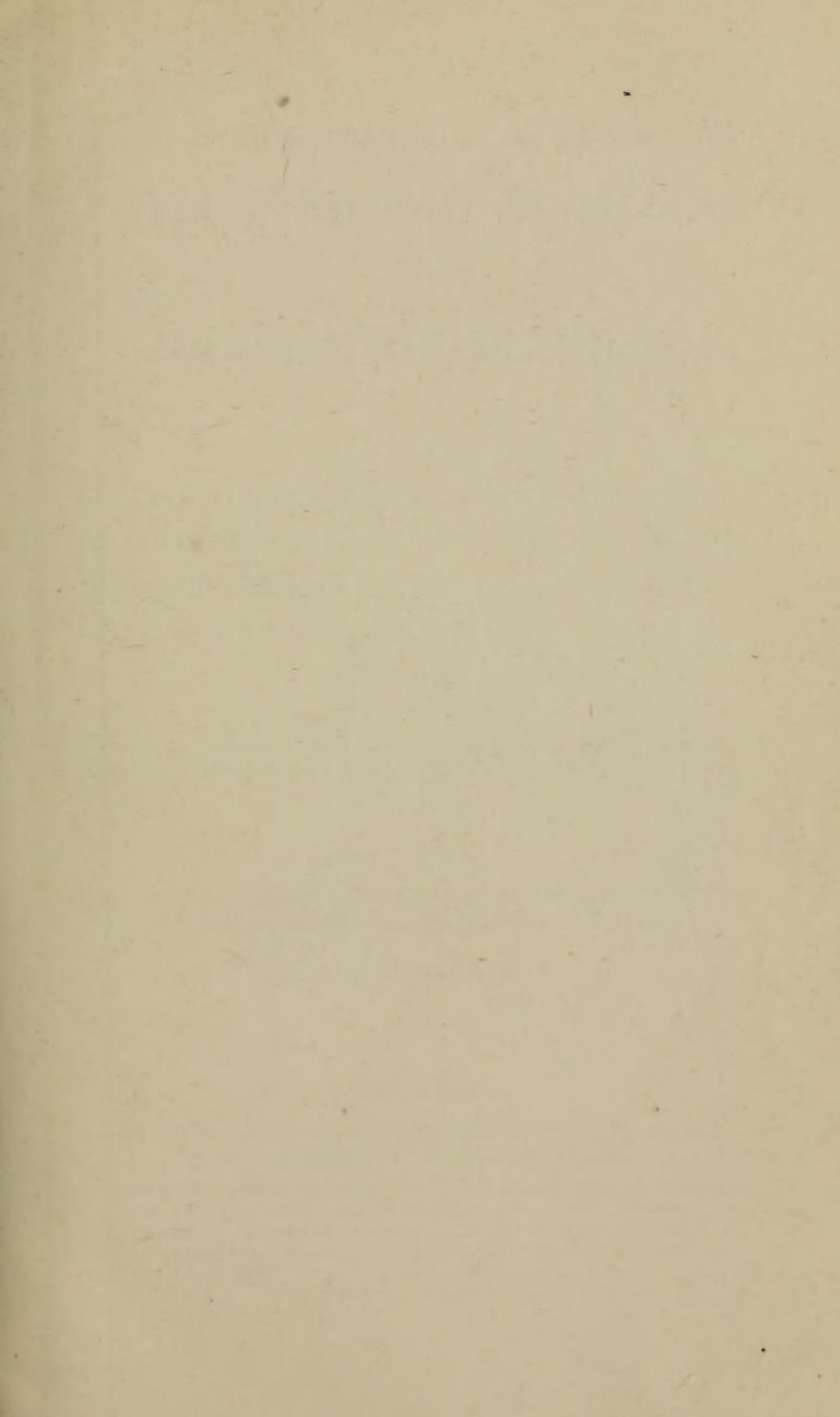
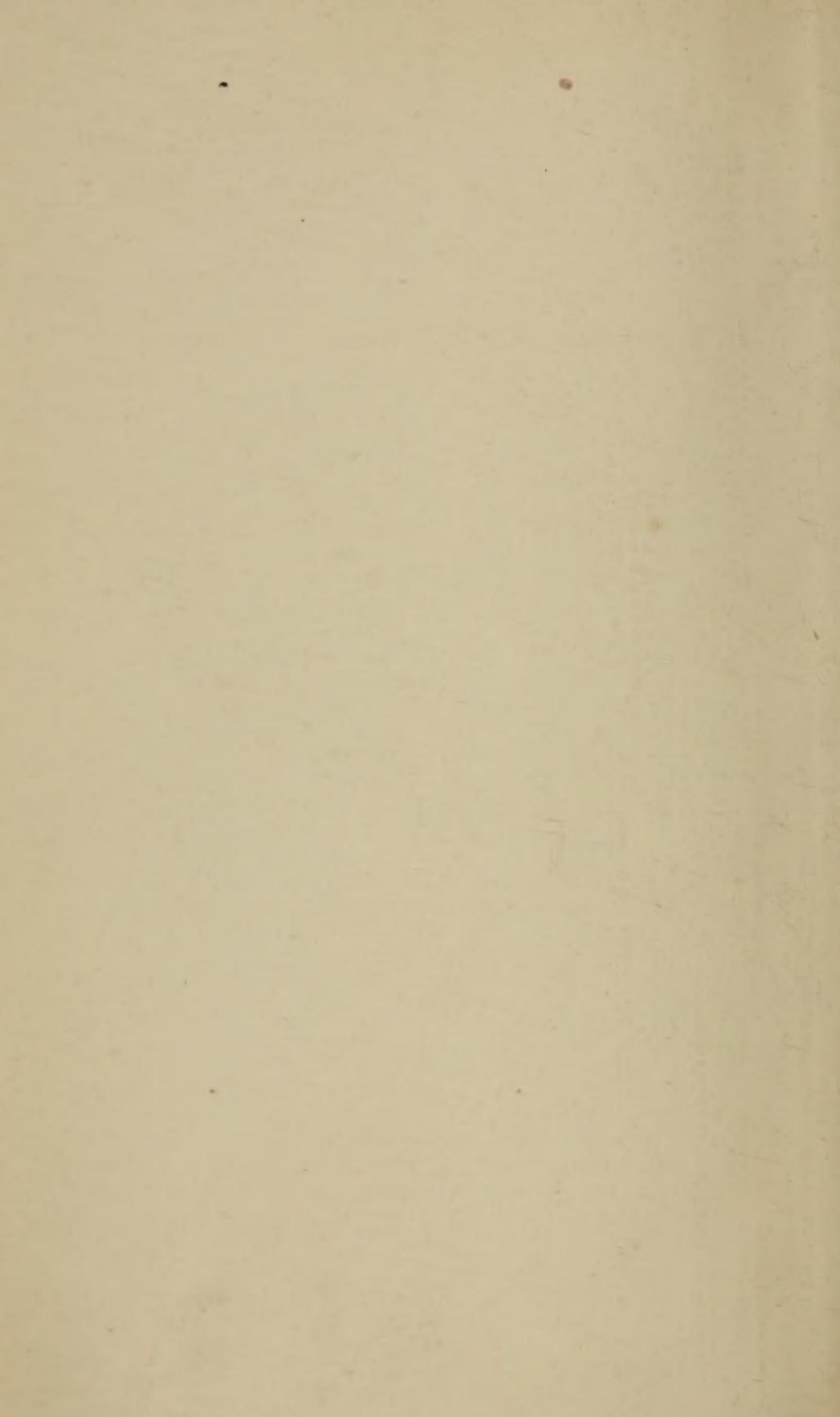


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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L.&E. &c.
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RICHARD PHILLIPS, F.R.S.L.&E. F.G.S. &c.
SIR ROBERT KANE, M.D. M.R.I.A.

“Nec araneorum 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. XXX.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE,
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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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- IV. Illustrative of Prof. Wartmann's Paper on Induction.
- V. Illustrative of Dr. Waller's Paper on the Microscopic Examination of the Papillæ and Nerves of the Tongue of the Frog.
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THE
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JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1847.

I. *On the Composition and Explosion of Gun-Cotton.*
By THOMAS RANSOME, Esq.*

SINCE the announcement by Prof. Otto of the process for preparing gun-cotton, many experiments have been tried both with reference to improvement in its manufacture and also to its explosive force, but the only explanation of its composition has been derived from the quantity of nitric acid taken up by the cotton. The following experiments were made to ascertain the quantities of carbon and hydrogen it actually contains.

The gun-cotton that I have analysed was prepared with carded cotton, and nitric acid of specific gravity 1·514 mixed with half its bulk of sulphuric acid, in the proportion of an ounce and a half of nitric acid to each 100 grs. cotton. Thus prepared it was quite white, and the fibre appeared unaltered; it left not the smallest residue upon ignition, and was quite insoluble in æther.

The carbon and hydrogen were estimated, as is usual in substances containing nitrogen, the cotton being mixed with oxide of copper, and the last few inches of the combustion-tube filled with metallic copper. It decomposed quite tranquilly, not giving out more gas at once than could conveniently pass through the potash apparatus.

In the first analysis 2·35 grs. gave 2·255 grs. of carbonic acid and ·665 gr. of water. In the second 2·075 grs. gave 2·01 grs. of carbonic acid and ·59 gr. of water.

Calculated to 100 parts—

	I.	II.
Carbon	26·16	26·41
Hydrogen	3·14	3·19

* Read to the Literary and Philosophical Society of Manchester, Dec. 1, 1846, and communicated by the Author.

The mean of three experiments to ascertain the increase in weight in converting cotton-wool into gun-cotton was 64·1 upon each 100 parts of cotton: if this increase was anhydrous nitric acid, the gun-cotton would contain 10·20 per cent. of nitrogen.

Composition in 100 parts, taking the mean of the analyses of carbon and hydrogen,—

Carbon.....	26·28
Hydrogen	3·16
Oxygen	60·36
Nitrogen	10·20
	<hr/>
	100·00

These numbers nearly represent the formula $C_{12}H_8O_{20}N_2$, which would contain in 100 parts—

Carbon.....	26·82
Hydrogen	2·94
Oxygen	59·70
Nitrogen	10·54
	<hr/>
	100·00

The cotton used was also analysed, being burnt with chromate of lead instead of oxide of copper.

1·435 gr. gave 2·325 grs. of carbonic acid and ·91 gr. of water.

In 100 parts—

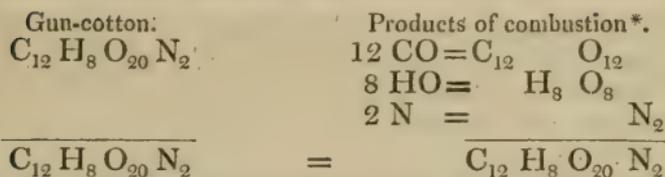
Carbon.....	44·20
Hydrogen	7·04
Oxygen	48·76
	<hr/>
	100·00

Theoretical composition of $C_{12}H_{10}O_{10}$ —

Carbon.....	44·44
Hydrogen	6·17
Oxygen	49·39
	<hr/>
	100·00

From these formulæ, it appears that gun-cotton is formed from ordinary cotton by the abstraction of two atoms of hydrogen and the addition of two atoms of nitric acid. If this is the case, 100 parts of cotton ought to increase to 165·4; by experiment they increase to 164·1. The abstraction of two atoms of hydrogen will make the nitrogen ·18 per cent. more, which brings it nearer to the theoretical composition.

If this formula be correct, the cotton in exploding without the presence of oxygen would be converted entirely into carbonic oxide, water and nitrogen, thus:—



To ascertain whether carbonic oxide was produced, and if the cotton would explode without air, the following experiment was tried, the object being to explode the cotton either *in vacuo*, or in some indifferent gas which could afterwards easily be removed. I selected carbonic acid as being easily obtained and readily removed by a caustic alkali, and also because the air was more completely excluded than in the partial vacuum of an air-pump.

The gas was generated in a Woolfe's bottle, and dried by passing through a tube filled with chloride of calcium; it was then conducted into a rather wide tube in which the cotton was placed (about the tenth of a grain for each explosion); to this a small bent tube was attached, the end of which was under the water of the pneumatic trough. The carbonic acid was allowed to escape until a small quantity collected in a test-tube was entirely absorbed by caustic soda; the cotton was then exploded by placing a spirit-lamp under the part of the tube containing it, at the same time the gas given out at the small tube was collected in an inverted jar: this was repeated several times until a sufficient quantity of gas was obtained; the carbonic acid was then absorbed by caustic soda.

The residual gas was inflammable, and when mixed with oxygen and exploded by an electric shock was partially absorbed by caustic potash.

Red vapours were not formed when the gas was mixed with oxygen, showing that nitric oxide had not been formed.

After the explosion not the slightest residue was left in the tube.

This experiment proves that gun-cotton will explode without the presence of oxygen, and also under the same circumstances that carbonic oxide is formed, but no nitric oxide.

Manchester, Nov. 28, 1846.

Since the above paper was read, I have estimated the nitro-

* Messrs. Porrett and Teschemacher have proved, in a paper read to the Chemical Society, that cyanogen is a product of the explosion of gun-cotton. As this is the case, the gases actually produced will not be exactly as described in the above diagram. It is possible that the gases would be produced in the proportion indicated above, if the gun-cotton exploded at a temperature lower than that requisite to cause the combination of the nitrogen with the carbon.—ED.

gen from the ratio between the volumes of carbonic acid and nitrogen formed by combustion with oxide of copper. In one experiment the volume of carbonic acid was 2·43 cubic inches, to nitrogen ·46 cubic in. or as 6 to 1; therefore taking the carbon as the mean of the two former analyses, or 26·28, the nitrogen would amount to 10·33 per cent. I also endeavoured to ascertain the amount of nitrogen by measuring the volume of gas given out by a known weight of gun-cotton, and subtracting the carbonic acid as calculated from the previous estimation of carbon; but the quantity of nitrogen obtained was always too small, owing to the air in the tube taking part in the combustion, the largest amount obtained being 8·20 per cent.

Manchester, Dec. 14, 1846.

II. *On the Meteor of September 25, 1846.*

By Sir J. W. LUBBOCK, Bart., F.R.S.*

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

AS I happened accidentally to witness the extraordinary phenomenon which occurred on the 25th of September last †, and as it does not seem to have formed the subject of any other communication to your Journal, I address the following remarks, well-aware how imperfect unfortunately they must appear.

I happened to be walking just outside the house at a quarter before 10 P.M., when suddenly the whole scene was illuminated with a light, as it seemed to me, equal to that of the sun at noonday: certainly all objects became much more clearly visible than I ever saw them in the brightest moonlight. But contrast might lead to an exaggeration in this estimate. As the light proceeded from behind me, I immediately turned round, and I saw in Ursa Minor, and, as nearly as I can guess, occupying the place I have assigned to it in the inclosed sketch, a most brilliant meteor. I scarcely saw it before it faded, and in a few seconds it had entirely disappeared. As it disappeared the stars became again visible, and as I took particular note of its position I cannot be far out in my estimate. But the occurrence came so suddenly, never having before heard of any such apparition, and feeling confident that it would be observed by persons more competent than myself, that I neglected at the instant to observe by my watch the number of seconds it remained visible. Instrumental observation under any circumstances would have been impossible. I do not

* Communicated by the Author. † See the Morning Post of the 26th.

apprehend that it *really* occupied so large a portion of the heavens as I have assigned to it; I think it probable that the



extreme rapidity of its motion left the impression upon the retina, but this can only be a matter of conjecture. It is much to be wished that any observation could be recorded of its position as seen from any other place, by which its parallax, if even roughly, could be obtained.

This body acquires still greater interest from its resemblance in external character to the *bolide* of the 21st March 1846, whose orbit has been calculated by M. Petit†, who assigns to it a period (round the earth) of $\cdot 1145685$ day, or about two hours.

Another bolide was seen in France on the 9th of October of this year‡, of which the external characters also resemble those of the meteor seen in London on the 25th of September 1846.

I am, Gentlemen,

Your most obedient Servant,

High Elms, near Farnborough, Kent,
November 25, 1846.

J. W. LUBBOCK.

P.S. Long since what precedes was written, the number of the *Comptes Rendus* of the 23rd November last has been

† See *Comptes Rendus*, October 12, 1846.

‡ *Ibid*, October 26, 1846.

received, and in it are accounts of two other meteors observed at Dijon on the 17th of October and on the 9th of November. M. Méline describes the latter in words which would almost exactly represent the phenomenon which I have attempted, so imperfectly however, to place on record.

“Je sortais des serres du jardin, quand j’ai été frappé tout à coup d’une lumière aussi intense que celle du jour; j’aperçus distinctement toutes les parties du jardin, les arbustes comme les arbres, les plantes, etc. Je vis tout avec une teinte *jaune serin*. D’abord je crus à un incendie; mais, en jetant les yeux au ciel, j’ai vu un globe de feu se mouvant plus lentement qu’une fusée, de l’ouest à l’est, horizontalement, à 60 ou 70 degrés de hauteur. Le météore a laissé, sur toute la longueur de la route qu’il a suivie, une immense traînée d’un blanc couleur de cendre.”

III. *Remarks on the Rev. B. Bronwin’s Method for Differential Equations.* By GEORGE BOOLE, Esq.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

I DESIRE very briefly to notice an error into which the Rev. B. Bronwin has fallen, in a paper on the Integration and Transformation of certain Differential Equations, published in the last Number of the *Philosophical Magazine*.

In doing this I shall adopt a notation which I have before employed in a similar description of analysis, and which has the advantages of brevity and simplicity. Changing for convenience p into m and y into u , we may observe that Mr. Bronwin’s transformations depend in all cases on the properties of two compound factors, which we shall designate by π_m and ρ , and which, upon whatever subject they may operate, combine in subjection to the relation

$$\pi_m \rho = \rho \pi_{m-1}, \dots \dots \dots (1.)$$

the equation to be solved being

$$\pi_m u = 0. \dots \dots \dots (2.)$$

Thus, in Mr. Bronwin’s first equation, D standing for $\frac{d}{dx}$,

$$\pi_m = x(D^2 + k^2) + 2mD, \rho = D^2 + k^2; \dots \dots (3.)$$

in the second,

$$\pi_m = D^2 + kx D - mk, \rho = D + kx; \dots \dots (4.)$$

in the third,

$$\pi_m = x(D^3 + k^3) + 3mD, \rho = D^3 + k^3; \dots \dots (5.)$$

and so on for the rest.

Now (1.) gives $\pi_m \rho v = \rho \pi_{m-1} v$,

whatever v may be; let $\rho v = u$, then $v = \rho^{-1} u$, and we have

$$\pi_m u = \rho \pi_{m-1} \rho^{-1} u;$$

so that the symbol π_m is equivalent in operation to the compound symbol $\rho \pi_{m-1} \rho^{-1}$; writing then $\pi_{m-1} = \rho \pi_{m-2} \rho^{-1}$, we have

$$\pi_m u = \rho^2 \pi_{m-2} \rho^{-2} u;$$

and finally,

$$\pi_m u = \rho^m \pi_0 \rho^{-m} u.$$

Hence the equation $\pi_m u = 0$ gives

$$\rho^m \pi_0 \rho^{-m} u = 0;$$

$$\therefore u = \rho^m \pi_0^{-1} \rho^{-m} 0. \quad . \quad . \quad . \quad . \quad (6.)$$

This equation is unquestionably true, whatever may be the interpretation of the symbols π_m and ρ , provided that they satisfy the combination law (1.).

Now taking Mr. Bronwin's first equation, viz.

$$x (D^2 + k^2) u + 2 m D u = 0,$$

and making $\pi_m = x (D^2 + k^2) + 2 m D$, $\rho = D^2 + k^2$,

we have $\pi_0 = x (D^2 + k^2)$;

whence (6.) gives

$$u = (D^2 + k^2)^m \{x (D^2 + k^2)\}^{-1} (D^2 + k^2)^{-m} 0.$$

But $\{x (D^2 + k^2)\}^{-1} = (D^2 + k^2)^{-1} x^{-1}$,

therefore $u = (D^2 + k^2)^{m-1} x^{-1} (D^2 + k^2)^{-m} 0.$

Without entering into any special examination of the above result, and merely resting on the analogy of many similar cases, I should at once assert that, when m is greater than 0, we ought to retain two of the constants which arise from the performance of the inverse operation $(D^2 + k^2)^{-m} 0$, and that it is not necessary that we should retain more than two. Thus one form of solution is unquestionably

$$u = (D^2 + k^2)^{m-1} x^{-1} (c \cos kx + c' \sin kx);$$

and there are, I believe, many equivalent forms. The nature of Mr. Bronwin's error consists in his virtually rejecting the factors x^{-1} and $c \cos kx + c' \sin kx$, and his result is accordingly nugatory whenever $m > 0$. Similar remarks apply to the other equations in his paper, of which however I have only examined a small number in detail.

Perhaps, in connexion with this subject, it may not be quite irrelevant to mention, that about two years since I obtained the solution of the purely symbolical equation

$$\pi_m \pi_n u + q \rho u = 0,$$

in which, beside the law of combination,

$$\pi_m \rho = \rho \pi_{m+1}, \quad \pi_n \rho = \rho \pi_{n+1},$$

the further condition was imposed,

$$\pi_m \pi_n = \pi_n \pi_m - a(m-n)\rho.$$

This solution, which will appear in the Cambridge and Dublin Mathematical Journal for January 1847, involves the solution of a class of differential equations of which that of Laplace's functions is only a very particular case. Such methods, limited in their individual application, and apparently indefinite in their number, seem however to be chiefly valuable as exercises in symbolical algebra. Linear differential equations, and linear equations in finite differences, may, as I have elsewhere shown, be reduced to the general form

$$u + \phi_1(\pi)\rho u + \phi_2(\pi)\rho^2 u \dots = U,$$

in which π and ρ satisfy the relations $f(\pi)\rho u = \rho f(\pi+1)u$ and $f(\pi)\rho^m = f(m)\rho^m$; and additional experience confirms my belief, that the methods which are founded on the employment of this form are sufficient for every case. Mr. Bronwin's equations, when thus treated, are at once seen to be integrable.

It is but justice to add, that Mr. Bronwin's method displays considerable ingenuity.

I remain, Gentlemen,

Your obedient Servant,

Lincoln, Dec. 4, 1846.

GEORGE BOOLE.

IV. *Observations on the Rev. B. Bronwin's Paper on the Integration and Transformation of certain Differential Equations.* By CHARLES JAMES HARGREAVE, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I TAKE the liberty of drawing your attention to an error which appears to exist in the paper contained in your December Number on the Integration and Transformation of certain Differential Equations.

I confine my observations in the first place to the first of the equations discussed by the author; but it will be seen that the same or similar remarks are applicable to the other equations.

The solution of the equation

$$x \left(\frac{d^2 y}{dx^2} + k^2 y \right) + 2p \frac{dy}{dx} = 0 \quad . \quad . \quad . \quad (1.)$$

is given as being $y = (D^2 + k^2)^p u,$

where $(D^2 + k^2)u = 0,$

a result which is evidently identical with $y = 0,$ which is indeed a solution of (1.), but not the solution of which the author is in quest.

The error consists in inferring the equation

$$D^2 x u + (2p - 4)D u + k^2 x u = 0$$

from

$$(D^2 + k^2) (D^2 x u + (2p - 4) D u + k^2 x u) = 0.$$

The deduced equation ought to have been

$$D^2 x u + (2p - 4)D u + k^2 x u = (D^2 + k^2)^{-1} 0 = a \sin kx + b \cos kx,$$

which will lead to the result

$$y = (D^2 + k^2)^{p-1} \left\{ \frac{1}{x} (D^2 + k^2)^{-(p-1)} (a \sin kx + b \cos kx) \right\};$$

more conveniently written

$$y = (D^2 + k^2)^{p-1} \left\{ \frac{1}{x} (D^2 + k^2)^{-p} 0 \right\}. \quad \dots \quad (2.)$$

In solving differential equations by successive operations of this nature, a difficulty frequently occurs with reference to the introduction of constants. Thus every operation denoted by $(D^2 + k^2)^{-1}$ introduces two constants; and in many cases all the constants thus introduced except two, which are arbitrary, must be determined in terms of these two arbitrary constants by reference to the original differential equation.

This difficulty does not occur in the above equation (2.), which may be written in the simpler form,

$$y = (D^2 + k^2)^{p-1} \left\{ \frac{1}{x} (a \sin kx + b \cos kx) \right\}.$$

If $p = 1, y = \frac{1}{x} (a \sin kx + b \cos kx).$

If $p = 2, y = \frac{2}{x^2} (a \sin kx + b \cos kx) + \frac{2k}{x^2} (b \sin kx - a \cos kx),$

&c.

&c.

It will be found, in like manner, that the solution of the second of the equations given by Mr. Bronwin, viz.

$$\frac{d^2 y}{dx^2} + kx \frac{dy}{dx} - pk y = 0,$$

is $y = (D + kx)^p \{ D^{-1} (D + kx)^{-(p+1)} 0 \},$

subject to the difficulty above adverted to as to the determination of the constants introduced by the operations.

Similarly, the solution of the third example,

$$x \left(\frac{d^3 y}{dx^3} + k^3 y \right) + 3p \frac{d^2 y}{dx^2} = 0,$$

is
$$y = (D^3 + k^3)^{p-1} \left\{ \frac{1}{x} (D^3 + k^3)^{-p} \cdot 0 \right\},$$

which will be found to be

$$y = (D^3 + k^3)^{p-1} \left\{ \frac{1}{x} (a \varepsilon^{-kx} + b \varepsilon^{\alpha kx} + c \varepsilon^{\beta kx}) \right\},$$

α and β being the imaginary cube roots of -1 .

The solution of the fourth equation, viz.

$$x \frac{d^2 y}{dx^2} + (2p + 2mx) \left(\frac{dy}{dx} + my \right) = 0,$$

will be found to be

$$y = (D^2 + 2mD + 2m^2)^{p-1} \left\{ \frac{1}{x} (a \varepsilon^{\alpha x} + b \varepsilon^{\beta x}) \right\},$$

where α and β are the roots of $t^2 + 2mt + 2m^2 = 0$.

A similar correction may be applied without difficulty to the other equations discussed by the author of the paper in question.

I am, Gentlemen,

Your obedient Servant,

69 Chancery Lane, Dec. 3, 1846. CHARLES JAMES HARGREAVE.

V. *On the Compounds of Phosphoric Acid with Aniline.*

By EDWARD CHAMBERS NICHOLSON, Esq.*

OF the inorganic acids there is none so remarkable for the number and variety of the compounds it forms with bases as phosphoric acid. The peculiar and complex reactions of the salts of this acid were first satisfactorily explained by Prof. Graham, in his well-known paper published in 1833†. The

* Communicated by the Chemical Society; having been read May 18, 1846.

† Phil. Trans. of Royal Society of London, part 2. 1833. Researches on the Arseniates, Phosphates and Modifications of Phosphoric Acid, by Thos. Graham, Esq., M.A., F.R.S.E., Professor of Chemistry in the Andersonian University of Glasgow.

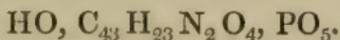
modifications of phosphoric acid by its amount of water of hydration, giving rise to pyrophosphoric and metaphosphoric acid, beautifully explained all that was enigmatical in the observed reactions.

Prof. Graham's excellent experiments have added a great deal to our knowledge of phosphoric acid; the study, however, of this acid and its salts is by no means exhausted. I consequently hope that any contribution to the subject will be accepted.

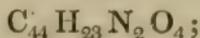
The following experiments, which were instituted in the laboratory of the Royal College of Chemistry, relate to the combination of phosphoric acid with a large and interesting group of bodies, viz. the organic alkaloids. It is curious that this class of salts has not been hitherto examined; indeed the composition of scarcely any phosphate of an organic base has been satisfactorily ascertained.

It was a very interesting question, whether these bases, which with monobasic acids comport themselves exactly the same as mineral oxides, were capable of combining with polybasic acids in the same remarkable proportions. In the absence of experiments, the only points which were known from whence we might, by way of analogy, have inferred any similarity in these compounds with the inorganic phosphates, are the salts of ammonia with phosphoric acid; ammonia being as it were the type of all the organic bases. A phosphate of ammonia indeed is known, containing two equivalents of the volatile alkali; but it still remained to be decided whether any true organic alkaloid unites in the proportions of two or more equivalents with one equivalent of phosphoric acid.

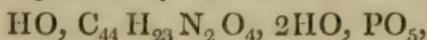
The only analysis of a phosphate with an organic alkali is published by Regnault*. This chemist determined the carbon and hydrogen in phosphate of strychnine. Regnault considered the salt which he analysed as a compound of 1 equiv. of phosphoric acid, 1 equiv. of strychnine and water,



However, Regnault's formula of strychnine has been corrected by Professor Liebig†, who showed that this base contains one equivalent of carbon more, *i. e.*



it is therefore evident that the composition of Regnault's phosphate is to be expressed by the formula



* Liebig's *Annalen*, vol. xxvi. p. 37.

† *Ibid.* vol. xxvi. p. 58.

as Professor Liebig* already remarks, and, as may be seen from the following comparison of the per-centages corresponding to this formula with his analytical results, which I have calculated with the new atomic weights of carbon:—

44 eqs. Carbon	3300·00	59·23	59·85
26 ... Hydrogen	325·00	5·83	5·85
2 ... Nitrogen	354·00	6·55	
7 ... Oxygen	700·00	12·66	
1 ... Phosphoric acid	892·28	15·73	
1 ... Phosphate of strychnine	<u>5571·28</u>	<u>100·00</u>	

It is evident from these numbers that the phosphate of strychnine analysed by Regnault is a common phosphate, corresponding to the phosphate of soda with 1 equiv. of fixed base.

Besides this, Dr. A. T. Thomson† relates an analysis of phosphate of morphine by Dr. Gilbert, from which however no conclusion relative to the composition of this salt can be deduced, the analysis being incomplete.

It being a matter of indifference whether the base which I took for experiment was a natural alkaloid or one prepared artificially in the laboratory, I selected aniline, deeming it especially suited to my purpose, as this base generally forms with acids readily crystallizable compounds.

I. COMPOUNDS OF TRIBASIC PHOSPHORIC ACID WITH ANILINE.

1. Phosphate with 2 equivs. of Aniline and 1 equiv. of Water.

On adding aniline in excess to a strong solution of tribasic phosphoric acid, the whole instantly solidified to a white crystalline mass, which was drained, pressed, dissolved in a large quantity of boiling dilute alcohol, and filtered through a warm funnel; on cooling, the whole became a mass of crystals; after pressing these between folds of bibulous paper, I finally dried them on a warm porous tile.

When dry, this salt appears as beautiful flesh-coloured nacreous plates, which might be readily taken for thionurate of ammonia, quite inodorous, slightly acid to test paper. They are very soluble in ether and water, sparingly soluble in cold alcohol, but very soluble in hot, so that upon cooling the whole

* *Handwörterbuch der Chemie*, article 'Organic Bases,' vol. i. p. 70.

† *Pharm. Journal*, vol. ii. p. 500, 'On the Preparation, Characters, and Composition of the Nitrate, Phosphate, Tartrates and Hydriodate of Morphia,' by Anthony Todd Thomson, M.D., F.L.S.

liquid becomes solid. This salt cannot be dried at 100° C. (212° F.), because at this temperature it gives off aniline, assuming a red colour. It melts at a gentle heat, and upon raising the temperature gives off its aniline and leaves metaphosphoric acid.

Analysis.

When burnt with oxide of copper—

I. 0·419 grm. of the substance gave 0·774 grm. of carbonic acid and 0·227 grm. of water.

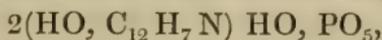
II. 0·646 grm. of the substance gave 1·183 grm. of carbonic acid and 0·356 grm. of water.

III. 1 grm. neutralized with ammonia and precipitated with nitrate of silver, gave 1·461 grm. of tribasic phosphate of silver.

These numbers correspond with the following per-cents. :—

	I.	II.	III.
Carbon	50·38	49·94	
Hydrogen	6·01	6·19	
Phosphoric acid	24·86

These determinations lead to the following formula,



as is seen by the following :—

24 eqs.	Carbon	1800·00	50·57
17 ...	Hydrogen	212·50	5·97
2 ...	Nitrogen	354·10	9·97
3 ...	Oxygen	300·00	8·42
1 ...	Phosphoric acid	892·28	25·07
1 ...	Phosphate of aniline	3558·88	100·00

Therefore the salt corresponds to the common phosphate of soda,



and also to an ammonia salt of the same constitution.

It is anhydrous, like this ammonia salt; but, unlike these salts, possesses, as before remarked, an acid reaction, like all the other salts of aniline, this base being incapable of destroying the properties of an acid.

2. Phosphate with 1 equiv. of Aniline and 2 equivs. of Water.

This compound is obtained by adding tribasic phosphoric acid to the salt mentioned before until it ceases to produce a precipitate with chloride of barium, and concentrating in the water-bath. In the course of a few hours the salt crystallizes in beautiful silky needles; these are to be washed with æther and dried on a warm tile.

When dry the salt is quite white, but by exposure to the

air becomes rose-red; it is very soluble in æther, alcohol, and water; in the latter with decomposition, the common phosphate being produced.

Analysis.

When burnt with oxide of copper—

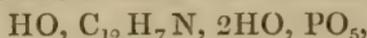
I. 0.456 grm. of the substance gave 0.633 grm. of carbonic acid and 0.223 grm. of water.

II. 0.403 grm. neutralized with ammonia and precipitated with nitrate of silver, gave 0.880 grm. of tribasic phosphate of silver.

These numbers correspond to the following per-cents. :—

	I.	II.
Carbon	37.86	
Hydrogen	5.44	
Phosphoric acid	37.12

These determinations lead to the formula



as is seen by the following:—

12 eqs.	Carbon	900.00	37.59	37.86
10 ...	Hydrogen	125.00	5.22	5.44
1 ...	Nitrogen	177.05	7.39	
3 ...	Oxygen	300.00	12.54	
1 ...	Phosphoric acid	892.28	37.26	37.12
1 ...	Phosphate of aniline	2394.33	100.00	

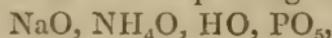
This salt corresponds to the biphosphate of soda,



Mitscherlich has analysed an ammonia compound of the same constitution.

I did not succeed in forming a phosphate in which the three equivalents of water in the hydrate are replaced by three equivalents of aniline. On adding aniline in excess to an alcoholic solution of the common phosphate, the base did not combine, and the common phosphate remained. I also tried to produce it by decomposing the phosphate with three equivalents of soda by oxalate of aniline; aniline however was instantly set free in the form of oily globules, and the common phosphate was produced with oxalate of soda.

I have not been more successful in endeavouring to prepare phosphates, into which aniline enters beside fixed bases. I have tried to obtain a salt corresponding to phosphorus salt,



by dissolving aniline in a solution of phosphate of soda with one equivalent of fixed base. The crystals, however, which

were formed did not contain soda, and were similar in appearance to the salt first-described. Similar results were obtained by neutralizing the acid phosphate of aniline with carbonate of soda. The salt produced did likewise not contain soda, the common phosphate appearing to be produced.

II. PYROPHOSPHATE OF ANILINE.

There are two pyrophosphates of aniline, acid and neutral, which are always formed together when aniline is added in excess to pyrophosphoric acid. The precipitate produced by addition of aniline to pyrophosphoric acid is gelatinous, and becomes quite hard when exposed to the air.

Several combustions showed me that the compound obtained in this way consists of a mixture of the neutral and the acid pyrophosphate. I have not yet been able to obtain the neutral salt in a state of purity, but I found that by keeping the pyrophosphoric acid in great excess, the acid salt may be procured. The best way of forming it is to add aniline to rather concentrated pyrophosphoric acid (obtained by decomposing pyrophosphate of lead by hydrosulphuric acid), when the acid salt is precipitated along with the neutral compound; the mixture is then to be heated until the whole is dissolved, an excess of acid added, and the solution evaporated in the water-bath; upon cooling, the whole solidifies to a mass of beautiful white needles, which are to be pressed in bibulous paper, washed with æther, and finally dried *in vacuo*. This salt appears as white silky needles, very similar to the common basic sulphate of quinine; they are very acid, soluble in water, and almost insoluble in alcohol and æther; the crystals and their aqueous solution, like all other aniline salts, become red when exposed to the air.

Analysis.

When burnt with oxide of copper—

I. 0·223 grm. of the substance gave 0·333 grm. of carbonic acid and 0·117 grm. of water.

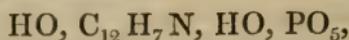
II. 0·269 grm. of the substance gave 0·389 grm. of carbonic acid and 0·132 grm. of water.

III. 0·350 grm. of substance, neutralized by ammonia and precipitated by nitrate of silver, gave 0·578 grm. of pyrophosphate of silver.

These numbers correspond to the following per-cents. :—

	I.	II.	III.
Carbon . . .	39·00	39·43	
Hydrogen . .	5·68	5·45	
Phosphoric acid	38·85

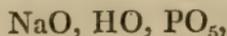
which lead to the formula



as is seen by the following:—

12 eqs. Carbon	900·00	39·44	39·22
9 ... Hydrogen	112·50	4·93	5·56
1 ... Nitrogen	177·01	7·75	
2 ... Oxygen	200·00	8·77	
1 ... Phosphoric acid	892·28	39·11	38·85
1 ... Pyrophosphate of aniline	2281·79	<u>100·00</u>	

This salt corresponds to the acid pyrophosphate of soda,



analysed by Professor Graham.

An analogous salt of ammonia is not known, pyrophosphate of ammonia existing only in solution, which, according to Graham's experiments, yields on evaporation crystals of the common phosphate of ammonia, with 2 equivalents of the volatile alkali and 1 equivalent of water.

III. METAPHOSPHATE OF ANILINE.

I obtained this salt by adding a great excess of aniline to a very strong solution of metaphosphoric acid (glacial acid perfectly pure), or by adding metaphosphoric acid to a solution of aniline in alcohol or æther; in both cases the salt precipitates as a gelatinous white mass; this is to be placed on a filter and well-washed with æther, until the excess of aniline is removed and no smell of this body perceived, pressed and dried over sulphuric acid *in vacuo*.

When dry the salt appears as an amorphous white mass, which changes rose-red in the air, becoming glutinous. It is acid to litmus paper, soluble in water, and almost insoluble in alcohol and æther.

Its solution dissolves metaphosphate of silver, which property belongs also to metaphosphate of soda; upon boiling, it changes red, and partly reduces the silver salt. When an aqueous solution is boiled for a long time it is transformed into common phosphate of aniline.

Analysis.

When burnt with oxide of copper—

I. 0·664 grm. of the substance gave 0·976 grm. of carbonic acid and 0·264 grm. of water.

II. 0·994 grm. of the substance, precipitated by acetate of

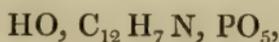
lead, and the precipitate treated with sulphuric acid, gave 2·617 grms. of sulphate of lead.

III. I also determined the acid by precipitating with acetate of lead in great excess and boiling for a long time; the metaphosphate of lead is completely changed into the tribasic salt. This is a very advantageous method of determining metaphosphoric acid. 0·5785 grm. of the substance gave 1·3485 grm. of tribasic phosphate of lead.

These numbers correspond to the following per cents. :—

	I.	II.	III.
Carbon	41·33		
Hydrogen	4·55		
Phosphoric acid	41·24	40·98

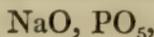
These analyses lead to the formula



as is seen by the following :—

12 eqs.	Carbon	900·00	41·48	41·33
8 ...	Hydrogen	100·00	4·62	4·55
1 ...	Nitrogen	177·01	8·11	
1 ...	Oxygen	100·00	4·62	
1 ...	Phosphoric acid	892·28	41·17	41·11
1 ...	Metaphosphate of aniline	2169·29	100·00	

This salt is analogous to Prof. Graham's metaphosphate of soda,



which, according to the experiments of this chemist, likewise becomes viscid on evaporation, and never presents the slightest trace of crystallization.

The corresponding metaphosphate of ammonia is only known in solution. On evaporation this salt assimilates 1 equivalent of water, passing into the common phosphate with 1 equivalent of ammonia and 2 equivalents of water.

The experiments which I have described prove that there exist the following series of phosphates of aniline :—

Phosphates	$\left\{ \begin{array}{l} 2(\text{HO, C}_{12}\text{H}_7\text{N}) \text{HO, PO}_5, \\ \text{HO, C}_{12}\text{H}_7\text{N, 2HO, PO}_5. \end{array} \right.$
Pyrophosphate	$\text{HO, C}_{12}\text{H}_7\text{N, HO, PO}_5.$
Metaphosphate	$\text{HO, C}_{12}\text{H}_7\text{N, PO}_5.$

This series embraces compounds corresponding to all the combinations of soda with phosphoric acid, except the phosphate with 3 equivalents of soda, and the pyrophosphate with 2 equivalents of fixed base.

The aniline salt, corresponding to the latter, however exists, but could as yet not be obtained in a state of purity.

All the compounds of aniline with phosphoric acid are anhydrous, like the other salts of this base which have been examined by Professor Hofmann. In this respect they differ from the corresponding soda salts, which nearly all contain a larger or smaller amount of water of crystallization. The two ammonia salts of phosphoric acid which we know, are however likewise anhydrous.

From the results of the preceding investigation we may conclude that the organic bases comport themselves also with polybasic acids like mineral oxides. I intend, however, to analyse the phosphates of some other organic alkaloids, particularly of those which occur in nature, and are frequently employed in medicine.

VI. *On the remarkable Barometric Depression of the 25th December 1821.* By W. R. BIRT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ON the 26th of November last the barometric depression, noticed at the close of my letter on the Great Symmetrical Barometric Wave, returned, making the sixteenth instance in eighteen years. The value of the depression at this station was 29.268 (unreduced): the observation was taken at 10 minutes past 8 in the morning. The value is about a tenth higher than the depression at Dublin on the 26th of November 1843. In that year the barometer commenced a decided rise on the 27th, which it also did at this station in the present year.

My friend E. W. Brayley, Jun., Esq., has suggested that this depression is somewhat allied to the great depression which occurred on the 25th of December 1821, and which has been made the subject of an investigation by Prof. Brandes of Breslau, and since by Prof. Dove of Berlin. The reader will find Prof. Brandes' paper in the *Annals of Philosophy*, N. S. vol. iv. p. 263, and Prof. Dove's in the *American Journal of Science and Art*, vol. xlv. p. 319. A paper on the same subject by Luke Howard is inserted in the *Philosophical Transactions*, vol. cxii. p. 113.

There can be no question that the depression of the latter end of November is periodical: the following interesting question consequently suggests itself. Is the depression of the 25th of December also periodical? To answer this question I selected all the readings given in Howard's *Climate of London* from

1806 to 1830 inclusive, on the 23rd, 24th, 25th, 26th and 27th of December. These readings, which consist of the maxima and minima, I have made the subject of a very careful discussion, the result of which forms the subject of this communication.

TABLE I.

Date.	Max.	Min.
1821.		
December 23.	29·17	28·75
24.	28·85	27·80
25.	28·45	27·80
26.	28·86	28·25
27.	28·97	28·50

This table gives the principal features of the depression of December 25, 1821, as it passed the neighbourhood of London. As we proceed with the investigation we shall find reason to isolate these observations.

TABLE II.—Barometric Maxima and Minima on Dec. 25.

Year.	Max.	Min.	Year.	Max.	Min.
1806	30·17	30·00	1819	29·50	29·40
1807	30·12	29·93	1820	29·81	29·70
1808	29·55	29·50	1821	28·45	27·80 ^b
1809	30·05	29·77	1822	30·47	30·40
1810	29·03	28·98 ^a	1823	29·88	29·70
1811	30·20	29·98	1824	29·80	29·44
1812	30·46	30·40	1825	30·05	29·77
1813	30·28	30·02	1826	30·55	30·42
1814	29·61	29·58	1827	30·56	30·48
1815	29·78	29·53	1828	29·73	29·50
1816	29·72	29·38	1829	30·53	30·26
1817	30·06	29·90	1830	29·61	29·58
1818	30·10	29·82			

From this table we learn that, with only two exceptions in twenty-five years, the barometer attained an elevation of at least 29·50 on the 25th of December in every year; the exceptions are the years 1810 and 1821 marked ^a and ^b.

TABLE III.

Date.	Max.	Min.
1810.		
December 23.	29·43	29·38
24.	29·46	29·26
25.	29·03	28·98
26.	29·77	29·51
27.	29·58	29·54

This table gives the principal features of the depression of 1810. It will immediately be seen from a comparison of the numbers of 1810 and 1821, that the depressions are essentially distinct.

TABLE IV.—Mean Barometric Maxima and Minima.

Day.	Max.	Min.	Range.
December 23.	29.91	29.72	.19
24.	29.99	29.80	.19
25.	30.00	29.84	.16
26.	29.96	29.78	.18
27.	29.94	29.75	.19

Rejecting on each day the readings of 1821 on the ground of the depression being so distinctly marked, and the readings being so considerably below any other of the series, and also rejecting on the 25th the readings for 1810 as evidently indicating a depression, and being nearly .5 below the lowest of the remaining readings, this table gives the means of the maxima and minima for the respective days; from which it appears that, on the average, the barometer passes a maximum on or about the 25th of December.

TABLE V.—Absolute Barometric Maxima and Minima.

Day.	Max.	Year.	Min.	Year.	Range.
Dec. 23.	30.40	{ 1818 } { 1826 }	28.65	1824	1.75
24.	30.48	1827	29.25	{ 1815 } { 1819 }	1.23
25.	30.56	1827	28.98	1810	1.58
26.	30.64	1826	29.00	1815	1.64
27.	30.80	1827	28.94	1814	1.86

From this table we learn that the years 1826 and 1827 were characterized by considerable elevations of the barometer from the 23rd to the 27th of December; and that in the years 1810, 1814, 1815, 1819 and 1824, the barometer sank to 29 inches or below, on the 23rd, 25th, 26th or 27th. In no instance except 1821 did the barometer fall below 29.25 on the 24th.

TABLE VI.—Showing the highest and lowest readings of the *Maxima* for the respective days and years in which they occurred.

Day.	Highest reading.	Year.	Lowest reading.	Year.	Range.
Dec. 23.	30·40	{ 1818 1826 }	29·35	1819	1·05
24.	30·48	1827	29·40	1819	1·08
25.	30·56	1827	29·50	1819	1·06
26.	30·64	1826	29·42	1816	1·22
27.	30·80	1827	29·27	1811	1·53

In this table the *highest* readings of the maxima show the absolute altitude which the mercurial column attained on the respective days in the years named in the third column.

The *lowest* readings of the maxima show that the barometer attained a greater elevation than 29·25 on each day, from the 23rd to the 27th of December inclusive, in each year from 1806 to 1830 also inclusive, 1821 and 1810 only excepted; the exception of the year 1810 extends only to the 25th.

The reply which this examination furnishes to the question proposed is this—that the depressions of 1810 and 1821 were *accidental*, that is, they did not form portions of a periodical series of depressions, such as the depression of the latter end of November (see table on p. 359 in the last volume). Mr. Brayley however suggests that they may be instances of a *longer* series. May they not result from annual movements, which from some cause or other are *displaced*? The usual epoch of the crest of the great wave is about the middle of November; on some occasions it occurs as early as the latter end of October, and on others as late as the latter end of November, having a range of about a month.

This result conducts us to another of a very interesting character: we have just noticed that the great symmetrical wave is to a certain extent *erratic* as to the period of its transit. The depression of the latter end of November is to a certain extent *constant*, the period of its transit being confined to about four days: this has been determined from a consideration of the barometric curves of the last four days of November. Mr. Howard's observations do not furnish sufficient data for projecting the curves; still the readings lead to the conclusion, that some time during the five days, from the 23rd to the 27th of December inclusive, the barometer passes a maximum. We have an elevation of the mercurial column about this time.

From this it appears highly probable that we have two

points approximately fixed in our *annual barometric curve*, the *depression* on or about the 28th of November, and the *elevation* on or about the 25th of December. It also appears that we have two kinds of atmospheric movements that affect the barometer; those of an *erratic* character as to their period—the great symmetrical wave being an instance—and those of a more *constant* character, as the depression and elevation just noticed.

We now pass on to the evidence which the Greenwich observations afford as to the elevation of the 25th of December.

TABLE VII.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1840.			
Dec. 23.	Noon.	30·060	
24.	Noon.	30·006	
25.	Noon.	30·245	} Max. 30·348 midnight. } Min. 30·135 2 A.M.
26.	Noon.	30·491	
26.	10 P.M.	30·558	Max. of the month.
28.	2 A.M.	30·402	
28.	Noon.	30·304	

From this table we find that the barometric readings on the 25th are strictly in accordance with the foregoing deductions, being considerably above 29·500; and we further learn that the maximum of this elevation occurred on the 26th at 10 at night; the readings of the 25th were therefore those of the *anterior* slope of the elevation. The maximum of the 26th was also the maximum of the month.

TABLE VIII.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1841.			
Dec. 23.	Noon.	29·741	
24.	10 A.M.	29·940	Maximum.
24.	Noon.	29·924	
25.	Noon.	29·604	
25.	10 P.M.	29·539	Minimum.
26.	Noon.	29·726	
27.	2 P.M.	30·042	Maximum.
28.	Noon.	29·999	

The readings of the 25th are still in accordance with the results already arrived at. The trough between the two maxima occurred at 10 P.M. of the 25th, so that the readings of the 25th were those of the posterior slope of the first elevation.

TABLE IX.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1842.			
Dec. 23.	Noon.	29.496	
24.	Noon.	29.586	
25.	10 A.M.	29.732	Maximum.
26.	12 P.M.	29.299	
27.	6 A.M.	29.149	Minimum
27.	Noon.	29.288	
28.	Noon.	29.957	

In this year the barometer passed a maximum on the 25th at 10 A.M., and this reading was the highest of the five days.

TABLE X.—Barometric Readings, Royal Observatory, Greenwich.

Day.	Hour.	Barometer corrected.	Remarks.
1843.			
Dec. 23.	Noon.	30.313	
24.	2 P.M.	30.374	
25.	10 A.M.	30.392	Maximum.
26.	Noon.	30.283	
27.	Noon.	30.369	
28.	Noon.	30.436	

The barometer again passed a maximum on the 25th at 10 A.M., and, as in 1842, this reading was the highest of the five days.

The observations in the four years fully confirm the results deduced from the observations as given in Howard's Climate of London.

By the kindness of Captain Childers, who has placed in my hands the hourly observations which he communicated to the British Association at its last meeting, I am enabled to ascertain the state of the barometer at St. Helier's, Jersey, for the above-named days in the years 1844 and 1845: it is strictly in accordance with all that has preceded.

TABLE XI.—Barometric Readings, St. Helier's, Jersey.

Day.	Hour.	Barometer uncorrected.	Remarks.
Dec. 23.	Noon.	30·185	Maximum.
23.	8 P.M.	30·260	
24.	Noon.	30·235	
25.	11 A.M.	30·120	
26.	Noon.	30·025	
27.	Noon.	29·980	
28.	Noon.	29·910	

The reading of the 25th occurred on the posterior slope of the elevation.

Captain Larcom's observations at Dublin, with Captain Childers' at Jersey for last year, will conclude the series. Captain Larcom's observations give a most decided and well-marked maximum on the 25th at 3 P.M.; the readings extend only to 9 A.M. of the 26th; but from the curve with which the Captain has most kindly furnished me, the reading on the 25th at 3 P.M. is the highest for the five days.

TABLE XII.—Barometric Readings, Ordnance Survey Office, Dublin.

Day.	Hour.	Barometer corrected.	Remarks.
Dec. 23.	Noon.	29·622	Maximum.
24.	Noon.	30·074	
25.	Noon.	30·134	
25.	3 P.M.	30·148	
25.	6 P.M.	30·124	
25.	9 P.M.	30·070	
26.	9 A.M.	29·631	

TABLE XIII.—Barometric Readings, St. Helier's, Jersey.

Day.	Hour.	Barometer uncorrected.	Readings.
Dec. 23.	Noon.	29·460	Maximum.
24.	Noon.	30·125	
25.	9 A.M.	30·260	
25.	3 P.M.	29·280	
25.	8 P.M.	30·300	
26.	Noon.	30·230	

Upon the whole, I apprehend we may fairly conclude, from the observations of thirty-one years, that on or about the 25th of December the barometer passes a maximum, and that this maximum is above 29.500. This conclusion is strengthened by the fact, that only two exceptions have been observed: a low barometer on the 25th will consequently demand close attention, and observations under such circumstances cannot fail of possessing great interest.

Should any of your readers be aware that between 1830 and 1840 depressions occurred on the 25th of December, a notice of them would be valuable.

I have the honour to be, Gentlemen,

Your very obedient Servant,

2 Sidney Place, Cambridge Road, Bethnal Green.
London, Dec. 5, 1846.

W. R. BIRT.

VII. *Relics of the Mechanical Productions of Abraham Sharp, the assistant of Flamsteed.* By the Rev. N. S. HEINEKEN.

GENTLEMEN,

Sidmouth, November 23, 1846.

AMONG my papers I have chanced to find one which I had intended *some years* since to forward for insertion in the *Philosophical Magazine*, if upon perusal you deemed it likely to interest any of your readers: again accidentally thrown in my way, I transmit it to you, hoping that, notwithstanding the interval which has passed, the statements therein made may still be found to be correct.

I am, Gentlemen,

Respectfully yours,

N. S. HEINEKEN.

It may perhaps be interesting to some of the readers of the *Philosophical Magazine*, to be informed that several of the mechanical productions of Abraham Sharp, the once celebrated assistant of Flamsteed, are still in existence and in good preservation. My attention was directed to this subject in consequence of observing his name mentioned in the review of the Rev. W. Whewell's *Flamsteed and Newton*. Feeling interested in everything which related to one so eminent in his day, I endeavoured, while on a visit at Bradford (Yorkshire), to obtain whatever information I could respecting the papers and "reliques" of the once "indefatigable calculator," and sought to procure some memento of him who is described by his biographer as having "had a clear head for contriving and a skilful hand for executing" any of the mechanical or philosophical apparatus which he required. The result of my inquiries I subjoin.

The lathe, mentioned in the *Encyclopædia Britannica* (article

‘Sharp’) for turning, rose-work, eccentric, swash-work, geometrical solids, &c., was sold some few years since at Bradford to Godfrey Wright, Esq., in the neighbourhood of Doncaster. It had for many years been used by its former possessor (W. Goodchild), and when I saw it was in excellent working condition; and many are the elaborate and beautiful specimens which I have seen of its efficiency, even at a period of upwards of a century since it was first constructed. Another smaller lathe, of a somewhat similar description, for turning ovals, and ovals combined with rose-work, which exhibits some ingenious contrivances of the same “master mind,” was purchased by a person whose name is “Murgy,” residing at Heaton near Bradford. The double sector mentioned in his biography and sculptured on his monument, is, I understand, in the possession of J. Bottomley, Esq. of Bradford, or his relative Miss Bamforth, of Manor Row, Bradford. The large (18-inch) elaborate ring dial, and also the set of calculating-rods in a walking-stick, were purchased by Mr. Muff, music-seller, Leeds. A beautifully divided plate for calculating, I was informed, was in the possession of — Skelton, Esq. of Bradford. A large equatorial (with the hour circle about thirty inches diameter) in complete preservation, was purchased at the sale at Bradford, and is now at the observatory of the York Philosophical Institution. A very ingenious and complete perambulator, measuring from *part of an inch* to 100 miles, together with some geometrical solids, turned in the first-mentioned lathe, now belong to my friend J. Waterhouse, Esq., of Well Head near Halifax. Of this perambulator I may observe, that it has only one *toothed wheel* in the whole of the machinery: all the intermediate measures are given from *the inch* to the *last index*, which moves *once in 100 miles*; and yet the disc of the dial is not more than about six inches in diameter and not a quarter of an inch in thickness. Of this perambulator I have made a model, which (with the consent of my friend) I will submit to you should you desire it. A mural quadrant of $5\frac{1}{2}$ feet radius, diagonally divided, &c., I have in my own possession, and shall feel pleasure in showing it to any who may feel an interest about it. A spirit thermometer, the *scale* of which I am certain was the work of Abraham Sharp, from the punches used for the figures, was fixed in the wainscot of the hall at Horton (his residence). If the *tube* is the *original* one, this might be valuable for reference in a meteorological point of view; for I remember having seen many meteorological journals, which had been kept by Abraham Sharp, and which were among the papers left at Horton: these I believe are now in the

possession of — Giles, Esq., the representative of the family. Two planispheres were also among these papers, drawn by pen and ink; but so exquisitely, that until I examined them with a magnifying lens, I could not but believe them to be engravings. A vice which was made to turn on its axis, and which had the contrivance of a second pair of chaps near the screw, for pressing, I imagine, horn into moulds for *buttons*, was used by the before-named Mr. Goodchild as his working vice, and I dare say is still retained by some part of the family; and as I have mentioned the contrivance for horn-pressing, I may state, that even one set of these *identical buttons* is still in existence!! They are ornamented with every variety of rose-work patterns, and were probably intended by the philosopher for his holiday suit!! They are now, together with some lenses, regarded by their owner, Mr. Fearnley of Shipley, as precious relics, though I believe within a few years they have decorated modern broadcloth. The steel punches for figuring his instruments were constantly used by Mr. Goodchild for the same purpose; as were also several chisels, which, singularly enough, were made of *remarkably soft iron, case-hardened* at the cutting part. I remember also to have seen a “shutter micrometer” of brass at the Hall. This, I suppose, not having been sold, is retained by — Giles, Esq. It is much to be regretted that the ingenious and laborious productions of such men should be thus scattered abroad, and in too many instances allowed to decay or be wantonly destroyed. Two lamentable examples of this are, I learn from a paper in the Magazine of Science, exhibited even at the Royal Society, where the reflectors of Newton and Hadley are in a *state of complete dilapidation*. In the case of Abraham Sharp, it was by chance only that I was fortunate enough to save the large mural quadrant from the hands of an ignorant brazier, who was anxious to purchase it for the purpose of—mending kettles!! I was given to understand with respect to his MSS., that *many years* since, when they had been neglected by the owner of the house and left in a closet, the cook was in the habit of supplying herself from the ample store for the purpose of lighting fires and singeing fowls! what remained have since, I believe, been carefully preserved by — Giles, Esq., but they are now the mere *membra disjecta*—the Sibyl’s scattered leaves.

I am, Gentlemen,

Respectfully yours,

N. S. HEINEKEN.

P.S. One of the philosopher’s walking-canes, having its crutch-head formed into an opera-glass, I also saw at the Hall, and I hope is still preserved.

VIII. *On some Formulæ which serve to indicate the limits of the application of Indeterminate Methods to the solution of certain Problems.* By JAMES COCKLE, Esq., M.A., of Trinity College, Cambridge; Barrister-at-Law, of the Middle Temple. *Second and concluding Part**.

[The first part will be found at pp. 181–183 of the preceding volume†.]

LET x_1, x_2, \dots, x_n denote the roots of

$$x^n + x'x^{n-1} + x''x^{n-2} + \dots + x^{(n)} = 0,$$

the general equation of the n th degree, and, by a similar notation, let y_1, y_2, \dots, y_n be the roots, and $y', y'', \dots, y^{(n)}$ the coefficients of the equation in y , when

$$y = \xi^{(0)} + \xi'x + \xi''x^2 + \dots + \xi^{xxi} x^{22},$$

it is required to point out how to determine $\xi^{(0)}, \xi', \dots$, so that we may have

$$y' = 0, \quad y'' = 0, \quad y''' = 0, \quad \text{and} \quad y^{iiii} = 0.$$

By means of $y' = 0$, eliminate $\xi^{(0)}$ from the three succeeding equations, and, conformably with the notation adopted in one of my previous communications to this work‡, represent the respective results by

$${}_2Y'_n = 0, \quad {}_3Y'_n = 0, \quad \text{and} \quad {}_4Y'_n = 0.$$

Next, by processes which I have already referred to in the present periodical§, let ${}_2Y'_n = 0$ be put under the form

$$h_1^2 + h_2^2 + \dots + h_{22}^2 = 0,$$

where $h_1 = \alpha'_1 \xi' + \alpha''_1 \xi'' + \dots + \alpha^{xxii}_1 \xi^{xxii}$,

and, in general,

$$h_r = \alpha_r^{(r)} \xi^{(r)} + \alpha_r^{(r+1)} \xi^{(r+1)} + \dots + \alpha_r^{xxii} \xi^{xxii}.$$

We have twenty-two quantities (ξ) at our disposal,—make

$$h_1^2 + h_2^2 = 0, \quad \text{(A.)}, \quad h_3^2 + h_4^2 = 0, \quad \text{(B.)},$$

and $h_5^2 + h_6^2 = 0, \dots, h_{21}^2 + h_{22}^2 = 0;$

with the aid of the last nine of these equations (previously put under the form

$$h_{r-1} \pm \sqrt{-1} h_r = 0),$$

let nine of the ξ 's be eliminated from the first two; after the elimination there will remain 22–9 or 13 of the quantities $\xi', \xi'', \dots, \xi^{xxii}$ still undetermined.

Now it is known|| that, whatever be the number of the quantities ξ , the above expression for y is equivalent to

$$y = \Xi^{(0)} + \Xi'x + \Xi''x^2 + \dots + \Xi^{(n-1)}x^{n-1},$$

* Communicated by Sir George Cayley, Bart.

† Phil. Mag. S. 3. vol. xxix.

‡ Ibid. vol. xxviii. p. 191.

§ Ibid. vol. xxvii. pp. 126, 292, 293.

|| Sixth Report of the British Association, p. 301 *et seq.*

where

$$\Xi^{(r)} = s_r^{(0)} \xi^{(0)} + s_r^{(1)} \xi' + s_r^{(2)} \xi'' + \dots + s_r^{(22)} \xi^{xxii};$$

so that the number of disposable and independent quantities (Ξ) contained in the expression for y can in no case exceed n ; neither, after the elimination of the nine ξ 's, can it exceed 13, as is seen on referring to the last of the above equations. The elimination of those quantities does not, however, diminish the number of disposable quantities (Ξ) except when n is greater than 13.

If then

$$\begin{aligned} h_1 &= K_1' \Xi' + K_1'' \Xi'' + K_1''' \Xi''' + K_1^{iv} \Xi^{iv} + B_1 \\ h_2 &= K_2'' \Xi'' + K_2''' \Xi''' + K_2^{iv} \Xi^{iv} + B_2 \\ h_3 &= K_3''' \Xi''' + K_3^{iv} \Xi^{iv} + B_3 \\ h_4 &= K_4^{iv} \Xi^{iv} + B_4, \end{aligned}$$

where B_1, \dots, B_4 are functions of the $n - 5$ quantities $\Xi^v, \Xi^{vi}, \dots, \Xi^{(n-1)}$, we see, by what precedes, that nine of the quantities ξ may be so determined as to enable us to decompose ${}_2Y_n = 0$ into

$$h_1^2 + h_2^2 = 0, \text{ (A.) and } h_3^2 + h_4^2 = 0, \text{ (B.)}$$

where h_1, \dots, h_4 have the forms last above given, and the $n - 5$ quantities $\Xi^v, \Xi^{vi}, \dots, \Xi^{(n-1)}$ are undetermined, and perfectly at our disposal; at least when n is not greater than 13, and when n exceeds 13, we have eight of them undetermined and disposable. But it will be seen below that, for our present purpose, this last case does not require consideration. $\Xi', \Xi'', \dots, \Xi^{iv}$, have as yet no other conditions than (A.) and (B.) to satisfy.

Depress (A.) and (B.) to linear equations, and eliminate Ξ''', Ξ^{iv} , from ${}_3Y'_n$ by their means. Then, on referring to my paper at pages 190-191 of the last volume but one of this work*, it will be seen that, without determining Ξ', Ξ'' , it will be possible to reduce the resulting equation to the form

$$(K'_1 \Xi' + K''_1 \Xi'' + B_1)^3 + (K''_2 \Xi'' + B_2)^3 = 0;$$

or, $h_1^3 + h_2^3 = 0; \dots \dots \dots$ (C.)

and also that B_1 and B_2 will not give the illusory results which (under a different notation) I have before† pointed out, provided the number of disposable quantities $\Xi^v, \dots, \Xi^{(n-1)}$ be more than three in number; this gives the condition

$$n - 5 > 3, \text{ or } n > 8 \dots \dots \dots (y.) \ddagger$$

With the aid of (C.) reduced to a linear form, eliminate Ξ' or Ξ'' from ${}_4Y'_n = 0$, and solve the resulting equation.

* Phil. Mag. S. 3. vol. xxviii.

† Ibid. pp. 190, 191, 395.

‡ This corresponds to the equation (y.) of p. 191 of Phil. Mag. S. 3. vol. xxviii.

In effect we solve (A.), (B.), (C.) and (D.), by means of $\Xi^I, \Xi^{II}, \Xi^{III}, \Xi^{IV}$; the remaining Ξ 's (excepting Ξ^0) are determined in effecting the reduction of ${}_3Y'_n$ to the form of (C.); and Ξ^0 will be obtained from $y'=0$, after substituting in it the values of the other Ξ 's.

The above investigations give the formula

$$n(1, 1, 1, 1) \text{ (or, } n(1^4))=9; \dots (317^{II}).$$

and shows that the *general equations of the NINTH and higher degrees may be transformed into others of the same degrees, from which the second, third, fourth and fifth terms disappear*; the corresponding formula for m (see the first part of this discussion*) is

$$m(1, 1, 1, 1) = 11 \text{ (or } 10). \dots (317.)$$

2 Church Yard Court, Temple,
November 28, 1846.

Postscript, Dec. 14, 1846.—On looking over the proof-slip, I observe that, in this paper, I have not had occasion to use the foregoing notation for the *roots* of equations. But the above may be considered to suggest the following permanent notation; viz. that x_r should represent a root, and $a^{(r)}$ the coefficient of the $(r+1)$ th term of an equation in X ; ${}_rX$ a quantity composed of symmetric functions of, and homogeneous and of the r th degree with respect to, x_1, x_2, \dots, x_n ; that ξ should denote the disposable quantities which enter (explicitly) into the equation for y and Ξ , the disposable quantities implicitly contained in that equation. When r is given as a number, we may, however, as above, express the coefficients by accents if r be small, or by Roman numerals if it be large. For facility of reference I have termed 'last' volume what was in fact the current one at the time of writing this paper.

IX. On the Solvent Action of Drainage-Water on Soils.

By JOHN WILSON, Esq.†

IN the autumn of 1844, being a resident in East Lothian, where the system of *thorough draining* is very extensively carried out, it occurred to me that the drainage-water during its percolation of the soil must necessarily dissolve out and carry away a great portion of the soluble constituents of it, which, by the practice as at present followed, are carried off the land and entirely lost to the farmer. I therefore took advantage of the first fall of rain sufficient to set the drains

* Phil. Mag. S. 3. vol. xxix. pp. 181-183.

† Communicated by the Chemical Society; having been read May 4, 1846.

running after the dry weather of the autumn, and collected some of the drainage-water, which I subjected to a partial analysis, the particulars of which were described in a paper read by Dr. W. Gregory at a meeting of the Royal Society, Edinburgh, in the early part of last year. The results I then obtained, though very incomplete, were quite sufficient to show me that they had a very important bearing on agriculture, and to induce me to go on with their further investigation.

About the usual quantity of rain had fallen during the time between November, when I collected the first sample, and April 29th, when I obtained the second, and during the whole of that period the land had laid ploughed as a winter fallow.

Immediately after the second sample was taken the field was prepared for seed and sown with guano and barley. In a few days after (May 16th) I was enabled to collect a third sample (of course from the same drains), and having submitted them to analysis, the following are the results:—

Second Sample.—18 lbs. of drainage-water on evaporation gave 15·2 grs. of solid residue, or about ·844 gr. to the pound.

Organic matter and water in combination	3·4
Silica	0·9
Silicate of alumina	0·4
Chloride of magnesium	1·12
Chloride of sodium	1·8
Chloride of calcium	3·0
Sulphate of alumina	0·85
Peroxide of iron	2·1
Phosphate of lime	0·3
	<hr/>
	13·87

Third Sample.—18 lbs. of drainage-water on evaporation gave 27·5 grs. of solid residue, or about 1·525 gr. to the pound.

Organic matter, &c.	7·8
Silica	0·7
Silicate of alumina	0·2
Peroxide of iron	2·25
Phosphate of magnesia	1·8
Magnesia ?	1·69
Chloride of sodium	2·615
Chloride of calcium	2·107
Carbonate of lime	2·7
Phosphate of lime	3·1
Phosphate of alumina	0·45
	<hr/>
	25·412

I should here observe that the first sample, collected in

November, after the drains had been dry for many weeks previous, contained 2·25 grs. of solid residue to the pound; whereas that collected in the following April (No. 2), after the land had been continually drenched by the winter's rains, only gave ·844 gr. to the pound.

On adding a quantity of easily soluble manure (guano) to the soil, the first waters (No. 3) that passed through not only brought with them an increased quantity (1·525 gr. to the pound), but they contained many of the very ingredients that constituted the value of the manure itself.

At the time the paper referred to was read at the Royal Society, Edinburgh, it was suggested that possibly the turbid portion first discharged from the drains after heavy rains might contain matter also very valuable to the soil; but on comparing the subjoined analysis with that of *the drained soil*, it appears to be composed of the same ingredients, with a decrease in the proportion of silica and an increase in the lime, both of which may be readily accounted for.

Analysis of Soil deposited from Turbid Drain-Water.

Silica	60·0
Silicate of alumina	17·5
Protoxide of iron	6·5
Sulphate of lime	9·4
Sulphate of magnesia	0·75
Phosphate of lime	0·6
Alumina	4·0
Water, &c.	1·25
	100·00

I do not for a moment wish to question the value of the principle of *thorough draining*; that is now I believe universally admitted; but if its results are deemed so beneficial to the farmer under the present practice, how much more so would they not become, if some remedy were devised either to prevent as much as possible this great abstraction, or else to render the enriched drainage-water again available to the soil!

This subject has not, I find, escaped the observant mind of Liebig, and in fact forms the basis of the 'Patent Manure,' according to his specification in October last. He has argued theoretically to the same end, and has proffered a remedy in the shape of a manure, by his patent process rendered much less soluble than before, which of course would not be acted upon so readily by the percolating rain-water, and would

consequently remain longer in the soil for the purposes of vegetation.

Mr. Smith of Deanston, a man to whom practical agriculture is deeply indebted, has suggested the application of all manures whatsoever in a largely diluted liquid state, and which I am informed has been most successfully applied on the western coast of Scotland.

Now both of these plans will most assuredly tend to lessen the loss at present sustained; the one by diminishing the solubility of the manures employed, and the other by rendering the drainage-water again available.

But should the farmer object to take advantage of either plan, it would appear expedient to avoid using as much as possible the very soluble manures, and instead of giving his land the usual good dose of manure that is expected to suffice for two or three seasons, to divide the quantity, and to apply it in as small a proportion and as frequently as the nature of his crops will permit him to do. In such case his crops will get more and his ditches less than by the present practice.

X. *Report of Proceedings in the Cambridge Observatory relative to the New Planet.* By Professor CHALLIS*.

St. Catharine's Hall Lodge, Dec. 12, 1846.

THE Syndicate appointed to visit the Observatory, conceiving the subject at the present time to possess peculiar interest, beg leave to submit to the senate the following statement of Prof. Challis, describing the course of observations, founded on the theoretical calculations of Mr. Adams of St. John's College, and made at the Observatory, with a view to the discovery of the new planet.

H. PHILPOT, <i>Vice-Chancellor.</i>	G. PEACOCK.
JOHN GRAHAM.	JAMES CARTMELL.
B. CHAPMAN.	CHAS. W. GOODWIN.
W. WHEWELL.	W. C. MATHISON.
JOSHUA KING.	G. G. STOKES.

At a meeting of the Observatory Syndicate, held at the Observatory on December 4, for the despatch of ordinary business, a strong desire having been expressed by the Vice-Chancellor and the members of the Syndicate generally, to receive from me a special report of observatory proceedings relating to the newly-discovered planet, drawn up in such a manner, and in such detail, as would enable them to lay complete information on the subject before the members of the

* Communicated by Professor Challis.

senate, I considered it to be my duty at once to comply with this request. A new body of the solar system has been discovered, by means depending on the furthest advances hitherto made in theoretical and practical astronomy, and confirming in a most remarkable manner the theory of universal gravitation. It is therefore on every account desirable that the members of the senate should be made fully acquainted with the part which has been taken by the Cambridge Observatory relatively to this important extension of astronomical science. The observations I shall have to speak of, and the reasons for undertaking them, are so closely connected with theoretical calculations performed by a member of this university to account for anomalies in the motion of the planet Uranus, that the history of the former necessarily involves that of the latter. I hope that for this reason, and because of the peculiar nature of the circumstances, I may be allowed to make a communication less formal and restricted in its character than a mere report of observatory proceedings.

The tables with which the observations of the planet Uranus have been uniformly compared were published by A. Bouvard in 1821. They are founded on a continued series of observations extending from 1781, the year of its discovery, to 1821. Previous to 1781, it had been accidentally observed seventeen times as a fixed star, the earliest observation of this kind being one by Flamsteed in 1690. Bouvard met with a difficulty in forming his tables. On an attempt to found them upon the ancient as well as the modern observations, it appeared that the theoretical did not agree with the observed course of the planet. He thought this might be attributed to the imperfection of the ancient observations, and consequently rejected all previous to 1781 in the formation of the tables finally published. These tables represent well enough the observations in the forty years from 1781 to 1821; but very soon after the latter year new errors began to show themselves, which have gone on increasing to the present time. It was now evident that the ancient observations had been rejected on insufficient grounds, and that from some unknown cause the theory was in fault. Were the tables calculated inaccurately? The difference between observation and theory (amounting in 1841 to $96''$ of geocentric longitude) was too great, and Bouvard's calculations were made with too much care to allow of this explanation. The effect of small terms neglected in the calculation of the perturbations caused by Jupiter and Saturn, could not be supposed to bear any considerable proportion to the observed amount of error. This state of the theory suggested to several astronomers the idea

of disturbances caused by an undiscovered planet more distant than Uranus. But there is no evidence of this hypothesis having been put to the test of calculation previous to 1843. The usual problem of perturbations is to find the disturbing action of one body on another by knowing the positions of both. Here an inverse problem, hitherto untried, was to be solved; viz. from known disturbances of a planet in known positions, to find the place of the disturbing body at a given time. Mr. Adams, Fellow of St. John's College, showed me a memorandum made in 1841, recording his intention of attempting to solve this problem as soon as he had taken his degree of B.A. Accordingly, after graduating in January 1843, he obtained an approximate solution by supposing the disturbing body to move in a circle at twice the distance of Uranus from the sun. The result so far satisfied the observed anomalies in the motion of Uranus as to induce him to enter upon an exact solution. For this purpose he required reduced observations made in the years 1818-26, and requested my intervention to obtain them from Greenwich. The Astronomer Royal, on my application, immediately supplied (Feb. 15, 1844) all the heliocentric errors of Uranus in longitude and latitude, from 1754 to 1830, completely reduced. Mr. Adams was now furnished with ample data from observation, and his next care was to ascertain whether Bouvard's theoretical calculations were correct enough for his purpose. He tested the accuracy of the principal terms of the perturbations caused by Jupiter and Saturn, and concluded that the small terms which Bouvard had not taken into account would not sensibly affect the final results, the chief of them being either of long period, or of a period nearly equal to that of Uranus. Besides which he introduced into the theory several corrections which had been derived from observation and calculation by different astronomers since 1821. The calculations were completed in 1845. In September of that year, Mr. Adams placed in my hands a paper containing numerical values of the mean longitude at a given epoch, longitude of perihelion, eccentricity of orbit, mass, and geocentric longitude, Sept. 30, of the supposed disturbing planet, which he calls by anticipation "the new planet," evidently showing the conviction in his own mind of the reality of its existence. Towards the end of the next month, a communication of results slightly different was made to the Astronomer Royal, with the addition of what was far more important, viz. a list of the residual errors of the mean longitude of Uranus, for a period extending from 1690 to 1810, after taking account of the disturbing effect of the supposed planet. This comparison of observation

with the theory implied the determination of *all* the unknown quantities of the problem, both the corrections of the elements of Uranus, and the elements of the disturbing body. The smallness of the residual errors proved that the new theory was adequate to the explanation of the observed anomalies in the motion of Uranus; and that as the error of longitude was corrected for a period of at least 130 years, the error of radius vector was also corrected. As the calculations rested on an assumption made according to Bode's law, that the mean distance of the disturbing planet was double that of Uranus, without the above-mentioned numerical verification, no proof was given that the problem was solved, or that the elements of the supposed planet were not mere speculative results. The earliest evidence of the complete solution of an inverse problem of perturbations is to be dated from October 1845.

Although the comparison of the theory with observation proved synthetically that the assumed mean distance was not very far from the truth, it was yet desirable to try the effect of an alteration of the mean distance. Mr. Adams accordingly went through the same calculations as before, assuming a mean distance something less than the double of that of Uranus, and obtained results which indicated a better accordance of the theory with observation; and led him to the conclusion, which has since been confirmed by observation, that the mean distance should be still further diminished. This second solution, taken in conjunction with the first, may be considered to relieve the question of every kind of assumption. The new elements of the disturbing body, and the results of comparing the observed with the theoretical mean longitudes of Uranus, were communicated to the Astronomer Royal at the beginning of September 1846. These were accompanied by numerical values of errors of the radius vector, the Astronomer Royal having inquired after the reception of the first solution, whether the error of radius vector, known to exist from observation, was explained by this theory. It would be wrong to infer that Mr. Adams was not prepared to answer this question till he had gone through the second solution. Errors of radius vector were as readily deducible from the first solution as from the other.

The preceding details are intended to point out the circumstances which led astronomers to suspect the existence of an additional body of the solar system, and the theoretical reasons there were for undertaking to search for it. No one could have anticipated that the place of the unknown body was indicated with any degree of exactness by a theory of this kind. It might reasonably be supposed, without at all mistrusting

the evidence which the theory gave of the *existence* of the planet, that its position was determined but roughly, and that a search for it must necessarily be long and laborious. This was the view I took, and consequently I had no thought of commencing the search in 1845, the planet being considerably past opposition at the time Mr. Adams completed his calculations. The succeeding interval to midsummer of 1846 was a period of great astronomical activity, the planet Astræa, Biela's double comet, and several other comets successively demanding attention. During this time I had little communication with Mr. Adams respecting the new planet. Attention was again called to the subject by the publication of M. Le Verrier's first researches in the *Comptes Rendus* for June 1, 1846. At a meeting of the Greenwich Board of Visitors held on June 29, at which I was present, Mr. Airy announced that M. Le Verrier had obtained very nearly the same longitude of the supposed planet as that given by Mr. Adams. On July 9 I received a letter from Mr. Airy, in which he suggested employing the Northumberland telescope in a systematic search for the planet, offering at the same time to send an assistant from Greenwich in case I declined undertaking the observations. This letter was followed by another dated July 13, containing suggestions respecting the mode of conducting the observations, and an estimation of the amount of work they might be expected to require. In my answer, dated July 18, I signified the determination I had come to of undertaking the search. Various reasons led me to this conclusion. I had already, as Mr. Adams can testify, entertained the idea of making these observations; the most convenient time for commencing them was now approaching; and the confirmation of Mr. Adams's theoretical position, by the calculations of M. Le Verrier, appeared to add very greatly to the probability of success. I had no answer to make to Mr. Airy's offer of sending an assistant, as I understood the acceptance of it to imply the relinquishing on my part of the undertaking.

I have now to speak of the observations. The plan of operations was formed mainly on the suggestions contained in Mr. Airy's note of July 13. It was recommended to sweep over three times at least, a zodiacal belt 30° long and 10° broad, having the theoretical place of the planet at its centre; to complete one sweep before commencing the next; and to map the positions of the stars. The three sweeps, it was calculated, would take 300 hours of observing. This extent of work, which will serve to show the idea entertained of the difficulty of the undertaking before the planet was discovered, did not appear to me greater than the case required. It will

be seen that the plan did not contemplate the use of hour xxi. of the Berlin star-maps, the publication of which was equally unknown at that time to Mr. Airy and myself. It may be proper here to explain, that the construction of a good star-map requires a great amount of time and labour both in observing and calculating, and that precisely this sort of labour must be gone through to conduct a search of the kind I had undertaken. The stars must first be mapped before the search can properly be said to begin. With a map ready made, the detection of a moving body, as it happened in this instance, might be effected on a comparison of the heavens with the map by mere inspection. Not having the advantage of such a map, I proceeded as follows. I noted down very approximately the positions of all the stars to the 11th magnitude, that could be conveniently taken as they passed through the field of view of the telescope, the breadth of the field with a magnifying power of 166 being $9'$, and the telescope being in a fixed position. When the stars came thickly, some were necessarily allowed to pass without recording their places. Wishing to include *all* stars to the 11th magnitude, I proposed in going over the same region a second time, to avail myself of an arrangement peculiar to the Northumberland equatoreal, the merit of inventing which is due to Mr. Airy. The hour-circle, telescope and polar frame, are moveable by clock-work, which may be regulated to sidereal time nearly. While this motion is going on, the telescope and polar frame are moveable *relatively to the hour circle*, by a tangent-screw apparatus, and a handle extending to the observer's seat. This contrivance enables the observer to measure at his leisure differences of right ascension however small, and therefore meets the case of stars coming in groups. The observations made by this method might include all the stars it was thought desirable to take, and therefore might include *all* the stars taken in the first sweep. The discovery of the planet would result from finding that any star in the first sweep was not in its position in the second sweep. If two sweeps failed in detecting the planet among the stars of the first sweep, it might be among the stars of the second, which would be decided by taking a third sweep of the same kind as the second. It will appear that this plan carried out, would not only detect the planet if it were in the region explored, but would also, in case of failure, enable the observer to pronounce that it was not in that region. The second mode of observing required the aid of my two assistants, Mr. Morgan and Mr. Breen, in reading off and recording the observations.

I commenced observing July 29, employing on that day

the first method, with telescope fixed. The next day I observed according to the second method, with telescope moving. On Aug. 4, the telescope was fixed as to right ascension, but was moved in declination in a zone of about 70' breadth, the intention of the observations of that day being to record points of reference for the zones of 9' breadth. On Aug. 12, the fourth day of observing, I went over the same zone, telescope fixed, as on July 30 with telescope moving. Soon after, Aug. 12, I compared to a certain extent, the observations of that day with the observations of July 30, taken with telescope moving; and finding, as far as I carried the comparison, that the positions of July 30 included *all* those of Aug. 12, I felt convinced of the adequacy of the method of search I had adopted. The observations were continued with diligence to Sept. 29, chiefly with telescope fixed, and were made early in right ascension for the purpose of exploring as large a space as possible before I should be compelled to desist by the approach of daylight. On Oct. 1, I heard that the planet was discovered by Dr. Galle at Berlin, on Sept. 23. I had then recorded 3150 positions of stars, and was making preparations for mapping them. The following results were obtained by a discussion of the observations after the announcement of the discovery.

On continuing the comparison of the observations of July 30 and Aug. 12, I found that No. 49, a star of the 8th magnitude in the series of Aug. 12, was *wanting in the series of July 30*. According to the principle of the search, this was the planet. It had wandered into the zone in the interval between July 30 and Aug. 12. I had not continued the former comparison beyond No. 39, probably from the accidental circumstance that a line was there drawn in the memorandum book in consequence of the interruption of the observations by a cloud. After ascertaining the place of the planet on Aug. 12, I readily inferred that it was also among the reference stars taken on Aug. 4. Thus, after four days of observing two positions of the planet were obtained. This is entirely to be attributed to my having on those days directed the telescope towards the planet's theoretical place, according to instructions given in a paper Mr. Adams had the kindness to draw up for me. I would also beg to call attention to the fact, that, after Aug. 12, the planet was discoverable by a closet-comparison of the observations; a method of observing, depending on novel and ingenious mechanism, having been adopted, by which I could say of each star, to No. 48, 'This is not a planet,' and of No. 49, 'This *is* a planet.' I lost the opportunity of announcing the discovery by deferring the dis-

cussion of the observations, being much occupied with reductions of comet observations, and little suspecting that the indications of theory were accurate enough to give a chance of discovery in so short a time. On Sept. 29, I saw for the first time the communication presented by M. Le Verrier to the Paris Academy on Aug. 31. I was much struck with the manner in which the author limits the field of observation, and with his recommending the endeavour to detect the planet by its disc. Mr. Adams had already told me, that, according to his estimation, the planet would not be less bright than a star of the ninth magnitude. On the same evening I swept a considerable breadth in declination, between the limits of right ascension marked out by M. Le Verrier, and I paid particular attention to the physical appearance of the brighter stars. Out of 300 stars, whose positions I recorded that night, I fixed on one which appeared to have a disc, and which proved to be the planet. This was the third time it was observed before the announcement of the discovery reached me. This last observation may be regarded as a discovery of the planet, due to the good definition of the noble instrument which we owe to the munificence of our Chancellor.

From the reduced places of the planet on Aug. 4 and Aug. 12, and from observations since its discovery extending to Oct. 13, Mr. Adams calculated, at my request, values of its heliocentric longitude at a given epoch, its actual distance from the sun, longitude of the node, and inclination of the orbit, which were published as early as Oct. 17. I am now diligently observing the planet with the meridian instruments; and when daylight prevents its being seen on the meridian, I propose carrying on the observations as long as possible with the Northumberland equatoreal, for the purpose of obtaining data for a further approximation to the elements of the orbit.

My report of proceedings relating to the planet here terminates. I beg permission to add a few remarks, which the facts I have stated seem to call for. It will appear by the above account that my success might have been complete if I had trusted more implicitly to the indications of the theory. It must however be remembered that I was in quite a novel position: the history of astronomy does not afford a parallel instance of observations undertaken entirely in reliance upon deductions from theoretical calculations, and those too of a kind before untried. As the case stands, a very prominent part has been taken in the University of Cambridge, with reference to this extension of the boundaries of astronomical science. We may certainly assert to be facts, for which there is documentary evidence, that the problem of determining

from perturbations the unknown place of the disturbing body was first solved here; that the planet was here first sought for; that places of it were here first recorded; and that approximate elements of its orbit were here first deduced from observation. And that all this may be said is entirely due to the talents and labours of one individual among us, who has at once done honour to the University and maintained the scientific reputation of the country. It is to be regretted that Mr. Adams was more intent upon bringing his calculations to perfection, than on establishing his claims to priority by early publication. Some may be of opinion, that in placing before the first astronomer of the kingdom results which showed that he had completed the solution of the problem, and by which he was, in a manner, pledged to the production of his calculations, there was as much publication as was justifiable on the part of a mathematician whose name was not yet before the world, the theory being one by which it was possible the practical astronomer might be misled. Now that success has attended a different course, this will probably not be the general opinion. I should consider myself to be hardly doing justice to Mr. Adams, if I did not take this opportunity of stating, from the means I have had of judging, that it was impossible for any one to have comprehended more fully and clearly all the parts of this intricate problem; that he carefully considered all that was necessary for its exact solution; and that he had a firm conviction, from the results of his calculations, that a planet was to be found.

XI. *On the Maximum Density of Water.*

By JAMES P. JOULE, *Esq.* and DR. LYON PLAYFAIR*.

IN all researches on specific gravity the selection of a proper standard of comparison is a matter of great importance. For obvious reasons water has been universally selected as this standard; but a diversity of opinion has been entertained as to the temperature at which the gravity of water should be called unity. Hence, whilst our continental neighbours have adopted the freezing temperature, our own countrymen have generally chosen the temperature of 60° Fahr. Water at 60° cannot be a desirable standard, on account of its high rate of expansion at that temperature; and for the same and other reasons the temperature of 32° is not at all more convenient. We conceive that it would be much more philosophical, and lead to many practical advantages, if water

* Communicated by the Chemical Society; having been read April 20, 1846.

at its maximum density were taken as the unit*. In that case, calculations would often be greatly facilitated; and if the temperature of the water should happen to be a degree under or over the maximum point in any experiment, the extreme slowness of the expansion would avert the possibility of a grave error.

Intending to give still greater accuracy to our future experiments, we deem the present a favourable opportunity for fixing upon a good unit of comparison. We propose therefore to occupy the present series with a brief discussion as to the point of temperature at which water arrives at its state of greatest density.

Although a variety of methods have been employed in the investigation of this subject, they may be classified under two general heads. The first of these embraces all the methods involving the necessity of an accurate acquaintance with the rate of the expansion of some solid body by heat. The inquiry, when made in this way, is one of great difficulty, and has occupied the attention of many of our most accurate experimenters. The Florentine Academicians, Croune, Deluc, Dalton, and others, compared the indications of a thermometer filled with water with one filled with mercury; Lefevre Gineau and Hallström weighed a solid body in water at different temperatures; and Blagden and Gilpin measured the variation of volume by the variation of the weight of water contained in the same vessel at different temperatures.

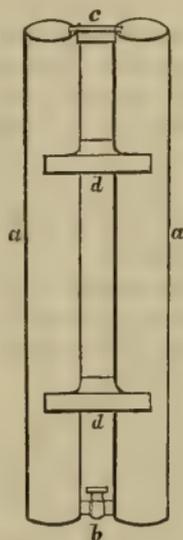
The other general method does not require a previous acquaintance with the expansion of a solid by heat: it consists virtually in weighing water in water—the heavier water descending, while the lighter ascends to replace it. This principle was introduced by Dr. Hope, who applied it in the following elegant manner:—He filled with water at different temperatures tall glass jars having thermometers at top, middle and bottom. In this way he observed that when water was cooled down to 40° at the surface, it sank to the bottom; and when cooled below 40° at the bottom, it rose again to the surface. Trallés, Count Rumford, Ekstrand and Despretz have repeated Hope's experiment with a similar apparatus.

Believing that the second general method is susceptible of a far greater degree of accuracy than the first, we at once determined to employ it in our own experiments. The particular

* Since this paper was communicated to the Society we have seen the *Annuaire du Bureau des Longitudes* for 1845, in which, under the high authority of Arago, water at its point of maximum density is assumed as the unit for comparison.

apparatus of Dr. Hope did not however appear to us to present the method in a form calculated to give results of great accuracy; and hence we have found it necessary to devise a new instrument combining all the theoretical advantages with the requisite facilities for exact observation.

Our instrument is represented in the accompanying sketch. *a a* are two upright vessels of tinned iron, each $4\frac{1}{2}$ feet high and 6 inches in diameter: they are connected at the bottom by means of a brass pipe *b*, furnished with an accurately wrought stop-cock. This pipe is altogether 6 inches long, and enters 1 inch within each vessel. When the stop-cock is opened a clear passage of 1 inch diameter throughout forms a communication between the vessels. A rectangular trough of tinned iron, *c*, 6 inches long, 1 inch broad, and 1 inch deep, forms a communication between the tops of the vessels. In the middle of this trough there is a slide, by means of which the motion of a current along the trough can be stopped when requisite.



The vessels were supported in two places by means of the wooden brackets *d d*, and in order to prevent the greater than desired effect of the atmosphere in raising or depressing their temperature, they were completely covered with haybands. During the experiments the instrument was placed upon a tripod stool resting upon a support quite independent of the floor of the laboratory, in order to keep it entirely free from vibration.

Now if the two vessels be filled with water and made to communicate with one another by opening the stop-cock and removing the slide, it is evident that a current will tend to flow through the trough connecting the tops of the vessels, if the density of the water in one of the vessels be in the least degree greater than that of the water in the other vessel. Although the changes in density are very minute near the maximum point, the extreme mobility of fluids led us to expect that we might in this way arrive at an exact and incontrovertible result.

The *thermometers* employed by us were of extreme accuracy, having been calibrated throughout their whole length, and their delicacy was such as to indicate a change of temperature considerably less than $\frac{1}{100}$ th of a degree Fahrenheit.

The freezing-points of the thermometers were carefully determined within a few hours of the experiments. Each vessel was furnished with a *stirrer*, consisting of a disc of tinned iron 4 inches in diameter attached to the end of a slender rod of iron, by means of which the water was thoroughly stirred before each determination of temperature.

In order to measure the motion of the water in the trough connecting the tops of the vessels, a hollow glass ball of about three-eighths of an inch diameter was placed in it. The weight of this glass ball was carefully adjusted so as only just to float: a matter of great importance, as the slightest buoyancy is accompanied by a certain degree of capillary attraction, and makes the ball liable to adhere to the sides of the trough.

The *water* employed in the experiments was distilled by ourselves in clean vessels of tinned iron; and the additional precaution was taken to prevent, as far as possible, the solution of air.

Our method of experimenting was as follows:—Having filled the vessel with distilled water at a temperature of about 37° , we increased the temperature of one of the vessels to $41^{\circ}5$ by the addition of a small quantity of hot distilled water. We then placed two delicate thermometers upon a proper stand, so that their bulbs dipped in the water to the depth of 6 inches. Having then closed the stop-cock and adjusted the slide, we stirred the water in each vessel thoroughly, and noted the temperatures indicated by the thermometers. The stop-cock was then opened and the slide carefully removed from the trough. After waiting three minutes the glass ball was put into the trough, and its motion watched for two or three minutes with the help of a graduated rule placed at the top of the trough. In conclusion, the stop-cock was again turned, the slide readjusted, the water stirred, and the temperatures again noted. The mean of the temperatures thus observed before and after each trial of the velocity of the current was taken as the temperature of the observation.

The following table contains the results of a series of observations taken in the above manner. The temperature of the laboratory being about 38° , the water in the warmer vessel cooled down more rapidly than the water in the other vessel increased in temperature; and therefore after two or three hours had elapsed, the water in the cooler vessel was found to have acquired greater buoyancy than that in the warmer vessel, although at the commencement of the experiments the current indicated a greater degree of buoyancy in the warmer water.

Series 1.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
41°183	37°348	39°265	280 from the warmer vessel
41°129	37°368	39°248	240 ditto
40°959	37°363	39°161	20 ditto
40°905	37°368	39°136	8 ditto
40°711	37°317	39°014	40 from the cooler vessel

We could hardly have anticipated more satisfactory results than those of the above table. They show clearly that while water at a temperature of 40°·905 is lighter than water at a temperature of 37°·368, water at 40°·711 is heavier than water at 37°·317: in other words, that 39°·136 is above, whilst 39°·014 is below the maximum point. By drawing a curve from the results, we find that the exact point of maximum density indicated by the above series of observations is 39°·102.

During the next series of observations the temperature of the laboratory was about 41°, which occasioned a gradual increase of the temperatures of both vessels.

Series 2.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
40°742	37°368	39°055	22 from the cooler vessel
40°758	37°420	39°089	8 from the warmer vessel
40°773	37°470	39°121	60 ditto

The point of maximum density indicated by this second series of observations is 39°·078.

Series 3.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
40°332	37°633	38°982	70 from the cooler vessel
40°402	37°682	39°042	80 ditto
40°425	37°709	39°067	60 ditto
40°440	37°745	39°092	8 ditto
40°448	37°791	39°120	30 ditto
40°467	37°837	39°152	12 from the warmer vessel
40°483	37°873	39°178	30 ditto

The point of maximum density indicated by the above series will be situated at about $39^{\circ}134$.

We now proceeded to apply a severer test to our method. In the next series of experiments we arranged matters so that the temperature of the water in one vessel was only a degree and a half higher than that of the other vessel. The expansion of water increasing as the square of the temperature from that of the maximum density, it was obvious that the current in the trough would be much more feeble than in the former experiments. We therefore allowed the vessels to be in perfect repose for six minutes before we introduced the glass ball, and we afterwards watched its motion for four or five minutes.

Series 4.

Temperature of the water in the warmer vessel.	Temperature of the water in the cooler vessel.	Mean of the temperatures of the two vessels.	Velocity of the current in inches per hour.
$39^{\circ}921$	$38^{\circ}382$	$39^{\circ}151$	30 from the warmer vessel
$39^{\circ}864$	$38^{\circ}398$	$39^{\circ}131\frac{1}{2}$	0
$39^{\circ}821$	$38^{\circ}362$	$39^{\circ}091$	0
$39^{\circ}782$	$38^{\circ}332$	$39^{\circ}057$	$2\frac{1}{2}$ from the cooler vessel.

The position of the point of maximum density according to the above fourth series of experiments will be at $39^{\circ}091$.

I.	Point of maximum density of pure water	$39^{\circ}102$
II.	...	$39^{\circ}078$
III.	...	$39^{\circ}134$
IV.	...	$39^{\circ}091$
	Mean	$39^{\circ}101$

Although in the different series of observations there are several irregular results, there is on the whole sufficient consistency among them to enable us to receive $39^{\circ}1$, the mean of the four sets of observations, as the actual point of maximum density. We think it highly probable that this temperature is within one-hundredth of a degree of the truth: it certainly cannot be more than one-twentieth of a degree in error. We were prevented by the mildness of the season from extending the experiments further, but we doubt not that by repeating them more frequently we should be able to bring the determination of the point to any required degree of accuracy. The result arrived at by Despretz from a very extensive series of experiments, with an apparatus similar to that employed by Hope, is $39^{\circ}176$ *, which agrees very well

* *Annales de Chimie*, 1839, t. lxx. p. 45.

with our determination. But other results, such as those of Hallström $39^{\circ}38$, Blagden and Gilpin 39° , Hope $39^{\circ}5$, Deluc 41° , Lefebvre Gineau 40° , Dalton 38° , Rumford $38^{\circ}8$, Muncke $38^{\circ}804$, Stampfer $38^{\circ}75$, &c., show by their discordance with one another, and their disagreement with our result, the little dependence which can in general be placed on the results of former methods.

We believe that our new method may be applied with great advantage to a variety of interesting problems. One of the most important of these applications is the determination of the dilatation of glass bulbs by heat, which, though formerly presenting great practical difficulties, can now be accomplished in the most simple and decisive manner. The bulb has only to be filled with pure water and reduced successively to two temperatures, one as much above as the other is below the point of maximum density—the rise of the liquid in the stem of course indicates the contraction of the glass in passing from the higher to the lower temperature. The expansion of the glass bulbs being thus accurately ascertained, they may be advantageously applied in determining the dilatations of solutions and other liquids.

XII. On a peculiar Organ found in the Rays (Raia, Cuv.).

By M. LE DR. CH. ROBIN*.

THERE exists upon each side of the tail of the Rays an organ which is not mentioned in any of the works which I have hitherto been able to consult. This apparatus however deserves, on more than one account, to attract the attention of physiologists, and probably of physicists also.

The two organs united form nearly the third of the entire bulk of the tail of the Rays. The bulk of each, at its largest part, is in one of these fishes of an average size, nearly that of the index-finger. Their origin is towards the union of the first and second quarter of the caudal appendage of the Rays, and they terminate in a point at the extremity of the latter. Their anterior extremity is soft and more or less slender, according to different individuals: it swells gradually as far as the middle of the tail of these fishes; the volume remains the same as far as the origin of the posterior quarter, whence it diminishes finally to the end. This organ is at first almost cylindrical, though a little flattened on the sides (to about the extent of the anterior quarter); in the whole of that part it is enveloped by several thin and concentric muscular layers. These muscles soon terminate in as many aponeurotic layers; the organ then becomes subcutaneous, and at the same time its form changes, becoming round externally and flattened

* From the Annals and Magazine of Natural History for January 1847.

within. In a word, this organ, separated from the tissues to which it adheres, presents the form of an elongated fusiform body, swollen in the centre, more or less blunt at the extremity, and always flattened upon its internal surface.

In a Ray one metre* long, the tail was 49, and the organ 36 centimetres in length; 11 millimetres in the transversal direction, and 13 from above downwards.

The tissue of this organ has the semitransparency of gelatine, but more consistence, and its colour is a clear translucent gray. It is furnished with a general fibrous envelope, which adheres to the adjoining tissues by aponeurotic membranous layers ranged at regular distances.

I have already said that its anterior portion was completely surrounded by some concentric muscular layers, and then that it became subcutaneous in its three posterior quarters, for the greatest part of its surface. I add, by way of more detail, that its internal surface alone is not subcutaneous, and is separated from the vertebral column by the two long muscular and tendinous bundles intended to move the caudal vertebræ. Its upper margin is traversed by a large subcutaneous vein, a branch of the lateral vessel; its external surface is traversed by the lateral vessel itself, which is accompanied by the lateral nerve. This nerve is situated between the organ and the skin, throughout the whole length of the subcutaneous portion.

After these details on the relations of this apparatus with the adjacent organs,—relations, moreover, common to all the species of Rays,—I proceed to make known the *texture* of the tissue peculiar to this organ and the distribution of its vessels and its nerves. For this purpose I shall adopt a comparative course, that is to say, at each step I shall refer to the relations of this organ with those which most resemble it in other fishes.

On examining attentively the apparatus in question, we observe that its substance does not constitute an uniform gelatinous mass, but that it is divided into a large number of polygonal flattened discs by the partitions of cellular tissue. These discs have consequently two surfaces larger than the rest, one turned forwards, the other backwards. With respect to the faces of the circumference, they are in number three, four or five, which gives the discs a triangular, tetragonal or pentagonal form; their smallest diameter measures the thickness of the disc, which is 1 millimetre in nearly all the species. The diameter of the large surfaces, which measures the height of the disc, is 2 mill., one more in the *Raia rubus* and *R. batis*, and 3 to 4 mill. in the *Raia clavata*.

From this difference it follows, in the greatest diameter of the

* The metre is equivalent to 39·37 English inches.

discs according to the species, that in a transverse section of the organ, from eleven to fifteen discs are counted in a *R. clavata*, and twenty to twenty-five in the *R. rubus* and the *R. batis*. The volume of the discs increases with age and the size of the individuals, but their number does not appear to increase.

These gelatinous discs are piled up one upon another, in the direction of the length of the apparatus, by the adherence of their broad faces, with the interposition nevertheless of a thin cellular partition. These longitudinal rows of discs are arranged side by side, reunited by a cellular partition thicker than that which separates each disc from that which follows or precedes it. The kinds of longitudinal columns represented by the piled-up discs are not rectilinear and do not all follow the length of one of the faces of the apparatus; but they are more or less contorted, and are interrupted at intervals. The interruptions proceed from the discs becoming at intervals irregular, more narrow, and the series of discs terminates ordinarily in a very small, triangular one. It follows from these anatomical arrangements, that on the surface of the organ we may perceive one of the small faces of each of the superficial discs, and study very regularly the elongated, quadrilateral or lozenge-shaped polygonal, sometimes hexagonal form which it assumes in consequence of the reciprocal pressure which it undergoes from the adjoining discs. We may moreover very easily perceive that when the discs of a series begin to lose their form and are interrupted, there exist at the side other irregular discs which commence a new series. It is to be remarked also that the discs are ranged more regularly on the internal surface of the apparatus in the *Raia rubus* and *R. batis* than in the *Raia clavata*; in the first two species we also observe on the internal surface of the organ, that one of the partitions which separates the series of discs on the internal surface, follows its whole length and is of greater thickness than the rest: it is formed by glistening aponeurotic fibres, and it forms a sort of longitudinal pile into which the vessels and the nerves penetrate.

With respect to the gelatinous substance of the discs, magnified 400 diameters, it is seen to be hollowed out by cavities, and the walls of the latter are hollowed by cavities gradually lessening in size. The substance too which circumscribes these areolæ (to which we shall recur hereafter) is hyaline, homogeneous and transparent; it is studded with extremely fine molecular granules. From one spot to another are very regular granular spheres of $0^{\text{mill}}\cdot 0050$, surrounded by a very pale circular mass of granules similar to the preceding. It is impossible to recognise veritable cellules with walls and nuclei, and it is easy to see that the preceding areolæ are not cellules; we shall soon speak of their uses. On the margins of the discs, the homogeneous gela-

tinous substance presents regularly undulated striæ which it would be impossible to take for fibres.

At the point at which we are arrived, it is impossible not to recognise a great analogy between the semitransparent gelatinous substance which essentially constitutes the discs of the peculiar organ of the Rays, and that of the prisms of the apparatus of the Torpedo, the rhomboidal meshes of the *Silurus electricus*, and those interrupted ones between the transversal and vertical fibrous laminæ of the *Gymnotus*.

Although there may be differences in form between the discs of the organ of the tail of the Rays and those which constitute the prisms of the electrical apparatus of the Torpedo, these differences are certainly less considerable than those of the portions of gelatinous substance circumscribed by the partitions and areolæ of the apparatus in the *Silurus* and *Gymnotus*, which however produce similar effects to those of the Torpedo.

The mode of arrangement of these discs is as regular in the Ray as in the Torpedo, and approximate much nearer to the latter than to that of the same parts in the apparatus of the *Silurus* and *Gymnotus*.

The nerves of this apparatus originate in the portion of the spinal marrow which is prolonged into the caudal vertebræ. I have an object in view in remarking that this portion of the spinal marrow must be composed of sensitive and motive nervous fibres, for it corresponds to the portion called *cauda equina* in the higher animals.

The nervous roots which originate from this organ do not take their rise together at the same level, but there springs alternately an anterior and a posterior root. It is always from the anterior one (before its anastomosis with the posterior) that the greatest number of nerves which exist in the apparatus proceed; lastly, some issue from the ganglion and the lowest branch of the two which proceed from it. These nerves are of the number of four to seven for each nervous pair. They are, as is seen, very numerous, and their diameter is from $\frac{1}{5}$ to $\frac{1}{2}$ millimetre. These nerves are finally distributed in the thickness of the partitions which separate the lateral muscles from the tail, when they penetrate into the organ, after being more or less subdivided. In the *Raia rubus* and *R. batis* the greatest number penetrate into the longitudinal pile of the internal surface; in the *Raia clavata* they penetrate into some one of the partitions of that surface. In these three species several branches wind round the superior and inferior margins of the apparatus to penetrate into one of the partitions of its subcutaneous portion. In the first two species these superficial branches freely anastomose before penetrating.

It results from these facts that a considerable number of nerves

extend into the partitions of each series of discs infinitely subdividing. From these subdivisions part the filaments which penetrate between the partition which separates each disc from that with which it is in contact. This filament expands opposite to the anterior face of each disc, but never does a single one penetrate into the substance of the disc. The nerves spread out on the internal surface of the partition between it and the disc. No single thread ever ramifies against the posterior face of the disc; we shall soon see that this surface receives only vessels.

The elementary fibres of the nervous filaments have a double character; that is, they are true elementary nervous tubes traversed by a semifluid substance which escapes in drops of variable forms from their extremities when torn across. [These observations however would require to be verified upon animals fresh captured.]

The elementary tubes which spread out against the prism are from 0^m·01 to 0·013, that is to say, half the diameter of the elementary tubes measured on the nerves at the point of their penetration into the apparatus. The elementary nervous tubes do not terminate in a net-work, but actually in very large meshes, to effect which they fork out several times into two or three branches and anastomose by inosculation.

These facts rest on the clearest evidence, being easily proved even with a magnifying power of 100 diameters. The semifluid nervous substance contained in these elementary tubes may be made to flow out, and be seen to penetrate into each of their subdivisions and anastomoses. These anastomotic terminations of the elementary nervous tubes have already been proved to exist by Savi, in his "Anatomical Investigation of the Torpedo (1844)." He has also proved this fact in the partitions which separate the discs of gelatinous substance of the electric apparatus of this fish.

The last facts which I have just established exhibit a still greater analogy between the organ in question and the apparatus of the electric fishes. It is true that these nerves proceed from the termination of the spinal marrow, that is to say, from the *cauda equina*, but the same fact takes place in the *Gymnotus*, the most potent in its discharges of the electrical fishes, whose electrical organs however, according to Hunter, do not receive a mass of nerves proportionably so considerable as those of the Torpedo. In the Ray, as in the *Gymnotus*, the mass of the nerves sent to the electric apparatus by each nervous pair, is at least as considerable as those which they transmit to the skin and the muscles. The lateral nerve does not in the Ray, any more than in the *Gymnotus*, send any filament to the organ in question.

The nerves of the electrical apparatus of the *Silurus*, examined

by Geoffroy St. Hilaire, M. Valenciennes, Rudolphi and Peters, proceed from the lateral nerve, a branch of the eighth pair.

Thus there is nothing constant in the origin of the nerves of the electrical apparatus, as they proceed sometimes from the eighth and ninth pair (*Torpedo*), sometimes from the eighth pair alone (*Silurus*), sometimes from the pairs which arise from the spinal marrow (*Gymnotus* and *Raia*). Their situation has also no constancy, as they are sometimes situated towards the head (*Torpedo*), around the body (*Silurus*), and on each side of the tail (*Gymnotus* and *Raia*).

The vessels of this organ are numerous and curiously arranged. Between the articulation of each vertebra there passes a vessel, alternately an artery and a vein, proceeding from the principal artery and vein of the tail. These two vessels never pass together to reach their foramina, and they never traverse the inferior spinal apophyses, like the nerves, to issue from the spinal canal. Beyond the vertebræ, the vessels follow the course of the nerves, and penetrate with them into the apparatus. Several branches ramify on its surface, surround it with their anastomoses, and from the plexus which they form, some branches are detached, destined either for the skin or the adjoining muscles.

Those vessels which penetrate the thickness of the organ are there subdivided infinitely in the partitions of connective tissue which separate the discs from one another.

From the plexus formed by the arterial and venous ramifications capillaries are given off, which are directed towards the posterior face of the disc which is in front of them, and penetrate into its substance. A capillary vessel never penetrates into the anterior surface of a prism; but we have stated that the nerves ramify opposite to or against that surface. The capillaries which penetrate the discs are very elegantly arranged in flexuous loops, and are sometimes agglomerated in the form of tufts. These loops and tufts are lodged and buried in the cavities by which the disc is hollowed out; these excavations exist only on the posterior face of the discs, whilst the anterior face against which the nerves are arranged is smooth. The capillaries which are buried in the discs are from $\frac{1}{10}$ to $\frac{1}{25}$ of a millimetre in diameter.

To sum up the matter, there exists in the Rays a pretty voluminous organ, situated in the tail of that fish, as in that of the *Gymnotus*. [From a letter which I have received from Prof. J. Müller, Rüppell would appear to have described an organ analogous to the former in the tail of the fishes of the genus *Mormyrus*.] This organ of the Rays receives fine but very numerous nerves. It is formed of a gelatinous semitransparent and firm matter, as in all the electrical organs known. This substance is,

as in all these fishes, divided into polygonal discs, regularly piled together, against which nerves ramify that terminate by successive bifurcations and anastomoses supplied from their elementary fibres. How can we help seeing in this an electrical apparatus? It is true that its position is not the same as in the Torpedo, but in the *Gymnotus* and the *Silurus* the organ is also situated in the tail or around the body. These conclusions are further confirmed by the following facts: I have proved that this apparatus is wanting in the tail of the Torpedo and the genera *Mustelus*, *Scyllium*, *Squatina*, *Zygæna*, *Acanthias* and *Carcharias*.

The presence of this apparatus in the tail of the Rays explains the immoderate proportional length of this organ, its flattened form beneath, and the absence of the inferior lobe on the caudal fin, which scarcely exists in the Rays. The anal fin is also wanting in the Rays, it is also wanting in the Torpedos; but all these fishes have a complete caudal fin, whereas it is wanting in the Rays, as I have just observed.

I am indebted to the kindness of M. Bibron for being enabled to ascertain that the other fishes allied to the Rays (*Cephaloptera*, *Myliobates* and *Pastinacus*) whose tail is terminated by a thin and extended whip or prolongation, do not possess this apparatus. The whip is formed of a portion of the tail, which the electrical apparatus would occupy if it existed.

As we have just seen, this organ cannot be regarded as a gland, for it has not the structure of one; it does not possess an excretory duct, it does not communicate in any part with the inside, and no gland receives so many nerves of animal life*.

XIII. Notices respecting New Books.

The Physical Atlas; a series of Maps illustrating the Geographical Distribution of Natural Phænomena. By H. BERGHAUS, LL.D., F.R.G.S. &c., and A. K. JOHNSTON, F.R.G.S. &c.

ALTHOUGH the idea embodied in the publication before us is not here met with for the first time, yet it will probably have all the attraction of novelty for the general public, since we believe that the work on which it is founded is not very extensively known in this country. To those who know the *Physische Atlas* of Dr. Berghaus, it will be a great gratification to find that the design has been appreciated by the able editor of the National Atlas, and that he has succeeded in obtaining the assistance, not only of Dr. Berghaus, but of some of the most distinguished followers of the sciences,

* Nevertheless the proof of its being an electrical organ must depend upon its power of giving electric shocks. Such a property, in our common Rays, if it existed, could hardly have escaped the notice of fishermen, in the constant habit of handling large Rays, Skates and Thornbacks immediately after their capture.—ED.

the results of which it comprehends, in the endeavour to produce a British Physical Atlas the execution of which shall be worthy of the grand generalizations on which it is based.

The advantages which attend this mode of conveying information it is scarcely necessary to enlarge upon ; it has all the simplicity of a tabular arrangement, without that bare abstract quality which leaves to the mind no connecting links, no means of association, and demands a distinct effort of memory for every fact ; while with the majority of persons the pictorial form of a map makes a peculiar kind of impression, which can be reproduced at pleasure in the consciousness, and becomes a ground on which the several facts may be retraced. The good effect that must result from the more accurate knowledge thus attainable by the "uninitiated" of the vast extent of the complex details which are necessary to the evolution of the simplest generalizations, will be at once admitted on looking abroad upon the crude speculations which are so hastily grasped in the daily increasing desire for knowledge of the deeper mysteries of nature. A man who has carefully and patiently studied these maps and pondered over the multitude of "experimental truths" on which the simplest of them depends, will probably pause ere he accepts cosmical theories which find their materials in popular "abstracts." Not that we undervalue the speculative faculty ; but it is evident that the department of observation and experiment is that to which those inquirers should restrict themselves, whose opportunities or inclinations prevent their acquiring a comprehensive knowledge of the subjects by which they are attracted.

To such persons this Atlas will be exceedingly valuable, as indicating the precise state of our information in regard to the facts, and thus pointing out to them where their labours may be most usefully directed. To the general student it will convey clearly and agreeably a mass of that knowledge which is daily becoming more necessary to him. To the rising generation, in whose education these subjects cannot well be neglected, where any pretension is made to keep pace with the intellectual progress of the times, these maps will throw new and powerful interest into the study of geography, leading not merely to the knowledge of relative position and political or other artificial divisions, but to an appreciation of the natural relations, regarded either in a scientific or œconomic point of view, of the various regions to each other, and a comprehension of the grand unity of this our terrestrial world, where each part is, as it were, complementary to all the rest.

In the execution of this design the various subjects are arranged under the two general heads of the phenomena of Inorganic and Organic nature ; the former comprehending three subdivisions,—1. Meteorology and Magnetism ; 2. Hydrology, and 3. Geology ; the latter nominally two, viz. Phytology and Zoology ; but apparently under the latter head we must include Ethnology, since among the maps already published we find an Ethnographic map of the British Islands.

Five parts of the work are now before us, which, according to the

prospectus, form half of the contemplated series. Each part contains three maps; and as these are not published in any regular arrangement, they are sufficiently varied to give a good idea of the intentions of the editors. The following is a summary view of the published maps.

Part I.—1. A Physical Chart of the Atlantic Ocean; 2. a map of the Mountain Systems of Europe; 3. map and elevations illustrating the Distribution of Plants. In the first will be found, in addition to the usual peculiarities of charts, such as distances, currents, &c., the temperature at different points, the situation and form of Fucus banks and the appearances of volcanic action, with the existing statistics of icebergs and doubtful islands, and the tracks commonly followed by navigators. The various phænomena are briefly described in the accompanying letter-press, as much as possible of the information being tabulated. The most striking feature of the second map is the application of the system of contour lines, without which indeed all plane representations of elevated tracts are but very vague. In the third map are embodied Schouw's phyto-geographic labours, together with statistical information of Humboldt and other travellers.

Part II.—1. The distribution of part of the Mammalia; 2. a Hyetographic map of the world; 3. the River systems of Europe and Asia. The first includes a view of the range of four mammiferous families, viz. the Quadrumana, Marsupialia, Edentata and Pachydermata; the types of these families being represented pictorially for the benefit of those unacquainted with scientific classification. The Hyetographic or Rain map is an attempt to indicate, by various degrees of shading, the relative amount of rain falling in different districts; it is accompanied by a quantity of statistical tables of great value. This map has a peculiarly novel appearance; and the contrasts obtained by means of the shading are very striking; the great deserts and the rainless districts of South America, so graphically described by Mr. Darwin in his journal, standing out strangely in their whiteness from the dark regions near the equator. The River systems are arranged so as to display the great basins of the two continents and course of the rivers, thus forming a kind of supplement to the view of the mountain chains. The description includes hydrographic tables, &c.

Part III.—1. Glaciers and glacial phænomena; 2. distribution of Carnivora; 3. Physical Chart of the Pacific. The first map has been compiled from the researches of Forbes, Raymond, Charpentier, Weiss, &c. This is accompanied by a descriptive treatise by Prof. J. D. Forbes, giving a detailed account of the European glaciers, founded on his own extensive researches. Those of South America, described by Captain King and Mr. Darwin, are also noticed, and the glaciers of Sir James Ross's great south polar continent. In addition to the general distribution of the Carnivora, the second sheet contains a map of the regions of the northern whale and seal fisheries, and those inhabited by fur-bearing animals. The Chart of the Pacific resembles that of the Atlantic, containing of course many peculiar points of interest.

Part IV.—1. Phenomena of Volcanic action; 2. Rain map of Europe; 3. distribution of Reptiles. The first of these is enriched by a collection of small but very important special maps of the most celebrated volcanic districts. On the general chart are given the lines of direction of the most important earthquakes of which we have records, and a view of the various volcanic regions. Among the small maps are Lower Italy and the Greek islands, according to Von Buch; the Sandwich Islands, &c., by Fitzroy; Iceland after Krug von Nidda, &c. The accompanying table gives the various statistics of height, date of eruption, &c. of all the volcanoes of which these particulars have been obtained. In the Hyetographic map the points receiving equal annual amounts of rain are united by a system of curved lines called Isohyetoses, analogous to the isothermal lines of Humboldt; also isotherombroses or curves indicating the distribution of summer rains; the annual number of rainy or snowy days, depth of fall, &c., and the direction of the rain winds. In this map Europe is divided into three regions:—the province of winter rains, comprehending part of the southern portion; of autumn rains, including the remainder of the southern and western portions; the summer rains belong to the whole interior of the continent. The limits of these regions are of course only approximative. In the distribution of reptiles there are separate sections for the Testudines, Sauria and Batrachia; the Ophidians have two, one for the innocuous species, and another for the venomous.

Part V.—1. Distribution of Birds; 2. mountain chains of North America; 3. Ethnographic map of Great Britain and Ireland. The distribution of Birds includes a general map of the world divided into sixteen provinces, and a map for Europe regarded as a single province. The second map contains, besides the mountain chains of the North American continent, a map of the Island of Trinidad and Humboldt's plan of the volcano of Jorulla. The Ethnographic map is the result of an endeavour to lay down, as far as the present condition of our knowledge will admit, the distribution of the various races by which our islands have been peopled. This is an attempt of acknowledged difficulty, since in a country which has attained a high degree of civilization, the intermixture of the elements which must have taken place necessarily leads to great uncertainty. The general conclusions to which the author of the map (the late Dr. Gustav Kohn) had arrived are as follows: there are five sub-varieties of the Celtic variety, and eight of the Teutonic; and the following is a probable average calculation of the ethnographic blood of the population of Great Britain and Ireland.

I. Pure Blood.

1. Teutonic in England, Scotland, east and north-east of Ireland	10,000,000
2. Celtic in Cornwall, Wales, Scottish Highlands and Ireland	6,000,000
	<hr/>
	16,000,000

II. Mixed Blood.

1. Teutonic (that is with prevalent Teutonic character) in England, Scotland, east and north-east of Ireland	6,000,000
2. Celtic (with prevalent Celtic character) in Cornwall, Wales, Highlands of Scotland and Ireland	4,000,000
	10,000,000
Total Teutonic, pure and mixed	16,000,000
Total Celtic, pure and mixed	10,000,000
	26,000,000

The Physical Atlas contains all the elements of success, high interest in the subject matter, as we have shown, and this brought forward in a manner well-worthy of it. The size of the maps is imperial folio, each of the maps occupying a sheet; the engraving and colouring are beyond all praise for their beauty and clearness. The enterprize which has produced this work at such a moderate price calls for extensive patronage, and the care of its execution fully deserves it. We feel bound, both in justice to the editors and our readers, to give it our warmest recommendations.

Introduction to Zoology for the Use of Schools. By R. PATTERSON, Vice-Pres. of the Nat. Hist. Soc. of Belfast. 12mo. London 1846.

We cordially recommend to the notice of our readers this valuable little work by Mr. Patterson, who is well known to have long and energetically endeavoured to introduce the study of natural history into the ordinary course of general education. His great attainments as a naturalist well qualify him for the task which he has undertaken; and we feel sure that the publication of this volume will greatly tend to promote his highly desirable object.

In the present publication, which is profusely illustrated by wood engravings, he treats of the Invertebrate animals, but proposes to continue the subject in a future volume on the Vertebrata. We have here in small space a very complete introduction to the study of the interesting tribes upon which it treats detailed in simple and popular language, so as to be intelligible to persons who have not made natural history a study; but at the same time there is nothing in it of what is vulgarly called "popular science," every part being of a truly scientific character. It is well fitted to be placed in the hands of the young, and highly deserving of perusal by the accomplished naturalist.

We wish him every success in his laudable endeavour.

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxix. p. 409.]

Nov 19, "ON the Automatic Registration of Magnetometers, 1846. and other Meteorological Instruments, by Photogra-

phy." By Charles Brooke, M.B., F.R.C.S.E. Communicated by G. B. Airy, Esq., F.R.S., Astronomer Royal.

The author enters into fuller details than he had done in his former communication to the Society, which was read on the 18th of June, respecting the construction of the instrument, the preparation of the highly sensitive photographic paper employed in the process, and the minute adjustments necessary for ensuring accuracy in registering the results.

In a supplement to the above paper, the author describes the methods he has contrived for obtaining a similar automatic registration of the heights of the barometer and thermometer, by suitable additions to the same apparatus which registers the magnetic variations.

W. R. Grove, Esq., M.A., F.R.S., delivered the Bakerian Lecture—"On certain Phenomena of Voltaic Ignition, and on the Decomposition of Water into its constituent Gases by heat."

The author refers to an eudiometer, an account of which was published by him in the 'Philosophical Magazine' for 1840, formed of a glass tube, into the closed extremity of which a loop of platinum wire was sealed. The gases to be analysed were mixed in this tube with a given volume of oxygen and hydrogen, and detonated or slowly combined by the voltaic ignition of the platinum wire. He was thence led to try a further set of experiments on the analysis, by this instrument, of such gases and vapours as are decomposable by heat; the process being capable of much greater exactness than the received one of passing them through ignited tubes. The results of the analyses of several gases by this means are given in the paper. When carbonic acid and hydrogen are mixed in equal volumes and exposed to the ignited wire, the hydrogen abstracts oxygen from the carbonic acid, and leaves carbonic oxide. Conversely, when carbonic oxide is exposed over water to the ignited wire, it abstracts oxygen from the aqueous vapour, and forms carbonic acid.

It thus appeared, that provided there were bodies present capable of absorbing by affinity the elements of water, ignited platinum would either compose or decompose water. The author was thence led to hope that he might by ignited platinum decompose water into its constituents, without absorption by other bodies, and thus produce converse effects to those already known. In this he ultimately succeeded by various methods, in some of which the ignition was produced by electrical means; in others by ordinary calorific processes, such as the oxyhydrogen blowpipe, &c.

A platinum wire is ignited at the closed extremity of a species of tube retort, full of pure water, and having a narrowed neck close above the wire; as soon as the wire becomes incandescent, it forms around itself an atmosphere of vapour which it immediately decomposes; a natural valve being formed by the conflict of ascending gas and descending water, the bubbles of mixed gas are cut off by an intermittent action, and thus, as their recombination is prevented, a volume of gas collects in the bend of the tube and is ultimately

expelled at its orifice. If, again, a button of platinum be fully ignited by the oxyhydrogen blowpipe, and plunged into water previously heated to nearly its boiling-point, bubbles of mixed gas ascend and may be collected by an inverted tube. The electrical spark is shown to be capable of decomposing aqueous vapour, and various other modes of producing the same results are given.

Some theoretical views are then advanced as to the spheroidal state which appears to the author to be intermediate between that of ebullition and decomposition; as to the probable non-existence of water or steam in the interior of the earth, and as to the antagonism between physical repulsion and chemical affinity.

In a supplementary paper, the author considers how far catalysis affects the phenomenon, and regards the decomposition thus produced as presenting a parallel effect produced by the force of heat, to that known to be produced by electricity; he considers it explanatory of the decomposition of water by the electrical spark as in the experiments of Pearson and Wollaston. Some further experiments are given, in which iridium and osmium and silica are substituted for platinum; and also some experiments on the liquids bromine and chloride of iodine, both of which yield pure oxygen when exposed to the ignited wire in Mr. Grove's apparatus. These last experiments cannot however be long continued in consequence of these liquids ultimately attacking both the glass and the platinum. In conclusion, the author calls attention to the general evolution of permanent gas from all liquids, except the metals, when exposed to intense heat.

Dec. 17.—“*Researches on Physical Geology.*”—Part I. The Figure and Primitive Formation of the Earth. By Henry Hennessy, Esq. Communicated by Major North Ludlow Beamish, K.H., F.R.S.

The author's investigations of the figure of the earth proceed on the hypothesis of its having originally been a heterogeneous fluid mass, possessing only such general properties as those which have been established for fluids; and independently of the supposition, with which the theory has generally been complicated, that the volume of the entire mass, and the law of the density of the fluid, have suffered no change in consequence of the solidification of a part of that fluid. Assuming the figure of the mass to be an ellipsoid of revolution, the author obtains general analytical expressions for its ellipticity, and for the variation of gravity at its surface. He gives a general sketch of the consequences that may result from the improved hypothesis of the primitive figure of the earth, to physical geology, that is, to the changes occurring upon the external crust of the earth during the process of its solidification, resulting both from calorific and chemical changes taking place among its different parts, and giving rise to a process of circulation throughout the fluid portions of the mass.

The present memoir is only the first of a series which the author announces it is his intention to communicate to the Society on the same subject.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxix. p. 232.]

November 13, 1846.—*Elements of Astræa.*

Mr. Graham, of Mr. Cooper's Observatory at Markree, has deduced the following elements, omitting any consideration of disturbing forces:—

1846. Jan. 1. Mean Anom.	318	55	21.5	} Mean Equinox, 1846.0.
$\pi - \varnothing$	353	55	49.2	
\varnothing	141	24	43.8	
i	5	19	22.1	
ϕ	10	45	57.7	
Log. a	0.4107004			
Log. n''	2.9339560			
n''	858''.9265			
Sid. Rev. 1509 days.				

The places from which these elements are deduced, are,—

	Greenwich M. T.	R. A.	Dec.			
1845. Dec. 17.460393	63	22	21.62	+12	40	18.29
1846. Feb. 8.053359	62	37	24.46	15	40	31.99
March 27.310391	77	59	41.13	+19	57	8.40

The first place is a mean of ten continental observations; the second is a mean of four meridian observations by Mr. Cooper, at Markree; the third, a single observation at Pulkawa.

Elliptical Elements of De Vico's Fourth Comet (Feb. 20, 1846),
by Mr. Hind.

The following orbit is based upon the Altona and Hamburg Observations of March 12, and Hamburg, March 31 and April 18. There appears no doubt that the elements are really elliptical:—

Perihelion Passage, 1846, March 5.57777, Greenwich Mean Time.

Longitude of Perihelion	90	34	45.79	} Mean Equinox, 1846.0.
Longitude of Ascending Node.....	77	35	35.90	
Inclination	84	57	12.61	
Angle of Eccentricity	72	37	42.99	
Log. semi-axis major	1.8062261			
Sidereal Revolution 55.4 years.				
Motion direct.				

Note on the First Comet of Brorsen, by Mr. Hind.

“The comet discovered by Mr. Brorsen at Kiel, on the 26th of February last, has been found to have an elliptical orbit with a period of $5\frac{1}{2}$ years only. Independent calculations by Dr. Brünnow, M. Goujon, and myself, have led to nearly the same results as regards the short time of revolution. This circumstance induced me to examine the path of the comet through our system more minutely. Assuming the last set of elements calculated by Dr. Brünnow, and published in No. 557 of the *Astronomische Nach-*

richten, I soon found that a very close approach to the planet Jupiter must have occurred about the 20th of May 1842: the comet at this time would be hardly further distant from Jupiter than the mean distance of his fourth satellite. The action of the planet would be more than ten times greater than that of the Sun; and it is very possible that a complete change might be produced in the elements of the comet's orbit. This encounter took place rather to the south of the plane of the ecliptic, in about $283\frac{1}{2}^{\circ}$ heliocentric longitude."

Peters's Comet (26th June 1846).

On the 26th of June, Dr. C. H. F. Peters, of the Observatory of Capodimonte, found a very small comet in the constellation Scorpius, near 595 Mayer, and about one degree to the south of the nebula H. vi. 19, or 535 of Smyth's Cycle. The new object was quite as pale as the nebula, and somewhat rounder. The motion was perceptible at the end of an hour, when Dr. Peters commenced his observations with the equatorial of Reichenbach. These were continued on following nights till stopped by moonlight. The following are the results corrected for refraction:—

1846.	Naples Sid. Time.			R.A.			Dec.			No. of Obs.
	h	m	s	°	'	"	°	'	"	
June 26	17	24	4.0	226	51	31.4	—21	38	42.4	3
27	16	19	3.0	227	18	53.9	20	57	19.9	7
28	17	2	18.0	227	49	20.2	20	14	19.8	4
29	16	48	45.6	228	19	3.8	19	33	4.3	6
30	17	53	10.3	228	48	48.3	18	50	36.0	5
July 1	18	24	25.7	229	19	15.9	18	10	37.4	5
11	16	57	24.8	234	5	53.5	12	31	31.1	2
12	17	19	34.1	234	33	39.8	12	2	18.7	4
13	17	18	29.1	235	2	16.3	11	34	45.7	4
14	17	6	56.8	235	29	45.7	11	8	19.7	4
15	17	39	10.5	235	59	18.8	10	40	10.3	3
16	17	19	26.3	236	26	47.1	10	16	1.4	5
17	17	31	47.6	236	55	9.8	9	50	33.5	4
18	17	43	22.4	237	26	5.3	9	27	23.5	4
19	17	41	14.2	237	52	22.4	9	3	53.1	4
21	17	51	58.3	238	47	28.0	—8	19	12.3	3

It was seen on the 23rd July, but it was too faint to be observed. Upon these observations Dr. Peters has calculated the following elements, taking into account all small corrections (*Parall. Aberr. Nutat.*).

Perihelion Passage 1846, May 30, 12^h 56^m 3^s.0, Berlin M.T.

Perih.	237	20	28.2	} Mean Equinox, July 1.
Q	258	45	12.6	
i	34	0	41.7	
Log. q.	0.204635			
	Motion direct.			

These elements differ considerably from a first rough sketch. Dr. Peters believes them to be near the truth; still they give from day to day considerable anomalies, when compared with the observations.

De Vico's Fifth Comet (July 29, 1846).

This comet was also detected by Mr. Hind, at the South Villa Observatory, on the night of July 29, 1846, about 11 o'clock, two hours after its first discovery by De Vico.

"The following positions are the results of instrumental comparisons at South Villa Observatory with known stars, not far distant from the comet; but owing to the extreme faintness of this object, the observations of August 4-15 inclusive, are very uncertain:—

	Greenwich M.T.	R.A.	Dec.
July 29	^h 12 ^m 6 ^s 6	48° 53' 47"	+60° 37' 2"
30	10 17 35	48 5 34	60 42 33
31	9 56 19	47 12 6	60 48 10
Aug. 4	11 11 21	43 11 19	61 8 14
13	9 21 21	32 4 0	61 15 8
14	9 50 23	30 35 57	61 11 5
15	12 56 27	28 55 22	61 5 10
21	9 46 27	19 39 27	+60 2 8

"On July 29 and 30 we obtained some micrometrical measures with two small stars. On the 29th, at 13^h 25^m 25^s Greenwich mean time, the comet followed a star of 8.9 magnitude, 2^m 12^s.33, and was south of it 16' 17".8. This star is found in Argelander's Zones; and the apparent place for July 29, as given in Professor Schumacher's Comet Circular, was

R.A. 3^h 13^m 11^s.86 Dec. +60° 53' 47".0,

whence the comet's place was

R.A. 3^h 15^m 24^s.19 Dec. +60° 37' 29".2.

"On July 30, at 11^h 23^m 13^s mean time, the comet followed a 9th magnitude star 1^m 25^s.07, and was south of it 45".04; but we have not yet identified this star in any of our catalogues. On this night, with great attention, and an exact knowledge of the position, the comet was just perceptible in the comet-searcher."

Elements of De Vico's Fifth Comet, by Mr. A. Graham of Markree Observatory.

The observations employed were, one at Mr. Bishop's observatory, July 29; that of Paris, on August 18; and another made by Mr. Cooper on August 31:—

Perihelion Passage, 1846, May 27.82285, Greenwich Mean Time.

π	82 39 19.7	} Mean Equinox, 1846.0.
ϖ	161 18 29.1	
i	57 36 24.2	
Log. q	0.1382020	

Motion Retrograde.

The errors of the elements at the time of the middle observation were in geocentric longitude $-1''.4$, and in latitude $+4''.1$. The corrections for aberration and parallax were deduced from M. Goujon's elements. The constants for August 30th, referred to apparent equinox, are,—

$$\begin{aligned} x &= [9.98351] \cdot r \cdot \sin(358^\circ 55' 9'' + v) \\ y &= [9.50659] \cdot r \cdot \sin(144^\circ 52' 34'' + v) \\ z &= [9.99340] \cdot r \cdot \sin(86^\circ 5' 25'' + v). \end{aligned}$$

Mr. Cooper's observation on August 31 gave the following position:—

Greenwich M.T.	R.A.	Dec.
August 31.46583	2° 54' 2.0''	+55° 37' 7.3''

De Vico's *Sixth Comet* (September 23, 1846).

This faint telescopic comet was discovered by Father De Vico, in the constellation Ursa Major. M. Rümker has reobserved the stars of comparison, and computed the following places from the original observations:—

	Rome M.T.	R.A.	Dec.
	h m s	° ' "	° ' "
1846. Sept. 23.	8 6 36.5	128 21 44.5	+64 14 23.4
	10 25 34.9	128 48 29.5	+64 14 18.5

M. Wichmann rediscovered this comet at Königsberg.

	Königsberg M.T.	R.A.	Dec.
	h m s	° ' "	° ' "
1846. Oct. 15.	8 39 19	187 33 5.1	+44 30 23.3
16.	7 45 11	188 36 23.9	+43 29 6.2

The following observations were made at Altona and Hamburg:—

	Altona M.T.	R.A.	Dec.	No. of
	h m s	° ' "	° ' "	Obs.
1846. Oct. 21.	6 42 19.7	193 11 40	+38 20 46	
	Hamburg M.T.			
21.	7 5 18.2	193 12 46.1	38 19 47.3	2
22.	16 20 50.1	194 17 34.3	36 57 52.6	12

The elements, as computed by M. Powalky at Altona, and by M. Niebour at Hamburg, prove the identity of the comet of Wichmann with De Vico's Sixth Comet.

Elements of the Sixth Comet of De Vico (September 23, 1846),
by Mr. Hind.

The following orbit depends on the observations at Rome, September 23; Königsberg, October 15; and Altona and Hamburg, October 21. All the small corrections taken into account.

Time of Perihelion Passage, 1846, Oct. 29.74708, Greenwich Mean Time.

Longitude of Perihelion	98 35 49.9	} True Equinox, 1846, Nov. 0.
Longitude of Ascending Node	4 41 3.7	
Inclination	49 41 17.1	
Log. distance in Perihelion	9.9193956	

Motion direct.

The co-ordinate constants for the apparent equinox at the beginning of November, are,—

$$\begin{aligned}
 x &= r \cdot [9\cdot9991563] \cdot \sin(v + 186^{\circ} 56' 50'') \\
 y &= r \cdot [9\cdot4735634] \cdot \sin(v + 108^{\circ} 29' 45'') \\
 x &= r \cdot [9\cdot9807954] \cdot \sin(v + 95^{\circ} 51' 36'')
 \end{aligned}$$

Hind's Comet (October 18, 1846).

“A few minutes before 4 o'clock on the morning of October 19, a very faint telescopic comet was discovered here, with the large refractor. It was just bright enough to bear a slight illumination sufficient to render visible the thick wires of the micrometer. By observations extending over rather more than one hour, or until day-break put a stop to them, the comet was found to have a direct motion in right ascension, of about four minutes in time daily, and a southerly motion in declination of about eleven minutes. Every night for three weeks afterwards was cloudy when the comet was above our horizon, and the observations of October 18 are, therefore, all we have been able to procure. Mr. Petersen, M. Rümker, and Mr. Brorsen, though immediately apprised of the discovery by Prof. Schumacher, have had no better success, owing to the overcast sky. The comet was compared instrumentally with β Leonis, for an approximate position, and subsequently, by means of a wire micrometer, with a star of the 9th magnitude.

“The instrumental comparisons gave,—

	Greenwich M. T.			R.A.			Dec.		
	h	m	s	h	m	s	°	'	''
Oct. 18.	16	15	11	11	59	49.1	+14	59	32
	17	6	17	11	59	57.5	+14	59	8

“By four micrometrical measures with the small star, the comet's right ascension was $38^{\circ}08'$ less than that of the star, and its declination $4' 26''\cdot 1$ greater than that of the star, at $16^{\text{h}} 54^{\text{m}} 16^{\text{s}}$ Greenwich mean time. We are not yet in possession of the accurate position of this star.

“This morning (Nov. 11) the sky was clear in the east for the first time since the date of the comet's discovery. A careful search was made for it, without success.”

Observations of Double Stars, made at Poonah, by Capt. W. S. Jacob.

The telescope is by Dollond, 5 feet focal length, and mounted on an universal equatorial stand, which mounting is not, however, so well adapted to high as to low latitudes. The telescope is a pretty good one, but shows rather large discs; hence Capt. Jacob has found no advantage in employing a higher power than 152. With this power he can generally measure angles of position when the stars are $1''\cdot 5$ apart. The distances are not to be much relied upon, as “little can be done in that department without clockwork.” More than half the stars are taken from Smyth's Cycle, the remainder are such as are too far south to be seen in England. Many of these, though discovered by Capt. Jacob, will probably be found in the forthcoming catalogue of Sir J. F. W. Herschel. In attempting to determine the

orbits of double stars, Capt. Jacob has only been satisfied with that of 12 Cassiopeïæ, which is circular, inclined to the eye at an angle of 33° ; period, 501.6 years; nearest approach, 1888.0; greatest and least distances, $12''\cdot6$ and $6''\cdot9$; position of node, $54^\circ\cdot1$. This seems a fair first approximation.

XV. Intelligence and Miscellaneous Articles.

BURATITE—A NEW MINERAL.

THIS mineral is a hydrocarbonate of zinc, copper and lime, in definite proportions; it crystallizes in bluish radiating needles; its specific gravity is 3.20. Its composition, according to the analysis of M. Delesse, is—

Carbonic acid	21.45
Oxide of zinc	32.02
Lime	8.62
Oxide of copper	29.46
Water	8.45

The relations between the elements lead to the formula $2(\text{Zn, Cu, Ca})\text{C} + \text{aq}$, and admitting the ideas of M. Scheerer on polymeric isomorphism, we obtain the very simple formula $\text{CO}^2(\text{RO})^2$.

Buratite has been found in the copper mines of Lotefskoi in the Altai mountains, at Chessy near Lyons, at Temperino in Tuscany, and in several other localities.—*Comptes Rendus*, Octobre 26.

ON BISULPHURET, TERSULPHURET, AND SULPHOCYANURET OF METHYLE. BY M. AUGUSTE CAHOURS.

The first-named compound is obtained with equal facility by passing a current of chloromethylic gas into an alcoholic solution of bisulphuret of potassium, or by distilling a mixture of the concentrated solutions of bisulphuret of potassium and sulphomethylate of lime. In both cases a slightly yellow limpid liquid is obtained, which begins to boil at 230° to about 234° F., its boiling-point reaching from 320° to 338° F.

If the alkaline sulphuret employed be very pure, the greater part of the liquid distils between 230° and 248° F. By separating this product, drying it over chloride of calcium and submitting it to one or two rectifications, a definite liquid is obtained which boils between 241° and 245° F.: this is the bisulphuret of methyle. When pure it is a colourless liquid, perfectly limpid, refracting light powerfully, and possessing an intolerable and persistent odour of onions. Its density is 1.046 at $64^\circ\cdot4$ F. It is scarcely soluble in water, but nevertheless imparts to it its peculiar smell: alcohol and æther combine with it in all proportions. On the approach of an ignited body it inflames and burns with a blue flame, yielding a strong smell of sulphurous acid.

Chlorine acts powerfully on this compound: there is at first
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formed an amber-coloured substance, which crystallizes in rhombic tables possessing much lustre. These crystals are soon destroyed, and are replaced by a yellowish liquid, which eventually becomes of a ruby-red colour; by continuing the action there are obtained chloride of sulphur $S Cl^2$, and perchloride of sulphuret of methyle $C^4 Cl^6 S$.

Bromine also acts upon this body, forming products derived from substitution.

Very dilute nitric acid has no sensible action in the cold on bisulphuret of methyle; but it is different when it is moderately dilute. In this case the action is extremely vivid; sulphuric acid is produced, and also a peculiar acid, which forms with potash a salt that crystallizes in long asbestiform needles, and with barytes a salt which crystallizes in colourless tables and possessing much splendour. It also forms with strontia, lime, and the oxide of lead salts which are soluble and crystallizable. Sulphuric acid when concentrated combines with it in the cold, and decomposes it when heated.

When heated in a retort with a concentrated solution of potash, it distils without appearing to undergo any alteration; when submitted to analysis it appeared to be composed of—

C ⁴	150·0, or in 100	25·53
H ⁶	37·5	6·38
S ²	400·0	68·09
	587·5	100·00

The density of the vapour of this substance, taking the mean of two experiments, was found to be 3·298; and the author observes that, supposing the molecule of this compound to give two volumes of vapour, the density would be 3·259.

Tersulphuret of Methyle.—When bisulphuret of potassium is replaced by the persulphuret in forming the above compound, a considerable quantity of bisulphuret of methyle is also obtained; but towards the end, and at the temperature of about 396° F., an amber-coloured product distils, which contains more sulphur, and which acts with chlorine and nitric acid in the same manner.

The high boiling-point of this substance, and the slight alteration which it undergoes by heat, prevented the author from ascertaining the density of its vapour. Analysis showed that it was a tersulphuret of methyle.

C ⁴	150·0, or in 100	19·05
H ⁶	37·5	4·76
S ³	600·0	76·19
	787·5	100·00

Sulphocyanuret of Methyle.—On distilling a mixture of equal parts of a concentrated solution of sulphocyanuret of potassium and sulphomethylate of lime, there passes over with the vapour of water a heavy yellowish liquid, which after being dried over chloride of calcium, possesses a fixed boiling-point; nine-tenths of the product

come over at about 270° F. ; the temperature at which the last portion comes over may amount to 280° F.

The preparation of this product is difficult on account of the bumping of the liquid, which sometimes comes over. It must be heated slowly, and the retort should be ten times larger than required to contain the liquid.

Sulphocyanuret of methyle, after it has been purified by digestion over chloride of calcium and rectification, has the following properties : it is a colourless and very limpid liquid ; it has an alliaceous odour ; its vapour is unpleasant and it stupefies ; it boils steadily at about 270° F. Its density is 1.115 at about 61° F. Water dissolves a very small portion, but it nevertheless acquires its odour ; it combines with alcohol and æther in all proportions.

In diffused light chlorine acts very slowly upon it, producing fine crystals of chloride of cyanogen ; and at the same time there is formed a large quantity of a heavy yellow oil, which solidifies when in contact with ammonia.

Potash scarcely acts upon it in the cold. A solution when heated decomposes it, and there are formed ammonia and bisulphuret of methyle ; the residue contains cyanuret of potassium and carbonate of potash.

Solution of ammonia alters it readily, giving rise to a brown substance analogous to ulmine, and a small quantity of a crystallized white matter. Heated in an alcoholic solution, the sulphocyanuret of methyle is decomposed ; there are produced bisulphuret of methyle and sulphocyanuret of potassium, easily recognised by the persalts of iron. Moderately concentrated nitric acid dissolves it when heated, but it separates entirely on cooling.

By analysis it appears to consist of—

C ^s	300.0, or in 100	32.87
H ⁶	37.5 ..	4.11
N ²	177.0 ..	19.18
S ²	400.0 ..	43.84
	<u>914.5</u>	<u>100.00</u>

The mean of two experiments gave 2.559 as the density of its vapour ; whereas, supposing its molecule to yield four volumes of vapour, calculation would give 2.537.—*Ann. de Ch. et de Ph.*, Novembre 1846.

ON SULPHOCYANURET AND BISULPHURET OF ETHYLE.

BY M. AUGUSTE CAHOURS.

Sulphocyanuret of Ethyle.—By distilling a mixture of sulphovinate of lime and persulphuret of potassium, both solutions being concentrated, a yellowish liquid is obtained, which after washing with water, digesting on chloride of calcium, and cautious rectification, becomes a colourless and very limpid liquid, the odour of which resembles that of sulphocyanuret of methyle. Its density is 1.020 at about 61° F. It is insoluble in water, but alcohol and æther dissolve it in

all proportions. It acts similarly to sulphocyanuret of methyle upon reagents.

Chlorine and bromine act energetically on the sulphur compounds of methyle and of ethyle. With bromine perfectly well-crystallized combinations are obtained.

By analysis this substance appears to be composed of—

C ¹²	450·0,	or in 100 parts	41·38
H ¹⁰	62·5	5·74
N ³	177·0	16·09
S ²	400·0	36·79
	1089·5		100·00

By experiment the density of the vapour of this substance was found to be 3·018, while calculation would lead to 3·032, supposing the molecule represented four volumes of vapour.

Bisulphuret and Tersulphuret of Ethyle.—The former was obtained in considerable quantity by distilling a mixture of sulphovinate of lime and persulphuret of potassium; but towards the end of the rectification of the rough product there came over a yellowish and slightly volatile liquid, which gave by analysis results which tended to show that it was to be considered as a tersulphuret of ethyle.

M. Cahours analysed the bisulphuret of ethyle, and his results agreed precisely with those previously obtained by M. P. Morin, C⁸ H¹⁰ S². The density of its vapour was found by experiment to be 4·270, and by calculation 4·240, on the supposition that the molecule of this compound furnishes two volumes of vapour.

Chlorine vividly acts upon the two compounds above described; moderately concentrated nitric acid does the same, a portion of the sulphur being converted into sulphuric acid: there is also produced an acid susceptible of forming soluble and crystallizable salts with potash, barytes, and oxide of lead.—*Ann. de Ch. et de Phys.*, Novembre 1846.

