin{matrix} Al_2O_3, 3SO_3+NH_4O \\ SO_3+24HO \end{matrix} \right\}$	454·26	227·1	25	225	279·6	25	275	1·652	1·625
Chrome alum...	$\left\{ \begin{matrix} Cr_2O_3, 3SO_3 + KO, \\ SO_3+24HO \end{matrix} \right\}$	504·1	215·8	24	216	276·0	25	275	1·833	1·826
Iron alum ...	$\left\{ \begin{matrix} Fe_2O_3, 3SO_3+NH_4O, \\ SO_3+24HO \end{matrix} \right\}$	481·03	228	25	225	280·0	25	275	1·749	1·718
Pyrophosphate of Soda ...	$2NaO, PO_5+10HO$	224·15	90	10	90	122	11	121	1·852	1·836

The peculiarity of the salts described in the above table is, that the quotient of the divisor for the potash alums in the solid state is not the same as in the state of solution, and that the ammoniacal alums possess one volume in solution greater than the corresponding potash alums, both of which peculiarities will find an explanation as we proceed. Pyrophosphate of soda shares this peculiarity, and is therefore introduced into the table.

We now proceed to describe a class of hydrated salts, in which the divisor for the solid volume is certainly not the number 11.

Carbonate of Soda, $NaO, CO_2+10HO = 143{\cdot}4$.—On dissolving 35·85 grains of this salt in 1000 grains of water, the increase was 22·5 in one experiment at 64°, and 22·9 in a second experiment at 65°.

I. NaO, CO_2+10HO, vol. in solution 90·0
II. ... ... 91·6
Mean . . 90·8

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* *Vide* conclusion for explanation of the high volume of ammonia alums.

On throwing 35·8 grains of the salt into turpentine, the increase was 24·7, 24·5, 24·6, and 24·8 in consecutive experiments with different specimens.

			Sp. gr.
I.	Carbonate of soda, vol. of salt	98·8 ...	1·451
II.	... ...	98·0 ...	1·463
III.	... ...	98·4 ...	1·457
IV.	... ...	99·2 ...	1·446
	Mean . .	98·6 ...	1·454

Rhombic Phosphate of Soda, $2\,NaO\ HO,\ PO_5 + 24HO = 359{\cdot}1$.—The eighth part of an equivalent of this salt, 44·9 grains, dissolved in 1000 grains of water with an increase of 27·0 in one experiment, and 27·1 in a second; by some mistake the temperature of the solution has not been recorded.

I.	Phosphate of soda, vol. in solution	216·0
II.	... ...	216·8
	Mean . .	216·4

The same quantity of salt thrown into turpentine caused an increase of 29·4 in two experiments, and 29·5 in a third.

			Sp. gr.
I.	Phosphate of soda, vol. of salt	235·2 ...	1·527
II.	... ...	235·2 ...	1·527
III.	... ...	236·0 ...	1·521
	Mean . .	235·5 ...	1·525

Sub-phosphate of Soda, $3NaO,\ PO_5 + 24HO = 381{\cdot}6$.—The eighth part of an equivalent of this salt, 47·7 grains, dissolved in 1000 grains of water, with an increase of 27·1 in two experiments at 48°.

I. II. Sub-phosphate of soda, vol. in solution 216·8

The same quantity of salt thrown into turpentine produced an increase of 29·4 in two experiments under favourable circumstances, although in another experiment, in which we were not satisfied with the state of hydration of the salt, the increase was only 28·9.

Sub-phosphate of soda 235·2 ... 1·622

Arseniate of Soda, $2NaO,\ HO,\ AsO_5 + 24HO = 402{\cdot}9$.—On dissolving 50·36 grains, the eighth part of an equivalent, in 1000 grains of water, an increase of 27·2 was obtained at a temperature of 54°.

Arseniate of soda, vol. in solution 217·6

The same quantity of salt thrown into a saturated solution caused an increase of 29·0 in several experiments. This salt loses its water with such facility that it is almost impossible to obtain it in a state well-fitted for experiment. In two specimens of salts, prepared at different times, the volume for the above quantity of salt was 29·7 and 29·8; but as in most cases it was only 29·0, we give the result most generally obtained.

Arseniate of soda, vol. of salt 232·0 ... 1·736

Sub-arseniate of Soda, $3NaO, AsO_5+24HO=425·2$.—The eighth part of an equivalent, 53·15 grains, of this salt dissolved in 1000 grains of water, with an increase of 27·0 in one experiment, and 26·9 in another, at a temperature about 55°.

I. Sub-arseniate of soda	216·0
II. ...	215·2
Mean . .	215·6

The same quantity of salt immersed in turpentine caused an increase of 29·4 and 29·5 in two experiments.

I. Sub-arseniate of soda, vol. of salt	235·2	1·808
II.	236·0	1·801
Mean . .	235·6	1·804

Cane-Sugar, $C_{12}H_{11}O_{11}=171·60$.—25·8 grains of sugar dissolved in 3140 grains of water caused an increase of 14·8 at 32°; 42·9 grains, or the fourth of an equivalent, gave an increase of 25·0 at 60° in two experiments.

I. Cane-sugar, vol. in solution	98·4
II.	100·0
III.	100·0
Mean . .	99·5

300 grains of sugar-candy thrown into alcohol previously saturated with it, caused an increase in the first experiment of 188·0, in the second of 188·75; in a third experiment, 49·65 grains thrown into turpentine caused an increase of 31·0; and the same quantity, in a fourth experiment, of 31·1, the temperature in all the cases being about 60°.

			Sp. gr.
I. Cane-sugar, vol. of solid	107·5	...	1·596
II.	107·9	...	1·590
III.	107·1	...	1·602
IV.	107·5	...	1·596
Mean . .	107·5	...	1·596

In this section a class of salts presents itself, in which the volumes are clearly not represented by any multiple of 11; yet they are uniform in their isomorphous relations, and are sensibly multiples of the same number. To discover whether the solid volume have any relation to that occupied by ice, we have determined the specific gravity of the latter with great care. The distilled water, which we converted into ice, was deprived of air by long-continued boiling, and a weighed portion of the ice was quickly immersed in water at 32°, the balance being kept at the same temperature during the weighing of the ice. The rise in the stem of the volumenometer, in which fragments of ice had previously been placed, indicated the volume due to the quantity of ice immersed. On treating in this manner 54·2 grains of ice, a rise in the stem of 59·0 was produced; and in a second experiment 52·8 grains of ice occasioned an increase of 57·5, the temperature in both cases being exactly 32°.

	Specific volume.		Specific gravity.
I. Ice volume	9·797	...	0·9186
II.	9·801	...	0·9183
Mean . .	9·799	...	0·9184

As the true specific gravity of ice is a subject of much importance, we place here all the recorded results, as given in Bottger's most useful work on specific gravity, and in the first volume of Scoresby's Arctic Regions.

Specific gravity of ice	0·888	Dulk.
... ...	0·937	Irvine.
... ...	0·945	Williams.
... ...	0·885	Meineke.
... ...	0·905	Heinrich Kraft.
... ...	0·927	Osann.
... ...	0·950	Roger and Dumas.
... ...	0·920	Scoresby.
Mean . .	0·919	

The mean of all these experiments, differing only $\frac{1}{1000}$ from our own determination, warrants us in concluding that our result is accurate, and that 9·80 may safely be taken as the specific volume of an atom of ice. Now it must at once strike the observer of the previous experiments, that this number forms the divisor for the volumes of the salts described in the present section.

TABLE IV.—Showing the volumes occupied by certain Phosphates, Arseniates, Carbonate of Soda and Cane-sugar.

Designation.			Volume in solution.			Volume of salt.				
Name.	Formula.	Atomic weight.	Volume in solution by experiment.	9, or volume of water as unity.	Volume by theory.	Volume of salt by experiment.	9·8, or volume of ice as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Carbonate of soda	NaO, CO_2+10HO	143·4	90·8	10	90	98·6	10	98·0	1·463	1·454
Phosphate of soda	$2NaO, HO, PO_5+24HO$	359·1	216·4	24	216	235·5	24	235·2	1·527	1·525
Sub-phosphate of soda	$3NaO, PO_5+24HO$	381·6	216·8	24	216	235·2	24	235·2	1·622	1·622
Arseniate of soda	$HO, 2NaO, AsO_5+24HO$	402·9	217·6	24	216	232·0	24	235·2	1·713	1·736
Sub-arseniate of soda	$3NaO, AsO_5+24HO$	425·2	215·6	24	216	235·6	24	235·2	1·808	1·804
Cane-sugar	$C_{12}H_{11}O_{11}$	171·6	99·5	11	99	107·5	11	107·8	1·591	1·596

Connected with the latter group, there is a class of salts which come out uniformly with themselves, but the divisor of which is not 11 in the solid state. We subjoin them in the following group.

Sulphate of Magnesia, $MgO, SO_3 + 7HO = 123·86$.—When this salt is dissolved in a large quantity of cold water, the volume observed after solution is always less at ordinary temperatures than that due to the water contained in the salt. That this diminution is due to a contraction caused by an affinity of the salt for water is shown by the fact, that anhydrous sulphate of magnesia dissolved in a large quantity of water actually lessens, instead of increasing the bulk of the water; and to compensate for this contraction, a certain temperature has to be given to the water. In the following experiments with the sulphates of magnesia, zinc and iron, this circumstance has been attended to, and the temperature is given at which the results come out exact.

31 grains of crystallized sulphate of magnesia were dissolved in 3140 grains of water at 32°, and caused an increase of 15·22; at 85° the increase was 15·75.

$MgO, SO_3 + 7HO$, vol. in solution 63.

Half an equivalent, 61·93 grains, being placed in a saturated solution of the salt, caused an increase of 37·5 in one experiment, but in three other experiments the increase was not greater than 37·2; the temperature in all the cases being 54°.

				Sp. gr.
I.	$MgO, SO_3 + 7HO$, vol. of salt	75·0	...	1·651
II.	... ...	74·4	...	1·664
III.	... ...	74·4	...	1·664
IV.	... ...	74·4	...	1·664
	Mean . .	74·55	...	1·660

Sulphate of Zinc, $ZnO, SO_3 + 7HO = 143{\cdot}43$.—This salt possesses the same property as sulphate of magnesia, of causing a contraction when the anhydrous salt is dissolved in a large quantity of cold water.

35·9 grains were dissolved in 3140 grains of water at a temperature of 32°, causing a rise in the stem of 14·03. At 90° the rise was 15·77.

$ZnO, SO_3 + 7HO$, vol. in solution 63.

The half of an equivalent of this salt, 71·71 grains, being thrown into a saturated solution, caused an increase of 37·1; and 35·85 grains produced a rise in the stem of 18·6 in two experiments, and of 18·5 in a fourth.

				Sp. gr.
I.	$ZnO, SO_3 + 7HO$, vol. of salt	74·2	...	1·933
II.	... ...	74·4	...	1·928
III.	... ...	74·4	...	1·928
IV.	... ...	74·0	...	1·937
	Mean . .	74·25	...	1·931

Sulphate of Iron, $FeO, SO_3 + 7HO = 138{\cdot}3$.—The fourth part of an equivalent, 34·6 grains, dissolved in 3140 grains of water at 32° with an increase of 15·25, which became 15·75 at 80°.

$FeO, SO_3 + 7HO$, vol. in solution 63.

The same quantity of salt thrown into turpentine gave in one experiment an increase of 18·6, in another of 18·7, and in a third of 18·6.

				Sp. gr.
I.	$FeO, SO_3 + 7HO$, vol. of salt	74·4	...	1·860
II.	... ...	74·8	...	1·850
III.	... ...	74·4	...	1·860
	Mean . .	74·5	...	1·857

Sulphate of Nickel, $NiO, SO_3 + 6HO = 131{\cdot}74$.—This salt we found to contain only 6 atoms of water instead of 7 atoms, as usually described; but it is known to crystallize with both proportions. On dissolving 35 grains in 1000 of water, the increase obtained was 14·0 at a temperature of 55°.

$NiO, SO_3 + 6HO =$ vol. in solution 52·7.

We have not ourselves obtained the specific gravity of this salt, but this has been determined by Kopp, who gives it at 2·037, without however describing the character of the hydrate which he examined. It is possible, therefore, that it may not be the same as that which we have examined; but presuming it to be so, the volume of this salt, according to Kopp, would be

		Sp. gr.
$NiO, SO_3 + 6HO$, vol. of salt	64·6 ...	2·037.

The volumes of the magnesian sulphates with 7 atoms of water are obviously less than those which would result were they multiples of the volume 11. But as we have already seen that the water of hydration does not always enter into combination with the volume 11, but occasionally with that of 9·8, or the volume of ice, the results obtained may be explained on this view. Graham*, in his researches on the phosphates, and on the heat of combination, drew attention to the fact, that the atoms of water seem to be attached together in *twos*. Millon† more lately has shown that the two last atoms of water in sulphate of magnesia are less firmly attached than the five remaining atoms; that a magnesian sulphate in fact may be viewed as

$$MgO, SO_3 + 5HO + 2HO.$$

That 5 atoms of water form the natural numbers for the magnesian sulphates we have evidence in the salts of copper and manganese, both of which possess these 5 atoms of water in combination with a volume of 11, at least.

$CuO, SO_3 + 5HO$ vol. of salt 55·4 (P. and J.)
$MnO, SO_3 + 5HO$ ... 57·6 (Kopp.)

As then the two additional atoms of water are retained by a less feeble affinity than the remaining five, may we not assume that they are present, as in the case of other salts possessing a feeble affinity for water, with the volume of ice, whilst the original 5 atoms possess the higher volume of 11? The following table will show that this hypothesis gives results by calculation which do not differ widely from those obtained by experiment.

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[*] Phil. Trans., part 1. 1837, page 67.

[†] *Annales de Chimie*, 3 série, t. xii. p. 134.

TABLE V.

Designation.			Volume in solution.			Volume of salt.				
Name.	Formula.	Atomic Weight.	Volume in solution.	9, or volume of water as unity.	Volume by theory.	Volume of salt by experiment.	11 as unity for 5 atoms, and the volume of ice for 2 atoms.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Sulphate of magnesia	$MgO, SO_3+5HO+2HO$	123·86	63	7	63	74·55	5+2	74·6	1·660	1·660
Sulphate of zinc	$ZnO, SO_3+5HO+2HO$	143·43	63	7	63	74·25	5+2	74·6	1·926	1·931
Sulphate of iron	$FeO, SO_3+5HO+2HO$	138·3	63	7	63	74·5	5+2	74·6	1·854	1·857
Sulphate of nickel ...	$NiO, SO_3+5HO+HO$	131·74	52·7	6	54	64·6	5+1	64·8	2·033	2·037

Before leaving this section we would sum up some of the principal facts observed. In the first place, it is of much importance to know that these salts dissolve in water without increasing its bulk more than is due to the water attached to them as crystallized water. The acid and bases entirely disappear in the water which is attached to them; and so closely does this rule prevail, that the atom of basic water in the tribasic arseniates and phosphates has ceased to play the part of water, either in solution or in the solid state. In the condition of solid salts, we find four classes to which we have drawn attention. The first of these is represented by salts having their water firmly attached, and possess as a divisor for their atomic volume a number equal or approaching to 11; and we have concluded, as the quotient of this divisor is always the same as the number of atoms of water attached to the salt, that 11 is the volume of an atom of water in combination; and hence that the salts have disappeared in this attached water, adding to its weight, but not to its observed bulk.

The second class of salts in this section is represented by potash alum, in which the astonishing result is obtained, that the 23 anhydrous atoms of this salt have combined in some way with the 24 atoms of water, so as to cease to occupy bulk in solution. The peculiarity of this group is, that an additional 11 becomes attached to the solid salt, so that the quotient of the divisor is 25 instead of 24. This fact, and that connected with the ammoniacal alums in the same group, cannot be discussed with propriety in the present place.

The third group of salts in this section is one of high interest, and is represented by salts having their hydrate water attached by a feeble affinity. In them the volume of the water is exactly the same as that of ice itself. Sugar belongs to this category, not because the H_{11} O_{11} are feebly attached, for it has yet to be shown that they are present *quasi* water. The fact however that these 11 atoms of hydrogen and oxygen take up the same space as liquid water in solution, and as ice in the solid state of sugar, and that the 12 atoms of carbon have ceased to occupy space, is a matter of supreme interest, and cannot fail to lead to important results when we come to the consideration of organic compounds.

The fourth class in this section finds its representatives in the sulphates of the magnesian class of metals, and perhaps ought to include the magnesian chlorides also. They possess their constitutional water with the usual volume of 11, while the water feebly attached is present with the volume of ice.

Although, then, we have four distinct groups in the section of salts possessing a large amount of hydrate water, we have only two modifications of volume, the one represented by a number equal or approximating to 11, the other by the volume of ice itself, viz. 9·8.

We now proceed to the consideration of salts which either are destitute of water, or contain it in small proportion only. The volumes affected by them must be volumes peculiar to themselves, and not, as in the present section, to the water with which they are combined.

SECTION II.

Sulphates with a small proportion of Water of Hydration, Anhydrous and Double Sulphates.

Sulphate of Potash, $KO, SO_3 = 87{\cdot}25$.—Half an equivalent of this salt, dissolved in 3140 grains of water of 37°, increased 7·2, and at 80°, 9·0; the same quantity dissolved in 1000 grains of water at 66°, increased 9·0.

I. II. KO, SO_3, vol. in solution 18·0

A whole equivalent of the salt being placed in its saturated solution effected a rise in the stem of 33·0 at a temperature of 55°; and a repetition of the experiment gave the increase 33·1.

		Sp. gr.
I. KO, SO_3, vol. of salt	33·0 ...	2·644
II.	33·1 ...	2·636
Mean . .	33·05	2·640

Sulphate of Potash and Sulphate of Water, $KO, SO_3 + HO, SO_3 = 136{\cdot}35$.—The fourth of an equivalent (34·08 grains), being dissolved in 1000 grains of water, caused an increase of

9·0 at a temperature of 59°, and 33 grains dissolved in the same quantity of water occasioned a rise of 8·75 at 44°.

I. $KO, SO_3 + HO, SO_3$, vol. in solution	36·0
II.	36·1
Mean . .	36·05

Half an equivalent (68·2 grains) of the salt, previously fused, immersed in a saturated solution, produced a rise in the stem of 27·5; and a second experiment with the same quantity, but with salt which had not been fused, of 27·6, the temperature on both occasions being 55°.

		Sp. gr.
I. $KO, SO_3 + HO, SO_3$, vol. of salt	55·0 ...	2·479
II.	55·2 ...	2·470
Mean . .	55·1 ...	2·475

Sulphate of Ammonia, $NH_4O, SO_3 + HO = 75·25$.—In three separate experiments, in which 75·25 grains of this salt were dissolved in 1000 grains of water, the increase was exactly 36·0 at 60°.

I. II. III. $NH_4O, SO_3 HO$, vol. in solution	36·0

Half an equivalent (37·6 grains) being immersed in a saturated solution at 49°, caused, in two experiments, an increase of 21·5.

		Sp. gr.
I. II. $NH_4O, SO_3 + HO$, vol. of salt	43·0 ...	1·750

Sulphate of Ammonia and Sulphate of Water, $NH_4O, SO_3 + HO, SO_3 = 115·35$.—Half an equivalent (57·7 grains) of this salt, dissolved in 1000 grains of water, gave a rise in the stem of 23·0 at 56°, in two separate experiments.

I. II. $NH_4O, SO_3 + HO, SO_3$, vol. in solution	46·0

The same quantity of salt being placed in its saturated solution, caused an increase of 32·5 in one experiment and of 33·0 in a second, the temperature in both cases being 58°.

		Sp. gr.
I. $NH_4O, HO, 2SO_3$, vol. of salt	65·0 ...	1·775
II.	66·0 ...	1·747
Mean . .	65·5 ...	1·761

Sulphate of Soda and Sulphate of Water, $NaO, SO_3 + HO, SO_3 = 120·64$.—The fourth of an equivalent (30·16 grains) dissolved in 1000 grains of water, in the first experiment with an increase of 4·6, in the second of 4·7, both at a temperature of 56°.

I.	$NaO, SO_3 + HO, SO_3$, vol. in solution	18·4
II.	... ...	18·8
	Mean . .	18·6

The same quantity of salt thrown into a saturated solution caused, in two experiments, an increase of 11·0 at a temperature of 54°.

		Sp. gr.
I. II. Bisulphate of soda, vol. of salt	44·0 ...	2·742

Ammoniacal Sulphate of Copper, $CuO, SO_3, HO + 2NH_3$ = 123·0.—The fourth of an equivalent (30·8 grains) of this substance in beautiful large indigo-blue crystals, dissolved in 1000 grains of water, with an increase of 13·3 in one experiment and 13·0 in another, the temperature being 54 and 50°.

I.	$CuO, SO_3, HO + 2NH_3$, vol. in solution	53·2
II.	... ...	52·0
	Mean . .	52·6

61·5 grains of this salt placed in the solution from which it had crystallized, caused an increase of 34·3, and on a repetition of the experiment of 34·4, at a temperature of 60°.

			Sp. gr.
I.	$CuO, SO_3, HO + 2NH_3$, vol. of salt	68·6 ...	1·793
II.	... ...	68·8 ...	1·788
	Mean . .	68·7 ...	1·790

Sulphate of Copper and Sulphate of Potash, $CuO, SO_3 + KO, SO_3 + 6HO$ = 221·31.—The fourth of an equivalent (55·32 grains) dissolved in 3140 grains of water at 32°, increased to 16·3, and at 72° to 18·0.

$CuO, SO_3 + KO, SO_3 + 6HO$, vol. in solution 72·0

The same quantity of the salt placed in its saturated solution, caused an increase of 24·7 in one experiment and of 24·6 in a second, the temperature on both occasions being 55°.

			Sp. gr.
I.	$CuO, SO_3 + KO, SO_3 + 6HO$, vol. of salt	98·8 .	2·239
II.	... ...	98·4 .	2·249
	... ... Mean	98·6 . .	2·244

Sulphate of Copper and Sulphate of Ammonia, $CuO, SO_3 + NH_4O, SO_3 + 6HO$ = 199·88.—On dissolving 50 grains of this salt in 1000 grains of water, an increase was occasioned in the first experiment of 20·2, in the second of 20·3, both at a temperature of 59°.

I.	Sulphate of copper and ammonia, vol. in solution	80·8
II.	... ...	81·2
	Mean . .	81·0

On immersing the same quantity in a saturated solution, an increase of 26·4 was obtained in the first experiment, and of 26·45 in the second, both at a temperature of 59°.

		Sp. gr.
I. $CuO, SO_3 + NH_4O, SO_3 + 6HO$, vol. of salt	105·6 ..	1·892
II.	105·8 ..	1·889
Mean	105·7 ..	1·891

Sulphate of Zinc and Sulphate of Potash, $ZnO, SO_3 + KO, SO_3 + 6HO = 221·86$.—The fourth of an equivalent of this salt (55·46 grains), on being dissolved in 1000 grains of water, increased to 18 at a temperature of 60° in two experiments.

I. II. $ZnO, SO_3 + KO, SO_3 + 6HO$, vol. in solution 72.

The same quantity immersed in a saturated solution, caused an increase also in two experiments of 24·7, the temperature being 56°.

		Sp. gr.
I. II. $ZnO, SO_3 + KO, SO_3 + 6HO$, vol. of salt	98·8 ...	2·245

Sulphate of Zinc and Sulphate of Ammonia, $ZnO, SO_3 + NH_4O, SO_3 + 6HO = 200$.—On dissolving 45 grains of this salt in 1000 of water, an increase of 18·0 was occasioned in three separate experiments at a temperature of 58°.

I. II. III. $ZnO, SO_3 + NH_4O, SO_3 + 6HO$, vol. in solution 80

On adding the fourth of an equivalent (50 grains) to a saturated solution, an increase of 26·4 was occasioned in the first experiment, and of 26·3 in the second, both at a temperature of 55°.

		Sp. gr.
I. Sulph. zinc and ammonia, vol. of salt	105·6 ...	1·894
II.	105·2 ...	1·901
Mean . .	105·4 ...	1·897

Sulphate of Magnesia and Sulphate of Potash, $MgO, SO_3 + KO, SO_3 + 6HO = 202·29$.—When a quarter of an equivalent of this salt (50·57 grains) is dissolved in as many as 3140 grains of water, the volume at 32° is only 15·45, but is 18 at 80°. This gives for the salt in very dilute solution—

$MgO, SO_3 + KO, SO_3 + 6HO$, vol. in solution 63 at 40°
... ... 72 at 80°

The same quantity of salt after immersion in a saturated solution gave in the first experiment an increase of 24·3, and in the second of 24·4, both at a temperature of 57°.

		Sp. gr.
I. Sulph. magnesia and potash, vol. of salt	97·2 .	2·081
II.	97·6 .	2·071
... ... Mean . .	97·4 .	2·076

Sulphate of Magnesia and Ammonia, $MgO, SO_3 + NH_4O, SO_3 + 6HO = 181·12$.—The fourth of an equivalent (45·28 grains) being dissolved in 1000 grains of water, caused an increase of 20·0 at 60°; and a repetition of the experiment at the same temperature gave the increase 20·1.

I. Sulph. magnesia and ammonia, vol. in solution	80·0
II.	80·4
Mean . .	80·2

The same quantity of salt placed in a saturated solution, gave on two occasions a rise in the stem of 26·3 at a temperature of 60°.

		Sp. gr.
I. II. Sulph. magnesia and ammonia, vol. of salt	105·2	1·721

Sulphate of Iron and Potash, $FeO, SO_3 + KO, SO_3 + 6HO = 216·73$.—The eighth of an equivalent (27·09 grains), when dissolved in 1000 of water, caused an increase of 9 at a temperature of 65°.

Sulph. iron and potash, vol. in solution	72.

The same quantity immersed in a saturated solution occasioned a rise in the stem in two experiments 12·3 at a temperature of 61°.

		Sp. gr.
I. II. Sulph. iron and potash, vol. of salt	98·4	2·202

Sulphate of Iron and Ammonia, $FeO, SO_3 + NH_4O SO_3 + 6HO = 195·55$.—On dissolving 33·45 grains of this salt in 1000 of water, the increase in the first experiment was 13·4, in the second 14, both at a temperature of 59°; a third experiment with 66·9 grains, gave the increase 28 at the same temperature.

I. Sulph. iron and ammonia, vol. in solution	78·3
II.	81·8
III.	81·8
Mean . .	80·7

48·89 grains of the salt being projected into a saturated solution, caused in the first experiment an increase of 26·4, in the second of 26·5.

		Sp. gr.
I. Sulph. iron and ammonia, vol. of salt	105·6	1·851
II.	106·0	1·845
Mean . .	105·8	1·848

In the last section we gave the volumes occupied by those salts which did not occupy any space of themselves, but merely that due to their combined water. The divisor for the volumes observed in solution was therefore necessarily 9, or the atomic volume of water itself. But in this section we have experimented upon salts which take up space quite independent of their water of crystallization, even when they contain water, and yet the most interesting result follows, that the same divisor, 9, continues for the volumes ascertained by experiment. The volumes in solution of the salts examined, allowing for errors of observation, are therefore always multiples of 9,—the atomic volume of water. The volumes of the solids are, like those of the previous section, multiples of one and the same number, that number being also, as in the former case, 11; but the ammonia salts do not arrange themselves under this divisor for reasons which will be explained presently. The averages of the experiments on all the salts are thrown into the following table, into which is also introduced the exact numbers which would have resulted had there been a strict accordance with the law obviously indicated by experiment.

TABLE VI.—Showing the volumes of certain Sulphates with a small proportion of Water of Hydration, Anhydrous and Double Sulphates.

Designation.			Volume in solution.			Volume in state of salt.				
Name.	Formula.	Atomic weight.	Volume in solution by experiment.	9, or volume of water as unity.	Volume by theory.	Volume of salt by experiment.	11, or supposed vol. of combined water as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Sulphate of potash..	KO, SO_3	87·25	18·0	2	18	33·05	3	33	2·644	2·640
Bisulphate of potash.	KO, SO_3+HO, SO_3	136·35	36·05	4	36	55·1	5	55	2·479	2·475
Sulphate of ammonia	NH_4O, SO_3+HO	75·25	36·0	4	36	43·0	4	44	1·710	1·750
Bisulphate of ammonia............	NH_4O, SO_3+HO, SO_3	115·35	46·0	5	45	65·5	6	66	1·747	1·761
Bisulphate of soda...	NaO, SO_3+HO, SO_3	120·64	18·6	2	18	44·0	4	44	2·742	2·742
Ammoniacal sulphate of copper	$CuO, SO_3HO+2NH_3$	123·00	52·6	6	54	68·7	...	...	......	1·790
Sulphate of copper and potash......	$CuO, SO_3 + KOSO_3 + 6HO$	221·31	72·0	8	72	98·6	9	99	2·235	2·244
Sulphate of copper and ammonia...	$CuO, SO_3 + NH_4O, SO_3 + 6HO$	199·88	81·0	9	81	105·7	9	99	2·019	1·891
Sulphate of zinc and potash......	$ZnO, SO_3+KO, SO_3 + 6HO$	221·86	72·0	8	72	98·8	9	99	2·241	2·245
Sulphate of zinc and ammonia...	$ZnO,SO_3+NH_4O,SO_3 + 6HO$	200·00	80·0	9	81	105·4	9	99	2·020	1·897
Sulphate of magnesia and potash	$MgO, SO_3+KO, SO_3 + 6HO$	202·29	72·0	8	72	97·4	9	99	2·043	2·076
Sulphate of magnesia & ammonia	$MgO, SO_3 + NH_4O, SO_3 + 6HO$	181·12	80·2	9	81	105·2	9	99	1·829	1·721
Sulphate of iron and potash	$FeO, SO_3+KO, SO_3 + 6HO$	216·73	72·0	8	72	98·4	9	99	2·190	2·202
Sulphate of iron and ammonia...	$FeO,SO_3+NH_4O SO_3 + 6HO$	195·55	80·7	9	81	105·8	9	99	1·975	1·848
Sulphate of nickel and potash......	$NiO, SO_3+KO, SO_3 + 6HO$	218·99	71·5	8	72	100·0	9	99	2·212	2·190

The correspondence between the observed and calculated results in the preceding table, as far as regards the potash salts, is so striking as to remove any doubt of the basis upon which the calculations are made. It is therefore of interest to consider the results indicated by the table a little more in detail. The first point of remark is, that in every case the ammoniacal salt has one volume greater in solution than the corresponding potash salt. Sulphate of potash possesses two volumes in solution; sulphate of ammonia divested of one volume for its atom of water possesses three. These volumes are respectively carried through the whole class of double sulphates. The volumes of these double sulphates are made up of the sum of the volumes of their constituent salts, which appear, therefore, to be united unchanged. We saw in the previous section that the magnesian sulphates dissolve in water without increasing its bulk more than is due to their water of combination. The same takes place in their double sulphates, for subtracting the volumes of the atoms of water which have been carried by the sulphates into their union with sulphate of potash, the remainder shows the volumes belonging to the latter salts, as indicated by direct experiment. This is strikingly exemplified also by bisulphate of soda. Sulphate of soda was shown in the last section to possess no volume in solution, and in this acid salt we find that the sulphate of soda has in solution ceased to occupy space, for the resulting volume of the acid salt is only 18 or 9×2, which is the atomic volume of sulphate of water, as ascertained by the volume occupied by it in bisulphate of potash and bisulphate of ammonia, and as determined also by a calculation, which we have made, of the volume occupied by hydrated sulphuric acid in a *dilute* solution, founded upon recorded specific gravities.

Although the ammoniacal sulphates, on account of their analogy to the potash salts, have been introduced into the above table, it is obvious that the numbers representing their volumes are too wide from the theoretical numbers to be considered multiples of 11. Hydrated sulphate of ammonia affects four volumes, 11×4, but the anhydrous salt obeys a different law. On immersing in turpentine 33·15 grains of anhydrous NH_4O, SO_3, the increase was 19·6 and 19·5, the mean being 19·55 water-grain measures. This gives 39·1 as the vol. of the equivalent, and $9{\cdot}8 \times 4 = 39{\cdot}2$. Anhydrous sulphate of ammonia affects, therefore, 4 vol. of ice; and the double salts consist of the magnesian sulphates with 6 equivalents of water attached to an equivalent of anhydrous sulphate of ammonia, as will be seen from the following table of their solid volumes and specific gravities.

