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

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

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

J. B. Pinelli ad Mazonium.

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ERRATUM

Page 193, line 14 from top, *for* multiplied *read* divided.

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JANUARY 1854.

I. *On the Mechanical Action of Heat.* *By* William John Macquorn Rankine, *C.E., F.R.SS. Lond. and Edinb. &c.*[*]

Prefatory Remarks.

By the permission of the Council of the Royal Society of Edinburgh, the following series of communications are republished, which, having been read to the meetings of that body at various periods during the sessions of 1849–50 and 1850–51, were first printed in the First and Second Parts of the Twentieth Volume of their Transactions.

These investigations may be considered under a twofold aspect,—either as determining the theoretical and practical results, so far as heat and expansive power are concerned, of the law of the mutual convertibility of physical powers, or as developing the consequences of an hypothesis respecting the constitution of matter, called that of Molecular Vortices, whereby that law, as respects heat and elasticity, is made a particular case of the mechanical principle of the conservation of *vis viva*.

The law of the mutual convertibility of physical powers has long been a subject of abstract speculation, and may appear to some minds in the light of a necessary truth. As we cannot, however, expect it to be generally received as such, its practical demonstration must be considered as having been effected by the experiments of Mr. Joule.

In the following paper, as it originally appeared, the mechanical equivalent of the heat required to raise the temperature of a given mass of liquid water by one degree was calculated independently of those experiments, from the observed velocity of sound in air, and from the specific heat of air under constant

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[*] Communicated by the Author.

pressure as compared with that of water, according to the experiments of De la Roche and Bérard. The result thus obtained was 695 feet of fall per degree of Fahrenheit, being about one-tenth part less than Joule's equivalent. But shortly afterwards Mr. Joule's latest and most accurate series of experiments were published, the mutual agreement of which made it evident that his determination of the value of one degree of Fahrenheit's scale in liquid water, viz. 772 feet of fall, is correct to about $\frac{1}{300}$dth of its amount. All the numerical results in the following paper, therefore, have been corrected to correspond with this value; and amongst others, the specific heat of air, the theoretical computation of which has recently been exactly confirmed by the experiments of M. Regnault.

With respect to the consequences of the law of the mutual convertibility of heat and expansive power, the results of the following investigations will be found to coincide in many points with those arrived at by Messrs. Mayer, Helmholtz, Holtzmann, Clausius, and Thomson.

Considered as the development of the consequences of the hypothesis of molecular vortices, these researches constitute the second part of a paper on the Centrifugal Theory of Elasticity, the first part of which, treating of the Statical Relations of Heat and Elasticity, was read to the Royal Society of Edinburgh along with the present investigation, and published in the Philosophical Magazine for December 1851.

The introduction of an hypothesis into the theory of a physical science has for its sole legitimate object, to deduce the laws of one class of phænomena from those of another class whose laws are better known, and thus to diminish the number and the complexity of first principles.

Motion and force being the only phænomena of which we thoroughly and exactly know the laws, and mechanics the only complete physical science, it has been the constant endeavour of natural philosophers, by conceiving the other phænomena of nature as modifications of motion and force, to reduce the other physical sciences to branches of mechanics.

In the preface to the Principia, we find Newton expressing a wish for the extension of this kind of investigation in these words:—"UTINAM CÆTERA NATURÆ PHÆNOMENA EX PRINCIPIIS MECHANICIS DERIVARE LICERET."

The theory of radiant heat and light having been reduced to a branch of mechanics by means of the hypothesis of undulations, it is the object of the hypothesis of molecular vortices to reduce the theory of thermometric heat and elasticity also to a branch of mechanics, by so conceiving the molecular structure of matter, that the laws of these phænomena shall be consequences of those of motion and force.

This hypothesis, like all others, is neither demonstrably true nor demonstrably false, but merely probable in proportion to the extent of the class of facts with which its consequences agree. It must however be remarked, that whether the hypothesis be probable or improbable, the theoretical and practical results arrived at in the following paper respecting the mechanical action of heat remain unaffected, being deduced from principles which have been established by experiment and demonstration.

The chief contents are as follows:—

The *Introduction* explains the hypothesis.

The *First Section* treats generally of the mutual conversion of heat and expansive power;

The *Second Section*, of real and apparent specific heat, especially in the state of perfect gas;

The *Third Section*, of the latent and total heat of evaporation, especially for water.

The *Fourth Section* treats of the mechanical action of steam, treated as a perfect gas, and the power of the steam-engine, and contains practical formulæ and tables. In a supplement to this section, the results of these formulæ and tables are compared with the experiments of Mr. Wicksteed on the Cornish engine at Old Ford; and a graphic solution is given of the problem, to determine the rate of expansion in a Cornish engine, so that the sum of the annual cost of fuel, and the interest of capital, shall be the least possible as compared with the work done.

The *Fifth Section* treats of the œconomy of heat in expansive machines generally.

A sixth section was read to the Royal Society of Edinburgh in January 1853, and is published in their Transactions.

Besides the numerical corrections already referred to, the only alterations which this paper has received consist in the addition of some notes, the cancelling of some statements and calculations which have been superseded by subsequent researches, and a few slight improvements in the notation.

Glasgow, November 1853.

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*On the Mechanical Action of Heat, especially in Gases and Vapours**.

INTRODUCTION.

Summary of the Principles of the Hypothesis of Molecular Vortices, and its application to the Theory of Temperature, Elasticity, and real Specific Heat.

The ensuing paper forms part of a series of researches respecting the consequences of an hypothesis called that of Molecular

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* Read to the Royal Society of Edinburgh, February 4, 1850. [With

Vortices, the object of which is, to deduce the laws of elasticity, and of heat as connected with elasticity, by means of the principles of mechanics, from a physical supposition consistent and connected with the theory which deduces the laws of radiant light and heat from the hypothesis of undulations. Those researches were commenced in 1842, and after having been laid aside for nearly seven years, from the want of experimental data, were resumed in consequence of the appearance of the experiments of M. Regnault on gases and vapours.

The investigation which I have now to describe, relates to the mutual conversion of heat and mechanical power by means of the expansion and contraction of gases and vapours*.

In the introduction which I here prefix to it, I purpose to give such a summary of the principles of the hypothesis as is necessary to render the subsequent investigation intelligible.

The fundamental suppositions are the following:—

First,—That each atom of matter consists of a nucleus, or central physical point, enveloped by an elastic atmosphere, which is retained in its position by forces attractive towards the nucleus or centre.

Suppositions similar to this have been brought forward by Franklin, Æpinus, Mossotti, and others. They have in general, however, conceived the atmosphere of each nucleus to be of variable mass. I have treated it, on the contrary, as an essential part of the atom. I have left the question indeterminate, whether the nucleus is a small body of a character distinct from that of the atmosphere, or merely a portion of the atmosphere in a highly condensed state, owing to the mutual attraction of its parts.

According to this first supposition, the boundary between two contiguous atoms of a body is an imaginary surface at which the attractions of all the atomic centres of the body balance each other; and the elasticity of the body is made up of two parts: *First*, the elasticity of the atomic atmospheres at the imaginary boundaries of the atoms, which I shall call the superficial-atomic elasticity; and, *secondly*, the force resulting from the mutual actions of distinct atoms. If the atmospheres are so much condensed round their nuclei or centres that the superficial-atomic elasticity is insensible, and that the resultants of the mutual

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the original paper are incorporated some corrected numerical results formerly contained in a supplementary paper read on the 2nd of December 1850, which were deduced from Mr. Joule's latest experiments on the mechanical equivalent of heat. The foot-notes have all been added since the original paper was read.]

* The statical relations between the density, heat, temperature, and pressure of gaseous bodies, as deduced from this hypothesis, form the subject of a paper "On the Centrifugal Theory of Elasticity," published in the Philosophical Magazine for December 1851.

actions of all parts of the distinct atoms are forces acting along the lines joining the nuclei or centres, then the body is a perfect solid, having a tendency to preserve not only a certain bulk, but a certain figure; and the elasticity of figure, or *rigidity*, bears certain definite relations to the elasticity of volume.

If the atmospheres are less condensed about their centres, so that the mutual actions of distinct atoms are not reducible to a system of forces acting along the lines joining the atomic centres, but produce merely a cohesive force sufficient to balance the superficial-atomic elasticity, then the condition is that of a *perfect liquid*; and the intermediate conditions between this and perfect solidity constitute the gelatinous, plastic, and viscous states*.

When the mutual actions of distinct atoms are very small as compared with the superficial-atomic elasticity, the condition is that of *gas* or *vapour*; and when the substance is so far rarefied that the influence of the atomic nuclei or centres in modifying the superficial elasticity of their atmospheres is insensible, it is then in the state of *perfect gas*.

So far as our experimental knowledge goes, the elasticity of a perfect gas at a given temperature varies simply in proportion to its density. I have therefore assumed this to be the law of the elasticity of the atomic atmospheres, ascribing a specific coefficient of elasticity to each substance.

The *second supposition*, being that from which the hypothesis of molecular vortices derives its name, is the following:—*That the elasticity due to heat arises from the centrifugal force of revolutions or oscillations among the particles of the atomic atmospheres; so that quantity of heat is the* vis viva *of those revolutions or oscillations.*

This supposition appears to have been first definitely stated by Sir Humphry Davy. It has since been supported by Mr. Joule, whose valuable experiments to establish the convertibility of heat and mechanical power are well known. So far as I am aware, however, its consequences have not hitherto been mathematically developed.

To connect this hypothesis with the undulatory theory of radiation, I have introduced a *third supposition*:—*That the medium which transmits light and radiant heat consists of the* nuclei *of the atoms, vibrating independently, or almost independently, of*

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* I have applied these ideas to the laws of the elasticity of solid bodies, in a paper read to the British Association for the Advancement of Science on the 1st of August 1850, and published in the Cambridge and Dublin Mathematical Journal for February 1851. Further developments of this view of the theory of the elasticity of solids were published in the numbers of the same journal for November 1851 and November 1852.

their atmospheres;—so that the absorption of light and of radiant heat is the transference of motion from the nuclei to their atmospheres, and the emission of light and of radiant heat, the transference of motion from the atmospheres to their nuclei*.

Although in all undulations of sensible length and amplitude, such as those of sound, the nuclei must carry their atmospheres along with them, and vibrating thus loaded, produce a comparatively slow velocity of propagation; yet in all probability the minute vibrations of light and radiant heat may be performed by the atomic nuclei in transparent and diathermanous bodies, without moving the atmospheres more than by that amount which constitutes absorption; and those vibrations will therefore be transmitted according to the laws of the elasticity of *perfect solids*, and with a rapidity corresponding to the extreme smallness of the masses set in motion, as compared with the mutual forces exerted by them.

This supposition is peculiar to my own view of the hypothesis, and is, in fact, the converse of the idea hitherto adopted, of an æther surrounding ponderable particles.

The second and third suppositions involve the assumption, that motion can be communicated between the nuclei and their atmospheres, and between the different parts of the atmospheres; so that there is a tendency to produce some permanent condition of motion, which constitutes equilibrium of heat. It is now to be considered what kind of motion is capable of producing increase of elasticity, and what are the conditions of permanency of that motion.

It is obvious, that the parts of the atomic atmospheres may have motions of alternate expansion and contraction, or of rectilinear oscillation about a position of equilibrium, without affecting the superficial atomic elasticity, except by small periodical changes. Should they have motions, however, of *revolution* about centres, so as to form a group of *vortices*, the centrifugal force will have the effect of increasing the density of the atmosphere at what I have called the bounding surfaces of the atoms, and thus of augmenting the elasticity of the body.

In this summary, I shall not enter into the details of mathematical analysis, but shall state results only. The following,

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* The consequences of this supposition, in the theory of polarization and double refraction, are pointed out in a paper read to the Royal Society of Edinburgh on the 2nd of December 1850, and published in the Philosophical Magazine for June 1851. A modification of this third supposition, according to which the motion constituting light is an oscillation of the atomic nuclei round transverse axes, and is transmitted by means of a species of polarity, forms the subject of a paper read to the British Association at Hull, and published in the Philosophical Magazine for December 1853.

then, are the conditions which must be fulfilled, in order that a group of vortices, of small size as compared with the bulk of an atom, and of various diameters, may permanently coexist, whether side by side, or end to end, in the atomic atmospheres of one substance, or of various substances mixed.

First, The mean elasticity must vary continuously; which involves the condition, that at the surface of contact of two vortices of different substances, side by side, or end to end, the respective densities at each point of contact must be inversely proportional to the coefficients of elasticity. Hence *the specific gravities of the* atmospheric parts of *all substances, under precisely similar circumstances as to heat and molecular forces* (a condition realized in perfect gases at the same pressure and temperature), *are inversely proportional to the coefficients of atmospheric elasticity.* Therefore let μ represent the mass of the atmosphere of one atom of any substance, b its coefficient of elasticity, and n the number of atoms which, in the state of perfect gas, occupy unity of volume under unity of pressure at the temperature of melting ice;—then

$$n\mu b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(I.)}$$

is a constant quantity for all substances.

Secondly, The superficial elasticity of a vortex must *not* be a function of its diameter: to fulfill which condition, the linear velocity of revolution must be equal throughout all parts of each individual vortex.

Thirdly, In all contiguous vortices of the same substance, the velocities of revolution must be equal; and in contiguous vortices of different substances, the squares of the velocities must be proportional to the coefficients of elasticity of the molecular atmospheres.

The second and third conditions are those of equilibrium of heat, and are equivalent to this law:—

TEMPERATURE *is a function of the square of the velocity of revolution in the molecular vortices divided by the coefficient of elasticity of the atomic atmospheres*; or,

$$\text{Temperature}=\phi\left(\frac{w^2}{b}\right), \quad . \quad . \quad . \quad . \qquad \text{(II.)}$$

where w represents that velocity.

The *mean* elasticity which a vortex exerts endways is not affected by its motion, being equal to

$$b\rho, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(III.)}$$

where ρ is its mean density. The superficial elasticity at its lateral surfaces, however, is expressed by

$$\frac{w^2\rho}{2g}+b\rho. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(IV.)}$$

The additional elasticity $\frac{w^2\rho}{2g}$, being that which is due to the motion, is independent of the diameter. The divisor g (the force of gravity) is introduced, on the supposition of the density ρ being measured by weight.

Supposing the atmosphere of an atom to be divided into concentric spherical layers, it may be shown that the effect of the coexistence of a great number of small vortices in one of those layers whose radius is r, and mean density ρ, is to give it a centrifugal force, expressed by

$$\frac{w^2}{gr}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(V.)}$$

which tends to increase the density and elasticity of the atmosphere at the surface, which I have called the boundary of the atom. The layer is also acted upon by the difference between the mean elasticities at its two surfaces, and by the attraction towards the atomic centre; and these three forces must balance each other.

I have integrated the differential equation which results from this condition, for substances in the gaseous state, in which the forces that interfere with the centrifugal force and atmospheric elasticity are comparatively small; and the result is

$$\mathrm{P}=b\frac{\mu}{\mathrm{M}}\mathrm{D}\left(\frac{w^2}{3gb}+1\right)(1-\mathrm{F})+f(\mathrm{D}). \quad . \quad . \quad \text{(VI.)}$$

P is the entire elasticity of the gas, and D its mean density. M represents the total mass of an atom, measured by weight, and μ that of its atmospheric part; so that $\frac{\mu}{\mathrm{M}}\mathrm{D}$ is the mean density of the atomic atmospheres*.

$f(\mathrm{D})$ denotes the effect of the mutual actions of separate atoms.

The first term represents the superficial-atomic elasticity. F denotes the effect of the attraction of the nucleus in modifying that elasticity, and can be represented approximately by a converging series, in terms of the negative powers of $\frac{w^2}{3gb}+1$, commencing with the inverse square, the coefficients being functions of the density D.

By using the first term of such a series, and determining its coefficient, and the quantity $f(\mathrm{D})$ empirically, I have obtained formulæ agreeing closely with the results of M. Regnault's ex-

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* The corresponding differential equation for substances in any state whatsoever is integrated in a paper on the Centrifugal Theory of Elasticity, published in the Transactions of the Royal Society of Edinburgh, vol. xx. part 3.

periments on the expansion of atmospheric air, carbonic acid and hydrogen*.

In a perfect gas, the above expression is reduced to

$$P = b\frac{\mu}{M}D\left(\frac{w^2}{3gb}+1\right). \quad . \quad . \quad . \quad . \quad \text{(VII.)}$$

Let n, as before, denote the number of atoms of a substance which, in the state of perfect gas, occupy unity of volume under unity of pressure at the temperature of melting ice, so that nM is its specific gravity in that state: then

$$P = \frac{D}{nM}n\mu b\left(\frac{w^2}{3gb}+1\right). \quad . \quad . \quad . \quad \text{(VIII.)}$$

The factor by which $\frac{D}{nM}$ is here multiplied fulfills the condition of being a function of $\frac{w^2}{b}$, and of constants which are the same for all substances, and is therefore fitted for a measure of temperature. It obviously varies proportionally to the pressure of a perfect gas of a given density, or its volume under a given pressure.

Let τ, therefore, denote temperature, as measured from an *imaginary zero*, C degrees of the scale adopted, below the temperature of melting ice, at which

$$\frac{w^2}{3gb}+1=0;$$

then for all substances

$$\left.\begin{aligned} &\tau = Cn\mu b\left(\frac{w^2}{3gb}+1\right), \\ &\text{and in perfect gases} \\ &P = \frac{\tau}{C}\frac{D}{nM}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad \text{(IX.)}$$

τ may be termed *absolute temperature*, and the point from which it is measured, the *absolute zero of temperature*. This, as I have observed, is an imaginary point, being lower than the absolute zero of heat by the quantity $Cn\mu b$, which is the same for all substances.

The value of C, or the absolute temperature of melting ice, as determined from M. Regnault's experiments, is

274°·6 Centigrade,

being the reciprocal of

0·00364166 per Centigrade degree,

the value to which the coefficients of dilatation of gases at the temperature of melting ice approximate as they are rarefied.

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* Philosophical Magazine, December 1851.

For Fahrenheit's scale $C=494^{\circ}\cdot 28$.

In the sequel I shall represent temperatures measured from that of melting ice by $T=\tau-C$.

We have now to consider the absolute quantity of heat, or of molecular *vis viva*, which corresponds to a given temperature in a given substance. It is obvious that

$$\frac{\mu w^2}{2g}$$

represents, in terms of gravity, the portion of *vis viva*, in one atom, due to the molecular vortices; but besides the vortical motion, there may be oscillations of expansion and contraction, or of rectilinear vibration about a position of equilibrium. The velocity with which these additional motions are performed will be in a permanent condition when the mean value of its square, independent of small periodic changes, is equal throughout the atomic atmosphere. We may therefore represent by

$$\frac{\mu v^2}{2g}=k\frac{\mu w^2}{2g} \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{X.})$$

the total *vis viva* of the atomic atmosphere. To this we have to add that of the nucleus, raising the quantity of heat in one atom to

$$\left.\begin{array}{l} \dfrac{Mv^2}{2g}=q, \\ \text{while the quantity of heat in unity of weight is} \\ \dfrac{v^2}{2g}=Q. \end{array}\right\} \quad . \quad . \quad (\text{XI.})$$

The coefficient k (which enters into the value of specific heat) being the ratio of the *vis viva* of the entire motion impressed on the atomic atmospheres by the action of their nuclei, to the *vis viva* of a peculiar kind of motion, may be conjectured to have a specific value for each substance depending in a manner yet unknown on some circumstance in the constitution of its atoms. Although it varies in some cases for the same substance in the solid, liquid and gaseous states, there is no experimental evidence that it varies for the same substance in the same condition. In the investigation which follows, therefore, I have treated it as sensibly constant.

The following, then, are the expressions for quantity of heat in terms of temperature. In one atom:—

$$\left.\begin{array}{l} q=\dfrac{v^2}{2g}M=\dfrac{3kM}{2Cn\mu}(\tau-Cn\mu b). \\ \text{In unity of weight:—} \\ Q=\dfrac{v^2}{2g}=\dfrac{3k}{2Cn\mu}(\tau-Cn\mu b). \end{array}\right\} \quad . \quad . \quad . \quad (\text{XII.})$$

Real specific heat is consequently expressed by the following equations :—

For one atom :—

$$\frac{dq}{d\tau}=\frac{3k\mathrm{M}}{2\mathrm{C}n\mu}. \quad . \quad . \quad . \quad .$$

For unity of weight :—

$$\frac{d\mathrm{Q}}{d\tau}=\frac{3k}{2\mathrm{C}n\mu}. \quad . \quad . \quad . \quad . \qquad . \quad . \quad . \qquad \text{(XIII.)}$$

For so much of a perfect gas as occupies unity of volume under unity of pressure at the temperature of melting ice :—

$$n\frac{dq}{d\tau}=\frac{3k\mathrm{M}}{2\mathrm{C}\mu}. \quad . \quad . \quad . \quad . \quad .$$

The laws established experimentally by Dulong, that the specific heats of simple atoms, and of certain groups of compound atoms, bear to each other simple ratios, generally that of equality, and that the specific heats of equal volumes of all simple gases are equal, show that the specific factor $\frac{k\mathrm{M}}{\mu}$ depends on the chemical constitution of the atom, and thus confirm the conjecture I have stated respecting the coefficient k.

As I shall have occasion, in the investigation which follows, to refer to and to use the equation for the elasticity of vapours in contact with their liquids, which I published in the Edinburgh New Philosophical Journal for July 1849, I shall here state generally the nature of the reasoning from which it was deduced.

The equilibrium of a vapour in contact with its liquid depends on three conditions :—

First. The total elasticity of the substance in the two states must be the same.

Secondly. The superficial-atomic elasticity must vary continuously; so that if at the surface which reflects light there is an abrupt change of density (which seems almost certain), there must there be two densities corresponding to the same superficial-atomic elasticity.

Thirdly. The two forces, which act on each stratum of vapour parallel to the surface of the liquid, namely, the preponderance of molecular attraction towards the liquid, and the difference of the superficial-atomic elasticities at the two sides of the stratum, must be in equilibrio.

Close to the surface of the liquid, therefore, the vapour is highly condensed. The density diminishes rapidly as the distance from the liquid increases, and at all appreciable distances has a sensibly uniform value, which is a function of the temperature and of certain unknown molecular forces.

The integration of a differential equation representing the third condition of equilibrium, indicates the *form* of the approximate equation

$$\log \mathrm{P} = a - \frac{\beta}{\tau} - \frac{\gamma}{\tau^2}, \quad . \quad . \quad . \quad . \quad . \quad \text{(XIV.)}$$

the coefficients of which have been determined empirically by three experimental data for each fluid. For proofs of the extreme closeness with which the formulæ thus obtained agree with experiment, I refer to the Journal in which they first appeared.

In the Philosophical Magazine for December 1851 is given a table of the coefficients for water, alcohol, ether, turpentine, petroleum, and mercury, in the direct equation, and also in the inverse formula

$$\frac{1}{\tau} = \sqrt{\frac{a - \log \mathrm{P}}{\gamma} + \frac{\beta^2}{4\gamma^2}} - \frac{\beta}{2\gamma}, \quad . \quad . \quad . \quad \text{(XV.)}$$

by which the temperature of vapour at saturation may be calculated from the pressure.

For turpentine, petroleum, and mercury, the formula consists of two terms only,

$$\log \mathrm{P} = a - \frac{\beta}{\tau}, \quad . \quad . \quad . \quad . \quad . \quad \text{(XVI.)}$$

the small range of the experiments rendering the determination of γ impossible.

SECTION I.—*Of the Mutual Conversion of Heat and Expansive Power.*

1. The quantity of heat in a given mass of matter, according to the hypothesis of molecular vortices, as well as every other hypothesis which ascribes the phænomena of heat to motion, is measured by the mechanical power to which that motion is equivalent, that being a quantity the total amount of which in a given system of bodies cannot be altered by their mutual actions, although its distribution and form may be altered. This is expressed in equation XII. of the Introduction, where the quantity of heat in unity of weight, Q, is represented by the height $\frac{v^2}{2g}$, from which a body must fall in order to acquire the velocity of the molecular oscillations. This height, being multiplied by the weight of a body, gives the mechanical power to which the oscillations constituting its heat are equivalent. The real specific heat of unity of weight, as given in equation XIII. of the Introduction,

$$\frac{d\mathrm{Q}}{d\tau} = \frac{3k}{2\mathrm{C}n\mu},$$

represents the *depth of fall,* which is equivalent to *one degree of rise of temperature* in any given weight of the substance under consideration.

We know, to a greater or less degree of precision, the ratios of the specific heats of many substances to each other, and they are commonly expressed by taking that of water at the temperature of melting ice as unity.

2. Mr. Joule has made several very interesting series of experiments, in order to ascertain the quantity of heat developed in various substances by mechanical power employed in different ways, viz. by electric currents excited by the rotation of a magnet, by the forcing of water through narrow tubes, by the agitation of water, mercury and oil with a paddle, by the friction of cast iron, by the compression of air, and by the friction of air rushing through a narrow orifice. In his latest and best series of experiments (Phil. Trans. 1850), the mechanical equivalent of heat in liquid water was determined by means of the friction of water, mercury, and cast iron, the mean results being as follows:—

One Centigrade degree in liquid water = the action of gravity through 1389·6 feet = 432·54 mètres. One degree of Fahrenheit in liquid water = the action of gravity through 772 feet. So far as I can judge from the mutual agreement of the experiments, the probable error of this result does not exceed $\frac{1}{300}$dth of its amount.

3. Means of determining the mechanical equivalent of heat in air and other gases are furnished by experiments on the velocity of sound, which, according to the received and well-known theory of Laplace, is accelerated by the heat developed by the compression of the medium.

The accuracy of this theory has lately been called in question. There can be no doubt that it deviates from absolute exactness, in so far that the magnitude of the displacements of the particles of air is neglected in comparison with the length of a wave. It appears to me, however, that the Astronomer Royal, in his remarks on the subject in the London and Edinburgh Philosophical Magazine for July 1849, has shown, in a satisfactory manner, that although the effect of the appreciable magnitude of those displacements, as compared with the length of a wave of sound, is to alter slowly the form of the function representing the wave, still that effect is not sufficiently great to make Laplace's theory practically erroneous.

4. The expression already given for the real specific heat of unity of weight of a given substance may be resolved into two factors, thus:—

$$\frac{dQ}{d\tau}=\frac{1}{CnM}\times\frac{3kM}{2\mu}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The first factor, $\frac{1}{\mathrm{C}n\mathrm{M}}$, may be considered in general as a known quantity; for C represents, as already stated, 274·6 Centigrade degrees, the absolute temperature of melting ice, and $n\mathrm{M}$ the theoretical weight, in the perfectly gaseous state, of unity of volume of the substance, under unity of pressure, at that temperature; or what is the same thing, $\frac{1}{n\mathrm{M}}$ is the height of an imaginary column of the substance, of uniform density, and at the temperature of melting ice, whose pressure by weight upon a given area of base is equal to its pressure by elasticity, supposing it to be perfectly gaseous. The determination of the ratio $\frac{3k\mathrm{M}}{2\mu}$ is necessary, to complete the solution of the problem.

5. The relation now to be investigated between heat and mechanical power, is that which exists between the power expended in compressing a body into a smaller volume, and the increase of heat in consequence of such a compression, and conversely, between the heat which disappears, or, as it is said, becomes *latent*, during the expansion of a body to a greater volume, and the mechanical power gained or developed by that expansion. Those phænomena, according to the hypothesis now under consideration, as well as every hypothesis which ascribes heat to motion, are simply the transformation of mechanical power from one shape into another.

It is obvious, in the first place, without the aid of algebraical symbols, that the general effect of the compression of an oscillating atomic atmosphere, or molecular vortex, must be to accelerate its motion, and of its dilatation, to retard its motion; for every portion of such an atmosphere is urged towards the nucleus or atomic centre by a centripetal force equal to the centrifugal force arising from the oscillation; so that when, by compression, each portion of the atmosphere is made to *approach* the centre by a given distance, the *vis viva* of its motion will be *increased* by the amount corresponding to the centripetal force acting through that distance; and conversely, when by expansion each portion of the atmosphere is made to *retreat* from the centre, the *vis viva* of its motion will be *diminished* by a similar amount.

It is not, however, to be taken for granted, that *all* the power expended in compressing a body appears in the form of heat. More or less power may be consumed or developed by changes of molecular arrangement, or of the internal distribution of the density of the atomic atmospheres; and changes of molecular arrangement or distribution may develope or consume heat, independently of changes of volume.

6. We shall now investigate, according to the hypothesis of

molecular vortices, the amount of heat produced by an indefinitely small compression of one atom of a body in that state of perfect fluidity which admits of the bounding surface of the atom being treated as if it were spherical; its radius being denoted by R, and the radius of any internal spherical layer of the atmosphere by multiplying R by a fraction u*.

I shall denote by the ordinary symbol of differentiation d, such changes as depend on the various positions of portions of the atomic atmosphere relatively to each other, when changes of volume and temperature are not taken into consideration; while by the symbol δ of the calculus of variations, I shall represent such changes as arise from the variations of volume and temperature.

Let us consider the case of an indefinitely thin spherical layer of the atomic atmosphere, whose distance from the nucleus is $\mathrm{R}u$, its thickness $\mathrm{R}du$, its area $4\pi\mathrm{R}^2u^2$, and its density

$$\frac{\mu}{\mathrm{M}}\,\mathrm{D}\psi\,(u, \mathrm{D}, \tau).$$

The weight, then, of this layer is

$$4\pi\mathrm{R}^3\,\frac{\mu}{\mathrm{M}}\,\mathrm{D}u^2\psi\,(u, \mathrm{D}, \tau)\,du.$$

Its velocity of oscillation is v, and having, in virtue of that velocity, a mean centrifugal force, as explained in the Introduction (Equation V.), equal to

$$\text{its weight} \times \left(\frac{v^2}{gk\mathrm{R}u} = \frac{2\mathrm{Q}}{k\mathrm{R}u}\right),$$

it is kept in equilibrio by an equal and opposite centripetal force, arising from attraction and elastic pressure, which is consequently represented by

$$4\pi\mathrm{R}^2\,\frac{\mu}{\mathrm{M}}\,\frac{v^2}{gk}\,\mathrm{D}u\psi\,(u, \mathrm{D}, \tau)\,du$$

$$= 8\pi\mathrm{R}^2\,\frac{\mu}{k\mathrm{M}}\,\mathrm{QD}u\psi\,(u, \mathrm{D}, \tau)\,du.$$

Let the mean density of the atom now be increased by the indefinitely small quantity $\delta\mathrm{D}$. Then the layer will approach the nucleus through the distance $-\delta(\mathrm{R}u) = -u\delta\mathrm{R} - \mathrm{R}\delta u$, and being acted upon through that distance by the centripetal force already stated, the *vis viva* of oscillation will be increased by a

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* In a subsequent paper, read to the Royal Society of Edinburgh on the 15th of December 1851, I have shown, that the same results, though by a more complicated analysis, are obtained, supposing the boundaries of the atoms to be of any figure whatsoever. The supposition that they are spherical, therefore, is to be regarded as merely an expedient to simplify calculation, and not as an essential part of the theory. (See Trans. Royal Soc. Edinb. vol. xx. part 3.)

quantity corresponding to the mechanical power (that is to say, the *heat*), represented by the product of that distance by that force, or by

$$-8\pi R^2 \frac{\mu}{kM} QDu\psi(u, D, \tau)du \times \delta(Ru)$$

$$= -8\pi R^3 \frac{\mu}{kM} QD\psi(u, D, \tau)u^2 \left(\frac{\delta R}{R} + \frac{\delta u}{u}\right)du,$$

which, because $\frac{\delta R}{R} = -\frac{1}{3} \cdot \frac{\delta D}{D}$, and $\frac{4\pi R^3 D}{3} = M$, is equal to

$$+QM \cdot \frac{2\mu}{kM}\psi(u, D, \tau)u^2 \left(\frac{\delta D}{D} \cdot\ 3\frac{\delta u}{u}\right)du.$$

We must suppose that the velocity of oscillation is equalized throughout the atomic atmosphere, by a propagation of motion so rapid as to be practically instantaneous.

Then if the above expression be integrated with respect to *du*, from $u=0$ to $u=1$, the result will give the whole increase of heat in the atom arising from the condensation δD; and dividing that integral by the atomic weight M, we shall obtain the corresponding development of heat in unity of weight. This is expressed by the following equation :—

$$-\delta Q' = 2Q \frac{\mu}{kM}\left\{\frac{\delta D}{D}\int_0^1 du \,.\, u^2\psi(u, D, \tau) \right.$$
$$\left. -3\int_0^1 du \,.\, u\delta u\psi(u, D, \tau)\right\} \quad . \quad . \quad . \quad . \quad (2)$$

The letter Q′ is here introduced to denote, when positive, that heat which is consumed in producing changes of volume and of molecular arrangement, and when negative, as in the above equation, the heat which is produced by such changes.

The following substitutions have to be made in equation (1) of this Section.

For Q is to be substituted its value, according to equation XII. of the Introduction; or abbreviating $Cn\mu b$ into κ,—

$$Q = \frac{3k}{2Cn\mu}(\tau - \kappa). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The value of the first integral in equation (2) of this Section is

$$\int_0^1 du \,.\, u^2\psi(u, D, \tau) = \frac{1}{3}.$$

The value of the second integral,

$$-3\int_0^1 du \,.\, u\delta u\psi(u, D, \tau),$$

remains to be investigated. The first step in this inquiry is given by the condition, that whatsoever changes of magnitude a given spherical layer undergoes, the portion of atmosphere between it and the nucleus is invariable. This condition is expressed by the equation

$$0=\left(\delta u\frac{d}{du}+\delta\tau\frac{d}{d\tau}+\delta D\frac{d}{dD}\right)\int_0^u du\,.\,u^2\psi(u,D,\tau), \quad . \quad (4)$$

from which it follows that

$$\delta u=-\frac{1}{u^2\psi(u,D,\tau)}\left(\delta\tau\frac{d}{d\tau}+\delta D\frac{d}{dD}\right)\int_0^u du\,.\,u^2\psi(u,D,\tau),$$

and consequently that

$$-3\int_0^1 du\,.\,u\delta u\psi(u,D,\tau)=$$
$$+\left(\delta\tau\,.\,\frac{d}{d\tau}+\delta D\frac{d}{dD}\right)3\int_0^1\frac{du}{u}\int_0^u du\,.\,u^2\psi(u,D,\tau).$$

Hence, making

$$9\int_0^1\frac{du}{u}\int_0^u du\,.\,u^2\psi(u,D,\tau)=U, \quad . \quad . \quad (5)$$

the second integral in equation (2) is transformed into

$$+\frac{1}{3}\left(\delta\tau\frac{d}{d\tau}+\delta D\frac{d}{dD}\right)U.$$

By means of those substitutions we obtain, for the mechanical value of the heat consumed in unity of weight of a fluid by any indefinitely small change of volume or of molecular distribution—

$$\left.\begin{array}{l}\delta Q'=-\frac{\tau-\kappa}{CnM}\left(\delta D\left(\frac{1}{D}+\frac{dU}{dD}\right)+\delta\tau\frac{dU}{d\tau}\right),\\ \text{or taking } V=\frac{1}{D} \text{ to denote the volume of unity}\\ \text{of weight of the substance,}\\ \delta Q'=\frac{\tau-\kappa}{CnM}\left(\delta V\left(\frac{1}{V}-\frac{dU}{dV}\right)-\delta\tau\frac{dU}{d\tau}\right)\end{array}\right\} \quad . \quad . \quad (6.)$$

Of this expression, the portion $-\frac{\tau-\kappa}{CnM}\cdot\frac{\delta D}{D}=\frac{\tau-\kappa}{CnM}\cdot\frac{\delta V}{V}$ represents the variation of heat arising from mere change of volume.

$-\frac{\tau-\kappa}{CnM}\delta V\frac{dU}{dV}=-\frac{\tau-\kappa}{CnM}\delta D\frac{dU}{dD}$, denotes the variation of heat produced by change of molecular distribution dependent on change of volume.

$-\frac{\tau-\kappa}{CnM}\delta\tau\frac{dU}{d\tau}$ expresses the variation of heat due to change of molecular distribution dependent on change of temperature.