ANALYSIS OF THE WHITE DIOPSIDE.

M. Hermann found this substance from Achmatowsk to consist of—

Silica	53·97
Lime	25·60
Magnesia.....	17·86
Protoxide of iron.....	2·00
Protoxide of manganese ..	0·57
	100·00

This analysis corresponds to the formula Si⁴ M⁴ O⁶: [Si⁴ (Ca² Mg²) O⁶].—*Journ. de Pharm. et de Ch.*, Novembre 1846.

ANALYSES OF THE NATIVE PHOSPHATES OF COPPER.

BY M. HERMANN.

The phosphates of copper do not occur more extensively anywhere than in the mines of Nischnotaglsk in the Uralian Moun-

tains; M. Hermann has submitted to analysis some fine specimens of this mineral from them.

The method of analysis which he adopted was the following:—he reduced the mineral to fine powder, calcined it to determine the water, and fused the residue with four times its weight of soda at a red heat; it was then treated with boiling water, and the remaining oxide of copper was weighed after heating to redness. The phosphoric acid was afterwards ascertained by deficiency, after having ascertained the absence of the oxides of iron and manganese in the oxide of copper, as well as of alumina and manganic acid in the alkaline solution.

The *libethenite* of Tagilsk yielded 65.89 oxide of copper, 28.61 phosphoric acid, 5.50 water. These results differ a little from those obtained by M. Kühn,—66.94 oxide of copper, 29.44 phosphoric acid, 4.01 water; and also from those of Berthier,—63.9 oxide of copper, 28.7 phosphoric acid, and 7.4 water.

Dihydrate (prismatic phosphate of copper) yielded 68.2 oxide of copper, 25.3 phosphoric acid, 6.28–6.69 water. Arfwedson obtained nearly the same results with a phosphate from Rheinbreitenbach.

The *phosphorocalcite* is the most widely diffused of the phosphates of copper. M. Hermann analysed three specimens of it:

	Tagilsk.	Tagilsk.	Virneberg, near Rheinbreitenbach.
Oxide of copper	68.75	67.73	67.25
Phosphoric acid	23.75	23.47	24.55
Water	7.50	8.80	8.20
	100.00	100.00	100.00

Except that the water is in rather larger quantity, this phosphate has the same composition as the dihydrate.

The analyses of Klaproth, Kühn and Lunn differ much from those of M. Hermann; it would appear that Lunn analysed a different mineral.

Under the name of *ehlite* another phosphate of copper has been described; it is found at Ehl, near Rheinbreitenbach. M. Hermann found a specimen from Tagilsk to consist of oxide of copper 66.86, phosphoric acid 23.14, water 10.00; he describes, under the name of *tagilithe*, an earthy phosphate of copper found at Tagilsk, on hydrate of iron, in the form of mammillated or spongy deposits. Its specific gravity is about 3.50. It dissolves in acids without giving out any gas.

The *trombolite* is another phosphate of copper found at Tagilsk; it has already been described by M. Plattner; it is remarkable for its conchoidal fracture; it was found to consist of 39.2 oxide of copper, 41.0 phosphoric acid, 16.8 water. M. Hermann has discovered the phenakite in granite to the west of Lake Ilmen, at some versts from Miask, accompanied with topaze and bluish stilbite.—*Ibid.* Nov. 1846.

ON CHIOLITE—A NEW MINERAL. BY M. HERMANN.

There is found in the granite of the district of Miask in Russia a white mineral, which has much resemblance to cryolite, but the composition is different.

This new mineral occurs in compact and granular masses, and sometimes spathose or leafy. The lamellar portions are somewhat translucent, and give an appearance to the mineral of its being impregnated with moisture or with fatty matter. The cleavage faces form an angle of 66° .

Its hardness is that of fluor spar; its lustre is between vitreous and greasy. Its density is 2.72. It fuses below the melting-point of glass, without yielding a trace of water. Heated in an open tube, it evinces the action of hydrofluoric acid. It fuses readily with borax and a phosphate, giving colourless glasses. When mixed with sulphuric acid, the powdered mineral disengages much hydrofluoric acid. Carbonate of ammonia precipitates pure alumina from the solution; the liquor separated from the precipitate contains pure sulphate of soda, unmixed with potash or lithia.

M. Hermann found it to consist of—

Sodium	23.78
Aluminium	18.69
Fluorine	57.33
	100.00

Cryolite contains, for the same quantity of fluoride of sodium, only half the quantity of fluoride of aluminium contained in chiolite.—*Ibid.*

PURIFICATION OF MERCURY. BY M. MILLON.

Mercury may be perfectly purified by agitating it for a considerable time with weak nitric acid. With two pounds of the metal, about an ounce and a half of the acid diluted with twice its volume of water may be employed. The mercury freed from the nitrate thus formed is to be boiled with pure nitric acid sufficient to dissolve about nine-tenths of the metal; the resulting nitrate of mercury is to be reduced to red oxide by heat, and this is to be calcined in a porcelain retort to reduce it.

By the action of the first portion of nitric acid the more oxidable metals are acted upon; the second portion of acid leaves the metals less oxidable than mercury in the undissolved portion.

As the mercury reduced by this process dissolves a notable quantity of oxide, this last is to be separated by agitation with sulphuric acid; it is afterwards to be washed with a very large quantity of water, and dried in the receiver of the air-pump over sulphuric acid. Mercury thus purified was employed by M. Regnault in his third determination of its density.

M. Millon states that when a saline solution, such as chloride of

calcium, hydrochlorate of ammonia, nitrate of potash, &c. is added to mercury in a bottle, the mercury is always divided into rounded globules, which remain separated from each other for a long time; but what is very striking is, that the size of the globules, which varies enormously, is always connected with the nature of the aqueous solution. Some solutions immediately cause extreme division in the mercury; others, on the contrary, produce only very large globules, to whatever extent the agitation may have been carried; and the same effect is always produced with the same solution. It is to this influence of saline solutions that is due the difficulty often witnessed in collecting mercury when it has been reduced in the moist way.—*Ann. de Ch. et de Phys.*, Novembre 1846.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1846.

Chiswick.—November 1. Foggy. 2, 3. Very fine. 4. Overcast. 5. Foggy and drizzly. 6, 7. Foggy: hazy. 8. Foggy. 9. Hazy and cold. 10. Dusky clouds: fine. 11. Cold and dry. 12. Overcast. 13. Hazy. 14. Foggy. 15. Cloudy. 16. Overcast: cloudy and fine. 17. Densely overcast: slight rain at night. 18. Cloudy: very fine. 19. Cloudy and damp: very fine. 20. Cloudy: boisterous. 21. Very fine: rain. 22. Fine: clear. 23. Slight rain: overcast. 24. Hazy and damp: rain: overcast. 25. Rain. 26. Clear and fine: foggy. 27. Dense fog. 28. Clear and fine. 29. Frosty: clear and fine. 30. Sharp frost: overcast.

Mean temperature of the month	44°·53
Mean temperature of Nov. 1845	44 °26
Mean temperature of Nov. for the last twenty years ...	42 °91
Average amount of rain in Nov.	2·56 inches.

Boston.—Nov. 1. Foggy. 2. Fine. 3, 4. Cloudy. 5. Fine. 6—9. Cloudy. 10, 11. Fine. 12—14. Cloudy. 15. Cloudy: rain P.M. 16. Cloudy. 17. Cloudy: rain P.M. 18. Cloudy. 19. Fine. 20. Stormy. 21. Cloudy: rain P.M. 22. Fine: rain P.M. 23. Cloudy: rain P.M. 24. Fine. 25. Cloudy. 26. Fine. 27. Cloudy: rain A.M. and P.M. 28. Cloudy: snow P.M. 29. Fine: snow on the ground. 30. Fine: melted snow in the gauge.

Sandwick Manse, Orkney.—Nov. 1. Showers: cloudy. 2, 3. Cloudy. 4. Bright: rain. 5. Bright: cloudy. 6. Hazy: rain. 7. Bright: showers. 8. Bright: clear: fine. 9. Clear: very clear and fine. 10. Hoar-frost: cloudy: fine. 11—15. Cloudy: fine. 16. Bright: clear: aurora. 17. Rain: clear: aurora, south. 18. Clear: showers. 19. Clear: aurora. 20. Rain: showers. 21. Showers. 22. Rain: clear. 23. Damp: cloudy. 24. Showers. 25. Showers: cloudy. 26. Showers. 27. Hail-showers. 28. Snow: hail-showers. 29. Snow-drift: clear. 30. Cloudy: shower: thaw.

Applegarth Manse, Dumfries-shire.—Nov. 1. Slight showers P.M. 2. Showers morning and evening. 3. Rain P.M. 4. Moist A.M.: rain P.M. 5. Showers. 6. Fair, but moist. 7. Fair, but dull and cloudy. 8. Fair and fine. 9. Fair, but cloudy. 10. Hoar-frost: clear and fine. 11. Fair and fine: hoar-frost A.M. 12—15. Fair, but dull. 16. Clear: piercingly cold. 17. Wet. 18. Showers. 19. Heavy rain. 20. Heavy rain: river in flood. 21. Heavy rain: thunder P.M. 22. Rain. 23. Heavy rain. 24. Rain A.M. 25. Rain and fog. 26. Dull and drizzly. 27. Fair. 28. Frost: shower of snow. 29. Hard frost: clear. 30. Frost: thick and foggy.

Mean temperature of the month	44°·4
Mean temperature of Nov. 1845	42 °7
Mean temperature of Nov. for 23 years	40 °2
Mean rain in Nov. for 18 years	3·63 inches.

THE
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[THIRD SERIES.]

FEBRUARY 1847.

XVI. *The ASTRONOMER ROYAL on Sir David Brewster's
New Analysis of Solar Light.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Royal Observatory, Greenwich, Jan. 15, 1847.

IN the Notes to the second edition of the History of the Inductive Sciences, Dr. Whewell has (with my consent) referred to experiments, made by me and described by me at a meeting of the Cambridge Philosophical Society, tending to invalidate the conclusions drawn by Sir David Brewster in a paper headed "On a New Analysis of Solar Light, indicating three Primary Colours, forming Coincident Spectra of equal length," which is printed in the twelfth volume of the Edinburgh Transactions. I think it therefore incumbent on me to give to the public the best account that I am now able to give of these experiments. The reason of the partial imperfection of the account is the following. I never drew up in writing any orderly statement either of the experiments or of the reasoning connected with them. I had the pencil notes, written each at the instant of making an experiment; and from these notes, or rather from the strong recollection of every experiment detailed in the notes, I made my oral statement (as far as the mere experimental facts were concerned) to the Cambridge Philosophical Society. These notes I have mislaid; and in consequence I am unable to give details on the names and combinations of the different absorbing substances which I employed. But upon the method, upon the results, and upon the reasonings, my recollection is as perfect as it was on the days on which the experiments were made. Having premised this, I shall give my statement in the same order in which I gave it (in the year 1833, I believe) to the Cambridge Philosophical Society.

Phil. Mag. S. 3. Vol. 30. No. 199. Feb. 1847.

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First, then, I remark that, supposing Sir David Brewster's alleged experimental facts to be placed beyond doubt, the inference as to the three colours is exceedingly doubtful. It appears certain that combinations of other colours might be made to produce the apparent colours of the spectrum; and there seems therefore to be no sufficient reason for selecting those adopted by Sir David Brewster. It is quite proper that the reader's attention should be called to this; for in consequence of Sir David Brewster having given the details of his theory in considerable extent before he gives the experiments, the reader may be led to suppose that the experiments do actually exhibit three such spectra as are implied in the title of the paper. Those, however, who will enter into the minutiae of the paper, will see that the experiments exhibit no such thing. The simple facts which the experiments are supposed to exhibit are, that by the use of different absorbing media, certain portions of the spectra undergo certain changes of colour. And the only question which I am disposed to entertain is this: Is it established that the colour of any portion of the spectrum is changed by the use of any absorbing medium?

My second remark applies to what I conceive to be one possible cause of error in Sir David Brewster's experiment. It does not appear from his paper that the spectrum in its modified state was compared immediately with an unmodified spectrum. Now I state unhesitatingly as my own belief, that the eye has no memory for colours; and I have no confidence in any evidence upon the change of colour, unless the colour which is not altered and the colour in which change is suspected are placed side by side.

Thirdly, I remark that it does not appear that care was taken to exclude light coming to the eye from other sources. Indeed, in an experiment of Sir William Herschel's, to which Sir David Brewster refers, it is certain that there was a mixture of other light. When the spectrum was thrown upon clean brass, "the colour of the brass made the red rays appear like orange, and the orange colour was likewise different from what it ought to be." But on referring to Sir William Herschel's paper, it will be seen that the spectrum was formed by a prism placed in the upper part of an open window; and it is not difficult to predict what must be the effect of the light coming from all other parts of the window.

I now proceed to describe the form in which my own experiments were made.

The windows of my lecture-room were darkened by closing all the shutters, the joints of which were almost absolutely impervious to light. Through a hole in one shutter, a beam

of sun-light was thrown in a convenient direction by means of an external reflector. In front of this hole, but at a distance sufficient to leave room for the insertion of any of my absorbing substances, was placed a metallic plate with a narrow slit about two inches long and adjustable in width. The sun-light passing through this slit was received upon a very good prism placed with its axis parallel to the slit, at the distance of about a foot; a large lens was placed in front of the prism, and by means of this the rays diverging from the slit and falling on the prism were made to converge, so as to form upon a paper screen a very good spectrum, sufficiently pure to exhibit several of the stronger Fraunhofer's lines. Care was taken to prevent any other light from falling directly on the screen.

In order to exhibit the effect of any absorbing medium, it was only necessary to place it behind the slit, in such a manner as to receive and transmit the light which was to pass through a portion of the slit. The corresponding portion of the spectrum underwent the modification produced by the absorption: but the modified portion of the spectrum was in no degree dislocated from the unmodified part (Fraunhofer's lines running without dislocation through both), and the modified portion was separated from the unmodified portion only by the small interruption which is usually occasioned by the edges of any refracting substance. It is impossible to conceive circumstances more favourable than these for the comparison of the modified and unmodified spectra.

Of the absorptive media which I used, I am unable (as I have already stated) to give a complete catalogue. I had however several specimens of smalt-blue glass, all the varieties of coloured glass which I had been able at different times to procure from a glazier, and all the strikingly coloured fluids which I could obtain from a chemist, as well as port wine, porter, &c. The fluids were used in flat perfume-bottles: it would have been better to use them in slightly prismatic vessels, which would admit of the use of different thicknesses of the fluids.

Of the results of these experiments I can give little more than the single negative one,—that no change was produced in the qualities of the colours. After the most patient examination, I could not even suspect a change except in two or three instances, when I imagined that the blue colour was extended both ways, so as to trench upon the green on one side and upon the violet on the other side: but even these suspicions were removed when I closed with greater care the openings by which a little stray light entered between the shutter-hole and the slit in the plate of metal.

The evidence which I am able to offer upon this subject is necessarily, from the nature of the subject, only negative; and the conclusion to which it leads will be destroyed by the establishment of any one fact in opposition to it. But such a fact must be established beyond doubt: and I have no hesitation in saying that no form of experiment anterior to my own has been such as to place its conclusions beyond doubt. I insist upon the two points—of juxtaposition of the modified and unmodified spectra, and of exclusion of other light—as indispensably necessary for accuracy.

The experimenter must also be on his guard against the apparent effects of mere intensification or dilution of light in modifying its quality. Every one knows, for instance, that when the setting sun is beginning to assume a slightly yellow colour, its light received on a white plaster wall appears of a full blush red, although there is not the smallest reason to think that the quality of the light is really altered.

I am, Gentlemen,

Your obedient Servant,

G. B. AIRY.

XVII. *On the Effects of Magnetism upon the Dimensions of Iron and Steel Bars.* By J. P. JOULE, Esq.*

ABOUT the close of the year 1841, Mr. F. D. Arstall, an ingenious machinist of this town, suggested to me a new form of electro-magnetic engine. He was of opinion that a bar of iron experienced an increase of bulk by receiving the magnetic condition; and that, by reversing its polarity rapidly by means of alternating currents of electricity, an available and useful motive power might be realized. At Mr. Arstall's request I undertook some experiments in order to decide how far his opinions were well-founded.

The results of my inquiries were brought before the public on the occasion of a *conversazione* held at the Royal Victoria Gallery of Manchester on the 16th of February 1842, and are printed in the eighth volume of Sturgeon's *Annals of Electricity*, p. 219. As many of my readers may not have access to that work, I will, with the permission of the Editors of the *Philosophical Magazine*, quote a small portion of the paper, which is necessary to complete the history of this interesting branch of investigation.

“A length of thirty feet of copper wire, one-twentieth of an inch thick, and covered with cotton thread, was formed into a

* Communicated by the Author.

coil twenty-two inches long and one-third of an inch in interior diameter. This coil was secured in a perpendicular position; and the rod of iron, of which I wished to ascertain the increment, was suspended in its axis so as to receive the magnetic influence whenever a current of electricity was passed through the coil. Lastly, the upper extremity of the rod was fixed, and the lower extremity was attached to a system of levers which multiplied its motion three thousand times.

“A bar of rectangular iron wire, two feet long, one quarter of an inch broad, and one-eighth of an inch thick, caused the index of the multiplying apparatus to spring from its position and vibrate about a point one-tenth of an inch in advance, when the coil was made to complete the circuit of a battery capable of magnetizing the iron to saturation, or nearly so. After a short interval of time, the index ceased to vibrate, and began to advance very gradually in consequence of the expansion of the bar from the heat which was radiated from the coil. On breaking the circuit, the index immediately began to vibrate about a point, exactly one-tenth of an inch lower than that to which it had attained.

“By multiplying the advance of the index by the power of the levers, we have $\frac{1}{30000}$ th of an inch, the increment of the bar, which may be otherwise stated as $\frac{1}{72000}$ th of its whole length.

“Similar results were obtained by the use of an iron wire, two feet long and one-twelfth of an inch in diameter. Five pairs of the nitric acid battery produced an increment of the thirty-thousandth part of an inch; and when only one pair of the battery was employed, I had an increment very slightly less, viz., the thirty-three thousandth part of an inch.

“This increment does not appear to depend upon the thickness of the bar; for an electro-magnet made of iron, three feet long and one inch square, was found to expand under the magnetic influence to nearly the same extent, compared with its length, as the wires did in the previous experiments.

“I made some experiments in order to ascertain the *law* of the increment. Their results proved it to be *very nearly proportional to the intensity of the magnetism and the length of the bar*.

“Trial was made whether any effect could be produced by using a *copper* wire, two feet long and about one-tenth of an inch in diameter; but I need scarcely observe that the attempt was unattended with the slightest success.

“A very good way of observing the above phænomena is to examine one end of an electro-magnet with a powerful microscope while the other end is fixed. The increment is then

observed to take place with extreme suddenness, as if it had been occasioned by a blow at the other extremity. The expansion, though very minute, is indeed so very rapid that it may be felt by the touch; and if the electro-magnet be placed perpendicularly on a hard elastic body, such as glass, the ear can readily detect the fact that it makes a slight jump every time that contact is made with the battery.

“When one end of the electro-magnet is applied to the ear, a distinct musical sound is heard every time that contact is made with, or broken from, the battery; another proof of the suddenness with which the particles of iron are disturbed.”

In another part of the lecture I stated my reasons for supposing that whilst the bar of iron was increased in length by the magnetic influence, it experienced a *contraction* at right angles to the magnetic axis, so as to prevent any change taking place in the bulk of the bar. I intended as soon as possible to bring this conjecture to the test of experiment, and I prepared some apparatus for the purpose; but owing to other occupations I was obliged to relinquish the experiments until the beginning of last summer. In the meantime the inquiry has been taken up by De la Rive, Matteucci, Wertheim, Wartmann, Marrian, Beatson and others, whose ingenious experiments have invested the subject with additional interest. The researches of Beatson have taken a similar direction to mine; and he appears also to have employed a somewhat similar apparatus to that which I shall presently describe. I have confirmed several of the results at which this gentleman has arrived, and have added new facts, which I hope will throw further light upon this rather obscure department of physics.

In order to ascertain how far my opinion as to the invariability of the *bulk* of a bar of iron under magnetic influence was well-founded, I devised the following apparatus. Ten copper wires, each 110 yards long and one-twentieth of an inch in diameter, were bound together by tape so as to form a good, and at the same time very flexible conductor. The bundle of wires thus formed was coiled upon a glass tube 40 inches long and $1\frac{1}{2}$ inch in diameter. One end of the tube was hermetically sealed, and the other end was furnished with a glass stopper, which was itself perforated so as to admit of the insertion of a graduated capillary tube. In making the experiments, a bar of annealed iron, one yard long and half an inch square, was placed in the tube, which was then filled up with water. The stopper was then adjusted, and the capillary tube inserted so as to force the water to a convenient height within it.

The bulk of the iron bar was about 4,500,000 times the

capacity of each division of the graduated tube; consequently a very minute expansion of the former would have produced a perceptible motion of the water in the capillary tube: but, on connecting the coil with a Daniell's battery of five or six cells (a voltaic apparatus quite adequate to saturate the iron), no perceptible effect whatever was produced either in making or breaking contact with the battery, whether the water was stationary in the stem, or gradually rising or falling from a change of temperature. Now had the usual increase of length been unaccompanied by a corresponding diminution of the diameter of the bar, the water would have been forced through twenty divisions of the capillary tube every time that contact was made with the battery.

Having thus ascertained that the bulk of the bar was invariable, I proceeded to repeat my first experiments with a more delicate apparatus, in order, by a more careful investigation of the laws of the increment of length, to ascend to the probable cause of the phenomenon.

A coiled glass tube, similar to that already described, was fixed vertically in a wooden frame. Its length was such that when a bar one yard long was introduced so as to rest on the sealed end, each extremity of the bar was a full inch within the corresponding extremity of the coil. The apparatus for observing the increment of length consisted of two levers of the first order, and a powerful microscope situated at the extremity of the second lever. These levers were furnished with brass knife edges resting upon glass. The connexion between the free extremity of the bar of iron and the first lever, and that between the two levers, were established by means of exceedingly fine platinum wires.

The first lever multiplied the motion of the extremity of the bar 7·8 times, the second multiplied the motion of the first 8 times, and the microscope was furnished with a micrometer divided into parts each corresponding to $\frac{1}{2220}$ th of an inch. Consequently each division of the micrometer passed over by the index indicated an increment of the length of the bar amounting to $\frac{1}{130520}$ th of an inch.

The quantities of electricity passing through the coil were measured by an accurate galvanometer of tangents, consisting of a circle of thick copper wire one foot in diameter, and a needle half an inch long furnished with a suitable index.

The quantities of magnetic polarity communicated to the iron bar were measured by a finely suspended magnet 18 inches long, placed at the distance of one foot from the centre of the coil. This magnetic bar was furnished with scales precisely in the manner of an ordinary balance, and the weight

required to bring it to a horizontal position indicated the intensity of the magnetism of the iron bar under examination.

After a few preliminary trials, a great advantage was found to result from filling the tube with water. The effect of the water was, as De la Rive had already remarked, to prevent the sound. It also checked the oscillations of the index, and had the important effect of preventing any considerable irregularities in the temperature of the bar.

The first experiment which I shall record was made with a bar consisting of two pieces of very well-annealed rectangular iron wire, each one yard long, a quarter of an inch broad, and about one-eighth of an inch thick. The pieces were fastened together so as to form a bar of nearly a quarter of an inch square. The coil was placed in connexion with a single constant cell, the resistance being further increased by the addition of a few feet of fine wire. The instant that the circuit was closed, the index passed over one division of the micrometer. The needle of the galvanometer was then observed to stand at $7^{\circ} 20'$, while the magnetic balance required 0.52 of a grain to bring it to an equilibrium. It had been found by proper experiments that a current of $7^{\circ} 20'$ passing through the coil was itself capable of exerting a force of 0.03 of a grain upon the balance; consequently the magnetic intensity of the bar was represented by 0.49 of a grain. On breaking the circuit, the index was observed to retire 0.3 of a division, leaving a permanent elongation of 0.7, and a permanent polarity of 0.42 of a grain. More powerful currents were now passed through the coil, and the observations repeated as before, with the results tabulated below.

Experiment 1.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
— $7^{\circ} 20'$	128	1.0 E.	1.0	—0.49	240
0	0	0.3 S.	0.7	—0.42	252
— $9^{\circ} 30'$	167	2.9 E.	3.6	—0.93	240
0	0	1.2 S.	2.4	—0.74	228
— $14^{\circ} 48'$	264	5.9 E.	8.3	—1.42	243
0	0	3.8 S.	4.5	—1.00	222
— $23^{\circ} 10'$	428	10.3 E.	14.8	—1.87	236
0	0	7.6 S.	7.2	—1.26	220
— $47^{\circ} 25'$	1088	16.1 E.	23.3	—2.22	211
0	0	13.9 S.	9.4	—1.35	194
— $58^{\circ} 50'$	1653	14.8 E.	24.2	—2.21	202
0	0	13.3 S.	10.9	—1.35	168

Everything being left in the same position, the electricity was now transmitted in the + or contrary direction, so as first to remove the polarity acquired by the - current, and then to induce the opposite polarity. The total elongation of 10.9, acquired in the last experiment, is carried forward in the 4th column of the following second table of results.

Experiment 1 (continued).

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+ 6 15	109	3.4 S.	7.5	-0.12	2
0	0	0	7.5	-0.17	4
+ 9 55	175	0.1 E.	7.6	+0.57	43
0	0	1.0 S.	6.6	+0.25	9
+15 40	280	3.7 E.	10.3	+1.30	164
0	0	4.0 S.	6.3	+0.78	97
+38 45	802	16.8 E.	23.1	+2.30	229
0	0	14.5 S.	8.6	+1.12	148
+51 30	1257	16.7 E.	25.3	+2.35	218
0	0	16.3 S.	9.0	+1.05	122

The next series of results was obtained with a fresh bar of exactly the same size and temper as the preceding. To avoid an unnecessary occupation of space, I shall omit the fresh heading of the table for a change in the direction of the current, and simply designate the commencement of a new condition by ruling a line.

Experiment 2.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+ 5 0	87	0.1 E.	0.1	+0.08	64
0	0	0	0.1	+0.03	9
+ 8 27	148	1.9 E.	2.0	+0.50	125
0	0	1.0 S.	1.0	+0.30	90
+13 27	239	5.8 E.	6.8	+1.16	198
0	0	3.1 S.	3.7	+0.69	129
+33 50	670	18.8 E.	22.5	+2.20	215
0	0	14.3 S.	8.2	+1.01	124
+53 50	1368	19.0 E.	27.2	+2.32	198
0	0	17.1 S.	10.1	+1.03	105
- 7 5	124	2.0 S.	8.1	-0.15	3
0	0	0.1 S.	8.0	-0.07	0
-55 10	1437	20.0 E.	28.0	-2.20	173
0	0	14.6 S.	13.4	-1.39	144

The next experiment was with a bar of well-annealed iron, one yard long and about half an inch square. Its weight was $45\frac{1}{2}$ oz. I have introduced an additional column into the table, in order to reduce the action on the magnetic balance to the section of the former bars, the weight of each of which was 8 oz.

Experiment 3.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Magnetic intensity per unity of section.	Square of magnetic intensity per unity, divided by elongation.
+ 5 10 0	90 0	0.4 E. 0.1 S.	0.4 0.3	+ 1.18 + 0.45	0.21 0.08	110 21
+ 8 2 0	141 0	0.7 E. 0.2 S.	1.0 0.8	+ 1.82 + 0.67	0.32 0.12	102 18
+14 43 0	262 0	2.0 E. 1.0 S.	2.8 1.8	+ 4.10 + 0.90	0.72 0.16	185 14
+40 3 0	840 0	12.0 E. 8.4 S.	13.8 5.4	+11.08 + 1.20	1.95 0.21	275 8
+54 0 0	1376 0	13.8 E. 12.0 S.	19.2 7.2	+13.53 + 1.20	2.38 0.21	295 6
+62 5 0	1887 0	14.4 E. 13.5 S.	21.6 8.1	+14.13 + 1.20	2.48 0.21	285 5
- 6 30 0	114 0	1.2 S. 0	6.9 6.9	- 0.70 - 0.30	0.12 0.05	2 0
-14 25 0	257 0	0.7 E. 1.3 S.	7.6 6.3	- 3.80 - 1.15	0.67 0.20	59 7
-41 15 0	877 0	11.0 E. 8.8 S.	17.3 8.5	-11.33 - 1.50	1.99 0.26	229 8
-62 45 0	1941 0	16.0 E. 13.0 S.	24.5 11.5	-13.71 - 1.55	2.41 0.27	237 6
+ 5 35 0	98 0	0.8 S. 0	10.7 10.7	+ 0.16 - 0.40	0.03 0.07	0 0
+ 9 0 0	158 0	0.2 S. 0.2 S.	10.5 10.3	+ 1.17 + 0.15	0.21 0.03	4 0
+14 20 0	255 0	0.3 E. 1.2 S.	10.6 9.4	+ 3.30 + 0.50	0.58 0.09	32 1
+24 45 0	461 0	3.3 E. 3.4 S.	12.7 9.3	+ 7.16 + 0.82	1.26 0.14	125 2
+39 50 0	834 0	9.6 E. 8.0 S.	18.9 10.9	+11.43 + 0.95	2.01 0.17	214 2
+54 15 0	1389 0	12.6 E. 11.6 S.	23.5 11.9	+13.47 + 1.00	2.37 0.18	239 3
+60 45 0	1785 0	13.2 E. 12.4 S.	25.1 12.7	+13.84 + 1.00	2.43 0.18	235 3

Experiment 3 (continued).

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Magnetic intensity per unity of section.	Square of magnetic intensity per unity, divided by elongation.
- 7 13 0	127 0	1.0 S. 0.1 S.	11.7 11.6	- 1.13 - 0.50	0.20 0.09	3 1
-10 25 0	184 0	0 0.2 S.	11.6 11.4	- 2.16 - 1.00	0.38 0.18	12 3
-15 57 0	286 0	0.5 E. 1.1 S.	11.9 10.8	- 4.14 - 1.25	0.73 0.22	45 4
-26 0 0	488 0	3.5 E. 3.2 S.	14.3 11.1	- 7.45 - 1.50	1.31 0.26	120 6
-40 55 0	867 0	9.6 E. 8.0 S.	20.7 12.7	-11.48 - 1.70	2.02 0.30	197 7
-62 48 0	1946 0	14.6 E. 13.0 S.	27.3 14.3	-13.76 - 1.73	2.42 0.30	214 6

•From the last column of each of the preceding tables we may, I think, safely infer that *the elongation is in the duplicate ratio of the magnetic intensity of the bar*, both when the magnetism is maintained by the influence of the coil, and in the case of the permanent magnetism after the current has been cut off. The discrepancies observable will, I think, be satisfactorily accounted for when we consider the nature of the magnetic actions taking place. When a bar experiences the inductive influence of a coil traversed by an electrical current, the particles near its axis do not receive as much polarity as those near its surface, because the former have to withstand the opposing inductive influence of a greater number of magnetic particles than the latter. This phænomenon will be diminished in the extent of its manifestation with an increase of the electrical force, and will finally disappear when the current is sufficiently powerful to saturate the iron. Again, when the iron, after having been magnetized by the coil, is abandoned to its own retentive powers by cutting off the electrical current, the magnetism of the interior particles will suffer a greater amount of deterioration than that of the exterior particles. The polarity of the former may indeed be sometimes actually reversed, as Dr. Scoresby found it to be in some extensive combinations of steel bars. Now whenever such influences as the above occur, so as to make the different parts of the bar magnetic to a various extent, the elongation will necessarily bear a greater proportion to the square of the magnetic intensity measured by the balance than would otherwise be the case.

For similar causes the interior of the bar will in general receive the neutralization and reversion of its polarity before the exterior, and hence we see in the tables that there is a considerable elongation of the bar after the reversion of the current; even when the effect upon the balance has become imperceptible, owing to the opposite effects of the interior and exterior magnetic particles.

The bars employed in the preceding experiments were annealed as perfectly as possible. The next series was made with a bar of exactly the same dimensions and quality as the bars employed in Experiments 1 and 2, excepting that it was not annealed.

Experiment 4.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+ 9° 20' 0	164 0	0·2 E. 0	0·2 0·2	+0·15 +0·08	112 32
+15 20 0	274 0	0·9 E. 0·7 S.	1·1 0·4	+0·50 +0·33	227 272
+38 32 0	796 0	7·1 E. 5·2 S.	7·5 2·3	+1·36 +0·80	247 278
+50 30 0	1213 0	10·2 E. 9·6 S.	12·5 2·9	+1·76 +0·97	247 324
+57 40 0	1580 0	13·0 E. 11·8 S.	15·9 4·1	+1·94 +1·00	236 244
+62 20 0	1907 0	14·0 E. 14·0 S.	18·1 4·1	+2·10 +1·01	243 249
- 6 50 0	120 0	1·2 S. 0	2·9 2·9	+0·58 +0·65	116 145
-10 35 0	168 0	0·4 S. 0	2·5 2·5	+0·21 +0·35	17 49
-14 57 0	267 0	0 0·2 S.	2·5 2·3	-0·30 -0·13	36 7
-40 10 0	844 0	5·7 E. 4·6 S.	8·0 3·4	-1·36 -0·88	231 228
-53 30 0	1351 0	10·0 E. 9·5 S.	13·4 3·9	-1·70 -0·95	215 231
+ 9 27 0	166 0	1·3 S. 0·1 E.	2·6 2·7	-0·36 -0·40	50 59
+22 30 0	414 0	0·1 S. 0	2·6 2·6	+0·38 +0·22	55 18
+38 27 0	794 0	4·9 E. 4·6 S.	7·5 2·9	+1·50 +0·97	300 324

In the foregoing series the discrepancies before adverted to do not make their appearance to any considerable extent, except in the case of the reversion of the magnetic polarity. Taken altogether the series is strikingly confirmatory of the law of elongation already announced.

The next series of observations was obtained with a piece of soft steel wire one yard long and a quarter of an inch in diameter. Its weight being exactly 8 oz., no reduction of magnetic intensity had to be made for unity of section.

Experiment 5.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+38° 10'	786	1·4 E.	1·4	+0·94	631
0	0	0·6 S.	0·8	+0·65	528
+50 45	1224	2·8 E.	3·6	+1·43	568
0	0	1·8 S.	1·8	+0·98	533
+60 25	1761	3·8 E.	5·6	+1·71	521
0	0	3·1 S.	2·5	+1·12	502
+67 50	2454	5·0 E.	7·5	+1·88	471
0	0	4·5 S.	3·0	+1·23	504
+69 20	2651	5·5 E.	8·5	+1·97	456
0	0	4·5 S.	4·0	+1·28	409
-41 40	890	1·3 S.	2·7	-0·76	214
0	0	1·0 S.	1·7	-0·35	72

Experiments 6 and 7 were made with fresh steel bars of precisely the same sort as that employed in the last series.

Experiment 6.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+38° 25'	793	0·8 E.	0·8	+0·78	760
0	0	0·5 S.	0·3	+0·46	705
+60 50	1792	5·2 E.	5·5	+1·60	466
0	0	3·4 S.	2·1	+0·99	467
+70 30	2824	7·0 E.	9·1	+1·88	388
0	0	5·8 S.	3·3	+1·16	408
-16 28	295	1·8 S.	1·5	+0·82	448
0	0	0·2 S.	1·3	+0·94	680
-38 50	805	0	1·3	-0·64	315
0	0	0·3 S.	1·0	-0·33	108

Experiment 7.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+38° 20'	790	1·4 E.	1·4	+0·74	391
0	0	0·7 S.	0·7	+0·46	302
+61 5	1810	5·3 E.	6·0	+1·64	448
0	0	3·2 S.	2·8	+1·07	409
+69 55	2735	4·7 E.	7·5	+1·90	481
0	0	4·5 S.	3·0	+1·20	480
-26 40	502	3·0 S.	0	+0·20	
0	0	0·2 S.	-0·2	+0·32	

The uniformity of the numbers in the last columns of the preceding tables shows that where the metal possesses a considerable degree of retentive power the anomalies occasioned by the reaction of the magnetic particles upon one another, which have been already adverted to, do not exist to any considerable extent, and presents an additional confirmation of the law I have stated, viz. *the elongation is proportional, in a given bar, to the square of the magnetic intensity.*

I now made trial of a bar of steel one yard long, half an inch broad, and a quarter of an inch thick, weighing 23 oz. It was hardened to a certain extent throughout its whole length, but not to such a degree as entirely to resist the action of the file.

Experiment 8.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Magnetic intensity per unity of section.	Square of magnetic intensity per unity, divided by elongation.
+39° 0'	810	0	0	+1·11	0·38	
0	0	0·2 E.	0·2	+1·36	0·47	1104
+52 35	1307	0·8 E.	1·0	+4·09	1·42	2016
0	0	0·3 E.	1·3	+2·85	0·99	754
+60 15	1750	0·5 E.	1·8	+5·10	1·77	1740
0	0	0·1 E.	1·9	+3·52	1·22	783
+69 45	2710	0·6 E.	2·5	+5·91	2·06	1697
0	0	0·2 E.	2·7	+4·20	1·46	790
-41 15	877	1·6 S.	1·1	-0·43	0·15	20
0	0	0·1 E.	1·2	+0·35	0·12	12
-56 5	1487	1·4 E.	2·6	-3·90	1·36	711
0	0	0·1 E.	2·7	-2·63	0·91	307

In the above table it will be observed that the hard steel bar was slightly increased in length every time that contact with the battery was broken, although a considerable diminution of the magnetism of the bar took place at the same time. I am disposed to attribute this effect to the state of tension in the hardened steel, for I find that soft iron wire presents a similar anomaly when stretched tightly.

On inspecting the tables, it will be remarked that the elongation is, for the same intensity of magnetism, greater in proportion to the softness of the metal. It is greatest of all in the well-annealed iron bars, and least in the hardened steel. This circumstance appears to me to favour the hypothesis that the phænomena are produced by the attractions taking place between the magnetic particles of the bar, an hypothesis in perfect accordance with the law of elongation which I have pointed out.

[To be continued.]

XVIII. *Remarks on the Existence and Mechanism of the Negative or Protecting Rays of the Sun.* By JOHN WILLIAM DRAPER, M.D., Professor of Chemistry in the University of New York*.

IN a letter published in the Philosophical Magazine, Nov. 1842, I had occasion to make some incidental remarks respecting a class of rays existing in the sun-light, which have the quality of exerting a negative or antagonizing action upon those engaged in producing Daguerreotype results.

In October last, MM. Foucault and Fizeau having made a communication to the French Academy of Sciences to a similar effect, and M. Edmond Becquerel, in criticising their results, having referred to me as the original author of the fact, I may on this occasion be excused for offering a few observations on this, which perhaps is destined to become one of the most important phænomena in relation to the chemical action of the sun-light.

That the opposite ends of the solar spectrum possess opposite qualities is an idea which, far from originating with me, has been floating among chemists for many years. The first distinct statement with which I am acquainted in relation to it occurs in a work published by Mr. B. Wilson, the second edition of which dates as early as 1776. It is entitled "A Series of Experiments on Phosphori." He shows that it is the more refrangible rays which excite the phosphorescence of sulphuret of lime, *but the less refrangible ones extinguish it when shining.*

* Communicated by the Author.

In 1801 Ritter found that chloride of silver which had been blackened in the violet rays, had its colour partially restored when placed in the red. He states also that phosphorus, which is oxidized with the production of fumes in the invisible red, is instantly extinguished in the violet.

The well-known experiments of Wollaston with guaiacum served to show the opposite relations of the red and violet rays. It is remarkable, that he subsequently abandoned this interpretation of the phænomenon, on discovering that green guaiacum changed its colour by the application of a hot silver spoon.

In 1839 Sir J. Herschel encountered the same action in the case of some of the preparations of silver. His first idea was that of a positive and negative polarity of the spectrum; but this was subsequently modified for the reasons set forth in his memoir*.

From the time when I examined the Daguerreotype spectrum in Virginia in 1842, I have never doubted the actual existence of these negative or protecting rays; and on this occasion, when that existence is re-asserted by Lerebours, Fizeau and Foucault, I will make known certain new facts, premising that I do not think the views taken by M. Becquerel are correct. They are founded on what seems to me to be a misapprehension of the phænomenon of the Daguerreotype.

A Daguerreotype plate can exhibit three different varieties of surface:—1st, a black aspect on those regions where it has been unaffected by light; 2nd, various shades of white; 3rd, a coloured blackness, the tint of which may be of a deep watch-spring, or sometimes of an olive shade. Persons familiar with the process will understand completely what I mean. The first of these conditions is represented in the deep shadows of such a photograph, the places where the light never acted; the second is exhibited in the various intensities of whiteness, which constitute the figures of the picture, the whiteness varying in intensity according to the intensity of the light; the third is the solarized or overdone condition, which arises from too long an exposure to the rays. Like the first this may be spoken of as a blackness, but in reality it is a dark green, or blue, or tawny tint. It is this solarized condition of surface which M. Becquerel confounds with the first, the blackness arising from the unchanged state; and it is precisely on this point that the whole argument turns. For the sake of having distinctive words to mark out these three conditions, I will call the first the unaffected state, the second the white state, and the third the solarized state.

* Phil. Trans. 1840, § 60, &c.

The observations I made in Virginia were as follows. That if a solar spectrum was received on a Daguerreotype plate on which a weak daylight was simultaneously acting, the red, orange, yellow, green, and part of the blue rays, arrested the action of the daylight on that portion of the plate on which they fell, and maintained it in the unaffected state; whilst the residue of the blue, the indigo and violet, carried their part of the plate to a completely solarized condition. This therefore seemed to justify the assertion, that the less refrangible rays *protect* Daguerre's preparation from the action of a diffused daylight.

It was also found that if the plate was exposed to the daylight for a few seconds, so that had it been then mercurialized it would have whitened uniformly all over, on being made to receive the spectrum the less refrangible rays actually carried it back to the unaffected condition, reversing what had been already done. Whilst the more refrangible rays were forcing it on to the solarized state, these were returning it into the condition of shadow: they therefore not only *protect*, but seem even to exert a *negative* or antagonistic action.

The following observation appears to me to be in contrast with the beautiful explanation which Sir J. Herschel gives of the phænomena of one of these specimens, and moreover to have a very significant meaning. I found that it was perfectly immaterial whether the exposure to the spectrum was for thirty seconds or one hour—the result was the same. The final action had been produced, the less refrangible rays had carried their region to the unaffected state, while the more refrangible had solarized theirs. Now if the phænomenon was due, as M. Becquerel supposes, to an unequal action of the same kind in the different rays, it is obvious that the final result ought to depend on the time of exposure; the red ray, aided by the daylight, should carry its portion through the various shades of white, and solarize it at last. But this in the longest exposure never takes place; that part of the plate remains as though a ray of light had never fallen upon it.

Such are the facts I observed, and they seem to have been reproduced by M. Foucault and Fizeau; but there are also others of a much more singular nature. In these Virginia specimens *the same protecting action reappears beyond the violet.*

The only impressions in which I have ever seen this protecting action beyond the violet, are those made in Virginia in 1842; they were made in the month of July. Struck with this peculiarity, on my return to New York the following August I made many attempts to obtain similar specimens, but in no in-

Phil. Mag. S. 3. Vol. 30. No. 199. Feb. 1847. H

stance could the extra-violet protecting action be traced, though the analogous action of the red, orange, yellow, green and blue, was perfectly given. Supposing, therefore, that the difference must be due either to impurities in the iodine or to differences in the method of conducting the experiment, I tried it again and again in every possible way. *To my surprise I soon found that the negative effect was gradually disappearing; and on Sept. 29 it could no longer be traced, except at the highest part corresponding to the yellow and green rays. In December it had become still more imperfect, but on the 19th of the following March the red and orange rays had recovered their original protective power. It seemed, therefore, that in the early part of the year a protective action had made its appearance in the red ray, and about July extended over all the less refrangible regions, and as the year went on it had retreated upwards.*

Are there then periodic changes in the nature of the sun's light? The absorptive action of the earth's atmosphere is out of the question: if that were the cause, the character of these spectrum impressions should vary with the hour of the day. Or is it not more probable that these singular phænomena rather depend on incidental changes in the experiment, such as external temperature, variations of moisture, the colour of the sky, &c.?

Under proper circumstances there is no difficulty in exhibiting the power which the less refrangible rays exert in arresting the action of the daylight: under such circumstances a Daguerreotype impression of the sun's spectrum yields all three of the varieties of surface before alluded to. The plate in the less refrangible and extreme violet region is unaffected; a narrow space of white separates these unaffected portions from the indigo and violet spaces, which are in a highly solarized state.

But a totally different result is obtained when the daylight is *not* allowed to fall on the plate, either before or during its exposure to the spectrum. Under these circumstances the rays which would otherwise protect now act on the plate and slowly whiten it. A Daguerreotype spectrum formed in *darkness* and without previous exposure to the light, exhibits a white stain over all the less refrangible regions, and bears a marked contrast to one formed under the simultaneous action of a weak daylight. For brevity I will call the former the spectrum of darkness, and the latter the spectrum of daylight. The following are some additional observations.

In the spectrum of darkness there is in the white stain a

point of maximum action. This corresponds with the maximum of protection in the spectrum of daylight.

The white stain of the spectrum of darkness is apparently narrower than the protected space in the spectrum of daylight.

Rays of luminous or of non-luminous heat projected on the darkness or daylight spectra during their formation appear to exert no kind of special influence on the result.

The white fringe which borders the solarized portion is not due to anything analogous to conduction. These chemical changes, unlike thermal changes, cannot be conducted.

By interposing between the prism and the Daguerreotype plate a convex lens of short focus, so as to intercept in succession each of the coloured rays, I threw all over the plate, while the spectrum was in the act of being impressed upon it, red, orange, yellow, &c. lights in succession; the object being to ascertain how far the impressed spectrum would change when these monochromatic rays were used along with daylight, Sir J. Herschel having previously showed in similar experiments that new phænomena arise during the conjoint action of rays*. The following are some of the observations I made; their date is Sept. 24, 1842.

The red ray when projected increases the length of the solarized portion, and also of its white extremities.

The yellow ray shortens the solarized portion.

The green ray exerts a greater action of the same kind.

The indigo ray gives a most remarkable result. It totally inverts the action of the less refrangible rays; and they solarize the plate, acting precisely in the same way that the more refrangible rays commonly do, causing it to exhibit a watch-spring lustre.

I further found that when different rays are brought to act upon each other, the result does not alone depend upon their intrinsic differences, but also on their relative intensities. Thus the green and lower half of the blue rays, when of a certain intensity, protect the plate from the action of the daylight; but if of a less intensity, they aid the daylight.

The red and orange rays, when of a certain intensity, increase the action of daylight on the plate; but if of a less intensity, they restrain it.

These facts seem to be connected with the circumstance that there is often to be traced on Daguerreotype plates a remarkable difference between the central and lateral parts of a spectrum. Thus if a line be drawn through the centre of such a spectrum and a parallel to it on one of the edges, the action at any point on the central line is the reverse of that at

* Phil. Trans. 1840, § 64.

the corresponding point on the edge. A similar remark, as respects impressions on paper, has been previously made by Sir J. Herschel.

Such are the chief facts I have observed in relation to the Daguerreotype spectrum. It would seem at first sight that their diversity is so great that we can have but little hope of reducing them to a common system of results originating in the same cause. I have however been long led to believe that the explanation is to be met with in the great and fertile principle of interference. From this point of view I regard the action of rays of every kind as being essentially positive, and that action mainly consists in impressing a vibratory movement on the atoms of the decomposing substance. It is to my mind a fact of no common significance, that in those Virginia specimens the places of maximum protection in the less and more refrangible regions fall where the lengths of the luminous waves have the extraordinary relation of 2 : 1. Then, when we also see that, before a perfect neutralization of action between two rays ensues, those rays must be adjusted in intensity to each other, does it not show that interference of some kind is going on? Again, the yellow ray is in numberless instances the ray which most completely antagonizes those at the red and violet extremes of the spectrum: to use the language of Sir J. Herschel, "this ray may be considered as marking a sort of chemical centre, a point of equilibrium, or rather a change of action in the spectrum." I cannot avoid seeing that these phænomena are connected with the remarkable fact, that the waves of red, yellow and violet light, are of lengths which correspond to 2, $1\frac{1}{2}$, 1.

If then a powerful yellow ray can hold in check a feeble violet one, and prevent it from decomposing iodide of silver, merely because their relation of length is in the proportion of $1\frac{1}{2}$ to 1, it should follow on the same principles, that a red ray acting conjointly with a violet should give rise to an increased effect, because the lengths have now become 2 : 1. And that this in reality is the case I found by direct experiment; for on projecting the red upon the violet, so that the colours should half overlap each other, I found that at the point of concurrence the plate instantly solarized, and assumed a splendid green metallic colour.

I have now explained the acceptation in which I receive the term *negative ray* as a synonym (in this instance of iodide of silver) for the yellow ray, and alluded to the mechanism which seems to be the cause of protecting action generally. Perhaps on a review of his own experiments M. Becquerel may find reason to believe that there are in reality antagonizing

actions in different parts of the spectrum; actions not limited to the Daguerreotype, but occurring in all kinds of cases. They have been met with by every one who has examined the spectrum with sensitive papers, and, in a totally different series of phenomena, M. Becquerel has himself furnished a most conclusive illustration. He shows that when sulphuret of lime and other phosphorescent bodies in a shining state are exposed to the spectrum, the more refrangible rays increase the glow, but the less extinguish it at once. What is to be made of a fact like that?

It is proper to observe, that some of the phenomena recorded in this communication which seem to be in opposition to the principle set forth are not so in reality. All reasonings founded on the decomposition of light by the prism, and the action of the prismatic spectrum on changeable surfaces, are liable to error. In a memoir which I shall soon publish "On the Production of Light by Heat," this point will be particularly dwelt on; for the present it is sufficient to say, that the only method free from these difficulties is to employ the interference spectrum formed by a ruled surface or a grate; a method which was proposed eight years ago by Sir J. Herschel with a view of getting rid of the disturbing agencies arising from the ideal coloration of glass, and which I first carried into effect in 1844 with so much success, that the resulting Daguerreotype impressions contained Fraunhofer's lines, even with microscopic minuteness. With this spectrum we avoid a far more serious difficulty than that of the ideal coloration of glass, a difficulty arising from the magnitude of the refracting faces of the prism. It is this which makes a prismatic spectrum blacken paper, made sensitive with the bromide of silver, from the red to the violet end; whereas the interference spectrum shows that the true action is confined to the more refrangible side, and stops short of the centre of the yellow space.

University, New York,
Dec. 24, 1846.

XIX. *Note to a former paper on the Theory of the Aberration of Light.* By the Rev. Professor POWELL.

SINCE the publication of my paper in a late Number of this Journal*, I have been favoured with several communications from the distinguished individuals on whose opinions I have there commented; and their friendly criticisms seem to call for a brief supplementary remark.

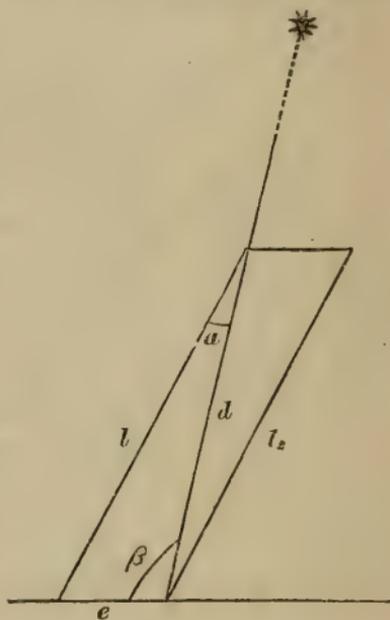
* Phil. Mag., Dec. 1846.

In the view which I gave in that paper of the explanation of the aberration, my object was to put the principle proposed by Prof. Challis in such a form as I conceived might divest it of certain difficulties which had appeared to some to attach to it.

I had in fact attempted to *combine* Prof. Challis's principle with what seemed necessary for explaining the direction of vision. But that principle is in itself quite independent of this last consideration; and the whole may thus be more distinctly expressed by keeping them separate as follows:

Supposing the same construction as in my former paper, fig. 2, then—

(1.) The observer being at the bottom of l , let the ray from the star have arrived at the top of l , which point may represent the place of the telescope-wire, or any other terrestrial object to which the star is referred, and from which rays emanate in all directions. Of these, that one which proceeds in the direction d will pursue that direction along with the ray from the star, and will in *every respect* coincide with it; and they will arrive together at the bottom of d at the same time as that at which the observer's eye arrives there.



Both objects will consequently be referred to the *same* direction, though the wire has really moved into a new position, viz. to the top of l_2 .

This is all that is essential to the idea of aberration according to Prof. Challis's view. It is the angular difference α between these two directions which is defined to be aberration.

(2.) *What* the direction is, to which the *eye* actually refers the star and the wire, is a distinct question. By some it may be regarded as altogether needless; and so I admit it is as far as the above abstract idea of aberration is concerned: yet it seems to me at least desirable to take it into account; and it may, I conceive, be best put as follows:—

While the rays come down d the telescope has moved along e parallel to itself, into the position l_2 , *independently* of the ray which had started from the wire down d : yet at every

instant of its course that ray has one point coinciding with a point in the axis of the tube in motion. Thus by composition of the two motions, this ray, though by a different *modus operandi*, comes down the axis of the telescope: whether this will, in theory, be the direction in which the eye sees both the star and wire, will depend on the consideration of a similar question to that stated in p. 433.

But there can be no doubt that in point of fact the axis of the telescope *is* the direction to which the eye (by whatever means) is led to refer the two objects seen together by rays pursuing a different route, relatively to space.

Prof. Challis has favoured me with the following additional illustration.

“Suppose two ships to be sailing due north at the same rate, and one to be due east of the other: a boat is despatched from the eastern ship to the western, and goes in a *rectilinear* course from the one to the other. If the crew of the western ship, seeing the other ship due east, and knowing that the boat came from her, concluded that the course of the boat had been exactly from east towards west, they would plainly be mistaken. It is precisely this error that the astronomer commits.”

Among some valuable remarks on the entire subject which I have received from Mr. Stokes, I learn that the very cursory statement I made as to the difference between the theoretical investigations of that gentleman and Prof. Challis, does not convey a perfectly accurate idea of the point at issue; I am therefore anxious to represent it more correctly as follows:—

Mr. Stokes at first contended that it was necessary to suppose the expression ($u dx + \&c.$) to be an exact differential, while Prof. Challis rejected that restriction. Subsequently however Prof. Challis agreed in Mr. Stokes's view: but he showed that, on that supposition, the expression for the change in the direction of the normal is such as to *allow us* to suppose the motion of the æther at the surface of the earth relatively to the earth to be of *any* amount. But Mr. Stokes differs in opinion as to the *fact*; conceiving it most probable that the æther within the earth and close to its surface is at rest relatively to it.

In my paper however I professedly avoided entering on this part of the question; and the remark just made in no way affects the other parts of my observations.

XX. Note on a Class of Differential Equations.

By GEORGE BOOLE, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following may be deemed a proper supplement to the remarks, offered in common by Mr. Hargreave and myself, in the January Number of the Philosophical Magazine on the subject of the Rev. B. Bronwin's differential equations.

In the greater number of those equations, the independent variable x only entered in the first degree. All linear equations which possess this character, whether in differentials or in finite differences, admit of a complete symbolical solution, from which the particular solutions assigned in the papers referred to, may be regarded as deductions.

Every equation, indeed, to which this characteristic belongs, may be expressed in the form

$$x \varphi(D)u + \psi(D)u = X; \quad \dots \dots \dots (1.)$$

D standing for $\frac{d}{dx}$, X being a function of x , and φ and ψ denoting arbitrary functions or combinations of the symbol to which they are affixed.

The complete solution of (1.) is

$$u = \{\varphi(D)\}^{-1} \varepsilon^{\chi(D)} x^{-1} \varepsilon^{-\chi(D)} X, \quad \dots \dots (2.)$$

the form of $\chi(D)$ being given by the equation

$$\chi(t) = \int \frac{\psi(t)}{\varphi(t)} dt.$$

The analogy which exists between the above solution and that of the linear differential equation of the first order, it is scarcely necessary to notice. It belongs to a class of subjects which have been considered in a paper on the Theory of Developments, published in the Cambridge Mathematical Journal, vol. iv. p. 214.

As a particular illustration of (2.), let us suppose that the given equation is of the form

$$x \varphi(D)u + m \varphi'(D)u = X.$$

We have

$$\begin{aligned} \chi(t) &= \int \frac{m \varphi'(t)}{\varphi(t)} dt = m \log \varphi(t), \\ \varepsilon^{\chi(D)} &= \{\varphi(D)\}^m. \quad \varepsilon^{-\chi(t)} = \{\varphi(D)\}^{-m}; \\ \therefore u &= \{\varphi(D)\}^{m-1} x^{-1} \{\varphi(D)\}^{-m} X, \end{aligned}$$

from which the solution of most of the equations considered by Mr. Bronwin will obviously follow. There are however various other cases in which the general solution is interpretable.

I remain, Gentlemen,
Your obedient Servant,

Lincoln, Jan. 1, 1847.

GEORGE BOOLE.

XXI. *Observations and Experiments respecting the Origin of the Voltaic Current.* By PROFESSOR GIANALESSANDRO MAJOCCHI*.

AFTER the discussion which took place in the last sittings, and principally in that of yesterday, respecting the origin of voltaic electricity, among some of our distinguished physicists, and particularly between the illustrious president of the section Prof. Orioli and Prof. Botto, I have been induced to extract the following paper from an essay of mine on the same subject.

At the Turin Congress in 1840, the question of the two theories, the *chemical* and that of *contact*, was agitated, with a view to explain the origin of voltaic electricity. There is no doubt that where a chemical action takes place, there is also a development of electricity; but the fluid which becomes free requires certain conditions to form a current. In like manner it is certain that, on placing two heterogeneous bodies in contact, there is a development of electricity; and in this precisely consist the fundamental facts which led Volta to the discovery of his pile; and on the same principle is founded the ingenious apparatus of insulating plates, armed with leaves of heterogeneous metals, which our colleague Prof. Marianini exhibited at the Congress of Turin, and which, variously arranged, he yesterday showed to this assembly†. The chemico-electricians maintain that the fundamental facts of Volta depend on the chemical action upon the metal, produced by the moisture of the hand, of the air, &c.; but the careful experiments instituted by Marianini, Pfaff, Belli, Peltier, and some other physicists, appear, in the development of electri-

* Translated from the *Annali di Fisica, Chimica e Matematiche*, and communicated by the Author.

The chief portion of this paper was read by the author at the Congress of Milan, Sept. 27, 1844. See *Annali*, t. xvi. p. 120; and *Atti della sesta Riunione degli Scienziati Italiani*, p. 118-119.

† It is mentioned in his work *Memorie di Fisica sperimentale*, Modena, 1838, and in another work printed at Modena under the same title in 1841, in which is contained the *Memorie sui coibenti armati che si caricano da sè col mutuo contatto d' un' armatura coll' altra*.

city by Volta's two dissimilar metals, to exclude any cause foreign to that which arises from the contact of the two bodies. Moreover, the period of thirty-three years, for which Prof. Zamboni's dry pile has now existed, confirms such a mode of electrical disturbance*.

How then does electricity by contact take place? Contact is not a force; it is the simple indication of the mode in which metals arrange themselves in the case of the voltaic experiment; and no fluid whatever, the electric for instance, can be put in motion without an agent. At the Turin Congress in 1840 I expressed my opinion, that in the so-called contact, although the action cannot be considered chemical, there is a force of attraction which is the principle of the action itself, I mean that force recognised and admitted by all, investigated by many at the close of the last century and the beginning of the present, that force, namely, which is called adhesion or attraction of simple surfaces. I shall not repeat here what I said on that occasion†; but will rather observe, that such facts have confirmed me in my opinion concerning the phenomenon of electricity in the fundamental experiments of Volta.

My purpose here is to examine the conditions necessary in the pile to the generation of the current, which is quite distinct from the simple electrical disturbance in the fundamental experiments of Volta. In a circuit, in order to give birth to the electric current, it is necessary that there should be two forces; one to emit the electric fluid in a given direction, the other to carry it on from the place where it is lodged: these two forces in Volta's pile are,—the one, adhesion, by which the direction of the current is determined; the other, the chemical action which takes place in electrolization, or in other terms, in the decomposition of the intermediate liquid by which the electricity is evolved. It matters little whether this decomposition is perceptible or imperceptible; since the polarization of two platinum electrodes intended to complete the circuit with the intermediate electrolyte, proves that, if there is no perceptible separation in the components of the liquid, there is an imperceptible one; for the polarity of the electrodes themselves cannot happen without the decomposition of the liquid in which they are immersed. The above-named conditions are verified in any voltaic arrangement, both in the original piles of Volta, and in those of Wollaston, Novellucci, Michelotti, Daniell, Grove, &c. In the fundamental experiments

* See *Annali*, t. viii. p. 14.

† See the *Annali*, t. i. p. 45; and the Proceedings of the Second Meeting of Italian Savans, p. 14.

of Volta there is no chemical decomposition, there being no intermediate electrolyte; and thence an electrical disturbance only is shown, an electricity of tension, which to be formed into a current requires the circuit to be completed with a liquid. For if this liquid does not exert any chemical action in any of the metals placed in contact, then no current is manifested on the galvanometer. Gold and platinum show an electrical disturbance on the condenser, but no current when they are immersed in nitric acid, and united with one another *metallically* by means of the wire of the galvanometer. But if some drops of hydrochloric acid are added to the nitric acid, the gold only is attacked, and immediately an electric current is manifested on the galvanometer. In the chemical action of the acid upon gold or on the zinc of the common voltaic pair, electricity is evolved; or this fluid, by virtue of that force, is separated and set free from the ponderable molecules which undergo a decomposition to form a new compound, and the adhesion between the two bodies in contact, which would produce a simple electrical disturbance, gives the discharge and determines the course in which the current of the fluid is directed precisely according to the chemical action. In the circuit of the battery between every pair, where the electricity is simply disturbed and would show only a tension, an electrolyte is placed, which is decomposed by the affinity which one of the metals of the pair exerts upon it: by this decomposition the electricity of the ponderable matter is developed and becomes free, and is emitted from the electro-negative metal into that of the electro-positive. This discharge proceeds from one pair to another, and the electric fluid set free by the chemical action is put into a continuous current along the circuit. From the first experiments of Volta, and from those which have been since instituted in various ways, it is known that the chemical action alone occasions a development of electricity, and a second force is necessary to put this fluid in motion. Moreover we know that, if this chemical action is impeded without taking away the conductivity from the circuit, the current is weakened, and is sometimes in the end entirely destroyed, not being perceptible by the most delicate galvanometers. In a voltaic circuit, where two platinum electrodes are interposed, immersed at a distance from each other in acidulated water, the hydrogen gas is deposited upon one of these, polarizes it, and generates a contrary current, which weakens or annuls the primary current, because, whilst the chemical action evolving the electric fluid still exists in the pairs of the pile, there is opposed to the impelling force, by which the current is determined, another contrary one, arising

with the polarization of the platinum electrode, which weakens or entirely annuls the effect of the first. For if the liquid is placed together with the two platinum electrodes, by which the voltaic circuit is completed, under the receiver of the air-pump where a vacuum is effected, the hydrogen gas is forced to diffuse itself by its expansibility in the same receiver, whereby the negative electrode is not polarized, and the electric current continues its course, until the gas can in such manner separate itself from the surface of the platinum, and diffuse itself in the vacuum of the air-pump.

With a view to ascertain whether this mode of explaining the phenomenon of the current in the battery has any probability and contains the cause of the same, it occurred to me to form an entirely metallic circuit, without any intermediate electrolyte, to examine if the simple chemical action was able of itself to generate a current. I shall first observe, that an homogeneous metallic wire, forming a continuous circuit and returning into itself, and perfectly identical throughout its whole extension, if heated in any part, gives rise to no electrical current; because the propagation of the caloric, proceeding uniformly from the two sides of the portion heated, gives rise to two electrical currents in an opposite direction, which become perfectly equal, and thence counteract one another, and obstruct the continuous circulation of the fluid itself. And if any obstacle, as a knot or a defect of homogeneity in the wire, renders the propagation of the caloric more difficult in one direction than in another, then one of the electrical currents which arise prevails over the other, and one results of intensity equal to the difference of the two unequal elements, which shows itself precisely on the galvanometer. In the same way, if the homogeneous metallic wire, forming the closed and re-entering circuit, is afterwards touched by a cold body in a point near the heated part, an electrical current is generated, which is indicated by the galvanometer. It seems then that the cooling produced on one side of the heated part weakens the cause from which the electric current springs, and thence gives room to the other to circulate in the wire and make its effects perceptible on the galvanometer. This case is very different from that in which the thermo-electric circuit is composed of two metals; the one positively electric and the other negatively electric; since then there are two forces from which the electric current springs in a given direction.

Following this principle, applied to the development of electricity by chemical action, I reasoned thus:—If the chemical action were able of itself to generate a current without the aid of another force, it is clear that, on forming a wholly

metallic circuit in one part of which a chemical action is by any matter produced, there ought to arise, in the same circumstances of the wire above described, an electric current. Before entering on experiments on this point, I foresaw that no current would be caused in that entirely metallic circuit, notwithstanding that from one side of the place where the chemical action would be exerted, an obstacle should be interposed by any heterogeneity in the conductor forming the circuit.

First experiment.—A strip of copper of the length of 1·30 metre, and 2 centimetres wide, is united by its extremities to the terminals of a long wire-galvanometer; afterwards with those of another with a short wire, and successively with my universal galvanometer disposed as a duplicator. By means of nitric, sulphuric, or hydrochloric acid, solutions of potass, soda or of ammonia, a chemical action was successively exerted in one part of this metallic band; but no indication of current appeared in the needle of these instruments. The experiment was made in a like manner with a similar strip of iron, zinc, and any other metal, and no current appeared which caused the needle to deviate on either of the galvanometers. In this experiment therefore the circuit was quite homogeneous, proving that the thermo-electric current originated from the different conditions of the wire.

Second experiment.—The metallic strip is compressed by means of a clamp in one place, then it bends again upon itself in many folds, being held by a clamp; then it is subjected in that place again to tempering, and then it is also hammered in a cold state: in each of these cases, operating in the same manner, a chemical action was exerted with one of the above-named materials in a portion of the strip without obtaining any indication of current on the needle of one of the galvanometers. The strip was divided into two parts, which were joined, bending back upon one another, and held fast together by the clamp: exciting afterwards a chemical action with one of the acids or the alkalis above-mentioned, there resulted no current perceptible by the galvanometer.

Third experiment.—The preceding copper strip is connected by means of the clamp with a similar one of zinc. These two strips thus united, by one of their extremities placed beneath, formed a continuous and wholly metallic circuit with the wire of the galvanometer. On causing a chemical action upon one of the strips, no current was manifested. The effect was null upon producing a chemical action on one of the metallic lists with an acid and on the other with an alkali, and also upon alternating this double action.

This third experiment shows that, although there may be disengagement of electricity in the chemical action of liquids upon metals, no current is manifested; because the other force necessary to the production of this exists indeed in the contact of the copper with the zinc; but it acts in two places in a contrary direction; whereas on the one hand the contact of the copper with the zinc tends to discharge the electricity in one direction, whilst on the other hand the contact of the same metals emits it in an opposite direction; and in this manner there are two contrary forces of adhesion, which, as far as regards disturbance of the electric equilibrium, destroy one another. Neither of them can therefore avail in setting in motion the electricity which is developed in the chemical action, by which this fluid is equally distributed throughout the metallic circuit, and in the liquid which is contained in the cup and exerts the chemical action on the metal.

Those who adopt the purely chemical theory, following De la Rive, in order to explain how the current originates in the battery of Volta at so weak a tension, admit, speaking according to the system of the dualists, that the two electricities separated by the chemical action tend to unite by their reciprocal attraction; and it is therefore that in this point, where there is a separation of the two electricities, they in fact unite again in part. The tension therefore does not become very great, because it is only the portions of the two electricities remaining separate that form themselves into a current. The neutralization or the equilibrium of the electricity evolved by chemical action, according to the followers of the pure chemical theory, does not occur at all by virtue of the action itself; whilst, according to us, this disturbance would be produced by a second force, which would be adhesion. Now those who adopt purely the theory of contact, it appears, see in the chemical action itself only a means of augmenting the points of contact of the liquid with the metal, by which the electricity finds various ways of disposing itself in a current. But how will they explain so many facts where the chemical action is manifest by the development of electricity? Moreover the simple combination of the metalloids chlorine, bromine, iodine, &c. with the metals is not accompanied by a development of the electric fluid; and it therefore seems that there is only a development of this fluid, when by affinity one of the elements of the voltaic pair tends to combine with one of the components the electrolyte or conductor of the second class.

The gas battery devised by Grove is easily explained in the above-described manner of considering the generation of

the electric current in the voltaic apparatus. For adhesion, we know, is not only manifested between solids, as the metals, but also between a solid and a liquid or aëriform fluid. And many facts of chemical reactions meet with easy explanation from such a force, by which affinity appears to be excited and thence the electrical current to arise, which determines a decomposition and a new combination. However long a time, in fact, a mixture of hydrogen and oxygen is left to itself, these gases do not combine even when placed in presence of bodies which have a great affinity for water, and which, it would seem, ought to produce their union, as for instance sulphuric acid, potass, lime and the like. We know that if a piece of platina be immersed in the mixture their combination follows on the metallic surface, and extends in certain cases to the whole aëriform mass, so as to cause the explosion of the mixture. Grove himself recognises an analogy in the phænomenon of his gas battery and that of the ordinary combination of the two gases, oxygen and hydrogen, by means of the presence of platina. The decomposition of ammoniacal gas by means of incandescent copper is one of the few instances in which the decomposition of a gaseous body is excited by a solid body. The attraction of gold for chlorine excites the decomposition of nitric acid united with hydrochloric acid when gold-leaf is placed in aqua regia; for aqua regia only contains free chlorine after being heated, or after having been left to itself for a long time. Numerous other facts of this nature have been collected by Mitscherlich*, and related in the Reports of the Royal Academy of Berlin, of December 1841, and other similar ones by Reiset and Millon communicated to the Royal Academy of Paris in June 1843. All these facts are called by them *phænomena of contact*, and Mitscherlich calls the bodies by which they are produced *contact-substances*. Berzelius has introduced a new name into science, *catalytic force*, to denote the cause of adhesion; but with the new name he has not thrown any light upon the nature of this force. We see in all these facts the adhesion of a solid for a fluid bring into activity the affinity by which, when bodies are conveniently disposed in a complete circuit, electricity is evolved, which then forms a continuous current in the mode explained.

From all that we have said it would follow that, with a single force, mechanical, chemical or physical, there is only an electrical disturbance, or the phænomena of statical electricity: thus the mechanical action of friction produces in the ordinary machine a simple disturbance of electricity; the

* See Taylor's Scientific Memoirs, Part XIII.

chemical action of an acid solution on a metal gives signs of electrical disturbance on the condenser; the physical force of magnetism produces by induction an instantaneous disturbance in the natural electrical fluid of a metallic wire, without setting it in a continuous current. But when two forces are in action, one of which is capable of disturbing the natural electricity of the ponderable matter, and the other of evolving it from the integrant molecules of the same, that fluid may be set into a continuous current in a complete circuit. Notwithstanding the appearance of truth in this mode of explaining the phenomenon of the continuous electric current, there are some facts which merit a deeper investigation, aided by experiment, before assigning it as the just cause of this phenomenon. A voltaic pair composed of two different metals, for example, gives an electric current in a given direction when it is immersed in one liquid, and the direction may be inverted when another different liquid is substituted for the first. The examination of facts of this kind will probably furnish materials for a second note.

XXII. *On the Equation of Continuity in Fluid Motion.* By ALFRED WILLIAM HOBSON, B.A. St. John's College, Cambridge*.

THE proof of this equation given by the various authors of treatises on hydrodynamics is the same in all; in fact nearly every writer since Poisson has contented himself with a mere translation of his words in art. 648 of his *Traité de Mécanique*. As however I do not remember to have seen anywhere a statement of the reasons for the assumption from which the equation is obtained, the following investigation may be interesting to some, especially as the equation itself is found by a much shorter process than the usual one, and unencumbered with several steps which are perhaps not quite satisfactory to those reading the common method for the first time.

The fluid is supposed to be divided into small portions or 'elements,' each of which is acted upon by accelerating forces X, Y, Z parallel to the coordinate axes. The first thing to be remarked is that X, Y, Z must be the same for the whole extent of the element on which they are supposed to act, *i. e.* we must take the element so small that there is no variation in either X, Y or Z in passing from any one of its parts to another. Now since these forces are considered as functions of (x, y, z) varying for any, the slightest variation in either x , y or z , it is plain that if we consider the forces *mathematically*

* Communicated by the Author.

continuous, *i. e.* as changing their value continuously for any change of x, y or z , then in order for X, Y, Z to be the same for every part of an element, that element must be also considered a *mathematical* point, having no distinction of parts at all.

In nature, however, the forces are *physically* and not *mathematically* continuous, and the elements on which they act may have an infinite range of magnitude. The reasoning by which such forces are submitted to calculation is however precisely the same as if they were mathematically continuous.

The element of fluid, then, may be of any finite magnitude below a certain limit, and will consist of a number of ultimate particles, on each of which the forces acting are the same. For the whole extent of this element X, Y, Z are the same, and it may be taken of any size consistent with this condition.

The next thing to be attended to is, that the element must not be acted upon by any *new* forces during the instant (dt). During this interval, therefore, every particle in the element is acted upon solely by the forces X, Y, Z , which vary or may vary with the time, but vary *similarly* for each of these particles. In other words, there is no *disturbing* force introduced during (dt) into the system of ultimate particles, which together make up the element we are considering; therefore there can be no disturbance of the *relative positions* of these particles. Therefore the *number of particles* composing the element remains the same during the interval (dt); and this condition expressed analytically gives us the equation of continuity.

Let ρ be the density of the element at the commencement of the time (dt); this is supposed to be the same for the whole of the element; V the volume of the element at the beginning of (dt) and V' at the end. We have then (ρ' being also the density at the end of the time (dt))

$$\rho V = \rho' V'.$$

We have now merely to express this condition in another form. The notation is the same as Poisson's. $V = (x' - x)(y' - y)(z' - z)$: where for $(x' - x)$ we may write δx , for $(y' - y)$, δy , and for $(z' - z)$, δz . We have to state in terms of the velocities (u, v, w) parallel to the coordinate axes, that the variation of $(x' - x)(y' - y)(z' - z)\rho$ is nothing during the instant (dt). Now calling the whole differential coefficient of $(x' - x)(y' - y)(z' - z)$ with regard to the time $D_t V$, and similarly for that of ρ ,

$$D_t V = (x' - x)(y' - y)(w' - w) + (x' - x)(z' - z)(v' - v) + (y' - y)(z' - z)(u' - u)$$

$$\begin{aligned}
&= \delta x \cdot \delta y \cdot \delta z \left(\frac{d\omega}{dx} \delta x + \frac{d\omega}{dy} \delta y + \frac{d\omega}{dz} \delta z \right) + \delta x \cdot \delta z \left(\frac{dv}{dx} \delta x + \frac{dv}{dy} \delta y + \frac{dv}{dz} \delta z \right) \\
&\quad + \delta y \cdot \delta z \left(\frac{du}{dx} \delta x + \frac{du}{dy} \delta y + \frac{du}{dz} \delta z \right) \\
&= \delta x \cdot \delta y \cdot \delta z \left[\frac{du}{dx} + \frac{dv}{dy} + \frac{d\omega}{dz} \right] \\
&+ \delta x \cdot \delta y \cdot \delta z \cdot \left[\frac{du}{dy} \cdot \frac{\delta y}{\delta x} + \frac{du}{dz} \cdot \frac{\delta z}{\delta x} + \frac{dv}{dx} \cdot \frac{\delta x}{\delta y} + \frac{dv}{dz} \cdot \frac{\delta z}{\delta y} + \frac{d\omega}{dx} \cdot \frac{\delta x}{\delta z} \right. \\
&\quad \left. + \frac{d\omega}{dy} \cdot \frac{\delta y}{\delta z} \right]. \\
D_t \rho &= \frac{d\rho}{dt} + u \cdot \frac{d\rho}{dx} + v \cdot \frac{d\rho}{dy} + \omega \cdot \frac{d\rho}{dz};
\end{aligned}$$

∴ we have $D_t(\rho V) = 0$, or $\rho \cdot D_t V + V \cdot D_t \rho = 0$;
or

$$\begin{aligned}
\rho \cdot \delta x \cdot \delta y \cdot \delta z \cdot \left(\frac{du}{dx} + \frac{dv}{dy} + \frac{d\omega}{dz} \right) + \rho \cdot \delta x \cdot \delta y \cdot \delta z \cdot \left(\frac{du}{dy} \cdot \frac{\delta y}{\delta x} + \frac{du}{dz} \cdot \frac{\delta z}{\delta x} + \&c. \right) \\
+ \delta x \cdot \delta y \cdot \delta z \left(\frac{d\rho}{dt} + u \cdot \frac{d\rho}{dx} + v \cdot \frac{d\rho}{dy} + \omega \cdot \frac{d\rho}{dz} \right) &= 0.
\end{aligned}$$

Now since there is no necessary connexion between δx , δy and δz , the middle term in this equation, viz. that containing the quantities $\frac{\delta y}{\delta x}$, &c., cannot affect the other two: according therefore to a well-known principle, the two parts must separately = zero.

Therefore, finally, dividing out by the common factor $\delta x \cdot \delta y \cdot \delta z$, we get the usual equation of continuity,

$$\rho \left(\frac{du}{dx} + \frac{dv}{dy} + \frac{d\omega}{dz} \right) + \frac{d\rho}{dt} + u \cdot \frac{d\rho}{dx} + v \cdot \frac{d\rho}{dy} + \omega \cdot \frac{d\rho}{dz} = 0;$$

or

$$\frac{d\rho}{dt} + \frac{d(\rho u)}{dx} + \frac{d(\rho v)}{dy} + \frac{d(\rho \omega)}{dz} = 0.$$

It is obvious that this equation only requires that the *number* of particles in the element should remain unaltered during (dt); but the mode in which it is obtained implies, not only an identity in the *number* of particles at the beginning and end of (dt), but an identity of *the particles themselves*. This observation has also been made by Poisson, whose words are (art. 651), “c’est pour abréger que l’on a considéré le volume de cette partie du fluide comme infiniment petit; et si l’on divise

le volume total en parties de grandeur finie, mais insensible, dont chacune renferme néanmoins un nombre extrêmement grand de molécules l'équation [of continuity] exprime réellement que chacune de ces parties renferme toujours les mêmes molécules, et que par conséquent sa masse est invariable."

In all cases where new forces are being continually introduced, as in the flowing of rivers or of water through pipes of variable form, it is impossible to calculate the motion, and at the same time the equation of continuity is no longer applicable. But, on the other hand, there are cases in which this equation holds, where nevertheless the other three equations of motion do not hold, and in fact where there may actually occur a split or division in the fluid. As an instance may be mentioned the flowing of a stream of water over the edge of a perpendicular precipice, where gravity suddenly comes into action. In this case the number of particles may remain the same in an element which may be conceived to consist of particles that have fallen over as well as of those that remain. But the equation of motion would require the new force of gravity to be taken into account.

XXIII. On the Integration of some Equations in Partial Differentials. By the Rev. BRICE BRONWIN*.

IN the present paper I shall extend the mode of integrating certain differential equations, which I gave in this Journal in December last, to the integration of similar equations in partial differentials. Let D stand for $\frac{d}{dx}$, D' for $\frac{d}{dy}$; and as a first example, let

$$x\left(\frac{d^2z}{dx^2} - a^2\frac{d^2z}{dy^2}\right) + 2p\frac{dz}{dx} = 0,$$

where p , as throughout the paper, is a positive integer. Make

$$aD'\sqrt{-1} = k,$$

then
$$x\left(\frac{d^2z}{dx^2} + k^2z\right) + 2p\frac{dz}{dx} = 0.$$

This is the first example in the paper above referred to, where by making $z = (D^2 + k^2)^p u$, it is reduced to

$$\frac{d^2u}{dx^2} + k^2u = 0;$$

or, restoring the suppressed factor, to

$$(D^2 + k^2)^p x(D^2 + k^2)u = 0.$$

* Communicated by the Author.

Therefore

$$x(D^2 + k^2)u = (D^2 + k^2)^{-p}0 = (D + k\sqrt{-1})^{-p}(D - k\sqrt{-1})^{-p}0 = \varepsilon^{-xk\sqrt{-1}} D^{-p} \varepsilon^{2xk\sqrt{-1}} D^{-p} \varepsilon^{-xk\sqrt{-1}} 0 = \varepsilon^{-xk\sqrt{-1}} D^{-p} \varepsilon^{2xk\sqrt{-1}} (A + A_1x \dots + A_{p-1}x^{p-1}).$$

We must now put $aD'\sqrt{-1}$ for k , and change $A, A_1, \&c.$ into functions of y . Then

$$\varepsilon^{2xk\sqrt{-1}} A = \varepsilon^{-2axD'} f(y) = f(y - 2ax), \&c.,$$

and

$$x(D^2 + k^2)u = \varepsilon^{axD'} D^{-p} \{ f(y - 2ax) + x f_1(y - 2ax) \dots + x^{p-1} f_{p-1}(y - 2ax) \} = \varepsilon^{axD'} \{ \varphi(y - 2ax) + x \varphi_1(y - 2ax) \dots + x^{p-1} \varphi_{p-1}(y - 2ax) + \psi(y) + x \psi_1(y) \dots + x^{p-1} \psi_{p-1}(y) \},$$

where $\psi(y), \psi_1(y), \&c.$ are the arbitraries of the integrations. And here I must observe, that the functions denoted by $\varphi, \varphi_1, \psi, \psi_1, \&c.$ are supposed to be changed when necessary, although the same symbols are still retained to represent them.

Operating upon the last result with

$$(D^2 + k^2)^{p-1} x^{-1} = (D + aD')^{p-1} (D - aD')^{p-1} x^{-1} = \varepsilon^{-axD} D^{p-1} \varepsilon^{2axD'} D^{p-1} \varepsilon^{-axD'} x^{-1},$$

we have

$$z = \varepsilon^{-axD'} D^{p-1} \varepsilon^{2axD'} \{ x^{-p} \varphi(y - 2ax) + x^{-p+1} \varphi_1(y - 2ax) \dots + x^{p-2} \varphi_{2p-2}(y - 2ax) + x^{-p} \psi(y) \} = \varepsilon^{-axD'} D^{p-1} \{ x^{-p} \varphi(y) + x^{-p+1} \varphi_1(y) \dots + x^{p-2} \varphi_{2p-2}(y) + x^{-p} \psi(y + 2ax) \} = x^{-2p+1} \{ \varphi(y - ax) + \psi(y + ax) \} + x^{-2p+2} \{ \varphi_1(y - ax) + \psi_1(y + ax) \} \dots + x^{-p} \{ \varphi_{p-1}(y - ax) + \psi_{p-1}(y + ax) \}.$$

Substituting this value in the given equation, we find $\varphi_1, \varphi_2, \&c.$, and $\psi_1, \psi_2, \&c.$ to be the successive differential coefficients of φ and ψ ; and ultimately

$$z = x^{-2p+1} \{ \varphi(y - ax) + \psi(y + ax) \} + ax^{-2p+2} \{ \varphi'(y - ax) - \psi'(y + ax) \} + a^2 \frac{p-2}{2p-3} x^{-2p+3} \{ \varphi''(y - ax) + \psi''(y + ax) \} + \&c.,$$

to the term containing x^{-p} inclusive.

As a second example, let

$$x \left(\frac{d^2 z}{dx^2} + 2 \frac{d^2 z}{dx dy} + 2 \frac{d^2 z}{dy^2} \right) + 2p \left(\frac{dz}{dx} + \frac{dz}{dy} \right) = 0.$$

Or if k be put for D' ,

$$x \left(\frac{d^2 z}{dx^2} + 2k \frac{dz}{dx} + 2k^2 z \right) + 2p \left(\frac{dz}{dx} + k z \right) = 0.$$

Making $z = (D^2 + 2kD + 2k^2)^p u$,

this, by the paper before referred to, reduces to

$$(D^2 + 2kD + 2k^2)u = x^{-1}(D^2 + 2kD + 2k^2)^{-p}0,$$

after restoring the suppressed factor and dividing by it.

$$\text{If } 1 + \sqrt{-1} = \alpha, \quad 1 - \sqrt{-1} = \beta,$$

$$\text{we have } D^2 + 2kD + 2k^2 = (D + \alpha k)(D + \beta k).$$

But

$$(D + \alpha k)^{-p} (D + \beta k)^{-p} 0 = \varepsilon^{-\alpha x k} D^{-p} \varepsilon^{2x k \sqrt{-1}} D^{-p} \varepsilon^{\beta x k} \\ 0 = \varepsilon^{-\alpha x k} D^{-p} \varepsilon^{2x k \sqrt{-1}} \{A + A_1 x \dots + A_{p-1} x^{p-1}\}.$$

Now, putting D' for k , and $f(y)$ for A , &c., we have

$$x^{-1} (D + \alpha k)^{-p} (D + \beta k)^{-p} 0 = x^{-1} \varepsilon^{-\alpha k x} D^{-p} \{f(y + 2x \sqrt{-1}) \\ + x f_1(y + 2x \sqrt{-1}) \dots + x^{p-1} f_{p-1}(y + 2x \sqrt{-1})\} \\ = x^{-1} \varepsilon^{-\alpha k x} \{\varphi(y + 2x \sqrt{-1}) + x \varphi_1(y + 2x \sqrt{-1}) \dots \\ + x^{p-1} \varphi_{p-1}(y + 2x \sqrt{-1}) + \psi(y) + x \psi_1(y) \dots \\ + x^{p-1} \psi_{p-1}(y)\} = \varepsilon^{-\alpha x k} \{x^{-1} \varphi(y + 2x \sqrt{-1}) + \varphi_1 \\ (y + 2x \sqrt{-1}) \dots + x^{p-2} \varphi_{p-1}(y + 2x \sqrt{-1}) + x^{-1} \psi(y) \\ + \psi_1(y) \dots + x^{p-2} \psi_{p-1}(y)\},$$

where ψ , ψ_1 , &c. are introduced as the arbitraries of the integration.

From the last result we easily see that

$$z = \varepsilon^{-\alpha x k} D^{p-1} \varepsilon^{2x k \sqrt{-1}} D^{p-1} \varepsilon^{-2x k \sqrt{-1}} \{x^{-1} \varphi(y + 2x \sqrt{-1}) + \&c. \\ + x^{-1} \psi(y) + \&c.\} = \varepsilon^{-\alpha x k} D^{p-1} \varepsilon^{2x k \sqrt{-1}} D^{p-1} \{x^{-1} \varphi(y) + \&c. \\ + x^{-1} \psi(y - 2x \sqrt{-1}) + \&c.\} = \varepsilon^{-\alpha x k} D^{p-1} \varepsilon^{2x k \sqrt{-1}} \{x^{-p} \varphi(y) \\ + x^{-p} \psi(y - 2x \sqrt{-1}) + x^{-p+1} \psi_1(y - 2x \sqrt{-1}) \dots \\ + x^{p-2} \psi_{2p-2}(y - 2x \sqrt{-1})\} = \varepsilon^{-\alpha x k} D^{p-1} \{x^{-p} \varphi \\ (y + 2x \sqrt{-1}) + x^{-p} \psi(y) + x^{-p+1} \psi_1(y) \dots + x^{p-2} \psi_{2p-2}(y)\} \\ = \varepsilon^{-\alpha x k} \{x^{-2p+1} \varphi(y + 2x \sqrt{-1}) + x^{-2p+2} \varphi_1(y + 2x \sqrt{-1}) \dots \\ + x^{-p} \varphi_{p-1}(y + 2x \sqrt{-1}) + x^{-2p+1} \psi(y) + x^{-2p+2} \psi_1(y) \dots \\ + x^{-p} \psi_{p-1}(y)\} = x^{-2p+1} \{\varphi(y - \beta x) + \psi(y - \alpha x)\} + x^{-2p+2} \\ \{\varphi_1(y - \beta x) + \psi_1(y - \alpha x)\} + \dots \&c.$$

In these reductions the values of α and β have sometimes been put for these quantities, and k has been retained as more convenient than D' .

As in the last example, by substitution in the proposed, we find

$$z = x^{-2p+1} \{ \phi(y - \epsilon x) + \psi(y - \alpha x) \} + x^{-2p+2} \{ a\phi'(y - \epsilon x) + b\psi'(y - \alpha x) \} + x^{-2p+3} \{ a_1\phi''(y - \epsilon x) + b_1\psi''(y - \alpha x) \} + \dots$$

to the term containing x^{-p} inclusive. The constant coefficients $a, a_1, \&c., b, b_1, \&c.$ are easily determined, and could not be conveniently put down.

Let
$$x^2 \frac{d^2 z}{dx^2} + h \frac{dz}{dx dy} - p(p+1)z = 0.$$

Make $k = hD'$, and this becomes

$$x^2 \frac{d^2 z}{dx^2} + k \frac{dz}{dx} - p(p+1)z = 0.$$

If $z = D^{p-1}u$, by a paper of mine in the Mathematician for November last, this reduces to

$$x^2 \frac{du}{dx} + (k - 2px)u = 0,$$

or rather

$$x^2 \frac{du}{dx} + (k - 2px)u = D^{-p}0 = A + A_1x \dots + A_{p-1}x^{p-1}.$$

Integrating

$$\epsilon^{-\frac{k}{x}} x^{-2p} u = \int dx x^{-2p-2} \epsilon^{-\frac{k}{x}} (A + A_1x \dots + A_{p-1}x^{p-1}).$$

Restoring the value of k , and changing $A, A_1, \&c.$ into $f(y), f_1(y), \&c.$, we have

$$\begin{aligned} u &= x^{2p} \epsilon^{\frac{h}{x} D'} \int dx x^{-2p-2} \left\{ f\left(y - \frac{h}{x}\right) + x f_1\left(y - \frac{h}{x}\right) \dots \right. \\ &\quad \left. + x^{p-1} f_{p-1}\left(y - \frac{h}{x}\right) \right\} = x^{2p} \epsilon^{\frac{h}{x} D'} \left\{ x^{-2p} \phi\left(y - \frac{h}{x}\right) \right. \\ &\quad \left. + x^{-2p+1} \phi_1\left(y - \frac{h}{x}\right) \dots + \phi_{2p}\left(y - \frac{h}{x}\right) + \psi(y) \right\} \\ &= \phi(y) + x\phi_1(y) \dots + x^{2p} \phi_{2p}(y) + x^{2p} \psi\left(y + \frac{h}{x}\right). \end{aligned}$$

We must sometimes change the order of the terms, as in this example; for if in substituting in the given equation we begin at the wrong end of the series, we shall have $\phi, \int \phi dy, \iint \phi dy^2, \&c.$ instead of $\phi, \phi', \phi'', \&c.$

Thus

$$\begin{aligned} z = D^{p-1}u &= x^{p+1} \phi(y) + x^p \phi_1(y) \dots + \phi_{p+1}(y) + x^{p+1} \psi\left(y + \frac{h}{x}\right) \\ &\quad + x^p \psi_1\left(y + \frac{h}{x}\right) \dots + x^2 \psi_{p-1}\left(y + \frac{h}{x}\right). \end{aligned}$$

Substituting this value in the given equation, we find

$$z = x^{p+1} \left\{ \phi(y) + \psi\left(y + \frac{h}{x}\right) \right\} + x^p \left\{ a\phi'(y) + b\psi'\left(y + \frac{h}{x}\right) \right\} +$$

&c., the series in ϕ , ϕ' , &c. stopping at x^0 , that in ψ , ψ' , &c. at x^2 ; the values of the constants a , a_1 , &c., b , b_1 , &c. are not put down, for the same reason as in the last example.

If in this last example we make $k = PD' + Q$, P and Q being functions of y , the equation to be integrated will be

$$x^2 \frac{d^2 z}{dx^2} + P \frac{d^2 z}{dx dy} + Q \frac{dz}{dx} - p(p+1)z = 0,$$

which by $z = D^{p-1}u$ reduces to

$$x^2 \frac{du}{dx} + P \frac{du}{dy} + (Q - 2px)u = f(y) + x f_1(y) \dots + x^{p-1} f_{p-1}(y),$$

which is only of the first order. We must not proceed in this case as heretofore, but must expand

$$\varepsilon^{\pm \frac{k}{x}} = \varepsilon^{\pm} \left(\frac{PD' + Q}{x} \right)$$

by the powers of $\frac{PD' + Q}{x}$, if we wish to integrate as in the preceding examples. Thus

$$\varepsilon \frac{PD' + Q}{x} f(y) = \left\{ 1 + \frac{PD' + Q}{x} + \frac{1}{2} \left(\frac{PD' + Q}{x} \right)^2 + \&c. \right\}$$

$$f(y) = f(y) + x^{-1} (PD' + Q) f(y) + \frac{x^{-2}}{2} (PD' + Q) (PD' + Q) f(y) + \&c.,$$

the powers, after the expansion, being changed into successive operations. In this case we might not obtain a solution in finite terms.

Sometimes it may be desirable to proceed with D' in the same manner as with D . Let

$$x \left(\frac{d^2 z}{dx^2} + \frac{d^2 z}{dx dy} \right) + \frac{dz}{dx} = 0;$$

$$\text{or} \quad x D^2 z + x D D' z + D z = 0.$$

But, as in the papers referred to,

$$x D^2 z = D^2 x z - 2 D z, \quad x D D' z = D D' x z - D' z.$$

$$\text{Therefore} \quad D^2 x z + D D' x z - D z - D' z = 0;$$

$$\text{or} \quad (D + D') (D x z - z) = 0;$$

or

$$D x z - z = (D + D')^{-1} 0 = \varepsilon^{-x D'} D^{-1} \varepsilon^{+D'} 0 = \varepsilon^{-x D'} f(y) = f(y - x);$$

that is
$$x \frac{dz}{dx} = f(y-x),$$

and therefore
$$z = \int \frac{dx}{x} f(y-x) + \phi(y).$$

Sometimes we may succeed by partially adopting these processes. Let

$$x \frac{d^2z}{dx^2} + (xy+1) \frac{d^2z}{dx dy} + y \frac{d^2z}{dy^2} + \frac{dz}{dy} = 0;$$

or
$$x D^2 z + (xy+1) D D' z + y D'^2 z + D' z = 0.$$

Now
$$y D' z = D' y z - z,$$

therefore

$$x y D D' z = x D D' y z - x D z, \quad y D'^2 z = D'^2 y z - 2 D' z.$$

Therefore by substitution

$$x D^2 z + x D D' y z + D D' z + D'^2 y z - x D z - D' z = 0.$$

Or
$$(x D + D')(D z + D' y z - z) = 0,$$

which may be put under the forms

$$(x D + D')(D + y D') z = 0;$$

or
$$(D + x^{-1} D')(D + y D') z = 0.$$

Make
$$(D + y D') z = u;$$

then
$$(D + x^{-1} D') u = 0, \text{ and } u = x^{-D'} f(y).$$

Therefore

$$(D + y D') z = x^{-D'} f(y) = \epsilon^{-lx D'} f(y) = f(y-lx);$$

which gives
$$z = \epsilon^{-xy D'} \int dx \epsilon^{xy D'} f(y-lx).$$

It must be remembered that

$$\epsilon^{\pm xy D'} = 1 \pm x(y D') + \frac{x^2}{2} (y D')^2 \pm \&c.$$

$$= 1 \pm xy D' + \frac{x^2}{2} y D' y D' \pm \&c.,$$

the powers being converted into successive operations.

Sometimes an equation may be reduced an order lower, or to a more simple form of the same order, by these transformations, as in the following example.

Let
$$\frac{d^3z}{dx^3} + hx^2 \frac{d^3z}{dx^2 dy} - 2h \frac{dz}{dy} = 0.$$

Make $z = (D + hx^2 D') u$, and the above will reduce to

$$\begin{aligned} \frac{d^3 u}{dx^3} + hx^2 \frac{d^3 u}{dx^2 dy} + 4hx \frac{d^2 u}{dx dy} &= (D + hx^2 D')^{-1} 0 \\ &= \epsilon^{-\frac{hx^3}{3} D'} D^{-1} \epsilon^{\frac{hx^3}{3} D'} 0 = \epsilon^{-\frac{hx^3}{3} D'} f(y) = f\left(-\frac{hx^3}{3}\right). \end{aligned}$$

If $\frac{du}{dx} = v$, the last equation becomes

$$\frac{d^2 v}{dx^2} + hx^2 \frac{d^2 v}{dx dy} + 4hx \frac{dv}{dy} = f\left(y - \frac{hx^3}{3}\right).$$

Then

$$z = (D + hx^2 D') u = \varepsilon^{-\frac{hx^3}{3} D'} D \varepsilon^{\frac{hx^3}{3} D'} (f v dx + \phi(y)).$$

Examples might be given of reduction to a more simple form of the same order.

The omission of the factors

$$x^{-1} (D^2 + k^2)^{-p}, x^{-1} (D^2 + 2kD + 2k^2)^{-p}, \&c.$$

before the cipher of the second member of the equations treated of in my paper in the Number of this Journal for December last was an oversight, which I did not discover till I received the January Number, notwithstanding the present paper was composed immediately after it.

Gunthwaite Hall, Near Barnsley, Yorkshire,
Jan. 12, 1847.

XXIV. Mr. JOHN SOUTHERN'S *Experiments on the Density, Latent Heat and Elasticity of Steam**.

To Richard Taylor, Esq.

DEAR SIR,

IN the various statements which have been published and re-published from time to time respecting the nature and properties of steam, the experimental investigations of Mr. John Southern have not become so generally known as they deserve to be, and if you can find space in your Magazine for reprinting his papers they would convey useful information to your readers.

Mr. Southern was an able mathematician †, who was retained by Mr. Watt in the manufactory of steam-engines at Soho, for the purpose of assisting him in investigating the best proportions of the various parts of those machines, and calculating the requisite dimensions which (according to the ascertained strength of the materials employed) would give

* Several important recent investigations of this subject will be found in the 4th Volume of Taylor's Scientific Memoirs, just completed, among which may be mentioned Holtzmann on the Heat and Elasticity of Gases and Vapours, and on the principles of the Theory of Steam-Engines: Magnus on the Expansive Force of Steam; and on the Force requisite for the production of Vapours; and Regnault on the Elastic Forces of Aqueous Vapour.—Ed.

† Mr. Southern contributed two papers to the early volumes of the Philosophical Magazine. Vol. xi. p. 97, and xvii. 120.

security against breaking, without any unnecessary weight. An experimental inquiry into the properties of steam was necessarily a part of such a course, and Mr. Southern stated the results he had obtained in a letter to Mr. Watt, which was printed by Dr. Brewster in his new edition of the articles written by Dr. Robison for the *Encyclopædia Britannica*. The article Steam-engine in that edition was revised by Mr. Watt, in 1814, and he added some notes and an appendix, which contains Mr. Southern's letter.

I am, dear Sir,

67 Guildford Street, Russell Square, Yours very sincerely,
London, Jan. 16, 1847. JOHN FAREY.

Letter from Mr. Southern to Mr. Watt.*

The experiments, of which the particular circumstances are hereafter related, were made in 1803, with the view of ascertaining, chiefly, the density of steam raised from water under different pressures above that of the atmosphere, an apparatus having then been made for a different purpose, which seemed pretty well adapted to this object, as it did equally so to that of ascertaining the latent heat of steam.

It may be premised, that the thermometers employed in all the experiments which will be now related, were made and graduated with the greatest care, the tubes having been accurately measured as to the proportional capacity of their different parts, the boiling-point of each ascertained, according to the rules prescribed by a committee of the Royal Society, in 1777, (viz. the bulbs and tubes being in steam when the barometer stood at 29·8 inches, this degree of temperature being called 212°,) and in all cases the bulb and the tube, as high as the mercury ascended in it, were kept in the steam or the water whose temperature was to be noted. This latter circumstance was effected in the case of steam, by sliding the tube of the thermometer through a stuffing-box, or collar, made tight, till the mercury in it could just be seen above it. The tube had known marks on it, from which measurements were taken to the mercury, and thence the temperature known.

The quantity of steam was measured by filling a cylinder with it (inclosed in the steam), whose diameter was about 3·16 inches, and driving it out by the motion of a piston, which had 18 inches stroke regulated by the rotation of a crank. The solid contents of the piston-rod, which was 0·86 inch diameter, diminished the contents of the cylinder, leaving the quantity discharged each stroke by the motion of the piston,

* In all the experiments, of which an account is given in this letter, Mr. Southern was assisted by Mr. William Creighton.

very nearly 130·7 cubic inches; but as the piston did not rise high enough to touch the top that closed the cylinder, and there was also unavoidably a space between the valve and the cylinder, these spaces together were computed to equal 1·7 cubic inch. Of course, had the elasticity of the steam been just equal to that of the atmosphere, no addition to the 130·7 cubic inches would need to be made; but as in the three successive experiments it was about $\frac{1}{3}$, $\frac{5}{3}$, and $\frac{9}{3}$ greater, these proportions of the spaces would escape when the valve was open that allowed the discharge of the steam to be made into the atmosphere, and must therefore be added respectively to the contents discharged by the motion of the piston.

These additional quantities are $1\cdot7 \times \frac{1}{3} = \cdot57$; $1\cdot7 \times \frac{5}{3} = 2\cdot83$; and $1\cdot7 \times \frac{9}{3} = 5\cdot1$; which, added to 130·7, gives 131·27 in the first experiment; 133·53 in the second, and 135·8 in the third, for the quantity of steam discharged at each stroke of the piston; and therefore the number of strokes which would discharge one cubic foot in each of the three experiments, would be 13·164, 12·941, and 12·724, respectively.

The steam was conducted from the cylinder, after passing the valve, by means of an iron pipe attached to a small copper one, having its end bent down, and immersed a short depth into a cistern of water. The cistern was made of fir-wood, and painted inside and outside with white paint; was about 30 inches square, and 26 inches deep; and the quantity of water in it was ascertained by weighing it, as was also the accession to it by the condensed steam.

The elasticity of the steam was ascertained by measuring an actual column of mercury which it supported; and the number of strokes was ascertained by a machine called a counter.

The following table contains the principal facts of these experiments:

Number of the experiment.	Duration of the experiment in minutes.	Whole number of strokes.	Number of strokes per minute.	Weight of water in cistern at beginning.	Temperature of ditto.	Weight of water gained by condensed steam.	Temperature of water in cistern at the end.	Temperature gained.	Elasticity of steam in bo'er.	Temperature of ditto.
I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.
1	12 $\frac{1}{3}$	5154	42·3	lb. 721 $\frac{3}{4}$	45 $\frac{0}{3}$	lb. 20·25	76 $\frac{0}{3}$	30 $\frac{0}{3}$	Inches merc. 40	229 $\frac{0}{3}$
2	51 $\frac{1}{2}$	2434	41 $\frac{1}{4}$	722	48	20·00	80 $\frac{3}{4}$	32 $\frac{1}{4}$	80	270
3	38 $\frac{1}{4}$	1599	41·8	722	48	19·45	79 $\frac{3}{4}$	31 $\frac{3}{4}$	120	295

If the whole number of strokes in each experiment be divided by the number, found as above, that were required to discharge one cubic foot of steam, the whole number of cubic feet of steam discharged in each experiment will be given; viz. $5154 \div 13.164 = 391.53$; $2434 \div 12.941 = 188.09$; and $1599 \div 12.724 = 125.66$; the quantity of steam formed and discharged in the first, second and third experiments respectively, in cubic feet.

If the weight of water gained by the condensation of steam in each experiment be multiplied by 27.65, the number of cubic inches of water in a pound weight, and divided by the number of cubic feet of steam which were condensed, the quotient will give the portion of water, in cubic inches, required by each cubic foot of steam for its formation; and hence also the comparative density.

Thus $20.25 \times 27.65 \div 391.53 = 1.430$ } inches of water to
 $20.00 \times 27.65 \div 188.09 = 2.940$ } form each cubic
 $19.45 \times 27.65 \div 125.66 = 4.279$ } foot of steam;

and these numbers are proportional to $\left\{ \begin{array}{l} 40.00 \\ 82.24 \\ 119.70 \end{array} \right\}$ the relative

densities, while the elasticities were as $\left\{ \begin{array}{l} 40 \\ 80 \\ 120 \end{array} \right\}$ respectively.

These results appear to support the conclusion that *the density of steam is nearly, if not accurately, proportional to its elasticity*; at least this may be affirmed of it within the limits of these experiments.

From the above experiments may be calculated the latent heat of steam developed in the three cases; for if the weight of the water which received the augment of heat, be multiplied by the number of degrees of temperature communicated to it, and if this product be divided by the accession of weight to the water (which only could have communicated the accession of temperature), it is evident that the quotient will give the temperature which the steam lost; and if to this be added the temperature which it retained (viz. that of the water in the cistern at the conclusion of the experiment), a number will be obtained showing the *whole heat*, or the *sum* of the latent and sensible heat of the steam. Hence, by subtracting the sensible heat of the steam from this sum, the latent heat will

be found. That is, $\frac{\text{col. 5} \times \text{col. 9}}{\text{col. 7}} + \text{col. 8} =$ the sum of the latent and sensible heat; or,

if W = weight of cold water, T = its temperature
 w = accession of water by the condensed steam,
 t = the temperature of warm water, and
 x = the sum of the latent and sensible heat of the
 steam condensed,

$$\frac{W + w \cdot t - W T}{w} + x.$$

Either of these equations will be found to give in the three experiments 1157° , 1244° , and 1256° , from which subtract the numbers, col. xi. 229° , 270° , and 295° , and there remain 928° , 974° , and 961° , the latent heat.

Three other experiments were instituted with the intention of ascertaining the latent heat of steam under the three same degrees of elasticity, viz. equal to the support of 40, 80 and 120 inches of mercury. The steam was raised or generated in the same boiler used in the previous experiments, and from the end of a cast-iron pipe of $1\frac{1}{2}$ inch diameter which united with it, a small copper pipe was taken (its diameter about $\frac{5}{16}$ inch) and bent down so that its end could conveniently be immersed an inch or two under the surface of the water. The end of this pipe was closed by a thin disc of copper, in which a circular hole was made $\frac{5}{1000}$ th of an inch diameter, through which the steam from the boiler was blown into the cold water. The water which received the heat was contained in a tinned iron vessel that weighed 3.77 lbs., and its capacity for heat may therefore be called equivalent to $\frac{1}{2}$ lb. of water. In each of the experiments, the water employed to receive the steam weighed 28 lbs., to which, in the following table, recording the principal facts, is added this $\frac{1}{2}$ lb., in lieu of the vessel.

Number of experiment.	Duration of the experiment.	Weight of cold water.	Temperature of cold water.	Temperature at the end of experiment.	Temperature gained.	Weight of water gained.	Temperature of steam.	Elasticity of steam.
1	12,45"	lbs. 28 $\frac{1}{2}$	48	80	32	lbs. ,878	229	Inches. 40
2	5,50	28 $\frac{1}{2}$	48	81 $\frac{1}{3}$	33 $\frac{1}{3}$,857	270	80
3	4,00	28 $\frac{1}{2}$	47 $\frac{3}{4}$	81	33 $\frac{1}{4}$,826	295	120

If either of the equations above be applied here to the facts noted in this table, the sum of the latent and sensible heat will come out 1119° , 1190° and 1228° ; and the latent heat 890° , 920° and 933° . It was observed, however, that the tin vessel lost heat to the surrounding air very sensibly, and an experiment was made to determine the amount of this effect; and it

was found when the contained water was at 80° , 1° was lost in five minutes; when at 60° , 1° was lost in ten and a half minutes; it would therefore probably lose 1° in eight minutes during the time of an experiment, the mean temperature being about 65° ; and as the excess of temperature at the beginning and end of an experiment above that of the air was nearly the same in all three, the loss would be nearly proportional to the duration of each. Hence, to the acquired heat should be added, in the first experiment, $1\frac{5}{8}^{\circ}$; in the second, $\frac{3}{4}^{\circ}$; and in the third, $\frac{1}{2}^{\circ}$; being severally proportional to the said duration. These being respectively added to the temperatures in column V. and VI., give in the former $81\frac{5}{8}^{\circ}$, 82° and $81\frac{1}{2}^{\circ}$; and, in the latter, $33\frac{5}{8}^{\circ}$, 34° and $33\frac{5}{8}^{\circ}$; and if either of these sets of numbers be used in the calculation, according as one or the other of the equations is adopted to develop the results, they will be found to be 1171° , 1212° , and 1245° for the sums of the latent and sensible heat; and consequently the latent heat in each experiment will be 942° , 942° and 950° .

It may be remarked, that no allowance was made in calculating from the former experiments, for the heat which would be taken by the cistern, but which in the first of them, lasting two hours, would probably be very sensible, and may account for the principal part of the deficiency of latent heat brought out by the calculation from that experiment, in comparison with that from the two succeeding ones.

The opinion which I entertain from these experiments as to the *latent heat* of steam is, that it is a *constant* quantity, and perhaps this opinion obtains support from the modern discoveries of definite proportions. But it is necessary, however, to explain the limitation with which I here use this term, "*constant quantity.*" It is well known that if common air be expanded, cold is produced*; and it must therefore happen, that if a given quantity of it at a given temperature could be gradually expanded, and as it was so expanded, gradually supplied with heat, so as to keep the temperature unaltered, this supply of heat would become *latent*; the thermometer would not show it. It is probable, both from analogy and experiment, that this effect takes place in the expansion of steam. It is *not of this part* of its heat, though latent, and in the experiments above related undistinguishable, that I would

* An opportunity occurred to me some years back, which enabled me to determine, with tolerable precision, the degree of cold produced by the expansion of common air from the bulk of two to three, which I found to be 19° or 20° .

be understood to speak when I state my opinion to be as just mentioned; but it is of that which, when water alters its state to that of an elastic fluid, becomes essential to it in every degree of elasticity, besides that which belongs to its expanded state. This latter may be called the latent heat of *expansion**, while the other may perhaps properly be called *constitutional*.

Allow me here to illustrate hypothetically this matter: If this essential or constitutional part of *latent* heat be added to water having the necessary portion of *sensible* heat, and perfectly confined in a close vessel, I conceive the water would be in the state of an elastic fluid; would in fact be steam, as dense as water (possibly compressible, and capable of greater density), and would then require no latent heat of expansion; but if the containing vessel be now conceived to expand, for instance into double the space, I then imagine it would require some addition of heat during this expansion to maintain its proportional elasticity. It must be observed, however, that while this expansion was calling for more latent heat, the sensible heat necessary for the diminishing elasticity would be lessening; but it does not follow that these quantities should necessarily balance each other †.

When this fluid, steam, is raised in low temperatures, and of course under a low degree of elasticity, it obtains from its source, at the same instant, not only the constitutional part of its latent heat, but also that of expansion, and thus the two kinds are confounded; and, in experiments where they are developed by total condensation, are only to be detected together in sum; and it *may be* that this sum, together with the sensible heat, in different states of elasticity, may make a constant quantity; but if the latent heat of expansion from a denser to a rarer state be greater than the diminution of the sensible heat necessary only for the latter, the sum of the sensible and total latent heat will be more in steam raised in low temperatures than in high ones, which the result of your experiments made in low temperatures seems to countenance.

In all that I have said above, when speaking of steam, I have always intended that fluid in the state in which it is raised from water, viz. saturated therewith; but undoubtedly this fluid, after it is so raised under any temperature, and being clear from any additional accession of water, may be heated above that temperature, and cooled down to it again with

* I have no view here to any substances not having the natural power of expansion, as water, ice, &c.—J. S.

† I have, for very many years, entertained a similar hypothesis; but I know of no experiment whereby the truth of it can be demonstrated conclusively.—W. *This note by Mr. Watt.*

changes of elasticity corresponding to those of temperature, like as common air may be, without limitation of temperature, as far as is known. This, however, is a view of the subject which has been totally excluded.

Besides the experiments first related, in which the temperature of steam raised under *high* pressures was observed, others had been made some years before (in 1797 and 1798) for that purpose only; and as they were made with the greatest circumspection, both the manner of making them and their results may be here described, as may also the results of other experiments, made with equal care, to ascertain the temperature of steam raised under *low* pressures.

The instrument used in the former was a Papin's digester, similar to what you had used in your original experiments, and to that described in the *Encyclopædia Britannica*, art. Steam, No. 22, the leading differences being in adapting a metallic tube to it to contain the thermometer, or rather as much of it as contained mercury, in the manner mentioned in the beginning of this letter, and instead of a valve, by the load on which to measure the elasticity of the contained steam, a nicely-bored cylinder was applied, with a piston fitting it, so as to have very little friction, and to the rod of this was applied a lever, constructed to work on edges like those of a scale-beam, by which the resistance against the elastic force of the steam could be accurately determined; and at your suggestion, to be assured that no inaccuracy had crept into the calculation, by which this resistance through the medium of the lever was ascertained, an actual column of mercury of 30 inches high was substituted, and the correspondence was found to be within $\frac{1}{100}$ of an inch.

The observations at each of the points of pressure noted were continued some minutes, the temperature at each being alternately raised and lowered, so as to make the pressure of the steam on the under side of the piston alternately too much and too little for the weight with which it was loaded; and thence a mean temperature was adopted, the extremes of which were, as well as I now recollect, not more than half a degree on each side of it. The load on the piston, including its own weight, &c., was calculated to be successively just equal to 1, 2, 4, and 8 atmospheres of 29.8 inches of mercury each, and the temperature of the steam was varied as above till that of each point was determined: the results were thus—

Atmospheres.	Pressure in inches of mercury.	Temperatures.
1	29.8	212.0
2	59.6	250.3
4	119.2	293.4
8	238.4	343.6

The experiments for ascertaining the temperature of steam below the atmospheric pressure were made with an apparatus essentially similar to that which you originally used, and with scrupulous care and attention; and I met with the same incidents as you had done; such as the production of a bubble of air whenever, after any experiment, the tube was inclined to refill the ball; and also the extraordinary suspension of a column of mercury of 35 inches vertical height, and of 7 inches of water above that, although the counterpoise was only that of the atmosphere, then under 30 inches. I found also that the tube required a considerable degree of tabouring or shaking to make the column subside and leave a space in the ball. This phænomenon was not produced till after much pains taken in inverting and re-inverting the tube again and again, nor till it had been suffered, after these operations, to stand for three or four days undisturbed in the exhausting position, and then discharging the air that had been accumulating in the interval.

The results, to be found in the table below, were deduced from the observations as you had done, viz. by adding to the height of the column of mercury in the tube (ascertained by a gauge floating on the surface of the mercury in the basin), that of the water above it, or rather of an equivalent column of mercury, and subtracting their sum from the height of the common barometer at the time. All these results were taken from observations made after the apparatus had been so perfectly exhausted of air as to produce the phænomenon just mentioned.

Tempera- ture.	Elasticity.				Tempera- ture.	Elasticity.			
	1st set.	2nd set.	3rd set.	Mean.		1st set.	2nd set.	3rd set.	Mean.
52	In.	In.	In.	In.	122	In.	In.	In.	In.
62	0.53	0.42	0.40	0.41	132	3.58	3.54	3.58	3.57
72	0.73	0.52	0.52	0.52	142	4.68	4.65	4.72	4.68
82	1.03	0.73	0.73	0.73	152	6.05	6.00	6.14	6.06
92	1.42	1.02	1.02	1.02	162	7.86	7.80	7.89	7.85
102	1.98	1.41	1.42	1.42	172	9.98	9.96	10.04	9.99
112	2.67	1.92	1.95	1.95	182	12.54	12.72	12.67	12.64
	2.67	2.63	2.66	2.65		16.01	15.84	15.88	15.91

The following formula will be found to give the elasticity belonging to a given temperature, and *vice versa*, with a sufficient degree of accuracy for most purposes, within the range of the experiments, at least, from which they have been formed.

Let t = temperature, e = elasticity, in inches of mercury ;

$$T = t + 52, \text{ and } E = e - \frac{1}{10}, m = 94250,000000.$$

Then
$$\frac{T^{5.14}}{m} = E$$

$$\sqrt[5.14]{Em} = T$$

But as the calculation is most easily performed by logarithms, let L signify the logarithm of the quantity to which it is prefixed : then

$$5.14 LT - 10.97427 = LE$$

$$\frac{LE + 10.97427}{5.14} = LT.$$

The following table shows the observed elasticities, those derived from calculation by the formula, and the differences of the two, which appear to me to be as small as can be expected, taking a general view.

Temperature.	Observed elasticities.	Calculated elasticities.	Differences.	Temperature.	Observed elasticities.	Calculated elasticities.	Differences.
32	In.	In. 0.18		142	In. 6.06	In. 6.20	+0.14
42	0.25		152	7.85	7.99	+0.14
52	0.35		162	9.99	10.19	+0.20
62	0.52	0.50	-0.02	172	12.64	12.86	+0.22
72	0.73	0.71	-0.02	182	15.91	16.08	+0.17
82	1.02	1.01	-0.01	192	19.91	
92	1.42	1.42	0.00	202	24.45	
102	1.95	1.96	+0.01	212	29.80	29.80	0.00
112	2.65	2.67	+0.02	250.3	59.60	59.69	+0.09
122	3.57	3.58	+0.01	293.4	119.20	118.32	-0.88
132	4.68	4.74	+0.06	343.6	238.40	237.60	-0.80

I believe it is now generally considered that the temperature 212° is that of water boiling when the barometer is at 30 inches instead of 29.8 ; and if in the above algebraic expressions the following alterations be made, the results from the formulæ will correspond with the adjustment of that point, and fully as well with the experiments generally.

Let $T = t + 51.3$; the index of the power and of the root be 5.13, instead of 5.14 ; and $m = 87344,000000$. So the two last equations will be : $5.13 LT - 10.94123 = LE$; and $\frac{LE + 10.94123}{5.13} = LT.$

The table will stand as follows, supposing the thermometer had been graduated for 212° to correspond with 30 inches of the barometer :

Temperature.	Observed elasticities.	Calculated elasticities.	Differences.	Temperature.	Observed elasticities.	Calculated elasticities.	Differences.
	In.	In.	In.		In.	In.	In.
32°	*0.16	0.18	+0.02	142°	6.10	6.22	+0.12
42	*0.23	0.25	+0.02	152	7.90	8.03	+0.13
52	*0.35	0.35	0.00	162	10.05	10.25	+0.20
62	0.52	0.50	-0.02	172	12.72	12.94	+0.22
72	0.73	0.71	-0.01	182	16.01	16.17	+0.16
82	1.02	1.01	-0.01	192	20.04
92	1.42	1.42	0.00	202	24.61
102	1.96	1.97	+0.01	212	30.00	30.00	0.00
112	2.66	2.68	+0.02	250.3	60.00	60.11	+0.11
122	3.58	3.60	+0.02	293.4	120.00	119.17	-0.83
132	4.71	4.76	+0.05	343.6	240.00	239.28	-0.72

I remain, with the greatest esteem and respect,

Dear Sir, your very obedient Servant,

Oakhill, 26th March, 1814.

JOHN SOUTHERN.

To James Watt, Esq., Heathfield.

P.S. Some circumstances which occurred in the performance of the experiments (made in 1797 and 1798) of which the results are last related, suggested the trials of a mixture of air with the steam; and I made a few, not indeed with the greatest nicety, but as they furnished a strong probability that the following law of elasticity of a given mixture was either nearly or accurately correct, it may be of use to say, that the apparatus used in the steam experiments being prepared as if for a repetition of them, and as perfectly exhausted of air as for them, a known measure of common air was sent up the tube through the mercury and water, and took its place in the ball; the water surrounding which was heated, and its temperature observed at different periods as before; and indeed the process was precisely the same as the former, with the additional notice of the space in the ball occupied by the expanded air and steam jointly. This process was repeated three or four times with different quantities of air, but the notes not being preserved, I can only now mention the conclusion they induced me to form as to the law above-mentioned, viz. that whatever the elastic force of the air admitted would be in its expanded state, supposing it dry and to occupy the whole

* These are inserted from numerous experiments made by Mr. W. Creighton. Mr. W. Creighton published a theorem for the elasticity of steam at different temperatures in the *Philosophical Magazine*, 1819, vol. liii. p. 266.—Ed.

empty space in the ball (not occupied by water), after adding for the increase of its elasticity by the increase of temperature, it was yet to be augmented by the elasticity which steam alone of the same temperature would possess, to give the elasticity of the mixture.

Let b = bulk of air introduced into the ball, measured before its introduction.

A = its elasticity (expressed in inches of mercury, or the height of the common barometer).

B = bulk occupied by it jointly with the steam in the ball, when their common temperature, governed by that of the water in the pan, is t .

$1 : r$ = ratio of elasticity which the air had before introduction to what it would have by augmenting its temperature to t ; or, which is nearly the same, the ratio of expansion of air by the augment of temperature to t , when under the same pressure.

E = pillar of mercury (in inches) which steam of the temperature t would support:

Then $E + \frac{2bA}{B}$ = pillar of mercury (in inches) which would be supported by the elasticity of the mixture at the temperature t .

Example. Suppose $b = \frac{1}{2}$ cubic inch; $A = 30$ inches; $t = 102^\circ$ 10 to 11, ratio of elasticity of air at the temperature at which it was introduced to what it would possess at 102, consequently $2 = 1.1$; $B = 6$ inches, and E (by the table) 1.95. Then $1.95 + \frac{1}{2} \times 30 \times 1.1 \div 6 = 4.70$ for elasticity of the mixture, or column of mercury it would support.

XXV. *On the Exhalation of Bicarbonate of Ammonia by the Lungs.* By LEWIS THOMPSON, M.R.C.S., &c.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HAVING lately had occasion to ascertain the amount of moisture given off by the lungs of several healthy individuals during a fixed period, I was induced to examine the nature of the fluid thus condensed. The result has proved that bicarbonate of ammonia is constantly exhaled from the lungs to the extent of rather more than three grains every twenty-four hours for each individual; and although this quantity may appear trifling, yet the amount arising from a large population like that of London is well worthy of notice,

and must exceed 150 tons of solid bicarbonate of ammonia per annum: and if, as is extremely probable, other animals also exhale this substance, the atmosphere must not only always contain enough of this agent for the purposes of vegetation, but, by a reciprocal action, the mutual increase of vegetables and animals would only tend to render the air better adapted for the due development of both. The existence of ammonia in the breath may easily be demonstrated, by respiring air which has passed through diluted sulphuric acid, and then expiring it through a tube surrounded by water at 32° F., to the further end of which a vessel is attached to receive the fluid which condenses. On acidulating this fluid with one or two drops of pure muriatic acid and evaporating to dryness on a water-bath, a residue will be obtained, which, when dissolved in five or six drops of water and introduced into a small test-tube, will give off ammonia on the addition of two or three drops of a strong solution of potash, as evidenced by its action on turmeric paper and muriatic acid, or by its peculiar smell. The respiratory process should be continued for an hour or two.

It would be interesting to know whether any difference is observable in the amount of ammonia exhaled from the lungs of individuals suffering from disease of the kidney, diabetes, &c.

I am, Gentlemen,

Your most obedient Servant,

Byker Bar, Newcastle-on-Tyne,

LEWIS THOMPSON.

Jan. 13, 1847.

XXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 59.]

Jan. 7, "QUELQUES Recherches sur l'Arc Voltaïque; et sur 1847. **Q** l'influence qu'exerce le Magnétisme, soit sur cet Arc, soit sur les Corps qui transmettent les Courants Electriques Discontinus." By M. Auguste De la Rive, Foreign Member of the Royal Society, Professor in the Academy of Geneva, Corresponding Member of the Academy of Sciences of Paris, &c.

In the first section of this memoir the author gives a detailed description of the phenomena exhibited by the luminous voltaic arc produced either in a vacuum or in atmospheric air, or in hydrogen gas, by employing electrodes of different kinds of conducting substances, in the form either of points or of plates. He examines minutely the transfer of particles which takes place from one pole to the other under these various circumstances, and the differences which occur when the currents are reversed. He observed that when a positive metallic point is presented to a negative plate, particles of the former are transported by the voltaic arc, and deposited

on the latter, forming a ring of a regular form, having as its centre the projection of the point on the plate. This happens in atmospheric air whether highly rarified or of the ordinary density, but not in hydrogen gas. This deposit consists always of oxidized particles of the positive metal which forms the pointed electrode. In the case of platinum, the circular spot is of a blue colour, and presents the appearance of the coloured rings of Nobili. This effect the author is disposed to ascribe to the action of the oxygen brought by the voltaic current into that particular condition which Schœnbein first described under the name of *Ozone*. While this deposit is taking place, a vivid blue light is emitted.

In the second section the author investigates the action exerted by a powerful electro-magnet on the voltaic arc. He describes the remarkable modifications which the length, the form, and even the nature of the arc undergoes when the electro-magnet is brought extremely near to it, and the magnetization of the electrodes themselves, when they are susceptible of that affection by their approximation to the electro-magnet. He notices the singular phenomenon of a peculiar sound emitted by the luminous arc, when subjected to this magnetic influence; a sound which varies both in its nature and its intensity according to the nature, the form and the temperature of the electrodes, consisting sometimes of a shrill whistle, and at other times of a series of slight detonations.

The third section is devoted to the investigation of a remarkable phenomenon presented by all the conducting bodies while transmitting discontinuous electric currents, under the influence of a powerful electro-magnet; namely, the emission of a sound resembling that of the revolving toothed-wheel in Savart's experiments. This sound is distinctly heard, and is peculiarly loud with prismatic bars of lead, bismuth, tin, &c., about three-quarters of an inch square and a foot and a half long, whether placed in the direction of a line joining the poles of an electro-magnet, or in a direction at right angles to such line: it was weakened only by increasing the distance between the poles and the bar. The intensity of the sound appeared to depend much less on the nature of the substance which was subjected to this action, than on its form, its volume, and its mass. All conducting bodies, whatever may be their nature, or state of aggregation, are capable of yielding these sounds. They are produced by charcoal of all kinds and shape. Mercury contained in a cylindrical glass tube, of similar dimensions with the metallic bars, emits a sound of great intensity; and a still louder sound arises from a wire coiled as a helix around a cylinder of wood, and also by tubes formed of different metals. Similar phenomena are also observable by the action of a helical coil substituted for the electro-magnet.

On the whole, the author arrives at the conclusion that the phenomena noticed in this paper are altogether molecular, and that they establish the following principles: first, that the passage of the electric current modifies, even in solid bodies, the arrangement of the particles; and secondly, that the action of magnetism, in like manner, produces an analogous modification in the molecular constitu-

tion of all bodies. This has already been demonstrated by Faraday in the case of transparent bodies, in its effects on polarized light; and is now extended by M. De la Rive to opaque conducting bodies, by employing, instead of polarized light, a discontinuous electric current.

“On the Ganglia and Nerves of the Virgin Uterus.” By Robert Lee, M.D., F.R.S., &c.

The author states that his recent dissections have enabled him to verify the descriptions he gave of the ganglia and nerves of the uterus in his papers already published in the *Philosophical Transactions*, and also to detect the existence of ganglia situated in the muscular coat of the uterus, and of plexuses of nerves accompanying all the blood-vessels and absorbents ramifying in its walls, between the peritoneum and lining membrane. By examining the hearts of a fœtus, of a child of six years of age, of an adult in the sound state, a human heart greatly hypertrophied, and the heart of an ox, he found that there exists a striking analogy between the ganglia and nerves of the uterus and those of the heart. He ascertained by microscopic observation that the muscular and vascular structures of the auricles and ventricles are endowed with numerous ganglia and plexuses of nerves, which, as far as he knows, have not yet been described, and which enlarge simultaneously with the natural growth of the heart, and also continue to enlarge during its morbid conditions of hypertrophy. The author also finds that the size of the ganglia and nerves of the left auricle and ventricle, in the normal state, is more than double that of the corresponding parts on the right side. A description is then given of two elaborate drawings which accompany the paper.

“On a new and practical form of Voltaic Battery of the highest powers, in which Potassium forms the positive element.” By John Goodman, Esq. Communicated by S. Hunter Christie, Esq., A.M., Sec. R.S.

The author succeeded in constructing a voltaic arrangement of some power by fixing a piece of potassium to the end of a copper wire, placed in a tube containing naphtha, and bringing it in contact with a small quantity of mercury, held by a layer of bladder closing the lower end of the tube, which was itself immersed in acidulated water immediately over a piece of platinum, and then completing the circuit by establishing a metallic contact between the copper wire and the platinum. This battery acted with energy on the galvanometer, and effected the decomposition of water. A series of twelve pairs of similar plates exhibited a sensible attraction of a slip of gold leaf. Thus it appears that the substance which possesses the highest chemical affinity manifests also the greatest power of electrical tension.

Jan. 21. “On Photographic Self-registering Meteorological and Magnetical Instruments.” By Francis Ronalds, Esq., F.R.S., &c.

The apparatus employed by the author at the Kew Observatory, and which he terms the Photo-Electrograph, is described by him in the following words:—“A rectangular box, about sixteen inches

long and three square, constitutes the part usually called the *body* of a kind of lucernal microscope. A voltaic electrometer (properly insulated, and in communication with an atmospheric conductor) is suspended within the microscope, through an aperture in the upper side, and near to the *object* end. That end itself is closed by a plane of glass, when daylight is used, and by condensing lenses, when a common Argand lamp is employed. In either case an abundance of light is thrown into the microscope. Between the electrometer and the ether, or eye-end of the microscope, fine achromatic lenses are placed, which have the double effect of condensing the light upon a little screen, situated at that eye-end, and of projecting a strong image of the electrometer, in deep *oscu*ro, upon it. Through the screen a very narrow slit, of proper curvature, is cut (the chord of the arc being in a horizontal position), and it is fitted into the back of a case, about two-and-a-half feet long, which case is fixed to the eye-end of the microscope, at right angles with its axis, and vertically. Within the case is suspended a frame, provided with a rabbet, into which two plates of pure thin glass can be dropped, and brought into close contact by means of six little bolts and nuts. This frame can be removed at pleasure from a line, by which it is suspended, and the line, after passing through a small aperture (stopped with grease) cut through the upper end of the long case, is attached to a pulley (about four inches in diameter), fixed, with capacity of adjustment, on the hour arbor of a good clock. Lastly, counterpoises, rollers, springs, and a straight ruler are employed for ensuring accurate rectilineal sliding of the frame, when the clock is set in motion.

“A piece of properly prepared photographic paper is now placed between the two plates of glass in the moveable frame; the frame is removed (in a box made purposely for excluding light), and is suspended in the long case; this is closed, so as to prevent the possibility of extraneous light entering with it; the clock is started, and the time of starting is noted.

“All that part of the paper which is made to pass over the slit in the screen, by the motion of the clock, becomes now therefore successively exposed to a strong light, and is consequently brought into a state which fits it to receive a dark colour on being again washed with the usual solution, excepting those small portions upon which dark images of the lower parts of the pendulums of the electrometer are projected through the slit. These small portions of course retain the light colour of the paper; and from the long curved lines or bands, whose distances from each other, at any given part of the photograph, i. e. at any given time indicate the electric tension of the atmosphere at that time.

“By certain additions to the instrument above described, the kind as well as the tension of electrical charge is capable of being registered; and by the employment also of a horizontal thermometer, &c., it is adapted to the purposes of a *Thermograph*, as well as *Photo-barometrograph* and *Magnetograph*.”

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxix. p. 66.]

February 1846.—The Mathematical Theory of the two great Solitary Waves of the first Order. By S. Earnshaw, M.A.

The nomenclature of this paper is adopted from a Report on Waves by Mr. J. S. Russell, printed in the Proceedings of the British Association. From the extreme comprehensiveness of the equations of fluid motion, the author infers a necessity of appealing to experiments for the suggestion of data which may be used in modifying the generality of those equations so as to suit actual cases of known fluid motion. With this view he has made use of the experiments recorded in Mr. Russell's report, and thence selected the two following properties:—1st. The velocity of transmission of a wave in a uniform canal is constant. 2nd. The horizontal velocity is the same for all particles situated in a vertical plane, cutting the axis of the canal at right angles. By reference to Mr. Russell's report, it will be seen that these two properties, selected on account of their simplicity and ready experimental examination, are distinguishing characteristics of what he has denominated the two great solitary waves of the first order. By the aid of them the equations of motion take such modified forms as to admit of exact integration; so that without employing any analytical approximations the author is enabled to obtain theoretical expressions for all the circumstances of the two solitary waves. The results are tested by a comparison of the velocities of transmission of various waves given by theory and by experiment. The greatest difference of these in the case of the positive wave is not found to exceed $\frac{1}{38}$ th part of the whole velocity; but in the case of the negative wave it is found to be much greater, and to amount in one instance to as much as $\frac{1}{6}$ th of the whole velocity. The reason of this discrepancy is conjectured; and the agreement in the case of the positive wave is considered to be exact.

It is found in the course of the investigation that one of the necessary conditions of fluid motion is not satisfied; and it is shown that it cannot be satisfied as long as the two principles, adopted from Mr. Russell's report, are supposed to coexist. They are proved in fact to be incompatible with each other. But as the second principle was found by Mr. Russell to be so nearly exact that he could not detect any deviation from it in his experiments, it is shown by theory that from this circumstance there will be a rapid degradation of the summit of the wave, and a consequent loss of the velocity of its transmission, both which results of theory were observed to be true experimentally. The memoir concludes with pointing out the agreement of theory with some minor phænomena noticed by Russell.

May 25, 1846.—Cases of Morbid Rhythmical Movements, with observations. By G. E. Paget, M.D., Fellow of Caius College and of the Royal College of Physicians, London.

Seven cases were related. The movements were vibratory, rotatory, bowing, &c. In some of the cases they were incessant; in others paroxysmal; and in others again they were of both kinds,

the predominant movement being replaced at intervals by distinct paroxysms.

On a comparison of these cases with the few others on record, and with the experiments of Flourens and Majendie, it was inferred as probable, that one class of the movements, viz. the rotatory, depended on disorder in the cerebellum or its transverse commissure, the pons. With regard to the other movements, it appeared that there were no sufficient grounds for even a probable conjecture as to the particular part of the encephalon, the excitement or disorder of which might act as an immediate cause of the movements.

The *remote* causes were such as under other circumstances are known to excite the common convulsive diseases, such as chorea and epilepsy. These remote causes were in most cases *eccentric*.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxix. p. 232.]

Nov. 13, 1846.—An Explanation of the observed Irregularities in the Motion of Uranus, on the Hypothesis of Disturbance caused by a more distant Planet; with a Determination of the Mass, Orbit, and Position of the disturbing body. By J. C. Adams, Esq., M.A., F.R.A.S., Fellow of St. John's College, Cambridge*.

The author introduces the subject by remarking, that when Bouvard constructed his Tables of Uranus (those now commonly in use), he found it impossible to reconcile the ancient observations, made before the discovery of Uranus as a planet, with the modern observations, and that therefore in the formation of his tables he relied solely upon the latter; but that in a very few years the still more modern observations exhibited a departure from the tables nearly as great as the ancient ones, and therefore there seemed now to be no sufficient reason for rejecting the ancient observations. The author then states that his attention was first directed to this subject by reading the report on the recent progress of astronomy made to the British Association at their meeting in Oxford; and that in July 1841 he formed a design of investigating the yet unaccounted-for motions of Uranus, in order to discover whether they could be explained by an exterior disturbing planet. In 1843 he made a first attempt, supposing the orbit of the disturbing planet to be a circle, and its mean distance twice that of Uranus. This investigation was founded exclusively on the modern observations, using, as far as 1821, the errors given in the equations of condition in Bouvard's tables, and for subsequent years the errors given in the *Astronomische Nachrichten*, and the Cambridge and Greenwich Observations. The result showed a good general agreement of the observed disturbance with the disturbance which would be produced by the action of such a planet. In February 1844, the author received from the Astro-

* This paper was presented to the Society on the evening of Nov. 13, 1846. It has since been published entire as an extract from the Appendix to the Nautical Almanac for 1851.

nomer Royal the results of the general reduction of the Greenwich Planetary Observations.

In the meantime the Göttingen Academy had proposed for the subject of a prize the theory of Uranus, and though the author had no hope of being able to complete an essay in time to compete for the prize, he was stimulated by the publication of this proposal again to enter on the investigation. He now took into account the possible eccentricity of the disturbing planet to the first order, retaining the same assumption for mean distance. For the modern observations, the tabular errors used, as far as 1830, were exclusively those of the Greenwich Observations, except one by Bessel in 1823; after 1830 the Cambridge and Greenwich determinations and those in the *Astronomische Nachrichten* were used. Those for the observations anterior to the discovery of the planet were taken from Bouvard.

Results for the elements of the disturbing planet were obtained, which were communicated in September 1845 to Prof. Challis, and in October 1845 (slightly altered) to the Astronomer Royal. Afterwards the investigation was repeated, supposing the mean distance diminished by about $\frac{1}{30}$ th part. The results were communicated to the Astronomer Royal in September 1846. They seemed to show that the mean distance ought to be still further diminished.

The author, after adverting to the dates of M. Le Verrier's papers, and showing that his own calculations were earlier in date, says, "I mention these dates merely to show that my results were arrived at independently and previously to the publication of M. Le Verrier, and not with the intention of interfering with his just claims to the honours of the discovery; for there is no doubt that his researches were first published to the world, and led to the actual discovery of the planet by Dr. Galle, so that the facts stated above cannot detract, in the slightest degree, from the credit due to M. Le Verrier.

The investigations proceeded as follows:—First, to diminish the number of equations, the results were collected in groups of three years each; and these were so arranged as to present results nearly independent of the error of radius vector. Thus twenty-one equations were obtained; and these, without extension for the two or three last years (which might subsequently have been included, but which would have disturbed the similarity of the calculations) were also used in the subsequent calculations for a different assumed mean distance. Then all the principal inequalities in the recognised theory of Uranus were verified, and corrections for an error pointed out by Bessel, and for the altered mass of Jupiter, were applied, as well as for some terms of the second order of masses pointed out by Hansen. Other inequalities of higher orders were neglected; as their effects may be represented, either by a very slow alteration of the epoch and mean motion, or by a very slow alteration of the perihelion and eccentricity; both which may, without sensible error, be assumed as constant, during the comparatively short period through which Uranus has been observed. The author then gives a table of the differences between the theoretical longitudes (thus corrected), and the observed longitudes: the maximum values are as follows:—

In 1712	+92.7	In 1804	+24.2
1750	-47.6	1840	-66.6

These are then converted into corresponding errors of mean longitude, which the author finds more convenient.

Then, formulæ are investigated for the effects of small corrections of the elements of the orbit of Uranus, and for the perturbations of mean longitude produced by a disturbing planet, expressed in the notation of Pontécoulant. These are expanded as far as the second order of eccentricities (involving only the first power of the eccentricity of the unknown planet), and the whole is reduced to numbers, with no symbols remaining, except for functions of the corrections of the elements of Uranus, and functions of the epoch, longitude of perihelion, eccentricity and mass, of the disturbing planet. All the numerical quantities are computed on the supposition that the mean distance is double that of Uranus. Any one of these expressions, adapted to a certain time, being made equal to the error in the tabular place of Uranus for the same time, furnishes an equation of condition.

These equations of condition are treated by the method of least squares; and the successive steps of elimination are given. The author considers that the modern observations are scarcely sufficient to give the eccentricity and longitude of perihelion of the disturbing planet; but when the ancient observations (always omitting that of 1690 as uncertain) are combined, there are ample means for determining these elements. The equations, after the elimination had proceeded to a certain degree, were solved by successive substitution. The results thus obtained were—

Hypothesis I.

Assumed mean distance = $2 \times$ that of Uranus.

Mean longitude, October 6, 1846	325° 7'
Longitude of perihelion	315 57
Eccentricity of the orbit	0.16103
Mass (that of the sun being 1).....	0.0001656

which were communicated to the Astronomer Royal in October 1845.

The author then states that he made a second investigation, on the supposition that the mean distance of the disturbing planet = mean distance of Uranus $\times \frac{1}{0.515}$. The process, with very little difference, is the same as that for the former assumption of mean distance. The formulæ, the equations, &c., are given in the same manner as before. The elements obtained thus are as follows:—

Hypothesis II.

Assumed mean distance = $1.942 \times$ that of Uranus.

Mean longitude, October 6, 1846.....	323° 2'
Longitude of perihelion	299 11
Eccentricity of the orbit.....	0.12 0615
Mass (that of the sun being 1)	0.00 015003

The corrections to the elements of the orbit of Uranus are investigated on both hypotheses. Then on substituting the effects of the

corrections and the effects of the perturbations, the residual errors are obtained, of which the following are the maximum values:—

	Hypoth. I.	Hypoth. II.
1712	+ 6 ^u .7	+ 6 ^u .3
1715	− 6.8	− 6.6
1753	+ 5.7	+ 5.2
1764	− 5.1	− 4.1
1771	+ 11.8	+ 12.8 Single observation.

After this time, to the year 1840, the largest error is 2^u.35.

After 1840, the errors increase on both hypotheses. They are,—

	Hypoth. I.	Hypoth. II.
1843	+ 7 ^u .11	+ 5 ^u .77
1844	+ 8.79	+ 7.05
1845	+ 12.40	+ 10.18

It appears from this extremely probable that the mean distance of the disturbing planet ought to be assumed nearly = mean distance of Uranus $\times \frac{1}{0.574}$.

The residual errors for the single observation of 1690 are,—

Hypoth. I.	Hypoth. II.
+ 44 ^u .5	+ 50 ^u .0

It seems probable that these errors would be increased by still further diminishing the mean distance.

Expressions are then investigated for the correction of radius vector produced by the correction of elliptic elements, and by the effects of perturbation. The numerical values are as follows:—

	Hypoth. I.	Hypoth. II.
1834	+ 0.00505	+ 0.00492
1840	+ 0.00722	+ 0.00696
1846	+ 0.00868	+ 0.00825

The author states that no satisfactory results could be found for the node and inclination of the planet's orbit, as deduced from the irregularities in the latitude of Uranus.

The author then remarks that the perturbations of Saturn produced by the new planet will be undoubtedly sensible; and he suggests that it would be interesting to examine anew the theory of Saturn, and to ascertain whether the masses of Jupiter and Uranus deduced from it are consistent with those obtained by other methods. He remarks that the published reductions of the Greenwich Observations now make such an inquiry comparatively easy.

December 14, 1846.—*Reappearance of Astræa.*

Extract of a letter from Prof. Schumacher to Mr. Hind, dated Altona, Nov. 27, 1846.

“ M. Otto Struve has already reobserved Astræa, which appears brighter than he expected she would.

		M.T. Pulkowa.			R.A.			Dec.		
		h	m	s	°	'	"	°	'	"
1846	Nov. 4	17	58	49	193	55	28.4	-3	14	22.7
	13	18	4	11	198	12	42.8	-4	47	41.6

These positions are free from parallax. The resulting corrections for M. D'Arrest's ephemeris are,—

		R.A.			Dec.		
		°	'	"	°	'	"
1846	Nov. 4	-1	7.5		+0	39.3	
	13	-1	8.1		+0	37.1	

Mr. Lassell forwarded a more complete account of his physical observations on Le Verrier's planet, with his Newtonian reflector, 2-feet aperture, mounted equatorially:—

Oct. 3. He received an impression of a ring, not much open, and nearly at right angles to the parallel of daily motion. Speculum A and plane metallic reflector.

Oct. 10. The same impression of a ring in the same direction. A minute star just steadily visible, with full aperture of 24 inches, powers 316 to 567, distance $2\frac{1}{2}$ to 3 diameters, a little to the right, and apparently below the ring continued. Speculum B and Merz's prism.

Nov. 10. The planet very like Saturn, as seen with a small telescope and low power, but much fainter. Same speculum and prism as before.

Nov. 11. The planet still retains its appearance. A faint point of light considerably distant, in the direction of the ring and below it. Speculum A and plane reflector.

On these nights several persons saw the supposed ring, and all in the same direction, as shown by independent diagrams.

Nov. 30. A minute star above, and a little to the left of the continuation of the ring, distance 2 diameters. Speculum B and Merz's prism.

Dec. 3. The same relative appearance exactly of planet and small star as on October 10th. The direction of the ring estimated at about 70° with the parallel of daily motion. The small star about 3 diameters distant and 50° N. following. Telescope as before.

Dec. 4. No minute star visible, though carefully looked for. Telescope as before.

Mr. Lassell says that the points of light seen on Oct. 10, Nov. 30, and Dec. 3, were, so far as he could judge, the same in appearance and brightness. There were no other stars in the immediate neighbourhood. Hence he conceives the probability is in favour of the star being a satellite.

With respect to the ring, Mr. Lassell says that "he has never looked at the planet, under tolerable circumstances, without receiving the same impression of its existence;" and that so far as he can judge, the direction of the supposed ring makes a constant angle with the meridian, and not with the horizon; but this is not very certain.

In speaking of the quality of his telescope, Mr. Lassell says that he finds Merz's prism gives him more light than a plane reflector,

and with no loss of distinctness ; and that he has succeeded in bringing the foci of his exterior and inner surface to agree within a hundredth of an inch. The telescope shows seven stars in and two near the trapezium of Orion, and so far approaches to *resolving* the nebula, that it shows " stars, the centres of nebulous clouds, scattered all over the nebula."

In confirmation of Mr. Lassell's idea that there is a ring about the new planet, Mr. Hind stated that the South Villa telescope shows it oblong, and that the major axis makes an angle of about 30° with the meridian.

Reduction of Tycho Brahe's Observations of the Comet of 1590, with Elements deduced therefrom. By Mr. Hind.

" The comet of 1590 was first perceived by Tycho Brahe on the evening of March 5, while he was employed in observing the planet Venus. It was situated near the Northern Fish, between Aries and Andromeda. The diameter of the head was three minutes, and a faint tail was visible, extending from 7° to 10° , and directed towards the zenith at about $7^h 30^m$ P.M.

" M. Pingré, in his *Cometographie*, tome i. p. 554, has given a series of observations on this comet made by Tycho, and extracted from a manuscript preserved at the Dépôt de la Marine in Paris. The observations are detailed at considerable length in this manuscript; but the table in M. Pingré's work is an abstract made by Tycho himself, and I have used it as the basis of my calculations. For deducing the places of the comet, we have the apparent times of observation at Uraniburg, the distances from known stars, and the declinations for the same times as these distances: also in most cases, the comet's altitude, and frequently its azimuth from the south towards the west. I have calculated the positions from the observed declinations and distances, duly corrected for parallax and refraction, making use of the altitudes in the preliminary computations only. On two occasions the comet was observed on the meridian *sub polo*; in these cases I have performed the reductions in the usual manner, applying refraction and parallax."

Mr. Hind then presents tabular results of his reductions at the following stages:—

An ephemeris of the sun.