Name.	Solid volume by experiment.	Solid volume by theory.	Specific gravity by experiment.	Specific gravity by theory.
Sulphate of ammonia..................	39·1	39·2	1·695	1·691
Sulphate of copper and ammonia......	105·7	105·2	1·891	1·900
Sulphate of zinc and ammonia.........	105·4	105·2	1·887	1·901
Sulphate of magnesia and ammonia...	105·2	105·2	1·721	1·721
Sulphate of iron and ammonia.........	105·8	105·2	1·848	1·858

As one of the members of the group of double salts here described takes up no space of itself, it became of importance to ascertain the volume of the salt when deprived of water, and also the space occupied by the double salt reduced to the same state. In this examination it was quite unnecessary to obtain the volumes in solution, because it was obvious that salts not occupying in solution a greater volume than that due to their water of hydration, would, in their anhydrous condition, take up no space at all. In fact, we had ascertained that not only was there no increase in dissolving such salts in water, but that actually there was a contraction if the water were in large proportion to the salt; when this is not the case, the increased expansibility of the solution prevents the contraction being observed.

In the following examination will be found almost all the salts previously described in their hydrated condition, with the exception of the phosphates and arseniates, which we reserve for another paper.

Sulphate of Magnesia, $MgO, SO_3 = 60{\cdot}86$.—Half an equivalent of this salt, 30·43 grains, thrown into turpentine, caused an increase of 11·0; but in a second experiment the increase was 11·5, the temperature in both cases being 65°.

			Sp. gr.
MgO, SO_3, vol. of salt	22·0	...	2·766
......	23·0	...	2·646
Mean	22·5	...	2·706

Sulphate of Zinc, $ZnO, SO_3 = 80{\cdot}43$.—Half an equivalent of this salt, 40·22 grains, projected into turpentine, caused an increase of 11·05 and, in another experiment, of 10·8.

			Sp. gr.
ZnO, SO_3, vol. of salt	22·1	...	3·639
... ...	21·6	...	3·723
Mean	21·85	...	3·681

Sulphate of Copper, $CuO, SO_3 = 79{\cdot}88$.—Half an equivalent, 39·94 grains, of the salt placed in turpentine, caused in several successive experiments an increase of exactly 11·0.

			Sp. gr.
CuO, SO_3, vol. of salt	22·0	...	3·631

Sulphate of Iron, $FeO, SO_3 = 75·3$.—Half an equivalent of this salt, 37·65 grains, caused, in two experiments with the same salt, a rise of 12·0, which gives for the equivalent 24·0 and a specific gravity of 3·138.

Sulphate of Cobalt, $CoO, SO_3 = 77·69$.—On immersing 19·42 grains of this salt in turpentine, an increase of 5·5 was obtained in two experiments; this gives for the equivalent 22·0, and for the specific gravity 3·531.

Sulphate of Soda, $NaO, SO_3 = 71·43$.—On throwing a whole equivalent of this salt into turpentine, the increase was only 27·5 in several successive experiments, which gives for the specific gravity 2·597. Karsten found its specific gravity to be 2·631, a result approximating to our own; attention is drawn to this circumstance because both results are anomalous.

Sulphate of Silver, $AgO, SO_3 = 156·48$.—On immersing in turpentine 78·24 grains of this salt, the increase was 14·7, which gives as the volume of the equivalent 29·4, and a specific gravity of 5·322.

Chromate of Silver, $AgO, CrO_3 = 168·49$.—The fourth of an equivalent of this salt, 42·12 grains, gave an increase, when thrown into turpentine, of 7·3 in two successive experiments. This gives 29·2 for the volume of the equivalent, and 5·770 for the specific gravity of the salt.

Sulphate of Copper and Potash, $CuO, SO_3 + KO, SO_3 = 167·31$.—41·82 grains, the fourth of an equivalent, thrown into turpentine, caused an increase of 14·9 in one experiment and of 15·0 in another, the temperature in both cases being 54°.

			Sp. gr.
$CuO, SO_3 + KO, SO_3$, vol. of salt	59·6	...	2·807
... ...	60·0	...	2·788
Mean . .	59·8	...	2·797

Sulphate of Nickel and Potash, $NiO, SO_3 + KO, SO_3 = 164·99$.—41·54 grains caused an increase of 14·2 in one experiment and 14·5 in a second, the temperature in both cases being 54°.

			Sp. gr.
$NiO, SO_3 + KO, SO_3$, vol. of salt	56·4	...	2·925
... ...	57·5	...	28·69
Mean . .	56·95	...	2·897

Sulphate of Zinc and Potash, $ZnO, SO_3 + KO, SO_3 = 167·86$.—41·96 grains, the fourth of an equivalent, placed in turpentine, caused an increase of 14·9 in two experiments.

				Sp. gr.
$ZnO, SO_3 + KO, SO_3$, vol. of salt	59·6	...	2·816

Sulphate of Magnesia and Potash, $MgO, SO_3 + KO, SO_3 = 148{\cdot}29$.—37·07 grains, or the fourth of an equivalent, caused in one experiment an increase of 13·9, and in a second of 13·8, the temperature being 55°.

			Sp. gr.
$MgO, SO_3 + KO, SO_3$, vol. of salt	55·6	...	2·667
... ...	55·2	...	2·686
Mean . .	55·4	...	2·676

Sulphate of Manganese and Potash, $MnO, SO_3 + KO, SO_3 = 163{\cdot}07$.—40·8 grains of this salt, one-fourth of an equivalent, placed in turpentine, caused an increase of 13·5 in one experiment and 13·6 in another, at a temperature of 55°.

			Sp. gr.
$MnO, SO_3 + KO, SO_3$, vol. of salt	54·0	...	3·020
... ...	54·4	...	2·996
Mean . .	54·2	...	3·008

Sulphate of Copper and Ammonia, $CuO, SO_3 + NH_4O, SO_3 = 145{\cdot}88$.—36·53 grains of this salt thrown into turpentine, caused an increase of 16·7 in one experiment and 16·6 in another, at a temperature of 60°.

			Sp. gr.
I. Sulphate of copper and ammonia	66·6	...	2·190
II.	66·2	...	2·204
Mean . .	66·4	...	2·197

Sulphate of Zinc and Ammonia, $ZnO, SO_3 + NH_4O, SO_3 = 146{\cdot}0$.—30 grains of this salt thrown into turpentine, caused an increase of 13·5 at 60°.

			Sp. gr.
Sulphate of zinc and ammonia, vol. of salt	65·7	...	2·222

Sulphate of Magnesia and Ammonia, $MgO, SO_3 + NH_4O, SO_3 = 127{\cdot}12$.—The fourth of an equivalent (31·78 grains) placed in turpentine, caused an increase of 16·5 in the first experiment, and of 16·4 in the second.

			Sp. gr.
I. Sulphate of magnesia and ammonia	66·0	...	1·926
II	65·6	...	1·938
Mean . .	65·8	...	1·932

Sulphate of Alumina, $Al_2O_33SO_3 = 171{\cdot}95$.—This salt, and the anhydrous alums, offer difficulties to the correct estimation of their specific gravity on account of their great porosity and liability to carry down air. The best mode of obviating this source of error is to introduce a metallic wire previously moistened with turpentine into the volumenometer, and employ this to break the numerous air bubbles which arise on immersing the salts. The following estimations were taken with great care, but from this source of error may possibly be inaccurate.

The eighth part of an equivalent (21·49 grains), immersed in turpentine and treated as described above, gave results varying from 9·8 to 10·0, the mean result being 9·9.

		Sp. gr.
$Al_2O_3 3SO_3$, vol. of salt	79·2	2·171

Sulphate of Alumina and Potash, $Al_2O_3 3SO_3 + KO, SO_3$ = 259·36.—The eighth part of an equivalent (32·42 grains) of anhydrous alum, immersed in turpentine and treated as described in the case of sulphate of alumina, gave an increase of 14·5 and 14·6 in two experiments.

			Sp. gr.
I.	Alum, vol. of salt	116·0 ...	2·236
II.	... ...	116·8 ...	2·220
	Mean . .	116·4 ...	2·228

Ammonia Alum, $Al_2O_3 3SO_3 + NH_4O, SO_3$ = 238·2.—The eighth part of an equivalent of this salt (29·77 grains), treated as in the previous cases, gave an increase of 14·6 in two experiments.

		Sp. gr.
Ammonia alum, vol. of salt	116·8 ...	2·039

Carbonate of Soda, NaO, CO_2 = 53·47.—The equivalent of this salt, thrown into turpentine, gave an increase of exactly 22·0, which makes its specific gravity 2·430.

Chloride of Magnesium, MgCl = 48·12.—The anhydrous chloride of magnesium used in the experiment was made by saturating equal portions of muriatic acid with magnesia and ammonia, mixing together, evaporating to dryness, and heating to redness.

Half an equivalent (24·06 grains), thrown into turpentine, caused an increase of 11·0 in one experiment and of 11·1 in a second.

			Sp. gr.
I.	MgCl, vol. of salt	22·0 ...	2·187
II.	... ...	22·2 ...	2·167
	Mean . .	22·1 ...	2·177

Chloride of Calcium, CaCl = 55·92.—This salt was rendered anhydrous by fusing it in a platinum crucible for some time. 28 grains of the fused salt, thrown into turpentine, caused an increase of 11·3 at a temperature of 63°.

			Sp. gr.
$CaCl$, vol. of salt	22·5	...	2·480

Chloride of Cobalt, $CoCl = 65·0$.—On throwing the fourth of an equivalent (16·25 grains) of anhydrous chloride of cobalt into turpentine, an increase of 5·5 was obtained in two experiments, and of 5·6 in a third trial.

				Sp. gr.
I.	$CoCl$, vol. of salt	22·0	...	2·954
II.	... ...	22·0	...	2·954
III.	... ...	22·4	...	2·902
	Mean . .	22·13	...	2·937

TABLE VII.—Showing the volume occupied by certain Hydrated Salts rendered Anhydrous.

Designation.			Volumes of anhydrous salts.				
Name.	Formula.	Atomic weight.	Volume of salt by experiment.	11, taken as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Sulphate of magnesia	MgO, SO_3	60·86	22·5	2	22	2·766	2·706
Sulphate of zinc	ZnO, SO_3	80·43	21·85	2	22	3·656	3·681
Sulphate of copper...	CuO, SO_3	79·88	22·0	2	22	3·631	3·631
Sulphate of iron......	FeO, SO_3	75·3	24·0	2	22	3·423	3·138
Sulphate of cobalt ...	CoO, SO_3	77·69	22·0	2	22	3·531	3·531
Sulphate of soda......	... NaO, SO_3	71·43	27·5	...	...	...	2·597
Sulphate of silver ...	AgO, SO_3	156·48	29·4	...	...	...	5·322
Chromate of silver...	AgO, CrO_3	168·49	29·2	...	...	...	5·770
Sulphate of alumina .	$Al_2 O_3 3SO_3$	171·95	79·2	...	...	...	2·171
Sulphate of copper and potash	$CuO, SO_3 + KO, SO_3$	167·31	59·8	5	55	3·042	2·797
Sulphate of nickel and potash	$NiO, SO_3 + KO, SO_3$	164·99	56·95	5	55	2·998	2·897
Sulphate of zinc and potash	$ZnO, SO_3 + KO, SO_3$	167·86	59·6	5	55	3·034	2·816
Sulphate of magnesia and potash	$MgO, SO_3 + KO, SO_3$	148·29	55·4	5	55	2·694	2·676
Sulphate of manganese and potash.	$MnO, SO_3 + KO, SO_3$	163·07	54·2	5	55	2·964	3·008
Sulphate of copper and ammonia ...	$CuO, SO_3 + NH_4O, SO_3$	145·88	66·4	6	66	2·192	2·197
Sulphate of zinc and ammonia ...	$ZnO, SO_3 + NH_4O, SO_3$	146·0	65·7	6	66	2·212	2·222
Sulphate of magnesia and ammonia	$MgO, SO_3 + NH_4O, SO_3$	127·0	65·8	6	66	1·924	1·932
Potash alum	$Al_2 O_3 3SO_3 + KO, SO_3$	259·36	116·4	...	...	...	2·228
Ammonia alum	$Al_2 O_3, 3SO_3 + NH_4O\ SO_3$	238·2	116·8	...	...	...	2·039
Carbonate of soda ...	NaO, CO_2	53·47	22·0	2	22	2·427	2·427
Chloride of cobalt ...	Co, Cl	65·0	22·1	2	22	2·955	2·937
Chloride of magnesium	Mg, Cl	48·12	22·1	2	22	2·187	2·177
Chloride of calcium .	Ca, Cl	55·92	22·5	2	22	2·542	2·485

The preceding table exhibits various points of great interest as regards isomorphism. Hydrogen has for a long time been recognised by chemists as equivalent to a magnesian metal; and hence the sulphate of a metal of this class should possess the volume of sulphate of water. The volume of bisulphate of potash is 55·0 by experiment, which leaves 22·0 for that of sulphate of water, on deducting the volume of sulphate of potash, which is 33·0; and the same result follows when the volume of sulphuric acid is deduced from bisulphate of soda, if we suppose the sulphate of soda to enter that salt with two volumes. Thus we have—

Sulphate of water . . . . $= 22 \div 11 = 2$
Sulphate of a magnesian oxide $= 22 \div 11 = 2$.

We now see that bisulphate of potash (sulphate of water and sulphate of potash) is exactly equivalent to the double sulphates of the magnesian class. (*Vide* Section V.)

Bisulphate of potash ($HO, SO_3 + KO, SO_3$) $= 55$
Sulphate of magnesia and potash ($MgO, SO_3 + KO, SO_3$) $= 55$.

It is now comprehensible why bisulphate of soda should have a volume of 44·0 in the solid state and only of 18·0 in a state of solution; because sulphate of soda which assumes a volume in the solid state becomes added to the same volume possessed by sulphate of water, while in the state of solution the proper volume of sulphate of soda disappears altogether.

Bisulphate of ammonia possesses a volume due to a combination of sulphate of water and sulphate of ammonia, with a volume of 11 × 4, and it will be observed that the same result attends the double sulphates of the magnesian metals with sulphate of ammonia.

Bisulphate of ammonia ($NH_4O, SO_3 + HO\ SO_3$) $= 66$
Sulphate of ammonia and copper ($NH_4O, SO_3 + CuO, SO_3$) $= 66$.

The cause of this singular result is in the mutual convertibility of the primitive volumes 9·8 and 11.

It is very curious to observe the large number of volumes which have disappeared when the salt combines with water. Thus sulphate of alumina in its anhydrous state possesses a bulk equal to 79·2, which has ceased to occupy space in the hydrated salt; and still more remarkable instances of this are seen in the alums, which add to this the volumes of their alkaline sulphates. A curious result obtained in the examination of the hydrated alums is now explicable. We found that the potash alums took up in solution only the space due to their water; but that the space occupied by them in the state of salts was one volume in addition to this quantity. In the preceding section we observed that sulphate of potash possessed the singular property of expanding one volume in becoming solid; 9×2 in a state of solution becoming 11×3 in the state of a salt. It is impossible to refrain from accepting this as an explanation of the increase of one in the quotient obtained by dividing the volumes by their proper numbers 9 and 11—24×9 becoming 25×11.

The difficulties, to which we have already alluded, prevent us placing much confidence in our results for the anhydrous alums. Sulphate of alumina seems to affect eight volumes of ice, $9·8 \times 8 = 78·4$; in ammonia alum the latter becomes united to the volume of anhydrous sulphate of ammonia, $9·8 \times 8 + 9·8 \times 4 = 117·6$; while potash alum should consist of $9·8 \times 8 + 11 \times 3 = 111·4$. It is unnecessary to remark that these theoretical numbers possess only an approximation to our experimental results. (*Vide* remarks on Section V.)

The sulphates of soda and silver and the corresponding chromate are also obviously exceptions to the general rule of the solid volume being multiples of 11. But in the last section we had similar exceptions in salts which ranged themselves under 9·8 or the volume of ice. The sulphates now under consideration have the same divisor, if sulphate of soda be not considered an exception, as the variation is decidedly too great to be attributed to a mere error of experiment; it ought however to be observed that Mohs gives for the specific gravity of this salt 2·462, a number much more in accordance with theory than our own result; but as our experiments have been often repeated, they may perhaps be viewed as an argument in favour of an opinion, deduced from other considerations, that sulphate of soda has a double atom $27·5 \times 2 = 55$, which is 11×5.

Name.	Volume by experiment.	9·8, or volume of ice as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Sulphate of soda ……	27·5	3	29·4	2·430	2·597
Sulphate of silver ……	29·4	3	29·4	5·322	5·322
Chromate of silver……	29·2	3	29·4	5·711	5·770

SECTION III.

Nitrates, &c.

The nitrates do not in general affect a large proportion of water of hydration, and are therefore well-calculated to show the volume occupied by anhydrous salts. It will be observed that they present some peculiarities.

Nitrate of Potash, $KO, NO_5 = 101{\cdot}3$.—The half of an equivalent of this salt, 50·65 grains, being dissolved in 1000 grs. of water, gave an increase of 18·05 at 45°.

KO, NO_5, vol. in solution 36·1

The same quantity of salt, 50·65 grains, thrown into turpentine, caused a rise in the stem of 24·5, 24·4, 24·5 in three successive experiments.

			Sp. gr.
I. KO, NO_5, vol. of salt	49·0	...	2·067
II.	48·8	...	2·075
III.	49·0	...	2·067
Mean . .	48·9	...	2·070

Nitrate of Ammonia, $NH_4O, NO_5 = 80{\cdot}3$.—The volume of nitrate of ammonia in solution was determined by dissolving 40·15 grains of this salt in 1000 grains of water. In one experiment the increase in the stem was 22·5, the temperature being 57°; in a second the rise was 23·0 at 63°.

I. NH_4O, NO_5, vol. in solution	45·0
II.	46·0
Mean . .	45·5

Half an equivalent of this salt, well-dried (40·15 grains), on being immersed in turpentine produced an increase in three experiments of 24·7, 24·5, 24·5.

			Sp. gr.
I. NH_4O, NO_5, vol. of salt	49·4	...	1·625
II.	49·0	...	1·639
III.	49·0	...	1·639
Mean . .	49·1	...	1·635

Nitrate of Soda, $NaO, NO_5 = 85{\cdot}45$.—On dissolving 85·45 grains, or one equivalent, of this salt in 1000 grains of water, an increase of 27·1 was obtained, the temperature being 59°; but on repetition of the experiment at the same temperature the increase was only 26·0.

I. NaO, NO_5, vol. in solution	27·1
II.	26·0
Mean . .	26·5

The half of an equivalent of this salt, 42·72 grains, well-dried, produced an increase on being thrown into turpentine of 19·6 in three experiments and 19·5 in a fourth trial.

			Sp. gr.
I.	NaO, NO_5 vol. of salt	39·2	2·180
II.	... ...	39·2	2·180
III.	... ...	39·2	2·180
IV.	... ...	39·0	2·190
	Mean	39·1	2·182

Nitrate of Silver, AgO, NO_5 = 170·0.—On dissolving 42·5 grains of this salt in 1000 grains of water, an increase of 6·8 was effected at a temperature of 59°.

AgO, NO_5, vol. in solution 27·2.

The same quantity of salt, 4·25 grains, thrown into turpentine, produced an increase of 9·8.

		Sp. gr.
AgO, No_5 vol. of salt	39·2	4·336

Nitrate of Lead, PbO, NO_5 = 165·75.—This salt gives very unsatisfactory results on being dissolved in water; at low temperatures the volume for the atom is equal to nearly 18·0, or 9 × 2. But at higher temperatures the volume in solution approaches nearly to 27·0, or 9 × 3; and, although the results do not come out exact, unless corrected for expansion, we are inclined to view the latter as the true result. 83 grs. dissolved in water gave an increase of 12·5; in a second experiment of 12·7, both at a temperature of 65°.

PbO, NO_5, vol. in solution	25·0
... ...	25·4
Mean	25·2

The fourth part of an equivalent, 41·43 grains, immersed in turpentine, gave an increase of 9·7; 82·87 grains gave the increase 19·2; and in a third experiment 19·0.

			Sp. gr.
I.	PbO, NO_5, vol. of salt	38·8	4·272
II.	... ...	38·4	4·316
III.	... ...	38·0	4·362
	Mean	38·4	4·316

Nitrate of Barytes, BaO, NO_5 = 130·85.—Half an equivalent of this salt, 65·42 grains, dissolved in 1000 grains of water with an increase of 13·5 at a temperature of 60°; and a repetition of the experiment was attended with the same result.

BaO, NO_5, vol. in solution 27·0.

The same quantity of salt, immersed in turpentine, caused an increase of 19·8 in three experiments, and 20·0 and 20·2 in two other experiments; the salts being all different specimens, and decrepitated previously to the experiment.

				Sp. gr.
I.	BaO, NO_5, vol. of salt	39·6	...	3·304
II.	... ...	39·6	...	3·304
III.	... ...	39·6	...	3·304
IV.	... ...	40·0	...	3·271
V.	... ...	40·4	...	3·238
	Mean . .	39·84	...	3·284

Nitrate of Strontia = 106·0.—Half an equivalent of this salt, 53 grains, was dissolved in 1000 grains of water, with an increase of 13·0, the temperature being 62°; 106 grains dissolved in 1000 grains of water, with an increase of 27·0 at a temperature of 63°.

I.	SrO, NO_5, vol. in solution	26·0
II.	... ...	27·0
	Mean . .	26·5

53 grains immersed in turpentine gave an increase of 19·6; and this result was confirmed by a second experiment.

			Sp. gr.
SrO, NO_5, vol. of salt	39·2	...	2·704

Nitrate of Black Oxide of Mercury, $Hg_2O, NO_5 + 2HO$ = 282·0.—This salt, in beautiful large transparent crystals, was dissolved in water containing nitric acid, to prevent the formation of a subsalt; 70·5 grains thus treated caused an increase of 13·5.

Protonitrate of mercury, vol. in solution 54·0

On immersing the same quantity of salt in turpentine, the increase in three experiments was 14·8, 14·7, and 14·7.

				Sp. gr.
I.	Protonitrate of mercury, vol. of salt	59·2	...	4·763
II.	... ...	58·8	...	4·796
III.	... ...	58·8	...	4·796
	Mean . .	58·9	...	4·785

Nitrate of Copper, $CuO, NO_5 + 3HO = 120·8$.—Half an equivalent (60·4 grains) dissolved in 1000 grains of water with an increase of 22·4 at 60°, and in a second experiment of 22·6; in a third experiment, 30·2 grains, dissolved in the same quantity of water, gave an increase of 11·4.

I.	CuO, NO_5, vol. in solution	44·8
II.	... ...	45·2
III.	... ...	45·6
	Mean . .	45·2

In two experiments, 60·4 grains thrown into turpentine caused an increase of 29·5, which gives for an equivalent of the salt the volume 59·0, and a specific gravity 2·047.

Nitrate of Magnesia, $MgO, NO_5 + 6HO = 128·8$.—The fourth part of an equivalent of crystallized nitrate of magnesia (32·2 grains) dissolved in 1000 grains of water at 60°, with an increase of 18·1 and 18·3 in two experiments.

I.	$MgO, NO_5 + 6HO$, vol. in solution	73·2
II.	... ...	72·4
	Mean . .	72·8

The same quantity thrown into turpentine produced an increase of 22·0, which gives for the volume of an equivalent of the salt 88·0, and for its specific gravity 1·464.

Nitrate of Bismuth, $BiO, NO_5 + 3HO = 160·33$.—This salt, being decomposed when thrown into water, is not fitted for determining volume by solution; but when 80·16 grains were thrown into turpentine, the increase was obtained in two experiments of 29·2 and of 29·4.

			Sp. gr.
I.	$BiO, NO_5 + 3HO$, vol. of salt	58·4	2·745
II.	... ...	58·8	2·727
	Mean . . .	58·6	2·736

Basic Nitrate of Mercury, $2HgO, NO_5 + 2HO = 291·0$. —This salt cannot be dissolved in water without the formation of a subsalt, unless the water is used in small proportion; it is therefore unfitted for our experiments, as far as regards the volume in solution. On immersing 68·7 grains in turpentine, an increase of 16·2 was obtained in two successive experiments. This gives 68·6 as the volume of the equivalent, and a specific gravity of 4·242.

Basic Nitrate of Lead, $2PbO, NO_5 = 277·72$.—This salt is so insoluble, that it is difficult to determine its volume in solution with any great degree of accuracy. The sixteenth part of an equivalent dissolved in 1000 grains of water gave an increase of 2·6, which seems to indicate a volume of 9 × 5.

69·43 grains, being immersed in turpentine, gave an increase of 12·3 in several experiments.

		Sp. gr.
Basic nitrate of lead, vol. of salt	49·2	5·645

The same multiple relation of 9 is carried through all the salts of this class dissolved in water. The divisor for the solid volume is, however, different from the salts of the previous sections. Exceptional cases were pointed out in their examination, in which 9·8, or the volume of ice, became the divisor; and in the present group of salts we observe a wonderful uniformity in this respect.

TABLE VIII.—Showing the volumes occupied by certain Nitrates.

Designation.			Volume in solution.			Volume in state of salt.				
Name.	Formula.	Atomic weight.	Volume by experiment.	9 taken as unity.	Volume by theory.	Volume of salt by experiment.	9·8, or volume of ice as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Nitrate of potash...	KO, NO_5	101·3	36·1	4	36	48·9	5	49·0	2·067	2·070
Nitrate of ammonia	NH_4O, NO_5	80·3	45·5	5	45	49·1	5	49·0	1·639	1·635
Nitrate of soda......	NaO, NO_5	85·45	26·5	3	27	39·1	4	39·2	2·180	2·182
Nitrate of silver	AgO, NO_5	170·0	27·2	3	27	39·2	4	39·2	4·336	4·336
Nitrate of lead......	PbO, NO_5	165·75	25·4	3	27	38·4	4	39·2	4·228	4·316
Nitrate of barytes ..	BaO, NO_5	130·85	27·0	3	27	39·8	4	39·2	3·338	3·284
Nitrate of strontia ..	SrO, NO_5	106·0	26·5	3	27	39·2	4	39·2	2·704	2·704
Nitrate of black oxide of mercury	Hg_2O, NO_5+2HO	282·0	54·0	6	54	58·9	6	58·8	4·796	4·785
Basic nitrate of mercury........	$2HgO, NO_5+2HO$	291·0	...	...	...	68·6	7	68·6	4·242	4·242
Basic nitrate of lead	$2PbO, NO_5$	277·72	...	...	....	49·2	5	49·0	5·667	5·645
Nitrate of bismuth .	BiO, NO_5+3HO	160·33	...	...	...	58·6	6	58·8	2·727	2·736
Nitrate of copper...	CuO, NO_5+3HO	120·8	45·2	5	45	59·0	6	58·8	2·054	2·047
Nitrate of magnesia	MgO, NO_5+6HO	128·8	72·8	8	72	88·0	9	88·2	1·460	1·464

It is almost superfluous to offer any remarks upon this group of salts, especially as we shall have to consider several of them in a future section. It cannot escape attention, that the nitrates of soda, silver, lead, strontia and barytes possess the same atomic volume, as might have been expected from the isomorphism of several of them. Nitrates of soda and potash do not possess the same atomic volume, and therefore their alleged isomorphism, deduced from the observation by Frankenheim* of microscopic crystals of nitrate of potash similar to those of nitrate of soda, is highly questionable. The principal exception to the volumes of the nitrates now described being multiples of ice is that of nitrate of lead, which has a volume of 38·4, instead of 39·2; but this must be due to the nature of the salt, which comes out as unsatisfactorily in a state of solution as in the solid state.

SECTION IV.

Chlorides, Bromides and Iodides.

Chloride of Potassium, $KCl = 74·7$.—On dissolving 37·5 grains of this salt in 1000 grains of water, the increase was 13·3 at a temperature of 57°; a second experiment with the

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* Poggendorff's *Ann.*, vol. xl. p. 447.

same quantity gave the increase 13·5 at 58°; and a third experiment gave 13·7 at 65°.

I.	KCl, vol. in solution	26·5
II.	... ...	26·8
III.	... ...	27·2
	Mean . .	26·8

The whole of an equivalent thrown into turpentine (the salt having been decrepitated) increased 39·6, and 39·3 in two experiments. Half an equivalent, 37·35 grains, caused a rise in the stem of 19·6 in two experiments.

				Sp. gr.
I.	KCl, vol. of salt	39·6	...	1·887
II.	... ...	39·3	...	1·900
III.	... ...	39·2	...	1·905
IV.	... ...	39·2	...	1·905
	Mean . .	39·3	...	1·900

Chloride of Ammonium, $NH_4Cl = 53·66$.—Half an equivalent, 26·83 grains, dissolved in 1000 grains of water with an increase of 17·5 at a temperature of 60°; in two other experiments at 63°, the increase was 18·0.

I.	NH_4Cl, vol. in solution	35·0
II.	... ...	36·0
III.	... ...	36·0
	Mean . .	35·7

Our experiments on the specific gravity of this salt gave 1·578 as an uniform result, indicating a volume of 34·0.

Bromide of Potassium, KBr = 117·6.—The fourth part of an equivalent, 29·4 grains, on being dissolved in water at 49°, gave in two experiments an increase of 7·2; which gives for the volume of the salt in solution 28·8. The same quantity of salt, immersed in turpentine at 63°, caused an increase of 11·0 in two experiments.

			Sp. gr.
KBr, vol. of salt	44·0	...	2·672

Iodide of Potassium, KI = 165·82.—This salt was decrepitated, and on dissolving 41·5 grains gave an increase of 11·0 at 57°; a second experiment with 83 grains gave an increase of 22·0 at 55°.

I.	KI, vol. in solution	44
II.	... ...	44
	Mean . .	44

On projecting 41·45 grains of this salt, previously decrepitated, into turpentine, an increase of 13·6 and 13·5 was produced in two successive experiments.

			Sp. gr.
I.	KI, vol. of salt	54·4 ...	3·048
II.	... ...	54·0 ...	3·070
...	Mean . .	54·2 ...	3·059

Chloride of Sodium, $\mathrm{NaCl}=58{\cdot}78$.—The whole of an equivalent of this salt, previously decrepitated, dissolved in 1000 grains of water at 60° with a rise of 18·0; and in a second experiment of 18·2; in a third experiment 118 grains of salt were dissolved in 1000 grains of water at 62° with an increase of 38·0.

I.	NaCl, vol. in solution	18·0
II.	...	18·2
III.	...	18·9
	Mean . .	18·3

80 grains of salt were treated as described (page 457), in the mercurial volumenometer, and the empty part of the tube, after the restoration of the mercury, showed a volume of 40·0. The same quantity thrown into alcohol previously saturated with it, gave an increase of 39·5. The whole of an equivalent, 58·78 grains, thrown into a saturated solution, caused an increase of 29·3.

			Sp. gr.
I.	NaCl, vol. of salt	29·4 ...	2·000
II.	...	29·0 ...	2·026
III.	...	29·3 ...	2·006
	Mean . .	29·23 ...	2·011

Bromide of Sodium, $\mathrm{NaBr}+3\mathrm{HO}=128{\cdot}70$.—On dissolving 25·7 grains of this salt in water, an increase of 9·2 was occasioned in two experiments at a temperature of 53°.

I. II. $\mathrm{NaBr}+3\mathrm{HO}$, vol. in solution 46

The same quantity of salt put into turpentine caused an increase of 11.

$\mathrm{NaBr}+3\mathrm{HO}$, vol. of salt 55 ... 2·340

Chloride of Barium, $\mathrm{BaCl}+2\mathrm{HO}=122{\cdot}83$.—30·7 grains dissolved in 1000 grains of water, increased 7·0 at a temperature of 58°; a second experiment, in which 20 grains of the salt were dissolved, gave an increase of 4·5.

I.	$\mathrm{BaCl}+2\mathrm{HO}$ vol. in solution	28·0
II.	... ...	27·6
	Mean . .	27·8

The fourth of an equivalent, 30·7 grains, being immersed in a saturated solution, gave an increase of 9·7 at a temperature of 60°; and the same quantity in two other experiments gave an increase of 9·8.

				Sp. gr.
I.	$BaCl + 2HO$ vol. of salt	38·8	...	3·166
II.	... ...	39·2	...	3·133
III.	... ...	39·2	...	3·133
	Mean . .	39·07	...	3·144

Perchloride of Mercury, $HgCl = 136·9$.—The fourth of an equivalent, 34·2 grains, of corrosive sublimate, on being dissolved in 1000 grains of water, gave an increase of 4·6 at a temperature of 62°; a second experiment with the same quantity was attended with the same result.