7. The function U is one depending on molecular forces, the nature of which is yet unknown. The only case in which it can be calculated directly is that of a perfect gas. Without giving the details of the integration, it may be sufficient to state, that in this case

$$\left.\begin{array}{l} U=\frac{\kappa}{\tau}, \\ \text{and therefore that} \\ \frac{dU}{d\tau}=-\frac{\kappa}{\tau^2}; \qquad \frac{dU}{dV}=0. \end{array}\right\} \quad . \quad . \quad . \quad (7)$$

In all other cases, however, the value of this function can be determined indirectly, by introducing into the investigation the principle of the *conservation of vis viva*[*].

Suppose a portion of any substance, of the weight *unity*, to pass through a variety of changes of temperature and volume, and at length to be brought back to its primitive volume and temperature. Then the absolute quantity of heat in the substance, and the molecular arrangement and distribution, being the same as at first, the effect of their changes is eliminated; *and the algebraical sum of the* vis viva *expended and produced, whether in the shape of expansion and compression, or in that of heat, must be equal to zero*:—that is to say, if, on the whole, any mechanical power has appeared and been given out from the body in the form of expansion, an equal amount must have been communicated to the body, and must have disappeared in the form of heat; and if any mechanical power has appeared and been given out from the body in the form of heat, an equal amount must have been communicated to the body, and must have disappeared in the form of compression. This principle, expressed symbolically, is

$$\Delta\Pi-\Delta Q'=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where Π, when positive, represents expansive power given out, when negative, compressive power absorbed; and Q' represents, when negative, heat given out, when positive, heat absorbed.

To take the simplest case possible, let the changes of temperature and of volume be supposed to be indefinitely small, and to occur during distinct intervals of time, so that τ and V are independent variables. Let the initial absolute temperature be τ,

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[*] The function U is determined by direct integration in a paper already referred to on the Centrifugal Theory of Elasticity (Trans. Roy. Soc. Edinb. vol. xx. part 3).

the initial volume V, and the initial total elasticity P; and let the substance go through the following four changes:—

First. Let its temperature be raised from τ to $\tau+\delta\tau$, the volume remaining unchanged. Then the quantity of heat absorbed is

$$\delta\tau\left(\frac{dQ}{d\tau}-\frac{\tau-\kappa}{CnM}\frac{dU}{d\tau}\right),$$

and there is no expansion nor compression.

Secondly. Let the body expand, without change of temperature, from the volume V to the volume V+δV. Then the quantity of heat absorbed is

$$\delta V.\frac{\tau+\delta\tau-\kappa}{CnM}\left(\frac{1}{V}-\frac{d}{dV}.(U+\frac{dU}{d\tau}\delta\tau)\right),$$

while the power given out by expansion is

$$\delta V\,(P+\frac{dP}{d\tau}\delta\tau).$$

Thirdly. Let the temperature fall from $\tau+\delta\tau$ to its original value τ, the volume V+δV continuing unchanged; then the heat given out is

$$-\delta\tau\left(\frac{dQ}{d\tau}-\frac{\tau-\kappa}{CnM}\frac{d}{d\tau}(U+\frac{dU}{dV}\delta V)\right),$$

and there is no expansion nor compression.

Fourthly. Let the body be compressed, without change of temperature, to its original volume V; then the heat given out is

$$-\delta V\frac{\tau-\kappa}{CnM}\left(\frac{1}{V}-\frac{dU}{dV}\right),$$

while the power absorbed in compression is

$$-\delta V.P.$$

The body being now restored in all respects to its primitive state, the sum of the two portions of power connected with change of volume, must, in virtue of the principle of *vis viva*, be equal to the sum of the four quantities of heat. Those additions being made, and the sums divided by the common factor $\delta V\delta\tau$, the following equation is obtained:—

$$\frac{dP}{d\tau}=\frac{1}{CnM}\left(\frac{1}{V}-\frac{dU}{dV}\right). \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The integral of this partial differential equation is

$$U=\phi.\tau+\int dV\left(\frac{1}{V}-CnM\frac{dP}{d\tau}\right). \quad . \quad . \quad (10)$$

Now $\phi . \tau$ being the same for all densities, is the value of U for the perfectly gaseous state, or $\frac{\kappa}{\tau}$; for in that state the integral $=0$.

The values of the partial differential coefficients are accordingly

$$\left.\begin{aligned} \frac{d\mathrm{U}}{d\mathrm{V}} &= \frac{1}{\mathrm{V}} - \mathrm{C}n\mathrm{M}\frac{d\mathrm{P}}{d\tau} \\ \frac{d\mathrm{U}}{d\tau} &= -\frac{\kappa}{\tau^2} - \mathrm{C}n\mathrm{M}\int d\mathrm{V} . \frac{d^2\mathrm{P}}{d\tau^2} \end{aligned}\right\}^{*} \quad . \quad . \quad . \quad (11)$$

and they can, therefore, be determined in all cases in which the quantity $\kappa = \mathrm{C}n\mu b$, and the law of variation of the total elasticity with the volume and temperature are known, so as to complete the data required in order to apply equation 6 of this section to the calculation of the mechanical value of the variations of heat due to changes of volume and molecular arrangement.

The total elasticity of an imperfect gas, according to equations VI. and XII. of the introduction, being

$$\mathrm{P} = \frac{\tau}{\mathrm{C}n\mathrm{MV}}\left(1 - \mathrm{F}\left(\mathrm{D}, \frac{\tau}{\kappa}\right)\right) + f(\mathrm{D}),$$

its first and second partial differential coefficients with respect to the temperature are

$$\frac{d\mathrm{P}}{d\tau} = \frac{1}{\mathrm{C}n\mathrm{MV}}\left(1 - \left(1 + \tau\frac{d}{d\tau}\right)\mathrm{F}\left(\mathrm{D}, \frac{\tau}{\kappa}\right)\right),$$

$$\frac{d^2\mathrm{P}}{d\tau^2} = -\frac{1}{\mathrm{C}n\mathrm{MV}}\left(2\frac{d}{d\tau} + \tau\frac{d^2}{d\tau^2}\right)\mathrm{F}\left(\mathrm{D}, \frac{\tau}{\kappa}\right).$$

Consequently, for the imperfectly gaseous state;

$$\left.\begin{aligned} \mathrm{U} &= \frac{\kappa}{\tau} + \left(1 + \tau\frac{d}{d\tau}\right)\int d\mathrm{V} . \frac{\mathrm{F}\left(\mathrm{D}, \frac{\tau}{\kappa}\right)}{\mathrm{V}} \\ \frac{d\mathrm{U}}{d\mathrm{V}} &= \frac{1}{\mathrm{V}} \cdot \left(1 + \tau\frac{d}{d\tau}\right)\mathrm{F}\left(\mathrm{D}, \frac{\tau}{\kappa}\right) \\ \frac{d\mathrm{U}}{d\tau} &= -\frac{\kappa}{\tau^2} + \left(2\frac{d}{d\tau} + \tau\frac{d^2}{d\tau^2}\right)\int d\mathrm{V} . \frac{\mathrm{F}\left(\mathrm{D}, \frac{\tau}{\kappa}\right)}{\mathrm{V}} \end{aligned}\right\} . \quad (12)$$

(8.) It is to be observed, that the process followed in ascertaining the nature of the function U is analogous to that em-

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* The substitution of these values in equation 6 reduces it to the following form, which, being the more convenient, has been employed in most subsequent investigations :—

$$\delta\mathrm{Q}' = (\tau - \kappa) . \left\{\delta\tau\left(\frac{\kappa}{\mathrm{C}n\mathrm{M}\tau^2} + \int\frac{d^2\mathrm{P}}{d\tau^2}d\mathrm{V}\right) + \delta\mathrm{V} . \frac{d\mathrm{P}}{d\tau}\right\}.$$

ployed by M. Carnot in his theory of the motive power of heat, although founded on contrary principles, and leading to different results.

Carnot, in fact, considers heat to be something of a peculiar kind, whether a condition or a substance, the total amount of which in nature is incapable of increase or of diminution. It is not, therefore, according to his theory, convertible into mechanical power; but is capable, by its transmission through substances under particular circumstances, of causing mechanical power to be developed. He supposes a body to go through certain changes of temperature and volume, and to return at last to its primitive volume and temperature, and conceives, in accordance with his view of the nature of heat, that it must have given out exactly the same quantity of heat that it has absorbed. The transmission of this heat he regards as the cause of the production of an amount of mechanical power, depending on the quantity of heat transmitted and on the temperature at which the transmission has taken place. According to these principles, a body, having received a certain quantity of heat, is capable of giving out not only all the heat it has received, but also a quantity of mechanical power which did not before exist.

According to the theory of this Essay, on the contrary, and to every conceivable theory which regards heat as a modification of motion, no mechanical power can be given out in the shape of expansion unless the quantity of heat emitted by the body in returning to its primitive temperature and volume is *less* than the quantity of heat originally received; the excess of the latter quantity above the former disappearing as heat, to appear as expansive power, so that the sum of the *vis viva* in those two forms continues unchanged.

[To be continued.]

II. *On the dried Coffee-leaf of Sumatra, which is employed in that and some of the adjacent Islands as a substitute for Tea or for the Coffee-bean.* By John Stenhouse, *LL.D., F.R.S.*[*]

I RECENTLY received from my friend Daniel Hanbury, Jun., Esq., a quantity of dried coffee-leaves which had been prepared in Sumatra, under the direction of N. M. Ward, Esq. of Padang[†]. The sample had a deep brown colour, and consisted of the leaves of the coffee-tree mixed with fragments of the stalks. The leaves had been very strongly roasted in rather a rough manner, and had consequently acquired a slightly empy-

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[*] Communicated by the Author.

[†] See a paper by Mr. Hanbury in the 13th volume of the Pharmaceutical Journal, p. 207.

reumatic odour. In this respect they pretty closely resemble Paraguay tea, the leaves and twigs of the *Ilex paraguayensis*, which is subjected to a somewhat similar process. The coffee-leaves when digested with boiling water yielded a deep brown infusion, which in taste and odour closely resembled an infusion of a mixture of coffee and tea. On the addition of milk and sugar, it formed a very tolerable beverage; and as the roasted coffee-leaf can be imported into Europe for rather less than twopence per pound, the poorer classes of society are likely to find it a very useful substitute for tea and coffee. Should a more moderate temperature be employed in drying the coffee-leaf, I think its flavour would be greatly improved.

The coffee-leaf, as might almost have been expected, contains the two characteristic constituents of the coffee-bean, viz. theine or caffeine, and caffeic acid. In this respect the coffee-leaf differs essentially from chicory or any of its adulterations, such as roasted turnips, mangelwurzel, carrots, &c., the usual substitutes for coffee, which do not contain a trace of either of these principles.

The theine or caffeine was extracted from the coffee-leaves in the usual way, by precipitating the colouring matter and other impurities, first by acetate and then by subacetate of lead. The acetate of lead threw down a dark brown precipitate containing all the caffeic acid, and the subacetate produced a scanty bright yellow precipitate. The excess of lead was then removed from the clear solution by sulphuretted hydrogen, and the sulphide of lead having been collected by filtration, the theine crystallized when the liquid, after being sufficiently concentrated, had been set aside in a cool place for some days.

The crystals of theine as first obtained were of a brownish colour, but after being strongly pressed between folds of blotting-paper and repeatedly crystallized, they were rendered nearly colourless.

I. 1000 grains of dried coffee-leaves, when treated in the way just described, yielded 12·5 grains theine $=1·25$ per cent.

II. 1000 grains of coffee-leaves in a subsequent trial gave 11·54 grains $=1·15$ per cent.

The amount of nitrogen in the dried coffee-leaves was also determined by Will's method.

1·344 grm. substance gave 0·2005 platinum $=2·118$ per cent. nitrogen.

0·7775 grm. gave 0·1185 platinum $=2·165$ per cent.

Now it has been ascertained as the result of numerous experiments[*], that "coffee contains from 0·80 to 1 per cent. of theine,

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[*] See the 32nd page of "The Chemical Report on the mode of detecting Vegetable Substances mixed with Coffee for the purposes of adultera-

and that tea contains 2 per cent. of the same principle." "And the nitrogen in coffee-beans (see page 27 of the same Report) lies between $2\frac{1}{2}$ and 3 per cent."

By a recent determination I found that 1000 grains of a good black tea gave 21·3 grains of theine = 2·13 per cent.

1000 grains of a black tea grown in the East India Company's tea plantations at Kemaon on the Himalayas, gave 19·7 grains = 1·97 per cent.

0·4705 grm. of the same Kemaon tea gave 0·1175 platinum = 3·5 per cent. nitrogen.

Many years ago I detected the existence of theine in what is called Paraguay tea, the dried leaves and twigs of the *Ilex paraguayensis*, but I neglected to determine its amount. I have recently found that 1000 grains of Paraguay tea yielded 12·3 grains of theine = 1·23 per cent.

II. 1000 grains ditto gave 11 grains = 1·1 per cent.

1·748 grm. gave 0·1865 platinum = 1·51 nitrogen.

1·031 grm. gave 0·123 platinum = 1·70 nitrogen per cent.

From these results it is clear that dried coffee-leaves are somewhat richer in theine than the coffee-bean, and contain, as nearly as may be, the same amount of that principle as Paraguay tea.

From the violent roasting to which the coffee-leaves had been subjected, I feel convinced a portion of their theine has been dissipated; and were they only dried at a moderate temperature, I confidently expect that they would yield $1\frac{1}{2}$ per cent. of theine. The theine obtained from the coffee-leaves was not subjected to analysis. This I considered unnecessary, as it possessed all the well-known properties of ordinary theine, crystallizing in fine silky crystals, which readily sublimed when heated; and when digested with nitric acid and cautiously evaporated to dryness, they gave when treated with ammonia the characteristic red coloration so closely resembling that from uric acid when acted on by the same reagents.

With regard to caffeic acid, the other characteristic proximate principle of coffee, the leaf of the coffee plant contains it also in larger quantity than the berry. Caffeic acid is precipitated of a deep yellow colour by acetate of lead, but is apparently uncrystallizable; at least the numerous attempts which I have made to obtain it in a crystalline state have hitherto proved unsuccessful. Caffeic acid does not precipitate solutions of gelatine, and it is therefore not a species of tannin, as has been sometimes asserted. The most remarkable property of caffeic acid is that first stated at the 34th page of the joint Report on the adulteration of coffee already quoted. "Caffeic acid appears to be ana-

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tion," drawn up by Professor Graham, Dugald Campbell, Esq. and myself, for the British Government, and communicated to it in December 1852.

logous to kinic acid, the acid of cinchona barks, for it yields kinone when oxidated by means of sulphuric acid and binoxide of manganese. To observe this property, the coffee is boiled with water and a little slaked lime, the infusion filtered and evaporated down to the consistence of a syrup. The syrupy liquid is then mixed in a retort with four times its weight of binoxide of manganese and one part of oil of vitriol diluted with an equal bulk of water. Sufficient heat is produced by the action of the sulphuric acid upon the other materials to bring over the greater portion of the kinone, and the lamp need not be applied till towards the close of the operation. The distillate consists of yellow crystals of kinone, which usually coat the neck and sides of the retort, and a bright yellow liquid which is a saturated aqueous solution of kinone with a considerable quantity of formic acid. Kinone is easily discernible by its volatility and peculiarly acrid odour, which greatly resembles that of chlorine. The solution of kinone gives with ammonia a sepia black colour. It is decolorized by sulphurous acid. The beautiful green hydrokinone is obtained by exactly neutralizing the solution of the yellow kinone with sulphurous acid, great care being taken not to introduce the latter in excess.

"The peculiar acid in Paraguay tea agrees with caffeic acid, to which it is no doubt related, in yielding kinone to similar oxidizing agencies; so does the acid of the leaves of the common holly, *Ilex aquifolium*, and the whole of the Cinchona tribe."

When coffee-leaves are boiled with a considerable quantity of water and a slight excess of milk of lime, the dark brown strongly alkaline liquor is cautiously evaporated to dryness, and then treated in the way already described with three times its weight of black oxide of manganese and one part of sulphuric acid diluted with its own bulk of water, a much larger quantity of crystals of kinone are obtained than can be procured from an equal weight of the coffee-bean. This clearly shows that the leaves are richer in caffeic acid than the beans.

Kinone may, I find, be obtained in small quantity by a similar process from a great number of our commonest plants. Thus I also obtained indications of kinone from the leaves and branches of the privet, *Ligustrum vulgare*; from the common ivy, *Hedera helix*; from the *Quercus Ilex*, the evergreen oak of our gardens and shrubberies, a native of Turkey; from the *Quercus robur*, the common British oak; from the *Ulmus campestris*, the common small-leaved elm; from the ash, *Fraxinus excelsior*; and from the bush-tea of the Cape of Good Hope, the *Cyclopia latifolia* of DeCandolle, a plant of the natural order Leguminosæ.

From numerous plants which I tried I could not obtain a trace of kinone. This was the case among others with laburnum, *Cytisus laburnum,* with tobacco, with *Prunus spinosa,* the sloe, and others too numerous to mention.

The kinone was only obtained in crystals from the coffee-bean, from the coffee-leaf, and from the holly, the *Ilex aquifolium.* In all the other cases its existence was detected by the deep yellow liquid which distilled over at a comparatively low temperature, and which yielded the dark humus-like coloration with ammonia, so characteristic of solutions of kinone. Kinone is so exceedingly soluble in water, that unless a considerable amount of it is evolved, and proper precautions are employed, a deep-coloured solution of it only is obtained either from the coffee-leaf, the berry or the leaves of the holly. The comparatively small amount of the kinone yielding substance, or perhaps we should say substances, present in such plants as the privet, the oak, &c., is most probably the only reason why crystals of kinone have not likewise been obtained from these plants.

In order to assist in forming an estimate of the comparative value of coffee-leaves as a beverage as compared with the bean, I determined the amount of soluble matter which each of them yielded to boiling water. 6·048 grms. of dried coffee-leaves and 6·038 grms. of roasted and ground coffee-beans were repeatedly treated with precisely similar quantities of boiling water, till the liquid which came off from them was nearly colourless. The 6·048 grms. coffee-leaves were found to have lost 2·348 grms. =38·8 per cent., while the 6·038 grms. roasted coffee-beans had lost 1·759 grms. =29·1 per cent. From this determination it is clear that coffee-leaves yield to boiling water nearly 10 per cent. more soluble matter than the bean. In this respect, therefore, the coffee-leaf has an advantage over the berry.

So far as regards the two characteristic principles of coffee, viz. caffeic acid and theine or caffeine, these are common both to the leaf and the bean, the leaf being decidedly richer in both. In other respects, however, they differ considerably. The coffee-leaf contains some tannin, and scarcely any sugar or fat. The coffee-bean contains about 12 per cent. of fat and "8 per cent. of cane-sugar." (See Joint Report.)

So far as I can judge, the infusion of the coffee-leaf has a much greater resemblance to that of tea than to a decoction of the coffee-bean; so that should the coffee-leaf ever come into general use in European countries, it will be rather as a substitute for tea than for coffee. If the coffee-leaves were only dried at a somewhat lower and better regulated temperature, I have little doubt that they would yield a much more agreeable beverage than can be made with the present roughly-roasted and partially empyreumatized product.

Chemical Laboratory, St. Bartholomew's Hospital,
December 1, 1853.

III. *Examination of the Crystalline Deposit which forms in Oil of Bitter Almonds. By* JOHN STENHOUSE, *LL.D., F.R.S.**

WHEN oil of bitter almonds is kept for some time in loosely-stoppered bottles, a crystalline deposit not unfrequently forms in it, especially if the oil is exposed to the influence of the light. So long ago as 1823, M. Stange declared this crystalline deposit to be simply benzoic acid produced by the oxidation of the oil, a statement which was confirmed in 1830 by Robiquet and Boutron-Challard. (*Ann. de Chim. et de Phys.* vol. xliv. p. 364.) It does not appear, however, that any of these chemists subjected the supposed benzoic acid to the test of analysis. Subsequently the late Dr. Jonathan Pereira, in the first edition of his Elements of Materia Medica, page 1107, stated that he had met with "three kinds of a crystalline substance from oil of bitter almonds, differing essentially from each other and from benzoic acid." These deposits were examined by Dr. Pereira, and one of them, that marked No. 3, by Dr. Letheby. None of them were subjected to analysis, but they were tested by the action upon them of oil of vitriol, caustic potassa, &c. These crystalline deposits were given to Dr. Pereira by Mr. George Whipple, who kindly furnished me a few weeks ago with no fewer than ten different specimens of a similar character which had formed in as many portions of oil of bitter almonds. The weight of the crystals exceeded three ounces.

The crystals had a deep yellow colour, and were strongly impregnated with oil of bitter almonds. As much of the oil as possible was removed by strongly compressing the crystals between folds of blotting-paper. The crystals were then digested with boiling water, in which they readily dissolved. After repeated solutions and crystallizations they presented the usual characters of benzoic acid. On analysis, 0·4215 grm. substance dried at 212° F., when burnt in a current of oxygen gas, gave 1·069 carbonic acid and 0·2005 water =

	Calculated numbers.	Found numbers.
14 C . . .	68·853	69·167
6 H . . .	4·915	5·283
4 O . . .	26·232	25·460
	100·000	100·000

The silver salt was prepared by neutralizing the acid with ammonia and then adding a solution of nitrate of silver. When subjected to analysis, 0·3883 grm. salt gave 0·525 carbonic acid and 0·317 water.

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* Communicated by the Author.

1·1711 grm. salt gave 0·5516 silver.

	Calculated numbers.	Found numbers.
AgO	50·654	50·533
14 C	36·684	36·909
5 H	2·183	2·317
3 O	10·479	10·241
	100·000	100·000

From these results it is clear that the crystalline deposits from oil of bitter almonds examined by me were simply benzoic acid, and this I have reason to believe was also the case with two out of the three samples described by Dr. Pereira; for through the kindness of Professor Redwood I obtained three small specimens of these deposits from Dr. Pereira's collection, now in the possession of the Pharmaceutical Society. That marked No. 1 I found to be simply benzoic acid. I received also a specimen given to the Pharmaceutical Society by Mr. Whipple; it was also benzoic acid. The specimen marked No. 3, and which amounted only to a few grains in weight, was that examined by Drs. Pereira and Letheby. It consisted of small, acicular, lemon-yellow crystals, which were insoluble in water, alcohol, æther and acetic acid. Sulphuric acid reddened and then dissolved them, forming a deep yellow solution. They were not dissolved by a solution of caustic potassa in the cold; when heated with it, however, ammonia was evolved, which proved that the crystals contained nitrogen. When the crystals were strongly heated on platinum foil they melted and were decomposed, a small amount of sublimate only being produced.

The origin of the crystalline deposit No. 3 is very obscure. Mr. Whipple informs me that he only obtained it on one occasion, many years ago, when the essential oil of almonds was prepared by distillation over the naked fire; but ever since the operation has been effected by means of a steam apparatus, he has never met with it again, and has merely obtained deposits of benzoic acid. I strongly suspect that the yellow insoluble crystals marked No. 3 were produced by the action of ammonia on oil of bitter almonds, and that the ammonia on that occasion was probably evolved by the charring of a portion of the nitrogenous pulp of the almonds by the overheating of the bottom of the retort. The crystalline deposits which form in oil of bitter almonds must be regarded therefore (unless in the single somewhat inexplicable instance just stated) as simply benzoic acid resulting from the oxidation of the oil.

IV. *On Xanthoxyline, a new Crystalline principle from Japanese Pepper, the fruit of the* Xanthoxylum piperitum *of* DeCandolle. *By* JOHN STENHOUSE, *LL.D., F.R.S.**

A FEW months ago I received from my friend Daniel Hanbury, Jun., Esq., a small quantity of a very singular fruit from Japan, known in commerce as Japanese pepper. Mr. Hanbury likewise kindly furnished me with the subjoined botanical account of the tree from which it is obtained.

"Japanese pepper is the produce of *Zanthoxylum piperitum*, DeCand. (*Fagara piperita*, Linn.), a tree of Japan of the natural order *Rutaceæ*, figured and described by Kæmpfer in 1712†.

"It consists of roundish sessile capsules of the size of a peppercorn, which appear to have been normally four in number, situated at the extremity of a peduncle, though but one or two are usually fully developed. The capsules, which are externally reddish-brown, have their outer covering beset with numerous prominent tubercles enclosing an acrid liquid to which the pepper owes its pungent flavour. The seeds are black, shining, and devoid of pungency; from dehiscence of the capsules, they are generally wanting in the sample which we have examined.

"The flavour of Japanese pepper is aromatic and agreeable, with a pungency not unlike that of pellitory (*Radix pyrethri*). Its odour when bruised is remarkably fragrant.

"It is employed as a condiment by the Japanese and Chinese."

The Japanese pepper, after being reduced to a coarse powder, was digested with successive portions of spirits of wine till it was rendered nearly tasteless; the greater portion of the spirit was then drawn off by distillation, and the residue on standing for a few days became filled with dark-coloured crystals of considerable size. Their colour was owing to the presence of a resinous matter, which adhered to them very tenaciously. The best way of removing this dark-coloured resin is to wash the crystals with cold liquor ammoniæ, which dissolves the resin, leaving the crystals unacted on. The crystals were rendered quite white, and were easily obtained an inch in length by repeatedly dissolving them in æther, or in a mixture of alcohol and æther.

Through the kindness of Professor W. H. Miller of Cambridge, I am enabled to subjoin the following figure and description of the crystalline form of xanthoxyline.

The crystals of xanthoxyline belong to the oblique system.

The angles between normals to the faces are:—

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* Communicated by the Author.

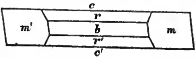
† *Amœnitates*, pp. 892–895.

	°	′
bc	90	0
rb	37	10
rc	52	50
mb	58	50
mc	83	30

Cleavage. *b*, very perfect and easily obtained; *c*, perfect.

The crystals are extremely thin in a direction perpendicular to the face *c*.

The faces *r*, *m* were so imperfect that the angles they make with *b* and *c* could not be determined with much accuracy.



Xanthoxyline is quite insoluble either in cold or hot water, but it readily dissolves in alcohol or æther. Its solutions are quite neutral to test-paper, and do not appear to possess either acid or basic properties. The crystals of xanthoxyline melt at 80° C. Their taste is aromatic and resinous, not unlike that of elemi or olibanum. The amount of xanthoxyline in Japanese pepper is very considerable. A quantity of xanthoxyline, after being dried at 100° Centigrade, was subjected to analysis in the usual way.

I. 0·179 grm. substance gave 0·401 CO^2 and 0·104 water.

II. 0·1142 grm. substance gave 0·2558 CO^2 and 0·07 water.

	I.	II.
Carbon . .	61·09	61·09
Hydrogen .	6·45	6·8

I subsequently found that xanthoxyline contains a small quantity of nitrogen, the amount of which I was unable to determine from want of material. I confidently expect to obtain in the course of a few months a considerable quantity of Japanese pepper, when I hope to be able to complete its analysis. Notwithstanding that xanthoxyline contains nitrogen, it does not exhibit any basic properties, but in its general properties considerably resembles the so-called stearoptens.

V. *Solution of a Question in the Theory of Probabilities. By* GEORGE BOOLE, *LL.D., Professor of Mathematics in Queen's College, Cork**.

THE question considered by Mr. Cayley in the Philosophical Magazine for October 1853, p. 259, is a particular case of a problem proposed by me in the Cambridge and Dublin Mathe-

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* Communicated by the Author.

matical Journal in the month of November 1851. This may justify my offering a few remarks upon the subject.

The problem as considered by Mr. Cayley is thus stated.

Given the probability α that a cause A will act, and the probability p that A acting the effect will happen; also the probability β that a cause B will act, and the probability q that B acting the effect will happen: required the total probability of the effect, supposed impossible in the absence of both the causes. The solution given by Mr. Cayley is as follows. Let λ be the probability that the cause A acting will act efficaciously, μ the probability that the cause B acting will act efficaciously. Then

$$p=\lambda+(1-\lambda)\mu\beta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$q=\mu+(1-\mu)\lambda\alpha, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which determines λ, μ, and the total probability u of the effect will be given by

$$u=\lambda\alpha+\mu\beta-\lambda\mu\alpha\beta. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Mr. Cayley shows that this leads to a correct result when $\alpha=1$. He further remarks, that the problem presents no difficulty.

I think it to be one of the peculiar difficulties of the theory of probabilities, that its difficulties sometimes are not seen. The solution of a problem may appear to be conducted according to the principles of the theory as usually stated; it may lead to a result susceptible of verification in particular instances; and yet it may be an erroneous solution. The problem which Mr. Cayley has considered seems to me to afford a good illustration of this remark. Several attempts at its solution have been forwarded to me, all of them by mathematicians of great eminence, all of them admitting of particular verification, yet differing from each other and from the truth. Mr. Cayley's solution is the only published one I have seen, and I feel I must extend to it the same observations. But in doing this, I willingly add that I have two or three times attempted to solve the problem *by the same kind of reasoning*, and have not approached so near the truth as Mr. Cayley has done. To illustrate these remarks, I will first complete Mr. Cayley's solution, and give one or two apparent verifications, then exhibit the true solution; and lastly, make a few observations upon the general subject.

1st. To complete Mr. Cayley's solution, eliminate λ and μ from (1), (2), and (3); the result is

$$\frac{(1-\alpha(1-p)-u)(1-\beta(1-q)-u)}{1-u}=(1-\alpha)(1-\beta), \quad . \quad (4)$$

a quadratic equation from which u must be determined. Suppose that $p=1$ and $q=1$, the above equation gives

$$1-u=(1-\alpha)(1-\beta),$$
$$\therefore\ u=1-(1-\alpha)(1-\beta),$$

a result obviously correct. For if either cause necessarily produces the effect, the probability of the effect is equal to the probability that both the causes will not be absent.

Again, suppose that α and β are very small, so that their product may be neglected, then the solution gives

$$(1-u)^2-\big(\alpha(1-p)+\beta(1-q)\big)(1-u)=(1-\alpha)(1-\beta)(1-u),$$

whence we find

$$u=\alpha p+\beta q.$$

Now this is the known form of the solution when the causes are mutually exclusive, so that they cannot coincide. But the smaller their separate probabilities, the smaller is the probability of their coincidence. And in the limit the two probabilities correspond.

I suppose that few persons, after reading Mr. Cayley's solution and noticing the above verifications, would feel any doubt of its correctness; and yet it is certainly erroneous. Take the particular cases of $p=1$ and $q=0$. It is evident that the probability of the effect ought then to be α. If the cause A always produces the effect, and the cause B never, the probability of the effect, there being no other causes to which it can be ascribed, will be equal to that of the cause A. The equation (4), however, becomes on clearing of fractions,

$$(1-u)(1-\beta-\alpha)=(1-\alpha)(1-\beta)(1-u),$$

whence either $u=1$, or

$$1-\beta-u=(1-\alpha)(1-\beta),$$

wherefore

$$u=(1-\beta)-(1-\quad)(1-\beta)$$
$$=\alpha(1-\beta).$$

But neither of these results is correct.

2nd. I will now exhibit the true solution of the problem. Representing, as before, by u the probability of the effect, the value of that quantity will be determined by the solution of the quadratic equation

$$\frac{(1-\alpha(1-p)-u)(1-\beta(1-q)-u)}{1-u}=\frac{(u-\alpha p)(u-\beta q)}{\alpha p+\beta q-u}. \quad . \quad (5)$$

Of this equation that root must be taken which is at the same time not less than each of the two quantities

$$\alpha p \text{ and } \beta q, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and not greater than any one of the three quantities

$$1-\alpha(1-p), \quad 1-\beta(1-q), \quad \alpha p+\beta q. \quad . \quad . \quad (7)$$

Moreover, whenever the data of the problem are *real, i. e.* represent a possible experience, there will exist one root, and only one root, of the above equation satisfying the conditions described. The conditions of a possible experience are, that each of the three quantities in (7) exceeds each of the two in (6).

The above solution may readily be verified in the case considered by Mr. Cayley, and in the three cases discussed in this paper. Of these I will here confine myself to the last.

Clearing the equation of fractions, and making $p=1$, $q=0$, we have

$$(1-u)(1-\beta-u)(\alpha-u)=(1-u)(u-\alpha)u.$$

This equation is satisfied by the values

$$u=\alpha \quad u=1;$$

but the conditions above stated restrict our choice to the first, which we have before seen to be the true one.

3rd. Upon the nature of the errors which are most to be apprehended in the solution of questions in the theory of probabilities, I will only remark that they are not usually mathematical, in the ordinary sense of that term, but arise from the necessity of employing a logic of a peculiarly subtle or highly complex character. When the data are the probabilities of independent simple events, the method of procedure is sufficiently easy; but if those data relate to events occurring in combinations, or connected by causal relations, the principles which suffice for the former case become either inadequate or inapplicable. Laplace has to some extent investigated the additional new principles (derivable from the prior definitions and axioms of the science) of which it is then necessary to take account. But all these aids carry us but a short way in advance; and of this I am fully assured, that no *general* method for the solution of questions in the theory of probabilities can be established which does not explicitly recognize, not only the special numerical bases of the science, but also those universal laws of thought which are the basis of all reasoning, and which, whatever they may be as to their essence, are at least mathematical as to their form. Such a method I have exhibited in a treatise now on the eve of publication, and to which I must refer for the investigation of the problem, the solution of which has been exemplified in this paper.

5 Grenville Place, Cork,
Nov. 30, 1853.

VI. *Reports on the Progress of the Physical Sciences.* *By* Dr. JOHN TYNDALL, *F.R.S.*

On the Conductibility of Metals for Heat. By G. WIEDEMANN *and* R. FRANZ, Poggendorff's *Annalen*, vol. lxxxix. p. 497.

[With a Plate.]

TO the sense of touch as a thermoscope in experiments on the conduction of heat succeeded the expedient of Ingenhousz, which consisted in coating rods of the substances to be examined with wax, bringing one end into connexion with a source of heat and observing the distance to which the wax melted on the surface. Exact numerical results were, however, wanting until Despretz took up the subject nearly thirty years ago. In his experiments the bodies examined were reduced to the shape of prismatic bars; in each bar, and at equal distances apart, cavities were hollowed out for the reception of mercury, into which dipped the bulbs of a number of sensitive thermometers. One end of the bar was heated by a lamp with a constant flame, and the heating was suffered to continue until the temperature of the bar became constant. This of course took place when the amount of heat received by any cross section of the bar from the section immediately preceding, was equal to the quantity imparted to the following section and lost by radiation in the same time. The thermometers under these circumstances formed a series decreasing in height from the hottest end of the bar onwards. From the temperatures thus observed, it is possible, by means of Fourier's formula, to calculate the conductive power of the bar for heat. In these experiments, however, the breach of continuity caused by the introduction of the cavities and their contents must have exerted a disturbing influence. In the subsequent experiments of Langberg this difficulty was avoided by the application of a thermo-electric element, which was caused to press against the heated bar; the temperature being estimated from the deflection which it produced upon a suitable galvanometer. A little reflection, however, will suggest various expedients by which the accuracy of the mode of observation introduced by Langberg may be increased; and this appears to have led to the recent resumption of the subject by MM. Wiedemann and Franz, whose careful experiments constitute the matter of the present report.

In an inquiry of this nature it is necessary that the exterior conductivity, as it is called, should be the same in all the substances examined. Despretz secured this by coating all his bars with the same description of varnish, thus imparting to them the same radiative power. In the experiments of Wiedemann and Franz the same object is attained by silvering the bars. As the latter were thin, a very necessary precaution was to shelter them from currents of air, and to preserve the surrounding air at a constant temperature. The arrangement also permitted of the bars being examined *in vacuo*. The following description will, we doubt not, render the manner of experiment sufficiently plain without having recourse to a figure.