A catalogue of the stars of comparison.

An *approximate* ephemeris of the comet, for computing the effects of aberration and parallax.

The observed distances of the comet from the stars corrected for refraction, aberration and parallax, with the Uraniburg mean time, and the resulting right ascension and declination of comet.

" The following longitudes and latitudes are computed from a mean of each day's observations of right ascension and declination, omitting, however, the third result for March 6, and the first for March 7. They are referred to the apparent equinox of the date :

Mean time at Uraniburg.	Comet's geocentric longitude.	Comet's geocentric latitude.
March 5·36087	18° 15' 25"	+18° 12' 58"
6·29647	25 46 21	19 27 44
7·38396	33 21 14	20 21 32
8·32461	38 56 11	20 51 1
10·32981	48 6 39	21 20 26
11·43862	52 2 3	21 14 33
12·36629	54 49 1	21 9 26
13·35634	57 23 19	21 5 22
14·39625	59 40 14	20 58 14
16·33600	63 10 12	+20 47 55

After a great many trials I at last fixed upon a parabolic orbit, which appears to represent the observations with as much, or nearly as much, accuracy as they admit. It would be manifestly a hopeless business to attempt any determination of the particular conic section described by the comet, from positions extending over so short a period, and at the same time so rough. Should the comet reappear, the observations of 1590 will be in a more tangible form than heretofore."

Parabolic Elements.

Passage through Perihelion, 1590, Feb. 8·0624, Uraniburg Mean Time, New Style.

Longitude of Perihelion on the orbit	217° 57' 12"	} App. Eq. March 10.
Ascending Node	165 56 56	
Inclination to the Ecliptic	29 29 44	
Log. distance in Perihelion.....	9·7541386	
Heliocentric motion.		Retrograde.

"These elements, compared with the above longitudes and latitudes; give the following differences:—

	Computed—Observed Place.	
March 5	Long. + 34"	Lat. — 75"
6	+ 38	0
7	— 23	+160
8	—126	+ 91
10	+ 75	—346
11	+ 29	+ 5
12	— 10	+106
13	— 95	+ 6
14	+ 8	+ 3
16	+ 69	—272

"The comet of 1590 is described by Martin Mylius in his *Annales Gorlicenses*, published as part of a work entitled *Scriptores Rerum Lusaticorum*, Lipsiæ et Budissæ, 1719, in folio. It was also the subject of a treatise by Matthias Menius, printed in 1591. Lubienietki, in his great work *Theatrum Cometicum*, gives us a chart showing the path of the comet in the heavens: he quotes Ricciolus and other writers."

It is almost unnecessary to say that the reductions have been made on the best existing data, and according to the most approved methods.

On Luminous Rings round Shadows. By the Rev. Baden Powell, Savilian Professor of Geometry, Oxford.

The remarkable optical phenomena attending total solar eclipses, especially the formation of a luminous ring round the discs, are now on the whole so well established, that speculations have been hazarded as to their cause: and though these speculations as yet scarcely amount to more than very general illustrations, yet as experimental facts have been elicited which seem to bear a resemblance to the phenomena in question, they deserve notice, even in the present uncertain state of the inquiry.

The author disclaims any idea of bringing forward such facts as a complete explanation; but he is induced to offer them because *one* experiment devised by him long since was thought by the late Mr. Baily worthy of being laid before the Society, though communicated to him without any such intention. (Ast. Soc. Notices, March 10, 1843.)

The following is the author's historical summary of the observations of the luminous ring:—

The phenomenon appears to have been observed as early as the time of Plutarch, as appears by a passage from him cited in the *Comptes Rendus*, 1842, i. 847. In more modern times, probably the earliest recorded instance is in the eclipse of 1567 [this is clearly wrong in date or name] observed by Kepler. The ring was observed at Naples in 1605 (Phil. Trans. vol. xl. 177); and from the year 1706 we have the full and well-known records of the most eminent astronomers down to the present day, which establish the phenomenon as a regular part of the appearance of a total eclipse. The author refers to instances in the Mem. Ast. Soc., vol. i. p. 144, and vol. x. p. 9–16, 37, &c., and to the Notices of this Society, vol. v. p. 207; as also to the *Comptes Rendus* for 1842, parts 1 and 2. He remarks that remarkable discrepancies are found in the accounts of the same, as well as of different eclipses, and he mentions instances. Some of these discrepancies admit of easy explanation. In the transit of Venus in 1769 light was seen surrounding the part of the planet *off* the sun, both at immersion and emersion; but this has been ascribed to her atmosphere. (Ast. Soc. Mem. vol. x. p. 27.) The singular *protuberances* observed in the eclipses of 1733 and 1842 do not seem to belong to the ring; and it seems to the author hardly conceivable that they can be purely optical phenomena, as suggested by M. Valz and M. Arago.

The author next gives an account of the attempts of different astronomers to assign a *cause* for the luminous ring. Kepler considered it to be due either to combustion round the sun, or to refraction of his rays by the moon's atmosphere. Others have more generally attributed it to some peculiar effect of the solar atmosphere; and some, especially Olbers, expressly attribute it to the zodiacal light. De l'Isle, and more lately Arago and Valz, regard it altogether

as a phenomenon of diffraction. Several instances are then given of luminous rings surrounding terrestrial objects seen in shadow, as observed by different philosophers.

Experimental imitations of the luminous ring, by artificially eclipsing the sun, were long ago made by De l'Isle and La Hiri (*Mém. Acad. Paris*, 1715, p. 166), and such an intimation is also spoken of by Mr. Baily (*Ast. Soc. Notices*, vol. v. p. 212).

Among the experiments described by Newton in the third book of his *Optics*, one, which is emphatically mentioned as unfinished, seems to bear closely on the present subject. The sun's light being admitted through a hole one quarter of an inch in diameter, and partly intercepted by a knife-edge fixed along one side of a hole three quarters of an inch square in a screen, and falling on a paper beyond, Newton saw "two streams of faint light shoot out both ways from the beam of light into the shadow like the tails of comets." Again, placing his eye to receive the light, he saw "a line of light upon the edge (of the knife) all along it." . . . "It was contiguous to the edge and narrower than the innermost fringe," and "between the edge and the first fringe." Newton thus clearly distinguishes between this peculiar phenomenon and the ordinary effects of diffraction.

The author then proceeds to detail his experiments made for the purpose of producing the luminous ring, remarking that the essential conditions in all diffraction experiments are, that the origin of the light be as nearly a point as may be, and that the area of the rays diverging from it extend beyond the edge of the opaque diffracting body.

He finds that with apertures up to a quarter of an inch or more, and whether the area of the rays reach beyond the edge of the opaque body, or lie even considerably within it, a small circular disc, seen at a distance, either with or without a telescope, is edged by a *bright luminous ring*, which cannot be seen with the eye-lens, and is therefore *not* an optical image like the diffraction fringes, but is seen in the telescope distinctly when in focus for the opaque disc. With a straight edge the same phenomenon is observed as a line of light running along it. If the origin be a lens of short focus the ring is seen as before, but less perfectly; while the diffraction fringes are seen perfectly with the eye-lens. The origin being a hole, the ring is visible under different variations of the area of the rays, but ceases to be visible when the area is less than about a quarter of the disc. Changes in the distance of the eye from the disc produce very little, if any, change in the breadth of the ring.

The ring was examined by a telescope with cross-wires in its focus. The intersection of the wires was fixed on the edge of the disc before the light was admitted, and on its admission the ring extended sensibly beyond; on its being then fixed on the edge of the ring, the edge of the disc fell within it on shutting out the light. The experiment was repeated in various ways to determine the influence of reflexion from the edge, &c.

When the area is a hole, the disc being within the area of the

rays, if the shadow of the disc be examined by an eye-lens of two inches' focus, there appear at all points of the circumference streaks of yellowish light radiating into the shadow, and crossing at the centre they form there a round bright spot; but the streaks are supposed to be chiefly due to irregularities in the edge. An analogous phenomenon is seen when a straight edge is used. A modification of the same phenomenon is observed in the case described by the author to the British Association, at the meeting in 1846.

The experiments above were all made by means of the sun's light. There are well-known difficulties in performing any diffraction experiments by candle-light; but the author succeeded in exhibiting the ring by means of the light of a flame placed in the focus of a short lens limited by a circular aperture. When the shadow is thrown on a white screen at about one foot distance from the disc, the middle of the shadow appears faintly illuminated; but no streaks, as in the case of solar light, could be detected, nor is the faint illumination perceptible to the eye viewing the disc directly. The author in conclusion makes mention of the theory of M. Babinet, which attempts to explain the phenomenon on the undulatory hypothesis on this principle, that "at points exterior to the area of the rays there is no light, owing to the natural destruction of the secondary waves. If, then, of the two rays proceeding to any such point to destroy each other we intercept one by an obstacle, the other remains, and gives rise to a point of light at that point; that is, just beyond the obstacle, a series of luminous points thus created by the removal of interfering rays will give rise to the luminous borders on the edge." (*Nouv. Bulletin des Sciences*, Nov. 1832. Quetelet, Appendix to Translation of Herschel on Light.)

The author, in conclusion, remarks that, imperfect as the whole investigation confessedly is, it is by thus making the facts known that we may most reasonably hope to elicit some better elucidation of them.

XXVII. *Intelligence and Miscellaneous Articles.*

ON NEWTON'S TELESCOPE AT THE ROYAL SOCIETY.

To the Editors of the Philosophical Magazine and Journal.

Royal Society, Somerset House,
Jan. 22, 1847.

GENTLEMEN,

MY attention has been directed to a communication by Mr. Heineken, in the last Number of the *Philosophical Magazine*, wherein he states, in allusion to the destruction of mechanical and other instruments, "Two lamentable examples of this are, I learn from a paper in the *Magazine of Science*, exhibited even at the Royal Society, where the reflectors of Newton and Hadley are in a state of complete dilapidation."

To this statement I beg to give an unqualified contradiction. The reflectors are in excellent preservation; and Sir Isaac Newton's

telescope, which is preserved under a glass case with the greatest care, is (considering its great age) in almost perfect condition. It is much to be regretted that individuals do not, before repeating unfounded statements, make inquiries as to their accuracy, which could easily have been done in this instance.

I am, Gentlemen,

Your obedient Servant,

CHARLES RICHARD WELP,
Assistant Secretary.

THE CENTRAL SUN.

Professor Mädler of Dorpat, has published a pamphlet, in which he announces his belief that the centre of the great nebula in which our system lies, or of the congeries of stars which form the Milky Way, is in the Pleiades; and that the star Alcyone is more likely than any other to merit the title of the Central Sun. This question is not like that of an asserted planet—one which can soon be settled in the affirmative. If within the next half-century opinion on the subject should have arrived at something like either positive reception or positive rejection, it is perhaps as much as can now be expected. Meanwhile the pamphlet is before us (*Die Central Sonne*, Leipzig, 1847), and its leading points are, shortly, as follows:—

The observations of astronomers have made it highly probable, and Professor Mädler considers it as an established fact, that Newton's law of gravitation reigns throughout the sidereal space and governs the movements of all stars; which he thinks chiefly proved by the nature of the orbits of binary systems.

Setting out from this fundamental principle, he shows that, whatever may be the form of a system of fixed stars, the proper movements of the individual bodies must be *accelerated* as the distance from the central point *increases*, and that all the times of revolution of these different bodies around their common centre are nearly equal, as long as the mass in the centre of attraction has not too considerable a predominance over all the other masses.

By a very extensive and laborious set of observations and comparisons, the Professor found that the group of the Pleiades forms the only point in the heavens to which the preceding conclusion is applicable; that really the velocity of the *true proper movements of the fixed stars increases insensibly from this group in all directions*; and that, moreover, most of them are moving in the same sense.

He therefore assigns the Pleiades as the central group of that stellar system which is terminated by the Milky Way (to which our own and all the isolated suns belong), and the star Alcyone as that body which, most probably, is the proper central sun of this system.

From the proper movement and parallax of 61 Cygni (taking the latter = $0''.3483$), he attempts to deduce the distance of Alcyone from our sun, and obtains the following approximate results:—

The parallax of Alcyone = $0''.006097$, or the distance from the sun 34 millions of radii of the earth's orbit.

The sun accomplishes one entire revolution around this star in 18,200,000 years, and moves in its orbit at the rate of 36·8 English miles per second.

The sum of all the masses contained within a globe described around Alcyone with the radius vector of the solar system amounts to 117,400,000 times the mass of the sun.

The ascending node of the sun's orbit is situated in long. $236^{\circ} 58'$ of the ecliptic of 1840; and the sun will pass through this point about the year 154,500 of our chronology.

The inclination of the sun's orbit to the ecliptic of 1840 is $84^{\circ} 0'$.

Regarding the constitution of this immense system, Professor Mädler has come to the following conclusions:—

The centre is marked by a group consisting of a great number of stars and considerable individual masses. Around this stretches a narrow zone, comparatively devoid of stars. Then follows a broad and rich ring-shaped layer, then another intermediate zone comparatively poor, and so on, a succession of a still unknown number of rings, the two utmost of which form the Milky Way. These rings are connected in several places with each other by intermediate parts, like bridges; and the rings themselves are not everywhere of the same density, but show now and then something approaching to the formation of groups. In general, however, they consist only of isolated, single or double stars.

In the course of his investigations, the learned Professor points out the importance of studying more closely the proper movements of the fixed stars; and recommends chiefly those in the neighbourhood of the Pleiades to the attention of astronomers.—*Athenæum*, Jan. 9, 1847.

ON THE FERMENTATION OF TARTARIC ACID AND ITS PRODUCTS.

BUTYRO-ACETIC ACID. BY M. NICKLES.

In preparing tartaric acid from crude tartar, it often happens, during the heat of summer, that the tartrate of lime still containing some fermentable matters, suddenly begins to ferment, and is converted, in a short time, into an acid which has long been taken for acetic acid.

On the occurrence of this fermentation, M. Noellner saturated the product with oxide of lead, and obtained fine octahedral crystals, on attentively examining which, he considered them as the salt of a peculiar acid different from acetic acid, and to which he gave the name of *pseudo-acetic acid*. An experiment performed with crude tartar, without the addition of lime, yielded merely acetic acid.

M. Berzelius, in his Annual Report for 1843, p. 132, considers this acid as a mixture of acetic and butyric acids. M. Noellner, however, had obtained a salt of lead crystallizing in octahedrons, a salt of soda also octahedral, and a mammillated magnesian salt, none of which forms occur in the corresponding salts of acetic and butyric acids.

To clear up the point, M. Nicklès saturated eight pounds of crude tartar with lime; the filtered liquor was boiled with gypsum; the

tartrate of lime produced by the second reaction was added to that obtained by the first, with all the organic substances, and exposed to a fine summer sun at a temperature of 95° to 104° F. and 113° F. The disengagement of gas took place very slowly, and it required six weeks to produce a small quantity of a substance possessing all the properties about to be described.

It appears from the experiments of M. Nicklès, that changes effected in tartaric acid by fermentation, may take place in three modes; if this acid be free or combined with potash, it is converted into acetic and carbonic acids; if the potash be replaced with lime, the tartaric acid yields carbonic acid, acetic acid and butyric acid (*pseudo-acetic acid*); and lastly, under circumstances which as yet are undetermined, the tartaric acid is converted into carbonic acid, acetic acid, and a new acid which M. Nicklès calls *butyro-acetic acid*, it containing the elements both of butyric and acetic acids, and these acids occur among the products of its decomposition.

On determining the silver of a salt which M. Noellner had sent to M. Nicklès, the latter obtained the same result as the former; when, however, the acid was set free by the decomposition of the salt of lead with sulphuric acid, then saturated with carbonate of ammonia and precipitated by nitrate of silver at a boiling heat, M. Nicklès obtained, at first, fine crystals of acetates, and then from the residual solution, magnificent dendritical crystals of butyrate of silver. The separation of the two acids may also be effected by chloride of calcium; this salt dissolves the acetic acid only, whereas the butyric acid collects it in an oily form on the surface; M. Nicklès states several new facts as to the mode in which a mixture of acetic and butyric acid acts. Thus butyric acid does not precipitate acetate of lead if mixed with a small quantity of acetic acid. The influence of the latter extends yet further; if a mixture of butyric and acetic acids be saturated with oxide of lead, and excess of ammonia be added, small rose-coloured acicular surbasic crystals of butyrate of lead are formed, which owing to a small quantity of interposed acetate are soluble in water. This surbasic salt absorbs carbonic acid rapidly from the air. A mixture of acetate and butyrate of barytes is capable of dissolving a large quantity of chloride of lead, but without forming any definite compound.

Butyro-acetic acid possesses properties which prevent its being considered as a mere mixture of butyric and acetic acids. It was obtained by the decomposition of a salt of lead which had been exposed to the air more than two years; its odour resembles that of a mixture of the two acids, but less pungent; it is soluble in water, alcohol and æther. It is obtained pure by decomposing its salt of soda by phosphoric acid; it then rises to the surface of the liquid in the state of an oily stratum; its boiling-point is nearly 284° F. When heated with sulphuric acid, it is slightly altered with the evolution of sulphurous acid gas.

The crude acid yields a liquid which produces with barytes a prismatic compound; it is very soluble in water, especially when hot, and but slightly soluble in absolute alcohol; it is perfectly per-

manent in the air, but still yields the odour of rancid butter. Exposed to a current of dry air at 212° , this salt loses 3.25 per cent. of water; at 392° it fuses, and again loses 2.80 per cent. Thus dried, the salt consists of $[C^3(H^3Ba)O^2]$. By dry distillation it yields a non-acid oily body; the last mother-waters yield acetate of barytes. Butyro-acetic acid readily yields an æther with alcohol and sulphuric acid. It has the smell of fruit. With respect to this acid, it will be observed that it is isomeric with metacetic acid, obtained during the last year by M. Gottlieb from potash and sugar. M. Nicklès has not had sufficient opportunity to ascertain the identity of these two acids.—*Journ. de Chim. Méd.*, Novembre 1846.

ON THE IGNITION OF BRASS WIRE IN THE VAPOUR OF ALCOHOL.
BY M. REINSCH.

It has hitherto been supposed that the property of igniting in the wick of a spirit-lamp was peculiar to a coil of platina or palladium wire. The author has lately discovered that all other metals possess this property, which do not melt at a red heat, even whether they oxidize or not. If a coil of piano-wire be placed in the wick of a spirit-lamp (absolute alcohol), and it be allowed to burn till the spiral is sufficiently red-hot and then extinguished, the spiral remains red-hot for a few seconds and then ceases to be so. If the lamp be thus lighted for three or four times, the iron wire remains red-hot exactly like a platina wire, and the peculiar odour of aldehyd and acetic acid is at the same time developed. If the experiment does not succeed, the flame of the alcohol must be covered with a glass tube, and not extinguished till this precaution has been taken. M. Reinsch states that he could not succeed at first with brass, silver and copper wire; brass wire should be thin, and the spirals made as close to each other as possible, but they must not touch; the flame of the alcohol also should not be continued longer than is necessary to ignite the brass wire, and after this it ought to be suddenly extinguished to prevent the wire from melting, and to prevent the spirals from touching each other. Copper and brass wire continue as vividly red-hot as platina wire, though they are gradually acted upon by the acetic acid and afterwards volatilized. The silver wire does not remain so long ignited. The wires of the metals mentioned absorbing heat too rapidly, remain but incompletely red-hot, but a thicker iron wire remains red, being not so good a conductor of heat. Silver wire becomes but imperfectly ignited, because being ignited, it becomes so soft that the spirals soon touch each other. The same occurs with a gold wire.

The author also succeeded in producing ignition with charcoal; for this purpose a fragment of charcoal is placed in the wick; the alcohol is removed from the lamp, except sufficient to moisten the wick, which is to be lighted and the flame is to be cautiously extinguished; these experiments, the author observes, explain a fact which has hitherto been an enigma—the ignition of platina and the inflammation of hydrogen gas by spongy platina. Thus in the first experiment, the vapour of alcohol being imperfectly consumed

by the fire of the brass wire still hot, there is produced exactly the heat necessary to continue it incandescent. If the spirals of a brass wire approach each other without touching, the vapour of the alcohol oxidizes, and can act on a large surface; the heat is so much increased by it, that the spirals remain red-hot; if the spirals be further removed from each other, ignition ceases, the heat produced by the imperfect combustion not being sufficient to heat the spirals, which before almost touched.

As to what relates to spongy platina, the globules are surrounded with an atmosphere of hydrogen, oxidation takes place at the same time, heat is consequently developed by the large surface of the spongy platina, and a reiterated contact of the gases so increases the heat, that the gas is inflamed. Platina would not possess this specific quality if it were not so refractory as not to fuse by the heat of inflamed hydrogen.

M. Reinsch has also found that this property belongs to metals which fuse readily and are not capable of being made into wire. He made the following experiment: some asbestos was moistened with a solution of these metals, and after having made it red-hot, and it had become brittle by the ignition, it was moistened with a little alcohol, after which it was cautiously rolled round the wick; the metallized asbestos was kept for some time red-hot in the flame of the alcohol, a glass tube being held over it: asbestos which had been prepared with chloride of platina remained vividly red-hot. Iron answers best for this experiment: asbestos moistened with a solution of sulphate of iron and then immersed in solution of ammonia, and washed in water and dried quickly, remains red like an iron wire. Asbestos moistened with a solution of gold and heated to redness, remains red-hot in the vapour of alcohol like a platina wire. The same occurs when asbestos is moistened with a solution of silver. It has been above stated that gold and silver wires possess the property of ignition but imperfectly; which shows that they are subject to soften readily. A wire prepared with chloride of tin and oxalate of ammonia, remains only a few seconds red, even under the glass tube, the tin being too suddenly converted into oxide. The case is nearly the same with lead, but it remains red rather longer. Cobalt and nickel possess the property of igniting like iron. Manganese and chromium remain red for a very short time. Arsenic is suddenly volatilized, the wire however remains red for a few seconds; the same occurs with mercury. Bismuth possesses this property of ignition in a rather less degree than iron. Cadmium resembles tin. Zinc is the only metal which does not at all ignite.

The author is of opinion that platinized asbestos may become an important substance for the condensation of several gases, as also for the preparation of vinegar.—*Journ. de Pharm. et de Ch.*, Nov. 1846.

ON THE DISTILLATION OF MERCURY. BY M. M. E. MILLON.

When mercury is distilled, it is observable that the volatilization takes place more slowly at the period in which the last portions of

the metal pass into the receiver. If the mercury distilled at the commencement of the operation be kept separate from that obtained at the end of it, it is easily proved that these two portions are of unequal volatility.

Fifty kilogrammes of mercury were distilled, and the first and last kilogramme distilled were kept separate; both of them were re-distilled and then submitted to the following experiment: four small and similar retorts were selected, each of which when half-filled was capable of holding 100 grammes of mercury. Each of the retorts charged with 100 grammes of the metal was immersed into an alloy-bath in fusion; and the heat was raised till the mercury in all of them boiled. The mercury distilled condensed in the neck, and was collected and weighed. The four retorts did not yield equal quantities; but on putting aside those which differed the most, it was easy to select two, which, subjected to the above proof, furnished nearly an equal quantity of mercury. Thus in three parallel operations with these two retorts distinguished by the letters A and B—

		In the same time, in the same bath.	
Retort A. First operation.	Mercury distilled	48·5	grammes.
Retort B.	47·5	..
Retort A. Second operation.	69·0	..
Retort B.	63·0	..
Retort A. Third operation.	66·0	..
Retort B.	64·0	..

It is to be remarked that in these three operations the retort A. always yielded a little more than the retort B. This circumstance was attended to in the experiments which follow.

The following are the differences obtained by submitting to comparative trial in the alloy-bath, mercury taken from the first and last kilogramme obtained from the distillation of 50 kilogrammes of the metal.

The retort A., which yielded the most, received the mercury collected at the close of the distillation; the retort B. that obtained at the commencement.

		Mercury distilled during the same time in the same bath.	
Retort A. First operation.	Containing 100 grms. of metal	19·0	grms.
Retort B.	49·0	..
Retort A. Second operation.	15·7	..
Retort B.	41·5	..

Care must be taken, in these comparative distillations, to moderate the temperature of the alloy-bath as soon as the mercury begins to spot the dome of the retort.

Mercury thus affected in a marked manner in its mode of distillation would appear to possess some difference in its degree of purity; but the author attempted in vain to discover any difference by re-agents; the mercury of the first and last kilogramme was similarly affected in all the examinations to which it was submitted.

It then occurred to the author to try whether the addition of any foreign metal, in quantity so small as to escape analysis, could affect the volatility of the mercury. The results were of considerable in-

terest; the thousandth or even ten-thousandth of a foreign metal is sufficient to cause very characteristic differences in the results of parallel experiments.

A ten-thousandth of lead added to mercury stops its distillation almost entirely. In the following comparative experiments, the mercury which distilled with the greatest difficulty was put into the retort A., which yielded most quickly.

First operation.—Retort A., containing 100 grammes of mercury, to which 1-10,000dth of lead had been added, yielded in a given time by distillation in the bath 5 grammes of mercury.

Retort B., containing the same quantity of mercury without any lead, heated for the same time in the same bath, gave by distillation 67 grammes of mercury.

Second operation.—Retort A., containing 100 grammes of mercury, to which 1-1000dth of lead had been added, yielded in a given time by distillation in the bath 2 grammes of mercury.

Retort B., containing the same quantity of mercury without any lead, heated for the same time in the same bath, gave by distillation 55 grammes of mercury.

Zinc was substituted for lead, and when in the proportion of 1-10,000dth, the mercury distilled was to that obtained without the zinc as 6·5 grms. to 72.

Some metals have no sensible influence on the distillation of mercury: among these are gold, iridium, silver, copper, tin, nickel, cadmium and arsenic. The action of platina is precisely the reverse of that of lead and zinc: it accelerates distillation, but less than lead and zinc retard it; in order that platina may produce this effect, it must be digested in the mercury for a day or two at the temperature of 122° to 176° F.—*Ann. de Ch. et de Phys.*, Novembre 1846.

EASY METHOD OF PREPARING IODIDE OF POTASSIUM.

M. Pypers subjects to a moderate heat a mixture of 100 parts of iodine, 75 of carbonate of potash, 30 of iron filings and 120 of water. The mass is to be dried, and then heated to redness; the resulting reddish powder is to be treated with water, and the solution obtained filtered and evaporated to dryness. One hundred parts of iodine yield 135 of very white, but slightly alkaline iodide of potassium.—*Journ. de Chim. Méd.*, Decembre 1846.

FORMATION OF GLYCERINE. BY M. ROCHLEDER.

If castor-oil be dissolved in absolute alcohol and a current of dry hydrochloric acid gas be directed into the heated solution, the oil undergoes decomposition. When the liquor is agitated with water, after the hydrochloric gas has acted for a sufficient time, an emulsion is obtained, which gradually separates into two portions, one of which is oily and floats, the other is watery and extremely acid; the latter is to be poured off and evaporated by a water-bath. At first hydrochloric acid is disengaged, and there remains a syrupy and yellowish mass

which is to be treated with æther, which dissolves one portion and leaves another which is insoluble: this last is glycerine, and the solution contains the æthers of the fatty acids of the castor-oil.—*Journ. de Pharm. et de Ch.*, Decembre 1846.

ON THE COMPOSITION OF BOHEMIAN GLASS.

According to M. Peligot, the composition of the white Bohemian glass varies but very little in the different manufactories of that country. He has analysed various specimens which were perfectly pure and colourless, and they all yielded very nearly the same results. Their composition was found to be as follows:—

Silica	76
Potash	15
Lime	8
Alumina	1
	100

The composition of the *verre-agate*, or *verre de pâte de riz* of Bohemia is remarkable; it is a simple silicate of potash, the semi-opacity of which is derived from imperfect vitrification, which has left grains of quartz, not fused, interposed in the mass. It contains, according to M. Peligot's analysis,—

Silica	80·9
Potash	17·6
Lime	0·7
Alumina and a trace of iron	0·8
	100·0

This glass does not attract moisture from the air; when long boiled in water it is not acted upon. It differs from the soluble glass of Fuchs in containing about 10 per cent. more silica.

A specimen of Venetian artificial *avanturine* yielded—

Silica	67·7
Lime	8·9
Peroxide of iron	3·5
Oxide of tin	2·3
Oxide of lead	1·1
Metallic copper	3·9
Potash	5·5
Soda	7·1
	100·0

M. Peligot found that a specimen of *verre à glaces soufflées* consisted of—

Silica	67·7
Potash	21·0
Lime	9·9
Alumina	1·4
	100·0

ON OXYPICRIC ACID.—STYPHNIC ACID.

In experimenting on Indian yellow, M. Erdmann discovered a new acid, to which he gave the name of oxypicric acid.

This substance has also been obtained by MM. Boettger and Will, without their being aware of the results of M. Erdmann's experiments. It was procured by these chemists by treating several gum-resins with nitric acid, as for example ammoniacum, assafœtida, &c.; they also procured it from the watery extract of Brazil wood, sanders and yellow wood, and they have called it styphnic acid, from *στυφνός*, *astrigent*.

They prepare it by heating one part of assafœtida with from four to six parts of nitric acid of 1·20. At first they heat it to about 158° to 163° F.; as soon as the violent action which then takes place is moderated, it is to be boiled till all solid matter disappears. When the solution, on the addition of water, deposits a powder which is granular to the touch, the oxidation is finished; if the water separates a flocculent precipitate, the ebullition with the nitric acid must be continued: it is then to be evaporated to the consistence of a syrup, much water is to be added, and after having boiled it, carbonate of potash is to be added as long as effervescence ensues, taking care to add no excess, in order that the resinous portions which the liquor may still contain be not dissolved. After neutralization the liquor is to be filtered and evaporated, and set to crystallize. There then separates a salt of potash which is very slightly soluble, having the form of a brown crust, or of fine needles aggregated in mammillated forms. This salt is to be dissolved and re-crystallized in water, and the oxypicric acid is to be precipitated by nitric acid.

This acid is precipitated in the state of a white powder, or in scales having the form of fern leaves; it is to be washed with cold water and then dissolved in boiling absolute alcohol; from this it separates in prismatic crystals of considerable size.

The properties of this acid are that its taste is neither sour nor bitter, but slightly astringent; its solution, either in alcohol or water, colours the epidermis permanently.

It is yellow, reddens litmus strongly, and readily decomposes the alkaline carbonates. It requires 88 parts of water at about 144° F. for solution: alcohol and æther dissolve it readily. When cautiously heated on platina foil it fuses, and on cooling becomes a radiated mass: at a higher temperature it yields vapours which readily inflame. When suddenly heated it detonates slightly.

Nitric and hydrochloric acids even when concentrated do not act upon it at a boiling heat. It dissolves readily in concentrated nitric acid, and water precipitates it from solution in the form of a white powder.

Aqua regia decomposes it entirely with the production of oxalic acid. Concentrated sulphuric acid when heated also decomposes it. If a piece of very dry potassium be sprinkled with this acid, and it be slightly pressed with a pestle, the acid inflames: sodium does not produce the same effect.

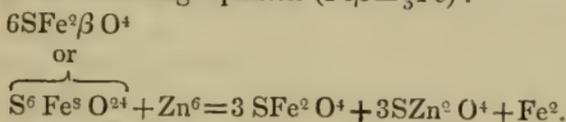
A concentrated aqueous solution of oxypicric acid, especially when

heated, readily dissolves zinc and iron. Sulphuretted hydrogen has no action upon it; but an alcoholic solution of hydrosulphate of ammonia gives it a deep brown red colour. Assafœtida yields about 3 per cent. of this acid.

MM. Boettger and Will found, as M. Erdmann had done, that its composition is $C^6 H^3 N^3 O^8$, or $C^6 (H^3 X^3) O^2$. This acid is bibasic. It is capable of forming several salts with two different bases. All the oxyperates, in common with the picrates, possess the property of exploding with considerable force when heated gently.—*Journ. de Pharm. et de Ch.*, Novembre 1846.

REDUCTION OF THE PERSALTS OF IRON BY ZINC.

Metallic zinc reduces the persalts of iron to the state of protosalts, and precipitates at the same time some metallic iron, often in the state of brilliant scales. According to M. Poumarède, it is equal to one-third of that which remains in solution, the reaction taking place according to the following equation ($Fe\beta = \frac{2}{3}Fe$):



By superoxidizing the mixture of protosalts which results from a first reduction, a fresh quantity of iron may be precipitated; and by successive operations, the whole of the iron of a solution may be precipitated.

If weak solutions be operated upon, there is usually disengaged a certain quantity of hydrogen, which, according to the author, is derived from a secondary decomposition, operated on water by the iron set free; in fact, the zinc is always covered with a very slight coating of oxide of iron.

According to M. Poumarède, the solutions of nickel, manganese, and even of alumina, yield similar results.—*Journ. de Pharm. et de Ch.*, Novembre 1846.

AWARD OF MEDALS.—LINNÆAN SOCIETY.

A Special General Meeting of this Society was held on Friday the 8th of January, to consider the subject of the following Statement and Resolution of Council relative to the Bequest of the late Edward Rudge, Esq., F.L.S.

The Council, after much patient and anxious deliberation, had unanimously come to the following resolution:—

“Resolved,—That in the opinion of this Council, on a full consideration of the terms of the bequest of the late Edward Rudge, Esq., of the interest of a sum of £200, for the purpose of establishing a Medal to be awarded by the President and Council of the (Linnæan) Society, at their discretion, to the Fellow of the said Society who shall write the best communication in each volume which after his (the testator's) decease shall be published by the said Society, in either of the four departments of Natural History, it is inexpedient

to accede to the liberal intentions of the testator under the conditions expressed in his will."

This Resolution, which received the entire concurrence of the President and of every Member of the Council, was chiefly founded on the following considerations:—

The great object of the Linnæan Society, as of all other bodies similarly constituted, is the production and publication of such essays as tend to the advancement of that branch of science which it cultivates. The principal question therefore in reference to Mr. Rudge's bequest, is the manner in which its acceptance would operate on the Society's publications, and the Council has arrived at the conclusion that its tendency would be prejudicial rather than favourable; inasmuch as while the Medal would offer no inducement to some of those Members who have hitherto been in the habit of communicating papers which have had a place in the 'Transactions,' they might, on the contrary, be unwilling to submit their future communications to this new ordeal; and it does not appear probable that the Medal would prove a stimulus to the production of more valuable Essays from any other class of the Society. On the other hand, it is probable that dissatisfaction would arise in the minds of some of those Members, who after contributing papers to more than one volume of the 'Transactions,' should fail in obtaining the award of a Medal.

A second objection to the acceptance of the bequest arises from the absence of any discretionary power of withholding the Medal, which is necessarily to be awarded to the best paper in every volume, and consequently to papers of very unequal value, thereby lowering the character of the Medal, and consequently affecting the scientific reputation of the Society itself.

Differences of opinion, and consequent dissatisfaction, would also be not unlikely occasionally to arise in deciding upon the comparative merits of papers in botany and zoology, the two branches of natural history, of which, for many years past, the Transactions of the Society have exclusively consisted.

Another point may still be noticed as decidedly unfavourable to the acceptance of the bequest, namely, the not improbable award of the Medal by the Council, in some cases to one of its own body, in strict conformity with the conditions of the will; conditions which neither the Council itself, nor (as it appears from the tenor and provisions of the will) any other party has the power to modify.

These objections have appeared to the Council so important as not to admit of any other course but that of respectfully declining to accept a bequest, the operation of which would in all probability be injurious to the best interests of the Society, by lowering the character of its publications, and endangering the continuance of that harmony which has hitherto prevailed in all essential points. The Council is at the same time deeply sensible of the kind and liberal intentions of Mr. Rudge, and entertains a sincere regret that the express terms of his will should have rendered the acceptance of his bequest liable to such grave objections.

The meeting was numerously attended, and the President (the

Bishop of Norwich) having read from the chair the above Statement on the part of the Council, some of the Fellows who had been Members of the Councils of the Royal and Geological Societies stated their opinion of the inconvenience and injurious tendency of the awarding of medals in those Societies, and their inutility for the promotion of science; after which the Resolution proposed by the Council was unanimously approved and adopted.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1846.

Chiswick.—December 1, 2. Foggy. 3. Cloudy: frosty 4. Sharp frost: fine. 5. Cloudy: clear and fine: overcast. 6. Clear: cloudy. 7. Fine: cloudy. 8. Cloudy: fine. 9. Slightly overcast: drizzly: slight rain. 10. Rain. 11. Frosty: snowing: clear and frosty. 12. Frosty: cloudy: clear and frosty. 13. Frosty: cloudy: severe frost at night. 14. Severe frost: clear and frosty throughout. 15. Severe frost: densely overcast: clear and frosty. 16. Sharp frost: clear and cold: frosty. 17. Densely overcast: fine: slight snow. 18. Sharp frost: clear: overcast. 19. Rain: foggy. 20. Rain: cloudy. 21. Rain: clear and frosty at night. 22. Slight frost and fog: fine: clear. 23. Rain. 24. Foggy. 25. Frosty: clear. 26. Clear and frosty. 27. Frosty: cloudy: clear and frosty. 28. Frosty and foggy. 29. Slight frost: overcast. 30. Densely overcast: frost at night. 31. Sharp frost: foggy.

Mean temperature of the month	31°·26
Mean temperature of Dec. 1845	40 ·41
Average temperature of Dec. for the last twenty years	40 ·04
Average amount of rain in Dec.	1·58 inch.

Boston.—Dec. 1. Fine: snow on the ground. 2. Cloudy: snow on the ground. 3. Fine: snow on the ground. 4. Cloudy: snow on the ground. 5. Fine: snow on the ground: rain P.M. 6. Fine. 7. Rain. 8. Fine: rain P.M. 9. Cloudy: rain P.M. 10. Fine. 11, 12. Snow: snow on the ground. 13—16. Cloudy: snow on the ground. 17. Cloudy: snow early A.M. 18*, 19. Cloudy: snow on the ground. 20. Fine: snow on the ground. 21. Cloudy: snow on the ground. 22. Fine: snow nearly all gone: melted snow. 23. Cloudy. 24. Snow: snow on the ground. 25, 26. Fine: snow on the ground. 27—30. Cloudy: snow on the ground. 31. Cloudy: snow on the ground: melted snow.

Sandwick Manse, Orkney.—Dec. 1. Showers: clear. 2. Bright: sleet-showers. 3. Hail-showers: sleet-showers. 4. Bright: showers. 5. Showers: sleet-showers. 6. Sleet-showers: cloudy. 7. Rain: clear. 8. Drizzle: cloudy. 9. Drizzle: shower: clear: aurora. 10, 11. Snow-showers: snow-drift. 12, 13. Snow-drift: snow-showers. 14. Snow-showers. 15. Snow-showers: snow-drift. 16. Snow-showers. 17. Snow-showers: snow: clear. 18. Thaw: quick thaw. 19. Frost: showers. 20. Bright: clear: hoar-frost. 21. Rain: showers. 22. Hail-showers: frosty. 23. Hail-showers: clear: aurora. 24. Hail-showers: cloudy. 25. Clear: cloudy. 26. Bright: cloudy. 27. Bright: rain. 28. Bright: drizzle. 29. Drizzle: clear. 30. Fine: clear: halo. 31. Drizzle.

Applegarth Manse, Dumfries-shire.—Dec. 1. Thaw. 2—4. Hard frost. 5. Wet. 6. Frost: clear. 7. Frost. 8. Frost, but dull. 9. Frost, slight. 10. Thaw: slight snow. 11—14. Keen frost. 15. Keen frost: slight snow. 16. Keen frost: sleet. 17. Keen frost: slight snow. 18. Frost A.M.: rain P.M. 19—21. Wet. 22. Frost, keen. 23. Frost, keen: slight snow. 24. Frost, keen: more snow. 25, 26. Frost, keen. 27. Thick fog: frost. 28. Thaw: fog: rain. 29. Thaw: thick fog. 30, 31. Thaw: fog.

Mean temperature of the month	33°·5
Mean temperature of Dec. 1845	39 ·5
Mean temperature of Dec. for twenty-three years	38 ·3
Mean rain in Dec. for eighteen years	3 inches.

* Not so cold a day in December since 28th December 1829, which was 16°·5. Not so cold a month of December for twenty years at least: December 1829 was very cold—average 33°·1.

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XXVIII. *Reply to the Astronomer Royal on the New Analysis of Solar Light.* By SIR DAVID BREWSTER, K.H., D.C.L., F.R.S. and V.P.R.S. Edin.*

THE remarks of Mr. Airy in the last Number of this Journal, on my Analysis of the Solar Spectrum, have, I doubt not, given as much surprise to its scientific readers as they have given to me, and this independently of the correctness or incorrectness of the results to which they refer. When a philosopher examines, and pronounces an opinion upon the researches of others, especially upon those which competent judges have recognised as sound, he is bound to repeat the identical experiments which he challenges, with similar apparatus and similar materials; to state the differences which he observes, to inquire into the causes by which such discrepancies have arisen, to establish his own views by new and effective experiments, and to publish his researches in vindication of his charges against a fellow-labourer in science. Mr. Airy, however, has not done this; but, as we shall presently see, has followed a course which is as unusual in the history of science as it may be injurious to its progress.

The question (the only one as Mr. Airy says) to which these observations bear reference is this: "*Is it established that the colour of any portion of the spectrum is changed by the use of any absorbing medium?*" To this question I and others have given the answer, that *absorbing media do change the colour of different portions of the spectrum.* I give the same answer still; and rather than believe that I have been blind for the last *fifty* years, I will hazard the supposition that the Astronomer Royal cannot distinguish colours, and is a genuine specimen of an *idiopt*, the name which his friend Dr. Whewell

* Communicated by the Author.

Phil. Mag. S. 3. Vol. 30. No. 200. March 1847.

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has invented to designate the unfortunate members of the colour-blind community.

But before I proceed to give a notice of the analysis of the spectrum by absorption, I must refer to the *mode* of making the experiments. Mr. Airy says that "*the eye has no memory for colours,*" and that it is therefore necessary to compare *immediately* the *modified* with the *unmodified* spectrum. It may be true that *the eye* has no memory of any kind, and therefore not for colours; but I know that I *have a memory* for colours, and that the colours are so much committed to my memory by fifty years' schooling, that without any immediate comparison I can tell whether or not the *green* space has become *yellower*, and whether or not, by certain combinations of absorbing media, I see before me a portion of *white* light standing close to a portion of *red* light in the spectrum, like an *almond* and a *cherry* stuck together.

But, apart from this consideration, I beg to remind Mr. Airy that I was the first person to use the methods which he considers his of placing the *modified* beside the *unmodified* spectrum, by employing the same slit for both; and that I showed it to him *fifteen* years ago, when he did me the honour of paying me a visit in Roxburghshire. In my paper On the Lines of the Spectrum, read to the Royal Society in 1833*, I write thus, when speaking of the coincidence of the Nitrous gas lines with those of Fraunhofer:—

"In order to afford ocular demonstration of this fact, I formed the solar and the gaseous spectrum *with light passing through the same aperture*, so that the lines in the one stood opposite those on the other, like the divisions on the vernier and the limb of a circle, and their coincidence or non-coincidence became a matter of simple observation. I then superimposed the two spectra when they were both formed by solar light, and thus exhibited at once the two series of lines, with all their coincidences and all their apparent deviations from it. Professor Airy, to whom I showed this experiment, remarked that he saw the one set of lines through the other, which is an accurate description of a phenomenon perhaps one of the most splendid in physical optics, whether we consider it as appealing to the eye or to the judgement."

On the 15th of April 1822, I laid before the Royal Society of Edinburgh a paper, in which I describe specific experiments, *proving* that the colour of parts of the spectrum is changed by absorbing media, and that a *distinct yellow band* can be insulated on the most refrangible side of the line D†. About the same time Sir John Herschel addressed a letter to me On the Absorption of Light by Coloured Media, &c.,‡

* Edinburgh Transactions, vol. xii. p. 525.

† Ibid. vol. ix. p. 433-444.

‡ Ibid. pp. 445-460.

containing experiments from which he drew the same conclusion that I did, as appears from that letter, and from the following passages in his Treatise on Light. Viewing the spectrum through a piece of smalt blue glass 0·042 of an inch thick, he makes the following observation:—

“This RED (the innermost of the two red bands)** is free from the slightest shade of ORANGE: its most refracted limit came very nearly up to the dark line D in the spectrum. A small sharp black line separated this RED from the YELLOW, which was a pretty well-defined band of GREAT BRILLIANCY AND PURITY OF COLOUR, of a breadth exceeding that of the first RED, and bounded on the GREEN side by an obscure but not quite black interval.”—Art. 496.

Upon this distinct and well-described observation, which is precisely what I have witnessed a hundred times, Sir John reasons as follows:—

“The two reds noticed in Art. 497 have absolutely the same colour, and cannot be distinguished. On the other hand, the transition from pure red to pure yellow, in the case there described, is quite sudden, and the contrast of colours most striking. * * * * What then, we may ask, has become of the ORANGE; and how is it, that its place is partly supplied by RED on one side and YELLOW on the other? These phenomena certainly lead us very strongly to believe that the analysis of white light by the prism is not the only analysis of which it admits. * * * * This idea has been advocated by Dr. Brewster in a paper published in the Edinburgh Transactions, vol. ix., and the same consequence appears to follow from other experiments* published in the same volume of that collection.”—Art. 506.

Having had occasion in the course of the ten years subsequent to 1822 to view the spectrum through, literally, hundreds of solid, fluid, and gaseous bodies in different combinations, I was led to the generalisations which I have described in a paper On a New Analysis of Solar Light, read to the Royal Society of Edinburgh on the 21st of March 1831†. I endeavoured to prove by specific experiments that the solar spectrum was compound, and capable of analysis by absorbing media, and I succeeded in insulating a portion of white light, which the prism could not decompose. But as these points are not now at issue between the Astronomer Royal and me, I shall make no further allusion to them.

Nearly three years after the reading of this paper, on the 6th of January 1834, the President and Council of the Royal Society made the following announcement:—

“The President announced that the Council had resolved to award the Keith Biennial Prize, for the second period, to Sir David Brewster, for his paper On a New Analysis of Solar Light.”

Being then resident in the country, I had no knowledge whatever of the intention of the Royal Society to do me this

* Edinburgh Transactions, § 5. p. 449.

† Ibid. vol. xii. p. 123.

honour. No doubts were expressed to me about the accuracy of my experiments, and no explanations asked; and being accidentally in Edinburgh on the 6th of January, the reading of the announcement at the meeting of the Royal Society that evening was the principal intimation I received of the adjudication of the prize. Prof. Forbes, the Secretary to the Physical Class, was then the representative of physical science in the Council. I knew that he had made many experiments on the action of absorbent media on the solar spectrum, and I have no doubt that he observed the great fundamental fact described by Sir John Herschel and myself, which, if a truth, settles the question between Mr. Airy and me.

It would appear that in the year 1833 Mr. Airy made experiments on the spectrum. He could not see any change of colour in the spectrum produced by absorbent media, and he mentioned this result ORALLY, and from recollection, at a meeting of the Cambridge Philosophical Society. He did not consider his experiments worthy of being preserved, for he did not even copy them from his pencilled notes: he did not think them deserving of publication; and yet Dr. Whewell, the Historian of science, takes them up *four* years afterwards, and setting aside Sir John Herschel's experiments, which he may not have known, and mine which he did know, and holding cheap the decision of the Royal Society, of which he probably was also ignorant, he adduces the experiments of Mr. Airy, without mentioning his name, as hostile to mine, by stating that my facts were "denied by *other experimenters*."

This gratuitous challenge of the accuracy of my experiments, on anonymous authority, was animadverted upon in one of the reviews of Dr. Whewell's work; and upon publishing a second edition of it, he naturally applies to his anonymous experimenter, who turns out to be Mr. Airy. Without making a single new experiment, without even having recourse to his pencil notes, which he seems to have lost, the Astronomer Royal authorizes the Master of Trinity to refer again to his experiments as invalidating my analysis of solar light. Dr. Whewell has of course done this; but he has done it in a note so exceptionable in its argument, and to me so unintelligible in its interrogatories, that I am most unwilling to criticise it. He asks me, for example, if it is meant, that is, if I mean, that Newton's experiments *prove nothing*? Who ever said that they proved nothing? Newton's *prismatic* analysis of the spectrum is, in my opinion, one of the greatest of his discoveries. "Or," he adds, "is Newton's *conclusion* allowed to be true of light which has not been analysed by absorption?" To this I reply, that Newton's conclusions respecting the apparent

division of the spectrum into seven colours, and their difference of refrangibility, is not in the slightest degree affected by the results of my analysis. "And where," he continues, "are we to find such light since the atmosphere absorbs?" I again answer, that Newton's prismatic analysis must, of course, be the analysis of light after it has experienced all the actions which it has undergone in passing through the atmospheres of the sun and the earth; but if I misapprehend the object of the question, it may be otherwise answered by stating that light which has not suffered absorption, and which does not want a single definite ray, may be found in the white artificial flames with which we are all familiar. I have distinctly stated in my original memoir, what Dr. Whewell seems not to have read, that Newton's analysis of light by the prism is perfect, so far as it goes; but Newton committed a mistake, if mistake is the proper term, when he asserted that to the same refrangibility always belongs the same colour.

Having authorized Dr. Whewell to refer again to his experiments on absorption, the Astronomer Royal has thought it necessary, in order to justify the reference, to publish his *recollections* of these experiments. This is the first time in the annals of science that the *recollections* of experiments have been given to the world. A philosopher in his decline, when his failing sight and his trembling hand are no longer fit for the delicate operations of experimental inquiry, may be excused for calling up the recollections of his manhood in support or in refutation of some exciting speculation; but no apology can be made for those who, with the means and the leisure for repeating their experiments, bring forward their recollections to discredit or to overturn the researches of others who have laboured patiently and successfully in the same field of scientific research.

In the present case these *recollections* have a still more peculiar character: they are admitted by their author to be "only negative." "Of the results of these experiments," says the Astronomer Royal, "I can give little more than the single negative one,—that no change was produced in the qualities of the colours." These *recollections* have still another peculiarity. Relating as they do to colours, they are the recollections of a person who confesses that he has no memory for colours, and so imperfect are they that he forgets the names of the miscellaneous contents of a chemist's shop through which he viewed the spectrum, and he remembers only the "port and porter" of his own cellar. Mr. Airy cannot even tell us what he actually saw when he absorbed the sun's rays by smalt-blue glass, port and porter. He does not describe the spectrum thus

produced; but merely tells us that the qualities of the colours were unchanged. Sir John Herschel, as we have seen, made the experiment with the same glass carefully, and describes the results of it minutely; and I have done the same; and both of us have found *that the qualities of the colours were decidedly changed*. But Mr. Airy may perhaps have made the experiment more skilfully than we did; and he takes credit for comparing the modified with the unmodified spectrum, and with excluding all extraneous light,—precautions which others have taken far more carefully and effectually than he did. With all these precautions, however, he neglected the most important. He received his spectrum upon a *paper screen*! and while he was viewing it, his retina was influenced by all the various colours which shone in his modified and unmodified spectrum. In the experiments of Sir John Herschel, I believe, but certainly in my own, not a ray of light entered the room but what passed through the narrow slit; the *retina was our screen*, and the absorbing medium was held close to the eye and immediately behind the prism, the *only method* of obtaining the purest spectrum from a given prism. I have now before me a modified and an unmodified spectrum. I can compare any portion of the one with any portion of the other, or I can examine it alone; and so completely are the colours changed with the combination of absorbents which I employ, that I should regard the person as colour-blind who does not see the change. In the *striped* spectrum which I have described in my paper, the phænomena are still more beautiful and instructive.

But it is not merely by absorbent substances that the qualities of the spectral colours may be changed. I have obtained, during a series of unpublished experiments, similar effects from the interference of pencils transmitted through perfectly colourless media, and prismatically analysed.

What, then, is the result of all this discussion? Sir John Herschel and I have minutely described certain experiments, in which we have proved to our own satisfaction and that of many others, that absorbing media change the colours of portions of the spectrum. Mr. Airy, by inferior methods as I think, but by superior methods as he thinks, recollects having, fourteen years ago, found that these colours were not changed! The Master of Trinity records the last of these results as undoubted scientific truth, and strives to transmit it to the latest posterity.

XXIX. *Additional Observations on Hail, and on the Organic Bodies contained in Hailstones, &c.* By AUGUSTUS WALKER, M.D.*

[With a Plate.]

IN a preceding Number of this Journal (August 1846) I have stated the results of a microscopic observation of hailstones previous to their fusion, and likewise of the water resulting from them. It was then found that this water contained particles of different shapes, which from their nucleated structure, globular form, and general appearance, were evidently of an organized nature. Since making these observations I have had an opportunity of repeating them, which occurred on the 1st of August, when London and the country in general were visited by that tremendous storm which has been the occasion of so much damage.

Aug. 1.—In my neighbourhood (Kensington) there occurred several falls of rain, which continued, with a few intervals, from about 4 P.M. to late in the evening: the fall of water was accompanied with frequent peals of thunder and flashes of lightning. With the drops of water particles of hail were occasionally found, which rarely reached the size of a bean. No damage was done to the hot-houses in this locality. One of these hailstones, perfectly white, when examined under the microscope in the ordinary way, presented the usual appearance of being composed of minute particles of ice, some spherical, others of a more irregular shape, and numerous bubbles of air escaped as it melted. The organized substances contained in the drop of water which resulted were numerous. Most of them were of an irregular shape with angular outlines. Others were globular; and some of these were completely black, with a nucleus in the centre. With higher magnifying powers were seen green globules about $\frac{1}{5000}$ th of an inch in diameter, which were either collected in clusters or adherent together in single lines, like the beads of a rosary, as represented in fig. 1 of my former paper on hail.

In order to preserve the drop of water for a leisure examination free from dust in the atmosphere, I placed it in a little apparatus nearly air-tight, which will be described hereafter.

Aug. 2.—The drop of water thus secured and left under the microscope was found unaltered in size, but the organized matter had entirely altered its appearance. Some of the organized particles had given off filaments in various directions, as represented in Plate I.†, presenting the ap-

* Communicated by the Author.

† The magnifying powers under which the objects are represented in this plate are as follows:—Fig. 1, magnified 360 diameters, fig. 2, 200 diameters, and fig. 3, 300 diameters.

pearance of hollow tubes, jointed at intervals, with a few branches or subdivisions generally arising at the joints. The particles which gave rise to this vegetation appeared to me to belong to those which I have before described as the irregular angular particles. Among these one or two infusoria were likewise seen, moving about with great rapidity. When at rest their form was oval (A), but when in motion their anterior extremity frequently became elongated, as at B. Their organization was extremely simple; no filaments nor appendages of any kind were to be detected about them. Their interior had a granulated appearance. All these characters indicate that they belong to the genus *Uvella* of the Monadinæ, and probably of the species *Uvella glaucoma* (Ehrenberg). C represents one perfectly circular, which was in a state of constant motion, revolving round its centre.

Aug. 3.—The vegetable filaments had greatly augmented in number. Small elongated cells, as represented, could be seen at various places, as if forming the first elements of the tubulous branches spread out around them. The infusoria had likewise greatly increased in number.

Aug. 4.—The same appearances were presented as the day before. Scarcely any alteration could be detected in the vegetable and animal bodies: the last were as active in their movements as before. I purposed to continue these observations every day, in order to ascertain their future changes; but unfortunately by some accident one of the slips of glass belonging to the apparatus was broken, and in consequence the water quickly dried up, so that when next examined the infusoria were found to be dead. The further addition of water was ineffectual to restore the vegetable particles to life.

For the purpose of repeating these observations, and likewise for many others of a microscopic nature, the little apparatus which I have made use of will be found very serviceable. It consists of a small square piece of sheet caoutchouc, more or less thick according as it may be required, with an aperture cut in the centre about the size of a shilling. On each side of it are placed two slips of glass of the same size, which, by a moderate degree of pressure, may generally be made to adhere to it so strongly as to render the little air-chamber, where the india-rubber has been cut away, perfectly secure from all access of dust and nearly air-tight. When the glass does not easily adhere to the caoutchouc, this last may be rendered sufficiently glutinous by heating it moderately. Instead of glass the plates may be of thin mica, which adheres still better; and from being so much more thin, will allow of the employment of the highest magnifying powers without the possibility of the access of foreign matter. I have sometimes

used a metallic framework with a screw, by means of which the whole is rendered perfectly air-tight, and the glasses may be pressed nearly into immediate contact, but in most cases this is superfluous. As an instance of its efficiency in preventing all evaporation, I may state that I have preserved vaccine lymph in a fluid state in this manner during more than two months, with merely the assistance of a degree of pressure on the glasses, in the first instance secured by a few turns of wire. This is decidedly the most effectual mode of preserving this substance fit for use at any moment. A few of the same kind of apparatus in the pocket-book will be found most useful companions to the microscopic observer, when the slips of glass in ordinary use are inapplicable.

Although the existence of organized particles in the upper regions of the atmosphere appears at first very improbable, we shall find that several facts exist which much decrease this apparent improbability, and even some that appear to corroborate this statement. M. Boussingault has found that organic matter may invariably be detected in the atmosphere. It is true that his experiments have been confined to the lower atmospheric strata, and it would be very desirable to ascertain whether they hold good for those which are several thousand feet above the sea. The same may be detected in rain-water; and what is still more to the point, I find that M. Girardin (*Académie des Sciences*, April 1834) has detected in the water resulting from the fusion of hailstones, "a strongly azotized organic matter, and also lime and sulphuric acid without any trace of ammonia being present." The frequent vegetations in snow show at what a low temperature the vital principle may be sustained. The red and green appearance assumed by it has been found to be produced by the *Hæmatococcus nivalis* and the *Protococcus viridis*, as these plants have been termed, and which are probably the same species at different periods of growth, which have been found to penetrate many inches below the surface of the snow. These vegetations are found under the microscope to consist of minute globules, some green and transparent, about the size of $0^{\text{mm}}\cdot 01$ to $0^{\text{mm}}\cdot 05$; others red, generally larger than the former, and frequently arranged in single rows, like the beads of a necklace, adherent together. The globules are found of all intermediate tints between the green and the red, and likewise others of larger dimensions of a red colour, about $0^{\text{mm}}\cdot 05$ to the $0^{\text{mm}}\cdot 055$, which have a more compound structure, as within them are seen several granules or smaller globules, which was not the case in the two former. M. Martens states, that in some of these globules microscopic infusoria may be detected (see

Kæmtz's Meteorology). I am not aware that the origin of this vegetation in snow has ever been satisfactorily accounted for; but when we consider how widely this vegetation is diffused, and the depth to which it penetrates, it does not appear unlikely that it may have fallen with the snow, in the same manner as the organic bodies in hail are brought from the upper regions of the atmosphere. Further observations must determine how far these surmises are correct, and whether these bodies are not the same in both cases.

Again, in the hands of Ehrenberg the microscope has been the means of detecting, in dust fallen on ships in the Atlantic, the remains of above sixty different species of infusoria. Mr. Darwin, in his interesting paper in the Transactions of the Geological Society, mentions that dust has been collected in this manner at a point midway between Senegal and Cayenne. The distance to which volcanic ashes may be conveyed by the higher atmospheric currents, in a direction contrary to that of the lower prevailing winds, is well known. These facts being duly weighed, the frequent and even habitual existence of minute cryptogamic sporules and infusorian ovules in the higher regions of the atmosphere, where hail is formed, will not appear more incredible than the continued suspension of globules of water composing clouds; which globules, as measured by diffraction, are found of far greater size than the former. The relation between the presence of these particles and other phænomena of meteorology remains to be determined, and how far they may be regarded as accompanying and detector agents of other atmospheric conditions following or preceding them; and whether the infusoria and the cryptogamia are the same at all seasons of the year; whether the locality in which they are collected has any influence; and lastly, whether they are the same in hail and water falling at about the same time.

In meteorology water likewise presents itself in what is termed the vesicular, as in clouds and fogs, in the condition of rain, snow and frost. In a paper which was read before the Royal Society, and which will appear in the Philosophical Transactions, 1847, will be found some experiments of mine upon water in the first-mentioned condition; with regard to the others I shall confine myself to the following remarks.

The transformation of fogs into rain is frequently taking place around us. Fogs, which at first are what is termed dry fogs, scarcely possessing the power of moistening surrounding objects, after a time are found to deposit numerous small globules of water, which as long as they remain attached to minute filaments retain their spherical form, but when in con-

tact with other bodies moisten them like common rain. This may be regarded as similar to the formation of rain in clouds, except that it is on a much smaller scale, as the thickness of the stratum of the fog is much less than the strata of clouds, as measured by M. Peytier.

We may examine the minute particles of rain or fogs, as I have already shown, by passing the air which contains them through essence of turpentine, lowered a few degrees below the common temperature. They are then left behind in the liquid; but at the same time they are mixed with others which arose from the condensation of the water contained in the air in an elastic condition. Difficulties exist with regard to the measurement of the globules while floating about. This is an objection to the employment of this method of fixing the globules. I have therefore endeavoured to use another means of observing these particles, and for this purpose have examined them as they exist on the filaments which compose the spider's web.

In order to examine these globules *in situ*, it will be found convenient to make use of two square frames of sheet cork of equal size, about two or three inches square. These are to be placed on each side of the web, then pressed together and bound. The whole of the web interposed may then be removed with the globules *in statu quo*, which may then be submitted to the microscope and measured. The globules may sometimes be as small as $\frac{1}{1500}$ th of an inch, which is less than the globules of the clouds in some instances as measured by Kæmtz. But generally these globules are much larger. They will be found placed at regular intervals, and while undisturbed show little tendency to coalesce; but when taken into a warm room they appear to lose with the increase of temperature, their cohesion to the line, and easily run off, coalesce with each other and rapidly evaporate; while so doing, instead of a globular form they become elongated ellipsoids, with the long diameter in the direction of the spider's line running through them. In the same fog I have found these globules always of the same dimensions. When the webs are examined over the extent of one or two miles at least, but in different fogs, their sizes are very different. It is necessary to warn the observer respecting an error that might be easily made in examining the web of the common spider, because in its natural state it is found covered with minute globules or beads, which contribute probably to secure the prey of the animal. These beads are arranged with great symmetry. Thus at nearly equal distances will be found globules, as in fig. 2, and between them others smaller dis-

posed with equal regularity. We may distinguish them readily, inasmuch as the globules of water soon disappear, whereas these beads are permanent, and unaffected by the vacuum of the air-pump. Mr. Blackwall has described them fully in the Transactions of the Linnæan Society.

Numerous particles of an organic nature may be detected in rain. But this investigation is liable to many errors which do not exist with regard to hail; because a solid body of ice will remain completely free from all foreign particles in the lower strata, and only contains those which were fixed within it at the moment of its solidification; while in rain we find a mixture of foreign bodies derived from various sources, which the drop of water meets on its passage from the point at which it is formed to the moment of reaching the ground, such as dust raised by the wind, pollen of flowers, and in the neighbourhood of towns, particles of soot continually floating in the atmosphere. It therefore requires great care to avoid coming to erroneous conclusions respecting the nature of the body under examination, and also to detect the existence of organic particles in the midst of others which mask its presence. For these reasons I avoid entering any further upon this subject at present, although deposits of rain-water have presented under the microscope several particles of an organic nature.

The most prevailing forms which the particles of snow in flakes assume, are minute acicular crystals irregularly grouped together, small regular rhombic prisms, often modified by secondary planes, and double six-sided pyramids, like the crystals of sulphate of potash. The more regular particles are generally found deposited on others more irregular, arranged in wreaths and various other shapes. A globular nucleus with small pyramids upon it is a frequent form in flakes of snow. Separate particles of snow affect much more regular forms. In these the hexagonal form, or some tendency to assume it, is generally remarked; and examples of all the forms given in Scoresby's voyage, and of others still more complex, may be detected by the microscope in snow of this climate. The spongy deposit formed on the outside of vessels containing a freezing mixture is found under the microscope to present crystalline forms, which resemble the common forms found in snow much more than those of any other deposit, such as frost, or the arborescent deposit often found on the windows in winter.

In these experiments the hygrometric conditions of the atmosphere, the degree of refrigeration, and even the rapidity with which heat was abstracted, appeared to exert great influ-

ence in modifying the crystalline forms that were obtained. In the same fall of snow a great uniformity is found in the forms of the separate crystals and of those which compose the flakes: sometimes there is only one prevailing shape, at others several very different distinct forms exist, falling at the same time. At the commencement of the fall the forms are generally more regular. In different falls of snow the forms of crystallization are scarcely ever alike or liable to be confounded; and even after a temporary cessation these are found to alter. In every respect we find the closest analogy between the formation of these crystals and those obtained from solutions in which the temperature, degree of condensation, and repose of the liquid, are found to have such influence. Some observations made on separate occasions will better elucidate these remarks.

Jan. 7, 1846.—There was a fall of snow, the flakes of which consisted of minute crystals of an octahedric form, placed on acicular ramifications of an irregular description. Besides these, small globules of ice studded with minute pyramidal crystals were perceived under the microscope. The snow examined at intervals was found to contain spherical globules without projecting crystals, which towards the end of the fall of snow gradually increased in size, and might be termed sleet, as they were visible to the naked eye: subsequently the sleet gave place to rain, consisting of minute drops. It is impossible to account for the formation of the mace-like particles of snow, without admitting, as in my experiments, two distinct periods or stages in their development. In the first stage the globular particle is deposited, and in the second the projecting pyramids are formed from moisture, condensed from either a gaseous or a liquid state.

Dec. 11, 1846.—A heavy fall of snow commencing early in the morning, which, after a temporary cessation at about 11 A.M., continued until 2 P.M. Many of the crystals were very regular, thin plates, which presented a hexagonal arrangement; some closely resembled fig. 15 of Scoresby's figures, as given in Kæmtz's Manual, without the projecting radii. Some were composed of a very thin hexagonal star, around which nearly circular apertures were disposed at regular intervals, and at the exterior of these six radii. The flakes were composed of irregularly-grouped particles; some of a lance-shape, size $\frac{1}{400}$ th of an inch; others regular six-sided prisms, like those of frost, about $\frac{1}{300}$ th of an inch; and lastly, others nearly globular.

Dec. 12.—At about 10 A.M. there was a slight appearance of snow during nearly an hour. It consisted of separate par-

ticles, which, from their size, form and colour, nearly resembled the eggs of the common blow-fly. At about 2 P.M. the snow fell more thickly, under the form of small conglomerations or balls, varying from the size of half a pea to three or four times larger. Pressed between the fingers, some of these pellets of snow were easily crushed; only a few offered any degree of resistance. To the naked eye they appeared to consist of spongy crystalline particles: under the microscope they were found to be composed of small regular crystals nearly cubical, mixed with others more irregular of complex outlines: the regular forms were exactly similar to the crystals of frost and the cube-like particles found in the snow on the day preceding. One of these balls submitted to the microscope on a slip of glass, began slowly to liquefy, from the contact of the glass and of the surrounding air. As this was taking place the liquid was diffused by capillarity over the entire mass, without any trace of moisture appearing externally until the entire ball was in a state of semi-fusion, and of a dull and semi-transparent white like a small hailstone: at the same time the ball contracted in size and assumed a biconvex or lenticular shape. While these changes were taking place the crystals became fused at the angles and edges, most of them becoming globular and ovular: in this condition several of the balls were exposed to the open air, then several degrees below the freezing-point. When examined the next morning they were found perfectly hard, of a regular lenticular shape, of a dull white colour, in every respect like certain hailstones which I had collected during the summer. Under the microscope elliptical and circular particles were seen, like those composing hailstones. Previous to and during their fusion, bubbles of air were seen exactly as in the hailstones, though in less abundance. Some of these were enclosed, as represented at p. 105, vol. xxviii. *Phil. Mag.* This observation shows in a direct manner, with a much greater degree of precision than I could have anticipated, the process of formation of hail as it takes place in general. In the first place, an agglomeration of crystalline particles, probably in consequence of certain vorticose currents, takes place in a globular shape. The ball of snow, thus formed by the capillary attraction which it possesses from its porous structure, absorbs any moisture that may exist around it. The effect of this moisture is twofold; first, as modifying the angular outlines of the component particles; and secondly, as binding them together into a consistent mass, which any further abstraction of caloric will reduce to the state of a hard and solid hailstone. If the addition of moisture is considerable, the liquefaction of the

snow may continue until all traces of separate crystalline particles have disappeared and the air has been disengaged, the whole becomes transparent, and still capable, by its low temperature, of easily solidifying and forming a transparent mass of ice. In either case the deposition of successive layers of transparent ice, or of confused solid grains, may be accounted for by a continuance of the same actions.

In order to exemplify this, it is not requisite to confine ourselves merely to snow already in a globular state: any kind of snow slightly pressed into a globular shape becomes hardened and modified by diffusion of water through it, in consequence of slight melting of its particles. According as this is more or less continued, we obtain all the dull white tints of various hailstones. If the mass thus obtained be now exposed to the air below the freezing-point, the process is still further completed by its entire solidification.

It still remained to be seen whether the conglomerations of snow contained, like hail, any organic particles. I was desirous to determine this point, because I considered them as being much less liable to contain foreign matter, such as dust &c. of the lower strata of air, than the common flakes of snow: at the same time we must admit the possibility of the introduction of foreign particles, even after the entire formation of the ball, much more than in a solid body of ice like a hailstone.

In order to obtain a ball as free from impurity as possible, one of the hardest was selected, and after the exterior parts had been carefully brushed off, it was inclosed in the little apparatus before described. Several conglomerations were operated upon in this manner. In these the foreign particles were numerous; they all appeared irregular and of an amorphous inorganic nature, except some green globules closely agglomerated together, some in large quantities, others only consisting of groups of three or four: their size was about $\frac{1}{3000}$ th of an inch, and they were very similar to some found in hail, the vegetation of which was not examined (see p. 105, vol. xxviii. *Phil. Mag.*). The drops of water resulting, with their contents, were examined every day. The first two days they were kept in a room which averaged a few degrees above congelation; no alteration of the particles was detected. The third day they were exposed to the rays of the sun: on the fourth day the green globules had emitted slight filaments. Every succeeding day these filaments increased, and near them were transparent granules scattered about which increased very slowly, as represented at fig. 3. These vegetations were preserved for more than a month by occasion-

ally adding a drop of distilled water, so as to prevent their complete desiccation. At the end of this time the group of vegetation had but slightly increased, but around were several other groups of vegetations of the same nature which were not perceived at the commencement of the examination. Their examination, which hitherto had been made nearly every day, was then abandoned as presenting no further object.

As it is possible to distinguish snow of different formations, its minute examination made simultaneously by different observers over a considerable extent of country, would enable us in all probability to determine several interesting points connected with the formation of snow. By their aid we might ascertain to what distance in certain cases the same crystalline form is preserved, and in what manner this is effected; whether by a simultaneous formation, or by a gradual propagation from one point to another; also at what rate of speed this is effected.

The frequent transformation of the vapours of water from the gasiform to the nebular state, commencing from a small cloud at the horizon and rapidly extending to the whole visible sky, bears a close analogy to the action of crystallization of bodies in solution in a liquid. In both cases a molecular action commencing from a circumscribed point is gradually propagated to the entire mass, when at a proper degree of saturation in the liquid solution the crystals formed are all alike. In the meteorological phenomenon, as it is a liquid that falls, the crystalline action cannot be observed; but the frequent production of hail renders it not unlikely that the clouds may sometimes be composed of solid particles.

From their connection with the preceding remarks I will mention the following experiments:—

Exp. 1. A saturated solution of sulphate of soda covered with a thin coating of oil was allowed to cool, and remained at rest for several hours. On a glass rod being brought in contact with the surface, the well-known phenomenon of the solidification of the solution ensued. The speed at which this solidification travelled from one extremity of the tube to the other was at about the rate of one foot in forty seconds. The crystalline particles thus formed consisted of long aciculæ about $\frac{1}{1200}$ th of an inch in thickness.

Exp. 2. In a similar solution there were deposited, previous to its solidification, beautiful transparent, hard crystals, which are known to differ from Glauber's salts by their containing only eight equivalents of water of crystallization to one of salt. After the solidification had taken place the crystals were found to be changed to a milk-white colour, although they still retained the same shape. Under the microscope they were

found to consist of minute irregular particles, which accounted for the change of colour and opacity. It appears, therefore, as if the molecular action, which causes the solidification, has the power of disaggregating or breaking up the crystalline arrangement of a body in a solid state.

Exp. 3. A solution of sulphate of soda which is not saturated, or has not been allowed to cool entirely, is affected by the contact of a foreign body much more slowly. After the lapse of several minutes, the formation of separate clusters of crystals much larger than the former is observed: after about half an hour, these clusters were seen at numerous intermediate points throughout the length of the tube, which was several feet long.

Frost and snow examined under a magnifying power are much more alike than we should expect from their appearance to the naked eye. I have already mentioned the small, nearly cubical prisms, precisely like those of frost, which are found in snow. In the following instance of hoar-frost the likeness is still more complete.

Dec. 28, 1846.—The ground and all minute filaments were covered with abundant crystals of frost: thermometer 27° F. The filaments of the spider's web were studded with quadrilateral prisms ranged transversely along the threads. In the afternoon a slight fall of snow occurred, whose particles were exactly like those of Dec. 12, except that they fell in large flakes. These were examined together with other particles of frost deposited on a web, and found to be precisely the same, except that the particles of snow were about a third smaller than those of frost.

Several salts manifest a peculiar tendency to ascend and crystallize on the sides of the vessel which contains them, but none of them so greatly as water when it solidifies in a cavity formed by moist earth. The first crystals formed are considerably above the surface of the water; and they increase until a solid crust is formed by them and the cavity below remains perfectly dry. Camphor, naphthaline, iodine and hydrosulphate of ammonia, resemble water in their volatile properties, and their tendency to solidify in a crystalline shape. The tendency of the first to deposit itself in crystals on the side of the glass nearest the light is well-known. Naphthaline in the same condition slowly deposits a few minute crystals on the same side: after the lapse of a month a few scattered clusters of them were formed, after which no further increase of them was perceived even at the end of several months. Iodine exposed in a large bottle to the sun during the summer formed deposits of different forms on various occasions, but all composed of rhombic

plates slightly modified. Some of these rhombic crystals were separate, about the tenth of an inch in size, and with angles of about 50° and 130° : some of the crystals formed long, thin, narrow blades, above half an inch in length, with a bright metallic lustre: on their edges were two or three indentations indicating the separate crystals.

Hydrosulphate of ammonia.—This is to be obtained by bringing into contact ammonia and sulphuretted hydrogen in a gaseous state and perfectly dry at a low temperature. We then observe the formation of a white crystalline deposit on the sides of the vessel in which the gases are received. Any further directions respecting the mode of obtaining it will be found in Thenard's Chemistry, Berzelius, &c.

Hydrosulphate of ammonia is so extremely volatile, that a piece of lint wetted with æther and placed in contact with any part of the bottle which contains it, causes the salt to form a deposit at the inner surface beneath. The crystals in these cases are frequently very regular hexagonal stars, like those of snow. A current of water falling on any part of the bottle rapidly produces the same effect. In summer it generally sublimates to the upper part of the bottle, on account of the greater heat which is communicated to the base of the bottle by contact with the shelf on which it is placed. In a clear night the opposite effect is produced when placed in contact with the ground. But it is particularly when employed as a differential thermoscope that the indications of this salt are interesting. Thus, during a clear night the salt rapidly accumulates towards the points at which the greatest radiation of caloric takes place. A bottle containing this salt placed between two unequal radiating surfaces, indicates, by its change of position, the difference in the radiation of these surfaces. In fact wherever the equable distribution of caloric is disturbed in the same direction for some time, the altered position of the hydrosulphate serves as an indicator of the course it has taken, and the forms of the crystals show to a certain extent the rapidity with which it has been effected, because where the action is rapid the crystals are much smaller and more confused, than when it has been more gradual. It is surprising how small a difference in the radiating power is sufficient to alter the position of the salt when properly disposed. I will not at present enlarge any further on the thermoscopic properties of the hydrosulphate. It presents all the conditions corresponding to those of water. During its preparation it is frequently observed in the air in a state of microscopic crystals representing snow: the crystals formed by rapid evaporation are exactly like the hexagonal plates of snow. A more gentle action gives

rise to crystallization, like those of frost and of congealed water. Although we have rarely seen it in a liquid state, yet during summer, under the influence of the sun's rays, its crystals become perfectly liquid and assume a globular form, and which they retain as they again solidify; forming, in these cases, conditions corresponding to water in a liquid state, ice and sleet.

XXX. *On a Machine for Calculating the Products, Quotients, Logarithms, and Powers of Numbers.* By the Rev. HENRY MOSELEY, M.A., F.R.S., one of Her Majesty's Inspectors of Schools, and lately Professor of Natural Philosophy and Astronomy in King's College, London*.

[With a Plate.]

I HAVE proposed to myself in the construction of this machine, to determine mechanically the products, quotients, logarithms, squares, square roots, and other powers and roots of the natural numbers, by means of combinations of greater simplicity than have hitherto been applied to the purposes of mechanical calculation.

The accompanying Plate II. is intended to illustrate the *principle* of the machine, but it does not show the mechanical details of its construction or the due proportion of its parts.

It will be observed that it consists principally of a cone and a screw. The application of the cone and the disc to various purposes of mechanical calculation has long been well known, and particularly by the ingenious applications made of the disc to dynamical admeasurement by MM. Poncelet and Morin.

The novelty of *this* instrument consists in the combination of the screw with the cone.

CD represents a small screw terminated by solid cylindrical axes or gudgeons, of which that shown by the letter D is prolonged, and carries an index T. The screw admits of being fixed in its bearings or made to revolve in them.

PQ is a wheel having a hollow cylindrical axis, into which is accurately fitted a hollow cylindrical piece, whose internal surface is traversed by the thread of a female screw working upon the male screw CD.

The hollow axis of the wheel PQ admits of being fixed upon the cylindrical piece inserted in it, or of being disengaged from that piece, so that the wheel PQ may be made to carry the female screw round with it in its revolutions, or to turn freely upon the outside of the cylinder which contains that screw, as upon an axis.

* Communicated by the Author.

The three arms of the wheel PQ are pierced by apertures, through which pass, accurately fitting them, the three rods of the rigid frame EF, which frame turns in fixed bearings at C and D upon hollow axes, through which axes pass the cylindrical extremities or gudgeons of the screw CD.

The wheel PQ traverses the frame EF longitudinally, and at the same time carries it round with it in its revolutions: and it admits of being *fixed* upon the frame at any distance from either extremity. The frame carries an index S.

AGH is a cone, whose side AG is parallel to the screw CD, which turns by means of a spindle AB upon fixed bearings, and which is continually pressed upon the edge of the wheel PQ by a spring acting upon the extremity of its spindle. The cone is of some soft metal, brass for instance. The edge of the wheel PQ is very thin, of steel, and milled, so that it imprints a minute tooth upon the soft face of the cone at every point where it revolves in contact with it.

The spindle AB carries an index R. The indices R, S, T point to equidistant divisions upon fixed circles, not shown in the Plate; and each of them is connected with a train of wheels and pinions, whose numbers of teeth are in the proportion of ten to one, and which serve to measure the complete number of revolutions and parts of a revolution made by each index.

Now certain dimensions (hereafter to be determined) being assigned to the different elements of this combination, it may be shown,—

1st. That if the frame EF be fixed, and the screw CD turned until the index T points to any given number N_3 , and then the frame be released, the wheel PQ *fixed* upon the frame, and the cone turned until the index R points to any other given number N_1 , then will the index S point to the product of the numbers N_1 and N_3 .

2nd. That if, as before, the frame EF be fixed, and the screw turned until the index T points to any number N_3 , and then the wheel PQ be *fixed* on the frame, the frame released, and the cone turned until the index S points to any other number N_2 , then will the index R point to the *quotient* of N_2 by N_3 .

3rd. That if the *screw* CD be fixed, the wheel PQ and the frame released, and certain other adjustments made, and if the cone be turned until the index S points to any number N , then will the index R point to the *logarithm* of that number.

4th. That if the spindle of the cone be connected with the axis of the screw CD by means of two bevelled wheels, so that the rotation of the one shall always bear a given relation to that of the other, then the frame EF and the wheel PQ being

both released, and the cone turned until the index R points to any number, the index S will point to the *square* of that number.

Theory of the Instrument.

Let ι be taken to represent half the angle at the apex of the cone, ρ the radius of the wheel PQ, and λ the distance between each two threads of the screw; also let n_1, n_2, n_3 represent severally the numbers of equidistant divisions on the circles to which the indices R, S, T severally point; then will $\frac{N_1}{n_1}, \frac{N_2}{n_2}, \frac{N_3}{n_3}$ represent the numbers of revolutions made severally by the indices whilst the numbers N_1, N_2, N_3 are registered. Now suppose that when the wheel PQ is at the apex of the cone the indices R, S and T each point to zero, and the frame EF being fixed, let the screw CD be turned until the index T shows the number N_3 ; then will the screw have made $\frac{N_3}{n_3}$ revolutions, and the wheel PQ will have advanced from the apex of the cone to a distance AP from it, represented by $\frac{N_3}{n_3} \cdot \lambda$; so that the radius PM of the circle described by that point whilst the cone revolves will be represented by $\frac{N_3}{n_3} \lambda \sin \iota$, and its circumference by $2\pi \frac{N_3}{n_3} \lambda \sin \iota$. Now let the cone be made to revolve (the wheel PQ being fixed upon the frame and the frame released) until the index R shows the number N_1 , or until the cone has made $\frac{N_1}{n_1}$ revolutions; then will the point P in it have been made to revolve through a space represented by

$$2\pi \frac{N_3}{n_3} \cdot \lambda \sin \iota \cdot \frac{N_1}{n_1},$$

or by
$$\frac{2\pi \lambda \sin \iota}{n_1 n_3} \cdot N_1 N_3.$$

But by reason of the continual pressure of the cone upon the edge of the wheel PQ, that wheel will have been carried round in the revolution of the cone, so that the circumference of the wheel will have described precisely the same space as the point P of the surface of the cone; if therefore N_2 represent the number pointed to by the index S, so that $\frac{N_2}{n_2}$ represents the number of revolutions made by the frame EF and wheel

PQ, and therefore $2\pi\rho \frac{N_2}{n_2}$ the space described by its circumference, we shall have

$$2\pi\rho \frac{N_2}{n_2} = \frac{2\pi\lambda \sin i}{n_1 n_3} \cdot N_1 \cdot N_3;$$

or
$$N_2 = \left(\frac{n_2 \lambda \sin i}{\rho n_1 n_3} \right) N_1 \cdot N_3.$$

Let now the quantities $n_1, n_2, n_3, \rho, \lambda, i$ be so assumed that

$$\left(\frac{n_2 \lambda \sin i}{\rho n_1 n_3} \right) = 1; \dots \dots \dots (\alpha.)$$

then
$$N_2 = N_1 \cdot N_3. \dots \dots \dots (1.)$$

that is to say, the number N_2 shown by the index S is the product of the numbers N_1 and N_3 shown by the indices R and T.

Dividing equation (1.) by N_3 ,

$$N_1 = \frac{N_2}{N_3}; \dots \dots \dots (2.)$$

whence it follows that if, proceeding in other respects precisely as before, we cause the cone to revolve, not until its own index R points to a given number, but until the index S points to the given number N_2 , then will the index R of the cone show a number N_1 , which is the *quotient* of N_2 and N_3 .

To adjust the instrument to show the *logarithms* of numbers, let the wheel PQ be brought to the apex of the cone, and let the screw CD be fixed, and the index S then made to point to zero. Let the cone then be turned until the index S points to unity, and in this position of the cone let the index R be made to point to zero. The frame EF being then allowed to turn freely, and the wheel PQ to traverse freely on the frame, the number N_1 shown by the index R will always be the logarithm of the number N_2 shown by the index S. For while the number N_1 is increased by the exceedingly small number ΔN_1 , let the number N_2 be increased by ΔN_2 ; the parts of a revolution made by the cone and wheel respectively whilst N_1 and N_2 receive these small increments, will then be $\frac{\Delta N_1}{n_1}$ and $\frac{\Delta N_2}{n_2}$, and the arcs to radius unity, $2\pi \frac{\Delta N_1}{n_1}$ and $2\pi \frac{\Delta N_2}{n_2}$. Moreover, the wheel PQ having made $\frac{N_2}{n_2}$ revolutions upon the *fixed screw* CD, will have been carried from the apex of the cone to a distance from it represented by

$\frac{N_2}{n_2} \lambda$; so that the radius PM of the circle in the act of being described by the point P upon the surface of the cone whilst the numbers ΔN_1 and ΔN_2 are being registered, will be $\frac{N_2}{n_2} \lambda \sin \iota$; and the exceedingly small arc described by that point whilst these numbers are registered, will be represented by

$$2\pi \frac{\Delta N_1}{n_1} \cdot \frac{N_2}{n_2} \lambda \sin \iota.$$

But this arc, described by P upon the cone, is equal to that described in the same time by a point in the circumference of the wheel, which last is represented by $2\pi \frac{\Delta N_2}{n_2} \rho$;

$$\begin{aligned} \therefore 2\pi \frac{\Delta N_1}{n_1} \cdot \frac{N_2}{n_2} \cdot \lambda \sin \iota &= 2\pi \frac{\Delta N_2}{n_2} \rho; \\ \therefore \left(\frac{\lambda \sin \iota}{n_1 \rho} \right) \Delta N_1 &= \frac{\Delta N_2}{N_2}; \end{aligned}$$

or passing to the limit and integrating between the limits *unity* and N_2 ,

$$\begin{aligned} \left(\frac{\lambda \sin \iota}{n_1 \rho} \right) N_1 &= \log_{\epsilon} N_2; \\ \therefore \left(\frac{\lambda \sin \iota}{\epsilon n_1 \rho} \right)^{N_1} &= N_2. \end{aligned}$$

Let the quantities $\lambda, \iota, n_1, \rho$ be so assumed that

$$\begin{aligned} \frac{\lambda \sin \iota}{\epsilon n_1 \rho} &= 10; \quad \dots \dots \dots (\beta.) \\ \therefore 10^{N_1} &= N_2; \\ \therefore N_1 &= \log_{10} N_2. \quad \dots \dots \dots (3.) \end{aligned}$$

whence it follows that the number shown by the index R (under this adjustment of the instrument) will in every position be the common logarithm of the number then shown by S.

If the spindle AB be made to carry round with it the screw CD by the intervention of two bevel wheels the numbers of whose teeth bear a given ratio, n , to one another, so that the screw CD may make n revolutions or parts of a revolution whilst the spindle AB makes one; if, moreover, the wheel PQ be so released from the female screw, which forms its centre, as that the outer surface of that screw may serve for an axis about which it may turn freely whilst it is still carried along the frame by the longitudinal motion of the screw; and if the in-

dices be all made to point to zero when the wheel PQ is at the apex of the cone, then, representing as before by N_1, N_2, N_3 the numbers shown at any time by the indices, $n \frac{N_1}{n_1}$ will represent the number of revolutions made by the screw whilst the number N_1 is registered by R, and therefore $\frac{nN_1}{n_1} \lambda$ the distance AP by which the wheel PQ will have been moved from the vertex of the cone; so that $\frac{nN_1}{n_1} \lambda \sin \iota$ will be the radius PM of the circle which the point P of the cone is in the act of describing, and $\frac{nN_1}{n_1} \lambda \sin \iota \cdot 2\pi \frac{\Delta N_1}{n_1}$ the small arc which it actually describes whilst the increment ΔN_1 is registered by the index R. But ΔN_2 being at the same time registered by the index S, the circumference of the wheel will describe a space represented by $\frac{\Delta N_2}{n_2} \cdot 2\pi \rho$;

$$\therefore \frac{\Delta N_2}{n_2} \cdot 2\pi \rho = \frac{nN_1}{n_1} \lambda \sin \iota \cdot 2\pi \frac{\Delta N_1}{n_1};$$

$$\therefore \Delta N_2 = \left(\frac{\lambda n n_2 \sin \iota}{n_1^2 \rho} \right) \cdot N_1 \Delta N_1.$$

And passing to the limit and integrating between the limits zero and N_1 ,

$$N_2 = \left(\frac{\lambda n n_2 \sin \iota}{2n_1^2 \rho} \right) \cdot N_1^2.$$

If, therefore, the quantities $\lambda, n, n_1, n_2, \rho, \iota$ be so assumed that

$$\left(\frac{\lambda n n_2 \sin \iota}{2n_1^2 \rho} \right) = 1, \quad \dots \dots \dots (\gamma.)$$

then $N_2 = N_1^2, \dots \dots \dots (4.)$

and $N_1 = \sqrt{N_2}; \dots \dots \dots (5.)$

so that the number N_2 shown by the index S (under this adjustment of the instrument) is, in every position, the square of the number shown by the index R; and conversely, the number shown by the index R is, in every position, the square root of the number shown by S.

If a second frame and screw and wheel, similar to EF and CD and PQ, be conceived to be placed parallel to the side AH of the cone, and if the cone be so pressed upon the edges of both these wheels as to carry them round with it in its revolution; if, moreover, the axis of the second screw be so connected

with the axis of the frame EF by two bevel wheels, as that the number of revolutions made by the latter shall bear a certain given ratio n' to the number made in the same time by the former; then will the number N_4 shown at any time by the index of this last frame be the *cube* of the number N_1 shown at the same time by the index R.

For $\frac{N_2}{n_2}$ representing the number of revolutions made by the first frame EF whilst the number N_1 is registered, $n' \frac{N_2}{n_2}$ will represent the number of revolutions made in the same time by the second screw, so that $n' \frac{N_2}{n_2} \cdot \lambda$ will represent the distance by which the second wheel will have been moved from the apex of the cone, and $n' \frac{N_2}{n_2} \lambda \sin i$ the radius of the circle which its point of contact with the cone will be in the act of describing. Moreover, $2\pi \frac{\Delta N_1}{n_1}$ is the angle which the cone describes whilst ΔN_1 is registered by the index R; so that $n' \frac{N_2}{n_2} \lambda \sin i \cdot 2\pi \frac{\Delta N_1}{n_1}$ is the small arc described during this time by the point of contact of the second wheel with the cone. But the increment ΔN_4 being registered during that time by the index of the second frame, the circumference of the second wheel will describe the arc $\frac{\Delta N_4}{n_4} \cdot 2\pi \rho$ (n_4 representing the number of equal parts into which the circle is divided to which the index of the second frame points);

$$\therefore n' \cdot \frac{N_2}{n_2} \lambda \sin i \cdot 2\pi \frac{\Delta N_1}{n_1} = \frac{\Delta N_4}{n_4} \cdot 2\pi \rho;$$

$$\therefore \Delta N_4 = \left(\frac{n' n_4 \lambda \sin i}{n_1 n_2 \rho} \right) \cdot N_2 \cdot \Delta N_1;$$

but $N_2 = N_1^2$ (by equation 4);

$$\therefore \Delta N_4 = \left(\frac{n' n_4 \lambda \sin i}{n_1 n_2 \rho} \right) \cdot N_1^2 \cdot \Delta N_1.$$

Therefore, passing to the limit and integrating,

$$N_4 = \left(\frac{n' n_4 \lambda \sin i}{3 n_1 n_2 \rho} \right) \cdot N_1^3.$$

If therefore n' be so taken that

$$\frac{n' n_4 \lambda \sin i}{3 n_1 n_2 \rho} = 1, \dots \dots \dots (\delta.)$$

then $N_4 = N_1^3, (6.)$

and $N_1 = \sqrt[3]{N_4}; (7.)$

so that (under this combination) the number N_4 shown by the index of the second frame is, in every position, the cube of that shown by the index R; and conversely, the number shown by the index R is the cube root of that shown, at the same time, by the index of the second frame.

In like manner, by placing any number of like systems of a frame, screw and wheel, upon the side of the cone, the third screw receiving its motion from the second frame, the fourth from the third, &c., any other powers of the number N_1 , or roots of the numbers N_2, N_4 , &c. may be conceived to be determined, the power shown by the last index, or the root by the first, being one greater than the number of frames so applied.

The four equations of condition ($\alpha.$), ($\beta.$), ($\gamma.$), ($\delta.$) between the nine quantities $n_1, n_2, n_3, n_4, n, n', \lambda, \iota, \rho$ leave five of them undetermined. These are to be assumed of such values as may be found most convenient in the construction of the machine, the *general principle* of which it is the object of this paper to explain.

The Plate in particular, as has already been stated, is by no means intended to show the details of the construction of the machine, or even the general proportions of its parts, but simply to illustrate its principle. There are, indeed, some elements of its construction, which, for the sake of simplicity, have been altogether omitted from it. These are the trains of wheels which must be connected with each of the axes carrying the indices R, S, T to register the *complete* numbers of revolutions made by those indices, and the divided circles to which the indices point, showing such portions of the numbers N_1, N_4 , &c. as correspond to less than one complete revolution.

It will be obvious that, since the number N_2 represents (in the first adjustment of the instrument) the product of the numbers N_1 and N_3 , the divisions on the circles pointed to by the indices R and T must be greater than those of the circle pointed to by the index S, or the numbers n_1 and n_3 less than the number n_2 .

It will, moreover, be observed, that the instrument, whilst it differs from other calculating machines, and claims to be superior to them in the simplicity of its combinations, differs also, and is inferior to them in this respect, that the truth of its registrations is dependent on the mechanical accuracy of its construction.

Those elements of the machine, on the mechanical truth

and adaptation of which its accuracy depends, are,—1st, the screw, the convolutions of whose thread must be of geometrical uniformity; 2nd, the cone, whose surface must be of geometrical truth; and 3rd, the wheel, whose edge must have so intimate a contact *with*, and hold *upon*, the surface of the cone, as to partake accurately in its motion at every point which it traverses.

My experience in the use of a similar cone and wheel in a steam indicator constructed at the expense of the British Association of Science, has convinced me that the required accuracy is in this last respect attainable.

In respect to the two first-mentioned sources of error, it may be observed that there are no mechanical forms of greater simplicity than the *cone* and the *screw*, and probably none in respect to which greater truth of construction is attainable.

Every such instrument must however have its error; its *amount* in respect to this instrument may be determined by an obvious method, and it is probable that it may in every case be corrected by a corresponding *adjustment*.

XXXI. *On the use of a mixture of Spirit of Wine and Camphine, as a Light for Optical Purposes.* By JOHN GEORGE CHILDREN, Esq., F.R.S.

To Richard Taylor, Esq.

MY DEAR SIR,

Halstead Place, Feb. 13, 1847.

IF you think the following trifle worth inserting in the Philosophical Magazine, it is at your service. I have lately assisted a friend in getting up an apparatus for dissolving views, &c. on a scale too large for sufficient illumination by the best Argand lamp, and we considered condensed hydrogen gas as too dangerous an agent for a plaything; especially in private houses in the country, where much must be left in the preparation of the gases, &c. to servants unused to such duties. We set to work therefore to find a substitute that might answer our purpose; and after a few trials we obtained a tolerable light, by throwing the flame of spirit of wine on a surface of quicklime by a current of oxygen gas, something after the manner of Lieut. Drummond's original experiment*. The illumination by the flame of spirit of wine alone, however, proved to be too feeble; but by mixing a portion of camphine (spirit of turpentine) with it (which readily dissolves in alcohol)

* Philosophical Transactions, 1826, p. 330. Drummond's object and ours were somewhat different. He wanted to get an intense but small sphere of light, "adapted to the nature of a (parabolic) reflector;" we also wanted as intense a light as we could obtain, but, as we could not advantageously use a reflector, one which should likewise extend over a much larger surface.

in the proportion, by measure, of one part of camphine to eight parts of spirit of wine, of the specific gravity of $\cdot 841$ at 60° F. (equivalent to the spirit of commerce of 60°), we obtained a light amply sufficient, not only for my friend's dissolving views, but also for his microscope and physioscope, and perfectly free from all danger, or even possibility of explosion. My friend's screen is about 22×18 feet; and if it were twice as large, the light is quite capable of illuminating it brilliantly; and it shows a microscopic object, magnified from half an inch to thirty feet, or 720 times, linear, with great distinctness and beauty. I do not mean that our toy can be compared to the almost solar intensity of the oxy-hydrogen light, also invented by the late Lieut. Drummond*, and now in common (too common?) use; but in a trial I made the other day in Francis Street, with an apparatus hastily put together for the purpose by Mr. Collins, I found its effect, measured by one of Wheatstone's photometers, equal to that of seventy-six of Brecknell and Turner's best platted-wick wax candles; and in several trials with our own apparatus, more carefully arranged, and with thoroughly well and recently burnt lime (an essential caution), we have found it quite equal to 108 of the same candles, and on one occasion to 120! The oxy-hydrogen light at Mr. Collins's, which was splendidly brilliant, being compared in the same manner, was found equal to 121 of the same candles.

In these experiments, no portion of the rays from either of the lights was intercepted; our object being to ascertain the comparative illuminating power of their entire surfaces, and not their comparative intensities only †.

Were it not for the peculiar odour of the so-called naphtha or coal-oil, which to some persons is highly offensive, even in its purest state, it might, when highly rectified, be advantageously substituted, in an œconomical point of view, for the spirit of wine. Four ounces of camphor dissolved in a pint of that liquid, gives, under the same circumstances, nearly as brilliant a light as the spirit and camphine.

The light from camphine alone is for a few seconds intensely brilliant, but it is soon quenched in the enormous mass of unburnt carbon, which partly condenses on the lime and partly escapes into the atmosphere, filling the whole apartment with a dense and almost suffocating cloud of floating black particles.

* Philosophical Transactions, 1830.

† Some attempts indeed were made in Francis Street to compare the intensities alone; but the apparatus, from want of time to prepare a better, was too imperfect to allow me to place any confidence in the results. As far as they went, they were greatly in favour of the superior *intensity* of the oxy-hydrogen light. I hope to repeat the trials with a more refined apparatus.

A sufficient and well-regulated supply of oxygen gas might perhaps remedy this defect.

Our apparatus consists of a copper lamp with two tubes lying close together, and each containing a wick formed of flat cotton rolled up into a cylinder; and a cylinder of lime, about three-eighths of an inch long and one-eighth of an inch in diameter, inclosed in a thin copper case. The pipe conveying the oxygen gas from the gasometer terminates in a small jet, inclining upwards, which lies between the two wicks slightly parted to receive it, and within rather less than one-eighth of an inch from the circular disc of lime, and about one-fourth of an inch above the lower edge of the copper case. We find that the common chalk of this neighbourhood furnishes a lime which gives a better light than that from the Bristol, or any other limestone we have tried*.

Mr. Collins, philosophical instrument-maker to the Royal Polytechnic Institution, makes lanterns for dissolving views and microscopes, fitted up with the spirit and camphine light, and will be happy to show its effect to any gentleman who may wish to see it. His address is "26 Francis Street, Tottenham Court Road," and at the Polytechnic Institution.

I am ever, my dear Sir, faithfully yours,

JOHN GEORGE CHILDREN.

XXXII. *On the Newly discovered Planet.* By M. ENCKE †.

THE new planet, the orbit and present position of which had been announced *à priori* by M. Le Verrier of Paris, was discovered almost immediately at the Berlin observatory by Dr. Galle: the very first night the Hora XXI. of the Academy's star-maps (which have been prepared with such extraordinary care and accuracy by Dr. Bremiker) was compared with the heavens, and thus afforded the most brilliant proof of the truth of theory, and the wonderful sagacity with which M. Le Verrier had made use of the existing data. With this confirmation, so far exceeding any expectations which could have been previously entertained, the name of M. Le Verrier will ever be connected, and acquire in consequence of it a celebrity as justly merited as it is unexampled in respect of the mode in which it is gained.

From the slowness of the motion of the planet, the observations hitherto made at the Berlin observatory, although they extend to twenty-five days, may all be connected with a star occurring in Bessel's zones, the mean place of which was preliminarily assumed to be

$$327^{\circ} 56' 56''.4 - 13^{\circ} 26' 9''.6$$

* Drummond also obtained the most brilliant light from chalk lime.

† From the *Berichten des Akad. der Wissenschaften zu Berlin*, Oct. 22, 1846.

until more accurately determined. To estimate the distance of the planet, a circular orbit was assumed by Dr. Galle, which agrees so accurately with all the individual positions, that the tempt to determine an elliptic orbit must be deferred for some time. A comparison with the assumed orbit,—

Epoch Sept. 24.0 mean Berlin time

Mean longitude. $326^{\circ} 58' 23''.5$

Ascending node 131 1 10.8

Inclination 1 52 51.5

Semidiameter 30.03885

Mean daily sidereal motion $21''.55171$

gave the following results:—

1846.	Mean Berlin time.	Observed AR.	Observed Declination.	Difference.	
				AR.	Declination.
Sept. 23.	h 12 0 14.6	328 19 16.0	-13 24 8.2	+2.3	+0.9
24.	8 54 40.9	18 14.3	24 29.7	+0.1	+0.1
25.	9 41 45.0	16 59.8	24 55.4	0.0	-0.5
26.	10 11 54.3	15 48.3	25 22.2	-1.1	+0.9
27.	8 29 48.9	14 42.8	25 44.6	-0.6	+0.6
28.	11 31 28.8	13 25.5	26 11.5	-0.3	+0.5
29.	9 14 3.7	12 23.4	26 32.6	+1.2	+0.3
Oct. 2.	11 5 35.2	9 7.0	27 41.4	+0.3	+0.3
6.	8 0 54.2	5 16.2	29 1.3	-0.1	+0.4
10.	8 52 56.3	1 42.2	30 14.7	-1.4	-0.7
15.	7 27 25.7	327 57 52.8	31 33.8	0.0	-0.1
18.	7 37 43.7	55 51.8	32 15.2	+2.5	+1.0
...	7 58 47.6	55 55.0	32 14.2	-1.2	-0.1

The observations made at other observatories, as far as I have become acquainted with them, present a similar agreement, since a constant difference from the Berlin observations must most probably be referred to the assumed position of the star.

1846.	Mean Berlin time.	Observed AR.	Observed Declination.	Difference.		
				AR.	Declination.	
Sept. 27.	h 9 41 46.1	328 14 35.8	-13 25 54.0	+2.9	- 8.9	Göttingen.
28.	9 37 37.9	13 28.2	26 15.0	+2.3	- 5.9	Hamburg.
29.	9 33 37.6	12 18.5	26 37.3	+5.3	- 4.7	Hamburg.
...	9 33 45.4	12 21.8	26 39.8	+1.9	- 7.2	Altona.
Oct. 1.	9 25 37.8	10 7.8	27 28.2	+6.4	-10.4	Hamburg.
...	9 25 45.0	10 11.7	27 24.5	+2.4	- 6.8	Altona.
3.	9 1 23.5	8 9.6	28 0.5	+0.2	0.0	Lond. (Hind).
4.	9 13 37.3	7 5.3	28 30.0	+4.4	- 8.4	Hamburg.
5.	8 26 20.5	28 41.4	+ 0.1	Lond. (Hind).
...	10 45 45.9	6 6.9	28 44.7	-0.3	- 1.4	Hamburg.
6.	9 5 37.8	5 10.5	29 9.6	+3.0	- 7.9	Hamburg.
...	9 5 45.5	5 12.6	29 6.3	+0.7	- 4.6	Göttingen.
8.	9 6 43.1	3 22.8	29 40.4	+0.9	+ 0.6	Turin.
9.	9 2 43.5	2 27.0	29 53.4	+4.3	- 4.5	Turin.
10.	8 58 43.9	1 32.2	30 17.9	+8.4	+ 2.5	Turin.

The elements are indeed merely approximative; but since a circular orbit, previously calculated by me from other observations, gave very nearly the same numbers, it may at least be concluded from them that the planet is much nearer than M. Le Verrier had assumed. Its distance from the sun should be 33 according to his elements, while according to the above it will not differ much from 30; consequently the period of revolution will also be shorter. If the orbit were actually circular, the period would be about 165 years.

In another point, however, with respect to which M. Le Verrier had only presumptions to go upon, he has hit the truth with most remarkable accuracy. According to him, the disc of the planet amounts to about $3''\cdot3$, while the mean of the measurements gives $2''\cdot7$.

I take this opportunity of expressing my views with regard to the name which I intend to assign to the planet for the next year, as I am compelled to come to some decision on account of the publication of the *Astronomical Jahrbuch*, in which the planet must be inserted as soon as its elements are more accurately determined. To every new planet, the astronomers who were countrymen of the discoverer at first added appellations, which were intended to call to mind some peculiar circumstance. Thus the English called and still call Uranus the *Georgian planet**, as a mark of gratitude to king George the Third, whose munificence enabled Herschel to construct his great reflectors; and the planet was detected, as is well known, by its disc. Piazzi named his planet Ceres *Ferdinanda*, in honour of the king of Naples, the founder of the observatory at Palermo. In the same manner Pallas was at first called *Olbersiana*, until Olbers himself pronounced strongly against the adjective. Subsequently the epithets have been disused by the German discoverers, Harding, Olbers and Hencke; and according to the suggestion of Bode, the name Uranus has been everywhere introduced except in England, and the name Ceres is now generally used without any addition.

The astronomers of Paris appear also to have had respect to this custom of naming the planets after the ancient divinities, principally of the Romans; since Le Verrier himself, in his first letter to Dr. Galle, Oct. 1, in answer to the announcement of the discovery of the planet, states at the conclusion, "the Bureau des Longitudes has pronounced for NEPTUNE, the sign a *trident*." This too appears to have been after some consideration; for as Dr. Galle had in his letter hinted at the name *Janus*, M. Le Verrier observes that "the name Janus

* In the Nautical Almanac only. Uranus is the name now in general use with British astronomers.—ED.

would indicate that this planet is the last of the solar system, which there is no reason whatever to suppose."

Moreover, this name [Neptune] has been widely bruited; for in a letter which I have received from Gauss, our first German authority in astronomy, dated Oct. 7, he says, "I consider the name Neptune chosen by M. Le Verrier to be very appropriate; perhaps a trident might be selected as the sign if it were not improper in any way to interfere with the rights of the discoverer."

Now in a letter which I have received from M. Le Verrier, dated Oct. 6, he states, "I requested my illustrious friend, M. Arago, to choose a name for the planet. I was somewhat startled at the decision (*j'ai été un peu confus*) which he made at a sitting of the Academy." This confirms the information we have received through other channels, that the name *Le Verrier* had been proposed by M. Arago.

Fortified by the high authority of Gauss, and the *Bureau des Longitudes* of Paris, I shall, under these circumstances, retain for the next year the name Neptune, and the sign of the trident, until public opinion in Germany has become sufficiently consolidated to establish a definitive appellation. Our German custom has prevailed in the case of four, we may even say five new planets, as Herschel was a German by birth; and as it cannot be in the least my intention to undervalue the great merits of M. Le Verrier, which I have most cordially admitted, so it is my opinion that his name will ever remain so firmly connected with Neptune, that it is not necessary, in order to keep up the recollection of the discovery, to introduce the somewhat incongruous collocation of the heathen gods with a modern name. Besides, in the present case, a German has the essential merit of having discovered the planet.

In letters which I have since received from Sir John Herschel, the planet is also called Neptune; and Struve, in Pulkowa, has pronounced decidedly for retaining this name. The first astronomical authorities, therefore, in Germany, France, England and Russia, have pronounced in favour of NEPTUNE.

Note.—A paper received by the Astronomer Royal from M. Struve, and inserted in the *Athenæum* for Feb. 20, assigns similar reasons for adopting the proposed name. We subjoin the following passage:—

"Far be it from us to have any intention of withholding our entire admiration from the eminent merit of M. Le Verrier. But impartial history will, in the future, make honourable mention also of the name of Mr. Adams, and recognise two individuals as having, independently of one another, discovered the planet beyond Uranus. In the same way, it attributes

the discovery of the infinitesimal calculus at once to Newton and to Leibnitz. Mr. Airy, the Astronomer Royal at Greenwich, has published a complete and authentic report on the labours of Mr. Adams relative to the existence of the Trans-Uranian planet. In that report, we see that in September 1845 Mr. Adams arrived at a result, and that in October he transmitted to Mr. Airy a paper containing elements of the present planet so nearly approximative that it might have been found in the heavens ten months before it actually was. But Mr. Adams's labours were unsuccessful, because the two astronomers (Mr. Challis of Cambridge and Mr. Airy of Greenwich) to whom they were known hesitated to admit them without further examination. Their doubts are explained by the importance and novelty of the object, and by the extraordinary difficulty of the research itself, which might well have been deemed beyond the powers of a young *savant* till then unknown. These doubts were accordingly not dissipated until the moment when M. Le Verrier published the results of his admirable investigations, which led to the most brilliant discovery in the astronomy of the solar system, while the other astronomers of Europe had no suspicion of the existence of Mr. Adams's labours. M. Galle of Berlin, was the first to find the planet indicated by M. Le Verrier. While we consider all these circumstances attendant on the discovery of the new planet, we at the same time conceive that we find the adhesion of M. Le Verrier to the name of Neptune, not only in his announcement to us of the 1st of October, but also in his later letters addressed to the Academy of Sciences and to two astronomers of the central observatory—letters which make no objection whatever to the name of Neptune chosen by the Bureau des Longitudes.

“Consequently, we will retain the name of Neptune; and will make no change unless hereafter the general voice shall determine in favour of another name.

“In the name of the astronomers of the Central Observatory,
“Polkowa, 17 (29) Dec. 1846.”

W. STRUVE.”

Mr. Airy adds, that he quite agrees with M. Struve in his reasons and in his conclusions.

XXXIII. On $\sqrt{-1}$. By H. S. WARNER, Esq.

[The following Note, appended to a communication received from Mr. Warner on the use of the symbol θ (but not adopted for insertion), relates to a Paper signed “Shadow” in our Number for September 1846.]

IN relation to the article of “SHADOW,” I may observe that I did not expect that my conclusions would be admitted
Phil. Mag. S. 3. Vol. 30. No. 200. March 1847. O

by those (of whom "SHADOW" seems one) who deny the correctness of the assumption upon which the argument depends; the agreement of my results with well-known formulas would be merely an additional argument in favour of the new mode of regarding imaginaries.

Opposed as his views appear to be to this manner of interpreting impossible quantities, it seems to me (unless I have mistaken the sense in which he uses the term "*incongruous*," which is the case perhaps) that they in reality support it.

Thus, incongruity in geometrical problems (and of course in those only can we consider $\sqrt{-1}$ a symbol of perpendicularity) can only affect the *length* or the *direction* of a line: if it affected the length of a line, this would be evidenced by our obtaining such a result as the coexistence of the two equations $A=B$, $A > B$.

When the incongruity affected the direction, it would be evidenced by the appearance of $\sqrt{-1}$. It being once admitted that $\sqrt{-1}$ marks an error in estimating direction, it is easy to show that this error is one of a right angle; for the error shown by $\sqrt{-1}$ being twice repeated, gives $-a$ in place of $+a$, or an incongruity of two right angles; hence the incongruity represented by $\sqrt{-1}$ must be half of two right angles, that is, one right angle.

Port of Spain, Trinidad,
Oct. 3, 1846.

XXXIV. *On the Induction of Atmospheric Electricity on the Wires of the Electric Telegraph.* By Prof. JOSEPH HENRY*.

THE action of the electricity of the atmosphere on the wires of the electrical telegraph is at the present time a subject of much importance, both on account of its practical bearing, and the number of purely scientific questions which it involves. I have accordingly given due attention to the letter referred to me, and have succeeded in collecting a number of facts in reference to the action in question. Some of these are from the observations of different persons along the principal lines, and others from my own investigations during a thunder-storm on the 19th of June, when I was so fortunate as to be present in the office of the telegraph in Philadelphia, while a series of very interesting electrical phænomena was exhibited. In connexion with the facts derived from these sources, I must ask the indulgence of the Society in frequently referring, in the course of this communication, to the results of my previous

* From the Proceedings of the American Philosophical Society, vol. iv. p. 260.

investigations in dynamic electricity, accounts of which are to be found in the Proceedings and Transactions of this Institution.

From all the information on the subject of the action of the electricity of the atmosphere on the wires of the telegraph, it is evident that effects are produced in several different ways.

1. The wires of the telegraph are liable to be struck by a direct discharge of lightning from the clouds, and several cases of this kind have been noticed during the present season. About the 20th of May the lightning struck the elevated part of the wire, which is supported on a high mast at the place where the telegraph crosses the Hackensack river. The fluid passed along the wire each way, from the point which received the discharge, for several miles, striking off at irregular intervals down the supporting poles. At each place where the discharge to a pole took place, a number of sharp explosions were heard in succession, resembling the rapid reports of several rifles. During another storm, the wire was struck in two places in Pennsylvania, on the route between Philadelphia and New York; at one of these places twelve poles were struck, and at the other eight. In the latter case the remarkable fact was observed, that every other pole escaped the discharge; and the same phænomenon was observed, though in a less marked degree, near the Hackensack river. In some instances the lightning has been seen coursing along the wire in a stream of light; and in another case it is described as exploding from the wire at certain points, though there were no bodies in the vicinity to attract it from the conductor.

In discussing these and other facts to be mentioned hereafter, we shall, for convenience, adopt the principles and language of the theory which refers the phænomena of electricity to the action of a fluid, of which the particles repel each other, and are attracted by the particles of other matter. Although it cannot be affirmed that this theory is an actual representation of the cause of the phænomena as they are produced in nature, yet it may be asserted that it is, in the present state of science, an accurate mode of expressing the laws of electrical action, so far as they have been made out; and that though there are a number of phænomena which have not as yet been referred to this theory, there are none which are proved to be directly at variance with it.

That the wires of the telegraph should be frequently struck by a direct discharge of lightning, is not surprising, when we consider the great length of the conductor, and consequently the many points along the surface of the earth through which it must pass peculiarly liable to receive the discharge from

the heavens. Also, from the great length of the conductor, the more readily must the repulsive action of the free electricity of the cloud drive the natural electricity of the conductor to the further end of the line, thus rendering more intense the negative condition of the nearer part of the wire, and consequently increasing the attraction of the metal for the free electricity of the cloud. It is not however probable that the attraction, whatever may be its intensity, of so small a quantity of matter as that of the wire of the telegraph, can of itself produce an electrical discharge from the heavens; although, if the discharge were started by some other cause, such as the attraction of a large mass of conducting matter in the vicinity, the attraction of the wire might be sufficient to change the direction of the descending bolt, and draw it in part or whole to itself. It should also be recollected that, on account of the perfect conduction, a discharge on any part of the wire must affect every other part of the connected line, although it may be hundreds of miles in length.

That the wire should give off a discharge to a number of poles in succession, is a fact I should have expected, from my previous researches on the lateral discharge of a conductor transmitting a current of free electricity. In a paper on this subject, presented to the British Association in 1837, I showed that when electricity strikes a conductor explosively, it tends to give off sparks to all bodies in the vicinity, however intimately the conductor may be connected with the earth. In an experiment in which sparks from a small machine were thrown on the upper part of a lightning-rod, erected in accordance with the formula given by the French Institute, corresponding sparks could be drawn from every part of the rod, even from that near the ground. In a communication since made to this Society, I have succeeded in referring this phenomenon to the fact, that during the transmission of a quantity of electricity along a rod, the surface of the conductor is charged in succession, as it were, by a wave of the fluid, which, when it arrives opposite a given point, tends to give off a spark to a neighbouring body, for the same reason that the charged conductor of the machine gives off a spark under the same circumstances.

It might at first be supposed that the redundant electricity of the conductor would exhaust itself in giving off the first spark, and that a second discharge could not take place; but it should be observed, that the wave of free electricity, in its passage, is constantly attracted to the wire by the portion of the uncharged conductor which immediately precedes its position at any time; and hence but a part of the whole re-

dundant electricity is given off at one place; the velocity of transmission of the wave as it passes the neighbouring body, and its attraction for the wire, preventing a full discharge at any one place. The intensity of the successive explosions is explained by referring to the fact, that the discharge from the clouds does not generally consist of a single wave of electricity, but of a number of discharges along the same path in rapid succession, or of a continuous discharge which has an appreciable duration; and hence the wire of the telegraph is capable of transmitting an immense quantity of the fluid thus distributed over a great length of the conductor.

The remarkable facts of the explosions of the electricity into the air, and of the poles being struck in interrupted succession, find a plausible explanation in another electrical principle which I have established, namely, in all cases of the disturbance of the equilibrium of the electrical plenum, which we must suppose to exist throughout all terrestrial space, the state of rest is attained by a series of diminishing oscillations. Thus in the discharge of a Leyden jar, I have shown that the phenomena exhibited cannot be explained by merely supposing the transfer of a quantity of fluid from the inner to the outer side of the jar; but in addition to this we are obliged to admit the existence of several waves, backwards and forwards, until the equilibrium is attained. In the case of the discharge from the cloud, a wave of the natural electricity of the metal is repelled each way from the point on which the discharge falls, to either end of the wire, is then reflected, and in its reverse passage meets in succession the several waves which make up the discharge from the cloud. These waves will therefore interfere at certain points along the wire, producing, for a moment, waves of double magnitude, and will thus enhance the tendency of the fluid at these points to fly from the conductor. I do not say that the effects observed were actually produced in this way; I merely wish to convey the idea that known principles of electrical action might, under certain circumstances, lead us to anticipate such results.

2. The state of the wire may be disturbed by the conduction of a current of electricity from one portion of space to another, without the presence of a thunder-cloud; and this will happen in case of a long line, when the electrical condition of the atmosphere which surrounds the wire at one place is different from that at another. Now it is well known that a mere difference in elevation is attended with a change in the electrical state of the atmosphere. A conductor, elevated by means of a kite, gives sparks of positive electricity in a perfectly

clear day; hence if the line of the telegraph passes over an elevated mountain ridge, there will be continually, during clear weather, a current from the more elevated to the lower points of the conductor.

A current may also be produced in a long level line, by the precipitation of vapour in the form of fog at one end, while the air remains clear at the other; or by the existence of a storm of rain or snow at any point along the line, while the other parts of the wire are not subjected to the same influence.

Currents of sufficient power to set in motion the marking machine of the telegraph have been observed, which must have been produced by some of these causes. In one case the machine spontaneously began to operate without the aid of the battery, while a snow storm was falling at one end of the line, and clear weather existed at the other. On another occasion a continued stream of electricity was observed to pass between two points at a break in the wire, presenting the appearance of a gas-light almost extinguished. A constant effect of this kind indicates a constant accession of electricity at one part of the wire, and a constant discharge at the other.

3. The natural electricity of the wire of the telegraph is liable to be disturbed by the ordinary electrical induction of a distant cloud. Suppose a thunder-cloud, driven by the wind in such a direction as to cross one end of the line of the telegraph at the elevation, say of a mile; during the whole time of the approach of the cloud to the point of its path directly above the wire, the repulsion of the redundant electricity with which it is charged would constantly drive more and more of the natural electricity of the wire to the further end of the line, and would thus give rise to a current. When the cloud arrived at the point nearest to the wire, the current would cease for a moment; and as the repulsion gradually diminished by the receding of the cloud, the natural electricity of the wire would gradually return to its normal state, giving rise to a current in an opposite direction. If the cloud were driven by the wind parallel to the line of the telegraph, a current would be produced towards each end of the wire, and these would constantly vary in intensity with the different positions of the cloud. Although currents produced in this way may be too feeble to set in motion the marking apparatus, yet they may have sufficient power to influence the action of the current of the battery so as to interfere with the perfect operation of the machine.

4. Powerful electrical currents are produced in the wires of the telegraph by every flash of lightning which takes place

within many miles of the line, by the action of dynamic induction; which differs from the action last described, in being the result of the influence of electricity *in motion* on the natural electricity of the conductor. The effect of this induction, which is the most fruitful source of disturbance, will be best illustrated by an account of some experiments of my own, presented to the Society in 1843. A copper wire was suspended by silk strings around the ceiling of an upper room, so as to form a parallelogram of about sixty feet by thirty on the sides; and in the cellar of the same building, immediately below, another parallelogram of the same dimensions was placed. When a spark from an electrical machine was transmitted through the upper parallelogram, an induced current was developed in the lower one sufficiently powerful to magnetize needles, although two floors intervened, and the conductors were separated to the distance of thirty feet. In this experiment no electricity passed through the floors from one conductor to the other; the effect was entirely due to the repulsive action of the electricity in motion in the upper wire on the natural electricity of the lower. In another experiment, two wires, about 400 feet long, were stretched parallel to each other between two buildings; a spark of electricity sent through one produced a current in the other, though the two were separated to the distance of 300 feet; and from all the experiments, it was concluded that the distance might be indefinitely increased, provided the wires were lengthened in a corresponding ratio.

That the same effect is produced by the repulsive action of the electrical discharge in the heavens, is shown by the following modification of the foregoing arrangement. One of the wires was removed, and the other so lengthened at one end as to pass into my study, and thence through a cellar window into an adjacent well. With every flash of lightning which took place in the heavens, within at least a circle of twenty miles around Princeton, needles were magnetized in the study by the induced current developed in the wire. The same effect was produced by soldering a wire to the metallic roof of the house, and passing it down into the well; at every flash of lightning a series of currents in alternate directions was produced in the wire.

I was also led, from these results, to infer that induced currents must traverse the line of a railroad, and this I found to be the case. Sparks were seen at the breaks in the continuity of the rail, with every flash of a distant thunder-cloud.

Similar effects, but in a greater degree, must be produced on the wire of the telegraph by every discharge in the heavens;

and the phænomena which I witnessed on the 19th of June in the telegraph office in Philadelphia, were, I am sure, of this kind. In the midst of the hurry of the transmission of the congressional intelligence from Washington to Philadelphia, and thence to New York, the apparatus began to work irregularly. The operator at each end of the line announced at the same time a storm at Washington, and another at Jersey City. The portion of the circuit of the telegraph which entered the building, and was connected with one pole of the galvanic battery, happened to pass within the distance of less than an inch of the wire which served to form the connexion of the other pole with the earth. Across this space, at an interval of every few minutes, a series of sparks in rapid succession was observed to pass; and when one of the storms arrived so near Philadelphia that the lightning could be seen, each series of sparks was found to be simultaneous with a flash in the heavens. Now we cannot suppose for a moment that the wire was actually struck at the time each flash took place; and indeed it was observed that the sparks were produced when the cloud and flash were at the distance of several miles to the east of the line of the wire. The inevitable conclusion is, that all the exhibition of electrical phænomena witnessed during the afternoon was purely the effect of induction, or the mere disturbance of the natural electricity of the wire at a distance, without any transfer of the fluid from the cloud to the apparatus.

The discharge between the two portions of the wire continued for more than an hour, when the effect became so powerful, that the superintendent, alarmed for the safety of the building, connected the long wire with the city gas-pipes, and thus transmitted the current silently to the ground. I was surprised at the quantity and intensity of the current; it is well known, that to affect a common galvanometer with ordinary electricity, requires the discharge of a large battery; but such was the quantity of the induced current exhibited on this occasion, that the needle of an ordinary vertical galvanometer, with a short wire, and apparently of little sensibility, was moved several degrees.

The pungency of the spark was also, as might have been expected, very great. When a small break was made in the circuit, and the parts joined by the fore-finger and thumb, the discharge transmitted through the hand affected the whole arm up to the shoulder. I was informed by the superintendent, that on another occasion a spark passed over the surface of the spool of wire, surrounding the legs of the horse-shoe magnet at right angles to the spires; and such was its inten-

sity and quantity, that all the wires across which it passed were melted at points in the same straight line as if they had been cut in two by a sharp knife.

The effects of the powerful discharges from the clouds may be prevented in a great degree, by erecting at intervals along the line, and aside of the supporting poles, a metallic wire, connected with the earth at the lower end, and terminating above at the distance of about half an inch from the wire of the telegraph. By this arrangement the insulation of the conductor will not be interfered with, while the greater portion of the charge will be drawn off. I think this precaution of great importance at places where the line crosses a river, and is supported on high poles; also in the vicinity of the office of the telegraph, where a discharge, falling on the wire near the station, might send a current into the house of sufficient quantity to produce serious accidents. The fate of Prof. Richman, of St. Petersburg, should be recollected, who was killed by a flash from a small wire, which entered his house from an elevated pole while he was experimenting on atmospheric electricity.

The danger, however, which has been apprehended from the electricity leaving the wire and discharging itself into a person on the road, is, I think, very small; electricity of sufficient intensity to strike a person at the distance of eight or ten feet from the wire, would, in preference, be conducted down the nearest pole. It will, however, in all cases be most prudent to keep at a proper distance from the wire during the existence of a thunder-storm in the neighbourhood.

It may be mentioned as an interesting fact, derived from two independent sources of information, that large numbers of small birds have been seen suspended by the claws from the wire of the telegraph. They had in all probability been instantaneously killed, either by a direct discharge, or an induced current from a distant cloud, while they were resting on the wire.

Though accidents to the operators, from the direct discharge, may be prevented by the method before mentioned, yet the effect on the machine cannot be entirely obviated; the residual current which escapes the discharge along the perpendicular wires, must neutralize for a moment the current of the battery, and produce irregularity of action in the apparatus.

The direct discharge from the cloud on the wire is, comparatively, not a frequent occurrence, while the dynamic inductive influence must be a source of constant disturbance

during the season of thunder-storms; and no other method presents itself to my mind at this time for obviating the effect, but that of increasing the size of the battery, and diminishing the sensibility of the magnet, so that at least the smaller induced currents may not be felt by the machine. It must be recollected that the inductive influence takes place at a distance through all bodies, conductors and non-conductors; and hence no coating that can be put upon the wire will prevent the formation of induced currents.

I think it not improbable, since the earth has been made to act the part of the return conductor, that some means will be discovered for insulating the single wire beneath the surface of the earth; the difficulty in effecting this is by no means as great as that of insulating two wires, and preventing the current striking across from one to the other. A wire buried in the earth would be protected in most cases from the effect of a direct discharge; but the inductive influence would still be exerted, though perhaps in a less degree.

The wires of the telegraph are too small and too few in number to affect, as some have supposed, the electrical condition of the atmosphere, by equalizing the quantity of the fluid in different places, and thus producing a less changeable state of the weather. The feeble currents of electricity which must be constantly passing along the wires of a long line, may, however, with proper study, be the means of discovering many interesting facts relative to the electrical state of the air over different regions.

XXXV. *The Form of the Earth no proof of Original Fluidity.*
By HERBERT SPENCER*.

IT has been generally considered that the spheroidal form of the earth—indicating as it does obedience to centrifugal force—implies a primary state of fluidity. If however it can be shown that, notwithstanding its apparent solidity, the earth must be at the present moment entirely subject to the influences affecting its general figure, and that so far as the gravitative and centrifugal forces are concerned it is plastic still, the theory of original fluidity, however probable on other grounds, can no longer be inferred from the earth's oblateness.

The facts indicative of a varying relationship between the bulk and tenacity of matter are of every-day observation. We constantly see a drop of water maintain its sphericity in spite of opposing forces—increase the mass, and it flows out in com-

* Communicated by the Author.

plete obedience to them. The mud in our streets stands in ridges behind the passing cart-wheel—when scraped together it appears liquid and assumes a horizontal surface. On the spade of the excavator, clay retains its square figure and its sharp angles; but when made into a bulky embankment, it will, if the slope be insufficient, spread itself out on one or both sides of the base, occasionally continuing to slip until it assumes an inclination of six to one.

A comparison of the physical powers of large and small animals exhibits a series of facts of an analogous character. A flea jumps several hundred times its own length, and is uninjured by collision with any obstacle. The great mammals, on the other hand, seem to possess no agility whatever; and a concussion borne by the insect with impunity would smash an elephant to a jelly. Between these extremes may be observed a gradation in the ratios of power to bulk; so that commencing with the smaller creatures, every increment of size is, *cæteris paribus*, accompanied by an under-proportionate increase of strength, until we arrive at that limit (to which the elephant has evidently approximated) where the creature is no longer capable of supporting its own framework.

These and innumerable like facts point to the inference that fluidity and solidity are to a great extent qualities of degree; that the cohesive tenacity of any piece of matter bears, as the mass of that matter is increased, a constantly decreasing ratio to the natural forces tending to the fracture of that matter; and that hence any substance, however solid to our perceptions, only requires to have its bulk increased to a certain point, to give way, and become in a sense *fluid* before the gravitative and other forces.

However repugnant to that “common sense” for which some have so great a respect, this proposition is capable of a very simple demonstration.

The strength of a bar of iron, timber, or other material subjected to the transverse strain varies as $\frac{BD^2}{L}$; B being the breadth, D the depth, and L the length. Suppose the size of this bar to be changed, whilst the ratios of its dimensions continue the same; then as the fraction $\frac{B}{L}$ will remain constant the strength will vary as D^2 , or (since D bears always the same proportion to B and L) as B^2 or L^2 . Hence in similar masses of matter the resistances to the transverse strain are as the squares of the linear dimensions. The same law still more manifestly applies to the longitudinal strain, when

the strength, depending as it does on the sectional area, must, in similar masses, vary as the square of any side. And in the torsion strain we may readily detect the like general principle, that, other things equal, the resistances to fracture bear a constant ratio to the squares of the dimensions.

Not so however with the powers tending to the disruption of matter. The effects of gravity, centrifugal force, and all agencies antagonistic to cohesive attraction, vary as the mass, that is, as the cubes of the dimensions.

However great, therefore, in a given portion of matter, may be the excess of the form-preserving force over the form-destroying force, it is clear that, if during augmentation of bulk the form-preserving force increases only as the *squares* of the dimensions, whilst the form-destroying force increases as their *cubes*, the first must in time be overtaken and exceeded by the last; and when this occurs, the matter will be fractured and re-arranged in obedience to the form-destroying force.

Viewed by the light of this principle, the fact that the earth is an oblate spheroid does not seem to afford any support to the hypothesis of original fluidity as commonly understood. We must consider that, in respect of its obedience to the geodynamic laws, the earth is fluid now and must always remain so; for the most tenacious substance with which we are acquainted, when subjected to the same forces that are acting upon the earth's crust, would exceed the limit of self-support determined by the above law, before it attained $\frac{1}{1,000,000,000}$ th of the earth's bulk.

Reference to a table of the resistances of various substances to a crushing force will render this manifest.

London, Jan. 1847.

XXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 128.]

Anniversary Meeting, November 30, 1846.

THE Marquis of Northampton in the Chair.

The noble President stated that the Council had awarded one of its Medals to M. Le Verrier; two Medals to Mr. Faraday, for his discoveries in the universal action of Electricity and Galvanism; and one of the Royal Medals to Prof. Owen, for his paper on the Blemnite.

After presenting the Medals, the President proceeded to the biographical notices of some of the deceased members, from which we select the following:—

JOHN BOSTOCK, M.D., has long occupied a distinguished station

among the zealous cultivators of Animal Chemistry, Physiology, and other branches of Medical Science. His father had been established as physician at Liverpool, and from his great talents would probably have risen to considerable eminence, had not these brilliant prospects been doomed to sudden extinction by an untimely death, before he had attained the age of thirty. He left an only child, the subject of the present notice, who was born in 1774, the year preceding that fatal event, and whose early education was conducted at the New College at Hackney, at the time when Dr. Priestley was delivering lectures on chemistry. Having imbibed, under such able tuition, an ardent love of science, young Bostock was led to make choice of medicine as his profession. He studied at Edinburgh, where he graduated in the year 1798, and soon afterwards commenced practice in his native town. The activity of his mind was there displayed, not only in numerous contributions to most of the medical and scientific journals, but also in the prominent part which he took in planning and establishing various charitable, scientific and literary institutions, and more particularly the Fever Hospital, and the Botanic Garden; and also the Philosophical and Literary Institution of Liverpool, where, in the capacity of Professor of Physiology, he delivered the first course of lectures there given.

Having secured a competent fortune, Dr. Bostock determined, in 1817, to relinquish his profession and fix his residence in London, where he could possess more extensive means of prosecuting his favourite studies, and enjoy a more enlarged society of scientific friends. He soon became an associate of most of the scientific societies of the metropolis, and an active labourer in their management. In 1818, he was elected a Fellow of the Royal Society; was several times placed on its Councils, and in 1832, filled the office of Vice-President. He was for some years Secretary of the Geological Society, and in 1826 was appointed its President. He long held the office of Treasurer of the Medical and Chirurgical Society, and took an active share in the management of the Fever Hospital, not only as a member of its Committee, but also as one of its House Directors. He was also, during a long period, one of the lecturers on Chemistry at the Medical School of Guy's Hospital, being appointed to that office on the death of his friend Dr. Marcet, in 1822.

Amidst these multiplied public avocations, he found leisure for the accomplishment of a great variety of literary and scientific labours, the aggregate amount of which would appear astonishing to any one who was not acquainted with his methodical habits, his persevering industry, and his advantageous employment of every portion of his time. His contributions to medical and scientific journals, transactions of societies and cyclopædias, amount to no less than sixty-nine; of which twenty are contained in Nicholson's *Journal and Annals of Philosophy*, eighteen in the *Medico-Chirurgical Transactions*, and twelve in the *Cyclopædias of Practical Medicine*, and of *Anatomy and Physiology*. Only one paper by him appears in the *Philosophical Transactions* (in 1829), namely, that "On the spontaneous purification of the Thames Water," recording the observa-

tions he made in the course of some analyses which he undertook at the request of the Commissioners appointed by the Crown to inquire into the supply of water to the metropolis.

Some of the more finished papers and essays which had appeared in these works, were afterwards republished by him in a separate form. Among these are his "Account of the History and present state of Galvanism," originally published in Brewster's Cyclopædia, and which appeared in 1818; and his "History of Medicine," which had been prefixed to the Cyclopædia of Practical Medicine. One of his earliest publications was an "Essay on Respiration;" his attachment to chemical pursuits having naturally led him to the particular study of this part of the animal economy. At a later period, he engaged in the compilation of a general work, embracing the whole subject of physiology, which he completed in three volumes, the last of which appeared in 1827, under the title of "Elementary System of Physiology;" the third and last edition, published in 1837, comprised the whole in one thick octavo volume of nearly 900 pages. It is a work of immense labour and research, containing condensed and elaborate analyses of all that had at that time been published, both as to facts and theories, in the wide field of physiology. It constitutes, in fact, an Encyclopædia of this department of medical science, where the student will find indicated, with scrupulous exactness, the authorities for every statement, and the sources which may supply him with whatever further information he might require on any particular subject. After he had completed this work, he projected a new translation of Pliny's "Natural History," to be accompanied with notes; in 1828 he printed, for private distribution, a specimen of the work, consisting of the first and thirty-third books; and he afterwards devoted a considerable portion of his time to the prosecution of this undertaking, in which he had made some progress at the time of his death. For the last two or three years his health had been declining, but the immediate cause of his death was an attack of cholera, which proved fatal on the 6th of August in the present year.

Respected and beloved by a wide circle of friends and relatives, his memory will long be cherished with affection by those who survive, and with gratitude by the votaries of those sciences which his labours have promoted and enriched.

JOHN CONSTANTINE CARPUE, a distinguished teacher of Anatomy and Surgery.

JOHN THOMSON, M.D., one of the ablest representatives of the last generation of medical men, was Professor of General Pathology in the University of Edinburgh, and died on the 11th of October last, at his residence in the vicinity of that city, at the advanced age of eighty-two years.

He was born in the town of Paisley in Renfrewshire, and in overcoming the impediments of an humble station, of straitened circumstances, and of a defective education, he early exhibited those vigorous intellectual powers which were afterwards so successfully exerted in the acquirement of information and the promotion of science.

His innate love of knowledge soon became apparent by the strenuous application of his mind, in succession, to various important objects of scientific interest. While yet a youth, and during the short intervals of leisure, stolen from the servile drudgery of a medical apprenticeship in his native town, he devoted himself with such energy and success to the study of Botany, as ever after to retain a lively interest in that pursuit. Having completed his medical education in the Universities of Glasgow and Edinburgh, and in the medical schools of London, he settled, in the concluding years of the century, as a general practitioner in Edinburgh. He there delivered lectures on Chemistry, and published a translation of Fourcroy's Elements of that science, accompanied with valuable notes.

Being appointed one of the Surgeons of the Royal Infirmary in the year 1800, he commenced his labours as a Teacher of Surgery, and on the institution, at his recommendation, of a Professorship of Surgery in the Royal College of Surgeons, he was appointed to that office. It was on his representation that the injurious system which then prevailed, of changing the surgeons at short intervals, was abandoned, and the tenure of that office rendered one of reasonable duration; and that the delivery of Clinical lectures by the surgeons in office, of all modes of instruction the most valuable, was commenced. To his counsel the College of Surgeons were indebted for the foundation of a Museum of Anatomy, both healthy and morbid, which, enriched as it has since been by many valuable collections, now ranks second only to that of the Royal College of Surgeons of England.

For a period of sixteen years Mr. Thomson delivered the lectures on Surgery in the Hall of the College to crowded auditories of students and practitioners. In the year 1806 he was appointed by the crown Professor of Military Surgery in the University of Edinburgh, a chair which had been newly created by the government of that period.

The publication on which his permanent reputation rests bears the title of "Lectures on Inflammation." It appeared in 1813, and was in effect a revival and masterly exposition of the views and doctrines of John Hunter, which, partly from a deficiency of perspicuity in that great man's style, and partly from the small degree of attention which they had excited among the profession, had never before obtained their due influence, nor had their truths been sufficiently recognised and established. It is a work which exhibits the results of acute discrimination, unwearied ardour, persevering research, and a clear and careful method of argumentation. It has been translated into many foreign European languages, and speedily became the standard authority on that important subject, constituting as it does the basis of all Pathology.

On the death of Dr. Gregory in 1821, Dr. Thomson resigned his surgical appointments, and having formerly graduated at King's College, Aberdeen, now joined the College of Physicians and practised as consulting physician, commencing at the same time a course of lectures on the practice of physic. This may be regarded as the

second remarkable period of his professional career. The novel and distinguishing feature of this course of lectures consisted in its being founded on pathological anatomy, or the structural changes induced by disease in the several tissues of the body, as auxiliary to the ancient system of pure symptomatology. To accomplish this object in the most effectual manner, Dr. Thomson had recourse to the aid of the draughtsman, and presented to the eye of the student accurate coloured delineations of morbid appearances, arranged on anatomico-physiological principles, and accompanied by their corresponding histories. The result of this spirited enterprise, prosecuted both abroad and at home during a period of several years, and at a very considerable pecuniary sacrifice, has been a splendid collection of anatomical drawings, surpassing all others at present existing in richness and extent.

In 1832, a Chair of General Pathology was added to the University of Edinburgh, to which endowment the suggestions and the evidence of Dr. Thomson before a commission appointed by Lord Liverpool's government in 1826, materially contributed. The appointment of first professor was conferred upon himself; but the pressure of advanced years and infirm health unfortunately compelled his retirement after he had held it for four years.

At various times Dr. Thomson visited the British metropolis and the continent of Europe, chiefly for the purpose of personally examining their pathological collections; and after the battle of Waterloo he repaired to Brussels, in order to avail himself of the advantages which were afforded on that occasion of examining the nature and treatment of gun-shot wounds and field injuries. The result of his observations he shortly afterwards published in a "Report of the wounded at Waterloo."

To Dr. Thomson's spirit of independence, not less than to his ardent love of knowledge, the school of Edinburgh is specially indebted; for in addition to his active exertions in the foundation of Professorships, of a museum and of a library, and the establishment of a dispensary for visiting the poor in their own dwellings, he was ever watchful of its interests, and as fearless in denouncing usages which he considered prejudicial as he was prompt in suggesting and earnest in promoting beneficial changes and reforms. To the jealousies which are ever engendered by merit, and by attempts at reform, and to the opposition he encountered from the magisterial authorities of the city, may be ascribed his failure of success when a candidate for the vacant chair of Dr. Gregory, although he was supported by overwhelming testimonials of superior claims from the most eminent medical men throughout Europe; and the same cause, together with the liberal bias of his political views and opinions, (for though he did not obtrude he never sought to conceal them) will also explain the anomaly, that although no professional opinion was more highly esteemed than his, yet he never attained to extraordinary popularity in practice. Some of the prejudices he had to contend against were the fruits of his scrupulous honesty, of the simplicity of his mind and manners, and of his hostility to every species

of professional charlatanism in whatever quarter it was displayed. It can excite no surprise that he stood high in the estimation of his many celebrated contemporaries of the Scottish metropolis, and that he should have enjoyed a cordial and continued intimacy with such men as Stewart, Allen and Mackintosh. Associated with the projectors of the Edinburgh Review, he was a contributor to some of the early numbers, of articles connected with medical philosophy. In other researches of science not strictly professional, Dr. Thomson evinced an erudition of greater extent and profundity than could have been attained by any individual not possessing his quickness of apprehension and unquenchable thirst for knowledge.

The profession at large are perhaps scarcely aware of the extent of their obligations to Dr. Thomson. Some however yet survive who can attest the peculiar interest he inspired in his pupils by his earnestness as a teacher, and the enthusiasm they imbibed from his precepts and cherished by his example. Unostentatious in his search after truth, he was neither so frequent or voluminous an avowed contributor to the common stock, as many men of much inferior talent; but he was continually engaged in fostering the spirit and directing the pursuits of others, of whom some have largely contributed, in their riper years, to extend the boundaries of medical and surgical knowledge.

The biography of Dr. Cullen, whose character he held in high veneration, occupied the years of his retirement. The vigour of his faculties remained unimpaired to the last, and he contemplated with calm serenity the approaching end of a life devoted to the cultivation and improvement of the profession he had chosen, and of which he was a distinguished ornament.

Astronomy has sustained a heavy loss, in the last year, by the death of BESSEL. FRIEDRICH WILHELM BESSEL was born at Minden, on the 22nd of July 1784. His father held a civil office under the Prussian government, with the title of *Justizrath*, but his means were narrow and his family numerous; and at the age of fifteen the future astronomer began his career as an assistant or apprentice in a mercantile house in Bremen. Having acquired a taste for Astronomy and Mathematics, he assiduously devoted his leisure time to the study of those sciences; and his progress was such that he soon attracted the notice and obtained the friendship of Dr. Olbers, then in the zenith of his fame. His first published essay, which appeared in Zach's '*Monatliche Correspondenz*' in 1804, was a reduction of the observations made by Harriot and Torporley of the comet of 1607. In communicating this paper to Zach, Olbers eulogised, in the warmest terms, the acquirements and industry of his young friend, and expressed his regret that so much talent and zeal, and such powers of calculation, should not have been acquired for the exclusive service of astronomy. An opportunity was soon found of making the desired acquisition; Bessel, in 1806, becoming successor to Harding in the office of assistant to Schroeter, in the Observatory of Lilienthal. In this congenial situation he rapidly acquired for himself a great reputation; and

indeed, almost from the first, took his place as one of the best astronomers of Europe. In 1810, he was appointed professor of astronomy in Königsberg; a post which he occupied during the remainder of his life, and over which his genius and labours have thrown an imperishable lustre.

The Observatory of Königsberg, now so celebrated, owes not only its celebrity, but its existence to Bessel. The building was begun in 1811 under his direction. It was finished in 1813; and the first published observations are dated in November of the same year. From 1815 the observations have been published regularly; accompanied by full descriptions of the different instruments successively employed, the elements of reduction, some valuable catalogues, and all the information necessary to inspire complete confidence in the results. This publication has exercised a powerful influence on the progress of practical astronomy.

Bessel's scientific life is one of extraordinary fertility, and it is only a few of the more important of his productions which can even be alluded to in this brief notice. His principal work, the '*Fundamenta Astronomiæ*,' is peculiarly interesting to English astronomers, from the circumstance of its being founded on observations made at the Royal Observatory at Greenwich. Bradley's observations, as is well known, were taken possession of by his representatives, and presented by them to the University of Oxford. From various causes, their publication was not completed until 1805, forty-three years after the death of Bradley; and although some results and some imperfect catalogues had been deduced from them, the great mass of the observations still remained in a state in which they were nearly useless. At the instigation of Olbers, Bessel undertook the formidable task of reducing the whole series. The reduction was commenced in the autumn of 1807, and, though frequently interrupted by his other avocations and duties, it was never wholly laid aside till its completion in 1818, in which year the '*Fundamenta*' made its appearance. Of this great work it would be difficult to speak in terms of too high praise. Besides elaborate determinations of all the principal elements of the reduction,—the errors of the instruments, the height of the pole, refraction, parallax, aberration, precession, proper motion,—it contains a catalogue of the mean places of 3222 fixed stars, observed between 1750 and 1762 with the best instruments in existence at that time; and reduced to the epoch of 1755 with a precision and accuracy of which there was no previous example. It now furnishes astronomers with the best existing means of determining all those data which can only be deduced from a comparison of observations made at considerably distant intervals of time, and may be considered, in fact, as having laid the foundations of the principal improvements which have been made in astronomy since the date of its publication.

The '*Tabulæ Regiomontanæ*,' published in 1830, may be regarded in some respects as a supplement to the '*Fundamenta*.' This collection was undertaken with a view to facilitate the reduction of planetary observations, past and future, on a uniform system;

and it contains all the tables necessary for that purpose as well as for ordinary star-reduction, for the century to which it applies, founded on elements derived from the comparison of Bradley's with Bessel's own observations, together with the formulæ for computation. It is in daily use in every observatory, and has led to the general adoption of a uniform and precise system of astronomical calculation which has been attended with signal advantage to the science.

The services rendered to astronomy by Bessel were not confined to the observatory and the closet. In the course of a series of experiments, undertaken in 1826, for the determination of the length of the seconds' pendulum, he detected and demonstrated the existence of a cause of error in the usual reduction to a vacuum, which, though it had long before been clearly pointed out by Du Buat, appears to have been entirely overlooked and forgotten. This consists in the fact that every oscillating body *drags* along with it a certain portion of air, or of the fluid in which it moves, so that the specific gravity of the actually moving mass is different from that of the body at rest; and as the quantity of *dragged* air depends on the form of the moving body, the specific gravity cannot be determined *à priori*, but must be found by experiment for each particular pendulum. It followed, therefore, that the results of all the previous pendulum experiments were erroneous to the extent occasioned by the neglect of this circumstance. Bessel's method of conducting the experiment was extremely ingenious; and it may be asserted that his determination of this important astronomical element—the absolute length of the seconds' pendulum—is the only one yet given which is deserving of full confidence. The details and results were published in the Berlin Memoirs in 1828.

Another very important result of the indefatigable activity of Bessel, is the 'Gradmessung in Ost-Preussen,' published in 1838. A series of geodetical measurements, comprehending the measure of an arc of the meridian of the Dorpat Observatory, had been executed by Struve and Von Tenner in Russia, and it was of great importance, in reference to the question of the figure of the earth, to connect them with those of the west and south of Europe. The operations necessary for this purpose were undertaken by the Prussian government in 1830, and executed under the direction of Bessel, who seized on the opportunity of measuring an arc of the meridian of his Observatory. In consequence of the novel methods employed both in the geodetical and astronomical parts of this operation the details are of extreme interest, more particularly with reference to the ingenious apparatus for measuring the base line, the mode of observing and connecting the terrestrial angles, and the application of a method of computation by which every geometrical relation subsisting among the angles over the whole triangulated surface is expressed by equations of condition, and the results, with their probable errors, deduced by the method of least squares. The 'Gradmessung' may be described as having done for geodesy what the 'Fundamenta' did for astronomy; both works exhibiting the application of the best and surest methods of deducing results from ob-

servations which science has yet placed within the reach of the computer.