I. II. $HgCl$, vol. in solution 18·4

Half an equivalent (68·45 grains), thrown into a saturated solution of the salt, caused an increase of 11·0 at a temperature of 56°.

				Sp. gr.
I.	$HgCl$. vol. of salt	22	...	6·223

Chloride of Hydrogen, $HCl = 36·47$.—It was of interest to ascertain the volume of hydrochloric acid, in order to compare it with the other chlorides of the magnesian metals when dissolved in water. It was natural to expect that the volume of muriatic acid in dilute solutions would be different from that possessed by it in its concentrated state; and therefore the following experiments must be viewed in this light. Peligot's salt, the bichromate of the chloride of potassium, on dissolving in water, was decomposed into bichromate of potash and muriatic acid, and the volume of the latter was obtained by deducting that due to the former salt and adding the volume of water. The fourth part of an equivalent of this salt, 44·75 grains, dissolved in 1000 grains of water with an increase of 13·5 at 65°; and of 13·6 in another experiment at 68°. This result gives for the whole volume of the salt when dissolved 54·0 and 54·4, from which must be deducted 45·0 for the volume of bichromate of potash, and 9 must be added on account of the equivalent of water.

I.	Muriatic acid, in dilute solutions	18·0
II.	... ...	18·4
	Mean . .	18·2

Chloride of Copper, $CuCl + 2HO = 85·18$.—Half an equivalent, 42·6 grains, was dissolved in 1000 grains of water with an increase of 13·4 at a temperature of 60°; on a second experiment, 47 grains occasioned an increase of 14·0 at a temperature of 58°.

I.	$CuCl + 2HO$, vol. in solution	26·8
II.	... ...	25·4
	Mean . .	26·1

Half an equivalent, 42·6 grains, being immersed in a saturated solution at 62°, caused an increase of 17·0; a second experiment with the same quantity of salt gave an increase of 16·6.

			Sp. gr.
I. $CuCl + 2HO$, vol. of salt	33·2	...	2·566
II.	34·0	...	2·505
Mean . .	33·6	...	2·535

Chloride of Copper and Ammonium, $CuCl + NH_4Cl + 2HO = 138·84$.—34·7 grains of this salt, being dissolved in 1000 grains of water, gave an increase of 15·5 in the first experiment and of 15·4 in the second, both at a temperature of 68°.

I. $CuCl + NH_4Cl + 2HO$, vol. in solution	62·0
II.	61·6
Mean . .	61·8

32·46 grains, thrown into a saturated solution, caused an increase of 16·1 in two experiments at a temperature of 60°, and a repetition of the experiment confirmed this result.

Chloride of copper and ammonium, vol. of salt 68·8 ... 2·018

Chloride of Copper and Potassium, $CuCl + KCl + 2HO = 159·88$.—34·7 grains of this salt, being dissolved in 1000 grains of water, caused an increase of 11·5 at 62°.

$CuCl + KCl + 2HO$, vol. in solution 53·0

The same quantity (34·7 grains), thrown into a saturated solution, caused an increase of 14·3.

$CuCl + KCl + 2HO$, vol of salt 65·9 ... 2·426

Chloride of Tin, $SnCl + 3HO = 121·39$.—One-fourth of an equivalent (30·35 grains) was dissolved in 1000 grains of water, acidulated with muriatic acid, with an increase of 9·0 at a temperature of 60°; a second experiment, with the same quantity of salt and at the same temperature, gave an increase of 9·2.

I. $SnCl + 3HO$, vol. in solution	36·0
II.	36·8
Mean . .	36·4

The same quantity, 30·35 grains, of the salt being immersed in a saturated solution, yielded an increase of 11·0, the temperature being 60°; and exactly the same result attended the repetition of the experiment.

			Sp. gr.
$SnCl + 3HO$, vol. of salt	44·0	...	2·759

Chloride of Tin and Ammonium, $SnCl + NH_4Cl + 3HO = 175·05$.—On dissolving 44 grains of this salt in 1000 grains of water, the increase was 18·3 at a temperature of 60°; a second experiment with the same quantity and at the same temperature, gave an increase of 18·5.

I. Chloride of tin and ammonium, vol. in solution	72·7
II.	73·5
Mean . .	73·1

On immersing 43·76 grains of the salt in a saturated solution, an increase of 20·8 was obtained at a temperature of 60°, which gives 83·2 as the volume of the equivalent, and 2·104 as the specific gravity of the salt.

Chloride of Tin and Potassium, $SnCl + KCl + 3HO = 196{\cdot}09$.—On dissolving 24·3 grains of the salt in 1000 grains of water, an increase of 8·0 was obtained at a temperature of 60°; and 48·5 grains, dissolved in the same quantity of water, gave an increase of 15·5.

I. Chloride of tin and potassium, vol. in solution	64·5
II.	62·7
Mean . .	63·6

On throwing the fourth part of an equivalent, 49 grains, into a saturated solution, an increase of 19·5 was obtained at a temperature of 54°.

		Sp. gr.
$SnCl + KCl + 3HO$, vol. of salt	78·0 ...	2·514

A. *Chloride of Mercury and Ammonium*, $HgCl + NH_4Cl + HO = 199{\cdot}8$.—On dissolving 49·95 grains of this salt in 1000 grains of water, an increase was obtained of 16·0, and in a second experiment of 16·2, the temperature being about 60° in both cases.

I. A. Chloride of mercury and ammonium, vol. in sol.	64·0
II.	64·8
Mean . .	64·4

The same quantity of salt, thrown into a saturated solution at 60°, occasioned an increase of 17·0 in two experiments, which makes the volume of the equivalent 68·0, and the specific gravity 2·938.

B. *Chloride of Mercury and Ammonium*, $NH_4Cl + 2HgCl + HO = 336{\cdot}4$.—On dissolving 42 grains of this salt in 1000 grains of water, an increase of 10·1 was occasioned in two experiments at 54°, and of 10·2 in a third experiment at 60°.

I. II. $NH_4Cl + 2HgCl + HO$, vol. in solution	80·9
III.	81·6
Mean . .	81·2

42 grains, or one-eighth of an equivalent, thrown into a saturated solution of the salt, caused a rise in the stem of 11 in two experiments.

			Sp. gr.
I. II. B. Chloride of mercury and ammonium, vol. of salt .	88·0	...	3·822.

Chloride of Mercury and Potassium, $KCl + 2HgCl + 2HO = 366{\cdot}5$.—The eighth part of an equivalent, 45·8 grains, being dissolved in 1000 grains of water, caused in two experiments an increase of 10·1 at a temperature of 53°.

I. II. Chloride of mercury and potassium, vol. in solution	80·8.

The same quantity of salt, 45·8 grains, thrown into a saturated solution, caused an increase of 12·0 in one experiment and of 12·4 in two other trials, the temperature in all the cases being 58°.

				Sp. gr.
I. Chloride of mercury and potassium, vol. of salt		96·0	...	3·818
II. ...	...	99·2	...	3·694
III. ...	...	99·2	...	3·694
	Mean . .	98·1	...	3·735

Chloride of Mercury and Sodium, $NaCl + 2HgCl + 4HO = 368{\cdot}5$.—On dissolving 46·06 grains of this salt in 1000 grains of water, the increase was 12·4 at 63°. This gives for the equivalent a volume of 99·2, or 11 equivalents. The same quantity of salt thrown into turpentine produced an increase of 15·3, which gives for the equivalent 122·4, and for the specific gravity 3·011.

A careful consideration of the previous experiments shows that there are two distinct classes of chlorides, &c. The first of these is placed in Table IX., and possesses 11 as the divisor of the solid.

TABLE IX.

Designation.			Vol. in solution.			Volume of solid.				
Name.	Formula.	Atomic weight.	Volume by experiment.	9, taken as unity.	Volume by theory.	Volume by experiment.	11, taken as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Bromide of potassium .	KBr	117·6	28·8	3	27	44·0	4	44	2·672	2·672
Chloride of ammonium.	NH_4Cl	53·66	35·7	4	36	34·0	3	33	1·626	1·578
Iodide of potassium ...	KI	165·82	44·0	5	45	54·2	5	55	3·015	3·059
Bromide of sodium ...	$NaBr+3HO$	128·70	46·0	5	45	55·0	5	55	2·340	2·340
Chloride of mercury ...	$HgCl$	136·9	18·4	2	18	22·0	2	22	6·223	6·223
Chloride of hydrogen...	HCl	36·47	18·2	2	18	.....	...	...	......	......
Chloride of copper......	$CuCl+2HO$	85·18	26·1	3	27	33·6	3	33	2·581	2·534
Chloride of tin	$SnCl+3HO$	121·39	36·4	4	36	44·0	4	44	2·759	2·759
Chloride of mercury and sodium	$2HgCl + NaCl + 4HO$	368·5	99·2	11	99	122·4	11	121	3·045	3·011

In the second class (Table IX. A.) the primitive volume is 9·8, or, as in the case of the double chlorides of tin, the metallic salt enters into combination with the volume 11; while $\mathrm{NH_4Cl}$ remains a multiple of 9·8. It is interesting to observe that $\mathrm{NH_4Cl}$ affects in combination as a solid the same number of volumes which it has as a liquid.

TABLE IX. A.—Showing the Volumes in solution and in the solid state of certain Chlorides.

Designation.			Vol. in solution.			Volume of salt.				
Name.	Formula.	Atomic weight.	Volume by experiment.	9, or volume of water as unity.	Volume by theory.	Volume of salt by experiment.	9·8, or volume of ice as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Chloride of potassium .	$\mathrm{K\,Cl}$	74·7	26·8	3	27	39·3	4	39·2	1·905	1·900
Chloride of sodium......	$\mathrm{Na\,Cl}$	58·78	18·3	2	18	29·2	3	29·4	2·000	2·011
Chloride of barium ...	$\mathrm{Ba\,Cl+2HO}$	122·83	27·8	3	27	39·07	4	39·2	3·133	3·144
Chloride of copper and potassium	$\mathrm{Cu\,Cl+KCl+2HO}$	159·88	53·0	6	54	65·9	7	68·6	2·331	2·426
Chloride of copper and ammonium ...	$\mathrm{Cu\,Cl+NH_4\,Cl+2HO}$	138·84	61·8	7	63	68·8	7	68·6	2·024	2·018
Chloride of tin and ammonium	$\mathrm{Sn\,Cl+NH_4\,Cl+3HO}$	175·05	73·1	8	72	83·2	8	83·2	2·104	2·104
Chloride of tin and potassium	$\mathrm{Sn\,Cl+KCl+3HO}$	196·09	63·6	7	63	78·0	8	78·4	2·501	2·514
A. Chloride of mercury and ammonium	$\mathrm{Hg\,Cl+NH_4\,Cl+HO}$	199·8	64·4	7	63	68	7	68·6	2·912	2·938
B. Chloride of mercury and ammonium	$\mathrm{2Hg\,Cl+NH_4\,Cl+HO}$	336·4	81·2	9	81	88	9	88·2	3·814	3·822
Chloride of mercury and potassium......	$\mathrm{2Hg\,Cl+KCl+2HO}$	366·5	80·8	9	81	98·1	10	98·0	3·739	3·736

The results of the experiments detailed in this section afford strong proofs of the law of multiple proportions, and exhibit at the same time that remarkable alteration of the divisor of the solid volumes which we have already noticed so frequently. Thus, while many of the chlorides and bromides are multiples of 11, we have decided exceptions in chlorides of potassium and sodium, which possess for their divisor the volume of ice, viz. 9·8; and this reappears in the double salts.

It is impossible, however, not to see that these results are somewhat singular, for in the double salts the chloride of potassium forces the double salt with which it is associated to assume the multiples of 9·8, and then exhibits its natural isomorphous relation to chloride of ammonium, which *per se* it did not possess. Chloride of ammonium, anomalous, in being a multiple of 11 in the solid state, assumes four volumes, multiples of 9·8, in the double chlorides, and then presents the same number for its solid volume as chloride of potassium. The isomorphism of potassium and sodium is so entirely hypothetical, that it will not excite surprise to find the volumes of the chlorides so different. We were less prepared to detect the difference between iodide and chloride of potassium; but have confirmed it by an examination of iodide of ammonium, 50 grains of which dissolved in 1000 of water with an increase of 18·7, which gives $\frac{54 \cdot 1}{9}$ = six volumes for the equivalent, a result confirmatory of our determination of five volumes for iodide of potassium; the increase of one volume being in conformity with the usual behaviour of ammoniacal salts. We shall return to the consideration of the chlorides in a future section.

SECTION V.

Chromates.

The chromates present a class of salts which offer some peculiarities with regard to their volumes, in elucidating which we had occasion to repeat our experiments very often, and, therefore, give the mean of the results, instead of taking up unnecessary space in the Transactions of the Society by describing each experiment individually.

Chromic Acid, $CrO_3=52 \cdot 19$.—The chromic acid used in our experiments was obtained by adding sulphuric acid to bichromate of potash. It was in beautiful distinct crystals of nearly a quarter of an inch in length, being the finest and purest specimen which we have obtained in many preparations of this acid.

The half of an equivalent, 26·09 grains, dissolved in 1000 of water with an increase of 9·0 at 72°; this gives 18·0 as the volume of chromic acid in solution.

The same quantity of acid, thrown into the solution from which it had crystallized, gave an increase of 9·7 and 9·8 in two experiments.

I. Chromic acid, volume	19·4	... 2·690
II.	19·6	... 2·663
Mean . .	19·5	... 2·676

Yellow Chromate of Potash, KO, $CrO_3=99 \cdot 50$.—On dissolving 50 grains of this salt in 1000 grains of water, the increase was 9·0 at a temperature of 58°; this gives 17·9 as the volume of the equivalent in solution.

The mean result of *ten* experiments, on immersing 49·75 grains in turpentine, was an increase of 18·55, which gives 37·1 for the volume of the equivalent, and 2·682 as the specific gravity of the salt.

Sesquichromate of Potash, $2KO, 3CrO_3 = 251{\cdot}09$.—This salt, which will be described in a future communication by one of us, is obtained by boiling a solution of bichromate of potash with an excess of finely pounded litharge. The oxide of lead removes only one-fourth of the chromic acid of the bichromate, and the solution on cooling deposits the sesquichromate in flattened prisms of a paler but more resplendent colour than the bichromate of potash. On dissolving the fourth part of an equivalent, 62·77 grains, in 1000 grains of water, the increase in four experiments at 58° was exactly 18·0; this gives 72·0 as the volume of the equivalent in solution.

The mean of six experiments, placing the fourth of an equivalent, 62·77 grains, in turpentine, was an increase of 23·7, which gives 94·8 as the volume of the equivalent, and 2·648 as the specific gravity of the salt.

Bichromate of Potash, $KO, 2CrO_3 = 151{\cdot}70$.—On dissolving 76 grains of this salt in 1000 grains of water, an increase of 22·5 and 23·0 were obtained in two experiments at 60° and 65°.

I.	$KO, 2\,CrO_3$, vol. in solution	44·9
II.	... ...	45·8
	Mean . .	45·3

Half an equivalent of the salt, 75·84 grains, immersed in turpentine, gave an increase, the mean of *ten* experiments, of 28·9, which gives 57·8 as the volume of an equivalent, and 2·624 as the specific gravity of the salt.

Terchromate of Potash, $KO, 3CrO_3 = 203{\cdot}92$.—This salt was obtained by mixing a solution of bichromate of potash with nitric acid and crystallizing. On dissolving 51 grains of the salt in 1000 grains of water, an increase was occasioned of 18·0 at 60°; this gives 71·9 as the volume of the equivalent in solution.

On immersing 50·98 grains in turpentine, the increase was 19·3 in two experiments, and 19·0 in a third trial.

I.	$KO, 3CrO_3$, vol. of salt	77·2	...	2·641
II.	... ...	77·2	...	2·641
III.	... ...	76·0	...	2·683
	Mean . .	76·8	...	2·655

Bichromate of Chloride of Potassium, $KCl + 2CrO_3 = 179{\cdot}08$.—The fourth part of an equivalent, 44·77 grains, being dissolved in 1000 grains measure of a dilute solution of muriatic acid, gave an increase of 15·7 in two experiments at 57°; this result makes the volume of an equivalent in solution 62·8.

The mean of various experiments on this salt gave an increase of 18·15 on immersing the above quantity of salt in turpentine, which yields 72·6 as the volume of the equivalent, and 2·466 as the specific gravity of the salt.

The results now described show that the chromates form a group different from the classes of salts hitherto given.

Table X.—Showing the Volumes occupied by certain Chromates.

Designation.			Volume of salt in solution.			Volume of salt.	
Name.	Formula.	Atomic weight.	Volume in solution by experiment.	9, taken as unity.	Volume by theory.	Volume of salt.	Specific gravity.
Chromic acid	CrO_3	52·19	18·0	2	18·0	19·5	2·676
Chromate of potash	KO, CrO_3	99·50	17·9	2	18·0	37·1	2·682
Sesquichromate of potash.	$2KO, 3CrO_3$	251·07	72·0	8	72·0	94·8	2·648
Bichromate of potash......	$KO, 2CrO_3$	151·70	45·3	5	45	57·8	2·624
Terchromate of potash ...	$KO, 3CrO_3$	203·92	71·9	8	72	76·8	2·655
Bichromate of chloride of potassium	$KCl + 2CrO_3$	179·08	62·8	7	63	72·6	2·466

An inspection of the previous table will show clearly that the chromates differ from the salts described in the former sections. In the volumes in solution there is no difference; they are multiples of 9, and follow the usual law of the sum of the volumes, being made up of the volumes of the constituents of the salt. Chromate of potash possesses two volumes in solution, exactly as is the case with its analogue sulphate of potash. The latter salt affects three volumes in the solid state, and so naturally should chromate of potash. In bichromate of potash we see these three volumes appearing in solution, united to two volumes possessed by the chromic acid attached to the chromate of potash; in sesquichromate of potash they again reappear, and so also in terchromate of potash. The fact that the number of volumes possessed in the solid state by the lowest member of a series of salts passes over into the higher members when in solution, finds examples in the carbonates and oxalates, and is not peculiar to the chromates.

The solid volumes of the chromates possess decided peculiarities, being neither multiples of 11 nor of 9·8. Chromic acid itself is obviously twice the volume of ice, 9·8 × 2 = 19·6, the experimental number being 19·5. But all the other salts in this group refuse to arrange themselves under either of the heads which we have found to explain most of the salts in the previous sections. In an exception of this kind we are entitled to make an assumption which will in all probability be near the truth, if by means of it we can bring into one uniform system a whole group of anomalous salts. Sesquichromate of potash is of great importance in the history of the chromates, from its frequent occurrence, although hitherto it has been altogether neglected by chemists. Chromic acid is actually able to displace sulphuric acid from sulphate of potash, in order to gratify its love for the potash in the peculiar condition of the sesquichromate. In numerous instances of decomposition, as will be pointed out by one of us in another paper, this sesquichromate appears. The sesquichromate is not formed readily, if indeed it is ever formed, by crystallizing chromate of potash with chromic acid in the proportion of sesquichromate, the result being bichromate of potash and chromate of potash, which crystallize separately. Here then is a remarkable point in the constitution of the chromates, which can only be explained by supposing that sesquichromate of potash contains a double atom of chromate of potash united to one of chromic acid. The decomposition of bichromate of potash by oxide of lead necessarily implies that its atom should also be doubled; $2KO, 3CrO_3 + CrO_3$ boiled with litharge, gives $2KO, 3CrO_3 + PbO, CrO_3$.

We have found the volume of KO, CrO_3 to be 37·1, not 33·0, as in the case of sulphate of potash. Karsten obtained the specific gravity 2·640, which gives the volume 37·6; and Thomson states the specific gravity to be 2·612, which gives the volume 38·1; the mean of all these experiments is 37·6, which, multiplied by 2, for the reasons already stated, gives as the volume of $2(KO, CrO_3)$, 75·2. The natural volume of chromate of potash, deduced from its analogy to sulphate of potash, would be 11 × 3, or on the double atom 11 × 6 = 66. Now, the assumption we make to explain this class of salts is, that the double atom of chromate of potash enjoys its anomalous character by adding to its natural volume the volume of ice, thus 66·0 + 9·8 = 75·8, which is not very far from the volume ascertained by experiment. This assumption of a volume of ice, in addition to other volumes of 11, has been shown to exist in the magnesian sulphates, and therefore its hypothetical existence in the chromates is by no means extravagant. Sesquichromate of potash must then be the double chromate of potash united to an equivalent of chromic acid, 75·8 + 19·6

=95·4, which is not very far from 94·8, the volume determined by experiment. Bichromate of potash would consist of a double atom of chromate of potash and 2 of chromic acid, or 75·8 + 39·2 = 115·0, which agrees pretty closely with the experimental determination of 115·6; and terchromate of potash, in like manner, is 1 atom of double chromate of potash with 4 of chromic acid, or 75·8 + 78·4 = 154·2, which is almost exactly the same as 154·4, found in the two consecutive experiments, and not far distant from 153·6, the mean of the three experiments.

This view receives confirmation from the volume of Peligot's salt, which certainly consists of the volume of KCl, when in combination, added to that of 2 atoms of chromic acid, 33·0 + 39·2 = 72·2, a number very close to the experimental result 72·6. It is quite true that we have made a gratuitous assumption at the outset of our explanation; but it is not surprising to find an unusual law prevailing in a class of salts so anomalous as the chromates. When the experimental numbers, and those calculated on the assumption, are so near as we have shown them to be, there is, we think, a good argument for the truth of the hypothesis.

TABLE X. A.

Name.	Formula.	Volume by experiment.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Chromic acid	CrO_3	19·5	19·6	2·663	2·676
Chromate of potash, doubled	$(2KO, 2CrO_3)$	75·2	75·8	2·627	2·646
Scsquichromate of potash	$(2KO, 2CrO_3) + CrO_3$	94·8	95·4	2·658	2·648
Bichromate of potash ...	$(2KO, 2CrO_3) + 2CrO_3$	115·6	115·0	2·638	2·624
Terchromate of potash ...	$(2KO, 2CrO_3) + 4CrO_3$	154·4	154·2	2·644	2·641
Bichromate of chloride of potassium	$KCl + 2\,CrO_3$	72·6	72·2	2·480	2·466

A singular result obtained in the examination of the anhydrous double sulphates seems to be explained by the behaviour of the chromates. We found sulphate of copper and potash and sulphate of magnesia and potash to affect a volume of 59·8 instead of 55·0, and we ascertained, by many experiments, that this high number was not due to an error of observation. Now, if we suppose the KO, SO_3 in these salts to behave like KO, CrO_3 in assuming one volume of ice on the double atom, then $2KO, SO_3 = 75{\cdot}8 + 2MO, SO_3 = 44{\cdot}0 = \frac{119{\cdot}8}{2} = 59{\cdot}9$, a number almost identical with the experimental result. On this view, then, anhydrous double sulphates are constituted on the type of red chromate of potash, the *two volumes* of CrO_3 being replaced by the *two volumes* of MO, SO_3. Anhydrous alum was found to have a volume of 116·4 instead of 111·4, but would be reconciled with theory, if we supposed it to contain the peculiar KO, SO_3 analogous to KO, CrO_3; in this case the theoretical volume would be 116·3.

SECTION VI.

Carbonates.

Carbonate of Potash, $KO, CO_2 = 69{\cdot}4$.—On dissolving 34·7 grains of carbonate of potash in 1000 grains of water, the increase was 4·6 at 62°; the atomic volume in solution is therefore 9·2. The same quantity of salt thrown into turpentine caused, in various experiments, an increase of 16·5; this makes the volume of the equivalent 33·0, and the specific gravity of the salt 2·103.

Bicarbonate of Potash, $KO, HO, 2CO_2 = 100{\cdot}6$.—The fourth part of an equivalent (25·1 grains) dissolved in 1000 grains of water, at 61°, with an increase of 8·9, and in another experiment of 9·0. The mean of these results, 8·95, gives as the volume of the equivalent in solution, 35·8. The same quantity of salt, thrown into turpentine, gave an increase of 12·0, in various experiments, which gives for the specific volume of the salt 48·0, and for its specific gravity 2·092. As this salt was one of the very few substances used in this inquiry not prepared by ourselves, we take the mean of our own result, and the only other recorded specific gravity of which we are aware, viz. that by Gmelin, 2·012, and adopt 49·0 as the correct volume, and 2·052 as the specific gravity.

Bicarbonate of Ammonia, $HO, NH_4O, 2CO_2 = 79{\cdot}3$.—This salt was made by exposing the carbonate of the shops to the air until it ceased to emit smell, and then crystallizing the remainder. On dissolving 19·82 grains, the fourth of an equivalent, in 1000 grains of water, the increase was 9·0 at 55°, and 9·4 in another experiment at 62°. The mean result gives 36·8 as the volume of the salt in solution.

On immersing 19·82 grains of the salt in turpentine, an increase of 12·5 was effected, which gives as the volume of the salt 50·0, and for its specific gravity 1·586.

Bicarbonate of Soda, $NaO, HO, 2CO_2 = 84{\cdot}64$.—On dissolving 42·32 grains of this salt in 1000 grains of water at 67°, an increase of 9·0 was obtained; this gives for the volume of an equivalent in solution 18·0. On immersing the same quantity of salt in turpentine, the increase was 19·4 and 19·2 in two experiments.

I.	$NaO, HO, 2CO_2$, vol. of salt	38·8 ...	2·181
II.	... ...	38·4 ...	2·204
	Mean . .	38·6 ...	2·192

Although we have examined other carbonates, we purposely avoid bringing them into the present paper, because they involve considerations upon which we are at present engaged in minute study, and do not wish to hazard without sufficient proof. We subjoin the few carbonates here examined in a tabular form.

TABLE XI.—Showing the Volumes occupied by the Alkaline Carbonates.

Designation.			Vol. in solution.			Volume of salt.	
Name.	Formula.	Atomic Weight.	Volume in solution.	9, taken as unity.	Volume by theory.	Volume of salt by experiment.	Specific gravity by experiment.
Carbonate of potash	KO, CO_2	69·4	9·2	1	9	33·0	2·103
Carbonate of soda	NaO, CO_2	53·47	...	...	...	22·0	2·427
Bicarbonate of potash ...	$HO, KO, 2CO_2$	100·6	35·8	4	36	49·0	2·052
Bicarbonate of soda	$HO, NaO, 2CO_2$	84·64	18·0	2	18	38·6	2·192
Bicarbonate of ammonia .	$HO, NH_4O, 2CO_2$	79·3	36·8	4	36	50·0	1·586

The results shown in this table will appear perplexing, unless the facts already observed in the previous sections be borne in mind. We find in carbonate of potash an astonishing difference between the liquid and the solid volume; and this is still more marked in the case of carbonate of soda, which ceases to occupy volume in solution. Both of these salts have 11 as the divisors of their solid volume, KO, CO_2 affecting three, and NaO, CO_2 two volumes. In the last section we saw that the three volumes possessed by chromate of potash in its solid state passed over into bichromate of potash; and in bicarbonates of potash and ammonia we observe the same circumstance, except that the volumes change from multiples of 11 to multiples of 9·8, and in solution are one less than in the state of a salt. It is probably owing to this circumstance that we do not in this case observe the usual increase of one volume in the ammoniacal over the corresponding salt of potash. The bicarbonates of potash, soda and ammonia are probably multiples of 9·8, or the volume of ice.

	Vol. by experiment.		Vol. by theory.	
Bicarbonate of potash .	49·0 ..	2·052 ..	$9·8 \times 5 = 49·0$ ..	2·052
... ammonia	50·0 ..	1·586 ..	$9·8 \times 5 = 49·0$ ..	1·618
... soda . .	38·6 ..	2·192 ..	$9·8 \times 4 = 39·2$ ..	2·159

SECTION VII.

Oxalates.

The oxalates offered an interesting group of salts for examination, especially on account of the accurate determination of their composition and hydration by Graham.

Oxalate of Water, $HO, C_2O_3 + 2HO = 63{\cdot}26$.—32 grains of oxalic acid, dissolved in 1000 grains of water, caused an increase of 18·5 at a temperature of 55°; the same quantity, being subjected to a second experiment, caused an increase of 19; and a third experiment, in which 21 grains were dissolved in 9½ ounces of water, occasioned an increase of 12 at 40°.

I.	$HO, C_2O_3 + 2HO$, vol. in solution	36·5
II.	... ...	37·5
III.	... ...	36·0
	Mean . .	36·6

A whole equivalent thrown into turpentine caused, in various experiments, an increase of 39·0, which gives for its specific gravity 1·622. Richter states the specific gravity to be 1·507; but it is impossible that he can have operated upon a pure specimen, as we have repeated the experiments upon this acid very frequently.

Oxalate of Potash, $KO, C_2O_3 + HO = 92{\cdot}39$.—A quantity of salt, 42·5 grains, being dissolved in 1000 grains of water, gave an increase of 13·0; and the same result attended a repetition of the experiment, the temperature in both cases being at 60°.

$KO, C_2O_3 + HO$, vol. in solution 28·2

46·2 grains of the same salt, being put into a saturated solution, caused a rise in the stem of 22·0; a repetition of the experiment with the same quantity gave the increase 21·9, the temperature in both cases being 61°.

				Sp. gr.
I.	$KO, C_2O_3 + HO$, vol. of salt	44·0	...	2·100
II.	... ...	43·8	...	2·109
	Mean . .	43·9	...	2·104

Oxalate of Ammonia, $NH_4O, C_2O_3 + HO = 71{\cdot}43$.—Half an equivalent of this salt (35·71 grains) was dissolved in 1000 grains of water with an increase of 18·0 at a temperature of 55°; and a repetition of the experiment with the same quantities and at the same temperature, gave exactly the same result.

I. II. $NH_4O, C_2O_3 + HO$, vol. in solution 36

35·71 grains, being immersed in a saturated solution, gave in the first experiment an increase of 24·5, in the second of 24·4; the first experiment being at 48°, the second at 50°.

				Sp. gr.
I. $NH_4O, C_2O_3 + HO$, vol. of salt	49·0	...		1·458
II. ...	48·8	...		1·464
Mean	48·9	...		1·461

Binoxalate of Potash, $KO, C_2O_3 + HO, C_2O_3 + 2HO = 146·63$.—To determine the volume of this salt, 18·33 grains were dissolved in 1000 grains of water with a rise of 6·8 at a temperature of 57°; and the same result attended a repetition of the experiment; in a third experiment, 25 grains at the same temperature caused an increase of 9·0.

I. II. Binoxalate of potash, vol. in solution	54·4
III.	52·8
Mean	53·6

Half an equivalent of the salt (73·31 grains), being immersed in a saturated solution, caused an increase of 37·4 in the first experiment, and of 37·2 in the second, the temperature in both cases being 55°.

			Sp. gr.
I. Binoxalate of potash, vol. of salt	74·8	...	1·960
II.	74·4	...	1·971
Mean	74·6	...	1·965

Oxalate of Copper and Potash, $KO, C_2O_3 + CuO, C_2O_3 + 2HO = 177·25$.—On account of the sparing solubility of this salt, 11·08 grains, or the sixteenth part of an equivalent, were dissolved in water, and caused an increase of 3·4 in two experiments at a temperature of 59°.

I. II. $KO, C_2O_3 + CuO, C_2O_3 + 2HO$, vol. in solution 54·4

The fourth of an equivalent (44·3 grains), placed in a saturated solution, caused an increase in one experiment of 19·5; in another of 18·9; and in a third of 19·7; all at a temperature varying from 54° to 57°.

			Sp. gr.
I. Oxalate of copper and potash, vol. of salt	78·0	...	2·272
II.	75·6	...	2·344
III.	78·8	...	2·249
Mean	77·5	...	2·288

Binoxalate of Ammonia, $NH_4O, C_2O_3 + HO, C_2O_3 + 2HO = 125·69$.—31·42 grains of this salt, dissolved in 1000 grains of water, caused in the first experiment an increase of 18·0 at 60°; in the second, of 18·2 at 61°; in a third experiment, of

17·8 at 54°; in a fourth, 42 grains dissolved in 4100 grains of water increased 24·0 at 53°.