Each bar was caused to occupy the axis of a cylindrical vessel of glass, being retained in this position by causing the ends of the bar to rest in two tubes fixed in the ends of the cylinder. The tube at one end was extended, and surrounded by a space which communicated with a vessel of boiling water. When this space was filled with steam, heat was communicated to the tube and to the bar within it, whence it propagated itself through the mass of the bar. Through the opposite end of the glass cylinder, and at some distance from its centre, a rod was caused to pass which was capable of sliding up and down air-tight. To the end of this rod was attached the thermo-electric element, which by turning the rod could be caused to press against the heated bar. Two thin wires, one of iron, the other of German silver, formed the thermo-electric element, the wires being cut across obliquely and thus soldered together. The moveable rod was graduated, and a fixed index was attached to the end of the cylinder; it was thus in the experimenter's power to cause the thermo-electric element to press against the bar at any required distance from its heated end. The points of the bar thus examined were at a constant distance of two inches apart. By means of a tube which communicated with the interior of the glass cylinder, the latter could be connected with an air-pump and exhausted. During the experiments the entire cylinder was immersed in a vessel of water, which was preserved at a constant temperature of 12° C.

The following tables contain the results of the experiments with the substances therein mentioned. To facilitate comparison, the temperature of the zero-point of each bar is reduced to 100 by multiplying each entire series of observations by a suitable factor. The figures under x represent the distances reckoned from zero, and taken, as before remarked, from two to two inches; the adjacent figures represent the corresponding temperatures, and the last column in each case contains the mean of experiments made with different bars.

I. *Experiments with the cylindrical vessel full of Air.*

1. Silver, 5 millims. thick.

x.				Mean.
0	100·0	100·0	100·0	100·0
2	78·2	78·1	78·1	78·1
4	61·1	60·6	61·0	60·9
6	48·0	46·7	47·0	47·2
8	37·4	36·5	36·4	36·8
10	27·7	28·5	27·5	27·9
12	19·0	21·9	20·3	20·4
14	12·2	16·6	……	14·4

2. Copper, 5 millims. thick.

x.						Mean.
0	100·0	100·0	100·0	100·0	100·0	100·0
2	75·4	75·6	76·2	76·1	76·0	75·8
4	57·6	56·1	58·0	58·1	57·4	57·4
6	43·7	41·5	44·6	44·4	42·4	43·3
8	32·6	30·5	32·9	33·5	31·7	32·2
10	23·0	21·6	23·6	24·1	22·4	23·0
12	15·9	14·1	16·3	16·4	14·9	15·5
14	9·8	……	……	……	……	9·8

3. Gold, 5 millims. thick. 4. Brass, 5 millims. thick.

x.			Mean.				Mean.
0	100·0	100·0	100·0	100·0	100·0	100·0	100·0
2	74·5	74·1	74·3	61·4	60·7	60·9	61·0
4	53·1	53·6	53·3	38·8	35·9	37·6	37·4
6	37·2	38·9	38·0	24·6	23·3	25·6	24·5
8	25·6	27·1	26·3	15·7	12·7	17·3	16·5
10	15·7	18·1	16·9	9·2	……	……	9·2
12	……	10·8	10·8	4·3	……	……	4·3

5. Iron, 5 millims. thick. 6. Steel, 5 millims. thick. 7. Platinum, 4·75 millims. thick.

x.			Mean.			Mean.	
0	100·0	100·0	100·0	100·0	100·0	100·0	100·0
2	48·8	50·0	49·4	47·4	50·1	48·7	44·0
4	25·6	25·8	25·7	24·4	24·8	24·6	20·4
6	14·2	13·4	13·8	14·0	13·4	13·7	10·1
8	7·1	6·9	7·0	7·3	6·6	6·9	5·7
10	2·8	3·3	3·0	3·4	3·0	3·2	

8. German silver, 5 millims. thick. 4*b*. Brass, 6·2 millims. thick. 9. Tin, 6·2 millims. thick.

x.	8. German silver	4*b*. Brass	9. Tin		Mean.
0	100·0	100·0	100·0	100·0	100·0
2	39·1	65·1	57·4	58·1	57·7
4	16·2	42·1	33·8	35·2	34·5
6	6·4	25·9	19·7	21·3	20·5
8	2·0	14·7	11·7	12·6	12·1
10			8·0	7·2	7·6
12			4·6	3·8	4·2

10. Lead, 6·2 millims. thick. 11. Rose's fusible metal, 6 millims. thick. 12. Bismuth, 6 millims. thick.

x.	10. Lead	11. Rose's fusible metal	12. Bismuth
0	100·0	100·0	100·0
2	47·8	29·7	17·9
4	23·9	10·2	4·4
6	12·1	4·1	
8	6·4		
10	3·4		
12	1·2		

II. *Experiments with cylinder exhausted.*

1. Silver, 5 millims. thick. 2. Copper, 5 millims. thick. 3. Gold, 5 millims. thick.

x.	1. Silver		Mean.	2. Copper	3. Gold		Mean.
0	100·0	100·0	100·0	100·0	100·0	100·0	100·0
2	86·8	86·1	86·4	82·0	82·9	82·9	82·9
4	74·0	73·7	73·9	68·8	67·5	67·5	67·5
6	64·1	62·9	63·5	56·8	54·1	53·4	53·7
8	54·1	53·6	53·9	45·3	43·1	43·4	43·3
10	45·4	45·6	45·5	35·0	33·6	33·6	33·6
12	37·9	38·9	38·4	25·6	……	23·8	23·8
14	30·8	32·4	31·6		……	16·0	16·0

4. Brass, 5 millims. thick. 5. Iron, 5 millims. thick. 6. Steel, 5 millims. thick.

x.	4. Brass	5. Iron		Mean.	6. Steel		Mean.
0	100·0	100·0	100·0	100·0	100·0	100·0	100·0
2	78·0	67·4	66·7	67·1	63·9	64·2	64·0
4	59·5	45·3	43·7	44·5	42·6	40·8	41·7
6	46·0	29·8	29·4	29·6	27·4	26·5	26·9
8	35·0	19·8	18·2	19·0	18·3	16·2	17·2
10	27·0	14·1	11·2	12·6	10·9	……	10·9
12	20·5						
14	15·5						

7. Platinum, 4·75 millims. thick. 8. German silver, 5 millims. thick. 4*b*. Brass. 6·2 millims. thick.

x.			Mean.		
0	100·0	100·0	100·0	100·0	100·0
2	63·5	64·8	64·2	63·1	79·2
4	40·7	43·7	42·2	38·0	61·2
6	29·1	29·9	29·0	24·0	47·0
8	20·2	20·3	20·2	15·8	35·2
10	12·9	14·1	13·4	8·9	25·8
12	8·6	10·9	9·6	5·6	19·1
14				3·3	12·3

9. Tin, 6·2 millims. thick. 10. Lead, 6·2 millims. thick. 11. Rose's fusible metal, 6 millims. thick.

x.			Mean.			Mean.	
0	100·0	100·0	100 0	100·0	100·0	100·0	100·0
2	72·8	73·7	73·2	66·5	63·9	65·2	60·1
4	53·5	54·8	54·2	44·6	41·5	43·1	36·2
6	39·1	39·9	39·5	27·9	26·5	27·2	22·9
8	28 5	28·5	28·5	18·6	16 6	17·6	15·6
10	20·4	20·6	20·5	11·7	9·7	10·7	8·5
12	14·0	14·5	14·2	6·6	4·5	5·6	5·3
14	9·0	8·8	8·9				

Taking the bar experimented with in each case as a datum line, marking the points at which the observations were made along it, and erecting at each point a perpendicular to express the temperature at that point, we obtain, by uniting the ends of all the perpendiculars, a curve which represents the manner in which the heat is distributed throughout the bar. In Plate I. this process is carried out for each bar, the curves which refer to the thicker bars being distinguished from the others by being drawn in dotted lines.

Remembering that the steepest curves belong to the substances which conduct heat the most imperfectly, we obtain from the diagram the order of conductivity as follows:—

I. *Thin bars.*

Silver,
Copper,
Gold,
Brass,
Iron,
Steel,
Platinum,
German silver.

II. *Thicker bars.*
Brass,
Tin,
Lead,
Rose's metal,
Bismuth.

The foregoing results enable us, however, to give numerical expression to the relative conductivity of the various bars. If a metallic rod with the transverse section Ω and periphery γ be heated at two points above the surrounding medium, and if the temperature v_0, v_1, v_2 of three different points which are separated from each other and a constant interval α be determined, then, according to Fourier, we have the following relation:—

$$q=\frac{v_0+v_2}{v_1}=e^{-\alpha\sqrt{\frac{h\gamma}{l\Omega}}}+e^{\alpha\sqrt{\frac{h\gamma}{l\Omega}}},$$

where l denotes the interior, and h the exterior conductivity of the rod.

Setting $e^{\alpha\sqrt{\frac{h\gamma}{l\Omega}}}=x$, we have $x+\frac{1}{x}=q$, and

$$l=\frac{h\gamma}{\Omega}\alpha^2\frac{1}{(\log x)^2}.$$

In the experiments at present under consideration, the distance α, and also the exterior conductivity h, are in all cases the same. Further, the cross section of the bar is a circle; and if its thickness $=d$, we have $\frac{\gamma}{\Omega}=\frac{1}{d}$. If two different bars, therefore, possess the conductivities l and l_1, the thicknesses d and d_1, and the values x and x_1 calculated from the observed quotients q and q_1, we have

$$l_1=l\,.\,\frac{d}{d_1}\cdot\frac{(\log x)^2}{(\log x_1)^2}.$$

It is easy to see that a very small alteration of the quotient q is sufficient to cause a considerable change in the conductivity l calculated from it. If, for example, the quotient varies between the numbers 2·032 and 2·035, the calculated conductivities change from 152 to 166. Since, therefore, a small error of observation carries along with it a large alteration of the calculated conductivity, the numerical values obtained from the quotients in question must not be considered more than approximate. The curves to which we have already referred the reader are certainly calculated to give a more correct notion of the distribution throughout the different bars than the numbers derived from the application of the foregoing formula. Nevertheless, as a numerical comparison may be desirable, we here present it to the reader. In the following table the conductivity of silver is assumed to be 100, and certain trifling corrections are introduced for the purpose of translating the indications of the galvanometer into those of the ordinary thermometer:—

Metals.	Cylinder full of air.		Cylinder exhausted.	
	q.	*t*.	*q*.	*t*.
Silver	2·0456	100	2·0145	100
Copper	2·062	73·6	2·0195	74·8
Gold	2·086	53·2	2·027	54·8
Brass	2·200	23·1	2·058	25·0
Brass 2 (thick) ...	2·154	24·1	2·051	23·0
Tin	2·264	14·5	2·076	15·4
Iron	2·393	11·9	2·144	10·1
Steel	2·405	11·6	2·1395	10·3
Lead	2·445	8·5	2·149	7·9
Platinum	2·597	8·4	2·163	9·4
German silver ...	2·772	6·3	2·201	7·3
Rose's metal	3·434	2·8	2·441	2·8
Bismuth	4·565	1·8		

Twenty years ago Professor Forbes was led to suspect that those metals which conducted heat most perfectly were also the best conductors of electricity*. We here transcribe a table, comparing the results arrived at by Wiedemann and Franz on heat, with those obtained by Riess, Becquerel and Lenz, upon electricity.

Name of body.	Conductivity for electricity.			Conductivity for heat.
	Riess.	Becquerel.	Lenz.	Wiedemann and Franz.
Silver	100	100	100	100
Copper	66·7	91·5	73·8	73·6
Gold	59·0	64·9	58·5	53·2
Brass	18·4	......	21·5	23·6
Tin	10·0	14·0	22·6	14·5
Iron	12·0	12·35	13·0	11·9
Steel	......	......	......	11·6
Lead	7·0	8·27	10·7	8·5
Platinum	10·5	7·93	10·3	8·4
German silver ...	5·9	......	......	6·3
Bismuth	......	......	1·9	1·8

Whatever the quality may be upon which calorific conduction depends, the above table renders it exceedingly probable that the same quality influences in a similar manner the transmission of electricity; for the divergences of the numbers expressing the conductivity for heat from those expressing the conductivity for electricity are not greater than the divergences of the latter alone, exhibited by the results of the different observers.

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* Phil. Mag. S. 3. vol. iv. p. 27.

VII. *On the Theory of Groups, as depending on the Symbolic Equation* $\theta^n=1$. *By* A. Cayley, *Esq.*[*]

Let θ be a symbol of operation, which may, if we please, have for its operand, not a single quantity x, but a system $(x, y \ldots)$, so that

$$\theta(x, y \ldots) = (x', y' \ldots).$$

Where x', y' .. are any functions whatever of $x, y \ldots$, it is not even necessary that x', y' .. should be the same in number with $x, y \ldots$ In particular x', y', &c. may represent a permutation of x, y, &c. θ is in this case what is termed a substitution; and if, instead of a set x, y .., the operand is a single quantity x, so that $\theta x = x' = fx$, θ is an ordinary functional symbol. It is not necessary (even if this could be done) to attach any meaning to a symbol such as $\theta \pm \phi$, or to the symbol 0, nor consequently to an equation such as $\theta = 0$, or $\theta \pm \phi = 0$; but the symbol 1 will naturally denote an operation which (either generally or in regard to the particular operand) leaves the operand unaltered, and the equation $\theta = \phi$ will denote that the operation θ is (either generally or in regard to the particular operand) equivalent to ϕ, and of course $\theta = 1$ will in like manner denote the equivalence of the operation θ to the operation 1. A symbol $\theta\phi$ denotes the compound operation, the performance of which is equivalent to the performance, first of the operation ϕ, and then of the operation θ; $\theta\phi$ is of course in general different from $\phi\theta$. But the symbols θ, ϕ .. are in general such that $\theta . \phi\chi = \theta\phi . \chi$, &c., so that $\theta\phi\chi$, $\theta\phi\chi\omega$, &c. have a definite signification independent of the particular mode of compounding the symbols; this will be the case even if the functional operations involved in the symbols θ, ϕ, &c. contain parameters such as the quaternion imaginaries i, j, k; but not if these functional operations contain parameters such as the imaginaries which enter into the theory of octaves, &c., and for which, *e. g.* $\alpha . \beta\gamma$ is something different from $\alpha\beta . \gamma$, a supposition which is altogether excluded from the present paper. The order of the factors of a product $\theta\phi\chi$ .. must of course be attended to, since even in the case of a product of two factors the order is material; it is very convenient to speak of the symbols θ, ϕ .. as the first or furthest, second, third, &c., and last or nearest factor. What precedes may be almost entirely summed up in the remark, that the distributive law has no application to the symbols $\theta\phi$ ..; and that these symbols are not in general convertible, but are associative. It is easy to see that $\theta^0 = 1$, and that the index law $\theta^m . \theta^n = \theta^{m+n}$, holds for all positive or negative integer values, not excluding 0. It should

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[*] Communicated by the Author.

be noticed also, that if $\theta=\phi$, then, whatever the symbols α, β may be, $\alpha\theta\beta=\alpha\phi\beta$, and conversely.

A set of symbols,

$$1,\ \alpha,\ \beta\ldots$$

all of them different, and such that the product of any two of them (no matter in what order), or the product of any one of them into itself belongs to the set, is said to be a *group**. It follows that if the entire group is multiplied by any one of the symbols, either as further or nearer factor, the effect is simply to reproduce the group; or what is the same thing, that if the symbols of the group are multiplied together so as to form a table, thus :—

		Further factors.			
		1	α	β	..
Nearer factors.	1	1	α	β	..
	α	α	α^2	$\beta\alpha$	
	β	β	$\alpha\beta$	β^2	
		$\vdots$			

that as well each line as each column of the square will contain all the symbols $1,\ \alpha,\ \beta\ldots$ It also follows that the product of any number of the symbols, with or without repetitions, and in any order whatever, is a symbol of the group. Suppose that the group

$$1,\ \alpha,\ \beta\ \ldots$$

contains n symbols, it may be shown that each of these symbols satisfies the equation

$$\theta^n=1\,;$$

so that a group may be considered as representing a system of roots of this symbolic binomial equation. It is, moreover, easy to show that if any symbol α of the group satisfies the equation $\theta^r=1$, where r is less than n, then that r must be a submultiple of n; it follows that when n is a prime number, the group is of necessity of the form

$$1,\ \alpha,\ \alpha^2\ldots\alpha^{n-1},\quad(\alpha^n=1).$$

And the same may be, but is not necessarily the case, when n is a composite number. But whether n be prime or composite, the group, *assumed to be of the form in question*, is in

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* The idea of a group as applied to permutations or substitutions is due to Galois, and the introduction of it may be considered as marking an epoch in the progress of the theory of algebraical equations.

every respect analogous to the system of the roots of the ordinary binomial equation $x^n-1=0$; thus, when n is prime, all the roots (except the root 1) are prime roots; but when n is composite, there are only as many prime roots as there are numbers less than n and prime to it, &c.

The distinction between the theory of the symbolic equation $\theta^n=1$, and that of the ordinary equation $x^n-1=0$, presents itself in the very simplest case $n=4$. For, consider the group

$$1,\ \alpha,\ \beta,\ \gamma,$$

which are a system of roots of the symbolic equation

$$\theta^4=1.$$

There is, it is clear, at least one root β, such that $\beta^2=1$; we may therefore represent the group thus,

$$1,\ \alpha,\ \beta,\ \alpha\beta,\quad (\beta^2=1);$$

then multiplying each term by α as further factor, we have for the group $1,\ \alpha^2,\ \alpha\beta,\ \alpha^2\beta$, so that α^2 must be equal either to β or else to 1. In the former case the group is

$$1,\ \alpha,\ \alpha^2,\ \alpha^3,\quad (\alpha^4=1),$$

which is analogous to the system of roots of the ordinary equation $x^4-1=0$. For the sake of comparison with what follows, I remark, that, representing the last-mentioned group by

$$1,\ \alpha,\ \beta,\ \gamma,$$

we have the table

	1,	α,	β,	γ
1	1	α	β	γ
α	α	β	γ	1
β	β	γ	1	α
γ	γ	1	α	β

If, on the other hand, $\alpha^2=1$, then it is easy by similar reasoning to show that we must have $\alpha\beta=\beta\alpha$, so that the group in the case is

$$1,\ \alpha,\ \beta,\ \alpha\beta,\quad (\alpha^2=1,\ \beta^2=1,\ \alpha\beta=\beta\alpha);$$

or if we represent the group by

$$1,\ \alpha,\ \beta,\ \gamma,$$

we have the table

	1	α	β	γ
1	1	α	β	γ
α	α	1	γ	β
β	β	γ	1	α
γ	γ	β	α	1

Or, if we please, the symbols are such that

$$\alpha^2 = \beta^2 = \gamma^2 = 1$$
$$\alpha = \beta\gamma = \gamma\beta$$
$$\beta = \gamma\alpha = \alpha\beta$$
$$\gamma = \alpha\beta = \beta\alpha.$$

Systems of this form are of frequent occurrence in analysis, and it is only on account of their extreme simplicity that they have not been expressly remarked. For instance, in the theory of elliptic functions, if n be the parameter, and

$$\alpha(n) = \frac{c^2}{n} \quad \beta(n) = -\frac{c^2+n}{1+n} \quad \gamma(n) = -\frac{c^2(1+n)}{c^2+n},$$

then α, β, γ form a group of the species in question. So in the theory of quadratic forms, if

$$\alpha(a, b, c) = (c, \quad b, a)$$
$$\beta(a, b, c) = (a, -b, c)$$
$$\gamma(a, b, c) = (c, -b, a);$$

although, indeed, in this case (treating forms which are properly equivalent as identical) we have $\alpha = \beta$, and therefore $\gamma = 1$, in which point of view the group is simply a group of two symbols $1, \alpha (\alpha^2 = 1)$.

Again, in the theory of matrices, if I denote the operation of inversion, and tr that of transposition, (I do not stop to explain the terms as the example may be passed over), we may write

$$\alpha = I, \quad \beta = tr, \quad \gamma = I \,.\, tr = trI.$$

I proceed to the case of a group of six symbols,

$$1, \ \alpha, \ \beta, \ \gamma, \ \delta, \ \epsilon,$$

which may be considered as representing a system of roots of the symbolic equation

$$\theta^6 = 1.$$

It is in the first place to be shown that there is at least one root which is a prime root of $\theta^3=1$, or (to use a simpler expression) a root having the index 3. It is clear that if there were a prime root, or root having the index 6, the square of this root would have the index 3, it is therefore only necessary to show that it is impossible that *all* the roots should have the index 2. This may be done by means of a theorem which I shall for the present assume, viz. that if among the roots of the symbolic equation $\theta^n=1$, there are contained a system of roots of the symbolic equation $\theta^p=1$ (or, in other words, if among the symbols forming a group of the order there are contained symbols forming a group of the order p), then p is a submultiple of n. In the particular case in question, a group of the order 4 cannot form part of the group of the order 6. Suppose, then, that γ, δ are two roots of $\theta^6=1$, having each of them the index 2; then if $\gamma\delta$ had also the index 2, we should have $\gamma\delta=\delta\gamma$; and 1, γ, δ, $\delta\gamma$, which is part of the group of the order 6, would be a group of the order 4. It is easy to see that $\gamma\delta$ must have the index 3, and that the group is, in fact, 1, $\gamma\delta$, $\delta\gamma$, γ, δ, $\gamma\delta\gamma$, which is, in fact, one of the groups to be presently obtained; I prefer commencing with the assumption of a root having the index 3. Suppose that α is such a root, the group must clearly be of the form

$$1,\ \alpha,\ \alpha^2,\ \gamma,\ \alpha\gamma,\ \alpha^2\gamma,\quad (\alpha^3=1);$$

and multiplying the entire group by γ as nearer factor, it becomes γ, $\alpha\gamma$, $\alpha^2\gamma$, γ^2, $\alpha\gamma^2$, $\alpha^2\gamma^2$; we must therefore have $\gamma^2=1$, α, or α^2. But the supposition $\gamma^2=\alpha^2$ gives $\gamma^4=\alpha^4=\alpha$, and the group is in this case 1, γ, γ^2, γ^3, γ^4, γ^5 $(\gamma^6=1)$; and the supposition $\gamma^2=\alpha$ gives also this same group. It only remains, therefore, to assume $\gamma^2=1$; then we must have either $\gamma\alpha=\alpha\gamma$ or else $\gamma\alpha=\alpha^2\gamma$. The former assumption leads to the group

$$1,\ \alpha,\ \alpha^2,\ \gamma,\ \alpha\gamma,\ \alpha^2\gamma,\quad (\alpha^3=1,\ \gamma^2=1,\ \gamma\alpha=\alpha\gamma),$$

which is, in fact, analogous to the system of roots of the ordinary equation $x^6-1=0$; and by putting $\alpha\gamma=\lambda$, might be exhibited in the form 1, λ, λ^2, λ^3, λ^4, λ^5, $(\lambda^6=1)$, in which this system has previously been considered. The latter assumption leads to the group

$$1,\ \alpha,\ \alpha^2,\ \gamma,\ \alpha\gamma,\ \alpha^2\gamma,\quad (\alpha^3=1,\ \gamma^2=1,\ \gamma\alpha=\alpha^2\gamma).$$

And we have thus two, and only two, essentially distinct forms of a group of six. If we represent the first of these two forms, viz. the group

$$1,\ \alpha,\ \alpha^2,\ \gamma,\ \alpha\gamma,\ \alpha^2\gamma,\quad (\alpha^3=1,\ \gamma^2=1,\ \gamma\alpha=\alpha\gamma)$$

by the general symbols

$$1,\ \alpha,\ \beta,\ \gamma,\ \delta,\ \epsilon,$$

we have the table

	1,	α,	β,	γ,	δ,	ϵ
1	1	α	β	γ	δ	ϵ
α	α	β	γ	δ	ϵ	1
β	β	γ	δ	ϵ	1	α
γ	γ	δ	ϵ	1	α	β
δ	δ	ϵ	1	α	β	γ
ϵ	ϵ	1	α	β	γ	δ

while if we represent the second of these two forms, viz. the group

$$1,\ \alpha,\ \alpha^2,\ \gamma,\ \alpha\gamma,\ \alpha^2\gamma,\quad (\alpha^3=1,\ \gamma^2=1,\ \gamma\alpha=\alpha^2\gamma),$$

by the same general symbols

$$1,\ \alpha,\ \beta,\ \gamma,\ \delta,\ \epsilon,$$

we have the table

	1	α	β	γ	δ	ϵ
1	1	α	β	γ	δ	ϵ
α	α	β	1	ϵ	γ	δ
β	β	1	α	δ	ϵ	γ
γ	γ	δ	ϵ	1	α	β
δ	δ	ϵ	γ	β	1	α
ϵ	ϵ	γ	δ	α	β	1

or, what is the same thing, the system of equations

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

An instance of a group of this kind is given by the permutation of three letters; the group

$$1,\ \alpha,\ \beta,\ \gamma,\ \delta,\ \epsilon$$

may represent a group of substitutions as follows:—

$$\frac{abc}{abc},\ \frac{cab}{abc},\ \frac{bca}{abc},\ \frac{acb}{abc},\ \frac{cba}{abc},\ \frac{bac}{abc}.$$

Another singular instance is given by the optical theorem proved in my paper "On a property of the Caustics by refraction of a Circle."

It is, I think, worth noticing, that if, instead of considering α, β, &c. as symbols of operation, we consider them as quantities (or, to use a more abstract term, 'cogitables') such as the quaternion imaginaries; the equations expressing the existence of the group are, in fact, the equations defining the meaning of the product of two complex quantities of the form

$$w+a\alpha+b\beta+\ldots$$

Thus, in the system just considered,

$$\begin{aligned}(w+a\alpha+b\beta+c\gamma+d\delta+e\epsilon)&(w'+a'\alpha+b'\beta+c'\gamma+d'\delta+e'\epsilon)\\ &=\mathrm{W}+\mathrm{A}\alpha+\mathrm{B}\beta+\mathrm{C}\gamma+\mathrm{D}\delta+\mathrm{E}\epsilon,\end{aligned}$$

where

$$\begin{aligned}\mathrm{W}&=ww'+ab'+a'b+cc'+dd'+ee'\\ \mathrm{A}&=wa'+w'a+bb'+dc'+ed'+ce'\\ \mathrm{B}&=wb'+w'b+aa'+ec'+cd'+de'\\ \mathrm{C}&=wc'+w'c+da'+eb'+bd'+ae'\\ \mathrm{D}&=wd'+w'd+ea'+cb'+ac'+be'\\ \mathrm{E}&=we'+w'e+ca'+db'+bc'+ad'.\end{aligned}$$

It does not appear that there is in this system anything analogous to the modulus $w^2+x^2+y^2+z^2$, so important in the theory of quaternions.

I hope shortly to resume the subject of the present paper, which is closely connected, not only with the theory of algebraical equations, but also with that of the composition of quadratic forms, and the 'irregularity' in certain cases of the determinants of these forms. But I conclude for the present with the following two examples of groups of higher orders. The first of these is a group of eighteen, viz.

$$\begin{gathered}1,\ \alpha,\ \beta,\ \gamma,\ \alpha\beta,\ \beta\alpha,\ \alpha\gamma,\ \gamma\alpha,\ \beta\gamma,\ \gamma\beta,\ \alpha\beta\gamma,\ \beta\gamma\alpha,\ \gamma\alpha\beta,\\ \alpha\beta\alpha,\ \beta\gamma\beta,\ \gamma\alpha\gamma,\ \alpha\beta\gamma\beta,\ \beta\gamma\beta\alpha,\end{gathered}$$

where

$$\begin{gathered}\alpha^2=1,\ \beta^2=1,\ \gamma^2=1,\quad(\beta\gamma)^3=1,\ (\gamma\alpha)^3=1,\ (\alpha\beta)^3=1,\\ (\alpha\beta\gamma)^2=1,\ (\beta\gamma\alpha)^2=1,\ (\gamma\alpha\beta)^2=1;\end{gathered}$$

and the other a group of twenty-seven, viz.

$$\begin{gathered}1,\ \alpha,\ \alpha^2,\ \gamma,\ \gamma^2,\ \gamma\alpha,\ \alpha\gamma,\ \gamma\alpha^2,\ \alpha^2\gamma,\ \gamma^2\alpha,\ \alpha\gamma^2,\ \gamma^2\alpha^2,\ \alpha^2\gamma^2,\\ \alpha\gamma\alpha,\ \alpha\gamma^2\alpha,\ \alpha^2\gamma\alpha,\ \alpha^2\gamma^2\alpha,\ \alpha\gamma\alpha^2,\ \alpha\gamma^2\alpha^2,\ \alpha^2\gamma\alpha^2,\ \alpha^2\gamma^2\alpha^2,\\ \gamma\alpha\gamma^2,\ \gamma\alpha^2\gamma^2,\ \gamma^2\alpha\gamma,\ \gamma^2\alpha^2\gamma,\\ \gamma^2\alpha\gamma\alpha^2,\ \gamma\alpha\gamma^2\alpha^2,\end{gathered}$$

where

$$\alpha^3=1,\ \gamma^3=1,\ (\gamma\alpha)^3=1,\ (\gamma^2\alpha)^3=1,\ (\gamma\alpha^2)^3=1,\ (\gamma^2\alpha^2)^3=1.$$

It is hardly necessary to remark, that each of these groups is in reality perfectly symmetric, the omitted terms being, in virtue of the equations defining the nature of the symbols, identical with some of the terms of the group: thus, in the group of 18, the equations $\alpha^2=1$, $\beta^2=1$, $\gamma^2=1$, $(\alpha\beta\gamma)^2=1$ give $\alpha\beta\gamma=\gamma\beta\alpha$, and similarly for all the other omitted terms. It is easy to see that in the group of 18 the index of each term is 2 or else 3, while in the group of 27 the index of each term is 3.

2 Stone Buildings, Nov. 2, 1853.

VIII. *On the Electricity of the Blowpipe Flame.*
By W. R. Grove, *F.R.S. &c.*[*]

VOLTA and Erman made known the first indications of the production of electricity by flame. Pouillet and Becquerel have experimented and reasoned on the statical effects of such electricity, while Andrews, and more recently Hankel and Buff, have published very interesting results on the effects of flame as to conduction and production of voltaic electricity.

The experiments of which I am about to give a notice were for the greater part made before I had read the papers of the two last authors; and while they in many respects differ from theirs, they give a means of producing a voltaic current from flame far more distinct and powerful than any which I have tried or read of.

The flame I have worked with is that of naphtha or spirits of wine, urged by an ordinary glass-worker's blowpipe; and with a galvanometer, the needles of which are barely deflected to 2° by any current which I can procure by the flame of a common spirit-lamp, I can by the blowpipe flame procure deflections of 20° or even 30°, and with great certainty and uniformity of direction.

I am not aware that the blowpipe flame has ever been used for the production of electricity, though I see by M. E. Bec-

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[*] Communicated by the Author.

querel's recent paper that M. Becquerel, sen. has used it as a means of adventitiously heating a spiral placed in another flame.

Two platinum wires of 6 inches long and $\frac{1}{70}$th of an inch diameter have their ends formed into coils of $\frac{1}{8}$th of an inch long and wide; these wires are attached to copper wires insulated by glass stands, and having their further extremities connected with a galvanometer. My galvanometer is by Ruhmkorf; the wire is not as long as those now constructed, being only 544 feet, but I have magnetized the needles so as to render them highly astatic; they take four minutes to make one oscillation.

An additional binding screw is connected with the wire at 54 feet, so that I can ascertain by the same instrument the effect of a slighter resistance.

When the flame of a spirit-lamp is urged by the blowpipe, one of the above-mentioned coils is placed in the full yellow flame just beyond the apex of the blue cone, and the other near the orifice of the brass jet, or at what may be called the root of the flame, just above the base of the blue cone, the distance between the two coils being $2\frac{1}{2}$ inches. The coil in the full flame is at a white heat brilliantly incandescent, the coil near the orifice or at the commencement of the flame is cherry-red. The galvanometer is deflected to an average of 6°, the coil near the orifice or at the root of the flame being positive, or related to the further coil as zinc to platinum in the voltaic trough. On reversing the position of the wires, the galvanometer is deflected 6° in the opposite direction.

This current is not due to thermo-electricity excited in the wires at the point of junction of the platinum and copper, for it is unaltered in direction by a powerful thermo-current excited in these by heating the points of junction by another spirit-lamp.

By making this thermo-current aid or counteract the flame-current, a slight difference in degree is perceptible in the deflection according as the point of junction of the one or other wire is heated, but no difference in direction.

The flame-current, moreover, scarcely affects the short wired galvanometer, while the thermo-current of copper and platinum whirls the needle to 90°.

It is not a thermo-electric current arising from the unequal heating of the two coils, for it is in the same direction when the further coil is removed from the full flame so as to be less heated than the coil at the root of the flame. It is also different in direction from the thermo-current produced by unequally heating the coils in similar parts of the flame, or the current described by M. Buff, and to which he ascribes the electricity of flame.

The flame-current proper in my experiments is opposed to, and conquers the thermo-current.

On advancing the coil from the root of the flame towards the further coil, the deflection lessens, but still preserves its direction until the wires get very closely approximated, when the deflection of the flame-current yields to that of the thermo-current, and the direction of the needle depends on the relative heat of the coils.

A wire of zinc near the root of the flame with the platinum coil in the full flame gave a much smaller deflection, only 2°; when the direction of these wires was reversed, the current was stronger, the galvanometer marking 5°: the direction of the deviation was in both instances the same as with platinum and platinum.

Iron and copper acted as zinc, but rather more feebly. As in these experiments the wires of zinc, iron, and copper respectively were stouter than the platinum wire, I attribute the superior amount of deflection when the oxidable metal was in full flame to the greater cooling effect of the thicker wire reducing the antagonizing thermic current.

As the above experiments seemed to show that there was a proper flame-current irrespective of, and even overcoming the thermic flame-current, I was led to expect that by uniting in direction these two currents I might get more marked results. The following experiment, it will be seen, realized this expectation.

I formed a little cone of platinum foil of $\frac{6}{10}$ths of an inch in depth, and the same width at the widest part; I suspended this in a ring of platinum wire and substituted it for one of the coils.

Being placed in the full flame, the coil being at the root, it was filled with water, and water dropped into it from a pipette to supply that which was boiled away. I now readily obtained a deflection of 20° in the same direction as in my original experiment, and frequently the needles deviated to 30°. When the cone filled with water was placed at the root and the coil in the full flame, the deviation was only 5°.

In all the above experiments with the blowpipe it will be seen that the direction of the current was, as far as a comparison can be instituted, the reverse of that indicated in the experiments of Hankel, excepting his experiment with the flame of hydrogen; and also the reverse of the greater part of the experiments of Buff, which he rightly attributes to thermo-electricity.

One result of M. Buff (*Archives d'Electricité*, vol. xvii. p. 275) when he places one wire in the centre and the other at the outer margin of the flame, is probably dependent on a similar cause to mine; though when he places the second wire in the full flame, the current is in a contrary direction to that which I obtain.

My experiments prove, I think, distinctly, that there is a voltaic current, and that of no mean intensity, due to flame and not dependent on thermo-electricity.

I know of no better theory to account for these results than that which Pouillet applied to the effects on the condenser, viz. that it is the result of combustion; the platinum at the commencement of chemical action, or where the elements are entering into combination, being as the zinc of the voltaic battery; and that at the termination of combustion, or at the points where the chemical action is completed, being as the platinum of the voltaic combination.

Although there is a distinct thermo-current produced by the contact of two unequally heated bodies with flame, yet when we see, as in the above experiments, a marked current, in a contrary direction to, and overcoming that which is excited by the thermo-current in the flame, and also that at the points of junction of the wires without the flame, I see no means of viewing the resulting current as a thermo-current. The blowpipe flame, from its definiteness of direction, brings out most distinctly this current; in other flames, from the more confused circulation of the heated and burning particles, the results are less significant; and the various flame-currents counteracting each other, the thermic current obtains a predominance.

The current from the blowpipe flame, when the platinum in the full flame is cooled, is so marked, that I have little doubt, by attaching to a powerful pair of bellows a tube from which a row of jets proceeds, and alternating pairs of platinum in flames urged by the jets, a flame-battery might be constructed which would produce chemical decomposition and all the usual effects of the voltaic pile.