Several of his minor papers on the subject of geodesy, published in the 'Astronomische Nachrichten,' are of great interest and value. He recomputed the French triangulation between Montjoux and Formentera (No. 438), and the sector observations of the English and Indian arcs measured by Mudge and Lambton (334, 336); and from the whole of the meridional arcs hitherto measured with admissible precision, deduced elements of the terrestrial spheroid which give probably the nearest approximation which has yet been made to the true form and magnitude of the earth.

In connexion with the measurement of the pendulum and arc of meridian, he also undertook a comparison of the standard measures of Prussia; determined their relation to the unit of the French measures; and directed the execution of a new standard which will probably become an object of great interest in some future age. The details of these delicate and difficult comparisons are described by him in an interesting work published by order of the Prussian government in 1839.

The question of the annual parallax of the fixed stars,—a vexed question in astronomy since the days of Bradley,—has of late years acquired an extraordinary interest through the labours of Struve, Henderson, and Bessel. Having previously made some unsuccessful attempts, Bessel at length determined to attack the problem in a new way, and to subject one individual star to a scrutiny which would infallibly decide whether any appreciable parallax existed in that particular case. The star which he selected was 61 *Cygni*, a binary system whose two members had been proved to be physically connected, and which, by reason of the very considerable distance between the component bodies, and its large proper motion, afforded strong presumptions of its being one of the nearest to the earth. With the splendid heliometer of the Königsberg Observatory, an instrument peculiarly adapted for such a purpose, he commenced a series of micrometrical measurements of the distances of the star from two others in its vicinity, which were continued from August 1837 to March 1840, and conducted with all the appliances and precautions for attaining extreme accuracy which were to be expected from the importance of the inquiry and the consummate skill of the observer. The results exhibited a periodic and systematic fluctuation of the places of the double star, agreeing exactly in its law and period with the effects of parallax, and referable to no other known cause. The parallax thus indicated was indeed an extremely minute quantity, amounting only to 31-100ths of a second; but no one in a condition to appreciate the evidence on which it rests will doubt the possibility of detecting such a quantity by such means; and astronomers, accordingly, regard the conclusion as one which, if not absolutely certain, is at least so extremely probable, that it must command assent until disproved by another equally good, and longer continued, series of observations.

The fruits of the extreme precision which Bessel so greatly con-

tributed to introduce into astronomical observations and reductions, have already begun to make their appearance. In an interesting disquisition published in some recent numbers of the 'Astronomische Nachrichten' (514, 515, 516), he seems to have established the existence of a very remarkable phenomenon which had been indicated, at different times, by Pond, namely, the variability of the proper motion of two of the principal stars, *Procyon* and *Sirius*. It is unnecessary to point to the boundless field of speculation respecting the constitution of the sidereal heavens which such a discovery lays open.

The annals of astronomy afford many illustrious examples of indefatigable industry and prolific labour, but few more remarkable than Bessel. From the date of his first publication in 1804, the astronomical periodicals of Germany teem with his productions. Zach's 'Monatliche Correspondenz,' Lindenau's 'Zeitschrift,' Bode's 'Jahrbuch,' the 'Berlin Ephemeris,' the 'Memoirs of the Berlin Academy,' and, more especially, Schumacher's 'Astronomische Nachrichten,' have all been enriched by his communications. The last-named journal, indeed, contains upwards of 180 bearing his signature, embracing almost every subject connected with the science and practice of astronomy, and questions of every order of difficulty. Some of his papers in the 'Berlin Memoirs' are extremely remarkable, not only by reason of the results arrived at, but as showing the resources and originality of the author, and his skill in the highest departments of analysis and physical astronomy. Among these may be mentioned, in particular, his researches on the great comet of 1807 (published in 1810), in which he proposed a method, which is now generally followed, of determining the orbit when it is necessary to take the perturbations into account; a most important memoir on the precession of the equinoxes (1815); another on the planetary perturbations (1824); and three on the right ascensions of Maskelyne's 36 stars (1818, 1819, 1825). Among his later productions are two quarto volumes of 'Untersuchungen' (or Astronomical Researches), and a third is said to have been nearly prepared for the press. Many other titles might be cited, but those now mentioned may suffice to give an idea of the wide extent over which he ranged. Indeed it may be said of Bessel, that there is no department of astronomy which he did not handle, and that nothing passed through his hands without receiving extension or improvement. But, perhaps, that which stands out as most remarkable in his writings is his profound knowledge of the theory of instruments; and perhaps also the influence of his example in teaching observers to trust less to the mechanical perfection of instruments, and to bestow more pains in determining their errors so as to remove them by numerical corrections, have contributed as much to the advancement of astronomical science as his own proper labours and discoveries. His countrymen adopted his methods as the models of their practice; the present state of German astronomy shows with how much advantage.

Bessel was elected a Foreign Member of this Society in 1825. He died at Königsberg on the 17th of March 1846, after a long and

painful illness occasioned by an internal disease. The history of his labours will occupy a large and prominent place in the history of astronomy during the first half of the present century.

The *BARON DE DAMOISEAU* was one of the most distinguished astronomers of the age. His most considerable work was his "*Mémoire sur la Théorie de la Lune*," which was presented to the Institute in 1821, but not published before 1827, when it appeared in the "*Mémoires des Savans Etrangers*." The methods which are used in this important memoir are, generally speaking, the same as those adopted by Laplace: the moon's true longitude being assumed as the independent variable, and the final equations solved by the method of indeterminate coefficients; the solutions being given in numerical and not in literal coefficients, as in the great work of *Plana* on the same subject. The approximations, also, are carried to a greater extent than in the "*Mécanique Céleste*." This memoir was followed by the celebrated "*Tables of the Moon*," which were founded upon it, and which appeared in 1824 under the title "*Tables de la Lune, formée sur la seule Théorie de l'Attraction et suivant la division du cercle en 400 degrés*." They form the first, and indeed the only expanded tables of the moon which are founded entirely upon theory, borrowing nothing whatever from observation but the simple elliptic elements, the proportion of distances of the sun and moon, and the masses. All preceding tables, such as *Mayer's*, *Borg's* and *Burckhardt's*, had derived many of their coefficients empirically from observation. These tables are the basis of those which are used by the present *Astronomer Royal* in the great lunar reductions which are now in progress under his superintendence.

The *Baron de Damoiseau* was also the author of "*Tables of the Satellites of Jupiter*," and of many other works and memoirs connected with the advancement of astronomical science: he was a very profound analyst, a most laborious and faithful calculator, and the author of the most important advancements which the lunar theory received in the period which intervened between the appearance of the great works of Laplace and *Plana* to which I have before referred.

Jan. 28, 1847.—"On the Lunar Atmospheric Tide at *St. Helena*." By *Lieut.-Colonel Edward Sabine*, R.A., For. Sec. R.S.

The results of the observations made by *Captain Lefroy*, of the *Royal Artillery*, *Director of the Magnetical and Meteorological Observatory at St. Helena*, are here given; from which it appears, on the examination of the barometrical changes during seventeen months, that a maximum of pressure corresponds to the moon's passage over both the inferior and superior meridians, being slightly greater in the latter case, and that a minimum corresponds nearly to the rising and setting, or to six hours before and after the former periods. The average atmospheric pressures are 28.2714 inches in the first case, and 28.2675 in the last; the difference being 0.0039 inch. The height of the cistern of the barometer above the sea is 1764 feet; and the latitude of the Observatory $15^{\circ} 57' S$. These results were still further confirmed by those of a series of observations during

two years. These observations also establish the conclusion that the moon exerts a greater influence on the amount of atmospheric pressure at the periods of her perigee than at those of her apogee.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 139.]

Jan. 8, 1847.—Reduction of the Observations of Halley's Comet, made at the Cambridge Observatory in the years 1835 and 1836. By the Astronomer Royal.

These observations were chiefly made with the 5-foot equatoreal. They were reduced as far as was practicable at the time, and the corrections for refraction and parallax applied. In this state they were published in the Cambridge Observations for 1835.

The instrumental errors, though pretty well known, and known to be small, were not then corrected. It was thought better to put off this part of the complete reduction until correct places of the small stars of comparison could be obtained. In 1836 and 1837 most of these stars were observed at Greenwich, and they are sufficiently numerous to establish all the results which can be drawn from the observations of the comet. Some stars could not be seen, though carefully looked for.

In exhibiting the results two methods have been followed. First, the place of the comet has been determined *differentially* with respect to small stars, which have themselves been subsequently determined. Secondly, the error of the instrument has been computed from the observations of known stars, admitting of accurate observation, and the error so deduced has been applied to correct the instrumental observation of the comet. A comparison of these results will often show which is to be preferred.

The final results are compared with the Ephemeris circulated by the Superintendent of the Nautical Almanac, under the date Dec. 30, 1835, which is the same, as to places unaffected by perturbation, with the Ephemeris in the Appendix to the Nautical Almanac of 1839. In this Ephemeris the effects of aberration are fully included, contrary to the supposition of the Astronomer Royal in the Cambridge Observations, 1835, p. lxiv.

It has not been thought advisable to deduce the elements of Halley's comet from these observations. That task will be best performed by some future Rosenberger, who, with all the observations from both hemispheres before him, may hope to bring a satisfactory result out of the whole mass.

The work is divided into sections :—

1. A catalogue of stars of comparison.
2. Errors of Ephemeris in R.A. from differential observations only.
3. Computation of the index errors of the equatoreal in R.A.
4. Errors of the Ephemeris in R.A. from the data of the previous section.

5. Errors of the Ephemeris in R.A. from differential transits at the mural circle.

6. Errors of the Ephemeris in N.P.D. from differential observations only.

7. Computation of the index errors in N.P.D. of the equatoreal.

8. Errors of the Ephemeris in N.P.D. from the data of the last section.

9. A tabular collection of all the previous results, with remarks.

On a proposed Alteration of Bessel's Method for the Computation of the Corrections by which the Apparent Places of Stars are derived from the Mean Place. By the Astronomer Royal.

After mentioning the great superiority in uniformity and simplicity of Bessel's method over those previously in existence, the Astronomer Royal remarks, that the strict attention to *sign* required both in the partial additions and in collecting the sum is exceedingly troublesome, and that more errors in observatory business arise from oversight as to *sign* than from any other cause. He has therefore been led to consider the possibility of avoiding changes of sign, and suggests the following method as probably an improvement in giving the data of the *corrections* in the Ephemeris and Catalogue.

On examining the *maximum* values of the quantities A, B, C, D in the Nautical Almanac, and of $a, b, c, d, a', b', c', d'$ in the British Association Catalogue, it will be seen that A, B, and D can never be equal to 25, that c' is always less than 25, and that up to a north polar distance of $3^\circ 45'$, c must also be less than 25. All the other numbers are less than 1.2.

$$\begin{array}{lll} \text{Let } E=A+25 & \text{Let } e=a+1.2 & \text{Let } e'=a'+1.2 \\ F=B+25 & f=b+1.2 & f'=b'+1.2 \\ G=C+1.2 & g=c+25 & g'=c'+25 \\ H=D+25 & h=d+1.2 & h'=d'+1.2 \end{array}$$

All the introduced symbols are necessarily positive.

Arranging and multiplying

$$\begin{array}{l} Aa=Ec-1.2E-25e+30 \\ Bb=Ef-1.2F-25f+30 \\ \text{\&c.} \quad \quad \quad \text{\&c.} \end{array}$$

And

$$\begin{array}{l} Aa'=Ee'-1.2E-25e'+30 \\ \text{\&c.} \quad \quad \quad \text{\&c.} \end{array}$$

Hence, collecting and arranging, it will be found that the sums of the corrections in R.A. and in north polar distance will be respectively,—

$$\begin{array}{l} \text{In R.A.} = 120.0 + Ec + Ff + Gg + Hh \\ \quad \quad \quad - (1.2E + 1.2F + 25G + 1.2H) \\ \quad \quad \quad - (25e + 25f + 1.2g + 25h) \\ \text{In N.P.D.} = 120.0 + Ee' + Ff' + Gg' + Hh' \\ \quad \quad \quad - (1.2E + 1.2F + 25G + 1.2H) \\ \quad \quad \quad - (25e' + 25f' + 1.2g' + 25h') \end{array}$$

where the numbers are seconds of time in the first group, and seconds of space in the latter.

It will be seen that the second line in each group is identical, and depends solely on E, F, &c.; that is on A, B, C, and D. This therefore could be tabulated in the *Ephemeris* for the day, under one symbol K. The third lines depend on *a, b, &c.* and *a', b', &c.*; and therefore admit of tabulation in the *Catalogue* as *k* and *k'* respectively for each star. The *corrections* could now be in this form:—

$$\text{In R.A.} = 120^{\circ} \cdot 0 + E e + F f + G g + H h - (K + k)$$

$$\text{In N.P.D.} = 120'' \cdot 0 + E e' + F f' + G g' + H h' - (K + k')$$

The greatest possible value of K is about 182, and the greatest values of *k* and *k'* are about 170. Let

$$L = 190 - K, \quad l = 170 - k, \quad l' = 170 - k';$$

then substituting these values in the last equations, the corrections are,

$$\text{In R.A.} = E e + F f + G g + H h + L + l - 240^{\circ} \cdot 00$$

$$\text{In N.P.D.} = E e' + F f' + G g' + H h' + L + l' - 240'' \cdot 0$$

in which every symbol is necessarily positive.

The Astronomer Royal finds that logarithms with five decimal places would be wanted for the corrections in R.A., and that four would be sufficient in N.P.D.

An additional column would thus be required for the *Ephemeris* and two columns for the *Star Catalogue*, but there would be no sign employed either in the tabulation or computation. The trouble of deducing E, F, &c., *e, f, &c.*, L, l, &c. is trifling, and might be made still easier by subsidiary tables. The change proposed is not recommended for uncatalogued stars observed only once. Stars which are nearer the pole than $3^{\circ} 45'$ require special treatment.

Orbits of Double Stars. Computed by Capt. W. S. Jacob, B.E.

These orbits have been computed from all the observations accessible to the author, including his own for the current year. The orbit of Castor has only been roughly deduced, as the observations are not yet sufficient to define the elements with precision. It was undertaken by Captain Jacob for the purpose of ascertaining what alteration was required in Mr. Hind's elements to make them represent his own observations, and those of Captain Smyth in 1838 and 1843.

In computing the orbits of the other stars, the method of least squares has been employed to correct the elements which were obtained in Sir J. Herschel's manner.

The stars which Captain Jacob has computed are Castor, ξ Ursæ, γ Virginis, and 70 Ophiuchi.

Notice respecting a pair of Chinese Planispheres brought from Chusan, and presented to the Society by Captain Sir E. Home, Bart. R.N. By Mr. Woolgar.

The hemispheres are of twenty-five inches internal diameter, projected stereographically on the plane of the ecliptic. The magnitudes of the stars are represented *conventionally* by a method which makes a star of the first magnitude less conspicuous than one of the third or fourth. There are six magnitudes. The principal stars are

connected by right lines. The groups thus formed sometimes do, and sometimes do not, agree with those found in some European maps.

The map is executed coarsely by block-printing. The positions and magnitudes are incorrect. There is no appearance of European origin. The selection of stars of the fifth and sixth magnitude could not well have been copied, and some stars are inserted which are not to be found in any common catalogue or map. The epoch seems to be about A.D. 1735.

A Historical Survey of Comets. By Dr. Michelsen.

The author commences with a general review of the early history of cometary astronomy, and notices the uncertainty attaching to ancient European accounts of comets: the information given us by the Chinese annalists, Ma-tuon-lin, as sketched by Mailla, Gaubil and De Guignes, presenting a more definite aspect. He remarks that the comet which appeared in the time of Anaxagoras may be considered the first established historically, though as yet unconfirmed by any astronomical calculation.

The author then proceeds to give a detailed description of the most celebrated comets, commencing with Halley's, which he endeavours to trace back as far as the year 426 B.C. The various confirmed appearances of the comet since 1456 are then described, and also circumstances relating to comets in previous centuries, which might possibly be identical with Halley's.

The comet of Encke is traced from its discovery by Mechain, in 1786, to the present time. The detection of a resisting medium in space from the motion of this comet, and the determination (from its perturbations) of the mass of Mercury, are also noticed.

Biela's comet is described at its different appearances since the year 1772. The author notices the supposed identity of the comets of 1264 and 1556, and of those of 1532 and 1661. The great comet of 1843 is described, and, assuming the period of revolution to be about 175 years, some ancient comets are mentioned which might possibly be the same.

Those comets which, though only observed at one appearance, yet remained visible long enough to allow their periodicity to be determined, are placed in a separate class. The comet of Olbers in 1815 is computed by Bessel to have a period of 74 years, and the next return is fixed for February 9, 1887. The comets of 1740 and 1666 do not show the least resemblance to this. The great comet of 1811 was found by Argelander to have a period of 3066 years. The second comet of 1811 was computed by Nicolai; it was much fainter than the first: the period assigned is about 763 years. Encke made the time of revolution of the comet of 1812 about 71 years. The comet of 1807 was computed by Bessel, who fixed the period at about 1714 years.

The author describes the celebrated comet of 1680, that of Lexell in 1770, the comets of 1769, 1780, 1783, 1793, &c., and concludes that there are three comets whose return is certain; five probably periodical, from the similarity of their elements with those of pre-

ceding comets; and nineteen for which elliptical orbits have been calculated with some degree of probability, making the total number of periodical comets twenty-seven.

In the latter part of the paper a general account of remarkable comets is given, commencing with that mentioned by Ovid, and continued down to 1843. The author has collected together the best determinations of the orbits of periodical comets, and many particulars relating to the physical appearance of these bodies.

XXXVII. Intelligence and Miscellaneous Articles.

ON NEWTON'S TELESCOPE AT THE ROYAL SOCIETY.

GENTLEMEN,

Sidmouth, Feb. 9, 1847.

YOUR correspondent Mr. Weld charges me, in a somewhat angry tone, with having made an *unfounded* statement respecting the telescopes of Newton and Hadley. For my own justification I subjoin the extract from which I derived my information, and leave the result in the hands of Sir James South and Mr. Weld.

With respect, I am, Gentlemen,

Yours obliged,

N. S. HEINEKEN.

"The Newtonian reflecting telescope was discovered by the head, and made by the hands, of Sir Isaac Newton in 1671. Its large speculum was two inches and three-tenths in diameter; its focal length was about five inches, and magnified thirty-eight times: it is in the possession of the Royal Society. I regret to say it is in a most dilapidated condition, and its eye-glass is lost. The next of any importance was made by Hadley in 1728; its large speculum's diameter was about six inches; its focal length about sixty-three inches; it magnified 230 times; in performance it equalled the great Huygenian refracting telescope of six inches diameter and 123 feet focus. He gave it to the Royal Society. Its metal is ruined, and its tube, its stand, and other of its appurtenances are lost, &c."—*Times*.

I have extracted the above from the Magazine of Science, vol. vii. p. 36.—N. S. H.

Note.—We learn, on making inquiry of eminent practical astronomers, on whose judgement we can rely, that there is no foundation for the representation given in the above-cited letter to the Times newspaper. At the same time we cannot see that any blame whatever attaches to our much-respected correspondent Mr. Heincken, relying as he has done on the authority which he quotes.—Ed.

FORMATION OF SULPHOVINIC ACID. BY M. M. E. MILLON.

The author states, that some years since wishing to prepare sulphovinic acid with peculiar care, he made a mixture of sulphuric acid and alcohol in a platina crucible surrounded with ice and common salt: the acid mixture being afterwards saturated, did not yield the slightest trace of sulphovinate.

Wishing afterwards to ascertain precisely the conditions under

which the formation of sulphovinic acid occurs, the author discovered that it depended on several circumstances, the principal of which are, —1st, the relative proportions of acid and alcohol; 2nd, the heat applied to the mixture; 3rd, the heating of the two liquids at the moment of mixing them; 4th, the time during which the acid and alcohol remain in contact. To ascertain these points, the author at first employed monohydrated sulphuric acid SO^3, HO , and absolute alcohol $\text{C}^4 \text{H}^6 \text{O}^2$, or else hydrated $\text{C}^4 \text{H}^6 \text{O}^2, \text{HO}$; there is no difference in the progress of the reaction, whether absolute or hydrated alcohol be employed.

To determine the quantity of sulphovinic acid formed, the same quantity of sulphuric acid as that used with the alcohol was mixed with water; and this last was used in such proportion as to make the diluted acid equal in volume to the mixture of acid and alcohol. Equal portions of these were then saturated with an alkaline solution of known strength, and by the difference the proportion of combined sulphuric acid was determined; it being ascertained that sulphuric, on becoming sulphovinic acid, saturates one-half less base.

When equivalents of sulphuric acid and anhydrous alcohol are employed, the following results are obtained: if the alcohol be rendered extremely cold, and the sulphuric acid be gradually added, so as to avoid raising the temperature, no combination whatever takes place. The sulphuric acid retains all its saturating power; and if it be kept constantly cooled by melting ice, the strength of the acid remains the same for several successive days. A temperature of 50° to 60° F. is sufficient to cause the conversion of 77 parts out of 100 of sulphuric acid into sulphovinic; thus about three-quarters of what the sulphuric acid is capable of yielding are produced; this proportion cannot be exceeded. It remains the same for several months, and it is useless to raise the temperature of the mixture. When equivalents of sulphuric acid and alcohol are used, 77 out of 100 of the acid is the largest proportion that can be formed into sulphovinic acid. This formation requires several days at 50° to 60° F.; but it occurs in a few hours at 86° to 95° F.; and it takes place in a few minutes if the mixture be immersed in boiling water; and it is a remarkable circumstance that the combination is instantaneous, if the alcohol be suddenly poured into the sulphuric acid so as to produce great heat.

It appears, therefore, that there are in reality, in this formation of sulphovinic acid, three sources of chemical action which are equivalents; they are time, the temperature applied, and the natural heating of the mixture. The action of light was repeatedly attempted to be substituted; but the direct application of the most intense solar rays never accelerated the formation of sulphovinic acid.

The mixture of two equivalents of alcohol with one equivalent of sulphuric acid acts exactly in the same manner as the preceding mixture; only it is more easy to avoid in it the formation of sulphovinic acid; the influence of time and heat is similarly exerted. The sulphuric acid produces a little less sulphovinic acid, from 73 to 74 in 100 parts; the non-action of light is similar.

If however two equivalents of sulphuric acid and one equivalent of alcohol be employed, circumstances are altered; sulphovinic acid is always formed; and still more, the proportion of sulphovinic acid is always the same, whether the acid be poured into the alcohol or the reverse; whether metallic vessels be used or not; and whether they are or are not cooled. The whole of the acid and alcohol do not however combine; the reaction does not take place on much more than half of the mixture, for not more than 54 of sulphovinic acid can be formed from 100 of sulphuric. This proportion does not change by prolonging the contact, or even by the application of a heat of 212° F. continued for several hours; the capacity of saturation of the acid mixture remains the same. In heating to 212°, the volume is scarcely diminished; it follows, therefore, that the alcohol which is not changed into sulphovinic acid must be retained by a peculiar affinity.—*Ann. de Ch. et de Phys.*, Fevrier 1847.

ON A METHOD OF PRODUCING LIGHTS AND SHADES IN EQUAL PERFECTION IN DAGUERREOTYPE PICTURES. BY MM. BELFIELD LEFEVRE AND LEON FOUCAULT.

It was observed soon after M. Daguerre had made known his wonderful discovery, that the iodized plate was not suited for producing a perfect image of every object, but that if there was a great variety of light and shade, *i. e.* very light and very dark parts, they would not be simultaneously brought out with correctness: one of these two alternatives must be chosen; either to stop the process when the light parts of the picture are brought out (in which case the dark parts will not be clearly distinguishable), or to allow the light to act for a longer time, in order to render the dark parts distinct; but in this case the light parts will be rendered indistinct, or, as it is called, burnt.

These evils have fortunately been remedied by the employment of certain substances, which not only allow of the operation being performed with greater rapidity, but also produce sensitive surfaces capable of receiving a much greater variety of tints. These are, however, far from being perfect; and therefore if any process could be discovered which would bring out the light and dark parts of the picture with equal distinctness, without increasing the sensitiveness of the plates, it would be advisable to have recourse to it in certain cases. If the operator knows how to work well upon these different sensitive surfaces, without confining himself exclusively to the most sensitive, he will be able to produce any effect desired; for instance, if requisite, he will moderate the intensity of the rays of the sun bearing upon objects of inadequate reflective powers, or heighten the tone of a picture.

With a view to facilitate the attainment of good results, Mr. Belfield and myself determined on making known a new mode of preparing the sensitive surfaces, the effect of which is to impart to the plates the property above-mentioned, and which assimilates them to the human retina.

Our method requires the employment of iodine and bromine, and is easily performed by persons who are accustomed to employ those substances separately. It consists in polishing and iodizing the plate in the ordinary manner, and afterwards causing it, by any convenient means, to absorb three times as much vapour of bromine as is usually thought sufficient to render the plates as sensitive as possible. Whilst the ordinary quantity of bromine does not visibly alter the tint of the iodized layer, that which we recommend causes it to assume a deep bluish violet tint.

The sensibility of the plates thus surcharged with bromine is reduced to a third of what it would be if the ordinary quantity only were used; but at the same time they are rendered capable of producing a perfect picture of subjects presenting the greatest variety of shade. This will be seen by inspecting a small picture, presented herewith, which was produced when the sun was shining. There will be perceived the clouds in the sky, white houses, with the shadows well-defined, and trees, the foliage of which is delineated much in the same manner as if executed by an artist.

We recommend the ordinary quantity of bromine to be exactly tripled; as if less than this quantity be used, the picture will not be properly brought out; and if more than this quantity were used, the mercury would not be properly condensed and the image would not be so well-defined.

This new property, communicated to the iodized plates by an excess of bromine, may be very usefully applied; and besides, as it appeared to us that this statement might be useful to the chemical world, we have been induced to make this communication to the Academy.—*Comptes Rendus des Séances de l'Académie des Sciences, (as inserted in the London Journal.)*

OBSERVATIONS ON THE EXPERIMENTS OF MESSRS. FOUCAULT AND FIZEAU, RELATIVE TO THE ACTION OF THE RED RAYS UPON DAGUERRETYPE PLATES. BY M. EDMOND BECQUEREL.

Messrs. Foucault and Fizeau, at the sitting of the Academy on the 5th inst. (October 1846), presented some observations concerning the action of the red rays upon Daguerreotype plates, from which it would appear that the least refrangible part of the solar spectrum acts upon the iodide of silver in an inverse manner to the most refrangible portion. As it appears that these gentlemen are not acquainted with the experiments which have been made during the last few years on this subject, and as the results stated by them do not appear to me to lead to the conclusions which they deduce, I take the liberty of submitting some remarks thereon to the Academy.

Dr. Draper (*Philosophical Magazine*, Nov. 1842), on examining the image produced by the action of the spectrum upon iodized plates of silver, made known, before these gentlemen, the existence of protecting rays modifying the influence of the solar rays, and even acting negatively upon the iodide of silver. Sir J. F. W. Herschel examined the pictures on that occasion, and in a very interesting experiment

(Philosophical Magazine, Feb. 1843) on the different appearances which the iodized plates assume when exposed for the same space of time to varying intensity in the light, and submitted to the vapour of mercury, attributed these effects to the unequal thickness of the substances deposited upon the plates of silver, serving as reflecting surfaces. He showed, moreover, that on operating upon paper covered with iodide of silver, nothing was observed which indicated the action of negative rays, but that all the active parts of the solar spectrum acted chemically in the same manner on the iodide.

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The experiments which I have made on the chemical action of the solar rays, from 1841 to 1844, have all led me to the same conclusion. Attention ought not therefore to be directed to the deposits formed on the surface of Daguerreotype plates, as if these were the only data to lead to the conclusion that the rays acted in various ways; for if so, there would be risk of defective results.

In support of the foregoing I will cite the following experiment, of the accuracy of which any person will be able to judge. "Let a Daguerreotype plate be prepared with iodine only (in order to avoid the admixture of active substances), and let the blue, indigo, and violet parts of a purified blue spectrum, presenting Fraunhofer's black lines, be thrown upon it. If the action only lasts a short time, after submitting it to the mercury vapour, the black lines will be seen to fix themselves upon a white ground, which represents those parts affected by the active parts of the spectrum*." But if the plate be exposed to the spectrum for an hour or more, then the appearance of the plate changes, on passing it through the mercury vapour; the lines of the spectrum are scarcely marked, and the action has been nearly uniform throughout its surface, but the lines which are visible appear white, and show very distinctly upon the ground, which approaches to blackness: the effect is quite contrary to what it was before. That portion of the plate which is acted upon by the violet part of the spectrum has, under these circumstances, the same appearance as the portion of the plate exposed to the red rays by Messrs. Foucault and Fizeau; and to produce this effect, it was only necessary to vary the time of exposure of the plate to the same portion of the spectrum. Must it be inferred, in the second case, that the iodide of silver had been acted upon by negative rays? Certainly not; for if the experiment be repeated upon iodide of silver laid upon paper, the paper will become darker and darker, in proportion to the time it continues exposed to the spectrum: and, besides, I have found (*Annales de Chimie et de Physique*, 3rd series, vol. ix. p. 268 *et seq.*) that the electrical effects arising from the chemical decomposition of the iodide always act in the same direction.

It will be seen that the conclusion to which Messrs. Foucault and Fizeau came, viz. that there exist in the red prismatic rays negative rays, cannot be received, simply from the fact that the Daguerreotype plates are not always the same in appearance. The contrary effects, as will be seen hereafter, are secondary effects produced by

* E. Becquerel on the Constitution of the Solar Spectrum.—*Bibliothèque Universelle de Genève*, August 1842.

several chemical reactions taking place simultaneously, and are not due to contrary action, exercised on the part of the solar rays, on the iodide of silver alone.

Another fact which I will call attention to is, that the least refrangible part of the spectrum, instead of possessing a negative action upon the iodide of silver, exercises a continuous influence upon most of the salts of silver alone, such as the iodide, the bromide, and the chloride; and also that the experiments upon which this proposition is founded have been verified by the commissioners of the Academy, charged with the examination of one of my memoirs.

In the foregoing, the only point discussed was the 'influence of light upon iodide of silver, or the simple salts of that metal. When plates of silver are exposed successively to the vapours of iodine, bromine or chlorine, the mixtures obtained may give rise to various kinds of chemical reaction, of which the result only is appreciable. For this reason these mixtures must not be employed without great caution, and the Daguerreotype plates must be used as little as possible in experimenting as to the nature of the active rays.

In order to show how far the mixture of sensitive materials is capable of influencing the effects of the spectrum, I will direct attention to the following observation of Sir J. Herschel:—

If paper be prepared, first with a strong solution of lead and afterwards with bromate of potash and nitrate of silver, a surface will be produced which will speedily become black on exposure to the light; on being presented to the spectrum, the black tint will be produced in the most refrangible rays, as far as green. But if the paper has been blackened by previous exposure to the light, on being covered with a dilute solution of iodide of potassium and exposed to the blue part of the spectrum, the paper will become white. This result proves that the iodide of potassium is decomposed, and that the silver which stained the paper being iodized, and coming in contact with an alkaline iodide, ceases to be affected by the light; the paper will therefore remain of a yellowish-white in that portion of the spectrum on which the reaction takes place.

If the layer of iodide of potassium employed is produced from a dilute solution of that salt, the paper not only becomes white in the violet part of the spectrum, but also becomes darker in the red rays, and even beyond, a neutral line being in the middle. It would appear therefore, from an examination of the image thus obtained, that two contrary results were produced; viz. the destruction of the colour in the violet part and its augmentation in the red. These effects may be easily explained as two distinct chemical reactions: first, the action of the light upon the iodide of silver, the colouring of which had commenced; second, the action of the light to effect the decomposition of the iodide of potassium, and the iodizing of the silver arising from the sub-iodide formed by the first reaction. As the red part of the spectrum contains those rays which continue the chemical action commenced upon the salts of silver, and as the first reaction is only commenced, the latter has most influence in that part of the spectrum. The second reaction is, on the contrary,

at its height in the violet. Thus these appearances of inverse action in the colouring do not arise from two distinct effects, *positive* and *negative*, produced by the rays upon the same sensitive surface, but are owing to two distinct chemical reactions, which predominate respectively in the red and violet parts.

If the blackened paper be covered with a fresh layer of iodide of potassium, it will begin to turn white at the least refrangible part, and the neutral line will again approach the red; if a sufficient quantity of iodide be used, the paper will turn white from the violet to the red; but if a very strong solution of iodide were employed, the paper would whiten, even in the dark; so violent is the action of the iodide of potassium upon metallic silver.

These results clearly prove that several chemical actions may take place simultaneously in the mixtures of sensitive substances, the results only of which are observable. Analogous effects must necessarily be produced on employing iodized plates of silver, and afterwards exposing them to the vapour of bromine or to chlorine; and perhaps even when using plates iodized according to M. Daguerre's plan. In fact, under these circumstances, the iodide, chloride, or bromide of silver, are in direct contact with the metallic silver; and as, by the decomposition of these salts, through the action of the light, subsalts are formed, the result is that iodine, chlorine and bromine are exposed directly to the above-named salts, and even to the metallic silver itself, at the moment when the solar action makes its influence felt. These reactions, which are sufficiently complex, become more so by the iodides, chlorides, and bromides of silver being submitted to the action of rays which always act with the same energy in the violet part of the spectrum; whilst in the red portion the rays react with greater energy, owing to certain chemical actions having commenced.

It is therefore essential to distinguish between the chemical reactions effected under the influence of light upon sensitive substances alone, and upon combinations of them. This has not been done by Messrs. Foucault and Fizeau: they have considered a Daguerreotype plate as offering a *separate sensitive surface*, whilst it is only by a mixture of substances that different effects can be produced in the various parts of the spectrum, as is proved by Herschel's experiment, and without the existence of rays acting in an inverse direction. Thus it has been proved by experiment, that the solar rays, although of various degrees of refrangibility, only act in one way upon iodide of silver; whilst a mixture of this substance with other matters may occasion several chemical reactions acting conjointly and hiding the principal effect.

If the light acts only in one way upon iodide of silver (the continuous rays included), the case may be different on other sensitive substances, and the rays may act sometimes positively and sometimes negatively. It is known, in fact, that each sensitive substance is differently affected by the solar rays; this might be explained by stating that each sensitive substance receives the rays in a manner peculiar to itself. I will cite as an example an observation of Wol-

laston's, with regard to the sensitive substance *guaiacum*, which becomes blue beyond the violet part of the spectrum, and again becomes colourless in the red and yellow rays.

I have confined myself to the foregoing observations, on the subject of Messrs. Foucault and Fizeau's note, to show that the complicated phenomena produced by the appearance of the Daguerreotype plates (although very important for the photographic images produced in the camera-obscura, and for the observation of active rays of very little intensity) cannot lead to definite and unvarying results, as regards the nature of the chemical action produced. It is therefore necessary to operate, as has been heretofore done, by means of simple products, with sensitive paper, regard being had to the electrical effect due to the chemical reaction produced under the influence of the solar rays.—*Ibid.*

RESEARCHES ON MELLON AND ITS COMPOUNDS.

BY MM. AUG. LAURENT AND CH. GERHARDT.

The authors observe that in their last memoir on this subject they had not repeated the experiments of M. Liebig; that they had merely modified a part of his theory, the authority of his name appearing to be a sufficient guarantee for the accuracy of his results.

According to M. Liebig, this substance is composed of—

Six atoms of carbon	=458·61	39·36
Eight atoms of azote	=708·16	60·64
One atom of mellon.	=1166·77	100·00

According to him, it is a radicle composed of carbon and azote, capable of uniting with hydrogen, oxygen and metals, so as to form compounds analogous to those into the composition of which cyanogen enters.

The authors state, and they do it with regret, that the reliance which they placed on the results of M. Liebig has totally misled them; and that it is not a part only of his labours on this subject which is erroneous, but that this is the case with the entire history of mellon, with all its transformations and all its reactions. The authors assert that they are also in a condition to prove that the supposed analogy with cyanogen does not exist; for instead of consisting only of carbon and azote, mellon contains besides one and a half per cent. of hydrogen. Various processes have been employed for obtaining mellon. That analysed by MM. Laurent and Gerhardt was obtained very pure by calcining chlorinated cyanamide (*cyanamide chlorée*) as long as hydrochloric acid and hydrochlorate of ammonia were disengaged.

The following are the results of four experiments performed to ascertain the composition of mellon:—

	I.	II.	III.	IV.
Carbon	36·0	35·8	36·4	35·73
Hydrogen	1·7	1·8	1·7	1·77
Azote	61·3	62·4	61·9	62·50
	99·0	100·0	100·0	100·00

The authors state that theory would give—

C ⁶	450·00	36·0
H ³	18·75	1·5
N ⁹	787·50	62·5
	1256·25	100·0

These numbers are confirmed by the results obtained by M. Voelckel in analysing glaucene, which, according to MM. Laurent and Gerhardt, is merely mellon derived from the dry distillation of poliene (A.), and of persulphohydrocyanic acid (B.). The results of M. Voelckel are as follows :—

	A.		B.
Carbon	35·4	35·7	36·2
Hydrogen	1·6	1·6	1·8
Azote	63·0	62·7	62·0
	100·0	100·0	100·0

From numerous experiments, MM. Laurent and Gerhardt have arrived at the following conclusions :—

1. Mellon is not, as supposed by M. Liebig, a binary compound, comparable with cyanogen ; for besides carbon and azote, which are not in the proportions stated by M. Liebig, it contains one and a half per cent. of hydrogen, and is represented by C⁶ H³ N⁹.

2. The formation of mellon by chlorinated cyanamide does not occur, as stated by M. Liebig, accompanied simply with hydrochlorate of ammonia, but there is also disengaged an enormous quantity of hydrochloric acid, amounting to about 16 per cent.

3. Chlorinated cyanamide contains double the quantity of chlorine stated by M. Liebig, and ought to be represented by C³ H⁴ Cl N⁵.

4. By the influence of potash, chlorinated cyanamide is converted into ammeline C³ H⁵ N⁵ O, or ammelide C³ H⁴ N⁴ O².

5. The new substance, the formation of which has been recently announced by M. Liebig from urea, is a body previously known by the name of ammelide.

6. Ammelide does not possess the composition attributed to it by M. Liebig ; it contains C³ H⁴ N⁴ O², as had been previously stated by one of the authors.

7. The melam of M. Liebig is an impure mixture of poliene C³ H⁶ N⁶ and mellon.

8. The dry distillation of persulphohydrocyanic acid, as stated by M. Liebig, does not occur ; but the assertions of M. Voelckel respecting this phenomenon are correct.

9. The sulphuret of cyanogen does not possess the composition adopted by M. Liebig : it contains hydrogen, and is represented by C³ HN³ S³ ; it cannot therefore be the radical of the sulphocyanurets.

10. Heat does not decompose mellon, as stated by M. Liebig, into three volumes of cyanogen and one volume of azote ; but the products vary according to the temperature, and always contain hydrogenated compounds, to the complete destruction of the mellon.

11. Potash does not dissolve mellon and yield the mellonide of M. Gmelin, as stated by M. Liebig; but it produces a tribasic salt $C^6(HM^3)N^8O^2$, containing hydrogen and oxygen, at the same time that ammonia is developed. Other products are also formed at the same time.

12. Mellon does not simply and purely combine with potassium; but the combination occurs with the evolution of ammonia, and the product is a bibasic salt $C^6N^8M^2$, a salt which on dissolving in water probably gives $C^6N^8M^2H^2O$, and appears to be identical with the mellonides of M. Gmelin.

13. Mellon does not integrally dissolve in sulphuric acid, for the liquid contains sulphate of ammonia.—*Ann. de Ch. et de Phys.*, Janvier 1847.

ON THE PREPARATION, PROPERTIES AND COMPOSITION OF
ACETAL. BY M. STAS.

The author states that by following precisely the directions of M. Liebig for preparing acetal, he obtained a product which, when redistilled, did not possess any fixed boiling-point. The liquid procured at 203° F. recommenced boiling at about 190°, and the temperature gradually increasing, finished at 226° or even 230°.

The product distilled at 190° was reduced to about half by a concentrated solution of potash; this action takes place in a few hours without the contact of air. The portion collected at about 220°, and below it, does not undergo any appreciable alteration by the alkaline solution without the contact of the air.

The potash solution, obtained by treating the first product, remains colourless; when saturated with carbonic acid and evaporated *in vacuo*, a residue is left, which yields to anhydrous alcohol a white salt, which is very soluble in water, and precipitates nitrate of silver in pearly white laminæ; it is merely acetate of potash without any trace of formiate.

These facts prove, according to M. Stas, that the acetal described by M. Liebig is a mixture of at least two substances, one of which is acetic æther, and the other is acetal, properly so called, and comparable to the methylal of M. Malaguti, as will hereafter be shown.

M. Stas observes that some chemists still doubting of the existence of acetal, he states the method of procuring it in a certain and rapid manner, and constant in composition, before he describes its properties; the process is as follows:—

Fragments of pumice-stone are to be washed with hydrochloric acid and then heated to redness; afterwards the pumice is to be moistened with alcohol which is nearly anhydrous, and to be introduced into a balloon capable of containing 80 to 100 pints. This balloon should have a short neck, but large enough to allow of introducing the arm. As many capsules as the balloon will contain are to be placed on the pumice; these capsules are to be as flat as possible, and covered with a thin stratum of platina black. The neck is to be perfectly covered with a glass plate, and the balloon is

then to be exposed to a temperature of about 68° , till nearly all the alcohol is converted into acetic acid.

Fresh air is to be occasionally admitted into the balloon, and care taken that the liquid is always below the pumice, and that the latter is never covered by the former. In about a fortnight or three weeks the liquid becomes more or less viscid, its consistence resembling that of sulphuric acid on being poured. At this period the liquid is to be removed, and replaced by an equal quantity of alcohol of 60 per cent. When several pints of very acid liquid have been thus obtained, it is to be neutralized by carbonate of potash, and as much chloride of calcium dissolved in it as it is capable of taking up. Dried acetate of potash may be substituted for chloride of calcium; in this case it is unnecessary to saturate the liquid by the carbonate of the same base.

In whatever mode the liquor is saturated, it is to be submitted to cautious distillation; only one-fourth of it is to be distilled, and in a well-cooled receiver; this is to be saturated with fused chloride of calcium, which immediately separates a considerable portion of a very volatile fluid of an extremely suffocating odour; this is to be separated by a pipette, and water is to be cautiously added to the saline solution as long as it continues to separate fresh quantities of æthereal fluid, which is to be added to that previously obtained. By careful distillation of the solution of chloride of calcium, a still further small portion of the organic matter may be extracted: as already remarked by M. Liebig, the substance thus separated is a mixture of aldehyd, acetic æther, alcohol and acetal.

In order to isolate the acetal, powdered chloride of calcium is to be added to the mixed fluids, as long as it continues to dissolve; when this is completed, the liquid is to be distilled from a water-bath, but always below its boiling-point, until the product ceases to reduce ammoniacal acetate of silver. The residual matter, deprived of aldehyd, but still containing some acetic æther and alcohol mixed with the acetal, is added to a great excess of very concentrated solution of potash, which completely destroys the acetic æther: in operating upon about 750 grains of the fluid, and very frequently agitating the mixture, it requires three or four days' action to decompose the last portion of the æther. It now remains only to wash the acetal with once or twice its volume of water, to digest it with recently-fused chloride of calcium, and to distil it, in order to have it perfectly pure.

The properties of acetal are, that it is a colourless æthereal and fluid liquid, but less so than æther, to which it has been compared. It has a peculiar sweet smell; its taste is cooling with a distinct nutty after-taste. Its density is 0.821, at about 72° F. It boils between 219° and 223° F. Water at 77° dissolves about $\frac{1}{18}$ th of its volume, and less as its temperature is increased. Chloride of calcium and all very soluble salts in general separate the acetal from solution in water; æther and alcohol dissolve acetal in all proportions; chloride of calcium does not separate it from alcohol unless water be added; aldehyd, as observed by M. Liebig, exhibits the same phænomenon.

Under the influence of platina-black or of the air, acetal is very rapidly converted, first into aldehyd and then into concentrated acetic acid; the action is extremely rapid when the platina is moistened; oxidizing bodies in general produce the same effect; thus diluted nitric acid furnishes aldehyd at first and afterwards acetic acid; chromic acid gives acetic acid only. A solution of ammoniacal acetate of silver has no action upon it at any temperature; out of the contact of air neither saturated solutions of potash, soda, the solid alkalis, nor potashed lime, have any action upon it, either at a low or high temperature; sulphuric acid at first dissolves it, then decomposes and blackens it; chlorine acts upon it, removes its hydrogen and forms chlorinated compounds, which the author has not had time yet to examine. By analysis it yielded—

C ⁶	450·0	61·01
H ⁷	87·5	11·85
O ²	200·0	27·14
	<u>737·5</u>	<u>100·00</u>

According to Liebig it consists of—

C ⁸	611·480	59·72
H ¹⁸	112·315	10·97
O ³	300·000	29·31
	<u>1023·795</u>	<u>100·00</u>

M. Stas observes, that on comparing the results of his analyses with the composition of alcohol, the latter, under the influence of the oxygen of the air and platina-black, has lost one-third of its oxygen, and two-ninths of its hydrogen; these quantities being equivalent to one of oxygen and two of hydrogen, as is readily ascertained. Acetal may be represented by a combination of two molecules of æther with one molecule of aldehyd, according to M. Stas, C¹² H¹⁴ O⁴ = 2(C³ H⁵ O), C³ H⁴ O²; while according to Liebig its formula is C⁴ H⁶ O, Ae O + H² O = C⁸ H¹⁸ O³, or one equiv. of aldehyd and one equiv. of æther.—*Ann. de Ch. et de Phys.*, Feb. 1847.

PRESENCE OF SULPHUR IN METALLIC SUBSTANCES STRUCK BY LIGHTNING.

On Sunday, the 14th of June 1846, the parish church of Saint Thibaud-de-Couz, three leagues from Chamberry, was struck by lightning; the church was filled with a dense smoke, accompanied by a strong smell resembling that of gunpowder. The gilt frame of a large picture was almost entirely blackened, and six gilt chandeliers were all rendered as black as copper would be after long exposure to sulphuretted hydrogen.

M. Bonjean procured some powder by scraping the surface of the chandeliers which had been most strongly coloured; by treating it with aqua regia he obtained a solution in which solution of nitrate of barytes gave a white precipitate insoluble in nitric acid.—*Journ. de Pharm. et de Ch.*, Decembre 1846.

ON CHROMATE OF CHROMIUM.

M. C. Rammelsberg states that when a solution of chrome-alum is mixed with one of neutral chromate of potash, the first portions produce a red brown colour; afterwards a brown precipitate is formed, on the surface of which a very yellow liquid floats.

This precipitate dissolves in hydrochloric acid with a yellowish-green colour; ammonia precipitates oxide of chromium, leaving chromic acid in solution. When digested with potash, it readily yields chromate of potash and oxide of chromium.

M. Rammelsberg gives the annexed formula [$3\text{Cr}^+\text{O}^3$, $2\text{Cr}^2\text{O}^3$, $9\text{H}^2\text{O}$].—*Journ. de Pharm. et de Ch.*, Novembre 1846.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1847.

Chiswick.—January 1. Frosty: overcast. 2. Hazy and cold. 3. Dry haze: snow at night. 4. Cloudy. 5. Hazy. 6, 7. Foggy. 8. Hazy. 9. Cold: hazy. 10. Sharp frost at night. 11. Frosty: fine: sharp frost. 12, 13. Foggy. 14. Sharp frost: foggy: frosty. 15, 16. Dense fog: frosty at night. 17. Dry haze: foggy. 18. Cloudy and cold. 19. Overcast: hazy. 20. Slight haze: snow at night. 21. Snowing. 22. Dense fog throughout. 23. Slight fog: cloudy: rain. 24. Densely clouded: rain: clear. 25. Fine: slight showers in the evening. 26. Partially overcast: fine: boisterous at night. 27. Clear: rain at night. 28. Boisterous: clear and frosty. 29. Fine, with sun: clear and frosty. 30. Clear and frosty: fine: densely overcast. 31. Light haze: cloudy: fine.

Mean temperature of the month 34°·26

Mean temperature of Jan. 1846 43°·54

Mean temperature of Jan. for the last twenty years ... 36°·81

Average amount of rain in Jan. 1·60 inch.

Boston.—Jan. 1. Foggy: snow on the ground. 2, 3. Cloudy. 4. Cloudy: snow early A.M. 5. Cloudy: rain early A.M. 6. Cloudy. 7. Rain. 8—11. Cloudy. 12. Cloudy: rain P.M. 13. Cloudy. 14—16. Fine. 17—20. Cloudy. 21. Cloudy: snow P.M. 22. Cloudy: snow on the ground. 23. Cloudy. 24. Cloudy: snow nearly all melted. 25. Fine: rain midday. 26. Fine. 27. Windy. 28. Rain. 29—31. Fine.—This January has been the coldest since January 1842, and the driest since 1833.

Sandwich Manse, Orkney.—Jan. 1. Fog. 2. Cloudy. 3. Damp: showers. 4. Showers. 5. Showers: rain. 6, 7. Damp. 8. Cloudy. 9. Bright: clear. 10—12. Bright: frost: clear. 13. Bright: clear. 14. Bright: frost: clear. 15. Bright: clear. 16. Drops: cloudy. 17—19. Cloudy: clear: aurora. 20. Fine: frost: clear: frost: aurora. 21. Sleet-showers. 22. Bright: clear. 23, 24. Cloudy: rain. 25—27. Bright: clear. 28. Clear: frost: cloudy. 29. Bright: clear. 30. Bright: snow-showers: aurora. 31. Hail-showers: snow-showers: aurora.

Applegarth Manse, Dumfries-shire.—Jan. 1. Clear, but moist. 2. Dull and raw: slight snow. 3. Slight frost: very chilly. 4. Dull: slight frost: rain P.M. 5. Dull: rain. 6. Fine and fair. 7, 8. Fair, but cloudy. 9, 10. Frost: clear. 11—13. Frost, hard. 14. Frost, hard, but cloudy. 15. Thaw. 16. Thaw: drizzle. 17—21. Frost again. 22. Frost: snow: thaw. 23. Thaw: sleet. 24. Heavy rain. 25. Slight frost A.M.: rain. 26. Heavy rain. 27. Rain: cleared P.M. 28. Fair, but dull. 29. Slight frost A.M. 30. Slight frost A.M.: cloudy. 31. Hard frost: snow P.M.

Mean temperature of the month 35°·97

Mean temperature of Jan. 1846 43°·0

Mean temperature of Jan. for 25 years 34°·9

Mean rain in Jan. for 20 years 2·60 inches.

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APRIL 1847.

XXXVIII. *On the Effects of Magnetism, &c. upon the Dimensions of Iron and Steel Bars.* By J. P. JOULE, Esq., Secretary of the Literary and Philosophical Society of Manchester.

[Continued from page 87.]

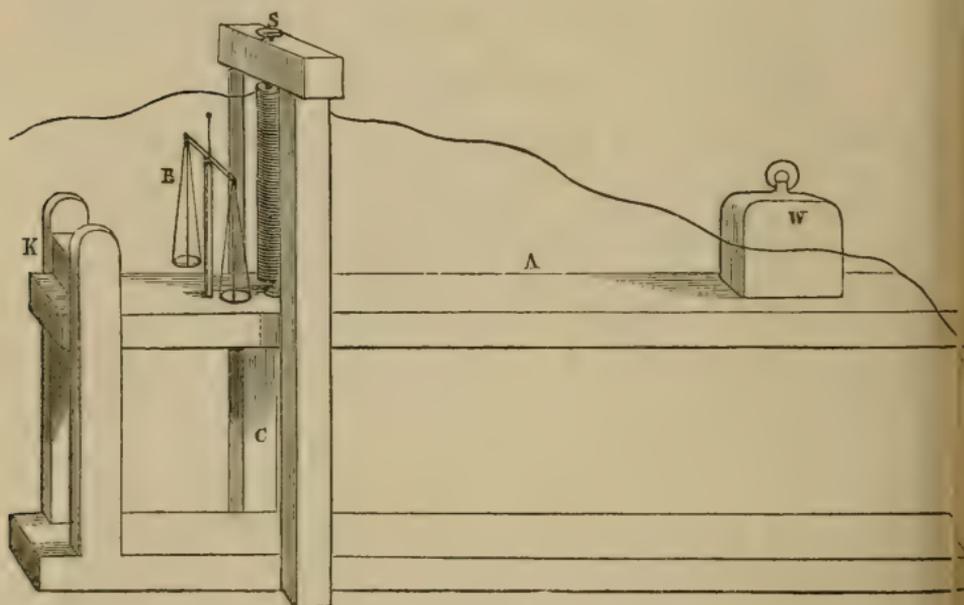
WITH a view to ascertain whether the lengthening effects observed in the experiments detailed in the former part of this paper were entirely independent of the diameter of the bars, I made a very extensive series of experiments, in which fine wires both of iron and steel, bundles of very fine iron wires, chains composed of copper and iron links, &c. were employed. In order to keep these flexible articles exactly in the axis of the coil, a weight was placed upon one of the levers, so as to exert upon them a force of tension equal to about eight ounces.

The results of the experiments in which wire of $\frac{1}{20}$ th of an inch thickness was employed, accorded very well with the previous experiments with thicker bars; but on employing iron wire which was only $\frac{1}{120}$ th of an inch thick, the phenomena assumed quite a different character; for on transmitting the current through the coil, the length of the wire became suddenly *diminished* instead of being increased. This phenomenon appeared to me exceedingly anomalous, and it was some time before I found out its cause. At last, thinking that the wire was attracted by the coil, I varied its position from the centre to either side, and increased the amount of its tension. The former of these operations produced no sensible effect; but the increase of tension caused the shortening effect of the current to be considerably augmented. It became manifest, therefore, that the weight of eight ounces, acting upon very fine wire, produced the anomalies in question. This was further demonstrated to be the case by diminishing

the tension as far as possible. I then found that the phenomena of elongation took place as in the case of the iron bars, only to a smaller extent, which was obviously owing to the degree of tension necessarily left in order to keep the wire in the axis of the coil.

The new field of inquiry thus opened appeared to me to be one of great importance, and calculated eventually to become the means of throwing a great deal of light upon some of the most interesting questions connected with molecular actions and the real character of magnetism. I therefore constructed an apparatus whereby the effects of tension and pressure might be further studied. This apparatus, which is represented by the adjoining woodcut, consisted of a strong wooden lever A,

Fig. 1.



furnished with a hard steel knife edge at K resting on a hard steel plate. At the distance of one foot from the knife edge a brass plate was fastened to the lever, into which a piece of iron or steel wire, one-fourth of an inch in diameter, could be screwed: the upper end of the wire could also be screwed into a brass bolt, the head of which rested upon the cross piece S. Weights could be placed on the lever at W to increase the tension. The magnetic balance, consisting of a bar-magnet eight inches long, properly furnished with scales, was situated at B. The further extremity of the lever was connected with a fine lever multiplying eight times, the index of

which was examined by the microscope employed in the former series of experiments. Each division of the micrometer passed over by the index indicated an elongation of the wire under examination equal to $\frac{1}{130900}$ th of an inch.

When pressure instead of tension was employed, a pillar of iron or steel wire, one quarter of an inch in diameter, was placed at C so as to support the weight of the lever; its ends abutted upon flat plates of copper or brass.

Every precaution that I could think of was taken in order to give accuracy to the results. In particular I may mention that the coil was not permitted to touch either the wire under examination, the lever, or any other part of the apparatus to which it might communicate motion through the change of its own molecular condition in consequence of the passage of the electrical current. In spite of these precautions the experiments were very troublesome, owing to the almost incessant vibrations of the index. Although my laboratory is situated quite out of the town, and detached from every dwelling, these vibrations were so extensive during the day time, that the experiments had, in general, to be carried on after eight o'clock P.M., when the greater part of the traffic had ceased. It was at all times impossible to make an observation when a cart was passing along a road at the distance of one or two hundred yards; nor could anything be done as long as much wind was blowing. Owing to these circumstances it was frequently very difficult to estimate an effect equal to one-tenth of a division of the micrometer. I believe, however, that the results of the tables are in no case more than two-tenths of a division wide of the truth.

The experiment which I first made was with a piece of soft iron wire, one foot long and one quarter of an inch in diameter. It was made as straight as possible, and its ends were ground perfectly true and flat. It was placed as a pillar under the lever, so as to support its pressure, which was equal to 82 lbs. The coil by which it was magnetized was formed out of a copper wire thirty-three yards long and one-tenth of an inch in diameter, well-covered with cotton-thread. Its length was $11\frac{1}{2}$ inches, and its interior diameter one inch. The same coil was employed in all the experiments with wires of a foot long. The temperature of the wire was in every case about 45° .

The method of experimenting was the same as I employed in the former section of this paper. A current was passed through the coil, the effects of which, both on the length of the iron pillar and on its magnetism, were noted. The current was then cut off; and the effect of so doing on the length of the pillar noted, as well as the quantity of magnetism remain-

ing in it. A more powerful current was then passed, and the observations repeated as before; and so on with still more powerful currents. The fifth column of the table gives the effect upon the magnetic balance in grains, abstraction being made of the effect due to the coil itself, which had been previously carefully determined by experiment.

Experiment 9. Pressure 82 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6° 5'	106	0	0	+0.6	
0	0	0	0	+0.3	
+11 0	194	0.1 E.	0.1	+1.4	1960
0	0	0.1 S.	0	+0.4	
+21 0	383	1.0 E.	1.0	+2.8	784
0	0	1.0 S.	0	+0.5	
+35 5	702	3.3 E.	3.3	+4.2	534
0	0	3.2 S.	0.1	+0.6	
+47 0	1072	5.4 E.	5.5	+5.0	455
0	0	5.1 S.	0.4	+0.7	
+53 45	1364	6.4 E.	6.8	+5.4	429
0	0	6.0 S.	0.8	+0.7	
- 6 45	118	0	0	-0.3	
0	0	0	0	+0.1	
-11 18	200	0.2 E.	0.2	-1.2	720
0	0	0.2 S.	0	-0.3	
-21 25	392	1.0 E.	1.0	-2.6	676
0	0	1.0 S.	0	-0.4	
-35 33	715	3.5 E.	3.5	-4.1	480
0	0	3.2 S.	0.3	-0.4	
-45 40	1023	4.9 E.	5.2	-4.9	461
0	0	5.0 S.	0.2	-0.5	
-54 5	1380	6.3 E.	6.5	-5.4	449
0	0	6.3 S.	0.2	-0.5	

In the following experiment the same bar was subjected to a pressure of 480 lbs. It possessed -0.5 of permanent magnetism to begin with.

Experiment 10. Pressure 480 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6 ⁰ / ₁₀	108	0.15 S.	0	+0.6	
0	0	0	0	+0.2	
+10 45	190	0.2 E.	0.2	+1.2	720
0	0	0.2 S.	0	+0.5	
+20 0	364	0.8 E.	0.8	+2.3	661
0	0	0.8 S.	0	+0.7	
+32 53	646	2.5 E.	2.5	+3.6	518
0	0	2.5 S.	0	+0.8	
+42 35	919	4.6 E.	4.6	+4.5	440
0	0	4.6 S.	0	+0.8	
+49 55	1188	6.8 E.	6.8	+5.2	398
0	0	6.8 S.	0	+0.8	

The numbers in the last columns of the preceding tables show that the elongation follows a rather higher ratio than the square of the magnetic polarity. In the former section, in which all the bars employed were a yard long, the ratio was somewhat lower than that of the square of the polarity in the case of well-annealed iron. I am inclined therefore to think that the anomalies referred to at p. 83, were occasioned rather by the too great length of the iron bars, which prevented them from being magnetized as much at the ends as at the middle part, than by their different magnetic conditions at the centre and surface.

From the above tables it appears evident that the augmentation of *pressure* does not make much difference in the amount of elongation for the same quantity of polarity. However, I thought it desirable to try the effect of a greater pressure. For this purpose I employed a piece of soft iron wire, six inches long and one-fourth of an inch in diameter. This iron pillar stood upon a small piece of flattened brass, resting upon a block of hard wood six inches high, in order that it might be sufficiently elevated to support the lever. It was placed in the axis of a suitable coil, $5\frac{1}{2}$ inches long and one inch in interior diameter, constructed of a covered copper wire twenty yards long and one-tenth of an inch in diameter.

Experiment 11. Six-inch Pillar. Pressure 82 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6° 30'	114	0	0	+0·8	
0	0	0	0	+0·2	
+11 15	199	0	0	+1·3	
0	0	0	0	+0·4	
+23 15	430	0	0	+2·9	
0	0	0	0	+0·5	
+41 55	898	0·6 E.	0·6	+5·5	504
0	0	0·6 S.	0	+0·5	
+51 50	1272	1·5 E.	1·5	+7·2	346
0	0	1·5 S.	0	+0·5	
+62 20	1907	2·6 E.	2·6	+9·4	340
0	0	2·6 S.	0	+0·5	

Experiment 12. Six-inch Pillar. Pressure 1380 lbs. Permanent magnetism of the pillar to begin with -0·6.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+ 6° 40'	117	0	0	+0·1	
0	0	0	0	-0·3	
+11 30	203	0	0	+0·6	
0	0	0	0	-0·1	
+23 25	433	0	0	+2·1	
0	0	0	0	+0·3	
+42 35	919	0·4 E.	0·4	+4·5	506
0	0	0·4 S.	0	+0·5	
+51 30	1257	0·9 E.	0·9	+5·9	387
0	0	0·9 S.	0	+0·7	
+63 30	2005	2·1 E.	2·1	+8·6	252
0	0	2·1 S.	0	+0·7	

A comparison of the last columns of the two preceding tables will show that pressure has no sensible effect upon the extent of the elongation. I had not sufficient voltaic force to saturate the short bars, but there appears no reason to doubt, that their elongation, when saturated, would be one-half that of the pillars one foot long, just as the latter were found to experience one-third of the elongation observed in the bars of a yard long employed in the first section. I may remark in

this place, that the greater proximity of the magnetic balance to the coil increased the numbers in columns 5, representing the magnetic intensity of the six-inch pillars. The two preceding tables are therefore only comparable with themselves. In all the other experiments with one-foot bars, the magnetic balance was at the uniform distance of $4\frac{1}{2}$ inches from the centre of the bar, so that they are strictly comparable with one another.

I now proceed to give an account of some experiments on the effects of the force of tension. The bar employed was a piece of soft iron wire $12\frac{2}{3}$ inches long and a quarter of an inch in diameter. Its extremities were formed into very fine-threaded screws, extending one-third of an inch, for the purpose of screwing into the brass plate and bolt, as shown in the figure already described. The effectual length of the bar, when screwed into its place, was exactly one foot. In the first experiment of this kind, the tension employed, being that occasioned by the weight of the lever alone, amounted to 80 lbs. In the subsequent ones the tension was further increased by placing weights on the lever.

Experiment 13. Iron wire one foot long and a quarter of an inch in diameter. Tension 80 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+ 6 30 0	114 0	0·1 E. 0·1 S.	0·1 0	+0·6 +0·2	360
+11 30 0	203 0	0·4 E. 0·4 S.	0·4 0	+1·5 +0·3	562
+21 25 0	392 0	1·0 E. 1·0 S.	1·0 0	+3·0 +0·4	900
+35 55 0	724 0	3·2 E. 3·2 S.	3·2 0	+4·5 +0·4	632
+46 38 0	1058 0	4·0 E. 4·0 S.	4·0 0	+5·1 +0·4	650
+53 10 0	1335 0	4·6 E. 4·6 S.	4·6 0	+5·4 +0·4	634
+61 25 0	1835 0	4·5 E. 4·5 S.	4·5 0	+5·6 +0·4	697

Experiment 14. Same wire with -0.4 permanent magnetism to begin with. Tension 408 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+ 6 30 0	114 0	0 0	0 0	+0.6 +0.2	
+11 10 0	197 0	0 0	0 0	+1.4 +0.3	
+20 55 0	382 0	0.2 E. 0.2 S.	0.2 0	+3.1 +0.4	4805
+35 5 0	702 0	0.6 E. 0.6 S.	0.6 0	+4.7 +0.4	3682
+45 20 0	1012 0	0.8 E. 0.8 S.	0.8 0	+5.1 +0.4	3251

Experiment 15. Same wire. Tension 740 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
+ 6 20 0	111 0	0 0	0 0	+0.9 +0.3	
+20 0 0	364 0	0.1 S. 0.1 E.	0.1 0	+3.4 +0.5	1237
+35 0 0	700 0	0.4 S. 0.4 E.	0.4 0	+4.9 +0.6	858
+45 30 0	1017 0	0.6 S. 0.6 E.	0.6 0	+5.4 +0.6	915
+58 0 0	1600 0	1.3 S. 1.3 E.	1.3 0	+5.7 +0.7	701

In tables 13 and 14 we notice the rapid decline of the effect of elongation, until at last, in table 15, with a tension of 740 lbs. it ceases altogether, and the new condition of shortening commences. With a tension of about 600 lbs. the effects on the dimensions of the wire would cease altogether in the limits of the electrical currents employed in the above experiments. From the last column of table 15, which consists of the columns 2 and 5 multiplied together and divided by column 4, we gather that the shortening effect is very nearly proportional to the magnetism of the wire into the current traversing the coil. The law of the square of the magnetism will still indeed hold good where the iron is sufficiently below

the point of saturation, on account of the magnetism being in that case nearly proportional to the intensity of the current. For the same reason, on examination of the previous tables, it will be found that the elongation is, below the point of saturation, very nearly proportional to the magnetism multiplied by the current. The necessity of changing the law arises from the fact that the elongation ceases to increase after the iron is fully saturated; whereas the shortening effect still continues to be augmented with the increase of the intensity of the current.

Experiment 16. Same wire. +0.5 magnetism to begin with. Tension 1040 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
- 6 20' 0	111 0	0 0	0 0	-0.4 0	
-11 5 0	196 0	0.1 S. 0.1 E.	0.1 0	-1.3 -0.2	255
-20 55 0	382 0	0.2 S. 0.2 E.	0.2 0	-2.7 -0.4	515
-34 45 0	694 0	0.8 S. 0.8 E.	0.8 0	-4.0 -0.4	347
-46 45 0	1063 0	1.5 S. 1.5 E.	1.5 0	-4.7 -0.5	333
-61 0 0	1804 0	2.8 S. 2.8 E.	2.8 0	-5.0 -0.5	322
+20 45 0	379 0	0.3 S. 0.3 E.	0.3 0	+3.0 +0.5	379
+35 10 0	704 0	0.8 S. 0.8 E.	0.8 0	+4.4 +0.5	387
+48 5 0	1114 0	1.8 S. 1.8 E.	1.8 0	+4.9 +0.5	303
+61 20 0	1829 0	2.7 S. 2.7 E.	2.7 0	+5.4 +0.5	366

Experiment 17. Same wire. Tension 1680 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
- 6 25	112	0	0	-0.8	
0	0	0	0	-0.2	
-11 10	197	0	0	-1.5	
0	0	0	0	-0.4	
-20 45	379	0.5 S.	0.5	-2.7	205
0	0	0.5 E.	0	-0.4	0
-34 50	696	1.5 S.	1.5	-3.8	176
0	0	1.5 E.	0	-0.4	0
-45 5	1003	2.4 S.	2.4	-4.3	180
0	0	2.4 E.	0	-0.4	0
-52 25	1299	3.3 S.	3.3	-4.4	173
0	0	3.3 E.	0	-0.4	0
-61 15	1823	4.5 S.	4.5	-4.7	190
0	0	4.5 E.	0	-0.4	0
+61 45	1861	4.4 S.	4.4	+5.0	211
0	0	4.4 E.	0	+0.5	0

The uniformity of the numbers contained in the last column of each of the two preceding tables affords conclusive evidence of the correctness of the law I have stated, viz. that *in the case of tension the shortening effect is proportional to the current traversing the coil multiplied by the magnetic intensity of the bar.*

In order to discover, if possible, what proportion the shortening effect bears to the force of tension, I have constructed the following table from the results observed with currents of about 700 and 1000 intensity in the preceding experiments.

Number of experiment.	Elongation or shortening.	Departure from elongation 4.4.	Tension.	Square of departure divided by tension.
9	4.4 E.	0	0	
13	3.6 E.	0.8	80	80
14	0.7 E.	3.7	408	335
15	0.5 S.	4.9	740	324
16	1.2 S.	5.6	1040	301
17	2.0 S.	6.4	1680	244

With the exception of the number 80, the results of the last column of the above table agree sufficiently well together

to render it extremely probable that the shortening effects are proportional *cæteris paribus* to the square root of the force of tension.

Experiments with Cast Iron.

The following experiments were made with a bar of cast iron one foot long and a quarter of an inch in diameter.

Experiment 18. Cast iron. Tension 80 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
- 6° 28'	113	0	0	-0.2	
0	0	0	0	-0.1	
-11 5	196	0	0	-0.4	
0	0	0	0	-0.2	
-21 18	390	0.1 E.	0.1	-0.9	810
0	0	0.1 S.	0	-0.5	
-36 0	726	0.8 E.	0.8	-2.0	500
0	0	0.5 S.	0.3	-1.3	563
-46 10	1041	1.0 E.	1.3	-2.5	481
0	0	1.0 S.	0.3	-1.5	750
-58 35	1637	1.7 E.	2.0	-3.0	450
0	0	1.7 S.	0.3	-1.7	963
+19 45	359	0.5 S.	-0.2	0	
0	0	0	-0.2	-0.4	
+59 25	1692	2.3 E.	2.1	+3.2	488
0	0	1.7 S.	0.4	+1.7	722

Experiment 19. Same bar. Tension 654 lbs. Permanent magnetism to begin with -1.7.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
+19° 15'	349	0.2 S.	0	-0.1	
0	0	0	0	-0.4	
+57 50	1590	1.4 E.	1.4	+3.0	643
0	0	1.0 S.	0.4	+1.7	722

On comparing Experiment 18 with Experiment 13, it will be observed that the elongation of the cast iron is equal, if not

superior to that of the soft iron, when magnetized to the same extent. It will also be remarked that the increase of tension does not produce half the diminution of elongation which it does in the case of the soft iron.

Experiments with Soft Steel Wire.

The following experiments with soft steel wire were made in precisely the same manner as those with soft iron wire, already described.

Experiment 20. Soft steel pillar, one foot long, a quarter of an inch in diameter. Pressure 82 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
-34 0	674	0.8 E.	0.8	-2.1	551
0	0	0.4 S.	0.4	-1.1	302
-47 0	1072	1.0 E.	1.4	-3.6	926
0	0	0.6 S.	0.8	-1.7	361
-59 30	1697	1.2 E.	2.0	-4.5	1012
0	0	0.6 S.	1.4	-2.4	411

Experiment 21. Same soft steel pillar. Pressure 480 lbs. Permanent magnetism to begin with -2.0.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of pillar.	Total elongation.	Magnetic intensity of pillar.	Square of magnetic intensity divided by total elongation.
+18 15	330	0.2 S.	0	-0.2	
0	0	0	0	-0.4	
+30 30	589	0.1 E.	0.1	+1.3	1690
0	0	0	0.1	+0.6	360
+40 0	839	0.4 E.	0.5	+2.6	1352
0	0	0.2 S.	0.3	+1.3	566
+45 0	1000	0.6 E.	0.9	+3.1	1068
0	0	0.6 S.	0.3	+1.6	853
+61 10	1816	1.8 E.	2.1	+4.1	800
0	0	1.6 S.	0.5	+2.0	800

Experiment 22. Soft steel wire, one foot long, a quarter of an inch in diameter. Tension 80 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+19° 50'	360	0	0	+1.0	
0	0	0	0	+0.5	
+34 40	691	0.4 E.	0.4	+2.6	1690
0	0	0.1 S.	0.3	+1.5	750
+46 10	1041	0.5 E.	0.8	+3.5	1531
0	0	0.2 S.	0.6	+1.9	601
+56 30	1511	0.5 E.	1.1	+4.0	1455
0	0	0.3 S.	0.8	+2.1	551
-20 50	380	0.8 S.	0	-0.2	
0	0	0	0	+0.3	
34 50	696	0.2 E.	0.2	-2.3	2645
0	0	0.1 S.	0.1	-1.1	1210
-47 45	1101	0.7 E.	0.8	-3.6	1620
0	0	0.3 S.	0.5	-2.0	800
-60 0	1732	0.7 E.	1.2	-4.4	1613
0	0	0.2 S.	1.0	-2.4	576

Experiment 23. Same soft steel wire. Tension 462 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total elongation.	Magnetic intensity of wire.	Square of magnetic intensity divided by total elongation.
+34° 55'	698	0	0	+2.6	
0	0	0.1 E.	0.1	+1.4	1960
+45 5	1003	0	0.1	+3.4	
0	0	0.2 E.	0.3	+2.0	1333
+60 15	1750	0.4 S.	-0.1	+4.2	
0	0	0.6 E.	0.5	+2.4	1152

Experiment 24. Same soft steel wire. Tension 1680 lbs. Permanent magnetism to begin with +1.8

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening effect.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
-21° 13'	388	0.2 S.	0.2	-0.4	
0	0	0.2 E.	0	+0.2	77
-35 13	706	0.8 S.	0.8	-2.2	
0	0	0.8 E.	0	-1.1	194
-41 35	985	1.3 S.	1.3	-2.9	
0	0	1.3 E.	0	-1.6	219
-58 45	1618	2.5 S.	2.5	-3.7	
0	0	2.5 E.	0	-2.2	244

From the above experiments it will be seen that the same remarks which were made with regard to the soft iron, apply also to the soft steel. The superior retentive power of the latter metal enables us however to trace the elongating effects of the permanent magnetism, which, we see, diminish with the increase of tension, until at last, as in experiment 24, they cease altogether.

Experiments with Hardened Steel Wire.

The following experiments were made with steel wire of the same kind as that employed in the previous experiments. It was however perfectly hardened, so as to resist the action of the file in every part.

Experiment 25. Hardened steel wire, one foot long, a quarter of an inch diameter. Tension 80 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
+ 6° 15' 0	109 0	0 0	0 0	0 0	
+11 0 0	194 0	0 0	0 0	+0·2 0	
+20 50 0	380 0	0 0	0 0	+0·5 +0·2	
+35 20 0	709 0	0 0	0 0	+1·1 +0·5	
+45 40 0	1023 0	0·1 S. 0·1 E.	0·1 0	+1·8 +0·9	1841
+52 0 0	1280 0	0·2 S. 0·2 E.	0·2 0	+2·3 +1·2	1472
+62 20 0	1907 0	0·4 S. 0·4 E.	0·4 0	+3·3 +1·9	1573

Experiment 26. Same hardened steel wire. Permanent magnetism to begin with $-1\cdot8$ Tension 408 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
+19° 30' 0	354 0	0 0	0 0	-1·1 -1·4	
+35 40 0	717 0	0 0	0 0	-0·2 -0·6	
+46 50 0	1066 0	0 0	0 0	+1·2 +0·2	
+60 20 0	1755 0	0·4 S. 0·4 E.	0·4 0	+2·8 +1·5	1228

Experiment 27. Same hardened steel wire. Permanent magnetism to begin with +1.3. Tension 1030 lbs.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of wire.	Total shortening.	Magnetic intensity of wire.	Current multiplied by magnetic intensity divided by shortening effect.
-21 15	389	0	0	+0.7	
0	0	0	0	+1.1	
-35 55	724	0	0	-0.1	
0	0	0	0	+0.4	
-48 33	1132	0.1 S.	0.1	-1.3	1470
0	0	0.1 E.	0	-0.5	
-52 10	1287	0.15 S.	0.15	-1.6	1373
0	0	0.15 E.	0	-0.6	
-61 10	1816	0.4 S.	0.4	-2.4	1089
0	0	0.4 E.	0	-1.2	
+35 45	720	0	0	+0.2	
0	0	0	0	-0.4	
+61 10	1816	0.4 S.	0.4	+2.7	1226
0	0	0.4 E.	0	+1.3	

From the above experiments we find that the induction of permanent magnetism produces no sensible effect on the length of a bar of perfectly hardened steel, and that the temporary shortening effect of the coil is proportional to the magnetism multiplied by the current traversing the coil. The shortening effect does not in this case sensibly increase with the increase of tension. I have made an experiment, in which a hard steel pillar was subjected to a pressure of 80 lbs., which I have not inserted, because the coil not being the same as I had hitherto employed, that experiment was not strictly comparable with the rest. Its result however showed that the hard steel pillar suffered a diminution of length equal to 0.1 of a division of the micrometer, with a current capable of giving a magnetic polarity of 1.7. This accords very well with the results obtained with a tension of 1030 lbs.

Copper is the only non-ferruginous metal which I have experimented on. In the trials made with wires of this metal, pressure and tension were successively applied, and very powerful currents transmitted through the coil; but I could in no case detect the slightest alteration in their dimensions.

I shall not prolong this paper by a discussion of the probable cause of the remarkable facts I have pointed out. The law of *elongation* naturally suggests the joint operation of the attractive and repulsive forces of the constituent particles of the magnet as the cause of that phenomenon. On the other

hand, the fact that the *shortening effect* is proportional to the magnetic intensity of the bar multiplied by the current traversing the coil, seems to indicate that, in this case, the effect is produced by the attraction of the magnetic particles by the coil. But then it will be asked, why so remarkable an augmentation of the effect is produced by the increase of tension in the case of the soft iron bars? When we are able to answer this question in a satisfactory manner, we shall probably have a much more complete acquaintance with the real nature of magnetism than we at present possess.

Fig. 2.

Postscript.

I have already, in the former part of this paper, described an experiment which indicated that no alteration in the *bulk* of a bar of soft iron could be produced by magnetizing it. I thought, however, that it would be interesting to confirm the fact by an observation of the alteration of the dimensions of the iron at right angles to the direction of its polarity. For this purpose I took a piece of drawn iron gas-piping one yard long, $\frac{3}{16}$ ths of an inch in bore, and $\frac{3}{16}$ ths of an inch in thickness. A piece of thick covered copper wire was inserted into this tube, and bent over the outside of it in the manner indicated by fig. 2. The lower extremity of the iron tube being fixed, and the upper end being attached to the micrometrical apparatus described in the first section of this paper, each division of which corresponded to $\frac{1}{138528}$ th of an inch, I obtained the following results:—

Experiment 28. Drawn iron tube.

Deflection of galvanometer.	Tangent of deflection.	Shortening or elongation.	Total shortening effect
+62 30 0	1921 0	3.2 S. 1.2 E.	3.2 2.0
+68 30 0	2539 0	1.0 S. 1.0 E.	3.0 2.0
— 5 0 0	87 0	0 0	2.0 2.0
—15 30 0	277 0	0.2 E. 0	1.8 1.8
—30 0 0	577 0	1.0 E. 0	0.8 0.8
—61 30 0	1842 0	2.0 S. 1.0 E.	2.8 1.8

Experiment 28 (continued).

Deflection of galvanometer.	Tangent of deflection.	Shortening or elongation.	Total shortening effect.
+15 30 0	277 0	0·1 E. 0	1·7 1·7
+30 20 0	585 0	0·7 E. 0	1·0 1·0
+45 30 0	1017 0	0·1 S. 0·1 E.	1·1 1·0
+60 0 0	1732 0	1·2 S. 0·6 E.	2·2 1·6
+72 30 0	3171 0	1·8 S. 0·8 E.	3·4 2·6

The results of the above table show that the length of the tube was diminished, in order to make up for the increase of its diameter, which, in this instance, was in the direction of the polarity. The quantity of the shortening effect, viz. 3·4, is however only one-third of that due to the maximum elongation of soft iron bars as observed in the first section. This is probably owing to the grain of the iron being in cross directions with respect to the polarity in the two cases; and partly perhaps to the iron tube not being fully saturated with magnetism. The experiment is worth repeating, especially as it affords a means of studying the magnetic condition of closed magnetic circuits.

XXXIX. *Second Report of Proceedings in the Cambridge Observatory relating to the New Planet (Neptune).* By the Rev. J. CHALLIS, M.A., Plumian Professor of Astronomy in the University of Cambridge*.

IN conformity with a wish expressed by the Vice-Chancellor and the Observatory Syndicate at their ordinary terminal meeting, held on March 15, I propose in this report to carry on, for the information of members of the senate, the account of proceedings in the observatory relative to the new planet, a first report of which was made on December 12 of last year. The theoretical grounds on which a search for the planet was instituted, the manner in which the search was conducted, and the degree of success that attended it, were stated in the former report, which brought the history of proceedings down to the date at which the planet was discovered. I have now to give an account of the subsequent observations both of its position in the heavens, and of its physical appearance, and to state the

* Communicated by Professor Challis.

results respecting the orbit which have been deduced from the observations by calculation.

A regular series of observations of the planet was commenced on October 3, 1846, and continued at all available opportunities, partly with the meridian instruments, and partly with the Northumberland equatoreal, to December 4, soon after which the planet became too faint to observe on the meridian on account of day-light. The observations were subsequently carried on with the equatoreal to January 15. The series was much interrupted by cloudy weather, particularly in the months of December and January. On the whole I have obtained twenty-eight positions of the planet with the meridian instruments, and twenty-five positions with the Northumberland equatoreal by means of ninety-two differential observations of right ascension and as many of north polar distance. The equatoreal measures were all referred to the same star, No. 7648 of the British Association Catalogue, the exact place of which was determined by sixteen observations with the transit, and eight observations with the mural circle. I have reason to think that the positions obtained with the equatoreal are entitled to very nearly the same weight as those obtained on the meridian. All the above observations I have completely reduced, and have placed the results at the disposal of Mr. Adams for deducing elements of the planet's orbit.

On January 12, I had for the first time a distinct impression that the planet was surrounded by a ring. The appearance noticed was such as would be presented by a ring like that of Saturn, situated with its plane very oblique to the direction of vision. I felt convinced that the observed elongation could not be attributed to atmospheric refraction, or to any irregular action on the pencils of light, because when the object was seen most steadily I distinctly perceived a *symmetrical* form. My assistant, Mr. Morgan, being requested to pay particular attention to the appearance of the planet, gave the same direction of the axis of elongation as that in which it appeared to me. I saw the ring again on the evening of January 14. In my note-book I remark, "The ring is very apparent with a power of 215, in a field considerably illumined by lamp-light. Its brightness seems equal to that of the planet itself." On that evening, Mr. Morgan, at my request, made a drawing of the form, which on comparison coincided very closely with a drawing made independently by myself. The ratio of the diameter of the ring to that of the planet, as measured from the drawings, is about that of 3 to 2. The angle made by the axis of the ring with a parallel of declination, in the south-preceding or north-following quarter, I estimated at 60° .

By a measurement taken with the position circle on Jan. 15, under very unfavourable circumstances, this angle was found to be 65° . I am unable to account entirely for my not having noticed the ring at an earlier period of the observations. It may, however, be said that an appearance like this, which it is difficult to recognise except in a good state of the atmosphere, might for a long time escape detection, if not expressly and repeatedly looked for. To force itself on the attention, it would require to be seen under extremely favourable circumstances. Previous to the observations in January, the planet had been hid for more than three weeks by clouds. The evenings of January 12 and 14 were particularly good, and the planet was at first looked at in strong twilight. Under very similar circumstances I have twice seen with the Northumberland telescope the second division of Saturn's ring,

I communicated to Mr. Lassell of Liverpool, who was the first to suspect the existence of a ring, my observations upon it, accompanied with a drawing; and I have received from him in return a drawing of the appearance presented in his twenty-foot reflector, closely resembling mine both as to the form and the position of the ring. Mr. Lassell writes, "I cannot refuse to consider that your observation puts beyond reasonable doubt the reality of mine." In this conclusion I concur, and accordingly in communications to the Royal Astronomical Society and to Schumacher's *Astronomische Nachrichten*, containing my reduced observations, I have ventured to express my conviction of the existence of a ring.

By micrometer measures taken with the Northumberland telescope, I find the apparent diameter of the body of the planet to be very nearly $3''$.

The above account includes all the observations on the planet I could obtain before its disappearance in the solar rays. By the kindness of Mr. Adams I am able to add some particulars respecting its orbit, which he has derived by calculation from the reduced places with which I furnished him. As was stated in the former report, Mr. Adams calculated first approximations to the elements, by employing the places I obtained on August 4 and 12 in the course of searching for the planet, with observations since the discovery extending to October 13. For the sake of comparison with the second approximations, I now give the first results.

Heliocentric longitude	$326^{\circ} 39'$	Aug. 4, 1846.
Longitude of the descending node.	309 43	
Inclination of the orbit	1 45	
Distance of the planet from the sun	30.05	

In calculating the following second approximations, Mr. Adams used the mean of the two places of August as a single place, and of the others he selected nine which seemed to be the best determined, and which were separated by convenient intervals. All the results are calculated for the epoch of 1846, August, 8.0 mean time at Greenwich.

	o	'	"
Heliocentric longitude of the planet referred to the mean equinox of 1847.0	326	41	12.3
Heliocentric motion in longitude in 100 days	36	5.52	
Heliocentric latitude south	30	34.4	
Change of heliocentric latitude in 100 days	1	4.44	
Longitude of the descending node	310	3	44.0
Inclination of the orbit	1	46	49.1
Distance of the planet from the sun	30.008		
Half the latus rectum of the orbit	30.228		

The first position on which the above results depend, that of August 4, was obtained sixteen days before the planet was in opposition, and the last position, that of January 15, thirty-two days before it was in conjunction. The great variation of the planet's elongation from the sun in this interval, is favourable to the correctness of the above determinations, which, although they cannot pretend to extreme accuracy on account of the short period over which the observations extend, are yet entitled to considerable weight. Mr. Adams has in fact calculated the probable errors of the above results by supposing each observation of right ascension or of north polar distance to be liable to an error of $3''$; and he finds that there is little probability of their receiving any great amount of correction by taking account of future observations. It may be remarked that the first and second approximations do not differ by any large quantities. Hence it may be inferred that the places of August are deserving of confidence, and that on account of the extension given to the period of observation by including those places, this second approximation to the elements is more accurate than it would have been if it depended solely on observations made since the discovery of the planet.

The calculations give $59' 8''$ for the planet's heliocentric motion from August 4 to January 15. This is so small an arc that it is not possible to deduce with any degree of certainty those elements the determination of which depends on change of the heliocentric distance. Mr. Adams has, however, discussed the observations with this object in view, and has obtained certain limiting results, which, as possessing considerable interest, I here subjoin.

The eccentricity of the orbit cannot exceed 0.18. The most probable value is 0.06, which differs but little from the eccentricities of the orbits of Jupiter, Saturn and Uranus.

The most probable longitude of perihelion is $49^{\circ} 58'$, and the probable true anomaly $276^{\circ} 43'$, according to which the planet is near the extremity of the latus rectum and is descending towards perihelion. These results are extremely uncertain.

The mean distance is 30.35, with a probable error of 0.25; and the corresponding sidereal period is 167 years, with a probable error of about two years. It is remarkable that the periodic time is very nearly double that of Uranus; so that these two bodies will offer an instance of mutual perturbations of large amount, differing in character from those of the other planets, but analogous to the mutual perturbations of the first and second, and second and third satellites of Jupiter.

According to Bode's law of the planetary distances, the mean distance of the new planet should be nearly 38. The actual mean distance differs so much from this, that we are compelled to conclude that this singular law, which holds with reference to the other planets, fails in this instance.

Since the apparent diameter of the new planet is to that of Uranus nearly in the ratio of 3 to 4, according to the foregoing determination of the distance its bulk is to that of Uranus in the ratio of 8 to 5.

The above is the sum of the results derivable from the first series of observations. For further and more exact information we must wait till the planet emerges from the solar rays. Before concluding this report, I am desirous of saying a few words respecting the *name* of the planet. I recently had the satisfaction of receiving from M. Struve the copy of a communication read by him at the general annual meeting of the Imperial Academy of Sciences of St. Petersburg, on December 29, in which he states the reasons that have induced himself and the other Poulkova astronomers to adhere to the name of *Neptune*, which name was first proposed by the French Board of Longitude, shortly after the discovery of the planet. These reasons are thus briefly expressed in a note addressed to me personally: "The Poulkova astronomers have resolved to maintain the name of Neptune, in the opinion that the name of Le Verrier would be against the accepted analogy, and against historical truth; as it cannot be denied that M. Adams has been the first theoretical discoverer of that body, though not so happy as to effect a direct result of his indications." M. Struve's communication has been published in this country by the Astronomer Royal, who has expressed his assent to

the reasons therein contained, and his determination to adopt the name of Neptune. Prof. Gauss and Prof. Encke have also, as I understand, adopted this name. I have only to add that it is my intention (and I am permitted to say, the intention of Mr. Adams also) to follow the example set by these eminent astronomers.

Cambridge Observatory,
March 22, 1847.

XL. On the Hourly Alterations of the Vapour Atmosphere at Bombay. By THOMAS HOPKINS, Esq.*

[With a Plate.]

I HAVE already availed myself of the observations made at Bombay under the superintendence of Dr. Buist, through a paper on the meteorology of that place by Colonel Sabine, on which I made some remarks that were inserted in the *Philosophical Magazine* for December 1846. But since those remarks were written I have had transmitted to me, through the kindness of Colonel Sykes, a lithographed copy of the observations themselves, in which are to be found important facts connected with the hourly production of aqueous vapour at Bombay, and its apparent influence on the movements of the barometer.

In addition to the hourly registrations of other meteorological instruments, Dr. Buist has furnished columns of the heights of the wet- and dry-bulb thermometers, showing the hourly depression of the wet below the dry thermometer for each month of the year 1843. From these columns, it appears that the wet was depressed below the dry instrument, to a certain extent varying both with the time of the day and the season of the year. The depression was the least in the wet season and the greatest in the dry one; and, with reference to the diurnal changes, the depression was generally, but not uniformly, the least about sun-rise and the greatest near to mid-day. These depressions of the wet below the dry thermometer are known to be results of the cooling power of evaporation of water on the bulb of the wet instrument; and the cooling thus produced is proportioned to the extent of evaporation of water that takes place; being small when the evaporation is little, and great when it is much. Thus the registration of the wet-bulb thermometer not only furnishes us with means of determining how far evaporation kept down the temperature of the thermometer exposed to its influence, but

* Communicated by the Author.

also of ascertaining what were the relative amounts of water evaporated during the different portions of the time,—the cooling being the effect of the conversion of certain quantities of water into aqueous vapour, and being in every separate part of that time proportioned to the quantity converted.

We have also in a separate column of Dr. Buist's returns, the difference between the dry and the wet thermometers; and this difference may be taken to express the relative quantities of water evaporated, and the force of evaporation. From about six in the morning, varying with the season, this difference generally increases until, say twelve or one o'clock in the day, when it declines until the following morning.

As already stated, evaporation is less in the wet than in the dry season; and there is also less difference between the morning and mid-day force of evaporation in the former than in the latter season. The cold part of the year is the period for the land and sea breezes, and also for the extensive daily fluctuations of the barometer; and Dr. Buist says that this season lasts five months, namely, during October, November, December, January and February, forming the winter of the part. But of these October is wavering and uncertain; we may therefore consider the other four months as the season when the sea and land breezes blow in the most decided manner, and in which the greatest daily fluctuations of the barometer take place; it is consequently desirable that we should carefully examine the returns for the whole of these months. To do this, we will take the mean hourly difference between the dry and wet thermometers for each month, and by adding them together and dividing by four, obtain the mean hourly force of evaporation for the winter season. This I have done, and the result is given in the following table, to which are added in other columns the mean hourly heights of the barometer, thermometer and wet-bulb thermometer, for the same period, that the whole may be seen at one view in juxtaposition. The same facts are shown in a diagram (Plate IV.), in which the curve of the wet-bulb thermometer is exhibited in accordance with the figures of the table; and also as a base line, from which the distance of the dry thermometer is shown, the intermediate space marking the relative force of evaporation in each hour of the day.

Table of the Mean Hourly state of the following instruments, and of the force of evaporation for the four winter months of 1843.

Hours.	Barometer.	Thermometer.	Wet-bulb thermometer.	Evaporation.
4 A.M.	29.838	74.9	68.4	6.5
5 A.M.	29.848	74.6	67.8	6.8
6 A.M.	29.867	74.1	67.4	6.7
7 A.M.	29.889	73.6	67.3	6.3
8 A.M.	29.911	74.9	68.1	6.8
9 A.M.	29.928	77.0	68.9	8.1
10 A.M.	29.928	78.4	69.1	9.3
11 A.M.	29.911	79.6	69.4	10.2
12 A.M.	29.884	80.8	69.5	11.3
1 P.M.	29.853	81.6	70.4	11.2
2 P.M.	29.827	82.2	71.9	10.3
3 P.M.	29.817	82.2	72.6	9.6
4 P.M.	29.815	81.9	72.8	9.1
5 P.M.	29.817	81.0	72.4	8.6
6 P.M.	29.835	79.6	72.2	7.4
7 P.M.	29.854	78.8	71.9	6.9
8 P.M.	29.874	78.4	71.6	6.8
9 P.M.	29.886	77.7	70.9	6.8
10 P.M.	29.887	76.9	69.8	7.1
11 P.M.	29.881	76.4	69.2	7.2
12 P.M.	29.873	76.1	69.0	7.1
1 A.M.	29.862	76.0	69.2	6.8
2 A.M.	29.848	75.7	68.9	6.8
3 A.M.	29.841	75.3	68.5	6.8

By examining this table we may see that evaporation at four in the morning is 6°.5, from which it rises, until at ten o'clock it is 9°.3, being an increase of 2°.8; and during this time the barometer is admitted to be raised to the height that it attains, 29.928 inches, through the pressure of the vapour that has been recently produced by evaporation and thrown into the atmosphere.

From ten to twelve o'clock, evaporation, as measured by the difference between the two thermometers, increases up to 11°.3, being a further rise of 2°; but at the same time the barometer, instead of rising higher, as might be expected, falls no less than .044, as by that hour it sinks to 29.884 inches.

Evaporation becomes rather less active after twelve o'clock; but it still shows great force, and continues to exhibit it till four o'clock, at which time it is 9°.1, being only .2 less than it was at ten in the morning. Yet during the whole of the time from ten in the morning to four in the afternoon, when evaporation was so energetic, the barometer was falling, and sunk no less than .113. What then became of the vapour

that was produced and discharged into the atmosphere within this period? The quantity passed into the atmosphere in the six hours must have been large, it being the product of an average evaporation of $10^{\circ}3$ for the whole time; whilst the average for the previous six hours was only $7^{\circ}3$ when the barometer was rising. Now, can it be supposed that a comparatively small additional quantity of vapour raised the barometer considerably from four to ten in the morning, and a further large addition to that quantity, acting in the same way from ten to four in the afternoon, not only ceased to raise it, but was attended by a fall of that instrument to an extent greater than the previous rise,—without some other cause coming into operation? It cannot be maintained that the increase of surface thermometric temperature had but little counteracting effect before ten, and great counteracting effect after that time until four o'clock. For the rise of the thermometer from four to ten in the morning was little less than it was from ten in the morning to two in the afternoon,—the time of the highest temperature,—it having risen so much as $3^{\circ}5$ in the former, and only $3^{\circ}8$ in the latter period. And it should be remembered that evaporation from ten to four was constantly adding fresh vapour to that which was previously in the atmosphere, where the whole was accumulating, and pressing with its aggregate weight on the barometer. There is, therefore, in the facts presented to us in the tables, no countenance for the supposition that temperature, as measured by the thermometer near the surface of the earth, counteracted the increase of vapour pressure after ten o'clock, and caused the fall of the barometer.

It is however sufficiently evident, from the facts given, that the vapour produced in the morning constituted the material which supplied the heat that at this time rendered the atmosphere warm and light in the locality, and caused the barometer to fall.

When the sun advances above the horizon it warms the surface of the globe, and not only increases evaporation of water, but heats that portion of the atmospheric gases that is near the surface,—which portion rises probably in separate streams or columns,—cooler columns at the same time descending to the surface and taking the place of the warmer. This process goes on as the sun rises higher, until at some particular time, depending on the locality and season, the ascending columns reach a height sufficient to enable the expanding gases to cool and condense some of the vapour which is intermingled with them. This ordinarily takes place from nine to eleven o'clock, or say at ten in the morning, from

which hour condensation is warming the atmospheric mass in the locality. This warmed mass, a mixture of gases and vapour, as it ascends is acted on by the different laws of cooling, of condensation, and of expansion by heat, which the constituent parts of the atmosphere obey; and the result is the formation of a buoyant column of cloud, of greater or less thickness, according to the quantity of vapour that has been condensed. The whole local column being thus made lighter by the liberated heat, it presses with less force on the surface of the earth, and consequently on the barometer. This diminished pressure is however effected through the liberated heat driving a portion of the material of the atmosphere, the gases, from the heated part to other parts of the atmospheric space; and thus we find that the heat just liberated by the condensation of vapour counteracts the increased pressure of the aqueous matter, which is at the same time passing into the atmosphere in the form of vapour. For it is here contended that the vapour that had been produced from four to ten in the morning, is, soon after the last-named hour, not only raised but condensed,—deprived of a part of its heat, and converted into minute particles of water, which float in the gaseous atmosphere as a cloud; and as such, undoubtedly form a part of the whole atmosphere and contribute to its weight. It is not therefore through a reduction in the quantity of aqueous matter in the local atmosphere, at this period of the day, that the barometer falls; but that fall is caused by the expanding power of liberated heat driving from the heated vertical column a part of the ponderable gases which previously existed within it, and, in that way, by removing a part of the material of the atmosphere, causing the remainder to press with less weight on the barometer.

The quantity of vapour that passes daily into the atmosphere while the temperature is rising, and which does not fall as rain, is returned to the earth as dew on its surface; and thus an equilibrium is established between the production and the condensation of vapour; but this does not take place during the period of which we have been treating, that is to say, from four o'clock in the morning to four in the afternoon. Vapour is not daily abstracted from the atmosphere by the formation of dew on the surface of the earth, until the barometer ceases to fall at four o'clock in the afternoon. At present we have to consider the influence of vapour during two periods of six hours each,—that in which the barometer is rising from four to ten in the morning, and that in which it is sinking from ten to four in the afternoon: and it has been shown that aqueous matter during the whole of this time was

increasing in quantity and accumulating in the atmosphere; and as far as that matter influenced the atmosphere, it must have increased its weight. The aggregate pressure of aqueous matter did not cease to increase at ten o'clock in the morning, when the barometer began to fall; that fall was produced by a new cause which then came into operation in the locality, namely, the expanding and displacing power of liberated heat.

It will probably be asked whether cloud forms near to, or over Bombay, after ten o'clock, and increases till four, as assumed? and to this inquiry I cannot give an answer that is likely to be quite satisfactory, not having full information on the subject. Dr. Buist, in his introductory remarks, does not particularly notice the formation of clouds within that portion of the day. In the hourly-observation tables there is a column for describing the appearance of clouds; and the entries in that column show that the cumulus, the cirro-cumulus, and the cirro-stratus were often formed, but they are not described in such a way as would justify me in adducing them as proofs of the agency here ascribed to them, although they do furnish rather strong presumptive evidence on the subject.

Clouds form pretty freely from ten in the morning till four in the afternoon, whilst from ten at night to four in the morning the atmosphere is represented as being generally clear: and taking the accounts given of the clouds during the two periods, we are fully authorized to say that they formed and remained suspended in the air during the former period, and were dissolved before the arrival of the latter period; showing that there were daily formation of cloud by condensation of vapour, and daily dissipation of those clouds by evaporation.

But in addition to the daily formation of visible clouds, there are indications that condensation takes place, but only to such an extent as to produce a haziness or misty appearance in the sky; and if this process is carried on until a considerable height is attained, it must warm the atmospheric mass and lighten it. Indeed, at the commencement of the process of gradual condensation, the cloud that is thereby formed is not seen; as it is only when a sufficient stratum of floating globules of water is produced that the cloud becomes visible. The first slight falling of the barometer before rain is probably caused in this way.

Judging from analogous cases, it may be presumed that palpable daily clouds at Bombay were first formed near the high ground to the east of that place, as such clouds in other similar places generally form near to, or against the sides of hills. Our old navigators have described such formations in

many parts of the world; but the following account given by Hutchison of Glasgow, contains a description of that which ordinarily takes place in localities similar to that of Bombay, although the distance of the mountains from the places of observation in the two cases, and the influences of the trade-winds may modify the process. Mr. Hutchison says that "the formation of clouds is finely illustrated by the phenomena daily exhibited during the dry season over what are called the Liguana, or Port Royal Mountains in the island of Jamaica. These mountains are situated about four or five miles to the north-east by east of Kingston, the principal port in the island, and their height above the level of the sea is about 4000 to 5000 feet. During the dry season, from the beginning of November till the middle of April, the sea and land breezes alternately succeed each other with an intermediate interval of atmospheric stillness, in the following manner. From sun-rise till about ten o'clock in the forenoon it is usually perfectly calm. About ten o'clock, the sea breeze, blowing at Kingston from the east, or a little to the south of east, commences and continues till about half-past three in the afternoon, when it gradually and entirely subsides." Again, "About eleven o'clock every forenoon, or between that and mid-day, the summits of the Port Royal Mountains begin to be covered with clouds, which, though thin, fleecy and transparent at first, gradually increase in density till about one o'clock. By this time the upper portions of the mountain, when viewed from Kingston, seem to be wholly enveloped in dense clouds, rain is apparently falling in torrents, flashes of lightning are seen, and the sound of distant thunder is heard. About half-past two o'clock in the afternoon, the clouds, gradually diminishing in density, begin to quit the mountains; so that their summits again become visible, as in the morning, and so continue till about eleven o'clock the following day. The clouds after quitting the mountains rise gradually to a greater altitude and float very slowly westward, assuming as they proceed the appearance of large heaped-up cumuli." See Hutchison on Meteorological Phænomena, p. 64.

The general trade-wind about Jamaica was from the east, and it bore the ascending clouds to the west in the afternoon: the trade-wind at Bombay was ordinarily from north of west, and it would doubtless modify the influence of the mountains on the clouds formed in the part. Were meteorological instruments corresponding with those kept at Bombay placed to the east of that place at the rise of the hills, and registered in the same way as at Bombay, it would probably throw further light on the daily atmospheric disturbances in this part

of the world, and enable us to form a better judgement of the alterations which take place in other parts. The daily range of the barometer at Poonah, which is on the eastern side of the ridge of the Ghauts, and far from the sea, is about as great as it is at Bombay. There can be no sea breeze at Poonah, as the mountain range is between it and the sea; but are there not diurnal winds of similar character to those on the coast? Corresponding registrations at Poonah, Bombay, and some intervening place on the west of the Ghauts, might furnish valuable additions to our stock of meteorological information, and enable us to trace the operating causes as they pass from one meridian to another, and might thus furnish us with more conclusive evidence of the nature and causes of the hourly alterations which occur in the atmosphere than any that can be adduced at present.

XLI. *Remarks on the Extractive Material of Urine, and on the Excretion of Sulphur and Phosphorus by the Kidneys in an unoxidized state.* By EDMUND RONALDS, Ph.D., Giessen*.

SOME months back, at the instigation of Dr. Golding Bird, I undertook some experiments to ascertain whether, in cases of diseased and imperfect function of the lungs or liver, when the normal quantity of carbon could not be discharged from the system by those channels, the kidneys undertook an extra duty, and whether under such circumstances an excess of carbon could be shown in the urine above that usually secreted under healthy conditions.

Should this question be answered in the affirmative, and should it be found that a larger amount of carbon was excreted by the urine in persons affected with such diseases, a practical application might reasonably be made of the fact. For by stimulating the kidneys to still greater exertion, the amount of work required of the lungs or of the liver could be lessened, and thus a better chance offered them of being restored to a healthy state.

The method proposed for solving this problem was, to precipitate the urine of different patients suffering from diseases of the kinds mentioned, with basic acetate of lead, keeping it slightly alkaline by the addition of a few drops of ammonia, then to ascertain the amount of organic matter contained in the precipitate, and in particular the amount of carbon, and lastly to compare these quantities with those obtained in a similar manner from the urine of healthy individuals.

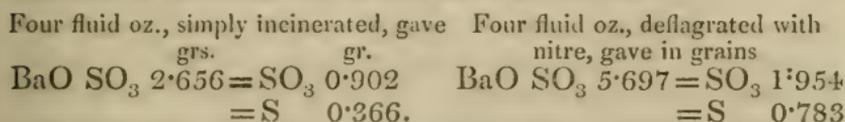
* From the Philosophical Transactions for 1846, part iv.; having been received by the Royal Society April 25, and read June 18, 1846.

In endeavouring to determine the amount of organic matter in the lead precipitate by burning, as likewise in determining the quantity of carbon by an elementary analysis of the same precipitate with oxide of copper, results were obtained which did not agree, and many difficulties arose which it is not necessary to state now, but which rendered it absolutely necessary to separate by some means the organic matter from the oxide of lead before submitting it to analysis, and even that we might obtain accurately its quantity. Whilst employed in seeking an accurate mode of separation, Dr. Scherer's paper appeared on the extractive matters of urine*, in which a successful mode of separation is described, and the question at issue answered. Dr. Scherer finds that the extractive or colouring matter of the urine contains a larger quantity of carbon and hydrogen when obtained from persons in whom the normal function of the lungs, of the liver or of the skin is deranged, than when taken from healthy subjects, and that the same excess of carbon passes off by the urine when the diet is more than usually rich in that element. From his paper however it does not appear that the quantity of this extractive or colouring matter passed during a certain space of time has been ascertained, and it strikes me that he assumes the quantity of extractive to be the same in all kinds of urine; this I think requires to be proved before it can be positively affirmed that more carbon and hydrogen do pass off by the urine in such diseased conditions, as a larger quantity of less highly carbonized extractive matter might compensate for the excess of carbon in the more highly carbonized, supposing the latter to be in less quantity. The relative quantity of these matters, and likewise the relative quantities of urea in a certain amount of urine, must be accurately determined before the conclusion can be considered as absolutely proved.

Whilst engaged with the foregoing researches, it occurred to me that it might not be devoid of interest to the physiologist to know the amount of sulphur which was secreted by the kidneys in an unoxidized state. That urine does contain sulphur, not in combination as sulphate, is evident from the smell of sulphuretted hydrogen which, mixed with that of ammonia, is evolved from it whilst undergoing spontaneous decomposition, also from the blackening which white lead paint suffers when exposed to the gases arising from putrid urine, and likewise from the fact, that urine allowed to putrefy in a glass vessel containing oxide of lead as one of its constituents, permanently blackens the glass. To set the fact beyond doubt, two portions of urine, previously deprived of mucus by acetic

* *Annalen der Chem. und Pharm.*, lvii. 180.

acid and filtration, each consisting of four fluid ounces, were measured; the one simply evaporated and burnt, the other evaporated and afterwards deflagrated with nitre. Each portion was then dissolved in dilute nitric acid, leaving a minute insoluble residue, and the sulphuric acid precipitated by chloride of barium; the results were as follows:—



The quantity of sulphur in four fluid ounces not excreted in the state of sulphate was therefore 0·417 grain. To ascertain the quantity of sulphur excreted by the urine in twenty-four hours in an unoxidized state, the whole quantity passed by three individuals during that time was collected on different days and measured; its specific gravity was taken, and after being filtered from mucus, two portions of each specimen were measured in a specific-gravity bottle containing 1000 grain measures; one portion was simply acidified with nitric acid, and the sulphuric acid precipitated by nitrate of barytes. The other portion was evaporated with nitre and deflagrated, and the fused mass treated with dilute nitric acid and nitrate of barytes. From the difference in the quantities of sulphate of barytes obtained, the amount of sulphur not in an oxidized state was ascertained. The results from five different experiments are shown by the following table.

Table, showing the relative proportions of sulphur free and combined with oxygen excreted by the kidneys in twenty-four hours.

Quantity of urine in 24 hours.	58 fluid-ounces.	41·5 fluid-ounces.	62·5 fluid-ounces.	56 fluid-ounces.	43·5 fluid ounces.
Specific gravity.	1·014.	1·019.	1·017.	1·022.	1·016.
	In 100 grs.	In 100 grs.	In 100 grs.	In 100 grs.	In 100 grs.
Sulphate of barytes from the acid existing in the urine. } Sulphate of barytes precipitated after oxidation of the free sulphur } Quantity of unoxidized sulphur	0·312	0·427	0·414	0·715	0·386
	0·411	0·563	0·546	0·826	0·507
	0·017	0·018	0·018	0·0153	0·0165
Free sulphur excreted in 24 hours	4·639	3·715	4·998	3·886	3·217

It thus appears that from three to five grains of sulphur pass off daily by the urine in some other combination than as

sulphuric acid, and that these three to five grains amount to about one-fourth of the whole quantity of sulphur excreted by the kidneys. With a view to ascertain what compound contained this sulphur in the urine, I have examined the precipitates produced in urine by neutral and basic acetate of lead, after the separation of the sulphates by baryta, and find that they only contain traces of sulphur. The colouring matter described by Scherer, and obtained by the method he adopts, likewise contains only a trace; whereas the liquid containing the urea, generally considered free from all other organic matter, which remains on the separation of the precipitate by basic acetate of lead, contains nearly the whole of this excess of sulphur, but how combined I have not yet been able to ascertain. The determination of this question, which I propose to examine, may possibly lead to some interesting facts, and perhaps throw some light upon the question respecting the formation of cystic oxide.

From some preliminary experiments made for the purpose of ascertaining whether phosphorus was contained in urine in any other compound than as phosphate, the following results were obtained.

To the four ounces of urine employed for the preliminary determination of the sulphuric acid and sulphur in the former experiment, after the precipitation of the sulphuric acid by barytes, some more nitrate of barytes was added and then the whole carefully neutralized with ammonia. The following quantities were obtained:—

Phosphate of barytes obtained from the PO_5 naturally contained in urine was	Phosphate of barytes obtained from urine after deflagration with nitre was
BaO PO_5 5.775 grains	BaO PO_5 6.532 grains
$= \text{PO}_5$ 1.834	$= \text{PO}_5$ 2.074
$= \text{P}$ 0.805;	$= \text{P}$ 0.910;

showing an excess of phosphorus over that contained as phosphate in the four ounces of urine to be 0.105 grain.

In another experiment, in which the same portions of urine were employed as at the fifth column of the foregoing table, the following were the results:—

Phosphate of barytes obtained from the PO_5 naturally contained in 1016 grs. of urine.	Phosphate of barytes obtained from 1016 grs. of urine after deflagrating with nitre.
BaO PO_5 3.135 grains	BaO PO_5 5.313 grains
$= \text{PO}_5$ 0.993	$= \text{PO}_5$ 1.687
$= \text{P}$ 0.435.	$= \text{P}$ 0.740

Therefore 0.305 grain of phosphorus was contained in the 1016 grains of urine in an unoxidized state, which, calculated for the whole amount of urine passed in twenty-four hours,

would amount to 5·896 grains. In some other specimens of urine which I examined there was however no approach to this quantity of phosphorus in an unoxidized state, and as the mode of analysis employed in these preliminary experiments was not the most accurate, I intend to make the determination of the quantity of phosphorus the subject of further experiments.

The only opportunity I have had of examining urine in a diseased state, was a portion obtained from a patient suffering from diabetes mellitus. As might have been anticipated from the character of this disease, the amount of sulphur which the urine contained in an unoxidized state, was considerably greater (by one-fourth) than in healthy urine.

The diabetic urine had a specific gravity of 1046.

Of this urine 1046 grains, precipitated with nitrate of barytes, gave—

4·308 grains sulphate of barytes = 1·479 grain SO_3 = 0·592 grain S.

After the precipitation of the sulphuric acid by baryta, the urine was evaporated down with nitric acid, mixed with nitre and deflagrated. An excess of nitrate of barytes having been used to precipitate the sulphuric acid, there remained on treating the fused mass with dilute nitric acid, an insoluble residue of sulphate of barytes, the sulphuric acid of which had been derived from the oxidation of the sulphur by the nitre; the sulphate of barytes amounted to

1·837 grain = 0·629 grain sulphuric acid = 0·251 grain sulphur, or 0·024 per cent.,

whilst in healthy urine the sulphur in this state never exceeded 0·018 per cent.

XLII. *Note on a System of Imaginaries.* By A. CAYLEY*.

THE octuple system of imaginary quantities, $i_1, i_2, i_3, i_4, i_5, i_6, i_7,$ which I mentioned in a former paper, (and the conditions for the combination of which are contained in the symbols

$$123, 246, 374, 145, 275, 365, 167,$$

i. e. in the formulæ

$$i_2 i_3 = i_1, i_3 i_1 = i_2, i_1 i_2 = i_3,$$

$$i_3 i_2 = -i_1, i_1 i_3 = -i_2, i_2 i_1 = -i_3,$$

with corresponding formulæ for the other triplets i_2, i_4, i_6 &c.,) possesses the following property; namely, if $i_\alpha, i_\beta, i_\gamma$ be any

* Communicated by the Author.

three of the seven quantities which do *not* form a triplet, then

$$(i_\alpha i_\beta) \cdot i_\gamma = -i_\alpha \cdot (i_\beta i_\gamma).$$

Thus, for instance,

$$(i_3 i_4) \cdot i_5 = -i_7 \cdot i_5 = -i_2;$$

but

$$i_3 (i_4 i_5) = i_3 \cdot i_1 = i_2,$$

and similarly for any other such combination. When $i_\alpha, i_\beta, i_\gamma$ form a triplet, the two products are equal, and reduce themselves each to -1 , or each to $+1$, according to the order of the three quantities forming the triplet. Hence in the octuple system in question neither the commutative nor the distributive law holds, which is a still wider departure from the laws of ordinary algebra than that which is presented by Sir Wm. Hamilton's quaternions.

I may mention, that a system of coefficients, which I have obtained for the rectangular transformation of coordinates in n dimensions (Crelle, t. xxxii. *Sur quelques propriétés des Déterminans gauches*), does not appear to be at all connected with any system of imaginary quantities, though coinciding in the case of $n=3$ with those mentioned in my paper "On Certain Results relating to Quaternions," Phil. Mag. Feb. 1845.

58 Chancery Lane, March 2, 1847.

XLIII. On Gun-Cotton.

By E. F. TESCHEMACHER, Esq.*

IN consequence of the discovery by Professor Schönbein of gun-cotton, and of the possibility of its substitution in many cases for gunpowder, I have been induced to enter into an examination of the mode of its formation and of its synthetical composition, principally with a view of ascertaining how far it would be likely to affect an important branch of trade—the production and value of saltpetre and nitrate of soda. The examination was not commenced with the view of publishing it, but some of the facts brought to light appeared to possess sufficient interest to lay before the Society. I must premise that the experiments relate only to the gun-cotton prepared by the process recommended by Mr. T. Taylor. Equal measures of nitric acid of sp. gr. 1.509, and of sulphuric acid sp. gr. 1.840, were mixed together, and constituted (No. 1) the acids used in these experiments. I dried 50 grs. of South American cotton (from La Guayra) over a water-bath, and found the cotton gave off 3.40 grs. of hygrometric water. The 46.60 grs. of cotton which remained after drying, I steeped in 487½ grs. of the mixed acids No. 1; the

* Communicated by the Chemical Society; having been read Nov. 2, 1846.

strong acids were in contact with the cotton about two minutes, and after squeezing well, the cotton was washed in water to free it from the acid: the quantity of water used was 5000 grs.

The strong acid squeezed out of the cotton weighed $113\frac{1}{2}$ grs. (No. 2), leaving 374 grs. of acid (No. 3) to be accounted for, to make up the original quantity used. The cotton thus prepared was perfectly dried in the air over a water-bath, and was found to weigh 79 grs., being an increase of 32.40 grs. on the cotton used, equal to an increase of $69\frac{1}{2}$ grs. upon every 100 grs. of cotton in its original state.

To ascertain whether the cotton would absorb anything more, I steeped 25 grs. of the above cotton, after it had been washed and dried, in fresh acid (No. 1), but no increase of weight took place. I also steeped a fresh portion of cotton for three minutes in the same quantity of fresh acid (No. 1); it weighed after washing and drying 78 grs.; a third portion was steeped for four minutes in one-third more acid than in the previous experiments, it weighed after washing 79 grs.; so that it appeared that the cotton had taken up its maximum from the acid by the first immersion.

As these 32.40 grs. increase of weight were clearly derived from the $487\frac{1}{2}$ grs. of the mixed acids, I made the following experiments to ascertain what alteration the acids had undergone.

I neutralized 100 grs. by weight of the original acid (No. 1) by carbonate of soda, and found that it required 52.80 grs. of soda for that purpose. I found likewise that to neutralize the $113\frac{1}{2}$ grs. of strong acid (No. 2) separated from the cotton, required 58.52 grs. of soda, and that the remaining 374 grs. of acid to be accounted for, contained in the washings, required 170.30 grs. of soda for neutralization, making together 228.82 grs. of soda required to neutralize the $487\frac{1}{2}$ grs. of acids No. 2 and 3, used for acting upon the cotton; therefore, if 100 grs. of the original mixed acids (No. 1) required 52.80 grs. of soda as above for neutralization, $487\frac{1}{2}$ grs. would require (if not acted upon by the cotton) 257.36 grs. of soda; it however only required 228.82 grs. as above. The difference, 28.54 grs. of soda, is therefore equal to the acid which disappeared, or was taken up by the immersion of 46.60 grs. of cotton in the mixed acids. To ascertain whether a portion of each of the mixed acids, or only one of them, was taken up by the cotton, I added to the solution of the 100 grs. of mixed acids (No. 1) (neutralized by the soda) chloride of barium, and obtained a precipitate weighing 126 grs. of sulphate of barytes.

I also, in the same manner, precipitated from the solution

No. 2, 165·40 grs. of sulphate of barytes, and from No. 3, of 374 grs., 449·30 grs. of sulphate of barytes, making together 614·70 grs.; therefore, if 100 grs. of the original acids (No. 1) give 126 grs. of sulphate of barytes, $487\frac{1}{2}$ grs. would give 614·25 grs.; they gave, as above, 614·70 grs., showing that no portion of the sulphuric acid was withdrawn, but that it was a portion of the ingredients of the nitric acid alone that combined with the cotton.

The quantity of soda neutralized by the acids (Nos. 2 and 3) was, as just stated, 28·54 grs.; therefore, if 32 grs. of soda neutralize 54 grs. of nitric acid, 28·54 grs. of soda will give 48 grs. of nitric acid, the quantity of acid which disappeared, or was taken up by the cotton.

From what took place, the following may be deduced as its synthetical composition:—48 grs. of dry nitric acid, containing 35·66 grs. of oxygen, were decomposed by the 46·60 grs. of cotton, and 32·40 grs. were added to the weight of the cotton. It is probable that the mixed acids combined with a portion of the constituent water of the cotton, which was replaced by oxygen and nitrogen, in the proportions of nitric acid, as there was no appearance of the evolution of either nitrogen or oxygen in the formation of the gun-cotton.

Its synthetical composition would stand thus:—

46·60	grs. of cotton deprived of
15·60	water, give
31·00	cotton without constitutional water, combined with
48·00	oxygen and nitrogen derived from the mixed acids,
79·00	the weight of gun-cotton produced, equal to
39·25	parts cotton,
60·75	... oxygen and nitrogen (nitric acid)

in 100·00 parts.

From these experiments we arrive at the following conclusions regarding the formation of this substance:—That as it requires 48 grs. of nitric acid, or its equivalent 90·66 grs. of saltpetre, to form 79 grs. of gun-cotton, it will require 60·75 grs. of nitric acid, or its equivalent 114·76 grs. of saltpetre, to form 100 grs. of gun-cotton; and according to the same calculation, it will require 97·76 grs. of nitrate of soda for the same purpose.

Taking into consideration the original cost of the cotton, the expensive manipulation of the conversion of the nitre into nitric acid, and the additional weight of nitre required to produce the same weight of gun-cotton, it is evident that the latter substance must be more expensive than gunpowder, taking weight for weight of each. How far, however, gun-

cotton may exceed gunpowder in its explosive force still remains to be ascertained, and this of course forms an important element in the calculation. Other processes may be already known, or may be hereafter discovered, calculated to reduce the expenses of the formation of the so-called gun-cotton, but it must be still borne in mind that an enormous quantity of oxygen, amounting to 45 parts in every 100 parts of gun-cotton produced, must be obtained from some extraneous source for combination.

Many other vegetable fibres may be substituted for cotton; but as far as a few experiments which I have made, it appears they do not possess the explosive force of cotton. In a trial upon flax, I found that 50 grs. increased in weight to 72 grs.; the explosive force was feeble; this was the case with sawdust similarly prepared; but it is possible that this latter form of impure lignin may eventually be of more importance than it appears at present.

Fifty grains of *deal* sawdust dried at 212° were first washed with dilute muriatic acid and then with dilute caustic alkali; they were found to have lost 6 grs. after washing and again drying. The 44 grs. remaining were steeped in the mixed acids for about five minutes, then washed and dried; they increased to 58.7 grs. The acids were examined by carbonate of soda in the manner before detailed, the sawdust was found to have neutralized 22.90 grs. of soda, equal to 38.65 parts of nitric acid, or 66.66 of nitric acid for every 100 parts of gun-sawdust. This gun-sawdust flashed off readily, but with less rapidity than gun-cotton, leaving a small carbonaceous residue.

The difference in quantity of nitric acid taken up by the sawdust and the cotton is no doubt owing to the former being a much more impure form of lignin than the latter.

XLIV. *Third Memoir on Induction.*

By Prof. ELIE WARTMANN*.

[With a Plate.]

§ VIII. *Does electro-magnetic induction affect luminous radiations otherwise than by causing their plane of polarization to rotate?*

80. **M**R. Faraday has recently stated that a rotation is impressed on the plane of polarization of luminous

* Communicated by the Author, having been read before the Vaudois Society of Natural Sciences on the 20th of May 1846 (see *Bulletins*, tom. ii. pp. 58, 61, 70, 75 and 98). It is a sequel to those which have been published in the *Phil. Mag.* vol. xxv. p. 266, and vol. xxvii. p. 547, and in the *Archives de l'Electricité*, tome iv. p. 34, and tome v. p. 440.

rays transmitted by various media, when these are the seat of a sufficient electro-magnetic induction*. I have shown that polarized calorific radiations are affected in a manner entirely similar†. Are these effects limited to the cases in which they have been detected? Would not the action of the magnet upon fluids, or rather on diaphanous and diathermanous bodies, be capable of impressing on light and caloric other modifications hitherto unperceived? The mere statement of these questions is enough to indicate an unlimited field of research. I shall content myself with describing the experiments which I have made upon the rays of the spectrum, which may be studied by processes capable of accurate admeasurement, and whose theory is intimately connected with that of light.

81. I long ago observed that the production of the rays, their direction, number and distribution, are not affected by the presence of a magnet in contact with the prism. I repeated these experiments, inducing magnetism in various media through which the luminous bundle had to pass.

82. My prism, of the finest flint glass, was made by Fraunhofer; its angle of refraction is $45^{\circ} 4' 20''$.

83. The diaphanous bodies submitted to induction were the following:—Among the gases, air and nitrous acid, dry and moist: these were inclosed in a glass tube $0^m \cdot 180$ long and $0^m \cdot 008$ in diameter. Lastly, among solids, a specimen of very pure flint glass was selected, in the form of a square prism $0^m \cdot 170$ long and $0^m \cdot 0195$ the side.

84. The rays were observed in a dark chamber with an opening in the shutter of six metres, with an excellent comet-seeker by Cauchoir. This instrument has a reduced opening of $0^m \cdot 069$ and $0^m \cdot 66$ focal distance: it was used with a magnifying power of seven times.

85. The light sent horizontally by a heliostate with a silvered or black mirror, or from a lamp, was polarized more or less completely by reflexion or by its passage through a Nicol's prism. A second similar prism served as analyser.

86. The electro-magnet is formed of a horseshoe of very soft iron, weighing $12^{\text{kil}} \cdot 5$, around which is wound a well-annealed copper wire of $0^m \cdot 003$ diameter and $70^m \cdot 8$ long.

* Philosophical Transactions, 1845, and Phil. Mag. vol. xxviii. p. 294, &c.

† *L'Institut* of May 6, 1846, No. 644. M. Ruhmkorff has recently repeated my experiments. He polarizes and analyses heat with Nicol's prisms of large size. The induction is obtained by means of one of those double helices, destined to reproduce the phænomena discovered by Mr. Faraday, and on which M. Biot has read a very favourable report to the Institute. According as the magnetism is engendered in this powerful apparatus, or the voltaic circuit is broken, the deviation of the rheometric needle varies several degrees. (October 1.)

This apparatus easily raises 312 kil. : it was set in action with a Bunsen's battery of from twenty to forty pairs. Moreover, its energy was augmented, according to circumstances, by laying on the poles the great helix (3), the three wires of which were connected by their ends, or a tube of soft iron containing the transparent media to be induced ; sometimes even the envelope of iron with the diaphanous body were placed in the hollow of the helix.

87. Whatever may have been the degree of polarization of the luminous bundles and the intensity of the magnetic forces developed, *the rays of the spectrum* produced by the rays of the sun or from an artificial source presented no appreciable change. Their number and their distance, varying with the nature of the light and with that of the prisms, were not modified by the new molecular arrangement produced by induction.

§ IX. *Has statical or dynamical induction any influence on chemical affinities ?*

88. Chemical affinities are intimately connected with the electric forces. The problem of their reciprocal dependence has not yet been solved, because this problem has not been regarded in all its bearings. Admitting, with most physicists, that the magnetic influence is felt in inponderable fluids in a mediate manner, by a disturbance or by a new and forced equilibrium in the constitution of the medium traversed by these fluids, it becomes highly interesting to examine whether this disturbance interferes with the forces of affinity, whether it can increase or diminish them. The following experiments were directed to this object.

89. Between the arms of the electro-magnet (86) was arranged a voltameter with lamina of platina, in which some acidulated water was electrolysed. The voltaic current could, at will and by a very simple arrangement, thus bring the magnet into action and develop a north or south pole at either of its extremities. The product of decomposition in a given time was estimated by the hydrogen always collected on the same electrode. Now, whatever the direction and intensity of the magnetism engendered, as well as the position of the voltameter within or without the polar arms, the volume of the gas remained the same*.

90. The same was the case also when the electro-magnet was replaced by the large helix (3), in the hollow of which the voltameter was placed.

* I have been informed by Prof. Grove that similar experiments, still unpublished, had led him to the same result. (October 1, 1846.)

91. To meet the objection that the influence of the magnet is not sufficiently felt on the lamina of platina, I constructed two new voltameters (Plate V. figs. 1 and 2) in which the electrodes are prisms of soft iron, gilded by the galvanic process, and which I magnetize by placing them upon the polar surfaces of the large magnet (86), covered with a very thin isolating leaf of mica. In one of these apparatus, the prisms are 0^m.030 in breadth. Repeated experiments, made with batteries of variable force, sometimes collecting the two gases, sometimes the hydrogen alone, and decomposing acidulated or alkaline water, have invariably led to the same results.

92. But, it will be said, the electric current which imparts to the magnet its power, is an uninterrupted current and incapable of exerting any action upon the electrolyte which is not in its circuit. In order to remove this difficulty, I substituted electricity of tension for that of the current. A fourth voltameter (fig. 3) was constructed so as to ensure the most perfect isolation between its poles. Behind these, against the sides of the glass vessel, I fixed two platina laminæ, one-half of which was immersed vertically 0^m.03 in the liquid to be decomposed, whilst the other was continued outside in a horizontal band terminated by a ball. A thick coating of wax on the exterior of the voltameter prevented all electric communication between these laminæ, except through the liquid. The hydrogen produced in the unit of time by the electrolysis of the acidulated water was measured carefully. The quantity of gas did not vary when one of the laminæ of platina was placed in relation with the conductor of a powerful electrical machine (with a plate three metres in circumference), the other lamina communicating with the ground; or when these laminæ were employed to connect the armatures of a battery of three large Leyden jars, kept constantly charged. The direction of the analysing current was changed without the effect being modified.

93. We may conclude from the preceding experiments, that *statical or dynamical induction has no influence upon the chemical actions engendered by electricity.*

§ X. *Are magnets capable of producing chemical action?*

94. The subject which I have alluded to in the preceding paragraph is so intimately connected with the controverted question of the chemical actions produced by magnets and by terrestrial magnetism, that I deemed it necessary to devote to it some special experiments. Most observers who have studied it have used only magnets of small power. I have employed electro-magnets of great energy, which were at my disposal, to

repeat their researches, being persuaded that a very intense force must produce corresponding decisive and considerable effects. Other circumstances have thrown doubt on the conclusions mentioned by those authors; account has not been taken of the relations between the surfaces which were the seat of the action to be measured, nor of the differences of temperature, of density, and of composition of the liquids employed. I have endeavoured to guard against these causes of error, and to obtain conclusive results by means of numerous trials of sufficient duration.

95. I procured cylinders of soft iron, taken from the same bar, and about $0^m\cdot027$ in height and $0^m\cdot02$ in breadth. Four tumblers were so arranged that the plane of their axes coincided with that of the magnetic meridian and four others arranged in a perpendicular direction. After having poured into each of them a solution of sulphate of copper in layers of equal depth, an iron cylinder was placed in the centre; and the pole of an iron horseshoe magnet was then set on the upper unsubmerged face. The four magnets, the strongest of which carried about forty kilogrms, were distributed, one N.S., the second S.N., the third E.W., and the fourth W.E., with relation to the north pole of the terrestrial magnetism; they were separated, so as not to exert any reciprocal influence. At the end of fifteen hours the deposits of copper on each cylinder presented everywhere the same appearance and the same consistence. The balance proved that they were all nearly of an equal weight; the slight differences found, which scarcely attained to one- or two-thousandths of the total quantity of copper reduced, are explained by the inequality of development and cleanness of the surfaces of the eight cylinders. The experiment was repeated a great number of times with solutions of copper more or less pure, and more or less concentrated, without the general result varying.

96. I will further cite the following experiment, which led to the same conclusions. Twelve iron cylinders were distributed in pairs, in six distinct vessels containing a solution of iron-alum as neutral as possible. The pairs of cylinders of the second, third, fourth and fifth vessels, were placed in contact with the poles of four electro-magnets, the first of which has been already described (86); the second is capable of carrying sixty kilogrms; the third at least forty, and the last more than 280. They were in other respects arranged as in the cases before described (95), and their poles were covered with a plate of mica. Lastly, the current of a battery of forty Bunsen's pairs was passed, so as to decompose the liquid by passing across the interval between each couple of cylinders, at the

same time setting the electro-magnet in action. The current went from north to south between the non-magnetized cylinders of the first vessel; then from north to south between those of the second vessel and the axis of the electro-magnet; then from west to east and from east to west, through the liquid and the axes of the magnets of the third and fourth vessels; then from south to north between the cylinders and in the axis of the magnet of the fifth; lastly, from south to north in the last vessel between its cylinders removed from all magnetic atmosphere. Weighings made with very delicate balances showed that the deposits of oxide of iron on the negative cylinders were not at all affected by the magnetism. The mean weight of these deposits, estimated, after having heated them to above 212° F., was 1.3 gramme.

97. Those authors who have treated of the question of the influence of magnets, have often confounded the part which they may play in chemical actions with that which they exercise in phenomena of molecular arrangements and of crystallization. I think I have proved that their part is null in the first case, but I do not deny its existence in the second. Every new arrangement in the particles of a body must be influenced by the magnet if it is accompanied by a disengagement of dynamic electricity, a more frequent circumstance perhaps than is generally supposed*.

98. My experiments, which are new in their arrangement and the power of the magnets employed, invalidate therefore the opinion of Von Arnim †, J. W. Ritter ‡, Ludicke §, Masch-

* A reservation must however be made for the modifications of molecular equilibrium which the changes of condition occasion. Thus the fusion of solids and the solidification of liquids do not liberate more electricity than the evaporation of pure water or the liquefaction of vapours chemically neutral. Any one may convince himself of this by placing in a horizontal glass tube a leaden cylinder, the extremities of which are connected with those of a good rheometer, and by effecting the fusion of the metal or its solidification by a requisite elevation or lowering of temperature. Another experiment consists in filling a platina crucible with melted wax, in which is suspended a great quantity of plumbago. In the midst of this conducting mass is immersed a platinum rod, isolated in its centre by a covering of glass, and the lower extremity of which is naked only for an extremely small length. The rheometric wire communicating on the one part with the crucible, and on the other with the projecting head of the rod, the wax may be melted or be allowed to congeal without any current resulting.

† *Ideen zu einer Theorie des Magnetismus*. Gilb. *Ann.*, tome iii. p. 59; tome v. p. 394, and tome viii. p. 279.

‡ *Beiträge zu näherer Kenntniss des Galvanismus*, tome ii. p. 55.

§ Gilb. *Ann.*, tome ix. p. 375. This observer for the rest has since maintained an opposite opinion. See Gilb. *Ann.*, tome xi. p. 117.

mann*, Hansteen†, Schweigger‡, Döbereiner§, Müller||, Kastner¶, Fresnel**, Murray††, Archbishop Rendu‡‡, the Abbé Zantedeschi§§, Ampère|||, and of Mr. Hunt¶¶, who have all asserted that magnets possess a chemical power. They agree, on the contrary, with the results published by Steinhauser***, Erman†††, Dulk‡‡‡, Wetzlar§§§, Otto-Linné Erdmann||||, Berzelius¶¶¶, the Marquis Rüdolfi****, and the Chevalier Nobili††††.

* *Ann. de Ch. et de Phys.*, tome xxxviii. p. 201.

† *Ibid.* p. 206.

‡ *Jahrbücher*, tome xiv. p. 84.

§ *Ibid.*

|| Kastner's *Archiv*, tome vi. p. 448.

¶ *Ibid.*

** *Ann. de Ch. et de Phys.*, tome xv. p. 219.

†† *Phil. Mag.* Nov. 1821. His conclusions are opposed to those of Maschmann and Hansteen.

‡‡ *Ann. Ch. et Phys.*, tome xxxviii. p. 196.

§§ *Bibliotheca Italiana*, April 1829. The author appears to have abandoned his first views; he does not cite his own experiments, and does not even mention the chemical power of magnets in his *Trattato del Magnetismo e della Elettricità*. Venice, 1845.

||| Becquerel, *Traité de l'Electricité*, tome i. p. 384. It is to be remarked that several special treatises pass wholly in silence this supposed property of magnetic bodies. Such are the *Treatises on Electricity, Magnetism, &c.* by Dr. Roget, in the *Library of Useful Knowledge*, the *Manual of Electricity and Magnetism in Lardner's Cabinet Cyclopædia, &c.*

¶¶ *Phil. Mag.*, Jan. 1846. For more details see *Researches on the Influence of Magnetism and Voltaic Electricity on Crystallization and other conditions of Matter*, in the *Memoirs of the Geological Survey of Great Britain*, vol. i. London, 1846. Mr. Hunt himself instances a contradiction between the various results he has obtained with iron wires supported by the poles of a magnet and submitted to a chemical action. He finds that in a solution of crystals of sulphate of copper, the copper is reduced in greater abundance around the north pole, whilst in the acidulated water the volume of hydrogen liberated is always more considerable at the south pole. Von Arnim, at the end of the last century, asserted that the north pole of a magnet is oxidized more in water than the south pole. Ritter subsequently maintained the opposite opinion; but notwithstanding his promise, he never published the sequel of his researches, from which we may infer that he discovered his error, and the equal indifference of the two extremities of a magnet.

*** Steinhauser of Halle employed a very powerful magnetic apparatus. *Gilb. Ann.*, tome xiv. p. 125.

††† *Gilb. Ann.*, tome xxvi. p. 139.

‡‡‡ Dulk experimented at Königsberg with a magnet which supported twenty-five pounds. *Kastn. Archiv*, tome vi. p. 457.

§§§ Schweigg. *Jahrb.*, tome lvi. p. 118.

|||| Erdmann of Leipsig has submitted to a scrupulous examination all the researches made by his predecessors. His work may be cited as a model of patience and accuracy. *Schweigg. Jahrb.*, tome lvi. p. 24.

¶¶¶ *Jahresbericht*, No. x. p. 43.

**** *Antologia di Firenze*, No. xix. 1822.

†††† *Memorie ed Istrumenti*, tome i. p. 301.

§ XI. *On a new instance of electro-magnetic rotation.*

99. I have had occasion to observe, in the preceding experiments, a phenomenon of rotation which merits to be noticed, not only as a new confirmation of the theory of Ampère, but also on account of the relations which it may have with the facts which will be stated in the following paragraph.

100. If, after having caused two soft iron cylinders to adhere to the poles of a horseshoe magnet, these cylinders are immersed in a solution of sulphate of copper contained in a vessel of any form and protected from vibration, we perceive, at the end of a few minutes, when the deposit of reduced copper is already quite visible, a double current established in the mass of the liquid. This current often affects much more the lower than the upper molecules; sometimes, according to the density of the fluid and the state of its surface, it is only established in its interior. In order to be able to examine it well, the liquid mass must be illuminated, either directly or by means of mirrors or refractors. A convenient arrangement consists in placing this mass in a glass vessel with smooth sides, and illuminating it in the dark chamber by a bundle of solar light. The tint of this bundle may be varied from red to violet, by interposing in its course coloured media, or by refracting it by a prism, without the phenomenon being modified. The movement is rendered perceptible from the displacement of the particles suspended in the liquid; if the latter is perfectly limpid, it is not observed. It is easily produced by employing a concentrated solution of sulphate of copper, from which some small particles of oxide have been precipitated by a few drops of potassa; these particles remain in suspension. This current ceases at the end of a longer or shorter time, with the chemical action which gives rise to it, when the quantities of copper deposited and of iron dissolved have reached a certain relation. It does not occur in solutions of subacetate of lead, of iron-alum, of triple sulphate of copper, zinc and iron, whatever pains are taken to establish in their interior precipitates of oxide, or to add to them light powders. Sometimes it lasts for more than two hours. Pure water, rendered slightly opaque by particles of oxide of copper, presents no movement.

101. The current is never produced except under the influence of the magnet. Soft iron cylinders immersed in sulphate of copper are of themselves incapable of producing it; they only acquire this power when magnetism is induced in them. If an electro-magnet be employed, the rotation varies with the direction of the electric current. It takes place circularly or elliptically around each magnetic pole, and is directed like the

current of Ampère. It goes therefore from north to south by the west around the north pole, or that by which the current of the battery enters, and from north to south by the east around the south pole, or that which communicates with the zinc of the Bunsen's apparatus (fig. 9). In general it does not appear to be more rapid near the cylinders than at a certain distance. It is especially apparent between them, but it also exists at a tolerable distance from the centres. I have more than once seen the velocity vary in an intermittent manner, as if it had to overcome passing obstacles.

102. The theory of these rotations is easily suggested. When we immerse a bar of iron in a solution of sulphate of copper, the electro-chemical action determines in the liquid an electric current, which proceeds from the peripheric parts to those immediately around the cylinder, in the direction of the prolonged radii of the latter. This may be convincingly shown with a good rheometer, the extremities of which, of thick well-polished platinum wire, are placed alternately, the one near the iron, the other toward the margin of the vessel which contains the solution. This current is analogous to one passed into a metallic ring full of mercury, which would tend, radiating towards the centre, to issue by a conductor placed perpendicularly to the surface. An external horizontal current, and near the vessel, would, according to its own direction, cause the mercury to rotate to the right or to the left. In our experiment the magnet is substituted for the horizontal current.

103. A chemical action is therefore necessary, and proceeding from currents of a certain intensity, for the double polary rotation to take place. The liquids cited as not producing it in a certain manner, evidently did not fulfill this condition: they did not possess a requisite relation of energy between the electric currents and the magnetization. I attribute the observed variations in the force of the electric current, and consequently of the velocity of rotation, to variations in the play of the affinities between the iron, all the molecules of which are not equally acted upon, and the liquid, the composition of which changes incessantly. The reduced copper, even upon polished cylinders, is always striated; it is not deposited therefore in a uniform manner and without intermittence. The temperature, by influencing either the chemical action or the viscosity of the liquid, is perhaps not wholly without influence upon the rapidity of the rotation. Moreover, it will be conceived that this latter becomes visible only when it is communicated to solid matter on which the light is reflected*.

* It is evident, that in employing very intense chemical actions, this

§ XII. *On the Lines of Chemical Affinity.*

104. The phænomenon which I now propose to describe is not the result of an inductive action, I however mention it here on account of the connection which it appears to me to have with the preceding facts.

105. The sulphates of copper of commerce vary much in purity. Amongst others, there is one which, when dissolved in common water, gives a greenish opaline liquid, which becomes blue and limpid by filtration. If a soft iron cylinder is placed in this solution, we perceive, as soon as its contour is reddened by the deposit of copper, very minute filaments, of a pale blue, produced radiating all around its immersed surface. These filaments increase rapidly in number and dimensions. They soon attain forty to sixty millimetres in length, and then present the appearance of stamens with corrugated filaments, terminated by heads or elongated anthers, distributed over a very regular circumference (fig. 10). The development of this curious figure depends on the concentration of the liquid, the capacity of the vessel which contains it, and perhaps on other causes, such as the differences of density between the upper and lower layers produced by the substitution of the sulphate of iron for the sulphate of copper, the increase in density of the surface layers owing to evaporation, &c. Be this as it may, the relative opacity of the heads contrasts with the limpidity of the liquid around the iron. When the decrease in the amount of copper has reached a certain limit, indicated by the grass-green tint which the solution takes, the deposit agglomerates gradually and subsides to the bottom of the vessel, which it had not before done, and the reaction is terminated.

106. The phænomenon becomes more instructive when manifested under the action of two centres (fig. 11). Then the rays which diverge arrive perpendicularly one against another, following the line of the shortest distance. Beyond that, they meet following directions more and more oblique. They never invade each other's domain: they are devoid of any power of penetrating into the interstices of each other. These domains are separated by a perfectly straight line, which intersects rectangularly the middle of the line of the smallest interval. The rays which, from one part and another, stop at this right line, there undergo an inflexion, the more

simple or double rotation is easily obtained. We may thus, as Mr. Grove has told me he had repeated it, cause little cork boats to circulate around a bar of iron suspended to a powerful magnet, and immersed in dilute sulphuric acid, &c. I am told that Mr. Christie had also noticed this rotation, but I believe his observation is unpublished. (October 1.)

sensible as it takes place at a less distance from the others, and resemble more or less decided hyperbolic arcs. The figure is of a truly geometric symmetry.

107. With three centres placed on the summits of an equilateral triangle (fig. 12), the dividing right lines proceed from an internal point situated at an equal distance from the summits, and are prolonged perpendicularly to the three sides of the triangle which are here the lines of the least interval. The divergent rays in two directions curve in a very marked manner. For the rest, the figure which they produce is perfectly symmetrical.

108. The radiations which appear to render visible the lines according to which the affinities are developed, are not sensibly altered by magnetism; this is at least the result of an experiment unfortunately unique. But availing ourselves of the attraction which causes the cylinders to adhere to the arms of a magnet to give them a moderate motion of translation, we see the whole figure, and especially the dividing line which constitutes the most marked part of it, transferred also without any alteration of form. A vibration, a sudden transfer, detaches, on the contrary, the solid particles which were geometrically grouped: they are precipitated to the bottom, and all is spoiled.

109. I suppose these particles to consist of a sub-sulphate of copper*; but I have not been able to collect a sufficient quantity of it to subject it to analysis. I propose however to publish soon a new series of researches on this interesting subject.

§ XIII. *Is there any reaction between currents of induction and continuous electric currents†?*

110. The apparatus which has served for the examination of this question is an electric machine constructed by M. Bonijol. On its reel are coiled two copper wires; one which makes 200 turns is traversed by the inductor current; the other, which is finer, forming 3400 revolutions, is the seat of the currents induced by the rapid alternatives of opening and breaking of the circuit of its neighbour. This

* Mr. H. Rose has told me that he is of the same opinion. (October 1.)

† This paragraph was communicated to the Société de Physique et d'Hist. Naturelle of Geneva, April 9, 1842. See also the *Bulletins des Séances* of the Société Vaudoise des Sciences Naturelles, tome i. p. 68. M. de la Rive has arrived at similar conclusions in his *Mémoire sur l'action combinée des courants d'induction et des courants hydro-électriques*.—*Archives des Sciences Phys. et Nat.*, tome i. p. 393 (1846).

double helix is pierced in the centre by an aperture of $0^{\text{m}}\cdot 04$ in diameter, in which may be fixed a bundle of soft iron wires, or a hollow cylinder of iron plate furnished externally with similar wires.

111. It is especially in valuing the heating effects of which the induced currents are capable, that we seek to resolve the proposed problem. After having placed in the circuit of the long wire Breguet's thermometer (4. c.), and having determined the number of alternate induced currents, which in the unit of time give the maximum of heating, an hydro-electric current was passed into the wire and into the thermometer. The inductor current was that of five large pairs of Daniell (6).

112. The following results were obtained:—

a. The calorific effect of the currents of induction is more considerable when the bundle of wires is placed in the helix than when the hollow cylinder is employed: the latter being removed, the temperature again decreases.

b. The result does not vary whether the induced wire is or is not traversed by a constant current.

c. The elevation of temperature of the thermometer produced by a continuous and constant current, which traverses a wire formed into a helix, is independent of the presence or absence of bundles of soft iron wires, solid or hollow, placed within it.

113. I have had constructed a large pair, of bismuth and soft iron, $0^{\text{m}}\cdot 2$ long, and the square section of which is $0^{\text{m}}\cdot 022$ at the side. The free extremities of these metals were bound to a thermo-electric rheometer (46) by a wire long enough for the instrument to be protected from all magnetic induction.

114. *No appreciable current was produced* on placing the pair in the large helix (3), and developing in it an intense electro-magnetic state with a Bunsen's battery of twenty elements.

115. The soft iron of the couple was enveloped in an isolated copper wire. Then, after having heated its solder to a constant temperature and determined the force of the thermo-electric current produced, a hydro-electric current was passed into the wire. *The deviation* of the rheometer did not vary, notwithstanding the electro-magnetic state induced in the iron.

XLV. *On the Chemical Composition of Gun-Cotton.*

By Messrs. ROBERT PORRETT and E. F. TESCHEMACHER*.

IN the paper from Mr. Teschemacher read before the Society on the 2nd of November, the composition of gun-cotton, as deduced from careful synthetical experiments, was as follows:—

39·25 of cotton deprived of its constitutional water.
 60·75 of dry nitric acid.
 100·00

We have now to show that this composition agrees very closely with the following formula, which we have obtained from the analytical experiments that will form an important part of the present communication.

The formula in question is as follows:— $C_{12}H_8O_8 + 4NO_5$
 =nitrated lignin; or in centesimal proportions,

Carbon	20·00
Oxygen $\left\{ \begin{array}{l} 17·78 \\ 44·44 \end{array} \right\}$	62·22
Hydrogen	2·22
Nitrogen	15·56—100·00

Or it may be thus described:

Lignin dried at 350°: Carbon	20
Water	20=40
Nitric acid (15·56 nitrogen, 44·44 oxygen)	$\frac{60}{100}$

This, it will be seen, very nearly agrees with Mr. Teschemacher's synthetical result. We have therefore taken the composition of nitrated lignin in all our experiments as identical with that of gun-cotton.