I. Binoxalate of ammonia, vol. in solution	72·0
II.	72·8
III.	71·2
IV.	71·8
Mean . . .	71·9

The half of an equivalent (62·84 grains), being immersed in a saturated solution, caused an increase of 40·3 in two experiments, and of 40·0 in a third.

			Sp. gr.
I. Binoxalate of ammonia, vol. of salt	80·6	...	1·559
II.	80·6	...	1·559
III.	80·0	...	1·571
Mean . .	80·4	...	1·563

Oxalate of Copper and Ammonia, $NH_4O, C_2O_3 + CuO, C_2O_3 + 2HO = 156{\cdot}38$.—The solution of 18·3 grains gave an increase of 8·6 at 65°; this gives 73·3 as the volume of this salt when in solution.

On immersing 20 grains in turpentine, an increase of 10·4 was obtained, which gives for the volume of the equivalent 81·3, and for the specific gravity of the salt 1·923.

Quadroxalate of Potash, $KO, C_2O_3 + 3HO, C_2O_3 + 4HO = 255{\cdot}11$.—32·0 grains dissolved in water, gave an increase of 15·0 at 60°; and a second experiment, in which 16 grains were dissolved in 1000 grains of water, gave the increase of 7·2 at a temperature of 44°.

I. Quadroxalate of potash, vol. in solution	119·4
II.	114·8
Mean . .	117·1

63·8 grains, the fourth part of an equivalent, thrown into a saturated solution, caused a rise of 35·1 in two experiments.

			Sp. gr.
I. II. Quadroxalate of potash, vol. of salt	140·4	...	1·817

Quadroxalate of Ammonia, $KO, C_2O_3 + 3HO, C_2O_3 + 4HO = 234{\cdot}15$.—On dissolving 20 grains of this salt in 3500 grains of water at 50°, the increase is 11·5, which gives 134·5 as the volume of the equivalent in solution.

58·5 grains of the salt, thrown into a saturated solution, caused in the first experiment an increase of 36·8, in the second of 36·9, both at a temperature of 62°.

			Sp. gr.
I. Quadroxalate of ammonia, vol. of salt	147·2	...	1·591
II.	147·6	...	1·586
Mean . .	147·4	...	1·589

The volumes of the oxalates can only be explained by an attentive consideration of the previous results. We have already seen numerous instances in which the primitive volumes 9·8 and 11·0 become mutually convertible; this is strikingly the case with the salts of the present section. Hydrated oxalic acid has a volume 9·8 × 4; oxalate of potash possesses the volume 11 × 3, and passes with this volume into the binoxalate and quadroxalate of potash, the oxalic acid in the binoxalate being associated as two volumes of ice, although the water of crystallization possesses the volume 11. Quadroxalate of potash is to be viewed as anhydrous binoxalate *plus* 2 equiv. hydrated oxalic acid, the latter having become 11 × 4 instead of 9·8 × 4. The same explanation applies to the binoxalate and quadroxalate of ammonia, the only difference being that anhydrous oxalate of ammonia, 9·8 × 4, takes the place of oxalate of potash. On these views the following table is constructed.

TABLE XII.—Showing the volumes occupied by certain Oxalates.

Designation.			Vol. in solution.			Volume in state of salt.				
Name.	Formula.	Atomic weight.	Volume in solution by experiment.	9, or volume of water as unity.	Volume by theory.	Volume by experiment.	11 and 9·8, taken as unity, as above described.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Oxalic acid	HO, C_2O_3+2HO	63·26	36·6	4	36	39·0	4	39·2	1·616	1·622
Oxalate of potash...	KO, C_2O_3+HO	92·39	28·2	3	27	43·9	4	44·0	2·100	2·104
Oxalate of ammonia	NH_4O, C_2O_3+HO	71·43	36·0	4	36	48·9	5	49·0	1·458	1·461
Binoxalate of potash	$KO, 2C_2O_3+3HO$	146·63	53·6	6	54	74·6	7	74·6	1·965	1·965
Binoxalate of ammonia............	$NH_4O, 2C_2O_3+3HO$	125·67	71·9	8	72	80·4	8	80·8	1·555	1·563
Oxalate of copper and potash......	$KO, C_2O_3+CuO, C_2O_3+2HO$	177·25	54·4	6	54	77·5	7	77·0	2·301	2·288
Oxalate of copper and ammonia...	$NH_4O, C_2O_3+CuO, C_2O_3+2HO$	156·38	73·3	8	72	81·3	8	80·8	1·935	1·923
Quadroxalate of potash	$KO\ 4C_2O_3+7HO$	255·11	117·1	13	117	140·4	13	140·6	1·814	1·817
Quadroxalate of ammonia	$NH_4O, 4C_2O_3+7HO$	234·15	134·5	15	135	147·4	14	146·8	1·595	1·589

The examination of the volumes occupied by the oxalates presents several points of great interest. The volume of oxalic acid itself is a multiple of the volume of ice, or 9·8 × 4. Oxalate of potash in its solid state possesses four volumes, 11 × 4, but loses one volume on passing into solution, as usually is the case with neutral salts of potash. As one of these volumes is due to its combined water, the proper number of volumes in anhydrous oxalate of potash is three, and these it carries into binoxalate of potash, which is therefore a simple combination of oxalate of potash and hydrated oxalic acid, the crystalline water of the latter having assumed the volume 11.

	In solution.		As a salt.
KO, C_2O_3	18	...	33·0
$HO, C_2O_3 + 2HO$	36	...	41·6
Binoxalate of potash . . .	54	...	74·6

The only difference between the volumes of this salt and those of its constituents, when uncombined, is that the crystalline water of the hydrated oxalic acid has assumed the volume 11. Quadroxalate of potash consists of anhydrous binoxalate of potash united to hydrated oxalic acid, as Graham has already announced in his researches on the oxalates. The three volumes affected by oxalate of potash in its solid state pass into solution with it in quadroxalate of potash, just as we saw in the case of chromate and bichromate of potash; and the attached oxalic acid affects 11 × 4 instead of 9·8 × 4.

	In solution.		As a salt.
I. Anhydrous binoxalate of potash	45	...	52·6
II. Hydrated oxalic acid	72	...	88·0
Quadroxalate of potash . . .	117	...	140·6

The assumption of two volumes in solution above those of binoxalate of potash was already characteristic of binoxalate of ammonia, and the same increase is seen in the quadroxalate, showing clearly that that salt must contain its ammonia *quasi* binoxalate and not as oxalate of ammonia. It is very possible that the volumes in solution of quadroxalate of ammonia should be 14, instead of 15, but the temperature 31°, at which it comes out 14 volumes, is so low, that it is more natural to keep the volumes we have given in the table.

It is interesting to observe how closely oxalate of copper relates itself to oxalate of water.

	Volumes in solution.		Volumes as salt.
Oxalate of copper and potash .	6	...	7
... water and potash .	6	...	7
... copper and ammonia	8	...	8
... water and ammonia .	8	...	8

Thus, even in the apparently anomalous behaviour of binoxalate of ammonia, in assuming two volumes more than the corresponding salt of potash, we find oxalate of copper and ammonia imitating its example. The reason of their increase will be explained in the next section.

SECTION VIII.

Subsalts and Ammoniacal Salts.

The salts which we have hitherto examined have been those soluble in water, and having a constitution to a certain degree well-defined. We have now to consider the insoluble subsalts, and, in some cases, their neutral insoluble types, and also to ascertain how far the results thus obtained serve to throw light on the constitution of ammoniacal salts.

Subsulphate of Copper, $CuO, SO_3, 4HO + 3CuO = 234·9$.—This well-known salt was made by adding ammonia to a solution of sulphate of copper. The fourth part of an equivalent, 58·7 grains, thrown into water, caused an increase of 19·1 and 19·0 in two successive experiments.

			Sp. gr.
I. Subsulphate of copper, vol. of salt	76·4	...	3·074
II.	76·0	...	3·090
Mean . .	76·2	...	3·082

Subsulphate of Zinc, $ZnO, SO_3, 3ZnO, 4HO = 237·3$.—This salt is apt to combine with more water than four atoms, but may be obtained with four by drying at 212°. On placing 29·66 grains, the eighth part of an equivalent, in turpentine, an increase of 9·5 was obtained; and on treating 22·8 in a similar manner, the rise in the stem was 7·3. Both of these experiments exactly agree in making—

			Sp. gr.
Subsulphate of zinc, vol. of salt	76·0	...	3·122

Sulphate of Protoxide of Mercury, $Hg_2O, SO_3 = 251·0$.—This salt was prepared in the usual way by digesting one part of mercury in 1½ part of sulphuric acid. The fourth of an equivalent, 62·75 grains, thrown into turpentine, increased 8·3.

			Sp. gr.
Hg_2O, SO_3, vol. of salt	33·2	...	7·560

Sulphate of Peroxide of Mercury, $HgO, SO_3 = 149·6$.—The salt used in the experiment was prepared by heating five parts of sulphuric acid mixed with a little nitric acid, with four parts of mercury until the whole became a dry saline mass. On immersing 37·5 grains of the salt thus prepared in turpentine, an increase of 5·8 was obtained, which gives 23·1 for the volume of the equivalent, and 6·466 for the specific gravity of the salt.

Subsulphate of Mercury, $HgO, SO_3 + 2HgO = 368·46$.—The last salt thrown into water and washed with warm water is converted into the beautiful yellow powder known as turpeth mineral. On throwing 57·4 grains of this salt thus prepared into water, an increase of 6·9 was obtained, which gives 44·3 as the volume of the equivalent, and 8·319 as the specific gravity of the salt.

Chromate of Lead, $PbO, CrO_3 = 163{\cdot}97$.—On throwing 81·98 grains of the chromate of lead, previously well-dried, into turpentine, an increase of 14·5 was effected; this gives 29·0 as the volume of the equivalent, and 5·653 as the specific gravity of the salt.

Subchromate of Lead, $PbO, CrO_3 + PbO = 275{\cdot}7$.—This salt was prepared by projecting chromate of lead into melted nitre, and afterwards washing out all soluble matter. On immersing 68·92 grains, the fourth part of an equivalent, an increase of 11·0 was obtained in two experiments. This gives 44·0 for the volume of the equivalent, and 6·266 as the specific gravity of the salt.

Sesquibasic Chromate of Lead, $2(PbO, CrO_3) + PbO = 439{\cdot}67$.—The mineral, melanchroit, is of the composition expressed by the above formula, and has a specific gravity of 5·75 according to Hermann; this gives the number 76·5 as the atomic volume of the compound.

Subnitrate of Copper, $CuO, NO_5, HO, 2CuO = 182{\cdot}17$.—The fourth part of an equivalent, 45·54 grains, caused an increase of 16·5 in two experiments, and of 16·4 in a third.

				Sp. gr.
I.	$CuO, NO_5, HO + 2CuO$, vol. of salt	66·0	...	2·760
II.	....	66·0	...	2·760
III.	... ...	65·6	...	2·777
	Mean . .	65·87	...	2·765

A. *Subnitrate of Bismuth*, $BiO, NO_5, HO + 2BiO = 300{\cdot}4$.—This salt was prepared in the same manner as subnitrate of copper, viz. by heating the nitrate to 400° or 500°. The fourth part of an equivalent, 75·1 grains, thrown into water, caused, in various experiments, an increase of 16·5, which gives 66·0 as the atomic volume, and 4·551 as the specific gravity of the salt.

B. *Subnitrate of Bismuth*, $BiO, NO_5 + 2BiO = 291{\cdot}4$.—This salt was prepared by adding nitrate of bismuth to a large quantity of water; the white powder which falls by this treatment, is composed, according to Phillips, of three equivalents of oxide of bismuth united to one of nitric acid. It is therefore the same salt as the one last described, deprived of its constitutional water. On immersing 72·85 grains of the salt in water, a rise in the stem of 13·9 was effected, and 36·42 grains treated in the same way gave an increase of 6·9.

				Sp. gr.
I.	$3BiO, NO_5$, vol. of salt	55·6	...	5·241
II.	....	55·2	...	5·279
	Mean . .	55·4	...	5·260

Subnitrate of Mercury, $HgO, NO_5\ HO + 2HgO = 391{\cdot}49$.—This salt was obtained in a yellow powder by adding the crystallized subnitrate of mercury to water, and washing it, according to the directions of Kane, with hot, but not boiling water. The fourth part of an equivalent, 97·87 grains, thrown into turpentine, caused an increase of 16·4, which gives 65·6 as the atomic volume of the salt, and 5·967 as the specific gravity.

Ammoniacal Sulphate of Copper, $CuO, SO_3\ HO, + 2NH_3 = 123{\cdot}0$.—This salt has already been described in a previous section; it had a volume of 54·0 or 9 × 6 in a state of solution, and of 68·6 or 9·8 × 7 in the solid state. The salt examined in that case was in fine large indigo blue crystals, and was prepared by ourselves. Another portion, made by Mr. Morson in small crystals, we found to possess a volume of 68·0 and specific gravity of 1·809. When this salt is heated it loses one equivalent of water and one of ammonia, being converted into a green powder, the formula of which is $CuO, SO_3 + NH_3$; 24·27 grains of this, thrown into turpentine, caused an increase of 9·8, which gives 39·2 as the volume of the equivalent, and 2·476 as the specific gravity of the salt. The latter salt on being moistened with water absorbs three equivalents, and therefore assumes the atomic weight of 124·07; the fourth part of which, 31·0 grains, thrown into turpentine, caused an increase of 15·9, making the atomic volume of $CuO, SO_3 + NH_3 + 3HO$, 63·6, and its specific gravity 1·950.

Ammonia-Sulphate of Zinc.—Kane describes several ammonia-sulphates of zinc, obtained by passing a stream of ammonia through a hot solution of sulphate of zinc, until the precipitate at first formed is redissolved. The solution thus obtained deposited transparent crystals in a few hours, but these effloresced so quickly after being dried, that we did not determine their specific gravity. The effloresced crystals have, according to Kane, the formula—

$$ZnO, SO_3 + 2\,NH_3 + 2\,HO = 132{\cdot}8.$$

We fear, however, that we have not been successful in procuring this salt in its proper state, as the determination of its volume varied between 57·5 and 64·0, results so discordant, that it would not be safe to take their mean as a correct result. On heating this salt it loses water and ammonia, being converted into $ZnO, SO_3 + NH_3$; 26·7 grains of which (the fourth of an equivalent), thrown into turpentine, caused an increase of 10·8, which gives 39·5 as the volume of the salt, and 2·479 for its specific gravity.

Ammonia-Sulphate of Mercury, $HgO, SO_3 + HgAd + 2HgO = 486·0$.—This salt, which Kane calls the Ammonia Turpeth, was prepared by heating turpeth mineral with ammonia until it became changed to a heavy white powder. The eighth part of an equivalent, 60·75 grains, immersed in water, caused an increase of 8·3 in two experiments; this makes the volume of the compound 66·4, and its specific weight 7·319.

Ammonia-Sulphate of Silver, $AgO, SO_3 + 2NH_3 = 190·86$.—This salt was obtained in the usual way, by dissolving sulphate of silver in ammonia and crystallizing. The first specimen tried was in small, indistinct crystals, in the second instance the crystals were large and well-defined. 25·62 grains gave an increase of 8·6, and 37·7 grains of the better specimen of salt gave the increase 13·2.

			Sp. gr.
I. $AgO, SO_3 + 2NH_3$, vol. of salt	64·0	...	2·979
II.	66·8	...	2·857
Mean . .	65·4	...	2·918

Ammonia-Chromate of Silver, $AgO, CrO_3 + 2NH_3 = 202·8$.—This salt was obtained in fine large crystals, in the same manner as the last salt. On immersing 25·35 grains in turpentine, the increase was 8·3, and on treating 50·7 grains in the same way, the increase was 16·5.

			Sp. gr.
I. $AgO, CrO_3 + 2NH_3$, vol. of salt	66·4	...	3·054
II.	66·0	...	3·073
Mean . .	66·2	...	3·063

Ammonia-Nitrate of Copper, $CuO, NO_5 + 2NH_3 = 128·4$.—On dissolving 64·2 grains, half an equivalent, in 1000 grains of water, the increase was 32·0 in two experiments at a temperature of 60°; this makes the atomic volume in solution 64·0. On putting the same quantity into turpentine, there was a rise in the stem, in three experiments, of 34·0, 34·0, and 34·8.

			Sp. gr.
I. $CuO, NO_5 + 2NH_3$, vol. of salt	68·0	...	1·888
II.	68·0	...	1·888
III.	69·6	...	1·845
Mean . .	68·5	...	1·874

Ammonia-Sub-Nitrate of Mercury, $HgO, NO_5 + 2HgO + NH_3 = 399·7$.—This salt was prepared by adding a dilute solution of ammonia to nitrate of mercury, and was of a pure milk-white colour, as described by Kane. On throwing 40 grains of this compound into water, an increase of 6·7 was obtained; this gives a volume of 67·0 on the equivalent, and 5·970 as the specific gravity of the salt.

Chloride of Copper, $CuCl=67·18$.—The volume of hydrated chloride of copper was shown to be 33 or 3 × 11; but we have not yet examined the bulk occupied by the anhydrous chloride. The chloride was deprived of its water by a heat considerably below that of redness, in order to prevent the formation of any sub-chloride. On throwing 33·59 grains, or half an equivalent, into turpentine, the increase in two experiments was exactly 11·0, which gives 22·0 as the volume of the salt, and 3·054 as its specific gravity.

Ammonia-Chloride of Copper, $CuCl+2NH_3+HO=110·3$.—This salt was made by passing a stream of ammonia through a solution of chloride of copper until the precipitate formed had completely redissolved. The crystals, which deposited as the solution cooled, were dried in a receiver containing slaked lime, so as to prevent the carbonic acid of the atmosphere acting upon the ammonia; but in spite of this precaution the crystals had slightly effloresced on the surface. The effloresced matter was removed, and the pure crystals employed. 27·6 grains of them, when thrown into turpentine, produced in two experiments an increase of 16·5, making the volume of the salt 66·0, and its specific gravity 1·672. On dissolving the same quantity of salt, 27·6 grains, in 1000 grains of water, the rise was 15·9 at 62°, making the volume of the salt when in solution 63·6.

On exposing this salt to heat, water and ammonia are expelled, and a green powder remains, having the formula $CuCl+NH_3$. 21·07 grains of this salt thrown into turpentine produced an increase of 9·6, making the volume of the equivalent 38·4, and the specific gravity of the salt 2·194.

Subchloride of Copper, $Cu_2Cl=98·89$.—The subchloride used in the experiment was made by adding protochloride of tin to a solution of chloride of copper. During the desiccation of the salt it became slightly green, showing that a little chloride had been formed by the absorption of oxygen; but the change was so slight as probably not to interfere materially with the result; 42·2 grains, thrown into turpentine, caused an increase of 12·5, which gives 29·2 as the volume, and 3·376 as the specific gravity of the salt.

Subchloride of Mercury, $Hg_2Cl=238·33$.—The fourth part of an equivalent, 59·58 grains, thrown into turpentine, caused an increase of 8·3.

			Sp. gr.
Calomel, vol. of salt	33·2	...	7·178

Hassenfratz states the specific gravity to be 7·176, a result very near our own determination.

Subchloride and Amide of Mercury, $Hg_2Cl + Hg_2Ad =$ 458·1.—The eighth part of an equivalent, 57·26 grains, thrown into water, caused an increase of 8·3 and 8·4 in two experiments.

			Sp. gr.
I. Black compound of calomel	67·2	...	6·816
II. ...	66·4	...	6·899
Mean . .	66·8	...	6·858

The salt used in the experiments was prepared in the usual way, by acting upon calomel with ammonia.

Chloride and Amide of Mercury, $HgCl + HgAd =$ 254·5.—The excellent researches of Kane, so often alluded to, have shown that the above formula represents the composition of white precipitate. It must be dried by a pretty strong heat, to get rid of all its hygrometric water. On projecting 63·8 grains, the fourth of an equivalent, into water, an increase of 11·2 was obtained in two experiments; this gives 44·6 as the specific volume of the compound, and 5·700 as its specific gravity.

Basic Chloride and Amide of Mercury, $HgCl + HgAd + 2HgO =$ 473·3.—This yellow compound was made in the usual way, by boiling white precipitate with water. On throwing 59·2 grains into water, the rise was 8·2 in one experiment, and 8·3 in another.

			Sp. gr.
I. The above salt, volume	65·5	...	7·220
II. ...	66·3	...	7·132
Mean . .	65·9	...	7·176

TABLE XIII.—Showing the Volumes occupied by certain Subsalts and Salts of Ammonia.

Designation.			Volume in solution.			Volume of salt.				
Name.	Formula.	Atomic weight.	Volume of salt in solution.	9, taken as unity.	Volume by theory.	Volume of salt by experiment.	11, taken as unity.	Volume by theory.	Specific gravity by theory.	Specific gravity by experiment.
Subsulphate of copper	$CuO, SO_3, 3CuO + 4HO$	234·9	...	...	...	76·2	7	77	3·051	3·082
Subsulphate of zinc	$ZnO, SO_3, 3ZnO + 4HO$	237·3	...	...	...	76·0	7	77	3·082	3·122
Protosulphate of mercury	Hg_2O, SO_3	251·0	...	...	...	33·2	3	33	7·606	7·560
Persulphate of mercury	HgO, SO_3	149·6	...	...	...	23·1	2	22	6·800	6·466
Subsulphate of mercury	$HgO, SO_3 + 2HgO$	368·46	...	...	...	44·3	4	44	8·374	8·319
Chromate of lead	PbO, CrO_3	163·97	...	...	...	29·0	...	...	...	5·653
Subchromate of lead	$PbO, CrO_3 + PbO$	275·7	...	...	...	44	4	44	6·266	6·266
Melanchroit	$2PbO, 2CrO_3 + PbO$	439·67	...	...	...	76·5	7	77	5·710	5·750
Subnitrate of copper	$CuO, NO_5 HO + 2CuO$	182·17	...	...	...	65·9	6	66	2·760	2·765
A. Subnitrate of bismuth	$BiO, NO_5, HO + 2BiO$	300·4	...	...	...	66·0	6	66	4·551	4·551
B. Subnitrate of bismuth	$BiO, NO_5 + 2BiO$	291·4	...	...	...	55·4	5	55	5·298	5·260
Subpernitrate of mercury	$HgO, NO_5, HO + 2HgO$	391·49	...	...	...	65·6	6	66	5·932	5·967
A. Ammonia-sulphate of copper	$CuO, SO_3, HO + 2NH_3$	123·0	54	6	54	68·7	...	...	...	1·790
B. Ammonia-sulphate of copper	$CuO, SO_3 + NH_3$	97·08	...	...	...	39·2	...	...	...	2·476
Hydrate of ammonia-sulphate of copper	$CuO, SO_3 + NH_3 + 3HO$	124·07	...	...	...	63·6	...	...	...	1·950
Ammonia-sulphate of zinc	ZnO, SO_3, NH_3	97·8	...	...	...	39·5	...	...	...	2·479
Ammonia-turpeth	$HgO, SO_3 + Hg Ad + 2HgO$	486·0	...	...	...	66·4	6	66	7·363	7·319
Ammonia-sulphate of silver	$AgO, SO_3 + 2NH_3$	190·86	...	...	...	65·4	6	66	2·890	2·918
Ammonia-chromate of silver	$AgO, CrO_3 + 2NH_3$	202·8	...	...	...	66·2	6	66	3·073	3·063
Ammonia-nitrate of copper	$CuO, NO_5 + 2NH_3$	128·4	64	7	63	68·5	...	...	...	1·874
Ammonia-nitrate of mercury	$HgO, NO_5 + 2HgO + NH_3$	399·7	...	...	...	67·0	...	...	...	5·970
Chloride of copper	$Cu Cl$	67·18	...	...	...	22	2	22	3·054	3·054
Ammonia-chloride of copper	$Cu Cl + 2NH_3 + HO$	110·3	63·6	7	63	66	6	66	1·671	1·671
B. Ammonia-chloride of copper	$Cu Cl + NH_3$	84·11	...	...	...	38·4	...	...	...	2·194
Subchloride of copper	$Cu_2 Cl$	98·89	...	...	...	29·2	...	...	...	3·376
Subchloride of mercury	$Hg_2 Cl$	238·33	...	...	...	33·2	3	33	7·221	7·178
Subchloride and amide of mercury	$Hg_2 Cl + Hg_2 Ad$	458·1	...	...	...	66·8	6	66	6·941	6·858
Chloride and amide of mercury	$Hg Cl + Hg Ad$	254·5	...	...	...	44·6	4	44	5·784	5·700
Basic chloride and amide of mercury	$Hg Cl + Hg Ad + 2HgO$	473·3	...	...	...	65·9	6	66	7·171	7·176

The important researches of Graham have shown that water plays a most important part in the constitution of salts; and that salts with an excess of base may be viewed as hydrates, in which oxide of hydrogen becomes replaced by a metallic oxide. The previous experiments will be found to give this theory the fullest confirmation. Sulphate of zinc crystallizes with seven atoms of water and affects a volume of 74·6; and sulphate of copper assumes the same state of hydration, when crystallized with the latter salt, although *per se* it assumes only five atoms. Placing together the subsulphates and hydrated sulphates of these metals, we perceive not only a close similarity in their formulæ but also in their volumes, as ascertained by experiment.

		Difference.
$ZnO, SO_3, 3HO, 4HO$, vol.	74·6	1·4
$ZnO, SO_3, 3ZnO, 4HO$, ..	76·0	
$CuO, SO_3, 3HO, 4HO$, vol.	74·6	1·6
$CuO, SO_3, 3CuO, 4HO$, ..	76·2	

The difference between the two states of the sulphates is probably greater as stated than it actually is. We have already shown that the magnesian sulphates with seven atoms of water do not possess a volume of 77·0, because two of the atoms possess a volume of 9·8 instead of 11·0; and perhaps a similar circumstance tends to reduce the volume of the subsulphates. Similar instances of replacement of water by a metallic oxide are seen in other parts of the table. We have already shown that nitrates of copper and bismuth possess a volume of 58·8 or 9·8 × 6. We have also seen instances in which 9·8, the volume of ice, in feeble compounds, became changed into the volume 11 when the salt entered into combination. In this point of view, the subsalts $MO, NO_5 + HO + 2MO$ become assimilated to the hydrated nitrates $MO, NO_5 + HO + 2HO$, the number of volumes in both cases being the same, the only difference being, that, in the former case, the salts are multiples of 11, and in the latter of 9·8, or the volume of ice. The hydrated type affects six volumes, and so do the subnitrates, as will be seen by the following table.

$CuO, NO_5, HO+2CuO$,	volume 65·9	or	11×6
$BiO, NO_5, HO+2BiO$,	... 66·0	...	11×6
$HgO, NO_5, HO+2HgO$	... 65·6	...	11×6

We have further evidence of the equivalency of water to the metallic oxide in anhydrous nitrate of bismuth, which has a volume of 55·0 or 66−11; the formula for the salt being BiO, NO_5+2BiO. The conversion of the volume 9·8 into 11 is by no means uncommon, and is again seen in the subchromates of lead. Chromate of lead has a volume sensibly the multiple of 9·8.

	By experiment.			By calculation.	
Chromate of lead	29·0 ...	5·653	...	9·8 × 3 = 29·4 ...	5·577

Boullay gives the specific gravity of oxide of lead as 9·5, which indicates the volume 11·7, a number not far from 11, which we take as the unit volume. Subchromate of lead consists of one equivalent of the neutral chromate united to one of oxide of lead, but the three volumes of ice in the former have changed in the subsalt to 11 × 3, and the same is the case in the mineral melanchroit, which contains two equivalents of chromate of lead united to one of oxide of lead.

Subchromate of lead, $PbO, CrO_3 + PbO = 44$ or 11×4

Melanchroit, $2 (PbO, CrO_3) + PbO = 76{\cdot}5$ or 11×7

In these salts we clearly see that oxide of lead takes up the volume and plays the part of an atom of water, although we are ignorant of their hydrated types. The same function of an oxide is seen in turpeth mineral, in which the 2HgO, attached to HgO, SO_3, assumes the volume of two atoms of water, $22 + 22 = 44$. There can be little doubt from the previous examples of the equivalency of CuO, ZnO, BiO, HgO, and PbO, not only to each other but also to water; and this will be still more strongly seen by placing the volumes of these and other anhydrous magnesian sulphates along with the volume of sulphate of water itself, as deduced from bisulphate of potash.

Sulphate of	Water, vol. by experiment		22·0
...	Zinc	...	21·8
...	Copper	...	22·0
...	Iron	...	24·0
...	Cobalt	...	22·0
...	Mercury	...	23·1

The only cases in which there is an appreciable difference from sulphate of water are those of sulphates of iron and mercury, neither of which salts can be obtained without difficulty perfectly pure in an anhydrous state.

But if the equivalency of the magnesian metals to each other and to hydrogen be left in any doubt by the preceding table, this doubt would be entirely removed by a consideration of the magnesian chlorides. The strongest muriatic acid obtained has, according to Thomson, a specific gravity of 1·203, and contains 40·66 per cent. of dry muriatic acid; which is equal to 5·91, obviously six atoms of water to one of muriatic acid, as pointed out by Kane. The atomic weight of this compound, divided by its specific gravity, is $\frac{90{\cdot}47}{1{\cdot}203} = 75{\cdot}2$, which is not far from 72·0 or 9 × 8, considering that the result remains uncorrected for expansion; this gives a volume of 18·0 or 9 × 2 for muriatic acid. The acid which possesses a constant boiling-point and distils over unchanged has a specific gravity of 1·094, and contains 19·19 per cent. of absolute acid, according to Davy, and 20·44 per cent. according to Thomson. The mean of their results indicates the acid to contain 16·4, or nearly 16 atoms of water. Now $\frac{180{\cdot}47}{1{\cdot}094}$ = 165, which is not far from 162, the volume of 9 × 18, making for the volume of muriatic acid in strong solutions 18·0 or 9 × 2, a result the same as that obtained by the last calculation. These results, and that given in a previous section, along with the fact that hydrochloric acid gas has twice the volume of steam, leave no doubt that muriatic acid affects two volumes; and converting the liquid into the solid volume, we have a volume of 22·0 or 11 × 2 as the atomic volume of solid muriatic acid. By contrasting this volume with the experimental results on the magnesian chlorides, we find a very great similarity.

Chloride of	Hydrogen,	volume	22·0	or 11 × 2
...	Cobalt	...	22·2	...
...	Magnesium	...	22·1	...
...	Calcium	...	22·4	...
...	Copper	...	22·0	...
...	Mercury	...	22·0	...

In dilute solutions muriatic acid affects only one volume, and this has been shown to be also the case with chlorides of copper and cobalt. Whether nitrate of water and nitrate of a magnesian oxide possess the same volume it is difficult to decide. Nitrate of water in the acid of specific gravity 1·42 seems to affect four volumes, and this acid, $HO, NO_5, + 3HO$ is constituted on the same type as $CuO, NO_5 + 3HO$; yet $\frac{90{\cdot}2}{1{\cdot}42} = \frac{63}{9} = 7$, which gives four volumes for HO, NO_5, while nitrate of copper certainly does not possess more than three volumes. Nitrate of water calculated on weak acids has three volumes; but there being no good fixed point upon which to make the calculation, we must leave at present this point undetermined.

An important question now arises as to the truth of the supposition that two atoms of a magnesian metal are equal to one of the family of which potassium stands as the type. In calomel and chloride of ammonium we have a direct case in point, and the similarity of volumes is very striking.

		Diff.
Chloride of ammonium, NH_4Cl	34·0	0·8
Calomel, Hg_2Cl	33·2	

In this case we have taken chloride of ammonium, because KCl assumes the volume of four atoms of ice.

Subchloride of copper, like NH_4Cl, possesses three volumes, according to Karsten's experiments and our own, but these three volumes are multiples of 9·8, and not of 11·0.

	By experiment.		By calculation.	
Subchloride of copper, vol.	29·2	3·376	$9·8 \times 3 = 29·4$	3·363

Another illustration is furnished in sulphate of protoxide of mercury and sulphate of potash.

		Diff.
Protosulphate of mercury, vol.	33·20	0·15
Sulphate of potash	33·05	

These are instances in which two atoms of a magnesian metal are at once shown to be equivalent to one of a metal of the potash family; but it does not thereby preclude the possibility of two atoms of a magnesian *oxide* being equivalent to one atom of potash. For example, a magnesian sulphate, MgO, SO_3, affects a volume 22, or 11 × 2, while the same salt united to an atom of constitutional water has the volume 33, or MgO, SO_3HO becomes equal to KO, SO_3, which also possesses a volume of 33. The most striking case, however, is seen when crystallized subnitrate of lead is compared with nitrate of potash.