IX. *Note on a Point of Notation.* *By* J. J. Sylvester, *F.R.S.*[*]

It frequently becomes important in algebraical investigations, and in the representation of results, to have a means of expressing that the sign + or − is to be affixed to an algebraical expression, according as certain indices $\theta_1, \theta_2, \theta_3, \ldots \theta_n$ which occur therein, and which represent the natural numbers from 1 to n in some regular or irregular order, can be derived from the fundamental arrangement $1, 2, 3, \ldots n$ by an even or by an odd number of interchanges. An example of this occurred in my

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[*] Communicated by the Author.

short paper in the last Number of the Philosophical Magazine, on the extension of Lagrange's Rule of Interpolation, where I used to denote that such a choice of signs was to be made, the awkward and unsuggestive symbol "?" There exists, however, a very simple algebraical mode of denoting the presence of the factor +1 or −1, according to the order of the natural numbers in the scale $\theta_1, \theta_2, \theta_3, \ldots \theta_n$.

ζ has been always consecrated by me to the purpose of signifying that the product of the squared differences is to be taken of the elements with which it is in regimen; and in the paper adverted to I introduced the highly convenient new symbol $\zeta^{\frac{1}{2}}$ to denote that the product is to be taken of the simple differences obtained by subtracting from each element in regimen therewith every subsequent element in the arrangement of the elements as set down. By aid of this new symbol $\zeta^{\frac{1}{2}}$, the positive or negative character of any permutation, as $\theta_1, \theta_2, \ldots \theta_n$, can be completely expressed; for $\zeta^{\frac{1}{2}}(\theta_1, \theta_2, \theta_3, \ldots \theta_n) \div \zeta^{\frac{1}{2}}(1, 2, 3, \ldots n)$ will be +1 or −1 according as $1, 2, 3, \ldots n$ and $\theta_1, \theta_2, \theta_3, \ldots \theta_n$ belong to the same group, or to opposite groups in the natural dichotomous separation of the permutations of the n symbols in question, and thereby the desired object of giving a functional representation of the ambiguous sign is perfectly attained.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. vi. p. 306.]

Nov. 17, 1853.—Dr. Wallich, V.P., in the Chair.

THE following papers were read:

1. "On the Nerves which supply the Muscular Structure of the Heart." By Robert Lee, M.D., F.R.S.

The author remarks that, in a paper entitled "On the Ganglia and Nerves of the Heart," published in the Philosophical Transactions, Part I. 1839, it is asserted, that "it can be clearly demonstrated that every artery distributed throughout the walls of the uterus and heart, and every muscular fasciculus of these organs, is supplied with nerves upon which ganglia are formed."

He then states that "recent dissections which I have made of the heart of the race-horse, in which both the muscular and nervous structures are largely developed, demonstrate, that from the outer surface to the lining membrane the walls are universally pervaded with nerves, on which ganglia are formed, or enlargements invested with neurilemma, into which nerves enter and from which they issue, as in all the other ganglia of the great sympathetic nerve.

"From these dissections it is seen that the ganglionic nerves which ramify on the surface of the heart, those which have hitherto been delineated in the works of anatomists, are few in number compared to those which are distributed throughout the muscular structure of the organ, many of which are wholly independent of the blood-vessels.

"This anatomical demonstration of the ganglia and nerves of the muscular structure of the heart, completely subverts the opinion still entertained by some physiologists, that the sensitive and contractile powers of the heart are independent of nervous influence. It further indicates the real source of the action of the heart as an entire organ, from the commencement to the termination of life; how the circulation of the blood is carried on when the fœtus has neither brain nor spinal cord, and how the detached parts of the heart continue to contract for a time in some animals after its total separation from the body.

"These dissections are now open to examination by any gentleman who may feel interested in the anatomy and physiology of the heart.

"When Mr. West's drawings of the nerves displayed in these dissections have been completed, they will be presented to the Royal Society, with a description of the appearances delineated."

2. "On the Influence of the Moon on the magnetic direction at Toronto, St. Helena, and Hobarton." By Colonel Edward Sabine, R.A., V.P. and Treas.

Having noticed the inference drawn by M. Kreil from the magnetic observations at Milan and Prague, that the moon exercises an influence on the magnetic direction at the surface of our globe, cognisable by a variation in the declination depending on the moon's hour-angle and completing its period in a lunar day, the author proceeds in this paper to state the results of an examination, analogous to that pursued by M. Kreil, into the influence of the moon on the magnetic declination at the three stations of Toronto, St. Helena, and Hobarton.

The observations employed in this investigation consisted of six years of hourly observation at Toronto, five years at St. Helena, and five years at Hobarton, forming, exclusive of observations omitted on account of excessive disturbance, a total of 105,747 observations.

The processes are related by which, after the separation of the disturbances of largest amount, the observations were treated, for the purpose of eliminating the variations due to solar influence, and of re-arrangement in a form by which the inequality of the moon's action at the different hours of each lunar day might be brought distinctly into view. The results are shown in tables exhibiting the amount of inequality at each of the three stations corresponding to each of the twenty-four lunar hours.

It appears from these results that the existence of a lunar diurnal variation in the magnetic declination is shown at each of the three stations of Toronto, St. Helena, and Hobarton, and that it has the same general character at each, viz. that of a double progression in a lunar day, having two easterly maxima nearly at opposite points of the hour-circle, and two westerly maxima also at nearly two opposite points of the hour-circle. The extreme elongations are not at precisely opposite points of the hour-circle at any of the three stations, nor have the amounts of the two elongations which take place in the same direction always precisely the same value; but the slight inequalities in these respects are within the limits which might be ascribed to accidental variations, and might therefore disappear with longer continued observations. It is otherwise, however, in the author's opinion, with the disparity between the amounts of easterly and westerly extreme elongations which presents itself at each of the three stations. At Hobarton and St. Helena the westerly elongations have the larger values, at Toronto the easterly (the north end of the magnet being referred to in all cases).

The times at which the extreme elongations in the two directions take place are not the same at the three stations, and are as follows:— At Toronto the easterly extremes take place about the hours of 0 and 12, being the hours of the upper and lower culminations; at St. Helena the westerly extremes about two hours before the culminations; and at Hobarton about two hours after the culminations. At Toronto the westerly extremes take place about the hours of 6 and 18; at St. Helena and Hobarton the easterly extremes respectively two hours before and two hours after the same hours of 6 and 18. The extreme inequality, or the amount of lunar variation measured from one extreme elongation to the other, is about 28″ of arc at Toronto, 20″ at Hobarton, and 11″ at St. Helena. The resolved portion of the terrestrial magnetic force which acts in the horizontal direction, and is opposed to any disturbing influence, is approximately 3·54 at Toronto, 4·51 at Hobarton, and 5·57 at St. Helena.

Nov. 24.—The Earl of Rosse, President, in the Chair.

The following extract of a letter from Lieut. Gilliss, U.S.N. to Colonel Sabine, R.A., was read:—

Washington, 12 Sept. 1853.

Lieut. Mackai returned to the United States in April, having made his magnetical observations successfully at all the elevations and at distances of 100 miles, entirely across the Pampas. Soon after leaving Mendoza he was thrown from his horse, breaking his barometer and so injuring his chronometer, that he has neither the longitudes of his magnetical stations nor barometric profile of the country. Being desirous to make his work complete, he volunteered to retrace his ground, and left the United States for the purpose more than a month ago, taking with him the declinometer and dip-circle, two Bunten's barometers, an apparatus for determining altitudes from the boiling-point, and some smaller instruments.

Conveyance of the unifilar would have required another mule.

Cursory inspection of the observations already made afforded evidence of their reliability, and as repetition would have involved many hours' detention at each station, it was not considered essential to cumber him. Should the Argentine provinces have become sufficiently quiet, he will first cross the Andes at the Planchan Pass, lat. 35° 20′, next at the Partillo (the most elevated) Pass, lat. 33° 40′, and finally at the Cumbre and Uspalata Pass, in lat. 32° 50′, where observations have already been made.

As he will remain at Santiago only a short time, I look for him home during February next.

His and all the magnetical observations will then be discussed, and the volume be ready for press by the close of 1854. Those on meteorology are very far advanced.

The astronomical observations will fill three volumes, one of which (Mars and Venus) will be ready about the same time as the magnetical and meterological volume; the zones not until two years later.

My report, embracing those of Lieut. Mackai and the naturalists, with the maps, plans, and drawings, will probably be the first published; and should Congress extend its usual liberality to my propositions, all the volumes will be presented to the world in a creditable manner.

A paper was also read, entitled "On the Typical Forms of the large Secreting Organs of the Human Body." By Thomas H. Silvester, M.D.

The author in this paper offers some observations on the large secreting organs of the human body, namely,—

The lungs,
The liver,
The kidney,
The stomach,
The intestines, and
The developments connected with reproduction.

He observes that there are great obstacles to an attempt to reduce them to a uniform type, but that further observation reveals a typical uniformity, in adaptation to special purposes, unequalled in the rest of the human œconomy. The greatest hindrance to the discovery of this law of formation has been the habit of regarding a single element of the glandular organ as the whole and not a mere part of the glandular apparatus; for instance, the ovary has been described as the ovuliferous gland, although in reality the latter consists of the ovary, the Fallopian tubes, the uterus, and the vagina: also the lung has been said to be a gland, meaning by this expression the secreting portion; whereas the lung-gland is composed of the vesicular structure, the excretory ducts, the bronchi, larynx, thyroid body, and even the nasal passages. The aim of the author has been to discover the constant elements of the glandular system and to compare them together in the several organs.

A secreting gland is described as a more or less developed fold of mucous membrane, or even a plain surface—in its simplest form a follicle; but in the organs to be described it is a highly complicated apparatus, composed of many parts, each possessing a peculiar function. It is an unvarying characteristic of these structures that the elements of which they are composed always bear the same relation to each other in position. An accompanying diagram exhibited the type of a perfect gland, constituted of five elements and an appendage.

1. The secreting element	Red.
2. The excretory element	Blue.
3. The receptacle element	Yellow.
4. The cervix with its glandular appendage	Purple.
5. The efferent duct	Green.

Sketches of the several glands of the human body accompanied the paper, coloured in the same manner, so that the corresponding parts might be seen at a glance. The elements are seldom wanting though sometimes scarcely discernible in their rudimentary condition, and retained apparently only in conformity to typical law.

There is great difference in degree of development of the elements. The secreting part of the liver is of large size compared to the ducts, whilst that of the generative organ in the female, namely, the ovarium, is comparatively minute. The appendage of the cervix is very large in the biliferous apparatus, but scarcely discoverable in the stomach and sigmoid flexure. The excretory duct in the digestive organ, represented by a contraction across the middle of the stomach, is of enormous length in the seminiferous gland, as the vas deferens.

The popular terms being inexact or without meaning, it was proposed to substitute for the ordinary names of the secreting organs designations more aptly descriptive of their functions and more in accordance with the principles of scientific nomenclature, as follows:—

The uriniferous gland.
The seminiferous gland.
The biliferous gland.
The pneumatiferous gland.
The intestinal gland.
The ovuliferous gland.
The lactiferous gland; and
The digestive gland.

The kidney, or uriniferous gland, presents an almost typical regularity of development. The secreting element or cortical portion is composed of small convoluted tubes covered with a net-work of blood-vessels.

The excretory duct or medullary structure arises from the cortical part by numerous straight tubuli, which terminate in a duct for the conveyance of the urine into

The receptacle or bladder. This organ has a cervix lined with mucous membrane in longitudinal folds, studded with minute follicles and a rudimentary glandular appendage in the male.

The efferent duct is constituted of the membranous portion of the urethra.

The seminiferous gland consists of the testicle, a collection of convoluted tubercles, of an excretory duct called the vas deferens, of a receptacle the analogue of the uterus, the vesiculæ seminales being the body, the prostate the glandular appendage surrounding the cervix. The efferent duct is of considerable length; the receptacle is lined with an alveolar fissure like the gall-bladder and some muscular fibres.

The ovuliferous organ consists of the ovarium or secretory element, the fallopian tubes or excretory duct, the uterus or receptacle, the cervix and its glandular appendage in the shape of mucous follices, and the vagina or efferent duct. The secretory structure is not tubular, but vesicular; the excretory duct is connected with the ovarium by its fimbriated extremity at certain periods only, in conformity with its peculiar function.

The lactiferous gland is remarkable for the singular distribution of its elements; the secreting portion or breast, and the excretory duct or nipple, being attached to one individual, and the receptacle or mouth, cervix, glandular appendage or tonsils, and efferent duct or œsophagus belonging to another, of which the mother and child afford an illustration.

The stomach or digestive gland does not appear at first sight to be formed upon the same type as the other glands; it is however divisible into three distinct cavities—the secreting or cardiac, the receptacular or pyloric, and the efferent or duodenum. It is a tube of enormous calibre, divided into three compartments of unequal area. The excretory tube is recognized in the central contraction of the muscles during digestion; the cervix, lined with plicated mucous membrane, has been poetically described as the pylorus.

Comparative anatomy as well as microscopical bear testimony to the correctness of this view with regard to structure and development.

The biliferous gland is constituted as follows:

The secreting element, popularly called the liver.

The hepatic duct is the excretory tube.

The gall-bladder is the receptacle terminating in the duodenum in conjunction with the pancreatic duct, the pancreas being the glandular appendage surrounding the slender cervix of the receptacle, and the duodenum performing the function of an efferent canal to the digestive and biliferous glands.

The glandular appendages are subject to great variety of development. The thyroid, the pancreas, and the prostate are large structures, whilst in the pylorus, the sigmoid flexure of the colon and the cervix uteri, the same element is little more than a fold of mucous membrane with follicles interspersed. This element, like the glandular apparatus, is greatly predisposed to cancerous degeneration.

The writer of the paper now proceeds to describe the most remarkable and important gland of the human œconomy, namely, the sanguiferous.

The jejunum and ileum constitute the secreting element of this organ. The ileo-cæcal valve or verminiferous appendage is the excretory duct in a rudimentary state. The colon, though enormously developed in a longitudinal direction, must be regarded as the receptacle; the sigmoid flexure folded upon itself like the letter S (as in the cervix of the gall-bladder) is the cervix of the colon receptacle; the rectum or the efferent duct completes the system.

The function of this system is not fully known; the tract is lined with mucous membrane and glandulæ for the secretion of air and other products of the blood. It can scarcely be denied that the intestinal tube, in its structure and form and arrangement of the elements, bears the closest analogy to the glandular apparatus as seen in the other large viscera of the human body.

The jejunum, as its name implies, is generally empty; it is convoluted, and its parietes are covered with a net-work of capillaries; it resembles therefore in all these particulars the tubes which form the secreting tissue of the testicle and kidney, and differs from them but in size. The jejunum and ileum, regarded as a continuous tube, constitute a magnificent secreting structure, and its function is probably something more than the supply of the air and mucus generally contained in its canal. It has been suggested, that the blood, the lymph and the fæces owe their existence to the active capillaries of the mesenteric arteries, but some certain proofs are still wanting in confirmation of this opinion.

In the lung-gland, or pneumatiferous organ, the vesicular structure is the secreting element; it secretes carbonic acid: the ramifications of the bronchia ending in the two bronchi represent the excretory duct; the trachea is the receptacle; and the glandular appendage covering the cervix (or cricoid cartilage) and marking its position, passes by the name of the thyroid.

The efferent duct commences at the cricoid and ends at the cartilaginous orifice of the nostrils.

The lacrymiferous organ is formed upon the same plan as the preceding; but in consequence of some peculiarities of development, that, together with the simpler secreting structures, will be reserved for a future opportunity.

Nov. 30.—*Anniversary Meeting.*—The President, the Earl of Rosse, K.P., M.A., addressed the Society as follows:—

Gentlemen,

I am happy that it is in my power again to congratulate you on the progress made in the researches which have been carried on, aided by the Grant the Government have placed at your disposal: after an experience of more than three years we may say with confidence that much has been accomplished. At first there were some misgivings: continental experience was not altogether relied upon. Although it had been very much the practice of foreign governments to take an active part in encouraging the pursuit of science, and with decided success, here the smallest effort in that direction was looked upon by some as an experiment little harmonizing with our institutions, our feelings, perhaps our prejudices, and only to be followed by failure and disappointment. It was feared that because occasionally there had been some difficulty in employing effectively the small fund which had been bequeathed to us, that therefore there would be increased difficulty in employing a larger fund; and this no doubt would have happened if there had been restrictions limiting the application of the larger fund to certain specific objects, or if the field of discovery had been of limited extent: the reverse however was the case. Your Council were not embarrassed by any unwise restrictions, and in science there is room for every one. It is one of the deductions of economic science that labour creates a demand for labour; in fact, that where a community is industrious, and labour accumulates and becomes capital, that there the people will be fully employed: the same is true in the inductive sciences, and it is true universally; there no modifying causes interfere to diminish the force, or limit the application of the great principle, and we see strikingly that as facts accumulate, and facts are the capital of inductive science, fresh employment is everywhere provided for those who are willing to work. Take any one of the inductive sciences as an example, and we at once see how this is. Take for instance chemistry, compare it as it now is with what it was when Priestley commenced his career. The whole of the science then consisted of an imperfect knowledge of the properties of a few of the metals, of sulphur, phosphorus, and the three alkalies as they were then called. There was a little known also about salts and acids, and the existence of hydrogen and carbonic acid gas had recently been ascertained. In a range so limited there was little room but for one master mind, when Priestley discovered oxygen, and at once an opening was made for researches into the nature of the atmosphere, of water, and of combustion, of the acids and the alkalies, and ample employment was provided for a host of distinguished philosophers for years to come. Other important discoveries were soon made, each becoming as it were a new origin of light, throwing perhaps at first but feeble rays upon the objects around us, but revealing so much of their strange forms as to excite curiosity, and awaken the strongest passion of the human mind,—the desire to discover the truth. Inorganic chemistry was then rapidly becoming a great science, when the foundations of organic chemistry were laid in a succession of brilliant discoveries. That was but a few years ago, but there were many men then ready trained for the work, and the progress was proportionally rapid. To take a few of the discoveries in organic chemistry, and show how each has been the germ of others, as it were the first term of a diverging series, and thus to exhibit the great principle at work that in science labour creates a demand for labour, might perhaps be of some interest, but it would lead me from the object which I have in view, which is simply to point out the grounds upon which I have ever felt a strong conviction, that whatever means were placed at the disposal of the Royal Society, no lasting difficulty could occur in turning them to useful account.

During the last year considerable progress has been made by Mr. Hopkins in the important experiments which he has been carrying on in conjunction with Mr. Fairbairn and Mr. Joule. You no doubt are aware, that, as we descend below the surface of the earth, it has been found that the temperature increases: numerous experiments made in different places with all the necessary precautions to guard against fallacy, seem clearly to have established the fact. The increase is about one degree of Fahrenheit for a depth of from 50 to 60 feet. If therefore the conducting power of the materials of the globe was the same at all depths, we should have a series, which would give us the depth proportional to every required temperature. Reasoning in this way, we conclude that a temperature higher than that of melting iron exists at a depth of thirty miles, and that at double that depth the materials of the surface of the globe, combined as we find them in nature, would enter into fusion. It has therefore been supposed by many that the solid crust beneath our feet is not more than forty or fifty miles thick. It has however been assumed that the increasing pressure at increasing depths does not alter either the conducting power of materials, or the temperature at which they melt. This no doubt is to a certain extent incorrect, and it is highly probable that the conducting power of the different strata increases considerably with the depth, the materials becoming more compact under augmented pressure. It is not improbable also that pressure may raise the temperature of fluidity. In either case the solid crust of the globe would be thicker than it had been supposed to be on the assumed data. With the view of throwing light upon this question so interesting to all geologists, Mr. Hopkins undertook, with the assistance of Mr. Fairbairn and Mr. Joule, to subject various substances under different temperatures to enormous pressure. Considerable time was required, even with Mr. Fairbairn's unlimited mechanical means, to construct the necessary apparatus; however, recently it has been completed, and in the few substances examined it has been found that the temperature of fusion has increased with the pressure: in the case of wax, by a pressure of 13,000 pounds to the square inch, the fusing-point was raised 30°. Whatever may be the influence of these experiments as affecting the great questions of Terrestrial Physics, we may predict with certainty that data will be obtained most valuable in philosophical research.

Mr. Joule, I find, has been actively engaged, in conjunction with Professor Thomson, in his experiments on the thermal effects of fluids in motion, and has determined with considerable accuracy, operating on a great scale, the depression of temperature when compressed air escapes into the atmosphere through a porous plug. The laws of the phenomena as to the temperature and pressure of the confined gas, will also soon be determined.

Carbonic acid gas has been found to give a depression four and a half times as great as atmospheric air, while it passes through the porous plug with greater facility than atmospheric air; equal volumes requiring pressures of 1 and 1·05 respectively in order to be transmitted in equal times. Certain heating effects of air rushing through a single orifice have been observed, which will probably lead to a further development of the mechanical theory of the temperature of elastic fluids in rapid motion.

The examination of the sedimentary deposits in the Nile valley, mentioned at the last Anniversary, is still going on. Mr. Horner states, that by the munificent aid of His Highness Abbas Pacha, the Viceroy of Egypt, a series of operations have been carried on at Heliopolis, and at another station thirteen miles above Cairo, which have led to interesting results. A pit has been sunk to the depth of 24 feet below the pedestal of the colossal statue of Ramses the Second, who reigned, according to the chronology of Bunsen, about 1400 years before Christ, and borings have been continued by which cylinders of soil have been extracted at an additional depth of 48 feet. A series of thirty-two pits has been sunk across the valley in a line between the Libyan and Arabian deserts, occupying a line of about five miles, passing through the site of the statue alluded to; and it is proposed to sink a similar line of pits next year about twenty miles lower down the river, passing through the site of the obelisk of Heliopolis. Above sixty persons were employed in the operations at Memphis. The plan, as proposed by Mr. Horner, was, through the intervention of the Hon. Charles Augustus Murray, Her Majesty's late Consul-General in Egypt, submitted to the Viceroy, and met with the most ready acceptance. He gave directions to his government that every assistance should be afforded for carrying on the proposed researches; he appointed an able engineer officer high in his service, M. Hekekyan Bey, to conduct them, and ordered that the whole expense should be defrayed by his government. Such enlightened liberality on the part of His Highness Abbas Pacha justly entitles him to the gratitude of all cultivators of science.

The other researches alluded to on the last occasion are proceeding satisfactorily, but there is nothing which seems to call for especial notice at present; I will therefore at once proceed to give some account of the steps which have been taken by your Council for the advancement of science in another direction.

In the history of individual sciences we perceive there have been always successive periods of activity and repose. In Astronomy, for many years we have had a period of activity. Physical Astronomy has achieved perhaps its greatest triumph within the last few years in the discovery of Neptune; and the discovery of the numerous Asteroids and Comets is evidence that Practical Astronomy has kept pace with it. Within the same period the nebulous contents of the Southern Hemisphere have for the first time been made known to us;

we have now a catalogue of the highest excellence, with an ample guarantee for its accuracy in the zeal, ability, and experience of Sir John Herschel.

That catalogue will be a record for future ages, leading probably to the detection of change in the wonderful objects revealed by the telescope, and so giving a clue to the mysterious laws which rule the remote universe.

The Cape observations were not long before the world when some astronomers expressed a desire that it should not be left to posterity to turn them to account, but that some effort should be made to employ them in the service of the present generation. It was suggested, that with such an admirable working list, much might be effected in a short time. A comparison of the Northern and Southern catalogues had led many to believe that the same instrument had effected more in the Southern than in the Northern Hemisphere. Whether that had been owing to a better atmosphere, or whether the objects themselves were more remarkable, in either case it was reasonable to expect that an instrument of great power would do more in a well-selected situation in the Southern Hemisphere than in these islands; and on that account alone there seemed to be grounds for a well-founded hope that interesting discoveries would be made; but there were other grounds.

In the present state of Nebular Astronomy, the best prospect we have of extending our knowledge, seems to be by carefully sketching and measuring every object sufficiently within reach of our instruments, to make details discernible. It is highly probable that the objects we see are presented to us in every variety of position, that they often differ in form merely because we see them in a different aspect, and that if all were similarly placed as to the line of sight, a few normal forms would represent the whole. If this is the case, had we a sufficient number of accurate sketches, it is probable that out of the apparent confusion we should succeed in extracting the normal forms. It is also probable that in the nebular systems motion exists. If we see a system with a distinct spiral arrangement, all analogy leads us to conclude that there has been motion, and that if there has been motion that it still continues. The apparent motion is probably very slow, owing to the immense distance of the nebulæ; still there are double stars known to be physically double from their motions, which are probably as distant as some of the nebulæ. In certain nebulæ stars are so peculiarly situated that we can scarcely doubt their connection with the nebular system in which we see them, and some of these stars are as bright as some of the stars known to be physically double; as bright even as some of the stars which the latest Pulkowa observations have shown to have sensible parallax, and whose distance therefore is approximately known. We have therefore some vague idea, resting on probable evidence, even of the distance of the nearest nebulæ. It seems therefore not unreasonable to expect that measurements perseveringly carried on will detect motion, and that a fulcrum will thus be obtained, by which the powers of analysis may be brought to bear upon the laws which govern these mysterious systems.

In the northern catalogue of Nebulæ, and the same observation no doubt applies to the southern also, there are vast numbers much too faint to be sketched, or measured, with any prospect of advantage; the most powerful instruments we possess, showing in them nothing of an organized structure, but merely a confused mass of nebulosity of varying brightness. The number of nebulæ in the northern hemisphere, in which details are well brought out, is not very large, and even in these, a great proportion of the measurements are necessarily rough, and wanting in that precision by which the motion of certain double stars was so soon detected. It is evident therefore that to obtain a true knowledge of the forms of the nebular systems, our sketches must be as numerous as possible; and to obtain evidence of motion with rough measures, our measures should be very numerous also; but the northern hemisphere presents but a very limited list of suitable objects, therefore it is desirable to take in the southern hemisphere also.

I have thus, I believe, in part at least, explained the views of those who felt anxious that the southern hemisphere should be examined with a telescope of great power: the first attempt to give practical effect to their wishes was at the meeting of the British Association held at Birmingham in 1849.

The President of the Association was on that occasion directed to apply to Government to send a telescope of great power, in charge of an experienced observer, to the southern hemisphere. That application was unsuccessful; the Government, while they acknowledged the importance of the proposed object, declined to proceed, from an apprehension of difficulties, through which they did not then see their way clearly. At a subsequent meeting of the British Association, the matter was again discussed, and a Committee was appointed, to take such measures as they considered best calculated to effect the object. The Committee, in the first instance, laid the whole matter before your Council, and the Council, approving of the suggestion, appointed a Committee to consider the subject in detail.

It was necessary before the Government could be applied to with a reasonable prospect of success, that a specific plan should be in readiness, complete in all its details: the size of the instrument, its optical principle, its mounting, and its site.

With the view of obtaining the best information, the Committee consulted several eminent men, conversant with the management of large instruments. The first question proposed was, whether the instrument should be a refractor or reflector, and it was decided in favour of the reflector—there was in fact no choice. In the present state of knowledge, there was no probability that a good refractor could be constructed of sufficient power. As to size, the Committee were disposed to recommend a telescope of 4 feet aperture, and 35 feet focal length, to be mounted equatorially; not that an instrument of that size was the best, a much larger no doubt would have been better, but it would have been obviously unwise to have recommended a plan involving a very large expenditure. The Committee proceeded one step further, they pointed out a mechanical engineer of character and experience in the construction of optical instruments, who was willing to undertake to make the instrument for a specific sum, and having proceeded thus far, they laid the whole matter before Government, with whom it at present rests.

Another application is also before the Government of a very different character, but with the same great object, the advancement of human knowledge: it is to provide a building for the reception of the Scientific Societies of the metropolis. That application did not proceed from your Council, but from a large number of individuals, many of them Fellows of this Society, the remainder distinguished members of the other Scientific bodies of the metropolis. The application was made in the shape of a Memorial to Government, the signatures amounting to about 200. There had been no effort, I believe, on the part of those who brought the subject forward to procure a large number of signatures, or in any way to obtain a direct representation of the Scientific bodies: the Memorial was a preliminary step, and in that stage nothing more was required, than to show that it was supported by a large number of persons of that class, from which science, if called upon, would have selected her representatives. The Memorial was presented by a deputation, and care was taken to explain to Government, that it did not emanate officially from this Society or from any other.

A detailed explanation was also entered into, showing in what way the cultivation of science would be promoted, by providing suitable accommodation for the scientific societies. Carleton ride was suggested as a convenient situation, and other sites were named in the same neighbourhood.

Reference was made to the suggested site at Kensington, and it was explained that it would be inconvenient to a large proportion of the working men of the Societies, many of them engaged in business, and whose time was most valuable, that they would be unable to attend regularly, and that the usefulness of the Societies would be thereby greatly impaired. It was also intimated that the purchase of a large tract of land at Kensington did not materially affect the question; as a small space would be sufficient for all the Societies, and therefore that a free grant at Kensington could be but little object. Should the Government accede to the Memorial, I presume that then the Societies will be regularly consulted. The site will then be pointed out, the plan of the building laid before you, and you will form your own opinion; you will decide whether to remain as you are, or to accept the enlarged accommodation, which the far-sighted liberality of the nation will have provided for you.

The interests of science appear to me to be deeply involved in the question of providing a suitable building for the Scientific Societies. It is a practical question, and we must look at it in all its bearings, as men of the world. Had there been no Societies, science would not therefore have stood still; but its progress would have been much slower. The desire of discovering the truth, which is so strongly implanted in every educated mind, would have been to some a sufficient motive for exertion; but the aid and inspiriting influence of association would have been wanting, and many active members of our scientific bodies would probably never have taken up science as a pursuit at all. The love of ease, the fascinations of society, the little prospect of advancing their material interests, through the path of scientific discovery, would have been to them a sufficient excuse for indolence. This point was very well put at the last Anniversary, by one of the distinguished men to whom you awarded a medal; he said, had he been like Robinson Crusoe on a desert island, the desire of discovering the truth would have been an inducement to work, but to obtain the good opinion of his fellow-men, associated together, was with him a very strong additional inducement. If a man, naturally gifted, and well-educated, attends scientific meetings, he will feel himself constrained to work, and therefore it is so important for the advancement of knowledge, that able men should be induced to join and attend the different societies; but nothing I think would have greater attractions than a building in a convenient central situation, where the business of science would be transacted, where there would be access to the best libraries, and where that kind of society most valued by scientific men would always be within reach.

Where the question is, shall a great country like England provide a suitable place of meeting for its scientific bodies, I should hope there are not many who would be disposed to count the cost, I scarcely think the question, *cui bono*, could be asked; should any one however ask it, I should answer, the object is to promote the increase of human knowledge, to extend the domain of reason; and "it is the understanding that sets man above the rest of sensible beings, and gives him all the advantage and dominion which he has over them." To take lower ground, science is a part of England's greatness: without science England would be nothing, and in the race with the world, if she is forsaken by science, she will fall far behind. The exact sciences are the foundation of navigation; they are the foundation of engineering; and many of our manufactures are based on principles, which have been evolved in the researches of a refined chemistry. Besides, have we not sometimes been at fault where science was not ready to guide us? We have heard of failures in ship-building: some ships sail slowly, and are therefore of little use; others roll, and strain, and are also condemned; where we have succeeded best we have taken our models from abroad, where high science has long been urged to lend its aid in naval construction; where such men as the two Bernouillis, Euler, Chapman and others, have been induced to examine and discuss the questions of most interest to the practical builder. In civil engineering too, our most eminent men, I am sure, would not hesitate to acknowledge their obligations to our neighbours. We have long been almost unrivalled in the applications of steam; and yet, who is there who has not sought for the theory of the locomotive in the essay of De Pampour, or the principles of steam in general in the researches of Regnault?

In military engineering too, as well as in naval architecture, has not science been sometimes on the side of our adversaries? and have we not in consequence suffered severely? But let me not be mistaken. I do not contend that science can in a moment increase our success in the arts, upon which the greatness of this country depends. If we were to say to the mathematician, give us the best lines for a ship suited to a given purpose, however profound his mathematical knowledge might be, he would fail; practice must be combined, but in due subordination with theory. It is where in a nation science is cultivated profoundly by a large class of persons, and circumstances exist tending to direct it to practice, that some men will always be found, gifted with the faculty of applying it in whatever way the interests of the country may require.

Popular science, however, will not do: it has its uses, subordinate as they are; it must be science of a high order; science as taught at our universities: there a power is created capable of effecting great objects, but in too many cases it is not applied at all, and it soon passes away without useful result. Were it possible to enlist that gigantic power into the service of the country by making our scientific Associations more inviting, by placing science in this metropolis in a position more attractive, a result would be obtained which the merest utilitarian would consider of immense value.

I deeply regret that the last accounts have brought no intelligence of Sir J. Franklin's expedition, and that science has sustained a heavy loss in the death of a distinguished French officer; the latest sacrifice to the perils of Arctic discovery.

Many now present were acquainted with Lieut. Bellot, and I am sure they will bear me out in this, that there was a singleness of purpose, and a propriety of feeling in everything he said and did, in perfect keeping with the tenor of his previous life; and though he was here so short a time, he was regarded as a friend by all who knew him.

You have heard no doubt with sincere pride, that British valour, and British perseverance, have at length solved the problem of the north-west passage. A question of great geographical interest has thus been settled, and an important fact has been added to the data of terrestrial physics. In all future inquiries relative to the oceanic currents, the tides, the variations of temperature, the winds, and meteorological phenomena generally, it will no longer be a doubtful assumption, that the sea flows freely around the northern coast of America.

It now only remains for me to state, and I have the greatest pleasure in doing so, that your Society is prosperous. The publication of your Transactions proceeds regularly, and they continue to be, as they have been for two centuries, the records of every important addition to British science. There is no indication of a diminished anxiety to share in your labours, the candidates for admission are numerous, and your Council have had no difficulty, though acting under a heavy responsibility, in pointing out to you the required number of persons in every way worthy of the Fellowship.

So far the present system of election appears to me to have worked extremely well. The Fellowship is sought for as a high honour, and here, as at the Universities, the claims of the respective candidates are tested by responsible persons.

In this, the Royal Society differs from every other Society; and I think upon the preservation of that distinction, the welfare, the position, perhaps the existence of our Society depends. For 150 years the Royal Society stood alone; unaided it bore the whole labour of wielding the power of Association, in the cause of progressing science. Recently other Societies were formed to meet more fully the wants of individual sciences; not as rivals to the Royal Society, (in all of them our Fellows have held very prominent places) but as the most friendly allies; not dependent on the Royal Society, but fully admitting its pre-eminence.

These Societies have rendered important services; much has been effected through their means which otherwise would not have been attempted. Science has been carried out by them in the utmost detail. Besides, it is a law of human nature, that we usually form a high estimate of the importance of the pursuit we are engaged in; and in a Society limited to one science, that feeling will necessarily predominate, and will act as a stimulus to exertion. Under its influence, labour will be cheerfully borne, from which under other circumstances we should recoil with disgust. That feeling, however, would proceed too far were there not here a power to restrain it: you hold that power: you exercise a presiding influence over all the Societies. The leading members of the scientific bodies have their places here, and science is fully represented.

You look at science as a whole, and you weigh the value of every new discovery as adding to the mass of human knowledge. The honour of contributing to your Transactions is eagerly sought for, the medals you award are in high estimation, and science is stimulated to its grandest efforts, because you are viewed by all as just and able judges. To hold securely that proud position, learning must be your distinguishing attribute; in the altered state of things it is learning which fits you for your new duties, and so long as the Fellowship is regarded as the reward of services in the cause of science of no common order, or of proved scientific eminence at the universities, so long, I think, we may predict with confidence that the Royal Society will flourish.

Dec. 15.—Thomas Bell, Esq., V.P. in the Chair.