Having by preliminary trials ascertained that with certain precautions those products might be evolved without danger and collected in close vessels, we proceed to state that our method consisted in introducing the charge of gun-cotton into a thin glass tube of about the diameter of a goose-quill, 7 inches in length, closed at one end, and bent at the other so as to be conveniently introduced under a glass receiver filled with and inverted over mercury, whilst the principal part of the tube was held in a horizontal position; the gun-cotton was exploded by heat externally applied by means of a spirit-lamp with a very small wick *to the part of the charge in the fore part of the tube*, so that its combustion proceeded backwards until the whole was consumed. The portion of the glass tube which is introduced into the mercury should

* Communicated by the Chemical Society; having been read Nov. 16, 1846.

be bent at such an angle as to prevent any of the mercury entering the tube previous to combustion, and the combustion must be continuous, which is readily effected by carefully applying the heat backwards as the cotton is ignited. With these precautions, the evolution of the gas was not attended with any such violence as to interfere with its collection, nor was any portion of the charge thrown out unconsumed, which would have been the case had the heat been applied to the hinder portion first. Having collected a sufficient quantity of the gas, we passed up into it, first a little caustic potash, which absorbed the carbonic acid mixed with cyanogen gas, and then green sulphate of iron, which took up the nitric oxide; the remaining gas was mixed with half its bulk of oxygen gas, and exploded in a detonating tube, showing the quantity of carbonic oxide present; the remaining gas after the separation of the carbonic acid thus formed was considered as nitrogen.

Some of these constituents were unexpected by us, especially the cyanogen; its presence however was fully proved, as we succeeded in forming prussian blue with it. The quantity of mixed gases obtained from 52.33 grs. of gun-cotton measured 100 cubic inches; these mixed gases have the following composition:—

	Relative volumes.	Cubic inches.	grs.	
Carbonic acid	2	14.286	6.759	= { 1.815 carbon 4.944 oxygen
Cyanogen . .	1	7.143	3.965	= { 1.815 carbon 2.150 nitrogen
Nitric oxide .	5	35.715	11.478	= { 6.091 oxygen 5.387 nitrogen
Carbonic oxide	5	35.715	10.714	= { 4.536 carbon 6.178 oxygen
Nitrogen . .	1	7.143	2.154	= 2.154 nitrogen
		<u>100.000</u>	<u>35.070</u>	

100 grs. of the same would therefore give 64.550 grs. of the mixed gases, and as the ultimate elements, 15.030 grs. of carbon, 31.680 grs. of oxygen, and 17.840 grs. of nitrogen.

By passing the gases from the combustion-tube through a tube containing fused chloride of calcium, we have separated 20 grs. of water, to be added to the gaseous products; and we observed a crystalline deposit, both in the combustion-tube and in the one leading from it, together with 5 grs. of carbon lining the combustion-tube, and resulting from the decomposition of a part of the crystalline deposits after heat-

ing the tube to redness. The weight of these together were 13.125 grs., of which 5 were carbon and 8.125 we ascertained to be anhydrous oxalic acid.

If we now collect these products, we find—

Carbon	{	in the gases	15.030	}	
		in the anhydrous oxalic acid	2.708		
		in the residue	5.000		
					22.738
Oxygen	{	in the gases	31.680	}	
		in the water	17.780		
		in the anhydrous oxalic acid	5.416		
					54.876
Hydrogen in the water					2.220
Nitrogen in the gases					17.840
					97.674
Leaving a deficiency, from unavoidable errors in				}	
the experiment, of					
					100.000

If these ultimate elements be compared with the composition we have assigned to nitrated lignin or gun-cotton, we shall observe the following near approximation:—

	From the analysis.	From the composition of nitrated lignin.
Carbon	22.738	20.000
Oxygen	54.876	62.220
Hydrogen	2.220	2.220
Nitrogen	17.840	15.560
	97.674	100.000

Showing in the analytical result a loss of about 7 per cent. of oxygen and a gain of about 5 per cent. upon the carbon and nitrogen taken together. We have not been able to make these discrepancies disappear, but have no doubt that they result from the difficulties of the experiments, necessarily performed on very small quantities, and multiplied when brought out by calculation as 100 grains.

It occurred to us as being very desirable to add to the gun-cotton some oxidizing agent in sufficient quantity to furnish the necessary oxygen for the complete combustion of all the elements of the gun-cotton; we tried chlorate of potash with this view; we could not however get the cotton to combine with as much as we desired from an aqueous solution of the chlorate, so as to adhere to the gun-cotton uniformly; but a combustion effected with 1 gr. of the cotton with 0.4 gr. of chlorate, and by treating the residue in the tube with a further portion of chlorate, gave us the following result:—

Carbonic acid	0·420	grs.	} containing 0·200 carbon.
Carbonic oxide	0·198	...	
Nitrogen	0·178	...	
Water	0·200	...	
	<u>0·996</u>	...	

which differs but little from the result obtained by the imperfect combustion, and agrees completely as far as the nitrogen is concerned.

Since reading this paper an article has appeared in the *Comptes Rendus* of the Academy of Sciences at Paris, dated 23rd November 1846, that Messrs. Fordos and Gelis had obtained from gun-cotton a *cyanic compound*; this tends to confirm our discovery of the presence of cyanogen among the gaseous products.

A view may be taken of the composition of gun-cotton agreeing with the ultimate results, but arranging the elements differently, by subtracting 1 atom of oxygen from the nitric acid and adding it to the lignin; gun-cotton would then be formed of nitrous acid + oxide of lignin; this new oxide may possess alkaline properties, and thus account for the singular fact of the non-acidity of gun-cotton: we intend to try if we can establish this composition by experiment.

We have tried the action of a small galvanic battery in decomposing the gun-cotton, but without any decided results; on the first vigorous action of the battery, reddened litmus paper was made blue in the negative cell, but as the power of the battery subsided this effect disappeared. As the result of several trials, it does not appear that gun-cotton is susceptible of explosion by the discharge from a Leyden phial.

In conclusion, we think proper to express our dissent from the mechanical view taken by Mr. Crum of the nature of gun-cotton. He considers that the tubular structure which the microscope develops in cotton accounts for the admission of nitric acid into its vessels, but does not explain why it should not get out again on washing; and he thinks that the preservation of its fibrous organization is inconsistent with its chemical combination with nitric acid. Now the fact is, that when cotton is immersed in a mixture of equal measures of sulphuric and nitric acids it combines with the latter only, to the exclusion of the former, which shows a peculiar affinity inconsistent with any mechanical view of the action.

XLVI. *Microscopic Examination of the Papillæ and Nerves of the Tongue of the Frog, with Observations on the Mechanism of Taste.* - By AUGUSTUS WALLER, M.D.*

[With a Plate.]

I HAVE described in some former observations the appearances presented by the papillæ, the vascular and muscular parts of the frog's tongue. At present I propose to examine it in reference to its functions as an organ of taste, and to study those portions of it in particular which enable it to perform its sensorial offices. As these are mainly carried on by means of nerves distributed within it, which collect and convey to the brain the gustatory impressions which are produced upon its surface, our attention will be principally directed to the ultimate ramifications of the nerves, and to the structure of those parts whence the sensations arise. This investigation is the more interesting from the circumstance of the minute anatomy of the ultimate ramifications of the optic, auditory, olfactory and the common sensitive nerves having been described with great care in man as well as the lower animals, whereas the structure of the gustatory nerves has been as yet unascertained. It is not from the unimportance of the subject in the estimation of physiologists that such is the case, as it presents some peculiarities with regard to its nervous structure of great interest, but that the microscopic inspection of a soft, fleshy and opaque body, like the tongue in most animals, offered greater obstacles than any of the other organs.

Valentin states that he has been unable to obtain a distinct view of the terminal plexus and loops of the nerves of the tongue. Burdach, who has so carefully examined the nerves of the frog's tongue, makes nearly the same observation; for he expressly mentions that he was unable to examine the mucous membrane of the upper surface of the tongue, on account of its thickness and opacity. I hope to succeed in demonstrating that the organ of taste, far from being the most difficult to examine, is the most accessible, and that the beautiful and simple mechanism of taste may be followed in the depth of the tissues during the continuance of life.

In briefly recapitulating the principal points that have already been ascertained respecting the structure and functions of the tongue, we find—

1. That anatomists distinguish three kinds of elevations upon the human tongue, viz. the conical, the fungiform, and the lenticular papillæ. The conical papillæ are the most numerous, the smallest, and the most liable to change under the influence of

* Communicated by the Author.

disease. The fungiform are larger, more red, nearly globular in form, and supported by a stem which gives them the appearance of a mushroom or berry with its stalk. They are much less numerous than the former, amongst which they are interspersed. They exist in greater numbers at the tip and borders of the tongue, where the sensation of taste is most acute. The lenticular papillæ are the largest, and are confined to the base of the tongue, and are only about fifteen in number.

2. In Berré's plates of microscopic anatomy, an injected fungiform papilla is represented, which appears entirely composed of vascular coils arranged in nearly a globular shape. Cruveilhier says, "Les papilles qui hérissent la langue représentent le corps papillaire de la peau, à son summum de développement. Elles reçoivent des nerfs. Haller les a poursuivis jusque dans les papilles. Je les ai également suivis, mais sans pouvoir déterminer comment elles se terminent."

3. Comparative anatomy shows the existence in the tongue of most animals of the same kind of papillæ as in man. Cuvier (*Anatomie Comparée*) mentions, "Ce sont les papilles fongiformes qui reçoivent tous les filets nerveux qui sont assez gros pour être suivis à l'œil nu, et cette circonstance jointe à celle de la dureté des papilles coniques dans certains animaux nous porte à croire que les fongiformes sont le siège principal du goût." Messrs. Todd and Bowman adopted the same opinion from observing the extreme thinness of the epithelium over the fungiform papillæ, as compared with that which invests the conical papillæ*.

4. The tongue in man is supplied by three pairs of nerves, the hypoglossus, the glossopharyngeal and the gustatory, a branch from the inferior maxillary. Numerous experiments on the lower animals and pathological facts in man have shown, that the first is the motory nerve of the tongue: much difference of opinion exists with respect to the functions of the latter. The experiments of Panizza would lead us to the conclusion that the glossopharyngeal is the sole nerve of taste, but the simple deductions of anatomy and the experiments of Longet are in favour of a conjoint action of the two, the glossopharyngeal being the nerve of taste for the base of the tongue, and the gustatory that of the tip and anterior third of the same.

If we now come to the tongue of the frog, we find in it, after it has been distended for the purpose of examination, the same elements as we meet with in the human subject. The frame-

* I must refer the reader to their excellent account of the tongue in their work on Physiological Anatomy, which I regret having consulted too late to avail myself of in this paper.

work of it consists of two muscles, the thin and membranous hypoglossus duplex muscle, arising from the lower border of the hyoid bone and advancing upwards and outwards, its fibres expanding in a fan-like shape; and the genioglossus, an impair muscle of a triangular form, whose base is inserted to the apex of the lower jaw-bone, from whence it ascends and joins the inferior surface of the hypoglossus. This last is short and thick, and does not extend beyond the posterior half of the tongue. Numerous transverse fibres of a muscular nature form several layers, and tend to increase the strength of the tongue. As in man, these vessels and nerves are principally situate at the lower surface of the organ, which becomes uppermost in this animal when spread out of the mouth. The upper surface is covered with a very thin membrane, nearly transparent like glass, with all the appearances and properties of cellular tissue. The lowermost, which is the papillary or gustatory surface, is much more thick and opaque.

The surface of the frog's tongue appears to the naked eye nearly smooth and constantly covered with mucus secreted upon it. When forcibly distended, it becomes sufficiently attenuated to allow of the passage of light, and we may then detect very minute elevations over the upper surface. By means of a simple microscope, we find that the apparent evenness arises merely from the minuteness of these bodies, and that it contains all the elements of the human tongue, with the exception of the lenticular papillæ. Small bodies of a conical, filiform, or cylindrical shape, are seen over the greater part of it, corresponding to the papillæ conicæ in man. Among these are others, fewer in number, larger, mostly of a globular form, and most abundant towards the extremity of the tongue: these are the fungiform papillæ. As in man, they are supported by a kind of neck or pedicel, which is narrower than their free extremity. Sometimes this pedicel disappears, owing frequently to the condition of forcible distension of the membrane; the papilla then appears simply globular*. These papillæ are those which to the naked eye resemble minute granulations slightly prominent above the rest of the membrane. They differ from the first-mentioned in another important feature, for they are found to contain a coil of blood-vessels, within which there is a very active circulation. The conical papillæ offer nothing of the sort; numerous vessels ramify among them at their base, without any appearance of permeating within them. The simple lens is the best to employ when we desire to trace the analogies between the organ in this animal and that of man; but when we wish to examine the papillæ

* See Phil. Mag. vol. xxix. plate 1.

more closely, we must use the higher powers of the compound microscope, which the transparency of the membrane permits.

The papillæ conicæ of the frog consist of small elevations of an irregular form, dispersed over the whole surface of the tongue. They may generally be regarded as consisting of small cones or cylinders, two or more of which are joined by a common stem to the mucous membrane. They are completely covered with scales of epithelium, and frequently appear to have small apertures upon their surface, as if leading to some cavity within them. In some, this opening is a small elongated depression at the apex of the papilla; in others it is sharp and well-defined, and below it some scales of epithelium appear lining the commencement of the papillary duct. When the tongue is much distended, these papillæ almost entirely disappear, as if they were simply formed of rugæ or folds of the mucous membrane. They present no appearance of circulation. At different ages of the animal they vary considerably. In the young they are smaller and more transparent than in those full-grown, where they are generally like truncated cones, distinct from one another. At their summit the elongated aperture already mentioned is generally seen. In the *Philosophical Magazine* one of the most common appearances of these papillæ is represented.

The fungiform papillæ, which are so easily distinguished from the former by their active circulation, deserve principally to arrest our attention. The blood, which when seen with the lower power appeared to move within a small cavity, without being contained in any capillary, is found, on being subjected to higher power, to circulate within the usual canals, which are coiled up into a very small space, so as to cause the blood to trace a very circuitous route before it is enabled to make its exit. The exact course which it takes may often be traced where the circulation is languid, by the gradual progress of the blood-discs. The coil is connected with two and sometimes with three small vessels, which supply it with blood. No difference can be detected between these supplying vessels, either with regard to calibre or thickness. They are to be considered as part of the capillary network, and they are often seen to convey the blood in opposite directions, when watched for a few minutes. In most instances the supplying vessels arrive from opposite points, and the circular papilla appears nearly on a level with the rest of the membrane; but in others the papilla is like a gourd or wide-necked flask with the larger end pendent and free. The vessels then run close together, traversing the neck, and after ascending upwards unite with the coil.

The nerves are derived from two pairs, the first of which traverse a foramen together with the vagus at the base of the cranium, between the occipital and sphenoid bones. The second arise from the first cervical nerves, and traverse the foramen between the first and second vertebræ of the neck. Both pairs make a curve which is directed forwards and downwards, and descend nearly in a parallel direction to the hyoid bone, from whence they ascend to the concavity of the lower maxillary and enter the tongue, where their main trunks, much diminished in size, may be traced to the two tubercles at its extremity. The cervical nerves, which at their commencement are nearly on the median line, and are internal to the cranial pair at their origin, retain the same relative position until they enter the tongue, where they begin to ramify, the two cranial nerves then becoming external. The cranial nerve in its passage to the inferior maxillary bone traverses the posterior belly of the digastricus, descends until it reaches the hyoid bone, when passing under the middle and over its great cornua, situate either beneath or by the side of the hypoglossus muscle, continues in that position until it enters the tongue, without giving off any branches to the muscles of the neck. The cervical nerve follows nearly the same direction until it reaches the hyoid bone under which it passes, and likewise places itself under the hypoglossus muscle, penetrating with the latter into the tongue, accompanied closely by the lingual arteries and veins. It supplies numerous branches to the muscles of the throat and neck. This nerve is much more considerable than the former, and is much more curved. The fifth pair or trifacial sends branches to the mucous membrane of the mouth, which appear to reach the posterior part of the tongue, but it gives off no nerves to that organ, corresponding to the lingual branch of the inferior maxillary.

Notwithstanding the transparency of the tongue when in a state of distension, we find considerable difficulty in tracing the ramification of its nervous trunks within the parenchyma. At the upper surface, where they are most superficial, we may under favourable circumstances, with the simple microscope, be enabled to follow the various ramifications of a large branch to a considerable extent, but we are unable either to command a general view of the distribution of a single trunk, or of distinguishing with any degree of certainty the ramifications of one pair from another.

The best mode to examine the nerves in the dead animal is to disarticulate the lower jaw-bone on one side, and to divide the throat on that side sufficiently to allow the jaw to be turned completely backwards. The passage of these nerves may be

followed from their exit from the cranium and spine, until their immersion into the tongue. After removing the mucous membrane of the surface, we may easily trace, by means of the scalpel, with the naked eye the distribution of the principal trunks nearly on the median line, giving off numerous branches, and finally proceeding much diminished in size nearly to the two tubercles on each side. By immersion in alcohol or naphtha, the nerves become whiter and still more distinct. The cervical and cranial pairs then appear unconnected with each other during their course; and Burdach is of opinion that no communication is ever found to exist between these two pairs of nerves, the first of which he terms the hypoglossus, and regards as a pure motor nerve, and the other the glossopharyngeal, which he looks upon as destined entirely to the mucous membrane.

My observations do not allow me to adopt this opinion entirely, as I have seen with the naked eye and with the microscope numerous communications, almost of a plexiform nature, between these trunks at their entrance into the tongue. I have therefore preferred to adopt the terms of cranial and cervical pairs, which are independent of all theoretical ideas. I consider the cervical as destined principally to the muscles, and the cranial to the mucous membrane, where I shall presently describe them. When the muscles have been divested of their papillary membrane, we are able to trace without difficulty, the various ramifications and nervous loops among the muscular fibres, where we see the nervous fibres nearly reduced to a single tubule, forming numerous loops or meshes, running either across or parallel to the striated muscular fibres. These nervous trunks and fibres appear all to be derived from the cervical nerve. Instead of completely removing the mucous membrane, which often renders the examination difficult, from the blood which issues from the divided vessel, we may remove a small portion of the membrane, which when done by tearing it away, will frequently be unattended by loss of blood. The surface thus exposed is usually found covered with a dense plexus of nervous ramifications, which appears to belong to the muscular system. Sometimes in spreading out the tongue we separate from it a kind of investing sac of cellular tissue, nearly as transparent as glass. The microscope detects in this numerous muscular fibres, generally accompanied with nerves and capillary vessels. The nervous fibres are usually accompanied with a satellite capillary vessel closely joined to them. To complete this brief account of the microscopic muscular appearances, I will mention that the circulation may be easily observed in these muscular capillaries, and that a

muscular contraction produces a constant acceleration of the blood, if previously stagnating in them. I have not detected any valves in these capillaries, such as are seen in some of the larger veins of the frog's tongue. Muscular striæ may be seen at the upper and lower surfaces of the tongue, through the layer of epithelium and cellular tissue. The contraction of the muscular fibres may be frequently observed, either uncovered or through the epithelium. I have examined them sometimes in an animal under the influence of strychnia; in none of these cases have I found either the zigzags described by Dumas and Prevost as characteristic of muscular contractions, or the approximation of the transverse striæ, as described by more recent observers.

To return to the nervous fibres. Burdach has employed another means of unveiling the distribution of the nerves, which consists in immersing the tongue for a few minutes in a solution of potassa, in the proportion of three or four drops of alkali to an ounce of water. By this method the membrane of the back of the tongue becomes rapidly converted into a transparent viscous substance like the white of egg, which is easily removed, leaving the subjacent parts nearly unaffected. By compressing the tongue between two pieces of glass, we may trace the distribution of all the principal nervous branches of the organ. The four nervous trunks advance forwards in a parallel direction to the tubercular extremity, giving off numerous branches on their external sides, which branches subdivide and ramify towards the borders of the organ. On their internal sides they give off no branches, and appear to have no anastomoses one with another. The same unilateral ramification is observed with regard to the vascular canals, and may in a great measure be accounted for by considering, that as they and the nerves are all near the median line of the tongue, the parts to be supplied are placed to the right and left sides of this line, and the nervous and vascular trunks are only in proportion to the parts to be supplied.

The cervical nerves are seen to form numerous plexiform anastomoses between their various branches, and to distribute frequent loops composed of nearly single fibres among the muscular parts. The cranial pair ramify in a unilateral manner until they attain the tubercular extremity, where branches are given off on each side of the main trunks: Burdach states that this nerve presents no plexiform arrangement like the former, that a few of its fibres at the extremities form curves or arches, but that generally they terminate abruptly in a bush-like manner, hence his term "*terminaison en buisson.*" In describing his process, Burdach has not concealed its defects in

destroying the papillary surface of the organ; and also he commits the mistake of regarding the papillæ as glandular bodies, as we may see by the following passages. "There are considerable difficulties, even in the frog, in recognizing the course of the nerves in the interior of the tongue; for independently of the thick layer of mucus secreted on its surface, this organ is obscured by a multitude of granules or opaque knots, globular in form, which are doubtless mucous glands; and likewise by numerous canals, variously twisted, of a thickness nearly equal to $\frac{1}{300}$ th of a line, which appear to be filled with corpuscles resembling the blood-discs, and which are either canals to convey the mucus, or lymphatic vessels; and lastly by numerous blood-vessels. Therefore we soon become convinced of the impossibility of distinguishing anything without the employment of artificial means An impossibility already experienced by Valentin and which my own researches have confirmed All chemical substances, whatever they may be, when applied during the life of the animal on the surface of the tongue, have always produced an increase in the afflux of the blood to the organ, and have still more injured its transparency*."

In order to detect the real extremities of these nervous filaments, we have but to examine with care the papillary surface of the frog's tongue. If this is well extended and not over-injected with stagnant blood, we shall have no difficulty in discerning, at the base of each fungiform papilla, a dark gray spot formed by a nerve nearly the size of a capillary vessel. In fig. 1 we have a very correct representation of a papilla as it appears in a favourable subject. The nerve, of which we see a portion, pursues a very winding course. Near the base of the pedicel of the papilla it is usually twisted, as is represented, into a kind of loose knot composed of several loops. This spot is the darkest portion of the nerve, and is to be seen in most of the fungiform papillæ. From this point the fibres ascend apparently less numerous, and accompany the blood-vessels in their numerous gyrations within the papilla. By reason of the darkness and opacity of the blood-vessels the numerous convolutions are generally concealed, and it is only at the intervals between the coils that we are enabled to detect the nervous tubes. The principal varieties which I have had occasion to remark with respect to these papillary nerves, are that sometimes the efferent and afferent capillaries run close together joined to the nerves, which in that case are in a great measure concealed. Sometimes the nerve, like the blood-vessels,

* Burdach, *Structure des Nerfs*.—*Annales des Sciences Naturelles*, vol. ix. 2^{ème} series.

arises from two branches which converge near the papilla as if they were afferent and efferent. It will be preferable that the blood should be found circulating in the papilla, in order to detect more readily the papillary nerve. In cases where the animal has been killed by strychnia, I have always found these nerves more easily distinguishable, as if this substance exerted some effect on the constitution of the nervous system.

I considered it important to determine whether any vesicular matter existed in the papilla, such as has been described to exist at the extremities of some of the nerves of sensation. In one or two instances I have distinguished some granular matter, but in most cases I have not been able, whether from its non-existence or from its being hidden by the engorged vascular coil, to detect the slightest trace of it.

Besides these nerves of the fungiform papillæ, there exist others which are spread over the lower surface of the tongue. They consist of nearly simple nervous fibres, forming a kind of network under the mucous membrane. This nervous network corresponds to the capillary network distributed to the mucous membrane, in the same manner as the nervous papillary coil corresponds to the vascular one. During winter the epithelium of the frog's tongue becomes thinner and more transparent, and the animal is also in an anæmic condition, from which causes the vessels of the tongue are but very slowly engorged; these cases are therefore the most suitable for the inspection of the nerves.

Tongue of the Toad.—The toad's tongue presents very close analogies to that of the frog. I have already described in this journal the slight differences which distinguish them from each other. I shall now merely state what a further investigation has shown me with respect to their nervous structure and papillæ. The difficulty of procuring these animals in the winter has prevented my thorough examination of the distribution of the nervous trunks of their central parts, but as far as I have examined I find a similar distribution to those in the frog. The toad's tongue is less extensible and more thick than that of the frog, and for that reason it is necessary to select a very small animal for microscopic examination. The nervous trunks which ramify in the muscles and accompany the vessels appear to be more numerous than in the frog. The muscular striæ are more distinct, and the fibres are arranged more like separate muscles. The papillæ conicæ, or rather those that correspond to them, may be compared in general to irregular folds or plaits of the intestines; others are like small cylindrical villi; while some fewer are convex elevations, with a very small dark circular foramen in the centre and a passage leading perpendicularly within them.

The fungiform papillæ do not generally project upwards, but consist of a circular area containing a vascular coil and surrounded by an elevated rim of the mucous membrane. Scales of epithelium cover the papillæ conicæ, but the mucous membrane is generally too opaque to admit of the circulation of the blood being clearly distinguished, except at the neuro-vascular papillæ, where it is seen in the coil and at numerous spots about the size of $\frac{1}{100}$ mm, where the mucous membrane appears to be removed, so as to expose fully the capillary circulation beneath. On these spots the forms of the blood-globules are so distinct as to give the appearance of not being confined in a vascular tube. The blood frequently stagnates at these points, or bursts through and is effused in small quantities over the papillary surface, where the separate globules may be seen to oscillate, evidently under the influence of the cilia spread over the surface. In favourable cases nervous filaments may be traced into the vascular area, similar to those of the frog, forming a knot, which afterwards gives off filaments accompanying the vascular coil.

These papillæ and their nerves were most clearly displayed in a toad not half-grown, which had been kept prisoner the whole of the winter. From some accidental cause it had been injured, which had produced great emaciation, and finally occasioned its death. When examined life was extinct, but the muscles were flaccid and circulation of blood existed in the tongue. This organ was beautifully transparent; the papillæ conicæ were as before described; some of the neuro-vascular papillæ had a circular area with coils; others, mostly confined to the extremity of the tongue, consisted of nearly globular transparent vesicles, projecting above the membrane and connected with it by a very narrow neck containing two capillary vessels and a nerve. Near the point of insertion of the vesicle to the membrane a nervous knot was seen, from whence numerous nervous fibres proceeded up the pedicel into the vesicular cavity. Numerous similar vesicles existed at the tubercles, where they formed nearly a continuous row, the transparency of the membrane allowing the nervous filaments which supplied them to be traced backward to the main trunk, which they joined at an acute angle. The vascular coil could likewise be followed to similar venous and arterial trunks, closely accompanying the nerve. This beautiful arrangement may be aptly compared to a bunch of currants, each currant representing a vesicle with its internal coil, and its pedicel to the vasculo-nervous neck of the papilla. As all the papillary nerves joined the main trunk at acute angles, we may easily figure to ourselves the appearance of the termination of the filaments of the cranial nerve, as mentioned by Burdach, by stripping off

the currants and leaving the principal stalk with their projecting pedicels.

Physiological action of the Tongue.—The fungi form or neurovascular papillæ are the parts which more especially deserve our attention, as their structural arrangement points them out as being evidently the principal if not the sole organs of taste in the tongue. Considered under the simplest point of view, they may be regarded as hollow vesicles, containing a coil of nerves and blood-vessels within them, and when a liquid is brought in contact with its external membrane, its thinness causes it to be rapidly permeated by the sapid substance which then comes in immediate contact with the nerve where the sensation of taste is created. The capillary coil then by its internal current and its extensive surface, rapidly carries it away and leaves the nerve free to receive a fresh impression. In analysing this mechanism, we may for convenience distinguish three separate periods or stages. In the first the body permeates the vesicular membrane; in the second it comes in contact with the nerve and produces an impression which is conveyed to the brain; in the third this substance is eliminated from the cavity and from the nerve. The first is a well-known property of all animal membranes, of allowing themselves to be traversed by the various liquid and gaseous substances with which they are in contact. We have an instance of this when venous blood contained in a bladder and exposed to the air or immersed in water containing air in solution, becomes crimson from the passage of the oxygen through the membrane into the blood. A similar action takes place when various other liquids and gases are separated from each other by an organic membrane. Acetic acid applied to the tongue of the frog over a blood-vessel filled with stagnating blood, destroys its red colour and dissolves the tunic of the vesicles in the same way as when it is applied to blood taken from the vessels. A still more notable example is presented by the branchiæ of the tadpole, which may be observed during life. There the blood is perceived circulating with great rapidity in the capillary vessels, and separated from the surrounding water by a very thin membrane. Nevertheless in virtue of the permeability of this membrane, a constant current exists which causes the oxygen in the water to be absorbed by the blood, and the carbonic acid of the blood to pass into the water. In this manner the respiration of the animal is effected. The rate at which a membrane is traversed by a liquid is *ceteris paribus* in proportion to the thinness of the membrane. At the branchial surface it is extremely thin, on account of the distinctness with which the globules of the blood appear through it. For the same reason, it is evident that the

membrane of the papilla, being also very thin, is easily permeated. On the contrary, the thickness of the epithelial scales over the rest of the surface indicates that there the same action is comparatively very feeble and slow. It is necessary for a substance to be in a state of solution before it is capable of traversing a membrane, for if merely suspended or floating in a liquid its progress is completely arrested by the membrane. The same remark also applies to the organ of taste, which is only capable of receiving an impression from a body which can be dissolved in the saliva, or is already in a state of solution.

To further accelerate the passage through the membrane, the body when placed in contact with the vesicle is subjected to a certain amount of pressure by means of the application of the tongue to the roof of the mouth. The effect of pressure in increasing the action of absorption is well-known from numerous facts; its influence in the action of taste is shown by the feebleness of the sensation generally caused by a body when merely applied on the surface of the tongue, compared to the acuteness of perception which ensues on its being compressed between the tongue and the palate. If the surface of the palate had been entirely smooth, the amount of pressure thus sustained by a substance in a liquid or a pulpy state would be very small, but by the existence of small folds and transverse depressions on its surface the body to be tasted becomes lodged and fixed between them, and thus exposed to a much greater amount of pressure. The importance of this compression of sapid substances receives additional evidence from the fact that the gustatory membrane is invariably disposed in a manner to ensure their compression; and that the soft palate, which likewise has the power of taste, may be regarded as squeezing the sapid substance when passing from the throat into the stomach.

When the substance is perfectly insoluble, the only sensation of its presence is of a tactile nature, like that of the hand, which enables us to appreciate the size, form, degree of resistance and temperature of the body. The resemblance of the papillæ conicæ to the papillæ of the organs of touch, render it very probable that they are destined for the same purposes and fulfill the same functions. The areolar network of the nervous fibres at the base of these papillæ is very similar to the terminations of the nerves in the skin, and tends to confirm the idea of the tactile nature of the papillæ conicæ. Supposing all impressions to arise from a molecular perturbation of the ultimate nervous fibres, the agent in this case, which causes the perturbation, is either simple mechanical contact or caloric. Neither of these agents is liable to accu-

multate around the peripheric extremities of the nerve, like a chemical substance; it is therefore a matter of no surprise that these nerves are not surrounded by so rich a vascular expansion. Nay, further, with respect to the action of caloric, we are aware that in virtue of its conductivity it tends quickly to attain a state of equilibrium, which in extreme cases may be in a degree hastened by the vascular circulation of a fluid of an equal temperature like the blood, which if developed to the same extent as in the fungiform papillæ, would, it is rational to suppose, so much equalize the distribution of caloric as to render the nerve incapable of distinguishing minute differences of temperature. But without entering any further into these speculations out of the pale of experimental science, it is proper to state that numerous facts derived from physiology and pathology are in favour of what I have deduced from the examination of the anatomical structure of the two species of papillæ, namely, the existence of two distinct species or sets of organs in the tongue, those for appreciating the chemical properties of bodies, and those for taking cognizance of their physical and mechanical attributes. In numerous instances the faculty of taste is entirely destroyed, leaving the tactile properties unaffected.

With regard to the second period or stage, when the blood comes in contact with the nerve and produces the impression which is conveyed to the brain, we can form no idea of the various actions which are produced within the nerve; we can only surmise that these impressions may either resemble electric currents, such as arise from the contact of two heterogeneous substances, or that they are caused by a kind of molecular perturbation, which is transmitted upwards to the brain, where it produces a kind of image of a more permanent nature. We know that different liquids vary greatly in their power of traversing the same membrane; alcohol and water, for instance, in a bladder become separated from each other by the water traversing the membrane and the spirit remaining behind. To this cause may perhaps in some small degree be attributed the difference in the sensations which different substances give rise to; but by far the most efficient cause is probably the peculiar manner in which the nerve is affected, as different rays of light excite different sensations independently of the intensity of their action. My experiments have not enabled me to ascertain whether the nervous fibrils supplying the two kinds of papillæ are the same or of different origin, or whether they are rendered distinct in their actions merely by means of the structure of the parts around them.

The third stage may be considered as simply a kind of per-

meation or imbibition, very similar to what we have described in the first. It is evident that if a substance were allowed to remain permanently in contact with the nerve, the impressions would be greatly defective, as the substances applied to it would be mixed. To remedy this evil, it is requisite that a means should be found of eliminating a substance from the nervous extremities nearly as quickly as it is allowed to reach them. The nerve when freed is capable of receiving a second impression as clear and distinct as the first. We may easily understand how this may be effected by means of the vascular coil; for if we suppose for a moment that this coil is filled with stationary blood, we see no reason to prevent the diffusion of the substance in the capillary area from taking place in the same manner as in the cavity of the vesicle. If the capillary is now emptied and filled with fresh blood, the same action of imbibition and of solution will take place a second time, and so on as fast as the blood is changed and renewed. In this way we obtain a constant current from the exterior to the interior of the vesicle; and if the sapid substance has quitted the surface of the tongue, the absorption will continue until by an action of endosmosis and exosmosis it becomes entirely removed from the vesicle. The existence of a constant current in the coil undoubtedly much accelerates the removal of the sapid substance from the vesicle, as we observe that whenever an active imbibition takes place, as in the branchiæ of the tadpole, there exists a corresponding increase in the vascular system.

EXPLANATION OF PLATE III.

Fig. 1. Fungiform papilla with nerve and blood-vessel.

a. Nerve at the base of the papilla, where it forms an intricate coil. Its tubules separate as they reach the internal convex border of the papilla, where they form numerous convolutions in the surface of the capillary.

b. Afferent capillary.

c. Efferent capillary.

Fig. 2. Upper surface of the tongue of a young frog, with conical and fungiform papillæ.

The papilla with the large dark area is a fungiform papilla in its early stage of development. Above is another papilla of the same kind, but more developed, with the central nucleus much diminished. Around these are seen conical or tactile papillæ.

XLVII. *Intelligence and Miscellaneous Articles.*

ON THE BEHAVIOUR OF METALLIC ZINC TOWARDS SOLUTIONS OF MERCURY. BY H. ROSE.

THE behaviour of metallic zinc towards solutions of the peroxide and perchloride of mercury is highly remarkable. The zinc

throws down the whole of the mercury from solutions of the pernitrate and persulphate, when the separation of basic salts has been prevented by the addition of nitric or sulphuric acid. The zinc does not combine with the mercury, but the latter separates in the form of gray globules, which collect at the bottom of the vessel; the zinc retains its appearance, becoming only somewhat more gray than before the experiment.

Zinc behaves in the same manner towards a solution of the perchloride of mercury; its appearance is not changed, and the mercury separates in gray globules; but when hydrochloric acid has been added to the solution of the perchloride, the immersed bar of zinc soon becomes quite bright and shining. In this reaction no evolution of gas occurs; only a few bubbles adhere constantly to the bright surface of the amalgamated metal; the mercury is only imperfectly precipitated, for even after several days its presence may be detected in the solution. It is true that, when the zinc is left for a longer time in contact with the liquid, the mercury is at last entirely separated, but not however as a precipitate of gray globules; but the whole of it combines with the zinc and amalgamates it. At all events, when free hydrochloric acid is present, the mercury is separated far more slowly, and under totally different phænomena, than from the solution of the pure perchloride.

When the experiment is somewhat modified, the result is still more surprising. For instance, if a bar of zinc be immersed in hydrochloric acid, and when the evolution of hydrogen is very rapid, a solution of perchloride of mercury be added, the disengagement of gas ceases instantly; the zinc becomes bright, resulting from its amalgamation; and after several days the mercury is only imperfectly separated from the solution. If a solution of perchloride of platinum be now added, evolution of hydrogen immediately recommences.

When the mercury has been entirely separated from a solution of the perchloride by zinc in the form of gray globules, and hydrochloric acid added, the zinc soon becomes bright and amalgamated, without however the mercury, which has been separated in the form of globules, wholly combining with the zinc.

The behaviour of amalgamated zinc is very remarkable. Döbereiner observed that no evolution of gas resulted on the contact of amalgamated zinc with acids; and recently Millon has drawn attention to the fact, that certain metallic solutions hasten the evolution of hydrogen by means of zinc and dilute sulphuric acid (a phænomenon which has been satisfactorily accounted for by Barreswil), while a solution of perchloride of mercury retards this disengagement considerably.

Zinc, as is well known, is most readily amalgamated by immersing it in hydrochloric acid, and then rubbing it over with mercury. Zinc thus amalgamated remains perfectly bright when placed in hydrochloric acid, in which it scarcely dissolves at all, or so little, that only a few bubbles of gas adhere to the shining surface. As is well known, amalgamated zinc behaves, in an electrical point of

view, almost like a distinct metal; and, notwithstanding it is but little acted upon by acids, is more positive than zinc alone. If, therefore, another metal be placed by the side of amalgamated zinc, which is in contact with a dilute acid, a powerful evolution of gas immediately results on the former when it touches the amalgamated zinc. It is requisite, however, that, in order to retain its remarkable behaviour, especially towards perchloride of mercury, there must always be some free acid present, without which it acts like ordinary zinc. When an amalgamated bar of zinc is immersed in a solution of perchloride of mercury, the mercury is separated exactly as with pure zinc in the form of gray globules, and the amalgamated zinc loses its bright coating. In the same way amalgamated zinc reduces solutions of the sulphate of copper, nitrate of silver and acetate of lead, like ordinary zinc, with this difference, that an addition of hydrochloric or nitric acid does not prevent or retard the reduction of the metals by the amalgamated zinc, as is the case with the solution of perchloride of mercury.

When only a few drops of hydrochloric acid are added to a solution of the perchloride of mercury, the zinc is amalgamated; but after some time mercury separates in the form of gray globules, which does not occur when a larger quantity of hydrochloric acid has been employed.

Dilute sulphuric acid acts like hydrochloric acid in preventing or retarding the precipitation of the mercury from the solution of the perchloride; frequently, in this case, a fine crystalline salt separates, which however is only perchloride of mercury, which is far less soluble in dilute sulphuric acid than in water; subsequently a crystalline powder separates in minute scales, coating the zinc, and frequently floating upon the surface of the liquid; this is the protochloride of mercury, which is not further altered by the zinc. It is remarkable that the protochloride is only separated by zinc from the solution of the perchloride when sulphuric acid has been added, and not from a solution of the pure chloride, nor on the addition of hydrochloric acid. Moreover, when a solution of the perchloride of mercury is poured upon zinc immersed in dilute sulphuric acid, the evolution of gas suddenly ceases, just as with the addition of hydrochloric acid. The zinc likewise becomes amalgamated, and a few bubbles of gas adhere for a long time to its surface. Nitric acid also prevents the precipitation of the mercury from the solution of the perchloride by zinc, in the same manner as hydrochloric and sulphuric acids; in this case the protochloride is also formed. When a violent disengagement of nitric oxide has resulted from immersing zinc in nitric acid, it is instantly stopped by the addition of a solution of the perchloride of mercury. It is well known that neither sulphuric nor nitric acids in the dilute, and also in the concentrated state, are capable of decomposing the perchloride of mercury.

Iron completely precipitates the oxysalts of mercury in the same manner as zinc; the metal is likewise wholly separated by iron from a solution of the perchloride, and an addition of hydrochloric acid neither prevents the evolution of gas nor the elimination of the mer-

cury; as the iron is not amalgamated, the mercury separates in the form of globules.

While zinc entirely precipitates the mercury from a solution of the protonitrate, it cannot decompose the protochloride even when recently precipitated and in the presence of water. The insolubility of the salt is by no means the cause of this phænomenon, as the chloride of silver is most quickly and rapidly decomposed by zinc in the presence of water. Even on the addition of hydrochloric acid, the protochloride is not decomposed by zinc, or only to a very small extent when it has been left for several days in contact with it and the acid. Scarcely any evolution of gas is perceptible in this instance; nevertheless the liquid, after a short time, contains oxide of zinc in solution.

The insoluble protosulphate of mercury is likewise not reduced by zinc in contact with water; however, after several days some oxide of zinc has dissolved, and the zinc is slightly amalgamated, more is reduced when dilute sulphuric acid is added, but still the quantity is very inconsiderable; there is no evolution of gas, but nevertheless the zinc is amalgamated.—Poggendorff's *Annalen*.

ON THE COMPOSITION OF THE ORGANIC ALKALIES.

BY M. AUG. LAURENT.

The author observes that the elementary analysis of the organic alkalies is attended with great difficulty, for a difference of two- or three-thousandths of hydrogen is sufficient to change their atomic formula. The greater number of analyses show an excess of hydrogen over the calculated result, which usually amounts to the quantities above-mentioned, or about one equivalent. The author contrived an apparatus to ascertain whether he could not determine the hydrogen to about one-thousandth; and he remarks that two formulæ are given for morphia; that by Liebig being $C^{34} H^{36} N^2 O^6$, and that by Regnault $C^{35} H^{40} N^2 O^6$. The result of the author's analysis is stated by him to be—

C^{34}	2550·0
H^{36}	237·5
N^2	175·0
O^6	600·0
	3562·5

And he further observes, that the analyses of Liebig and Regnault were calculated according to the ancient atomic weight of carbon; whereas with the number 75 the results of Regnault's analysis would be the same as his, or 71·7 and 71·85.

Quina.—M. Liebig's formula for this alkali is $C^{20} H^{24} N^2 O^2$, which, according to M. Regnault, ought to be doubled. M. Laurent repeatedly crystallized sulphate of quina, then extracted the alkali, and dissolved it in æther; the solution was evaporated, and the quina kept for a long time in fusion and then analysed. The results were—

C ³⁸	2850
H ⁴⁴	275
N ⁴	350
O ⁴	400
	3875

Cinchonia.—M. Liebig represents cinchonia by C³⁰ H²² N² O, and M. Regnault by C²⁰ H²⁴ N² O, or rather by double. M. Laurent's analysis gives—

C ³⁸	2850
H ⁴⁴	275
N ⁴	350
O ²	200

It appears therefore that cinchonia differs from quina in containing only half as much oxygen.

The author concludes by giving the following formulæ as the results of his analysis:—

Quina	C ¹⁹ H ²² N ² O ²
Cinchonia	C ¹⁹ H ²² N ² O
Morphia	C ¹⁷ H ¹⁹ NO ³
Quinoleina	C ⁹ H ⁷ N
Lophia	C ²³ H ¹⁶ N ²
Picryle	C ²¹ H ¹⁵ NO ²
Narcotina	C ²³ H ²⁵ NO ⁷
Cotarnina	C ¹² H ¹³ NO ³
Narcogenina	C ¹⁸ H ¹⁹ NO ⁵
Opianic acid	C ¹⁰ H ¹⁰ O ⁵
Opianate of ammonia	C ¹⁰ H ¹³ NO ⁵
Hemipinic acid	C ¹⁰ H ¹⁰ O ⁶
Hemipinate of ammonia	C ¹⁰ H ¹⁶ N ² O ⁶
Starch	C ¹² H ²⁰ O ¹⁰
Pyroxyline	C ¹² H ¹⁷ N ⁵ O ²⁷
Conina	C ⁸ NH ¹⁵

Ann. de Ch. et de Phys., Mars 1847.

ON THE CHANGE EFFECTED IN HYDRATED PEROXIDE OF IRON
WHEN KEPT IN WATER. BY M. G. C. WITTSTEIN.

It is well known that a solution of persulphate or perchloride of iron gives a reddish-brown precipitate with ammonia, and it is flocculent and very bulky: its formula is Fe² O³ + 3HO, and it dissolves perfectly in cold acetic acid. It does not however appear to be known, that this precipitate, when kept under water, and without having been previously dried, almost entirely loses its property of dissolving in acetic acid, as if it had been dried. Some other organic acids, which readily and completely dissolve recently-precipitated peroxide of iron, such as tartaric and citric acids, &c., also dissolve a much smaller quantity of the oxide which has been long kept under water. M. Wittstein states that some researches which he has made on this subject have afforded him very satisfactory results.

If the recently-precipitated and washed oxide be examined by the microscope, it will be seen to be composed of amorphous globules, among which no crystals are perceptible; on the contrary, the precipitate which has been long kept under water appears to be entirely crystalline: the small fragments of crystals are of a deep yellow colour and slightly translucent. The author states that he is not aware of the time required to convert the amorphous into the crystalline peroxide; but the precipitates on which he made his experiments had been prepared more than two years previously. He thinks, however, that the transformation had taken place for a considerable time; for he remembers to have remarked that in six months the precipitate had altered in appearance and become more compact. The hydrate, moreover, in assuming the crystalline form, loses half the water which it contained; the formula of the precipitate which has undergone this change is $2\text{Fe}^{\text{e}}\text{O}^{\text{s}} + 3\text{HO}$.

The difficulty then which attends the solution of hydrated peroxide of iron that has been long kept under water, depends upon two causes, the crystalline form and partial dehydration. It results from what has been above stated, that the peroxide of iron, in order that it may be dissolved by the acids named, and weak acids in general, ought to be employed soon after precipitation. It is probably not indifferent that the peroxide of iron employed as an antidote to arsenic should be recently-precipitated; at all events, preference ought to be given to recently-precipitated oxide, and it will be proper to renew it every six months, or annually. It is not requisite entirely to reject the hydrate which has been kept; it may be dissolved in hydrochloric acid and again precipitated by ammonia.—*Journ. de Pharm. et de Ch.*, Fevrier 1847.

ON VILLEMITE. BY MM. DELESSE AND DESCLOIZEAUX.

The mineralogical collection of M. De Drée contains a *brownish silicate of zinc* from Franklin, New Jersey, United States. It appears to have been already analysed by MM. Vanuxem and Keating, who have referred it to the villemite of Levy; but as it differs much in appearance from the villemite found in Europe, and as its analysis does not agree with that of a mineral analysed by Dr. Thomson, and identical as to its physical properties, and from the same locality, MM. Delesse and Descloizeaux thought it would be interesting to make a comparative examination of the two minerals.

The villemite which MM. Delesse and Descloizeaux analysed was from the zinc mines of Vieille-Montagne near Aix-la-Chapelle: it is in small crystals, of a light rose colour, and has the form of a regular hexagonal prism, terminated by an obtuse rhomb of an angle of $128^{\circ} 30'$, as described by Levy and Phillips. These crystals were contained in the druses of a ferruginous calamine.

The silicate of zinc from New Jersey was also crystallized, but confusedly so; sometimes however the crystals are very well defined, and they are referable, both as to form and cleavage, to the villemite of Levy. The angles are the same; the cleavages perpen-

dicular to the axis of the prism are also evident, as well as those which are parallel to the faces of the regular hexagonal prism. The specific gravity of the mineral from New Jersey was 4.154.

By calcination, the silicate of zinc from New Jersey and the villemite lose only 0.005: this small loss is probably owing to a little hygrometric moisture, and to the oxides of iron and manganese which accompany both minerals; the villemite becomes of a slight brick-red colour. As to the New Jersey silicate, it has a brown colour, a resinous lustre, and yields a powder of a light dirty green colour, which becomes deeper by calcination: like villemite it is infusible by the blowpipe; with carbonate of soda on platina foil it becomes green, indicating the presence of manganese; in borax it dissolves perfectly, with a slight colour indicating iron; with phosphoric salt, a skeleton of silica remains in the globule.

The silicate of zinc from New Jersey, like villemite, is very easily acted upon by hydrochloric acid, and the silica separates in the gelatinous state; it then produces an incrustation around the central part, and it is necessary to continue the ebullition for some time, that the action may be complete.

For a quantitative analysis about fifteen grains were dissolved in hydrochloric acid; after having separated the silica in the usual way, the liquor was evaporated a second time, by which a little more was obtained; the zinc was separated by solution in potash, by repeatedly treating the residue insoluble in acid: the iron was separated from the manganese by succinate of ammonia; no cadmium was detected.

There were by these means obtained, by the analysis of the villemite of Vieille-Montagne and the silicate of zinc from New Jersey the following results:—

Vieille-Montagne.

Silica	27.28
Oxide of zinc	72.37
Protoxide of iron	0.35
	100.00

New Jersey.

Silica	27.40
Oxide of zinc	68.83
Protoxide of manganese	2.90
Protoxide of iron	0.87
	100.00

The results of these analyses agree very well with those of villemite and silicate by MM. Thomson, Levy, Vanuxem and Keating. It follows that manganeseous silicate of zinc of New Jersey, and the villemite, though at first presenting different appearances, belong to the same mineral species, which is a tribasic silicate of zinc $\ddot{\text{Si}} \dot{\text{Zn}}^3$; or supposing that silica contains only two atoms of oxygen, they will be bibasic silicates of zinc $\ddot{\text{Si}} \dot{\text{Zn}}^2$.—*Ann. des Mines*, tome x. 1846.

SOLUBILITY OF CARBONATE OF LIME IN WATER CONTAINING
CARBONIC ACID. BY M. LASSAIGNE.

The solubility of carbonate of lime in water containing carbonic acid has been long known to geologists, mineralogists and chemists ; it is in this state that all waters which contain this calcareous salt convey it for the assimilation of plants and animals.

The spontaneous precipitation of this salt, by contact of the air, fully explains the cause of the deposits formed by the water, and of the incrustations, frequently so abundant, which occur on the surface of bodies exposed to the water for a certain time.

There are other facts, however, connected with the subject which have not been examined into : such are the degree of solubility of carbonate of lime at common temperatures in water saturated with carbonic acid, and the state of saturation in which the salt exists in this condition.

The plan on which M. Lassaigue operated was to prepare a solution of pure carbonic acid by agitating at a temperature of 50° F. and a pressure of 29·7 inches : in this solution chalk reduced to a fine powder was digested for twelve hours.

The solution obtained was filtered to separate the excess of carbonate of lime, and was perfectly limpid ; it reddened tincture of litmus slightly, but had no immediate effect on syrup of violets ; but after several hours' contact the solution became gradually green. The solution when treated with a little potash became immediately turbid, and deposited white flocculi of hydrated carbonate of lime, which soon diminished in volume by losing water, and were converted into a white powder, which partly adhered to the sides of the vessel ; the same effects were obtained with ammonia. When the solution was heated it became turbid, as the gas was expelled by the action of the heat, and carbonate of lime was precipitated.

It was found by experiment that the carbonic acid, which effected the solution of the carbonate of lime, was five times greater than that which existed in the carbonate of lime dissolved at 50°.

This result indicates that the calcareous salt to be dissolved in water by carbonic acid, is directly converted into sexticarbonate of lime, that is to say, into a compound of one equivalent of lime and six equivalents of carbonic acid, which would give $\text{CaO}, 6\text{CO}_2$ as the formula of the soluble salt. M. Lassaigue thinks it probable that this is the state in which the combination exists in certain spring waters which contain it, unless it be regarded as a bicarbonate of lime, rendered soluble by four equivalents of free carbonic acid. The author is, however, disposed to consider the first hypothesis as more conformable to the theory of the saturation of metallic oxides with oxacids.

In operating at the temperatures of 50° F. and 32° F., a difference was observed in the solubility of neutral carbonate of lime in the same solution of carbonic acid. At the former temperature, a volume of the saturated water dissolved 0·00088 of its weight of

carbonate of lime, while an equal volume at 32° dissolved only 0·00070.

This decided difference proves then that the solubility of carbonate of lime in water impregnated with carbonic acid, varies with the temperature, at least within the limits of 32° and 50° F., which were those at which the experiments were made.—*Journ. de Ch. Méd.*, Fevrier 1847.

SOLUBILITY OF PHOSPHATE OF LIME IN WATER SATURATED WITH CARBONIC ACID. BY M. LASSAIGNE.

The solution of carbonic acid employed in these experiments was saturated at the temperature of 50° F., and at a pressure of 29·9 inches; the phosphate employed was left in the solution in each case during twelve hours; and the quantity dissolved was ascertained by evaporating the same volume of each solution to dryness and weighing the residue.

1. *Pure phosphate of lime*, obtained by the double decomposition of a calcareous salt and alkaline phosphate; the solubility was 0·000750.

2. *Fresh bone*, a piece of nearly two inches long, $\frac{1}{100}$ dths of an inch wide, and $\frac{1}{100}$ dths thick; solubility 0·000166.

3. *Bone disinterred* after about twenty years' repose in a cemetery, the subsoil of which was sandy; solubility 0·000300.

M. Lassaigne remarks that these facts confirm an opinion advanced by M. Dumas, that bones which have suffered incipient decomposition in the earth, and which have consequently become less coherent, yield a larger quantity of their calcareous salts to water saturated with carbonic acid, than fresh bones containing all their organic matter.—*Journ de Ch. Méd.*, Janvier 1847.

CRYSTALLIZED BILE.

M. Verdeil effects the crystallization of bile by the following process:—Fresh ox-bile is to be dried by the water-bath, and the residue is to be treated with about twenty parts of absolute alcohol. By this the mucus remains insoluble, whilst the bilate of soda readily dissolves. The solution is to be filtered and treated with animal charcoal to decolorize it; æther is then to be cautiously added to it until it begins to turn milky, and is then to remain in a closed vessel. Crystallization occurs after some time: the bilate is then deposited on the sides of the vessel in the form of small acicular crystals in concentric groups. In twenty-four hours the liquor is to be poured off; and the crystals, after being washed with æther free from alcohol, are to be dried over sulphuric acid.

The crystals thus obtained always contain a little common salt; from this they are freed by placing them with absolute alcohol in a freezing mixture: this salt does not dissolve under these circumstances.

Pure bilate of soda dried at 212° F. yielded by analysis—

Carbon	59·84	59·77	60·07
Hydrogen	8·73	8·80	9·20
Azote	4·11	4·33	
Sulphur	3·78	3·89	
Oxygen	16·45	16·32	
Soda	7·09	6·89	
	<hr/>	<hr/>	
	100·00	100·00	

Journ. de Pharm. et de Ch., Fevrier 1847.

SINGULAR PROPERTY OF GUN-COTTON MIXTURE.

Dr. Draper has made the following observations:—Lecturers on chemistry have known for a long time, that one of the best methods of illustrating the properties of carbonic acid gas, is to evolve it from carbonate of ammonia by the action of monohydrated nitric acid. A dense white fume accompanies the gas, and marks all its movements in a striking manner.

Commercial nitric acid fails to produce the same effect. It sets the gas free in an invisible state. But if a mixture of commercial nitric acid and oil of vitriol be used, then the dense fume is at once produced. The explanation seems to be, that the oil of vitriol, by retaining water, allows some of the carbonate of ammonia to pass off with the carbonic acid in a dry state, and hence gives the gas a smoky aspect.

But it is singular, that though oil of vitriol will of course decompose carbonate of ammonia very rapidly, the gas which escapes is transparent.

Some months ago Dr. Ellet, of South Carolina College, published a process for preparing gun-cotton, which is unquestionably the greatest improvement yet made in the preparation of that explosive substance. His plan is to soak cotton in a mixture of oil of vitriol and saltpetre, and then wash it thoroughly from the adhering salt. Now if this mixture of oil of vitriol and saltpetre be made to act on carbonate of ammonia, like monohydrated nitric acid, or common nitric acid mixed with sulphuric, it evolves carbonic acid in the smoky state.

ON THE DEHYDRATION OF SULPHATE OF LIME UNDER VARIOUS CIRCUMSTANCES. BY M. M. E. MILLON.

The author observes that it is well-known that sulphate of lime which is slowly produced, retains its water of combination at a temperature at which precipitated sulphate loses it.

Sulphate of lime.—This salt contains two equivalents of water, $\text{SO}_4, \text{HO} + \text{CaO}, \text{HO}$; according to Graham it suffers no loss at 212°F ., and becomes anhydrous at 266° . The general results obtained by M. Millon effect considerable changes in this statement.

Sulphate of lime, of whatever form, origin or mode of formation, always suffered a first loss, varying from 15 to 17 per cent. This quantity represents 1·5 equivalent of water: its elimination is per-

fectly decided, and cannot be confounded with the total loss, which varies from 20 to 22 per cent. and corresponds to two equivalents of water. The following are the results of experiments:—

Artificial sulphate of lime, prepared by precipitating cold solutions of sulphate of zinc and chloride of calcium; when dried over sulphuric acid the result was invariable. By exposure for six hours to a temperature of 176° to 185° F. it lost 17 per cent.; no further loss occurred by exposure for an additional hour to the same temperature; when rendered perfectly anhydrous the loss was 22 per cent.

Artificial sulphate of lime, precipitated from mixed boiling solutions of sulphate of zinc and chloride of calcium; dried as before, the loss at 176° to 185° F. remained invariably at 15·71 per cent.; when rendered anhydrous the loss was 20·87 per cent.

Moistened plaster lost at the above temperatures 16·22 per cent.; rendered anhydrous, the loss was 20·39 per cent.

Precipitated sulphate of lime, dissolved in hydrochloric acid, and crystallized from it.—By fifteen hours' exposure to 176° up to 185°, it lost no weight, and none occurred till the temperature reached 221° F., when it lost 15·38 per cent. The same heat was afterwards continued during four hours without further loss. Rendered anhydrous, the loss was 20·78 per cent.

Arrow-headed gypsum from Montmartre.—This suffered no loss at 176° to 185° F.; at 221° it amounted to 15·37 per cent., and the total loss was 20·78 per cent.

Fibrous sulphate of lime.—In fifteen hours lost no weight at 185° F.; at 221° the loss was 17·60 per cent.; it was afterwards exposed for twelve hours to the same temperature with scarcely any diminution; the total loss was 22·62 per cent.

Alabaster from Volterra.—Suffered no diminution of weight by exposure at 176° to 185° F.; at 230° F. the loss was 15·61 per cent.; several hours' continued application of the same heat occasioned no further diminution of weight; the total loss was 20·83 per cent.

Snow-white gypsum (Gypse en neige).—Lost no weight at 185° F.; at 238° F. it was 15·57 per cent. No further diminution occurred by several hours' longer exposure to this heat. The total loss was 21·27 per cent.

Fibrous gypsum from America.—Lost no weight at 185° F.; at 230° F. lost 15·41 per cent.; total loss 20·59 per cent.

Prismatic gypsum from Sicily.—No diminution of weight at 185° F.; at 230° it was 15·58 per cent., and the total loss was 20·44 per cent.

It appears, therefore, that all native sulphates of lime retain their water at 185° F., and do not lose any below 221° to 236°. Artificial sulphate of lime crystallized in hydrochloric acid is similarly circumstanced. Artificial sulphate of lime, on the other hand, whether precipitated hot or cold, loses three-fourths of the water at 176° to 185°. Moistened plaster is also dehydrated at this lower temperature.

It will be observed that all the sulphates of lime undergo a fractional loss of water, and that the second state of hydration is expressed by $(\text{SO}^3)^2, \text{HO} + 2\text{CaO}$.

The last fourth of the water of hydration goes off very slowly, if the heat be not raised to nearly 400° or 570° F.; at 257° to 393° F. mere traces of water are separated in several hours. This resistance is unquestionably very favourable to the calcination of plaster intended for buildings; it prevents its complete dehydration, even at a somewhat higher temperature.

M. Millon has also observed that anhydrates which contain some hundredths of water, lose it in fractional quantities; one specimen lost 3.65 per cent. of water. The first loss of 2.89 per cent. occurred at 221° F.; the remainder, or 0.76 per cent. of water, required a higher temperature for its expulsion. On examining the anhydrate with a glass, opake portions were visible, which were small crystals of the trapezoidal variety of sulphate of lime, and were evidently formed by the anhydrate having absorbed moisture from the atmosphere.—*Ann. de Chim. et de Phys.*, Fevrier 1847.

COFFEE AS AN ANTIDOTE TO ACETATE OF MORPHIA.

An invalid took at one dose ten grains and nearly eight-tenths of acetate of morphia; thirty grains of emetic tartar were exhibited without occasioning vomiting; after a lapse of three hours, and not till then, and when the patient was perfectly comatose, a strong infusion of coffee with the grounds was given. In the course of twelve hours the invalid took about 11½ ounces of coffee; the coma ceased and he recovered.

This fact proves, among a hundred others, that even in the worst cases of poisoning, the medical man should never despair of the recovery of his patient. In the above-described case, in spite of a very strong dose of poison, and notwithstanding the absence of all assistance during three entire hours, and although it was impossible to evacuate any portion of the morphia, the patient recovered. If a similar accident should again occur, vomiting ought to be immediately attempted; if this fail the stomach-pump should be employed, and then concentrated coffee should be administered.—*Journ. de Pharm. et de Ch.*, Fevrier 1847.

ON THE FORMATION OF CYLINDRICAL MASSES OF SNOW IN ORKNEY.

To Richard Taylor, Esq.

Sandwick Manse by Stromness, Feb. 11, 1847.

MY DEAR SIR,—A curious phenomenon in this parish has astonished and perplexed all, and filled the superstitious with no small degree of consternation. Since the 6th inst. we have had hail- or snow-showers, on the 9th snow-drift, and yesterday a slight thaw with frost again in the evening.

During the night a heavy fall of snow took place which covered the plain to the depth of several inches. Upon this pure carpet there rest thousands of large masses of snow which contrast strangely with its smooth surface. A solitary mass may be seen in a field, but in general they occur in patches from one acre to a hundred in extent, while the clusters may be half a mile asunder, and not one mass to

be seen in the interval. These fields appear at a distance as if cart-loads of manure had been scattered over them and covered with snow, but on examination the masses are all found to be cylindrical, like hollow fluted rollers or ladies' swan-down muffs, of which the smaller ones remind me, from their lightness and purity, but most of them are of much greater dimensions and weight than any lady would choose to carry, the largest that I measured being $3\frac{1}{2}$ feet long and 7 feet in circumference. The weight however is not so great as might be expected from the bulk; so loose is the texture, that one near this house which was brought in and weighed, was found to be only 64 lbs., though it measured 3 feet long and $6\frac{1}{2}$ feet in circumference. The centre is not quite hollow, but in all there is a deep conical cavity at each end, and in many there is a small opening through which one can see, and by placing the head in this cavity in the bright sun, the concentric structure of the cylinder is quite apparent. So far as I am yet informed, they do not occur in any of the adjoining parishes, and they are limited to a space of about five miles long and one broad. They may occupy about 400 acres of this, and I counted 133 cylinders in one acre, but an average of a hundred would, at a rough computation, yield a total of about 40,000.

Now the question naturally arises, what is the origin of these bodies? I believe the first idea was that they had fallen from the clouds, and portended some direful calamity, and I hear an opinion that one had fallen on a corn-stack and been broken to pieces. It is a pity to bring down such lofty imaginations, and to deprive these cylinders of their high descent, but I prefer truth, when it can be discovered, to the loftiest theory. I must at once, then, set aside the idea that they fell from the atmosphere in their cylindrical form, as the first one I examined satisfied me that its symmetry and loose texture must have been immediately destroyed on coming in rude contact with this earth.

Farther observation has convinced me that they have been formed by the wind rolling up the snow, as boys form large snow-balls. This is proved by examination of the *bodies themselves*; their round form, concentric structure, and fluted surface all show this mode of formation. Again, it is proved by their *position*: none are found on the weather side of hills or steep eminences, where the wind could not drive them up, nor close to leeward of any wall or perpendicular bank from which they seem to have originated—the nearest well-formed small ones being 60 yards to leeward, and the large ones 100 yards. All nearer than this are fragments that have not gone on to completion, but broken down in their passage, and the different portions of the wreck form the nuclei of others. Many however are found blown to the windward side of walls or over the lee side of banks. Indeed, they are found almost exclusively on the leeward side of hills and eminences, where both the wind and declivity assisted in rolling them along, or on plains so exposed that the wind alone operated without the declivity.

I shall only add, that this mode of formation is proved by the *direction* in which these cylinders lie. The wind has been from the

north for four days, and I believe that it was so all night, when I am told it blew strong. Now they are all lying with their ends east and west, and their side to the wind; and farther, in some cases, their tracks are still visible in the snow for twenty or thirty yards on their north side, from which they have gathered up their concentric coats; and I understand these were still more evident at an early hour before a snow-shower obliterated them in many places.

I am, Sir, yours very truly,

CHARLES CLOUSTON.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1847.

Chiswick.—February 1. Overcast. 2. Slight snow. 3. Cloudy. 4. Cloudy: frosty. 5. Overcast. 6. Slight rain. 7. Overcast: snowing. 8. Sharp frost: snowing. 9. Clear and frosty: intense frost at night. 10. Severe frost: snowing. 11. Overcast: slight thaw: severe frost. 12. Intense frost: clear: severe frost. 13. Clear and frosty. 14. Rain. 15. Cloudy: boisterous. 16. Overcast: rain. 17. Fine. 18. Densely clouded: boisterous. 19. Boisterous: fine: clear and calm. 20, 21. Overcast: fine. 22. Hazy: overcast. 23. Hazy and cold. 24. Dry air: clear and frosty. 25. Slight haze. 26. Hazy. 27, 28. Cloudy and cold.

Mean temperature of the month	34° 79
Mean temperature of Feb. 1846	45 °32
Mean temperature of Feb. for the last twenty years	39 °55
Average amount of rain in Feb.	1·61 inch.

Boston.—Feb. 1. Cloudy: snow P.M. 2. Cloudy: snow early A.M.: snow nearly all day. 3. Cloudy: snow P.M. 4. Fine. 5—7. Cloudy. 8—10. Fine. 11. Cloudy: snow on the ground. 12. Cloudy. 13. Fine. 14. Cloudy: rain early A.M. 15. Cloudy. 16. Cloudy: rain early A.M. 17. Fine: rain early A.M. 18. Fine: rain P.M. 19. Stormy. 20—23. Cloudy. 24. Fine. 25. Cloudy. 26. Cloudy: snow early A.M. 27. Fine: snow P.M. 28. Fine: melted snow.

Sandwich Manse, Orkney.—Feb. 1. Snow: clear. 2. Snow: clear: frost: clear. 3. Bright: thaw: drizzle. 4. Damp: drizzle. 5. Showers: lightning. 6. Hail-showers: aurora. 7. Hail-showers: snow-showers: aurora. 8. Snow-showers: aurora. 9. Snow-drift. 10. Sleet: thaw: snow: frost. 11. Deep snow*: snow. 12. Deep snow: bright: showers: thaw. 13. Thaw: rain. 14. Sleet-showers. 15. Cloudy: showers. 16. Showers. 17. Showers: rain. 18. Showers. 19. Showers: clear. 20. Cloudy: rain. 21. Bright: showers. 22. Bright: clear: aurora: large halo. 23. Bright: clear. 24. Cloudy: clear: aurora. 25. Clear: frost: clear. 26. Bright: clear. 27. Clear: cloudy. 28. Cloudy.

Applegarth Manse, Dumfries-shire.—Feb. 1. Frost: snow lying half an inch deep. 2. Frost: slight shower: snow. 3. Frost. 4. Frost, but mild. 5. Thaw: slight rain. 6. Thaw: fair. 7. Frost: clear and fine. 8. Frost, hard. 9. Frost: threatening snow. 10. Frost: sprinkling snow. 11. Frost: fine: clear. 12. Frost: sprinkling snow. 13. Frost A.M.: rain P.M. 14. Thaw, soft and fine. 15. Frost, slight: thaw: rain. 16—18. Rain. 19. Rain and sleet: fierce wind. 20. Rain and wind. 21. Fair and fine: thrush singing. 22. Rain early A.M.: cleared. 23. Slight hoar-frost: clear. 24. Frost: clear and bright sun. 25. Hoar-frost. 26, 27. Frost. 28. Frost: clear and fine.

Mean temperature of the month	36° 25
Mean temperature of Feb. 1846	43 °4
Mean temperature of Feb. for twenty-five years	37 °2
Mean rain in Feb. for twenty years	2 inches.

* This morning the snow in many places is found rolled up in hollow fluted cylinders, the largest of which measures 3½ feet long and 7 feet in circumference; one which measures 3 feet by 6½ weighs 64 lbs.

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MAY 1847.

XLVIII. *On the Knowledge of Distance given by Binocular Vision.* By SIR DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edinb.*

[With a Plate.]

IN analysing Mr. Wheatstone's beautiful discovery, that in binocular vision we see all objects of three dimensions by means of two dissimilar pictures on the retina, I trust I have proved that the dissimilarity of these two pictures is in no respect the cause of our vivid perception of such objects, but, on the contrary, an unavoidable accompaniment of binocular vision, which renders it less perfect than vision with one eye †. On the other hand, it is quite true that, in Mr. Wheatstone's experiment of producing the perception of objects of three dimensions by the apparent coalescence of two dissimilar representations of such objects *in plano*, the dissimilarity of the pictures is necessary in the exhibition of that beautiful phænomenon.

In performing with the eye alone, the various experiments detailed in a former paper ‡, I was very much struck with the fact, that the apparent solid figure, produced by the union of its dissimilar pictures, never took its right position in absolute space; that is, in place of appearing suspended between the eye and the plane upon which the dissimilar figures were drawn, the base of the solid seemed to rest on that plane, whether its apex was nearer the eye or more remote than its component plane figures.

With the view of finding the cause of this, I placed the component figures on a plate of glass suspended in the air, so as to have no vision of the surface on which they rested, and

* Read at the Royal Society of Edinburgh, April 15, 1844. See their Transactions, vol. xv. p. 663.

† See this Journal for May and June 1844, vol. xxiv. pp. 356, 439.

‡ Ibid.

after uniting these figures by binocular vision, and concealing the two outstanding single figures, I obtained results which, though not entirely satisfactory, proved that there existed some disturbing cause which prevented the united image from placing itself in the *binocular centre*, or the intersection of the optical axes. This disturbing cause was simply the influence of other objects in the same field of view, whose distance was known to the observer.

In order to avoid all such influences, and to study the subject under a more general aspect, it occurred to me that these objects would be gained by using a numerous series of plane figures, such as those of flowers or geometrical patterns upon carpets or paper-hangings. These figures being always at equal distances from each other, and almost perfectly equal and similar, the coalescence of any pair of them, by directing the optic axes to a point between the paper-hangings and the eye, is accompanied with the coalescence of every other pair. When the observer, therefore, places himself in front of that side of a papered room in which there are neither doors nor windows, and conceals from his eye the floor, the roof, and the right and left-hand sides of the room, the whole of his retina will be covered with the images of the united plane figures, and there will be no interposing objects to prevent him from judging of the distance of the picture that may be presented to him.

Let the observer therefore now place himself *three feet* in front of the papered wall, and unite two of the figures, suppose two flowers, at the distance of *twelve inches*. The whole wall will now be presented to his view, consisting of flowers as before, but each flower will be composed of two flowers superimposed at the binocular centre, or the point of convergence of the optical axes. If we call D the distance of the eyes from the wall or *three feet*, C the distance between the eyes or *two-and-half inches*, and d the distance between the similar parts of the two flowers, we shall have x the distance of the binocular centre from the wall, $x = \frac{Dd}{C+d} = 30$ inches nearly, and $D-x=6$ inches, the distance of the binocular centre from the middle point between the two eyes.

Hence the whole papered wall, with all its flowers, in place of being seen, as in ordinary vision, at the distance of *three feet*, is now *suspended in the air, at the distance of six inches from the observer*. In maintaining this view of the wall, the eye will at first experience a disagreeable sensation; but after a few experiments the sensation will disappear, and the observer will contemplate the new picture with the same satisfaction

and absence of all strain as if he were looking directly at the wall itself; for there is a natural tendency in the eyes to unite two similar pictures, and to keep them united, provided they are not too distant.

When this picture is at first seized by the observer, he does not for a while decide upon its distance from himself. It sometimes appears to advance from the wall to its true position in the binocular centre, and when it has taken its place, it has a very extraordinary character:—the surface seems slightly convex towards the eye; it has a sort of silvery transparent aspect, and looks more beautiful than the real paper; it moves with the slightest motion of the head, either laterally or to or from the wall. If the observer, who is now *three* feet from the wall, retires from it, the suspended wall of flowers will follow him, moving further and further from the real wall, and also, but very slightly, further and further from the observer; that is, the distance of the observer from the real wall increases faster than the distance of the suspended wall from it, according to the law expressed by the preceding formula. The binocular centre, therefore, recedes from the eye as the observer retires, and the strain consequently diminishes.

In order to observe these phænomena in the most perfect manner, the paper should be pasted upon a large screen, previously unseen by the observer, unconnected with the roof or the floor, and placed in a large apartment. The deception will then be complete; and when the picture stands suspended before the observer, and within a few inches of himself, he may stretch out his hand and place it on the other side of the picture, and even hold a candle on the other side of it, so as to satisfy himself that in both cases the picture is between his hand and himself.

When we survey this picture with attention, several very curious phænomena present themselves. Some of the flowers, when narrowly examined, appear somewhat like real flowers. In some the stalk gradually retires from the general plane of the picture; in others it rises above it: one leaf will come further out than another, or the flower will appear thicker and more solid, deviating considerably from the plane representation of it seen by each eye separately. All this arises from slight and accidental irregularities in the two figures which are united, thus producing an approximation to three dimensions in the picture. If the distance, for example, of the ends of two stalks in two coalescing flowers is greater than the distance of corresponding points in other parts of the stalk, the end of the stalk will rise from the general surface of the figure, and *vice versâ*. In like manner, if the distance between

two corresponding leaves is greater than the distance between other two corresponding leaves, then the two first, when united, will appear nearer the eye than the other two, and hence the appearance of a solid flower is partially given to the combination. These effects are better seen in old and imperfectly made paper-hangings, than in those which are more carefully executed.

In continuing our survey of the suspended image, another curious phænomenon presents itself: a part of one of the pieces of paper, and sometimes a whole stripe from the roof to the floor, will retire behind the general plane of the image, or rise above it; thus displaying, on a large scale, an imperfection in the workmanship which it would have required a very narrow inspection to discover. This defect arises from the paper-hanger having cut off too much of the white margin of one or more of the adjoining pieces, so that when the two halves of a flower are united, part of the middle of the flower is left out; and hence when this defective flower is united with the one on the right-hand of it, and the one on the left-hand united with the defective one, the united or corresponding portion, being at a less distance, will appear further from the eye than those parts of the suspended image composed of complete flowers. In like manner, if the two portions of the flowers are not brought together, but separated by a small space, the opposite effect will be produced. This will be understood from fig. 1, Plate VI., where M N, O P represent portions of two separate pieces of paper, each twenty-one inches wide. In this specimen, there are only two flowers in each piece, namely one white flower, A or B, and two halves. If the two halves C, D, are united as in the figure, it is obvious that the flower is incomplete, a part of the central circle of the corolla having been cut off from each half. If we now, by straining the eye, unite C D with B, and also with A, then at the same time E will be united with the second or left-hand image of A, and G with the second or right-hand image of B. But since a piece has been cut out of C D, the half $\alpha\alpha$ of A is nearer the half D D than the other half aa is to the other half C C; and, in like manner, the half bb of B is nearer the half C C than the other half $\beta\beta$ is to the other half D D. Hence, when the strained eyes unite $\alpha\alpha$ to D D, the binocular centre is more remote than when aa is united to C, and the same is true of the other halves; consequently, the halves D D and bb must appear, as it were, sunk in the wall, or as further removed from the observer; and if the defective cutting exists along the line R S from the floor to the ceiling, the whole stripe of paper between R S and O P, from the

floor to the ceiling, will appear sunk in the papered wall. But if the defect is confined to a portion only of the flowers, then a rectangular space of the breadth $R O$, and of a height equal to the defective portion, will appear sunk in the paper. If every junction has the same defect as that at $R S$, then the whole will appear to consist of equal stripes, every alternate one being raised and the other depressed.

In the preceding example, there are only *two* flowers in a breadth, and their distance is $10\frac{1}{2}$ inches, which is also the breadth of the sunk stripes. But if the flowers are three or four in number, and their distance $\frac{21}{3}$, $\frac{21}{4}$ inches, the sunk stripes will vary according as we unite two flowers whose distances are in the one case 7 or 14 inches, and $5\frac{1}{4}$ or $10\frac{1}{2}$ or $16\frac{3}{4}$ or 21 in the other. Calling B the breadth of the paper, n the number of flowers or figures in that breadth, and W the width of the sunk stripe, then we have $W = \frac{B}{n}$ or $\frac{2B}{n}$ or $\frac{3B}{n}$ according as we unite the two nearest, or the first and second flower, the first and third, or the first and fourth. When $W = B$, the sunk stripes will cover the whole paper, and all the flowers will lie in the same plane.

These results afford an accurate method of examining and discovering defects in the workmanship of paper-hangers, carpet-makers, painters, and other artists whose profession it is to combine a series of similar patterns in order to form a uniform and ornamental surface. The smallest defect in the similarity and equality in the figures or lines which compose a pattern, and any difference in the distance of the single figures, is instantly detected; and what is remarkable, a small inequality of distance in a line perpendicular to the axis of vision, or in one dimension of space, is exhibited in a magnified form as a distance coincident with the axis of vision, and in an opposite dimension of space!

At the commencement of this class of experiments, it is difficult to realize, and very easy to dissolve, the singular binocular picture which we have been describing; but after the eyes have been drilled for a while to this species of exercise, the pictures become very persistent. Although the air-suspended image might be expected to disappear after closing one eye, and still more after having closed and re-opened both, yet I have found it in its original position in this latter case, and even after rubbing my eyes and shaking my head; and I have sometimes experienced a difficulty in ascertaining,

after these operations, whether it was the real or the air-suspended wall that was before me. On some occasions a singular effect was produced. When the flowers on the paper are distant six inches, we may either unite two six inches distant, or two twelve inches distant. In the latter case, when the eyes have been accustomed to survey the suspended picture, I have found that, after shutting and opening them, I neither saw the picture formed by the two flowers twelve inches distant, nor the papered wall itself, but a picture formed by uniting the flowers six inches distant! The binocular centre had shifted its place; and instead of advancing to the wall, as is generally the case, and giving us ordinary vision of it, it advanced exactly as much as to unite the nearest flowers, just as on a ratchet wheel the detent slips over one tooth at a time; or, to speak more correctly, the binocular centre advanced in order to relieve the eyes from their strain, and when the eyes were opened, it had just reached that point which corresponded with the union of the flowers six inches distant.

In the construction of complex geometrical diagrams consisting only of fine lines, and in which similar figures are repeated at equal distances, it is very difficult to attain minute accuracy. The points of the compasses sink to different depths in the paper, and the lines which join such points seldom pass through their centres. Hence arises a general inaccuracy which the eye cannot detect; but if we examine such diagrams by strained binocular vision, their imperfections will be instantly displayed. Some parts will rise higher than others above the general level, and the whole will appear like several cobwebs placed at the distance of a tenth or a twelfth of an inch behind each other*.

In all the experiments made by Mr. Wheatstone by the stereoscope, and in those described in my former paper, the dissimilar figures are viewed in a direction perpendicular to the plane on which they are drawn. A series of very interesting results however are obtained by uniting the images of lines meeting at an angular point, when the eye is placed at different heights above the plane of the paper, and at different distances from the angular point.

Let AC , BC be two lines meeting at C , the plane passing through them being the plane of the paper, and let them be viewed by the eyes at E''' , E'' , E' , E at different heights in a plane GMN perpendicular to the plane of the paper. Let R be the right eye and L the left eye, and when at E''' let them

* This effect is finely seen in the diagram of the Homogeneous Curve, which forms plate 9. of Mr. Hay's work "On the Harmony of Form."

be strained so as to unite the points A, B. The united image of these points will be seen at the binocular centre D''' , and the united lines AC, BC will have the position $D'''C$. In like manner, when the eye descends to E''' , E' , E, the united image $D'''C$ will rise and diminish, taking the positions $D''C$, $D'C$, DC till it disappears on the line CM, when the eyes reach M. If the eye deviates from the vertical plane GMN the united image will also deviate from it, and is always in a plane passing through the eye and the line GM.

If at any altitude EM the eye advances towards ACB in the line EG, the binocular centre D will also advance towards ACB in the line EG, and the image DC will rise and become shorter as its extremity D moves along DG, and after passing the perpendicular to GE it will increase in length. If the eye, on the other hand, recedes from ACB in the line GE, the binocular centre D will also recede, and the image DC will descend to the plane CM and increase in length.

The preceding diagram is, for the purpose of illustration, drawn in a sort of perspective, and therefore does not give the true positions and lengths of the united images. This defect however is remedied in fig. 3, where E, E' , E'' , E''' is the middle point between the two eyes, the plane GMN being, as before, perpendicular to the plane passing through ACB. Now, as the distance of the eye from G is supposed to be the same, and as AB is invariable as well as the distance between the eyes, the distance of the binocular centres O, D, D' , D'' , D''' , P, from G will also be invariable, and lie in a circle ODP whose centre is G, and whose radius is GO, the point O being determined by the formula $GO = GD = \frac{GM \times AB}{AB + RL}$. Hence, in order to find the binocular centres D, D' , D'' , D''' , &c. at any altitude E, E' , &c., we have only to join EG, $E'G$, &c. and the points of intersection D, D' , &c. will be the binocular centres, and the lines DC, $D'C$, &c. drawn to C, will be the real lengths and inclinations of the united images of the lines AC, BC.

When GO is greater than GC there is obviously some angle Λ , or $E''GM$ at which $D''C$ is perpendicular to GC. This takes place when $\cos \Lambda = \frac{GC}{GO}$. When O coincides with C, the images CD, CD' , &c. will have the same positions and magnitudes as the chords of the altitudes Λ of the eyes above the plane GC. In this case, the raised or united images will just reach the perpendicular when the eye is in the plane GCM, for since $GC = GO$, $\cos \Lambda = 1$, and $\Lambda = 0^\circ$.

When the eye at any position, E'' for example, sees the

points A and B united at D'' , it sees also the whole lines AC, BC forming the image $D''C$. The binocular centre must therefore run rapidly along the line $D''C$; that is, the inclination of the optic axis must gradually diminish till the binocular centre reaches C, when all strain is removed. The vision of the image $D''C$, however, is carried on so rapidly, that the binocular centre returns to D'' without the eye being sensible of the removal and resumption of the strain which is required in maintaining a view of the united image $D''C$.

If we now suppose A B to diminish, the binocular centre will advance towards G, and the length and inclination of the united images D C, $D' C$, &c. will diminish also, and *vice versa*. If the distance R L (fig. 2) between the eyes diminishes, the binocular centre will retire towards E, and the length and inclination of the images will increase. Hence persons with eyes more or less distant will see the united images in different places and of different sizes, though the quantities A and A B be invariable.

While the eyes at E'' are running along the lines A C, B C, let us suppose them to rest upon the points a, b equidistant from C. Join $a b$, and from the point g , where $a b$ intersects G C, draw the line $g E''$, and find the point d'' from the formula $g d'' = \frac{g E'' \times ab}{ab + RL}$. Hence the two points $a b$ will be united at d'' , and when the angle $E'' G C$ is such that the line joining D and C is perpendicular to G C, the line joining $d'' C$ will also be perpendicular to G C, the loci of the points $D'' d'' d' d$ will be in that perpendicular, and the image D C, seen by successive movements of the binocular centre from D'' to C, will be a straight line.

In the preceding observations we have supposed that the binocular centre D'' , &c. is between the eye and the lines A C, B C; but the points A, C, and all the other points of these lines, may be united by fixing the binocular centre beyond A B. Let the eyes, for example, be at E'' ; then if we unite A B when the eyes converge to a point, Δ'' (not seen in the figure), beyond G, we shall have $G \Delta'' = \frac{GE \times AB}{RL - AB}$;

and if we join the point Δ'' thus found and C, the line $\Delta' C$ will be the united image of A C and B C, the binocular centre ranging from Δ'' to C, in order to see it as one line. In like manner, we may find the position and length of the image $\Delta''' C$, $\Delta' C$, and ΔC corresponding to the position of the eyes at $E''' E$ and E. Hence all the united images of A C, B C, viz. C Δ''' , C Δ'' , &c., will lie below the plane of A B C, and extend beyond a vertical line N B continued; and they will

grow larger and larger, and approximate in direction to C G as the eyes descend from E''' to M. When the eyes are near to M, and a little above the plane of A B C, the line, when not carefully observed, will have the appearance of coinciding with C G, but stretching a great way beyond G. This extreme case represents the celebrated experiment with the compasses, described by Dr. Smith, and referred to by Professor Wheatstone. He took a pair of compasses, which may be represented by A C B, A B being their points, A C B C their legs, and C their joint; and having placed his eyes about E above their plane, he made the following experiment:—"Having opened the points of a pair of compasses somewhat wider than the interval of your eyes, with your arm extended, hold the head or joint in the ball of your hand with the points outwards, and equidistant from your eyes, and somewhat higher than the joint. Then, *fixing your eyes upon any remote object* lying in the plane that bisects the interval of the points, you will first perceive two pair of compasses (each by being doubled with their inner legs crossing each other, not unlike the old shape of the letter W). But by compressing the legs with your hand, the two inner points will come nearer to each other; and when they unite (having stopped the compression), the two inner legs will also entirely coincide and bisect the angle under the outward ones, and will appear more vivid, thicker and larger, than they do, so as to reach from your hand to the remotest object in view even in the horizon itself, if the points be exactly coincident*." Owing to his imperfect apprehension of the nature of this phænomenon, Dr. Smith has omitted to notice that the united legs of the compasses lie below the plane of A B C, and that they never can extend further than the binocular centre at which their points A and B are united.

There is another variation of these experiments which possesses some interest, in consequence of its extreme case having been made the basis of a new theory of visible direction by the late Dr. Wells†. Let us suppose the eyes of the observer to advance from E to N, and to descend along the opposite quadrant on the left-hand of N G, but not drawn in fig. 3, then the united image of A C, B C will gradually descend towards C G, and become larger and larger. When the eyes are a very little above the plane of A B C, and so far to the left-hand of A B that C A points nearly to the left eye and C B to the right eye, then we have the circumstances under which Dr. Wells made the following experiment:—"If we

* Smith's Optics, vol. ii. p. 388, § 977.

† Essay on Single Vision, &c., p. 44.

hold two thin rules in such a manner that their sharp edges (A C, B C in fig. 3) shall be in the optic axes, one in each, or rather a little below them, *the two edges will be seen united in the common axis* (G C in fig. 3); and this apparent edge will seem of the same length with that of either of the real edges, when seen alone by the eye in the axis of which it is placed." This experiment, it will be seen, is the same with that of Dr. Smith, with this difference only, that the points of the compasses are directed towards the eyes. Like Dr. Smith, he has omitted to notice that the united image rises above G H, and he commits the opposite error of Dr. Smith, in making the length of the united image too short.

If in this form of the experiment we fix the binocular centre beyond C, then the united images of A C, B C descend below G C, and vary in their length and in their inclination to G C, according to the height of the eye above the plane of A B C, and its distance from A B.

It is a remarkable circumstance, that no examples have been recorded of false estimates of the distance of near objects, in consequence of the *accidental* binocular union of similar images. This has, no doubt, arisen from the rare occurrence of these circumstances or conditions, under which alone such illusions can be produced. In a room where the paper-hangings have a small pattern, or similar figures recurring at the distance of 1, $1\frac{1}{2}$, or 2 inches, a short-sighted person might very readily turn his eyes on the wall, when their axes converged to some point between him and the wall, which would unite one pair of the similar images; and in this case he would see the wall nearer him than the real wall, and moving with the motion of his head like something aerial. In like manner, a long-sighted person, with his optical axes converged to a point beyond the wall, might see an image of the wall more distant, and of an aerial character:—or a person who has taken too much wine, which often fixes the optical axes in opposition to the will, might, according to the nature of his sight, witness either of the illusions above-mentioned.

In the preceding observations, we have confined ourselves to the binocular union of figures upon an opaque ground. This limitation almost necessarily precluded us from observing the results when the binocular centre is beyond the plane where these figures are situated, because it is not easy to adjust the eyes to a distant object, unless we look through the surfaces containing the figures. Now this is by far the most interesting form of the experiment, and it has the advantage of putting scarcely any strain upon the eyes, not only because the binocular centre is more distant, but because we cannot, in this

way, unite figures whose distance exceeds $2\frac{1}{2}$ inches, the interval between the eyes. Transparent patterns for these experiments may be cut out of stiff card-paper, or thin plates of metal, or they may be made of paper pasted upon large panes of glass. Experiments may be made with trellis-work, or with windows composed of small squares or lozenges; but the readiest pattern is the cane bottom of a chair, and I have performed my experiments by simply placing such a chair upon a high table, with its cane bottom in a vertical position. The distance of the centres of the eight-sided open figures in the direction of the width or depth of the chair, varies in different patterns from 0.54 to 0.76 of an inch. In order to simplify the calculations, we shall take the distance at 0.5, or half an inch. Then let

$D=12$ inches be the distance of the pattern from the eyes.

$d=0.5$ the distance of the centres of the similar figures.

$+\Delta$ =distance of suspended image from, and in front of, the pattern.

$-\Delta'$ =distance of suspended image from, and behind, the pattern.

$C=2.5$ the distance between the eyes.

Then we shall have

$$+\Delta = \frac{Dd}{C+d} \text{ and } -\Delta' = \frac{Dd}{C-d}. \text{ Hence}$$

$D-\Delta$ =distance of suspended image from the eye, and in front of the pattern, and

$D+\Delta'$ =its distance from the eye, and behind the pattern.

From these formulæ we have computed the following table, adapted to similar figures, whose centres are distant $\frac{1}{2}$ an inch, 1, $1\frac{1}{2}$, 2 and $2\frac{1}{2}$ inches; but in reference to the positive values of Δ and D , we may consider them as feet, 0.5 being in that case =6 inches.

D Inches.	$d=0.5.$		$d=1.0.$		$d=1.5.$		$d=2.0.$		$d=2.4.$		$d=2.5.$	
	$+\Delta.$	$-\Delta.$										
6	1	1.5	1.72	4	2.25	9	2.66	24	2.94	144	3	Infin.
12	2	3	3.43	...	4.50	18	5.33	48	5.88	288	6	Infin.
24	4	6	6.86	16	9	36	10.66	96	11.76	576	12	Infin.
48	8	12	13.7	32	18	72	21.33	192	23.52	1152	24	Infin.

Taking the case where D is 12 inches, and uniting the two nearest openings where d is 0.5, let $M N$ (fig. 4) be a section of the transparent pattern, L, R the left and right eyes, Lad, Lbe lines drawn through the centres of two of the open figures ab , and Rbd, Rce lines drawn through the centre of b and c ,

and meeting Lad , Lbe at d and e , d being the binocular centre when we look at it through a and b , and e the binocular centre when we look at it through b and c . Now, the right eye R sees the opening b at d , and the left eye L sees the opening a at d , hence the image at d consists of the similar images of a and b united. In like manner e consists of b and c united, and so on with all the rest, so that the observer at LR no longer sees the real pattern MN , but a suspended image of it at mn , three inches behind MN . If the observer now approaches MN , the image mn will approach to him, and if he recedes, mn will recede, being $1\frac{1}{2}$ inch distant from MN when the observer is six inches from MN , and twelve inches from MN when he is forty-eight inches from MN , the image mn moving from MN with a velocity one-fourth of that with which the observer recedes. These two velocities are in the ratio of D to $\frac{Dd}{C-d}$.

Resuming the position in the figure where the observer is twelve inches distant from MN , let us consider the important results to which this experiment cannot fail to lead us. If the observer, with his eyes at LR , grasp the cane bottom or pattern at MN , as shown in fig. 4, his thumbs pressing upon MN , and his fingers trying to grasp mn , he will then *feel what he does not see*, and *see what he does not feel*! The real pattern is absolutely invisible at MN , and stands fixed at mn . The fingers may be passed through and through—now seen on this side of it, now in the middle of it, and now on the other side of it. If we next place the palms of each hand upon MN , feeling it all over, the result will be the same. No knowledge derived from touch—no measurement of real distances—no actual demonstration from previous or subsequent vision, that there is a real solid body at MN , and nothing at all at mn , will remove or shake the infallible conviction of the sense of sight that the object is at mn , and that dL or dR is its real distance from the observer. If the binocular centre be now drawn back to MN , the image *seen* will disappear, and the real object be seen at MN . If it be brought still further back to f , the object MN will again disappear, and will be seen at $\mu\nu$, as described in a former part of this paper.

In making these experiments, the observer cannot fail to be struck with the remarkable fact, that though the openings at MN , mn , and $\mu\nu$ have all the same angular magnitude, that is, subtend the same angle at the eye, viz. dLe , dRe , yet those at mn appear larger than those at MN , and those at $\mu\nu$ smaller. If we cause the image mn to recede and $\mu\nu$ to approach, the figures in mn will invariably *increase* as they

recede, and those in $\mu\nu$ will *diminish* as they *approach* the eye, and their *visual magnitudes*, as we shall call them, will depend on the respective distances at which the observer, whether right or wrong in his estimate, conceives them to be placed.

Now this is a universal fact, which the preceding experiments demonstrate; and though the estimate of magnitude thus formed is an erroneous one, yet it is one which neither reason nor experience is able to correct.

When we look at two equal lines, whose difference of distance is distinctly appreciable by the eye, either directly or by inference, but whose difference of angular magnitude is not appreciable, the most remote must necessarily appear the smallest. For the same reason, if the remoter of two lines is really smaller than the nearer, and therefore its angular magnitude also smaller from both these causes, yet, even in this case, if the eye does not perceive distinctly the difference, the smaller and more remote line will appear the larger*.

The law of visual magnitude, which regulates this class of phænomena, may be thus expressed.

If we call A the angular magnitude of the *nearest* of two lines or magnitudes whose apparent distance is d , a the angular magnitude of the remoter line, whose apparent distance is D , and V , v the visual magnitudes of the two lines, then

$$V : v = A \times D : a \times D.$$

Now let the two lines MO , NP be the two sides of a quadrilateral figure seen obliquely by an eye at E , then, if the apparent distances of MO , NP are such, that

$$A \times d > a \times D, \text{ then } V > v,$$

and the lines MN , OP will converge to a vanishing point beyond NP . But if

$$A \times d = a \times D, \text{ then } V = v,$$

and the line MN , OP will appear to be parallel. And if

$$A \times d < a \times D, \text{ then } V < v,$$

* Malebranche seems to have been the first who introduced the *apparent* distance of objects as an element in our estimate of *apparent* magnitude. *De la Recherche de la Verité*, tom. i. liv. i.; tom. iii. p. 354. See also Bouguer, *Mém. Acad. Par.* 1755, p. 99. These views however have been abandoned by several subsequent writers, and the *real* distance of objects has been substituted for their *apparent* distance. Varignon, *Mém. Acad. Par.* 1717, p. 88. M. Lehot, for example, says, "L'expression de la grandeur visuelle d'un corps est égale à la grandeur réelle, multipliée par le logarithme de la *distance réelle* divisée par cette distance."—*Nouvelle Théorie de la Vision*, 1er Mém. Suppl. p. 7, 8. Paris, 1823. This estimate of distance is incompatible with experiment and observation.

and the lines MN, OP will converge to a vanishing point between MO and the observer.

These results may be considered as laying the foundation of a new art, to which we may give the name of Visual Perspective, in contradistinction to Geometrical Perspective. This art furnishes us with an immediate explanation of a great variety of optical illusions which have never yet been explained; and there is reason to believe that some of its principles were known to ancient architects, and even employed in modifying the nature and position of the lines and forms which enter into the construction of their finest edifices.

St. Leonard's College, St. Andrews,
April 10, 1844.

Appendix.

When I wrote the paragraph in page 314, I had no expectation of learning that any example of such an illusion had ever occurred. A friend, however, to whom I had occasion to show the experiments, and who is short-sighted, mentioned to me that he had been on two occasions greatly perplexed by the vision of these suspended images. Having taken too much wine, and being in a papered room, he saw the wall suspended near him in the air; and on another occasion, when kneeling and resting his arms on a cane-bottomed chair, he had fixed his eyes on the carpet, which accidentally united the two images of the open-work, and threw the suspended image of the chair-bottom to a distance, and beyond the plane on which his arms rested.

The following case, communicated to me by Professor Christison, is still more interesting. "Some years ago, when I resided in a house where several rooms are papered with rather formally recurring patterns, and one, in particular, with stars only, I used occasionally to be much plagued with the wall suddenly standing out upon me, and waving, as you describe, with the movements of the head. I was sensible that the cause was an error as to the point of union of the visual axes of the two eyes; but I remember it sometimes cost me a considerable effort to rectify the error; and I found that the best way was to increase still more the deviation in the first instance. As this accident occurred most frequently while I was recovering from a severe attack of fever, I thought my near-sighted eyes were threatened with some new mischief; and this opinion was justified in finding that, after removal to my present house—where, however, the papers have no very formal pattern—no such occurrence has ever taken place. The reason is now easily understood from your researches."

XLIX. *On the Universality of Magnetism.*

By Dr. DE HALDAT*.

THE question of the universality of magnetism, on which M. de Haldat presented a memoir to the Academy of Sciences in 1841 †, having given rise to some objections, has been subjected to new researches, in which the author has confirmed the fact, that all bodies in small masses and of an elongated form are subject to the influence of the magnet when exposed to its action, both when they are made to oscillate between the opposite magnetic poles, and when they are suspended in the same situation by silk filaments whose torsion is employed to value the force which they have acquired. A desire to trace the proximate cause of these phænomena induced the author to ascertain whether these bodies possess, of themselves, the property of acquiring the magnetic state, or whether they owe this property to the presence of iron, as many philosophers have maintained.

If we consider magnetism in a general point of view, we shall find that, to attribute this property to iron alone, is to assign to that substance a special virtue, which later researches have shown to exist in two other metals, and which the attraction of the magnetic needle by the rotatory discs composed of various metals contradicts in the most direct manner; and we must acknowledge that, in short, to deny to the fluid, the magnetic agent, the generality of influence which we are forced to recognise in the other agents, or imponderable fluids, as heat, light and electricity, is to contravene the law of analogy, which is one of our surest guides in the study of nature. How, in fact, can we suppose that an agent which, like caloric, but with an infinitely greater velocity, penetrates all bodies, can be deprived of the property of exercising upon them an influence analogous to that exerted by the other agents with which it has such analogy? How can we admit, not only a great resemblance but even an identity recognised by a great number of philosophers between electricity and magnetism, and deny the existence in one of its agents of that which we attribute to the other? Finally, if all the agents, as is generally believed, are only different modifications of one universal agent, how can we deny to magnetism the generality of influence attributed to the fluid, the universal agent, of which it would be, so to speak, one of the factors?

These reasonings, conformable to the principles of natural philosophy, and sufficient perhaps for philosophers who restrict

* From the *Annales de Chimie et de Physique*, Jan. 1847.† *Comptes Rendus des Séances de l'Acad. des Sciences*, t. xii, p. 950.

themselves to general considerations, being unable alone to solve the objections proposed by the partisans of special magnetism, the author had recourse to experiments directed according to the following considerations. If the property of acquiring the magnetic state belongs only to one body, the partisans of this special virtue must prove,—1st, that this metal exists in all bodies presenting magnetic phænomena; 2nd, that all bodies contain it, and in sufficient quantity to satisfy the magnetic phænomena developed in the experiments of this class; 3rd, that, whatever be the state of the metal, pure or combined with different bodies, it always preserves its characteristic property. The answers to these questions are given in an article specially devoted to an investigation of iron, and an explanation of the processes adapted to detect it in the various substances in which it exists, either in a state of simple mixture or of combination. And as excessively weak quantities are able to communicate the magnetic power to the bodies which would contain it, it was indispensable to adopt a process capable of indicating the minutest quantities. The characteristic precipitate of its solutions by the cyanide of potassium furnishing a simple and efficacious means of detecting this metal, the author adopted it, after having convinced himself that in forming, with the precipitate diluted with water, a transparent column two centimetres (nearly 0·8 of an inch) in length, the experimenter can, on receiving a ray of white light, render perceptible at least a five-thousandth of the quantity of iron in the substances subjected to the magnetic experiment. At the same time all the agents adapted to render the test more sensitive were had recourse to, and each analysis checked by means of a test liquor composed of persulphate of iron, the amount of which being excessively small, proved that if this metal had existed in the composition submitted to examination, it would necessarily have been detected.

Sixty substances deprived of iron by the usual means, and found to be chemically pure, were thus prepared and subjected to magnetic action, after having received the elongated form which ensures the success of these experiments. Among these substances are the following, whose absolute purity must be theoretically admitted, and which has moreover been proved by experiment: distilled water in the state of ice, hyaline quartz of perfect transparency, the carbonate and muriate of ammonia obtained by the combination of their component principles in a state of vapour, the carbon of the smoke of oil-lamps or pure resins, sulphur washed with hydrochloric acid and repeatedly sublimed, camphor several times sublimed,

gum, bees wax, &c.; we may also add gold obtained from deposit, silver reduced from the chloride, and copper precipitated by galvanism. Each of these substances was placed upon a sheet of very thin paper, previously purified and suspended by a double filament of silk, and submitted to the influence of the two poles of a horse-shoe iron magnet carrying twenty-five kilogrammes. With respect to their behaviour under the influence of this double power, they are divided into two classes, conformably to the observation of Mr. Faraday, previously recognised by M. Becquerel*. One class was placed in the direction of the magnetic current, the other transversely to this current. There is therefore no substance absolutely neutral with regard to the property of acquiring the magnetic power; but whatever be the direction which it takes, this can always be ascertained, and the acquired power determined, even measured, by means of the parts of the apparatus which measures the torsion.

Reverting to the question of the universality of magnetism and the speciality of the iron which it repels, the author has discussed the objections raised against the distinction of the two modes of magnetization, which, with the fact of the polarization of the luminous ray under the influence of the magnetic current, has strongly confirmed a theory already based on so many remarkable analogies and numerous experiments. He has refuted the fact of the variations in the direction of needles successively reduced to disproportionate lengths, which have been assigned to them from the consideration of the powerlessness of the most energetic magnets to act otherwise than upon the centre of the needles, and consequently to give them other than a transverse direction.

In rejecting the magnetic speciality of iron, the author has not denied the very remarkable pre-eminence of this metal with regard to the property of acquiring the magnetic power, which it always manifests with great energy, even when it only exists in an excessively small mass. An entire article is devoted to the valuation of the variations of which this arrangement is susceptible by the combination of oxygen, of several other substances, and above all by the passage of the metal to the saline state in which it is generally diamagnetic, even when the components of the salt are only united by a weak affinity. In this examination it has been found, that a quantity of iron inappreciable by the most delicate balance might be detected by the magnetic process, and even estimated approximately; that, however, this power diminishing with the quantity of the metal which puts it in play, it necessarily finds its limit in the

* *Annales de Chimie et de Physique*, 2nd series, t. xxxvi. p. 337.

extreme attenuation of which it is susceptible; and that, whatever be the quantity of iron contained in a body, it is appreciable by the magnetic process as much as it is by chemical means.

From general considerations, and from very numerous facts stated in this memoir, and from numerous others for which there was no room, it results:—

1. That iron, although eminently magnetic, is not the only body which possesses this property.

2. That its tendency to acquire this state is dependent upon its purity, and varies with its combination.

3. Lastly, that the magnetic power acting upon all bodies, either giving them a direction parallel to the current or transverse to this current, the fluid, the magnetic agent, possesses, like all other imponderable fluids or agents, universality of influence in nature.

L. On the *Metaphosphates*.

By ROBERT MADDRELL, *Esq.**

HAVING had my attention drawn to a new method for preparing pure phosphoric acid from bone-ash, proposed by Professor Gregory, and a new and apparently anomalous phosphate of magnesia, accidentally discovered by him whilst trying to obtain the above-mentioned acid pure by heating at a temperature above 600° F.†, I was induced at the suggestion, and with the kind assistance of Professor von Liebig, to repeat the process, and also to prepare a quantity of the salt, and subject it to analysis. The general results I have found are as follows:—If phosphoric acid, freed from lime and sulphuric acid‡, be heated to a temperature above 600° F., it deposits a white substance, having all the general physical characters of the new salt, but in which I found on analysis 22·47 per cent. of magnesia, corresponding to the metaphosphate of magnesia ($MgO PO_5$). After a considerable quantity of this substance had been deposited, I digested the mass in water, filtered, and evaporated the solution, and again heated the acid to upwards of 600° F., under the impression that it might still contain the new compound; by continuing this heat for half an hour, I again obtained a quantity of meta-

* Communicated by the Chemical Society; having been read Dec. 7, 1846.

† See *Transactions of the Royal Society of Edinburgh*.

‡ See Geiger's *Handbuch der Pharmacie*.

phosphate of magnesia; and it was not until after a third repetition of this process that I obtained Professor Gregory's compound, in which I found on analysis 16·276 per cent. of magnesia, corresponding to the quantity found by that chemist in his salt; but on estimating the quantity of phosphoric acid present, I only obtained 75·25 per cent. of PO_5 , leaving a loss of upwards of 8 per cent. Being convinced that another base must be present, from the enormous loss, I proceeded further with my analysis and found the presence of soda, which gave me 8·146 per cent. of soda.

This salt therefore is not an anomalous phosphate of magnesia, but a double metaphosphate of magnesia and soda, in the proportions are 3 equivs. of metaphosphate of magnesia and 1 equiv. of metaphosphate of soda, $3(\text{MgO}, \text{PO}_5) + \text{NaO}, \text{PO}_5$. It is nearly insoluble in hydrochloric acid and aqua regia, but soluble in concentrated sulphuric acid.

The analysis of the salt was made as follows:—I dissolved the salt in pure concentrated sulphuric acid, then diluted the solution with water, and precipitated the magnesia by an excess of ammonia; from the precipitate, after being heated to redness, was estimated the quantity of magnesia. To the filtered solution was added a solution of a salt of magnesia (with proper precautions) to precipitate the remainder of the phosphoric acid, and from this precipitate, as well as the former, was estimated the quantity of the PO_5 . A second quantity was taken for the determination of the soda, which was dissolved in concentrated sulphuric acid, diluted with water, and precipitated by an excess of ammonia. The filtered solution, containing phosphoric acid, was then precipitated by acetate of lead, the excess of lead by sulphuretted hydrogen, and the soda in the solution evaporated and estimated as a sulphate, which gave me the following results:—

Analysis, No. I.

Per cent.

·897 grm. substance gave	·399 $2\text{MgO}, \text{PO}_5$	·146 $\text{MgO} = 16\cdot276 \text{ MgO}$
·897 " " "	1·065 " "	·675 $\text{PO}_5 = 75\cdot250 \text{ PO}_5$
·712 " " "	·133 NaO SO_3	·058 $\text{NaO} = 8\cdot146 \text{ NaO}$
		99·672

No. II.

Per cent.

1·16 grm. substance gave	·511 $2\text{MgO}, \text{PO}_5$	·187 $\text{MgO} = 16\cdot12 \text{ MgO}$
1·16 " " "	1·386 " "	·878 $\text{PO}_5 = 75\cdot689 \text{ PO}_5$
1·23 " " "	·228 NaO, SO_3	·099 $\text{NaO} = 8\cdot048 \text{ NaO}$
		99·857

Reckoned.	Atom.	Found.	
		I.	II.
16.368 =	3 MgO	16.276	16.12
75.376 =	4 PO ₅	75.250	75.689
8.256 =	NaO	8.146	8.018
<u>100.000</u>		<u>99.672</u>	<u>99.857</u>

Formula, 3(MgO, PO₅) + NaO, PO₅.

The different result obtained by Professor Gregory no doubt arose from the method upon which his examination of this salt was conducted, viz. by determining the amount of magnesia present, and considering the loss on the weight of substance used to represent phosphoric acid, and probably from not thinking at the time of the presence of a salt of soda in bones.

With regard to the phosphoric acid prepared as above, after the salt has ceased to form, I find that it is not yet quite pure, but contains traces of magnesia as well as soda, and therefore this process will always yield an acid containing more or less of these substances.

Under the impression that the phosphoric acid obtained by this process was pure, I attempted to prepare with it the metaphosphates of nickel and cobalt, by adding the sulphates of these bases to an excess of the acid, and heating as before to upwards of 600° F., with the idea that they might be deposited in a somewhat similar manner to the metaphosphate of magnesia. This I found on analysis not to be the case, as the salts which I obtained in this way were not the pure metaphosphates, but double salts in combination with soda, closely allied in their composition to Gregory's salt.

The nickel compound obtained in this way was deposited in the form of a greenish-yellow, anhydrous powder, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid.

The analysis of this salt was made by fusing with carbonate of soda, and treating the fused mass with water to separate the metallic oxide from the alkaline phosphate. A second quantity was taken for the determination of soda, which I dissolved in concentrated sulphuric acid, then diluted with water, and precipitated the solution containing phosphoric acid by acetate of lead, and the excess of lead in the filtered solution by sulphuretted hydrogen. To the filtered solution was then added an excess of ammonia with sulphuret of ammonium to precipitate the nickel, and the solution containing soda was evaporated and determined as a sulphate.

	Found.	Atom.	Reckoned.
	Per cent.		Per cent.
·853 grm. substance gave	·254 NiO	= 29·789	6NiO = 29·805
2·24	·231 NaO, SO ₃	= 4·062	NaO = 4·134
	PO ₅ estimated as loss	= 66·149	7PO ₅ = 66·061
		<u>100·000</u>	<u>100·000</u>

Formula, 6(NiO, PO₅) + NaO, PO₅.

The cobalt salt is a beautiful rose-coloured, anhydrous compound, insoluble in water and dilute acids, but soluble in concentrated sulphuric acid.

The analysis was made by dissolving it in concentrated sulphuric acid, diluting with water, and precipitating by an excess of ammonia and sulphuret of ammonium. The sulphuret was then redissolved in nitric acid, and the solution diluted with water, filtered and precipitated by caustic potash. A second quantity was taken for the determination of the soda, and estimated as the preceding salt of nickel.

	Found.	Atom.	Reckoned.
	Per cent.		Per cent.
1·24 grm. substance gave	·403 Co ₃ O ₄	= 37·55	CoO = 29·775
1·12	·117 NaO, SO ₃	= 4·358	NaO = 4·136
	PO ₅ estimated as loss	= 65·460	7PO ₅ = 66·089
		<u>100·000</u>	<u>100·000</u>

Formula, 6(CoO, PO₅) + NaO, PO₅.

Thinking I might still obtain the pure metaphosphates of nickel and cobalt in the way above mentioned, I prepared a quantity of pure phosphoric acid by the oxidation of phosphorus with nitric acid, and the process, when tried with the acid prepared in this way, perfectly succeeded.

By this method I obtained also the metaphosphates of MnO, Al₂O₃, Fe₂O₃, Cr₂O₃, CuO, BaO, SrO, CaO, KO, NaO, though not in every case using the sulphates of the bases, as I find that almost any other salt will do equally well.

Metaphosphate of Oxide of Nickel.—This salt I obtained on adding a solution of sulphate of nickel to an excess of pure phosphoric acid, and by evaporating the mixture in a platinum vessel until the sulphuric acid had been expelled, then by heating it to upwards of 600° F., when it deposited as a greenish-yellow, anhydrous compound. It is insoluble in water, hydrochloric acid and aqua regia, but soluble in concentrated sulphuric acid.

The determination of the nickel was made as the preceding salt of nickel, and the phosphoric acid estimated from the loss.

	Found.	Atom.	Reckoned.
	Per cent.		Per cent.
1·565 grm. substance gave	·537 NiO	= 34·401	NiO = 34·485
	PO ₅ as loss	= 65·599	PO ₅ = 65·515
		<u>100·000</u>	<u>100·000</u>

Formula, NiO, PO₅.

Metaphosphate of Oxide of Cobalt was obtained by the same process as the preceding salt of nickel. It is a beautiful rose-coloured, anhydrous compound, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid.

The analysis was made by dissolving in concentrated sulphuric acid, diluting with water, and precipitating by an excess of ammonia and sulphuret of ammonium. The sulphuret was redissolved in nitric acid, the solution diluted with water, filtered, and precipitated by caustic potash. The PO_5 was estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
·983 grm. substance gave ·367 $\text{Co}_3\text{O}_4 = \cdot342$ $\text{CoO} =$	34·791	$\text{CoO} = 31\cdot452$
PO_5 as loss =	65·209	$\text{PO}_5 = 65\cdot548$
	<u>100·000</u>	<u>100·000</u>

Formula, CoO, PO_5 .

Metaphosphate of Protoxide of Manganese.—This salt was obtained in a similar manner to the preceding salts of nickel and cobalt. It is a white, anhydrous compound, insoluble in water and dilute acids, but soluble in concentrated sulphuric acid.

The determination of the manganese was made as in the preceding salt of cobalt, and the PO_5 estimated from the loss.

	Found. Per cent.	At. Reckoned. Per cent.
·857 grm. substance gave ·306 $\text{MnO}, \text{Mn}_2\text{O}_3 = \cdot2847$ $\text{MnO} =$	33·22	$\text{MnO} = 33\cdot321$
PO_5 as loss =	66·78	$\text{PO}_5 = 66\cdot679$
	<u>100·00</u>	<u>100·000</u>

Formula, MnO, PO_5 .

Metaphosphate of Alumina was obtained by dissolving alumina in an excess of phosphoric acid, evaporating, and heating up to the same temperature as with the preceding salts. It is a white, anhydrous compound, insoluble in water and concentrated acids.

The analysis for the determination of alumina was made, according to Berzelius's method, by fusing the salt with $1\frac{1}{2}$ part of silicic acid and 6 parts of carbonate of soda; and the PO_5 estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
1·253 grm. substance gave ·243 $\text{Al}_2\text{O}_3 =$	19·392	$\text{Al}_2\text{O}_3 = 19\cdot352$
PO_5 as loss =	80·607	$3\text{PO}_5 = 80\cdot648$
	<u>100·000</u>	<u>100·000</u>

Formula, $\text{Al}_2\text{O}_3, 3\text{PO}_5$.

Metaphosphate of Peroxide of Iron.—This salt was obtained on the addition of perchloride of iron to an excess of diluted phosphoric acid, and by evaporating and heating up to the same temperature as before; it is insoluble in water and diluted acid, but soluble in concentrated sulphuric acid.

The analysis was made by fusing the salt with carbonate of soda, then by treating the fused mass with water, to separate the peroxide of iron from the alkaline phosphate, and the phosphoric acid estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
·850 grm. substance gave ·224 Fe ₂ O ₃ =	26·353	Fe ₂ O ₃ = 26·767
PO ₅ as loss=	73·647	3PO ₅ = 73·233
	<u>100·000</u>	<u>100·000</u>

Formula, Fe₂O₃, 3PO₅.

Metaphosphate of Oxide of Chromium was obtained by dissolving oxide of chromium in an excess of diluted phosphoric acid, evaporating, and heating up to the same temperature as before. It is a beautiful green, anhydrous compound, insoluble in water and concentrated acids.

The analysis was made by fusing the salt with caustic potash; the fused mass was then digested in water (to separate the oxide of chromium from the alkaline phosphate), filtered, and the filtered solution neutralized with hydrochloric acid. To the solution was added ammonia, chloride of ammonium, and a salt of magnesia, to precipitate the phosphoric acid, and from the precipitate, after being heated to redness, the quantity of the latter was estimated. The Cr₂O₃ I was obliged to estimate as loss, as I found it exceedingly difficult to obtain a correct result, it being more or less oxidized by the action of the caustic potash.

	Found. Per cent.	Atom. Reckoned.
·67 grm. substance gave ·772 MgO, PO ₅ =·488	PO ₅ =72·836	3PO ₅ = 72·731
Cr ₂ O ₃ as loss=	27·164	Cr ₂ O ₃ =27·269
	<u>100·000</u>	<u>100·000</u>

Formula, Cr₂O₃, 3PO₅.

Metaphosphate of Oxide of Copper was obtained on adding a solution of nitrate of copper to an excess of diluted phosphoric acid, and evaporating and heating up to the same temperature as before. It is a bluish-white, anhydrous compound, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid. This salt I dissolved in concentrated sulphuric acid, diluted the solution with water, and precipitated by sulphuretted hydrogen; the sulphuret was then redissolved in hydrochloric acid, diluted with water, filtered, and precipitated by caustic potash, and the PO₅ estimated from the loss.

	Found.	Atom. Reckoned.
1·518 grm. substance gave ·542 CuO=	35·704	CuO= 35·713
PO ₅ as loss=	64·296	PO ₅ = 64·287
	<u>100·000</u>	<u>100·000</u>

Formula, CuO, PO₅.

Metaphosphate of Barytes was obtained by decomposing

carbonate of barytes in diluted phosphoric acid, then evaporating and heating it up to 600° F.; it is insoluble in water, hydrochloric acid and aqua regia, but decomposed by concentrated sulphuric acid. This salt was heated with concentrated sulphuric acid, digested in water, and filtered, and the PO₅ estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
1.236 grm. substance gave .974 BaO, SO ₃ = .639	BaO = 51.70	BaO = 51.746
	PO ₅ as loss = 48.30	PO ₅ = 48.254
	100.00	100.000

Formula, BaO, PO₅.

Metaphosphate of Strontian.—This compound was obtained by decomposing carbonate of strontian in diluted phosphoric acid, evaporating and heating as before; it is insoluble in water, hydrochloric acid and aqua regia, but decomposed by concentrated sulphuric acid.

This salt was heated with concentrated sulphuric acid, and then the mass digested in water, mixed with alcohol, and filtered. The PO₅ was estimated from the loss.

	Found. Per cent.	Atom. Reckoned.
1.014 grm. substance gave .755 SrO, SO ₃ = .425	SrO = 41.913	SrO = 42.043
	PO ₅ as loss = 58.087	PO ₅ = 57.957
	100.000	100.000

Formula, SrO, PO₅.

Metaphosphate of Lime.—This compound was obtained in a similar manner to the salts of barytes and strontian. It is a white, anhydrous powder, insoluble in water and diluted acids, but decomposed by concentrated sulphuric acid. The analysis of this salt was made the same as the salt of strontian.

	Found.	Atom. Reckoned.
.806 grm. substance gave .550 CaO, SO ₃ = .2281	CaO = 28.337	CaO = 28.52
	PO ₅ as loss = 71.663	PO ₅ = 71.48
	100.000	100.00

Formula, CaO, PO₅.

Metaphosphate of Magnesia was obtained by dissolving carbonate of magnesia in an excess of diluted phosphoric acid, evaporating, and heating as before; it may also be obtained by preparing phosphoric acid from bone-ash, according to the above-mentioned process. It is a white, anhydrous compound, insoluble in water and diluted acids, but soluble in concentrated sulphuric acid. This compound was dissolved in pure concentrated sulphuric acid, diluted with water, and precipitated by an excess of ammonia. The filtered solution containing the remainder of the phosphoric acid was precipi-

pitated (with the proper precautions) by a salt of magnesia and estimated accordingly.

gram.		Found.	Atom. Reckoned.
		Per cent.	
1.887	substance gave 1.157 2MgO, PO ₅ = .424	MgO = 22.47	MgO = 22.453
...	... 2.307 ... = 1.46	PO ₅ = 77.424	PO ₅ = 77.547
		99.894	100.000

Formula, MgO, PO₅.

Metaphosphate of Potash.—This salt was obtained by igniting strongly together equal parts of chlorate of potash and phosphoric acid (of a syrupy consistence), and by digesting the mass in water to separate the soluble part from the insoluble. It is an anhydrous compound, nearly insoluble in water, but soluble in diluted as well as concentrated acids. The acetic solution gives a white precipitate with nitrate of silver. This insoluble compound is decomposed on being treated with solutions of nitrate of silver, acetate of lead, and chloride of barium, into the metaphosphates of these metallic oxides.

This compound was dissolved in acetic acid, diluted with water, and the PO₅ in the solution precipitated by acetate of lead, and the excess of lead in the filtered solution by sulphuretted hydrogen. The solution containing potash was then evaporated, and the potash determined as nitrate.

	Found.	Atom. Reckoned.
1.468 gm. substance gave 1.25 KO, NO ₅ = .582	KO = 39.645	KO = 39.8
PO ₅ estimated as loss =	60.355	PO ₅ = 60.2
	100.000	100.0

Formula, KO, PO₅.

Metaphosphate of Soda was obtained by igniting strongly together equal parts of nitrate of potash and phosphoric acid (of a syrupy consistence), and by digesting in water to separate the soluble from the insoluble part. It is an anhydrous compound, nearly insoluble in water, but soluble in diluted and concentrated acids. The solution in acetic acid gives a white precipitate with nitrate of silver; it is also decomposed on being treated with the same salts as the salt of potash. This salt was dissolved in acetic acid, diluted with water, and the PO₅ precipitated in the solution (with proper precautions) by a salt of magnesia.

	Found.	Atom. Reckoned.
.943 gm. substance gave 1.042 MgO, PO ₅ = .659	PO ₅ = 69.883	PO ₅ = 69.537
NaO estimated as loss =	30.117	NaO = 30.463
	100.000	100.000

Formula, NaO, PO₅.

LI. *On the Amount of Sulphur and Phosphorus in various Agricultural Crops.* By HENRY CLIFTON SORBY, Esq.*

HAVING thought it highly probable that in the ordinary method of estimating the amount of sulphur and phosphorus in plants, viz. by burning them and analysing their ashes, there might be in many cases a loss from portions being rendered volatile by contact with combustible matter at a high temperature, I was induced to commence a series of analyses, employing a method against which there was no such objection, and, as will be seen, the quantities really present in various plants are much greater than has been hitherto anticipated. Having obtained a proper specimen, one portion of known weight was dried at 212° F. and weighed, and another in precisely the same condition was employed for the analysis, the amount which I generally used varying with the dryness of the substance from 200 grs. to 500 grs., which was cut into pieces if necessary, and introduced into a flask, and carefully heated with pure nitric acid, a little water being added if the substance was a dry one. No large quantity of the acid is required, and a gentle heat is kept up until the whole is digested down into a yellowish pulpy mass; water is then added, the whole boiled, and when cold filtered and washed, there being invariably a quantity of white fibrous substance left undissolved. To the yellowish solution thus obtained nitrate of baryta was next added, and though at first perhaps no precipitate was formed, sulphate of baryta was gradually deposited, and after standing for a day or so, the precipitate was collected on a filter, washed, dried, ignited and weighed, from which the amount of sulphur was calculated. It is well to ascertain whether it is pure sulphate of baryta, by adding to it after weighing a little hydrochloric acid, since sometimes there was present a little carbonate, which of course produces effervescence, and when that is the case, which seldom occurred to any but the most trifling extent, the amount is easily ascertained and a proper deduction made, to obtain the true weight of the sulphate alone.

To the solution filtered as above, I next added acetate of lead in moderate quantity, and then caustic ammonia in slight excess, filtered and washed the precipitate. When dry this precipitate was carefully calcined in a porcelain crucible over a lamp, at as low a temperature as convenient, stirring with a glass rod until the whole was converted into a mixture of metallic lead, oxide and phosphate of lead. These were then dissolved in nitric acid, and ammonia added until a consider-

* Communicated by the Chemical Society; having been read Dec. 7, 1846.

able precipitate of subnitrate was formed, when on adding acetic acid the whole of the phosphate alone was left insoluble, which, after standing awhile, was collected on a filter, washed, dried, ignited and weighed, and the amount of phosphorus obtained by calculation. That this method is correct, and that the phosphate which is thus obtained is $Pb_3P_2O_8$, I have proved by experiment.

It must be borne in mind that the amounts given below are those of sulphur and phosphorus, I having thought it right to give them as such, and not as sulphuric and phosphoric acids, since most certainly they do not exist invariably as such or as their salts in plants, and I therefore see no reason why they should not be given as elements, like the amounts of carbon, nitrogen, oxygen and hydrogen as usually given in such analyses.

The plants analysed, with only three exceptions, viz. the rye, mangel-wurzel, and hops, were obtained from the fields in which they were growing by myself, and hence I can vouch for their freedom from impurities, and have no doubt the others were as much so as possible.

Table of the Amounts of Sulphur and Phosphorus in various Agricultural Crops, calculated in 100 parts, dried at 212° F. until the weight remained constant.

Name of Plant, &c.	Sulphur.	Phosphorus.
Four species of grass (<i>Poa palustris</i> and <i>trivialis</i> , } <i>Festuca pratensis</i> , <i>Cynosurus cristatus</i>)	·165	·164
Rye grass (<i>Lolium perenne</i>)	·310	·183
Italian rye grass	·329	·145
Red clover (<i>Trifolium pratense</i>)	·107	·149
Ditto	·087	·131
Very fine white clover (<i>T. repens</i>)	·099	·183
Ordinary ditto	·151	·139
Very fine trefoil (<i>Medicago lupulina</i>)	·136	·052
Lucern (<i>M. sativa</i>)	·274	·046
Ditto	·452	·215
Ditto	·293	·353
Lints (<i>Vicia sativa</i>)	·178	·183
Kidney potatoes (<i>Solanum tuberosum</i>)	·094	·213
Ditto, tops	·389	·357
Ditto, fruit	·071	·597
American native potatoes	·082	·212
Ditto, tops	·206	·483
Carrot (<i>Daucus carota</i>)	·092	·255
Ditto, tops	·745	·382
Mangel-wurzel (<i>Beta altissima</i>)	·058	·190
Ditto, tops	·502	·293
Early yellow-top turnip (<i>Brassica rapa</i>)	·351	·352
Ditto	·421	·346
Ditto, tops	·758	·360
Ditto	·615	·380

Name of Plant, &c.	Sulphur.	Phosphorus.
Swede turnip (<i>Brassica oleracea</i>)	·435	·172
Ditto, tops	·458	·250
Rape (<i>B. oleifera</i>)	·448	·233
Drum-head cabbage (<i>B. campestris</i>)	·431	·267
Wheat plant entire (<i>Triticum vulgare</i>) when just gone } out of flower	·151	·248
Ditto	·170	·140
Ear of wheat when the grain was formed, but milky...	·075	·271
Straw from the above	·240	·132
Ear of wheat when ripe	·090	·336
Straw from the above	·213	·043
Red wheat	·070	·363
Ditto, straw	·293	·079
White wheat from the same field as the above red	·054	·366
Ditto, straw	·207	·112
Wheat (another sample)	·051	·410
Ditto, chaff	·091	·252
Very fine barley (<i>Hordeum distichum</i>)	·066	·498
Ditto, straw	·390	·087
Rather poor barley	·040	·367
Ditto, straw	·191	·065
Barley plant when in flower	·313	·236
Oat plant (<i>Avena sativa</i>) when just coming into flower	·226	·194
Ditto in flower	·189	·189
Green oats	·125	·317
Straw of ditto	·329	·128
Black tartarian oats	·080	·381
Straw of ditto	·271	·110
White oats	·090	·334
Straw of ditto	·401	·153
White oats (another sample)	·074	·382
Straw of ditto	·195	·057
Rye ears (<i>Secale cereale</i>) when young	·073	·076
Straw of ditto	·099	·153
Rye	·051	·160
Bean plant (<i>Vicia faba</i>) in flower	·045	·258
Beans	·071	·600
Ditto, straw	·148	·233
Peas (<i>Pisum sativum</i>)	·158	·206
Ditto, straw	·214	·076
Fine hops (<i>Humulus lupulus</i>)	·127	·574
Bind of ditto	·091	·138

From the above analyses it will be seen how very much the amounts vary in some cases, whereas in others they are pretty constant. This variation may probably be occasioned by that of the amount of phosphates and sulphates in the soils. I have observed as a general rule, that the finer the quality of the specimen the larger the proportions of sulphur and phosphorus present; though in the case of lucern, in which the phosphorus varies as much as in any crop of which I have analysed a sufficient number of specimens to be able to give an opinion, all the samples were growing luxuriantly. It may be presumed that when the amounts vary so much,

without apparently affecting the growth of the plant, the particular constituent so varying is not of such importance for the plant as when more constant.

It will be observed that in many plants different parts require different constituents, which indeed is well known; as for instance in the case of wheat it is probable that an abundance of sulphates in the soil would produce fine straw, and of phosphates, fine grain, and the converse, other necessary constituents being present.

It may also, perhaps, be inferred from these analyses that when grain is growing and ripening the amount of phosphorus increases in greater proportion than the total weight, and diminishes in the straw, the grain abstracting that constituent from it. Similar inferences may perhaps be derived from the above, many of which are given for such purposes.

The table given below is a practical application of these analyses, being intended to show the quantities of gypsum and bone-phosphate of lime represented by the sulphur and phosphorus which are removed annually from an acre of land by the respective crops; being also that which should be added to already fertile land to maintain it in that condition. No allowance for what would be removed by drainage is made, as that is too uncertain in its quantity, and cannot in many cases be at all material, or else uncultivated natural lands would long ago have become sterile. The quantities so removed are no doubt compensated for by the disintegration of the substrata, and the superficial removal of exhausted soil by washing.

In such a calculation as this, however, it is affectation to pretend to great exactness, the elements which unavoidably enter into it being so variable. The quantities on which I have calculated are the greatest which are shown in the analyses, unless where the variations are given, when I have taken the extremes, and the weight of crop per acre is what I learn to be a medium between the very best and average crops; but I am most willing to confess that I am not myself practically acquainted with this part of my subject, and hence cannot answer for their absolute correctness, but have obtained them from the best authorities accessible to me. The necessary alterations, however, are easily made by any one for himself, from the data I have given, but I think they cannot be far wrong. The quantities are pounds of gypsum and bone-phosphate of lime removed from one acre.

Name of Crop, &c.	Weight per acre of crop in tons.	Gypsum.	Bone-phosphate.
Hay	2	34 to 67	26 to 32
Clover hay (one crop)	2½	29	35
Clover, green (whole crop)	15	45	53
Lucern	12	93 to 152	14 to 103
Lints	8	34	31
Wheat	210 st.	7 to 10	44 to 50
Ditto, straw	1¾	38 to 55	7 to 18
Barley	190 st.	8	55
Ditto, straw	1½	62	12
Oats	220 st.	13	48
Ditto, straw	1¾	36 to 75	8 to 25
Beans	200 st.	10	69
Ditto, straw	1¼	20	27
Peas	200 st.	21	24
Ditto, straw	1½	33	12
Rye	180 st.	6	17
Ditto, straw	2	21	28
Potatoes	10	23	46
Ditto, tops	10	74	80
Carrots	20	26	64
Ditto, tops	3	46	21
Mangel-wurzel	30	27	76
Ditto, tops	4	24	12
Swede turnips	20	104	36
Ditto, tops	4	28	13
Early yellow-top ditto ...	18	81	59
Ditto, tops	5	48	21
Rape	20	140	64
Cabbage	20	88	47

LII. *Observations on the oxidizing power of Oxygen when disengaged by means of Voltaic Electricity.* By H. KOLBE, Esq., Ph.D.*

IN describing in a former paper † the properties of sesquichloro-carbohyposulphuric acid, I stated that this body resists the action of the most powerful oxidizing agents, such as chromic acid, nitric acid, and even nitro-hydrochloric acid. I likewise stated the change it undergoes by the action of a voltaic current, when an easily oxidizable metal is used for the positive pole. By this means another copulated acid is produced, in which one or more equivalents of chlorine are replaced by corresponding proportions of hydrogen.

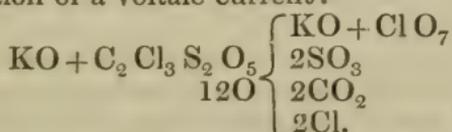
I have since observed that this acid is completely decomposed on employing two platina plates as electrodes; by which arrangement oxygen is disengaged at the positive pole. The following details, which contain a more perfect account of this observation, I hope will be acceptable to the Society.

* Communicated by the Chemical Society; having been read Dec. 7, 1846.

† Liebig's *Annalen der Chemie*, liv. pp. 156, 160.

On decomposing a concentrated solution of sesquichloro-carbohyposulphate of potash by a strong voltaic current (obtained by four elements of Bunsen's battery), at the commencement no hydrogen can be observed at the negative pole in consequence of the formation of the above-mentioned copulated acids, whilst chlorine, carbonic acid, and afterwards oxygen, are evolved at the positive pole. At the moment decomposition commences a distinct acid reaction of the solution is perceptible, owing to the formation of free hydrochloric and sulphuric acids; in a later stage of the process, these acids increasing in quantity, hydrogen appears at the negative pole, until finally, after all the hydrochloric acid has been decomposed, and the disengagement of chlorine has ceased, small octahedral crystals of perchlorate of potash are deposited from the solution, which now contains a large amount of free sulphuric acid and bisulphate of potash. The formation however of perchloric acid in this process is always preceded by that of chloric acid; for on evaporating the solution in an earlier stage of the decomposition, beside the before-mentioned octahedral crystals, the well-known rhombic plates of chlorate of potash are obtained.

The following formula represents the most probable decomposition sesquichloro-carbohyposulphuric acid undergoes by the action of a voltaic current:—



The production of perchloric acid in an acid solution is certainly a fact worthy of attention, for according to all the observations hitherto made, combination between chlorine and oxygen could only be effected in presence of a free alkaline basis ready to unite with the newly-formed acid; but ascertained as it is by a great number of careful experiments, it is another proof of the unparalleled negative power of oxygen when evolved by means of voltaic action.

These observations induced me to make the following experiments.

A neutral solution of chloride of potassium was decomposed in the same manner by a strong voltaic current; it immediately became alkaline, whilst hydrogen and chlorine were disengaged. The formation of chlorate of potash, which took place under these circumstances, can evidently be considered only as the result of the ordinary chemical action of chlorine upon caustic potash. On mixing however a solution of chloride of potassium with sufficient sulphuric acid to set free all the

muriatic acid, and passing the voltaic current through it as before, chlorate of potash was nevertheless formed, which was subsequently converted into perchlorate of potash.

The transformation of oxy-acids of chlorine, such as liquid hypochloric acid (ClO_4), or of a solution of chlorate of potash, into perchloric acid by means of voltaic action, has been mentioned by Berzelius. I have ascertained, moreover, that when a voltaic current is passed through hypochloric acid, especially when previously mixed with some sulphuric acid, free chloric and perchloric acids are formed, after the disengagement of a considerable quantity of chlorine.

A concentrated solution of chloride of ammonium evolves hydrogen at the negative pole but neither oxygen nor chlorine at the positive pole. But the surface of the platina plate representing the latter pole is covered with small yellowish oily drops of chloride of nitrogen, which as soon as the two poles are brought into contact decomposes with a more or less violent explosion, chlorine and nitrogen being evolved. This experiment illustrates at once the formation of this compound and its highly explosive character.

Cyanide of potassium if dissolved in water is easily oxidized by a voltaic current, and converted into cyanate of potash, but I did not succeed in obtaining a percyanate of potash. I was not more successful in endeavouring to form a fluorate of potash from the fluoride of potassium by the same means.

I have not prosecuted my experiments further upon inorganic substances, having from some observations on the behaviour of organic compounds under the action of the voltaic current obtained results of so much greater interest, as to induce me to give them my exclusive attention; and at a future time, when I have completed the investigation, I shall beg leave to communicate it to the Chemical Society.

LIII. *On the Discovery of Silurian Rocks in Cornwall.* By Sir RODERICK IMPEY MURCHISON, G.C. St. S., F.R.S., V.P.G.S. & R. Geogr. S., Honorary Member of the Royal Geological Society of Cornwall, Mem. Imp. Acad. Sc. St. Petersburg, Corr. Roy. Inst. France, &c. In a Letter addressed to Sir C. Lemon, Bart., M.P., President of the Royal Geological Society of Cornwall.

MY DEAR SIR CHARLES,

IN compliance with the promise I made when attending your last anniversary meeting at Penzance, I now give you a more decided opinion than I was then enabled to do, respecting the

age of the lowest and oldest of the sedimentary rocks of Cornwall.

Not having seen the fossils collected by Mr. Peach on the south coast of Cornwall, I then found it difficult to come to any other conclusion than that at which Professor Sedgwick and myself had long ago arrived; viz. that with the exception of the presence, in the north-eastern extremity of the county, of a portion of the culmiferous (carboniferous) trough of central Devon, the remaining and underlying strata of Cornwall were of the age of the Devonian or Old Red system. The few Cornish fossils which were then shown to me in your museum, were unquestionably similar to those with which I was formerly familiar in Devonshire and North Cornwall, as well as with those of the Rhenish provinces and the Eifel, which Professor Sedgwick and myself had shown to occupy a like geological position. They were, in fact, forms of the same type as those which, at the suggestion of Mr. Lonsdale and with the assistance of Mr. James Sowerby on one occasion*, and with the help of MM. de Verneuil and d'Archiac on another†, we had published as characteristic of a group of intermediate characters, pertaining to strata lying *beneath* the carboniferous rocks and *above* the Silurian system. In a word, they were identical with some of the numerous fossils of Devon and North Cornwall, published in the work of Professor Phillips‡; who, in pointing out in certain tracts the connexion of this group with the carboniferous fossils, which he had so well described, and in others with the Silurian forms I had published, had also concluded that the great mass of fossiliferous strata which rise up from beneath the culm measures of central Devon were of the same intermediate characters. In his valuable Maps of Cornwall and Devon, Sir H. De la Beche gave essentially the same views of geological succession; and lastly, in his Report upon the geological structure of that region, he described certain detailed sections in the southern districts of Cornwall, to which I will presently advert.

In proposing the word "Devonian," as applied to the intermediate strata in question, Professor Sedgwick and myself fortunately thus qualified our meaning in regard to the extension of such rocks into Cornwall:—"In asserting that the stratified rocks of Devonshire and Cornwall are, upon a broad scale, the equivalents of the Carboniferous and Old Red systems, we do not however deny, that in certain tracts *the lowest members of some of these rocks may represent the upper division of the Silurian system*; for although we have as yet found few if any of the fossils most typical of that system, we admit that when the sedi-

* Trans. Geol. Soc. n. s. vol. v. p. 633. † Ibid. vol. vi. pp. 221, 303.

‡ Palæozoic Fossils of Devon and Cornwall.

ments of a given epoch have been accumulated under peculiar conditions, we must expect to find considerable variations in the forms of animal life. Again, we know that the rocks of this region have undergone great changes in assuming their hard and slaty character; and under such circumstances, the difficulty of precisely limiting the boundary line of any portion of them is prodigiously increased*.”

The truth is, that neither Sir H. De la Beche and Professor Phillips, nor Professor Sedgwick and myself, had, at the time when our works were published, seen any fossils from *South Cornwall* sufficiently distinct to warrant the conclusion, that it contained forms of an older type than those which had been detected in North and South Devon and in the west of Cornwall. It was therefore believed (and all geological maps were coloured accordingly) that the zone of rocks occupying the southern headlands of Cornwall, between the Bay of Plymouth on the east and the Lizard Head on the west, were simply downward expansions of the fossiliferous “Devonian” strata. In this state of the question, your associate Mr. Peach began his labours in collecting fossils along the southern headlands of Cornwall. He first ascertained that certain forms first discovered by Messrs. Couch in the environs of Polperro were fishes, which he exhibited at the Cork Meeting of the British Association, and concerning which Professor Phillips and myself could only venture (so obscure did they appear to us) to give the guarded, though suggestive opinion, which Mr. Peach has recorded in your thirtieth Report. I then ventured to surmise, that these ichthyolites might belong to the Upper Silurian rocks, the oldest in which the remains of any vertebrated animals had yet been discovered, because “they occurred in rocks forming the axis of South Devon and Cornwall, which I had always considered to be the oldest in that country.”

In pursuing his researches, Mr. Peach published in 1844 a synopsis of the Cornish fossils from various localities, in which, besides the ichthyolites of Polperro, he identified several mollusca from Gorran Haven, Caerhayes, and Carn Gorran Bay, with typical Silurian species. These were the fossils I was so anxious to see at Penzance; and Mr. Peach having obligingly forwarded them to me in London, I no sooner unpacked the box, than I found that true Silurian and even Lower Silurian rocks existed in Cornwall,—the proofs being the presence of certain simple-plaited *Orthidae*, which are invariably typical of that age. But although Mr. Peach had come to a correct general conclusion, the specific names he attached to the South Cornish fossils in your thirtieth Report are not correct. In respect to the ichthyolites from the slates

* *Phil. Mag.* 1839, vol. xiv. p. 241.

of Polperro, Pentuan, &c., they have been referred to our mutual friend Sir Philip Egerton, who is better versed in the classification of Agassiz than any of our countrymen, and he thus writes to me concerning them:—"These remains are very enigmatical, and I cannot identify a single specimen with any form I know. I do not think any one of the fragments belongs either to *Cephalaspis* or *Holoptychius*. The nearest approach is to *Bothriolepis*. The dorsal fin named by Mr. Peach *Onchus Murchisoni* (Agass.) is not that species, as far as I can determine from the description of Agassiz, unless it be a more perfect specimen than he has seen. The longitudinal ribs, instead of being uniform (as figured by Agassiz), are notched, more after the manner of *Ctenacanthus*. The other *Onchus* may be *O. tenuiserratus*, but I have not here the means of comparison. From the general appearance of the collection, I should say they differ from any Old Red or Devonian fishes I have ever seen."

If these ichthyolites do not decisively help us to settle the age of the Polperro zone of rocks, they are still of great interest, as being the only group of fishes worth noticing which has been found in the older rocks of Devonshire and Cornwall*, and also as being associated with shells, which Mr. J. Sowerby identifies with the *Bellerophon trilobatus* (Sil. Syst.) and the *Loxomena lineata* (Phillips). The first-mentioned of these shells is characteristic of the tile-stones in Herefordshire and Shropshire, and is also found in strata of the same age in Cumberland (between Kirby Lonsdale and Kendal), which form the uppermost band of the Silurian rocks, or a transition from the Silurian into the Devonian system. Now as Professor Sedgwick and myself had inferred that the limestones of Looe and Fowey belonged to the lower calcareous zone of Devonshire, and as the sections of Sir H. De la Beche show that the Polperro beds dip beneath the Looe and Fowey rocks, the zoological evidences seem to harmonize with recorded physical facts, and we thus obtain reasonable grounds for believing, that the lowest Devonian and the uppermost Silurian strata are exposed in the district which ranges along the shores of that part of Cornwall, by Polperro, Pentuan, &c.

But if doubts should exist as to whether the Polperro slates ought to be referred to the bottom of the Devonian or top of the Silurian system, the discoveries of Mr. Peach in the headland of the Dodman, and in the prolongation of its strata to Veryan Bay, completely demonstrate, that still older and unquestionable Silurian rocks are there present. This is the district in which both

* Professor Phillips mentions two very imperfect and doubtful scales of fishes, the one in South Devon, the other in North Devon. Palæozoic Fossils, p. 133, figs. 256, 257.

Professor Sedgwick and Sir H. De la Beche had noted the existence of a line of elevation*, running from north-east to south-west, which bringing up certain quartzose or argillaceous slates had thrown the beds off, both to the south-east and north-west, the published section of the latter having clearly indicated these relations.

The fossils found by Mr. Peach at Gerrans Bay, as determined by Mr. J. Sowerby, are *Orthis lata*, *O. orbicularis*, another species resembling *O. plicata*, and a fourth which does not appear to have been published. At Caerhayes, Mr. Peach has collected other forms of *Orthide*, one of which approaches nearest to the *O. alternata* of the Silurian system. The remainder are not, however, referrible, as he had supposed, to *Leptæna lata*, *Terebratula micula*, *Atrypa striatula*, &c. The fossils from the Great Peraver quarries in Gorran Haven, on the eastern face of the Dodman, are still more decisive; for the species which Mr. Peach has named *Orthis flabellubum* and *O. testudinaria* both belong, unquestionably, to the *Orthis (callactis B)† calligramma* (Sil. Syst.), and with it is a form undistinguishable from the *Orthis (canalis) elegantula* (Sil. Syst.). The only well-preserved *trilobite* in this rock appears to me to be the *Calymene pulchella?* (Dalman); a second species resembles *C. Blumenbachii*.

No one accustomed to the Palæozoic rocks can throw his eye over the fossils from these three localities, without at once recognising them as true Silurian types. They have an entirely distinct *facies* from the fossils of the overlying Devonian system, and none of the species so abundant in North-western Cornwall are here present. With my imperfect knowledge of the country, it would be premature to say that subdivisions can be established in this highly dislocated region, so as to define Upper and Lower Silurian bands. But it may safely be asserted, that the fossils of Gorran Haven are Lower Silurian types; there being no one species more eminently characteristic of the inferior portion of that system than the *Orthis calligramma*, which in Shropshire and the adjacent Welsh counties is found to range downwards, from the very uppermost beds of the Caradoc sandstone into the heart of the Snowdon slates, and is equally typical of the Lower Silurian rocks of Russia and Scandinavia. At the same time, I do not think that the Gorran Haven beds lie deep in the Lower Silurian

* See Trans. Geol. Soc., n. s., vol. v. p. 666; and Report on Cornwall and Devon, p. 84.

† This shell was called *Orthis callactis B* in the Sil. Syst. pl. 19, fig. 5, but subsequent comparisons have shown that it is identical with the *O. calligramma* (Dalman) of Scandinavia, Russia, &c.

In like manner, the *Orthis canalis* of the Silurian system has proved to be the *O. elegantula* of Dalman; and the names of that author being the oldest, are now necessarily adopted.

group: they probably represent the upper portion only of the Caradoc sandstone; for the *Orthis canalis*, or *elegantula*, and the *Calymene pulchella** (Dalm.), closely allied to *C. Blumenbachii*, are Wenlock, as well as Caradoc, fossils. Judging from the fossils only, I should say that the beds at Gerrans Bay with the *Orthis orbicularis* are younger than those of Peraver and Gorran Haven. Time and careful researches will, however, determine this question of detail, and all I can now express is my opinion, that the quartzose rocks and killas which extend from the tracts above alluded to, to the mouth and centre of the bay and harbour of Falmouth (probably much further to the south-west), are of Silurian age also.