Nitrate of potash, KO, NO_5, vol.	49·0
Subnitrate of lead, $PbO, NO_5 + PbO$, vol.	49·0

The fact that two atoms of a magnesian oxide are equivalent to one of potash, appears to find its explanation in the circumstance that we uniformly find the salt of potash assuming one volume greater than the corresponding salt of magnesia. Hence, as the volume of the oxides corresponding to the latter body is equal to unity, the equivalency of two of their atoms to one of potash becomes a matter of necessity.

To sum up these remarks, we conceive (1.) that Graham has taken the correct view in supposing subsalts to represent hydrated salts, in which water has been replaced by a metallic oxide; and (2.) that the volume of two atoms of a metal of the magnesian family, in which we include hydrogen, is equal in volume to one of the potassium group; or two atoms of the former oxide, when combined, to one of the latter. We are now in a condition to consider the salts of ammonia.

It is quite unnecessary to remind chemists that there are two rival theories regarding the constitution of ammoniacal salts. One of them, proposed by the profound Berzelius, is that the salts of ammonia contain a hypothetical radical termed ammonium, consisting of one equivalent of nitrogen and four equivalents of hydrogen. Sulphate of ammonia is to be viewed as sulphate of oxide of ammonium, the latter hypothetical body being equivalent to potash; and hence the isomorphism between the salts of potash and ammonia. The other view of the constitution of ammonia is that proposed by Kane, and so elaborately supported by him in his paper on subsalts and ammoniacal compounds*. Dr. Kane supposes that an ammoniacal salt is formed on the type of a magnesian salt, carrying along with it constitutional water.

Sulphate of copper . . .	CuO, HO, SO_3
Sulphate of ammonia . . .	HO, NH_2H, SO_3

On this view, amide of hydrogen is equivalent to, and plays the part of an atom of water. If this be the case, amidogene must be analogous to oxygen, and ammonia and a magnesian oxide must possess the same atomic volume. At present all this is purely hypothetical, and must be subjected to the test of experiment before we can admit it as a safe foundation on which to rear a theory. The means of deciding this question seemed to present itself in an examination of the amides of mercury, and of the crystallized salts of copper and zinc, in which the ammonia is present *quasi* ammonia; and such compounds have been described in the beautiful researches of Kane on this subject. Wöhler's white precipitate, $HgCl + NH_3$ seems to be constituted in the most simple manner, and possesses a volume of 33·0, which, deducting the volume 22·0 for HgCl, leaves 11·0, or unity, as the volume of NH_3. But again, white precipitate, $HgCl + HgNH_2$, affects a volume of 44·6 by experiment, which, deducting 22·0 for HgCl, leaves $HgNH_2$ also equal to 22·0, and yet the latter compound should correspond in volume to NH_2H. The heavy yellow powder obtained by boiling white precipitate with water has a volume of 66·0, and is constituted according to the formula $(HgCl + HgAd) + 2HgO$; so that deducting 44·0, the ascertained volume of the double amide and chloride, 22 or 11×2 remains for *two* atoms of HgO, giving the same result as in the former subsalts, viz. the equivalency of HgO to HO, but not to $HgNH_2$; and another proof of this is afforded in the

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* Transactions of the Royal Irish Academy, vol. xix. part 1.

reduction of the volume of ammonia turpeth. From this circumstance, the view would appear probable that amide and chloride of mercury are equivalent, and hence would follow the equivalency of chlorine to amidogene. This receives further support from the volume of the double subamide and subchloride of mercury, Hg_2Cl+Hg_2Ad, which has a volume of 66·8, according to experiment. Calomel itself possesses the volume 33·2, which, deducted from that of the salt just described, gives 33·6 as the volume of Hg_2Ad, showing the complete equivalency of the latter to the subchloride.

It has been shown that chloride of mercury and chloride of hydrogen are equivalent, and it now remains to be shown by direct proof that amide of hydrogen (ammonia) is embraced in the same category. Ammonia-chloride of copper, $CuCl+NH_3$, was found with a volume of 39·2 or 9·8 × 4; chloride of copper itself affects two volumes, which leaves for Ad, H, as deduced from this salt, also two volumes. But the ammonia in $CuCl+2NH_3+HO$, if we were to suppose the salts constituted in a manner so simple as expressed by their empirical formulæ, would only have a volume of 33·0 for two atoms, or $1\frac{1}{2}$ volume for each.

In proceeding further, it will be seen that we involve ourselves in inextricable difficulties, if we insist upon the equivalency of NH_2H to HO; or suppose the ammoniacal salts, such as those described, to be constituted on the type of the hydrated salts. Thus, ammonia-sulphate of copper, CuO, SO_3+2NH_3+HO, has a volume of 68·6 or 9·8 × 7 in its solid state, and of 54 or 9 × 6 when in solution. Deducting 19·6 for CuO, SO_3, and 9·8 for HO, there is again left 39·2, or 9·8 × 4 for *two* atoms of ammonia. The simple salt, CuO, SO_3+HN_3, has a volume of 39·2, which leaves 19·6, or 9·8 × 2 for one atom of ammonia; but the same salt, when combined with three atoms of water, yields the volume 63·6, which would lead us to suppose that one atom of water is equal to one atom of ammonia. We also find ammonia with the volume 11, or unity, when calculated from the observed volume of hydrated sulphate of ammonia. But in the ammonia-chromate of silver, AgO, CrO_3+2NH_3, and in its corresponding sulphate, we find on deducting 33·0, or 11 × 3 for the salts themselves, the residual 33·0 for *two* atoms of ammonia. Again, however, we become perplexed by finding that the ammonia in ammonia-pernitrate of mercury possesses the volume of an atom of water. Thus, then, by considering the volumes of the ammoniacal salts as containing their ammonia *quasi* ammonia, and as constituted on the type of the hydrated salts, we obtain the contradictory and absurd result, that ammonia, though often taking a volume equal to unity, sometimes possesses a volume of $1\frac{1}{2}$, and occasionally two volumes. It is pretty certain, from these contradictory results, that the salts are not constituted on the hydrated type.

Graham has thrown out the ingenious idea*, that the salts now referred to may actually contain an ammonium in which the fourth equivalent of hydrogen is replaced by an equivalent of a magnesian metal. Thus $CuO, SO_3 + NH_3$ is constituted, according to Graham, NH_3Cu, O, SO_3, on the type of sulphate of ammonia, NH_3H, O, SO_3. There is nothing whatever opposed to this view in Kane's researches, as he himself admits, the only difference being that he considers the said salts to contain oxide of copper and water united to amide of hydrogen, instead of to *cuprammonium* and oxide of ammonium, according to the views of Berzelius and Graham. While, therefore, Kane admits that amide of hydrogen is very closely allied to chloride of hydrogen, he claims for the former body an equally close alliance to water, by asserting that it is equivalent to a magnesian oxide, although it is difficult to conceive why chloride of hydrogen has not a right to a similar claim. Amide and chloride of mercury have undoubtedly the same volume, viz. 22·0, and chloride of hydrogen also enjoys the same number; but water does not in any case do so. On this point alone, then, are we at issue with Kane, for there are many proofs that there is extreme probability in the view propounded by him of the presence of NH_2H and HO in ammoniacal salts. On the former view alone do we contest the accuracy of the opinion, leaving for future consideration and research, to which we are now devoting ourselves, a more defined notion of the reason why NH_2H and HO are equivalent in many instances, *not* in *all,* to potash. We have already stated the incongruous results which would flow from the conception that ammonia was simply attached to the salts examined. It is true that Kane gives to some of them a constitution more intimate, and when he does so his theory accords with our results. But his conception of the equivalency of NH_2H to HO has led him in other instances to attach the ammonia to the salt in place of water, and it is from these cases that we dissent. If he merely means that NH_2H can replace HO in a compound, as KO, SO_3 does in a magnesian sulphate, then we cease to differ, because the resulting compounds do not remain in strict parallelism; the only point we argue against being that HO and NH_2H are equivalent. Thus we have, supposing all of them to affect the primitive volume 9·8,—

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[*] Graham's Elements of Chemistry, p. 416.

$HgO, NO_5 + HgO + 2HO \quad = 6$ vol.
$HgO, NO_5 + HO \quad + 2HgO = 6$ ...
$HgO, NO_5 + NH_3 + 2HgO = 7$ ...

The first two members of the series have the same number of volumes, because HgO and HO are equivalent, and the last salt should also affect the same, if $NH_2H = HO$. But if we consider the last salt as equal to nitrate of ammonia, in which HgO replaces HO, then it becomes intelligible.

NH_2H, HO, NO_5 affects $5 + 2$ of $HO \quad = 7$
NH_2H, HgO, NO_5 ... $\quad 5 + 2$ of $HgO = 7$

On the same principle we would arrange the other ammoniacal compounds. Thus $CuO, SO_3 + NH_3$ obviously ought to be arranged NH_2H, CuO, SO_3, corresponding to NH_2H, HO, SO_3, anhydrous sulphate of ammonia, and both affect, as they should do on this formula, four volumes. We observed a very decided peculiarity in sulphate of ammonia; for while in its hydrated condition the NH_4O, SO_3 could only be equal to three volumes, in its anhydrous state, or when in combination with salts, it assumed four volumes. The latter peculiarity attends the *alpha* ammonia-sulphate of copper, and is shared also by ammonia-sulphate of zinc, while the hydrate assimilates itself to $NH_4O, SO_3 + HO$.

$NH_2H, HO, \ SO_3 = 39{\cdot}2$
$NH_2H, CuO, SO_3 = 39{\cdot}2$
$NH_2H, ZnO, \ SO_3 = 39{\cdot}2$
$(NH_2H, CuO, SO_3 = 39{\cdot}2) + (HO = 9{\cdot}8) + (NH_3 = 19{\cdot}6) = 68{\cdot}6$
$NH_2H, HO, SO_3 + HO = 44$
$(NH_2H, CuO, SO_3 + HO = 44) + (2HO = 19{\cdot}6) = 63{\cdot}6$

In ammonia-nitrate of copper we have an instance in which the ammonia may be present either as nitrate of ammonia or as ammonia; for if we suppose the volume 68·5, which obviously indicates $9{\cdot}8 \times 7 = 68{\cdot}6$, to be made up of $CuO, NO_5 + 2NH_3$, we must assume that two atoms of ammonia are equal to four atoms of ice, for we already have seen that CuO, NO_5 affects three volumes. On the supposition that the compound contains a substance equivalent to nitrate of ammonia, the volumes are equally intelligible.

$NH_2H, HO, NO_5 \ = 49{\cdot}0$
$NH_2H, CuO, NO_5 = 49{\cdot}0 + NH_2H = 19{\cdot}6 = 68{\cdot}6$

Perhaps, however, the clearest instances are seen in the ammoniacal chromate and sulphate of silver. AgO, CrO_3 and AgO, SO_3 affect a volume of $9{\cdot}8 \times 3$, and supposing a transformation into multiples of 11, of which we have seen frequent instances, $2NH_3 = 33{\cdot}0$, or $NH_3 = 16{\cdot}5$, or $1\frac{1}{2}$ times the number which we assume as unity. But on the supposition that AgO takes the place of HO, the difficulty ceases.

$NH_2H, HO, SO_3 = 44$
$NH_2H, AgO, SO_3 = 44 + NH_2H = 66$
$NH_2H, AgO, CrO_3 = 44 + NH_2H = 66$

Perhaps the most anomalous salt in the whole series examined is chloride of ammonium, which actually decreases one volume in becoming solid, 9 × 4 in solution being 11 × 3 in the state of a salt. Chloride of potassium refuses to share this anomaly, and we accordingly find it 9·8 × 4, and NH_4Cl associates itself to KCl in the double salts. Four volumes for NH_4Cl is undoubtedly what we should expect from its composition, and from that number being affected in solution and in its double salts. We also see the three volumes entering into *alpha* ammonia-chloride of copper, although the *beta* ammonia-chloride, according to our results, seems singularly enough to affect the proper four volumes.

$NH_2H, HCl = 33$
$NH_2Cu, HCl = 33 + NH_2H = 22 + HO = 11 = 66$
$NH_2Cu, HCl = 38{\cdot}4 \text{ or } 9{\cdot}8 \times 4 = 39{\cdot}2$

The double amides and chlorides, as we have already shown, affect the same number of volumes as NH_4Cl when in solution, and might be placed on the same type as NH_2Hg, HCl. Without denying that NH_2H and HO may be so intimately associated in the ammoniacal salts as to form the hypothetical body oxide of ammonium, we would call attention to the facts, which show that the resulting volumes of the ammoniacal salts are made up of the volumes of the hydrated acid and amide of hydrogen. It by no means militates against that view, that in hydrated sulphate of ammonia we have one volume in solution less, and also in the state of a solid, than should result from the combination of these two. CuCl has undoubtedly *per se* two volumes, just as HCl has in a concentrated state, or as NH_2H has in combination. But the CuCl in CuCl + 2HO possesses only one volume, the other having disappeared in the water; and HCl itself has only one volume in dilute solutions. The disappearance of one volume in combination with water is by no means so surprising as the disappearance of the volumes of 23 atoms of the constituents of alum in the water in which it is dissolved, especially when we find the salt under consideration, sulphate of ammonia, vindicating its proper volume when in combination. The oxalate of ammonia has its *proper* volume, just as has anhydrous sulphate of ammonia; the only exception is the decidedly anomalous salt—chloride of ammonium, although this also ceases to be anomalous in the double chlorides. By placing together the volumes of the hydrated acids and those of the ammoniacal salts, it will be seen that the latter are made up of the volumes of the hydrated acid united to amide of hydrogen affecting two volumes, like HCl.

Sulphate of ammonia $= HO, SO_3 = 2 + NH_2H = 2 = 4$
Nitrate of ammonia $= HO, NO_5 = 3 + NH_2H = 2 = 5$
Oxalate of ammonia $= HO, C_2O_3 = 2 + NH_2H = 2 = 4$

All the ammoniacal salts which we have described in this section may be arranged in a similar way with a like result.

We do not profess to have resolved the cause of the equivalency $HO + NH_3$ to KO; nor do we insist that they do not enter into more intimate union to form NH_4O. It must not be left out of consideration, however, that in almost every instance the ammoniacal salt affects one volume in solution more than the corresponding salt of potash, and that the number of volumes of the latter becomes augmented by one in passing from the liquid to the solid state, while the number of volumes of the ammoniacal salt remains unchanged. It requires a more minute knowledge of the constitution of salts than we now possess to decide the question at issue.

Conclusion.

Although we have examined many other salts than those described in the previous pages, with results quite confirmatory of our views, we do not feel warranted in extending our memoir, already much too long. We therefore conclude by summing up, in the form of propositions, the laws which we consider regulate the volumes of salts. At the same time we do so with strict reference to the salts which we have described, deprecating their hasty generalization, being ourselves quite satisfied that there are peculiarities in other cases, which must be subjected to close examination. This being only the first of several memoirs on the same subject which we intend to lay before the Society, we do not present this investigation as being in itself complete.

Prop. I.—*Compounds dissolved in water increase its volume for every equivalent either by* 9, *or by multiples of* 9.

This, in other words, signifies that the volumes of salts in solution are either equal to each other, or are multiples of each other; for 9, being the volume of nine grains, or an equivalent of water, is merely assumed as the standard of comparison.

a. Certain salts, such as the magnesian sulphates, the alums, &c., dissolve in water without increasing its bulk more than is due to the liquefaction of the water which they themselves contain; the anhydrous salt taking up no space in solution.

b. Anhydrous salts, or salts containing a small proportion of water, affect a certain number of volumes in solution, which pass along with them unchanged into their union with other salts.

c. The volume occupied by double salts when dissolved is the sum of volumes occupied by their constituents when separate, with the exception of certain cases described in the previous sections.

Prop. II.—*The volume occupied by a salt in the solid state has a certain relation to the volume of the same salt when in solution; and has also a fixed relation to the volume occupied by any other salt.*

a. The volume of an equivalent of any salt is either 11, or a multiple of 11, or of a number very nearly approaching the number 11.

b. Or the volume of a salt is 9·8, or a multiple of 9·8, or, in other words, of the volume occupied by an equivalent of solid water (ice).

c. Or the volume of a salt is made up of a certain multiple of the number 11, added to a certain multiple of the number 9·8.

On each of these heads we would offer a few remarks.

With two assumptions we have been enabled to connect with each other the volumes occupied by all the salts examined by us in the previous sections. These assumptions are, that the divisor for the volumes of salts is either 11, or a number very nearly approaching to it, or that the divisor is 9·8, the volume of ice itself.

We have been guarded in stating positively that the first divisor is absolutely 11, because we do not in the present memoir enter into the connection between this number and the volume of ice, 9·8. To show, however, that our experiments agree with those of recent accurate experimenters, and that the number 11, which we have at present to announce empirically, cannot be wide from the truth, we append the theoretical and experimental results upon the alums, which we stated to possess twenty-five volumes, in which therefore any considerable error in the number 11 would be multiplied by 25; and plainly show itself in the results. Notwithstanding this severe test, it will be seen that the theoretical and experimental numbers are actually within the errors of the balance.

	Theoretical. sp. gr.		By Kopp's* experiments.		By our experiments.		Mean of experiments.
Potash alum .	1·727	...	1·724	...	1·726	...	1·725
Chrome alum	1·833	...	1·848	...	1·826	...	1·837

The number 11 must then be very near the truth, if it be not absolutely the truth. We now append an equally severe test for our view that the volumes of many salts are multiples of 9·8, the number representing the volume of ice. If there be an error in this number, it must become very notable in the phosphates and arseniates, when multiplied by 24, or in carbonate of soda when multiplied by 10. Perhaps sugar itself will form as severe a test as could be desired, for we proceed on the extraordinary fact that the 12 atoms of carbon in sugar have ceased to occupy space, and that the bulk of an atom of sugar is just the bulk of H_{11} O_{11}, or its 11 atoms of hydrogen and oxygen, *quasi* water, frozen into ice.

	Theoretical sp. gr.	Sp. gr. according to our experiments.	Other authorities.	
Carbonate of soda .	1·463	1·454	1·423	Haidinger.
Phosphate of soda .	1·527	1·525	1·514	Tünnerman.
Subphosphate of soda	1·622	1·622	none	
Arseniate of soda .	1·713	1·736	1·759	Thomson.
Subarseniate of soda	1·808	1·804	none	
Cane-sugar . . .	1·591	1·596	1·600	Schübler & Renz.

Thus even in salts so difficult to obtain in a proper degree of hydration free from mechanical water, as those given in the above table, the difference between the theoretical and experimental numbers is not greater than might have been expected.

We give one other class of salts to illustrate position *c* in Prop. II., there being in these salts a certain number of volumes represented by 11, and a certain number by 9·8, CuO, SO_3 representing the number of volumes with the divisor 11.

	Theoretical sp. gr.	Sp. gr. by our experiments.	Sp. gr. by other authorities.	
Sulphate of copper .	2·270	2·254	2·274	Kopp.
... zinc . .	1·926	1·931	1·912	Hassenfratz.
... iron . .	1·854	1·857	1·840	*Idem.*
... magnesia	1·660	1·660	1·660	*Idem.*
... nickel .	2·033	...	2·037	Kopp.

We have selected these three classes of salts as being the most severe tests which we could apply to our theory, and any chemist who has had experience in this subject will at once admit that the theoretical and experimental numbers are

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[*] *Annalen der Pharmacie*, Bd. xxxvi. S. 10.

as near each other as the estimation of specific gravities by any two different experimenters. We do not rest the claims of our theories on our own experiments, but are willing to admit the accuracy of other experimenters, especially of Karsten, Hassenfratz, Kopp, and others who have preceded us on this subject*; while at the same time we believe that our methods of taking specific gravities have enabled us to introduce more uniformity into the results. The simplicity of the methods themselves is due to Bishop Watson, who was the first to take specific gravities by the increase in the stem of an instrument; and to Holker the suggestion is due of using a saturated solution instead of the water employed by Watson.

We conceive that the primitive volume 9·8 is transformable into the primitive volume 11, and *vice versâ*, and for this reason we sometimes see sulphate of ammonia $9{\cdot}8 \times 4$, at other times, in combination as in bisulphate of ammonia or the anhydrous double sulphates, it is 11×4; and numerous other instances of transformation are presented in the previous sections.

The liquid volume being to the solid volume either as 9 : 11 or as 9 : 9·8, these numbers, used as the divisor for the liquid and solid volumes respectively, usually yield the same quotient. Thus the liquid volume of sulphate of copper is 45, its solid volume is 55. $\frac{45}{9} = 5$, and $\frac{55}{11} = 5$; so that we say the salt affects the same number of volumes in the liquid and in the solid state. In the same manner subphosphate of soda has a volume of 216 in solution and of 235 in the state of salt. Now $\frac{216}{9} = 24$ and $\frac{235}{9{\cdot}8} = 24$, so that the number of volumes affected in solution and in the solid state are exactly the same. This is a general rule and a powerful argument of the accuracy of our position. The rule has exceptions in salts of potash, in which the volumes are increased by one volume on becoming solid; thus KO, SO_3 $\frac{18}{9} = 2$ in solution, and $\frac{33}{11} = 3$ in the solid state. This is

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* The only decided difference which we found from other experimenters is in the case of hydrated salts. Thus our determination of the volumes of the double magnesian sulphates and sulphate of potash (Table VI.) differs from Kopp's experiments as 99 : 103. These salts contain from 3 to 4 per cent. of mechanical water, as Graham long ago pointed out (Trans. R. S. E. vol. xiii. p. 12), and the neglect of this in Kopp's experiments has probably caused the difference. We take this opportunity of stating that when more than one specific gravity is given by us, the salts have been prepared at different times; in many instances this is not the case, but in much the largest proportion it is so.

not an accidental variation, but an actual augmentation of one volume, as is proved by the potash alums, in which KO, SO_3 has ceased to occupy space in solution, but on the crystallization of the alum the volume becomes increased by one, obviously owing to this peculiarity of KO, SO_3; thus alum in solution $\frac{216}{9} = 24$, becomes $\frac{275}{11} = 25$ in the state of a salt.

This peculiarity is very striking, especially in the case KO, CO_2, which with a volume of $\frac{33}{11} = 3$ as a solid, becomes $\frac{9}{9} = 1$ as a liquid. Let us endeavour to conceive the extraordinary amount of power exerted in this case; the water in the volumenometer, on dissolving an equivalent of KO, CO_2, descends from 33 to 9, so that a bulk of solid matter = 24 grains of water disappears within it. If we would compare the force to that which would be required to compress the water into this diminished bulk, we must deal in numbers of a magnitude truly immense. We have always been accustomed to view as an exception the expansion of water on becoming solid, but now we see with Longchamp, that the rule is universal; the salt (muriate of ammonia excepted?) takes up more space as a solid than it does in its liquid state in solution.

We have stated that we desire not to be held responsible for any rash generalization of these laws, which we do not extend at present beyond the salts examined by us. Let us consider the volumes of the ammonia alums, as an example of the danger of applying either of the laws without a proper comprehension of them. These volumes are certainly above 275, the volumes of the potash alums, and are between 279 and 280, according to our experiments and those of Kopp. Now let us suppose that the four volumes of NH_4O, SO_3 are represented in the alums, and that only $Al_2O_3, 3SO_3$ has ceased to occupy space, as it in fact does when hydrated, then an ammonia alum $Al_2O_3, 3SO_3 + NH_4O, SO_3 + 24HO$ may be viewed as $9 \cdot 8 \times (24 + 4) = 279 \cdot 4$, and the specific gravities would countenance this idea.

	Sp. gr. by Theory.	Sp. gr. by our experiments.	Sp. gr. by Kopp's experiment.
Ammonia alum . .	1·626	1·625	1·626
Ammonia iron alum	1·721	1·718	1·712

These results certainly approach the theoretical number very closely; and the theory may represent the truth. But at the same time it is difficult to believe that the ammonia alum is constituted on a different type from the potash alum. We might suppose that the only variation between them is the difference between the volumes of KO, SO_3 and NH_4O, SO_3, or the difference between 11 × 3 and 9·8 × 4. This difference, 6·2, added to the volume of potash alum 275 + 6·2 = 281·2, which is not very wide from the experimental results, and would give the specific gravity by theory for ammonia alum 1·616, and for ammonia iron alum 1·711. These are points which require further inquiry.

We do not refer here to the minor views embraced in the preceding investigation, being anxious principally for inquiry and confirmation into the three main theories propounded. With one assumption for the volume in solution, and with two assumptions for the volumes of solids, we have been enabled to explain, as we trust, the specific gravities detailed in the previous sections. We might perhaps with propriety indulge in speculation, and apply these laws in explanation of isomorphism and dimorphism, but we prefer the safer course of trusting to experimental investigation, part of which we shall in a short time lay before the Society in an inquiry upon the expansion of solutions, and on some other points connected with this important subject.

LXXV. *Analyses of the Ashes of Sugar-Canes from the West Indies.* By John Stenhouse, *Ph.D.**

WITHIN the last few years, it has been satisfactorily ascertained, through the labours of the ablest experimenters in agriculture, that the fertility of a soil is dependent on its containing certain mineral substances which are indispensable to the nourishment and full development of the plants which grow upon its surface. It is evident therefore that a knowledge of the constituents of the ashes of plants, as furnishing us with a view of all the inorganic substances which they derive from the soil, and which they remove from it in harvest, must always be of the utmost importance for the guidance of the scientific agriculturist. During the last three or four years, the ashes of the larger number of our cultivated plants have been subjected to very careful analysis, chiefly through the exertions of Professor Liebig and his scholars.

As hitherto, however, the sugar-cane has been almost entirely overlooked, I was induced, some six months ago, to apply to some of the leading colonial proprietors to furnish me with specimens of canes from different localities. I need scarcely say that my request was most courteously and promptly complied with. The following are the results of twelve analyses

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* Communicated by the Author.

of the ashes of sugar-canes from various localities in the West Indies.

The first table (A) contains the results of these analyses, in which, after the insoluble matter, consisting of coal and sand, and the loss (iron), have been abstracted, the constituents are calculated in the relative proportions per cent. which they bear to each other. In the Table A. an error has been committed, by calculating that portion of the sodium and potassium which undoubtedly existed in the canes in the state of chlorides, as potassa and soda, thus introducing an excess of oxygen, amounting in Nos. 3, 9 and 10 to 2 per cent., and in No. 8 to almost 4 per cent. To remedy this error, a second table (B) has been constructed, in which that portion of the potassium and sodium, which in the canes are united to chlorine, are calculated as chlorides.

A. Per-centic results after abstracting the charcoal and sand.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Silica …………	45·78	42·81	45·50	40·85	46·24	49·74	44·68	17·04	25·78	51·93	47·79	54·22
Phosphoric acid.	3·75	7·97	8·16	4·53	8·12	6·53	4·84	7·12	6·06	13·28	2·85	7·96
Sulphuric acid…	6·64	10·92	4·56	10·80	7·48	6·37	7·67	7·70	5·94	3·30	5·25	1·91
Chlorine ………	2·70	1·02	8·85	5·47	2·39	2·36	4·34	14·33	9·70	2·40	8·75	2·70
Lime …………	9·13	13·17	8·73	8·96	5·75	5·07	4·45	2·26	5·74	10·59	11·40	14·27
Magnesia ………	3·65	9·86	4·41	6·84	15·53	12·94	11·78	3·80	5·36	5·61	5·51	5·27
Potash …………	27·32	11·99	15·00	21·39	11·87	13·62	16·81	39·51	37·40	10·04	17·29	11·59
Soda…………	1·03	2·26	4·79	1·16	2·62	3·37	5·43	8·24	4·02	2·85	1·16	2·08

B. Contains the results of the same analyses as Table A., but the chlorine is represented as united to sodium and potassium.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Silica …………	45·97	42·90	46·46	41·37	46·48	50·00	45·13	17·64	26·38	52·20	48·73	54·59
Phosphoric acid.	3·76	7·99	8·23	4·59	8·16	6·56	4·88	7·37	6·20	13·04	2·90	8·01
Sulphuric acid…	6·66	10·94	4·65	10·93	7·52	6·40	7·74	7·97	6·08	3·31	5·35	1·93
Lime …………	9·16	13·20	8·91	9·11	5·78	5·09	4·49	2·34	5·87	10·64	11·62	14·36
Magnesia………	3·66	9·88	4·50	6·92	15·61	13·01	11·90	3·93	5·48	5·63	5·61	5·30
Potash …………	25·50	12·01	10·63	15·99	11·93	13·69	16·97	32·93	31·21	10·09	7·46	11·14
Soda…………	……	1·39	……	……	0·57	1·33	1·61	……	……	0·80	……	……
Chlor. of potass.	3·27	……	7·41	8·96	……	……	……	10·70	11·14	……	16·06	0·84
Chlor. of sodium.	2·02	1·69	9·21	2·13	3·95	3·92	7·25	17·12	7·64	4·29	2·27	3·83

Nos. 1, 2, 3 and 4 were very fine full-grown canes from Trinidad, consisting of stalks and leaves, but without the roots. I am indebted for them to the kindness of Messrs. Eccles and Co. of Glasgow. Nos. 5, 6 and 7 were very similar canes from Berbice, furnished me by James Laing, Esq., from his estates in that colony. No. 8, consisting of the stalks without leaves from Montrose estate, Demerara, was kindly given me by William Gourlie, Jun., Esq. No. 9, consisting of full-grown canes with but few leaves, from the island of Grenada, was given me by Mungo Campbell, Jun., Esq. No. 10, from Hampden estate, Trelawny, Jamaica, consisted of transparent canes in full blossom, grown about six miles from the sea, and manured with cattle-dung. No. 11 consisted also of transparent canes from Ironshore estate, St. James's, Jamaica; they grew about 200 yards from the sea, and were old rattoons, also manured with cattle-dung. No. 12, from Content estate, St. James's, Jamaica, were young transparent canes, grown about three and a half miles from the sea, and manured with cattle-dung, guano and marl. For these last three parcels I am indebted to the kindness of William Stirling, Esq. of Kenmure.

The canes after being carefully cleaned were dried in a stove, then charred on a plate of cast iron, and finally incinerated in a Hessian crucible at a very moderate heat, so as to prevent the ashes from melting. The method adopted in these analyses was chiefly that of Messrs. Fresenius and Will, except in regard to the phosphoric acid.

The method of proceeding was as follows:—

I. A quantity of the ash (1 gramme) was boiled with nitric acid, and the filtered liquor was employed to determine the chlorine with nitrate of silver in the usual way.

II. Another quantity (about 1·5 gramme), dissolved in muriatic acid, was employed for the determination of the sulphuric acid.

III. A third quantity (about 5·0 grammes) was boiled in a silver basin for about an hour with a very concentrated lye of caustic soda, to bring the silica into a soluble state, as the ashes were not completely decomposed by boiling with acids. The alkaline mass was then digested with nitric acid, evaporated to dryness, again digested with dilute nitric acid, and filtered. The silica, coal and sand in the insoluble residue were determined in the usual way. The liquor which passed through the filter was employed for the determination of the phosphoric acid, lime and magnesia. The whole of the liquor was measured in a graduated cylinder.

a. The sulphuric acid was determined in a portion of the liquor by chloride of barium.

b. Another measured quantity of the liquor, after being neutralized by ammonia, was precipitated by acetate of lead, and the phosphoric acid it contained determined according to the method given by Berzelius. The precipitate was well-washed with boiling water to free it entirely from chloride of lead, and the sulphate of lead precipitated along with the phosphoric acid was calculated from the quantity of sulphuric acid already found (in III. *a*), and was of course subtracted.

c. The liquor from which the phosphate of lead had been separated (*b*), when freed from lead by sulphuretted hydrogen, and rendered alkaline by ammonia, was employed for the determination of the lime and magnesia, by means of oxalate of ammonia and phosphate of soda in the usual way. In the last five cases the determination of the phosphoric acid was controlled by the process lately recommended by Fresenius with sulphate of magnesia. (See Liebig's *Annalen*, July 1845, and *Chemical Gazette* for October 15.)