The following communications were read :—

1. Extract of a Letter from Dr. Edward Vogel to Colonel Sabine, dated Mourzuk, Oct. 14, 1853.

"You will receive through the Foreign Office a packet of Astronomical, Meteorological, and Magnetical Observations, made on the way from Tripoli and since my arrival here. My instruments are almost all in good condition, although their cases have split from the effects of heat and dryness, notwithstanding their double, and in some instances triple, leather protection. I saw the great comet for the first time on August 23, but others had seen it three or four days sooner. Its nucleus was very bright, resembling a star of the first magnitude, with a distinctly defined disc of the apparent diameter of Jupiter. The tail made an angle of 86° or 87° with the horizon, inclining to the north. It was a single tail with almost precisely parallel sides. Its length was 10° on the 25th of August, 12° on the 26th, and 15° on the 27th and 29th of Augnst. I have seen here repeatedly the apparent fluctuation in the position of stars which is spoken of in the third volume of Cosmos, and have sent the particulars of my observations to Baron von Humboldt. There is no regular rainy season at Mourzuk, but slight showers occur sometimes in the winter and spring, seldom in the autumn. A heavy rain is considered a great calamity, as it destroys all the houses, which are built of mud dried in the sun. It would likewise kill the date trees, by dissolving the salt which is in large quantities in the soil. About twelve years ago there perished above 10,000 date trees in the neighbourhood of Mourzuk, on account of a rain which continued for seven days. The prevailing winds are south and east, the strongest generally west or north-west. Twice or three times I have seen whirlwinds pass through the town, a phenomenon which was common in the deserts between Benioleed and Mourzuk. All the whirlwinds I observed turned in the direction from east by north and west to south. In December and the first half of January the thermometer falls at sunrise (at Moorzuk) as low as 42°, and in places exposed to the wind water freezes during the night. At Sokna I found no one who could remember having seen snow; but at Ghadamis snow was seen by Mr. F. Warrington on the 15th of January 1851. At Tripoli we had heavy dews at night; and I observed the same until we had passed a small chain of mountains fifteen miles north of Sokna; from thence we had no dew, and it was even often impossible to get the dew-point with Daniell's hygrometer. In the desert the thermometer generally rose till 4 P.M., from the sand (which was sometimes heated to 140° at 1 P.M.) giving out its heat. Earthquakes are unknown in Fezzan; slight shocks are sometimes felt at Benioleed and Sokna, as was the case the end of last May. Shooting stars were observed in great quantities (about forty an hour) on the 7th, 8th, and 31st of July; very few on the evenings of the 9th, 10th, and 11th of August, averaging fifteen an hour, mostly coming from Cassiopea and Ursa Minor. On the 10th, at 8 A.M., I saw in ten minutes three shooting stars coming from α Cassiopeæ, and rising right upwards towards the zenith. About 4 A.M. on the 11th, I observed in a quarter of an hour about twenty very bright ones in Pegasus and Aries. Shooting stars were numerous also on the 1st, 2nd and 3rd of October."

2. Notice of a Comet seen from H.M. Brig Penguin off the Coast of South Africa.

"Sir,—I am not aware that it can be of any service, still I have thought proper to write you, for the information of the Royal Society, that on the 1st of May 1853, on board H.M. brig Penguin, in lat. 35° 0′ S., long. 21° 52′ E., at $6^h\ 30^m$ P.M. I observed a comet bearing N.W. by W. ¼ W., measuring from the centre star in the belt of Orion 14° 30′, the altitude of the comet being 26° 19′, its length being about 5°. It went down at $9^h\ 10^m$, bearing W. ¼ N., and from the altitude, time, and rate it appeared to move at, it must have been the first evening of being seen. On first observing it, it appeared to be making a retrograde motion, or tail first, and not travelling as fast as the two small stars above it, as by the time it set it had approached very close to them: the weather fine, warm, and cloudy. On the 3rd of May a gale came on which lasted till the 7th, after that time it became rather indistinct, not being seen but when very clear, or by the aid of a glass. Having sailed on the 29th April from Simon's Bay, Cape of Good Hope, and not hearing there, or seeing anything of a comet, in either the Nautical or Cape Almanacs, I concluded it had not as yet been observed, and therefore have thought it my duty to forward a rough sketch of its appearance on the days mentioned, and its positions, as near as I could place it with my left hand, my right unfortunately having been partially smashed and disabled on the night the gale commenced, and which also prevented me from measuring its distance from any of the stars, &c. A copy of the latitude, longitude, and bearings is from the ship's log. I found on our arrival at Quilemane that it had been observed by the other two cruisers, but not till the 6th of May. I must apologize for troubling you with the above, but considering it as a point of duty that I should do so, I have the honour to be,

"Sir, your very obedient Servant,

"W. B. Edwards,

"Master H.M. Brig Penguin."

XI. *Intelligence and Miscellaneous Articles.*

ON THE PRODUCTS OF THE DECOMPOSITION OF ROCKS UNDER THE INFLUENCE OF SULPHUROUS THERMAL WATERS. BY J. BOUIS.

ALL the products analysed by the author were derived from the strata whence issue the remarkable waters of Olette (Pyrénées Orientales), particularly the source of the cascade, the temperature of which rises to 181° F. Some alterations having led to the removal of the blocks of stone which closed the opening of the spring, the effects of the corrosive action of the water upon the rock became observable.

The rock is gray, veined with white quartz; its fracture is dull;

its specific gravity is 2·86; it resembles petrosilex. Its composition on an average of several analyses is as follows:—

Silica	82·6
Alumina	7·5
Protoxide of iron	1·2
Lime	1·5
Soda	4·2
Potash	0·7
Water	1·6

The rock when reduced to powder still retains a certain quantity of water which can only be got rid of at a red heat. In this circumstance, and its composition, this rock approaches the aquiferous granites and the petrosilex of China, of which analyses have been given by Ebelmen and Salvétat. In its transformation by the action of the water it first acquires a very bright red colour, due to some peroxide of iron; it then cracks as though it had undergone the action of a very high temperature, and afterwards becomes white and friable, acquires the appearance of pumice-stone, and consists only of pure silica.

Analyses of decomposed rocks gave 95·97, 98·00, 99·5, and lastly 100·00 per cent. of silica, according as their change advanced. The silica is usually snow-white, friable and porous, sometimes coloured by iron and manganese. In the clefts of the rock the silica is formed in stalactites, consisting of concentric layers which indicate the result of a gradual deposit. This variety of silica is often covered with crystals of sulphate of lime; it sometimes also contains very small crystals of quartz, a circumstance which may perhaps be appealed to by those geologists who admit that quartz has been formed by aqueous crystallization. The surface of the stalactiform silica is often of a very fine green colour, owing to the presence of cryptogamic vegetation. Lastly, the silica is met with in a gelatinous state, constituting transparent masses which are often confounded with glairine, or sometimes imbedding plants growing upon the rocks, and forming by spontaneous desiccation a gray felt-like substance, which might readily be mistaken for pasteboard.

The silica is consequently derived from two different sources: in one case it is due to the action of the water on the rock removing all the other elements; in the other it is deposited from the water holding it in solution by the aid of a high temperature, just as lime in incrusting waters is dissolved by carbonic acid. This view is confirmed by the fact that the porous silica is insoluble in weak alkaline or acid solutions, whilst the stalactiform silica dissolves with the greatest facility in these reagents.

The water carries off the soluble matters, and deposits a reddish mud; this is attacked by muriatic acid, which removes all the iron; the remainder consists of white plastic clay. This mud is composed of—

Silica	74·5
Alumina and oxide of iron	17·9
Water	7·5

This composition is nearly identical with that of the deposits found in Iceland. Dieffenbach and Hooker found in the interior of New Zealand a large number of volcanic springs of a high temperature, which deposited stony substances resembling chalcedony. These deposits, analysed by Thomson, gave—

Silica	77·35
Alumina	9·70
Peroxide of iron	3·72
Lime	1·54
Water	7·66

The composition is therefore the same, although the substances were derived from such different localities. The comparison of the analysis of the mud with that of the rock, shows that the potash and soda being very soluble, have been carried off by the water; that the protoxide of iron has been converted into peroxide and accumulated with the alumina in the deposit, whilst the silica has diminished; results which confirm Ebelmen's experiments.

A crust of from 1 to 2 millimetres in thickness, which had been formed on a rock bathed by the waters of the cascade, and which was detached with facility, was also examined. It is very white in the interior, but red on the outside; muriatic acid destroys this colour; it is very hard, and presents a radiate crystallization. From its composition it belongs to the class of zeolithes; it consists of—

		Calculated.	Found.
Silica	2267·2	57·6	57·6
Alumina	641·8	16·3	16·1
Lime	350·0	8·8	8·6
Water	675·0	17·1	17·6
	3934·0		

giving the formula $(CaO, SiO^3 + Al^2O^3, 3SiO^3) + 6HO$.

In the clefts of the source of the cascade, and under the arch through which it issues, abundant white or yellowish efflorescences are to be seen. Some portions of these are formed of potash alum, whilst others consist essentially of sulphate of soda. These salts are nearly always mixed, and it is sufficient to dissolve them to separate them from the silica. The solution when evaporated and exposed to the air deposits crystals of alum, and the efflorescent sulphate of soda is readily got rid of.

These productions are due evidently to the action of the air upon the sulphuretted hydrogen which is disengaged from the waters. The rock exposed to the vapour of the water is much disintegrated; it becomes porous, and causes the sulphur to pass to the state of sulphuric acid, which assists in its turn in the attack upon the rock by extracting the alkalies. The author believes that the sulphur met with also in these and similar localities only makes its appearance when the rock is not porous. The water containing an alkaline sulphuret, agitated in contact with the air, deposits sulphur.

The comparison of these facts with those observed by C. Deville on the rocks forming the soufrière of Guadaloupe, will show the complete concordance which exists between the phænomena presented in the neighbourhood of volcanos and those produced near sulphurous thermal springs. In the Pyrenees, where these waters are so abundant, earthquakes were formerly very frequent, and the masses of water which pour constantly from the earth serve, so to speak, as safety-valves for the prevention of greater convulsions.—*Comptes Rendus*, August 8, 1853, p. 234.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1853.

Chiswick.—November 1. Cloudy and fine. 2. Very fine. 3. Foggy. 4. Very fine: overcast. 5. Uniform haze: clear at night. 6. Rain: foggy: uniformly overcast. 7. Foggy: overcast. 8. Foggy: fine: clear. 9. Clear and fine. 10. Frosty: fine: foggy. 11. Dense fog: clear at night. 12. Overcast. 13. Foggy: densely overcast. 14. Foggy: overcast. 15. Dense fog: rain at night. 16. Fine. 17. Frosty: fine. 18. Sharp frost: very fine. 19. Frosty: fine: clear and frosty. 20. Overcast: rain: clear and frosty. 21. Frosty: clear. 22. Foggy. 23. Dense fog. 24. Overcast: rain. 25. Hazy and dull: rain. 26. Overcast. 27. Fine. 28. Overcast throughout. 29. Densely overcast: rain. 30. Hazy: slight rain.

Mean temperature of the month	40°·14
Mean temperature of November 1852	47 ·38
Mean temperature of Nov. for the last twenty-seven years	43 ·18
Average amount of rain in Nov.	2·38 inches.

Boston.—Nov. 1—4. Fine. 5. Cloudy. 6. Cloudy: rain A.M. 7. Cloudy. 8—12. Fine. 13. Fine: rain P.M. 14. Fine. 15. Foggy: rain P.M. 16. Rain: rain A.M. 17—19. Fine. 20. Rain: rain A.M. 21, 22. Fine. 23. Cloudy. 24, 25. Cloudy: rain P.M. 26. Rain: rain A.M. 27. Cloudy. 28. Cloudy: rain P.M. 29. Cloudy: rain A.M. and P.M. 30. Cloudy: rain A.M.

Sandwick Manse, Orkney.—Nov. 1. Cloudy A.M.: rain P.M. 2. Cloudy A.M.: clear P.M. 3. Clear, fine A.M.: clear P.M. 4. Damp A.M. and P.M. 5. Cloudy A.M. and P.M. 6. Rain A.M.: clear, fine P.M. 7. Drizzle, showers A.M.: clear P.M. 8. Rain A.M.: hail-showers, lightning P.M. 9. Cloudy A.M.: damp P.M. 10. Cloudy A.M.: showers P.M. 11. Sleet-showers A.M.: showers P.M. 12. Bright A.M.: clear, fine P.M. 13. Cloudy A.M.: clear, fine P.M. 14. Cloudy, frost A.M.: damp P.M. 15. Clear, frost A.M. and P.M. 16. Cloudy, frost A.M.: clear, frost P.M. 17. Clear, frost A.M. and P.M. 18. Showers A.M.: cloudy P.M. 19. Cloudy A.M.: rain, cloudy P.M. 20. Clear, fine A.M.: sleet-showers P.M. 21. Clear, fine A.M.: clear P.M. 22. Bright A.M.: clear, aurora S. P.M. 23. Bright A.M.: clear P.M. 24. Cloudy A.M. and P.M. 25. Bright A.M.: rain P.M. 26. Clear, frost A.M.: clear, aurora P.M. 27. Clear, frost A.M.: rain P.M. 28. Clear A.M.: clear, aurora P.M. 29. Rain A.M.: clear, aurora P.M. 30. Bright A.M.: clear, aurora P.M.

Mean temperature of Nov. for twenty-six previous years	42°·59
Mean temperature of this month	44 ·87
Mean temperature of Nov. 1852	41 ·52
Average quantity of rain in Nov. for thirteen previous years.	4·38 inches.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1853. Nov.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
● 1.	29·913	29·788	29·52	29·48	29·61	60	41	50	53	53	se.	s.	sse.	....	....	·12
2.	30·007	29·846	29·46	29·51	29·76	60	36	50	53	48	sw.	s.	ssw.	·02	....	·17
3.	30·076	29·973	29·76	30·02	30·12	56	36	44	47½	45	e.	ne.	ssw.	....	....	....
4.	29·944	29·854	29·63	30·02	29·92	53	45	46	50	50	e.	se.	se.	....	....	....
5.	29·835	29·773	29·55	29·83	29·78	50	44	48	50	49½	e.	ese.	sse.	·10	....	....
6.	30·050	29·851	29·60	29·94	30·07	58	45	48	50	46	se.	e.	s.	....	....	·30
7.	30·250	30·166	29·83	30·03	30·11	58	46	51	49	47	n.	w.	sw.	....	·38	·02
☽ 8.	30·322	30·252	29·88	29·88	30·00	55	25	42·5	45	42	w.	calm	w.	....	....	·18
9.	30·517	30·459	30·14	30·18	30·08	51	24	36	47½	46½	nw.	wnw.	ssw.	....	....	·11
10.	30·429	30·246	30·00	29·95	29·96	51	27	33	43	48½	nw.	w.	wsw.	....	....	....
11.	30·262	30·207	29·96	30·07	30·18	51	28	34	47	46	ne.	wnw.	w.	....	....	·26
12.	30·308	30·043	30·04	30·08	29·96	50	38	30·5	51	48	ne.	n.	sw.	·03	....	·06
13.	29·912	29·760	29·64	29·76	29·87	46	31	38	47	43	ne.	n.	w.	....	....	....
14.	29·740	29·672	29·43	29·82	29·67	45	30	38	40	43	nw.	nw.	calm	....	·04	....
○ 15.	29·685	29·631	29·43	29·62	29·63	40	36	30	42	38½	s.	nw.	s.	·11	....	·09
16.	29·695	29·644	29·45	29·62	29·66	46	25	39·5	42	40	w.	nw.	nw.	....	·07	....
17.	29·801	29·735	29·53	29·74	29·69	46	18	30·5	37½	37	n.	calm	sse.	....	....	·08
18.	29·995	29·956	29·70	29·53	29·62	41	21	27	46	45	sw.	nw.	s.	....	....	·26
19.	30·054	29·969	29·75	29·44	29·58	49	25	32	50½	45	s.	s.	w.	....	....	·06
20.	30·086	29·887	29·67	29·70	29·88	46	23	40	41	42½	s.	sw.	wsw.	·06	....	·10
21.	30·292	30·230	30·03	30·15	30·12	46	21	30	40	43	n.	n.	s.	....	·04	·13
22.	30·295	30·178	30·05	30·08	29·92	36	23	26	45	43	s.	nw.	sse.	....	....	....
☾ 23.	30·227	30·049	29·94	30·05	29·97	33	24	30	45½	42½	s.	calm	sse.	....	....	....
24.	30·057	29·924	29·79	29·66	29·85	41	29	33	44	42	s.	s.	sse.	·12	....	....
25.	30·095	29·959	29·80	29·65	29·52	46	33	34	42	45	se.	s.	s.	·26	·03	....
26.	29·974	29·781	29·55	29·88	30·04	43	31	40	37½	39	n.	ne.	se.	·05	·38	·20
27.	30·119	30·038	29·86	29·95	29·76	40	37	39	42½	43½	ne.	nw.	se.	....	·07	....
28.	30·291	30·126	29·84	29·82	29·94	45	37	38·5	45	38	s.	s.	se.	....	....	·09
29.	30·086	29·980	29·74	29·43	29·52	50	39	37	46	43½	s.	s.	sse.	·05	·05	·30
● 30.	30·058	30·025	29·72	29·70	29·93	52	47	47	41	49	se.	ssw.	se.	·01	·27	·09
Mean.	30·079	29·966	29·74	29·819	29·857	48·13	32·16	38·0	45·35	44·40				0·81	1·33	2·62

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XII. *On the Results of a series of Experiments on the Decomposition of Water by the Galvanic Battery, with a view to obtain a constant and brilliant Lime light. By the* Rev. N. J. Callan, *Professor of Natural Philosophy in the Roman Catholic College, Maynooth**.

SOON after I had discovered the nitric acid cast-iron battery, and before I had completed the large one which I made for the College, I began to make experiments on the decomposition of water with a view to obtain a lime light which might answer for lighthouses. I soon succeeded in obtaining a steady and brilliant light. In a paper on our new galvanic battery, dated April 6, 1848, and published in the London Philosophical Magazine of the following July, I stated that "I got the lime light by igniting mixed gases as they were produced by the decomposition of water and throwing the flame on lime." I believe I was the first who obtained a constant lime light by means of the galvanic battery. My experiments were frequently interrupted on account of the state of my health. They were at one time suspended for more than two years, and several times for five or six months. The same cause which obliged me to interrupt them now compels me to bring them to a close before I could complete all the experiments I intended to make. These experiments have led to the following results:—First, a new apparatus for applying with perfect safety the mixed gases, oxygen and hydrogen, to the production of a flame of the most intense heat, which, when thrown on lime, produces a most dazzling light. Secondly, a new voltameter, to which a common jet may be screwed, and the gases inflamed as they issue

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* Communicated by the Author.

from it without the smallest risk of injury, and by which the full decomposing effect of a battery of a hundred or five hundred pairs, arranged in one series, may be produced without exhausting the power of the battery more rapidly than if it contained only three or four cells. Thirdly, a new negative element far cheaper, far more durable, and one which may be made to act more powerfully than the platinized silver used in Smee's battery. Fourthly, a new mode of protecting iron against the action of the weather and of various corroding substances, so that iron thus protected may be used for all the purposes to which sheet lead and galvanized iron are applied. Fifthly, a method of producing a brilliant intermittent lime light by means of a small galvanic battery. Sixthly, a new mode of exhibiting the dissolving views by means of the lime light. Lastly, a new sine galvanometer, which is the only instrument yet made by which very powerful galvanic currents can be measured.

The first result was a new apparatus for applying with perfect safety the mixed gases to the production of the oxyhydrogen flame and lime light. In my first experiments on the decomposition of water, I made use of a glass vessel containing dilute sulphuric acid and four parallel plates of platinized platina, each having a surface of about 14 square inches. The mouth of the vessel was stopped by a thick piece of wood, through which the wires from the electrodes passed. In this wood was cemented a tapped brass nut to which a stopcock and Hemming's jet might be screwed; the mouth of the vessel was made air-tight by cement. I soon found that with a glass vessel the use of Hemming's jet was unsafe. On one occasion, when I employed twenty 6-inch cells of our cast-iron battery, the Hemming's jet did not let out all the gases produced in the vessel; for after breaking the connexion between the battery and electrodes, the gases continued for some time to issue from the jet and kept up the lime light, although the battery had not worked for more than a minute or two. Had I allowed the battery to work nine or ten minutes, the gases would have been condensed within, and would have burst the vessel; I therefore saw that I must either get the gases separately, or devise some means by which the mixed gases might be safely inflamed, and might at the same time pass without much resistance from the glass vessel. I attempted both. To get the gases separate, I put a plate of porous earthenware between the electrodes so as to form two air-tight cells. I thus succeeded in obtaining the gases separate; but finding so much difficulty in making the cells air-tight, I gave up the idea of looking for the gases separately. In order to avoid dangerous explosions in igniting the mixed gases, and at the same time to allow them to pass freely to the jet, I sent them through water contained in an iron vessel, to the top of which was screwed the jet belonging to our large gas microscope and polariscope. The first iron vessel which I used was about 5½ inches high and 2 inches in diameter; its sides were an inch thick. This vessel was found to be too small. On two occasions, all, or nearly all the water was thrown out of the vessel through the jet; the flame went back and exploded the gases in the bags. On these two occasions the pressure was very irregular. The quantity of the gases contained in the bags was too small to be forced out by weights on the pressure-boards. They were driven through the jet by a person who pressed frequently and violently on the bags with his arm. This violent and irregular pressure forced the water through the jet. Had the pressure been uniform, it is probable the explosions would not have occurred. Dangerous explosions can happen only because the water may be gradually ejected from the iron vessel through the jet or into the gas bag; or because, when the gases are made to pass rapidly through water in large quantities, there is a continuous series of large bubbles rushing through it; and should the uppermost bubble be ignited, the flame might possibly descend through the series of bubbles without being extinguished by the surrounding water. In the apparatus which I have made, a dangerous explosion from any of these causes is impossible. The apparatus consists of two wrought-iron vessels of unequal size. The smaller is the one already described. The large one is about 7½ inches high and 4 inches in diameter; its sides are about ⅞ths of an inch thick. On the top of the vessel is laid a collar of thick vulcanized India-rubber. An iron plate about ¾ths of an inch thick is then screwed down to it by five iron bolts. The vessel is thus made air-tight. The top of this vessel is connected by an India-rubber tube with the bottom of the small one; the bottom of it is connected by a similar tube with the gas bag, gasometer, or voltameter. The two vessels are nearly filled with water. The gas is sent into the bottom of the large one, ascends through the water, passes through the tube to the bottom of the small one, then through the water, and issues from the jet screwed to the top of the small vessel. Since the two vessels are of very unequal size, it is impossible that all the water should be carried out of both at the same time by the stream of the gases; and should an explosion occur after the small vessel became empty, the flame would be stopped by the water in the large vessel. In each vessel the gases are made to pass through wire-gauze or perforated zinc, or through small pieces of porous earthenware, in order to break the bubbles, and thus prevent the gases from ascending in a continued series of large bubbles. To prevent the water from being driven into the gas bag or voltameter which may be used, I have put a strip of vulcanized India-rubber across the hole through which the gases enter into each of the iron vessels. The strip of vulcanized India-rubber acts as a valve, which opens inwards and admits the gases into the vessel; and when pressed outwards by the expansive force of the exploded gases, it closes the hole and prevents the escape of any part of the water into the gas bag or voltameter. Hence it is evident that in our apparatus dangerous explosions cannot happen, either because the water may be gradually ejected from the iron vessel through the jet or back into the gas bag, or because the flame might ascend through a continued series of bubbles of the mixed gases. The size of the iron vessels should be in proportion to the quantities of mixed gases inflamed, and to the length of time the flame is kept up. Should a person wish to continue the lime light for a very long time, he would do well to have two pairs of vessels and change them every hour or half-hour, or examine occasionally whether the water was carried away by the stream of the gases. I have several times tried the large vessel alone without any accident, although the gases above the water occasionally exploded when they were not pressed through the jet with sufficient force. I believe that this vessel may be used alone without danger; but I would recommend in all cases the use of two vessels, one of which should be a good deal larger than the other, that the small one be about 3 inches in diameter and 6 inches high, and that the experimenter examine occasionally whether they contain water. I always kept the gas bag in a place in which, though an explosion should occur, no injury could be done to any person. On one occasion I filled the small iron vessel with shot instead of water. The shot prevented the return of the flame for a little time, but after five or six minutes the gases in the small vessel and in the upper part of the large one exploded, but the flame did not descend through the water. The mixed gases which I employed were always obtained by the decomposition of water. One of the gas bags commonly used for the gas microscope and polariscope may be filled in two or three hours by a cast-iron battery of six cells and as many zinc plates, each 6 inches square. I always employed nitrosulphuric acid which had been previously used. Acid which had been used before answers very well for producing decomposition; because, for this effect, the cells of the battery must be so arranged that its intensity will not exceed that of three, or at most four cells in series. When the gases are obtained by the decomposition of water, they are always mixed in the proportions in which they answer best for the production of intense heat and light; hence, when it can be done without danger, it is better to produce the lime light by the decomposition of water than by getting the gases into separate vessels, and then mixing them in the proper proportions. With half a dozen of cells of the cast-iron battery, a voltameter, a pair of gas bags, and the apparatus just described, the gas microscope and polariscope, as well as the dissolving views, may be exhibited. But the experimenter must, until he acquires experience, proceed with great caution.

The second result was a new voltameter, to which a common jet may be screwed, and the mixed gases inflamed as they issue from it without the slightest risk of injury, and by which the full decomposing power of a battery of 100 or 500 cells arranged in one series may be exerted without exhausting the power of the battery more rapidly than if it consisted of three or four plates. This voltameter is new in every respect,—in the material of which it is made, in the manner in which the electrodes are connected with the opposite ends of the battery whilst the vessel remains air-tight; new in the metallic plates employed as electrodes, and in the arrangement of these plates; finally, new in the fluid which is used for the decomposition. After having succeeded in making the apparatus for preventing dangerous explosions, I found that a glass vessel was totally unfit for a voltameter such as I wanted: first, because it is extremely difficult to make a glass vessel permanently air-tight; secondly, because it is not sufficiently strong to bear the pressure of the condensed gases; thirdly, because on one occasion, by unscrewing a bag which I had filled with the mixed gases, a spark which was produced by accidentally breaking connexion with the battery, or by the contact and separation of the electrodes, inflamed the gases and caused an explosion which shattered the vessel. I fortunately escaped unhurt. The vessel which I have used for nearly the last two years is made of wrought iron about an inch thick. Its form is cylindrical, its height about 16 inches, and its inside diameter 6 inches. It is open at the top, which was turned flat in a lathe; the bottom is laid on a circular piece of wood, which is placed on an iron plate about $\frac{5}{8}$ths of an inch thick and 8 inches square. On the top of the vessel is laid a thick collar of vulcanized India-rubber, and on this an iron plate similar to the one under the bottom. In the top plate there are two holes, to one of which a stopcock is adapted; to the other is fitted, air-tight, a perforated piece of brass which projects about an inch and a half above the plate. The hole in this brass is large enough to admit a thick wire. The upper part of the brass is tapped in order that a brass cap may be screwed to it, to prevent the escape of the gases through the hole. When the electrodes are placed in the vessel, one of them is connected by solder or pressure with the inside; to the other is soldered or riveted a copper wire, which passes through the hole in the brass screwed into the top iron plate. By means of four ¾-inch bolts of iron, which pass through the top and bottom iron plates, the top plate is screwed down on the India-rubber collar. The wire is then wedged against the side of the hole in the piece of brass, and the brass cap is screwed on. The board between the bottom of the vessel and under iron plate, and the India-rubber collar between the top and upper iron plate, insulate both plates from the iron vessel. The outside of the vessel is connected with one end of the battery, and the top or bottom plate with the other. Thus the two electrodes are connected with opposite ends of the battery; and if acidulated water, or water containing any of the alkalies, be poured into the vessel, it will be decomposed by the voltaic current. If a stopcock to which a jet is attached be screwed to the top plate, the gases will rush through the jet and may be ignited without the smallest danger, for the explosion of the gases contained within can never burst a vessel of such strength. I have had frequent explosions without producing any injurious effect, though the vessel was sometimes nearly half-filled with the mixed gases. Before discarding the glass vessel, I began to use sheet iron instead of platina electrodes. I found that when the intensity of the battery exceeded that of three or four cells, the power of the battery was soon exhausted; hence in using a battery of eighty cells, I was obliged to arrange them in twenty rows, each containing four cells, and to connect all the end zinc plates so as to form one, and all the iron cells at the other end so as to act as one. I also found, that, to obtain the full effect of the decomposing power of the battery, the acting surface of each electrode should be as large and a half as the acting surface of the zinc in each circle. Hence about 9 square feet of sheet platina, which would cost nearly 30*l.*, are necessary for a voltameter large enough for a battery of eighty 4-inch plates, when they are properly arranged for decomposition. Platina plates are not only very expensive, but they are also very easily torn and rendered unfit for use. I used a pair of sheet-iron plates about 4 feet long and 9 inches broad. To one of them I soldered a piece of thick sheet copper, and to the other a thick copper wire. I then covered one of them with linen, and rolled the two into a coil about 4 inches in diameter. I had then two plates of iron, nearly 3 square feet in surface, separated from each other by the interposed linen. The coil was put into the iron vessel. The copper plate soldered to one of the iron plates was connected with the inside of the vessel, and the copper wire attached to the other was connected with the perforated brass in the top. The vessel was then filled with a solution of carbonate of potash. I employed this solution in order to prevent the oxygen from attacking the positive electrode. When the plates were connected with the opposite ends of a battery, the water was rapidly decomposed, and a considerable quantity of the mixed gases obtained. From the intensity of the lime light produced by the gases, it appeared that the potash effectually prevented the combination of the oxygen with the positive electrode. The iron plates worked well for about an hour; the decomposition then began to decline very rapidly, though the battery was in good order. I then took up the iron plates, separated them from each other, and removed the linen cover. One of the plates was coated with a non-conducting black deposit. This was the first time I found such a deposit on either of the plates; on all former occasions they were separated from each other by slips of wood or gutta percha. I afterwards got a pair of lead plates, which I rolled up in the same way as the iron plates; they, too, ceased to act after some time, because one of them became covered with a black non-conducting substance. I then got four concentric hollow cylinders made of sheet iron $\frac{1}{8}$th of an inch thick, so that the largest of them fitted in the iron vessel. The first or innermost one was connected with the third, and the second with the fourth; they were separated from each other by wedges of wood, and also insulated from the bottom of the vessel. The first and third were connected with the top plate, and the second and fourth with the side of the vessel. These acted tolerably well; but when I used a pair of lead electrodes with a battery of ten 6-inch plates, the lime light was considerably larger and more steady than when I employed the iron cylinders. Hence I resolved, if possible, to give up the use of iron electrodes. I saw at the same time that lead plates would not answer; for when they are placed near each other, a slight pressure, and sometimes even their own weight, brings them into contact with each other. It then occurred to me to try strong tin plates immersed for a few seconds in melted lead, or in a liquid alloy of lead and tin, in which alloy the quantity of tin would be small compared with that of lead. Not having any new tin plates at hand, I cut up some old tin vessels, and made three plates, each nearly 4 inches wide and 6 long. I coated one of them, by means of a soldering iron, with an alloy, containing about seven parts by weight of lead and one of tin; and another with an alloy containing about three parts of lead and one of tin. On the third, after being coated with the alloy, I sprinkled some powdered sulphur, and held the plate over the fire until the sulphur was inflamed: the plate was thus covered with a black coating of burnt sulphur. I then tried each of the three, and also a plate of lead about the same size, as the negative element of a nitric acid battery; that is, I put each successively, instead of platina or cast iron, into a porous cell containing nitrosulphuric acid. Each of the three acted far more powerfully than the lead plate. The plate coated with the alloy containing the largest proportion of lead acted better than the one whose coating contained least lead, but not so well as the plate on which sulphur was burnt. This last plate produced a galvanic current very nearly equal to that of a platina or cast-iron plate. I afterwards put into concentrated nitric acid a piece of the tin which was coated with the alloy of lead and tin, and with sulphur, a piece of one of the alloys of lead and tin and a piece of lead, and left them in the acid for about twenty hours. On taking them out of the acid, I found that a good deal of the lead had been dissolved, but the piece of coated tin and the alloy were merely blackened on the surface. From the results of these experiments, it is evident that tin plates, coated with an alloy of lead and tin, in which the proportion of tin is small, are more passive in nitric acid, less oxidable, and consequently better suited for the electrodes of a voltameter than lead plates. The tin plates are stronger and more elastic than leaden ones, and therefore are not so easily brought into contact with each other. It was in last March or April that I discovered that tin plates, coated with an alloy of lead and tin, are less oxidable than lead. Since that time I have in all my experiments used the coated tin plates as electrodes. I have arranged these electrodes in two ways; in one way for a battery of low intensity, and in another for batteries of high intensity, or of a large number of cells all in one series. In one of the former arrangements there were twenty plates, each 12 inches by 4; they were all parallel, and separated from each other by slips of wood about $\frac{1}{16}$th of an inch thick. Ten of them were connected with one end of the battery; these were of course the alternate plates; the other ten were connected with the opposite end. The acting surface of each electrode, including both sides of each plate, was something more than 3 square feet. The electrodes for batteries of high intensity are also parallel and separated from each other, about one-sixteenth of an inch, by a non-conductor. But the two outside or terminal plates only are connected with the battery; one with the negative, the other with the positive end. The terminal plate, which is connected with the top iron plate of the voltameter, must be covered on the outside by a non-conductor, otherwise the voltaic current would pass to the side of the iron vessel, and would not pass through the plates and fluid interposed between the two outside or terminal plates. The cells between each pair of plates must be made nearly water-tight, and must be open only on the top, in order that when the terminal plates are connected with the battery, the voltaic current may have no way of passing from one end of the battery to the other but through the interposed plates and fluid. In each plate there should be a small hole near the bottom, that the cells may always remain nearly filled with the fluid. The fluid should never rise above the upper edge of the electrodes, otherwise a great part of the galvanic current would be transmitted by it from one terminal plate to the other without passing through the interposed plates or fluid. The number of cells formed by the interposed plates should be about one-fourth of the number of cells in the battery. Thus for a battery of 12 cast-iron cells, there should be 3 cells or 2 plates between the two terminal plates. For a battery of 100 cast-iron cells in series, there may be 25 decomposing cells or 24 interposed plates. A battery of 100 cells has twenty-five times the intensity of a battery of 4 cells, therefore the current from it will overcome twenty-five times as much resistance as the current from 4 cells, and will pass through 25 decomposing cells successively as freely as a current from a battery of 4 cells will pass through a single decomposing cell. If the current from a battery of a hundred well *insulated* cells be sent through 25 decomposing cells, and afterwards through the coil of a galvanometer, which coil is made of thick copper wire, it will be found that the deflection of the needle will be equal to that which will be produced by a current from a battery of 4 cells passing through one decomposing cell and through the coil of the same galvanometer. Hence there is as much of the mixed gases produced in each of the 25 decomposing cells as in the single cell through which the current from the battery of 4 cells passed, that is, twenty-five times as much of the mixed gases as is produced by a battery of four cells. Hence the full decomposing power of a battery of a hundred cells is exerted; and because the intensity of the current is reduced to that of a battery of four cells, the power of the battery is not exhausted more rapidly than if it consisted of four cells in series. If the current of a battery of a hundred cells in one series were sent through the electrodes as they are commonly arranged, the power of the battery would be exhausted about twice as soon as if the current passed through the electrodes arranged for batteries of high intensity, and the twelfth part of the full decomposing power of the battery would not be effective. To those who wish to show with the same battery the deflagrating power of the voltaic current, the coke light, and the decomposition of water, and the lime light, an arrangement of the electrodes similar to that which has been just described will be useful, because a battery arranged for intensity will answer for all these effects. But when a battery is put up for the sole purpose of decomposing water, it is better to arrange the cells in such a way that the intensity may not exceed that of four cells in series; because if a battery of 100 cells be arranged in series, a single bad porous cell or bad zinc plate will diminish considerably the power of the entire battery; but if the 100 cells be arranged in twenty-five rows, each containing four cells, and all the terminal zinc plates be connected so as to act as one plate, and all the end iron cells be connected so as to act as one, a bad porous cell or zinc plate will diminish the power only of the row to which it belongs, but not of the other rows.

Some of my experiments led me to believe, that, by means of the arrangement of the electrodes for a current of high intensity, the decomposing power of the battery may be considerably increased; from other experiments I was somewhat disposed to infer that by such arrangement no increase of power can be gained. I am at present in doubt, and must remain so till the state of my health enables me to repeat my experiments.