The energy of Mr. Peach having thus afforded us the key by which new lights are thrown upon the succession of Cornish strata, I cannot but hope that, when the government geological surveyors revisit Cornwall, they will define the exact demarcations between these Silurian masses and their overlying Devonian neighbours. In fact, I have within these few days been talking over this subject with my friend the Director-General of the Survey, and he has pointed out to me on his detailed map, how, from the enormous flexures which the strata have undergone in their range from Devon into Cornwall, it is highly probable that Silurian rocks (the equivalents of those alluded to) may be recognised in other parts of Cornwall. Thus, the quartzose rocks of Pydar Down or Moor, to the north of St. Columb, which form an east and west axis, dipping to the north and south under fossiliferous Devonian strata, may (he thinks) prove to be also of Silurian age. But, forbearing to speculate on the probable results of future researches, it is my decided belief, that the slaty rocks constituting the great southern headland of Devonshire, at least all the schists, &c. to the north of the Start Point, will eventually be classed with the Silurian group of South Cornwall; for if the Plymouth group of limestones, so prolific in animal remains, afforded us the means of deciphering the age of less clearly developed zones on the same horizon in Cornwall, the Silurian types collected by Mr. Peach may enable us to carry out a more correct classification in still older strata, from Cornwall into the obscure southernmost promontory of Devonshire. In the mean time, confining ourselves to what we now know, it is manifest that Cornwall exhibits in ascending order from north to south,—1st, a band of true Silurian rocks; 2nd, a zone of intermediate character, forming a transition between the Silurian and Devonian systems; 3rd, a copious Devonian system, characterized by

* The *Calymene pulchella* (Dalm.) occurs both in the inferior part of the Upper Silurian, and the higher part of the Lower Silurian, in Sweden, Siluria and Wales.

lower and upper limestones; and 4th, a limb of the culmiferous or carboniferous basin of Devonshire.

This view will, I trust, be perfectly intelligible to the members of your Society who have occupied themselves with the consideration of this branch of geology, and on which Mr. R. Q. Couch has recently written with perspicuity and talent. I doubt, however, if anything I have stated will make a due impression upon one of that number, my good-humoured antagonist the Rev. D. Williams, whose views of the Cornish succession of strata seem to be opposed to those of all his contemporaries. Geologists, however, who have long lived in Cornwall, and have so well illustrated its mineral structure, will, I am persuaded, be the first to admit the value of the Palæozoic classification, which having been worked out and established in tracts exempt from much dislocation and alteration, has been so applied, as to enable us to interpret the true history of the highly convulsed and metamorphosed rocks of their county. It is, in fact, the greatest triumph which could have been anticipated on the part of those who have steadily proceeded from the known to the unknown.

Looking from your own country to the opposite side of the channel, you are doubtless well-aware that there is the strongest analogy between the slates and granites of Cornwall and those of Brittany and Normandy. Many persons have remarked upon the strong resemblance between the Mounts St. Michael in the two countries; and no one can have traversed these two regions, without perceiving that, just as they evidently belong to the same mineral type, so are their respective inhabitants descended from a common stock, whose names of places have passed down to their French and English descendants. The existence of Caradoc sandstones and other Silurian rocks in that part of France having been already indicated (Devonian and carboniferous strata being also abundantly developed), their discovery in Cornwall is a happy addition to that union of geological and historical records, by which these widely-separated residences of the Celtic race are illustrated.

With regard to the highly mineralized or metamorphosed conditions of great portions of the killas and sandstone of Cornwall, I can do little more than refer you to the few observations I made at your anniversary meeting, and of which a brief abstract has appeared in your newspapers. There are, as you well said in your anniversary discourse, many analogies between the metamorphic rocks of Cornwall on the one hand and those of Scandinavia and the Ural mountains on the other; whilst the parallel is now drawn closer by the recent discovery of the Cornish Silurian rocks. Your last erupted granites, elvans, and porphyries, have played exactly the same part in traversing your Palæo-

zoic sediments, as like rocks have done in Norway. Cornwall may also be compared to large portions of Siberia, and notably to the Ural mountains, whose chief eruptions have taken place through deposits of Silurian, Devonian, and carboniferous age. In both countries the eruptive rocks are granites, porphyries, greenstones and serpentines: even in their superficial accumulations there is this striking resemblance, that the Cornish detritus and gravel (as clearly pointed out by Mr. Carne) is purely *local*,—the county being quite as exempt from all far-transported materials as the Ural mountains and Siberia. With this absence of all foreign transport or drift, Cornwall is as instructive as the Ural in never exhibiting those “*roches moutonnées*” and those polished and striated surfaces which have (in my opinion) been so erroneously referred to the action of land glaciers, in all those low regions of the earth, where they have clearly been caused by the action of powerful aqueous drift, in the manner I have elsewhere attempted to explain*.

Your *stanniferous* gravel bears, indeed, precisely the same relations to your granite and killas, as the *auriferous* deposits of the Ural to the eruptive and schistose rocks of that chain. Both are mere local, shingle accumulations, derived from veinstones which have been denuded from the surface of adjacent crystalline rocks. With these analogies there is however a marked distinction between Siberia and Cornwall. All richly auriferous chains (Humboldt first remarked the fact) have a meridian direction, as in the Ural, and various north and south parallel ridges in Siberia and other parts of the globe. The axis of Cornwall, on the contrary, is transverse to that direction, viz. from E.N.E. to W.S.W.; and though containing copper ore in common with the Russian mountains, it differs from them in not producing gold or platinum; whilst it is peculiarly distinguished by containing tin, which is unknown in the Ural. Let us hope that the day is fast approaching, when the cause of the production of such striking phenomena as these will receive some explanation at the hands of those physical philosophers, who are advancing a line of research in which your own countryman Mr. R. W. Fox has already so distinguished himself. But if gold does not exist (in any appreciable quantity at least) in your otherwise richly endowed mineral county, there are, I am happy to say, good grounds for hope, that in their most distant great colony Englishmen may find it abundantly. In an address to the Royal Geographical Society, delivered in May 1845, when commenting upon the valuable labours of Count Strzelecki in deciphering the structure of the great north and south chain which ranges along

* See Russia in Europe and Ural Mountains; and Journal of the Geol. Soc., No. 8.

the eastern shores of Australia, I specially insisted upon its striking resemblances to the Ural mountains, whether in direction, in structure, or in alluvia; remarking, by the way, that *as yet* no gold had been found in this alluvium. I now learn, however, that fine specimens of gold have been found on the western flank of the Australian cordillera, particularly at the settlement of Bathurst, where it occurs in fragments composed of the same matrix (*viz.* quartz rock) as in the Ural. My friend and associate in the Imperial Academy of Petersburg, Colonel Helmersen, has also recently suggested, that a careful search for gold ore in the Australian detritus will, it is highly probable, lead to its detection in abundance; since the Russians had long colonized the Ural mountains, and had for many years worked mines of magnetic iron and copper in solid rocks, before the neglected shingle, gravel and sand, on the slopes of their hills and in their valleys, were found to be auriferous. If, then, in the course of your statistical inquiries, you may know of any good Cornish miner about to seek his fortune in Australia, be pleased to tell him to apply his knowledge of the mode of extracting tin ore from his own gravel to the drift and debris on the flanks of the great north and south chain of Australia*, or any smaller parallel ridges of that vast country; for great would be my pleasure to learn, that through the application of Cornish skill, such regions should be converted into a British "El Dorado."

Requesting you to pardon this little digression, which after all may be turned to profit, and hoping that you will be as proud as I am of the connexion which is now established between Cornwall and Siluria,

Believe me to be, my dear Sir Charles,
Yours most faithfully,

R. I. MURCHISON.

* The grand, rich and well-watered region which lies between Moreton Bay on the south and the Gulf of Carpentaria on the north, is that to which I would specially direct attention, now that its true characters have been opened out to geographers and naturalists by the undaunted and able explorations of Dr. Leichhardt. Some of the tracts recently passed through with so much zeal, by the Surveyor-General of the colony, Sir Thomas Mitchell, may also prove valuable in gold, though they lie further from the axis of elevation. In the mean time, gold ore has been found on the other side of the Australian continent, in the ridges which extend northwards from Adelaide towards the scene of the adventurous and toilsome journey of Major Sturt. These gallant geographers, the pioneers of civilization, are explaining to us the condition of tracts which thousands of our countrymen may soon colonize with the best effects.—London, April 12, 1847.—R. I. M.

LIV. *On the Production of Light by Heat.* By JOHN WILLIAM DRAPER, M.D., *Professor of Chemistry in the University of New York**.

ALTHOUGH the phænomenon of the production of light by all solid bodies, when their temperature is raised to a certain degree, is one of the most familiar in chemistry, no person so far as I know has hitherto attempted a critical investigation of it. The difficulties environing the inquiry are so great, that even among the most eminent philosophers a diversity of opinion has prevailed respecting some of the leading facts. Thus Sir Isaac Newton fixed the temperature at which bodies become self-luminous at 635° , Sir Humphry Davy at 812° , Mr. Wedgwood at 947° , and Mr. Daniell at 980° . As respects the nature of the light emitted there are similar contradictions. In some philosophical works of considerable repute, it is stated that when a solid begins to shine it first emits red and then white rays; in others it is asserted that a mixture of blue and red light is the first that appears.

I have succeeded in escaping or overcoming many of the difficulties of this problem, and have arrived at satisfactory solutions of the main points; and as the experiments now to be described lead to some striking and perhaps unexpected analogies between light and heat, they commend themselves to our attention as having a bearing on the question of the identity of those imponderable principles. It is known that heretofore I have been led to believe in the existence of cardinal distinctions, not only between these but also other imponderable agents; and I may therefore state, that when this investigation was first undertaken, it was in the expectation that it would lead to results very different from those which have actually arisen.

The following are the points on which I propose to treat:—

1. To determine the point of incandescence of platinum, and to prove that different bodies become red-hot at the same temperature.

2. To determine the colour of the rays emitted by self-luminous bodies at different temperatures. This is done by the only reliable method—analysis by the prism.

From these experiments it will appear, that as the temperature rises the light increases in refrangibility; and making a due allowance for the physiological imperfection of the eye, the true order of the colours is red, orange, yellow, green, blue, indigo, violet.

* Communicated by the Author.

3. To determine the relation between the brilliancy of the light emitted by a shining body and its temperature.

Here we shall find that the intensity of the light increases far more rapidly than the temperature. For example, platinum at 2600° emits almost forty times as much light as it does at 1900° .

As I prefer to give a complete description of the apparatus employed in these investigations after the general results are stated, it is sufficient here to understand that the source of light is in all instances a very thin strip of platinum 1.35 inch long and $\frac{1}{20}$ th of an inch wide, brought to the temperature under investigation by a voltaic current. Platinum was selected from its indisposition to oxidize, and its power of resisting a high temperature without fusion.

The slip of platinum, thus to be brought to different temperatures by an electric current of the proper force, was fastened at one end to an inflexible support, and at the other was connected with a delicate lever-index, which enabled me to determine its expansion, and thereby its temperature. For this purpose I have used the coefficient of dilatation of Dulong and Petit. The temperatures here given are upon the hypothesis of the invariability of that coefficient at all thermometric degrees; they are therefore to some extent in error.

By the aid of resisting wires of different lengths and a rheostat, I was able to vary the force of the electric current in the platinum, and thereby vary its temperature. My first attempts were to discover the point at which the metal begins to emit light.

The platinum and the voltaic battery were placed in a dark room, the temperature of which was 60° ; and after I had remained there a sufficient length of time to enable my eyes to become sensible to feeble impressions of light, I caused the current to pass, gradually increasing its force, until the platinum was visible. In several repetitions of this experiment it was uniformly found that the index to which the platinum was attached stood at the eighth division when this took place. The metal had therefore dilated $\frac{1}{22}$ of its length; the elevation of its temperature was about 917° , which added to the existing height of the thermometer, 60° , gives for the temperature of incandescence 977° F.

To the correctness of the number it may be objected, that owing to the narrowness of the metallic strip it is not well calculated to make an impression on the eye when the light it emits is so feeble; nor can we take the dilatations given by the index, as representing the uniform temperature of the whole platinum, which must necessarily be colder near its

points of support, by reason of the conducting power of the metals to which it is attached.

Physiological considerations would also lead us to suspect that the self-luminous temperature must vary with different eyes. The experiments of Bouguer, hereafter to be referred to, indisputably show that some persons are much more sensitive to the impressions of light than others. So far as my limited investigation of this matter has gone, I have not however found appreciable differences in the estimate of the temperature of incandescence. Different individuals, observing the platinum, have uniformly perceived it at the same time.

Against the number 977° it may also be objected, that antimony melts at a much lower temperature, and yet emits light before it fuses. If this statement were true, it would lead us to believe that all bodies have not the same point of incandescence. But I think the experiments of Mr. Wedgwood on gold and earthenware are decisive of that question; and, moreover, I have reason to believe that the melting-point of antimony is much higher than commonly supposed.

With a view of determining directly whether different bodies vary in their point of incandescence, I took a clean gun-barrel, and having closed the touch-hole, exposed the following substances in it to the action of the fire:—platinum, chalk, marble, fluor spar, brass, antimony, gas-carbon, lead; each specimen was small; the platinum was in the form of a coil of stout wire.

When one of these bodies was placed in the gun-barrel and the temperature raised, it is clear that any difference in their point of incandescence would be detected by the eye. Thus, if the ignition of platinum required a higher degree than iron, on looking down the barrel the coil of wire should be dark, when the barrel itself begins to shine; or, if the platinum was incandescent first, the wire should be seen before the barrel is visibly hot; and these results might be corroborated by observing the inverse phenomena, when the barrel is taken from the fire and suffered to cool.

With respect to platinum, brass, antimony, gas-carbon and lead, they all became incandescent at the same time as the iron barrel itself. I could not discover the slightest difference between them, either in heating or cooling; and it is worthy of remark, that the lead was of course in the liquid condition. But the chalk and marble were visible before the barrel was red-hot, emitting a faint white light; and the fluor spar still more strikingly so, its light being of a beautiful blue; and even when the barrel had become bright red I could still see the spar, which had decrepitated to a coarse powder, by its

faint blue rays. In these cases it was not, however, incandescence but phosphorescence that was taking place. I infer then that all solids, and probably melted metals, shine at the same thermometric point.

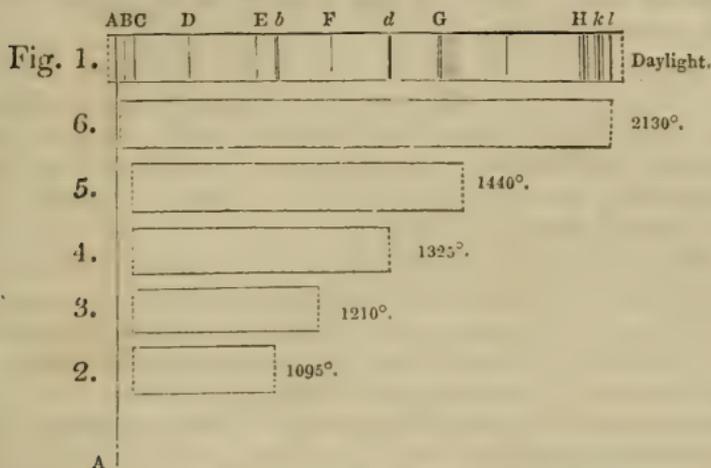
The temperature of incandescence seems to be a natural fixed point for the thermometer; and it is very interesting to remark how nearly this point coincides with 1000° of the Fahrenheit thermometer, when Laplace's coefficient for the dilatation of platinum is used. Upon that coefficient the point of incandescence is 1006° F.

In view of these considerations, and recollecting that the number given by Daniell is 980° , and that of Wedgwood 947° , I believe that 977° is not very far from the true temperature at which solids begin to shine. It is to be understood, of course, that this is in a very dark room.

I pass now to the second proposition. The rays emitted by the incandescent platinum were next received on a flint glass prism, placed so as to give the minimum deviation, and after dispersion viewed in a small telescope. A movement could be given to the telescope, which was read off on an annexed scale. However, instead of bringing the parts of the spectrum under measurement to coincide with the wires stretched across the field of the instrument, I found it more satisfactory to determine them by bringing them to one or other of the edges of the field; a process well adapted to ascertain the position of the extreme rays, the faint light of which contrasted well with the darkness by which it was surrounded. They could not have been so accurately seen while the rest of the spectrum was in view; and as it was absolutely necessary to have fixed points of reference, that all the observations might be brought to a common standard of comparison, and as there are no fixed lines in the light of incandescence, such as are in the sunshine and daylight, I therefore previously determined the position of the fixed lines in a spectrum formed by a ray of reflected daylight which passed through a fissure $\frac{1}{20}$ th of an inch wide and one inch long, occupying exactly the position subsequently to be occupied by the incandescent platinum. Fig. 1 represents the results of this observation.

The strip of platinum was now placed in the position of the fissure which had given the spectrum fig. 1, and its temperature was raised by the passage of a voltaic current. Although I could distinctly see the metal when the heat had reached about 1000° by the naked eye, yet the loss of light in passing the prism and telescope was so great that I found it necessary to carry the temperature to 1210° before a satisfactory observation could be made. At this point the spectrum extended

from the position of the fixed line B in the red, almost as far as the line F in the green; the colours present being red,



Spectra of incandescent platinum at different temperatures.

orange, and a tint which may be designated as gray. There was nothing answering to a yellow. The first rays visible through this apparatus may therefore be designated as red and greenish gray; the former commencing at the line B, and the latter continuing to F. The magnitude and other relations of this spectrum are given in fig. 3.

The voltaic current was now increased, and the temperature rose to 1325° . The red end of the spectrum remained nearly as before, but the more refrangible extremity reached to the position of the little fixed line *d*. Traces of the yellow were now visible; and, with a certain degree of distinctness, I could see red, orange, yellow, green, and a fringe of blue. Fig. 4 shows the result.

The temperature was now carried to 1440° . I thought the red extremity was advancing more to the line A: the blue had undergone a well-marked increase. It reached considerably beyond the line G, as shown in fig. 5.

On bringing the platinum to 2130° all the colours were present, and exhibited considerable brilliancy. Their extent was somewhat shorter than that of the daylight spectrum, as is seen in fig. 6.

Having thus by repeated experiments ascertained the continued extension of the more refrangible end as the temperature rose, it became necessary to obtain observations for points below 1210° , the limit of visibility through the telescope. I therefore carried the prism nearer to the platinum, and looking with the unassisted eye directly through it at the refracted image, I found it could be distinctly seen at a temperature as

low as 1095° . Under these circumstances the total length could not be compared by direct measurement with the other observations, and the result given in fig. 2 is from the best judgement I was able to form: the colours were red and greenish-gray.

The gray rays emitted by platinum just beginning to shine appear to be more intense than the red; at all events the wires in the field of the telescope are more distinctly seen upon them than upon the other colour. I give them the designation of gray, for they appear to approach that tint more closely than any other; and yet it is to be remarked that they are occupying the position of the yellow and green regions.

Already we have encountered a fact of considerable importance. The idea, that as the temperature of a body rises it begins to emit rays of increasing refrangibility, has obviously to be taken with a certain restriction. Instead of first the red, then the orange, then the yellow, &c. rays, in succession making their appearance, in which case the spectrum should regularly increase in length as the temperature rises, we here find, at the very first moment it is visible to the eye, it yields a spectrum reaching from the fixed line B to nearly F; that is to say, equal to about two-thirds the whole length of the interference spectrum, and almost one-half of the prismatic.

It is to be remarked, that while the more refrangible end undergoes a great expansion, the other extremity exhibits a corresponding though a less change. As very important theoretical conclusions depend on the proper interpretation of this fact, we must not forget that, to a certain extent, it may be an optical deception, arising from the increased brilliancy of the light. While the rays are yet feeble, the extreme terminations may be so faint that the eye cannot detect them; but as the intensity rises, they become better marked, and an apparent elongation of the spectrum is the consequence.

It is agreed by optical writers, that to the human eye the yellow is the brightest of the rays. In the prismatic spectrum the true relationship of the colours is not perceived, because the less refrangible are crowded together, and the more refrangible unduly spread out. But in the interference spectrum, where the colours are arranged side by side in the order of their wave-lengths, the centre is occupied by the most luminous portion of the yellow; and from this point the light declines away on one side in the reds, and on the other in the blues, the terminations being equidistant from the centre of the yellow space.

Now if the rays coming from shining platinum were passed through a piece of glass, on which parallel lines had been

drawn with a diamond point, so as to give an interference spectrum, even admitting the general results of the foregoing experiments to be true, viz. that as the temperature rises rays of a higher refrangibility are emitted, it is obvious that it by no means follows that the first ray visible should be the extreme red. Our power of seeing that depends on its having a certain intensity. Even when it has assumed that extreme brilliancy which it has in a solar beam it is barely visible. We ought therefore to expect that rays of a higher refrangibility should first be seen, because they act more energetically on our organ of vision; and as the temperature rises, the spectrum should undergo a partial elongation in the direction of its red extremity.

I may here remark, that the general result of these experiments coincides exactly with that of M. Melloni respecting heat at lower thermometric points. In his second memoir*, he shows that when the rays from copper at 390° and from incandescent platinum are compared by transmission through a rock-salt prism, as the temperature rises the refrangibility of the calorific emanations correspondingly increases. Those philosophers who regard light and heat as the same agent, will therefore see in this coincidence another argument in favour of their opinion.

In view of the foregoing facts I conclude, that, *as the temperature of an incandescent body rises, it emits rays of light of an increasing refrangibility*; and that the apparent departure from this law, discovered by an accurate prismatic analysis, is due to the special action of the eye in performing the function of vision.

As the luminous effects are undoubtedly owing to a vibratory movement executed by the molecules of the platinum, it seems from the foregoing considerations to follow, that the frequency of those vibrations increases with the temperature.

In this observation I am led by the principle, that "to a particular colour there ever belongs a particular wave-length, and to a particular wave-length there ever belongs a particular colour;" but in the analysis of the spectrum made by Sir D. Brewster by the aid of absorptive media, this principle is indirectly controverted; that eminent philosopher showing that red, yellow, blue, and consequently white light, exist in every part of the spectrum. This must necessarily take place when a prism which has a refracting face of considerable magnitude is used; for it is obvious that a ray falling near the edge, and one falling near the back, after dispersion, will paint their several spectra on the screen; the colours of the one not co-

* Taylor's Scientific Memoirs, vol. i. p. 56.

inciding with, but overlapping the colours of the other. In such a spectrum there must undoubtedly be a general commixture of the rays; but may we not fairly inquire whether, if an elementary prism were used, the same facts would hold good; or, if the anterior face of the prism were covered by a screen, so as to expose a narrow fissure parallel to the axis of the instrument, would there be found in the spectrum it gave every colour in every part, as in Sir David Brewster's original experiment? M. Melloni has shown how this very consideration complicates the phænomena of radiant heat; and it would seem a very plausible suggestion that the effect here pointed out must occur in an analogous manner for the phænomena of light.

I proceed now to the third branch of the inquiry,—to examine the relation between the temperatures of self-luminous bodies and the intensity of the light they emit, premising it with the following considerations.

The close analogy which is traced between the phænomena of light and radiant heat lends countenance to the supposition, that the law which regulates the escape of caloric from a body will also determine its rate of emission of light. Sir Isaac Newton supposed that whilst the temperature of a body rose in arithmetical progression, the amount of heat escaping from it increased in a geometrical progression. The fallacy of this was subsequently shown by Martin, Erxleben, and Delaroche; and finally Dulong and Petit gave the true law, “when a body cools *in vacuo*, surrounded by a medium whose temperature is constant, the velocity of cooling for excess of temperature in arithmetical progression increases as the terms of a geometrical progression, diminished by a constant quantity.” The introduction of this constant depends on the operation of the theory of exchanges of heat; for a body, when cooling under the circumstances here given, is simultaneously receiving back a constant amount of heat from the medium of constant temperature.

Whilst Newton's law represents the rate of cooling of bodies, and therefore the quantities of heat they emit, when the range of temperature is limited, and the law of Dulong and Petit holds to a wider extent, there are in our inquiry certain circumstances to be taken into account not contemplated by those philosophers. Dulong and Petit throughout their memoir regard radiant heat as a homogeneous agent, and look upon the theory of exchanges, which is indeed their starting-point and guide, as a very simple affair. But the progress of this department of knowledge since their times has shown, that precisely the same modifications as are found in the co-

lours of light, occur also for heat; a fact conveniently designated by the phrase "ideal coloration of heat;" and further, that the colour of the heat emitted depends upon the temperature of the radiating source. It is one thing to investigate the phænomena of the exchanges of heat-rays of the same colour, and another when the colours are different. A perfect theory of the exchanges of heat must include the principle of ideal coloration, and, of course, so too must a law of cooling applicable to any temperature.

There is another fact to some extent considered by Dulong and Petit, but not of such weight in their investigations, where the range of temperature was small, as in ours, where it rises as high as nearly 3000° F.; I mean the difference of specific heat of the same body at different temperatures. At the high temperatures considered in this memoir, there cannot be a doubt that the capacity of platinum for heat is far greater than that at a low point. This therefore must control its rate of calorific emission, and probably that for light also.

From these and similar considerations, we should be prepared to discover that as the temperature of an incandescent solid rises, the intensity of the light emitted increases very rapidly.

I pass now to the experimental proofs which substantiate the foregoing reasoning.

The apparatus employed as the source of the light and measure of the temperature was the same as in the preceding experiments,—a strip of platinum, brought to a known temperature by the passage of a voltaic current of the proper force, and connected with an index which measured its expansion.

The principle upon which I have determined the intensities of the light is that first described by Bouguer, and recently introduced by M. Masson. After many experiments I have been led to conclude that this is the most accurate method known.

Any one who will endeavour to determine the intensities of lights by Rumford's method of contrasting shadows, or by that of equally illuminated surfaces, will find, when every precaution has been used, that the results of repeated experiments do not accord. There is moreover the great defect, that where the lights differ in colour it is impossible to obtain reliable measures, except by resorting to such contrivances as that described by me*.

Bouguer's principle is far more exact; and where the lights differ in colour, that difference actually tends to make the

* Phil. Mag., August 1844.

result more perfect. As it is not generally known, I will indicate the nature of it briefly.

Let there be placed at a certain distance from a screen of white paper, a candle so arranged as to throw the shadow of a ruler, or other opaque body, on the screen. If a second candle be placed also in front of the paper and nearer than the former, there is a certain distance at which its light completely obliterates all traces of the shadow. This distance is readily found; for the disappearance of the shadow can be determined with considerable exactness. When the lights are equal, Bouguer found that the relative distances were as 1 : 8; he inferred therefore, correctly, that in the case of his eye, the effect of a given light was imperceptible when it was in presence of another sixty-four times as intense. The precise number differs according to the sensibility of different eyes, but for the same organ it is constant.

Upon a paper screen I threw the shadow of a piece of copper, which intercepted the rays of the incandescent platinum: then taking an Argand lamp, surrounded by a cylindrical metal shade through an aperture in which the light passed, and the flame of which I had found by previous trial would continue for an hour almost of the same intensity, I approached it to the paper until the shadow cast by the copper disappeared. The distance at which this took place was then measured, and the temperature of the platinum determined.

The temperature of the platinum was now raised; the shadow became more intense, and it was necessary to bring the Argand lamp nearer before it was effaced. When this took place the distance of the lamp was again measured, and the temperature of the platinum again determined.

In this manner I obtained several series of results, one of which is given in the following table. They exhibited a more perfect accordance among each other than I had anticipated.

Table of the Intensity of Light emitted by Platinum at different Temperatures.

Temperature of the platinum.	Distance of Argand lamp.		Mean.	Intensity of light.
	Experiment 1.	Experiment 2.		
980	0.00
1900	54.00	54.00	54.00	0.34
2015	39.00	41.00	40.00	0.62
2130	24.00	24.00	24.00	1.73
2245	18.00	19.00	18.50	2.92
2360	14.50	15.50	15.00	4.40
2475	11.50	12.00	11.75	7.24
2590	9.00	9.00	9.00	12.34

The intensity of the light of the platinum is of course inversely proportional to the square of the distance of the Argand lamp at the moment of the obliteration of the shadow.

In this table the first column gives the temperatures under examination in Fahrenheit degrees; the second and third the distances of the Argand lamp from the screen, in English inches, in two different sets of experiments; the fourth the mean of the two; and the fifth the corresponding intensity of the light.

From this it is at once perceived, that the increases in the intensity of the light, though slow at first, become very rapid as the temperature rises. *At 2590° the brilliancy is more than thirty-six times as great as it is at 1900°.*

Thus, therefore, the theoretical anticipation which we founded on the analogy of light and heat is completely verified; and we discover that as the temperature of a self-luminous solid rises, it emits light in a greater proportion than would correspond to the mere difference of temperature. To place that analogy in a still more striking point of view, I will here introduce some experiments I have made in relation to radiant heat. No chemist, so far as I am aware, has hitherto published results for high temperatures, or endeavoured to establish, through an extensive scale, the principle of Delaroché, that "the quantity of heat which a hot body gives off in a given time by way of radiation to a cold body, situated at a distance, increases, other things being equal, in a progression more rapid than the excess of the temperature of the first above that of the second."

As my object on the present occasion is chiefly to illustrate the remarkable analogy between light and heat, the experiments now to be related were arranged so as to resemble the foregoing; that is to say, as in determining the intensities of light emitted by a shining body at different temperatures, I had received the rays upon a screen placed at an invariable distance, and then determined their value by photometric methods; so, in this case, I received the rays of heat upon a screen placed at an invariable distance, and determined their intensity by thermometric methods. In this instance the screen employed was in fact the blackened surface of the thermo-electric pile. It was placed at a distance of about one inch from the slip of incandescent platinum, a distance sufficient to keep it from any disturbance from the stream of hot air arising from the metal; care also was taken that the multiplier itself was placed so far from the rest of the apparatus, that its astatic needles could not be affected by the voltaic current

igniting the platinum, or the electro-magnetic action of the wires used to modify the degrees of heat.

The experiments were conducted as follows:—The needles of the thermo-multiplier standing at the zero of their scale, the voltaic current was passed through the platinum, which immediately rose to the corresponding temperature, and radiated its heat to the face of the pile. The instant the current passed, the needles of the multiplier moved, and kept steadily advancing upon the scale. At the close of one minute, the deviation of the needle and the temperature of the platinum were simultaneously noted, and then the voltaic current was stopped.

Sufficient time was now given for the needle of the multiplier to come back to zero. This time varied in the different cases, according to the intensity of the heat to which the pile had been exposed: in no instance, however, did it exceed six minutes, and in most cases was much less. A little consideration will show that the usual artifice employed to drive the needles back to zero, by warming the opposite face of the pile, was not admissible in these experiments.

The needles having regained their zero, the platinum was brought again to a given temperature, and the experiment conducted as before. The following table exhibits a series of these results.

Table of the Intensity of Radiant Heat emitted by Platinum at different Temperatures.

Temperature of the platinum.	Intensity of heat emitted.		Mean.
	Experiment 1.	Experiment 2.	
980	·75	1·00	·87
1095	1·00	1·20	1·10
1210	1·40	1·60	1·50
1325	1·60	2·00	1·80
1440	2·20	2·20	2·20
1555	2·75	2·85	2·80
1670	3·65	3·75	3·70
1785	5·00	50·0	5·00
1900	6·70	6·90	6·80
2015	8·60	8·60	8·60
2130	10·00	10·00	10·00
2245	12·50	12·50	12·50
2360	15·50	15·50	15·50

In this table the first column gives the temperatures of the

platinum in Fahrenheit degrees; the second and third two sets of experiments, expressing the arc passed over by the needle at the close of a radiation lasting for one minute, each number being the mean of several successive trials; and the fourth the mean of the two. It therefore gives the radiant effect of the incandescent platinum upon the thermo-multiplier for the different temperatures.

Of course it is understood that I here take the angular deviations of the needle as expressing the force of the thermo-electric current, or in other words, as being proportional to the temperatures. This hypothesis, it is known, is admissible.

It therefore appears that the quantity of heat radiated by incandescent platinum at 980° being taken as unity, it will have increased at 1440° to 2.5; at 1900° to 7.8; and at 2360° to 17.8, nearly: the rate of increase is therefore very rapid. Further, it may be remarked, as illustrative of the same fact, that the increased quantity of heat radiated by a mass of platinum in passing from 1000° to 1300° , is nearly equal to the amount it gives out in passing from common temperatures up to 1000° .

I cannot here express myself with too much emphasis on the remarkable analogy between light and heat which these experiments reveal. The march of the phenomena in all their leading points is the same in both cases. The rapid increase of effect as the temperature rises is common to both.

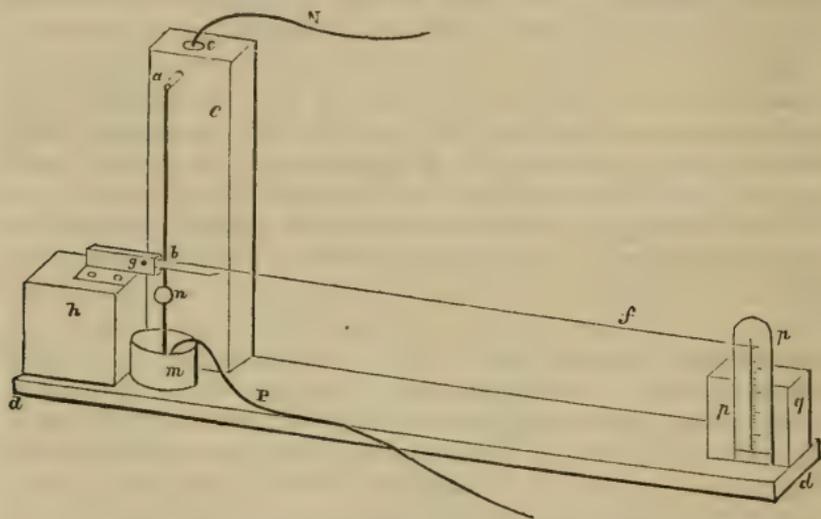
It is not to be forgotten, however, that in the case of light we necessarily measure its effects by an apparatus which possesses special peculiarities. The eye is insensible to rays which are not comprehended within certain limits of refrangibility. In these experiments, it is requisite to raise the temperature of the platinum almost to 1000° before we can discover the first traces of light. Measures obtained under such circumstances are dependent on the physiological action of the visual organ itself, and hence their analogy with those obtained by the thermometer becomes more striking, because we should scarcely have anticipated that it could be so complete.

Description of the apparatus employed in the foregoing experiments.

The source of light is in all instances a slip of platinum foil 1.35 inch long, and $\frac{1}{20}$ th of an inch broad, ignited by the passage of a voltaic current, and placed in such a position that its dilatation could be measured by the movements of an index over a graduated scale.

In fig. 7, *a b* represents the slip of platinum, the upper end of which is soldered to a stout and short copper pin *a*, firmly sunk in a block of wood *c*, which is immovably fastened on the basis *d d* of the instrument. A cavity *e*, half an inch in diameter, is sunk in the block *c*, and into this cavity the pin *a* projects; so that when the cavity is filled with mercury, a voltaic current may be passed through the pin and down the platinum.

Fig. 7.



The other extremity of the platinum *b* is fastened to a delicate lever *b f*, which plays on an axis at *g*, the axis working in brass holes supported on a block *h*. Immediately beneath the platinum strip, and in metallic communication with it, a straight copper wire dips down into the mercury cup *m*; on this wire there is a metal ball *n*, weighing about 100 grains. The further end of the index plays over a graduated ivory scale *p p*, which is supported on a block *q*, and can be moved a little up and down, so as to bring its zero to coincide with the index at common temperatures.

The action of the instrument is readily understood. In the mercury cup *e* dip one of the wires *N* of a Grove's battery of three or four pairs, the other wire *P* being dipped into the cup *m*. The current passes through the platinum, which immediately expands, the weight *n* lightly stretching it. The index *f* moves promptly over the scale, indicating the amount of expansion, and therefore the degree of heat. Remove the wire *N* out of its mercury cup *e*, the platinum instantly becomes cold, and pulls the lever to the zero point.

When the platinum is thin, so as to be quite flexible at the

point *b*, where it is fastened to the index, the movements take place with such promptitude and precision as to leave nothing to be desired. When the heat has been very high and long continued, the limit of elasticity of the platinum is somewhat overpassed, and it suffers a slight permanent extension. But as the ivory scale *p p* can slide up and down a little, the index is readily re-adjusted to the zero point.

The temperature of the platinum depends entirely on the force of the current passed through it. By intervening coils of brass wire of lengths adjusted beforehand, so as to resist the current to a given extent, any desired temperature may be reached. I found it convenient to intervene in the course of the current one of Prof. Wheatstone's rheostats, so as to be able to bring the index with precision to any degree, notwithstanding slight changes in the force of the voltaic battery.

The following are the dimensions and measures of the instrument I have used:—Length of the platinum strip, 1.35 inch; length of the part actually ignited, 1.14 inch; width of ditto, $\frac{1}{20}$ th of an inch; length of the index from its centre of motion to the scale, 7.19 inches; distance of the centre of motion of index from the insertion of the platinum at the point *b*, .22 inch; multiplying effect of the index, 32.68 times; length of each division on the ivory scale, .021 inch. From this it would appear, by a simple calculation, using the coefficient of dilatation of platinum given by Dulong and Petit, that each of the divisions here used is equal to 114.5 Fahrenheit degrees. For the sake of perspicuity I have generally taken them at 115°.

The Grove's battery I have employed has platinum plates three inches long and three quarters wide; the zinc cylinders are two inches and a half in diameter, three high, and one-third thick. As used in these experiments, it could maintain a current nearly uniform for an hour. I commonly employed four pairs.

Among writers on optics, it has been a desideratum to obtain an artificial light of standard brilliancy. The preceding experiments furnish an easy means of supplying that want, and give us what might be termed a "unit-lamp." A surface of platinum of standard dimensions, raised to a standard temperature by a voltaic current, will always emit a constant light. A strip of that metal, one inch long and $\frac{1}{20}$ th of an inch wide, connected with a lever by which its expansion

might be measured, would yield at 2000° a light suitable for most purposes. Moreover, it would be very easy to form from it an available photometer, by screening portions of the shining surface. An ingenious artist would have very little difficulty, by taking advantage of the movements of the lever, in making a self-acting apparatus, in which the platinum should be maintained at a uniform temperature, notwithstanding any change taking place in the voltaic current.

University, New York,
Feb. 27, 1847.

LV. *On the Acid contained in the North American Columbite.*
By HENRY ROSE*.

THE columbite of North America has the same crystalline form as that from Bodenmais in Bavaria, but is distinguished from it in general by a far lower specific gravity; however, we find the same difference in the specific gravity of the American mineral as occurs in the different crystals of the Bodenmais columbite. The lightest crystals from the last locality have the same specific gravity (5.704) as the heaviest crystals from North America (5.708).

I have already communicated two analyses of North American columbites, of one of which however it was doubtful whether it came from America. The following analysis of American columbite was made by M. Grewink in my laboratory; it yielded,—

Acid	80.06
Protoxide of iron	12.59
Protoxide of manganese	5.97
Oxide of tin	0.96
Oxide of copper and lead	0.44
	100.02

The specific gravity in fragments was 5.323; in powder, 5.3202.

This columbite comes nearest in composition and also in specific gravity to that examined by M. Schlieper.

I have on a former occasion shown that the different specific gravity of the crystals of the Bavarian columbite was owing to the different proportions of niobic and pelopic acids which are found in the different crystals. The specific gravity of these two acids is widely different, but unequally so, according to the temperatures to which they have been exposed previous to weighing.

Owing to want of material, I found it impossible to make a

* Translated from Poggendorff's *Annalen*.

thorough examination of the two acids which are contained in the North American columbite: I very soon ascertained, after the discovery of niobic acid, that this was the principal acid constituent in the American columbite, but I could not determine whether it was mixed with pelopic or with tantalic acid; I therefore addressed myself to Mr. B. Silliman of New-haven, who with the greatest readiness procured me a very considerable quantity (half a pound) of this now very rare mineral.

A large quantity of this columbite was used for the preparation of the acid. When treated in the same manner as that from the Bavarian columbite, it proved to consist principally of niobic acid combined with pelopic acid; but the amount of the latter was far smaller than in the Bodenmais mineral, so that I do not think it would have been possible for me to have examined the properties of pelopic acid so completely as was necessary in order to recognise it as an essentially distinct acid from tantalic acid, if I had had only the American mineral at my disposal. But both the acids were so perfectly identical in all their properties with the two acids prepared from the Bodenmais mineral, that I did not find the least difference, even as regards the specific gravity.

As the specific gravity of pelopic acid is considerably higher than that of niobic acid, when the two are heated in the same manner, the higher specific gravity of the Bavarian columbite is thus satisfactorily explained.

I have moreover found small quantities of tungstic acid in the acids from the American columbite, as well as in those from the Bodenmais mineral.

LVI. *Abstract of Meteorological Observations made during the year 1846 at Gongo Soco, in the interior of Brazil.* By WILLIAM JORY HENWOOD, F.R.S., F.G.S., Member of the Geological Society of France, Chief Commissioner of the Gold Mines of Gongo Soco, Catta Preta, and Antonio Pereira, &c. &c.*

I HAVE nothing to add to what has been already said† respecting the locality and the positions of the instruments. I have again to thank Captains Blaney, Luke and Guy, for the continuation of their midnight observations, as well as for a second series, made at 3 A.M.

* Communicated by the Author.

† Phil. Mag. 1846, xxviii. pp. 364, 366.

Table I.
Hourly mean and extreme temperatures for every month.

	3 A.M.			6 A.M.			9 A.M.			Noon.			4 P.M.			6 P.M.			8 P.M.*			9 P.M.			Midnight.		
	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.
Jan.	69.8	60.8	66.8	69.8	63.8	67.8	81.8	67.5	76.1	83.5	67.5	76.1	81.8	67.2	74.8	75.5	65.4	71.8	73.8	65.8	70.1	73.8	65.8	70.1	70.8	62.8	67.1
Feb.	73.8	61.8	68.5	71.8	63.2	68.8	86.8	66.8	78.6	86.2	67.1	78.8	83.8	66.2	75.8	77.8	65.8	72.7	76.8	64.6	71.7	76.8	64.6	71.7	72.8	61.8	69.5
Mar.	70.8	63.8	67.8	71.2	62.5	67.3	82.5	72.8	77.7	84.9	71.8	77.6	81.8	69.3	75.2	77.5	68.8	71.8	76.8	67.8	70.9	75.8	67.8	70.9	75.8	64.8	68.4
Apr.	69.8	51.8	62.3	67.5	51.2	68.8	78.8	67.3	72.6	78.3	67.5	73.1	73.8	65.8	69.7	70.3	60.6	67.6	71.5	69.7	66.5	70.3	59.5	66.5	70.8	54.8	63.7
May	68.8	52.8	61.1	68.8	53.5	61.1	80.8	63.8	71.1	78.2	63.8	71.6	75.8	61.8	67.9	75.5	59.2	65.4	74.8	64.4	58.6	64.4	58.6	64.4	70.8	55.8	62.5
June	61.8	43.8	54.4	61.5	43.8	54.6	67.8	53.8	66.6	73.8	59.8	67.2	66.8	54.8	62.3	64.5	50.7	59.1	63.6	49.5	58.1	63.6	49.5	58.1	61.8	45.8	56.2
July	58.8	40.8	52.4	59.5	41.8	52.3	67.8	58.8	63.7	68.5	56.8	64.2	64.7	53.8	60.2	63.8	50.8	56.5	62.5	49.8	55.6	62.5	49.8	55.6	60.8	41.8	52.8
Aug.	60.8	44.8	53.4	60.8	44.8	53.4	73.8	58.7	67.1	75.8	58.5	68.7	69.6	58.8	64.7	65.8	54.2	59.9	64.2	52.8	58.7	64.2	52.8	58.7	60.8	48.8	55.1
Sep.	70.8	48.8	58.3	70.8	45.8	57.3	82.8	58.8	71.5	82.8	58.6	72.6	80.1	56.8	68.5	75.8	54.5	64.1	73.8	52.5	63.1	73.8	52.5	63.1	71.8	48.8	59.8
Oct.	72.8	56.8	64.8	72.8	57.8	63.8	88.8	67.8	79.3	88.8	71.8	80.7	85.5	68.8	76.8	77.8	63.5	71.2	76.2	62.4	69.9	74.8	62.4	69.9	74.8	57.8	66.8
Nov.	69.8	58.8	65.7	71.5	59.2	66.5	90.8	68.5	75.9	91.7	69.5	77.8	84.8	67.8	74.4	76.8	65.8	71.4	75.7	64.3	70.5	74.8	64.3	70.5	74.8	60.8	67.5
Dec.	70.8	61.8	66.5	69.8	62.8	66.9	85.8	70.8	76.8	86.8	71.4	77.3	81.8	69.6	75.6	75.4	62.8	71.6	74.5	62.8	70.9	70.8	62.8	70.9	70.8	64.8	67.5

Table II.
Mean temperature of each month.

January	70.87	May	65.11	September ...	64.24
February.....	73.01	June	59.96	October	71.60
March.....	72.07	July	57.34	November ...	70.91
April	67.29	August	60.15	December ...	71.56

* The observations at 8 P.M. are not employed in obtaining the monthly means.

Table III.—Mean temperature of each of nine hours.

3 A.M.	. 61 ^o 7	4 P.M.	. . 73 ^o 74
6 61 ^o 73	6 70 ^o 42
9 66 ^o 78	8 66 ^o 85
Noon	. 73 ^o 08	9 65 ^o 86
		Midnight	. 63 ^o 07

This gives 67° as the mean temperature of the year 1846; a result which probably differs but very slightly, if at all, from the truth.

Table IV.—Comparative temperature in shade and in open sunshine.

Date.	Therm. shade.	Therm. sunshine.	Remarks.
Jan. 3, 4 P.M.	81 ^o 2	93 ^o 5	Gale N.
Feb. 8, ...	85 ^o 1	94 ^o 5	Light breeze N.E.
... 14, ...	86 ^o 7	104 ^o 5	Brisk breeze N.
... 15, Noon.	85 ^o 4	99 ^o 8	Brisk breeze W.
May 3, ...	73 ^o	86 ^o	Brisk breeze E.
... 24, ...	71 ^o 8	84 ^o	Brisk breeze N.E.
June 28, ...	63 ^o 2	80 ^o 7	Brisk breeze N.E.
July 19, ...	66 ^o 5	84 ^o 7	Light E.
Aug. 9, 4 P.M.	72 ^o	81 ^o	Light W.
... 22, ...	71 ^o	85 ^o 4	Light E.
... 23, Noon.	71 ^o 5	85 ^o 8	Brisk E.
... 30, ...	72 ^o 8	84 ^o 8	Gale E.
Sept. 27, ...	75 ^o	89 ^o 2	Brisk W.
... .. 4 P.M.	79 ^o 8	91 ^o 3	Light S.W.
Oct. 11, Noon.	80 ^o 8	96 ^o	Brisk E.
... 18, ...	77 ^o	87 ^o 5	Brisk W.
... 25, ...	84 ^o	95 ^o 2	Brisk E.
... .. 4 P.M.	87 ^o	90 ^o 5	Calm.
Nov. 15, Noon.	76 ^o 2	88 ^o 5	Light W.
Dec. 27. ...	77 ^o 4	96 ^o	Brisk N.W.

On the mornings of the 27th, 28th, 29th and 30th of July, there was sharp hoar-frost.

At 3 P.M. of the 28th of October there was a heavy fall of hail, the thermometer in the shade standing at 78°.

The first fire-fly appeared on the 7th of August (the mean temperature of the twenty-four hours being 61^o·6), and on the 11th they were numerous.

The American robin (*Sabea*) was first heard on the 4th of September, when the mean temperature was 60^o·7*.

* The song of this bird, almost our only songster, is considered by the natives a certain sign of the rainy season.

The coldest day was the 28th of July, when the mean of the twenty-four hours was 49° . The hottest was the 1st of November, which averaged $75^{\circ}\cdot 9$.

My hygrometrical observations are but few, yet they may not be entirely worthless.

Table V.

Date.	Temp. of air—shade.	Dew-point.	Remarks.
Oct. 4, Noon.	$70^{\circ}\cdot 8$	$61^{\circ}\cdot 5$	Cloudy.
... 11, 9 A.M.	70°	62°	Light clouds.
... .. Noon.	$80^{\circ}\cdot 8$	65°	Clear.
... 25, 9 A.M.	75°	$64^{\circ}\cdot 5$	Close, smoky.
... 26, 6 P.M.	$82^{\circ}\cdot 8$	$68^{\circ}\cdot 5$	Light clouds.
Nov. 1, Noon.	$86^{\circ}\cdot 8$	$67^{\circ}\cdot 5$	Hazy, thunder.
... 8, 9 A.M.	$68^{\circ}\cdot 3$	65°	Showery.
... 9, ...	71°	$63^{\circ}\cdot 5$	Showery.

Table VI.—Quantity of rain.

	No. of rainy days.	Rain.
January	20	$24^{\circ}\cdot 25$ inches.
February	22	$19^{\circ}\cdot 30$
March	15	$7^{\circ}\cdot 08$
April	15	$5^{\circ}\cdot 48$
May	10	$2^{\circ}\cdot 96$
June	6	$3^{\circ}\cdot 24$
July	11	$1^{\circ}\cdot 40$
August	3	$0^{\circ}\cdot 54$
September	10	$3^{\circ}\cdot 45$
October	5	$4^{\circ}\cdot 22$
November	22	$18^{\circ}\cdot 16$
December		$16^{\circ}\cdot 64$

Total in 1846 . . $106^{\circ}\cdot 72$

The heaviest showers during the year were

January 20, when $5^{\circ}\cdot 24$ inches fell in $10\frac{1}{2}$ hours.

October 1, ... $2^{\circ}\cdot 24$ $1\frac{1}{4}$...

... 29, ... $0^{\circ}\cdot 66$ 20 minutes.

The greatest quantity in twenty-four hours was on the 1st of December, when it amounted to $6^{\circ}\cdot 3$ inches.

No one can be more fully aware than I am that many other observations are necessary to present a perfect view of the climate; but the want of instruments, as well as other more pressing occupations, will, I hope, be a sufficient excuse for the deficiency.

W. J. HENWOOD.

Gongo Soco Gold Mines,
January 16, 1847.

LVII. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 130.]

May 11, 1846. **A** THEORY of Luminous Rays on the Hypothesis of Undulations. By the Rev. J. Challis, M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.

In this communication, the æther, which is supposed to be the medium of the transmission of light, is regarded as a continuous fluid substance, such that small increments of its pressure are proportional to small increments of density, and is treated mathematically according to hydrodynamical principles. The author shows, by means of the usual hydrodynamical equations, and by an additional equation of continuity, the existence and necessity of which he has considered in the Cambridge Philosophical Transactions (vol. vii. part iii. pp. 385 and 386), that a given slender cylindrical portion of the fluid may continue in motion without tendency to lateral spreading, while all other parts remain at rest. It is shown,—1, that the motion in this filament of fluid may be propagated with a uniform velocity; 2, that in one straight line, which may be called its axis, the motion is entirely longitudinal; 3, that at all other points the motion is partly longitudinal and partly transversal; 4, that the motion is vibratory, the vibrations both longitudinal and transversal following the law of sines; 5, that the condensation (s) in any transverse plane, at a point whose co-ordinates in that plane reckoned from the axis are x and y , is given by the equation

$$\frac{d^2s}{dx^2} + \frac{d^2s}{dy^2} + gs = 0,$$

g being a certain constant. It follows that the condensation in any transverse plane, being determined by a partial differential equation, is arbitrary, and by consequence that the transverse velocity varies at a given time from point to point of any transverse plane in an arbitrary manner. To obtain the foregoing equation, it is assumed that the condensation at any point of a transverse plane, has to the condensation at the intersection of the plane with the axis, a ratio not variable with the time.

Each fluid filament in vibration is supposed in this theory to correspond to a *ray* of light. The vibrations in different fluid filaments may co-exist, and consequently rays be propagated in the same direction independently of each other. A ray of common light has the condensation symmetrically arranged about the axis.

May 25, 1846.—A Theory of the Polarization of Light on the Hypothesis of Undulations. By the same Author.

This paper is a continuation of the foregoing. A ray in which the condensation is not arranged symmetrically about the axis is considered to be *polarized*. Polarization in this theory corresponds to difference of condensation in different directions transverse to the

axis of the ray. The sensation of light is due to the *transverse* vibrations. By assuming that the bifurcation of a ray takes place so that the transverse velocity at each point is resolved into two velocities at right angles to each other, and that these are respectively the velocities at the corresponding points of the two parts into which the ray is divided, Professor Challis finds,—1, that if the original ray be one of common light, the two parts are symmetrical about planes at right angles to each other passing through the axis, and are each of half the intensity of the original ray; 2, that if the original ray has been once polarized, the ratio of the two parts is equal to the square of the tangent of the angle which the plane of the second polarization makes with that of the first; 3, that whether the original ray be one of common light or a polarized ray, the two parts, on pursuing the same path, form a compound ray the intensity of which is independent of the difference of phase. According to this theory, elliptically or circularly polarized light is produced whenever a ray of first polarization is divided into two parts which subsequently pursue the same path in different phases. If the parts be made to meet in the same phase, they constitute the original polarized ray. Hence is explained the necessity of the analysing plate for the production of colours by polarized rays transmitted through thin pieces of uniaxal or biaxal crystals. The compound rays, if received directly by the eye on leaving the crystal, would be of the same intensity whatever be the difference of phase. But when they fall on the plate, those incident in the same phase, being equivalent to rays of first polarization, are incapable of reflexion, while the remainder, which are incident in the form of elliptically or circularly polarized light, are reflected in different degrees of intensity according to the difference of phase. The author states that he has extended this theory to the phænomena of double refraction.

On a Change in the State of Vision of an Eye affected with a mal-formation.

Twenty years ago, the author communicated to the Society a statement of the effects of a mal-formation in his left eye. The rays of light coming from a luminous point, and falling on the whole surface of the pupil, do not converge to a point at any position within the eye, but converge so as to pass through two lines at right angles to each other, and, in the ordinary position of the head, inclined to the vertical, as formerly described (*Transactions of the Society*, vol. ii.). As the luminous point is moved further from or nearer to the eye, the image of the point becomes a straight line in one or other of the positions above-mentioned. Since 1825 the inclinations of the two focal lines to the vertical, their length, and their sharpness do not appear to have undergone any sensible change, but the distances at which the luminous point must be placed to bring the focal lines respectively exactly upon the retina are increased, having been formerly 3·5 and 6 inches, and being now 4·7 and 8·9 inches. Thus while the shortsightedness of the eye is diminished the astigmatism remains the same,

“ On the Geometrical Representation of the roots of Algebraic

Equations." By the Rev. H. Goodwin, late Fellow of Caius College, and Fellow of the Cambridge Philosophical Society.

The changes of value of any function of x , $f(x)$, may be very clearly, and for some purposes very usefully represented, by tracing the curve defined by the equation $z=f(x)$; and the positive and negative roots of the equation $f(x)=0$ will be the distances from the origin at which the curve cuts the axis of x .

In this memoir a similar method is applied to the representation of the changes of value of a function of (x) , corresponding not only to real values of x , but also to values of the form $x+y\sqrt{-1}$. If we make $z=f(x+y\sqrt{-1})$, and restrict ourselves to real values of z , the equation separates itself into two, which, it is shown, may be represented symbolically by

$$z = \cos \left(y \frac{d}{dx} \right) f(x)$$

$$\text{and } 0 = \sin \left(y \frac{d}{dx} \right) f(x),$$

and these will correspond to a curve of double curvature, the intersections of which with the plane of xy will determine by the distance of those points from the origin the imaginary roots of the equation $f(x)=0$.

The properties of this curve are fully discussed for the case of $f(x)$ being equivalent to $x^n + p_1 x_{n-1} + p_2 x_{n-2} + \dots + p_n$, where p_1, p_2, \dots, p_n are real; and the following results are obtained.

1. The ordinate of the curve admits of no maximum or minimum value.
2. The curve goes off into infinite branches, which lie in asymptotic planes equally inclined to each other, and which tend alternately to positive and negative infinity.
3. Any plane parallel to the plane of xy cuts the curve in n points and no more.

From this last result the existence of n roots and no more for an equation of n dimensions is the immediate result.

Several well-known theorems are deduced from this view of the subject, and are given as illustrations.

The actual curves are traced, corresponding to the various cases of the quadratic, the cubic, and the biquadratic equations, and to the equation $x^n - 1 = 0$.

In the conclusion of the memoir it is remarked that the results obtained are not exclusively applicable to the case of algebraic equations, and the methods are applied to the case of $f(x) = \sin x$.

The author trusts that the contents of this memoir, though not adding to the number of known theorems, may yet be useful as putting the subject in a new light, and as furnishing a method of demonstrating the existence of the roots of algebraic equations more simple and direct than any other which he has seen.

LVIII. *Intelligence and Miscellaneous Articles.*

ELECTRICAL EXPERIMENTS.

PROF. HENRY, of Princeton, communicated to the American Philosophical Society the *result* of a series of experiments on electricity made last winter. They had reference, first, to the discharge of electricity through a long wire, connected with the earth at the farther end; secondly, to the discharge of a jar through a wire; and thirdly, to an attempt to account for the phenomena of dynamic induction.

Prof. Henry first showed, that when a charge of electricity is given to one end of a wire, the different parts of the wire become charged successively, as though a wave of electricity passed along it. He then showed that the charge passed along the surface of the wire, and not through its whole mass, as was supposed from the analogy of galvanic conduction. Hence he inferred that dynamical electricity obeys the same laws as the statical. He then detailed some experiments upon the passage of electricity through plates, and showed that when a charge was transmitted across a plate, the tension was greatest at the edges, the electricity apparently exercising a self-repelling action, while, if the charge were passed through two pieces of tinfoil, these slips attract each other.

Prof. Henry believes that it may be justly inferred, from these experiments, that the attraction is due to ponderable matter, while the repulsion is due to electricity; thus showing that electricity is a separate principle, and not a mere property of matter.

Prof. Henry next passed to the subject of the discharge of a jar. It was necessary, in his experiments, to get rid of the free electricity arising from the thickness of the glass, and it occurred to him that this might be done by removing the knob, and making the coating upon the inside of less area than that upon the outside. With this arrangement, when the discharge was made through a long wire, and a test jar brought near it during discharge, a bright spark passed; but upon approaching the jar to a delicate electrometer, it gave no indications of free electricity. Reflecting upon this, and upon an experiment of Prof. Wheatstone, he was led to believe that the jar is discharged by two waves, a negative and a positive one, starting simultaneously from the two ends of the wire. To prove this, he broke the wire, and interposed a pane of glass dusted with red lead and sulphur; two figures of positive and negative electricity were produced. He made several other experiments tending to prove this same fact. He showed how these experiments serve to explain that of Dr. Priestley, where a spark was found to pass between the ends of a long bent wire, the ends being brought within a few inches of each other.

He next passed to the connexion between statical and dynamical induction. Statical induction has heretofore only been observed at short distances. Prof. Henry's first experiment proved that it could be observed at the distance of nineteen feet, the floor of a chamber intervening, showing that statical induction takes place at great distances,

though not at so great distances as the dynamical. He then explained his views of the nature of dynamical induction. When a spark is thrown upon a wire, it passes in a wave, whose length might be determined if we knew the velocity of electricity; now, if we have another parallel wire, a negative wave will be formed in this, and the two waves will travel simultaneously in the same direction. But this is equivalent to a positive induced wave in the opposite direction. In this way the phenomena accompanying the discharge of a jar are easily explained. Again, if we conceive that in a galvanic battery the discharge consists of a series of such waves, we may very simply explain the phenomena of galvanic induction.

ON THE EXAMINATION OF THE ASHES OF ORGANIC BODIES.

BY H. ROSE.

Considerable attention has recently been paid to the analysis of the ashes of plants and animal substances. We now possess a very large number of quantitative analyses of different ashes, which may considerably enlarge our knowledge of the chemical composition of the inorganic constituents of organic substances. Frequently, however, very incorrect conclusions are liable to be drawn from the results of these analyses, as they have hitherto been made, with respect to the constituents in the organic body. This observation struck me on perusing M. Enderlin's paper*, who found in the ashes of the blood only phosphate of soda ($3\text{NaO} + \text{P}^2\text{O}^5$), phosphate of lime and magnesia, peroxide of iron and perphosphate of iron, sulphate of soda, sulphate of lime, chloride of sodium and potassium, but no alkaline carbonates. From the results found he asserted the total absence of carbonated alkalies and of alkaline salts with organic acids in the blood. It is evident, however, that these conclusions may be erroneous, in so far as at a high temperature the ordinary phosphate of soda ($2\text{NaO} + \text{HO} + \text{P}^2\text{O}^5$) is capable of expelling the carbonic acid from the carbonate of soda and forming basic phosphate of soda.

This observation, respecting the conclusions which M. Enderlin has drawn from his investigations, has already been made by several persons. Marchand† has attempted to refute Enderlin; Berzelius, in his most recent report, likewise draws attention to the subject; and Golding Bird‡ has prepared the basic phosphate of soda ($3\text{NaO} + \text{P}^2\text{O}^5$) by heating the ordinary phosphate of soda ($2\text{NaO} + \text{HO} + \text{P}^2\text{O}^5$) to redness with acetate of soda.

I have also examined the ashes of the blood (ox-blood), but have obtained very different results from those of M. Enderlin. The process I adopted was the following:—I exposed the blood in a covered platinum crucible to a very faint red heat, extracted the cold mass with water, and evaporated the colourless liquid to dryness; it consisted of alkaline chlorides and carbonates, with very minute quantities of alkaline sulphates and phosphates. The charred

* Chem. Gaz., vol. iii. p. 230.

† *Ibid.*, vol. iv. p. 209.

‡ Phil. Mag., vol. xxvi. p. 532.

mass, extracted with water, was now treated with hydrochloric acid; the filtered solution did not yield with ammonia a very considerable precipitate, which, although it looked almost like pure hydrated oxide of iron, contained some phosphoric acid as well as lime and magnesia. In the filtered solution, I obtained with oxalate of ammonia a pretty considerable precipitate of oxalate of lime, proving the presence of carbonate of lime in the charred blood; and in the liquid separated there was also a small quantity of magnesia. The cinder, after treatment with water and hydrochloric acid, yielded a very considerable quantity of a red-coloured ash on being burnt in an atmosphere of oxygen. It was in a semifused state, and contained peroxide of iron (which formed the chief part), earthy and alkaline phosphates*.

If the fixed constituents in plants are examined according to the above process, totally different results are frequently obtained to those yielded by the ash analyses which have been published up to this time. The ashes of peas have been examined by Fresenius and Will, Bichon, Thon and Boussingault; none of them enumerate carbonic acid among the constituents. From the investigations which have been made by Drs. Gibbs and Bromeis in my laboratory, the amount of carbonic acid in the salts which are extracted from the charred peas by water amounts to somewhat more than 27 per cent. Evidently, in the former method of reducing to ash, the carbonic acid has been entirely expelled. Phosphoric acid is only present in small quantity in the aqueous extract of charred peas. When the charred peas are subsequently treated with hydrochloric acid, a solution is obtained which contains a moderate quantity of earthy phosphates. If the charred peas are now perfectly reduced to ash in an atmosphere of oxygen, a very considerable quantity of ash is obtained, which consists principally of earthy and alkaline phosphates.

That the method of preparing the ash of organic substances has considerable influence upon the composition of the ash has already been noticed by several chemists, especially by Erdmann†, who showed that acid phosphates, which yielded white precipitates with nitrate of silver by ignition with carbon, lost a considerable portion of the phosphoric acid, and then produced a yellow precipitate in the solution of silver; he observed further, that chlorine and sulphuric acid might be contained in very different quantities in the ashes, according to the mode of preparing them; and that in the ashes of many seeds not a trace of chlorine had been found, while the aqueous extract of the seed contains very perceptible quantities of chloride of sodium. Mitscherlich‡ has likewise drawn attention to several circumstances, by which, in incinerating organic substances, the ashes are frequently decomposed and rendered impure.

The mode of determining the fixed constituents of an organic substance, as above described, appears to me more advantageous

* After these observations had been penned, I observed that Lehmann had proved the presence of alkaline carbonate in the blood by a different method.— See *Chem. Gaz.*, vol. v. p. 133.

† *Chem. Gaz.*, vol. iv. p. 230.

‡ *Ibid*, vol. iv. p. 69.

and far more rational than the methods usually employed. It may be objected to it, that it takes more time; but not only are far more correct results obtained by it, but it is also capable of answering several questions as to how or in what combinations the constituents found in the ash were contained in the organic substance. According to the method which I propose, the organic substance is charred at a very faint red heat, so that the water with which it is extracted is not coloured yellowish or brownish. At this temperature, which, owing to the volatilization of so many substances, is much lower than would appear, no alkaline chlorides are volatilized, nor can chlorine be expelled from them in the form of hydrochloric acid by acid phosphates. The alkaline and earthy phosphates are not able to expel the carbonic acid from the alkaline carbonates, either contained in the organic substance or formed by the charring; nor can phosphoric acid be eliminated from its combinations by silica, reduced by carbon, and volatilized in the form of phosphorus. The charring is effected either in a spacious covered platinum crucible over a spirit-lamp, or, with larger quantities of the organic substances, in a spacious covered Hessian crucible, especially if they do not fuse. When there is no longer much empyreumatic odour perceptible, the heating is discontinued, the cold mass left for some time in contact with water, and the solution of the soluble salts furthered by heating; the edulcoration requires considerable time and much hot water; but if the highest degree of accuracy is not desired, the edulcoration may be discontinued when several drops of the wash-water leave a scarcely perceptible residue on evaporation upon a slip of platinum; this point is very soon attained.

The aqueous extract contains the alkaline salts. The alkaline chlorides were contained as such in the organic substance previous to the charring, as well as at least a part of the alkaline sulphates and phosphates. If, as in most cases, carbonated alkali is found in the aqueous extract of the charred mass, it either pre-existed in the organic substance, or the alkali in it was combined with an organic acid or some other organic body, which acted the part of an acid towards the alkali. If the organic substance contain sulphate of lime, this, when carbonated alkalies are present in sufficient quantity in the charred mass, is converted, on treating the latter with water, into carbonate of lime and alkaline sulphate. In the same way, when phosphate of lime is present, a certain quantity of alkaline phosphates is formed from it in the aqueous extract by the alkaline carbonates. Carbonated alkali and phosphate of lime are not perfectly decomposed even by fusion at very high temperatures. The decomposition in presence of much water is likewise imperfect, and the more alkaline phosphate is obtained in the aqueous extract, the more concentrated the solution, the more carbonated alkali it contains, and the longer the charred mass has been digested at an elevated temperature. Alkaline sulphates and phosphates will however be found in far smaller quantities in the aqueous extract of the charred mass than was to be expected from the ash analyses that have been hitherto published. Frequently the two, and especially

the latter, are present only when too high a temperature has been employed in the charring. From this however it is evident that the nature of the salts in the aqueous extract may vary somewhat, according to the temperature employed and the longer or shorter digestion of the mass with water. When the charring is effected at too high a temperature, the greater portion of the carbonated alkalis are decomposed by the earthy phosphates.

The accurate examination of the salts in the aqueous extract is not accompanied with any great difficulties. One circumstance however renders it somewhat less easy; carbonate and phosphate of lime and magnesia frequently dissolve to a considerable extent in neutral solutions of alkaline salts, particularly of alkaline carbonates and phosphates; in the course of time they are deposited from the solutions, especially after the application of heat. When therefore the aqueous extract is evaporated, it frequently becomes somewhat turbid, and deposits small quantities of earthy salts. It should consequently be evaporated nearly to dryness, diluted with water, and the solution set aside for some time; when the earthy salts have subsided, it is filtered, the filtered solution evaporated to dryness, and its weight determined. When there is no alkaline phosphate or sulphate present, the examination is very easy. The quantity of carbonic acid is determined in a suitable apparatus by decomposition with nitric acid; and upon this that of the chlorine by a solution of silver; upon which, after removing the oxide of silver by hydrochloric acid and concentrating the liquid, the potash may be separated from the soda by chloride of platinum. With the presence of alkaline sulphate or phosphate, it is advisable to divide the quantity of the alkaline salts, and in the one half to determine the quantity of the chlorine and the alkalis, and in the other that of the carbonic acid by decomposition with hydrochloric acid; that of the sulphuric acid by a salt of barytes; and after removing the baryta by means of sulphuric acid, and supersaturating with ammonia, to ascertain the quantity of phosphoric acid by means of a solution of a salt of magnesia to which chloride of ammonium has been added.

The charred mass, exhausted with water, is now digested with hot hydrochloric acid for some length of time, and then washed with water. This operation requires considerably more time and water than in the treatment of the charred mass with water; and if the washing were to be continued until some drops of the filtered liquid no longer produced any opalescence in a solution of silver, an enormous length of time, several months, would be required, especially in operating upon large quantities. Theedulcoration therefore is only continued until a considerable quantity of the wash-water does not exhibit a trace of precipitate when treated with ammonia; it will then also be seen that a large quantity of the wash water, when evaporated upon platinum, no longer leaves any perceptible residue: this does not require much time, especially when hot water is used.

The acid solution contains the earthy phosphates which existed as such in the organic substance and the peroxide of iron. It is precipitated by ammonia, and after having determined the weight

of the precipitate, the bases are separated from the phosphoric acid. I shall subsequently describe in these pages a method by which this may be accomplished without any difficulty. On separating the earthy phosphates by means of ammonia, a small quantity remains dissolved in the filtered liquid, owing to the presence of chloride of ammonium; consequently upon adding oxalate of ammonia, a precipitate of oxalate of lime is obtained, but its quantity is usually larger than corresponds to the phosphate of lime dissolved by the chloride of ammonium; consequently a portion of the lime existed as carbonate of lime in the charred mass, or was formed by the decomposition of the sulphate and phosphate of lime by the alkaline carbonates. The liquid filtered from the oxalate of lime indicates, on the addition of a solution of phosphate of soda, the presence of some magnesia. The insoluble earthy salts which separated from the aqueous extract may be examined conjointly with those in the acid extract.

With respect to the charred mass, which has been exhausted with water and hydrochloric acid, it would be imagined that it could contain only silica or silicates undecomposable by dilute hydrochloric acid; but it yields a very large amount of ash on complete combustion, even when the organic substances contain no silica or mere traces.

I formerly effected the perfect combustion of the cinder in hard glass tubes, in which the mass was heated to redness while a current of oxygen was passed over it; this plan however has its inconveniences. If the quantity of organic substance employed is moderately large, so much cinder is obtained, that a glass tube, even of large diameter, would require to be filled with it several times to burn it entirely; moreover, the tube is very much acted upon, and rarely stands a second heating. The combustion is in most cases very imperfect, and requires much time if but a slow current of oxygen be passed; a rapid current is requisite for the combustion to succeed well; but then a considerable quantity of ash may readily be carried away, especially if it is of a very light nature; this, it is true, may be partially prevented by passing the gas on its exit through a stratum of water, by varying the height of which a different pressure may be obtained; but this does not entirely prevent the removal of the ash, and if the pressure is too great, the glass tube is liable to bulge when strongly heated*. But the greatest disadvantage attending the use of glass tubes is, that it is impossible to collect the ashes so as to determine their weight accurately. Frequently the ash has undergone slight fusion; it can then only be imperfectly separated from the glass by mechanical means or the use of solvents. The use of thin platinum or silver foil, which is introduced into the glass tube previous to the coal being placed in it, does not entirely remove these serious disadvantages, and gives rise to others. I therefore employ for the combustion of the charred mass a different method, with the results of which I am perfectly

* The water employed for this purpose contains cyanogen compounds when the charcoal is derived from nitrogenous bodies.

satisfied. For some time past I have no longer employed bulb tubes in reductions by means of hydrogen gas, but place the substance to be reduced in a platinum or porcelain crucible, provided with a platinum lid, which is perforated in the centre, into which passes a curved silver tube about 8 inches long, through which the dried hydrogen is conveyed into the crucible. During ignition the gas escapes between the lid and crucible. Ebelmen* has recently employed a similar apparatus for the reduction of the sesquioxide of manganese to the state of protoxide. This apparatus can be used with considerable advantage for burning the charred mass. The crucible is half-filled with the substance, and heated over a spirit-lamp, while a current of oxygen is passed into it; with proper care not a particle of ash is carried away, and the combustion proceeds very rapidly; a further quantity of the substance is conveyed from time to time into the crucible. By this means the ash obtained may be weighed with such accuracy as would not be easily accomplished in any other way.

When the combustion is effected in a platinum crucible, this is sometimes acted upon; a silver crucible is liable to partial fusion from the heat during the combustion; it is therefore advisable to employ a porcelain crucible, which is acted upon far less than glass; and if very thin and somewhat transparent, the progress of the combustion may be distinctly observed by the incandescence.

The weight of the ash obtained, added to that of the evaporated aqueous extract of the charred mass and to that of the insoluble earthy salts dissolved by the hydrochloric acid, gives the correct quantity of fixed constituents in the organic substance employed.

The ash obtained, especially when derived from vegetable substances, consists of the same constituents as were found in the aqueous and acid extracts; if alkalies were present in them, we likewise find them in the ash of the exhausted charred substance; otherwise it consists principally of earthy phosphates. I have already mentioned that nearly the whole of the iron of the blood is met with in this ash. Only about the tenth part of it is found in the acid extract of the charred mass, and indeed the less the more carefully the charring was effected with exclusion of the air. When the organic substance contains no silica, various views may be entertained respecting the origin of the ash from the charred mass which has been exhausted with water and acid. The most probable is perhaps to derive it from an imperfect exhaustion with the two solvents. When an organic substance is destroyed by heat, the charcoal formed may contain such cavities that the inorganic salts surrounded by them are protected from the action of the solvents. The globules of the blood, those of yeast, the cells of plants, form perhaps after charring extremely minute vesicles, with such small apertures that no liquid can penetrate into them. That the vessels of wood are capable of forming extremely thin filaments with minute apertures by charring is known from the investigations of Degen†. The charred mass of an organic substance (yeast), after it had been most carefully ex-

* Chem. Gaz., vol. i. p. 685. † Poggendorff's Annalen, vol. xxxv. p. 468.

hausted by water and hydrochloric acid, was ground to the very finest powder upon a plate of agate; the two solvents now extracted only imperceptible traces of fixed constituents, and after burning the exhausted charred mass I obtained the same large amount of ash as from the non-pulverized charcoal. It may nevertheless be supposed that the extremely minute vesicles were not destroyed and torn by the friction upon the agate plate. The microscope threw no light upon the subject.

It is known that charcoal is capable, by a weak kind of affinity, of removing certain salts from their solutions; I have also mentioned above, that when the charred substance has been treated with hydrochloric acid, it is almost impossible to remove the acid by washing with water; but the quantity of ash is too considerable for us to ascribe this origin to it, since it is known that the salts which the charcoal has combined with may be entirely separated by long treatment with water at different temperatures. I mixed sulphate of potash and phosphate of lime with sugar, and charred the mixture. Water, and after this hydrochloric acid, extracted the two salts so completely from the charred mass, that the latter, after combustion in an atmosphere of oxygen, left not a trace of ash.

Several acid phosphates (metaphosphates) are insoluble in hydrochloric acid after ignition; to this cause might be owing the residuary ash of the charred mass; but although phosphoric acid is almost constantly met with in this ash, it is not always combined with the bases in the form of acid salts; moreover, the ash itself, when it does not contain silica, is soluble in hydrochloric acid after the salts have been exposed, in the combustion of the carbon in oxygen, to a far higher temperature than that employed in charring the organic substance.

Ignited peroxide of iron is, it is true, not insoluble in hydrochloric acid, although very sparingly soluble; its non-extraction by acid from the charred blood might be attributed to this cause; but supposing it to be contained in the state of peroxide of iron, not only must it be more readily soluble in hydrochloric acid, owing to its finely divided state, but the oxide of iron which is found in the ash after the combustion of the coal, and which has been exposed to a very high temperature, is soluble in hydrochloric acid.

The alkaline and earthy phosphates which are found in the ash of the exhausted charred mass cannot have been contained in the form of metallic phosphurets of a peculiar kind mixed with coal, like the phosphuret of iron, which is perfectly insoluble in hydrochloric acid. It might be assumed that they were formed by the reduction of the alkaline and earthy phosphates by the carbon. It is scarcely necessary to notice this assumption, for it is well known that the neutral and basic alkaline and earthy phosphates are not reduced by carbon even at a high temperature, at least not in the presence of silica or any similar fixed acid. But even the excess of phosphoric acid cannot be reduced by carbon from the acid phosphates at the temperature which I employ in charring organic substances. In the case of the blood, however, phosphuret of iron

might be formed by the reduction of the phosphate of iron by the carbon. Although I was convinced that this could not happen at the temperature which I employed, I mixed phosphate of iron intimately with sugar, and charred the mixture at a higher temperature; hydrochloric acid, however, extracted the salt so completely from the charred mass, that this burnt without any residue in an atmosphere of oxygen.

It might be supposed that the salts found in the ash of the exhausted charred substances are so intimately combined with organic substances that they can only be detected by reagents after the complete destruction of the latter. But the organic substances are so destroyed by the charring, that if the inorganic salts found in the ash pre-existed as such in them, they should have been extracted by those agents in which they are soluble.

There still remains one view respecting the origin of these ashes. The salts found in them may perhaps not have pre-existed as such in the organic substances, but were first formed by oxidation after the burning of the coal. It has long been known that the proteine compounds, of both animal and vegetable origin, contain sulphur and phosphorus in an unoxidized state; but, as far as I am aware, the supposition has never been advanced, that the radicals of the earths and alkalis may likewise be contained in organic substances in an unoxidized state, perhaps combined with those elements. These would certainly constitute a very peculiar class of combinations, such as we are at present not acquainted with. If they are really combined with organic substances in the living body, they cannot have been essentially altered on destroying the organic body by charring, or they have entered into combinations with carbon and nitrogen, which are insoluble in water and in hydrochloric acid.

I have already observed that the salts found in the ash of the charred mass exhausted with water and acid, especially when derived from vegetable substances, are similar to those which occur in the aqueous and acid extract. This view can only be confirmed by a long series of investigations; but if it should be confirmed, then those salts which we find in the ash after the destruction of the living plants were probably contained in them only in part as such, and in part in an unoxidized state. The inorganic salts, therefore, which are taken up from the soil by the living plant, are partially deoxidized by it, and in this state form combinations with organic substances contained in the plant.

This view is far more probable with respect to several animal substances, especially the blood, than in reference to plants. It has long been suspected that the iron in the blood was contained in it in an unoxidized state; and, according to the recent investigations of Mulder, the iron is actually extracted by acids from hæmatine with evolution of hydrogen gas. This view acquires still greater probability from the experiments which I have related. On the other hand, it is very remarkable that the iron cannot be extracted from the charred blood by hydrochloric acid. I think it would be

extremely interesting to pursue this subject further. From what has been stated, it results that the view which I have advanced can neither be confirmed or refuted by any number whatever of analyses of the ashes of plants, which have been prepared according to the methods hitherto in use, but only by a process similar to that I have described, and which I have therefore pronounced a far more rational method of investigation.

I have already shown that frequently very considerable quantities of alkaline carbonates are extracted by water from several organic substances after charring, in the ashes of which no carbonic acid was found by former investigators. But all organic substances do not yield alkaline carbonates when treated in this manner, even though considerable quantities of alkali are contained in their ash. Highly remarkable in this respect is yeast, the ash of which, according to Mitscherlich*, contains no carbonic acid and no metallic chlorides; and my experiments show that they are likewise not to be found in the aqueous extract of the charred yeast. Yeast diffuses, on being charred, an odour similar to that of the proteine compounds; the aqueous extract did not turn litmus-paper blue, became turbid on evaporation, and deposited a large quantity of earthy phosphates. The mass, evaporated to dryness, yielded on filtration a clear solution, which faintly reddened litmus-paper, and contained therefore not a trace of alkaline carbonates; I could only find in it alkaline phosphates, with very minute traces of alkaline sulphates and chlorides. The charred mass gave, on treatment with hydrochloric acid, a solution, from which ammonia threw down a considerable precipitate of earthy phosphates. The cinder, exhausted with water and acid, furnished on combustion a very large quantity of ash, which contained the same constituents which had been extracted from the charred mass. These experiments, the results of which entirely agree with those obtained by Mitscherlich, were made with top-yeast, which had been perfectly purified by washing. Other results may perhaps be obtained by using unwashed yeast for the experiments; for the beery liquid, separated from the yeast by filtration and evaporated, did not diffuse on charring the same disagreeable odour as the yeast itself, or as nitrogenous substances generally at a high temperature. The evaporated solution, extracted with water from the charred mass, contained a large amount of carbonated alkali, and effervesced therefore strongly with acids, with much chloride of potassium, but only a little phosphate of potash. The charred mass, exhausted with water and hydrochloric acid, yielded a tolerable quantity of a light ash containing phosphates and a large quantity of silica, which latter element Mitscherlich likewise found in the ash of beer†.

* Chem. Gaz., vol. iv. p. 69.

† After this paper had been read before the Royal Academy of Berlin, I received a letter from Berzelius, to whom I had communicated the principal results, in which he states that he advanced a view similar to that which I have proposed on the ashes of the blood, more than 40 years ago, in his work on animal chemistry. It occurs also in Schweigger's 'Journal für Chemie und Physik,' vol. ix. p. 391, 1813. It has however never been expressed in the various editions of Berzelius's 'Manual of Chemistry.'

SUGAR IN HEALTHY BLOOD.

In the *Phil. Mag.* for May 1845, p. 422, are detailed some experiments by Dr. R. D. Thomson, which show that when starch has been digested in considerable quantities by animals, it passes into the condition of soluble starch, or dextrine and sugar, and being absorbed in the latter form into the blood, can be detected in that fluid during the period of digestion. The experiments detailed in the paper referred to were made in 1844. Magendie has lately, in a paper communicated to the French Academy (*Comptes Rendus*, xxiii. p. 189), obtained similar results. He found that when a dog was fed on cooked potatoes, the blood contained dextrine and grape-sugar. He observed also, that if starch be mixed with fresh serum, it is so transformed in a few seconds that it cannot be detected by reagents, and in a quarter of an hour sugar makes its appearance. This exactly corresponds with the previous results obtained by Dr. Thomson in 1844, who "was unable to detect any traces of starch in the serum of the blood" (*Phil. Mag.*, May 1845, p. 420), but easily obtained evidence of the presence of sugar in the same blood.

CHEMICAL ACTION OF LIGHT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The chemical action of light has of late attracted so much attention, that any fact connected with the subject will be received with interest by those engaged in researches on this branch of science, which induces me to ask the favour of your publication of the following.

I have been successful in obtaining well-defined photographic impressions on highly sensitive Daguerreotype plates, on which the object, when illuminated by a common *dip* candle, was impressed in ten minutes; with the smallest fish-tail burner of coal gas in three minutes; and by the oil lamp, viz. a solar lamp, in the same time. I included each flame in the picture by which they have recorded their size, and to some extent their illuminating power.

I at present confine myself to the recital of these facts, as I am following up my researches on the subject. I beg to forward the plates for your inspection*.

I am, Gentlemen,

234 Regent Street,
April 20.

Your most obedient Servant,
W. E. KILBURN.

ON THE COMPOUNDS OF CYANOGEN. BY M. AD. WURTZ.

Formation of Cyanuric Acid.—When a current of dry chlorine gas is passed over fused urea, very energetic reaction occurs; the urea is decomposed, swelling up and emitting abundant white vapours. The products of this action are cyanuric acid, hydrochlorate of am-

* These we have received. They appear to us very perfect, and the effect highly remarkable.—Ed.

monia, hydrochloric acid and azote. To separate the cyanuric acid, it is sufficient to treat the cooled mass, which remains in the vessel in which the decomposition was performed, with a little cold water. The hydrochlorate of ammonia dissolves in the water, and the cyanuric acid remains in the form of a white powder, which, if requisite, may be purified by solution in boiling water. The following equation explains the reaction of the chlorine upon the urea :

$3 (C^2 H^4 Az^2 O^2) + 3Cl = Az + H Cl + 2H^4 Az Cl + C^6 Az^3 H^3 O^6$.
The author considers this as the most convenient process for preparing cyanuric acid.

Chlorohydruret [hydrochlorate ?] of Cyanogen.—When a current of chlorine gas is passed into a solution of hydrocyanic acid, prepared by Trautwein's process, a slight increase of temperature occurs after the operation has been some time continued. The liquid emits a very perceptible odour of chloride of cyanogen, and a vapour is formed which condenses in small drops in the cold parts of the apparatus, and these constitute the principal product of the reaction: to collect it, a tube with chloride of calcium is to be adapted to the retort which contains the hydrocyanic acid, bent at a right angle, and passed into a long-necked receiver cooled by ice.

When the operation is finished, the receiver contains a liquid which is limpid, very volatile, fumes on exposure to the air, and emits a very irritating odour of chloride of cyanogen: this is impure chlorohydruret of cyanogen. It contains hydrochloric and hydrocyanic acids, from which it is freed by agitating it with two or three times its volume of cold water. The stratum of liquid which separates from the water is to be poured off, and submitted to a fresh distillation, its vapour being passed through a tube containing chloride of calcium.

The chlorohydruret of cyanogen thus prepared is a colourless, very fluid and corrosive liquid. It emits an odour which strongly irritates the bronchiæ, and especially the eyes. It boils at 68° F. Its vapour burns with a violet flame. It dissolves sensibly in water, and the solution precipitates nitrate of silver white.

When brought into contact with dry chlorine, it is entirely converted into solid chloride of cyanogen and hydrochloric acid, $C^6 Az^3 Cl^2 H + Cl = H Cl + C^6 Az^3 Cl^2$.

If this reaction takes place with some grammes of the substance, the sides of the vessel will be covered the next day with fine radiating needles, and there remains at the bottom a viscid liquid, which eventually becomes a mass of large crystals of chloride of cyanogen.

M. Wurtz states that he had some difficulty in analysing the chlorohydruret of cyanogen. This will readily be conceived, when the extreme volatility of this liquid is considered. The experiments performed by the author led to the formula $C^6 Az^3 Cl^2 H$, and this appears to be confirmed by the reactions which the substance undergoes.

This substance may therefore be considered as a compound of hydrocyanic acid with chloride of cyanogen, $C^3 Az^3 Cl^2$, the description of which will presently be given; or it may be regarded as solid

chloride of cyanogen, $C^6 Az^3 Cl^3$, in which one equivalent of chlorine is replaced by one equivalent of hydrogen.

Liquid Chloride of Cyanogen is prepared by treating the chlorohydruret of cyanogen with binoxide of mercury. In order to avoid too violent reaction, it is proper to mix this oxide with fused and powdered chloride of calcium, and to render the mixture very cold. After remaining for some hours, the mixture is to be distilled by means of a water-bath, and in the receiver, properly cooled, a colourless liquid condenses, which is a new modification of chloride of cyanogen.

It is a limpid liquid, which strongly irritates the mucous membrane of the bronchiæ, and occasions the eyes to water. It is more dense than water. It boils at $61^\circ F.$, and at $20^\circ F.$ becomes a solid mass of long crystalline laminae. Its vapour is incombustible. It falls to the bottom of a vessel of water, but is sensibly soluble in it. The solution does not precipitate nitrate of silver. The reactions described seem to indicate that the alkalies decompose the chloride of cyanogen into alkaline chloride, ammonia and carbonic acid. It is well-known that the two latter bodies are products of the decomposition of cyanic acid. Analyses performed by the author led him to conclude that the composition of liquid chloride of cyanogen is expressed by the formula $C^4 Az^2 Cl^2$. It appears therefore to be a new isomeric of chloride of cyanogen.—*Comptes Rendus*, Mars 11, 1847.

RESEARCHES ON HYDRATES, STANNIC AND METASTANNIC ACIDS. BY M. E. FREMY.

The author inquires whether it is to be generally admitted, that all acids which, by becoming anhydrous, have lost their property of combining with bases, are no longer to be considered as acids. In order to determine the acidity of anhydrous acids, almost all such as are known were made to act upon anhydrous bases, or on salts containing acids less fixed than those employed; the acids used were the carbonic, sulphurous, sulphuric, phosphoric, silicic, boracic, stannic, &c. These were found to possess all the characters of acids, without the intervention of water: thus, to cite one example, it was found that perfectly dried sulphurous acid completely decomposed carbonate of soda, when gently heated, forming sulphite of soda, which was decomposable by an anhydrous and more fixed acid.

It being established that a certain number of acids preserve their acid reaction when rendered anhydrous, the author examined whether certain salts did not exist in which water was indispensable; and he is of opinion that several classes of salts exist, the molecules of which are unquestionably ternary, and always formed by the combination of an acid, a base and water.

Supposing that if energetic acids, such as the sulphuric and nitric, form with equally energetic bases, salts which are almost always anhydrous, it appeared questionable whether the same would occur with acids whose affinity for basic water is but slight. The author's attention was directed to those hydrated metallic oxides which are soluble

in the alkalies, and which may be considered as weak acids. It was found that these bodies possess the property of combining with bases in the state of hydrates only. Thus, hydrated binoxide of copper dried in the air, the formula of which is $\text{Cu O}^2\text{HO}$, is perfectly soluble in excess of potash and soda; the solution is of a fine blue colour, and when evaporated, even *in vacuo*, it is decomposed, and deposits oxide of copper, which is insoluble in the alkalies. The hydrates of oxides of tin, antimony and chromium, SnO , HO ; Sb O^3 , HO ; Cr^2O^3 10HO , dissolve in the alkalies, and all become insoluble by losing water. The author satisfied himself that the insolubility was owing to the separation of the water, and not to the isomeric modifications which certain oxides undergo by calcination.

It was found impossible to obtain crystalline compounds of the above-mentioned hydrates with the alkalies; for by evaporation, even *in vacuo*, the alkali which it is requisite to employ in excess combines with the water of the hydrates, or in acting perhaps only by its presence occasions the precipitation of the anhydrous oxide.

The preceding compounds yield, however, incontestable proof of the existence of saline groups, which are not possible without the presence of water: they demonstrate that certain hydrates owe their solubility in the alkalies entirely to water of hydration.

The new facts stated establish marked differences between stannic acid and the acid which M. Fremy calls *metastannic acid*; and this he has found forms three different hydrates with water: the first is insoluble in nitric acid and soluble in ammonia; it is obtained by precipitating a metastannate by an acid; the second is produced by the reaction of nitric acid upon tin; its formula is Sn^5O^{10} , 10HO ; it is insoluble in ammonia and nitric acid; the third is prepared by drying the preceding hydrate at 266°F .; its formula is Sn^5O^{10} , 4HO .

Neutral metastannates, formed in the presence of great excess of alkali, have for their formula Sn^5O^{10} , MO , 4HO , whereas stannates are represented by $\text{Sn O}^2\text{MO}$. The equivalent of metastannic acid is then five times greater than that of stannic acid. The metastannates are always hydrated, are necessarily ternary, and decompose when dehydrated: in this case the metastannic acid loses its acidity, whereas stannates, like most other salts, may be rendered anhydrous without decomposing.

The essential action of water on the constitution of the metastannates, may be readily shown by the following experiments. If metastannate of potash be gently heated, so as to take away its water of combination, the separation of the acid and base is immediately effected: on treating the dehydrated salt with water, potash only is dissolved, which does not contain a trace of metastannic acid, and this acid remains in an insoluble state. In this case, therefore, a decomposition is effected upon a definite salt, similar to that which the binoxide of copper undergoes in potash when heated: these two phenomena are precisely of the same description.

The dehydration of metastannate of soda is perhaps still more remarkable, for it takes place below 212°F . This salt is soluble in

cold water; but if it be added to boiling water, it decomposes instantaneously, and the water contains pure soda only.

The author is of opinion that he has stated an important fact, which is, that the same oxide, by combining with different proportions of water, can form two acids which are distinguished by their properties, their equivalent, and their mode of combining with bases.—*Journ. de Pharm. et de Ch.*, Mars 1847.

ON SULPHOXIPHOSVINIC ACID AND ITS COMPOUNDS.

BY M. CLOEZ.

The author remarks that he and M. Bouyet had described a new kind of salts, the acid of which they designated sulphoxiarsenic acid, consisting of arsenic, oxygen and sulphur, water being deducted. The sulphoxiarsenate of potash, which was obtained by passing sulphuretted hydrogen into a solution of arseniate of potash, has for its formula $As O^3 S^2, KO + 2HO$.

The great analogy existing between the corresponding compounds of arsenic and phosphorus, rendered it probable that sulphoxiphosphates might also be formed. After many trials, M. Cloez succeeded in producing these salts by decomposing the chlorosulphuret of phosphorus of Serullas by an alkaline solution.

The chlorosulphuret of phosphorus, treated with common alcohol, yields an acid analogous to the phosphovinic acid of M. Pelouze. According to M. Cloez the formula of this acid is $PhO^3 S^2, C^4 H^5 O, 2(HO)$; it is the sulphoxiphosphovinic acid. With pyroxylic spirit sulphoxiphosphomethylic acid is formed. Amylic alcohol should produce the corresponding compound, sulphoxiphosphamylic acid.

By saturating the sulphoxiphosphovinic acid with the carbonate of barytes, lime, strontia, &c., well-defined crystalline salts are produced: the formula of the barytic salt is $PhO^3 S^2, C^4 H^5 O, 2BaO + Aq$.

The potash and soda salts are readily obtained by decomposing the chlorosulphuret of phosphorus with an alcoholic solution of potash or soda. These salts are soluble in alcohol. The following equation explains the reaction which occurs with potash: $Ph Cl^3 S^2 + C^4 H^6 O^2 + 5 (KO) = Ph O^3 S^2, C^4 H^5 O 2 (KO) + 3(KCl) + HO$.—*Comptes Rendus*, Mars 8, 1847.

ON ANHYDROUS HYDROFLUORIC ACID.

M. Louyet states that in 1846 he mentioned to the Academy that, in his opinion, anhydrous hydrofluoric acid was as yet unknown; in proof of its correctness, he showed that the acid supposed to be so, saturated a smaller quantity of a base than it ought to do, if it had been anhydrous.

This proof not having satisfied M. Dumas, the author was advised by him to pass hydrofluoric acid, hitherto regarded as anhydrous, over anhydrous phosphoric acid, and to collect the product in a platina vessel immersed in a freezing mixture. According to M. Dumas, if by this process a liquid hydrofluoric acid was always ob-

tained, it would be necessary to admit that the hydrofluoric acid, supposed by MM. Gay-Lussac and Thenard to be anhydrous, was really so.

M. Louyet performed this experiment by the aid of an apparatus which he describes, and the result of it is, that anhydrous hydrofluoric acid is gaseous at 10° F. at a medium pressure. In this respect it does not differ from other anhydrous hydracids, nor from the fluosilicic nor fluoboric acids. Hydrofluoric acid gas is extremely fuming, on account of its great affinity for water. It acts feebly on glass, and might probably be received in glass vessels over mercury.—*Ibid.* Mars 11, 1847.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1847.

Chiswick.—March 1.—3. Cloudy. 4. Fine: cloudy. 5, 6. Cloudy. 7. Slight showers: uniformly overcast: clear. 8. Small rain: cloudy. 9. Cloudy. 10. Hazy: severe frost at night. 11. Slight haze: cloudy: frosty. 12. Clear. 13. Cloudy. 14—18. Clear, with bright sun. 19. Clear: cloudy. 20, 21. Cloudy: clear. 22. Hazy. 23, 24. Cloudy. 25. Foggy: clear: cloudy. 26. Slight haze: fine. 27. Foggy: fine. 28. Uniformly overcast: rain. 29. Very clear: cloudy: frosty. 30. Clear: cloudy: clear and frosty. 31. Frosty: cloudy.

Mean temperature of the month	40°·14
Mean temperature of March 1846	43 ·43
Mean temperature of March for the last twenty years...	42 ·91
Average amount of rain in March	1·36 inch.

Boston.—March 1—5. Cloudy. 6. Fine. 7. Fine: rain early A.M.: rain P.M. 8. Cloudy. 9. Fine: snow A.M. and P.M. 10. Cloudy: hail and snow P.M. 11—13. Fine. 14. Cloudy. 15—18. Fine. 19. Fine: a luminous appearance of an extraordinary nature seen in the sky 9 P.M. 20, 21. Cloudy. 22. Fine. 23. Fine: rain P.M. 24, 25. Fine. 26, 27. Cloudy. 28. Cloudy: rain A.M. and P.M. 29. Fine: thick ice this morning: hail A.M.: rain P.M. 30. Fine: hail P.M. 31. Fine.

Sandwich Manse, Orkney.—March 1. Cloudy: clear. 2. Bright: clear. 3. Showers: clear. 4, 5. Cloudy. 6. Showers: cloudy. 7. Damp: cloudy. 8. Showers: snow-showers. 9. Snow-showers. 10. Snow: cloudy. 11. Showers: sleet-showers. 12. Showers. 13. Cloudy: showers. 14. Bright: cloudy. 15, 16. Cloudy. 17, 18. Bright: cloudy. 19, 20. Bright: clear: aurora. 21. Bright: clear. 22. Bright: large halo. 23. Damp: cloudy. 24. Damp. 25. Cloudy. 26. Damp: cloudy. 27. Cloudy. 28. Hail-showers: sleet-showers. 29. Shower: clear. 30, 31. Snow-shower.

Applegarth Manse, Dumfries-shire.—March 1. Thaw: slight rain. 2. Slight frost: very mild. 3. No frost: gray day. 4. Frost, slight. 5. No frost: clear and fine. 6. No frost: dull and cloudy. 7. No frost. 8. Slight frost: clear. 9. Frost, hard. 10. Frost, hard: sprinkling of snow. 11. Frost: sprinkling of rain P.M. 12. Frost again. 13. Fair and moderate weather. 14. Fair, but threatening: change. 15. Fair still, but cloudy. 16. Fair: rain P.M. 17. Rain all day. 18. Very fine: fair. 19. Very fine. 20. Rain: cleared P.M. 21. Heavy showers. 22. Fine: a few drops of rain. 23. Rain A.M.: thunder P.M. 24. Frosty: fine. 25. Slight frost. 26. Rain. 27. Very fine: rain A.M. 28. Clear and cold: slight snow. 29. Fair: cold: slight frost. 30. Fair: slight frost. 31. Slight snow: hail: frost.

Mean temperature of the month	42°·5
Mean temperature of March 1846	42 ·2
Mean temperature of March for 25 years	39 ·1
Mean rain in March for 20 years	2·35 inches.

THE
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AND
JOURNAL OF SCIENCE.

—◆—
[THIRD SERIES.]

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JUNE 1847.
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LIX. *On the Use of a double Circuit in the measurement of Electrical Forces.* By ISAAC B. COOKE, Esq.*

[With a Plate.]

ACCORDING to the theory of Ohm, the electric force circulating through each part of a galvanic circuit is equal to the *quotient* of the *sum* of the affinities in action divided by the *sum* of the resistances to be overcome. Thus

$$F = \frac{A}{R+r},$$

where F represents the force, A the sum of the affinities generating the current, R the resistance of the electromotor and the wires attached, and r the resistance of the wire of a galvanometer included in the circuit.

If a secondary wire, whose resistance is represented by w , have its ends attached to the two points of connexion between the wires of the electromotor and those of the galvanometer, so as to form with the galvanometer-helix a divided course for the circulation of the current, the equation for the force becomes

$$F = \frac{A}{R+L};$$

where

$$L = \frac{r^2 w}{r+w}.$$

The magnitude of that part of the current passing through the galvanometer wire is expressed by

$$G = \frac{A}{R+L} \cdot \frac{L}{r};$$

* Communicated by the Author.

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or, on substitution,

$$G = F \cdot \frac{\tau\omega}{r + \tau\omega}.$$

Let one of the wires of the electromotor be detached from its connexion with the other wires, and drawn along in contact with the secondary wire, thus modifying the relative length, and of course, in the same proportion, the relative resistances of the two courses open to the current, but leaving the sum of the resistances constant. The above equation will become

$$G' = F' \cdot \frac{\tau\omega'}{r + \tau\omega'}.$$

If the electromotor be not very weak, and the galvanometer be sensitive, in order to reduce the current passing through the galvanometer to a magnitude within the compass of its indications, the secondary wire will require to be short and stout, making the resistance ω very small, and consequently L very small in comparison with R ; F will then vary very slightly by the relative changes thus caused in r and $\tau\omega$, and for practical purposes F , as well as $r + \tau\omega$, may be considered constant; so that we may take with little error

$$G' = F \frac{\tau\omega'}{r + \tau\omega}, \text{ and } \therefore \frac{G'}{G} = \frac{\tau\omega'}{\tau\omega},$$

or the quantity of electricity passing through the galvanometer wire will vary sensibly as the length of that part of the secondary wire included between the wires of the electromotor.

We have thus, differing slightly from methods previously proposed, a means of graduating the galvanometer. By the use of two cells of Daniell's constant battery in series, this may be accomplished with sufficient accuracy by the above method. One battery wire, being in fixed connexion with one point of the double circuit, the other wire must be approximated to, or elongated from, that point, in contact with the secondary wire, until the needle of the galvanometer points to 1° . If the distance of the battery wires be doubled, so as to include a double resistance, the needle ought to point to 2° , if trebled to 3° , and so on, the variations in F , whatever be those in G and ω , being wholly inappreciable.

$F = G \cdot \frac{r + \tau\omega}{\tau\omega}$. If the section and length of the secondary wire be properly proportioned to those of the galvanometer wire, it will always be possible, by moving one battery wire backwards and forwards in contact with the secondary wire, while the other battery wire is in fixed connexion, as before

described, to make the value of G uniformly constant, the needle for example always pointing to 45° , whatever power of electromotor be applied; and as the sum $r + w$ continues constant, F in the above equation will then vary inversely as w . The relative force of different electromotors, however powerful or however weak, may thus be accurately measured by the same delicate galvanometer; being inversely proportional to the length of that part of the secondary wire contained between the wires of the electromotor; or

$$\frac{F'}{F} = \frac{w}{w'}$$

Moreover, if a compound constant battery of three or four pairs be used, and a voltameter be introduced between the battery and the double circuit, it will be easy to find the value of w answering to a certain F , which is also known in terms of the chemical equivalents decomposed in the voltameter in a unit of time. Thus, the wires being adjusted so as to cause the galvanometer needle to point to 45° , note the value of w , viz. the length of the secondary wire contained between the wires of the battery; note also the length of time required, other things being kept constant, to effect a certain amount of decomposition in the voltameter. The quantity decomposed in one minute, deduced from the above observation by simple proportion, is the F . The product of the F and w thus obtained may then be used in the formula

$$F' = \frac{Fw}{w'}$$

to ascertain the value,—also in terms of chemical equivalents,—of any other force F' , without the further use of the voltameter, and whether the force have energy sufficient for chemical decomposition or not; for the *quantity* necessary to deflect the galvanometer to a certain fixed point is always the same, whatever be the *intensity* of the current.

Thus the same rigorous measurement, in terms of the weight of matter naturally associated with fixed quantities of electricity, may be applied to all electrical forces, whatever their intensity, which the voltameter has applied to those capable of producing chemical decomposition, and this by an immediate observation, without the loss of time requisite for a chemical quantitative experiment.

Since the needle of the galvanometer is liable to slight changes in the intensity of its magnetism, the above-determined values of F and w will from time to time require verification.

To find the resistance of an electromotor. Let a certain

length of wire, of such section and conductivity as are best fitted for a standard of resistance, and whose known resistance may be represented by l , the units being linear inches, be introduced between the electromotor and the double circuit, and let the wires be again adjusted so as to deflect the needle to 45° , noting the new length of secondary wire included $=w'$. The equation representing the force, which, previously to the

introduction of the wire l , was $F = \frac{A}{R+L}$, now becomes

$$F' = \frac{A}{R+l+L'}$$

and since

$$\frac{F'}{F} = \frac{rw}{rw'}$$

therefore

$$\frac{R+L}{R+l+L'} = \frac{rw}{rw'}$$

from which, substituting for L and L' their values $\frac{r\tau w}{r+\tau w}$ and $\frac{r'\tau w'}{r'+\tau w'}$, and remarking that $r+\tau w = r'+\tau w'$, the value of R is deduced

$$= \frac{l\tau w}{r'\tau w' - \tau w} - \frac{\tau w \tau w'}{r+\tau w}$$

Excepting in the case of very weak forces, if the galvanometer be sensitive, having a long helix of fine wire, both τw and $\tau w'$ will be very small, while $r+\tau w$ will be very large. Therefore the term $\frac{\tau w \tau w'}{r+\tau w}$ will have no practical value; and unless the value of F be very small, R may be taken without sensible error $= \frac{l\tau w}{r'\tau w' - \tau w}$.

The resistance R of any electromotor which is constant in its action being thus discovered, any other resistance may be measured by the converse process. If it be introduced between the electromotor and the double circuit in the place of the standard wire of the last paragraph, and the value of w' be noted as before, the unknown resistance may be deduced from the equation

$$l = \frac{R \cdot (\tau w' - \tau w)}{\tau w} + \frac{\tau w'(\tau w' - \tau w)}{r + \tau w}$$

The second term of the latter branch of the equation can only be required when F is very small or l very large, and consequently $\tau w' - \tau w$ considerable.

With the help of an additional galvanometer, the resistance of the helix of the one in ordinary use may be measured by the above process; and also the resistance of the secondary wire of the double circuit may be estimated, by introducing as the l a piece of wire having the same section and conductivity as the secondary wire,—cut off for instance from the same piece. Thus $r + w$ becomes known, and may then be used for the determination of the resistances opposed to weaker forces. If the helix of the galvanometer be skilfully formed and well-insulated, the resistance $r + w$ once determined will not be liable to change.

The sum of all the tensions of the circuit, positive and negative, *i. e.* of the affinities producing the current, is known from the equation

$$A = F(R + L).$$

In the majority of cases $A = FR$ will be a sufficient approximation; or at least $A = F(R + w)$, as except in very weak forces, w will be so small that it will not materially differ from L or $\frac{r}{r + w} \times w$.

The practical form which I have given to an instrument for the above purposes, and which I call the “Detached Galvanometer Scale,” is represented in Plate VII., and may be thus described.

It consists essentially of four wires, a, b, c, d ; the first three, formed of copper, are one-fourth, one-eighth, and one-sixteenth of an inch in diameter respectively; the last, d , is also one-sixteenth of an inch in diameter, but is formed of platinum. Each of them is exactly eight inches long, and they are all partially sunk in grooves, parallel to, and about one inch distance from, each other, in a board eight inches long of hard well-dried wood, varnished and graduated into tenths of an inch, the lines being drawn through from side to side.

The four wires are joined into one continuous wire, commencing at A and terminating at E, by being soldered to the connecting pieces A, B, C, D, E, which are bored, and furnished with screws to clasp a wire from any electromotor whose force is to be measured.

The connecting pieces are of solid brass, and at least half an inch thick, in order to offer no appreciable resistance to a current passing through them from the wire of the electromotor to the wire of the scale.

When in operation, the wires from the galvanometer are always to be attached to the terminating connecting pieces A and E.

One wire from the electromotor must be attached to A, B, C or D, according to the magnitude of the force to be measured, and the other wire from the electromotor must be moved along in contact with the wire *a*, *b*, *c* or *d*, according as the first wire is connected with A, B, C or D, until the galvanometer needle points to 45° .

F is a vernier which is applicable for this purpose: the moveable wire of the electromotor, being passed through the small hole *e* and brought up again by the notch *f*, will lie in the groove *ef*, and the vernier sliding on the wire of the scale by the groove which is at right angles to *ef*, will enable the operator to read off the position of the moveable wire to $\frac{1}{100}$ dth of an inch. It is requisite to keep the wires of the scale and vernier bright, and to rub them well with wash-leather before each time of using, and the vernier will require little pressure to obtain a correct indication.

The diameter of wire *c* being half that of *b* and one-fourth that of *a*, its section is one-fourth and one-sixteenth of these respectively. Consequently, it will offer to an electric current four times the resistance which is offered by an equal length of *b* and sixteen times that offered by an equal length of *a*. The wire *d* is of the same diameter as *c*, but being platinum has a less conducting power, say one-fifth, and will therefore offer five times the resistance of an equal length of *c*. The ratio of the resistances of the wires *a*, *b*, *c*, *d*, however, since different pieces of the same material are apt to differ in their conducting power, and very slight differences of diameter become multiplied into importance in the section, must be determined by actual experiment, which is easily done with the help of a constant battery; and the product of F and w being also ascertained by experiment for one of the wires, may then be known by computation for the others. As the length of *d* is graduated on the board into eighty divisions, and indeed with the assistance of the vernier into 800, this instrument is calculated to measure a scale of electric forces, extending from its lowest or unity to $80 \times 5 \times 4 \times 4 = 6400$, or with the vernier through $64,000^\circ$.

With a galvanometer of moderate sensitiveness, having a helix of about 1000 inches of copper wire $\frac{1}{100}$ dth of an inch in diameter, a force capable of decomposing water sufficient to furnish (F) $\frac{6}{100}$ dths of a cubic inch of hydrogen gas in one minute, required (w) six inches of wire *d* to be interposed between its conducting wires to raise the index of the galvanometer to 45° , making the product of F and w for this wire = 36.

For smaller or larger electric forces, additional instruments with finer or coarser wires will be requisite.

The resistances of wires are to each other directly as their lengths, and inversely as the products of their sections and conductibilities. If the wire of the galvanometer helix be as above, 1000 inches in length and $\frac{1}{100}$ th of an inch in diameter, then eight inches of wire d , being of platinum, one-sixteenth of an inch in diameter, and having, suppose, one-fifth of the conducting power of copper, will equal in resistance about an inch and a quarter of the helix wire; therefore since

$r=1000$ and the greatest value of $w=1\frac{1}{4}$, the quantity $\frac{rw'}{r+w}$

cannot in this instrument be of any importance in the determination of resistances, and may always be safely neglected. For the accurate measurement, with an instrument having finer wires, of resistances offered to weaker forces, the fraction may be useful.

LX. *Physical and Geological Observations on the principal Geysirs of Iceland.* By A. DESCLOIZEAUX*.

IN Iceland the name of *Geysir* is generally given to a jetting thermal spring, and the names of *hver* (cauldron) and *laug* (baths) are applied to springs simply bubbling up, or wholly tranquil, contained in basins almost always regular, circular or elliptic.

The jetting springs most celebrated on account of their volume, the beauty of their eruptions, and the importance of the deposits which they have formed and which they still form at the present day, are those known by the names of the *Great Geysir* and *Strokkur*. Although most of the travellers who have visited Iceland have described the eruptions of these two springs which they witnessed, and of which some have published pretty accurate representations, I will here relate the manner in which this phænomenon takes place.

In the Great Geysir, whose waters, as I shall observe hereafter, have formed for themselves a very regular basin in the form of a truncated cone hollowed at the summit, the eruptions are preceded by subterraneous detonations, which have always been justly compared to a distant sound of artillery, and which powerfully shake the base and lateral parts of the cone. After each detonation, the column of water which occupies the central channel is upraised, in the form of a hemisphere, some metres† above the surface of the basin; then all becomes calm again.

These detonations and these upliftings of the liquid mass are produced pretty regularly every two hours, as we have

* Translated from the *Ann. de Chim. et de Phys.* for April 1847.—The Icelandic orthography has been preserved in this memoir.

† The metre is equivalent to about 3 feet 3¼ inches.—Tr.

ascertained by a series of observations carried on for several days, and consequently they are far from announcing a great eruption; but when this is approaching, detonations stronger and more frequent than the first are heard, the uprisings of the central column become more and more considerable, and all at once an immense column of water, three metres in diameter at its base, is projected into the air to a height varying from thirty to fifty metres, spreads like a fountain at its summit, and falls again in part into the large reservoir, eighteen metres in diameter, which forms the basin of the spring; the rest of the mass of water flows over the external sides of the cone, and spreads out into the little rills which furrow the plateau on which this cone rests. The column of water does not generally attain its greatest height until several consecutive jets at very short intervals have prepared the observer for the magnificent spectacle which the Geysir reserves for the close of the eruption, like a skilful firework-maker who terminates his exhibition by a splendid bouquet. These eruptions last from five to seven minutes.

In the Strokkur, the circumstances attending the eruption are a little different, and the form of the channel which discharges the emitted waters is doubtless the principal cause which gives the phænomenon a peculiar character. In this channel, which represents a well even with the ground, the water is at about three metres below the surface, and it boils up with incessant oscillation. When it is attempted to produce an artificial eruption by covering the surface of the water with clods of earth or turf, or when a natural eruption is about to take place, this boiling ceases for some instants, and a pointed, lanceolate jet issues suddenly to a certain height; it is soon followed by another, which is in its turn succeeded by a series of slender columns, very pointed at the summit, with serrated outlines, which cannot be better compared than to immense yew-trees. These columns, which succeed one another sometimes for twelve to fifteen minutes, attain various heights, which reach, as in the Geysir, to fifty metres. When the eruptions of the Strokkur take place naturally, the jetting water is quite as limpid as that of the Geysir; and, if the weather is calm and the sun bright, nothing can exceed the magnificence of these immense columns of clear water, perfectly vertical, and reflecting the brilliant colours of the rainbow. If however the Strokkur is excited by clods of earth or turf, the water is coloured black, often of a very deep tinge, and the phænomenon loses much of its splendour.

Before passing to the results of the thermometric experiments and the various observations made at the Geysir and

the Strokkur, I shall describe in a summary manner the topographical and geological position of these two springs, for the geological portion of the *Voyage en Islande* of the Scientific Commission of the North contains extensive details upon this subject.

The two principal geysirs of Iceland are situated in the south-east part of the island, at eighty-seven kilometres, in a straight line, to the east-north-east of Reykjavík, and at forty-six kilometres to the north-west of Hecla. These fountains, accompanied by a great number of *hvers* and *laugs*, occupy a space of 500 metres from north to south, by 100 metres from east to west, towards the northern part of a large plateau of volcanic tufa, bordered on three sides by large glaciers or *Jökulls*, the principal of which are,—on the north, the Láng-Jökull; on the north-east, the Hofsjökull and the Arnarfells-Jökull; on the east, the Skaptár-Jökull; on the south, Hecla and the Torfa-Jökull.

This vast plateau is traversed, from north-east to south-west, by two of the largest rivers in Iceland, the Hvítá and the Thjórsá, and by their numerous tributaries: these two principal rivers, which take their sources, the first in the snows of the Láng-Jökull, and the second at the foot of the Arnarfells-Jökull, flow into the sea at the south-west part of the plateau, which is terminated on this side by low sandy shores. At numerous points of the plateau containing the geysirs, rise mountains which have in many instances the form of isolated cones, composed of a conglomerate, the chief constituent of which is basalt and a brown tufa, the principal element of which appears to be the mineral which Sartorius von Waltershausen found in the ancient tufas of Etna, and which he has named *palagonite*. The height of these mountains, several of which have preserved the appearance of craters of eruption, and still present, on their slopes or at their base, debris of streams of lava, does not exceed 500 to 600 metres.

Beside these cones, we meet with some hillocks of a gray phonolite, inclosing numerous crystals of felspar, and very small crystals of pyroxene: some strips of ancient trap or compact basalt, often divided into columns, appear also at the surface of the plateau; and a sort of vitreous pitchstone, both green and reddish, which appears to have resulted from the fusion of the phonolite by the basalt, is ordinarily seen in contact with these two rocks.

The hill nearest to the geysirs, from which it is distant only about 360 metres, to the west, consists of upheaved and almost vertical strata of a bluish gray phonolite, much

disintegrated on the lower part, where it presents numerous traces of having been acted upon and undergone a slight alteration by the thermal waters, but it is tenacious and tolerably homogeneous towards the summit. The height above the plain of this hill, which has the name of Laugafjall, may be estimated at seventy-five metres. From the foot of its eastern escarpment, an undulating district extends for a length of about 350 metres, entirely composed of ancient siliceous concretions, slightly altered and friable. This district, the highest hills of which rise to nearly twenty-five to thirty metres' elevation above the plain, is pierced with a multitude of holes, from which steam and the vapour of sulphuretted hydrogen still escape; small quantities of alum and sulphur are deposited on the edges of some of these holes, one of which is situated at the very foot of the trachytic hill.

The existence of an ancient siliceous district, so extensive as that which I have just mentioned, certainly appears attributable to numerous extinct geysirs, the abundant deposits of which have at length completely stopped the orifices from which their eruptions took place. The lower part of this ancient district is separated from the deposits of the present Great Geysir by a small ravine of two metres in depth, situated at ten metres from the basin of the geysir, and directed nearly from south to north. The sides of this ravine, from whence acid vapours are also disengaged at several points, discloses both in the ancient and modern formation, numerous layers of siliceous concretions of a very varied aspect, distributed in the midst of a bolary clay in which the red colour predominates.

On the right side which bounds the deposits of the present geysir, and in a very plastic clay, we meet with small stems of birch-trees, of the size of those which still grow in the country, entirely silicified and converted into chalcedony; the clay itself is full of small crystals of white pyrites, formed from the iron which it contains and of the sulphur of the vapour springs. Above the chalcedonous stems, and approaching the most recent strata of concretions, we now only find birch-leaves and a network of herbaceous plants, forming a sort of siliceous travertine. In this same part, and always in the middle of a reddish clay, I have observed a thin, very regular, and extensive layer of zoned chalcedony; this substance, which is translucent as long as it remains immersed in the moist clay, becomes opaque and of an enamel white by a simple desiccation in the open air, without being able to re-acquire its transparency, even on a prolonged immersion.

The banks of the little river the Béríná, which flows at 300

metres to the east of the Geysir, and into which a part of the water that overflows after each eruption run, present a fact of the same kind: in the midst of a network of silicified plants forming several layers, the total thickness of which exceeds three metres, we found small portions of silix, which possessed all the properties of precious opal, as long as they are strongly hydrated, but which only preserve their vivid colours while they remain immersed in water, or are kept from drying. May we not hence conclude that the opals and chalcedonies of some ancient volcanic districts owe their origin to phænomena similar to those of the present geysirs of Iceland?

The left side of the small ravine spoken of above, which bounds the deposits of the ancient extinct geysirs, did not present chalcedonous wood, but we extracted from it birch stems transformed into very friable white silica, which appeared to have been derived from individuals larger than those we meet with at the present day in Iceland. Some specimens of these woods, in which the cells and vessels may still be observed under the microscope, are penetrated in various directions with small roots which appear to belong to the same species; we might without doubt thence conclude that the large stems were already in a state of advanced decomposition when the silica came and penetrated into their tissue,—a circumstance which would perhaps explain the reason why these woods have not been converted into chalcedony, but into pulverulent silica.

It is at all events certain that a birch-wood once existed between the ancient and recent geysirs; and, as these trees cannot live where the soil is covered with siliceous concretions, it must be admitted that these two classes of fountains did not exist simultaneously, and that the recent geysirs succeeded the primitive geysirs, when the orifices of these last became insufficient to give a passage to the masses of water which had to be cast forth*.

With respect to the duration of the ancient phænomena, the depth of twenty-five metres at least of the soil which they have formed, shows that it must have been very considerable. In fact, the concretions deposited by the present geysirs have not yet exceeded a thickness of four to five metres; and all seems to prove that, within the historical period of Iceland, the increase of this thickness, which is distributed over an extended surface, has been very small, so that we must attribute a very high antiquity to the recent geysirs themselves†.

* A very detailed description of the districts formed by the ancient and recent geysirs has been given by M. Robert in the geological part of the Voyage of the Northern Scientific Commission to Iceland.

† As it is almost impossible, in order to prove the thickness of deposits

The two jetting springs, the temperatures of which I have principally sought to determine during my stay in that locality with M. Bunsen of Marburg, are the Great Geysir and the Strokkur. I shall first direct my remarks to the Great Geysir.

The only observations of any accuracy which we hitherto possessed on the temperature of this spring are those of M. Lottin, a member of the Northern Scientific Commission, who visited Iceland in 1836; but these observations left much still to be desired, both from the method itself which M. Lottin followed, and from the experiments not having been sufficiently repeated to embrace the various phases of the moments of calm and of eruptions of the geysir. The determinations which M. Bunsen and I have made were taken immediately before a great eruption, immediately after that eruption, and in the interval of two great consecutive eruptions. The thermometers which I employed were double-bulbed, and constructed by Bunten after the directions of M. Regnault*: they were composed of a small cylindrical reservoir, surmounted by a stem of accurate calibre, and divided into parts of equal capacity; this stem ended in a finely drawn-out point terminating almost at the summit of a small cap of thin glass, soldered to the top of the stem, at the part where its diameter began to diminish, and containing a small excess of mercury.

In taking observations with these thermometers, the stem was entirely filled with mercury, at the ambient temperature; it was then immersed, by the side of a good standard, in a bath, the temperature of which was nearly constant, and lower than that which was to be observed: a portion of the mercury of the stem was raised into the small upper bulb. When the mercury was stationary and occupied exactly the extremity of the small drawn-out point, the temperature of the standard formed in a given time, to establish at the Geysir itself a fixed mark capable of resisting the ignorant curiosity of certain visitors, I endeavoured to construct one easy to be recognised only by the forewarned and attentive eye. For this purpose, I chose two contiguous *laugs*, situated at nearly an equal distance from the Strokkur and the Geysir, proceeding towards the Laugafjall: the most northern *laug*, without apparent overflow, is contained in a nearly elliptical basin, the major axis of which is seven metres, the minor one 3^m.40, and the depth 2^m.55; the temperature at the surface was found to be 98°·5. The second *laug*, separated only from the first by a small natural wall, has a more irregular basin, of a depth of 9^m.30, and the water of which, indicating a temperature of 96°·5, flows off incessantly by a small rivulet which passes near the Strokkur, depositing on its way abundant incrustations. In the narrowest part of the wall which separates these two *laugs* I made an aperture, the successive obliteration of which it will always be easy to ascertain. The following were the dimensions of this aperture on July 12th, 1846:—Depth 0^m.065; length, from east to west, 0^m.260; breadth, from north to south, 0^m.110.

* See Scientific Memoirs, Part XVI. p. 588.

was noted; the thermometer, inclosed in a copper or iron case, the lid of which was hermetically closed by means of a screw and red-lead cement, was let down into the spring; after remaining there thirty minutes, it was raised and compared again with the standard; the mercury stopped at a certain division of the stem, and the quantity of mercury which had escaped from the stem during the observation, converted into centigrade degrees and added to the first temperature indicated by the standard, gave the temperature of the spring. The thermometers employed by M. Bunsen were constructed upon the same principle; only that, instead of a capsule soldered to the upper part of the stem, the capillary opening of the latter was closed by a small point of iron, kept vertical by means of a spring, which the mercury could raise, so as to fall over into the case.

In each of the five experiments which we made at the Great Geysir, we employed five thermometers arranged on a single line, and separated by nearly equal intervals.

It is known that the Great Geysir has formed by successive incrustations a very regular basin, having the shape of a truncated cone, the exterior slopes of which have a mean inclination of 8° , whose summit is occupied by a cup or vase*, presenting only on the edges some small clefts through which is carried the overflow of the water, when its level rises. The mean inclination of the sides of this cup is 13° .

In the centre of the basin is a circular well, which appears perfectly regular as far as the eye can discern. Between two consecutive eruptions, this well and the cup which surrounds it are completely filled with water, a very small portion only of which overflows after the upheavings, which take place about every hour and half: but immediately after the great eruptions, the cup is completely empty, and in the well the water descends below the bottom of the cup from one metre to $2^m \cdot 50$, according as the projected column had attained a greater or less height; the former level is not restored until after six or seven hours.

The following are the principal dimensions which we have found for the different parts of the Geysir:—

Diameter of the basin or cup, on the edges, from north to south	m 16'00
Diameter of the basin or cup, in a perpendicular direction	18'00
Diameter of the central well	3'00

* Sir W. J. Hooker thus describes it:—"To compare great things with small, the shape of this basin resembles that of a saucer with a circular hole in its middle." *Tour in Iceland*, p. 117.—Tr.

Depth of the well, including that of the basin :—	
1. In the direction of the first diameter . . .	23·50
2. In the direction of the second diameter . . .	21·50
Depth of the cup in the centre	1·57

As we took care, in each experiment, to alter the order of our thermometers, that their indications might be checked, I shall indicate by numbers and by the letter B those of M. Bunsen, and by the letter D those which belonged to me.

It will be conceived that the temperature at the surface of the water which fills the basin must be excessively variable; for that temperature depends at the same time on the temperature of the sides of the basin, on that of the air, on the force and direction of the wind, and on the hygrometric state of the atmosphere, which allows of a more or less speedy evaporation. This is the reason why the numbers given by different travellers, for the temperature of the water on the edge of the basin, agree so little; we ourselves made a series of observations, the terms of which differ considerably among themselves. They are as follow :—

1846.	h m	Temp. at the margin.
3rd July, at 10 30 A.M.; basin filled .		86°·0 centigrade.
3rd July, at 10 58 A.M.; <i>Id.</i> . . .		89·0
3rd July, at 11 30 A.M.; <i>Id.</i> . . .		88·0
3rd July, at 3 25 P.M.; <i>Id.</i> . . .		89·7
4th July, at 12 35 P.M.; <i>Id.</i> . . .		76·0
4th July, at 1 17 P.M.; <i>Id.</i> . . .		85·0
9th July, at 1 50 P.M.; basin half-full		82·5 in the wind.
9th July, at 1 50 P.M.; <i>Id.</i> . . .		86·0 under the wind.

We will admit that the mean temperature of the water at the surface and in the centre of the basin is 85° C.

The following table indicates the temperatures found in different points of the liquid column, immediately before a great eruption :—

<i>Third Experiment.</i>		<i>Fourth Experiment.</i>	
The 7th July, at 2 ^h 55 ^m in the afternoon: four hours before a great eruption. Basin filled: total depth, 23 ^m ·50; length of the line, 22 ^m ·85.		The 7th July, at 6 ^h 58 ^m in the afternoon: ten minutes before a great eruption. Basin filled: total depth, 23 ^m ·50; length of the line, 22 ^m ·85.	
	Heights. m		Heights. m
	85·0		85·0
Therm. No. 1, D	85·2	Therm. No. 5, D	84·7
Therm. No. 2, B	106·4	Therm. No. 1, B	110·0
Therm. No. 5, D	120·4	Therm. No. 6, D	121·8
Therm. No. 6, D	123·0	Therm. No. 1, D without result.	5·00
Therm. No. 1, B	127·5	Therm. No. 2, B	126·5
	Bottom.		Bottom.
Mean temperature of the column of water, 108°·33.		Mean temperature of the column of water, 109°·19.	

The mean temperature of the column of water was obtained by multiplying half the sum of the indications of two consecutive thermometers, by the interval which separates them, and dividing the sum of all these products by the sum of the intervals, or the length of the line.

Twelve minutes after the fourth experiment, at 7^h 40^m in the evening, the great eruption took place.

The following are the results of the fifth experiment commenced the 7th July, at 9^h 45^m in the evening, two hours after the great eruption; basin half-filled: height of the liquid column, 22^m·75; length of the line, 22^m·50.

	85°0		22·50	
Thermometer No. 1, D . . .	103·0		13·50	
Thermometer No. 3, D . . .	121·0		9·70	
Thermometer No. 5, D . . .	lost		4·90	
Thermometer No. 6, D . . .	122·5		0·30	
				Bottom.

Mean temperature of the column of water 108°·83.

The three preceding tables show, in an evident manner, that the mean temperature of the column of water which fills the central well of the Great Geysir, remaining in other respects constant, the distribution of the temperature, in different points of that column, varies with the moment when the observation was made. We see, on examining these tables, that there is at the bottom of the well a maximum, immediately before the eruptions, which may be estimated at 127°; and a minimum, immediately after, which may be estimated at 123°.

The results given in the two following tables, obtained at different periods between two consecutive eruptions, show that the phænomenon always passes in the same manner, and with tolerable regularity:—

<i>First Experiment.</i>		<i>Second Experiment.</i>	
The 6th July, at 0 ^h 30 ^m in the morning: three hours after a great eruption, and eleven hours before the following eruption. Basin half-full: height of the liquid column, 22 ^m ·75.		The 6th July, at 8 ^h 20 ^m in the morning: nine hours after a great eruption, and twenty-three hours before the following eruption. Basin filled: height of the column, 23 ^m ·50; length of the line, 22 ^m ·85.	
	Heights. m		Heights. m
85°0	22·50	85°0	22·85
Therm. No. 5, D 95·0	19·70	Therm. No. 6, D 82·6	19·20
Therm. No. 3, D 109·0	16·30	Therm. No. 5, D 85·8	14·40
No result.	12·90	Therm. No. 2, B 113·0	9·60
Therm. No. 2, B 121·1	9·50	Therm. No. 3, D 122·7	4·80
Therm. No. 1, B 121·6	6·00	Therm. No. 1, B 123·6	0·30
Corresponding number of the fifth exp..... 122·5	0·30		Bottom.
	Bottom.		
Mean temperature of the column of water, 112°·68.		Mean temperature of the column of water, 102°·30.	

Thus, notwithstanding the relative lightness of the most heated water, which tends to rise to the surface, we see that, even after the eruptions, the temperature of the different strata does not become equalized; still further, in proportion as the inferior strata become heated, to reach their maximum, the upper strata become cool, undoubtedly in consequence of evaporation, and of the heat communicated to the sides of the well and the basin; so that the greatest difference of temperature at the two extreme levels corresponds precisely to the moment of the eruptions.

We can now ascertain what would be the boiling-point of water submitted to a pressure equal to that of the atmosphere, augmented by the liquid column the height and mean temperature of which we have determined in our five experiments. For this purpose, we must first express the pressure of this liquid column in millimetres of mercury at 0° . The density of the water of the Geysir, found at from $1^{\circ}0010$ to 10° centigrade; the curve which represents the results obtained by M. Isidore Pierre for the dilatation of distilled water, and which I admit as being applicable to the water of the Geysir, extending it up to 113° ; lastly, the density of the mercury at 0° , compared to that of distilled water at 4° , and found at $13\cdot596$ by M. Regnault, enables us to make this reduction. We thus find that the heights of the columns of water in our different experiments correspond, in millimetres of mercury at 0° , to the following numbers:—

Third and fourth experiment	1 ^m ·645
Fifth experiment	1 ^m ·593
First experiment	1 ^m ·587
Second experiment	1 ^m ·654

From the equations

$$\log e = a - ba^x, \quad x = t - 100,$$

and from the data

$$\log a = \overline{1}\cdot9977641, \quad \log b = 0\cdot4692291, \quad a = 5\cdot8267890,$$

laid down by M. Regnault, in which e represents the tension of steam at saturation, expressed in millimetres of mercury, and x the temperature to which that pressure corresponds, we can derive the value of x and of t ; and this last quantity will precisely represent the boiling-point of water submitted to the pressures

$$\begin{array}{ll} 0\cdot760 + 1^{\text{m}}\cdot645, & 0\cdot760 + 1^{\text{m}}\cdot593, \\ 0\cdot760 + 1^{\text{m}}\cdot587, & 0\cdot760 + 1^{\text{m}}\cdot654. \end{array}$$

The corresponding values of t are as follow :—

Third and fourth experiment . . .	$t = 136^{\circ} \cdot 151$
Maximum temperature at the bottom	127
Difference . . .	9 ^o ·151
Fifth experiment	$t = 135^{\circ} \cdot 398$
Minimum temperature at the bottom	122 ^o ·5
Difference . . .	12 ^o ·898
First experiment	$t = 135^{\circ} \cdot 31$
Temperature at the bottom . . .	122 ^o ·5
Difference . . .	12 ^o ·81
Second experiment	$t = 136^{\circ} \cdot 28$
Temperature at the bottom . . .	123 ^o ·60
Difference . . .	12 ^o ·68

Thus, at the point to which the lower thermometer can penetrate, the water does not attain the temperature of ebullition which it should have under the pressure to which it is subjected; but it approaches that temperature in proportion as a great eruption is near. We may thence conclude that the centre of heat which raises the temperature of the water of the Geysir is situated at a certain distance below the presumed bottom of the central well, and we may deduce from this a plausible explanation of the manner in which the eruptions are determined.

Let us in fact suppose that the column of water in the central basin communicates, by a long and sinuous channel, with the space, be it what it may, which receives the direct action of the subterranean heat: after an eruption during which a projection of a great quantity of water and vapour has taken place, the lower parts of the liquid mass are cooled, and the steam which is formed in the reservoir submitted to the action of the heat has a less tension than that at which the weight of the central column and that of the atmosphere are in equilibrium; this vapour, as it forms, becomes condensed in contact with the water which fills the sinuous channel, and it imparts to that water its latent heat. The increase of temperature of the water of the channel is transmitted by degrees to the lower part of the central column where the thermometer can reach; but this increase is retarded by the atmospheric air and the other gases which accompany the vapour; however, at the lapse of a shorter or longer time, the water of the channel must boil, and the steam which continues to form cannot longer condense there; this vapour must therefore accumulate, and acquire a gradually increasing tension, until this

tension is able to overcome the resistance of the column of water which fills the basin, and to project it into the air.

A thermometer placed at four metres above the bottom of the central well during an eruption which took place July 14, 1846, at 3^h 15^m in the morning, and which raised the column of water to 49^m·37, according to a measurement taken by M. Waltershausen, indicated a temperature of 124°·24, which corresponds perfectly to what would have been found at the same point in the third and fourth experiments.

In the intervals of the great eruptions it often happens that the column of water is raised to one metre, or 1^m·50 above the usual level, and it is easy to conceive that it must be thus; for, if the vapour formed in the subterranean reservoir is not condensed entirely at its contact with the water of the sinuous channel of which I have spoken, and if it is accompanied with a sufficient quantity of atmospheric air, or of other gases, it must in the end divide the liquid column, and escape at the surface, producing those subterranean detonations and those jets which all travellers have remarked. A fact worthy of attention is, that these detonations appear to be produced periodically with sufficient regularity; whereas the great eruptions only take place at quite unequal intervals, as is shown by the following table of a series of observations which we continued for several days. This difference might be explained by admitting that the production of vapour is not perfectly regular. In the following table, the three columns indicate the date of the observations, the hours when the detonations or jets took place, and those when great eruptions were produced:—

Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.
	h m		h m		h m
3rd July ...	9 45 a.m.	4th July ...	10 30 a.m.	5th July ...	6 53 p.m.
" "	10 58 a.m.	" "	11 36 a.m.	" "	8 20 p.m.
" "	12 15 p.m.	" "	1 13 p.m.	" "	9 45 p.m.*
" "	1 5 p.m.	" "	2 4 p.m.	Interruption.	
" "	2 5 p.m.	3 39 p.m.*	6th July ...	7 40 a.m.
" "	3 25 p.m.	4th July ...	7 45 p.m.	" "	9 25 a.m.
" "	4 0 p.m.	" "	9 0 p.m.	11 0 a.m.*
" "	8 0 p.m.	" "	11 15 p.m.	Interruption.	
" "	8 25 p.m.	" "	11 23 p.m.	6th July ...	3 0 p.m.
" "	9 55 p.m.	Interruption.		" "	5 45 p.m.
" "	10 48 p.m.	5th July ...	5 0 a.m.	" "	6 45 p.m.
4th July ...	12 14 a.m.	" "	8 45 a.m.	" "	8 0 p.m.
Interruption.		" "	9 44 a.m.	" "	9 30 p.m.
4th July ...	6 0 a.m.	" "	11 15 a.m.	" "	10 50 p.m.
" "	7 50 a.m.	" "	2 5 p.m.	Interruption.	
" "	9 7 a.m.	" "	4 34 p.m.	7th July ...	9 15 a.m.

* The great eruptions are denoted by an *.

Table (continued).

Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.	Dates of the Observations.	Explosions and great Eruptions.
7th July ...	h m 10 52 a.m.	10th July...	h m 12 45 p.m.	Interruption.	h m
" "	1 30 p.m.	" "	2 10 p.m.	12th July...	5 57 p.m.
" "	2 53 p.m.	" "	3 30 p.m.	" "	7 46 p.m.
" "	3 30 p.m.	" "	4 24 p.m.	" "	8 55 p.m.
" "	5 40 p.m.	" "	5 30 p.m.	Interruption.	
" "	7 40 p.m.*	" "	7 15 p.m.	13th July...	5 22 a.m.
" "	10 50 p.m.	" "	9 33 p.m.	" "	6 48 a.m.
" "	11 10 p.m.	Interruption.		" "	7 25 a.m.
" "	11 35 p.m.	11th July...	3 30 a.m.	" "	8 40 a.m.
Interruption.		" "	4 52 a.m.	" "	9 52 a.m.
8th July ...	3 0 a.m.	Interruption.		" "	11 7 a.m.
" "	4 20 a.m.	11th July...	8 30 a.m.	" "	11 28 a.m.
" "	6 10 a.m.	" "	9 55 a.m.	" "	12 32 p.m.
" "	8 27 a.m.	" "	11 30 a.m.	Interruption.	
" "	10 16 a.m.	Interruption.		13th July...	3 47 p.m.
" "	12 35 p.m.	11th July...	1 46 p.m.	Interruption.	
Interruption.		" "	3 35 p.m.	13th July...	6 45 p.m.
9th July ...	10 45 a.m.*	" "	4 35 p.m.	" "	7 53 p.m.
Interruption.		" "	6 10 p.m.*	" "	9 12 p.m.
9th July ...	3 25 p.m.	Interruption.		" "	9 47 p.m.
" "	5 45 p.m.	12th July...	5 5 a.m.*	" "	10 55 p.m.
" "	7 25 p.m.	Interruption.		" "	11 43 p.m.
" "	9 25 p.m.	12th July...	9 6 a.m.	Interruption.	
Interruption.		" "	10 45 a.m.	14th July...	3 15 a.m.*
10th July ...	8 0 a.m.*	" "	12 55 p.m.	Interruption.	
Interruption.		" "	2 50 p.m.	15th July...	9 0 a.m.*

The heights of the two most remarkable eruptions during our visit to the Geysir, measured by M. Sartorius von Waltershausen with a theodolite, gave—

The 6th of July, at 11^h 00^m A.M., 145 French ft. = 47^m·101.

The 14th of July, at 3^h 15^m A.M., 152 French ft. = 49^m·375.

I have said, at the commencement of this memoir, that the water, when it had completely left the basin, after the great eruptions, descended into the central well, at about 2^m·50 below its orifice. The following are two numbers I observed:—

The 11th of July, at 6 o'clock in the evening, a depth
of water below the orifice of the well 2^m·00

The 14th of July, at 3^h 15^m in the morning † . . 2^m·60

* The great eruptions are denoted by an *.

† If we calculate the quantity of water which, to produce this lowering, escaped from the basin, during and after the eruption, to flow almost wholly into the Beína, we find that this quantity was, in the first case, 157^{mc}·585, and, in the second case, 161^{mc}·825: now, admitting that at 135°·8 and under the pressure of three atmospheres, the steam has a volume equal to 600 times that of the water which produced it, and a density of 0·0016145, a quantity of vapour represented by 94,551 cubic metres = 152,652 kilogrammes in the first case, and by 97,095 cubic

I also availed myself of my visit to the Geysir to make some chemical experiments upon the water of this remarkable spring, which it would have been difficult to render so conclusive with water carried to Paris. I paid particular attention to the estimation of the sulphuretted hydrogen, the odour of which is very perceptible in the vapour which incessantly escapes from the basin, and to ascertain whether the soda contained in that water is in the caustic or carbonated state.

The sulphydrometer of M. Dupasquier served me for the first determination. On operating with necessary caution, I found that one litre of water of the Geysir, perfectly limpid and cooled, protected from contact with the air, to about 40°, contained 2^{cc}.448 of sulphuretted hydrogen.

To ascertain the state of the soda, we collected some water in a well-stoppered bottle, acidulated it with some drops of hydrochloric acid, and boiled it; the vapour, collected in some lime-water, yielded a very perceptible precipitate. Mixed with some ammoniacal chloride of calcium, the water was not rendered perceptibly turbid; it therefore does not contain free carbonic acid.

The presence of chlorine or of alkaline chlorides was likewise ascertained by means of nitrate of silver.

To obtain a complete analysis of this water, I collected a quantity in a flask with a drawn-out neck and closed it over the lamp, and in glass bottles closed with a good cork stopper covered with caoutchouc.

The water of the first flask was employed to estimate the carbonic acid combined with the soda; this determination was made in the laboratory of the Val-de-Grâce, where M. Millon was kind enough to place at our disposal one of the apparatus which he employs to estimate the carbonic acid in organic analysis. A second operation, made comparatively on water of the bottles closed with a caoutchouc stopper, gave a result nearly identical with that of the first experiment.

metres = 156,760 kilogrammes in the second case, on condensing upon the lower part of the Geysir, might re-establish its primitive level.

I have stated above that this level was re-established in about seven hours, and that from that moment only a small quantity of water flowed over after each of the detonations which took place every two hours.

The deficit in the Geysir, after a great eruption, might therefore be made good by a subterraneous disengagement of vapour, varying from 1807 kilogrammes to 2394 kilogrammes an hour. Now an ordinary steam-engine, without coating or covering and working without pressure (*sans enveloppe et sans détente*, consuming five kilogrammes of pit-coal an hour, expends about thirty kilogrammes of vapour by horse-power. We see therefore that the present phænomenon of the Geysir requires for its production, only a development of vapour corresponding to nearly that of an engine of 700 horse-power, which gives an approximate idea of the order of this phænomenon.

M. Damour kindly undertook the analysis of the residue which this water leaves on evaporation, and I here transcribe only the results which he has given in a separate notice. A litre of this water, evaporated at 65° centigrade, gives a residue weighing 1·3900 gr., and effervescing with acids. Heated to 300°, the weight of this residue was reduced to 1·1830; and at a cherry-red heat the residue agglutinates, and now amounts to only 1·0540.

The same quantity of water contains,—

	gr.
Chloride of sodium	0·2638
Sulphate of magnesia	0·0091
Sulphate of potass	0·0180
Sulphate of soda	0·1343
Soda	0·1227
Silica	0·5190
Carbonic acid	0·1520
Sulphur	0·0036
	1·2225

The quantities of oxygen of the silica and the bases are in the following relation:—

	gr.		Oxygen.	Relations.
Silica	0·5190		0·2696	3
Soda	0·3427	0·0876	0·0892	1
Potass	0·0097	0·0016		

On subtracting from the weight of the alkalis the quantity necessary to saturate the chlorine and the sulphuric acid, there remains,—

	gr.		Oxygen.	Relations.
Silica	0·5190		0·2696	9
Soda	0·1227		0·0314	1

In several analyses, M. Damour found that the relation 3 : 1 between the oxygen of the silica and that of the bases was constant, and he concluded therefrom that the silica is probably dissolved first in the water of the Geysir, in the state of alkaline silicates, NaO, SiO³, 3NaO, 2SiO³, which can be produced artificially, and which are very soluble in water; then, by the action of the sulphurous and hydrochloric vapours, sulphates and chlorides are formed: the quantity of alkali combined with the silica undergoes a successive reduction, and the primitive relation of 3 : 1 becomes 9 : 1. In this state of saturation, a portion of the silica ceases to be soluble, and is deposited, without doubt, in a quantity corresponding to the amount of alkali saturated every day by the action of the vapour springs, and consequently of the

oxidation of the alkaline sulphurets in contact with the atmosphere.

We can conceive that the water of the Geysir, passing into the state of vapour at a very high temperature in subterranean conduits formed by felspathic rocks, attacks their sides, and is thus charged with the salts which the analysis discovers in it.

Beside the analysis of the water of the Geysir, the memoir of M. Damour contains those of several of the springs of Reykir, on the south coast of the island, and that of the water of Laugarnes, situated at a short distance from Reykjavík. All these waters are alkaline and siliciferous; and, although the proportions of the substances which they contain are different in each, they offer, like that of the Geysir, a constant relation between the oxygen of the silica and that of the bases: only, the thermal springs of Reykir deposit, like the Geysir, numerous siliceous concretions, whilst that of Laugarnes forms none at all. The experiments of M. Damour explain this difference very well; for in the waters of Reykir, the constant relation between the oxygen of the silica and that of the bases is as 2 : 1; and after subtracting the quantity of alkalies necessary to saturate the chlorine and sulphuric acid, this relation becomes as 8 : 1. In this state of saturation, a part of the silica ceases to be soluble and is deposited.

In the water of Laugarnes, on the contrary, the primitive relation of the quantities of oxygen is as 3 : 1, as for the Geysir, and this relation, after the saturation of the chlorine and the sulphuric acid, becomes as 6 : 1, numbers which represent an alkaline silicate entirely soluble in water.

Strokkur.

The Strokkur, the Icelandic name of which, signifying *churn*, well indicates the continual movement of the water in the channel which contains it, is situated at about sixty metres to the south of the Great Geysir. It has not, like the latter, formed itself a regular and conical basin; the waters occupy a sort of level well, the edges of which are only encircled by a rounded mound, and worn by the frequent passage of the waters.

The dimensions of this channel are as follows:—

Total depth below the surface	13 ^m ·55
Diameter of the opening	2 ^m ·40
Diameter at 8 ^m ·30 below the surface	0 ^m ·26

The channel of this second jetting spring is therefore irregular, and its diameter diminishes rapidly from the surface to

the bottom. The level of the water, which generally stands at rather above three metres below the ground, falls principally after great eruptions, but it sometimes rises suddenly; and the surface of the water is incessantly boiling.

The following are some observations I made upon these oscillations:—

8th July, at 4 ^h 30 ^m P.M.: depth below the surface .	3·40 ^m
9th July, at 11 o'clock A.M.: after a great eruption .	4·22
9th July, after a second eruption, immediately after the first	4·55
9th July, at 3 o'clock P.M.: Strokkur calm; depth .	3·35
9th July, at 4 ^h 15 ^m P.M.: after a great eruption . .	4·40
9th July, at 4 ^h 59 ^m P.M.: Strokkur calm	4·40
9th July, at 5 ^h 32 ^m P.M.: <i>Id.</i>	3·05
9th July, at 7 ^h 15 ^m P.M.: <i>Id.</i>	3·50
10th July, at 6 ^h 57 ^m P.M.: <i>Id.</i>	3·55

To determine the temperature of this column of water, we employed the same thermometers as at the Great Geysir; but we only arranged three on a line. The following are the results obtained in these three experiments:—

<i>First Experiment.</i>		<i>Second Experiment.</i>	
8th July, at 4 ^h 38 ^m P.M.: height of the column of water above the bottom, 10 ^m ·15.		9th July, at 5 ^h 32 ^m P.M., an hour after a great eruption: height of the column of water above the bottom, 10 ^m ·50.	
	Heights. ^m		Heights. ^m
100·0	10·15	100·0	10·50
Therm. No. 1, D..... 108·0	6·00	Therm. No. 3, D..... 100·5	9·20
Therm. No. 3, B..... 111·4	3·00	Therm. No. 3, B..... 109·3	6·20
Therm. No. 8, B..... 112·9	0·30	Therm. No. 6, D..... 114·2	2·95
	Bottom.		Bottom.
Mean temperature of the column of water, 104°·77.		Mean temperature of the column of water, 105°·79.	

<i>Third Experiment.</i>	
10th July, at 6 ^h 57 ^m P.M., six hours after a great eruption: height of the column of water above the bottom, 10 metres.	
	Heights. ^m
99·9	10·00
Thermometer No. 3, D	99·9 8·85
Thermometer No. 3, B	113·7 4·65
Thermometer No. 6, D	113·9 0·35
	Bottom.
Mean temperature of the column of water, 105°·278.	