IV. A fourth quantity of ashes (about 2–3 grammes) was employed to determine the alkalies. It was digested with an excess of barytes and a little water in a silver basin for about two hours. The alkaline mass was acidified with muriatic acid, and evaporated to dryness. It was then redissolved in distilled water, and caustic barytes added to the solution, to precipitate all the phosphoric acid, sulphuric acid, lime and magnesia it contained. The filtered liquor was then freed from barytes by means of carbonate of ammonia, and the clear solution was evaporated to dryness and heated to redness till nothing remained but the chlorides of the fixed alkalies. The potash was separated from the soda by means of chloride of platinum in the usual way.

It is evident, from the results of the analyses contained in the preceding tables, that the sugar-cane, to ensure its successful cultivation, requires to be furnished with a very large quantity of silicate of potash, and also with a considerable amount of the phosphates. In fact, there are few of our cultivated plants, except perhaps wheat, barley and the other Cerealia, which require so large an amount of these substances. It is not wonderful therefore that the cultivation of the sugar-cane, from the inconsiderate way in which it has hitherto been too often conducted, should have been found rapidly to deteriorate, and in the course of time to exhaust most ordinary soils. I apprehend, however, that this exhaustion of the soil by the cultivation of the sugar-cane is by no means an unavoidable result, and that by means of suitable arrangements successive crops of sugar might be raised without the soils being materially injured. Wheat, or any other kind of grain, necessarily causes the removal of a portion of the valuable inorganic constituents of the soil, such as the alkalies, phosphates, &c., which can only be returned to it indirectly; but with sugar the case is quite otherwise. Sugar is a purely organic substance, consisting of carbon and the elements of water, all of which can be derived from the atmosphere, and contains neither alkalies nor phosphates; so that if the ashes of the canes were carefully collected and returned to the soil in an available state, I do not see why cane crops might not be grown upon the same land almost indefinitely.

Under the present system, however, the crushed canes which have been passed through the sugar-mill to squeeze out the juice they contain, are burned under the coppers of the boiling-house to concentrate the syrup. As this operation requires a strong, brisk fire, the furnace in which they are burned has usually a considerable draught, and therefore so high a heat, that the ashes, from the large amount of alkalies and silica they contain, are invariably fused into a hard, insoluble glass or slag. This slag or glass is usually thrown away, but even if it were pulverized and spread upon the fields, from its almost total insolubility, I apprehend it would be found nearly useless as a manure. The only way to render the cane-ashes available for this purpose, is either to burn the canes in an open fire, at a much lower heat, so as not to fuse them, or, what will probably be found much more advisable in practice, to reduce the slags to a fine powder, and then to fuse them for an hour or so, either in a large iron crucible, or in a reverberatory furnace, with an intimate mixture of one and a half part American black ashes and one of carbonate of soda. This operation, which is by no means either difficult or expensive, has the effect of reconverting the slags into soluble alkaline silicates, and thus rendering them quite available for the nourishment of the canes. After fusion with excess of alkalies, the slags may be readily reduced to powder, and with a small quantity of either bone-dust or guano they will form an excellent manure for either canes or wheat. It is advisable to apply the guano and the fused ashes separately, as the excess of alkali present in the ashes would have the effect of dissipating part of the ammonia of the guano. The ashes of almost any kind of wood, if burned in an open fire, so as not to be melted, would, I believe, be found very beneficial to the sugar-cane, and of course the more potash the ashes contained so much the better.

The fluxing of the cane-ashes with alkalies, just recommended, might, I think, be readily effected in most of the colonies themselves, as a great deal of fuel is by no means necessary, and a bright red heat kept up for an hour or so is amply sufficient; but as the quantity of cane-ashes produced on an estate will not exceed a very few tons in a year, probably not more than four or five, if so much, they might perhaps be advantageously sent to Great Britain, where fuel is much cheaper, and then returned to the colonies in a manufactured state.

Glasgow, November 11, 1845.

LXXVI. *On a New Class of Properties of Lines and Surfaces of the Second Order.* By the Rev. J. Booth, *LL.D., M.R.I.A., Vice-Principal of, and Professor of Mathematics in the Liverpool Collegiate Institution*[*].

It has sometimes been made an object of inquiry with geometers, whether foci and directrices exist as well for the minor as the major axes of a conic section; and the conclusion generally acquiesced in by mathematicians seems to be, that for the minor axes these points and lines are imaginary; this it would appear proceeds from assuming as a definition of a conic section, some property which, instead of being fundamental, is merely a particular case of a more general theorem; thus the definition of Boscovich, which is usually adopted as the fundamental definition in elementary analytical treatises on this subject at the present day, that a conic section is the locus of a point whose distances from a fixed point, and from a given line, are in a constant ratio, is merely a simplified case of the following more general theorem:—*That if in any surface of the second order with three unequal axes, two planes (called directrix planes) are drawn through a certain line, parallel to the circular sections of the surface, and if a certain point be assumed (which may be termed the focus of the surface), the square of the distance of any point on the surface from this focus, bears a constant ratio to the product of the perpendiculars from this point on the two directrix planes.*

When the surface is one of revolution round the transverse axe, the two groups of circular sections becoming coincident in direction, the two directrix planes coalesce, and the perpendiculars are coincident and equal; hence the above quadratic relation may be depressed to the common linear condition between the focus, directrix plane and any point on a surface of revolution round the transverse axe, or on a section of this surface passing through it.

When the surface is an oblate spheroid, the directrix planes are in this case also identical; the point termed the focus coincides with the centre; and if any central plane be drawn, it will cut the surface in a conic section and the directrix plane in a right line, which is termed in the following paper the minor directrix of the conic section.

Another definition of a focus has been assumed, that the focus is a point in the plane of the curve whose distance from any point on the curve is a linear function of the corresponding co-ordinates; and in a very elaborate and masterly paper, published in the number of the Philosophical Magazine for

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[*] Communicated by the Author.

January 1843, Mr. Davies treats the question of directrices and foci in a manner perfectly general, and arrives at the conclusion, that there are but two foci and two directrices in a conic section, a result arising from the arbitrary nature of the definition which he had assumed as the basis of his investigation; a definition to which it would appear there lie two objections; first, that it is merely an analytical condition having no geometrical representation, at least such a one as would show that these points should occupy the important position they do in the theory of conic sections, a fact which becomes at once manifest, when these points are defined as the points of contact of a plane touching two spheres inscribed in a right cone, and which cuts it in a conic section; and again, because the distance of the focus to any point of the common directrix, or of the centre to any point of the minor directrix, is a linear function of the co-ordinates of the point of contact of a tangent drawn through this point of the directrix to the curve, so that other points may be found which are not on the curve whose distances to the focus are linear functions of the co-ordinates of certain *corresponding* points upon the curve.

It is true, that so long as a conic section is defined as a plane section of a right cone, the minor directrices cannot be exhibited by any construction at all comparable in elegance with the geometrical method of defining the common foci and directrices above alluded to; but if we define a conic section as a central plane section of any surface of the second order, the difficulty at once vanishes, and we can exhibit the minor directrices with as much ease as we may the ordinary foci and directrices: to give some of the leading properties of the former is the object of the following paper.

Let a and b be the semiaxes of a central conic section, e the eccentricity $= \left(\frac{a^2 - b^2}{a^2}\right)^{\frac{1}{2}}$; and, guided by the analogy of the ordinary directrices and foci, let us draw two right lines perpendicular to the minor axe, at the distance $\frac{b}{e}$ from the centre, these lines may be termed the *minor directrices*; and on the same axe let two points be assumed at the distance $b\,e$ from the centre, these points may be called the *minor foci.*

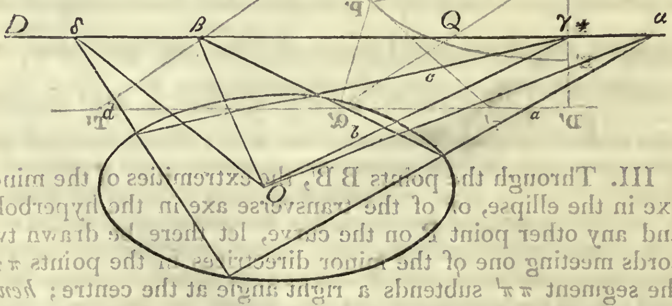
In the hyperbola, as the minor axe is imaginary, the new directrices must be drawn in a somewhat different manner. Let $2\,\omega$ be the angle between the asymptots of the hyperbola, and on the transverse axe let two points be assumed at the distance $a \sin \omega$ from the centre; through these points let perpendiculars to the transverse axe be drawn, these lines are the *minor directrices* of the hyperbola; and if two other points be assumed at the distance $\frac{a}{\sin\omega}$ from the centre, these points are the minor foci. It is almost needless to mention that the distances of the common directrices and foci from the centre of the hyperbola are $a\cos\omega$ and $\frac{a}{\cos\omega}$ respectively; hence, in the equilateral hyperbola, where $\omega=\frac{\pi}{4}$, the ordinary and minor directrices coincide, as do also the common and minor foci; whence we may deduce the very general and remarkable conclusion that,

The common directrices and foci of the equilateral hyperbola *possess two distinct classes of properties, those which belong to them as being the common or ordinary directrices and foci, as also that other new and equally extensive class, to which they are in like manner related, as being the minor directrices and foci of a central conic section.*

In the circle the minor directrices are infinitely distant, and the minor foci coincide with the centre, as is also the case with the ordinary directrices and foci.

Of the following theorems, most of which are, it is believed, new, the demonstrations have been suppressed, as to any who are moderately versed in analysis, and who take an interest in the subject, it will give but little trouble to supply them. It may, however, be remarked, that the solutions of many of the theorems may be effected with great brevity and simplicity by the application of a new method in analytic geometry,—the *theory of tangential co-ordinates*, an outline of which was published some years ago by the author *.

Fig. 1.



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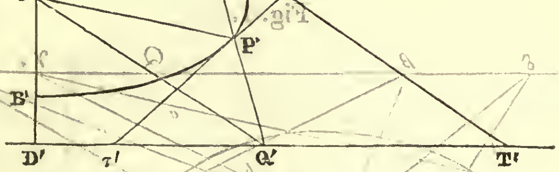
* On the Application of a new Analytic Method to the Theory of Curves and Curved Surfaces. London, Simpkin and Marshall: Cambridge, Deightons.

Theorem I. Let a quadrilateral be inscribed in a conic section, whose sides, a, b, c, d, being produced, meet one of the minor directrices D Q in the points $\alpha, \beta, \gamma, \delta$, the segments $\alpha\,\delta$, $\beta\,\gamma$ subtend angles at the centre, which are together equal to two right angles.

When the section becomes a circle, the minor directrices recede to an infinite distance, hence the lines O α, O β, O γ, O δ are parallel respectively to the sides a, b, c, d of the quadrilateral; and therefore the angle between a and d is equal to the angle α O δ, and the angle between c and b equal to the angle γ O β; hence we infer, *that the opposite angles of a quadrilateral inscribed in a circle are together equal to two right angles.*

II. When two of the sides of the quadrilateral a and d, suppose, are fixed, the point of intersection of b and c upon the curve being variable, the angle β O γ is constant; or if two fixed points be assumed on a conic section and a third variable, the cords which pass through the latter and the two former intercept on one of the *minor directrices* a segment which subtends at the centre a constant angle; hence we may infer, as in the last proposition, that the angle in a given segment of a circle is constant.

Fig. 2.



III. Through the points B B', the extremities of the minor axe in the ellipse, or of the transverse axe in the hyperbola, and any other point P on the curve, let there be drawn two cords meeting one of the minor directrices in the points $\pi\,\pi'$, the segment $\pi\,\pi'$ subtends a right angle at the centre; *hence the angle in a semicircle is a right angle.*

IV. Through the centre of a conic section let two right lines be drawn at right angles to each other, one meeting the curve in P, the other a minor directrix in R; the right line P R envelopes a circle whose diameter in the case of the ellipse is the minor axis, and in that of the hyperbola the transverse axe.

V. Let a right line be drawn (fig. 2) cutting the conic section in the points P P′, and the minor directrices in the points Q Q′, the segments P Q, P′ Q′ subtend equal angles at the centre.

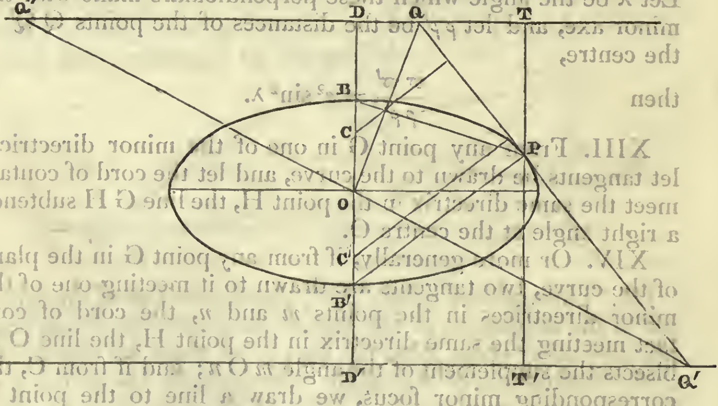
VI. Through the points P and P′ let two tangents to the curve be drawn, one meeting the minor directrices in the points T T′, the other meeting the same directrices in the points $\tau \tau'$; the sum of the angles subtended at the centre by the segments of the tangents T T and $\tau \tau'$ intercepted between the directrices is double of the angle subtended by Q Q′, the cord of contact, at the same centre.

VII. From the points Q, Q′, let perpendiculars be let fall on O T, O τ, and O T′, O τ' respectively; these four perpendiculars are equal.

VIII. Calling the length of this perpendicular p, we shall have the relation $\dfrac{\sin \text{P O P}'}{\sin \text{Q O Q}'} = \dfrac{p}{b}$.

IX. Let a right line touch a conic section in P (fig. 3), and meet the *minor directrices* in Q Q′, the distances O Q, O Q′ of the points Q, Q′ from the centre are rational functions of the co-ordinates of the point of contact P; and the rectangle under these distances is to the square of the central radius vector passing through the point of contact, as the square of the di-

Fig. 3.



stance of the major directrix from the centre is to the square of the abscissa α of the point of contact,

or $$OQ \cdot OQ' : OP^2 :: \frac{a^2}{e^2} : \alpha^2.$$

X. Let the lines O Q, O Q′ meet the *same* minor directrix in the points Q Q″, then in the triangle O Q Q″ we shall have the base Q Q′ to the sum of the sides O Q + O Q″, as the eccentricity of the conic section is to unity, or

$$\frac{QQ''}{OQ+OQ''} = e, \text{ in the hyperbola } \frac{QQ''}{OQ-OQ''} = e.$$

XI. On this tangent let perpendiculars be let fall from the *minor foci* C C′, the ratio of these perpendiculars is equal to that of the distances of Q and Q′ from the centre. Hence the ratio of those perpendiculars is also the same as that of the segments P Q, P Q′; thus the perpendiculars from the minor foci on a tangent, the distances of the points Q Q′ (where this tangent meets the minor directrices) to the centre, and the segments of this tangent between the point of contact and the minor directrices are all in the same ratio.

XII. The product of the focal perpendiculars on a tangent to the curve, is to the square of the central perpendicular on the same tangent as the square of the semi-diameter passing through the point of contact is to the square of the semi-major axe,

or $$\frac{\varpi\varpi'}{P^2} = \frac{a'^2}{a^2}.$$

Let λ be the angle which these perpendiculars make with the minor axe, and let $\rho\,\rho'$ be the distances of the points Q Q′ to the centre,

then $$\frac{\varpi\varpi'}{\rho\rho'} = e^2 \sin^2 \lambda.$$

XIII. From any point G in one of the minor directrices let tangents be drawn to the curve, and let the cord of contact meet the same directrix in the point H, the line G H subtends a right angle at the centre O.

XIV. Or more generally, if from any point G in the plane of the curve, two tangents are drawn to it meeting one of the minor directrices in the points m and n, the cord of contact meeting the same directrix in the point H, the line O H bisects the supplement of the angle $m\,O\,n$; and if from C, the corresponding minor focus, we draw a line to the point G meeting the directrix in the point H′, the line O H′ bisects the angle $m\,O\,n$; hence the lines O H, O H′ are always at right angles to each other.

XV. If from any point G in a minor directrix two tangents are drawn to the curve meeting the parallel axe in the points $\mu\,\mu'$, the line $\mu\,\mu'$ has to the line O G a constant ratio, or $\frac{\mu\mu'}{\mathrm{G\,O}} = \frac{2\,b}{h}$, as the minor axis $2\,b$ is to h the distance between the minor focus and minor directrix.

The very same relation holds when the point is assumed on the common directrix; for if from any point G on the common directrix two tangents are drawn meeting the parallel axis in the points $\nu\,\nu'$, the segment $\nu\,\nu'$ has to the line G F (drawn to the corresponding focus F) a constant ratio, or $\frac{\nu\,\nu'}{\mathrm{G\,F}} = \frac{2\,a}{h'}$, as the major axis $2\,a$ is to h' the distance between the common directrix and corresponding focus.

XVI. From any point P in a conic section, perpendiculars are let fall on the minor directrices, the rectangle under these perpendiculars is to the square of the semi-diameter passing through the point P in a constant ratio, or

$$\frac{\mathrm{P\,T\,.\,P\,T'}}{\mathrm{P\,O}^2} = \frac{b^2}{a^2\,e^2} = \frac{k^2}{a^2},$$

k being the distance between the centre and the minor directrix.

This theorem is analogous to that from which the ordinary definition of a conic section is usually derived; for let $\rho\,\rho'$ be the focal radii vectores of any point on the curve $p\,p'$, the perpendiculars from this point on the common directrices, then

$$\frac{p\,p'}{\rho\,\rho'} = \frac{1}{e^2} = \frac{a^2}{e^2\,a^2} = \frac{k^2}{a^2},$$

k' being the distance between the centre and major directrix, a result perfectly analogous to the former.

XVII. From any point P in a conic section, let two cords be drawn through the extremities B B′ of the minor axe, the conic section of which the point O is focus, and which touches the cords P B, P B′, will also touch the minor directrices of the curve, and the ordinate to the transverse axe through the point P will be the major directrix of this curve.

XVIII. Let ϕ be the angle between a pair of tangents to a conic section, $\rho\,\rho'$ the distances of the point of intersection of the tangents to the ordinary foci on the transverse axe, and ϖ the perpendicular from the intersection of the tangents on the focal cord passing through one of the points of contact, we shall have the theorem

$$\rho\,\rho' \sin\phi = 2\,a\,\varpi.$$

XIX. If from any point Q in a given right line meeting a minor directrix in P there are drawn two tangents to the curve, meeting the minor directrix in the points μ and ν, the product of the tangents of $\frac{1}{2}$ P O μ and $\frac{1}{2}$ P O ν is constant.

On Systems of Conic Sections having the same minor directrices.

XX. We shall find that peculiar relations exist between conic sections having the same minor directrices, somewhat analogous to those of confocal conic sections; we cannot on the present occasion give more than a few of them. Let k be the distance between the common centre and one of the directrices, then the axes are connected by the relation

$$\frac{1}{b^2} = \frac{1}{a^2} + \frac{1}{k^2}.$$

XXI. Two rectangular radii vectores being drawn from the common centre of two conic sections having the same minor directrices, one to each section; the sum of the squares of the reciprocals of those semi-diameters is constant, and the line which joins their points of intersection with the curves envelopes a circle; and if tangents are drawn to the curves through the points where the central radii vectores intersect them, the locus of the intersection of those tangents will be a conic section, having the same centre and minor directrices as the former.

XXII. Let a common tangent be drawn to two conic sections having the same minor directrices, the line connecting the two points of contact subtends a right angle at the centre.

XXIII. From any point in one of the minor directrices let a series of pairs of tangents be drawn to the conic sections, all the cords of contact will meet in a point on this directrix; or more generally, let there be a series of sections having the same centre and minor directrices, and from a point in the external one, let pairs of tangents be drawn to each of the internal sections; the cords of contact will all meet in a point on the tangent to the external section drawn through the given point.

XXIV. The difference of the squares of the reciprocals of any two coincident semi-diameters of two conic sections having the same minor directrices is constant.

XXV. Let a series of conic sections, all having the same minor directrices, be cut by a transversal; the segments of this line between any pair of sections subtend equal angles at the centre, and if through every pair of points in which this line intersects the sections, tangents are drawn intercepted both ways by the directrices, the sum of the angles which any pair of these tangents drawn to the same conic subtends at the centre is constant.

XXVI. Let a right line Q Q′, and a point O be assumed, and round the latter as pole let a constant angle revolve whose sides meet the given line in the variable points Q, Q′; let two other fixed points P P′ be assumed, the locus of the intersection of the lines P Q, P′ Q′ is a conic section, which passes through the points P P′, and of which (when O is the centre) the given right line is a minor directrix.

XXVII. Let a series of concentric conic sections, having the same minor directrices, be cut by a common diameter, the tangents drawn through the points where this diameter intersects the curves, envelope a concentric conic section.

The solution of this question is so simply obtained by the method of tangential co-ordinates, that I am induced to give it here. Let

$$b^2 v^2 \pm a^2 \xi^2 = 1 \; . \; . \; . \; . \; . \; . \; . \quad (1.)$$

be the tangential equation of one of the series of ellipses or hyperbolas, and as they all have the same minor directrices,

$$\frac{1}{a^2} + \frac{1}{k^2} = \frac{1}{b^2}; \; . \; . \; . \; . \; . \; . \; . \quad (2.)$$

let $y = n\,x$ be the projective equation of the diameter, then

$$\frac{b^2 v}{a^2 \xi} = n. \; . \; . \; . \; . \; . \; . \quad (3.)$$

Eliminating *a* and *b* between the equations (1.), (2.), and (3.), we find

$$v^2 \mp \xi^2 - \left(\frac{n^2-1}{n}\right) v\,\xi = \frac{1}{k^2},$$

the equation of a concentric equilateral hyperbola or ellipse.

In an early number we hope to return to this subject, and apply this theory not only to oblate spheroids and central surfaces of revolution generally, but to surfaces of the second order having three unequal axes, as also to systems of surfaces having coincident circular sections, groups which bear a striking analogy in their relations to systems of conic sections having the same minor directrices; showing among other remarkable properties, that every surface of the second order has *four* directrix planes parallel two by two respectively to the circular sections of the surface, as also four foci situated two by two on the umbilical diameters.

LXXVII. *Second Memoir on Induction.*

By Prof. ELIE WARTMANN*.

§ VII. *On the Non-interference of Electric Currents.*

58. AMONG the theories of electricity, those of statical and dynamical induction appear to be of the greatest importance. It may even be said that the explanation of the phænomena of induction would serve to establish the true theory of electricity. On the other hand, some experimental data on the properties by which this fluid is allied to, or distinguished from, caloric and light, would be very useful for a clear comprehension of that action of influence which seems to belong to it peculiarly. I propose, in the present memoir, to examine whether dynamical electricity can cause interferences analogous to those which the two other imponderable agents present.

59. Are two electrical currents issuing from similar or the same sources, and primitively equal, capable of neutralizing each other wholly or in part, when, one of them preserving a constant intensity, that of the other varies until it becomes almost evanescent compared with the first? This is the problem which I have sought to solve by three independent methods, that of induced currents, that of direct and continuous currents, and that of derivations. The necessity of measuring with great accuracy the conductors which the currents traverse, and the absolute ignorance we are in as to the lengths of the electric undulations (if indeed electricity is a phænomenon in which the movements of the æther have a share), render the experiments which I proceed to describe very delicate and tedious. They have all been repeated a great number of times: the principal ones have been laid before the Société des Sciences de Lausanne.

A. *Method of Induced Currents.*

60. In my first memoir I showed (22) that on passing direct currents of the same direction by two inductor wires, a current was induced equal to the sum of their elementary effects, whilst (23, 24), if the two inducing currents are perfectly equal and in contrary directions, the two induced currents neutralize one another, or, rather, have no sensible existence. But on lengthening one of the inductor wires and leaving the other constant, the equality of their conductibility and of their inductive power ceases; then the needle of the rheome-

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* Read March 19, 1845, before the Société Vaudoise des Sciences Naturelles. The first memoir was inserted in the Philosophical Magazine, vol. xxv. p. 266, and in the *Archives de l'Electricité*, vol. iv. p. 34.

ter, which is in the circuit of the induced wire, indicates an instantaneous current equal to the difference which that lengthening has produced.

61. Two cases might present themselves between the limits of induction, which correspond to zero and to an infinite length of the additional wire, applying the word infinite to that which compels the current to discharge itself wholly by the constant wire. If for constantly increasing lengths we should find the values of the induced current constantly increasing, or *intermittent* values, one while greater, at another less, then only there would be interference in the induced circuit. Now, a multitude of experiments made with the apparatus and wires previously described in § I., and in which the lengthening of the inductor wire is effected by infinitely slow degrees, have shown that the angles of deviation increase without any alternation, and have led to the logarithmical laws laid down in § III. There is therefore no interference under the circumstances in which I have sought to produce it.

62. This result was obtained in November 1841, and communicated to the students attending my lectures at the commencement of the following year. It might perhaps be objected, that if the induction, whose characteristic it is to be instantaneous, is produced by a single wave, it is not certain that the phænomena of interference are manifested in its development. This objection would be analogous to that which has been raised against the explanation by electric undulations of the remarkable intermittence discovered by Professor De la Rive [*], in the conductibility of certain lengths of one and the same metallic wire for magneto-electrical currents submitted to alternative and rapid changes of direction [†]. With a view to remove all doubt in this respect, I have endeavoured to cause two continuous currents, launched simultaneously in the same wire, to interfere, either in a contrary or in the same direction [‡].

B. *Method of Direct and Continuous Currents.*

63. The wires which I have used are noted in the following Table. They have been chosen quite cylindrical, and

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[*] *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève*, vol. ix. See the sequel of these researches in the *Archives de l'Electricité*, vol. i. p. 75, where will be found the explanation of this apparent interference.

[†] Lamé, *Cours de Physique de l'Ecole Polytechnique*, § 860.

[‡] M. Peltier has applied the same means, without employing it for the same object, as I have, to graduate thermo-electric rheometers by his *method of the sum of the united currents*, § XXII. of his memoir in the *Annales de Chimie et de Physique*, vol. lxxi. p. 225.

annealed with care. Their lengths not being capable of being directly ascertained with sufficient accuracy, as they were not geometrically rectilinear, have been calculated by the formula

$$L = 318{\cdot}47 \frac{P}{\Delta R^2},$$

in which

L is the length sought, in millimetres,
P the weight of the wire, in grammes,
Δ its density,
R its radius.

The weighings were made by Borda's method, and with an excellent balance constructed by Fortin. The radii were estimated by the method of *enroulement.* The densities are taken from the best tables known.

Name of the wires.	Densities.	Diameters.	Weights.	Lengths.
		metre.	grammes.	metres.
Platinum wire, No. 2	22·0	0·00200	59·700	0·864111
... ... No. 3	22·0	0·00140	31·404	0·927720
Copper wire, No. 2 ...	8·9	0·00300	66·892	1·067160
... ... No. 3 ...	8·9	0·00300	65·705	1·046243
... ... No. 4 ...	8·9	0·00300	200·255	3·184726
... ... No. 5 ...	8·9	0·00300	52·050	0·828810
Brass wire, No. 3	8·4	0·00050	21·593	11·576000
... ... No. 4	8·4	0·00100	28·116	4·216216
Steel wire, No. 1	7·8	0·00025	3·599	9·405405

64. I constructed a rheometer with the copper-wire No. 5. This wire makes only four revolutions, and is not covered with silk. The two needles (selected from fifty) are very light, magnetized to saturation, and make an entire oscillation in $10''{\cdot}4$. The point of the upper needle carries a small strip of blackened paper, which allows the deviations to be read upon a frame of $0^{m}{\cdot}9$ diameter.

65. In order to have sources of electricity always comparable, and of a perfect constancy, I employed two thermo-electric pairs formed of metals which undergo no alteration in the conditions in which they were to be placed. These metals are platinum and copper, pure and perfectly annealed. I avoided all foreign metal, by rejecting the use of solder. The wires of platinum No. 2 and of copper No. 3, those of platinum No. 3 and of copper No. 2, were stretched in a vice to a length of $0^{m}{\cdot}010$; then, after having connected these surfaces of contact by a copper wire finer and better annealed, they were plunged into equal quantities of pure mercury, intended to equalize promptly the temperature throughout their whole extent. This mercury was inclosed in two German glass test-

tubes, of equal dimensions, and made from the same tube; corks fixed from distance to distance separate the two wires of each pair. The test-tubes, united by a leaden plate, intended to keep them vertical and parallel, were immersed in a tinned copper reservoir half filled with a solution of chloride of calcium in water. Lastly, a double current lamp keeps this bath at a constant temperature, indicated by an excellent standard thermometer graduated on its own tube.

66. All the communications from the extremities of the pairs with the rheometer are effected by means of M. Poggendorff's clamps, which have the advantage of securing an excellent contact without springs or mercury. They are massive parallelopipedons of pure copper, in which at intervals three parallel cylindrical apertures had been bored, which are traversed perpendicularly to their axis by three screws.

67. To elongate the circuit of the pair of variable length, I employ Mr. Wheatstone's rheostat. The one I have used is formed of two cylinders of $0^{m}\cdot06$ diameter; their clamp receives the brass wire No. 3, which makes sixty revolutions, and the serviceable length of which is $11^{m}\cdot3$. This additional wire must be of an homogeneous texture, perfectly cylindrical, and of a very good conducting nature, without which it would occasion perturbations in the notations, which would obscure the final result.

68. The arrangement adopted for the experiment consists in placing the rheostat in the circuit of one of the pairs, and in compelling the current of the other pair to traverse the rheometer in an inverse direction to that of the first*. Each current then possesses two ways of communication; either by the large, short and homogeneous wire of the rheometer, or through the heterogeneous and comparatively longer circuit of the other pair. Experiment and calculation prove that the derivation produced by this second method is so small that it need not be regarded. For example, the current of the second pair giving 14° deviation at + 114° Cent., the addition of the circuit of the other pair, not heated, causes the needle to fall to 13° 30′. The first pair having produced 26° at + 114° Cent., the derivation caused by the other reduced the index to 25° 40′, and no longer affected it sensibly, when the deviation was reduced to 4° or 3° by the interposition of a sufficient length of the wire of the rheostat. As the

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* The following is a numerical example, intended to prove the delicacy of my measuring apparatus. The thermometer marking + 143°,—

The first pair made the needle deviate from 0° to 39° east
The second ... ... 0 ... 18 west
And the opposition of their currents ... ... 0 ... 29 east.

application of heat diminishes the conductibility of the metals which I have employed, it may be imagined that the heterogeneous wire, the solder of which was raised from 116° to 140°, according to circumstances, would no longer leave a passage, except for an imperceptible fraction of the current.

69. Leaving the circuit of one of the pairs invariable, that of the other was modified by insensible degrees (as the rheostat allows of conveniently), by shortening the whole length of its additional wire 11 metres, or by adding it to it, which nearly destroyed its own current. The course of the needle of the rheometer never indicated other than a *progressive* increase or diminution of deviation, and proved the preponderating influence of one current over the other, or their mutual neutralization, on account of their equality in opposite directions.

70. The experiment was repeated, sending the two currents in the same direction by the wire of the rheometer. It gave an analogous result, that is to say, *the most complete absence of intermittence* in the progress of the needle for a constantly increasing addition or subtraction of the additional wire.

71. I have made other trials with hydro-electric piles (6), employing Breguet's thermometer (5, *c*) as a means of measuring. Two of Daniell's pairs were connected with that instrument by means of two very equal brass conductors, No. 4. On establishing a second circuit with two other similar wires, so that the current, issuing from the same poles, should traverse the thermometer in an inverse direction to the first, the needle returned to its zero of departure and remained there. In order to be sure that this neutralization was not apparent only and attributable to the circumstance that the double circuit sufficed for the complete discharge of the battery outside the spiral, I repeated the experiment with four steel wires No. 1, forming a double circuit, a much worse conductor than that of the brass wires, and with ten pairs instead of two. The result was the same.

72. The following objections might perhaps be raised against the method of direct and continuous currents:—

a. It is advisable to employ a common source instead of two similar electrical sources. In the phænomena of luminous or calorific interference, the bundles which destroy one another are derived from the same radiation.

b. On the undulatory hypothesis of electricity, the circumstance that the vibrations must traverse a part of a circuit (the wire of the rheostat) the diameter of which is diminished, might prevent the possibility of their ulterior interference with a current the vibrations of which have not undergone a like modification *c.* In a similar manner, the variations in length of one of the circuits act upon the *intensity* of the current which emanates from a constant source. Now it is possible that two currents of unequal intensity may be incapable of interfering in conditions in which they would be mutually destroyed if their intensities had been equal.