The fluid which I first used in the voltameter was a solution of caustic potash. When a current was sent through the solution, the iron vessel was soon filled with foam, which came out through the jet with the mixed gas and extinguished the flame. I then tried a solution of carbonate of soda. The soda prevented the oxidation of the positive electrode as well as potash, and did not foam so much. However, even with soda, the quantity of foam was so great, that the iron voltameter, 16 inches high and 6 inches in diameter, would be very soon filled with it if a battery of more than about twelve 6-inch cells were employed. I tried various means of preventing the foam. I first covered the electrodes all round with a cloth, so that the gases should pass through it. Many of the bubbles were thus broken; but when a powerful battery was used, the voltameter was soon filled with foam. I then put about a teaspoonful of coal naphtha into the vessel. This prevented the foam from rising to any considerable height in the vessel, but it was somewhat injurious to the light. I also tried turpentine: it diminished the foam, but injured the light more than the naphtha. I think it increased the violence of the explosion of the gases. I afterwards tried some nitre, and also common salt; each of them prevented the foam, but destroyed the light. After failing in all my efforts to prevent the foam, I thought there was no alternative but to get an iron vessel so large that all the foam which could be produced by a powerful battery would be contained between the top of the electrodes or surface of the fluid, and the top of the vessel without rushing through the jet. Latterly, I have tried carbonate of ammonia instead of soda, and have found that it foams much less, and that it prevents the action of the oxygen on the positive electrode. A solution of the proper strength will be obtained by dissolving an ounce and a quarter, or an ounce of the carbonate of potash, soda, or ammonia in a quart of water. If there be more than an ounce and a quarter to each quart of water, the quantity of foam will be very considerable; if there be less than an ounce, the conducting power of the solution will not be sufficient, and the quantity of the gases produced will be greatly diminished.

For either of the two arrangements of the electrodes I have described, a cylindrical vessel does not answer well. For them a prismatic vessel having a rectangular bottom is the most convenient. The inside of the iron vessel should be coated with an alloy of lead and tin, or of lead, tin and antimony, in which the proportion of tin, or of tin and antimony, is small,—first, in order to preserve it from rust; secondly, to protect it against the action of sulphuric acid, and thus render it fit for a voltameter, in which the positive electrode is platina; the negative one, tin plates coated with an alloy of lead and tin; and the fluid through which the voltaic current passes is dilute sulphuric acid. The coated sides of the vessel may be the negative electrode. With an iron voltameter such as I have described, the mixed gases may, without the slightest danger, be inflamed as they are produced by the decomposition of water, and a constant and brilliant lime light may be obtained.

The third result is a new negative element, cheaper, more durable, and one which may be made to act with greater power than the platinized silver used in Smee's battery. It is sheet tin, coated with an alloy of lead and tin, in which the proportion of tin is not greater than that of lead, or of lead, tin and a small quantity of antimony. On tin plates thus coated, the dilute sulphuric acid commonly used in Smee's battery will scarcely exert any action. It may be platinized like sheet silver; or it may be coated with borax, and will then answer as well, or very nearly as well, as if it were platinized. It is evident that tin plates thus prepared are far cheaper and more durable than platinized silver; and because they can be brought nearer than platinized silver to the zinc plates without danger of touching them, they may be made to act with greater power.

The fourth result is a new means of protecting iron against the action of the weather and of various corroding substances, so that iron thus protected will answer for all the purposes to which sheet lead and galvanized iron are applied. Besides the experiments by which I have proved the superiority of tin plates (that is, of sheet-iron plates), coated with an alloy of lead and tin, over leaden ones, as the electrodes of a voltameter, I have made many others, in order to compare the action of concentrated nitric, sulphuric and muriatic acid, as well as of dilute sulphuric and muriatic acid on lead and galvanized iron, with their action on iron coated with an alloy of lead and tin, in which the quantity of lead was about equal to that of tin, or from two to seven or eight times as great as that of tin; and from these experiments I infer that iron, coated with any of the above-mentioned alloys, is less oxidable and less liable to corrosion than lead or galvanized iron, the zinc coating of which, as every one knows, is rapidly dissolved by the acids, even when they are greatly diluted with water. Iron, then, coated with an alloy of lead and tin, in which the quantity of lead is nearly equal to, or exceeds that of tin, will answer as well as lead or galvanized iron for roofing, cisterns, baths, pipes, gutters, window-frames, telegraphic wires, for marine and various other purposes. A small quantity of zinc, mixed with the alloy with which the iron is coated, hardens the coating, but diminishes its power of resisting corrosion. But the addition of a little antimony not only hardens the coating, but also makes it less oxidable and less liable to corrosive action. Iron, coated with the alloy of lead and tin, or of lead, tin and antimony, may answer better than lead for vitriol chambers. It may be sometimes used instead of copper for the sheathing of ships; and bolts and nails of coated iron may be sometimes employed instead of copper bolts and nails. It may also be used for some of the purposes for which enamelled iron or cast iron is employed, such as the enamelled cast-iron cisterns and pipes used in water-closets. At the last meeting of the British Association at Hull, Dr. Gladstone stated that the owners of iron-built ships object to sugar cargoes, because the saccharine juices that exude from the casks corrode the metal. If the casks or the ships were lined with thin sheet iron, coated with an alloy of lead and tin, containing two or three times as much lead as tin, it is highly probable that the corrosion of the ships would be prevented. I have left for a considerable time a small piece of an alloy of lead and tin, in which the quantity of lead was four or five times as great as that of tin, in a solution of sugar and common water; and the alloy came out as bright as when it was put into the solution.

The fifth result is a new mode of producing, by means of a small galvanic battery, a brilliant intermittent lime light. By means of a battery of twelve 4-inch cast-iron cells, or of four cells each 6 inches by 8, a small voltameter, such as I have described, and a good Hemming's jet, a constant lime light, about a quarter of an inch in diameter, may be produced. If then the jet be attached to a stopcock, by which the gases are confined in the iron voltameter for fifty-five seconds in every minute, and are allowed to issue from the jet only for five seconds in each minute, twelve times as much of the gases must pass through the jet in these five seconds as would pass through it in the same time were the stopcock always open. Hence if the gases produced by the battery are ignited for five seconds in each minute as they issue from the jet, and are confined in the voltameter for the remaining fifty-five seconds, the flame will, when thrown on lime, give a light twelve times as large as one a quarter of an inch in diameter, or nearly seven-eighths of an inch in diameter. If the breadth of the hole in the key of the stopcock be $\frac{1}{24}$th of the circumference of the key, and if the key make a revolution in every two minutes, the stopcock, because it is opened twice in each complete revolution of the key, will be opened once in every minute for five seconds, and will be closed for fifty-five seconds. Now by clockwork it is very easy to make the key of the stopcock perform a revolution once in every two minutes, and consequently to produce a lime light seven-eighths of an inch in diameter for five seconds in every minute. It is easy to make the motion of the key and the ratio of the diameter of the hole to the circumference of the key such that the stopcock will be opened once in two minutes for ten seconds, and will be closed for a minute and fifty seconds. In order to light the gases whenever the stopcock is opened, it is necessary to have a small flame of gas or of a candle always at the nozzle of the jet. A lime light of seven-eighths of an inch in diameter would be seen at a far greater distance, particularly in foggy weather and in snow-storms, than the light of a lamp; and is therefore of the utmost importance in lighthouses. Had there been such a light in the Bayley Lighthouse, the Victoria would in all probability not have been lost. The expense or trouble of such a light would not be very great, and would be amply compensated by saving the lives of many every year. The light might be used only in foggy weather or in snow-storms, when a light capable of penetrating through a dense atmosphere or a shower of snow would be required. On other occasions the ordinary lights might be employed. The expense of an intermittent lime light, such as I have described, would be the cost of working a cast-iron battery containing four zinc plates, each 6 inches by 8, for sixteen or seventeen hours a day, and of a very minute flame of common coal-gas for the same space of time, as often as the light might be required. Surely such an expense could not be very great. Should it be apprehended that the action of the battery would not be sufficiently constant to give the light required, then three large gas bags, each of which would contain as much of the mixed gases as would be sufficient to maintain a lime light three-fourths or seven-eighths of an inch in diameter for half an hour, might be filled during the day with the mixed gases by two or three small batteries. Then, by means of the apparatus which I have devised for safely applying the mixed gases to the production of intense heat and light, of a proper jet, a suitable adjustment of clockwork, and a very small flame of coal-gas, the intermittent light may be kept up for eighteen hours; for the light would last only five seconds in each minute, and consequently only five minutes in each hour, or an hour and a half in eighteen hours. Therefore, since the three gas bags, filled with the mixed gases, would maintain a constant light for an hour and a half, they should keep up the intermittent light for eighteen hours. I believe such an intermittent would answer very well for lighthouses. In the lighthouse on the eastern pier at Kingstown, the light is a revolving one, which completes a revolution in about a minute. The brilliant white light, the only one which can be seen at a distance, shines out only for about five seconds in each minute. Hence I infer that an intermittent lime light which lasts for five seconds in each minute would be sufficient for lighthouse purposes. It is necessary to observe, that, when gases are gradually cut off from the jet, the gases in the upper part of the voltameter, or vessel to which the jet is attached, will explode if a Hemming's jet be not used; and this explosion, though perfectly free from danger, will be attended with inconvenience, for a vacuum will be produced in the upper part of the voltameter or vessel, and no gases can pass to the jet till this vacuum is filled. Hence, in producing an intermittent lime light, a Hemming's jet should be employed. Since the discovery of the cast-iron battery, several persons have obtained patents for apparatuses for the coke light, or, as it is now called, the electric light. If they arranged their apparatus so as to produce an intermittent coke-light similar to the intermittent lime light I have described, they would confer a great benefit on society. When the coke light is constant, the battery is soon exhausted, the coke points are consumed, and must be frequently renewed; but were the light intermittent, a single charge of the battery might last for an entire night, and it would be sufficient to renew the coke points two or three times in the course of the night. And if an intermittent coke light were used in lighthouses only on very dark and foggy nights and in snow-storms, the expense could not be considerable. I think the coke light is more intense than the lime light, and also somewhat less expensive. To produce a coke light sufficient for all illuminating purposes, forty cast-iron cells, each containing a zinc plate 2 inches by 4, will suffice. To obtain a lime light of equal illuminating power, a battery containing at least twice as large a surface of zinc will be required. A battery with a given charge will scarcely work twice as long in decomposing water as in igniting a pair of coke points. Therefore, on the whole, I think the lime light is more expensive, but much more easily managed than the coke light. The former requires a battery of only four large plates; the latter depends on the intensity of the current, and requires a battery of at least thirty-five or forty cells. The coke points will require to be changed more frequently than the lime; and there is more reason to fear that the coke light will fail on account of the destruction of the positive coke point, than that the lime light will go out on account of the wearing of the lime. For the coke light, the zinc plates should not contain more than 8 square inches of surface. If they be larger, the positive coke point will be rapidly destroyed, without a proportional increase of light.

The sixth result is a new mode of exhibiting the dissolving views by means of the lime light. In the common apparatus for showing the dissolving views, the apertures of the lanterns are gradually opened and gradually closed, in order to make the figures come gradually into view and gradually disappear. By using the mixed gases, the apertures may be left always open, and the figure in either lantern may be gradually brought out on the screen, and may be made to fade away by degrees; for by opening very slowly the stopcock through which the gases pass to the jet, the light in the lanterns may be made to increase very slowly till it has acquired its full intensity, and consequently the image thrown on the screen will become gradually brighter till it attains its full brightness; and by very slowly closing the stopcock, the light, and consequently the image, will gradually fade away. By exhibiting the dissolving views in this way, there is a great saving of the mixed gases; for the light shines out fully in each lantern only when it is necessary to exhibit the image in full brightness on the screen. Besides, by diminishing the quantity of the gases which passes through one jet, the quantity which passes through the other jet, and consequently the brightness of the full image, is increased.

The last result is a new sine galvanometer, which is the only instrument yet devised for measuring with accuracy very powerful galvanic currents[*]. Some of my experiments raised doubts in my mind about some of the generally received theories relating to the action of the galvanic battery. The galvanometers which I had were not fit for measuring very powerful currents, such as I employed. I was therefore obliged to get a new one. After a good deal of reflection, I resolved to get one which would answer for a sine instrument, for the common tangent instrument, and for the new tangent galvanometer, the principle of which is demonstrated in the *Comptes Rendus* of the 24th of last January. The galvanometer which I have made consists of a mahogany circle 2 feet 4 inches in diameter and nearly 2 inches thick, in the circumference of which is turned a groove half an inch wide and $3\frac{1}{2}$ inches deep; of seven concentric coils of $\frac{3}{8}$-inch copper wire in the groove, and well insulated from each other; of a strong frame in which the circle is moveable on an axis, and

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[*] The voltameter will not measure powerful currents produced by a single circle, or by two or three circles.

always kept in vertical position; and of a compass-box, which, by means of a slide 3 feet long and at right angles to the circle at its centre, may be moved in a direction perpendicular to the circle to the distance of 3 feet from it, so that the centre of the needle, which is a bar-needle $5\frac{1}{8}$ inches long, will always be in the axis of the circle and of the coil, and that the line joining the north and south points of the compass-box will be always parallel to the horizontal diameter of the mahogany circle and coil. From this description of the instrument, it is evident that (no matter where the compass-box is placed on the slide) the needle is parallel to the mahogany circle and coil, or perpendicular to their axis, whenever it points to 0°. Hence, if a voltaic current, sent through the coil, deflect the needle, and if the circle and coil be turned round so as to follow the needle until it points to 0°, the needle, no matter where it may be placed on the slide, will then be parallel to the coil and perpendicular to its axis. The magnetic power of the coil is exerted in the direction of its axis. The effective part of the earth's magnetism in impelling the needle to the magnetic meridian is also exerted in the direction of a perpendicular to the needle or of the axis of the coil, but opposite to that in which the magnetic force of the coil acts. Since the needle is kept at rest by these two forces acting in opposite directions, they must be equal. But the effective part of the earth's magnetism in impelling the needle to the magnetic meridian, varies as the sine of the angle which it makes with that meridian. Therefore the magnetic power of the current flowing through the coil also varies as the sine of the angle which the needle, when it points to 0°, or the mahogany circle D, makes with the magnetic meridian. If the connexion with the battery be broken, the needle will immediately return to the magnetic meridian. The graduated circle of the compass-box will give the number of degrees the needle was deflected from the magnetic meridian. For measuring the angle of deviation, I have used a large graduated circle about 13 inches in diameter, which is attached to the upper part of the mahogany circle, and at right angles to it and to the axis about which it is moveable. By means of seven brass bolts, each three-eighths of an inch in diameter, I can send the current from the battery through one coil, or seven, or through any intermediate number of coils. I scarcely ever use more than one coil, and that is the outside one, the diameter of which is about 2 feet 2 inches. When the current is sent through seven coils, the deflection is so great that only very feeble currents can be measured on the sine galvanometer. When the needle is in the centre of the coil, this galvanometer, used as a sine instrument, large as is its diameter, is not capable of measuring the power of a current produced by a single circle of the cast-iron battery, in which the zinc plate is an inch broad and 4 inches long. The coil, when made to follow the needle, drives it always before it, so that the needle will never remain parallel to the coil. Hence, in Pouillet's sine galvanometer, in which the centre of the needle is in the centre of the coil, only feeble galvanic currents can be measured. But by sliding the compass-box and needle to 2 or 3 feet from the coil, a current of very great power may be measured. A current which produces a deflection of 75° when the needle is in the centre of the coil, will produce a deflection of only 3° when the needle is at a distance of 3 feet from the coil and its centre in the axis of the coil. Hence, by our galvanometer, in which, and in which alone, the needle may be moved to a distance from the mahogany circle or coil whilst its centre is in the axis of the coil, currents of enormous power may be measured. With this galvanometer I expect to determine,—first, the smallest number of circles the currents of which will be fully effective, or at least as effective as any larger number of galvanic circles in decomposing water, and consequently the number which may be used with the greatest advantage in decomposition; secondly, whether Mr. Faraday's law, viz. that the quantity of the mixed gases produced by a voltaic current is proportional to the quantity of electricity which passes, holds for currents of great power; thirdly, the proportion which the surface of the decomposing plates should bear to the acting surface of zinc in each circle; fourthly, whether a battery with a given charge will work longer in producing the lime light by decomposing water than in producing the coke light, and which of the two lights is the more œconomical.

To determine the smallest number of cells of the cast-iron battery which are as effective as any larger number in decomposing water, I measured the quantity of the mixed gases produced in two minutes by 3, 4, 5, 6 and 12 cells, and also the deflection of the needle produced by the current from each battery when it passed through a solution of carbonate of soda in which there was about an ounce and a half of soda to each quart of water*. From the steadiness of the needle, it was evident that the current from each battery was perfectly constant during the two minutes it was allowed to act. The quantity of the gases produced by 3 cells was less than three-fourths of the quantity produced by 4, and less than half the quantity produced by 6.

Hence 3 cells are not as effective as 4, 5 or 6. The quantity of the gases produced by 3 cells was sensibly less in proportion to the sine of the angle of deviation shown by the sine galvano-

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* The quantities of the gases produced by 1 or 2 cells were so small, compared with that which was produced by 3 or 4, that I did not measure them.

meter, than it was when 4 or 5 cells were employed. Hence it appears, that, for want of sufficient intensity, a sensible portion of the electric current produced by 3 cast-iron cells passes through a solution of carbonate of soda without producing decomposition; and therefore in decomposing water, 3 cells cannot be used with advantage. On one occasion the current was sent from 3, 4 and 6 cells, through a solution of sulphate of soda mixed with some carbonate of soda. The quantity of the mixed gases was very nearly in proportion to the number of cells and to the sines of the angle of deviation shown by the sine galvanometer. The sine of deviation appeared to be a little less, in proportion to the quantity of the gases produced, with 4 than with 3 or 6 cells. In this case, the sulphate of soda, as well as the water, was decomposed; for when the flame of the gases was thrown on lime, the bright white light produced by the oxyhydrogen flame was surrounded by another dim light of a purple hue. With the solution of carbonate of soda alone, the quantity of the gases produced by 4, 5, or 6 cells appeared proportional to the number of cells and to the sines of deviation. Hence, cast-iron batteries of 4, 5, or 6 cells will be equally effective in producing decomposition. But a battery of 4 cells will work as long and a half with a given charge as one of 6 cells, and as long and a quarter as one of 5; because in these batteries the quantity of electricity constantly flowing through each cell is proportional to the number of cells. Hence in preparing a battery for decomposing water, all the cells should be arranged in rows of 4 each. Four cells produced more than half the quantity of the gases produced by 12 cells of the same size. A battery of 60 cells, arranged in one series, after having worked for some time, produced in one minute very little more than 4 cells of the same size. I have not yet tried the decomposing power of a battery of a larger number acting in one series; but I shall have occasion, in the month of February, to show to my class a battery of about 250 cells; and I will then measure the quantity of the gases produced in one minute when all act in one series. Eight cells, arranged in two rows of four each, so that the two end zinc plates were connected as one, and the two iron cells also acted as one, produced considerably more of the mixed gases in two minutes than was produced by 12 cells of the same size acting in one series. The quantities of the gases produced by the 8 and the 12 cells were in the ratio of $11\frac{1}{2}$ to 9. But with the 12 cells, the sine of deviation shown by the sine galvanometer appeared to be greater in proportion to the quantity of the gases produced than when the battery of 8 cells in two rows was employed. Hence it would appear, that when the intensity of the current is much greater than that of 4 or 6 cells of the cast-iron battery, the ratio of the quantity of water decomposed to the quantity of electricity passing through it is diminished; and consequently that Mr. Faraday's law of definite electro-chemical decomposition does not hold for powerful currents, which differ much in their intensity. In my last experiments I perceived a defect in the magnetic needle employed, and therefore do not feel myself warranted in drawing a positive conclusion. Besides, I am sure that my experiments were not made with as much accuracy as Mr. Faraday's were; but they incline me to believe that the decomposing power of strong currents decreases when their intensity increases. I am inclined to believe that there is a certain intensity above and below which there is a loss of decomposing power; and that, in a cast-iron battery, it is the intensity of 4 cells in one series. I am inclined to think that, when the intensity is greater than that, some of the electricity passes through water without meeting the resistance or reaction necessary for decomposition; and that the greater the intensity the greater the quantity which passes without the required resistance. The current from a battery of 12 cast-iron cells does not meet with much resistance in passing through a solution of carbonate of soda. For when the current was sent, without passing through the fluid, through the coil of the tangent galvanometer, it produced a deflection of 69°; and when sent first through the fluid and then through the coil, the needle was deflected to 68°. When a current was sent from 60 cells, first through the fluid and then through a pair of coke points, the deflection was the same as when it passed through the coke points without passing through the fluid. Hence a current from a battery of 60 cells appears to meet with as little resistance in passing through a fluid as in passing through a wire. Hence a current of very high intensity experiences little or no resistance or reaction from the fluid, and therefore can produce little effect on it.

In comparing the deflection of the needle produced by a current from 6 cells through a solution of carbonate of soda with the deflection produced by the current from the same battery sent directly through the coil of the galvanometer, it occurred to me that if 12 cells were arranged in two rows, and the two end zinc plates connected as one, and the two iron cells connected so as to act as a single cell, the current from these two rows would, after passing through a fluid, produce a greater deflection of the needle than they would, if, whilst they acted in one series, the current were sent directly through the coil of the galvanometer. Subsequent experiments proved that my conjecture was well founded. When 12 cells were arranged in two rows of six each, and the current from both was sent through a solution of carbonate of soda and then through the coil, a deflection of 67° was produced in the needle of the tangent galvanometer. When the current was sent from the same 12 cells acting in one series through the coil alone, the deflection was only 64°. Hence more electricity passed from the two rows of six each through the fluid, than from 12 cells through the wire. Now it is well known, that when a current of electricity passes from one end of a battery to the other through a conductor in which it meets no resistance, the same quantity will pass in a given time between the ends of the battery, whether it consist of a single pair, or of a hundred or of a thousand circles, or of any number whatever. Hence more electricity will pass in any time through a solution of carbonate of soda from 12 cells arranged in two rows of six each, than will pass in the same time through a wire or fluid from a thousand, or any number of cells of the same size. Therefore whether Mr. Faraday's law hold for powerful currents of great intensity, or whether the conclusion drawn from my experiments be just, 12 cells, arranged in two rows of six each, will produce more decomposition in a given time than will be produced by a thousand, or any number of cells acting in one series. I have found, in the same way, that 8 cells, in two rows of four each, will produce as much, or nearly as much, of the mixed gases in a given time as any number acting in one series. Hence, with the common voltameter, a battery of 500 cast-iron cells, arranged in rows of four, will produce more than fifty times as much of the mixed gases as it will produce when all the cells are arranged in one series. Besides, when the cells are arranged in rows of four, the battery with a given charge will act about twice as long as when all act in one series; for in the latter case about twice as much electricity passes constantly through each cell as in the former, and consequently the power of the battery is exhausted about twice as soon. Hence, on the whole, with the common voltameter, a battery of 500 cast-iron cells arranged in one series will not produce the hundredth part of its full decomposing effect; but with the voltameter I have described, it will, as I have shown, produce its full effect. If a battery of 4 cast-iron cells, and another of 1000 or of any number of cells of the same size be similarly charged, the former will, before its power is exhausted, produce as much of the mixed gases as the latter; because the former will produce in each minute as [half much as?] the latter, and it will work twice as long.

I will here mention one of the experiments from which I inferred, that in a voltameter for a battery of a large number of cells arranged in one series, the number of decomposing cells should be about one-fourth of the number of cells in the battery. When the current was sent from 12 cells in series, through 4 decomposing cells and through the coil of the galvanometer, the deflection was a little less than that which was produced by the current from 3 cells when it passed through one decomposing cell and the coil. The difference arose from the imperfect insulation of the cast-iron cells. But when the current was sent from the same 12 cells through 4 decomposing cells, and then through the helix of an electro-magnet by which a small magnetic machine was driven, the speed of the machine was considerably greater than when the current from 3 cells passed through one decomposing cell, and then through the coil of the electro-magnet. The wire coiled on the electro-magnet was about 50 feet long and one-eighth of an inch thick. The coil of the galvanometer is about 7 feet long and three-eighths of an inch in diameter. The resistance in the latter was insensible compared with the resistance in the former.

The ratio which the acting surface of each electrode should bear to that of the zinc in each circle may be found by putting a pair of large plates into a glazed vessel, and connecting them with the opposite ends of a battery of 4 cast-iron cells in each of which the zinc plate is small, so that the current will pass through the coil of a galvanometer. If a solution of carbonate of soda be then gradually poured into the glazed vessel until the needle ceases to recede from the magnetic meridian, that is, until no more electricity is transmitted through the fluid, it will be found that the acting surface of each of the electrodes, which is covered by the fluid, is about once and a half as great as the acting surface of the zinc plate in each circle. By pouring the fluid to any height whatever into the glazed vessel so as to increase the acting surface of the electrodes, no increase will be produced in the deflection of the needle. Hence there is a limit to the conducting power of fluids for electricity of low intensity. A solution of carbonate of soda (no matter how thick and short the column of fluid may be) will not, when interposed between the opposite ends of a nitric acid battery of 4 cells, conduct more than about one-half of the electricity which will be conducted by a short, thick wire connected with the opposite ends of the same battery. In investigating the ratio which the surface of the electrodes should bear to the surface of each zinc plate of the battery, I used solutions of carbonate of soda of different degrees of strength, and found that the conducting power of a solution, which contains an ounce in each quart of water, is very little inferior to that of the strongest solution, whilst it foams far less. If a neutral salt could be found, which, when mixed with carbonate of soda, would prevent its foaming, and would not be decomposed by the voltaic current, sheet-iron plates coated with an alloy of lead and tin would be in every way preferable to platina electrodes. I have tried nitrate of potash, bisulphate of potash, bichromate of potash, sulphate of soda, borate of soda, and chloride of sodium mixed with carbonate of soda. But they were all decomposed by the galvanic current, as was evident from the light which the gases produced. When the current was sent from the battery through a solution containing three ounces of bichromate of potash and five of carbonate of soda in about five quarts of water, there was scarcely any foam, but the light produced by the gases was tinged with red, and not so intense as the oxyhydrogen light. The heat of the flame appeared not inferior to that of the oxyhydrogen flame. When bisulphate of potash was mixed with carbonate of soda, the light and heat produced were the same as when bichromate of potash was used. The light was injured much less by mixing bichromate of potash, or bisulphate of potash with the carbonate of soda, than by the mixture of any of the other salts with it.

To estimate the comparative expense of the coke light and the lime light, it is necessary, first, to determine the size of the battery which will produce a brilliant coke light, with that of one, which, by decomposing water, will produce a lime light of equal illuminating power; and secondly, to determine the time each battery will work with a given charge. Although I made a battery of a size which I thought would be most advantageous for producing the coke light, I have not as yet succeeded in determining satisfactorily either of these two points. The battery which I prepared consisted of 60 cast-iron cells in which each zinc plate was 4 inches by 2. In the battery which I commonly used, the zinc plates were 4 inches square. I always found that this battery soon destroyed the coke points. On one occasion, about five years ago, I got a coke light sufficient for the gas microscope from a battery of 24 cells, which were only half-filled with acid. Hence I inferred that zinc plates, 2 inches by 4, would be large enough for a brilliant coke light. When the battery of 60 cells, in which the zinc plates were 2 inches by 4, was prepared, I sent the current, first, from thirty of them through a pair of coke points. The light was sufficiently brilliant for all illuminating purposes, but did not last very long. I then changed the connexion that I might try the effect of 40 cells. The light was then both steady and brilliant. I think that this is the smallest, and therefore the least expensive battery, by which, with the aid of a good apparatus for adjusting the coke points, a continuous light of great illuminating power can be obtained. If the plates be smaller, the illuminating power of the coke points will not be sufficient; and if the number of cells be less than 40, the electric current will not have sufficient intensity to pass through the flame between the coke points. With this battery a pair of coke points lasts a long time. I have found that when the battery is not very powerful, the brilliant light comes only from the positive coke point. For, if a coke point and a copper point be used instead of two coke points, and if the coke point be connected with the positive end of the battery, the light is as brilliant as if two coke points were employed. But if the coke point be connected with the negative, and the copper point with the positive end, the light will be worthless. I forgot to make this experiment when I had occasion to use a very powerful battery. I may mention here that with 60 cast-iron cells in which the zinc plates were 2 inches by 4, I was able to fuse and deflagrate a round piece of steel ¼ inch diameter. Although I made a far greater number of experiments on the lime light than on the coke light, I am not able to state positively the size of the battery, which, by decomposing water, would produce a lime light equal in illuminating power to that of a pair of coke points ignited by a battery of 40 cast-iron cells in which the zinc plates are 2 inches by 4. I can only say that I think it probable that a battery twice, or very nearly twice the size, would be required. With regard to the length of time the two batteries would work with a given charge, I am equally uncertain. In decomposing water, all the cells must be arranged in rows of four each, so that the intensity of the current may not exceed that of 4 cells. Now, when a current is sent from 4 cells through a good conducting fluid, the quantity of electricity which passes through the fluid and through each cell of the battery is only about one-half of the quantity which would pass if the ends of the battery were connected by a short, thick wire. This I have found by the galvanometer. Hence the battery would work about twice as long in the former as in the latter case. When the coke points were ignited by 40 cells, I measured the angle of deviation of the sine galvanometer, and found it to be 31° when the needle was in the axis of the coil, and nearly 10 inches from its centre. When they were ignited by 60 cells, the deviation was 40°. Unfortunately, I did not measure the angle of deviation which would be produced by the current passing directly through the coil of the galvanometer. The experiments were made before my class, and I intended to measure on the next day the angle of deviation which would be produced by the current from 4 cells of the same size, sent through a fluid. I was prevented from making the experiment until I had occasion to remagnetize the needle, and consequently I cannot now depend on its indications being the same they would have been on the day the battery of 40 and 60 cells was used. I have since tried the effect of a current from four of the cells on the sine galvanometer after passing it through a solution of carbonate of soda, and found that a deflection of $22\frac{1}{2}°$ was produced in the needle of the sine galvanometer. But I am inclined to think that had I made the experiment the day I used the 40 cells, the deflection would have been considerably less, because the needle was not then so highly magnetized. The impression made by all my experiments inclines me to believe that a battery would work nearly twice as long in decomposing water as in igniting coke points; that, therefore, the coke light and the lime light are nearly equally expensive, but that the former is somewhat more œconomical. I shall soon have occasion to exhibit a battery of about 250 cast-iron cells, and intend then to compare the quantity of electricity which will pass between a pair of coke points ignited by 40 cells, with that which will pass through a thick wire connected with the opposite ends of the same 40 cells. I intended to investigate the decomposing power of the coil, but I must defer the investigation till health and leisure permit me to resume my experiments.

Maynooth College,
January 2, 1854.

P.S. I have got a new iron vessel made of a rectangular form, for which I am preparing two sets of electrodes; one for a battery of low intensity, the other for a battery of high intensity. The former will consist of 100 iron plates coated with an alloy of lead and tin; 50 of them (the alternate plates) will be connected with one end of the battery, and the other 50 with the opposite end. The 100 plates will be divided into twenty groups, each containing 5 plates. Each group will be covered all round with linen, so that the foam produced by the ascent of the gases in the solution of soda may be made to pass through the linen, and that thus the bubbles may be broken. This arrangement of the electrodes will answer for a battery of 60 cast-iron cells arranged in fifteen rows of four each, and in which each zinc plate is 6 inches by 4. The quantity of the gases produced by each group of 5 plates will be equal to that which would be produced by 3 cast-iron cells, or the $\frac{1}{20}$th of 60. Now I have found that a linen cover on the electrodes prevents all foam when the battery does not contain more than 5 or 6 cells, in which each zinc plate is 6 inches by 4. Therefore in the arrangement just described there can be no foam. The electrodes for a battery of high intensity will consist of 156 coated iron plates, divided into twelve groups each containing 13 plates. The 13 plates of each group will form 12 decomposing cells nearly water-tight, and open only at the top. The first plate of each group will be connected with one end of the battery, and the last with the opposite end. Hence when the cells are nearly filled with a solution of soda, the voltaic current will pass simultaneously from the first to the last plate of each group, through the intermediate plates and fluid, and produce decomposition in each cell. This arrangement is intended for a battery of 60 cells, in which each zinc plate is 6 inches by 4, all acting in one series. The intensity of the current from this battery will be reduced to that of 5 cells by passing through the 12 decomposing cells. Each group of 13 plates will be covered with linen, which will prevent all foam, because in each group the quantity of the gases will be only equal to that which would be produced by 5 cells, or by $\frac{1}{12}$th of 60.

January 25, 1854.

XIII. *On some Experiments made with* Ruhmkorff's *Induction Coil. By* John P. Gassiot, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

On the 7th of January, 1852, Mr. Grove communicated a paper to the Royal Society (Phil. Trans. 1852, part 1) entitled "On the Polarity of the Gases;" the experiments described in this paper were made by him with a secondary coil apparatus made by M. Ruhmkorff. Mr. Grove, who in his paper fully describes its construction, kindly procured a similar instrument for me, and a short time since I obtained another from this celebrated mechanician of Paris. This last is of the same diameter, but nearly twice the length of the former coil; and there is a commutator attached to the primary coil by which the direction of the current can be instantly reversed. Three cells of Grove's nitric acid battery are sufficient to excite the action.

With this apparatus the transverse non-luminous or dark bands first pointed out by Mr. Grove in the above paper, and more particularly explained by him in a note dated June 9, 1852, published in the Philosophical Magazine of that year, are obtained in a very distinct manner. It is impossible to describe the beauty of this experiment; it must be seen to be appreciated; but those experiments I am about to mention will explain the power of the instrument and the nature of the investigations to which it is applicable. It opens out a variety of new ideas as to the extraordinary action or nature of induced currents, and as such may probably not be uninteresting to the readers of the Philosophical Magazine.

I may explain that the end or terminals of the secondary coil, as described by Mr. Grove in his paper, are supported by two small glass pillars; to these can be attached by binding-screws, wires of any required length to form the connexion with the apparatus necessary to be used in the following experiments:—

1. If the ends of the connecting wires are brought within 0·5 of an inch of each other, a discharge takes place through the air.

2. Through the flame of a spirit-lamp the discharge passes at a separation of 1 to 2 inches; and if the inner and outer coatings of a moderate-sized Leyden jar are attached to the terminals of the coil by other wires, this discharge is much heightened in its intensity, a long, clear and brilliant white electrical stream passing through the flame with a sharp rattling noise.

3. If the terminals are of platinum wire, and the ends are approximated to within 0·1 of an inch, a rapid continued discharge takes place, and in a short time the negative wire becomes red-hot; the current being reversed, this wire instantly cools, and the heat appears in the other, which has now become the negative.

This heating effect of the secondary current has already been noticed by Masson, but the heat appears in the contrary direction to that which is produced in the primary. It is well known that the disruptive discharge which takes place in a secondary coil only appears in the form of a spark in breaking contact with the primary; but if the ends of the terminal wires of a secondary coil rest on a piece of bibulous paper saturated with a solution of iodide of potassium, the iodine will be evolved at one terminal on making, but considerably more at the other on breaking contact. In describing the terminals as positive or negative, it must therefore be understood that the positive is that at which the iodine is evolved at breaking, and remains cool in the discharge throughout while the negative attains a red heat.

I have been thus particular in the description, because the heating of the positive electrode in the voltaic battery is in itself a curious phænomenon long since observed (Phil. Mag. 1838, p. 436), and the cause of which has not as yet been satisfactorily explained.

4. If the discharge is taken between two metallic or two charcoal balls in the vacuum of a good air-pump, the effects are very marked; the lower half of the negative ball is surrounded by a bright, blue glow, while from the positive proceeds a clear, bright, red stream of light. As the vacuum becomes more perfect this increases in brilliancy, until the dark space is left between the red flame and blue glow, as in Mr. Grove's original experiment, while the negative wire is surrounded with the glow, the intensity of each light increasing in proportion to the vacuum. Where the negative wire is sealed in a glass tube having only its section exposed, the dark space is no longer perceptible, as also noticed by Mr. Grove.