From the water bubbling incessantly at the surface, we

always supposed that its temperature there was that of boiling water; the second and third experiments show, in fact, that at one metre below the surface it is so. Lastly, a thermometer placed at the bottom of the channel during an eruption, indicated a temperature of 115° C.

We see that the column of the water of the Strokkur possesses, at different heights, much more constant temperatures than those of the Geysir, and that the influence of the eruptions is very little felt; this depends evidently on the continual boiling of the water, which determines incessantly ascending currents in the column and a sort of intermixture. The mean height of the column of water of the Strokkur is equal to a column of mercury at 0° , of $0^{\text{m}}\cdot 7179$.

The temperature at which the water, submitted to this pressure and to that of the atmosphere, would begin to boil, is $120^{\circ}\cdot 043$.

We have seen that the maximum temperature at the bottom of the Strokkur was 115° ; the difference between this maximum and the point of ebullition of the water is therefore $5^{\circ}\cdot 043$. This number is sensibly smaller than the corresponding number found at the Great Geysir, which may be owing to the point attained by the thermometer in the Strokkur being nearer to the subterranean source of heat, or to the channels which form the communication between the liquid column and that central point being narrower; the heat parted with by the vapour condensed to the water of this channel is transmitted more rapidly to the base of the column.

The eruptions of the Strokkur, which are quite as high as those of the Great Geysir, instead of presenting, like the latter, the appearance of a beautiful fountain with a base three metres in diameter, and a crown of about eighteen metres, present only an *ensemble* of sharp, jagged points, resembling very tall and slender yew-trees. In the Geysir, the eruptions always take place naturally; at the Strokkur, on the contrary, they are either natural or artificial. All travellers in fact have remarked that, if the orifice of the Strokkur is filled with light stones or pieces of earth and grass, the oscillations of the water cease for some instants; then, at the end of five or ten minutes, a jet of a blackish water charged with earth is shot out to a great height, and is followed by other intermitting jets during eight to ten minutes. This experiment, which can be repeated several times a day with success, seems to prove that the small excess of pressure resulting from the accumulation of clods of earth upon the water suffices to hinder the normal disengagement of the vapour through the column of water, and to augment the tension of that which

is formed in the subterranean reservoir, to the extent of producing a sudden detonation.

Be this as it may, as we desired to know whether the natural eruptions of the Strokkur were produced more regularly than those of the Geysir, and had any connexion with them, we only forced the eruptions twice at our arrival, and we noted, in the following table, all the great natural eruptions:—

Dates.	Forced Eruptions. Heights.	Natural Eruptions. Hours.
5th July, at 3 ^h 57 ^m P.M.	30 ^m .210	
9th July, at 4 ^h 15 ^m P.M.		49 ^m .375
9th July, at 9 ^h 25 ^m P.M.		43 ^m .528
10th July, at 12 ^h 15 ^m A.M.		not measured.
12th July, at 8 ^h 2 ^m A.M.		not measured.
13th July, at 3 ^h 30 ^m P.M.		47 ^m .426

It will be seen from this table that the natural eruptions of the Strokkur do not present more regularity than those of the Geysir, and that their coincidence with the latter, if it sometimes happens, is not general.

Some observers have admitted that there was a direct communication between the Strokkur and the Geysir: unfortunately I was not able, during my visit, to collect water from the Strokkur sufficiently freed from the earthy matters coming from the clods of earth which we threw upon it during the first days of our arrival, to bring it back and submit it to an accurate analysis; but the quantity of sulphuretted hydrogen, which I found to be 1^{cc}.748 per litre, differs sufficiently from that which the water of the Geysir contains, to allow us to conceive that these two springs, subjected perhaps to the same cause of subterranean heat, are not, as has been thought, in immediate connexion.

LXI. *On the existence of a New Vegeto-Alkali in Gun-Cotton.* By ROBERT PORRETT, Esq.*

AT the last meeting of this Society on the 7th instant, a joint communication from Mr. Teschemacher and myself "On the Chemical Composition of Gun-Cotton" was read †, and in the last paragraph but one thereof I suggested that a view

* Communicated by the Chemical Society; having been read Dec. 21, 1846.

† This communication will be found at p. 273 of the present volume of this Journal.

might be taken of the arrangement of its elementary particles which would account for the singular fact of its non-acidity, by supposing it a compound of nitrous acid and oxide of lignin, the said new oxide being also supposed to possess alkaline properties.

It was before mentioned that the action of a small voltaic battery on gun-cotton gave but very slight indications of its decomposition, but that slight as they were they were rather favourable to the suggestion; the indifference of this compound to the electrical action of the battery I found was owing to its highly non-conducting nature, but I succeeded rather better by moistening the gun-cotton with acetic acid, and placing it in a thin layer between a plate of silver and one of zinc, completing the circuit with a copper wire and leaving them for forty-eight hours so arranged; at the end of that time, on dismounting this little voltaic arrangement, the inner surface of the silver plate was found to be encrusted with a small quantity of matter, which when dry looked white like starch, which powerfully restored the blue colour of reddened litmus paper, and which when heated to redness left a black carbonaceous residue; the minute quantity obtained made further experiments with it impossible, but it increased my desire to be able to procure the same substance by chemical means in any quantity that might be wanted.

After several trials I succeeded by the following process.

I took 2 ounces by measure of nitric acid of the specific gravity 1.45, to which I added 50 grains of gun-cotton; the mixture being gradually heated to 100° of Fahrenheit's scale and kept below 180° , became quite transparent and fluid, and all the cotton disappeared, being quietly dissolved without any evolution of gas; when a portion of the solution in this state was dropped into water a white precipitate was formed, having all the properties, structure excepted, of the original gun-cotton, the liquid was therefore a simple solution in acid, without decomposition; but on heating the liquid further up to 240° , deep red acid vapours were given off abundantly for a long time, and these collected in a receiver surrounded with ice proved to be hyponitrous acid. The same effect exactly took place when sulphuric acid was used instead of nitric at the same temperature, so that the hyponitrous acid was merely eliminated in both cases, and not formed from the excess of nitric acid used in the former instance; the cold and very concentrated nitric solution containing a great excess of acid was now brought nearly, but not quite, towards a neutral state by pouring into it a strong solution of subcarbonate of potash, after which a solution of bicarbonate of potash was used

until perfect neutrality was obtained: the liquid in this state became thickened, not only by the quantity of minute crystals of nitrate of potash formed in it beyond what it could hold in solution, but also by an abundant precipitate of a whitish-gray colour, consisting of the new alkali probably in a state of carbonate. The liquid was heated nearly to ebullition, during which its colour became darker, and it was set aside in order that the crystals of nitrate of potash formed on cooling might be separated and the liquid disembarassed of them as much as possible; after this it was evaporated to dryness by the heat of a water-bath, and the brownish residue was acted upon first by sulphuric æther, which however would not dissolve any portion of it; next by alcohol, specific gravity 0·813, which took up a very small quantity of the new alkali; then spirit of wine was tried; the solvent power was greater than that of the alcohol, but it had the disadvantage of taking up with the alkaline matter a very minute quantity of the nitrate of potash which accompanied it, so that I could not get an exclusive solvent for the former; the principal quantity by far of the carbonated alkaline oxide of lignin still remained, and I was obliged to use a small quantity of cold, distilled water for its solution, notwithstanding that the water took up at the same time some of the nitrate of potash and bicarbonate of potash remaining in the mass. This aqueous solution contains the new alkali in abundance, but mixed as stated with the other two salts, it acts very powerfully upon reddened litmus paper.

When a portion of the hyponitrous acid is neutralized by an aqueous solution of the new alkali, hyponitrite of oxide of lignin, probably identical with gun-cotton or with xyloidine, is reproduced and precipitated as an insoluble compound.

Thus I have verified the accuracy of my suggestion, excepting that instead of nitrous acid neutralized by a new alkali existing in gun-cotton, it is the hyponitrous acid so neutralized that constitutes that substance. For this new alkali I have proposed the name *Lignia*, and believe that it opens a wide field for scientific research.

I suspect that in the natural decay of woody fibre from leaves and rotting plants this alkali is produced in combination with acetic acid, and that the acetate of lignia so formed exists in the sap of all vegetables; further, that it is decomposed and deoxidated by light acting on the leaves and bark of trees, reproducing lignin for the assimilation and growth of the plant: its action also on the animal system deserves inquiry. I lay no stress however on these speculations, excepting so far as they may stimulate others to enter into in-

vestigations which it is impossible for me, consistently with my other duties, to find time for carrying on, and I leave the subject without reserve to those who have more leisure.

I would remark, that the composition of lignia must be that of lignin plus 2 atoms of oxygen given up by the nitric acid in becoming hyponitrous acid; and I would further observe, that the latter acid and lignia forming an insoluble compound, may probably be employed as mutual tests of each other's presence in combinations.

LXII. *On some new Researches in Animal Chemistry. Extracted from a Letter from Professor LIEBIG to Dr. A. W. HOFMANN*.*

I AM at present occupied with the investigation of the constituents of the animal fluids which are found without the blood and lymphatic vessels. The fluid from flesh, for example, reacts strongly acid, and the question was, whence arose this acidity? After overcoming more difficulties than I have ever experienced in any investigation, I have for the first time indisputably proved that free lactic and phosphoric acid exist in the whole organism wherever muscle is found. How curious, that in the absence of all proofs on the part of the opponents of lactic acid, I should now demonstrate to them its existence in the flesh of oxen, fowls, calves, and sheep, by preparing and analysing the most beautifully crystallized zinc and lime salts! How wonderful, that in the animal organism acids and alkalies are found separated by a membrane, constituting myriads of little galvanic circles, which, as such, must produce chemical and electrical effects! To the latter class I refer all the observations of Matteucci, which can now be easily explained.

I have further found that the flesh of the muscles of oxen, fowls, sheep, calves, and the carnivorous pike contain creatin, prepared by Chevreul eleven years ago, and which, from Berzelius's not being able to reproduce it, has since then, in a measure, disappeared from the field of science. Creatin is a beautiful substance, having the formula $C_8 N_3 H_{11} O_6$. At the temperature of $100^\circ C.$ it loses 2 equivs. of water, and becomes $C_8 N_3 H_9 O_4 =$ glycocoll + ammonia or caffenin + amidogen and water. Heated in a stream of hydrochloric acid, creatin loses 4 equivs. of water and takes up 1 of hydrochloric acid. By this treatment, however, its nature is entirely altered, being now converted into a beautiful organic base, the properties of which are totally different from those

* Communicated by the Chemical Society; having been read Dec. 21, 1846.

of creatin. It becomes now soluble in water, and forms with bichloride of platinum a fine crystallized double salt.

I have, finally, discovered two other new bodies in the same fluids, of which the one crystallizes in needles, the other in plates of the lustre of mother-of-pearl. Unfortunately I have obtained scarcely sufficient for two analyses from 40 lbs. of the flesh of oxen and 20 of that of fowls.

I see a boundless field before me, and doubt not that for every *quality* of the animal body, something which can be estimated *quantitatively*, will also be discovered to which it is indebted for its properties.

I have also satisfied myself as to the part which common salt plays in the bodies of animals. I have found that the fluids without the blood and lymphatic vessels contain only potash-salts, viz. chloride of potassium and phosphate of potash, with phosphate of magnesia, whilst the blood and lymph contain merely those of soda (phosphate of soda). If, therefore, the latter are indispensable to the formation of blood and the processes of life, it is evident that an animal on the continent, which finds in plants only potash-salts, should have chloride of sodium given to it, by means of which the phosphate of potash of the seeds and of the rest of the plant is transformed into chloride of potassium and phosphate of soda. I found further that the salt brine which flows from salted meat contained certainly alkaline phosphates, and that scurvy is hence easily explained by the deficiency in the salted meat of the alkaline phosphates necessary to the formation of blood. The soup from boiled meat contains the soluble phosphates of the flesh, and the meat itself the insoluble. Neither the soup nor the flesh alone can maintain the processes of life, but both must be taken together. The English have in this respect hit upon the proper practice. In a theoretical point of view their food is more correctly combined than that of the Germans.

Still more wonderful results have been obtained by the oxidation of casein by means of peroxide of manganese and sulphuric acid, by M. Gugelberger. Three products are obtained: the first of which is aldehyde, the second oil of bitter almonds, and the third a fluid æthereal body with a composition similar to metacetone. The aldehyde was analysed as aldehydite of ammonia, of which a considerable quantity was obtained. From oil of bitter almonds the most beautiful benzoic acid was produced by the action of chlorine.

From these results a sort of conception may be obtained how and wherefore many medicines have a certain deleterious or useful action.

Urea, creatin, glycocoll, leucin, cystin, &c. are organic bases, and only products of the animal body or its elements, and organic bases are partly poisonous, partly beneficial in their action. I have caused the new experiments of Mulder on his protein to be repeated. The substance prepared by Fleitmann in this laboratory, according to his new method, and supposed to be free from sulphur, still contains 1.5 per cent., as does likewise a similar preparation by Laskowski.

I beg of you to communicate this short notice to the Chemical Society, of which I have the honour to be a member.

LXIII. *On the Salts of Sulphurous Acid.*

By J. SHERIDAN MUSPRATT, Esq., Ph.D.*

I HAVE been induced to return to this subject by a paper lately published by Dr. Rammelsberg in Poggendorff's *Annalen* †. In his treatise he differs slightly from me in the quantities of water contained in some of the sulphites, but as he invariably took his water as loss and as mine was generally determined by combustion with chromate of lead, I did not think it worth while going over all the analyses that I had performed in Giessen, being so thoroughly convinced of the accuracy of those results. The only point where we materially disagree is regarding the constitution of the red sulphite of copper, and which will be subsequently discussed under that head. When I first undertook, in Baron Liebig's laboratory, the investigation of the salts of sulphurous acid, very little was known of their constitution; but since that time they have occupied the attention of numerous chemists, and are now invested with as much interest as the compounds of any other acid. They are very readily decomposed either by moisture or heat, and on this account may have led different chemists to the assumption of various formulæ for the same salt. For example, there is scarcely any salt of this acid which does not contain traces of sulphate, which are always overlooked in the analysis; and moreover, unless great care is exercised in oxidizing the sulphurous acid when its quantity is to be determined, serious errors may arise. I shall now proceed to the description of the sulphites under their respective heads.

Sulphite of Soda.—This salt is obtained by transmitting sulphurous acid through a solution of carbonate of soda until the liquid becomes acid, and then allowing the solution to

* Communicated by the Chemical Society; having been read Dec. 21, 1846.

† lxvii. pp. 245, 391; or No. 89 of the Chemical Gazette, July 1, 1846, p. 254.

repose for some days under a bell-jar over sulphuric acid. The crystals possess a cooling and sulphurous taste, and are slightly alkaline to test-paper.

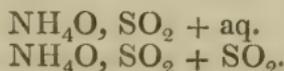
Analysis:—7·10 grs. burned with chromate of lead gave 4·19 grs. of water = 59·01 per cent.

Formula, $\text{NaO}, \text{SO}_2 + 10 \text{ aq.}$

Vauquelin has described a sulphite of soda of the following constitution:— $\text{NaO}, \text{SO}_2 + 8 \text{ aq.}$; and Rammelsberg analysed one which agreed with the annexed formula: $\text{NaO}, \text{SO}_2 + 7 \text{ aq.}$ The varying proportions of water can only be accounted for by the different temperatures at which the salts have been formed.

Bisulphite of Soda.—Dr. Rammelsberg obtained an acid salt agreeing with the formula $2(\text{NaO}, 2\text{SO}_2) + \text{aq.}$ I find however on referring to his results that the formula $\text{NaSO}_2 + \text{HO SO}_2$, which I gave in my former paper, corresponds quite as well with his numbers as the one given by him.

Sulphite of Ammonia.—I have not yet succeeded in procuring from an aqueous solution an ammoniacal sulphite of sufficient stability for analysis. The salts formed in alcohol and æther, and heretofore described*, possessed the subjoined formulæ:—



I have produced another ammonia sulphite by passing sulphurous acid into a very strong solution of caustic ammonia until there was no smell either of the acid or alkali, and then adding cautiously absolute alcohol. After a short time a brilliant white saline mass of crystals subsided, which were allowed to remain undisturbed for twenty-four hours. I threw the whole on to a filter and dried the crystals over sulphuric acid. This salt is alkaline, and evolves traces of ammonia.

Analysis:—23·09 grs. of the salt gave 33·88 grs. sulphite of barytes = 9·32 sulphurous acid or 40·36 per cent.

The formula $2(\text{NH}_4\text{O}, \text{SO}_2) + \text{NH}_3 + 3 \text{ aq.}$ requires 40·09 per cent. sulphurous acid.

Sulphites of the Protoxide of Iron.—Finely divided metallic iron dissolves pretty readily in sulphurous acid, imparting to the liquid a slight green tinge. When this liquid is evaporated *in vacuo*, very small and almost colourless crystals are obtained, which are but slightly soluble in water. Exposed to the air in a dry state they are not readily altered, but if moist are rapidly converted into sulphate. I found my sulphurous acid, on analysing this compound, to agree so closely

* *Ann. Chem. und Pharm.*, Bd. l. p. 285.

with what Fordos and Gelis had previously given, that I neglected determining the iron and water. 5.65 grs. gave 7.00 grs. sulphate of barytes = 1.92 grs. sulphurous acid or 33.98 per cent. Centesimally represented,—

		Theory.	F. & G.	Mean.
1 eq. protoxide of iron	439.21	37.28	37.23	
1 ... sulphurous acid .	401.17	34.06	34.04	33.98
3 ... water	337.44	28.76	28.73	
	<u>1177.82</u>	<u>100.00</u>	<u>100.00</u>	

Formula, $\text{FeO}, \text{SO}_2 + 3 \text{ aq.}$

Sulphites of the Peroxide of Iron.—When sulphurous acid is passed through water holding in suspension freshly-precipitated peroxide of iron, the whole readily dissolves, and the liquid acquires a blood-red colour, which disappears on exposure to the air. If this solution be boiled so as to expel the uncombined acid, and then left to the atmospheric action for some days, it acquires a light brownish colour, and deposits a reddish brown compound containing sulphurous acid and peroxide of iron. Dr. Kœne of Brussels expresses the constitution of this salt by the formula



I found that when caustic potash was added very gradually to the blood-red solution above-mentioned, a yellowish crystalline precipitate formed, the liquid remaining still acid. This compound, after filtration, edulcoration and desiccation, gave the following results :—

9.01 grs. gave 2.18 grs. peroxide of iron.

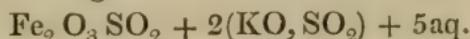
6.21 grs. gave 6.80 grs. sulphate of barytes = 1.87 grs. sulphuric acid.

10.11 grs. gave 1.50 gr. water.

Composition per cent. :—

		Theory.	Found.
3 eqs. Sulphurous acid.	1203.51	30.66	30.11
2 ... Potash	1179.84	30.06	
1 ... Peroxide of iron.	978.42	24.93	24.19
5 ... Water	562.50	14.35	14.83
	<u>3924.27</u>	<u>100.00</u>	

These numbers agree with the formula

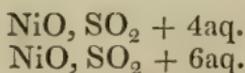


This double sulphite of iron and potash appears to be Kœne's salt, in which 2 equivalents of water are replaced by 2 equivalents of sulphite of potash.

The sulphite of the peroxide of iron forms a salt with sulphite of ammonia, but it is very difficult to get a sufficient

quantity of it for analyses. It contains 33·68 per cent. of peroxide of iron.

Sulphites of Nickel.—The two salts which I described in my first paper on the sulphites possessed the subjoined formulæ :—

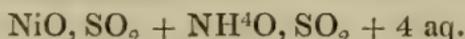


The latter agrees with the results of Fordos, Gélis and Rammelsberg. I have again prepared the former by transmitting a stream of sulphurous acid gas into water holding in suspension carbonate of nickel. When the whole had dissolved, the solution was filtered and boiled, which liberated a mass of small greenish crystals, almost insoluble in water.

6·02 grs. salt burned with chromate of lead gave 2·08 grs. water = 34·55 per cent.

The formula $\text{NiO, SO}_2 + 4\text{aq.}$ affords 34·06 per cent. of water.

Dr. Böttinger has described a double sulphite of nickel of the following constitution :—



Sulphites of Cadmium.—Carbonate of cadmium in water is readily dissolved by sulphurous acid, and when the resulting solution is treated with absolute alcohol an aluminous-looking precipitate appears, which when dried and heated in a test-tube affords no water—it must be the anhydrous sulphite of cadmium. If however the gelatinous precipitate is allowed to remain in the alcoholic menstruum for some days it disappears, and there deposit in its stead beautiful silvery-looking crystals, which turn out to be the same salt as that obtained by Fordos and Gélis when dissolving the metal in the acid.

Analysis:—6·79 grs. of salt gave 6·94 grs. sulphate of barytes = 1·90 sulphurous acid.

8·11 grs. salt gave 1·29 grs. water.

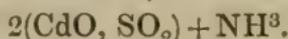
Centesimally represented,—

	Theory.	Found.	
1 eq. Sulphurous acid	401·17	28·19	27·98
1 ... Oxide of cadmium	796·77	55·99	
2 ... Water	224·96	15·82	15·90
	<hr/> 1422·90	<hr/> 100·00	

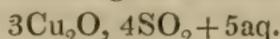
Formula, $\text{CdO, SO}_2 + 2\text{aq.}$

It is oxidized very slowly when exposed to the air in a dry state; is sparingly soluble in water, and readily dissolved by dilute acids. I have not formed any double salts with the sul-

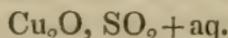
phite of cadmium. Dr. Rammelsberg analysed a salt which gave him the annexed formula,—



Sulphites of Copper.—The brilliant red salt which is formed either by boiling together solutions of sulphate of copper and sulphite of ammonia, or passing sulphurous acid into water, holding in suspension oxide of copper, was first analysed by Chevreul, whose numbers agree with the formula—



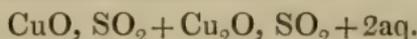
I gave the annexed formula for this salt,*—



Böttiger† analysed the compound after me, and his results favour the formula—



Rammelsberg however has stated that the salt is a double one, and constituted as follows:—



I have reprepared the red salt by passing a stream of sulphurous acid into water containing pure oxide of copper. When the whole had dissolved, the liquid was boiled. The red compound deposited in pretty large crystals, which were thrown upon a filter and effused with cold water *until the filtrate did not afford the slightest milkiness with a solution of chloride of barium.* They were then dried *in vacuo.* I tested the salt for *sulphuric acid* before submitting it to analysis, and found traces present, even after taking every precaution.

Analyses.

			Per cent.
I.	8.84 grs. gave	9.36 BaO, SO ₃ =2.55	SO ₂ =28.84
II.	7.41 ...	8.32 BaO, SO ₃ =2.28	SO ₂ =30.76
III.	6.52 ...	7.11 BaO, SO ₃ =1.95	SO ₂ =29.90
IV.	9.62 ...	10.03 BaO, SO ₃ =2.759	SO ₂ =28.68
V.	18.47 ...	20.71 BaO, SO ₃ =5.69	SO ₂ =30.80
I.	13.41 grs. salt with	PbO, CrO ₃ gave	1.38 HO=10.29
II.	11.13 PbO, CrO ₃ ...	0.97 HO= 8.71
III.	10.74 PbO, CrO ₃ ...	1.02 HO= 9.49

Mean of the above per cent.

Sulphurous acid	. . .	29.796
Water	9.496
Base or loss	60.708
		<hr/> 100.000

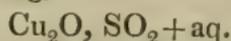
* *Ann. Chem. und Pharm.*, bd. l. p. 285.

† *Ibid.* bd. li, p. 412.

The copper has also been determined in the salt by my brother Frederick and myself.

					Per cent.
F. M.	6.94	grs. salt gave	4.70	CuO = 4.22	Cu ₂ O = 60.80
Dr. M.	11.41	7.48	CuO = 6.72	Cu ₂ O = 58.88

It is evident from the preceding results, *that if the base of this salt be only suboxide of copper*, its constitution may be assumed as the following,—



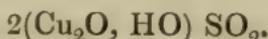
Dr. Rammelsberg found the annexed quantities of sulphurous acid,—

35.12 35.06 34.47,

which I can only account for by his salt not being so well washed as mine. My salt dissolves in hydrochloric acid, affording a colourless liquid, and when a weak solution of potash is added to this, a white precipitate subsides. Yellow prussiate of potash gives a whitish precipitate in the acid solution, soluble in an excess of muriatic acid. When the red salt is placed on a filter and affused with boiling water until the filtrate affords no milkiness with barytes, half of its sulphurous acid is removed.

7.37 grs. gave 4.41 grs. BaO, SO₃ = 1.21 SO₂ = 16.41 per cent.

This quantity favours the formula—

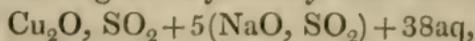


Böttiger states in his paper, “That by long boiling with the liquid this salt loses sulphurous acid and becomes lighter in colour, which confirms my own observation. The analysis of such a precipitate gave me 66.60 per cent. of Cu₂O, which will account for the varying results obtained by different chemists when analysing this red salt.”

Sulphite of Copper and Soda.—When rather strong solutions of sulphate of copper and sulphite of soda are mixed together and treated with absolute alcohol, a dark reddish-looking liquid, which greatly resembles nitrobenzide, subsides, and if this be collected and left *in vacuo* for some time, fine yellow crystals appear, which after washing and desiccation afforded the subjoined results:—

					Per cent.
16.40	grs. salt gave	7.27	grs. HO		= 44.32
15.17	1.81	... CuO	= 1.62	Cu ₂ O = 10.67
6.13	5.44	... BaO, SO ₃	= 1.49	SO ₂ = 24.30

These numbers agree very closely with the formula—



if there was no uncombined sulphite of soda present.

Represented per cent.—

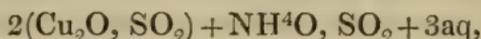
		Theory.	Found.
1 eq. Suboxide of copper	891·39	9·36	10·67
6 ... Sulphurous acid	2407·02	25·62	24·44
5 ... Soda	1954·50	20·52	
38 ... Water.	4274·24	44·50	44·32
	<u>9527·15</u>	<u>100·00</u>	

This salt, when heated in a test-tube, first gives off large quantities of water, then sulphurous acid, and the residue consists principally of sulphates of copper and soda, with traces of sulphur, &c.

Sulphite of Copper and Ammonia.—I mixed a solution of sulphate of copper with a large excess of sulphite of ammonia, then put the mixture into a long wide tube containing a thermometer, and applied heat. I removed the solution from the sand-bath at 136° F., because at this temperature silvery-looking flakes began to deposit, which powerfully refracted the light. In a short time a large quantity of a shining compound had separated, which when dried over sulphuric acid and submitted to analysis gave the annexed results:—

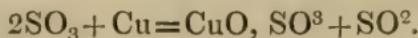
8·690 grs. salt gave 10·73 sulphate of barytes = 2·952 grs. sulphurous acid = 33·97 per cent.

As these numbers agreed so closely with Böttinger's formula*,—



I thought further determinations unnecessary. When this compound is heated with water, it is decomposed and the red gritty salt separates. Great care is required in the preparation of this sulphite, for it is difficult to prevent some of the red salt depositing with it. The best way to obtain it in large quantities, is to heat slightly a strong solution of sulphate of copper with an excess of bisulphite of ammonia, then cork the vessel and allow the mixture to remain undisturbed for some days.

All the sulphurous acid for my experiments was obtained by the deoxidation of sulphuric acid by means of metallic copper,—



* Liebig's *Annalen*, bd. li. p. 411.

LXIV. *Analysis of the Bohemian Glass as found in the Combustion Tubes employed in Organic Analysis.* By Mr. THOMAS ROWNEY*.

THE difficultly fusible variety of Bohemian glass has been repeatedly the subject of chemical investigation. We are indebted to Berthier†, Dumas‡, Gras§, and lately Peligot¶ for an analysis of this substance. The glass analysed by these chemists was of the kind usually employed in the manufacture of Bohemian goblets, an art in which Bohemia has excelled for centuries.

It appeared desirable to compare with the results of these analyses the composition of the glass which is used for the combustion of organic substances, and which likewise is chiefly manufactured in Bohemia. The properties most valued in this variety of glass are essentially different from those sought for in other kinds, and it was to be expected that this material, which has so greatly contributed to the progress of organic chemistry, and upon which the German manufacturers have of late bestowed so much attention, would also show some marked peculiarity of composition.

The following analysis was made in the laboratory of the Royal College of Chemistry. The glass was selected from the stock of the laboratory, recently imported from Germany, and which numerous trials had proved to be of first-rate quality. The tubes, though perfectly tractable in an energetic blow-pipe flame, scarcely altered their form during the longest combustion in a strong charcoal fire; they never cracked during the process, even on suddenly increasing the temperature, and not uncommonly the same tube could be employed repeatedly in analysis.

A careful qualitative examination of the specimen having proved the presence of silicic acid, lime, sesquioxide of iron, alumina, oxide of manganese, magnesia, soda and potassa, the quantitative determination was proceeded with.

a. Determination of Silicic Acid.

The finely-powdered glass was ignited with carbonate of soda, treated with hydrochloric acid in excess, evaporated to dryness, once more ignited and digested with hydrochloric acid, after which all the silicic acid remained insoluble; it was collected on a filter, washed, ignited and weighed.

I. 2.1890 grms. of glass gave 1.5946 grm. silicic acid.

II. 2.1284 grms. of glass gave 1.5628 grm. silicic acid.

* Communicated by the Chemical Society; having been read Dec. 21, 1846.

† Gmelin's *Handbuch*, vol. ii. 366.

‡ Dumas' *Traité de Chimie*, vol. ii. 528.

§ *Ibid.*

b. Determination of Lime.

The acid filtrate from the silicic acid was neutralized by ammonia, then slightly acidulated with acetic acid and precipitated by oxalate of ammonia, and the oxalate of lime converted into carbonate in the usual manner.

I. 2·1890 grms. of glass gave 0·4203 gm. of carbonate of lime.

II. 2·1284 grms. of glass gave 0·3843 gm. of carbonate of lime.

c. Determination of Alumina and Sesquioxide of Iron.

To the liquid filtered from the precipitate of oxalate of lime (*b.*), containing a large quantity of chloride of ammonium, ammonia was added in excess, by which alumina and sesquioxide of iron were precipitated and the oxide of manganese and magnesia were retained in solution. The joint precipitate of alumina and sesquioxide of iron was collected, washed, ignited and weighed; then dissolved in hydrochloric acid and thrown down with potassa, which kept the alumina in solution. The precipitate of sesquioxide of iron (thus obtained), once more dissolved in hydrochloric acid and reprecipitated by ammonia, was now collected, ignited and weighed; the difference in the amount of the two weighings gave the amount of the alumina thus:—

2·1890 grms. of glass gave 0·0096 gm. of mixed oxides and 0·0029 gm. of sesquioxide of iron; the difference, 0·0067 gm., is the amount of alumina.

d. Determination of Oxide of Manganese.

This oxide was thrown down by carbonate of soda as carbonate of manganese, from the filtrate, separated from the oxide of iron and alumina (*c.*); the precipitate was washed, dried, ignited, and calculated as the compound oxide and sesquioxide of manganese (Mn_3O_4).

I. 2·1890 grms. of glass gave 0·0114 gm. of the above-named double oxide.

II. 2·1284 grms. of glass gave 0·0160 gm. of the double oxide.

e. Determination of the Magnesia.

The filtrate from the carbonate of manganese (*d.*) was precipitated by phosphate of soda; the phosphate of magnesia and ammonia was washed, dried, ignited and weighed as pyrophosphate of magnesia.

I. 2·1890 grms. of glass gave 0·0127 gm. of pyrophosphate of magnesia.

II. 2.1284 grms of glass gave 0.0177 grm. of pyrophosphate of magnesia.

f. Determination of the Alkalies.

To determine the amount of the alkalies, a portion of the glass was fused with hydrate of baryta; the fused mass was digested in hydrochloric acid, evaporated to dryness and ignited, the residue again digested in hydrochloric acid, and then filtered from the insoluble silicic acid. Carbonate of ammonia was then added to the filtrate, to separate the alumina, sesquioxide of iron, baryta and lime; the solution filtered from the precipitate was evaporated to dryness and ignited, to drive off the ammoniacal salts, and the magnesia separated by means of baryta; the excess of baryta was separated by means of carbonate of ammonia, and filtered. The filtrate, evaporated to dryness and ignited, yielded the mixed chlorides of the alkaline metals; they were dissolved in water, bichloride of platinum added, and the solution evaporated to dryness on a water-bath; the residue was digested in strong alcohol, when the potassio-chloride of platinum remained undissolved; it was collected on a weighed filter, washed with alcohol, and dried in a water-bath until it ceased to lose weight. The filtrate from the potassio-chloride of platinum was evaporated to dryness, and, after the addition of some oxalic acid to the residue, ignited till it was completely decomposed; the residue was digested in water, filtered, and the filtrate evaporated to dryness in a weighed capsule; its increase in weight gave the quantity of chloride of sodium.

1.5645 grm. of glass gave 0.9330 grm. of potassio-chloride of platinum, and 0.0905 grm. of chloride of sodium.

From the results enumerated, the calculated per-centage is as follows:—

	I.	II.	Mean.
Silica	72.84	73.42	73.13
Lime	10.75	10.11	10.43
Alumina	0.30	...	0.30
Peroxide of iron	0.13	...	0.13
Magnesia	0.21	0.30	0.26
Protoxide of manganese	0.37	0.54	0.46
Soda	3.07	...	3.07
Potash	11.49	...	11.49
	<u>99.16</u>		<u>99.27</u>

The amount of oxygen in the bases stands to the amount of oxygen in the silicic acid nearly as 1:6.

From the preceding analysis, it results that the composition of the glass for combustion-tubes does not differ very much

from the material employed in goblets, as may be seen from the following comparison with the results of the chemists formerly named:—

	Berthier.	Dumas.	Gras.	Peligoť.
Silicic acid	71·7	69·4	71·6	76
Lime	10·3	9·2	10·0	8
Alumina	0·4	9·6	2·2	1
Sesquioxide of iron .	0·3	...	3·9	
Oxide of manganese	0·2	...	0·2	
Magnesia	2·3	
Soda	2·5	
Potassa	12·7	11·8	11·0	15
	<u>98·1</u>	<u>100·0</u>	<u>101·2</u>	<u>100</u>

In one of the last numbers of Dr. Otto's German translation of Professor Graham's Elements of Chemistry, recently published, I find the analysis of a combustion-tube enumerated, performed in the laboratory of Dr. Otto, which has not yet been published in the Journals. He obtained the following results:—

Silicic acid	74·0
Lime	7·2
Alumina	} 0·1
Sesquioxide of iron . .	
Potassa	18·5
	<u>100·0</u>

The oxygen in the bases of this glass stands to the oxygen in the silicic acid likewise as 1 : 6. Soda is entirely absent, and completely replaced by potassa. The presence however of a small amount of soda does not seem to be injurious to the glass.

LXV. *On a Generalization of a Theorem of Euler in reference to the Products of the Sums of Squares.* By J. R. YOUNG, Professor of Mathematics in Belfast College*.

THE recent researches of Sir W. R. Hamilton, of Professors Graves and De Morgan, and of Messrs. J. T. Graves and Cayley, in reference to the new analytical theory of imaginary quantities, have revived attention to Euler's theorem, that the sum of four squares, multiplied by the sum of four squares produces the sum of four squares; to which theorem an extension was given by Lagrange, by the introduction of coefficients into the component squares.

These recent researches have suggested the inquiry as to

* Communicated by the Author.

whether or not the theorem admits of generalization; and in the Philosophical Magazine for April 1845, Mr. J. T. Graves announced that he had arrived at the truth that "the product of two sums of eight squares is a sum of eight squares;" but adds, that "the full statement and proof of the theorem must be reserved for another time." I have anxiously inquired at every likely source of information to which I have access, for the publication of this proof; and am disposed to conclude, from the result, that such publication has not yet been furnished.

I have been thus led to enter into an independent investigation of the subject; and find that the theorem holds not only for four and eight, but also for sixteen, and indeed for any number of squares expressed by an integral power of 2. This investigation I propose to forward to the British Association, at its meeting in June, provided I be assured that I have not been anticipated in the generalization here announced. By thus alluding to the results at which I have arrived, in the pages of this Journal, I shall afford timely opportunity for the information being communicated to me, if the above-mentioned extension of Euler's theorem has ever as yet been published.

The notation in which the subordinate theorems and the general theorem is announced is this, viz.

$$\Sigma_2(\square) \times \Sigma_2(\square') = \Sigma_2(\square''), \quad \Sigma_4(\square) \times \Sigma_4(\square') = \Sigma_4(\square''),$$

and generally

$$\Sigma_n(\square) \times \Sigma_n(\square') = \Sigma_n(\square''),$$

where n is any integral positive power of 2.

The theorem holds too when certain coefficients are introduced: thus, taking eight squares, it is true that

$$\begin{aligned} & (s^2 + bt^2 + cu^2 + bcv^2 + bcw^2 + cx^2 + by^2 + z^2) \\ & \times (s'^2 + bt'^2 + cu'^2 + bcv'^2 + bcw'^2 + cx'^2 + by'^2 + z'^2) \\ & = s''^2 + bt''^2 + cu''^2 + bcv''^2 + bcw''^2 + cx''^2 + by''^2 + z''^2. \end{aligned}$$

And also that

$$\begin{aligned} & (s^2 + \alpha t^2 + \alpha^2 u^2 + \alpha^3 v^2 + \alpha^4 w^2 + \alpha^5 x^2 + \alpha^6 y^2 + \alpha^7 z^2) \\ & \times (s'^2 + \alpha t'^2 + \alpha^2 u'^2 + \alpha^3 v'^2 + \alpha^4 w'^2 + \alpha^5 x'^2 + \alpha^6 y'^2 + \alpha^7 z'^2) \\ & = s''^2 + \alpha t''^2 + \alpha^2 u''^2 + \alpha^3 v''^2 + \alpha^4 w''^2 + \alpha^5 x''^2 + \alpha^6 y''^2 + \alpha^7 z''^2. \end{aligned}$$

And the same has place for any number of squares expressed by a power of 2.

Belfast, May 8, 1847.

LXVI. *On a Method for the Analysis of Bodies containing Nitric Acid, and its application to Explosive Cotton.* By WALTER CRUM, F.R.S.*

AT the first meeting of the present Session of the Philosophical Society, I gave an account of some experimental inquiries into the nature of gun-cotton, a body whose composition was then little known. I had at that time chiefly occupied myself with its nitrous contents, and described a method by which some approximation could be made to a quantitative result for nitric acid. On resuming the subject, I found that much was wanting to render the method a rigorously accurate one; and I shall now relate what I have since done to simplify and complete it. I shall first, however, give an account of its application to nitrate of potash,—a body of known composition, and easily obtained in a state of purity,—to which I had recourse as a means of proving the accuracy of the method, and detecting any fallacy to which it might be liable.

Nitric Acid in Nitrate of Potash.—The salt I employed was purified by repeated crystallization, and fused at little more than its melting heat. A glass jar, eight inches long and an inch and a quarter in diameter, is filled with, and inverted over mercury. A single lump of the fused nitrate, weighing about six grains, is let up into it, and afterwards fifty grains of water. As soon as the nitrate is dissolved, 125 grains of sulphuric acid, ascertained to be free from nitric acid, are added. By the action of the mercury upon the liberated nitric acid, deutoxide of nitrogen soon begins to be evolved, and usually in about two hours, without the application of heat, the whole of the nitric acid is converted into that gas. Occasional agitation is necessary, and it is easily performed by giving a jerking horizontal motion to the upper part of the jar. The surface of the sulphuric acid is then marked, and three-fourths of a cubic inch of solution of sulphate of iron, recently boiled, let up into the jar. The gas is rapidly absorbed, except a small portion at last, which must be left several hours to the action of the solution, or be well agitated in a smaller tube with a fresh portion of it. No correction of the nitric oxide has to be made for moisture; for the mixture of acid and water which I employed, as I ascertained by direct experiment, has no perceptible force of vapour. In one experiment,

5·40 grains nitrate of potash yielded

4·975 cubic inches of gas, at 60° Fahr., and bar. 30 inches.

The residue not absorbable by sulphate of iron, was

* Read before the Philosophical Society of Glasgow, April 14, 1847, and communicated by the Author.

0.015 cubic inch; leaving

4.96 cubic inches of nitric oxide = 1.594 grains NO_2 , and which correspond to

2.869 grs. nitric acid, or 53.13 per cent. of the nitrate of potash.

Four consecutive experiments made in this manner yielded—

53.13

53.14

53.73

53.29

Mean 53.32

or leaving out the third experiment, 53.19.

The calculated per-centage of nitric acid in nitrate of potash, the acid being represented by 6.75, and the potash by 5.8992, is 53.36*.

In order further to determine whether the presence of organic matter would interfere with the liberation of the nitric oxide, the experiment was repeated with the addition of three grains of cotton wool, which was first dissolved in the sulphuric acid; the result was 53.24.

Other nitrates are analysed in the same manner. For salts in powder, which it is difficult to pass through mercury without loss, I cut a quarter-inch glass tube into little cylinders for them, of half an inch long, and close up the ends with thin paper fastened with gum. In the analysis of numerous samples of crude nitrates, the residue, which is azote, may be taken as a constant quantity, and the jar graduated in such a manner that the volume of gas may be read off at once as the per-centage of nitric acid.

Preparation of Gun-Cotton.—The cotton I employed was fine Sea Island. It was first thoroughly carded, and then bleached, by boiling in caustic soda, and steeping in solution of bleaching powder; then caustic soda again, and afterwards weak nitric acid. It was well washed and beaten in a bag with water after each operation. When burnt, 10,000 parts left 9 of ashes. It was considered to be lignine, nearly pure.

The cotton, dried and carded after bleaching, was exposed in parcels of 10 grains each, for several hours, to the heat of a steam-bath, and each parcel was immersed, while hot, in an ounce measure of the following mixture:—

One measure sulphuric acid, spec. grav., 1.840.

Three measures of pale lemon-coloured nitric acid, of 1.517. After one hour it was washed in successive portions of water, till no trace of acid remained, and dried in the open air.

* By Thomson's numbers the per-centage of nitric acid in nitre is 52.94; by Berzelius, 53.44.

30 grs. of bleached cotton wool, dried at 65° F., became, after being some hours in a steam bath,

28.32 grs., and lost, therefore, 5.6 per cent. of water. It increased to

51.08 grs. when made into gun-cotton, and dried in the open air.

Dried further *in vacuo*, over sulphuric acid, it was reduced to 50.40 grs., and lost therefore 1.33 per cent. of water.

100 of dry cotton produced 177.9 of dry gun-cotton.

The gun-cotton thus prepared is whiter, but less transparent, than the original bleached wool. It appears to be little liable to change, but a slight elevation of temperature causes a commencement of decomposition, and the colour becomes more or less brown. It is much less tenacious than cotton wool. Dissolved in nitric acid, and tested with chloride of barium, it gives no indication of sulphuric acid.

The increase of weight above stated is the greatest I have been able to obtain; and I had completed its analysis in the manner I shall describe, when I found reason to believe that it still contained a portion of unaltered cotton. With a view to saturate that portion, it was immersed a second time, and for twenty-four hours, in the same mixture of acids, but without yielding any greater quantity of nitric acid.

An immersion of one hour in nitric acid alone gave a better result. It lost in weight by this second process 0.47 per cent. It was little altered in appearance, but after being dried in the open air, it lost in the air-pump only 0.69 per cent., instead of 1.33, as in the former case. It is this substance of which I shall now relate the analysis.

Ashes in Gun-Cotton.—Sixteen grains of gun-cotton were dissolved in nitric acid. The solution being evaporated by degrees, and burnt to ashes, left 0.035 gr. of a reddish ash, or 0.22 per cent.

Nitric Acid in Gun-Cotton.—In this process the same apparatus is employed as for nitrate of potash. About 6 grains of the gun-cotton, containing a known quantity of water, is collected into a ball—squeezed between the finger and thumb to free it as much as possible from air—and let up into the jar, over the mercurial trough. 125 grains of sulphuric acid are added to it. Nitric acid is liberated, and, being acted upon by the mercury, produces nitric oxide. After one hour, when about three-fourths of the whole gas has been evolved, and the gun-cotton is entirely dissolved, 50 grains of water are added. In another hour the increase of gas ceases; in a few hours more its boundary is noted, then treated with sulphate of iron, and the residue measured. It consists of azote

from the common air introduced with the gun-cotton, and a minute portion also, which is always accidentally entangled between the mercury and the glass. Its oxygen is absorbed by the mercury, when in the state of nitrous acid.

In one experiment—

6.02 grs. of gun-cotton = 5.978, after being dried over sulphuric acid *in vacuo*, and =

5.964 grs., after deducting ashes, produced

5.513 cubic inches of gas, bar. 30 in., therm. 60°, of which 0.08 was left by sulphate of iron.

5.433 cubic inches, therefore, were deutoxide of nitrogen, =

1.746 grs. NO_2 , which represent

3.143 grs. of nitric acid, or 52.70 per cent.

Another experiment gave 52.68 per cent.

The gun-cotton prepared by a single immersion gave only 51.42 per cent. of nitric acid.

Carbon in Gun-Cotton.—Having failed to obtain good results by burning this substance with oxide of copper, I used chromate of lead, precipitated from the nitrate, and heated to redness. I employed for the combustion an apparatus which I used many years ago for the analysis of indigo, and I still find it very convenient for substances which do not require a strong red heat. It consists of a tube of hard glass, eight inches long and three-eighths of an inch in diameter; the gases from which are led by a small bent tube under the receiver in a mercurial trough.

1 inch at the closed end of the tube is filled with 8 grains chlorate of potash, ground with chromate of lead.

$4\frac{1}{2}$ inches are filled with chromate of lead, among which is ground to powder 3 grains of the gun-cotton.

$1\frac{1}{2}$ inch contains chromate of lead that has been used to wash out the mortar.

A glass plug separates these materials from the perforated cork which joins the two tubes. The materials are gradually heated with broad-wicked spirit-lamps. Carbonic acid comes over, mixed, when in the receiver, with nitric oxide and the azote of the apparatus; and when all the gun-cotton is consumed, the lamps are extended to the chlorate of potash. The oxygen gas thus liberated, which in other cases is useful to consume carbonaceous matter that may have escaped the chromate, expels in this case all remains of carbonic acid, and passing itself into the receiver, mixes there with the nitric oxide, and causes its entire absorption by the mercury. Oxygen and azote are then the only gases left along with the carbonic acid, and as those are not absorbable, an addition of half a cubic inch of solution of caustic soda indicates exactly the quantity of carbonic acid present.

In one experiment, 2.993 grs. of gun-cotton (after deducting water and ashes) yielded 7.952 cubic inches of gas, of which 5.733 was carbonic acid, = 0.739 grs. carbon, or

24.69 per cent.

A second experiment gave 25.16

Mean . . . 24.92

Elements of Water in Gun-Cotton.—To burn gun-cotton for the purpose of collecting its oxygen and hydrogen in the state of water, I ground up 10 grains of it with pounded flint, and used the combustion tube already described, having attached to it a chloride of calcium tube, and afterwards a tube with asbestos moistened with sulphuric acid. But along with water, ammonia and other matters were obtained, which destroyed the result. I next used a thin glass tube of a foot and a half long, bent so that a foot in the middle of it could dip into cold water. Such water as would condense at 65° Fahr. was collected. The gas was led through it into a mercurial trough, and measured. A trace of cyanogen appeared in the last portions of gas, while the oxygen from the chlorate of potash was burning a quantity of charcoal that had escaped the nitric acid.

After the experiment, the refrigerating tube was found studded with large crystals of bicarbonate of ammonia. It contained very little water in the liquid state. The crystals and the liquid were washed out with more water, converted into muriate of ammonia, and found to contain 0.675 gr. NH_3 2CO_2 , the hydrogen of which represents 0.299 gr. of water. There were besides

2.025 grs. water in the tube. In the 22 inches of gas which were obtained, assuming it to be saturated with moisture, which is doubtful, there was

0.088 gr. of water—making in all

2.412, from which must be deducted

0.160 gr. hygrometric water in the gun-cotton and in the flint, leaving

2.252 for the water in 9.92 grs. of dry gun-cotton or 22.70 per cent.

In a second experiment, where the only difference was in having moistened cotton for the gas to pass through before entering the mercurial trough, the water obtained only amounted to 20.61 per cent. I did not proceed further. These were the two last of a number of experiments, and the determinations of nitric acid and carbon are so much more satisfactory, that I prefer resting the water contents upon their results.

Purified cotton wool (lignine) is composed of $\text{C}_{12} \text{H}_{10} \text{O}_{10}$.

During its transformation into gun-cotton, there is no indication of change in the proportions of its oxygen and hydrogen. The difference, therefore, between the weight of the substance employed and that of the nitric acid and carbon found by experiment, is oxygen and hydrogen in the proportions which form water.

The experiments I have related give the following for the composition of gun-cotton:—

52·69 nitric acid,
24·92 carbon, and leave
22·39 for the elements of water.

100·00

These numbers are nearly in the proportions of 12C, 7HO, 3NO₅,

Found.	Calculated.
52·69	52·69 = 3NO ₅ .
24·92	23·41 = 12C.
22·39	20·49 = 7HO.
100·00	96·59

Leaving a remainder of 3·41 per cent., consisting of 1·51 carbon, and 1·90 water. These however, are nearly the proportions which form lignine.

Found.	Calculated.	
1·51	1·51 = 12C	} = lignine
1·90	1·88 = 10HO	

Gun-cotton, from the form in which it is produced, is not one of those substances we can expect to obtain in absolute purity. Every previous improvement in its preparation had diminished this excess of unaltered cotton, and I had no reason to suppose the last portion perfect, considering the difficulty with which some of the previous stages of improvement had been attained.

The specimen I have thus examined consists, therefore, of—

96·59 gun-cotton (12C, 7H, 7O, 3NO₅).
3·41 lignine (12C, 10H, 10O).

100·00

And pure gun-cotton consists of—

24·24 = 12C.	24·24 = 12C.
21·21 = 7HO.	2·36 = 7H.
54·55 = 3NO ₅ .	14·14 = 3N.
100·00	59·26 = 22O.
	100·00

It is lignine in which three atoms of water are replaced by three atoms of nitric acid.

LXVII. *On the Conversion of Relief by Inverted Vision.* By
 Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S.
 Edin.*

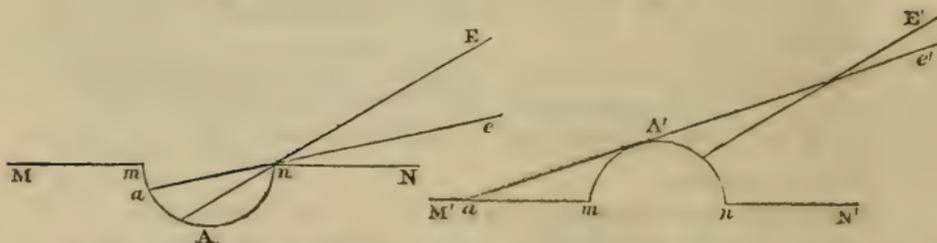
UNDER the name *Conversion of Relief*, an expression first used by Mr. Wheatstone, I include all those optical illusions which take place in the vision of cameos and intaglios, of elevations and depressions, whether they are produced with opaque or transparent bodies,—no surfaces with or without shadows,—in reflected or transmitted light,—while using one or both eyes,—or by erect or inverted vision. In these various forms of the phænomenon, the illusion is modified by certain secondary causes, which were regarded both by Mr. Wheatstone† and myself‡ as primary causes; so that we were led away, each in a different direction, from the right path of inquiry.

The phænomenon occurs in its most general and simple form, when it is produced by viewing a shadowless depression, or elevation, made in an extended surface, through an inverting microscope, or the inverting eye-piece of a telescope, and at an angle intermediate between 0° and 90° . In so far as I know, the phænomenon has never been thus limited, and, consequently, no explanation of it has ever been given. That which I shall now submit to the Society is capable of the most rigorous demonstration; and when it is once in our possession, we can have no difficulty in recognising the secondary causes which increase or diminish the influence of the primary one, and which, in its absence, are sometimes the immediate cause of the illusion.

Let A, fig. 1, be a deep spherical concavity, and A', fig. 2,

Fig. 1.

Fig. 2.



a high spherical convexity in an extended horizontal table MN, M'N', and let them be shadowless, or illuminated by a

* Read at the Royal Society of Edinburgh, May 6, 1844. See their Transactions, vol. xv. p. 657.

† Philosophical Transactions, 1838, pp. 383, 384.

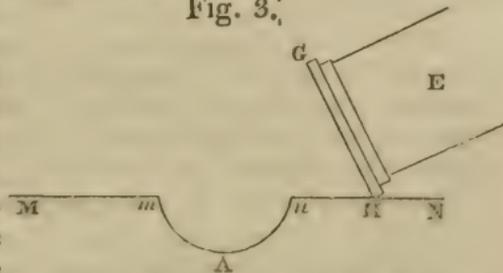
‡ Edinburgh Transactions, vol. xv. p. 365; Edinburgh Journal of Science, vol. iv. p. 97; and Letters on Natural Magic, p. 98.

quaquaversus light, like that of the sky. If the observer, placed at a moderate distance, view these objects in the directions $E A$, $E' A'$, either with one or with both eyes, his accurate appreciation of the distances $E A$, $E' A'$, will prove to him that A is a *concavity*, and A' a *convexity*; but if $E A$, $E' A'$ approach to equality, either from the distance of the observer, or from the shallowness of A , or the slight elevation of A' , he will cease to recognise any difference in the distances $E A$, $E' A'$, and will be unable to tell which is the convexity, and which the concavity. So great, indeed, is this uncertainty, that from causes which he cannot discover, they will sometimes appear convex and sometimes concave. In this indetermination of the judgement, a touch of A , A' by the finger, or the introduction of a shadow, will remove or confirm the illusion, whatever it may be. The same result will be obtained if we view A and A' vertically, with an erect or inverting eye-piece. In all these cases, we suppose that the circular, or rather the elliptical, base of the convexity or concavity is distinctly seen.

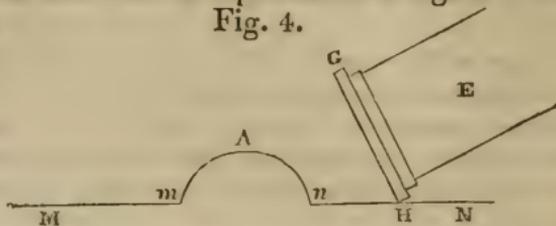
Let us now look at A , A' , at obliquities varying from 0° to 90° . In fig. 1 the *concavity* A will have an elliptical section at all obliquities, till, at 90° , it appears a straight line; but in the *convexity* the effect is very different. In passing from 0° to the position E' , fig. 2, the circular section of A' will appear an ellipse; but in passing from E' to 90° , the appearance of A' will lose all resemblance to A . When the eye is at e' , for example, the summit A' of the convexity will cover the point a of the table, and am will be invisible; and near 90° , the convexity A will eclipse the whole surface of the table $m M$, however extended it may be, and will rise above it.

Let us now suppose that the eye at E , fig. 3, views the concavity A through the inverting eye-piece $E G H$, the horizontal table $M N$ must obviously be inverted as well as the hollow A ; but the *apparent* change, produced by inversion, is very different from the real change. The surface $M N$, out of which A is excavated, and upon which the observer leans, and rests the lower end H of his inverting eye-piece, appears to remain where it was, and still to look upwards, in place of appearing inverted, and looking downwards. When he strikes the table with the end H of the eye-piece through which he looks, he believes that it is the lower end of

Fig. 3.



the field of view that strikes the table, and rests upon it. With these convictions, he sees what is represented in fig. 4. The concavity $m A n$, fig. 3, appears inverted; and as the visible part of the concavity $A m$, fig. 3, is nearest the eye in fig. 4, and the invisible part $A n$, fig.

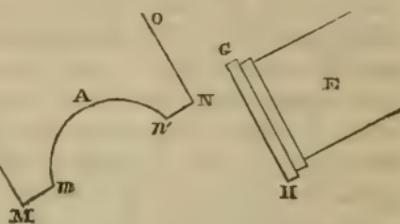


3, furthest from the eye in fig. 4, $m A n$ must appear a concavity in fig. 4, solely because it seems to rise out of the surface $M N$, which looks upward, as if it had not been inverted by the eye-piece.

Now, in this experiment, the conversion of the concavity into a convexity depends on two separate illusions, one of which springs from the other.

The *first* illusion is the belief that the surface $M N$ is looking upwards, whereas it is really inverted, as shown in fig. 5; and the *second* illusion, which arises from the first, is, that the point n appears *furthest* from the eye, whereas it is

Fig. 5.



nearest to it, as shown in fig. 5. All these observations are equally applicable *mutatis mutandis* to the vision of convexities; and hence it follows, that the conversion of relief, occasioned by the use of an inverting eye-piece, is not produced directly by the inversion, but by an illusion, in virtue of which we conceive the remotest side of the convexity or concavity to be nearest our eye when it is not.

In order to demonstrate the correctness of this explanation, let the concavity $m A n$ be made in a *narrow stripe* of wood, as in fig. 5, and let it be viewed, as formerly, through the inverting eye-piece. It will now appear, as in fig. 5, really inverted, and free from both the illusions which formerly took place. The narrow surface $M N$ being now wholly included in the field of view, and the thickness $N O$ of the stripe of wood distinctly seen, the inversion of the surface $M N$, which now looks downward, will be at once recognised. The edge n of the concavity will appear *nearest* the eye*, as it really is, and *the concavity, though inverted, will still appear a concavity*. The very same reasoning is applicable to a convexity on a narrow stripe of wood.

* The inversion of an object never makes the *nearest* part of an object *more remote*, nor the *remote* part *nearest*.

When, as in fig. 4, the concavity is seen as a convexity, let it be viewed more and more obliquely. The *elliptical margin of the convexity will always be visible*, which is impossible in a real convexity; and the elevated apex will gradually sink till the elliptical margin becomes a straight line, and *the imaginary convexity completely leveled*. The struggle between truth and error is here so singular, that while one part of the figure *m A n* has become concave, the other part retains its convexity!

In like manner, when a convexity is seen as a concavity, the concavity loses its true shape, as it is viewed more and more obliquely, till its remote elliptical margin is encroached upon by the apex of the convexity; and, towards an inclination of 90° , the concavity disappears altogether, under circumstances analogous to those already described.

If, in place of using an inverting eye-piece, we invert the concavity *m A n*, by looking at its image in the focus of a convex lens, it will sometimes appear a convexity, and sometimes not. In this form of the experiment the image of the concavity, and consequently its apparent depth, is greatly diminished. Hence any trivial cause, such as a preconception of the mind, or an approximation to a shadow, or a touch of the hollow by the point of the finger, will either produce a conversion, or prevent it.

In the preceding experiments we have supposed the convexity to be high and the concavity deep and circular, and we have supposed them also to be shadowless, or illuminated by a *quaquaversus* light, such as that of the sky in the open fields. This was done to get rid of all secondary causes, which interfere with and modify the normal cause when the concavities and convexities are shallow, and have distinct shadows, or when the concavity has the shape of an animal, or any body which we are accustomed to see convex.

Let us now suppose that a strong shadow is thrown upon the concavity. In this case the normal experiment, already explained and shown in fig. 5, is much more perfect and satisfactory. The illusion is complete, and invariable when the concavity is in an extended surface; and it as invariably disappears when it is in a narrow stripe.

In the secondary forms of the experiment, the inversion of the shadow becomes the principal cause of the illusion; but, in order that the result may be invariable, or nearly so, the concavities must be shallow, and the convexities a little raised. At great obliquities, however, this cause of the conversion of form ceases to produce the illusion, and in varying the inclination from 0° to 90° , the cessation takes place sooner with

deep than with shallow cavities. The reason of this is, that the shadow of a concavity is very different at great obliquities from the shadow of a similar convexity. The shadow never can emerge out of a cavity so as to darken the surface in which the cavity is made; whereas the shadow of a convexity soon extends beyond the outline of its base, and finally throws a long stripe of darkness over the surface on which it rests. Hence it is impossible to mistake a convexity for a concavity, whenever its shadow extends beyond its base.

When the concavity is a horse or a dog upon a seal, it will often rise into a convexity when seen through a single lens which does not invert it; but the illusion disappears at great obliquities. In this case the illusion is favoured, or produced, by two causes: the first is, that the convex form of the horse or dog is the one which the mind is most disposed to seize; and the second is, that we use only one eye, with which we cannot measure depths as well as with two. The illusion, however, still takes place when we employ a lens *three* or more inches wide, so as to admit the use of both eyes, but it is less certain, as the binocular vision enables us to keep in check, to a certain degree, the other causes of illusion.

The influence of these secondary causes is strikingly displayed in the following experiment. In the armorial bearings upon a seal, the shield is often more deeply cut than the surrounding parts. With binocular vision the shallow parts rise into a convexity sooner than the shield, or continue so while the shield remains concave; but if we shut one eye, the shield then becomes convex like the rest. In these experiments with a single lens, a slight variation in the position of the seal, or a slight change in the direction or intensity of the illumination, or particular reflexions from the interior of the stone, will favour or oppose the illusion. In viewing the shield, or the deepest portion, with a single lens, a slight rotation of the seal round the wrist, backwards and forwards, will remove the illusion, in consequence of the eye perceiving that the change in the perspective is different from what it should be.

In a paper in the *Edinburgh Journal of Science*, already referred to, I have described several other examples of the conversion of form, in which inverted vision is not employed. As seen by the naked eye, hollows in *mother-of-pearl*, and other semi-transparent bodies, rise into relief; and the same thing happens on surfaces of agate and woods of various kinds when transparent circular portions are illuminated by refraction, at those parts of their circumference where they would have been illuminated had they been convexities*. But the

* In examining, under the microscope, the shallow fluid cavities *within*

most interesting cases of conversion of form are those in which *the mind alone* operates, and receives no aid either from inversion, shadow, or monocular vision. "If we take, as I have elsewhere remarked, one of the Intaglio moulds, used in making the bas-reliefs of that able artist Mr. Henning, and direct the eyes to it steadily, without noticing surrounding objects, we may coax ourselves into the belief that the Intaglio is actually a bas-relief. It is difficult at first to produce the deception, but a little practice never fails to accomplish it. We have succeeded in carrying this deception so far as to be able, by the eye alone, to raise a complete hollow mask of the human face into a projecting head. In order to do this we must exclude the vision of other objects; and also the margin or thickness of the cast. This experiment cannot fail to produce a very great degree of surprise in those who succeed in it; and it will, no doubt, be regarded by the sculptor (who can use it) as a great auxiliary in his art*."

From these observations it will be seen that the conversion of form, excepting in the normal case, depends upon various causes which are effective only under particular conditions; such as the depth of the hollow or the elevation of the relief, the distance of the object, the sharpness of vision, the use of one or both eyes, the inversion of the shadow, the nature of the object, and the means used by the mind itself to produce the illusion. In the normal case, however, where the cavity or convexity is shadowless, and upon an extended surface, and where inverted vision is used, the conversion of form depends solely on the illusion, which it is impossible to resist, that the side of the cavity or elevation next the eye is actually furthest from it—an illusion not produced by inversion, but by a false judgement respecting the position of the surface on which the form is placed.

St. Leonard's College, St. Andrews,
May 4, 1844.

LXVIII. *Experiments proving the common nature of Magnetism, Cohesion, Adhesion and Viscosity.* By Sir GRAVES C. HAUGHTON, K.H., M.A., F.R.S., Foreign Associate of the Institute of France, &c.†

PART I.

VARIOUS methods of experimenting have been employed of late years to extend the knowledge of magnetic phæthe substance of a film of sulphate of lime, described in the Edinburgh Transactions, vol. x. p. 35, they frequently appeared as *elevations* on the surface of the plate next the eye.

* Edinburgh Journal of Science, No. VIII. p. 109, Jan. 1826.

† Communicated by the Author.

nomena, and to which we are indebted for many very valuable discoveries. That which I am now about to explain differs from all that have preceded it, and possesses at least the advantage of simplicity united to singular delicacy*.

It must have been clear to every one, that the phænomena observed by M. Arago in his well-known experiments made in the year 1824, when he caused a needle to oscillate over the surfaces of various substances, were due to the proximity of the needle to the body observed, as much as to the nature of the body itself; and the discovery of this distinguished natural philosopher was confirmed by Dr. Seebeck of Berlin, on making similar observations. Those I have now to detail are entirely due to the same cause, carried to the extent of *actual* and *forced* contact.

I now proceed to explain the mode in which the experiments have been performed.

Mode of operating.

If the substance to be examined is flat, as a piece of glass, for instance, it should be set up on a *very steady* table, in a vertical position, and a magnetic needle, of which a description will be presently given, is to be placed on the point of a fine sewing-needle, about half an inch in height, which may be run through a slice of cork to answer as a support; but that which I myself employ has a flat brass foot about the size of a shilling, with the fine end of a sewing-needle fixed in the centre, and the brass foot is truncated close to the supporting needle, to enable it and the object to be brought close together for the purpose of observation. If a cork support is used, the sewing-needle should for the same reason be run through it near the edge.

When a minute object, such as a precious stone or any small fragment of metal, is to be examined, it should be placed on a piece of cork or any other suitable body, in such a position to the natural direction of the needle, that on the latter

* The general principle upon which these observations have been made was discovered so far back as the month of March 1841; it was not however applied to ascertaining whether the various classes of substances were magnetic or not till the spring of 1845, when nearly 140 substances were found to be magnetic, and a few measurements of the intensities were obtained, but it was at the commencement of 1846 that the greatest part of them were made. The remainder have been only recently completed; though these remarks were in such a state of forwardness as to lead to a hope of their being published in the summer of 1845. Want of health has been the cause of these various delays.

being urged to it by a bar-magnet, its point will exactly touch some portion that is smooth and vertical. These conditions being strictly attended to, and the object under examination being placed about a quarter of an inch or so from the point of the magnetic needle, the pole of the bar-magnet that is of a contrary character to the point of the needle intended to touch the object, is to be approached with extreme gentleness and very slowly, so as to cause the least tremulous motion possible in the needle. When the bar-magnet is brought so as to cause the needle to press with a slight force against the substance examined, the former is to be kept in the same steady horizontal position, for at least ten or fifteen seconds after it has been observed that all tremor has ceased in the needle; upon which the bar-magnet may be withdrawn as slowly and as carefully as it was brought near. And this is the place to observe, that one great advantage of having the supporting pivot so short, and consequently the magnetic needle so low down, is that it enables the observer to allow the magnet to lie upon the table, and to advance and draw it back at pleasure, as well as to have its axis in the same horizontal plane as that in which the needle revolves; for should it be higher or lower than the latter, it would cause an inclination, and a consequent displacement of the needle, from gravitation the moment the bar was withdrawn, that might interfere with the success of the experiment. If all the conditions just described have been attended to, the needle will be found to have attached itself firmly to the glass, copper, or other body operated upon, and will in many cases so remain any indefinite time, if not disturbed. But it should be well-remarked that *time* must generally be given to the magnetic needle to settle, and thereby coerce the substance to which it is to attach itself. *This is an essential condition in most cases.* The slightest movement in the room, even the motion of the air from the impulsion given by the breath of the observer, are to be carefully avoided in the nicer experiments. Any vibration in the house also will, in most cases, from the extreme delicacy of the operation, cause a failure. Some time, too, will in all probability elapse before the experimenter will have acquired the dexterity necessary to the success of the difficult cases, and he must be careful that the point of the magnetic needle does not catch itself against the object under examination when it is rough; and there is also great danger of its becoming jammed when it approaches an angle of 90° with the magnetic meridian. To prevent such a consequence, the object should be so placed, when the amplitude is large, that only the *side* of the point of the magnetic needle may rest

against it. Either pole of the needle may be used, and the bar-magnet may be made to influence it either by attraction or repulsion, as is most convenient for the observation. The operator will likewise find it an advantage to use a low seat, as he will by that means be able to have his eye in the same level as the object examined as often as it may be necessary, particularly when minute objects are observed.

All that has just been said merely regards the mode of avoiding a failure; but if it is desired to measure the relative magnetic intensities of the substances under examination, then a graduated disc is to be employed, which may be placed under the supporting needle, its N. and S. being in the line of the magnetic meridian; but a very convenient substitute for these two will be found in detaching the bottom of one of the compass-boxes, of which I shall immediately speak, covered with its engraving of the rhombs and degrees, and cutting away so much of it as might interfere with the approximation of the different bodies. The nearer the needle can be brought to 90° by the attraction or repulsion of the bar-magnet, the greater is to be considered the magnetic intensity of the object, which is to be shifted after each measurement so as to increase the angle of amplitude till it is found that the needle will no longer attach itself, which consequently gives the limit of its magnetic intensity. This being an exhausting and tentative process, renders such measurements extremely tedious in many cases, and yet it cannot be remedied; for, owing to the time lost in waiting for the oscillations of the needle to cease, as well as for it to connect itself with the substance, and in the various adjustments of the latter, much time is expended; added to which, the experiment must be often repeated in difficult cases, to be quite sure that a failure has really arisen, from a want of attractive affinity between the needle and substance. Nearly an hour has been consumed in an experiment where much attention and delicacy of manipulation were required.

Great care of course must be taken not to confound effects purely electric with those that are magnetic. To prevent such a possibility of error, it is only necessary to breathe upon a suspected substance, which will effectually remove all free electricity. Thus Iceland spar, which cannot be touched with the fingers without becoming electric and remaining so for days, may in the space of a few seconds be rendered fit for examination, as the humidity of the breath quickly evaporates and carries off the superabundant electricity.

It is well-worthy of remark, as showing how much *time* is necessary, not merely for the exhibition of magnetic attraction

by some substances, but also for the loss of this quality, that in many cases where the needle was with difficulty made to attach itself, on contact being broken a fresh connexion was produced with the greatest ease, if only a short delay had taken place, and no change whatever had been made in the position of the object. Time, too, was still more necessary to the success of the experiments when the angles were to be measured; for it not unfrequently happened that, when I had supposed the greatest amplitude had been attained, on maintaining the forced connexion for a few minutes with an enlarged angle, a firm connexion took place; thus throwing a doubt upon the measures I had previously noted as the highest attainable in other substances. Such measures, therefore, even in any case, can only be considered as approximative, as greater deviations may possibly be yet obtained by myself or other observers. Thus in the case of a spark of diamond, I was able to increase the angular distance from 42° to 65° . So likewise in the case of quartz, it rose gradually from 45° to 90° . Now when it is considered that the attachment of the needle takes place generally with the greatest readiness when the angle of deviation is small, say 10° or 15° , owing to its limited deviation from the meridian in such cases, it can leave but little doubt that this peculiarity arises from the slow effect of magnetic propagation in the substance examined, in which, to use language that has become familiar in describing magnetic phænomena, *saturation* takes place but slowly.

The foregoing facts entirely accord with the remark made by Sir John Herschel and Mr. Babbage, when in 1825 they varied the experiments of M. Arago. A remarkable confirmation of this peculiarity was exhibited by chromium, which was entered, after many trials with a magnetic needle, as a failure, yet on repeating the experiment, and allowing a long time for the needle to operate, the measurements rose successively, according to the notes taken on the occasion, as follows: 20° , 25° , 30° , 40° , 45° , 49° , 55° , 70° , 90° . The notes on the subject having been overlooked, a fresh attempt at measurement, after the lapse of more than a year, was recently made, when it was again entered as a failure, as there was not the least tendency to a connexion between the needle and the chromium after the ordinary time given to other difficult substances.

On reflecting on the necessity of time being given to develop the magnetism of some substances, and remembering that the needle, which was the active agent in its production, did not weigh four grains, and that the parts of the needle and the substance in contact were merely two minute points, it struck

me that the magnetism might be much increased by keeping it in close contact for a short time with a more powerful magnet. The result fully justified this anticipation, and the attractive affinity between the needle and the substance was immediately augmented; but generally in those cases where no attraction had been already discovered, none made its appearance by the additional magnetic influence to which it was subjected. Want of health and time prevented me from establishing more than the general character of these last facts, but I shall have to speak more fully of the particular cases tried as I proceed.

Among the few substances placed in contact with the bar-magnet for the purpose of increasing their magnetic energy, the following seem to be totally uninfluenced by its presence, viz. copper, lead, marble, selenite, alum and pure lac.

The following experiment will show how strong the connexion often is that takes place between the needle and non-ferruginous substances. A magnet having been brought vertically over the needle contained in a small box-compass, which was screened by a piece of crown-glass, the needle attached itself instantly, or, to speak more exactly, *per saltum*, so firmly to the glass, that the box might be pushed backwards and forwards from one end of the chimney-piece on which it lay to the other, without contact being broken; and in this state it continued for six days, though the door of the room was often shut with considerable force; and the connexion was at last only broken by the box having inadvertently received a shock from the hand. The contact was occasionally renewed by the same means, sometimes for two or three days, and always continued till broken at pleasure. This experiment will not succeed if the pivot of suspension is either too high or too low; and the needle must be of the pointed kind.

It is proper to describe here a very useful electrometer which I employed to ascertain whether substances exhibit any *electrical effects*. It is of extreme simplicity, and is not merely *much more* sensitive than that employed by Häüy, but even more so than the most delicate gold-leaf electrometer. It consists of a magnetic needle five-eighths of an inch in length, with an agate cap. Upon each end of the needle a piece of oaten straw, one inch in length and about one-thirtieth of an inch in diameter, is fixed. When this part of the system is placed, as in the former case, on the point of a sewing-needle with a brass stand, the instrument is complete. It has an additional advantage over that of Häüy, that, owing to the

slight magnetism of the needle, the points of which are inserted in the straws, it preserves a definite position, without the slight magnetism of the needle apparently interfering with its sensibility. It has likewise the advantage of being easily set up or dismounted to protect it from injury, and is capable of being contained in a very small box. I ought to remark likewise that it is so susceptible, that if unprotected by a screen it is kept in a sort of perpetual movement, by the otherwise inappreciable currents of air in the room.

Proper forms of the needles and magnets.

After the trial of needles of various forms and lengths, I have found none that answer so well as those which are employed in the compasses of commerce, set in mahogany boxes two inches square, and to which I have already alluded. The needles in these boxes are an inch and a half long, very slender and *pointed*, weighing about three grains and a half. Their caps are of brass, which in this case is an advantage; for needles with those of agate are much too tremulous from their great sensibility, and the experiment will be almost sure to fail if the point of the needle slips in the least from the spot where it first settles, the circumference of its action being as limited as the part of the point of the needle actually in contact. Long needles, therefore, owing to their great oscillations, are particularly unfit for these experiments; while, on the contrary, I have succeeded extremely well with a needle not more than five-eighths of an inch long.

The preceding directions simply concern the kind of magnetic needle that is to be employed; but it is, as I have said, by the aid of a bar-magnet that the needle is made to attach itself to non-ferruginous bodies; for by no other means could it be caused to approximate itself by a slow, steady, and gentle movement, and be left without disturbance when the motive agent is withdrawn, a condition upon which the success of the experiment entirely depends in the cases of low intensity; but for those of higher attraction, I shall presently show a simpler mode of attaining the same end without a magnet. The bar-magnet which I use is very convenient; it is six inches and a half long, three-quarters of an inch wide, and rather better than a quarter of an inch thick. If the magnet is long, it is managed with difficulty; and if small, it requires to be brought very near the object, and there is consequently danger of disturbing the position of the needle when the magnet is withdrawn, which had better likewise be wrapt in soft paper, to enable it to be laid down on the table without causing a jar

or shock. I now proceed to an important step in these discoveries.

On the relative dynamic values of the degrees of the compass, and on the cause of the needle resting in the magnetic meridian.

Shortly after I had begun to measure the angles of deviation, I saw that it was a matter of the first necessity to ascertain whether the degrees of the compass after 90° increased in strength, or went on diminishing in some given ratio, as it appeared to me that the connexions were made with increased facility in proportion as the N. pole of the needle approached nearer to 180° . For the purpose of settling this question, I made a number of most careful measurements, after drawing proper lines upon a sheet of paper, which all crossed one another in a common centre, by ascertaining the distances at different azimuths at which a bar-magnet, placed with its axis directed to the centre of a magnetic compass-needle (the pivot of suspension of which was exactly over the common centre of the lines of which I have spoken), caused the needle to assume a position in which its axis was in the same line with that of the magnet, I found that these points of distance form a peculiar curve, which was nearest to the axis of the magnet at the east and west azimuths, and receded from it as it advanced to the north and south; and was twice the distance from it at the north that it was at the south azimuth. Such is the sum total of the results which I communicated to the Royal Society in June 1846, and of which a short account has been given in the Abstracts of the Papers read before the Society, and republished in this Journal for last November*. It since however occurred to me, that the longest distance from the east and west azimuths would be better ascertained by operating by the means of repulsion than by attraction, as I had done; for it is evident that repulsion will sooner discover a minute deviation of the needle when in its natural position at N. than can be effected by attraction, because if the axis of the bar-magnet deviates in the slightest degree from the line, a false value will be obtained; and there always therefore remains a doubt in proceeding by attraction, respecting the point where the reciprocal action of the needle and the magnet ceases, in consequence of the identity of their axis. Now in proceeding by repulsion, the reverse takes place; and the minutest difference, except so far as the friction of the needle on its pivot is concerned, is immediately detected. I was surprised therefore to find that where I had only been able to discover a devia-

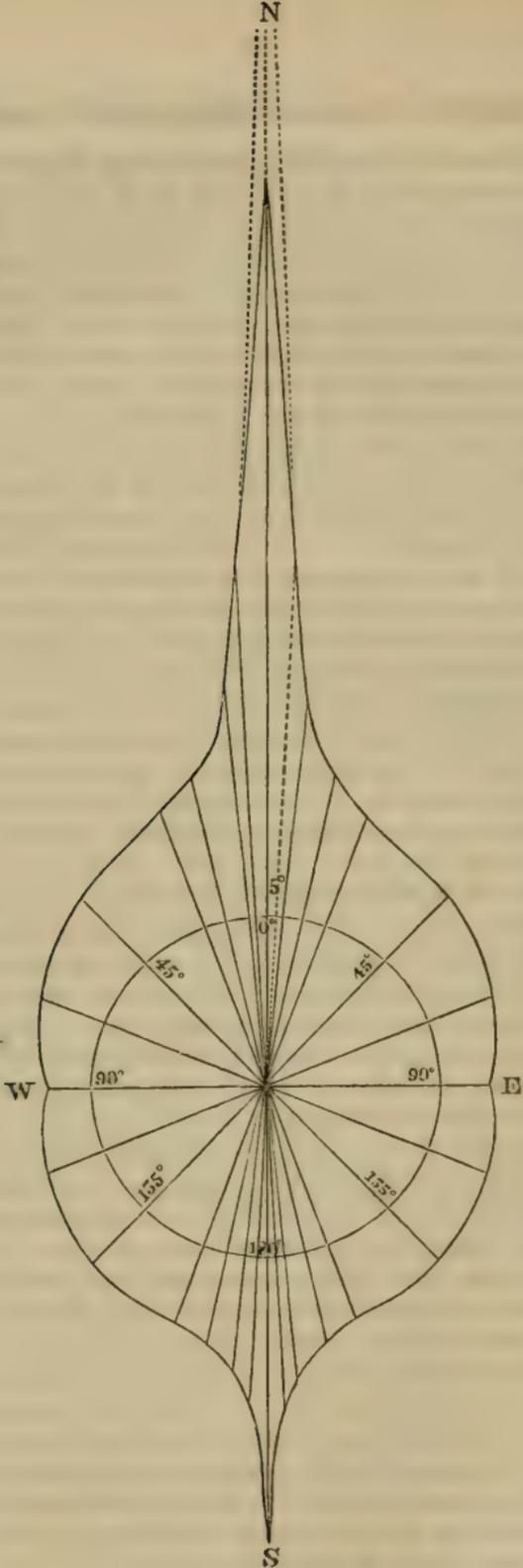
* Vol. xxix. p. 405.

tion at eight inches distance, owing to the minute quantities concerned, I could obtain a deviation at sixty inches, after which all effect ceased. As the distance was exactly four inches from the centre when the north end of the needle was drawn to 180° by the bar-magnet, and as there must be some definite ratio between the two distances thus obtained, I assume that the true length of the greater azimuth is sixty-four inches, and that the four inches not discovered in the measurement resulted from the friction of the needle on its pivot, though it was an extremely delicate instrument that had been made by M. Pixii. Instead therefore of the greater distance being merely the duplicate length of the shorter one, it is related to it as its cube.

The reason of this disproportion of the needle when in its natural position, from that which is discovered when it is forced round to the opposite pole, may, I think, be found, when it is remembered that, as the earth is either *per se* or by induction a magnet, the latitude in which the experiment has been made being at such a distance from the magnetic equator, there can be no longer an equality in the effects. It would be therefore a question, I think, not devoid of interest, to ascertain whether such a disproportion exists at the magnetic equator.

I will now describe the means by which a long line was obtained, upon the exactness of which the accuracy of the observation necessarily depended. A white thread of fine cotton, weighted at each end with lead, was laid over a long and steady table, and the weights being allowed to hang down, a perfectly right line was instantly obtained, which had only to be adjusted so as to agree with the magnetic meridian.

The length of the azimuths obtained in the lines N., E. and S. being respectively 64, 2, 4, it follows that by the law of the inverse square of the distance they are related to one another in the proportion of 1·1024 and 256, and that consequently every degree rises gradually from N. or 0° , till at E. or 90° its strength is 1024, after which there is a rapid decrease up to 180° ; and it may therefore be practically assumed, to save circumlocution, that at 175° its proportional strength is 256 $^\circ$, or only one-fourth the strength it has at 90° , that being the true proportion as it reaches S. or 180° , where the needle is necessarily in a state of equilibrium by its balanced attraction to the west; but all this will be much more evident by a reference to the curve itself, which is preserved in the form that was laid before the Royal Society, as it would be inconvenient to represent on paper the due proportion from the centre to N. In fact, for all practical purposes, the new correction is



The true proportions of the curve, from the centre to E. or W., is 1, to S. 2, and to N. 16; but as the distance from the centre to N. could not be represented conveniently on paper, the dotted lines on each side of the line N. must be supposed to be elongated to that extent.

of no consequence, as anything lower than the azimuth line that passes through 5° , I have found to be as low as can be ever required for any useful measurement; and it will be seen that by the dotted line I have drawn passing through it, and the dotted lines that are intended to represent the correction of the curve, that azimuth remains unaffected. Measured by a rule, the proportion of the azimuth line passing through 90° is as 1 to 2 with that passing through 5° . Now by the inverse ratio of the distance, it has only one-fourth of the strength at 5° that it has at 90° ; and practically, therefore, is the same in strength at 5° as it is at 175° . Hence 90° may be considered as a middle point, from which the degrees diminish by equal decrements to 5° and 175° ; and it will be seen by a reference to the curve, that the azimuth lines cutting 45° and 135° are essentially the same in length. The ascertainment of the true strength of the degrees from 90° to 180° was a necessary desideratum, which has led to the unexpected discovery that the position of the magnetic needle north and south is the effect of equilibrium arising from equal repulsions from east and west; and that so far from there being any attraction at N. and S., they are just the two points where the needle is in the greatest state of freedom; and that in the case of a needle of low magnetic power, the mere resistance caused by friction on its pivot, will be sufficient to prevent its taking up its true position in the line of the magnetic meridian. These facts prove that the movement of the needle from 90° to zero depends upon repulsion, commencing at 90° , and diminishing gradually till it stands in equilibrium at zero; and that the return of the needle to 90° , when the north end is brought to 180° , is the result of an attraction that increases up to 90° , when the repulsion commences.

The mathematical expression of these results is that—

The value of every degree of the compass is inversely as the square of the length of the ordinate or co-ordinate passing through it, the abscissa being considered as zero.

The general principle of the values of the various degrees of the compass obtained by the preceding system of azimuths, was further verified by the following method:—

A bar-magnet $12\frac{7}{8}$ inches long, was properly suspended by silk, free from torsion, and the N. pole being brought round to the south, was allowed to return back by the east. The following is the mean of eight observations, made with great care, and which only slightly differed from one another. The bar required $11\frac{1}{2}$ seconds to complete its return from 180° to 135° . When made to start again from 135° to 90° it occupied six seconds; and when allowed to depart again from 90°

to 45° , the time occupied was $5\frac{1}{2}$ seconds; while its return from 45° to 0° , after being again left to itself, took $6\frac{7}{8}$ seconds.

It might be supposed, *à priori*, that the north end of the bar-magnet when brought to the south and left to itself would be repelled with violence; but so far from this, as *seems* to be the case when small needles are observed, its movement is extremely slow, and there is a loss of a second or two before it acquires the gradually augmenting momentum that is to carry it back: a momentum indeed which increases so rapidly, that though the bar consumed $11\frac{1}{2}$ seconds to carry it back from 180° to 135° , it only required $15\frac{1}{8}$ seconds to carry it from 165° to 0° , and of this time $13\frac{1}{8}$ seconds were occupied in reaching 45° , after which the remaining 45° were completed in 2 seconds; yet the bar required $6\frac{7}{8}$ seconds to return from 45° to 0° , when it had not the benefit acquired by the preceding momentum. As a further proof how much the slowness of the movement of the bar increases near 0° , when not assisted by previous momentum, $6\frac{7}{8}$ seconds was the space of time requisite to carry the bar from 5° to 0° , yet when it had the advantage of the momentum acquired between 10° and 5° , it completed the whole in $\frac{1}{8}$ second less time, that is the space from 10° to 0° in $6\frac{1}{8}$ seconds, while it has just been seen that it required only 2 seconds to carry it from 45° to 0° , when it had the advantage of the previous momentum acquired by setting off from 165° .

There was a peculiarity in the return movement of the magnetic bar which is worthy of attention. When the north end moved from 180° to 135° , it required, as has just been seen, $11\frac{1}{2}$ seconds to complete the amplitude, while it only took $6\frac{7}{8}$ seconds to traverse over the parallel amplitude from 45° to 0° . The reason of this difference is, that in the first case its momentum is in an increasing ratio, and the force that is to move it has gradually to augment, while from 45° it sets off with a strong momentum which it retains till it reaches 0° . What the nature is of this force which goes on increasing on the return of the bar up to 90° , and from that point continues decreasing down to 0° , is a question of much importance in the theory of terrestrial magnetism. It is evidently one at right angles to the natural position of the needle, and seems to be caused by a principle identical with that of the conjunctive wire of the pile, and would strongly tend to confirm Ampère's theory of electric currents from east to west, if the supposition of the efficiency of these were not opposed by considerations to which I may return on a future occasion.

While upon this point it will be interesting to see the relative times occupied by the needle when drawn back to 175° ,

in its return to 0° , measured through regular divisions of the compass. On account of the difficulty of starting the needle from 180° when *in equilibrio*, the measurement only began from 175° .

To simplify the expression of the results, and save the use of decimals, the signs *plus* (+) and *minus* (−) are used to denote time in excess and paucity, which in the mean of eight observations were too insignificant to be otherwise brought in.

Amplitudes.	Mean.	Decrements on each side of 90° .	
15 to 0	$6\frac{11}{8}$	$2\frac{11}{8}$	} Repulsion.
30 to 15	4	$\frac{7}{8}$	
45 to 30	$3\frac{1}{8}$	$\frac{3}{8}+$	
60 to 45	$2\frac{3}{4}$	$\frac{3}{8}-$	
75 to 60	$2\frac{3}{8}$	$\frac{1}{8}$	
90 to 75	$2\frac{1}{4}$	0	
<hr style="border-top: 1px dotted black;"/>			
105 to 90	$2\frac{3}{4}$	0	} Attraction.
120 to 105	$2\frac{5}{8}$	$\frac{1}{8}+$	
135 to 120	$3\frac{1}{4}$	$\frac{3}{8}-$	
150 to 135	4	$\frac{3}{4}+$	
165 to 150	$6\frac{7}{16}$	$2\frac{7}{16}$	
175 to 165	9	$2\frac{9}{16}$	

The mode I have described of measuring the *insensible magnetic attractions* of non-ferruginous bodies, has the rare advantage of the most perfect delicacy and precision in all angles up to 90° , but from thence to the moment when *sensible attraction* commences, there is considerable vagueness, as far as relative intensities are concerned. Where perfect *repulsion* occurs it is distinctly indicated by the impossibility of forming a connexion; but it is likewise evident that in many cases a low degree of repulsion to the magnetic needle is overcome by giving time for magnetic propagation, or by keeping the substance operated upon in contact with a powerful magnet. Where a body is very light, as cork or charcoal, the failure must be attributed to their want of density, for the part of the needle which comes in contact with the bodies is but a minute point, yet the molecules which compose the cork or charcoal in that spot, must stand at such a distance from one another as to render their magnetism ineffective, and therefore inappreciable.

The *delicacy* of this mode of measurement is sufficiently attested by its having been applied with equal success to a *Phil. Mag.* S. 3, Vol. 30, No. 203, June 1847. 2 II

hair of the human head, and to a spark of diamond; and the *accuracy* attained may be judged of by the fact, that the same hair having been measured a year afterwards, was found, without any remembrance of the previous measurement, to be what it was before, namely 80° . A specimen from another head which only gave 76° was lost, and the measurement could not be verified, but another hair from the same head being tried it was found to be 82° , a difference which may be accounted for by supposing that all the hairs on the same head are not possessed of equal magnetic intensity at the same moment of time, or by a change in the state of the health of the individual. The measurements may be relied upon as never in excess, though from causes of disturbance, such as the vibration of the house, &c., they must occasionally be capable of being pushed further. No pains however have been spared to obtain the utmost degree of accuracy. Those who shall undertake to repeat these experiments will be best aware of the extreme difficulty of the subject.

Of the marks of indication.

Substances not followed by any numerals have been ascertained to be magnetic, but have not been measured.

A 0 implies that no attraction could be discovered.

The sign + marks that the measurement could be carried further.

The sign \times after 90° is intended to show that the magnetism is still stronger than that of the highest case of +.

The double sign of \times (that is $\times \times$) is employed to show that the connexion has been effected by the delicate employment of the finger instead of the magnet; but that the magnetic intensity of the substance is still not sufficient to attract the needle, whether ferruginous or non-ferruginous of itself. This is the "simpler mode" already alluded to.

A marks such substances as attract the needle, though at short distances.

The magnetic needle was made to connect itself with all the undermentioned substances not followed by 0, which are arranged according to their classes; and the measurements of such as I had time to ascertain are inserted after each.

1st Class.

Gold (pure)	90°	Lead	90°	Cobalt	$\overset{\circ}{A}$
Do. (18 carats).....	90	Zinc	90	Plumbago	0
Silver (pure).....	90	Copper	$90+$	Iodine	Λ
Do. (coin).....	$90+$	Mercury	$90\times$	Arsenic	15
Platinum	65	Antimony	66	Brass	Λ
Palladium	68	Bismuth	51	German silver	Λ
Cadmium	90	Chromium.....	90	Bell-metal.....	90
Tin	90	Manganese	Λ		

I have already spoken of the manner of raising the magnetic intensity of various substances, by keeping them in close contact with a bar-magnet for a short time. The following metals had their magnetic intensities slightly raised by this means:—

Platinum, from 65° to 90° ;
Antimony, from 66° to 90° ;
Bismuth, from 45° to 80° .

It is very remarkable that it is not of the least consequence whether the substance is kept in juxtaposition with the N. or S. end of the magnet, for the result will be just the same in either case, and the N. or S. end of the needle may be made to attach itself with equal indifference to the part of the substance that has been so excited. Whether this proceeds from a want of polarity in ferruginous magnetism, or from the molecules of the substances having been rendered more obedient to the coercive energy of the needle, it is difficult to say. As the magnetism still remains, after such influence, of the insensible kind, it is clear that they cannot penetrate to any depth, and this may be the reason why no polarity can be formally proved. The possible existence of polarity is countenanced by the fact that the magnetism is capable of being heightened, and by the analogy of the connexion that takes place between two similar poles of magnets when brought forcibly into contact, as well as by what takes place with arsenic. The magnetism which is found to exist in all matter, and that to a degree which has never been anticipated, would be of tremendous operation in nature if it were not like or identical with cohesive affinity, and consequently acting only at distances that may be considered as *insensible*. In this respect it contrasts strongly with the power of gravitation, and with that even of the magnetism of steel.

The effects that take place by keeping substances in contact with a magnet cannot be attributed to induction, as they continue for a longer or shorter space of time after separation.

The measurements of the first class of substances speak so clearly for themselves, that there would be but little occasion for any remarks upon the greatest portion of them, if it were not for the contrast in which some of them stand to the important results obtained by Dr. Faraday, when he operated with a very powerful electro-magnet. From the effects he observed he divides all metals into two classes, namely, *magnetic* and *diamagnetic*, according as they showed attraction or repulsion; the first of these states being indicated by the cylinder placing itself in the line of the axis of the magnet, and

the other, on its taking up a position at right angles to the same axis. Now it is not a little remarkable that platinum and palladium, which he selects as magnetic, exhibit, it will be seen, a lower state of attraction than cadmium, copper, gold, mercury, and silver, and that these, in common with antimony, bismuth, lead, tin, and zinc, in which he found repulsion, all exhibit attraction, according to the present form of experiment, and some in a very strong degree.

The case of antimony and bismuth is deserving of attention, for by a method somewhat analogous to the present one, the late M. Lebaillif found that they exhibited repulsion; though in his experiments the bodies were not forced into contact, and therefore their occult properties were not elicited. In these two instances, as is the case with all bodies of low attractive power, the connexion was formed with some difficulty; but as it was repeated on many occasions at different intervals of time, and even within a day of these remarks, not the slightest doubt can exist on the point. But I shall have to return to this subject again.

Another fact resulting from the two modes of experimenting employed by Dr. Faraday and myself, is that the connexion could be formed on any side or end of the substance, and that form had no influence upon the result. Indeed this was to be expected, as the transverse position which the substances assumed in his hands resulted from their shape, as every oblong body under repulsion must necessarily place itself at right angles to the resultant of forces, with a facility proportioned to its length and slenderness. Where the shape of the substance examined would permit of it, the end has of late always been selected in preference to the side, with a view to ascertaining this point; and this was particularly the case with the antimony and bismuth, which were in bar, and had been cut across.

I have introduced iodine into the class of metals and metalloids, as its semi-metallic lustre seemed to justify it, and because I found that it attracts the magnet of itself; but it shows no polarity. All that is necessary is to hold a scale of this body against the point of the magnetic needle, when, on drawing it back, the needle will remain connected with it for 40° or 50° of the compass. A piece of arsenic too, which showed very low magnetic intensity (15°), acquired a similar power, but in a higher degree, after it had been for a short time in close juxtaposition with the end of a bar-magnet; but it was remarkable that this power only appeared on the flat side of the arsenic where it had pressed against the crucible in which it was melted, the other broken parts of the sides

being only raised by the same process to 85° . I attribute this peculiarity to the greater number of points that are brought in contact with the magnet where the surface is flat. The character of the magnetism thus acquired is entirely the same as that of steel, attracting and repelling in the same manner, but comparatively in a very low degree, and consequently quite superficial. If brought quite close to that end of the needle for which it exhibits repulsion, it will, like a magnet, in consequence of the proximity, attract it. It lost its magnetic power in the course of a few weeks, but it has since been renewed with the same facility as before.

It appeared to me that the needles got out of order, for a short space of time, by their contact with the iodine and arsenic; but upon this point I cannot at the present moment speak positively. Certain it is that they became quite paralysed on two or three occasions, but were restored by having recourse to *the touch*, and subsequently by only letting them lie quiet an hour or two. I suspected the same effect to have taken place once or twice, after keeping them in contact with antimony, but at other times I could assign no cause for the change.

All the specimens of brass I have examined are very magnetic, and may have that property considerably increased by putting them in contact with a magnet. One specimen, which is a bar, causes attraction at one end, but the repulsion which existed at the other has disappeared. The former state appears to continue unchanged, at least I have seen no difference in the course of a year. Bell-metal has very low magnetic powers; so much so indeed that I despaired of making the needle connect itself with it; nor was this object effected till they had both been kept in connexion for some time, and even then there was every symptom that this was attained only by overcoming a natural repulsion, as in the case of chromium, of which I have already spoken. No change seemed to be effected by keeping it in contact with a magnet. German silver is magnetic, and if brought within about the eighth of an inch of the needle will attract it. But the specimen I had, seemed by its colour to contain an unusual proportion of copper, or otherwise it might have proved more magnetic. No increase of magnetism resulted from connection with a larger magnet. Cobalt and manganese, as is known, attract. The needle clung at once to a globule of mercury, against which it had been urged by the magnet.

All the rarer substances, including pure gold, in the preceding class, I believe to be chemically pure, as they were obtained from Messrs. Boyveau and Pelletier, and M. Ber-

hemot of this city (Paris), who considered them as such; but the tin and lead were taken from the ordinary specimens of commerce, and if the supposition which accounted for the magnetism of non-ferruginous bodies by their containing minute portions of iron, had any foundation, it would have been apparent in these two substances; yet it will be seen that the magnetism of both shows nothing very remarkable, and that the tin exhibits the same degree of attraction as the specimen of cadmium which I believe to be chemically pure, and these two will be found, nearly in all circumstances, to exhibit as remarkable a conformity magnetically as they are known to do chemically. When I show, however, the mutual attractions of non-ferruginous bodies, it will be seen that the notion of Coulomb, and other experimenters posterior to him, cannot be accepted as accounting for the results they observed.

2nd Class.

Selenium 10° | Sulphur 28°

Selenium and sulphur both exhibit very low intensities with the magnetic needle, and even these are obtained with so much difficulty as to demonstrate that they are forced states. I have paid particular attention to these two singular substances, and have found that they resemble one another as much by their magnetic properties as by those that are chemical. I will here anticipate the branch of this inquiry which is to follow, by stating that they are remarkable in nearly agreeing in their affinities for the metals, but particularly for gold. They, however, show not the slightest attraction for one another, and therefore must be considered as mutually repulsive. I have invariably found that where there is a chemical resemblance there is likewise similarity in magnetic properties, and these two substances maintain their resemblance in the same way as tin and cadmium, which only exhibit a difference of nature with respect to arsenic; but everything regarding this last body seems anomalous, which, like sulphur, has no affinity for selenium. Contact with the magnet appears to elevate their power of attraction, at least this was the case, as will be seen hereafter, when they were tested with brass.

3rd Class.

Diamond	65	Oural emerald	32	Agate.	0
Do. (2nd specimen)	65	Garnet		Cornelian (red).....	35
Ruby	90+	Tourmaline	32	Do. (white)	37
Sapphire	90+	Amethyst	65	Blood-stone	37
Topaz	74	Turquoise	47	Quartz	90
Do. (pale).....	66	Opal	0	Flint	90
Jargoon.....	90+	Mexican opal.			
Emerald	90+	Moon-stone.			

The present class exhibits many various degrees of attraction for the magnetic needle, but the ruby, the emerald, the sapphire and the jargoon, it will be seen, stand high in rank for this property, while tourmaline and ousal emerald are but low. The diamond was the only one of these precious stones that was placed in contact with the magnet, and it was found to have its energy raised by the connexion. Though the diamond does not exhibit much attraction for the magnetic needle, it will be seen hereafter that this is compensated for by its affinity for copper, platinum and glass of every kind; and it will be generally found that what is of difficult connection with the magnetic needle is remarkably the reverse with glass, and they may respectively be placed at the head of a binary division of all substances; and what makes the fact the more remarkable, is that the attractive properties of glass are heightened in a very sensible degree by its remaining in contact with the magnet. Glass has a striking affinity for cornelian, red and white, and for blood-stone, while a connexion between the magnetic needle and these stones is formed with extreme difficulty.

It might have been expected that flint would have shown much resemblance to quartz in its attraction for the magnetic needle, but the connexion was always formed with great difficulty, and on many occasions, though it appears as high as 90°, it could not be raised above 20° in some particular specimens, and many were tried. Yet the needle attached itself with great facility at 40° and 50° to quartz, and finally reached 90°, but not by any forcing process: that measure however is still its ultimate limit. Flint has a decided repugnance for the magnetic needle, but still the magnet can overcome it, and a refractory specimen was raised by this means from 20° up to 40°.

4th Class.

Flint glass (prism).. 30	White cameo (artificial)..	Felspar 10
Do. (tumbler) 90+	Brown ground of do.	Selenite..... 15
Eye glass 43	Porcelain (dentist's) 74	Talc 60
Crown glass 90 ×	Blue enamel 36	Sulphate of iron.. 48
Blue glass..... 90	Gypsum-stone 26	Protosulphate of
Green fancy glass... 90	Alabaster 25	iron 90 × ×
Green bottle glass.. 70	Marble (yellow) ... 10	Carbonate of iron 16
Red glass.	Marble (gray) 55	Sulphate of copper.
	Slate 30	Alum..... 15
Stone-ware 75	Hone..... 24	Rock salt.
China.	Red chalk.	Saltpetre.
Porcelain (French).. 90	White chalk.	Sugar candy (white and
Do. (English).	Crystallized tin..... 0	brown).
Delf (English).	Iceland spar 90	Lump sugar.

The prism was raised by the magnet from 30° to 60° , but the green glass, to which the magnetic needle attached itself with great difficulty, seemed to acquire repulsive rather than attractive powers by the same means. This was a specimen of fancy glass, of an apple-green colour, and cut to represent the calyx of a flower. I am ignorant as to what metal it owed its colour. It will be seen hereafter that the prism, which was of English fabric, exhibited an extraordinary affinity for all the metals, as well as for flint glass. The common green glass employed for wine bottles, and crown glass, notwithstanding the iron they contain, exhibited no striking attraction for the magnetic needle, which can be easily accounted for by remembering that the iron is in intimate combination with other bodies, and having its affinity employed, simulates neutrality.

The particular kind of porcelain used by dentists has very little attraction for the magnetic needle, and in other respects exceeds even the glasses in its affinity for non-ferruginous metals and gelatinous bodies. It and stone-ware are much alike.

The crystals of various kinds in this class, with the exception of protosulphate of iron, exhibit very low magnetic intensities for the needle, and confirm what I have said about the common glasses. Carbonate of iron, it will be seen, has but little affinity for the magnet. Iceland spar is as difficult and capricious as flint in its attraction for the needle, but is remarkable in its affinity for non-ferruginous bodies. Talc has but little attraction for the magnet and zinc, but it is otherwise with iron and gold. It was not tried with other bodies. The specimen, which was remarkably thin, came from Delhi. The other substances were not measured, but the needle connected itself with them. Crystallized tin failed with the magnetic needle, but rose to 90° after contact with the bar-magnet.

5th Class.

Pearl.....	$90^{\circ}+$	Horn (cow's).....	85°	Human hair (female's,	$^{\circ}$
Nacre.....	$90^{\circ} \times$	Do. (chamois').....	90	young, 2nd specimen)	76
Ivory (new).....	$90^{\circ}+$	Leather.		Do. (do. 3rd specimen)	82
Do. (old).....	58	Black seal's skin ...	62	Do. (female's, old) ...	$90^{\circ}+$
Enamel (human		Quill.		Do. (gray, male's) ...	$90^{\circ}+$
tooth).		Pig's bristle.		Do. (do. female's) ...	$90^{\circ}+$
Bone (mackerel's)	90	Human nail.		Do. (beard, gray).....	90
Whalebone.....	90	Human hair (brown,		Hare hair (gray).....	90
Tortoise-shell ...	85	male's).....	90	Do. (black).....	90
Horn (buffalo's)..	90	Do. (female's, young)	80	Cat's whisker.....	$90^{\circ} \times$

Amongst the animal products, pearl and mother-of-pearl are conspicuous in this class for their attraction for the magnetic needle, and for gold, copper, brass, lead, platinum, zinc, tin, and in all probability for the other metals, though they were not tried. Glass attracts it with equal facility. Horn of all kinds is extremely magnetic. The hair of the head has been already alluded to, and it must excite surprise that a body so fine could contain such high magnetic properties; and the fact is of extreme use as proving the very circumscribed extent of the spot, or rather of the point, which is connected with the needle in these experiments, for the very finest hair of the head that can be found exhibits as strong an affinity as the thickest of the same kind. Amongst hair the white whisker of a cat, of the mixed Angora breed, must not be omitted, as it was distinguished above all for its attractive properties which it maintained for all the substances with which it was tested.

6th Class.

Jet.	°	Spermaceti.	°	Sealing-wax (black).....	25
Sepia.....	24	Composition candle.		India rubber.....	80
Indian ink.....	70	Lac (pure)	35	Gas coal	15
Amber	75	Do. (red)	17	Pit coal.....	0
Resin.....	90	Do. (yellow).		Charcoal (of commerce)	0
Bee's wax.		Sealing-wax (red).....	36	Do. (box).....	0

The substances of this class show no remarkable attraction for the magnetic needle, but this is not surprising when their nature is considered.

7th Class.

Deal wood.....	30	Cane* (outside).	°	Vegetable ivory (2nd speci-	°
Box wood.		Do. (inside).		men)	0
Rose wood.		Wheat straw.		Do. (3rd specimen)	0
Poplar	52	Plain card	80	Glue	29
Oak.		Polished card.		Dry paste on paper	0
Lignum vitæ	33	Cedar wood	0	Letter paper (thin French).	
Beech.		Sandal wood	0	Do. (thin English)	0
Walnut wood.....	0	Cork	0	Do. blue laid (hot pressed)	0
Mahogany.		Vegetable ivory	56	Do. thin Bath (hot pressed)	0

* *Calamus rotang.*

The woods, as might be expected, did not exhibit much attraction for the needle; but I shall have to speak of them again when I treat of the magnetism of non-ferruginous bodies, to which I now proceed.

LXIX. *On Quaternions; or on a New System of Imaginaries in Algebra.* By Professor Sir WILLIAM ROWAN HAMILTON, L.L.D., V.P.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, and of other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.

[Continued from vol. xxix. p. 328.]

29. **I**F we denote by α and β two constant vectors, and by ρ a variable vector, all drawn from one common origin; if also we denote by u and v two variable scalars, depending on the foregoing vectors α, β, ρ by the relations

$$\left. \begin{aligned} u &= 2S.\alpha\rho = \alpha\rho + \rho\alpha; \\ v^2 &= -4(V.\beta\rho)^2 = -(\beta\rho - \rho\beta)^2; \end{aligned} \right\} \dots \dots (1.)$$

we may then represent the central surfaces of the second degree by equations of great simplicity, as follows:—

An ellipsoid, with three unequal axes, may be represented by the equation

$$u^2 + v^2 = 1. \dots \dots (2.)$$

One of its circumscribing cylinders of revolution has for equation

$$v^2 = 1; \dots \dots (3.)$$

the plane of the ellipse of contact is represented by

$$u = 0; \dots \dots (4.)$$

and the system of the two tangent planes of the ellipsoid, parallel to the plane of this ellipse, by

$$u^2 = 1. \dots \dots (5.)$$

A hyperboloid of one sheet, touching the same cylinder in the same sheet, is denoted by the equation

$$u^2 - v^2 = -1; \dots \dots (6.)$$

its asymptotic cone by

$$u^2 - v^2 = 0; \dots \dots (7.)$$

and a hyperboloid of two sheets, with the same asymptotic cone (7.), and with the two tangent planes (5.), is represented by this other equation,

$$u^2 - v^2 = 1. \dots \dots (8.)$$

By changing ρ to $\rho - \gamma$, where γ is a third arbitrary but constant vector, we introduce an arbitrary origin of vectors, or an arbitrary position of the centre of the surface, as referred to such an origin. And the general problem of determining that individual surface of the second degree (supposed to have a centre, until the calculation shall show in any particular question that it has none), which shall pass through *nine given*

points, may thus be regarded as equivalent to the problem of finding three constant vectors, α, β, γ , which shall, for nine given values of the variable vector ρ , satisfy one equation of the form

$$\{\alpha(\rho-\gamma) + (\rho-\gamma)\alpha\}^2 \pm \{\beta(\rho-\gamma) - (\rho-\gamma)\beta\}^2 = \pm 1; \quad (9.)$$

with suitable selections of the two ambiguous signs, depending on, and in their turn determining, the particular species of the surface

30. The equation of the ellipsoid with three unequal axes, referred to its centre as the origin of vectors, may thus be presented under the following form (which was exhibited to the Royal Irish Academy in December 1845):

$$(\alpha\rho + \rho\alpha^2) - (\beta\rho - \rho\beta)^2 = 1; \quad \dots \dots (1.)$$

and which decomposes itself into two factors, as follows:

$$(\alpha\rho + \rho\alpha + \beta\rho - \rho\beta)(\alpha\rho + \rho\alpha - \beta\rho + \rho\beta) = 1. \quad \dots (2.)$$

These two factors are not only separately linear with respect to the variable vector ρ , but are also (by art. 20, Phil. Mag. for July 1846) conjugate quaternions; they have therefore a common tensor, which must be equal to unity, so that we may write the equation of the ellipsoid under this other form,

$$T(\alpha\rho + \rho\alpha + \beta\rho - \rho\beta) = 1; \quad \dots \dots (3.)$$

if we use, as in the 19th article, Phil. Mag., July 1846, the characteristic T to denote the operation of taking the tensor of a quaternion. Let σ be an auxiliary vector, connected with the vector ρ of the ellipsoid by the equation

$$\sigma = \rho(\alpha - \beta)\rho^{-1}; \quad \dots \dots (4.)$$

we shall then have, by (3.), and by the general law for the tensor of a product,

$$T(\alpha + \beta + \sigma) \cdot T\rho = 1; \quad \dots \dots (5.)$$

but also

$$(\alpha - \beta + \sigma)\rho = (\alpha - \beta)\rho + \rho(\alpha - \beta), \quad \dots (6.)$$

where the second member is scalar; therefore, using the characteristic U to denote the operation of taking the versor of a quaternion, as in the same art. 19, we have the equation

$$U(\alpha - \beta + \sigma) \cdot U\rho = \mp 1; \quad \dots \dots (7.)$$

and the dependence of the variable vector ρ of the ellipsoid on the auxiliary vector σ is expressed by the formula

$$\rho = \pm \frac{U(\alpha - \beta + \sigma)}{T(\alpha + \beta + \sigma)}. \quad \dots \dots (8.)$$

Besides, the length of this auxiliary vector σ is constant, and equal to that of $\alpha - \beta$, because the equation (4.) gives

$$T\sigma = T(\alpha - \beta); \quad \dots \dots (9.)$$

we may therefore regard $\alpha - \beta$ as the vector of the centre C of a certain auxiliary sphere, of which the surface passes through the centre A of the ellipsoid; and may regard the vector $\alpha - \beta + \sigma$ as a variable and auxiliary *guide-chord* AD of the same *guide-sphere*, which chord determines the (exactly similar or exactly opposite) direction of the variable radius vector AE (or ρ) of the ellipsoid. At the same time, the constant vector -2β , drawn from the same constant origin as before, namely the centre A of the ellipsoid, will determine the position of a certain fixed point B, having this remarkable property, that its *distance* from the extremity D of the variable *guide-chord* drawn from A, will represent the *reciprocal of the length of the radius vector* ρ , or the *proximity* $(AE)^{-1}$ of the point E on the surface of the ellipsoid to the centre (the use of this word "proximity" being borrowed from Sir John Herschel). Supposing then, for simplicity, that the fixed point B is external to the fixed sphere, which does not essentially diminish the generality of the question; and taking, for the unit of length, the length of a tangent to that sphere from that point; we may regard AE and BD' as two equally long lines, or may write the equation

$$\overline{AE} = \overline{BD'}, \quad (10.)$$

if D' be the other point of intersection of the straight line BD with the sphere.

31. Hence follows this very simple *construction** for an *ellipsoid* (with three unequal axes), by means of a sphere and an external point, to which the author was led by the foregoing process, but which may also be deduced from principles more generally known. From a fixed point A on the surface of a sphere, draw a variable chord AD; let D' be the second point of intersection of the spheric surface with the secant BD, drawn to the variable extremity D of this chord AD from a fixed external point B; take the radius vector AE equal in length to BD', and in direction either coincident with, or opposite to, the chord AD; *the locus of the point E, thus constructed, will be an ellipsoid*, which will pass through the point B.

* This construction has already been printed in the Proceedings of the Royal Irish Academy for 1846; but it is conceived that its being reprinted here may be acceptable to some of the readers of the London, Edinburgh, and Dublin Philosophical Magazine; in which periodical (namely in the Number for July 1844) the first *printed* publication of the fundamental equations of the theory of quaternions ($i^2=j^2=k^2=-1$, $ij=k$, $jk=i$, $ki=j$, $ji=-k$, $kj=-i$, $ik=-j$) took place, although those equations had been communicated to the Royal Irish Academy in November 1843, and had been exhibited at a meeting of the Council during the preceding month.

32. We may also say that *if of a quadrilateral (ABED'), of which one side (AB) is given in length and in position, the two diagonals (AE, BD') be equal to each other in length, and intersect (in D) on the surface of a given sphere (with centre C), of which a chord (AD') is a side of the quadrilateral adjacent to the given side (AB), then the other side (BE), adjacent to the same given side, is a chord of a given ellipsoid.* The form, position, and magnitude of an *ellipsoid* (with three unequal axes), may thus be made to depend on the form, position, and magnitude of a *generating triangle ABC*. Two sides of this triangle, namely BC and CA, are perpendicular to the *two planes of circular section*; and the third side AB is perpendicular to *one of the two planes of circular projection* of the ellipsoid, because it is the axis of revolution of one of the two circumscribed circular cylinders. This *triple reference to circles* is perhaps the cause of the extreme facility with which it will be found that many fundamental properties of the ellipsoid may be deduced from this mode of generation. As an example of such deduction, it may be mentioned that the known proportionality of the difference of the squares of the reciprocals of the semiaxes of a diametral section to the product of the sines of the inclinations of its plane to the two planes of circular section, presents itself under the form of a proportionality of the same difference of squares to the rectangle under the projections of the two sides BC and CA of the generating triangle on the plane of the elliptic section.

[To be continued.]

LXX. *Observations on the Analysis of the Spectrum by Absorption.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S.; and V.P.R.S. Ed.*

IN a paper On the Production of Light and Heat by Dr. Draper, published in the last Number of this Journal, the ingenious author has made some observations on my Analysis of the Solar Spectrum, which it is necessary that I should notice. While he admits that I have “shown that *red, yellow, blue,* and consequently *white light* exist in every part of the spectrum,” he ascribes this result to the non-coincidence, or the overlapping of the colours of the several spectra formed on a screen by the rays falling near the edge, and those falling near the back of a prism with a refracting face of considerable magnitude. “In such a spectrum,” he says, “there must undoubtedly be a general commixture of the rays; but may we not fairly inquire whether, if an elementary prism were

* Communicated by the Author.

used, the same facts would hold good; or, if the anterior face of the prism were covered by a screen, so as to expose a narrow fissure parallel to the axis of the instrument, would there be found in the spectrum it gave every colour in every part, as in Sir David Brewster's original experiment?"

As my experiments were not made upon spectra formed upon screens by prisms with large refracting surfaces, they are not liable to this criticism, even if it were otherwise well-founded. The spectra which I use are so pure, and free from all commixture, that Fraunhofer's *black* lines are distinctly visible; and the results are precisely the same when the refracting face of the prism is reduced to the smallest possible dimensions.

My analysis of the spectrum by absorption, therefore, does not *indirectly controvert* the principle, that "to a particular colour there ever belongs a particular wave-length, and to a particular wave-length there ever belongs a particular colour," as Dr. Draper states, in theoretical language, the well-known proposition of Sir Isaac Newton, but it *directly controverts* it, and *absolutely overturns* it.

St. Leonard's College, St. Andrews,
May 13, 1847.

LXXI. *Intelligence and Miscellaneous Articles.*

ON THE PREPARATION OF THE AMMONIACO-MAGNESIAN PHOSPHATE FROM URINE. BY M. BOUSSINGAULT.

THE author remarks that some time since Mr. Stenhouse proposed a process for rendering the phosphoric acid and alkaline phosphates contained in urine useful in agriculture. It is indeed sufficient to add milk of lime to it, to occasion a precipitate of phosphate of lime.

It occurred to M. Boussingault that it was possible, by means of a magnesian salt, to collect both the phosphoric acid and a part of the ammonia developed during the putrefaction of the urine; and the result was that of obtaining a manure which contains two substances useful to vegetation—phosphoric acid and ammonia.

In the month of June M. Boussingault mixed a solution of hydrochlorate of magnesia with about 123 pounds of urine, fresh collected in the morning. At the end of five days the urine assumed a milky appearance, and from this time the deposit of ammoniaco-magnesian phosphate increased rapidly. A month afterwards the solution was poured off, and the phosphate was collected on a cloth to wash it. The salt was very white, and in small well-terminated crystals; when dried in the air, it weighed about one pound and nearly 100 grains avoirdupois. The presence of the magnesian salt greatly diminished the infectious odour emitted by the putrefying urine. It will be ob-

served besides, that no disengagement of carbonate of ammonia would occur, for the alkali developed was immediately converted into a fixed salt.

M. Boussingault observes that the above-described process appears to be susceptible of being advantageously employed in situations in which a large quantity of urine may be collected, as in the workshops, hospitals, prisons, &c. of towns, because an efficacious manure would be obtained which is particularly easy of transport. The diminution of the bad smell ought also to be considered; for it is evident that a magnesian salt acts under these circumstances as a disinfectant. In establishments situate in agricultural districts, the process would possess but little utility, because the urine could be there employed immediately and without any preparation; and the only advantage which could result from the use of a magnesian salt under these circumstances, would be the prevention of the loss of ammonia, a loss which the author thinks has been much exaggerated. —*Ann. de Ch. et de Phys.*, Mai 1847.

CHEMICAL RESEARCHES ON THE YOLK OF EGGS.

M. Gobley states that the fresh researches which he has made on this subject were undertaken with a two-fold object, one of which was to consider certain points which he had scarcely touched upon in his first communication, another object was to determine how far the objections of M. Sacc against his conclusions were well-founded; with these views the author modified several of his analytical processes, so as to be quite certain that he would avoid the causes of the errors which had been supposed to be discovered.

M. Gobley concludes from his recent experiments,—

1st. That the fatty matter of the yolk of egg is formed, as had previously been stated, of two distinct parts; one a fixed oil, the *oil of egg*, and of a soft, not fusible substance, *viscous matter*.

2nd. That the phosphorus does not occur in the oil, but in the viscous matter.

3rd. That oleic, margaric, phosphoglyceric, lactic acids, and the extract of meat are not products of oxidizement.

4th. That the viscous matter is not, as the author imagined, a combination of oleic, margaric and phosphoglyceric acids with ammonia, but constitutes a body of a complex nature from which two different substances have been separated, to which the author has provisionally given the names of *phosphorized matter* and *cerebric matter*.

5th. That the phosphorized matter which forms in the future animal the substance described by M. Fremy as *oleophosphoric acid*, yields oleic, margaric and phosphoglyceric acids with the greatest facility, as products of decomposition in the presence of acids and mineral alkalies under the influence of water or alcohol, and without the intervention of the oxygen of the air.

6th. That the cerebric matter is analogous, if not identical with the substance which Vauquelin, MM. Courcbe and Fremy have succes-

sively described under the names of *fatty matter of the brain*, *cérébrote* and *cerebric acid*.—*Comptes Rendus*, Avril 12, 1847.

ON PLASTER OF PARIS. BY M. PLESSY.

All chemists have agreed with M. Gay-Lussac, that sulphate of lime which had lost the whole of its water of crystallization by the action of heat, amounting to 21 per cent. or two equivalents, might be re-hydrated, and the setting of plaster was explained by it. M. Millon has, however, communicated to the Academy the following remarks: "Sulphate of lime heated to 230° F. loses 1½ equivalent of water; at 284° to 293° F., mere traces of water are separated; lastly, when heated to 572°, the plaster is rendered anhydrous and does not then set."

These results, opposed to those obtained by M. Gay-Lussac, induced M. Plessy to occupy himself with a question which chemists had supposed to have been long settled, the consideration of which M. Millon has resumed.

The facts stated by M. Plessy are,—1st, that gypsum and crystallized sulphate of lime, artificially prepared, lose 21 per cent. of water at 230° to 239° F. in a current of dry hydrogen, a gas which is well known to exert no action on the salt under the circumstances in which M. Plessy operated; 2nd, plaster heated in the open air from 230° to 239° F. readily lost 15 per cent. of water, and there then appeared a stop to occur; but at 266° to 284° F., the limit at which plaster is dehydrated, according to Mr. Graham, M. Plessy separated, not traces of water, but 2 per cent., and this was effected in a very short space of time; lastly, at 392° to 482° F., the plaster operated on suffered a total loss of 21 per cent. The author satisfied himself that it was anhydrous, by heating it strongly with a spirit-lamp, and yet it recombined with the whole of its water of crystallization so perfectly, that after having set, it lost 21 per cent., or two equivalents of water.

It appears from the above statement that plaster does not retain very precisely half an equivalent of water; for at 270° to 284° F., the loss is 17 per cent., and the half equivalent of water is greatly endangered; the compound $S^{\circ}O^6 2CaO, HO$ of M. Millon is destroyed, and yet it is formed at 230° to 239° F.; at this temperature, moreover, it does not exist in a current of dry gas; this combination has, notwithstanding, been obtained. M. Johnston found in the boiler of a steam-engine, prismatic crystals of sulphate of lime which contained half an equivalent of water.

It results from the experiments now detailed, that sulphate of lime which has lost the whole of its water of crystallization, may regain it when they are brought into contact.

In concluding, M. Plessy remarks that his results are opposed to those of M. Millon, but that they have been previously announced by M. Gay-Lussac and several other chemists.—*Comptes Rendus*, Avril 12, 1847.

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SUPPLEMENT TO VOL. XXX. THIRD SERIES.

LXXII. *On the Phenomena exhibited by Polarized Light when its plane of polarization is rapidly rotated.* By Professor DOVE*.

AS unpolarized light exhibits the same physical properties in all planes throughout the ray, whilst in rectilinearly polarized light these are different in the different planes, and attain their maxima and minima in two planes situated at right angles to each other, and gradually pass into one another by intermediate stages in those which lie between them, the question arises, what will be the properties of polarized light the plane of polarization of which is put into rapid rotation. The experimental answer to this question gives rise to very numerous phænomena, which illustrate in a remarkable manner several positions in the theory of light, which have as yet been only theoretically investigated.

Two supports of the same height as the other supports were placed to the triangular prism of the polarizing apparatus, described by the author in the *Scientific Memoirs*, vol. i. p. 86, in the fixed rings of which, hollow rollers could be set in rapid rotation by a band passing round a wheel with two grooves. Both the rollers can rotate in the same direction, and, by crossing one of the bands, in the opposite direction. Within and on both sides of the hollow rollers, Nichol's prisms, circularly polarizing plates of mica or ground crystalline plates can be screwed. The condensing lens of the apparatus is so placed as to concentrate the light of an Argand or monochromatic lamp in the polarizing apparatus, whether at rest or in rotation. The results of the experiments were as follows:—

1. Light which is completely polarized rectilinearly when the Nichol's prism is at rest, possesses exactly its ordinary properties when it is set in uniform rapid rotation, if the analysing apparatus is not rotated. On examination by a doubly

* Translated by Dr. Griffith from the *Berichte der Berliner Akademie* for Feb. 1847.

refracting prism, when the rotation is slow, it constantly exhibits figures of the same brightness. The intense complementary colours of their overlapping margins, on introducing a lamina of selenite, mica, or a thin layer of calcareous spar, completely disappear. The complementary system of rings of calcareous spar and apophyllite, ground at right angles to the axis, the splendid isochromatic curves of nitre, ground at right angles to the line bisecting the angle formed by the two axes, completely neutralize each other, becoming white. The same applies to the colours of unannealed and compressed glass. The centre of the system of rings which in rock-crystal (on slowly rotating the polarizing apparatus) passes through the colours of the spectrum, appears perfectly colourless on rapid rotation.

We thus have in the rotating polarizing apparatus, a circle of colours for the complementary colours; and not only for those which occur in Newton's rings, but also for the complicated system of rings of those crystals the binary axes of which are either situated in one plane with several lines of bisection, or in different planes with the same line of bisection (selenite, borax).

We may easily convince ourselves of the true existence of the polarization on the one hand, and the coloured curves on the other, however rapid the rotation may be. We need only place a Leyden jar, which spontaneously discharges itself, in the situation of the lamp, to illuminate for an instant the Nichol's prism rotating in the dark, and to convert the rotation into apparent repose*. With monochromatic light, the dark lines of interference vanish in a uniform illumination. If the red system of rings be separated from the violet in a binaxial crystal by a glass coloured by cobalt, both disappear during the illumination in an indefinite coloured mixture, in which it is indifferent whether, as in nitre, the red axes form the lesser angle, or, as in carbonate of lead, the violet.

* If light which has been perfectly polarized by reflexion from a mirror be transmitted first through an unannealed glass and then through two glass appendages, of which the refracting plane of one is situated in the reflecting plane of the polarizing mirror, the refracting plane of the other being at right angles to it, the colours of the cooled glass entirely vanish and it appears white, provided each of the two analysing apparatus is so inclined to the incident ray as to polarize an unpolarized incident ray to the same extent. With the slightest alteration of either apparatus the complementary images immediately appear, when the figure previously appeared colourless. This at once affords a very simple means of comparing the power of two polarizing apparatus, based upon simple refraction, and of illustrating the views of Sir David Brewster on the dependence of the polarization upon a number of plates, their refractive power, and the incidence of the light.

2. If the polarizer and analyser be rotated with equal velocity in opposite directions, that system of rings is seen which is obtained when both are in repose, and inclined to each other at an azimuth of 45° . This is particularly distinct when the light is rendered monochromatic by using a flame of spirit, coloured yellow by common salt. The cause of this phænomenon, which appears so remarkable at first, is at once seen when we recollect that, when one apparatus only rotates, the analysing or the polarizing, during an entire revolution, the arms of the white cross fall twice as often on the same spot as those of the black, and the dark lines of interference alternate with the bright lines of coincidence exactly in the same spots. When however both apparatus rotate in opposite directions, the system of rings with the white cross appears at the points 0° , 90° , 180° , 270° , whilst the system of rings with the black cross corresponds to the points 45° , 135° , 225° and 315° . As the bright arms of the white coincide with the coloured spaces of the dark cross, the impressions of the two are added, and we obtain the appearance above stated. If the rapidity of rotation of both rollers is exactly the same, the figure appears motionless; but if the rapidity of rotation of the two is slightly different, it slowly revolves, because the points of coincidence gradually alter.

3. If the polarizing and analysing apparatus revolve rapidly with the same velocity in the same directions, the linear system of rings becomes converted into that which is obtained when circularly polarized light is circularly analysed. If, before rotation, the Nichol's prisms are so placed that in calcareous spar the black cross is visible, on rotation we obtain the Newtonian reflected rings with a black centre, but without the cross; but if, when at rest, the white cross is visible, we then see, on rotation, the Newtonian transmitted rings with a white centre. If the polarizing and analysing apparatus do not rotate with the same rapidity, we see the remarkable phænomenon, that the reflected Newtonian rings with the black centre, and the transmitted ones with the white centre, constantly alternate in definite periods in the same spot of the field.

4. If a circularly polarizing apparatus be rotated in an opposite direction to the linear analyser, the same phænomenon is seen as when a linear polarizer and a linear analyser revolve in opposite directions, but it is less distinct (2).

5. If the circularly polarizing apparatus be rotated, and the linear analyser be kept at rest, we obtain the system of rings appearing at the quadrants exactly on rotation as on repose.

6. If the circularly polarizing apparatus be kept at rest,

and the linear analyser be rotated, we obtain a faint system of rings without a cross, with a gray centre surrounded by a black circle. The direction of the rotation is here indifferent.

7. If the circularly polarizing apparatus be rotated in the same direction as the linear analyser, we obtain the appearance, but fainter, which is seen when a linear polarizer and a linear analyser are rotated in the same direction, the rotating velocity not being the same; hence the alternating system of rings (3).

8. But if a circularly polarizing and a circularly analysing apparatus be rotated, either in the same or in opposite directions, this has no influence on the appearance, which is perfectly identical with that which is seen when both apparatus are at rest.

9. If a lamina of mica, circularly polarizing at an azimuth of 45° , be introduced between the Nichol's prisms, and be kept at rest whilst the polarizing Nichol's prism rotates, the systems of rings neutralize each other on linear and circular analysis, forming white light, in uni- and binaxial crystals and in rock-crystal.

10. If the lamina of the polarizing apparatus, which produces circular polarization at an azimuth of 45° , be rotated between the Nichol's prisms which are at rest, we obtain as the resultant of all the phænomena of light polarized elliptically, circularly and rectilinearly in a single plane, on linear analysis, the phænomena of partly rectilinearly polarized light, on circular analysis, those of a mixture of ordinary and circular light, *i. e.* in the first case the system of rings with a dark cross, in the latter the system of rings which vanishes at the quadrants; they are however faint.

In all the foregoing experiments the Nichol's prisms were crossed before the rotation was commenced.

11. It is so difficult to set two laminæ of mica or two Nichol's prisms either at an exactly equal or exactly opposite rotating velocity by means of a band passing round a wheel, that in those experiments in which the laminæ of mica being kept at rest, the Nichol's prisms rotate in the same or in opposite directions, or in which the Nichol's prisms being at rest, the laminæ of mica rotate, the relative position of the laminæ or of the Nichol's prisms is constantly becoming somewhat changed, and hence the phænomena of elliptical become mixed with those of the circular light. Hence, for the production of these phænomena, the motion must be effected by wheel-work.

12. Light, originally polarized rectilinearly, when transmitted through a rotating Nichol's prism and then analysed, yields light partially polarized in the original plane.

13. A rotating velocity, which continues to decrease and increase through an entire revolution, in (1) and (9) produces partially polarized instead of ordinary light.

14. Unannealed glasses rotating between Nichol's prisms at rest, do not neutralize each other and form white light.

15. Light from a rotating Nichol's prism falling upon a metallic mirror, possesses the ordinary characters.

16. The twin-growth of crystals gives rise to very complicated phænomena of colour. This is very beautifully seen in plates of calcareous spar, ground at right angles to the axis of the inclosing individual. The author has formed some of them artificially, by introducing a lamina of selenite of a definite thickness between two accurately centred plates*. If a plate of a natural twin-crystal be rotated, after having been accurately centred as regards the axis of rotation, the colours become neutralized, forming white light; whilst a simple plate, rotated on its axis, preserves its system of rings unchanged.

N.B. [The phænomena of *interrupted crystals*, described in paragraph 16 of this paper, were observed by Sir David Brewster previous to 1819; and the method of forming them artificially by enclosing laminæ of selenite and mica between accurately centred plates of calcareous spar, and between equal plates of the *spath calcaire basée*, was practised by him previously to March 1827, the date of which is signed, in his Journal of Experiments, by the Vice-President of the Royal Society of Edinburgh. The experiments, too, are briefly described in his Treatise on Optics, Part II. chap. xxii. § 115, p. 197.—ED.]

LXXIII. *On the Depolarization of Light.* By Prof. DOVE.

IF rectilinearly polarized light be allowed to fall upon a rough surface, as a white wall, it is perfectly depolarized. This phænomenon is most distinctly seen, by allowing the spectra of a prism of rock-crystal, polarized at right angles to each other, to fall upon such a surface, and analysing the purple-red resulting from the violet end overlapping the red extremity, with a Nichol's prism. On rotating the latter, there is not the slightest alteration in colour. This depolarization, which eight years ago the author, in a paper communicated to the Berlin Society of Friends of Natural History, showed also to occur on the rough internal surface of bodies covered with a reflecting layer, progressively diminishes with the obliquity of the incidence, so that even the dullest surface ceases to depolarize at very acute incidences. With perpendicular incidence, it also extends to circular and elliptical light, which, when reflected by a rough surface, is depolarized, *i. e.* possesses the properties of common light. If the explanation of the depolarizing property of rough surfaces is found in the supposition that such a surface reflects in all directions, by the converse, the combination of light polarized in all possible

* Poggendorff's *Annalen*, vol. xxxv. p. 594.

planes yields common light by reflexion. A truncated hollow cone, the smaller circular section of which was about fourteen lines in diameter, and the larger seventeen lines, was ground into a glass plate, four lines in thickness and three inches in diameter. The lowermost surfaces of the glass were roughened at the margin of the circular section and blackened, and the larger circular section was covered with a glass plate, upon which a circular piece of tin-foil was pasted, so that its centre coincided with the axis of the truncated cone. The angle of the cone at the apex amounted to $70^{\circ} 50'$. When this apparatus is turned towards the sun, so that the rays fall at right angles to the base of the cone, they enter the annular space between the tin-foil and the dark coating of the sheath, and arrive at the reflecting surface of the hollow cone at an angle of $35^{\circ} 25'$, thus becoming polarized in all the planes of reflexion, and after reflexion, crossing at one point of the axis. In the apparatus made by Cœrtling, this point was exactly in the plane of the smaller aperture of the cone, and was visible when received upon the surface of a piece of white paper placed there. As the polarized light would be depolarized on account of the depolarizing action of this rough surface, it could only be determined that it was unpolarized. This was effected by polarizing the light incident upon the cone by means of an inserted glass plate, and introducing a plate of mica between the inserted glass plate and the cone. The point remained colourless.

LXXIV. *On Thialdine and Selenaldine, two new artificial organic bases.* By WÖHLER and LIEBIG. Communicated by Dr. A. W. HOFMANN*.

1. Thialdine.

THE peculiar deportment of hydrated cyanic acid with aldehyde, which gives rise to a new acid containing the elements of aldehydite of ammonia and hydrated cyanic acid, induced us to study the action of some other substances on aldehyde and aldehydite of ammonia.

The most interesting product which we obtained along with others, is a new organic alkali free from oxygen, and containing carbon and hydrogen in the same proportions as acetic acid, combined with sulphur and the elements of sulphide of ammonium. This substance, which we call thialdine †, is highly remarkable from its composition, and we have no doubt that the method which led to its discovery, when applied to other compounds analogous to aldehyde and aldehydite of ammonia, will yield a long series of new and inter-

* Communicated by the Chemical Society; having been read January 4, 1847.

† Contracted from *θειον* and aldehyde.

esting products. The study of these substances promises to become highly important for the theory of the formation of organic bases, in which the action of ammonia is frequently very prominent, and it may be expected that a close investigation of the production of organic substances containing sulphur will throw some light upon their generation in the organism of the plant.

The preparation of thialdine is very simple. One part of aldehydite of ammonia free from æther and alcohol is dissolved in from twelve to sixteen parts of water; one ounce of this liquid is mixed with from ten to fifteen drops of solution of ammonia, and treated with a slow stream of hydrosulphuric acid; after half an hour the solution begins to become turbid, and crystals are deposited of the appearance of camphor and of considerable size; after four or five hours the liquid again becomes clear and the operation is terminated. The crystals are collected on a funnel and washed with water until all the sulphide of ammonium is separated. Pressure between bibulous paper removes the adhering water. The dry crystals are dissolved in æther, the solution mixed with a third of its volume of alcohol and left to spontaneous evaporation. The liquid soon deposits very regular rhombic tables, which can be obtained of half an inch in dimensions if too rapid an evaporation of the solution has been avoided.

If no more of the solution is left than is required to cover the crystals formed, the mother-liquid is decanted off; the crystals are then dried by leaving them for some time between folds of bibulous paper. The mother-liquor yields some more thialdine on evaporation. The crystals thus obtained are however not quite colourless. The remaining liquid contains much sulphide of ammonium, part of which arises from the ammonia added.

It happens sometimes that on passing hydrosulphuric acid gas through the solution of aldehydite of ammonia, no crystalline body, but a heavy colourless oil of offensive odour is deposited. This oil is a mixture of two bodies, the greater part being thialdine, the fusing-point of which is depressed to the temperature of the atmosphere by the presence of a liquid substance*. In order to obtain pure thialdine from this mixture, the supernatant aqueous layer is separated, and the remaining liquid (consisting principally of the oil and but a small amount of the aqueous solution) is agitated with half its bulk of æther, which instantaneously dissolves the oil. The æthereal solution, which may be easily separated from

* Crystals of thialdine when placed in contact with sulphide of ammonium for some time are entirely converted into a heavy oil, insoluble in water, which has not been further examined.

the water, is agitated in a stoppered bottle with a little concentrated hydrochloric acid. The mixture generally solidifies as a mass of fine needles, which may be separated from the oil by washing with æther. In this manner hydrochlorate of thialdine is obtained, from which pure thialdine may easily be prepared by moistening the dry crystals with a concentrated solution of ammonia, and extracting the separated base by means of æther. The æthereal solution yields by spontaneous evaporation crystals of pure thialdine. Addition of alcohol to the æthereal solution facilitates the crystallization exceedingly; the crystals become larger, more regular, and perfectly transparent.

Pure thialdine is heavier than water; its specific gravity is 1.191 at 18° C.; it forms large, transparent, colourless, shining crystals of the form of common gypsum, possessing a highly refracting power and a peculiar aromatic, but after some time very disagreeable odour. The crystals fuse at 43° C.; the fused crystals solidify at 42°; they are volatile at the temperature of the atmosphere without residue. Thialdine distils with the vapour of water, but is decomposed when distilled alone, a heavy oil of offensive odour passes over, solidifying only partly and after some time, whilst a brown, syrupy residue containing sulphur is left in the retort. Thialdine in this respect resembles aldehydite of ammonia, which, though volatile itself, cannot be exposed to a higher temperature without decomposition. If a crystal of thialdine remains for several hours in an atmosphere containing acid vapours, such as is the case in the laboratory, a white envelope of fine silky needles is found at a certain distance from the planes of the crystals.

Thialdine is but little soluble in water; it is soluble in alcohol, and much more so in æther; when powdered it deliquesces at the common temperature in vapour of æther, or in an atmosphere containing vapour of æther.

An alcoholic solution of thialdine exhibits the following reactions:—

With acetate of lead, at first no precipitate, but after some time a yellow deposit appears, which soon becomes red and finally black.

Nitrate of silver causes a precipitate, which at first is white, then yellow, afterwards black.

With chloride of mercury a white precipitate is produced, turning rapidly yellow.

With bichloride of platinum a dingy, yellow precipitate is thrown down after some time.

Thialdine has no reaction on vegetable colours; it is soluble in all acids with which it combines, forming crystallizable salts; the hydrochlorate and the nitrate are remarkable for

the facility with which they crystallize and for the beauty of the crystals.

Thialdine and its salts are decomposed when gently heated with a solution of nitrate of silver; sulphide of silver is produced with disengagement of a combustible gas, easily inflammable and possessing the odour and all the properties of aldehyde; namely, it is entirely absorbed by concentrated sulphuric acid and solution of potassa, the former becoming brown immediately, the latter remaining clear but depositing when gently heated a yellowish-brown precipitate of resin of aldehyde, and assuming the peculiar and offensive odour which accompanies the decomposition of aldehyde under these circumstances.

When thialdine is ignited with lime, chinoline appears among the products of decomposition, which may be easily recognised by its characteristic odour and by its beautiful platinum salt.

With cyanide of mercury thialdine exhibits a peculiar comportment. On mixing the two solutions a white precipitate is produced, which on boiling is converted into amorphous, black sulphide of mercury. If this decomposition takes place in a retort its neck becomes coated with fine crystalline needles, extremely volatile, insoluble in water but easily soluble in alcohol and æther. This new body, in which part or all the sulphur appears to be replaced by cyanogen, is formed in but small quantities, which up to this moment has prevented us from studying it any further.

The analysis of thialdine was performed in the usual way; the combustion with oxide of copper is attended with difficulties, as the conversion of carbon into carbonic acid is obstructed by the large quantity of sulphur contained in the body forming a corresponding amount of sulphide of copper at the point of contact between the substance and oxide of copper.

On burning thialdine with oxide of copper and separating the sulphurous acid formed by means of peroxide of lead, a gas is obtained containing volumes of nitrogen and carbonic acid in the proportion of 1 : 12. Aldehydite of ammonia, from which thialdine is produced, contains 4 equivs. of carbon to 1 equiv. of nitrogen, from which it is evident that aldehydite of ammonia on being converted into thialdine by means of hydrosulphuric acid has lost two-thirds of its nitrogen.

We have mentioned that thialdine, on being gently heated with a solution of nitrate of silver, has its sulphur converted into sulphide of silver. This mode of decomposition was employed to determine the sulphur. The nitrogen of the thialdine remains in the residuary liquid in the form of ammonia, from

which it was precipitated by bichloride of platinum, and determined in the usual way*.

0.3633 grm. of thialdine gave 0.5845 grm. of carbonic acid and 0.267 grm. of water.

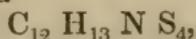
0.509 grm. of thialdine gave 0.816 grm. of carbonic acid and 0.3720 of water.

0.4508 grm. of thialdine gave 0.566 grm. of ammoniochloride of platinum.

0.6430 grm. of thialdine gave 1.923 grm. of sulphide of silver.

0.3140 grm. of thialdine gave 0.8420 grm. of water.

The atomic weight having been ascertained by the analysis of the hydrochlorate and nitrate, the preceding numbers lead to the following formula—



as will be seen on comparing the theoretical and experimental numbers.

		Theory.	Mean of experiments.
12 eqs. of Carbon . .	72	44.17	43.80
13 ... Hydrogen . .	13	8.04	8.04
1 ... Nitrogen . .	14	8.58	8.54†
4 ... Sulphur . .	64	39.26	39.14
	163		

Hydrochlorate of Thialdine.—Thialdine is dissolved in large quantity by hydrochloric acid: the saturated solution has an acid reaction. On spontaneous evaporation, or concentration on the water-bath, it deposits large, regular, transparent prisms of great lustre, and frequently an inch in length. The hydrochloric solution of crude thialdine contains in admixture a foreign body of most offensive odour; on agitating with æther this odour disappears immediately, and the liquid which formerly was turbid becomes perfectly transparent. Hydrochlorate of thialdine is rather soluble in water, less so in alcohol; both liquids dissolve a large quantity when hot, and deposit beautiful and perfectly regular crystals on cooling. Hydrochlorate of thialdine is insoluble in æther. When heated in the dry state this salt is decomposed without fusion; it becomes brown, and chloride of ammonium sublimes with evolution of a gas possessing a most offensive odour and burning with a dull flame. From the solution of this salt nitrate of silver throws down a yellow precipitate, which, on being gently heated, turns black with evolution of aldehyde. This black precipitate is a mixture of chloride and sulphide of silver; all the nitrogen of the base remains in the liquid in the form of ammonia.

* The following analyses were made by Dr. Strecker at Giessen, and Dr. Städler in Göttingen.

† Calculated from the proportion of 12 C : 1 N.

0.4577 grm. of hydrochlorate of thialdine gave 0.5933 grm. of carbonic acid and 0.2850 grm. of water, corresponding to 35.35 per cent. of carbon and 6.92 per cent. of hydrogen.

0.726 grm. of hydrochlorate of thialdine gave 0.789 grm. of ammonio-chloride of platinum.

0.7735 grm. of hydrochlorate of thialdine gave 0.8225 grm. of ammonio-chloride of platinum. In the mean, 6.79 per cent. of nitrogen.

0.7798 grm. of hydrochlorate of thialdine gave 1.890 grm. of sulphide of silver = 32.09 per cent. of sulphur.

0.7735 grm. of hydrochlorate of thialdine gave 1.915 grm. of sulphide of silver = 31.92 per cent. of sulphur.

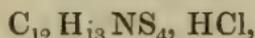
0.7598 grm. of hydrochlorate of thialdine was precipitated with nitrate of silver and boiled until evolution of gas had ceased; the precipitate, when extracted with ammonia and precipitated with nitric acid, gave 0.5283 grm. of chloride of silver = 17.14 per cent. of chlorine.

0.7735 grm. treated in the same way, gave 0.5405 grm. of chloride of silver = 17.24 per cent. of chlorine.

0.776 grm. dissolved in alcohol and precipitated in the same manner with nitrate of silver, gave 1.912 grm. of sulphide of silver = 31.903 per cent. of sulphur and 0.551 grm. of chloride of silver = 17.551 per cent. of chlorine.

0.441 grm. ignited with a mixture of lime and nitrate of potassa, gave 0.32 grm. of chloride of silver = 17.94 per cent. of chlorine.

These results lead to the following formula for hydrochlorate of thialdine,—



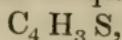
as may be seen from the following comparison between the theoretical and experimental numbers:—

		Theory.	Mean.
12 eqs. of Carbon . .	72	36.10	35.35
14 ... Hydrogen . .	14	7.02	6.79
1 ... Nitrogen . .	14	7.02	6.92
4 ... Sulphur . .	64	32.09	31.97
1 ... Chlorine . .	35.4	17.77	17.47
	199.4	100.00	

Nitrate of Thialdine.—This salt may be obtained by mixing this base directly with dilute nitric acid; it is, however, preferable to dissolve crude thialdine in æther, and agitate the liquid with moderately concentrated nitric acid; the solution solidifies into a crystalline mass, which is washed with æther, dissolved in water, and crystallized by evaporation and cooling.

Nitrate of thialdine forms fine white needles, more soluble in water than the hydrochlorate; they are soluble in cold, and more so in hot alcohol, from which liquid they likewise

tained. We will here merely allude to an explanation of the constitution and basic character of thialdine, at once obvious, namely, that it may be regarded as a copulated compound of sulphide of ammonium with 3 eqs. of a body



which is the sulphide corresponding to the oxide in aldehyde, namely, sulphide of acetyl.

2. Selenaldine.

The existence of thialdine and the mode in which this base is formed, induced us to try the preparation of a corresponding selenium compound. We perfectly succeeded in obtaining that body: selenaldine, however, is so soon and easily altered, that we have not as yet been able to subject it to a more profound investigation.

Selenaldine was produced by passing into a concentrated solution of aldehydite of ammonia hydroselenic acid gas, disengaged from selenide of iron by means of sulphuric acid. In order to prevent the decomposition of hydroselenic acid by the oxygen of the atmosphere, the whole apparatus had been filled previously with hydrogen. The excess of the poisonous hydroselenic acid was condensed in a potassa apparatus. After some time the solution of aldehydite of ammonia begins to become turbid, and crystals of selenaldine are deposited. When the mass of crystals is no longer augmented, the excess of hydroselenic acid is expelled by a stream of hydrogen gas; the solution of selenide of ammonium, covering the crystals, which soon turns red and deposits selenium, is now displaced by introducing a stream of cold water, free from atmospheric air, through one of the glass tubes connected with the bottle. The crystals are then collected upon a filter, pressed between folds of bibulous paper and dried over sulphuric acid.