73. It must be remarked, that the two first objections do not apply to the method of induced currents. But, in order to deprive them of all value, I have imagined a third, which consists of employing only a single thermo-electric pair, the current of which passes at the same time in part into the wire of the rheometer, and in part into a wire of derivation. This wire was the copper one No. 4, and the pair that of the platinum wire No. 2 and the copper one No. 3.

C, *Method of Derivations.*

74. When the circuit of a pair is closed with the rheometer, three derivations may be effected:—from the platinum wire to the copper one, by thus compelling a part of the current not to traverse the measuring apparatus; from the platinum wire to itself, by establishing at certain points of its length junctions with the extremities of the wire of deviation, and causing a variation both in the distance of these points and in the length of this wire; lastly, from the copper wire to itself, by proceeding in an analogous manner.

75. The wire of derivation was 0m·003 in diameter, and consequently could not be wound round the rheostat. To ensure a constant contact on its surface (an extremely important condition), and to cause its length to vary so slowly that this was necessary, I employed a tube of pure copper, 0m·05 long, with a bore of 0m·0034, and fixed in one of the holes of a Poggendorff's clamp; its two extremities were split like a porte-crayon, and the lips might be partially closed at will by compressing rings.

76. *None of the three derivations occasioned phænomena of interference.*

77. The second derivation proved the remarkable action of the conductibility on the intensity of the current, for the copper wire No. 4 being a much better conductor than the platinum wire of the pair, increased the deviation of the needle sensibly. In one of the experiments, by making the junctions on the one hand on the platinum wire immediately after its exit from the test-tubes, and on the other with the rheometer, the index was propelled 15° from its first position. This increase in the deviation became weaker, but without any intermittence, when the first point of contact was brought near the second.

78. The third derivation produced no sensible action on the needle, as was easy to foresee, since the wire of derivation and that of copper of the pair were of the same diameter, and as each one, taken separately, had a conductibility of its own and dimensions sufficient to transmit the whole thermo-electric current.

79. The method of derivations appears to me to be free from the third objection (72, *c*), that of the intensities. In fact, the difference in the intensity of the currents which re-united after issuing from one and the same source, and having followed two entirely similar ways, was capable of being rendered as feeble as possible without any result of interferences. Now analogy being here our sole guide, it is necessary to remember that in wholly similar circumstances, the vibrations of the æther which constitute light, and those of the elastic fluids which engender sound, have presented very evident phænomena of mutual destruction[*].

LXXVIII. *On* Fresnel's *Theory of Double Refraction.* *By* R. Moon, *M.A., Fellow of Queen's College, Cambridge, and of the Cambridge Philosophical Society*[†].

HAVING on several previous occasions treated of the theory of unpolarized light, and having, as I trust, effectually exposed the futility of the celebrated hypothesis devised by Fresnel for the elucidation of many of the principal phænomena in that department of optics, I now come to the consideration of the subject of polarized light; upon his treatment of which Fresnel's great fame now principally rests, and to whose views in regard to which many of his adherents, who have felt themselves compelled to give up his theory of diffraction, still cling with unshaken fidelity. What my own faith on this subject may be, it is unnecessary at present to disclose further than this, that whether the original idea of transversal

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[*] In the fundamental experiment of Fresnel, the bundles of light do not necessarily reach the two mirrors under the same incidence, and have not the same intensity when they interfere after reflexion. After M. W. Weber had shown that the surfaces according to which sound disappears around a vibrating diapason are hyperbolically curved, Dr. Kane succeeded, following out an idea of Sir John Herschel, in constructing united tubes, the lengths of which are in the relation of 2 to 3, or of 6 to 7, and which destroy by interference one, in a determined number, of the sounds which is made to pass through them. (Philosophical Magazine, vol. vii. p. 301; Poggendorff's *Annalen der Physik*, vol. xxxvii. p. 485.)

[†] Communicated by the Author.

vibrations is due to Young only or not, I pledge myself to prove that the researches of Fresnel have not advanced us one step beyond it.

Fresnel's first step in the mathematical part of his theory, is to prove the existence of the axes of elasticity: "a proposition," says Mr. Smith (Cambridge Mathematical Journal, vol. i. p. 1), "on which the whole theory of double refraction depends, and which Fresnel has proved by a method which has the advantage of geometrical distinctness, but which is long and rather difficult to follow out on that account." On the same account I shall prefer to give Mr. Smith's elegant analysis in preference to the cumbrous processes of Fresnel, trusting my readers will take my word for it, that whatever it may want in "geometrical distinctness," it gains in logical clearness.

"The proposition is thus stated:—In any system of particles acting on each other with forces which are functions of their mutual distances, there are three directions at right angles to each other, along which if a particle be displaced, the forces of restitution will act in the same direction.

"Let $x\,y\,z$ be the co-ordinates of the attracted point $x_1\,y_1\,z_1$; $x_2\,y_2\,z_2$ ... be the co-ordinates of the attracting points; $r_1\,r_2\,r_3$ ... the distances between the attracted and attracting points; $\phi_1(r_1)\ \phi_2(r_2)\ \phi_3(r_3)$ ... the attractions; X Y Z the total resolved forces along the axes, then we shall have

$$\mathrm{X} = \frac{x_1 - x}{r_1}\phi_1(r_1) + \frac{x_2 - x}{r_2}\phi_2(r_2) + \&\mathrm{c.};$$

and similarly for Y and Z. Now let

$$\mathrm{R} = -\Sigma \int \phi(r)\,dr,$$

then

$$\left.\begin{aligned} \mathrm{X} &= \frac{d\,\mathrm{R}}{d\,x} = 0, \\ \mathrm{Y} &= \frac{d\,\mathrm{R}}{d\,y} = 0, \\ \mathrm{Z} &= \frac{d\,\mathrm{R}}{d\,z} = 0, \end{aligned}\right\} \text{when the particle is in equilibrio.}$$

"Let the particle receive a small displacement, the projections of which on the co-ordinate axes are δx, δy, δz. Then supposing the displacement to be very small, the force of restitution may be taken as proportional to it, so that we have

$$\left.\begin{aligned} X &= \frac{d^2 R}{dx^2}\delta x + \frac{d^2 R}{dx\,dy}\delta y + \frac{d^2 R}{dx\,dz}\delta z, \\ Y &= \frac{d^2 R}{dx\,dy}\delta x + \frac{d^2 R}{dy^2}\delta y + \frac{d^2 R}{dx\,dz} d z, \\ Z &= \frac{d^2 R}{dx\,dz}\delta x + \frac{d^2 R}{dy\,dz}\delta y + \frac{d^2 R}{dz^2}\delta z. \end{aligned}\right\} \ldots (A.)$$

"Now the force of restitution will be in the direction of the displacement, if X Y Z be proportional to δx, δy, δz. Let then

$$s = \frac{X}{\delta x} = \frac{Y}{\delta y} = \frac{Z}{\delta z},$$

then putting

$$\frac{d^2 R}{dx^2} = A, \quad \frac{d^2 R}{dy^2} = B, \quad \frac{d^2 R}{dz^2} = C,$$

$$\frac{d^2 R}{dz\,dy} = A', \quad \frac{d^2 R}{dz\,dx} = B', \quad \frac{d^2 R}{dx\,dy} = C',$$

and substituting in the former equations, they become

$$(A - s)\delta x + C'\delta y + B'\delta z = 0,$$

$$C'\delta x + (B - s)\delta y + A'\delta z = 0,$$

$$B'\delta x + A'\delta y + (C' - s)\, dz = 0,\text{"}$$

from which it is easy to prove, supposing the above process correct, "that there are three directions at right angles to each other, along which, if a particle be displaced, the force of restitution acts in the same direction."

But the fact is, the above process is entirely fallacious if it is meant to apply to the case of the motion of a particle of the æthereal medium by which light is produced. What is meant by the mysterious principle, that "supposing the displacement to be very small, the force of restitution may be taken as proportional to it," I profess myself unable to comprehend; but this I do understand, that when the co-ordinates of the particle, whose motion is being considered, vary from x, y, z to $x + \delta x$, $y + \delta y$, $z + \delta z$, the co-ordinates of the other particles of the medium will vary from $x_1 y_1 z_1$, $x_2 y_2 z_2$, &c. to $x_1 + \delta x_1$, $y_1 + \delta y_1$, $z_1 + \delta z_1$, $x_2 + \delta x_2$, $y_2 + \delta y_2$, $z_2 + \delta z_2$, &c.; and that therefore the above values (A.) for the resolved parts of the force on the particle whose motion is being considered, are entirely fallacious.

The true value of X in this case is,

$$\frac{d^2 R}{dx^2}\delta x + \frac{d^2 R}{dx\,dy}\delta y + \frac{d^2 R}{dx\,dz}\delta z$$

$$+\frac{d^2 R}{dx\,dx_1}\delta x_1+\frac{d^2 R}{dx\,dy_1}\delta y_1+\frac{d^2 R}{dx\,dz_1}\delta z_1$$
$$+\frac{d^2 R}{dx\,dx_2}\delta x_2+\frac{d^2 R}{dx\,dy_2}\delta y_2+\frac{d^2 R}{dx\,dz_2}\delta z_2$$
$$+\ \&c.,$$

and similarly for Y and Z; from which it is evident, that to talk of the existence of axes of elasticity in every system of particles acting on each other is mere absurdity. And hence it appears, that the "proposition on which the whole theory of double refraction depends" is altogether untrue.

Will it be urged, however, that although the general proposition does not hold, there still may be particular systems of particles for which it does hold? I do not hesitate to state my belief, that the existence of such a system is *impossible*; and at any rate would challenge any analyst whatever to suggest any such.

The case then stands thus:—A writer states a proposition as the basis of a theory; he offers a proof of the proposition, which turns out to be fallacious; and not only is the proof itself erroneous, but during the investigation there appears a degree of evidence approaching to certainty, that the proposition itself, after modifying it in every conceivable way consistent with the case to which it is meant to apply, is untrue; and there is moreover a *perfect certainty* that it is incapable of proof. Thus we have a fundamental proposition of which a false proof is given, a certainty that if true, it must always remain a mere assumption incapable of independent proof; and this in the face of the fact that there is every reason to suppose it untrue. Such a combination of circumstances would have decided the fate of any other theory; why is this to be made an exception to the rule? But to return.

Assuming the existence of the axes of elasticity, we are next introduced to the surface of elasticity. Referring the co-ordinates to the axes of elasticity, we have

$$\left.\begin{aligned} X &= a\,\delta x = a\,r\cos\alpha \\ Y &= b\,\delta y = b\,r\cos\beta \\ Z &= c\,\delta z = c\,r\cos\gamma \end{aligned}\right\}\text{ where } a,\ b,\ c \text{ are constants.}$$

The rest I shall give in the words of Sir John Herschel (vide Encyclopædia Metropol., art. Light, 1004): "M. Fresnel next conceives a surface, which he terms the 'surface of elasticity,' constructed according to the following law:—On each of the axes of elasticity, and on every radius r drawn in all directions, take a length proportional to the square root of the elasticity exerted on the displaced molecule by the medium in the direction of the radius, or to $\sqrt{\mathrm{F}}$; then if we call R this length, or the radius vector of the surface of elasticity, we shall have

$$R^2 = \{a\, r\,.\,\overline{\cos\alpha}^2 + b\, r\,.\,\overline{\cos\beta}^2 + c\, r\,.\,\overline{\cos\gamma}^2\} \times \text{const.};$$

the values of R parallel to the axes are then had by the equation

$$R^2 = \text{const.} \times a\,r,\quad R^2 = \text{const.} \times b\,r,\quad R^2 = \text{const.} \times c\,r,$$

which we shall express simply as a^2, b^2, c^2, so that the equation of the surface of elasticity will be of the form

$$R^2 = a^2 \cos^2 X + b^2 \cos^2 Y + c^2 \cos^2 Z;$$

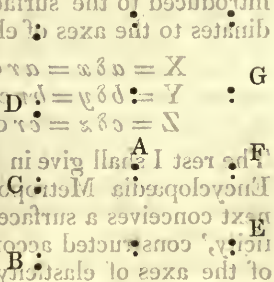
where X Y Z now stand for $\alpha\,\beta\,\gamma$, the angles made by R with the axes of co-ordinates.

"Let us now imagine a molecule displaced and allowed to vibrate in the direction of the radius R, and retained in that line, or at least let us neglect all that portion of its motion which takes place at right angles to the radius vector. Then the force of elasticity by which its vibrations are governed will be proportional to R^2, and the velocity of the luminous wave propagated by means of them, in a direction transverse to them (or at right angles to R), will be proportional to R."

Of this extraordinary proposition the accomplished author does not offer one syllable of proof or explanation. Whether Fresnel's writings are equally deficient, I am not aware; but another eminent mathematical writer, the present Astronomer Royal, after bestowing, as we may reasonably suppose, some degree of diligence on the study of Fresnel's papers, appears to have found nothing better in the way of a demonstration than the following (vide Airy's Tracts, 2nd edition, p. 341):—

"To explain on mechanical principles the transmission of a wave in which the vibrations are transverse to the direction of its motion.

"In figure adjoined let the faint dots represent the original situations of the particles of a medium, arranged regularly in square order, each line being at the distance h from the next. Suppose all the particles in each vertical line disturbed vertically by the same quantity, the disturbances of different vertical lines being different. Let x be the horizontal abscissa of the second row, $x - h$ that of the first, and $x + h$ that of the third; let u u_1 and u' be the corresponding disturb-



ances. The motions will depend upon the extent to which we suppose the forces are sensible. Suppose the only particles whose forces on A are sensible, to be B, C, D, E, F, G (omitting those in the same line, as their attractions are equal and in opposite directions); and suppose them to be attractive, and as the inverse square of the distance; and the absolute force of each $= m$. The whole force to pull A downwards is

$$\frac{m(h+u-u_1)}{\{h^2+(h+u-u_1)^2\}^{\frac{3}{2}}}+\frac{m(u-u_1)}{\{h^2+(u-u_1)^2\}^{\frac{3}{2}}}$$

$$-\frac{m(h-u+u_1)}{\{h^2+(h-u+u_1)^2\}^{\frac{3}{2}}}+\frac{m(h+u-u')}{\{h^2+(h+u-u')^2\}}$$

$$+\frac{m(u-u')}{\{h^2+(u-u')^2\}^{\frac{3}{2}}}-\frac{m(h-u+u')}{\{h^2+(h-u+u')^2\}^{\frac{3}{2}}}.$$

"Expanding these fractions and neglecting powers of $u-u_1$ and $u-u'$ above the first, the force tending to diminish u is

$$\left(1-\frac{1}{2^{\frac{3}{2}}}\right)\cdot\frac{m}{h^3}(2u-u_1-u').$$

Putting for u_1,

$$u-\frac{du}{dx}h+\frac{d^2u}{dx^2}\frac{h^2}{2},$$

and for u',

$$u+\frac{du}{dx}h+\frac{d^2u}{dx^2}\frac{h^2}{2},$$

we find

$$\frac{d^2u}{dt^2}=\left(1-\frac{1}{2^{\frac{3}{2}}}\right)\frac{m}{h}\frac{d^2u}{dx^2},$$

an equation of exactly the same form as that for the transmission of sound. The solution therefore has the same form; and therefore the transversal motion of particles supposed here follows the same law, that is, it follows the law of undulation." And moreover, if the above were correct, the velocity of the luminous wave would be proportional to the square root of the force of elasticity in a direction transverse to the direction of the course of the wave.

Whether the above illustration—for at best it would be nothing more—is due to Fresnel or Mr. Airy himself, I am not aware: but the whole is erroneous from beginning to end. The mathematics not only fail to meet the case under consideration, but there is a palpable mathematical error in the process, which, even admitting the data, completely vitiates the result. I need but advert to the circumstance, that in the approximate values substituted for u_1 and u', it is assumed that h is small with respect to u, or that the distances between the particles are small compared with their actual motions, a supposition entirely at variance with the assumed data of the problem. Hence it is plain that this supposed illustration is for every purpose entirely worthless.

Thus as we were compelled to assume the existence of the axes of elasticity, not only in the default, but in the face of evidence, so we are compelled to assume this rule as to the mode of calculating the velocity on a bare analogy to a case presenting the most striking difference from that under consideration, namely that of the direct transmission of an undulation when the vibrations are in the direction of transmission. We are not only compelled to assume the existence of *undulations* consisting of vibrations executed in directions perpendicular to the course of the wave,—respecting which it is not too much to say, that it is impossible for the mind to conceive the possibility of their existence,—but we are to suppose ourselves acquainted with an exact law to which they are subject*. Of the worth of such a theory I leave my readers to judge. The discussion of the remaining portion of it I must defer to another opportunity.

Liverpool, November 8, 1845.

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* It is easy to conceive of transversal as the consequence of direct vibration, but I confess myself unable to conceive the possibility of there being a *surface* of transversal vibrations in the same phase—that of a sphere for example. The case of a stretched cord affords no analogy to guide us, for there the *wave* is in the direction of the motion. At all events, if the hypothesis of transversal vibration is to hold its ground, it must have much more thought bestowed upon it than it has yet received. The most painful circumstance connected with the later history of the undulatory theory, is the manner in which ideas, in themselves perhaps valuable as hints, have been dressed up into a settled theory. A truly philosophical mind, to which the idea of transversal vibrations had once suggested itself, would have set itself to work to discover, if possible, some method by which such motion could be conceived, and would not have rested satisfied so long as a doubt existed as to the perfect feasibility of the scheme. Thus it is that we may account for Young not having attempted to carry his first notion any further. He saw, no doubt, the difficulties by which the idea of transversal vibrations was beset, and was well-aware that till these were got over, it was hopeless to attempt to enter with any chance of success into the discussion of their nature and consequences. Fresnel, on the contrary, was satisfied with a series of possibilities, upon which he has built a theory, not only of no value in itself, as having nothing solid to rest upon, but from its crudity and manifold errors discreditable to himself and to the age by which it has been received.

LXXIX. *Addenda to* Mr. Drach's *asserted Proof of* Fermat's *Theorem*[*].

PORTIONS of the above paper (vide Phil. Mag., p. 286, for October last) possibly requiring further elucidation, it may be observed that the conclusion in the odd case is better proved as follows:—

As the binomial theorem for whole powers extends to fractional subdivisions of the root $\left(e.g.\ \frac{1}{3}+\frac{2}{3}=1,\ \frac{4}{5}+\frac{31}{5}=7\right)$, the equations between a, c, Z, Y, exist if even these quantities are fractional. Let k be any whole number, and make $a=z^{2k+1}$, $\mathrm{Z}=z^{m-k}$, $c=y^{2k+1}$, $\mathrm{Y}=y^{m-k}$,

$$\therefore \mathrm{Z}=z^{m-k}=z^{2km+m}p^{n}+\Sigma\,\mathrm{A}_i\,z^{(2k+1)(m-i)}y^{2ki+i}p^{n-2i}q^{2i},$$

multiplying by $2z^{-2km-m}$ and then by $z^{kn\pm\frac{n}{2}}$,

$$2z^{-kn}=2p^{n}\left\{1+\Sigma\,\mathrm{A}_i\,\overline{\frac{q}{n}}^{\,2i}\cdot\overline{\frac{y}{z}}^{\,2ki+i}\right\},$$

$$2z^{\frac{n}{2}}=2p^{n}z^{kn+\frac{n}{2}}\left\{1+\Sigma\,\mathrm{A}_i\,\overline{\frac{q}{n}}^{\,2i}\cdot\overline{\frac{y}{z}}^{\,2ki+i}\right\}=$$

$$=\left(pz^{k+\frac{1}{2}}+qy^{k+\frac{1}{2}}\right)^{n}+\left(pz^{k+\frac{1}{2}}-qy^{k+\frac{1}{2}}\right)^{n},$$

$$\therefore 2y^{\frac{n}{2}}=\left(pz^{k+\frac{1}{2}}+qy^{k+\frac{1}{2}}\right)^{n}-\left(pz^{k+\frac{1}{2}}-qy^{k+\frac{1}{2}}\right)^{n}$$

$$\therefore \frac{p}{q}\left(\frac{z}{y}\right)^{k+\frac{1}{2}}\left\{\overline{z^{\frac{n}{2}}+y^{\frac{n}{2}}}^{\frac{1}{n}}-\overline{z^{\frac{n}{2}}-y^{\frac{n}{2}}}^{\frac{1}{n}}\right\}=\left\{\overline{z^{\frac{n}{2}}+y^{\frac{n}{2}}}^{\frac{1}{n}}+\overline{z^{\frac{n}{2}}-y^{\frac{n}{2}}}^{\frac{1}{n}}\right\}.$$

The foot indexes of B ought to have been doubled, and the equation transforms to

$$2z^{\frac{1}{2}}\cdot\frac{p}{q}\cdot\left(\frac{z}{y}\right)^{k+\frac{1}{2}}\left\{\overline{\frac{1}{n}\cdot\frac{y}{z}}^{\,\frac{n}{2}}+\mathrm{B}_{2i+i}\overline{\frac{y}{z}}^{\,in+\frac{n}{2}}\right\}$$

$$=2z^{\frac{1}{2}}\left\{1+\frac{1}{n}\cdot\frac{1-n}{2n}\left(\frac{y}{z}\right)^{n}+\mathrm{B}_{2i}\overline{\frac{y}{z}}^{\,in}\right\};$$

$$\therefore 0=\left\{1+\frac{p}{q}\cdot\left(\frac{y}{z}\right)^{m-k}\cdot\frac{2in-1}{(2i+1)n}\right\}\mathrm{B}_{2i}\cdot\left(\frac{y}{z}\right)^{in};$$

A_i being independent of k, $n=1$ gives $p=1=q$; $n-1$ enters all B_i except $\mathrm{B}_0=1$, $\therefore 0=1-(z\div y)^{k}$ is the equation in this case. If $k=0$, z and y are indeterminate; for any other value of k, $z=y$, and therefore $x=0$, an excepted case. When $n>1$, and $i>0$, all the terms are negative, whatever k may be; of the term $i=0$, or of $1-\frac{p}{nq}\cdot\left(\frac{y}{z}\right)^{m-k}$, it can-

[*] Communicated by the Author.

not be immediately shown if this is + ve and less than the remaining terms; but we may make k as great as we please, and $z > y$ will always possibly render this factor negative, without restricting the generality of the original equations, ∴ we may always assume when $n > 1$, $0 =$ sum of exclusively *negative real quantities*; the absurdity of which proposition shows that n cannot exceed unity.

Case 2.—Page 288, line 9, is derived from division, and by line 12,

$$\frac{p^2}{q^2} = \left(\frac{z+y}{z-y}\right)^2 \left\{\frac{4g+1}{4h+2}, \text{ or } \frac{4g+2}{4h+1}\right\},$$

∴ the bracketed factor can evidently never become a square.

Line 21.—The introduction of $\sqrt{-1}$ follows from Euler, ch. xii.

Line 26 to 28, deduced from the binomial theorem.

Page 289, line 2, add respectively.

Page 289, line 8 (after magnitude), from the symmetry of the binomial expansion, setting out from either extreme.

P.S. This case is immediately deducible from the first, by considering x^2, y^2 and z^2 as raised to the odd mth power.

Are Fermat's papers preserved in no *château* or *bibliothèque*?

S. M. D.

November 21, 1845.

LXXX. *Intelligence and Miscellaneous Articles.*

ON RESPIRATION. BY PROF. MAGNUS.

IN a paper read before the Royal Academy at Berlin, the author first enumerated the different views on the nature of respiration which had been proposed, and especially urged against all those theories which suppose that a chemical combination takes place in the lungs between the oxygen and the blood, that they do not explain how it is that blood, after having been darkened by agitation with carbonic acid, is again rendered of a bright red by oxygen or atmospheric air, and is capable of again assuming its former arterial colour, if its arterial tint depends upon oxidation; for carbonic acid cannot deoxidize the blood, and how can it be conceived that blood which has once been oxidized can be again oxidized a second or third time, and so on, without having been deoxidized? This objection appears to the author so conclusive, that he considers it sufficient to refute any theory which supposes chemical combination of oxygen with the blood to occur.

He then alluded to the theory which he had proposed in 1837, according to which the inspired oxygen does not combine chemically with the blood, but is only absorbed, and thus arrives in the capillary vessels, where it is applied to the oxidation of certain substances, converting them into carbonic acid, and perhaps also into water. The carbonic acid, instead of oxygen, is then absorbed by the blood, and reaches the lungs with it, to be removed by contact with the atmosphere; a fresh quantity of oxygen is then absorbed instead of it, and undergoes the same changes.

The quantities of oxygen which were then separable from the blood by the air-pump were but small. The author has now endeavoured to procure larger quantities from it, and has been occupied with an examination of the general absorbent properties of the blood, especially with regard to oxygen. With this object, the blood was agitated with continually renewed portions of atmospheric air, and to ascertain subsequently how much air it contained from absorption, it was placed in a vessel filled with mercury, and which was closed with an iron stop-cock. This was then screwed upon a second vessel, likewise closed with a stop-cock, and containing carbonic acid. On opening the cock, the mercury fell out and the carbonic acid ascended to the blood. The vessels were then separated, and the blood continually agitated with the carbonic acid. The vessel was then screwed upon another completely filled with mercury, and the gas was allowed to collect in this. Carbonic acid was then in the same manner again mixed with the blood, which was then agitated and the gas also conveyed into this vessel; and this process was repeated several times. Finally, the collected gas was examined, the carbonic acid being absorbed by caustic potash, the oxygen being detonated with hydrogen, and the remainder considered as nitrogen.

Although these experiments appear so simple, it was at first impossible to carry them out, because the time which it was requisite to allow for the subsidence of the froth after each agitation was so great that decomposition commenced in the blood before the completion of the experiment.

This difficulty was subsequently removed by adding a drop of oil, which when placed on the surface of the blood soon caused the disappearance of the froth.

Numerous and repeated experiments, made in this manner, on the blood of calves, cattle and horses, have yielded tolerably uniform results, the minimum of oxygen being 10 per cent. and the maximum 12·5 per cent. of the volume of the blood, and the minimum of nitrogen being 1·7 per cent. and the maximum 3·3 per cent. reduced to the temperature of 32° F. and the mean barometric pressure.

The proportion in which the oxygen and nitrogen found in the gas obtained from the blood are, affords another indirect proof that no air had entered the vessels from without during the experiment. Had this been the case, the quantities of the two gases found would have been more nearly in the proportions existing in the atmosphere, whilst the oxygen ordinarily amounted to 3 and frequently 4 and 5 times as much as the nitrogen.

Although the difference between 10 and 12·5 per cent. is not inconsiderable, still it might appear remarkable that the experiments agree so well with one another, especially as they do not liberate the whole of the absorbed gas; but the more frequently fresh carbonic acid is added to the blood, the greater must be the quantity of gas. This is also true in practice; but after renewing the carbonic acid 3 or 4 times, the increase in the quantity of gas was found to be so small, that it fell within the limits of error of observation. Moreover, all the experiments were carried out under the same conditions. About 400 cubic centimetres of blood were usually employed; a smaller quantity was only used for some experiments. Too small a quantity could not be applied, as the amount of gas obtained is very small. The volume of carbonic acid, which was each time shaken with the blood, was never less than that of the blood itself; nor could much larger quantities be taken, because the vessels when completely filled with mercury would be managed with too much difficulty, and would be readily broken. Even in their present size containing about 700 cubic centimetres, they must be made of exceedingly strong glass. They are tall and cylindrical, with a narrow neck, and are graduated in cubic centimetres.

The quantity of oxygen which, as shown by these experiments, the blood was capable of absorbing, is, as the author has proved, sufficient to allow of the supposition that the whole quantity of air inspired is absorbed by the blood; but it is doubtful whether the arterial blood contains as much oxygen as that repeatedly shaken with atmospheric air, and whether the quantities obtained were but a small portion of the oxygen really absorbed.

To decide this, the experiments were varied, so that the blood was first agitated with constantly renewed quantities of carbonic acid, to remove the whole of the oxygen and nitrogen absorbed. The blood was then repeatedly shaken with measured quantities of atmospheric air, and the remaining air was again measured, and the quantity of carbonic acid, oxygen and nitrogen ascertained by the method previously described, when the quantity of oxygen and nitrogen absorbed was found. From numerous experiments conducted in this manner, the minimum amount of oxygen absorbed amounted to 10 per cent. and the maximum to 16 per cent. of the volume of the blood. In several experiments as much as 6·5 per cent. of nitrogen was absorbed.

In one of these experiments calves' blood was uninterruptedly shaken with atmospheric air; it was then repeatedly agitated with carbonic acid. By this treatment it yielded 11·6 per cent. of its volume of oxygen, but absorbed 154·9 per cent. of carbonic acid. This blood was then shaken with separate portions of a measured quantity of atmospheric air; it thus absorbed 15·8 per cent. of oxygen, yielding at the same time 138·4 per cent. of carbonic acid. Finally, it was again shaken with carbonic acid, and again yielded 9·9 per cent. of oxygen, whilst it absorbed 92·1 per cent. of carbonic acid.

It is thus evident that by agitation with carbonic acid we can separate almost the whole of the oxygen absorbed by the blood, which would be the most striking proof that the oxygen is not chemically combined with the blood, but exists in it merely as absorbed.

These experiments show that the blood is capable of absorbing $1\frac{1}{2}$ times its volume of carbonic acid, a result which has also been obtained by other observers. They moreover show that it is capable of absorbing from 10 to 12·5 per cent. of its volume of oxygen from the atmosphere, consequently 10 to 12 times more than water under the same circumstances, and that the absorption of nitrogen amounts to 6·5 per cent.

Moreover, experiments similar to those above described were instituted on the real arterial blood of horses which were of advanced age. By agitation with carbonic acid there was obtained—

Oxygen.	Nitrogen.
10·5	2·0
10·0	3·3

whence it is evident that the oxygen exists in at least as great quantities in the arterial blood of horses.

The author then shows, that from some observations upon the quantity of the inspired air and of the blood which flows into the lungs in a given time, we may conclude that the blood does not absorb more than half the oxygen which the experiments above detailed have shown to exist in it. This portion is each time consumed in the capillary vessels, and the remainder, amounting to the other half, remains in the venous blood.—*Chemical Gazette*, No. 71.

ANALYSIS OF TITANIFEROUS IRON.

M. H. Rose remarks, that the earlier analyses of this substance are discordant, on account of the imperfect separation of the titanic acid and oxide of iron. The titaniferous iron of Egersund in Norway has been analysed by M. Mosander, M. Kobell and M. H. Rose; the results obtained by the two first agree pretty well, but differ considerably from those of M. Rose.

In three operations on the above-named mineral M. Mosander obtained the following results:—

	I.	II.	III.
Titanic acid	39·04	42·59	41·08
Peroxide of iron	29·16	23·21	25·95
Protoxide of iron	27·23	29·27	29·04
Protoxide of manganese	0·21		
Magnesia	2·30	1·22	1·94
Oxide of cerium and of yttria	...		0·58
Oxide of chromium	0·12	0·33	
Silica	0·31	1·65	0·07
	98·37	98·27	98·66

The results of M. Kobell's analysis are,—

Titanic acid	43·24
Peroxide of iron	28·66
Protoxide of iron	27·91
	99·81

The same mineral analysed by M. Rose gave,—

Titanic acid	43·73
Peroxide of iron	42·70
Protoxide of iron	13·57
	100·00

Ann. de Ch. et de Phys., Nov. 1845.

ANALYSIS OF SPHENE.

M. Rose remarks, that the difficulty which exists in separating silica and titanic acid occasions the few analyses of sphène which have been made to differ considerably from each other.

Klaproth first analysed the sphène of Passau by fusing it with hydrate of potash; the mass dissolved in water, supersaturated with hydrochloric acid, left the silica as a residue, and the filtered solution was precipitated by solution of carbonate of potash; the precipitate after drying was digested in hydrochloric acid and left an additional residue of silica; ammonia poured into the filtered liquor precipitated the titanic acid, and after its separation the lime was obtained by carbonate of potash.

Klaproth afterwards analysed the sphène of Salzbourg by digesting with heat in hydrochloric acid; the insoluble residue was fused with carbonate of potash, treated with water, and the solution was saturated with hydrochloric acid;—the hydrochloric solutions were mixed and precipitated by ammonia, and the filtered liquor was freed from lime by carbonate of potash. The mixture of silica and titanic acid, precipitated by ammonia, was moderately dried and treated with hydrochloric acid, which dissolving the titanic acid, left the silica undissolved.