5. The wires were attached to my double plate gold-leaf electroscope, the plates of which are 8 inches in diameter; these were separated about 0·4 of an inch, the leaves of the electroscope attached to the outside terminal of the coil diverged at an angle of 45°, and the discharge took place with a loud snap, the air between the plates being charged and discharged as a Leyden jar.

6. I coated about two-thirds of the inside of a Berlin glass beaker of 4 inches depth by 2 inches width with tin-foil, leaving about 1·5 of an inch of the upper portion uncoated. On the plate of the air-pump I placed a glass plate, and on it the glass beaker, covering the whole with an open-mouth glass receiver, on which was placed a brass plate having a thick wire passing through collars of leather; the portion of this wire within the receiver was enclosed in an open glass tube. One end of the secondary coil was attached to the wire and the other to the plate of the air-pump. As the vacuum improves, the effect is truly surprising; at first a faint, clear, blue light appears to proceed from the lower part of the beaker to the plate; this gradually becomes brighter, until by slow degrees it rises, increasing in brilliancy until it arrives at that part which is opposite or on a line with the inner coating; the whole being intensely illuminated, a discharge then commences from the inside of the beaker to the plate of the pump in minute but diffused streams of blue light; continuing the exhaustion, at last a discharge takes place in the form of an undivided continuous stream overlapping the vessel, as if the electric fluid was itself a material body running over. When first witnessed, it appears at the moment impossible to divest the mind of such a conclusion.

7. If the position of the beakers is reversed by placing the open part on the plate of the air-pump, and the upper wire is either in contact or within an inch of the outside of the vessel, streams of blue lambent flame appear to pour down the sides to the plate, while a continuous discharge takes place from the inside coating.

8. A thin piece of talc or very thin glass coated on one side with tin-foil, and the other having a figure as a star, cross letters, &c., also of tin-foil, produces a very beautiful experiment. The larger coating is attached to one end of the coil; the wire of the other is then by means of a glass rod brought into contact with the figure of the star, cross, &c., which is immediately illuminated in the most brilliant manner, and the well-known odour of ozone, as from the electrical machine, is perceptible to an unusual degree.

9. When the air in a glass tube 4 feet 2 inches long and 2 inches in diameter, similar to those used for the aurora experiments, is exhausted, the discharge takes place, illuminating the entire tube.

I will not occupy your valuable space with any further detail of experiments made by myself with this really beautiful instrument, the construction of which reflects so much credit on the ingenuity and talent of M. Ruhmkorff.

I am, Gentlemen,
Yours obediently,
JOHN P. GASSIOT.

Clapham Common,
January 11, 1854.

XIV. *On the Action of Alkalies on Rocks.* *By* M. Delesse*.

IN studying the action of alkalies on rocks, I treated the powder of the specimen under examination in a solution containing five times its weight of potash; I then sought what substances existed in the new solution.

Since many rocks contain water, I dried and weighed the residue from the potash solution, I then heated it to redness and weighed again: I thus obtained for the different rocks comparable results, as shown in the following table:—

Description of the rock.	1	Trachyte, brownish-red, with gray globules and black mica, from Hungary.	Trachyte molaire, with a little vitreous orthose and some mica, from Hungary.	Retinite, blackish-brown, from Planitz.	Retinite, globular, deep red, from Meissen.	Retinite, black with resinous lustre, from Sardinia.	Perlite, grayish-white, nacreous, from the 'Cap de Gatis.'	Obsidian, dark black, from Lipari.	Eurite, argillaceous brick-red, from Saxony.	Palagonite tufa, with a brown-yellowish paste, from Iceland.	Melaphyre, with dark green paste and greenish-white labradorite, from Belfahy.	Basalt, black paste and crystal of augite, from Bohemia.	Lava, black, of the flow of Oct. 1819, from Vesuvius.	Porphyry, quartziferous, reddish-gray cellular paste, and much hyaline quartz, from Saxony.
Silica dissolved by the potash	2	36·00	17·06	19·40	12·23	9·50	19 55	18·39	11·45	7·05	8·50	7·68	4·50	5·35
Alumina dissolved by the potash	3	traces	2·39	3·75	1·16	1·25	1·85	3·78	1·55	2·10	2·20	2·85	2·60	traces
Total loss of rock after calcination of the residue......	4	37·85	27·27	30·15	17·89	16·55	26·85	24·44	17·20	18·60	18·41	15·35	8·50	5·80

Some general conclusions can be drawn from my researches.

When an alkali attacks a rock, it dissolves from it not only silica, but also alumina, water, potash and soda. Besides a little lime, magnesia and traces of oxide of iron are taken up by the alkaline liquor.

The quantity of silica dissolved is always greater than that of all the other substances.

Granite may be said to be unaffected by boiling in a solution of alkali; quartziferous porphyry only loses a few hundredths.

Lava, basalt and melaphyre lose not more than 20 per 100.

Trachyte, retinite, perlite and obsidian are the most attackable; within my experience their loss did not exceed 40 per 100.

A rock containing water is much less easily affected by the alkalies after it has been calcined. Thus, in the perlite from the Cape of Gatis, the action of the alkali before and after calcining varied in the ratio of $2\frac{1}{2}$ to 1.

On the other hand, a rock is much more easily affected when in a state of decomposition. Thus, argillaceous eurites and

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* Communicated by Sir Robert Kane.

Oils.	Phosphoric acid, syrupy.	Sulphuric acid × nitric acid.	Aqua regia.	+ Caustic soda, spec. grav. 1·340.
Olive ····	slight green.	orange-yellow.	0	fluid white mass.
Gallipoli	ditto.	dark brown.	0	fibrous yellowish-white mass.
India nu	0	orange-white.	0	fibrous white mass.
Pale rap	0	dark brown.	0	fibrous yellowish-white mass.
Poppy	0	slight yellow.	0	fluid intense rose-coloured mass.
French	brown yellow.	dark brown.	yellow.	fibrous orange mass.
Sesame	0	green becoming intense red.	ditto.	fluid orange mass with brown liquor beneath.
Castor	0	brownish-red.	0	fibrous pale rose-coloured mass.
Hempse	green.	green becoming black.	green.	fibrous light brown mass.
Linseed	brown yellow green.	ditto.	greenish-yellow.	fluid orange mass.
Lard ····	0	brown.	0	fluid pink mass.
Neat'sfo	0	dark brown.	slight yellow.	fibrous brownish-yellow mass.
Sperm	dark red.	dark brown.	ditto.	fluid orange yellow mass.
Seal ····	ditto.	ditto.	ditto.	ditto.
Cod liver	ditto.	ditto.	yellow.	ditto.

kaolins, which are merely decomposed granitic rocks, undergo in the alkali losses much greater than those of granitic rocks.

Other conditions being the same, the action of alkalies on rocks increases with the amount of silica, with the absence of crystalline structure and of hyaline quartz. On this account, vitreous rocks which contain little or no quartz, as retinite, obsidian, trachyte, are very strongly affected by alkalies.

When instead of alkalies we use alkaline carbonates, some rocks, especially the vitreous, are still acted upon, but much less energetically than by the caustic alkalies.

The readiness with which alkalies, and even alkaline carbonates, act upon rocks, shows how cautiously they should be used for the separation of the free and directly soluble silica that may be mechanically mixed in a rock, as particularly in the kaolins and the finer clays.

I may remark, that, for instance, in obsidian the silica dissolved is not free, but in the state of soluble silicate; moreover, in retinite the silica is not in the state of opal, as is supposed, but in that of soluble hydrosilicate. Generally in all vitreous or porphyritic rocks, hydrous or anhydrous, the silica is held in a combination, not definite, forming the paste of these rocks, and which is attacked by the alkali.

The water penetrating rocks by infiltration always contains, even near the surface of the earth, small quantities of alkaline salts; it is easy, then, to conceive that these salts should aid in decomposing rocks and in producing pseudomorphisms. But at a greater depth water becomes largely impregnated with alkaline salts, the temperature and pressure increasing rapidly; it then influences greatly the rocks with which it is in contact. This is the case with the water of mineral springs, of the geysers, of mud volcanos, and in general of volcanic regions. Consequently the action of the alkalies and of the alkaline salts plays an important part, not only in the formation of pseudomorphs, but also in the chemical reactions which take place in the interior of our planet.

XV. *On the Adulteration of Oils.*

By F. Crace Calvert, *Esq.*[*]

In consequence of the large quantities of oils used at the present day for machinery, woollen, &c., many varieties are introduced into the market, and much temptation exists to mix or adulterate the most expensive ones. Having been at various times called upon to examine samples of oil, I ascertained that the processes known to discover adulteration were too general in

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[*] Communicated by the Author.

their application to enable me to obtain satisfactory results. To this class belong the delicate process recommended by M. F. Boudet, principally for the detection of drying oils in olive by the action of hyponitric acid; and M. Rousseau's diagometer, which is based on the very inferior conducting power of olive oil as compared with that of the others.

For distinguishing one class of oils from another we have M. Faure's method, which consists in the brown or black tinge which fish oils exclusively assume when a stream of chlorine gas is passed through them; and M. Maumené's, by which the drying oils may be distinguished from the non-drying ones, owing to the fact that the latter, when mixed with strong sulphuric acid, give rise to a much higher temperature; but although M. Fehling has endeavoured lately to give more precision to M. Maumené's researches, it is far from being satisfactory.

There are other processes the characters of which are not sufficiently distinct to be employed with any degree of certainty; such is M. Faure's, which consists in adding a given quantity of caustic ammonia to oils, and noticing after they have been mixed the peculiar appearance which the white or yellow thick fluids present. The same may be said of the process proposed by M. Heidenreich with monohydrated sulphuric acid, or that of M. Diesel with concentrated nitric acid; for the chemical actions are so violent, that the characteristic colorations, which are at first produced, rapidly disappear in consequence of the destruction of the oils.

These facts induced me to examine what would be the action, on oils, of the above acids when diluted, and the satisfactory results obtained are described in this paper.

The marked colorations produced may be considered as derived from two distinct chemical actions:—first. They appear due to certain foreign matters which are dissolved in the oils, and which existed in the substance from which they were extracted; secondly: The diluted acids have probably an action on the component parts of the oils themselves; for if caustic soda be added to oils so acted upon, a different result is obtained than when no acid has been previously applied; this fact being clearly illustrated with French nut oil, as it gives a semi-saponified fluid mass when caustic soda of spec. grav. 1·340 is alone mixed with it, and a fibrous mass when treated by dilute nitric acid previous to the addition of the alkali.

It may be interesting here to remark, that fish oils have presented distinct reactions from other animal or vegetable oils; consequently, in my opinion, not only has cod-liver oil a different composition to that of other oils, as shown by the researches of M. Winckler, but also probably sperm and seal oils.

The most difficult part of my researches has been to procure oils the purity of which I could depend on, and to arrive at this object I was obliged in many cases to obtain samples from their sources of production on the Continent; and even then I took the precaution of ascertaining their degree of purity by applying to them the various tests which I shall describe further on.

The reason why I employed so many reagents is, that the adulterations which commercial interests may have or dictate, are numerous; and that the reactions presented by organic substances, and especially oils, are exceedingly delicate. I would strongly recommend that samples of pure oil be tested comparatively with those suspected of being adulterated, and never to apply one only of the proposed tests, but all those giving characteristic reactions with a given oil.

I have great pleasure in acknowledging the intelligence, industry, and chemical knowledge exhibited in these tedious researches by my assistant, Mr. Charles Lowe.

As the reactions presented by the various oils depend upon the special strengths and purity of the reagents, not only great care should be taken in their preparation, but also the exact mode and time required for the chemical action to become apparent should be attended to; these I have taken care to give with each reagent.

Caustic Soda Solution of spec. grav. 1·340.—The reactions given in the following table are obtained by adding one vol. of the above test-liquor to five vols. of oil, well mixing them, and then heating the mixture to its point of ebullition :—

Dark colorations.				Light colorations.			
Fish oils.		Vegetable oils.		Animal oils.		Vegetable oils.	
Sperm..	red.	Hempseed	thick, yellowish-brown.	Neat'sfoot	dirty yellowish-white.	Pale rapeseed	dirty yellowish-white.
Seal ..		Linseed..	yellow fluid.	Lard....	pinkish-white.	Poppy	
Cod liver						French nut ..	
						Sesame......	
						Castor	white.
						India nut (thick)	
						Gallipoli	yellow.
						Olive........	

Caustic soda of spec. grav. 1·340 is principally useful to distinguish fish from other animal and vegetable oils, owing to the distinct red colour which the former assume, and which coloration is so distinct, that 1 per cent. of fish oil can be detected in any of the others. This table should also be consulted when the question is, not to discover other adulterations, but to distinguish some of the oils: for instance, hempseed acquires a brown yellow colour, and becomes so thick that the vessel which contains it may be inverted without losing any of its contents;

whilst linseed assumes a much brighter yellow colour, and remains fluid. India nut oil is characterized by giving a white mass, becoming solid in five minutes after the addition of the alkali, which is also the case with gallipoli and pale rape, to the exclusion of the other oils which remain fluid.

Although it is probable that the reason why some of the oils, on the application of this reagent, acquire a mucilaginous appearance whilst others become stringy or fibrous, is due to the greater or less facility with which they are saponified, still I regret that I have not had the time to examine this point carefully.

Action of diluted Sulphuric Acid on Oils.

As different strengths of this acid have distinct reactions on the oils I had at my disposal, and as they may be employed to discover some known commercial adulterations, I shall discuss separately each series of reactions.

Sulphuric Acid of spec. grav. 1·475.—The mode of applying this acid consists in agitating one vol. with five vols. of oil until complete admixture, and then allowing the whole to stand for fifteen minutes, when the appearance is taken as the test reaction.

Not coloured.		Coloured.					
Animal.	Vegetable.	Fish.		Animal.		Vegetable.	
Lard.. dirty.	India nut.	Sperm....	light red.	Neat's-foot	yellow tinge.	Olive ..	green tinge.
	Pale rapeseed.	Seal......				Gallipoli ..	
	Poppy.	Cod liver..	purple.			Sesame ..	
	Castor.					Linseed ..	green.
						Hempseed	intense green.
						French nut..	brownish.

The most striking reactions in this table are those presented by hempseed and linseed; for the green coloration which they acquire is such, that if they were used to adulterate any of the other oils to the amount of 10 per cent., their presence would be indicated by the distinct green tinge they would communicate to the others. The red colour assumed by the fish oils with this test are also sufficiently marked to enable us to detect them in the proportion of one part in one hundred of any other oil; and it is at the point of contact of the oil with the acid, on their being allowed to separate by standing, that the colour is principally to be noticed.

Sulphuric Acid of spec. grav. 1·530.—Having obtained by the application of the preceding acid a certain number of characteristic reactions, I was induced to try the influence of a stronger one, and I therefore agitated one vol. of it with five vols. of oil, and allowed the mixture to stand five minutes.

Light colorations.				Dark colorations.			
Animal.		Vegetable.		Fish.		Vegetable.	
Lard	dirty white.	Olive	greenish-white.	Sperm Seal	red.	Gallipoli French nut	gray.
Neats'foot	brownish dirty white.	Sesame	greenish dirty white.	Cod liver	purple.	Hempseed	intense green.
		India nut Poppy Castor	dirty white.			Linseed	dirty green.
		Pale rapeseed	pink.				

As hempseed, linseed, fish, gallipoli, and French nut are the only oils that assume with the above reagent decided colorations, they can be discovered in any of the others.

Sulphuric Acid of spec. grav. 1·635.—This acid was used in a similar manner to those above, and the coloration noted after two minutes.

Not coloured.	Distinctly coloured.					
Vegetable.	Fish.		Animal.		Vegetable.	
Poppy. Sesame. Castor.	Sperm Seal Cod liver	intense brown.	Neat'sfoot	brown.	Olive (light) Hempseed (intense) Linseed	green.
			Lard	light brown.	Gallipoli Pale rapeseed French nut India nut (light)	brown.

I wish to draw especial attention to this acid, as it gives distinct and widely differing reactions from those of the former acids. The colorations produced by sulphuric acid, spec. grav. 1·635, are so marked that they may be consulted with great advantage in many cases of adulteration; for example, I have been able to detect, distinctly, 10 per cent. of rapeseed in olive oil, of lard in poppy, of French nut in olive, of fish oil in neat's-foot.

I was much struck by the gradual increase of coloration assumed by some of the oils when treated by sulphuric acid of different strengths. Thus I found that gallipoli, which was white with No. 1 sulphuric acid, becomes brown with No. 3; pale rape, which was white with No. 1 acid, gives a pink colour with No. 2, and a brown with No. 3; whilst neat'sfoot is of a light yellow with No. 1, but becomes brown with No. 3. These results therefore clearly show the decomposing action of sulphuric acid on oils, and that an acid of spec. grav. 1·635 is the maximum strength that can be used, for nearly all the oils beginning to carbonize, their distinct colorations are destroyed.

Action of Nitric Acid of different strengths on Oils.

Owing to the reasons given in the first part of this memoir, I employed diluted acid and obtained a series of reactions, some of which will, I hope, prove useful in some special cases of adulteration, and interesting as showing the influence of gradual oxidation on oils.

Nitric Acid of spec. grav. 1·180.—One vol. of this acid was agitated with five vols. of oil, and the appearance, after standing five minutes, is described in the following table:—

Not coloured.			Coloured,					
Fish.	Animal.	Vegetable.	Fish.		Animal.		Vegetable.	
Cod liver.	Lard.	India nut.	Sperm	slight yellow.	Neat's-foot	light yellow.	Olive......	greenish.
		Pale rapeseed.	Seal....	pink.			Gallipoli ..	
		Poppy.					Hempseed..	dirty green.
		Castor.					French nut	yellow.
							Sesame (orange)	
							Linseed ..	

This test is sufficiently delicate to detect, distinctly, 10 per cent. of hempseed in linseed, as the latter assumes the greenish hue so characteristic of the former. Although olive acquires also a green colour, still its shade is such that it is easily distinguished from that of hempseed.

Nitric Acid of spec. grav. 1·220.—I employed this stronger acid with the view of increasing the colorations of certain oils, so as to render them sufficiently marked to ascertain the presence of these oils when mixed with others. The proportions used and the time of contact were the same as above.

Not coloured.			Coloured.					
Fish.	Animal.	Vegetable.	Fish.		Animal.		Vegetable.	
Cod liver.	Lard.	India nut.	Sperm	light yellow.	Neat's-foot	light yellow.	Poppy (yellow)	red.
		Pale rapeseed.	Seal..	light red.			French nut ..	
		Castor.					Sesame......	
							Olive ..	greenish.
							Gallipoli	
							Hempseed	greenish, dirty brown.
							Linseed....	yellow.

The chief characters in the above table are those presented by hempseed, sesame, French nut, poppy, and seal oil; and they are such that not only may they be employed to distinguish them from each other, but are sufficiently delicate to detect their presence when mixed with other oils in the proportion of 10 per cent.

Nitric Acid of spec. grav. 1·330.—One vol. of this acid was mixed with five vols. of oil, and remained in contact five minutes.

Not coloured.	Coloured.					
Vegetable.	Fish.		Animal.		Vegetable.	
India nut.	Sperm ..	red.	Lard....	very slight yellow.	Poppy..........	red.
Pale rapeseed.	Seal....		Neats'foot..	light brown.	French nut (dark)	
Castor.	Cod liver				Sesame (dark) ..	
					Olive	greenish.
					Gallipoli	
					Hempseed	greenish dirty brown.
					Linseed	green becoming brown.

The colorations here described are very marked, and can be employed with advantage to discover several well-known cases of adulteration; for instance, if 10 per cent. of sesame, or French nut, exists in olive; as for poppy with the same oil, the tinge produced not being so intense as the preceding ones, so small an adulteration cannot be detected with certainty; and admitting any doubt remained in the mind of the operator as to whether the adulterating oil was sesame, French nut, or poppy, he would be able to decide by applying the test described in the next table, where he will find that French nut oil gives a fibrous semi-saponified mass; sesame a fluid one, with a red liquor beneath; and poppy also a fluid mass, but floating in a colourless liquor.

The successive applications of nitric acid of spec. grav. 1·330, and of caustic soda of spec. grav. 1·340, can also be successfully applied to detect the following very frequent cases of adulteration:—

1st. That of gallipoli with fish oils, as gallipoli assumes no distinct colour with the acid, and gives with soda a mass of fibrous consistency, whilst fish oils are coloured red and become mucilaginous with the alkali.

2nd. That of castor with poppy oil, as the former acquires a reddish tinge, and the mass with the alkali loses much of its fibrous appearance.

3rd. That of rapeseed with French nut, as nitric acid imparts to the former a more or less red tinge, which on addition of the alkali not only increases, but also renders more fibrous the semi-saponified mass.

The colorations which divers oils assume under the influence of the three above nitric acids clearly illustrate the remarks made at the commencement of this paper, that the reason why the chemists who preceded me in these tedious researches had not arrived at satisfactory results in distinguishing oils and their various adulterations, was owing to the acids they employed being so concentrated that all the distinctive colorations were lost, the oils becoming yellow or orange. But there is no doubt that the above reagents will enhance the value of M. F. Boudet's process, as they afford very useful data to specify the special oils mixed with olive.

Caustic Soda of spec. grav. 1·340.—The following reactions were obtained on adding ten vols. of this test-liquor to the five vols. of oil which had just been acted upon by one vol. of nitric acid:—

If a fibrous mass is formed.				If a fluid mass is formed.			
Animal.		Vegetable.		Fish.	Animal.	Vegetable.	
Neat's-foot	white.	Gallipoli	white.	Sperm.	Lard.	Olive	white.
		India nut		Seal.		Pale rapeseed	
		Castor		Cod liver.		Linseed	yellowish.
		French nut	red.			Poppy (light)	red.
		Hempseed	light brown.			Sesame brown liquor beneath	

Having given in a previous paragraph some of the most useful reactions contained in this table, I shall simply call attention to the following mixtures:—neat'sfoot with rape, gallipoli with poppy, castor with poppy, hempseed with linseed, sperm with French nut, and gallipoli with French nut. It is also necessary here to mention, that the brown liquor on which the semi-saponified mass of sesame swims is a very delicate and characteristic reaction.

Phosphoric Acid.—One vol. of syrupy trihydrated phosphoric acid was agitated with five vols. of oil and gave the following results:—

Not coloured.		Coloured.			
Animal.	Vegetable.	Fish.		Vegetable.	
Lard.	India nut.	Sperm	dark red.	Olive (slight)	green.
Neat'sfoot.	Pale rapeseed.	Seal		Gallipoli (slight)	
	Poppy.	Cod liver		Hempseed	
	Sesame.			Linseed (brown yellow)	
	Castor.			French nut	brown yellow.

The only reaction to be noticed is the dark red colour rapidly becoming black, which phosphoric acid imparts exclusively to the fish oils, as it enables us to detect one part of these oils in 1000 parts of any other animal or vegetable oil; and even at this great degree of dilution, a distinct coloration is communicated to the mixture.

Mixture of Sulphuric and Nitric Acids.

The results given in the following table are obtained on agitating one vol. of a mixture of equal volumes of sulphuric acid of spec. grav. 1·845, and nitric acid of spec. grav. 1·330, with five vols. of oil and allowing the whole to stand two minutes:—

If coloured.					
Fish.		Animal.		Vegetable.	
Sperm	dark brown.	Lard	brown.	Gallipoli	dark brown.
Seal		Neat'sfoot (dark)		Pale rapeseed	
Cod liver				French nut	
				Sesame (becoming intense red)	green.
				Hempseed (becoming black)	
				Linseed (becoming black)	
				Olive (orange) slight	yellow.
				Poppy (slight)	
				Indian nut (orange) slight	white.
				Castor	brownish-red.

As three oils remain nearly colourless, viz. those of poppy, olive, and India nut, we are enabled to detect in them the presence of any of the others; and when olive or poppy are adulterated with sesame, the green colour at first produced is much more persistent than with sesame, consequently it is necessary that the acid and the oil suspected of containing it should remain in contact for about ten minutes in order to obtain the ultimate brownish-red colour of the sesame; in fact it is so intense, that it may be usefully employed to detect this oil when mixed with others.

Aqua Regia.—In consequence of the results obtained with nitric acid I was induced to try the action of aqua regia; but I found that when it was composed in the ordinary way of three vols. of hydrochloric and one of nitric acid, the reactions produced nearly coincided with the last-named acid; I therefore prepared several aquæ regiæ, in which I gradually increased the proportion of hydrochloric acid, and after having tested them, I adopted one composed of twenty-five volumes of hydrochloric acid of spec. grav. 1·155, and one volume of nitric acid of spec. grav. 1·330, and allowed them to stand about five hours. The reactions described in the following table are those which took place when a mixture of five vols. of oil with one vol. of aqua regia was agitated and allowed to stand five minutes:—

Not coloured.		Coloured.					
Animal.	Vegetable.	Fish.		Animal.		Vegetable.	
Lard.	Olive.	Sperm (slight)	yellow.	Neat's-foot	slight yellow.	French nut	yellow.
	Gallipoli.	Seal (slight)..				Sesame	
	India nut.	Cod liver				Linseed (greenish)	
	Rapeseed.					Hempseed	green.
	Poppy.						
	Castor.						

When the facts contained in this table are compared with the preceding ones, we are struck with their uniformity, and are led to infer that no marked action had taken place; but this conclusion is erroneous, as most of them assume a vivid and distinct coloration on the addition of an alkali of spec. grav. 1·340, as seen in the following table:—

If a fibrous mass is formed.				If a fluid mass is formed.					
Animal.		Vegetable.		Fish.		Animal.		Vegetable.	
Neat's-foot	brownish-yellow.	Gallipoli (yellowish)	white.	Sperm ..	orange-yellow.	Lard..	pink.	Olive..	white.
		India nut		Seal				Poppy..	intense rose.
		Pale rapeseed (yellowish)..		Cod liver				Sesame	orange with brown liquor beneath.
		Castor	pale rose.					Linseed	orange.
		French nut ..	orange.						
		Hempseed ..	light brown.						

The characters presented in this table are such that we can discover with facility 10 per cent. of a given oil in many cases of adulteration; for example, poppy in rape, olive, gallipoli, and India nut, as all of them assume a pale rose-colour; but when poppy is mixed with olive or castor oils, there is a decrease in the consistency of the semi-saponified mass.

By the aid of this reagent we can also ascertain the presence of 10 per cent. of French nut in olive or linseed, as the semi-saponified mass becomes more fluid; and as to French nut in pale rape, gallipoli and India nut, it is recognized in consequence of their white mass acquiring an orange hue. With respect to linseed in hempseed, it is detected, as it renders the fibrous mass of the latter more mucilaginous. Sesame also gives with this reagent the same characters as those which it afforded with nitric acid and an alkali; and poppy is distinguished from all other oils by giving in this case a semi-saponified mass of a beautiful rose colour.

To give an idea how the tables are to be used, I shall suppose a sample of rapeseed oil adulterated with one very difficult to discover. I first apply the caustic alkali test, which in giving a white mass proves the absence of the fish oils together with those of hempseed and linseed; and as no distinct reactions are produced by the oils under examination when mixed with the three sulphuric and nitric acids above mentioned, poppy and sesame are thrown out, as they are reddened; neat'sfoot, India nut, castor, olive, and lard resting only in the scale of probability. In order to discover which of these is mixed with the suspected oil, I agitate a portion of it first with nitric acid of spec. grav. 1·330, and then with caustic soda; and their mutual actions exclude neat'sfoot, India nut and castor, as the sample does not give a fluid semi-saponified mass. The absence of olive is proved by no green coloration being obtained on the application of syrupy phosphoric acid. As to the presence of lard oil in the rape, it is ascertained on caustic soda being added to the oil which has been previously acted on by aqua regia, as the latter gives a fibrous yellowish semi-saponified mass, whilst the former yields a pink fluid one.

In conclusion, I trust that the reagents described in this paper, and the new method of applying successively two of them to any particular oil, will prove useful, not only to detect the numerous admixtures of oils we have noticed, but also to trace and determine in a given oil the presence of any of the others which we have examined; and I give a general table of the preceding reactions in order to facilitate the research for any adulteration.

Royal Institution, Manchester,
January 11, 1854.

XVI. *On the Mechanical Action of Heat.* By WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.SS. Lond. and Edinb. &c.*

[Continued from p. 21.]

SECTION II.—*Of Real and Apparent Specific Heat, especially in the State of Perfect Gas.*

(9.) THE apparent specific heat of a given substance is found by adding to the real specific heat (or the heat which retains its form in producing an elevation of one degree of temperature in unity of weight) that additional heat which disappears in producing changes of volume and of molecular arrangement, and which is represented by Q′ in equation 6 of Section I., and taking its *total* differential coefficient with respect to the temperature. Hence, denoting total apparent specific heat by K,

$$K=\frac{dQ}{d\tau}+\frac{d.Q'}{d\tau}=\frac{dQ}{d\tau}+\frac{dQ'}{d\tau}+\frac{dQ'}{dV}\cdot\frac{dV}{d\tau}$$

$$=\frac{1}{CnM}\left\{\frac{3kM}{2\mu}+(\tau-\kappa)\left(\frac{dV}{d\tau}\left(\frac{1}{V}-\frac{dU}{dV}\right)-\frac{dU}{d\tau}\right)\right\}. \qquad (13)$$

Another mode of expressing this coefficient is the following:—

$$\left.\begin{array}{l}\text{Denote the ratio}\\ \qquad\qquad \dfrac{2\mu}{3kM}\text{ by N,}\\ \text{and the real specific heat by}\\ \qquad\qquad \mathfrak{k}=\dfrac{1}{CnMN}.\end{array}\right\} \quad . \ . \ . \ . \ . \ . \quad (14)$$

Then

$$K=\mathfrak{k}\left\{1+N(\tau-\kappa)\left(\frac{dV}{d\tau}\left(\frac{1}{V}-\frac{dU}{dV}\right)-\frac{dU}{d\tau}\right)\right\}. \quad . \quad (15)$$

The value of $\frac{dV}{d\tau}$ is to be determined from the conditions of each particular case; so that each substance may have a variety of apparent specific heats, according to the manner in which the volume varies with the temperature.

If the volume is not permitted to vary, so that $\frac{dV}{d\tau}=0$, there is obtained the following result, being the apparent specific heat at constant volume:—

$$K_V=\frac{1}{CnM}\left(\frac{1}{N}-(\tau-\kappa)\frac{dU}{d\tau}\right)$$

$$=\mathfrak{k}\left(1-N(\tau-\kappa)\frac{dU}{d\tau}\right). \quad . \ . \ . \ . \ . \ . \quad (16)$$

(10.) When the substance under consideration is a perfect gas, it has already been stated (equation 7) that

$$\frac{dU}{d\tau}=-\frac{\kappa}{\tau^2},\quad \frac{dU}{dV}=0;$$

and because the volume of unity of weight is directly as the absolute temperature and inversely as the pressure,

$$\frac{1}{V}\frac{dV}{d\tau}=\frac{1}{\tau}-\frac{1}{P}\frac{dP}{d\tau}. \quad . \quad . \quad . \quad . \quad . \qquad (17)$$

Hence the following are the values of the apparent specific heats of unity of weight of a theoretically perfect gas under different circumstances.

$$\left.\begin{array}{l}
\text{General value of the total apparent specific heat:—}\\
K=\frac{1}{CnM}\left\{\frac{1}{N}+(\tau-\kappa)\left(\frac{\kappa}{\tau^2}+\frac{dV}{Vd\tau}\right)\right\}\\
\quad=\frac{1}{CnM}\left\{\frac{1}{N}+(\tau-\kappa)\left(\frac{\kappa}{\tau^2}+\frac{1}{\tau}-\frac{dP}{Pd\tau}\right)\right\}.\\
\text{Apparent specific heat at constant volume:—}\\
K_V=\frac{1}{CnM}\left\{\frac{1}{N}+\frac{\kappa}{\tau}-\frac{\kappa^2}{\tau^2}\right\}\\
\quad=k\left(1+N\left(\frac{\kappa}{\tau}-\frac{\kappa^2}{\tau^2}\right)\right).\\
\text{Apparent specific heat under constant pressure:—}\\
K_P=\frac{1}{CnM}\left(\frac{1}{N}+1-\frac{\kappa^2}{\tau^2}\right)\\
\quad=k\left\{1+N\left(1-\frac{\kappa^2}{\tau^2}\right)\right\}.
\end{array}\right\} \quad . \qquad (18)$$

The ratio of the apparent specific heat under constant pressure to the apparent specific heat at constant volume is the following:—

$$\frac{K_P}{K_V}=\frac{1+N\left(1-\frac{\kappa^2}{\tau^2}\right)}{1+N\left(\frac{\kappa}{\tau}-\frac{\kappa^2}{\tau^2}\right)}=1+N\frac{1-\frac{\kappa}{\tau}}{1+N\left(\frac{\kappa}{\tau}-\frac{\kappa^2}{\tau^2}\right)}. \quad . \qquad (19)$$

The value of κ is unknown; and, as yet, no experimental data exist from which it can be determined. I have found, however, that practically, results of sufficient accuracy are obtained by regarding κ as so small in comparison with τ, that $\frac{\kappa}{\tau}$, and *à fortiori* $\frac{\kappa^2}{\tau^2}$, may be neglected in calculation*.

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* The mean value of κ, as computed from experiments made by Mr. Joule and Professor William Thomson in 1852, is about 2°·1 Centigrade; but

Thus are obtained the following *approximate* results, for perfect gases, and gases which may without material error be treated as perfect.

$$\left.\begin{array}{l}
\text{General value of the total apparent specific heat:—} \\
\mathrm{K}=\dfrac{1}{\mathrm{C}n\mathrm{M}}\left(\dfrac{1}{\mathrm{N}}+\dfrac{\tau}{\mathrm{V}}\dfrac{d\mathrm{V}}{d\tau}\right)=\mathfrak{k}+\mathrm{P}\dfrac{d\mathrm{V}}{d\tau} \\
\quad=\dfrac{1}{\mathrm{C}n\mathrm{M}}\left(\dfrac{1}{\mathrm{N}}+1-\dfrac{\tau}{\mathrm{P}}\dfrac{d\mathrm{P}}{d\tau}\right). \\
\text{Apparent specific heat at constant volume:—} \\
\mathrm{K_V}=\dfrac{1}{\mathrm{C}n\mathrm{MN}}=\mathfrak{k}, \\
\text{being equal to the real specific heat.} \\
\text{Apparent specific heat under constant pressure:—} \\
\mathrm{K_P}=\dfrac{1}{\mathrm{C}n\mathrm{M}}\left(\dfrac{1}{\mathrm{N}}+1\right)=\mathfrak{k}(1+\mathrm{N}).
\end{array}\right\} \quad . \quad (20)$$

Ratio of those two specific heats:—

$$\frac{\mathrm{K_P}}{\mathrm{K_V}}=1+\mathrm{N}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

This ratio is the quantity called by Poisson γ, in his researches on the propagation of sound.

(11.) It is unnecessary to do more than to refer to the researches of Poisson, and to those of Laplace, for the proof that the effect of the production of heat by the compression of air is the same as if the elasticity varied in proportion to that power of the density whose index is the ratio of the two specific heats; so that the actual velocity of sound is greater than that which it would have if there were no such development of heat, in the proportion of the square root of that ratio*.

The following is the value of the velocity of sound in a gas, as given by Poisson in the second volume of his *Traité de Mécanique*,

$$a=\sqrt{g\,.\,\gamma\,.\,(1+\mathrm{ET})\frac{mh}{\Delta}}, \quad . \quad . \quad . \quad (22)$$

where a denotes the velocity of sound, g the velocity generated by gravity in unity of time, E the coefficient of increase of elasticity with temperature, at the freezing-point of water, T the

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this result is subject to some uncertainty. The details of the calculation are given in the sixth section of this paper, published in the Transactions of the Royal Society of Edinburgh, vol. xx. part 4.

* This theorem is proved for all substances whatsoever in a paper on the Centrifugal Theory of Elasticity, published in the Transactions of the Royal Society of Edinburgh, vol. xx. part 3.

temperature measured from that point, m the specific gravity of mercury, Δ that of the gas at the temperature of melting ice, and pressure corresponding to a column of mercury of the height h. It follows that the ratio γ is given by the formula

$$\gamma = 1 + \mathrm{N} \text{ nearly} = \frac{a^2 \Delta}{gmh(1+\mathrm{ET})}. \quad . \quad . \quad (23)$$

Calculations have been made to determine the ratio γ from the velocity of sound; but as many of them involve erroneous values of the coefficient of elasticity E, the experiments have to be reduced anew.