Selenaldine, as it is deposited from the liquid in which it forms, presents itself in small colourless crystals, doubtless isomorphous with thialdine. In contact with the atmosphere it turns immediately yellow. This substance has a feeble but disagreeable odour; it is slightly soluble in water, on which account it should not be washed too long. The aqueous solution and the last washings become soon turbid in contact with the atmosphere, and deposit an orange-yellow body. The same deportment is exhibited by the solution of selenaldine in alcohol and æther, in both of which it is easily soluble. In consequence of its being so easily altered, we did not succeed in obtaining this base in crystals from any of the solutions mentioned. On evaporating them *in vacuo* over

sulphuric acid, the greater portion of the substance is volatilized with decomposition, the yellow body being likewise produced, while sulphate of ammonia is formed in the sulphuric acid. Also, when heated alone, selenaldine is decomposed, with disengagement of a gas of most offensive odour. Selenaldine is a base like thialdine; it is soluble in dilute hydrochloric acid, and reprecipitated by ammonia as a crystalline mass. The hydrochloric solution also immediately decomposes with evolution of a most offensive odour. Selenaldine is altered in a similar manner by boiling with water. The yellow body, the formation of which seems to be always attended with a liberation of aldehydite of ammonia, when collected is orange-yellow, amorphous, soluble in alcohol and æther, and fuses, when heated with water, to a reddish yellow mass, which remains soft for a long time. When heated alone, this body is charred with evolution of an oil containing selenium and possessing a most offensive odour.

We did not obtain a decisive result when trying to produce a *telluraldine*, the tellurium which we employed in the preparation of the hydrotelluric acid accidentally containing so large an amount of selenium, that merely selenaldine was deposited from the purple solution, containing, as it appeared, only telluride of ammonium. We intend to repeat this experiment with tellurium free from selenium.

LXXV. *Some Remarks on the Air and Water of Towns.*
By ROBERT ANGUS SMITH, Ph.D.*

HAVING given considerable attention to the inquiry into the causes affecting the health of towns, I was anxious to find what the real evil in their polluted atmosphere consisted of; the air has been frequently examined, but the differences found do not sufficiently account for the differences perceptible in breathing for the first time in entering a large town from the country, or for the very great difference in the colour and appearance of both when contrasted side by side, by an individual outside a town, having both before his eyes. The accumulation is great in this case, it is true, but therefore so much the more fitted for showing us its true character.

I first examined the rain-water taken from a cistern; a little was boiled down and the solid residue was like fatty matter, which burnt and gave the smell of a fat, leaving afterwards a strong odour of nitrogenized organic matter. As this amounted to nearly one per cent., I was disposed to con-

* Communicated by the Chemical Society; having been read January 4, 1847.

sider it an accidental impurity, and collected some rain in a porcelain vessel previously carefully cleaned. A smaller amount was obtained, but still from 500 grains of the water a sufficient quantity to make the smell distinct. I know that organic matter has been found in rain-water, but I am not aware that the smell of nitrogenized matter has been distinctly observed. It has been obtained also by boiling down large quantities: I have never found it to fail in evaporating as little as 1000 grs. The amount was generally about $\cdot 01$ of a grain in 1000 grs., but the average cannot be got without trying many seasons and many places.

1000 grs. collected on the 23rd of November, 1846, in a platinum vessel, after raining thirty hours with little intermission, gave with nitrate of silver a precipitate weighing $0\cdot 11$ grs. = $0\cdot 027$ of chlorine, and with a salt of barytes $0\cdot 1$, equal to $0\cdot 0343$ grs. of sulphuric acid, besides a perceptible smell of organic matter as before.

I do not adduce this analysis as of any great importance in showing the amount; the quantity operated upon is small, and I was desirous, for the sake of the salts, not to expose it to the contact of any vessel which could be acted on; but I mention it to show, that the quantity is not such as can be only procured by the questionable process of boiling down large quantities in vessels too large to admit of being perfectly cleaned.

The water is often alkaline—I say often, but I have always found it so; by boiling this is lost, proving, as I think, that there is more carbonate of ammonia set free from the burning of coals than is necessary to saturate the sulphuric acid in the atmosphere.

The air is often acid, as evidenced by test-papers exposed to it; and it certainly proves very acid when certain quantities of coal are burnt.

It would have been well to have examined water from the country falling at the same time, but being persuaded that water so impure would long ago have been examined if it even fell over large districts in such a state, I preferred looking to its origin.

As far as I am aware no examination of unventilated places has given us a good proof that carbonic acid is the greatest agent in making them so actively injurious. I breathed, and had others to breathe, through a bent tube into a large jar, and examined the moisture in the jar. As it always contained organic matter in large quantities, I inferred that the same must occur when the perspiration and breath condense upon the windows of crowded rooms.

This condensed moisture is not easily obtained in sufficient quantities; but on the occasion of a concert at the Mechanics' Institution here, I collected 200 grs. from one window, letting it drop into a small platinum basin. By burning 5 grs. of this the smell was made apparent; by burning 150 grs. a strong smell of human sweat came off, continuing until the substance was dry. It will therefore be difficult to tell how much organic matter was contained in the whole, as a great deal evaporated, but above 2 grs. were left. When heated, this smelt like burning flesh, and was very disagreeable.

We know that volatile products escape from the bodies of animals, and are mixed with the air so as to undergo complete oxidation; but this cannot be any important part which the air has to perform, as the following facts show that the oxidation is best effected in the earth. It may however be important that bodies should decompose in this manner, because they are also more readily conveyed by the atmosphere to plants.

When we see the volatile and decomposing matter from the body so distinctly as in the last case mentioned, it is possible for us to conceive that infectious matter is not a substance so very subtle as has been supposed; we have only to allow it to be in a peculiar state of decomposition.

Burning in a platinum crucible and the odour discover a very small quantity of organic matter, such as it is scarcely safe to trust to the results of the balance. I hope in a future paper to show the qualities of several water districts in this respect, but at present give an abstract only of the results of such observations as I have made, hoping that the constant and almost monthly variation of the water of whole districts will be a sufficient excuse for the absence of an arranged set of results.

When water from a peaty district is boiled down and the ashes burnt, the smell of peat is distinctly observed. The river Dee at Chester contains 25 grs. of organic matter to the gallon, and the smell is as perceptible as at Llangollen, close to a peaty district. 500 grs. of the water are sufficient to indicate this very strongly. Water from the Lancashire and Yorkshire hills may be known to contain peat also by testing the same quantity, although many of the streams do not contain 15 grs. per gallon. Some of the canals near Manchester may be known to be supplied from peaty districts by the same tests. But whenever we approach a town the smell from the burnt ash changes, and organic matter from the decomposition of protein compounds is distinctly traced. So easily is this known, that when water was brought to me

from the Dee, above Llangollen and below, it appeared evident that even this small town had affected the river. Before the water arrives at Chester the peat predominates too much to make it perceptible that matter from towns has been thrown into it; below it is distinct. Water from surface-drainage indicates by the same mode of treatment nitrogenous matter undecomposed.

Observing this increase of organic matter on approaching towns, I naturally expected to find a large quantity in the wells, of which there are many in Manchester, placed as they often are in the neighbourhood of cesspools, besides being exposed to the infiltration of the water of drains.

A well was first examined in a garden near the town, the proprietor having complained of it as being unwholesome. It contained 40 grs. of common salt per gallon, 14 grs. of organic matter and salts of ammonia, besides other ingredients to be expected in connection with organic matter. I next examined a well near a burying-ground; it was loaded with inorganic salts. Its taste was very bitter; when evaporated and burnt, the remaining inorganic matter gave out a pungent acid vapour. Four wells in the same neighbourhood were examined, all with the same results. I then went to nearly all the churchyards in Manchester, of which there is a considerable number, but finding that the neighbourhood of the collegiate church gave the same indications as all the rest, although there have been no burials there for many years, I was induced to ascribe it to the presence of large dung-heaps close to the well. Other wells distant from churchyards, but near cesspools, gave the same result. In the suburbs they were not so much tainted, but in some newly-built streets close to the town, in gravelly soil, were very bad.

I have no doubt that the churchyards and the cesspools act in a similar manner, as we might naturally expect. The chief impurity is a collection of inorganic salts. It is remarkable how rapidly the organic matter becomes oxidized and converted into nitric acid in the soil. In one case I found as much as 70 grs. of nitrates in a gallon of water, besides magnesia, lime and alkalis. This water seems to be a favourite with some persons; when not quite so heavily charged it has a fine sparkling appearance. The wells in the centre of the town have nearly disappeared, otherwise it is probable that organic matter would have been found there in a less oxidized state.

As these wells have undergone several changes during the autumn and early part of the winter, I shall not give the results of the analyses which I have made, until a few seasons shall enable me to make a more complete examination of the

subject. At present, when the ground is so wet, it is difficult to obtain a sufficient quantity of the salts, which are peculiar. At present I may say that sulphuric acid added to the water gives off chlorine; that heat evolves chlorine and peroxide of nitrogen from it; and that the remaining salt is a mixture of a chloride and a nitrate chiefly. This will be sufficient to show that neither the water nor the air of Manchester has so very minute an amount of impurity as to put it without the power of detection.

LXXVI. *On certain Atmospheric or Barometric Waves which traversed Europe during November 1842.* By WILLIAM RADCLIFF BIRT*.

AT the sittings of the Geological Society on the 3rd and 17th of May in the year 1816, a paper was read by Dr. Macculloch On the Employment of the Barometer in measuring heights. In this paper the author endeavoured to show that the *differences* indicated by different instruments, placed at various stations on the earth's surface, are *real* differences of atmospheric pressure, and that the changes which take place at several stations are far from being simultaneous: he also expressed an opinion, that the differences of pressure, combined with their progressive character, are in some way or another connected with the direction of the wind, especially the two cardinal currents N.E. and S.W. of our island; but the imperfect state of the barometer at that time was such as to prevent an efficient investigation of the subject.

In the year 1832, Professor Forbes, in his Report on the Recent Progress and Present State of Meteorology, presented to the British Association for the Advancement of Science during its sitting at Oxford, alluded to the *accidental* variations of barometric pressure as greatly influenced by latitude, the amount of variability increasing towards the poles. He appeared to regard these variations as resulting from *great atmospheric tidal waves*, which perpetually traverse our oceans and continents, and he viewed a future and more advanced state of meteorological science, as furnishing data from which the most interesting and important conclusions might be drawn in immediate connexion with such atmospheric waves †.

On the 21st of March in the year 1835, Sir John Herschel made a series of hourly meteorological observations at Feld-

* From Report of the British Association for the Advancement of Science for 1846.

† Report of the First and Second Meetings of the British Association for the Advancement of Science, p. 235.

hausen Wynberg, near the Cape of Good Hope. These were accompanied by a simultaneous set at the Royal Observatory near Cape Town. The observations thus set on foot were continued at numerous stations during the following three years; and the discussion of the whole, collected by Sir John, forms the subject of a report presented by that gentleman to the British Association during its sitting at Cork in the year 1843. One of the objects to which Sir John directed his attention in this discussion, was the barometric fluctuations considered with a view to the propagation of atmospheric waves. For this purpose the stations were distributed in groups, in one of which (the European) Sir John succeeded in detecting and tracing two well-defined waves; one on the 21st of September 1836, the other on the 21st of December 1837. These waves were so vast in their dimensions, that to take in (says Sir John) an effect of this nature, we must enlarge our conception of an atmospheric wave till it approaches in some degree, in the extent of its sweep and the majestic regularity of its progress, to those of the tide-waves of the ocean*.

The principle on which the discussion of these hourly observations was conducted, was that of projecting the barometric curves for all the stations of one group on a single sheet for each separate term, the scale being one inch to an hour in *time*, and one inch to 0ⁱⁿ.066 of barometric altitude. From the nature, however, of the observations, no one curve was continued longer than twenty-four hours, except on the occasion of a remarkable storm. During the progress of this work, it was deemed desirable to obtain the complete rise and fall of the barometer; so that a complete wave might be observed at least at one station. Observations undertaken with this view, and projected in curves, illustrate Sir John Herschel's report. As a test of any conclusions that may be drawn from a discussion of observations on this principle, the observations may be so combined as to exhibit, not only the barometric relations with regard to *time*, but also with regard to space: in other words, the distribution of pressure over a tract of country, at any given epoch, will indicate the real character of the waves then transiting.

The last report which I had the honour to present to the British Association during its sitting at Southampton, contains an examination of certain barometric observations exclusively on the last-named principle. These observations were made during November 1842, the same month in which the observations were made from which some of the curves illustrating

* Report of the Thirteenth Meeting of the British Association for the Advancement of Science, pp. 60-103.

Sir John Herschel's report were projected. The result of the examination has been the apprehension of several large atmospheric waves of so distinct a character, that the barometric movements over an area included by the angular points, the Orkneys, St. Petersburg, Geneva and Cork, have been fully explained, especially during the earlier portion of the month. In order clearly to place these barometric waves before the readers of the *Philosophical Magazine*, I shall at once proceed to make various extracts from the report bearing immediately upon the subject, commencing with the

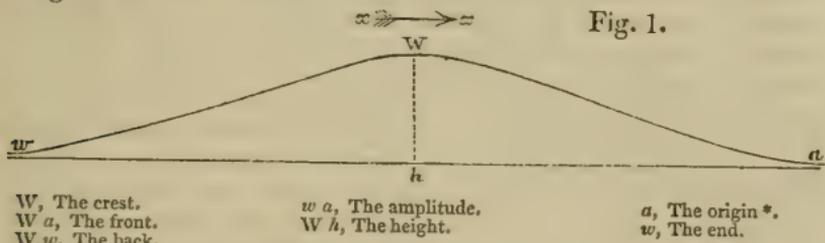
Definition and Phenomena of an Atmospheric Wave.

When a number of barometric observations are projected on paper according to a suitable scale, and continued for months and years, the eye on contemplating them will recognise a variety of curved forms, some of large and some of small amplitude; some rising to a considerable altitude, others sinking far below the level, representing the mean barometric pressure at the station of observation. At first there appears but little regularity in these curvilinear records of the ever-shifting state of our atmosphere, but here and there the attentive observer will notice some similarity existing between two or more *individual* curves, and he may notice some which possess a certain symmetrical arrangement of the ascents and descents. In consequence of this similarity and symmetrical arrangement, he examines more carefully the records of barometric pressure, and not only discusses the observations at *one* station, but compares those observations with others made at *various* stations; and here again he finds apparent irregularity and confusion. The curves to a certain extent agree, but in many minor points they differ often very considerably, in some cases rising at one station while falling at another; this induces a still more minute and careful investigation: the distribution of pressure over the largest area he can command is carefully examined; and whether his stations are few or many at any given time, he finds on this area a point of maximum pressure and a point of minimum pressure; between these points he finds various pressures, generally increasing from the point of least pressure to the point of greatest pressure. On some occasions he finds a *line of high* pressure, stretching quite across the area, and on others a *line of low* pressure. By continuing his inquiries for successive epochs, he finds these lines of high and low pressure move across the area, or in other words, the high pressure or low pressure is gradually transferred from one point to another. He also finds at still more remote epochs other lines of high and low pressure, some having the same direction with the lines originally noticed, and others crossing the direction of the original lines at various angles.

The questions which now suggest themselves are the following:—What are these movements? How can they be represented? In what manner can they be explained? A simple consideration of the curves suggests the idea of waves as explanatory of the phæno-

mena, and the term atmospheric wave has been used to designate that *ideal individuality* which the mind attributes to the process which it observes of the successive change of place which the barometric maxima and minima undergo, and by which they regularly succeed each other over the area under examination; this ideal individuality has been employed as a mean of examining the movements just alluded to.

Mr. Scott Russell, in his admirable report on waves, presented to the British Association during its sitting at York in 1844, has given the elements of an aqueous wave. These elements appear so clearly to express the relations of the barometric phenomena (simply considered as such), that I shall avail myself of Mr. Russell's terms to indicate the distribution of pressure over the surface of the earth, as in figure 1.



In reference to this figure, the line of high pressure stretching across the area (the figure being supposed to cut this line transversely) has been termed the *crest*, W ; the line of low pressure in advance of the crest, the *anterior trough*, a (the origin of Mr. Scott Russell's water wave); the line of low pressure succeeding the crest, the *posterior trough*, w (the end of Mr. Scott Russell's water wave); the line $w \dots h$, as measured by the mercurial column, the altitude of the wave; the slope $W a$, the anterior slope or front of the wave; the slope $W w$, the posterior slope or back of the wave; $w a$ constitutes the amplitude of the wave, and $x \text{ --- } x$ in the same direction, the axis of translation.

The observations to which allusion has been made, were published by Mr. William Brown in a paper that appeared in the Philosophical Magazine for April 1846. This paper is entitled "On the Oscillations of the Barometer, with particular reference to the Meteorological Phenomena of *November 1842*." The object of the author is to show that the barometric oscillations are produced by the *meeting* of opposite or nearly opposite aerial currents; that one current thus meeting or impinging on another, deflects it, and under some circumstances produces a rise of the mercurial column, but under others occasions a fall in many cases of considerable magnitude. In order to elucidate his views, Mr. Brown has collected barometric observations from eleven stations, which are scattered over an area included by the following angular points:—The Orkneys, Christiania in Norway, Paris, Plymouth and Cork. These observations are in most cases given as read

* Mr. Scott Russell designates the point a the *origin*; a better term I apprehend would be *commencement*.

off from the scale. In addition to these the paper is accompanied by six plates, in which the direction of the wind at numerous stations is indicated for every day during twenty-six days in the month by arrows. The anemonal observations published in the body of the paper not being in all cases for consecutive days, a comparison of them with the plates is rendered difficult; nevertheless the plates form a very valuable portion of the communication, and if they have been laid down from accurate observations, they furnish us with an important addition to our knowledge of the *arrangement* of the aerial currents, especially with respect to the distribution of pressure. It is a matter of regret that Mr. Brown did not so arrange his observations and plates, that the accuracy of the latter could have been seen by inspection.

This paper is peculiarly interesting at the present time, when the attention of meteorologists is directed to the important and interesting problem of the barometric oscillations. I have already noticed the views which have been taken of these oscillations as the results of waves. In this paper they are regarded as the effects of currents; and it appears that meteorologists are now divided as to which of these hypotheses they shall be referred. Professor Dove remarks, in his letter to Colonel Sabine on the Magnetical and Meteorological observations*, "the so-called irregular variations of the barometer are regarded by some persons as only the effects of currents of air of unequal temperature and moisture; other persons distinguish the effects of currents from the effect of undulations *progressing in the manner of waves of sound, and propagating themselves with great velocity over large portions of the earth's surface.*"

It is worthy of notice, that these movements have been referred to two very distinct kinds of waves. Prof. Forbes speaks of them as great atmospheric *tidal* waves, and Prof. Dove speaks of a class of individuals who regard them as progressing in the manner of *waves of sound*.

It is not my intention to enter into an examination of the conclusions and results which Mr. Brown has arrived at; as the question is open, I apprehend I have not committed an injustice towards that gentleman by having employed a rather different process to that which he has used, and further discussed the observations he has given. I beg to acknowledge the obligations I am under to him for these observations, and especially for the plates, of which I have before spoken: they are extremely interesting in the present inquiry.

In accordance with these remarks, I have selected the following stations from Mr. Brown's list:—the Orkneys, Belfast, Shields, Cork, Bristol, Plymouth, London, Paris, and Christiania. The reason I have omitted Glasgow and Armagh will be apparent from Mr. Brown's notes. As I have discussed these observations with especial reference to the wave hypothesis, I have most cautiously avoided in my remarks any thing that may at all bear on Mr. Brown's views. The plan I have proceeded on is as follows. I have selected the

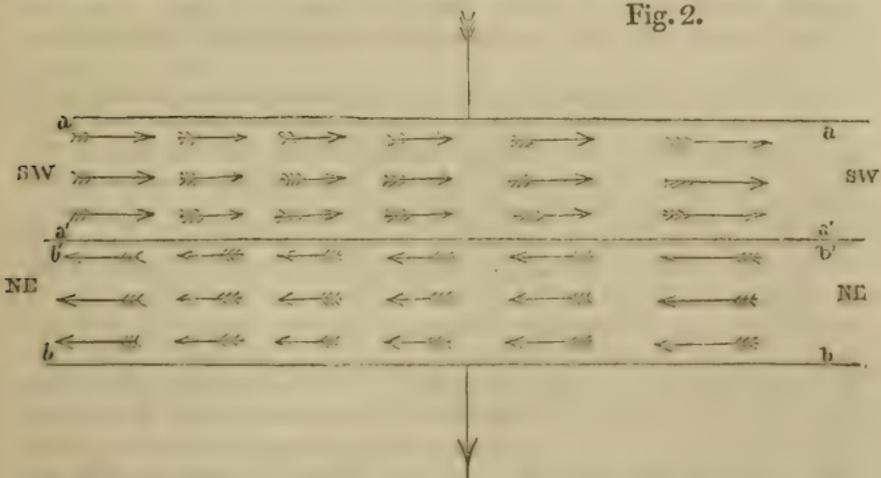
* Report of the Fifteenth Meeting of the British Association for the Advancement of Science, p. 30.

middle observation of each day; at those stations where only two are given morning and evening; I have taken a mean of them. These observations I have so arranged as to exhibit the distribution of pressure over the area for each day—the line or lines of the greatest diminution of pressure—and the relation of such distribution and of such lines to the aerial currents or winds. As a convenient method of readily expressing these various relations and giving to the discussion that completeness which otherwise it would want, I have adopted the wave hypothesis, and to every line of barometric maxima applied the term *crest* and to every line of minima the term *trough*. In a word, I have regarded the progress of the barometric and anemomal phænomena as the progress of *waves*. The observations remain the same both in Mr. Brown's and my own discussions, the results only are different; and it remains for other philosophers, by more closely investigating the subject, and submitting the observations to a more rigorous and searching discussion, to advance this interesting inquiry and to become more intimately acquainted with the causes of these interesting phænomena.

In addition to the discussion of these observations making us acquainted with several large waves similar in character to the definition before given, it also reveals to us the arrangement of the aerial currents with respect to such waves, or rather by means of these observations and the plates accompanying them, we are able to ascertain, not only the general direction of the aerial currents, but also the barometric phænomena resulting from their transit.

Prof. Dove, in his second letter to Col. Sabine relative to the magnetical and meteorological observations, has announced his opinion that the equipoise of the atmosphere is maintained in the temperate zone by currents *on the same level flowing in opposite directions**; thus we have a bed or stratum of air moving from the S.W., and on each side of this are strata of N.E. winds. We

Fig. 2.



* Report of the Fifteenth Meeting of the British Association for the Advancement of Science, p. 61.

may here inquire, how are these alternate aërial currents related to the waves before alluded to? It is one of the objects of the discussion to exhibit this relation, which may be thus briefly expressed, at least in so far as the examination of the observations has yet extended*. Let the strata $a a a' a'$, $b' b' b b$, fig. 2, represent two parallel aërial currents, $a a a' a'$ being from S.W. and $b' b' b b$ from N.E., and conceive them both to advance from the N.W. in the direction of the large arrow, that is the strata themselves will advance with a *lateral* motion. Now conceive the barometer to commence rising just as the edge $b b$ passes any line of country, and to continue rising until the edge $b' b'$ arrives at that line, when the maximum is attained. The wind now changes and the barometer immediately begins to fall, and continues to fall until the edge $a a$ coincides with the line of country on which $b b$ first impinged. During this process we have all the phenomena exhibited by an atmospheric wave; when the edge $b b$, fig. 2, passes the line of country, the point a , fig. 1, of the wave (the anterior trough) transits that line of country and the barometer begins to rise with a N.E. wind. During the period the stratum $b' b' b b$, fig. 2, transits the line the anterior slope $W a$, fig. 1, passes; when the conterminous edges of the strata $a' a'$ $b' b'$, fig. 2, pass, the crest W , fig. 1, extends in the direction of the preceding trough: the barometer now begins to fall, and when the edge $a a$, fig. 2, occupies the place of $b b$, also fig. 2, the descent of the mercurial column is completed; the posterior slope $W w$, fig. 1, has passed, and the posterior trough w , fig. 1, now occupies the line in which the anterior trough extended.

This arrangement of the aërial currents in connexion with a barometric wave may be termed the *air-motion* of the wave. In contemplating the transference of the barometric maxima and minima, we regard only the *wave-motion*. In contemplating the direction and force of the wind, we regard also the *air-motion*. The *wave-motion* consists in the *lateral* transference of the parallel beds of aërial currents; the *air-motion* being these oppositely directed parallel currents themselves.

From these considerations, we readily see that the *wave* is a convenient method of representing the barometric fluctuations. We have seen that the rise is due to the anterior slope and the fall to the posterior; and we now further learn that the direction of the aërial current on the anterior slope is at right angles to the axis of translation directed towards the *left-hand*, while on the posterior slope it is the reverse; still at right angles to the axis of translation, but directed towards the *right-hand*.

Having thus noticed the *wave-motion* with its accompanying *air-motion*, these interesting questions suggest themselves:—How are the forces of this *air-motion* arranged? Do all the particles move with the same velocity? Are there different velocities in different parts of the wave? Our anemometers will answer these questions. In the troughs, the edges $b b a a$, the forces are strongest; as the barometer rises, the force gradually subsides; when the crest passes,

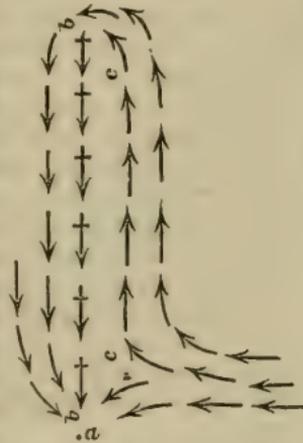
* For this knowledge I am indebted to Mr. Brown's plates.

it is zero; and as the barometer falls, it increases until the trough passes, when it is again strongest.

M. Dove has suggested that the parallel currents, by which the equipoise is maintained in the temperate zones, may be shifting ones, and we have supposed that the parallel currents of N.E. and S.W. winds may advance from the N.W. with a lateral motion. The same cause that produces the well-known opposite and superposed equatorial and polar currents, will also give rise to the same opposite *but* parallel currents in the temperate zones, namely, the ascending column of heated and consequently rarefied air. Now it is well known that in stormy weather, when the wind is blowing with great force, the barometer being nearly at its minimum, upon the *wind changing* the barometer *commences rising*; the wind however continues to blow with about the same force as it did with the previous falling barometer. Upon M. Dove's view of parallel and opposite currents, somewhere in or near the line forming the boundary between the currents, towards or in the torrid zone, we ought to find the point

of rarefaction, and to this point the N.E. current would rush with the greatest force to supply the ascending column of heated air*. This N.E. current would be compensated by a S.W. current of nearly or quite the same force, situated just to the S.E. of it, as in fig. 3, in which let a point of rarefaction, *a* for instance, exist in any locality, so that a N.E. current may be established to supply the ascending column; suppose the greatest force to exist along the line of crossed arrows *b b*, the air would be drawn from the end of this line to fill up the vacuum at *a*, and a compensating S.W. current, *c c*, established. This S.W. current would be established partly by the descent of the overflowing current at *a*, and partly by the rush to supply the air

Fig. 3.



* In attributing the greatest force to the N.E. current, I do not by any means wish to put forward or support any hypothesis that would at all interfere with the well-known fact, that the greatest force is usually manifested by S.W. winds. The point to which I wish more particularly to solicit the attention of the Association is this, *the cause which induces the south-westerly current itself*. This must reside in or near the torrid zone. Here we have a sufficient cause; we are presented with phenomena fully adequate to explain an influx of *cool* air from the N.E. This is the current that must first be established, and in the first instance its force will be *greatest*. We have however only to turn to Prof. Dove's letter to Col. Sabine (Report of the British Association, 1845, p. 61), and we shall at once find the reason why S.W. winds manifest by means of our instruments the greatest force. The N.E. currents are *narrower*, and the force *soon abates* as they pass over towards the S.E.; while on the other hand the same station is not only *oftener*, but *longer* in the S.W. currents, and as the line of greatest force approaches, the force increases, on some occasions very rapidly, until the wind changes. The line of greatest force soon passes the station, so that upon a mean of numerous observations the south-westerly wind exhibits the *greatest* force.

constantly drawn off to feed the ascending column. When however it is once established, the velocity of the line of S.W. current nearest to the N.E. would probably be equal, or nearly so, to that of the N.E. current itself.

In this way it is easy to conceive that a complete barometric wave may be produced; the lines of greatest velocity of the parallel currents will indicate the trough; the rapidity with which the currents pass in opposite directions greatly diminishes the pressure, and according to this view somewhere near the direction of the trough and to the S.W. of it, we ought to find the point of greatest rarefaction: the velocity decreases on each side this trough, and with this decrease of velocity the pressure increases, so that we have a distribution of pressure of a *wave form* gradually rising on each side the trough, the *pressure* being dependent on the velocity of the parallel currents.

The constant ascent of air at the point of rarefaction would continually draw off a quantity of air from the S.E. side of the line of greatest velocity $b-b$, fig. 3, and this would be attended with two results; first, there would be a *real hollow* or trough formed in the line of junction of the parallel currents; and secondly, this line would gradually advance towards the S.E.; for as more air would be drawn off from that side, the whole body of air would advance in that direction to supply the deficiency; and should the rarefying process cease, we can readily conceive that not only will the *wave-form* be continued, but also *wave-motion*. The establishment of the parallel currents will give the *air-motion*; the diminution of pressure towards the lines of greatest velocity will give the *wave-form*; and the drawing-off of air from the S.E. will induce the *wave-motion*. The wave thus generated is *negative*; it consists of a hollow produced by the ascending current of heated air carrying off a considerable portion of air set in motion by this ascending column, and its direction of motion is determined by more air being drawn off from the S.E. slope than the N.W.

Cross Waves.

The examination of the transit of a single wave by means of barometric and anemonal observations, would be comparatively easy, but it seldom happens, from the operation of natural causes, that an isolated or solitary wave is produced. In almost every instance (except in those in which the generating power is very much greater than any which occasions the production of smaller waves) the wave is contemporaneous with others of equal, if not of greater magnitude, so that different systems are in motion at the same time, each individual pursuing its own course, and although perfectly independent of every other, yet greatly modifying the *resulting phenomena* as exhibited by the barometer and anemometer. When therefore we proceed with the examination of certain barometric and anemonal phenomena in the manner above alluded to, we are speedily perplexed with the barometric and anemonal effects

of *cross waves*; the flowing of one set of waves in a certain direction is apparently interrupted and interfered with by another in a different direction, and before the first set can be exhibited with its proper proportions, and the true altitudes, amplitudes, velocities, and directions of its individual waves assigned, all the phænomena of the other set must be carefully disentangled and separated from the aggregate phænomena presented by the contemporaneous systems. The barometric curve, including a complete rise and fall at any one station, is *not* the curve resulting from the transit of any *one* wave; it does *not* represent the *form* of any *reality* in nature; but it *does* represent, and is an exponent of the effects resulting from the contemporaneous transits of waves, or systems of waves, such as have been described.

The discussion of Mr. Brown's observations has clearly brought to light a set of parallel and opposite currents at right angles to those we have been contemplating, namely, from N.W. and S.E. with a *wave-motion* towards the N.E., producing the cross waves which occasion the complexity before alluded to. The late Professor Daniell has remarked that the curves increase in range towards the N.W., and in general the neighbourhood of *water* presents curves remarkable for the boldness of their contour and the large extent of their range. In venturing a speculation on these cross waves from the S.W. with parallel and opposite currents from N.W. and S.E., I should be inclined to attribute them to the effect of the solar influence on the terrestrial surface, extending from Cape Verd in Africa to the extreme north of Lapland in Europe. This surface extends from S.W. to N.E., or somewhat in that direction. It may be remarked, that to the *north-east* of Cape Verd is situated the Sahara or Great Desert of Africa, and here we have a great rarefying surface. To the north-west or west-north-west of this extensive rarefying surface, the broadest part of the Atlantic ocean is situated. The relative positions of the Great Desert and the broadest extent of the Atlantic will produce a great indraught of cool air from the ocean; the direction of this wind will be W.N.W. or N.W. To the north-east of this current, probably in the neighbourhood of Morocco, Fez, Algiers, Spain and Portugal, and the north-west portions of the Mediterranean sea, we ought to find the counter current from the S.E. or E.S.E., the two portions in juxtaposition moving with the greatest velocity. Somewhere in the Atlantic the turning-point of these oppositely directed currents should exist. The line of junction of these parallel currents will determine the trough of the wave, and as before shown, in consequence of the air being drawn off from the north-east to supply the ascending current, the wave will progress towards that quarter; the barometer first descending with the S.E. wind as the trough approaches stations to the N.E., and rising with the N.W. as the current produced by the rarefaction approaches, until the crest passes, when the new counter current or slope of the next wave would set in*.

* In the above suggestion I have considered the northern portion of the African

Pursuing this idea further, there can be no question that Ireland and Scotland become points, or unitedly constitute a great point of rarefaction, forming as they do the nearest land to the northern part of the Atlantic, the land becoming hotter than the neighbouring water, and in consequence a N.W. current with its compensating current from the S.E. is induced. Not only will the rapidity of the currents reduce the pressure, but the ascending column from the land will transfer some of the air into the general current of the atmosphere, and there will be a real difference in the distribution of air as well as pressure; a section transverse to the line of greatest velocity will exhibit a hollow or trough, and the same phenomena will result from this arrangement of the aerial currents as we noticed arising from the N.E. and S.W. currents, the only difference being in direction.

In thus considering these rectangularly posited systems of parallel and opposite currents, many complex anemional and barometric phenomena receive an easy explanation, particularly the revolution of the vane in one uniform direction, and the barometric wind-rose. When the conterminous edges of any two currents pass a station, the barometer is either at a maximum or minimum with respect to that particular system of currents; the wind also changes at this time. If the barometer has previously been rising with a north-easterly wind, it now begins to fall with a south-westerly: the cross currents are however passing at this time with a lateral motion towards the N.E.; in this set of cross currents the barometer will rise with a north-westerly wind and fall with a south-easterly. Suppose while the posterior slope of a N.W. wave transits, wind S.W., and before its trough passes, the trough of the cross wave from the S.W. also transits, and is immediately succeeded by the following anterior slope with its N.W. current, the wind will pass from S.W. to W. Now while this slope continues, upon the trough of the N.W. system passing, the wind changes to N.E., and the resultant of the two currents is N. It is easy to pursue this reasoning, and thus trace the changes of the wind arising from these two cross systems completely round the compass.

The two systems of cross currents naturally divide themselves into four beds of opposite currents, namely, N.E. S.W., N.W. S.E.; with the first of each system, N.E. N.W., the barometer rises, and with the last of each, S.W. S.E., it falls, so that in the barometric wind-rose the maximum is found about the N.E., the prevailing system, and the minimum near the S.W., the opposite current of this system.

The extent of arc which the wind-vane frequently describes, especially in stormy weather, also receives an explanation from these systems of cross currents. A contemporaneous S.W. with a N.W. wind will occasion large arcs to be described between these points;

continent as inducing the N.W. current, but of course, the entire surface, as far as the extreme north of Europe, including Great Britain and Ireland, will act as a rarefying surface.

the south-westerly gusts prevailing, directing the vane to that quarter; and the north-westerly immediately following, instantly occasions a change carrying the vane towards the N.W. These sudden and extensive changes are rendered more distinctly perceptible by means of a small kite flown with about 250 or 300 feet of string, or even more; the distinctness and independence of the direction of the two currents are readily seen, as well as the difference in their strength.

Discussion of Mr. Brown's Observations.

In the discussion to which I have alluded, I have first arranged such of the observations collected by Mr. Brown, or deductions from them, as indicate the barometric pressure about the middle of each day at the stations before-named, as near as the data furnished by that gentleman will allow. These observations or deductions will be found in Table I. The arrangement is such that the eye may readily ascertain the barometric state of the atmosphere at any station on any day embraced by the area and period included in the table. The changes at any one station are also readily seen, the altitudes above 30 inches being distinguished from those between 29 and 30, and those below 29 also being distinguished from the rest. This table forms the basis of the conclusions which have been thus arrived at. The values corresponding to each day have been arranged with especial reference to the maximum and minimum of that day *in space*, that is, the station exhibiting the greatest pressure on any particular day has generally been placed first on the list for that day; and that exhibiting the least, last. At the head of each list are placed the directions of the crests as indicated by the observations. Crests passing from N.W. to S.E. are distinguished by the odd numbers, and those passing from S.W. to N.E. by the even. When the observations give two slopes from a crest or trough passing between such slopes, the observations have been arranged to exhibit this. After the arrangement of the observations, the lines of the greatest diminution of pressure corresponding in a majority of cases to transverse sections of the waves, and exhibiting either their anterior or posterior slopes, are inserted. These are succeeded by the direction of the wind on each side of the crests as given in Mr. Brown's plates, and the discussion of each day's observation is concluded by a few explanatory notes*.

* For the complete discussion of Mr. Brown's observations, the reader is referred to the author's third report on atmospheric waves in the Report of the Sixteenth Meeting of the British Association for the Advancement of Science, held at Southampton in 1846, pp. 141 to 162.

TABLE I.—Barometric Observations, November 1842.

Station.	In.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.
Orkneys . . .	30	<u>·16</u>	<u>·23</u>	<u>·24</u>	<u>·49</u>	<u>·52</u>	<u>·46</u>	<u>·15</u>	<u>·63</u>	<u>·80</u>	<u>·39</u>	<u>·24</u>	<u>·10</u>	<u>·35</u>
Belfast . . .	30	<u>·33</u>	<u>·18</u>	<u>·18</u>	<u>·45</u>	<u>·55</u>	<u>·51</u>	<u>·43</u>	<u>·04</u>	<u>·41</u>	<u>·57</u>	<u>·02</u>	<u>·21</u>	<u>·27</u>
Shields . . .	30	<u>·17</u>	<u>·19</u>	<u>·10</u>	<u>·34</u>	<u>·33</u>	<u>·35</u>	<u>·27</u>	<u>·97</u>	<u>·28</u>	<u>·58</u>	<u>·99</u>	<u>·07</u>	<u>·24</u>
Cork. . . .	30	<u>·15</u>	<u>·92</u>	<u>·83</u>	<u>·16</u>	<u>·32</u>	<u>·30</u>	<u>·33</u>	<u>·01</u>	<u>·42</u>	<u>·20</u>	<u>·91</u>	<u>·31</u>	<u>·40</u>
Bristol . . .	30	<u>·18</u>	<u>·05</u>	<u>·96</u>	<u>·14</u>	<u>·20</u>		<u>·18</u>	<u>·07</u>	<u>·60</u>	<u>·46</u>	<u>·03</u>	<u>·31</u>	
Plymouth . .	30	<u>·21</u>	<u>·04</u>	<u>·91</u>	<u>·15</u>	<u>·22</u>	<u>·24</u>	<u>·24</u>	<u>·13</u>	<u>·72</u>	<u>·48</u>	<u>·12</u>	<u>·46</u>	<u>·46</u>
London. . . .	30	<u>·17</u>	<u>·10</u>	<u>·96</u>	<u>·13</u>	<u>·12</u>	<u>·16</u>	<u>·13</u>	<u>·08</u>	<u>·70</u>	<u>·64</u>	<u>·00</u>	<u>·33</u>	<u>·26</u>
Paris	30	<u>·04</u>	<u>·86</u>	<u>·73</u>	<u>·80</u>	<u>·75</u>	<u>·83</u>	<u>·89</u>	<u>·90</u>	<u>·76</u>	<u>·63</u>	<u>·25</u>	<u>·43</u>	<u>·53</u>
Geneva	30	<u>·34</u>	<u>·09</u>	<u>·88</u>	<u>·82</u>	<u>·79</u>	<u>·86</u>	<u>·93</u>	<u>·01</u>	<u>·08</u>	<u>·06</u>	<u>·84</u>	<u>·84</u>	<u>·10</u>
Christiania. .	29	<u>·78</u>	<u>·11</u>	<u>·31</u>	<u>·37</u>	<u>·27</u>	<u>·21</u>	<u>·02</u>	<u>·67</u>	<u>·37</u>	<u>·24</u>	<u>·48</u>	<u>·20</u>	<u>·94</u>
St. Petersburg	29	<u>·45</u>	<u>·80</u>	<u>·21</u>	<u>·22</u>	<u>·25</u>	<u>·16</u>	<u>·96</u>	<u>·96</u>	<u>·92</u>	<u>·89</u>	<u>·66</u>	<u>·87</u>	<u>·62</u>
Station.	In.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.	26.
Orkneys . . .	29	<u>·76</u>	<u>·01</u>	<u>·22</u>	<u>·35</u>	<u>·18</u>	<u>·91</u>	<u>·96</u>	<u>·86</u>	<u>·43</u>	<u>·33</u>	<u>·10</u>	<u>·07</u>	<u>·10</u>
Belfast . . .	29	<u>·91</u>	<u>·82</u>	<u>·06</u>	<u>·51</u>	<u>·37</u>	<u>·86</u>	<u>·91</u>	<u>·95</u>	<u>·42</u>	<u>·32</u>	<u>·79</u>	<u>·82</u>	<u>·04</u>
Shields . . .	29	<u>·82</u>	<u>·83</u>	<u>·03</u>	<u>·45</u>	<u>·42</u>	<u>·77</u>	<u>·85</u>	<u>·89</u>	<u>·30</u>	<u>·27</u>	<u>·78</u>	<u>·82</u>	<u>·99</u>
Cork. . . .	29	<u>·60</u>	<u>·37</u>	<u>·70</u>	<u>·31</u>	<u>·18</u>	<u>·92</u>	<u>·80</u>	<u>·83</u>	<u>·58</u>	<u>·10</u>	<u>·54</u>	<u>·80</u>	<u>·04</u>
Bristol . . .	29	<u>·65</u>	<u>·61</u>	<u>·78</u>	<u>·36</u>	<u>·42</u>	<u>·98</u>		<u>·79</u>	<u>·39</u>	<u>·26</u>	<u>·79</u>	<u>·84</u>	<u>·14</u>
Plymouth . .	29	<u>·68</u>	<u>·64</u>	<u>·70</u>	<u>·36</u>	<u>·47</u>	<u>·14</u>	<u>·73</u>	<u>·79</u>	<u>·53</u>	<u>·30</u>	<u>·91</u>	<u>·93</u>	<u>·20</u>
London. . . .	29	<u>·80</u>	<u>·62</u>	<u>·79</u>	<u>·36</u>	<u>·53</u>	<u>·06</u>	<u>·77</u>	<u>·82</u>	<u>·28</u>	<u>·59</u>	<u>·92</u>	<u>·88</u>	<u>·17</u>
Paris	29	<u>·67</u>	<u>·55</u>	<u>·50</u>	<u>·99</u>	<u>·38</u>	<u>·17</u>	<u>·55</u>	<u>·57</u>	<u>·13</u>	<u>·41</u>	<u>·02</u>	<u>·01</u>	<u>·17</u>
Geneva	30	<u>·10</u>	<u>·08</u>	<u>·86</u>	<u>·89</u>	<u>·33</u>	<u>·53</u>	<u>·10</u>	<u>·71</u>	<u>·73</u>	<u>·86</u>	<u>·55</u>	<u>·36</u>	<u>·27</u>
Christiania. .	29	<u>·35</u>	<u>·70</u>	<u>·86</u>	<u>·94</u>	<u>·11</u>	<u>·91</u>	<u>·60</u>	<u>·54</u>	<u>·55</u>	<u>·62</u>	<u>·66</u>	<u>·62</u>	<u>·57</u>
St. Petersburg	29	<u>·53</u>	<u>·14</u>	<u>·45</u>	<u>·68</u>	<u>·85</u>	<u>·16</u>	<u>·87</u>	<u>·78</u>	<u>·42</u>	<u>·59</u>	<u>·03</u>	<u>·08</u>	<u>·11</u>

The numbers in the columns immediately succeeding the names of the stations indicate the initial inch of the barometric readings of the 1st and 14th of November, the succeeding numbers are decimals of an inch. Observations above 30 inches are not underlined. Those between 29 and 30 inches have a single line —, and those below 29 inches a double line ==.

Results of the Discussion of Mr. Brown's Observations.

In collecting the results of this discussion, I have arranged in Tables II. and III. the principal lines of diminution of pressure; the succession of waves as well as the distinct systems become very apparent from these tables. The succeeding Tables IV. and V. exhibit the principal features of the respective waves of each system. The most prominent result appears to be the confirmation of Prof. Dove's suggestion of parallel and oppositely directed currents. The diagrams of the wind in connection with the barometric observations clearly exhibit such currents, and we see by a glance at Tables IV. and V. that the beds of these currents varied considerably in breadth. At the opening of the observations they were very much *broader* than at the close, and the N.W. system (waves No. 2, 4, 6) were altogether larger than the S.W. We have in fact two systems of waves or currents crossing each other at right angles, the individuals in both gradually decreasing in size. In the speculation which has been ventured relative to the S.W. system, the mass of terrestrial surface forming the N.W. boundary of the great eastern continent has been assumed as the rarefying surface, producing the set of parallel and oppositely directed S.E. and N.W. winds, the currents gradually shifting towards the N.E. The gradual contraction of the beds of each system as the observations proceed is a highly interesting feature, which requires a more extensive discussion for its elucidation.

TABLE II.—Exhibiting the principal lines of the greatest diminution of pressure of the N.W. system of waves, Nos. 2, 4, and 6.

Epochs.	Directions.	Values.	Slopes.
Nov. 1	Belfast to Paris	·29	Anterior, No. 2
2	Orkneys to Paris.....	·37	2
4	Orkneys to Paris.....	·69	2
5	Belfast to Paris	·80	2
6	Belfast to Paris	·68	2
7	Belfast to Paris	·54	2
9	Paris to Orkneys.....	·96	Posterior, No. 2
10	London to Cork	·44	2
11	Paris to Cork	·34	2
16	Orkneys to Paris.....	·72	Anterior, No. 4
17	Belfast to Paris	·52	4
18	London to Cork	·35	Posterior, No. 4
18	London to Orkneys.....	·35	4
20	Orkneys to Paris.....	·41	Anterior, No. 6
21	Belfast to Paris	·38	6
22	Cork to Paris	·45	6
24	Paris to Cork	·48	Posterior, No. 6
25	Paris to Cork	·21	6
26	Paris to Cork	·13	6

TABLE III.—Exhibiting the principal lines of the greatest diminution of pressure of the S.W. system of waves, Nos. 1, 3, 5, 7, 9, 11, 13, 15.

Epochs.	Directions.	Values.	Slopes.
Nov. 1	Belfast to Christiania ...	·55	Anterior, No. 1
3	Christiania to Paris.....	·58	Posterior, No. 1
11	Christiania to Cork.....	·57	3
12	Christiania to Shields ...	·13	3
12	Plymouth to Shields ...	·39	Anterior, No. 5
13	Paris to Christiania.....	·59	5
14	Belfast to Christiania ...	·56	5
15	Orkneys to Cork.....	·64	Posterior, No. 5
17	Belfast to Christiania ...	·57	Anterior, No. 7
18	London to Christiania...	·42	7
21	Belfast to Christiania ...	·41	Anterior, No. 11
23	Christiania to Cork.....	·52	Posterior, No. 13
24	Christiania to Cork.....	1·12	13

TABLE IV.—Exhibiting the principal features of the waves of the N.W. system Nos. 2, 4, and 6.

Wave No. 2.

Epochs.	Phases.	Directions and Localities.	Altitudes.	Winds.	
				Anterior Slope.	Posterior Slope.
Nov. 1	Crest.	N.W. of the United Kingdom			
2	Crest.	N.W. of the United Kingdom	N.E.	
3	Crest.	N.W. of the United Kingdom	N.E.	
4	Crest.	N.W. of the United Kingdom	30·49+	N.E.	
5	Crest.	From Cork to the Orkneys	30·55	N.E.	
6	Crest.	S.E. of Belfast	30·51+	N.E.	
7	Crest.	S.E. of the Orkneys	30·43+	N.E.	S.W.
8	Crest.	Passes Plymouth.....	30·13		
9	Crest.	S.E. of Paris	29·90+		S.W.
9	Post. Trough.	Passes the Orkneys.....	28·80		S.W.
10	Post. Trough.	Near the eastern coast of Ireland extending to Christiania.....	29·24		S.W.
11	Crest.	Considerably S.E. of Paris.....			
11	Post. Trough.	Passes Plymouth.....	29·12		S.W.

Wave No. 4.

Nov. 17	Crest.	Passes Belfast	30·51	N.E. E.	
18	Crest.	Passes London	30·53	N.E.	S.W.
19	Crest.	Considerably S.E. of Paris.....			

Wave No. 6.

Nov. 21	Crest.	N.W. of Belfast and Cork	N.E.	
22	Crest.	Near Cork, Belfast and Orkneys.....	29·58		
23	Crest.	Passes London	29·59		S.W.
24	Crest.	Near to or S.E. of Paris.....		S.W.

TABLE V.—Exhibiting the principal features of the waves of the S.W. system Nos. 1, 3, 5, 7, 9, 11, and 13.

Wave No. 1.

Epochs.	Phases.	Directions and Localities.	Altitudes.	Winds.	
				Anterior Slope.	Posterior Slope.
Nov. 1	Crest.	Belfast to Paris	30·33	N.W.	S.E.
2	Crest.	Between Belfast and Christiania ...	30·23+	N.N.W.	S.E.
3	Crest.	W. or S.W. of Christiania	30·31+	N.W.	S.E.
7	Post. Trough.	N.E. of Belfast and Paris			

Wave No. 3.

Nov. 7	Ant. Trough.	N.E. of Belfast and Paris			
8	Crest.	S.W. of Belfast and London	N.W.	
10	Crest.	Belfast to Paris	29·64	N.W.	S.E.
11	Crest.	Near Christiania	29·48		S.E.
12	Post. Trough.	Between Belfast and Shields			

Wave No. 5.

Nov. 12	Ant. Trough.	Between Belfast and Shields			
13	Crest.	S.W. of Cork, Plymouth and Paris...			
14	Crest.	Belfast to London	29·91	N.W.	S.E.
15	Crest.	Passes the Orkneys.....	30·01		S.E.
16	Crest.	Between Orkneys and Christiania...			E. ^b

Wave No. 7.

Nov. 17	Crest.	Passes Belfast	30·51		S.E. E. ^b
18	Crest.	Between Cork and the Orkneys.....			
19	Post. Trough.	Near Belfast and Shields			S.E.

Wave No. 9.

Nov. 19	Ant. Trough.	Near Belfast and Shields			
20	Crest.	Passes the Orkneys.....	29·96		S.E.

Wave No. 11.

Nov. 21	Crest.	Near Belfast and Shields	29·95	N.W.	S.E. E. ^b
22	Post. Trough.	Near Belfast and Shields			

Wave No. 13.

Nov. 23	Crest.	S.W. of Christiania.....	29·62		
	Post. Trough.	Cork to Bristol			

^b Resultants of N.E. and S.E. currents.

General Conclusion.

From the collation of Mr. Brown's with the St. Petersburg and Geneva observations which have been added to Table I., it is readily apparent that the results arrived at in the discussion to which reference has been made have been fully confirmed, and there appears to be but little doubt that the waves as determined in the first instance by a discussion of observations from the stations announced in my first report*, and further identified and illustrated by the observations collected by Mr. Brown, as well as those which have been brought to light by means of Mr. Brown's observations, and confirmed and illustrated by the St. Petersburg observations, had a *real existence*; an *individuality* has been attributed to certain arrangements of aerial currents and distribution of pressure in connexion with such currents, the aggregate phenomena forming an atmospheric wave. Of the waves thus brought to light, *two* occupy very prominent positions, they stand out as it were from the others; the individuality of each is very striking, and the velocities with which they traversed the area isolate them from their predecessors and exhibit them not as gregarious, but solitary waves. These waves are B° and crest No. 4, the first occurring just previous to the setting in of the great wave, and the last forming its crown. The wave, crest No. 4, appears from its elevated position on the symmetrical or normal wave, admirably adapted to crown our investigations with success, especially in so far as its amplitude, velocity and path are concerned, we are now, I apprehend, in possession of materials to determine with a considerable approximation to accuracy, these elements. Its longitudinal direction appears to have been very extensive. This element would receive considerable elucidation by means of observations from the south of France, Spain, Portugal and the north of Africa. It is highly probable that this wave in the direction of its length stretched from the extreme south to the very north of Europe.

The waves just alluded to will furnish material for a further paper, in which their essential features, elements, &c. will be fully discussed. In the mean time the attention of the reader is directed to several interesting features of waves Nos. 1 and 2.

Wave crest No. 1.—An inspection of Table V. shows that this crest extended from Belfast to Paris on the 1st; on the 4th it passed Christiania, and on the 5th it was vertically over St. Petersburg. On the same day, the 5th, Mr. Brown's observations indicate that the crest No. 2 extended from Cork, past Belfast, towards the Orkneys; so that the point of *intersection* of the two crests, Nos. 1 and 2, must have been situated toward the north-west of Norway. This at once explains the greater amount of pressure in the north-west of Europe in the early part of November.

During the first eight days of November, the movements at St. Petersburg and Geneva were strictly in accordance with the transit

* Report of the Fourteenth Meeting of the British Association for the Advancement of Science, page 267.

of a large wave from S.W. to N.E., its amplitude being about double the distance between the stations: they were of a precisely opposite character, as indicated in the following table:—

TABLE VI.

Epoch.	Geneva.	St. Petersburg.
Nov. 1	30·34	29·45
2	30·09	29·80
3	29·88	30·21
4	29·82	30·22
5	29·79 ^m	30·25 ^M
6	29·86	30·16
7	29·93	29·96
8	30·01	29·96

^m maximum. ^M minimum.

The line joining Belfast and Paris, the direction of crest, No. 1 on the 1st, when produced, will pass near Geneva, the anterior slope extending to or beyond St. Petersburg. The numbers in the above table clearly show, that when the barometer was *falling* at Geneva from the posterior slope, it was *rising* at St. Petersburg from the anterior. On the 5th the crest passed the latter station, when the posterior trough passed Geneva. This gives for the semi-amplitude, or half breadth of the wave, 1365 miles; the velocity of the *crest*, from its passing Geneva to its arrival at St. Petersburg, being 14·22 miles per hour. The altitude, as measured by the mercurial column, equal to ·46 inch. The fall at St. Petersburg, from the posterior slope of this wave, and the rise at Geneva, from the anterior slope of wave crest No. 3, is also clearly seen in the above table.

Wave crest No. 2.—Table IV. shows that on the 5th this crest extended from Cork to the Orkneys. The anterior trough of this wave was also in the neighbourhood of Geneva on this day. This gives for the semi-amplitude 792 miles. The altitude of the wave appears to have been ·76 inch, and the velocity of the crest about 8·25 miles per hour.

This wave, with its front towards the south-east, was very distinctly developed during the early part of November. The altitude of the crest appears to have *subsided* as the wave progressed; the highest reading at Belfast was 30·55 on the 5th, at London 30·16 on the 6th, and at Paris 29·90 on the 8th. The following tables exhibit the features of the anterior slope. Table VII. shows the barometric rise and fall at stations arranged more or less with regard to a line cutting the crest of the wave transversely. The depressing influence of the wave, crest No. 1, is clearly seen at London and Paris on the 5th. Tables VIII., IX. and X. exhibit the depression of the south-easterly stations below those to the north-west of them while the anterior slope passed.

TABLE VII.—Barometric differences arising from Anterior and Posterior Slopes of Crest No. 2.

Epoch.	Belfast.	Bristol.	London.	Paris.
Nov. 2	-.15	-.13	-.07	-.18
" 3	.00	-.09	-.14	-.13
" 4	+.27	+.18	+.17	+.07
" 5	+.10	+.06	-.01	-.05
" 6	-.04	+.04	+.08
" 7	-.08	-.01?	-.03	+.06
" 8	-.39	-.11	-.05	+.01

TABLE VIII.—Barometric differences arising from Anterior Slope of Crest No. 2.

Epoch.	Belfast.	London.	London. ±
Nov. 1	30.33	30.17	-.16
" 2	.18	.10	-.08
" 3	.18	29.96	-.22
" 4	.45	30.13	-.32
" 5	.55	.12	-.43
" 6	.51	.16	-.35
" 7	.43	.13	-.30
" 8	30.04	30.08	+.04

TABLE IX.

Epoch.	London.	Paris.	Paris. ±
Nov. 1	30.17	30.04	-.13
" 2	.10	29.86	-.24
" 3	29.96	.73	-.23
" 4	30.13	.80	-.33
" 5	.12	.75	-.37
" 6	.16	.83	-.33
" 7	.13	.89	-.24
" 8	30.08	29.90	-.18

TABLE X.

Epoch.	Belfast.	Paris.	Paris. ±
Nov. 1	30.33	30.04	-.29
" 2	.18	29.86	-.32
" 3	.18	.73	-.45
" 4	.45	.80	-.65
" 5	.55	.75	-.80
" 6	.51	.83	-.68
" 7	.43	.89	-.54
" 8	30.04	29.90	-.14

Upon comparing the numbers at the Orkneys with those at Paris, as given in Table I., we at once recognise a considerable difference in the oscillation at the two stations. At the Orkneys it amounted to 1.72, at Paris to .79, range at the Orkneys in excess .93. It appears probable that this difference of oscillation at two stations, as the Orkneys and Paris, may be thus explained. The curves in the north-west of Ireland, as determined by the discussion of Sir John Herschel's hourly-observations, are remarkable for boldness and freedom of contour and great range of fluctuation. The late Professor Daniell found, from an examination of the Manheim observations, that the range increased towards the north-west, and that the greatest oscillation occurred in the neighbourhood of water. Now a wave generated in any way and approaching the continent of Europe from the north-west, would most probably impinge on it with a high and in some cases acuminated crest , but as it passed onward the crest would gradually subside , so that at stations considerably to the south-east the fluctuations would be very much less than at or near its point of genesis. Again, a negative wave, with a deep trough also approaching from the north-west , would present large fluctuations as it impinged on the land; but after passing onwards, the opposite to subsidence would take place; the depth of trough would decrease, , and the oscillations to the south-east would also decrease. Such phenomena appear to be presented by the observations from the 5th to the 10th of November 1842.

In connexion with this subsidence of crest No. 2, as it passed from N.W. to S.E., it may be interesting to notice the passage of crest No. 3, the circumstances of the transits of the two waves being different; wave No. 2 coming *from* the sea, and passing over Ireland, Scotland and England, and the land forming the central part of Europe; while wave No. 3 skirted the western and north-western boundaries of Europe, and more or less passed over the waters of the Atlantic and North Sea. The crest No. 3 came from the S.W., so that a line from Plymouth to Christiania would cut it more or less transversely; the ranges however are nearly the same at both stations. The crest which traversed England on the 1st arrived at Christiania on the 4th; at this time the barometer had commenced rising at Plymouth from the anterior slope of crest No. 2, and it continued rising until the 7th, when the crest passed. At Christiania the barometer had fallen from the posterior slope of crest No. 2. It appears from a careful comparison and consideration of the barometric movements at Plymouth and Christiania, that crest No. 2 passed Christiania about a day earlier than it did Plymouth, that is, the longitudinal direction of the crest was such as to cause it to pass over Christiania while Plymouth was still under the anterior slope of the wave, the sections passing over Christiania and Plymouth being separate and distinct. The character of the passing wave is well-

determined at both stations, the posterior slope exhibiting a rapid and deep fall, which took place alike at Christiania and Plymouth.

The crest No. 2 passed Cork, Belfast and the Orkneys on the 5th, Plymouth on the 7th, and Paris on the 8th, with a diminution of oscillation. We find however no diminution of oscillation at Christiania as compared with Plymouth. It is highly probable the subsidence of the crest, as it proceeded towards Paris, resulted from the influence of the land in England, while both at Plymouth and Christiania the crest was but slightly interfered with by the influence of land, the difference of level resulting from the anterior slope of crest No. 3.

These considerations exhibit a large wave of considerable breadth and slow motion, extending in a longitudinal direction from the extreme south-west of England, past Norfolk, towards the Swedish capital.

The elements of the waves, as referred to in Tables II., III., IV. and V., have been determined by observations made *daily*; and a perusal of the discussion of these observations will clearly show that, in order to detect and examine the characters of the *larger* undulations, daily observations may suffice. For this purpose it will be best to choose the middle of the day. The Astronomer Royal has determined, from four years' observations at Greenwich, that at 2 hours, Göttingen mean time (20 minutes past 1 P.M. Greenwich time), the barometer varies but $0^{\text{in}}.001$ from the mean of the year, that is, if the mean pressure is to be determined from an isolated observation each day, this hour is the most suitable, the mean correction being $0^{\text{in}}.001$ to be added. This at once indicates, that for the present purpose this hour is also the most suitable, the observations being, to a great extent, free from the effects of the diurnal oscillation. If two observations are made, the most suitable epochs are 7:20 A.M. and 1:20 P.M.; and if three, 7:20 P.M. in addition. In order however to detect and examine smaller undulations, observations at shorter intervals equally disposed throughout the twenty-four hours are essential.

LXXVII. *Experiments proving the common nature of Magnetism, Cohesion, Adhesion and Viscosity.* By Sir GRAVES C. HAUGHTON, K.H., M.A., F.R.S., Foreign Associate of the Institute of France, &c.

[Continued from p. 457.]

PART II.

Of the mutual magnetic attractions of non-ferruginous bodies.

AFTER a great number of substances had been measured according to the preceding plan, it occurred to me that by a slight modification of the needle, the mutual magnetic relations of non-ferruginous bodies might possibly be ascertained. Through the obliging readiness of M. Pixii of this city, I had

two needles constructed, half of which were magnets and the other half brass; the junction of the brass and steel being effected exactly mid-way where the caps were attached. As it was anticipated that the movement of such a needle would be slow, as the whole directive energy could only be obtained from the magnetic end, their caps were of stone. I had the pleasure to find that though their movement was necessarily sluggish in comparison with that of the common needle, yet that it was perfectly uniform and certain; and that the brass end attached itself to various substances with even more tenacity than had been the case with the purely magnetic needle, or with its own magnetic portion. I next had two needles made, one with a gold and the other with a silver termination; but owing to a misapprehension of the workman their caps were of brass, and from this circumstance the increased friction prevented their free movement, particularly in the case of the silver needle. However, sufficient was discovered by their employment to see that the gold and silver could likewise be made to attach themselves to non-ferruginous bodies.

It was only now necessary to make such a modification of the new principle of measurement as should enable it to be applied to any metal, and for this purpose I had short tubes soldered to the ends of half magnetic needles where they were joined to their agate caps. Into these small tubes needles of any malleable metal could be inserted at pleasure; their weight and length being just sufficient to balance the steel portion of the needles. The steel ends were necessarily made thicker than the magnetic needle I have already described, to balance the increased weight of the tubes and metal needles; and the whole weighed, when complete, in one case sixteen grains, and in the other, which was a little larger, nearly eighteen grains. The length of the steel half was twenty-three millimètres, and the whole needle therefore forty-six millimètres long. It is necessary to bear in mind that the metal needles should not exceed the magnetic ends in length, otherwise, according to the principle of the lever, they would be moved with greater facility than the steel portion, and give proportionably higher results.

The form of these needles is here given with and without their metal ends.



By a further modification, the arrangement was fitted to be

employed with any small fragment or substance that it was desirable to use as a needle. This consisted simply in making a slender needle of wood of the same form as those of metal, by means of which any fragment, though not exceeding the size of the head of a pin, could be attached with wax, and then employed, the same as if it were a needle of malleable metal, after being inserted in the little tube of the magnetic needle. The deficient weight of the wooden needle can be made up by twining round it a fine strip of sheet-lead; and in like manner the metal needles may be made to balance with equal nicety, when too light, by rolling round them a small strip of tin foil; a necessity that must often occur, owing to the different densities of the metals employed. All these details are requisite, as the experimenter must in so many cases become his own workman, to meet each particular exigency.

These needles were urged to the substances by a bar-magnet, in the same way as in the preceding experiments, and were employed either in measuring the purely magnetic intensities or those of non-ferruginous bodies, by using one or the other end as occasion required. The unmagnetic ends were likewise made to connect themselves with substances, for which they had a strong affinity, by means of the finger instead of a bar-magnet as in the former cases, but to a much greater extent, for it was the only means I had at my command of ascertaining that the junction was made with more than usual readiness, and this result is here marked by the letter *f*, instead of the signs $\times \times$ as before. All the other signs formerly used are again employed in similar cases, and it will be seen, perhaps with some surprise, that the mutual affinities of non-ferruginous bodies greatly exceed those of the magnetic needle, there being few cases in which the measures did not reach 90° .

In the preceding branch of this subject, whatever has been said on the importance of giving time for the influence of the needle to take effect, is no longer necessary; and all that is required is to afford the time requisite for oscillation entirely to cease; for here the effects are purely *normal*, as non-ferruginous bodies only are mutually concerned. This result might have been easily anticipated, for it is now merely natural and spontaneous affinities that are brought into play. Magnetism, in the common acceptation of the term, will be seen to be an *abnormal* and exceptional state, and all the results that proceed from it must partake of the same character. The more powerful therefore the magnet or the loadstone, the more abnormal and exceptional must be its results.

The phænomena brought to light by Dr. Faraday's mode of experimenting are indeed very remarkable, and they appear to me to have a surprising analogy with what he and Thilourier effected, when operating on the gases. By the resources of science, those bodies which in their normal state only exhibit repulsion, were made to reveal their occult power of attraction; and in a similar manner in these new experiments, bodies that had been only remarked for indifference to magnetism, were brought within its pale. Yet the instances which I have already shown, and those which are to follow, will make it more than doubtful whether the two categories of attraction and repulsion under which bodies arranged themselves in his hands, are really those that would result from their normal conditions, if these could be known with exactness. Thus by his experiments he considers platinum as magnetic, and flint glass as diamagnetic, and they ought not therefore to agree in their affinities; yet I find that they both have a remarkable attraction for lead; crown glass too, so far from having a feeble attraction for the magnet, as we are led to suppose from his experiments, has really a very considerable affinity for it, as will be evident by the instance I gave of the needle in the compass-box, which attached itself *per saltum* to its glass screen on a magnet being held over it. Palladium and platinum also, which Dr. Faraday considers as magnetic, agree with all his diamagnetic metals with which I have operated in their attraction for glass of every kind, and I find that both palladium and platinum exhibit a much feebler degree of attraction for the magnetic needle than they do for iron that is free from magnetism. Yet in his scale crown glass ranges between palladium and platinum.

The obscure nature of affinity cannot be better exemplified than in considering these experiments, where we have a fresh demonstration of its universality. As no instance has yet been discovered of a body existing free from repulsion, for no bodies have yet been found with their molecules in close contact, so in the present case we shall see that where attraction could not be more than imagined, that that property exists in considerable strength, and that it is owing to it, by the mere affinity of points for points, that is of molecules for molecules, that bodies hold together with such tenacity, though each point is totally inert beyond its own sphere of action; indeed so much so that the finest hair or a fragment of any substance not so large as the head of a pin, has as much influence, magnetically, on the various needles that have been tried, as the largest mass. It is different however where the electro-magnet is concerned, for through its powerful influence the

magnetic energies of bodies brought within its range are heightened to a surprising degree. Magnetism proves the co-existing and alternating nature of attraction and repulsion, and that where we find one we may be quite certain that the other is only in abeyance, existing potentially, and ready to spring into action the moment the conditions of bodies are such as to admit of this law of nature coming into activity. I say bodies, because, as it is a law of relation, there must be the mutual presence of two things to enable it to exist. Now the *insensible* relations which the present experiments reveal, *seem* identical with those which Dr. Faraday was able to make *sensible*, even to an audience in a theatre, by the means of a powerful electro-magnet, just in the same way as when a bit of paper excited positively or negatively, and scarcely affording the slightest indication of its excitement, will be roused into strong repulsion by the approach of any electric body in a *similar* state with itself, but more highly excited; and indeed the degree of its repulsion will be proportioned to the energy of the electric state of the body with which it is in relation; it being one of the mysterious peculiarities of affinity that the body that exhibits it in the strongest degree raises the feebler related body up to a state of equality.

The neutral state which Dr. Faraday supposes to exist through the struggle of what he terms magnetic and diamagnetic forces, in various saline solutions, is according to the preceding views due entirely to the relative strength of the latent attraction or repulsion roused into a state of exaggeration, and if I may so say *abnormalism*, by the surpassing energy of the electro-magnet; but were we to suppose the phenomena thus exhibited to be the normal state of nature, we should interpret wrong, and mistake the exception for the rule.

But as facts are better than all reasoning, I proceed to exhibit the mutual attractions and presumed repulsions which I elicited in about 600 additional experiments, together with the measures so obtained.

The measurements of the experiments which follow were first thrown into a tabular form, the substances being arranged perpendicularly on the left side of the paper, and the needles in succession at the top, both of them being between ruled lines in opposite directions. This arrangement had the great advantage of affording an easy contrast at a glance, but notwithstanding the number of experiments that have been made it left too many *lacunæ*, owing to the great number of needles and substances which I had not time to try; for the sake of saving space it was abandoned therefore for the present plan.

The needles employed are indicated by the letters which

follow. The glass needle was of flint glass. It is possible that one of crown glass might have given even higher results; but if so its effects could not have been appreciable in the cases above 90° . By a reference to the counter experiments where crown and flint glass were the substances, and the metals the needles, there will be seen to be a great equality in the results.

M magnet; I iron; P. G. pure gold; G18c. gold 18 carats; S silver; P palladium; Pl. platinum; Cad. cadmium; T tin; L lead; Z zinc; Cop. copper; B brass; Germ. S. German silver; Ars. arsenic; Gl. glass.

1st Class.

Gold (pure) with M 90° , difficult; I $90^\circ+$; P. G. $90^\circ+$; G 18c. 90° ; P 90° ; Pl. 81° ; Cad. 90° f.; T $90^\circ+$; Z $90^\circ+$.

Gold (18 carats) with M 90° , difficult; I 90° ; T $90^\circ+$; B $90^\circ \times$.

Silver (pure) with M $90^\circ+$; I $90^\circ+$; Cad. 90° ; T $90^\circ \times$.

Silver (coin) with M $90^\circ+$; Cad. $90^\circ+$; T 88° .

Palladium with M 68° ; I $90^\circ+$; P 40° ; Cad. 60° ; T 90° .

Platinum with M 65° ; I 90° ; P. G. 86° ; Pl. 75° ; P 25° ; Cad. 81° ; T 82° ; Gl. 90° f. easy.

Cadmium with M 90° ; Pl. 76° ; after many efforts it would not come up to the 81° of the cadmium needle and platinum; Cad. 90° , easy; T 90° , very difficult.

Tin with M $90^\circ+$; I 90° , but difficult; Cad. 90° , weak; T 90° , difficult at every step.

Lead with M $90^\circ+$; I $90^\circ+$; P. G. 90° f. very easy; Pl. 90° , easy; L 84° ; Z 90° , difficult; B 90° f.; Gl. 90° f. very firm.

Zinc with M 90° ; Cad. 90° ; T $90^\circ \times$; L $90 \times$; Z 90° ; Cop. 90° .

Copper with M $90^\circ+$; P. G. 90° f. firm; Pl. $90^\circ+$; T $90^\circ \times$; Z 90° ; Cop. 90° ; B 90° f. firm.

Iron with I 90° , very easy; T 90° , rather difficult.

Mercury with M 90° , sticks; G. 18c. 90° , sticks; T 76° .

Antimony with M 66° ; I 27° ; P. G. $90^\circ+$; Pl. 50° ; Cad. 90° , very difficult; T 90° ; Cop. 90° f.; B 90° , firmish with the cut end.

Bismuth with M 51° ; I 77° ; P. G. $90^\circ+$; Pl. 94° ; Cad. 90° , difficult; T 90° ; Cop. 90° ; Gl. 90° .

Chromium with M 90° ; at first it quite failed, but rose by waiting a very long space of time up to 90° ; I 60° ; Pl. 90° ; T failed; L 36° .

Manganese with M. A.; Pl. failed; T 10° .

Cobalt with M 90° ; T failed.

Brass with M 90° ; I 90° , not easy; P. G. 90° f.; Pl. 90° ; T $90^\circ \times$; L 90° , f. very easy and firm; B. 90° , f. very easy, firm.

German silver with M. A.; P. G. 90° , f.; L 90° , f. very easy and firm; Cop. 90° , f. most easy; B 90° , f. strong; Gl. 90° , f. very strong.

Bell-metal with M 90° ; P. G. 90° , f.; T 90° , junction easy after the use of the magnetic needle; L 90° , f. easy; Cop. 90° , f. firm; B 90° , f. very firm; Germ. S. 90° ; Gl. 90° , f. easy.

Arsenic with M 15° ; I 34° ; P. G. 19° ; S failed; P 34° ; Pl. 0° ; Cad. $90^\circ +$; T failed; L 90° ; Z 28° ; Cop. 57° ; B failed; Germ. S. failed; Ars. 65° ; Gl. 52° .

Plumbago with M failed; I failed; Pl. failed.

In this and the following classes, the results already obtained with the magnetic needle are again repeated for the sake of affording an easy means of comparison. The iron needle was of soft iron wire, and on bringing it near a delicate magnetic needle it caused no repulsion, but attracted the N. and S. poles; I therefore believe that if it had any magnetism it did not influence the results, and if so it would have had a tendency rather to diminish than to raise the measurements, for often, as in the case of palladium, platinum, bismuth and arsenic, iron has an advantage over the magnetic needle, though, as may be observed with regard to antimony and chromium, it often falls considerably behind it. Iron in its normal state, that is free from adventitious magnetism, discovers less affinity for other bodies, and *even for itself*, than gold, cadmium, tin, copper, and some other metals. When it was made to attach itself to a bit of its own wire, the connexion was broken with as much facility as was exhibited by most of the metals. This result will be scarcely anticipated, and is a further proof that it was devoid of free magnetism. The polarity that iron exhibits is due entirely to extraneous causes, and as my opinions have been long made up upon the subject, I hope before long to make them known, and to show that it depends upon causes of the simplest kind, if that expression may be applied to phænomena in which so mysterious an agency as affinity is concerned. Manganese exhibited only an attraction of 10° for tin, and none for platinum. Chromium had likewise no attraction for tin, and indicated only 19° for gold, while for platinum it had an attraction of 90° , yet both chromium and platinum are magnetic metals according to Dr. Faraday's experiments, and gold diamagnetic. But all

these unexpected results will be seen at a glance by referring to the list.

Antimony and bismuth stand high with such non-ferruginous metals as have been tried; platinum, for which the attraction of both is low, being excepted. With cadmium the attraction for antimony was raised to 90° by keeping the latter in contact with a bar-magnet for a short time; yet tin, between which and cadmium there is such a similarity, attained 90° without being kept in contact with the bar-magnet; but I see by a reference to the note taken at the time, that it was accomplished with some difficulty. It is remarkable that antimony was twice raised with the brass needle to 90° with great ease, though before it could not be got higher than 30° . In the same way antimony was last year made to connect itself at 90° with the copper needle, while now the cut end could only be got up to 40° , and the side to 55° , but this sort of capriciousness has occurred from time to time without any assignable cause.

2nd Class.

Selenium with M 10° ; I 75° ; P. G. $90^\circ+$; G. 18c. 30° ; P 20° ; Pl. 39° ; Cad. $90^\circ+$; T $90^\circ+$; L 35° ; Z 90° ; Cop. failed; B 63° ; Ars. failed; Gl. 90° , f.
Sulphur with M 28° ; I 39° ; P. G. $90^\circ+$; P 20° ; Pl. 65° ; Cad. 90° ; T 90° ; L $90^\circ\times$; Z $90^\circ+$; Cop. $90^\circ+$; B 65° ; Gl. 90° f.

What has already been said with regard to sulphur and selenium in the first part of this paper, leaves but little to be added. They will be found to show only a remarkable affinity for glass, but their contrast with lead is striking. The energy of both was heightened by being kept in contact with a magnet, and then tested with a brass needle. The preference that selenium shows for iron over the magnetic needle, is worthy of attention, as well as the contrast it stands in with sulphur, in its low attraction for lead.

3rd Class.

Diamond with M 65° ; I 90° , not very difficult; G 18c. 90° ; Pl. 90° , f. strong; Cad. 90° , not easy; T $90^\circ+$; Z 90° ; Cop. 90° , f. easy; B $90^\circ+$; Gl. 90° , f.
Ruby with M $90^\circ+$; Cad. $90^\circ+$; T $90^\circ+$.
Sapphire with M $90^\circ+$; Cad. $90^\circ+$; T $90^\circ+$.
Topaz with M 74° ; Cad. 75° ; T 68° .
Topaz (pale) with M 66° ; Cad. 83° ; T 80° .
Jargoon with M $90^\circ+$; Cad. $90^\circ+$; T $90^\circ+$.
Emerald with M $90^\circ+$; Cad. $90^\circ+$.

Amethyst with M 65° ; Pl. 90° , difficult; Cad. $90^\circ \times$; T $90^\circ \times$.

Opal with M failed; Cad. $90^\circ +$; T $90^\circ +$.

Cornelian (red) with M 35° ; I 90° , f. very strong; P. G. 90° , f. strong; T 90° , f. strong; Z 90° , f. strong; Cop. 90° , f. strong; Gl. 90° , f. very easy and strong.

Cornelian (white) with M 37° ; I 90° , f. easy; P. G. 90° , f. strong; T 90° , f. strong; Z 90° , f. strong; Cop. 90° , f. strong; Gl. 90° , f. very easy and strong.

Blood-stone with M 37° ; I 90° , f. very easy; P. G. 90° , f. very strong; T 90° , f. strong; Z 90° , f. strong; Cop. 90° , f. strong; Gl. 90° , f. very easy and strong.

Quartz with M 90° ; Pl. 90° , very easy; T 90° , f. strong; Cop. 90° , f. easy; B 90° , f. firm.

Flint with M 90° ; I 90° , very difficult; P. G. 90° , f.; Pl. 90° , not very firm; Cad. 90° ; T 45° ; L 90° , f. easy; B 90° , f.; Gl. 90° , f. very strong.

This class has been tested but to a limited extent with non-ferruginous needles, but the results are sufficiently plain to prove that they follow the same law as the preceding classes, and there can be no doubt from the indications afforded by what have been tried, that they would have exhibited those strong affinities, belonging to all substances of the vitreous class. A second specimen of diamond, which was a mere spark, was tested with the magnet and iron, but the results were the same as with the first specimen. I am indebted for many of these specimens, to Messrs. Widdowson and Veale of the Strand, who very obligingly afforded me the use of them.

4th Class.

Flint glass (prism, English) with M 30° ; P. G. 90° , f.; G 18c. 73° , difficult; P 90° , f.; Pl. 90° , f.; Cad. 90° , f.; T 90° , f.; L 90° , f.; Z 90° , f.; Cop. 90° , f.; B 90° , f.; Gl. 90° , f.

Flint glass (tumbler) with M $90^\circ +$; I 90° , f.; P. G. 90° , f.; G 18c. 90° ; P 90° , f.; Pl. 90° , f.; Cad. 90° , f.; T 90° , f.; L 90° , f.; Z 90° , f.; Cop. 90° , f.; B. 90° , f.; Gl. 90° , f.

Crown glass with M 90° ; I 90° , f. very easy and strong; P. G. 90° , f. not easy; G 18c. 90° , very easy; P 90° , f.; Pl. 90° , f.; Cad. 90° , f.; T 90° , f.; L 90° , f.; Z 90° , f.; Cop. 90° , f.; B 90° , f.; Ars. 90° , f. most easy.

Blue glass with M $90^\circ +$; I 90° , f.; P. G. $90^\circ +$; G 18c. 85° ; P 90° , f.; Pl. 90° , f.; Cad. 90° , f.; T 90° , f.; L 90° , f.; Z 90° , f.; Cop. 90° , f.; B 90° , f.; Germ. S. 90° ; Gl. 90° , f.

Green fancy glass with M 90° ; I failed; P. G. 90° , f.; G 18c. 70° ; P. 90° , f.; Pl. 90° , f.; Cad. 90° , f.; T 90° , f.;

L 90°, f.; Z 90°, f.; Cop. 90° f.; B 90°, f.; Germ. S. 90°, f.; Gl. 90°, very easy.

Red glass with P. G. 90°, f.; P. 90°, f.; Pl. 90°, f.; Cad. 90°, f. very strong; T 90°, f. very strong; L 90°, f.; Z 90°, f.; Germ. S. 90°, f.

Green bottle glass with M 70°; P 90°, f.; Pl. 90°, f.; Cad. 90°, f.; T 90°, f.; L 90°, f.; Z 90°, f.; Cop. 90°, f.; B 90°, f.; Gl. 90°, f.

Porcelain (French) with M 90°; P. G. 90°, f.; Pl. 90°; B 90°, f.; Gl. 90°, f.

Porcelain (dentist's) with M 74°; Pl. 90°, f.; L 90°, f.; Z 90°, f.

Blue enamel with M 36°; T 90° x.

Gypsum stone with M 26°; Gl. failed.

Alabaster with M 25°; Gl. 90° x.

Marble (gray) with M 55°; G 18c. 90°, difficult; T. 90°.

Marble (yellow) with M 10°; P. G. 90°+; G 18c. 56°; P 80°; Pl. 90°; Cad. 90°+; T 90°+; Z 47°; Gl. 73°.

Gas coal with M 15; Gl. 0°.

Iceland spar with M 90°; I 90°+; P. G. 90°, f.; G 18 c. 90°; S 90°, f. easy; P 90°, f.; Pl. 90°, f.; Cad. 90°, f.; T 90°, f.; L 90°+; Z 90°, f. easy; Cop. 90°, f. firm; B 90°, f. strong; Gl. 90°, f.

Felspar with M 10°; Cad. 30°; T 45°; Gl. 90°, f. very firm.

Selenite with M 15°; P. G. 86°; T 85°; Gl. 90°+.

Talc with M. 60°; I 90°+; G 18c. 90°; Z 40°.

Protosulphate of iron with M 90°, f. very easy; P. G. 90°, f. very easy; Gl. 90°, f. very easy and strong.

Carbonate of iron with M 16°; I 20°; P. G. 90°, f. rather difficult; S 90°, f. firm; P failed; Pl. failed; Cad. 90°, f. firm; T 90°, f. firm; L failed; Z 90°, f. most firm; Cop. 14°; B 14°; Germ. S. 9°; Gl. 90°+.

Sulphate of copper with Gl. 90°, f. easy and strong.

Crystallized tin with M failed; I 90°, f.; P 90°, not difficult; Pl. 90°, difficult; L 90°, f.; Z 90°, f. contact instantaneous; Cop. 90°, f.; B 90°, f. strong; Germ. S. 90°; Gl. 90°, f. contact instantaneous.

Alum with M 15°; Gl. 90°, f.

Rock salt with Gl. 90°, f. very strong.

Sugar candy (white) with P. G. 90°+; T 90°+; Gl. 90° f.

Sugar candy (brown) with Gl. 90° f.

In this class it will be seen that iron in many cases maintains a great superiority over the magnet. The glasses seemed

to deserve particular attention, and have been carefully tested. Their strong attraction for non-ferruginous bodies must prove interesting, and Iceland spar vies with them in that property, for the needles had only generally to be pushed against it to form the connexion. The porcelains exceed even the glasses in their affinities for non-ferruginous bodies. The attraction of glass for sugar candy, rock salt and alum, is worthy of attention. The metals were not tried, for want of time, with the exception of gold and tin.

The case of carbonate of iron is very remarkable, for it might *à priori* be supposed that from the large proportion of iron it contains, it would show considerable attraction for the magnetic needle; but this is so far from being the case, that it will be seen to be one of the lowest in the scale, and after being kept eighteen hours in contact with the N. pole of a horse-shoe magnet capable of raising thirty pounds, its affinity for the magnetic needle was not increased. It might also have been expected, from its great density, and the analogy of similar cases, that in consequence of its feeble attraction for the magnet, it would exhibit a strong affinity for glass. It however is otherwise; and by a reference to all the metals with which it was tested, in consequence of these peculiarities, it will be seen to be one of the most remarkable and interesting substances that have been tried, exhibiting a very low state of attraction for almost all bodies, gold, silver, cadmium, tin and zinc being excepted, and for these metals it exhibits a remarkable affinity. The results of many of the experiments on various bodies will show a kind of elective affinity, as is observed in chemical combinations; and no substance exemplifies this remark so completely as carbonate of iron. Thus though it has the strongest affinity for zinc, it does not show an attraction for brass above what it exhibits for copper, which is as low as 14° . Yet most bodies have a surprising affinity for brass. Carbonate of iron affords a fresh instance that the iron in a body may be quite inert, in consequence of its combination with another substance neutralizing its affinities.

The low affinity of the magnetic needle for marbles is quite in accordance with the experiments of Dr. Seebeck, and the numbers he obtained for bismuth, platinum and antimony, bear a singular resemblance to those afforded by the present experiments. There may be the same accordance between the others, but there is no means of marking by the present mode, the nice shades of difference after 90° . Silver, which he places next to iron in power, seems to take the place of brass, which I find to be generally singularly magnetic.

Amongst the alloys that become neutral by combination, he mentions copper and nickel; now this alloy, which is popularly called German silver, I believe, I found to be so magnetic as to draw the needle after it; but then the proportions of the two alloys most probably differed; his specimen contained two parts of copper and one of nickel, while mine, as I have already remarked, seemed to have an unusual proportion of copper, to judge from its colour.

5th Class.

- Nacre with M 90° , f. firm; I 90° , f.; P. G. 90° , f. easy; Pl. 90° , f. not very easy; T 90° , f. firm and easy; L 90° , f. firm; Z 90° , f. most easy; Cop. 90° , f. firm; B 90° , f. firm; Ars. 90° , f. most easy; Gl. 90° , f. most easy.
- Ivory (old) with M 58° ; T 48° ; Gl. 58° .
- Ivory (new) with M 90° +; T 90° +; B 90° , f. firm.
- Enamel (human tooth) with T 90° .
- Bone (mackerel's) with M 90° ; Cad. 90° .
- Whalebone with M 90° ; I 90° ; Cad. 90° +.
- Horn (buffalo's) with M 90° ; I 90° ; P. G. 90° +; Cad. 90° +; B 90° , f. very strong.
- Horn (cow's) with M 85° ; I 90° ; P. G. 90° +; Cad. 90° +; B 90° , f. not very firm.
- Horn (chamois') with M 90° ; B 90° , f. not very firm.
- Albumen with Gl. 90° f., strong.
- Indian ink with M 70° ; Cad. 90° ; L 90° +.
- Amber with M 75° ; Cad. 90° ; T 90° .
- Gas coal with M 15° ; Gl. 0.
- Glue with M 29° ; I 90° +; P. G. 90° , difficult; P. 90° +; Pl. 90° +; Cad. 90° +; T 90° +; Z 90° +; Gl. 90° +?
- Pig's bristle with T failed.
- Cat's whisker with M 90° x; I 90° x; G 18c. 90° x; Cad. 90° x; T 90° x; L 90° x; Z 90° , difficult; Cop. 90° x.
- Human hair (brown, male's) with M 90° ; T 90° .
- Human hair (female's) with M 80° ; T 90° +.
- Human hair (female's, 2nd specimen) with M 76° ; T 90° +.
- Human hair (gray, male's) with M 90° +; T 50° .
- Human hair (gray, female's) with M 90° +; T 90° .
- Human hair (beard, gray) with M 90° +; Cad. 90° .
- Horsehair (gray) with M 90° ; T 90° .
- Horsehair (black) with M 90° ; T 90° .

In this class of animal products the magnetism of the needle is tolerably strong, while that of the other bodies is still more remarkable. The specimen of ivory (a paper-cutter), which could not be less than thirty old years since the time of its importation, only exhibited 58° , but was raised to 74° by contact

with a magnet; while the second specimen, which was comparatively recent, as was evident from its fine colour, gave 90° with the magnetic needle; and the probable cause of this difference will be shortly seen, when I speak of the results obtained with a needle made out of a part of this very piece of ivory. Glue exhibits phenomena of a very interesting character. Its affinity for the magnet was only 29° , after the most painful efforts were repeatedly made to push it further, but on being left in contact with the bar-magnet for some hours, it instantly attached itself at 38° . Its affinity for the iron was $90^\circ +$, and it exhibited the same amount of attraction for pure gold, platinum, cadmium, tin, zinc and arsenic, which were all the metals it was tested with. But I have some doubts about all the high measurements, as I fear that the dampness of the weather may have influenced the results, though the different specimens appeared perfectly hard.

7th Class.

Deal wood with M 30° ; T 30° ; B 90° , f.

Box wood with T 15° .

Rose wood with T 90° .

Oak with I 90° , very difficult; B 90° , f.

Lignum vitæ with M 33° ; I 18° ; Cad. 22° ; T. 90° ; L 90° .

Cedar wood with M failed; B $90^\circ +$.

Vegetable ivory with M 56° ; I 67° ; G 18c. 90° ; Cad. $90^\circ +$; T 90° , very difficult.

Vegetable ivory (2nd specimen) with M failed; I 50° ; P. G. 90° , f. firm; L 80° ; Z $90^\circ +$; Cop. $90^\circ +$; Gl. 90° , f. strong.

Vegetable ivory (3rd specimen) with M failed; I 70° .

Mahogany with B 90° , f.; Gl. 24° .

Lac (pure) with M 35° ; P. G. 90° ; G 18c. 74° ; Cad. 90° ; T $90^\circ +$; L 90° ; Cop. 65° ; Gl. 90° f.

Sealing-wax (black) with M 25° ; I failed; T 70° .

India rubber with M 80° ; Cad. $90^\circ +$.

India rubber (bottle) with P. G. 70° ; B $90^\circ +$; Gl. 90° , f.

Resin with M 90° ; T $90^\circ \times$; Gl. 48° .

Gum with Gl. $90^\circ +$.

Macaroni with Gl. 40° .

Dry paste on paper with M failed; Gl. 23° .

The peculiarity connected with the attraction of lignum vitæ and vegetable ivory for the magnet and other substances is remarkable. The lignum vitæ was raised from 33° to 49° by contact for a short time with a magnet, and the same process raised it from 18° to 30° with iron, while with tin and lead it had exhibited 90° , and only 22° are marked for cad-

mium. Three specimens of vegetable ivory were tried on many occasions with great care, and the results they afforded were always uniform, but differed from one another. The first specimen gave 56° with the magnetic needle, but the two others failed altogether. On two or three occasions the magnetic needle connected itself with one of the specimens that had previously failed, after it had been kept in contact with a magnet; but I cannot positively say that the magnet was the cause, as on repeating the same experiment on other occasions, no such result took place; still the needle did not connect itself till after the attempt to raise its magnetism, though many trials had previously been made. The attraction of iron for all three specimens is deserving of notice. This singular substance exhibited a strong affinity for all the needles with which it was tested. The affinity of lac for various metals and substances, including glass, is worthy of attention. Sealing-wax and India rubber of the bottle kind exhibit a strong affinity for many non-ferruginous bodies.

I have thought that it would be useful to sum up all the preceding results, in such a way as to exhibit their relative affinities; and with this view they have been thrown into the following tabular form, which though it does not contrast cases altogether analogous, because the substances the needles were tried with were not always the same, still some general inferences may be drawn from them, not devoid of interest.

Results of the Measurements.

Substances.	90° and above.	Under 90° .	Failed.
Magnet.....	51	54	10
Iron.....	25	8	3
Pure gold.....	24	3	0
Gold, 18 carats...	12	5	0
Silver.....	5	1	1
Palladium.....	13	6	1
Platinum.....	21	7	3
Cadmium.....	33	7	0
Tin.....	43	9	2
Lead.....	21	4	1
Zinc.....	20	3	0
Copper.....	23	3	0
Brass.....	30	2	0
Glass.....	38	5	2
Total.....	360	117	23

If the relative affinities thus elicited could be depended upon, the substances would be arranged as follows, and their

relative intensities would be according to the ratio assigned to each:—

Brass	16·00	Lead.....	5·144
Pure gold.....	9·00	Tin	4·909
Copper.....	8·333	Iron.....	3·750
Zinc.....	7·666	Gold, 18 carats	3·400
Glass	6·428	Platinum.....	3·100
Silver.....	6·666	Palladium.....	2·857
Cadmium.....	5·714	Magnet.....	1·640

To judge by the facility with which the needles attached themselves, brass is undoubtedly worthy of the high place it maintains in the preceding list; but I should certainly say from my own convictions, that glass is much stronger in its adhesions than even pure gold, and perhaps equal to brass. The advantage that iron has over the magnetic needle is conspicuous, and is deserving of attention, though the ratio of the magnet might very fairly be raised to 2·00, for it is in part owing to the experiments with the larger range of substances, that it stands so low.

The preceding experiments include a vast variety of substances in the mineral, vegetable, and animal kingdoms, that exhibit such strong attractive affinities for one another, that however much they may differ in their external appearances, and in their very natures, they are bound together by common bonds that connect them all into a single family; for we find the metal attaching itself to crystalline, animal and vegetable substances; and again, the crystal, whether we call it by the name of diamond, salt, or sugar-candy, connecting itself readily to metallic, animal and vegetable bodies. In a similar way animal bodies attach themselves to those that are mineral and vegetable; and to complete the circle, the vegetable kingdom, by its woods, its gums, its lac and its resins, is connected with them all.

Out of about 600 measurements the proportion of those that have exhibited repulsion, or what may be presumed repulsion, for there is only negative proof on the subject, is about 1 in 25. Could a counter system of measurements be established it is possible this might be reversed, and that the same amount of repulsions would be obtained, and this perhaps is the fact in Dr. Faraday's remarkable experiments, owing to the influence of the electro-magnet. Still the facility with which the majority of these connexions was established, can hardly lead to a doubt that they are purely of a normal character, where the magnetic needle is not concerned, because they can only result from the spontaneous effects of

nature; but in the case where the magnetic needle has been the coercing agent, and the measures indicate low intensities, and where the connexion was made with much difficulty, I should draw a contrary inference. This view is strongly supported by what took place with the cornelians.

The two extremes of the preceding scale give two alloyed metals as the highest and the lowest, namely, brass and the magnet. In seeking for two *unmixed* bodies that should properly represent the natural scale in the mineral kingdom, that is, one in which the magnetic needle is not arbitrarily assumed as the standard of reference, but one that considers the natural classes of substances, I would prefer taking platinum and diamond, and exclude brass and the magnet. The first would take the lead of all metals, namely *metalloids*, while diamond would be placed at the head of all substances of the vitreous class, or *haloids*, that is to say, all the precious stones and all crystals; platinum as the heaviest, and diamond as the hardest of all known bodies. The class of metalloids will be as naturally closed by potassium, as it will be seen the haloids are by gum, two of the lightest of the metallic and vitreous classes of bodies. The reason for adopting this classification is, that I find that iron, from which the magnet is formed, has no remarkable affinity for other bodies, nor for itself, and that the magnet, constituting an exception to the *rule* of nature, is itself dependent for its exalted effects upon electricity and other extraneous causes, and could only keep its high position while attractive affinity, by our ignorance of natural laws, was supposed to be exclusively its peculiar property. Such a division is by no means meant to undervalue the importance of the magnet; for though it has led to the discovery of the affinities of all the rest, there can be no doubt it exists by an exceptional law; and we might as well place iron at the head of all metals, because we are indebted to it for the implements which have enabled us to penetrate to those very metals. The term magnetism therefore will always maintain its ground as the general expression of the magnetic attractions which connect all bodies together, even though they have nothing to do with iron; in the same way that the term electricity has been extended to all those phænomena in which attraction or repulsion result from friction, and yet having not the slightest resemblance to the *electron* or amber that led to their discovery.

In the mineral kingdom, metallic and crystalline bodies form the mass of its constituents, and they generally stand clearly contrasted with one another; yet platinum as well as diamond, and all vitreous substances, have a remarkable affi-

nity for other bodies; and if we fix upon a metallic and vitreous type, no two substances are more entitled to be considered as the chiefs of their respective classes; and if platinum had not been known, gold would have taken its place; when those two precious bodies, gold and diamond, that have been so long conjoined in the associations of mankind, would still have maintained their rank.

Such are the views I am led to entertain of universal magnetism, that is of magnetism, cohesion, adhesion, and viscosity. And what is viscosity but the employment of a watery vehicle, which is afterwards to disappear, partly through the effects of heat, but mainly through its greater affinity for air and other substances which are said to absorb it, if inwards, by vessels, through what is called *endosmosis*, and if outwards, *exosmosis*? This fluid helps to hold together gelatinous, albuminous and glutinous substances, in such a manner as to bring all their particles into close connexion with themselves, as well as with the bodies to which they are to be attached, and thus by expelling air from between them all, brings every portion into close contact, when, the fluid part having disappeared, the whole is converted by exsiccation into a concrete mass through the power of the affinities of which we have just had so many examples. In the same manner bodies like lac, resin, pitch, &c., that only require the application of heat to become liquid, are enabled to mould themselves to the inequalities of the bodies upon which they are dropped, and thus making counter-impressions connect themselves with a force proportioned to their surfaces, that is to say, of the collection of points of which these are composed. What too is the water that generally forms the vehicle in the former cases, but a crystal or salt called ice (oxide of hydrogen) liquefied by heat, and of such universal strength of affinity while fluid, as to connect itself with nearly every body it touches, which it is then said popularly to wet, and that too in a degree always in proportion to the quantity of caloric it contains at the moment, those substances being excepted between which and it there exists such a decided repugnance, that they exhibit a repulsion that nothing can overcome, but the union of a third body that neutralizes both, and which we see exemplified in *water, grease, and soap*? Is it desired to strengthen the adhesiveness of gelatinous and albuminous bodies? we then have recourse to a fluid still more powerful than water, to one that has a still stronger affinity for these substances, and we substitute alcohol, the result being a compound of such tenacity that it is considered as a *cement*; and such is the affinity of alcohol for other bodies that it is employed to clear away, or

to speak more philosophically, to dissolve the film of particles that covers object-glasses and mirrors so rapidly, owing to the affinity or magnetism that all bodies possess for one another, and which the astronomer and the optician unconsciously heighten by the very means to which they have recourse, as the particles that float in the air again attach themselves with increased facility by the use of the alcohol. In a similar, but reverse sense, water will not attach itself to many metals, but if they are kept in a horizontal position, the fluid, in its attempt to shrink away, will collect itself up into little drops as if the surface were greasy, and it is only prevented from flying away by its density, which condemns it to obey the law of gravitation. This repugnance of water for metals I consider to be the main cause why dew does not settle upon them, when neighbouring bodies are completely covered by it.

I had arrived at the preceding convictions, when a discovery I made excited in me a degree of surprise that will, I doubt not, be felt by others. Having formed a needle of the fresh specimen of ivory which I have already mentioned, I was tempted some days afterwards, by a movement almost of idle curiosity, as I had just succeeded with one of sealing-wax against glass, to test it also with the same substance, when I found that by a mere push of the finger, it attached itself firmly to it. In consequence of this most unexpected result, the ivory needle was tried with all the following substances, and it succeeded in every case at 90° by the mere use of the finger, except with those preceded by an asterisk, for which the magnet was obliged to be employed, and therefore their intensities must be considered a shade lower than those of the others, as well as a few that were lower still, but which have their measurements assigned.

Gold.	Cornelian.	Rock salt.
Silver.	Flint.	Horn (cow's).
Palladium.	Marble.	Ditto (buffalo's).
Platinum.	Alabaster.	*Ivory (old).
Cadmium.	Flint glass.	Ditto (new).
Tin, 49°.	Crown glass.	Albumen.
Bismuth, 76°.	Plaster of Paris (stone)	Gluc.
Lead, 81°.	37°.	Nacre.
*Zinc.	Stone-ware.	Vegetable ivory.
Copper.	Porcelain (French).	Ditto (2nd specimen).
Brass.	Ditto (dentist's).	Indian ink.
Bell-metal.	*Felspar.	Lac.
German silver.	*Sulphate of copper.	Resin, 75°.
Sulphur.	*Carbonate of iron.	*Gum.
Selenium.	Crystallized tin.	
Diamond.	Iceland spar.	
Opal.	Talc.	

The ivory needle would not connect itself with gypsum, arsenic and antimony, and indeed when the coercing magnet

was withdrawn from the latter, the needle seemed always to set off with more than usual haste, as if strongly repelled, but no symptom of repulsion was exhibited when the ivory was brought near the magnetic needle standing in repose.

Here was a curious problem to be solved. What was the cause of this extraordinary affinity of a substance like ivory for so many heterogeneous bodies, of which porcelain offered the most remarkable instance, as the specimen employed, which stood on a surface of glass, drew the ivory as much as one-fourth of an inch after it on one occasion, and always some small distance when it was merely pushed away? The diamond exhibited the same phenomenon, but in a minor degree, and careful testing proved that electricity in no way contributed to the results.

On referring to the tabulated measurements of which I have spoken, and in which the measures of substances stand in ready contrast, I observed that the needles had attached themselves with remarkable facility to animal bodies, such as ivory, pearl, mother-of-pearl, horn, hair, glue, &c. It was evident, therefore, that it was the affinity of something common to all these substances, that caused such easy contact between them and the needles. This step gained it was not difficult to discover that the gelatine which was common to all these animal products, was the substance that chiefly produced with equal facility a contact with gold, diamond, Iceland spar, marble, alabaster, horn, vegetable ivory, gum, and such dissimilar substances, in the mineral, vegetable and animal kingdoms. The low attraction afforded by the piece of old ivory of which I have spoken, I suppose to have resulted from partial change in its structure through age; and the same cause may be assigned for the difference exhibited by the pearl and the mother-of-pearl in their measures, for from the colour and the style of setting of the pearl, less than fifty years could not be assigned for its workmanship.

I have already alluded to the facility with which all vitreous bodies form connexions, and to include the present class of substances in their proper category, we must get rid a little of our familiar notions, and remember that if gelatine, albumen, gluten, resin, lac and gums are devoted to particular uses that make us only think of them as means of adhesion, they are in fact, when considered under a more extended point of view, animal and vegetable glasses; and what fully establishes this character is the extraordinary fact which I have already mentioned, that glue was raised to 38° by contact with a bar-magnet; but in truth since that portion of this communication was despatched for the press, the piece of glue was raised to

78° by being kept in contact for four-and-twenty hours with the great horse-shoe magnet already spoken of, and the magnetic needle attached itself with singular readiness, contrary to what had been previously found to be the case. The whole effect however *gradually* disappeared, the attractive power declining, as was ascertained by successive measurements, till it fell back to its normal state of 28°, in about twenty minutes. It was by mere accident, as may be supposed, that it was kept so long in contact, and most likely so long a time is not necessary to produce the effect*. This result is well-worthy of being contrasted with what has been said about carbonate of iron, and it will be admitted, I am sure, that two cases less reconcilable with analogy could scarcely be produced. We thus see that gelatine, and in all probability similar substances, conform to the laws that have been seen to prevail in vitreous bodies, and it is only necessary to observe their fractured edges when hard, and particularly those of glue, dried albumen and resin, to be convinced of this fact. Even India rubber when in a low temperature approaches to the same state, and it has been already noted that several needles attached themselves firmly to it. To be convinced that this view, which connects these substances with the glasses, is not fanciful, it is only necessary to call to mind the case of gelatinous hydrate of silicium, which is obtained from a mineral glass that dissolves in water; and it is not amongst the least remarkable of the analogies that exist throughout nature, to connect the *liquor of flints* with broth, white of egg, gum water and paste, and the last of these therefore, by a series of links, with the mysterious powers of the magnet, that caused St. Augustin to faint, and made Thales to believe that it was animated by a soul!

To be quite convinced of the strong affinities that ivory, mother-of-pearl, horn, albumen, tortoise-shell and quill exhibit in common, I formed needles of these substances, and tested them to an extent that left no doubt that they possess the same qualities as ivory; but on the whole the quill and mother-of-pearl approached nearest to a state of equality with that substance, though there was a very slight difference between them all. These fresh proofs leave therefore no

* It has since however been ascertained that length of time was essentially the cause of raising the magnetism of the glue, for on keeping it subsequently in contact for thirteen hours, its magnetism could only be raised to 49°. Another very important point was likewise fully established, namely, that either pole of a magnet might be used without the glue showing any signs of polarity, for when 78° were obtained it had been kept in contact with the N. end of the magnet, and when 49° was the amount of measurement the S. end was employed, while in each case it was the S. end of the needle that formed the connexion.

doubt that it is owing mainly to the gelatine they contain, that these needles show such a remarkable degree of attraction for nearly all bodies.

I do not mean to say that all cohesion is purely *magnetical* in the extended sense I have given to the term, for such an opinion is scarcely reconcilable with what is observed in chemical combinations, where we see elements changing places according to what is called elective affinity. In the various theories that have been put forth to account for chemical combinations and the cohesion of bodies, the attractions and repulsions of different electrical states have been proposed by the celebrated Berzelius; but though this might, as a theory, obtain currency amongst scientific men, and enable them to reason with facility, as long as acids and their bases are considered, yet it is clearly in fault the moment it is applied to explain the cohesion of simple bodies, such as the pure metals. Many instances may be observed in the preceding experiments, that favour the notion of the existence of elective affinity between the various bodies that have been experimented upon, but to which *immediate* electric agency can with difficulty be conceded; and we must therefore have recourse in such cases to what are called *the laws of nature* for their explanation. With regard however to the combinations of acids and alkalis, I believe the varying preferences they exhibit when brought near one another to result from electricity, though magnetism is not without its share in effecting their union; and I hope shortly to be able to present some considerations that will render this opinion highly probable, by making known a discovery I have made of the antagonism of heat and electricity, as well as the proofs of a single electric fluid.

I cannot take leave of this subject without expressing my unfeigned admiration for the skill exhibited by Dr. Faraday, in his experiments on the same subject, as well as on so many other occasions; and if we differ in our views of magnetical phenomena, it will be easily accounted for, by the opposite results we have obtained, owing to our methods of experimenting, and the greater range and variety of observations that the present one has given birth to, over those recorded by that eminent experimenter.

I hope that the state of my health, which has compelled me to dictate this communication, and which has prevented me from giving it the benefit of after-perusal, will be accepted as a sufficient excuse for any oversights that may be observed.

Paris, 15, Rue de Tournon,
19th May, 1847.

LXXVIII. *Intelligence and Miscellaneous Articles.*

ON CAROTIN.

M. ZEISE observes that the peculiar matter of the *Daucus carota* has not hitherto been known in a pure state; for by the process usually employed for its extraction, which consists in treating the juice coagulated by æther, its evaporation, and treating the residue with alcohol or with ammonia, the carotin is obtained always combined with a large quantity of fat oil, and the product is merely a soft mass, containing crystals of carotin. M. Zeise has tried several processes to obtain it in a pure state, and has succeeded by the following:—

The expressed juice of the roots is to be diluted with four or five volumes of water, and to the mixture is to be added sulphuric acid diluted with ten volumes of water. A very slight addition of this acid occasions the precipitation of the whole of the colouring matter of the juice. The clear liquor is to be poured off, and the mass thus obtained is to be washed with water; after this it is to be boiled for an hour, or an hour and a half, with a strong solution of hydrate of potash: the potash saponifies the fat oil, and dissolves a residue of albumen without acting upon the carotin. The carotin is to be separated by filtration and washed with water.

Thus obtained, the carotin still contains a saline body, which must be decomposed to prevent its admixture with the carotin in the remaining operation. For this purpose, the mass diffused through water and heated is to be treated with excess of dilute sulphuric acid. After having again collected the carotin on a filter and well-washed it, the fatty acid is to be separated by alcohol, at first diluted, but eventually nearly anhydrous. By this process very little carotin is dissolved. The mass is then to be dried by the water-bath.

When perfectly free from oil, carotin is difficultly soluble in æther; but in order to complete its purification, M. Zeise has taken advantage of a property of carotin, discovered on this occasion, which is, that it is extremely soluble in sulphuret of carbon. When the well-dried mass is treated with this liquid, a solution of a deep red colour is obtained, which is to be separated from a grayish substance which remains undissolved. After having separated nearly three-fourths of the solvent by distillation, anhydrous alcohol is to be added to the residue. When the mixture is left in an open capsule, the carotin soon separates in the form of small, deep red-coloured crystals. The mother-water is to be decanted, and the crystals are to be washed with anhydrous alcohol on a filter, till it comes away nearly colourless. The carotin is then perfectly pure; and its appearance is somewhat similar to that of coarsely powdered cinnabar, or to that of copper, reduced from the oxide by hydrogen. The crystals are very small, and have a strongly metallic lustre. Carotin has a very slight but somewhat agreeable odour; it is heavier than water, and perfectly insoluble in it; nearly insoluble in alcohol, and also in pyroxylic spirit, when it is well-freed from oil; it is very slightly

soluble in æther and in acetone. It fuses at about 334° F. into a deep red-coloured and very transparent liquid, and on cooling it becomes a vitreous mass. After having been fused, it dissolves readily in æther and in alcohol; but these solutions, when evaporated, yield merely amorphous masses. At about 544° F. it carbonizes, yielding a small quantity of an oily body, and very little gas. When heated in the air, it burns with flame, and leaves no trace of residue.

Dry chlorine does not act upon dry carotin; but water saturated with chlorine converts it into a perfectly colourless chlorinated body, which is insoluble in water, but is considerably soluble in alcohol and æther, and very much so in sulphuret of carbon. The solutions, when evaporated, yield merely a friable resinous mass.

By elementary analysis, carotin appears to be a compound of carbon and hydrogen in the atomic relation : $10(C^9 H^8)$. It is therefore a polymeric of oil of turpentine, a circumstance which is of unquestionable interest.—*Ann. de Ch. et de Phys.*, Mai 1847.

ON THE PREPARATION OF IODIDE OF LEAD.

M. Boudet observes that M. Depaire, pharmacien of Brussels, has stated that when iodide of lead is prepared by the reaction of acetate of lead and iodide of potassium, the supernatant liquor contains a considerable quantity of iodine. He found that this liquor, in which acetate of lead produced no turbidness, gave a yellow precipitate of iodide of lead on the addition of a little nitric acid, a precipitate of trisiodide of lead with trisacetate, and free iodine with sulphuric and hydrochloric acids; and submitted to evaporation, leaves a white residue, which on the addition of water, regenerates iodide of lead.

From these facts M. Depaire concluded, that in employing acetate of lead to extract iodine from iodine baths, loss of iodine was sustained; to avoid which, he proposes, as M. Cottereau, jun. had previously done, to employ the trisacetate of lead, which precipitates the iodine perfectly.

M. Depaire afterwards examined in what state the iodine could be retained in the solution from which the iodide of lead was precipitated; and after having ascertained that it was not in a state of simple solution, he appeared disposed to think that it exists in the state of potassio-iodide of lead.

M. Boudet states that experiments, which he performed four years ago, but which he neglected to publish, confirm the remarks of M. Depaire; and he is enabled to add some observations, which, he thinks, will not be uninteresting.

Having ascertained that in preparing iodide of lead with acetate and excess of acid, the weight of the product obtained was less than indicated by theory, to the amount of about 10 per cent., M. Boudet added some nitric acid to the solutions which ceased to precipitate with acetate, and he obtained an additional yellow precipitate of iodide of lead. This suggested the employment of nitrate instead of acetate of lead in preparing the iodide; and on trial it was found to yield such a product as theory would indicate, minus the

iodide dissolved. The use of nitrate of lead appears to have been preferred by several chemists; and M. Baudrimont expressly recommends it, on the ground that the acetate yields some oxiodide.

This first point being established, it remained to explain the loss of the 10 per cent. occurring by the use of acetate of lead; and presuming that the acetate of potash, formed with the iodide of lead, took some part in this phenomenon, the action of this salt on iodide of lead was examined, and the results were as follows:—

When iodide of lead is triturated with acetate of potash, the mixture becomes rapidly moist, gradually loses its colour, and soon becomes perfectly white. This pasty mixture does not act upon litmus paper and pulverized acetate of lead. When exposed to the air it partially liquefies, and partially crystallizes in silky needles of double iodide of potassium and lead: on contact with water it becomes immediately yellow, reviving yellow iodide, and the liquor becomes acid; and when filtered it yields a pale yellow precipitate with triacetate of lead, and regenerates bright yellow iodide, when nitric acid, or dilute sulphuric acid is added. By evaporation it leaves a white deliquescent residue, which regenerates yellow iodide on contact with water. If, instead of water, alcohol be added to the pasty mixture of acetate of potash and iodide of lead, it partly dissolves; the residue, which is almost white, yields yellow iodide on contact with a weak acid; and the supernatant liquor does the same on the addition of nitric acid.

When, on the other hand, iodide of lead is treated with a hot solution of acetate of lead, a colourless acid liquor is obtained, which yields white silky crystals of double iodide of potassium and lead, and a yellowish-white insoluble residue of oxiodide of lead.—*Journ. de Ph. et de Ch.*, Avril 1847.

[*Note.*—A parallel case occurs when acetate of lead is decomposed by chloride of sodium, as will appear by the following remarks in the last edition of my translation of the *Pharmacopœia*, p. 280. “I have lately found, however, that the decomposition is not complete. A double salt is formed, the nature of which I have not yet examined, but which is to a considerable extent soluble in water. Hydrochloric acid occasions the precipitation of more chloride of lead after the action of the chloride of sodium is over.”—R. P.]

ON THE ACTION OF BROMINE UPON CERTAIN SALTS.

BY M. AUGUSTE CAHOURS.

Alkaline oxalates.—When bromine is gradually dropped into a solution of oxalate of potash or soda, containing a slight excess of alkali, no action ensues if the temperature be low; but if it be raised to 104° or 122° F., action immediately commences, and becomes very vivid; much gas is disengaged, which extinguishes flame, yields a white precipitate with lime-water, and is totally absorbed by a solution of potash; thus possessing all the properties of pure carbonic acid. The liquor contains the bromide of potassium or sodium, which may be obtained in fine crystals by spontaneous evaporation. The oxalates of barytes, lead and silver, are similarly acted upon.

The action is very simple. The bromine combines with the metal of the oxide, while the oxygen of the latter acts upon the oxalic acid to form carbonic acid; the result being $C^+O^3, MO + Br^2 = MBr^2 + 2C^2O^2$.

Action of Bromine on Acetates.—When bromine is poured into a solution of acetate of potash or of soda, no reaction takes place, nor is there any increase of temperature; the only observable phenomenon is the solution of a considerable quantity of bromine. The liquor is gradually decolorized during spontaneous evaporation, owing to the disengagement of bromine, accompanied with the odour of the hypobromites; if it be heated the decolorization is more rapid. The liquid when evaporated to dryness yields a residue which is almost entirely soluble in alcohol. The alcoholic liquor, when exposed to a gentle heat, leaves pure acetate of potash.

The modes of action of bromine upon alkaline oxalates and acetates are so distinct, that it is evident, if citric and tartaric acids contained oxalic and acetic acid, the former would be entirely converted into carbonic acid, while the latter would remain unacted upon, unless in the nascent state in the presence of bromine, it formed new derivative products.

Action of Bromine on Citrate of Potash.—When bromine is gradually added to a concentrated solution of citrate of potash, it soon disappears, and the liquid becomes hot, accompanied with the brisk effervescence of pure carbonic acid.

Bromine is to be added till effervescence ceases, and the liquor becomes of a permanent reddish tint; if the excess of bromine be then removed by the cautious addition of a dilute solution of potash, an oily, colourless, very heavy and fluid liquid is precipitated, the aromatic odour of which resembles that of bromoform.

This liquid is extremely complex, and contains three distinct substances: the most volatile is easily obtained in a pure state; it is bromoform. The least volatile product is solid and crystallizable; it is partly altered by heat, and is evidently a new substance. The third compound, which arises during the reaction, is formed only in very small proportion, and it is impossible to deprive it entirely of bromoform or the crystallized substance.

In order to separate these substances, the rough oil is first to be washed with water to deprive it entirely of bromide of potassium; the mixture of oil and water is then to be submitted to careful distillation, stopping it as soon as but few small drops only of oil pass over with the vapour of water. This first liquid is separated from the water by decantation, dried over fused chloride of calcium, and then distilled alone, as long as its boiling-point remains constant.

The product thus purified is a colourless liquid of an agreeable aromatic and somewhat intoxicating odour; its sp. gr. is 2.90 at $54^\circ F.$; it boils at $305^\circ F.$ When boiled with a concentrated solution of potash it is decomposed, giving rise to the formation of chloride [bromide] of potassium and formiate of potash.

It appeared to consist of, in 100 parts,—

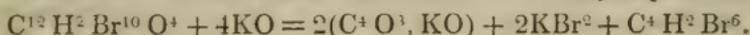
C ⁴	4.85
H ²	0.40
Br ⁶	94.75
	<hr/> 100.00

This product possesses therefore the properties and composition of bromoform; the density of its vapour was by experiment 8.632; calculation would give 8.55, on the supposition that the molecule of this compound yields 4 volumes of vapour.

When the distillation of the rough oil with water is continued, a period arrives at which the vapour, on condensing, deposits only a few oleaginous drops; the operation must then be stopped. The oily matter which remains at the bottom of the distilling apparatus concretes on cooling and assumes a crystalline appearance.

To purify this product, it is first washed with water, and then dissolved in boiling alcohol; the solution on cooling deposits long, brilliant, white silky needles: if, on the contrary, excess of alcohol be employed, and the solution be exposed to spontaneous evaporation, large colourless tabular crystals are obtained. The product thus purified is insoluble in water, either cold or hot; alcohol, when it is concentrated, dissolves it in considerable quantity when heated; on cooling the greater portion is obtained in very regular crystals; æther and pyroxylic spirit also dissolve it.

This substance fuses at about 167° F.: when subjected to distillation it is partly altered with the disengagement of bromine; but a considerable portion distils, and condenses on the cool parts of the retort, in long very brilliant needles. A cold concentrated solution of potash dissolves it; when heated complete decomposition occurs. When the operation is over, the retort is found to contain bromide of potassium and oxalate of potash; while the vapour of water carries off an oily aromatic matter which possesses the composition of bromoform. The reaction which occurs is readily explained,—

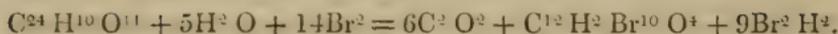


Concentrated sulphuric acid gently heated dissolves a small quantity; but if heated to ebullition the substance is altered, vapour of bromine and an oily substance being disengaged. Moderately strong nitric acid, when cold, does not alter it; but when heated a small quantity is dissolved, which is deposited on cooling in the form of fine, white, brilliant needles; the portion which does not dissolve assumes the appearance of an oil, and again concretes on cooling.

By analysis this substance appeared to consist of—

C ¹²	7.67
H ²	0.21
Br ¹⁰	85.30
O ¹	6.82
	<hr/> 100.00

The formation of this product by means of solution of citrate of potash may be explained by means of the following equation,—



On account of the decomposition of this substance by the alkalis the author proposes to call it *bromoxiform*. In this operation there is then first a case of oxidizement which resolves the molecule of citric acid into more simple products; then the bromine acting by itself on one of the products formed, gives rise to a phænomenon of substitution.

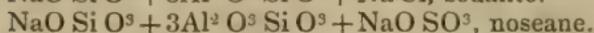
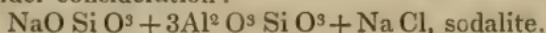
On observing the preceding product so readily decomposed on the presence of bases and of water, may it not be admitted that this compound is first formed, and that the bromoform is merely the result of a secondary action?

The citrates of soda and barytes act in the same manner as the citrate of potash, bromoform and the crystalline matter being both obtained with them.—*Ann. de Ch. et de Ph.*, Avril 1847.

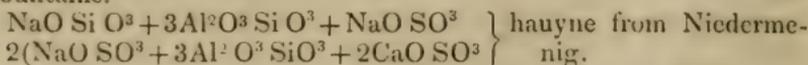
A CHEMICAL INVESTIGATION OF SOME SILICATES CONTAINING CHLORINE, SULPHURIC AND CARBONIC ACID. BY M. WHITNEY OF BOSTON, UNITED STATES.

Long ago Arfvedson observed, in examining sodalite, that it dissolved entirely in nitric acid, although it formed a gelatine with hydrochloric acid. Gustave Rose made a similar observation upon cancrinite from the Ilmen mountains, which dissolves to a clear liquid in concentrated hydrochloric acid, but suddenly gelatinizes on boiling. Mr. Whitney found that a whole series of silicates, which, as regards composition, exhibit great similarity to the above minerals, possess the property of dissolving to a clear liquid in all dilute acids. All these minerals gelatinize with concentrated acids, with the exception of cancrinite, which is likewise soluble in them. To this series belongs, besides sodalite and the cancrinite from the Ilmen mountains, elæolite from Litchfield (Maine), noseau from the Lachersee, hauijne from the Albanese mountains and from Niedermerzig, and the cancrinite discovered by Dr. Jackson of Boston, along with the above elæolite at Litchfield.

The author has examined all these minerals with the greatest care, and the following are the interesting results of his investigation:—sodalite, noseau and hauijne, may be regarded as combinations of a silicate of alumina and soda, of the composition $NaO Si O^3 + 3(Al^2 O^3 Si O^3)$, with chloride of sodium, sulphate of soda, and sulphate of lime. This silicate has hitherto never been met with in the isolated state. It possesses the composition which was formerly ascribed to elæolite. The following are the formulæ of the minerals under consideration:—



$NaO Si O^3 + 3Al^2 O^3 Si O^3 + 2CaO SO^3$, hauyne from Albanese mountains.



The cancrinite from the Ilmen mountains and that from Litchfield are combinations of true elæolite, $2\text{NaO Si O}_3 + 2(\text{Al}^2 \text{O}_3 \text{Si O}_3)$, with carbonate of lime and carbonate of soda.

$\text{Na}^2 \text{O}^2 \text{Si O}_3 + 2\text{Al}^2 \text{O}^3 \text{Si O}_3 + \text{CaO CO}^2$, cancrinite from Ilmen mountains.

$\text{Na}^2 \text{O}^2 \text{Si O}_3 + 2\text{Al}^2 \text{O}^3 \text{Si O}_3 + \left\{ \begin{array}{l} \frac{1}{2} \text{NaO} \\ \frac{1}{2} \text{CaO} \end{array} \right\} \text{CO}^2 + \text{HO}$, cancrinite from Litchfield.—*Bericht der Berliner Akademie*, Feb. 1847.

NEW BOOKS.

Scientific Memoirs, Part XVII., commencing the 5th Volume, containing Schmidt's Contributions to the Comparative Physiology of the Invertebrate Animals, being a Physiologico-Chemical Investigation; Fresnel on the Colours produced in Homogeneous Fluids by Polarized Light; Jamin on Metallic Reflexion; Dove's Researches on the Electricity of Induction.

Manual of British Botany, containing the Flowering Plants and Ferns arranged according to the Natural Orders, by Charles Cardale Babington, M.A., F.L.S. &c. 2nd edit.

The Elements of Botany, Structural and Physiological, by John Lindley, Ph.D., F.R.S. &c. 3rd edit.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1847.

Chiswick.—April 1. Slight haze: sleet and hail-showers: frosty. 2. Overcast. 3. Cloudy and cold: rain. 4. Overcast. 5. Cloudy: fine. 6. Cloudy: rain at night. 7. Cloudy: rain. 8. Rain: cloudy: clear. 9. Clear and cold. 10. Cloudy: fine. 11. Uniformly overcast: rain. 12. Rain: cloudy. 13. Densely clouded. 14. Bleak and cold: slight hail-showers. 15. Snow and hail: clear, cold and dry: sharp frost at night. 16. Sharp frost: clear and cold. 17. Frosty and foggy: cold and dry. 18. Clear. 19. Clear: cloudy: slight frost. 20. Foggy: fine, with sun: cloudy. 21. Clear: overcast. 22. Cold haze: frosty. 23. Fine: clear at night. 24. Slight haze: fine, with sun: frosty. 25. Foggy: fine. 26. Rain: overcast. 27. Boisterous, with showers. 28. Fine: showery. 29. Heavy showers, partly hail: thunder. 30. Clear: heavy clouds and showers.

Mean temperature of the month	44°·28
Mean temperature of April 1846	47 °36
Mean temperature of April for the last twenty years ...	47 °20
Average amount of rain in April	1·47 inch.

Boston.—April 1. Cloudy: snow A.M. and P.M.: rain yesterday. 2. Cloudy: large fall of snow A.M.: rain P.M. 3. Cloudy: snow A.M.: rain P.M. 4—6. Fine. 7. Cloudy. 8. Rain: rain early A.M.: stormy, with rain P.M. 9. Stormy. 10. Fine. 11. Fine: rain A.M. and P.M. 12, 13. Cloudy. 14. Fine. 15. Cloudy: snow-showers. 16. Fine. 17, 18. Fine: ice this morning. 19. Cloudy. 20—23. Fine. 24. Fine: rain P.M. 25. Cloudy: rain and hail P.M. 26. Rain: rain A.M. 27. Windy: rain P.M. 28. Windy. 29. Fine. 30. Cloudy: thunder and rain P.M.

Sandwich Manse, Orkney.—April 1. Snowing: snow. 2. Snowing: showers: snow-showers. 3. Cloudy. 4. Bright: damp. 5. Sleet-showers: showers. 6. Bright: showers. 7. Bright: hail-showers: aurora. 8. Rain: showers. 9. Showers: clear: aurora. 10. Clear. 11. Clear: rain. 12. Bright: cloudy. 13. Snow-showers: cloudy. 14, 15. Cloudy. 16. Clear: cloudy. 17. Showers: clear. 18. Showers: cloudy. 19. Fine: clear. 20. Showers: clear. 21. Cloudy: clear: frost. 22. Bright: cloudy. 23. Bright: showers. 24. Showers: drops. 25. Bright: cloudy. 26. Bright: clear. 27. Bright: rain. 28. Showers: drizzle. 29, 30. Bright: clear.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Cirswick, 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.					
	Max.	Min.	8 ^h a.m.	8 ^h p.m.	9 a.m.	9 p.m.	9 ^h a.m.	8 ^h p.m.	Max.	Min.	9 ^h a.m.	8 ^h p.m.	Max.	Min.	9 ^h a.m.	8 ^h p.m.				
1817.																				
April.																				
1.	29.404	29.300	29.08	29.42	29.37	29.42	29.42	29.42	46	23	38	34	36	34	s.	sw.	ne.	0.05	0.08	
2.	29.333	29.247	28.95	29.54	29.50	29.58	29.58	29.54	44	28	36.5	37	37	37	n.	calm	ne.	0.05	0.07	
3.	29.452	29.340	29.04	29.60	29.60	29.68	29.68	29.60	44	32	35	38	38	38	nw.	calm	w.	0.04	0.18	
4.	29.702	29.693	29.39	29.46	29.46	29.33	29.33	29.46	50	37	42	43	43	43	w.	ne.	sw.	0.02	0.22	
5.	29.745	29.629	29.33	29.35	29.35	29.45	29.45	29.35	56	39	42	41	43	41	n.	n.	nw.	0.04	0.21	
6.	29.853	29.745	29.36	29.65	29.65	29.45	29.45	29.65	55	41	46	43	43	43	n.	calm	w.	0.10	0.08	
7.	29.837	29.813	29.36	29.43	29.43	29.48	29.48	29.43	56	42	49	44	44	44	w.	calm	w.	0.36	0.07	
8.	29.625	29.549	29.08	28.61	28.61	28.54	28.54	28.61	58	38	51	41	40	40	w.	calm	nw.	0.02	0.41	
9.	29.775	29.689	29.16	29.18	29.18	29.48	29.48	29.18	55	34	47.5	40	40	39	w.	w.	nw.	0.09	0.62	
10.	29.900	29.695	29.49	29.70	29.70	29.82	29.82	29.70	58	30	46	44	44	39	w.	w.	nw.	0.02	0.43	
11.	29.945	29.784	29.69	29.78	29.78	29.54	29.54	29.78	50	47	43	44	44	44	s.	calm	se.	0.10	0.02	
12.	29.706	29.677	29.30	29.68	29.68	29.80	29.80	29.68	64	42	53	44	44	44	sw.	calm	nw.	0.28	0.11	
13.	29.922	29.841	29.50	30.01	30.01	30.18	30.18	30.01	51	33	47	41	41	38	ne.	e.	n.	0.01	0.03	
14.	29.947	29.912	29.64	30.17	30.17	30.08	30.08	30.17	48	34	47	42	42	42	ne.	ne.	w.	0.01	0.05	
15.	30.025	29.906	29.61	30.05	30.05	30.02	30.02	30.05	48	23	41.5	46	46	43	ne.	e.	s.	0.01	0.05	
16.	30.026	29.926	29.69	29.85	29.85	29.72	29.72	29.85	49	20	41	45	45	45	n.	e.	ssc.	0.01	0.15	
17.	29.824	29.757	29.52	29.69	29.69	29.84	29.84	29.69	54	22	43	44	44	43	se.	se.	sw.	0.03	0.03	
18.	29.876	29.790	29.54	29.95	29.95	29.90	29.90	29.95	52	34	45	44	44	40	ne.	n.	calm	0.03	0.03	
19.	29.813	29.712	29.39	29.76	29.76	29.76	29.76	29.76	57	27	41	43	43	40	sw.	calm	ne.	0.06	0.06	
20.	29.884	29.742	29.50	29.84	29.84	29.95	29.95	29.84	60	30	46	43	43	42	sw.	calm	ne.	0.06	0.06	
21.	30.027	29.962	29.57	30.02	30.02	30.06	30.06	30.02	61	36	51.5	45	45	38	w.	n.	calm	0.06	0.06	
22.	30.064	29.930	29.71	30.06	30.06	30.05	30.05	30.06	60	26	50	47	47	41	ne.	calm	calm	0.04	0.04	
23.	30.024	29.974	29.67	29.93	29.93	29.93	29.93	29.93	59	30	49	45	45	42	e.	calm	e.	0.12	0.12	
24.	29.987	29.962	29.60	29.92	29.92	29.92	29.92	29.92	58	28	48.5	43	43	41	e.	s.	sw.	0.11	0.11	
25.	29.997	29.951	29.57	29.89	29.89	29.66	29.66	29.89	62	36	51	45	45	41	sw.	w.	w.	0.11	0.21	
26.	29.788	29.781	29.30	29.25	29.25	29.39	29.39	29.25	57	43	43	43	43	43	sw.	w.	w.	0.01	0.01	
27.	29.728	29.613	29.11	28.90	28.90	28.48	28.48	28.90	61	43	56	49	49	45	w.	w.	w.	0.01	0.35	
28.	29.696	29.579	29.18	28.80	28.80	29.00	29.00	28.80	60	40	53	47	47	45	sw.	calm	calm	0.12	0.12	
29.	29.571	29.528	29.10	29.31	29.31	29.50	29.50	29.31	57	35	51	45	45	41	sw.	calm	calm	0.20	0.13	
30.	29.794	29.667	29.24	29.59	29.59	29.68	29.68	29.59	61	30	46	47	47	41	nw.	w.	w.	0.02	0.28	
Mean.	29.809	29.756	29.38	29.611	29.611	29.619	29.619	29.611	55.03	33.53	45.9	43.40	43.40	41.11				1.08	1.77	3.32

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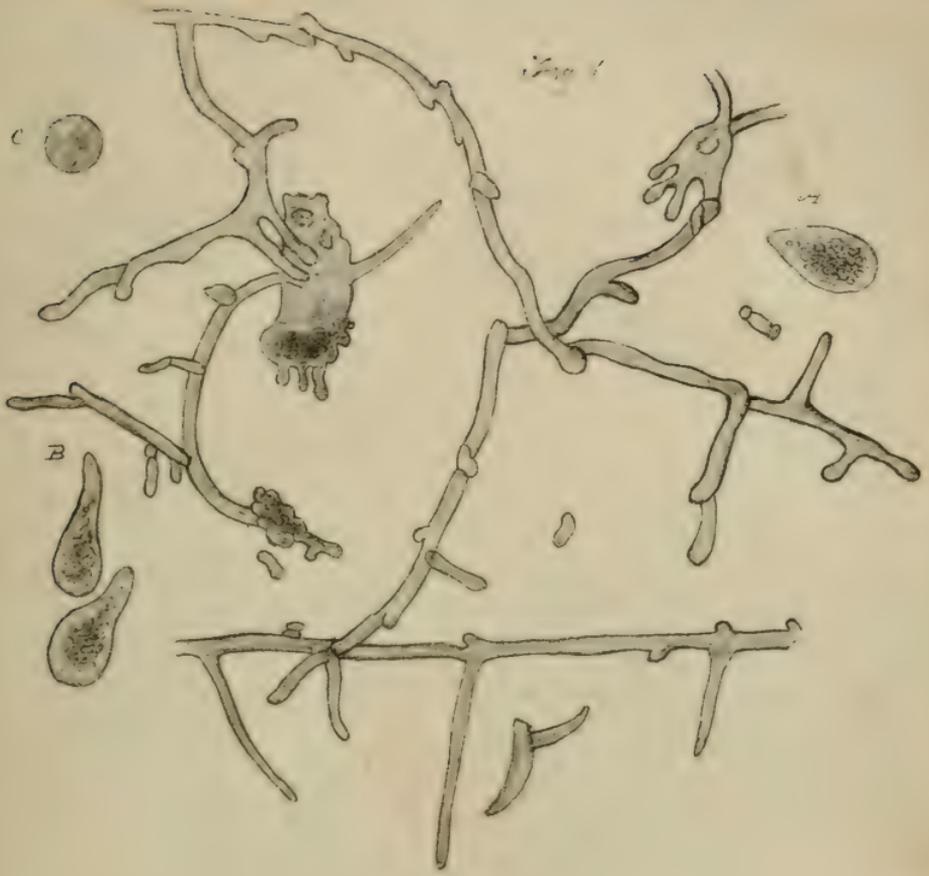
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END OF THE THIRTIETH VOLUME.





Organic Bodies contained in Huilstones.



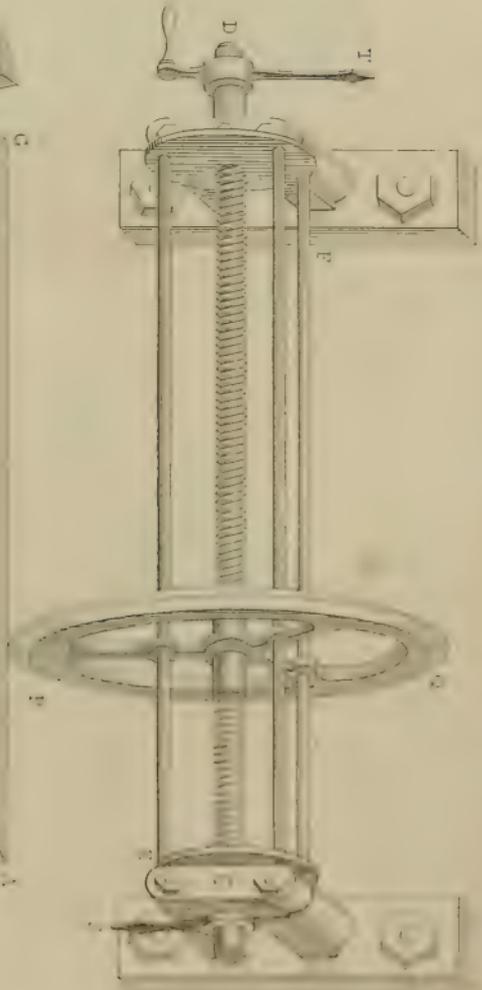
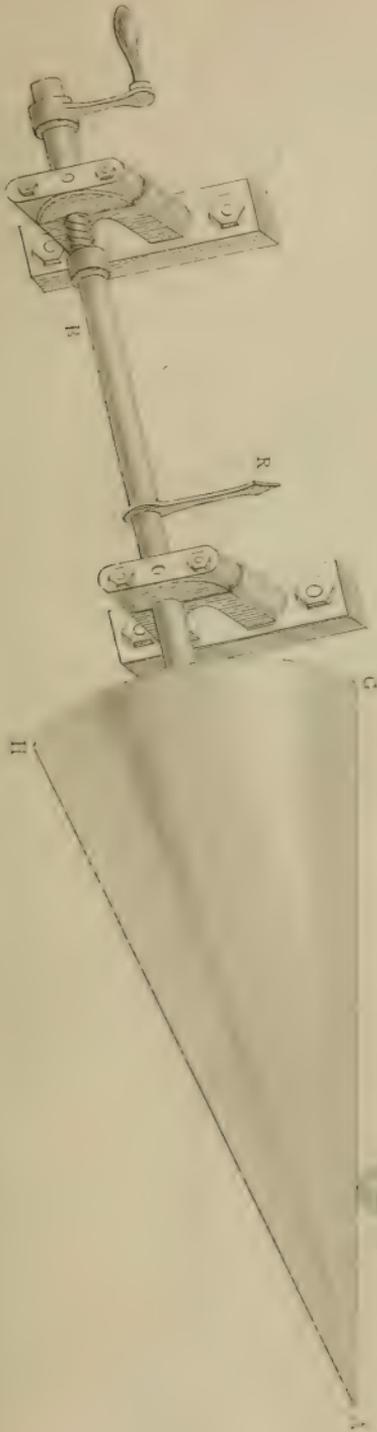




Fig. 1.

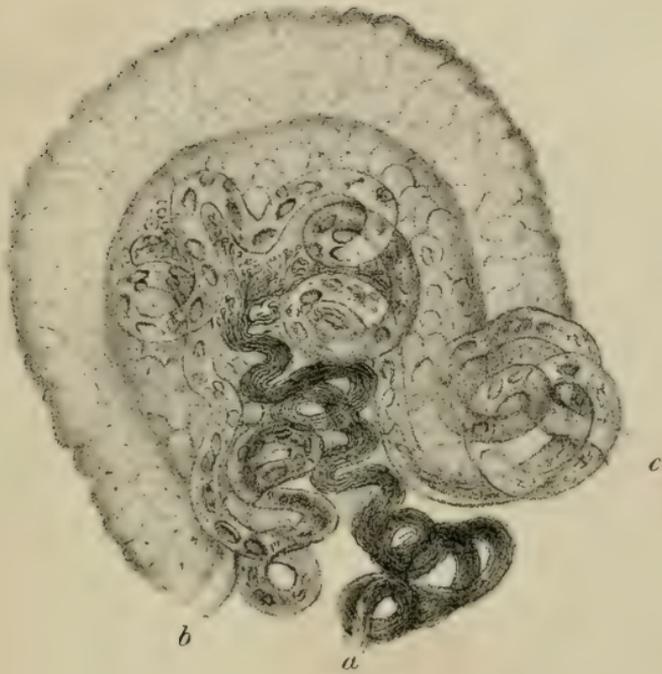
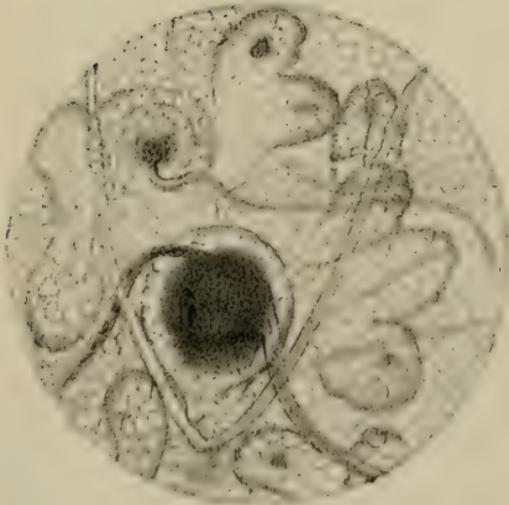


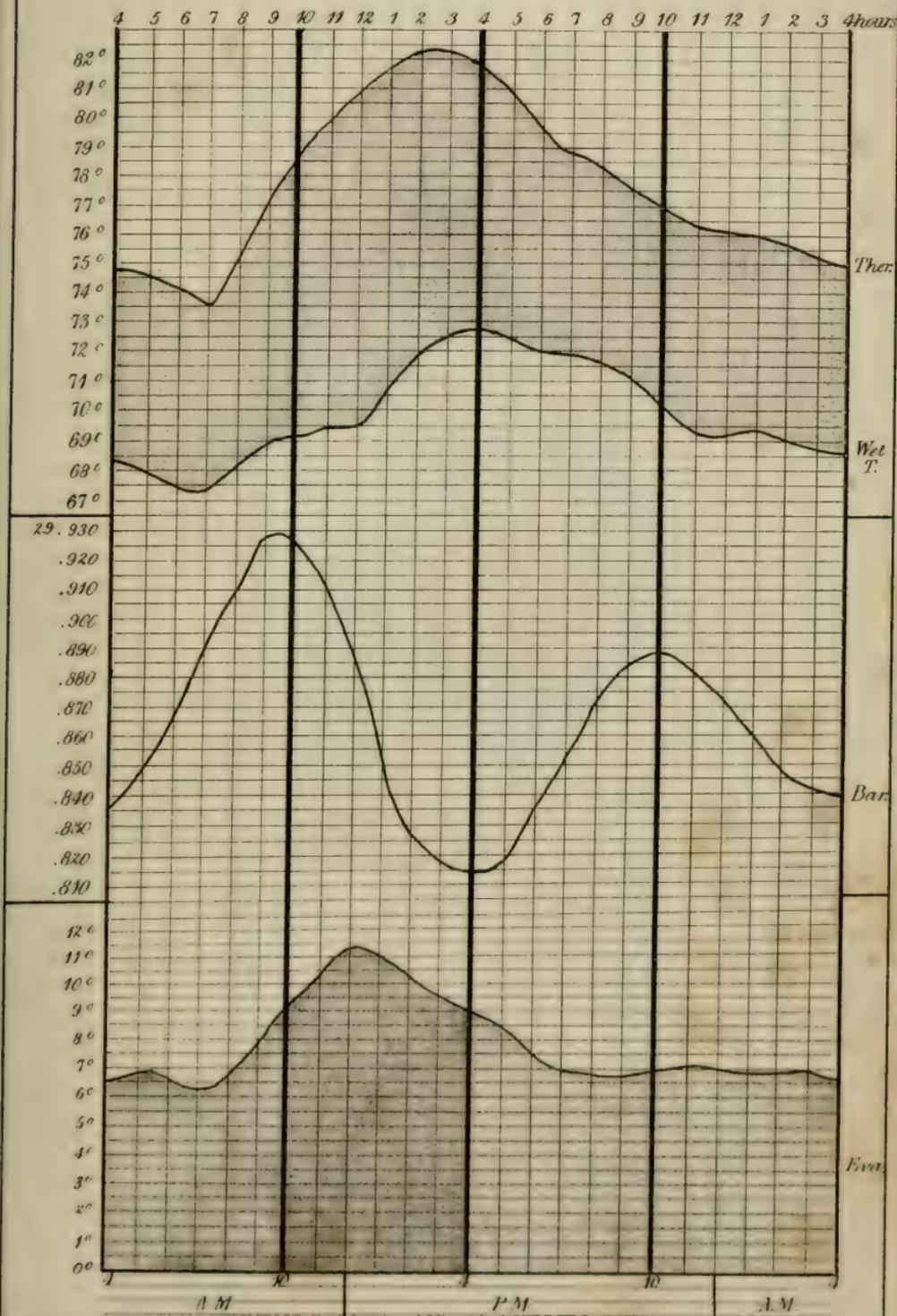
Fig. 2.



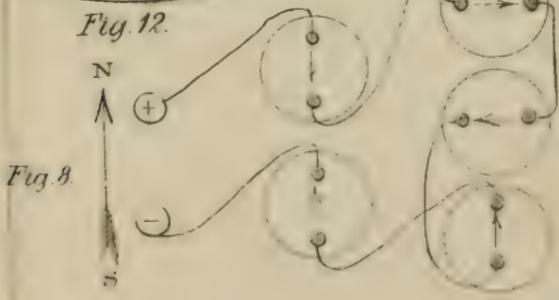
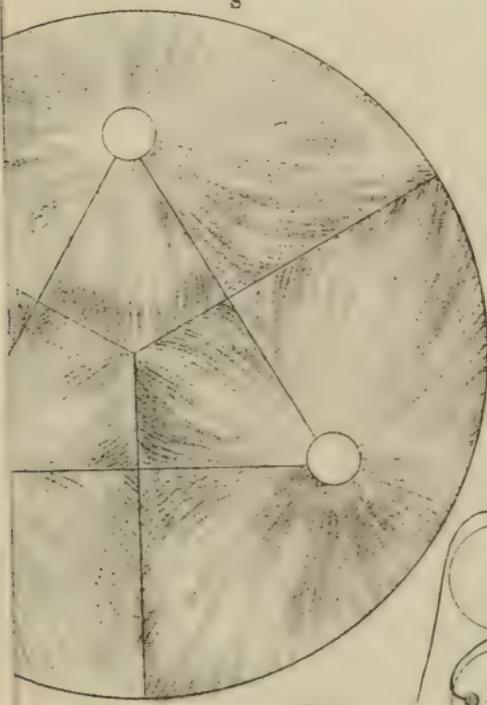
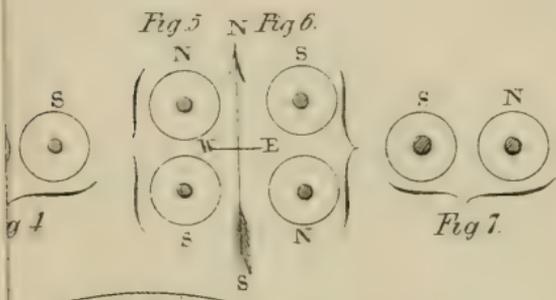


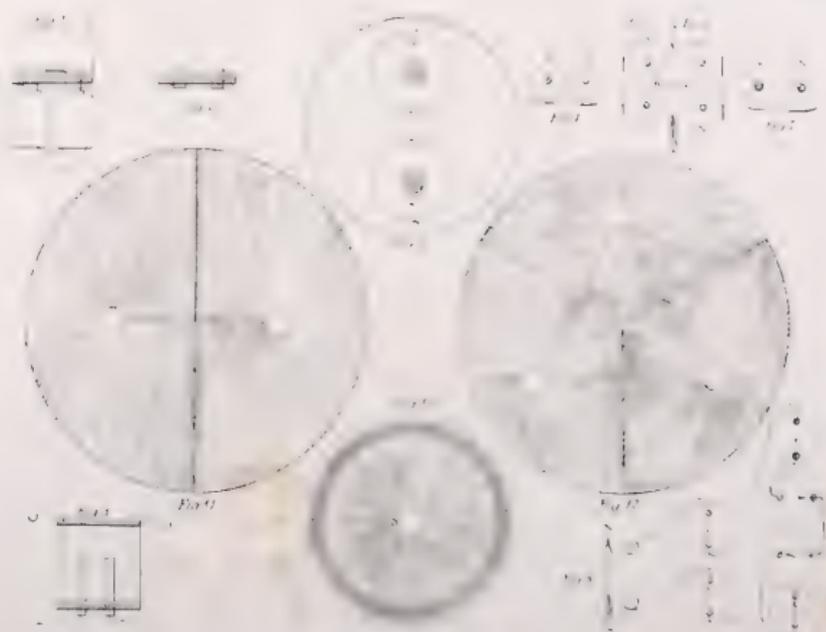
Diagram

Of the hourly heights of the Barometer, the dry Thermometer, the wet bulb Thermometer, and the relative amounts of the force of evaporation at Bombay.



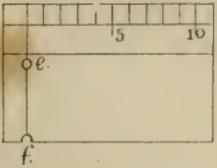






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Upper Surface F.



Under Surface F.