The results of these analyses were as follows:—

	Passau.	Salzbourg.
Silica	35	36
Titanic acid	33	46
Lime	33	16
Magnesia	trace	
Water		1
	101	99

These results differ greatly from each other; it could not be otherwise, on account of the methods employed: the second was better adapted than the first to give approximative results; but yet the quantity of lime obtained was too small.

Cordier found the sphène of Saint Gothard to consist of,—

Silica	28·0
Titanic acid	33·3
Lime	32·2
	93·5

He acted upon it by hydrochloric acid, evaporated to dryness, collected the silica on a filter, precipitated the titanic acid by ammonia, and afterwards the lime by caustic potash. M. Rose observes, that these three analyses, which have been made for a considerable time, are not entitled to great confidence; he afterwards states, that Fuchs has recently given an analysis of the sphène of Schwartzenstein in Zillerthal, by a very ingenious method. The action upon it by hydrochloric acid not succeeding, he fused it with potash, and boiled the calcined mass in distilled water, in order to get rid of the excess of potash, and separated by the usual processes a small quantity of silica dissolved in the operation. The residue was dissolved in concentrated hydrochloric acid, and the solution out of the contact of air was left for several days in contact with copper turnings at a moderate temperature. The silica formed a jelly, whilst the titanic acid, reduced by the copper to the state of oxide of titanium, remained dissolved in the liquor, which became of a deep blue colour. —The silica being separated, the oxide of titanium contained in the liquor was converted into titanic acid, and precipitated by ammonia; an oxalate poured into the filtered solution precipitated the lime. The quantity of copper dissolved by the hydrochloric acid gave that of the titanic acid.

The results were,—

Titanic acid	43·21
Silica	32·52
Lime	24·18
	99·91

M. Rose states, that the best method of analysing sphène, when the determination of its three elements directly is desired, consists in the use of concentrated sulphuric acid. The mineral, reduced to an impalpable powder, is put into a platina crucible with sulphuric acid diluted with half its weight of water; the mass is to be heated and stirred till the sulphuric acid begins to volatilize. The titanic acid dissolves in the sulphuric acid, the lime and silica separate; when the mass is cold, it is to be treated with a large quantity of water; the silica which remains is not however pure, but is of a yellowish colour, and it requires to be five times treated with sulphuric acid to be rendered pure and colourless. When the solution is treated with carbonate of soda it gives an insoluble residue, certainly more considerable in quantity than any of those yielded by the same treatment with other silicates, but much smaller than the residue left by the silica of sphène acted upon by hydrochloric acid.

1·022 gramme of the sphène of Zillerthal gave 0·338 of silica, neglecting, after its treatment with carbonate of potash, a residue of 0·052. This residue was completely acted upon by fusion with bisulphate of potash; the fused mass, treated with water, left 0·044 of insoluble silica. The filtered liquor contained no titanic acid; but by the usual process 0·008 of lime was separated from it. After the separation of the silica, the titanic acid was precipitated by ammonia; it weighed 0·436 gramme, and contained 0·011 oxide of iron, which was separated from the solution of the titanic acid in the concentrated sulphuric acid; the lime obtained after the separation of the titanic acid weighed 0·264. The result of this analysis is then—

Silica	32·29
Titanic acid	41·58
Oxide of iron	1·07
Lime	26·61
	101·55

Sphène is very readily fused with bisulphate of potash, and the analysis is easier than by sulphuric acid; but the results obtained are less certain.—*Ann. de Ch. et de Phys.*, Nov. 1845.

ANALYSIS OF CASCARILLA—CASCARILLINE. BY M. DUVAL.

The following principles were found by the author in cascarilla:—Albumen, Tannin of a peculiar kind, Crystallizable bitter matter (Cascarilline), Red colouring matter, Fatty matter having a nauseous smell, Wax, Gummy matter, Volatile oil of an agreeable odour, Resin, Starch, Pectic acid, Chloride of potassium, Salt of lime, Lignin.

Of these various principles, the bitter matter, or *cascarilline*, is the most interesting in the opinion of the author; it was extracted by the following process:—The cascarilla reduced to a moderately fine powder was introduced into a percolating apparatus, and treated with water; the infusions being mixed, acetate of lead is to be added; filter and get rid of the excess of lead by sulphuretted hydrogen; filter again and evaporate the liquor to about $\frac{2}{5}$, then add a little animal charcoal and filter again; the evaporation is to be continued at a very low temperature; at a certain period a pellicle is formed, and the liquor, before it acquires the consistence of a syrup, is to be allowed to cool. Sometimes the matter does not crystallize; it then assumes a resinous appearance, and the greater part of it adheres to the vessel; this substance hardens on cooling.

In order to purify the product, whether crystalline or resinous in appearance, it is to be powdered, put into a tube, and alcohol of sp. gr. 0·8763 added cold, and rather more than is requisite to moisten it. After sufficient digestion, the alcoholic liquid, which is highly coloured, is to be separated; by these means the greater part of the colour and fatty matter of the cascarilline are removed; and the residue, which is evidently decolorized, is to be dissolved in boiling alcohol of sp. gr. 0·8439; a little animal charcoal is to be added, and after filtration the liquor is to be suffered to evaporate spontaneously. The residue is to be dried on filtering-paper, triturated with cold and dissolved in boiling alcohol, filtered and set aside to evaporate spontaneously, and by this the cascarilline is obtained pure.

The properties of cascarilline are, that it is white and crystallized; the crystals, when examined by the microscope, have usually the form of prismatic needles, and sometimes that of hexagonal plates. Cascarilline is inodorous, bitter, but not immediately so, on account of its slight solubility in water; when heated in a glass tube, it fuses and yields a syrupy liquid resembling burnt sugar in appearance; its melting-point is lower than that of salicine; on cooling after melting it retains its transparency and becomes brittle like resin; if, on the contrary, the action of heat upon it be continued, it decomposes, yielding a vapour which reddens litmus paper; when rubbed with a little potash and heated in a tube so as to be decomposed, it does not restore the blue colour of reddened litmus; when heated on platinum foil, it burns without leaving any residue. Cascarilline is soluble in alcohol and æther, but is very slightly soluble in water, but nevertheless imparts strong bitterness to it; the solution does not alter litmus, and is not precipitated either by acetate or diacetate of lead, by tannin or the alkalies. Cold concentrated sulphuric acid dissolves cascarilline and becomes immediately of a deep red colour with a purple tint; water added to it occasions precipitation; the liquor then appears of a grass-green colour, which is more or less deep according to the quantity of water added; as the precipitate falls the liquor becomes colourless, and the precipitate only retains the green colour. Excess of ammonia does not dissolve this precipitate, but renders it of an ochre-yellow colour.

Concentrated nitric acid also dissolves cascarilline; the solution is yellow and is precipitated by ammonia, excess of which does not dissolve the precipitate.

Hydrochloric acid also dissolves it and becomes of a violet colour, which the addition of a small quantity of water renders blue, and more renders it green.

As cascarilline contains no nitrogen, and does not combine either with acids or alkalies, it must be classed with the neutral non-azotized bodies, such as salicine, colombine, &c.—*Journ. de Pharm. et de Ch.*, Août 1845.

SPHEROIDAL CONDITION OF LIQUIDS *.

To Richard Taylor, Esq.

SIR,

I cannot think that Mr. Armstrong's explanation of the fact,—that the temperature of liquids in the above condition is lower than their boiling-point,—is the true one; or rather, I ought to say, that his paper does not in any way account for the phænomenon it so ably describes.

The *separation* of the liquid from the dish depends upon the molecular relations between the liquid and the dish being altered by the increased temperature of the latter; and this is, I think, all that can be said of the matter in the present state of our knowledge.

But, taking the separation as a fundamental fact, we must, to explain the *diminution* of temperature, solve the following problem, at least approximately:—

"A spheroidal mass of a vaporizable liquid is surrounded by a medium of a temperature t, t varying from point to point; required

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* Mr. Armstrong's paper to which this note refers will be found at p. 52 of the present volume.

the temperature of the fluid at any point:" t must be previously determined.

We now see, on distinctly annunciating the question, that, in the vast difference between the maximum value of t and the temperature of the dish will be found a clue to the true explanation.

I remain, dear Sir, with great respect,
Your obedient Servant,
October 14, 1845. S. X.

P.S. I shall perhaps at a future time attempt a formal solution of the problem. It does not exclude M. Boutigny's views.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1845.

Chiswick.—October 1. Very fine. 2, 3. Rain. 4. Cloudy and fine. 5. Cloudy and fine: clear. 6. Foggy: cloudy: rain. 7. Rain: cloudy: clear. 8. Foggy: fine. 9. Rain; clear. 10. Heavy rain. 11. Rain, with fog: showery: clear: slight frost. 12. Dense fog: cloudy: fine. 13—16. Very fine. 17—19. Overcast and fine. 20. Densely overcast: clear and fine. 21, 22. Fine. 23. Fine: clear and frosty at night. 24. Dense fog: very fine. 25, 26. Foggy: partially overcast. 27. Overcast: clear. 28—30. Very fine. 31. Overcast.—Mean temperature of the month ½° below the average.

Boston.—Oct. 1. Fine. 2. Rain: rain early A.M. 3. Cloudy: rain A.M. and P.M. 4. Fine: rain P.M. 5. Fine. 6. Fine: rain P.M. 7. Rain: rain early A.M.: rain A.M. 8, 9. Fine: rain P.M. 10. Fine. 11. Rain: rain early A.M. 12. Fine. 13. Cloudy: rain early A.M. 14—16. Fine. 17, 18. Cloudy. 19. Cloudy: rain A.M. 20. Cloudy. 21. Fine. 22—24. Cloudy. 25, 26. Fine. 27. Cloudy. 28. Fine. 29. Cloudy. 30, 31. Fine.

Sandwick Manse, Orkney.—Oct. 1. Showers. 2. Bright: clear. 3. Cloudy: drops. 4. Showers. 5, 6. Bright: clear. 7. Rain. 8. Rain; damp. 9, 10. Rain. 11. Showers: rain. 12. Bright: cloudy: halo large. 13. Rain. 14. Cloudy: rain. 15. Cloudy. 16. Showers. 17. Showers: sleet: cloudy. 18. Showers: clear. 19. Cloudy: rain. 20. Rain: showers. 21. Cloudy: damp. 22. Damp: drizzle. 23. Damp: cloudy. 24. Bright: rain. 25. Bright: cloudy. 26. Showers: rain. 27. Showers: bright: cloudy. 28. Rain. 29. Drizzle: cloudy. 30, 31. Showers: clear.

Applegarth Manse, Dumfries-shire.—Oct. 1. Fair, but threatening. 2. Dull: rain. 3. Deluge of rain: flood. 4. Soft rain all day. 5, 6. Fair and clear: hoar-frost. 7. Fair, but cloudy. 8. Fair A.M.: rain P.M. 9. Rain early A.M.: cleared. 10. Slight showers P.M. 11. Fine: rain. 12. Fair, but cloudy: halo. 13. Rain. 14. Fair and fine. 15. Wet A.M.: cleared. 16. Fair and clear. 17. Rain all day. 18. Frequent showers. 19. Rain. 20. Bitter showers. 21. Moist, but no fall. 22. Fair and fine. 23. Fair and fine: cloudy. 24. Drizzling P.M. 25. Beautiful day: one slight shower. 26. Fair, but chilly: rain P.M. 27, 28. Wet. 29. Wet: very wet P.M. 30. Wet early A.M.: fine P.M. 31. Fair and fine.

Mean temperature of the month	49°·6
Mean temperature of Oct. 1844	47 ·2
Mean temperature of Oct. for twenty-three years	46 ·4

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

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1845. Oct.	Max.	Min.	8½ a.m.	9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	Max.	Min.	9¼ a.m.	8½ p.m.								
● 1.	29·946	29·844	29·50	29·60	29·54	29·35	29·45	61	46	50	56½	43	51	48	sw.	calm	sw.	w.	·03	......	......	·53
2.	29·744	29·680	29·23	29·52	29·52	29·61	29·75	64	58	53	55½	48	48	43	sw.	calm	sw.	n.	·07	·08	......	·31
3.	29·594	29·510	29·08	29·40	29·35	29·83	29·72	67	45	61	49½	43½	46½	45	s.	calm	e.	ne.	·17	......	......	......
4.	29·568	29·528	29·03	29·40	29·45	29·64	29·59	65	47	56·5	49½	45½	44	42	sw.	w.	ne.	nw.	......	·33	......	......
5.	29·969	29·891	29·39	29·74	29·80	29·76	29·87	60	29	50	52	32	47	39	w.	calm	w.	se.	......	·03	......	·22
6.	29·871	29·404	29·50	29·76	29·63	29·84	29·79	52	46	44	55	34	50	48	e.	calm	e&sw.	se.	·22	......	......	......
7.	29·491	29·433	28·93	29·38	29·24	29·73	29·32	65	47	53	53½	44½	46	48	sw.	calm	sse.	e.	·01	·36	2·10	......
☽ 8.	29·450	29·339	29·04	29·23	29·07	29·35	29·39	61	34	45	55	38	39	48½	sw.	calm	sse.	e.	·01	·08	......	·92
9.	29·394	29·295	28·92	29·15	29·20	29·38	29·43	52	33	48·5	55	43	47	49½	s.	calm	s.	e.	·35	·08	......	·63
10.	29·431	29·389	29·02	29·19	29·17	29·48	29·52	58	38	45	53½	41	48	48½	sw.	calm	s.	e.	·43	·11	......	·20
11.	29·851	29·399	29·06	29·26	29·55	29·57	29·69	58	29	50·5	54	45	49	46	w.	calm	sw.	ese.	·09	·06	......	·21
12.	30·213	30·083	29·72	29·82	29·97	29·87	30·03	61	39	47	55	40	48	47	sw.	calm	sw.	e.	......	·17	......	·50
13.	30·419	30·282	29·86	29·94	30·06	29·95	29·94	67	38	50	57½	46½	49½	53	sw.	calm	s.	ese.	......	·02	......	·12
14.	30·435	30·304	29·90	30·14	30·02	30·03	29·87	76	36	54	63½	54	55	55	s.	w.	sw.	s.	......	......	0·53	·52
○ 15.	30·242	30·030	29·72	29·90	29·89	29·82	29·70	66	51	51	60	53½	53	50	s.	sw.	wsw.	ssw.	·01	......	......	·19
16.	30·196	30·099	29·59	29·88	29·84	29·61	29·45	62	37	51	55	46½	49	49	w.	calm	wsw.	sw.	......	......	......	·25
17.	30·126	30·031	29·60	29·73	29·65	29·54	29·54	58	48	48·5	58	50	48	48	sw.	calm	sw.	w.	......	......	......	·24
18.	30·218	30·111	29·55	29·76	29·93	29·38	29·85	60	48	56·5	60	53½	50½	48	w.	w.	wsw.	nw.	......	......	......	·28
19.	30·253	30·123	29·68	29·96	29·80	29·95	29·53	61	51	56	56½	46½	48	48½	w.	w.	w.	se.	......	......	......	·20
20.	30·148	30·020	29·39	29·66	29·91	28·79	29·78	61	38	54	56	46½	49	45	w.	w.	wnw.	nw.	......	·07	......	·28
21.	30·341	30·254	29·77	30·13	30·18	30·00	30·08	57	34	46	52	41½	49	50	w.	w.	w.	w.	......	......	0·73	·07
22.	30·477	30·405	29·93	30·24	30·30	30·09	30·11	57	44	50	54½	45	50	50	w.	calm	w.	w.	......	......	......	·04
☾ 23.	30·510	30·477	30·00	30·28	30·25	30·08	30·00	55	27	50	58	46½	51½	51	w.	calm	w.	sw.	......	......	......	·24
24.	30·402	30·144	29·95	30·13	29·94	29·96	29·69	59	29	48	53½	48½	51½	47½	s.	calm	sw.	w.	......	......	......	......
25.	30·318	30·176	29·79	30·13	30·18	30·03	29·90	57	29	44	52	38½	45	47½	n.	calm	w.	wsw.	......	......	......	·12
26.	30·321	30·230	29·90	30·03	29·87	29·74	29·52	50	35	38	50½	37½	45½	42	sw.	calm	sw.	......	......	......	......	·16
27.	30·113	30·060	29·60	29·75	29·70	29·70	29·79	54	40	47	53½	48	43	37½	w.	w.	sw.	......	......	......	......	·53
28.	30·069	30·036	29·57	29·78	29·78	29·59	29·55	60	48	52	56	52	47	52½	sw.	calm	sw.	sw.	......	......	......	·48
29.	29·970	29·859	29·52	29·71	29·55	29·65	29·59	59	36	51·5	53½	50	52	50	s.	calm	sw.	se.	......	......	......	·29
● 30.	29·966	29·884	29·40	29·65	29·93	29·53	29·81	63	43	50	54½	50	50	49	sw.	calm	wnw.	wsw.	......	......	0·83	·13
31.	30·159	30·118	29·74	30·00	30·00	29·77	29·87	55	34	43	54½	35	50	49½	ne.	calm	wnw.	w.	......	......	......	·16
Mean.	30·039	29·917	29·51	29·750	29·750	29·697	29·713	60·03	39·90	49·8	55·9	44·7	48·38	47·59					1·39	1·39	4·19	7·82

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END OF THE TWENTY-SEVENTH VOLUME.

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Fig: 1.

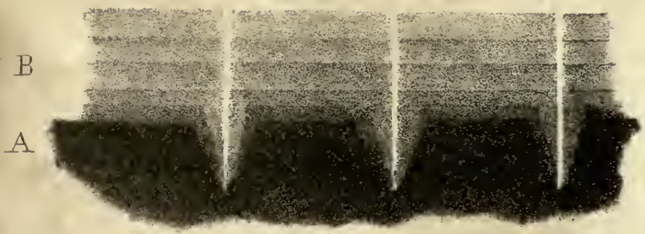
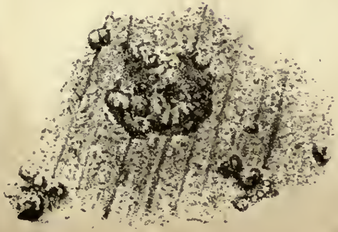


Fig. 2.

Fig: 4.

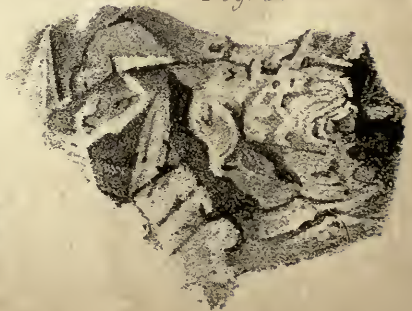
Scale for Fig: 4.
of 1000ths of an inch

Fig. 5.

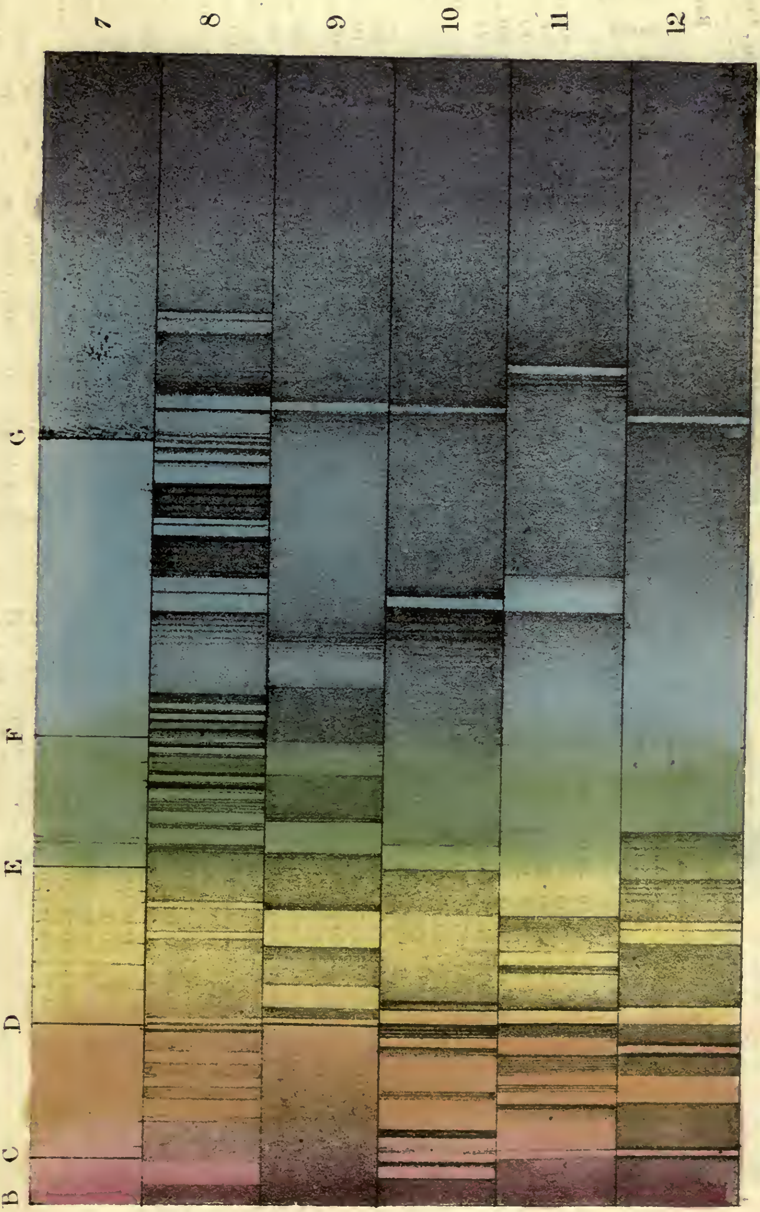


Scale for Fig. 5.
of 1000ths of an inch

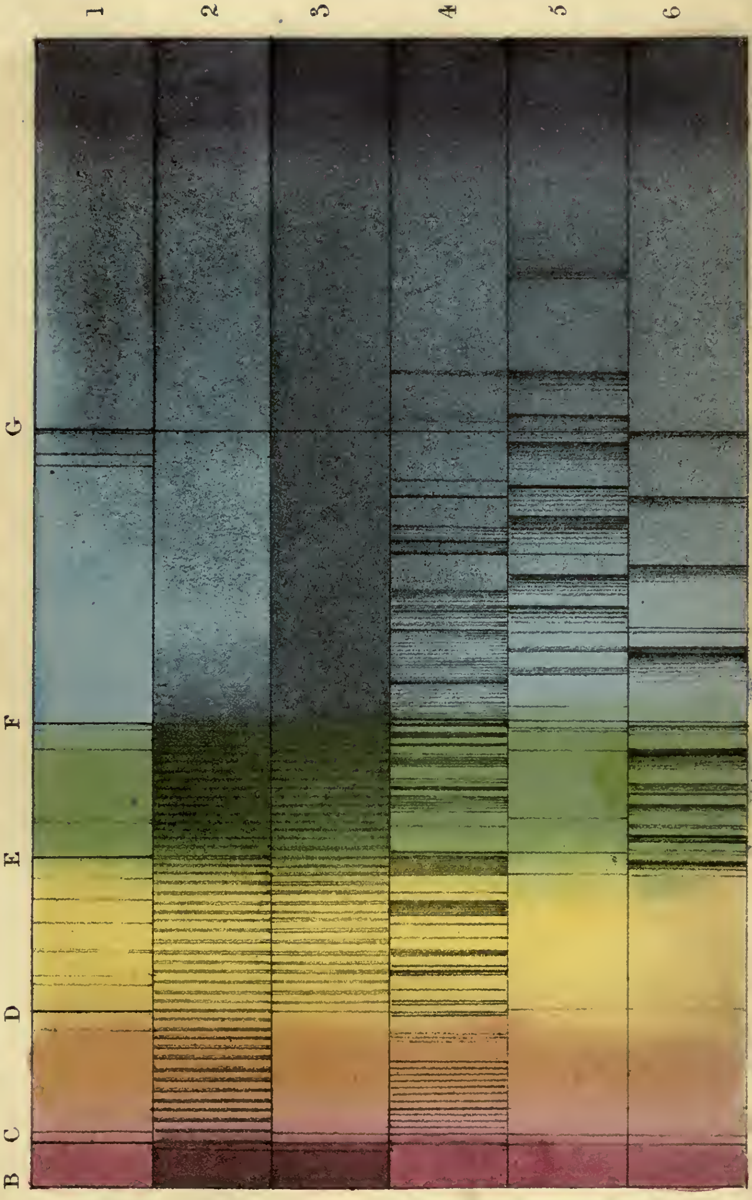
Fig: 3.



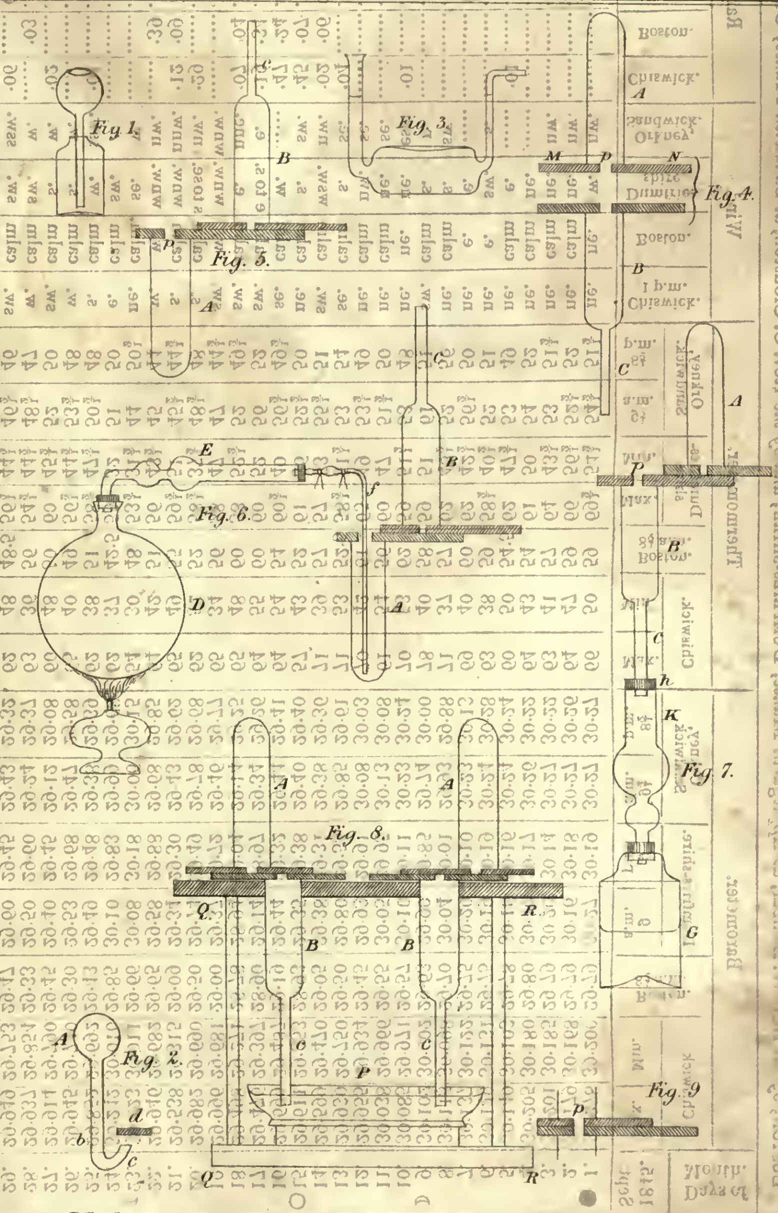
Scale of Figs. 2 & 3
of 500ths of an inch



Spectra of Coloured Flames.



Spectra of Daylight through Coloured Glasses & Vapours.



Dr. Draper on the Allotropism of Chlorine.

J. Basire, lith.

Fig. 1.

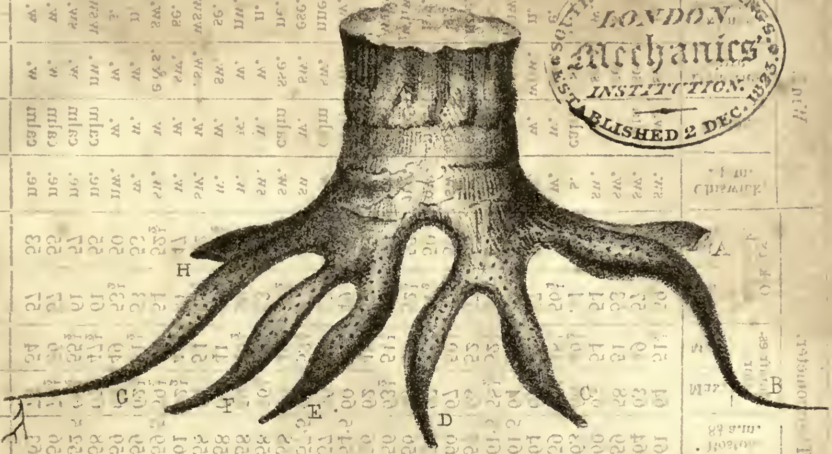


Fig. 2.

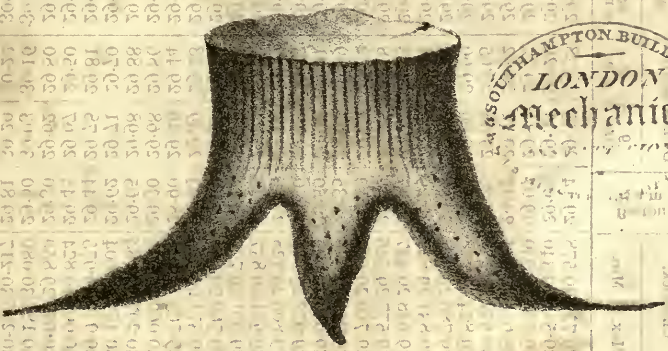
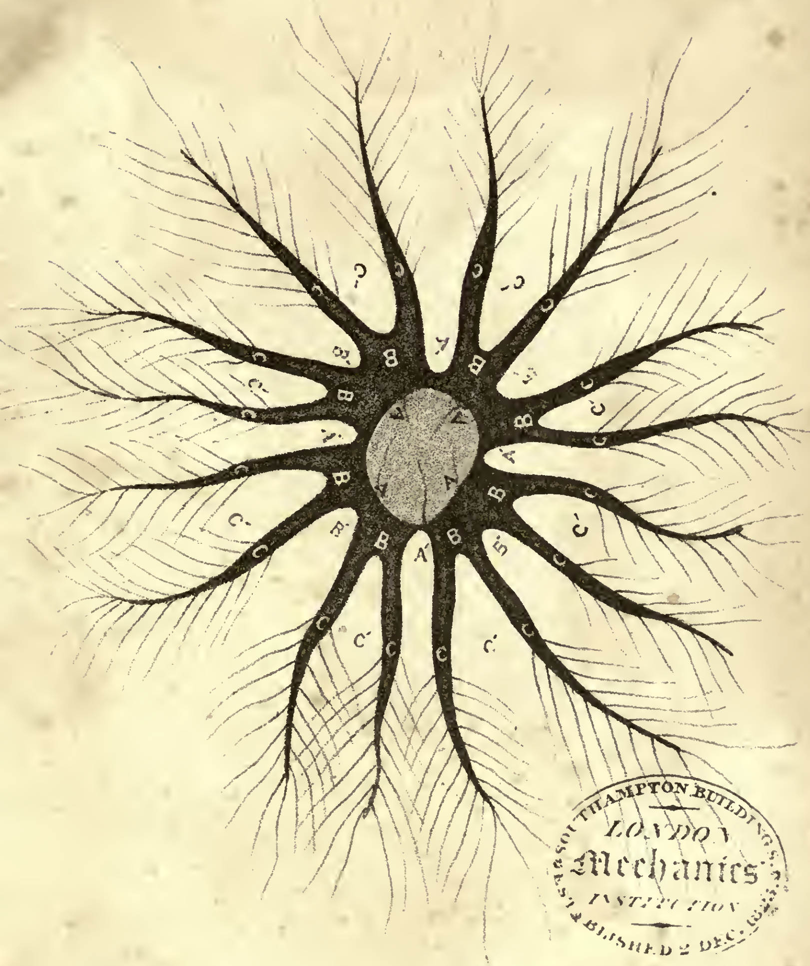


Fig. 3.



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