The following calculation is founded on an experiment quoted by Poisson on the velocity of sound in atmospheric air, the values of E, m, and Δ being taken from the experiments of M. Regnault.

$$a = 340{\cdot}89 \text{ metres per second.}$$

$$g = 9^{\mathrm{m}}{\cdot}80896. \qquad h = 0^{\mathrm{m}}{\cdot}76. \qquad \mathrm{T} = 15^{\circ}{\cdot}9 \text{ Centigrade.}$$

$$\mathrm{E} = 0{\cdot}003665\,; \ \frac{m}{\Delta} = 10513.$$

Consequently, for atmospheric air,

$$\gamma = 1{\cdot}401.$$

The results of a reduction, according to correct data, of the experiments of Dulong upon the velocity of sound in atmospheric air, oxygen, and hydrogen, are as follows:—

Atmospheric air	$\gamma = 1{\cdot}410$
Oxygen	$1{\cdot}426$
Hydrogen	$1{\cdot}426$

Thus it appears, that for the simple substances, oxygen and hydrogen, the ratio N is the same, while for atmospheric air it is somewhat smaller*.

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* The following are some additional determinations of the value of γ for atmospheric air, founded upon experiments on the velocity of sound:—

Observers.	T. Centigrade.	a. Metres per second.	γ.
Bravais and Martins: mean of several experiments at temperatures varying from 5° to 11° Centigrade, reduced to 0° (*Comptes Rendus*, xix.).........	$\overset{\circ}{0}$	332·37	1·40955
Moll and Van Beek: reduced to . .	0	332·25	1·40853
Stampfer and Myrbach: reduced to 0° (not corrected for moisture) . .	0	332·96	1·41456
Académie des Sciences, 1738 (not corrected for moisture)	6·1	337·10	1·418

A variation of one metre per second in the velocity of sound at 0° corresponds to a variation of ·0085 in the value of γ. See also Phil. Mag. for June 1853.

(12.) The ordinary mode of expressing the specific heats of gases is to state their ratios to that of an *equal volume* of atmospheric air at the same pressure and temperature.

When $\frac{\kappa}{\tau}$ is a very small fraction, specific heats of *unity of volume* of a perfect gas are given by the equations

$$\left.\begin{aligned} n\mathrm{MK_V} &= \frac{1}{\mathrm{CN}} \\ n\mathrm{MK_P} &= \frac{1}{\mathrm{C}}\left(\frac{1}{\mathrm{N}}+1\right) \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \qquad (24)$$

That is to say, the specific heat of unity of volume at constant volume is inversely proportional to the fraction by which the ratio of the two specific heats exceeds unity; a conclusion already deduced from experiment by Dulong.

The following is a comparison of the ratios of the apparent specific heats under constant pressure, of unity of volume of oxygen and hydrogen respectively, to that of atmospheric air, as deduced from equation (24), with those determined experimentally by De la Roche and Bérard.

	Ratio $\frac{n\mathrm{MK_P}\,(\text{gas})}{n\mathrm{MK_P}\,(\text{atmos. air})}$	
Gas.	By theory.	By experiment.
Oxygen	0·973	0·9765
Hydrogen	0·973	0·9033

This comparison exhibits a much more close agreement between theory and experiment than has been hitherto supposed to exist, the errors in the constants employed having had the effect of making the ratio 1 + N seem greater for atmospheric air than for oxygen and hydrogen, while in fact it is smaller.

To treat the other substances on which both M. Dulong and MM. De la Roche and Bérard made experiments as perfect gases, would lead to sensible errors. I have therefore confined my calculations for the present to oxygen, hydrogen, and atmospheric air*.

(13.) The heat produced by compressing so much of a perfect gas as would occupy unity of volume under the pressure unity, at the temperature 0° Centigrade, from its actual volume $n\mathrm{MV_1} = \frac{\tau}{\mathrm{PC}}$ into a volume which is less in a given ratio s (when κ is neglected as compared with τ), is expressed by the following equation:—

$$n\mathrm{MQ'} = -\frac{1}{\mathrm{C}}\int_{\mathrm{V_1}}^{s\mathrm{V_1}} d\mathrm{V}.\frac{\tau}{\mathrm{V}} = -n\mathrm{MV_1}\int_1^s \mathrm{P}ds, \qquad . \quad (25)$$

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* For a comparison of the theory with the later and more accurate experiments of M. Regnault, see the Philosophical Magazine for June 1853.

being, in fact, equal to the mechanical power used in the compression. When the temperature is maintained constant, this becomes

$$nMQ'_{(\tau)}=\frac{\tau}{C}\log_e.\frac{1}{s}, \quad . \quad . \quad . \quad . \quad . \quad (26)$$

which is obviously independent of the nature of the gas.

Hence *equal volumes of all substances in the state of perfect gas, at the same pressure and at equal and constant temperatures, being compressed by the same amount, disengage equal quantities of heat;* a law already deduced from experiment by Dulong.

(14.) The determination of the fraction N affords the means of calculating the mechanical or absolute value of specific heat, as defined by equation 1, Section I. The data for atmospheric air being taken as follows,

$$N=0{\cdot}4, \qquad C=274^{\circ}{\cdot}6 \text{ Centigrade},$$

$\frac{1}{nM}$ = height of an imaginary column of air of uniform density, at the temperature 0° Cent., whose pressure by weight on a given base is equal to its pressure by elasticity,

$$=7990 \text{ metres},$$
$$=26214 \text{ feet}:$$

the real specific heat of atmospheric air, or the depth of fall equivalent to one Centigrade degree of temperature in that gas, is found to be

$$\mathfrak{h}=\frac{1}{CnMN}=72{\cdot}74 \text{ metres} = 238{\cdot}66 \text{ feet}. \quad . \quad . \quad (27)$$

The ratio of its real specific heat to the apparent specific heat of water at 0° Centigrade is therefore

$$\frac{\mathfrak{h}}{K_W}=\frac{238{\cdot}66}{1389{\cdot}60}=0{\cdot}1717, \quad . \quad . \quad . \quad . \quad (28)$$

K_W being the mechanical value of the apparent specific heat of liquid water, as determined by Joule.

The apparent specific heat of air under constant pressure, as compared with that of liquid water, is

$$\frac{K_P}{K_W}=0{\cdot}1717\times 1{\cdot}4=0{\cdot}2404. \quad . \quad . \quad (28A)$$

The value of this last quantity, according to the experiment of De la Roche and Bérard, is

$$0{\cdot}2669,$$

the discrepancy being about *one-ninth* of the value according to Joule's equivalent*.

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* According to the experiments of M. Regnault, $\frac{K_P}{K_W}$ for air $=0{\cdot}2379$, differing by about one-hundredth part from the result of theory.

(15.) Besides the conditions of constant volume and constant pressure, there is a third condition in which it is of importance to know the apparent specific heat of an elastic fluid, namely, the condition of vapour at saturation, or in contact with its liquid.

The apparent specific heat of a vapour at saturation is the quantity of heat which unity of weight of that vapour receives or gives out, while its temperature is increased by one degree, its volume being at the same time compressed so as to bring it to the maximum pressure corresponding to the increased temperature.

It has been usually taken for granted, that this quantity is the same with the variation for one degree of temperature, of what is called the *total heat* of evaporation. Such is, indeed, the case according to the theory of Carnot; but I shall show, that, according to the mechanical theory of heat, these two quantities are not only distinct, but in general of contrary signs.

I shall for the present consider such vapours only as may be treated in practice as perfect gases, so as to make the first of the equations (20) applicable.

It has been shown that the logarithm of the maximum elasticity of a vapour in contact with its liquid may be represented by the expression

$$\log \mathrm{P} = a - \frac{\beta}{\tau} - \frac{\gamma}{\tau^2}.$$

The coefficients α, β, γ being those adapted for calculating the common logarithm of the pressure, I shall use the accented letters α', β', γ' to denote those suited to calculate the hyperbolic logarithm, being equal respectively to the former coefficients $\times 2{\cdot}3025851$.

Then for vapour at saturation,

$$\frac{d\mathrm{P}}{\mathrm{P}d\tau} = \frac{\beta'}{\tau^2} + \frac{2\gamma'}{\tau^3}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (29)$$

Making this substitution in the general equation (21), we find the following value for the apparent specific heat of perfectly gaseous vapour at saturation :—

$$\left.\begin{aligned} \mathrm{K_S} &= k + \mathrm{P}\frac{d\mathrm{V}}{d\tau} = k\left(1 + \mathrm{N} . \frac{\tau}{\mathrm{V}}\frac{d\mathrm{V}}{d\tau}\right) \\ &= k\left\{1 + \mathrm{N}\left(1 - \frac{\tau}{\mathrm{P}}\frac{d\mathrm{P}}{d\tau}\right)\right\} \\ &= \frac{1}{\mathrm{C}n\mathrm{M}}\left(\frac{1}{\mathrm{N}} + 1 - \frac{\beta'}{\tau} - \frac{2\gamma'}{\tau^2}\right). \end{aligned}\right\} . \quad . \quad . \qquad (30)$$

(16.) For the vapours of which the properties are known, the negative terms of this expression exceed the positive at all ordinary temperatures, so that the kind of apparent specific heat now under consideration is a negative quantity; that is to say, that if a given weight of vapour at saturation is increased in temperature, and at the same time maintained by compression at the maximum elasticity, the heat generated by the compression is greater than that which is required to produce the elevation of temperature, and a surplus of heat is given out; and on the other hand, if vapour at saturation is allowed to expand, and at the same time maintained at the temperature of saturation, the heat which disappears in producing the expansion is greater than that set free by the fall of temperature, and the deficiency of heat must be supplied from without, *otherwise a portion of the vapour will be liquefied, in order to supply the heat necessary for the expansion of the rest**.

This circumstance is obviously of great importance in meteorology, and in the theory of the steam-engine. There is as yet no experimental proof of it. It is true that, in the working of non-condensing engines, it has been found that the steam which escapes is always at the temperature of saturation corresponding to its pressure, and carries along with it a portion of water in the liquid state; but it is impossible to distinguish between the water which has been liquefied by the expansion of the steam, and that which has been carried over mechanically from the boiler†.

The calculation of the proportion of vapour liquefied by a given expansion, requires the knowledge of the latent heat of evaporation, which forms the subject of the next section.

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* This conclusion is applicable only when the mechanical power produced by the expansion of the vapour is expended in moving another body; for example, the piston of an engine. When this power is expended in producing currents in the vapour itself, the friction of those currents eventually reconverts the whole of the power into heat; so that the vapour, instead of being partially liquefied, is super-heated, as has been shown theoretically by Professor William Thomson, and experimentally by Mr. Charles W. Siemens. (Civil Engineer and Architect's Journal, September 1852.)

† The experiments of Mr. Daniel Kinnear Clark, on the expansive action of steam in locomotive engines, described in his work "On Railway Machinery," show that a large amount of liquefaction generally takes place during the expansion of the steam. It appears, however, as Mr. Clark has pointed out, that a considerable portion of this liquefaction arises from the transference of heat to the metal of the cylinder, and is followed by re-evaporation when the heat is transferred back, upon the pressure of the steam falling below that due to the temperature of the metal. How much of the liquefaction is due to this cause, and how much to the expansion of the steam, it is impossible to determine, in the present imperfect state of our knowledge of the bulk occupied by a given weight of steam at a given pressure and temperature.

SECTION III.—*Of the Latent and Total Heat of Evaporation, especially for Water.*

(17.) The latent heat of evaporation of a given substance at a given temperature, is the amount of heat which disappears in transforming unity of weight of the substance from the liquid state, to that of vapour of the maximum density for the given temperature, being consumed in producing an increase of volume, and an unknown change of molecular arrangement.

It is obvious that if the vapour thus produced is reconverted into the liquid state *at the same temperature*, the heat given out during the liquefaction must be equal to that consumed during the evaporation; for as the sum of the expansive and compressive powers, and of those dependent on molecular arrangement during the whole process, is equal to zero, so must the sum of the quantities of heat absorbed and evolved.

The heat of liquefaction, at a given temperature, is therefore equal to that of evaporation, with the sign reversed.

(18.) If to the latent heat of evaporation at a given temperature is added the quantity of heat necessary to raise unity of weight of the liquid from a certain fixed temperature (usually that of melting ice) to the temperature at which the evaporation takes place, the result is called the total heat of evaporation from the fixed temperature chosen.

According to the theory of Carnot, this quantity is called the constituent heat of vapour; and it is conceived, that if liquid at the temperature of melting ice be raised to any temperature and evaporated, and finally brought in the state of vapour to a certain given temperature, the whole heat expended will be equal to the constituent heat corresponding to that given temperature, and will be the same, whatsoever may have been the intermediate changes of volume, or the temperature of actual evaporation.

According to the mechanical theory of heat, on the other hand, the quantity of heat expended must vary with the intermediate circumstances; for otherwise no power could be gained by the alternate evaporation and liquefaction of a fluid at different temperatures.

(19.) The law of the latent and total heat of evaporation is immediately deducible from the principle of the constancy of the total *vis viva* in the two forms of heat and expansive power, when the body has returned to its primitive density and temperature, as already laid down in article 7.

That principle, when applied to evaporation and liquefaction, may be stated as follows:—

Let a portion of fluid in the liquid state be raised from a certain temperature to a higher temperature: let it be evaporated at the higher temperature: let the vapour then be allowed to expand, being maintained always at the temperature of saturation for its density, until it is restored to the original temperature, at which temperature let it be liquefied:—then *the excess of the heat absorbed by the fluid above the heat given out, will be equal to the expansive power generated.*

To represent those operations algebraically, let the lower absolute temperature be τ_0: the volume of unity of weight of liquid at that temperature, v_0, and that of vapour at saturation, V_0: let the pressure of that vapour be P_0: the latent heat of evaporation of unity of weight, L_0: and let the corresponding quantities for the higher absolute temperature τ_1, be v_1, V_1, P_1, L_1. Let K_L represent the mean apparent specific heat of the substance in the liquid form between the temperatures τ_0 and τ_1. Then,—

First. Unity of weight of liquid being raised from the temperature τ_0 to the temperature τ_1, absorbs the heat

$$K_L(\tau_1-\tau_0),$$

and produces the expansive power,

$$\int_{v_0}^{v_1} dv\,.\,P.$$

Secondly. It is evaporated at the temperature τ_1, absorbing the heat

$$L_1,$$

and producing the expansive power,

$$P_1(V_1-v_1).$$

Thirdly. The vapour expands, at saturation, until it is restored to the original temperature τ_0. In this process it absorbs the heat

$$\int_{\tau_0}^{\tau_1} d\tau\,.\,K_S,$$

and produces the expansive power,

$$\int_{V_1}^{V_0} dV\,.\,P.$$

Fourthly. It is liquefied at the original temperature, giving out the heat

$$L_0,$$

and consuming the compressive power,

$$P_0(V_0-v_0).$$

The equation between the heat which has disappeared, and the expansive power which has been produced, is as follows:—

$$\left.\begin{aligned} &L_1-L_0+K_L(\tau_1-\tau_0)-\int_{\tau_0}^{\tau_1}d\tau\,.\,K_S \\ &=P_1(V_1-v_1)-P_0(V_0-v_0)+\int_{v_0}^{v_1}dv\,.\,P+\int_{V_1}^{V_0}dV.P. \end{aligned}\right\} \quad (31)$$

If the vapour be such that it can be regarded as a perfect gas without sensible error, the substitution of $\mathfrak{k}+P\frac{dV}{d\tau}$ for K_S, and of $\frac{\tau}{CnM}=\mathfrak{k}N\tau$ for PV, transforms the above to

$$\left.\begin{aligned} &L_1-L_0+\{K_L-\mathfrak{k}(1+N)\}(\tau_1-\tau_0) \\ &=-P_1v_1+P_0v_0+\int_{v_0}^{v_1}dv\,.\,P=-\int_{P_0}^{P_1}dP\,.\,v \end{aligned}\right\} \quad . \quad . \quad . \quad (32)$$

In almost all cases which occur in practice, v is so small as compared with V, that $-\int dP\,.\,v$ may be considered as sensibly $=0$; and therefore (sensibly)

$$L_1+K_L(\tau_1-\tau_0)=L_0+\mathfrak{k}(1+N)(\tau_1-\tau_0). \quad . \quad . \quad . \quad (33)$$

Now this quantity is the total heat required to raise unity of weight of liquid from τ_0 to τ_1 of absolute temperature, and to evaporate it at the latter temperature. Therefore *the total heat of evaporation, where the vapour may be treated as a perfect gas, increases sensibly at an uniform rate with the temperature of evaporation; and the coefficient of its increase with temperature is equal to the apparent specific heat of the vapour at constant pressure,* $\mathfrak{k}(1+N)$.

(20.) The experiments of M. Regnault prove that the total heat of evaporation of water increases uniformly with the temperature from 0° to 200° Centigrade.

The coefficient of increase is equal to

$$K_W \times 0{\cdot}305.$$

Its mechanical value is consequently
129·18 metres = 422·83 feet per Centigrade degree, or
235·46 feet per degree of Fahrenheit. (34)

Although the principle of the conservation of *vis viva* has thus enabled us to ascertain the *law of increase* of the total heat of evaporation, it does not enable us to calculate *à priori* the constant L_0 of the formula, being the latent heat of evaporation at the fixed temperature from which the total heat is measured; for the changes of molecular arrangement which constitute evaporation are unknown*.

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* Other investigations have shown that the latent heat of evaporation is connected with the increase of volume by the equation

$$L=(\tau-\kappa)\frac{dP}{d\tau}(V-v);$$

but the exact values of V are yet unknown.

When the fixed temperature is that of melting ice, M. Regnault's experiments give 606·5 Centigrade degrees, applied to liquid water as the value of this constant; so that

$$\left.\begin{aligned} L+K_L T &= K_W(606^\circ\!\cdot 5+\cdot 305\,T^\circ)\\ &\qquad\text{for the Centigrade scale,}\\ &=K_W(1091^\circ\!\cdot 7+\cdot 305(T^\circ-32^\circ))\\ &\qquad\text{for Fahrenheit's scale,}\end{aligned}\right\}\quad\ldots\quad(35)$$

is the complete expression for the heat required to raise unity of weight of water from the temperature of melting ice to T^0 above the ordinary zero, and to evaporate it at the latter temperature.

It must be remarked, that the unit of heat in M. Regnault's tables is not precisely the specific heat of water at 0° Centigrade, but its mean specific heat between the initial and final temperatures of the water in the calorimeter. The utmost error, however, which can arise from this circumstance, is less than $\frac{1}{1200}$ of the total heat of evaporation, so that it may safely be neglected.

[This section formerly concluded with a computation of the specific heats of steam on the supposition of its being sensibly a perfect gas; but the error of this supposition, though not material in calculating the power of steam-engines, is in all probability sufficiently great to vitiate the computation in question, which has therefore been cancelled.]

[To be continued.]

XVII. *On the Electricity of Plants.* *By* Professor H. Buff*.

ABOUT twenty-seven years ago Pouillet (*Ann. de Chim. et de Phys.* xxxv. 401) described a number of experiments, from which he concluded that plants, during their germination and growth, impart positive electricity to the air and negative electricity to the soil. The majority of physicists accepted this conclusion without further proof, inasmuch as it appeared to render a satisfactory account of the origin of atmospheric electricity. A repetition of Pouillet's experiments, with more delicate apparatus, was undertaken by P. Riess, who however was unable to obtain the results of the former investigator. Two years ago, Wartmann (Phil. Mag. [4] i. 578.) and Becquerel (*Ann. de Chim. et de Phys.* [3] xxi. 40) communicated observations, according to which growing plants impart negative electricity to the atmosphere and positive electricity to the soil, the two electricities thus moving in directions opposite to those inferred from the observations of Pouillet.

The mode of experiment pursued by the last two physicists was as follows:—Homogeneous platinum wires were brought

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* Communicated by the Author.

into contact with the parts of the plant whose electric deportment was to be ascertained, the wires were connected with a galvanometer, and the action upon the needle of the instrument was observed. This procedure has, however, a source of error connected with it from which the results of the observations cannot be set free. It is well known, although not sufficiently attended to by investigators of the electro-chemical school, that platinum, in contact with different liquids, exhibits different degrees of electric excitation. The sum or difference of these actions must of necessity change the quantity, and perhaps also the quality of the original action due to the plant alone. The observations communicated by Wartmann and Becquerel, even though of themselves perfectly correct, could therefore give no answer to the question, whether plants, in their natural condition, and during their free growth, discharge electricity.

To examine the electric deportment of a plant as it occurs in nature, it is absolutely necessary to bring it into such relation with the apparatus used in the experiments, that its natural conditions shall be changed in the least degree possible. Now the roots of thriving plants generally ramify themselves through moist earth. The surface of their leaves, though they may not be wet by rain or dew, are hygroscopically moist. It appeared to me, therefore, necessary to establish the connexion of the plants, or parts of plants, with the electrical apparatus, by means of water alone.

Upon this idea the following arrangement of the apparatus is founded. Two glass beakers were filled with mercury to a height of half an inch above the bottom, and then filled nearly to the rim with water. Platinum wires, smelted into glass tubes, dipped with their well-amalgamated ends, which projected for some lines only beyond the glass tubes, into the mercury, their other ends being connected with the helix of a very delicate multiplying galvanometer. To complete this circuit, it was only necessary to unite the water of both glasses by a conductor. When the circuit was established by means of a strip of moist bibulous paper, the needle continued motionless. A small addition of common salt or of the acid sap of a plant to the one or the other beaker, produced a deflection. By heating the water, or by mixing the contents of both beakers well together, the equilibrium was again established. A shaking of the fluid, which, when platinum wires are immersed in it, easily causes electrical excitation, was in the present case without influence, inasmuch as the wires were protected by the glass tubes which surrounded them from being unequally wetted. When an electric current was conducted through the circuit closed by the bibulous paper, the mercury surface became indeed polarized, but much less so than platinum, and the equilibrium was soon established again, either of itself, or by the moderate moving of the liquid metal by a glass rod.

To examine the electric condition of a plant, it was placed between the two beakers in the place of the bibulous paper. At the termination of the experiment the circuit was again closed with the paper, in order to be assured that at the commencement of a new experiment every foreign influence was effectually removed.

In the first place, plants of the most varied kinds, having their roots washed in flowing water, were examined. The roots, including the attached fibres, dipped into one of the beakers; a portion of the uninjured leaves into the other. Having observed the direction of the consequent deflection, the experiment was interrupted, and, when the equilibrium had once more established itself, the position of the plant was reversed and the experiment repeated; that is to say, the roots were now caused to dip into the beaker, which in the former experiment contained the leaves; the current, so far as it was dependent on an electric excitation due to the plant, must therefore in the latter case be reversed in direction.

The observed deflection sometimes amounted to a few degrees merely, sometimes it was a large arc. The direction of the deflection was however in all cases the same, and announced the existence of a current which passed through the plant from the root to the leaves.

As the numerous plants experimented with were not all equally sappy, nor did they possess equal lengths and thicknesses, they did not present the same surface of contact to the water; great differences as to the resistance offered to the passage of the current were thus unavoidable. The inequality of the currents was certainly in a great measure due to this circumstance; in several cases the magnitude of the deflection might be increased at pleasure, when several plants of the same kind were placed, in the same direction, one above another.

The same mode of experiment enables us to examine, not only the whole plants, but also any portions of them. The place of severance from the plant (sometimes after the removal of the exterior bark) dipped into one glass vessel, the leaves, and frequently only a single leaf, dipped into the other. The current was in no case absent, and its direction was always from the injured portion, for example, from the place of severance, to the external surface of the leaves. Severed branches, which had remained several days in water, or even the fallen and half-withered leaves, still acted, though with diminished energy, in the same manner as the freshest.

When the interior of the plant was exposed at any place, whether on the upper or under portions of the stem or branch, and when this injured place was brought into contact with the water of one of the beakers, either immediately or by the intervention of wet bibulous paper, while uninjured leaves dipped into the other beaker (it being a matter of indifference what portion of the plant they belonged to), the direction of the current was always from the wounded portion of the plant to the leaves.

Two leaves of the same plant immersed, one into the first beaker, the other into the second, produced no current, or at least none whose direction could be predicted beforehand. If, however, a portion of the surface of one of the leaves was removed and the place brought into contact with the water, this was sufficient to generate a current towards the uninjured leaf. Even when the connexion between the liquids was established by one and the same leaf, the place of severance from the tree being kept outside, then by scratching the one or the other of the immersed portions, a current could be generated which proceeded uniformly from the injured place to the uninjured one.

Exactly the same deportment as that observed in the green leaves was exhibited by blossoms, flowers and fruits. The fresh young bark exhibited the deportment of the leaves. Each uninjured portion was positively electric compared with the root or with a wounded portion of the plant, no matter where the wound was inflicted.

From these observations we obtain the following rule, which is universally valid:—*the roots, and all the interior portions of the plant filled with sap, are in a permanently negative condition; while the moist or moistened surface of the fresh branches, leaves, flowers and fruits are permanently positively electric.*

The external cuticle which embraces the fresh branches and leaves, the epidermis, is known to contain a substance of the nature of wax, which possesses the property of interrupting the transmission of the acid and saline liquids which are contained within the plant, without losing in the same degree the capability of being moistened, or of permitting water to permeate it, and of conducting electricity. Between the moist surface of the plant, therefore, and the liquids in its interior, a definite limit is always present, which however does not interrupt the mutual contact and the connexion necessary for conduction. All the conditions for a permanent electro-motive activity are therefore present, an activity, in virtue of which, as experience teaches, the entire exterior surface assumes positive electricity, while the interior portions, to the roots, and the latter included, assume a negative electric state.

It might be expected that an equally strong electric excitation must occur when the plant comes in contact with the water of the earth. But it may be proved, that in an aqueous solution composed of successive layers gradually increasing in concentration, the sum of all the excitations which occur at the places of contact of the different layers is less than the excitation between pure water and the most concentrated of the layers; and thus, no matter what the nature of the solution may be, pure water will be positively electric at the place of transition to the most concentrated layer of the solution.

Now this is the exact deportment of plants in the experiments described above. The epidermis forms a sharp and permanent limit between the sap in the highest possible state of concentration behind it, and the pure water on its exterior, while by means of the roots a very gradual transition to water is effected. In like manner the sap exuding from a wound into the surrounding water can spread itself with gradually diminishing concentration.

The electroscopic actions of the electricity of plants correspond to the origin of the latter, as explained above; they are very feeble. Even by means of the condenser and dry-pile electroscope, no electric charges of sufficient magnitude can be obtained to enable us to attribute them in a certain manner to the plant as source. By forming a compound circuit of plants, I succeeded in obtaining stronger tension effects. Each uninjured leaf, with its severed end, forms as it were a galvanic element. It was therefore only necessary to arrange suitably a number of such leaves in a series to obtain a compound galvanic circuit. Thus, with a battery of twelve sappy leaves, a tension was obtained which amounted to somewhat more than half of that exhibited by an element of zinc and copper immersed in water.

XVIII. *On the Detection of Gold in Lead and its Compounds. By* JOHN PERCY, *M.D., F.R.S., Lecturer on Metallurgy at the School of Mines, Jermyn Street; and* RICHARD SMITH, *Assistant in the Metallurgical Laboratory*[*].

IN the Philosophical Magazine for April 1853, it was stated that gold had been detected in numerous samples of lead and its compounds met with in commerce, and that the experiments upon which the statement was founded should appear on a future day. Those experiments are now given. The investigation is still in progress, and will embrace the examination of a variety of ores.

All the experiments have been made by Mr. Smith, and the

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[*] Communicated by the Authors.

visible specimen of burnished gold obtained in each experiment has been preserved in a hermetically sealed tube.

The diffusion of gold, as proved by the experiments in question, is at least curious, and may probably suggest chemical and geological considerations of special interest at the present time. The fact of gold existing in certain soluble compounds of lead is remarkable; and it may be that sea-water will one day be found to contain the precious metal, though in infinitesimally small proportion.

The quantity of gold obtained in each experiment was far too minute to be capable of estimation by the most delicate balance. In order, however, to give an approximate and comparative notion of the quantities respectively extracted, the following scale of comparison will be adopted in the description,—trace, minute trace, very minute trace, and just perceptible trace.

At present it has not been possible to prepare lead free from a trace of gold.

Method of Examination.

The lead was separated from the compounds of lead examined by a process of reduction described under each experiment; a known weight of the lead was then submitted to the process of cupellation in the usual way, and the button of silver left after cupellation was carefully detached from the cupel, flattened under a hammer to free it from adherent matter, transferred to a small watch-glass, treated first with very dilute nitric acid at a very gentle heat until all action had ceased, and then with strong nitric acid. The black residual matter was carefully washed with distilled water by decantation, transferred to a small piece of writing-paper, dried at a gentle heat, rubbed with a steel burnisher, gummed to the paper, and preserved in a small glass tube hermetically sealed. When necessary, this process was performed under a microscope.

Lead.

Exp. I. A specimen of lead pipe from the Great Exhibition of 1851. Cupelled 2000 grs., the button obtained, treated with nitric acid, left a *trace* of gold.

Exp. II. A specimen of "Pattinson's crystallized lead." Cupelled 2000 grains, treated the remaining button with nitric acid, and obtained a *minute trace* of gold.

Exp. III. Specimen of lead from the Nenthead Works near Alston. Cupelled 2000 grs., the button obtained, parted with nitric acid, left a *very minute trace* of gold.

Exp. IV. Specimen of lead from Tuscany, from the Great Exhibition of 1851, hard and brittle, with a close-grained crystalline fracture, probably due to the presence of antimony. Cupelled 2000 grs., the residual button, parted with nitric acid, left a *very minute trace* of gold.

Exp. V. Specimen of lead from the Austrian collection in the Great Exhibition of 1851, labelled "23, Bleiberger Probierblei." Cupelled 2000 grs., treated the remaining button with nitric acid, and obtained a *minute trace* of gold.

Exp. VI. Specimen from ditto, labelled "25, Przibramer Weichblei." 2000 grs. cupelled, and the residual button treated with nitric acid, left a trace of black matter; but the colour of gold could not be *distinctly* obtained by burnishing, probably owing to a small amount of silver left undissolved.

Exp. VII. Repeated Exp. VI. upon 2000 grs., and obtained a *very minute trace* of gold.

Exp. VIII. Specimen from ditto, labelled "26, Przibramer Hartblei." 3340 grs. scorified to a small bulk and then cupelled, left a button weighing ·1 gr.; treated with nitric acid, left a *minute trace* of gold.

Red Lead.

Exp. IX. Specimen made from Snail-beach lead, Shropshire. Mixed 2 lbs. troy of red lead with excess of finely-powdered charcoal, heated the mixture in a Cornish crucible, and poured out the reduced lead into an ingot mould. Of this lead, cupelled 2000 grs., parted the residual button with nitric acid, and obtained a *trace* of gold.

Exp. X. Specimen made from Derbyshire lead. Weighed out 5000 grs. of red lead and 300 grs. of powdered charcoal, heated the mixture in a Cornish crucible, the reduced lead weighed 3840 grs. Of this lead, cupelled 2000 grs., the button obtained weighed about ·1 of a grain; parted with nitric acid, left a *trace* of gold.

Litharge.

Exp. XI. Sample purchased at Mr. C. Button's, Holborn Bars (in the form of small, thin scales). Mixed 6000 grs. with 300 grs. of powdered charcoal, reduced the mixture in a Cornish crucible, lead obtained weighed 5460 grs. Of this lead, cupelled 2000 grs., obtained a small button of silver; parted with nitric acid, left a *very minute trace* of gold.

Exp. XII. Sample bought at Mr. G. James's, 72 Wardour Street (in powder). 6000 grs. reduced by heating with 300 grs. of powdered charcoal, gave of lead 4910 grs. Of the lead thus obtained, cupelled 2000 grs., parted the small residual button of silver with nitric acid, and obtained a *very minute trace* of gold.

Exp. XIII. Sample purchased at Mr. Caplin's, 42 Great Pulteney Street (in powder and small lumps). 6000 grs. mixed with 300 grs. of powdered charcoal and reduced in a Cornish crucible, gave of lead 5310 grs. Of this lead, cupelled 2000 grs., treated the small remaining button of silver with nitric acid, and obtained a *minute trace* of gold.

Exp. XIV. Sample brought from Birmingham (in small, thin scales). Reduced 2880 grs. with 150 grs. of powdered charcoal in a Cornish crucible; lead obtained weighed 2270 grs. Of this lead, cupelled 2000 grs.; the small button of silver obtained, parted with nitric acid, left a *minute trace* of gold.

White Lead.

Exp. XV. Specimen purchased at Mr. Button's, Holborn Bars, sent as "pure carbonate of lead," and said to have been prepared by precipitation from a solution of the nitrate of lead by carbonate of soda. 6000 grs. mixed with 200 grs. of powdered charcoal and heated in a Cornish crucible, gave of reduced lead 4190 grs. Of this lead, cupelled 2000 grs., left a very minute button of silver; parted by nitric acid, a just perceptible trace of black matter remained undissolved, but no *distinct* metallic lustre could be obtained by burnishing.

Exp. XVI. Repeated Exp. XV. 2 lbs. troy (11,520 grs.) reduced by heating in a Cornish crucible with 400 grs. of charcoal, gave of lead 8550 grs. Of this lead, 4000 grs. cupelled left a very small button of silver; parted by nitric acid, left a *just perceptible trace* of gold.

Exp. XVII. Sample bought at Mr. G. James's, 72 Wardour Street. 6000 grs. mixed intimately with 200 grs. of powdered charcoal and heated in a Cornish crucible, gave of lead 4340 grs. Of this lead, cupelled 2000 grs., parted the small residual button of silver with nitric acid, and obtained a *very minute trace* of gold.

Exp. XVIII. Sample purchased at Mr. Caplin's, 42 Great Pulteney Street. Mixed 6000 grs. with 200 grs. of powdered charcoal, heated the mixture in a Cornish crucible, obtained of lead 3840 grs. Of this lead, cupelled 2000 grs.; the small globule of silver obtained, parted by nitric acid, left a *very minute trace* of gold.

Oxychloride of Lead.

Exp. XIX. Pattinson's oxychloride. 1 lb. troy (5760 grs.) mixed intimately with 4 ozs. (1920 grs.) of dried carbonate of soda, and ½ oz. (240 grs.) of powdered charcoal, heated the mixture in a Cornish crucible, lead obtained weighed 4140 grs. Of this lead, cupelled 4000 grs. (a small portion of the lead was lost during the operation), no button of silver was obtained (?). There was not a sufficient quantity of this sample left to repeat the experiment.

Exp. XX. Sample purchased at Messrs. Blundell, Spence and Co., 9 Upper Thames Street, as Pattinson's genuine oxychloride of lead. Reduced 3 lbs. troy in the same way as Exp. XIX. Of the lead obtained, cupelled 4000 grs.; the small button of silver obtained, parted by nitric acid, left a *very minute trace* of gold.

Exp. XXI. Repeated Exp. XX. on 4000 grs. of lead; treated the button of silver left by cupellation with nitric acid, and a *very minute trace* of gold was obtained.

Acetate of Lead.

Exp. XXII. Sample purchased at Mr. C. Button's, Holborn Bars, slightly coloured with oxide of iron. 2 lbs. troy (11,520 grs.) reduced by projecting it in small quantities at a time into a hot Cornish crucible, and finally heating until all the lead was separated; lead obtained weighed 5700 grs. 2000 grs. of this lead, cupelled, did not leave any visible trace of silver.

Exp. XXIII. Repeated Exp. XXII. upon 2000 grs., but no button of silver was obtained.

Exp. XXIV. Repeated Exp. XXII. Took of acetate of lead 2 lbs. troy (11,520 grs.) and reduced it by heating in a Cornish crucible; lead obtained weighed 5860 grs. 4000 grs. of this lead cupelled to about 200 grs. on a large cupel; it was then transferred to a small cupel and the operation completed; a very minute globule of silver remained, which was treated with nitric acid, and a *just perceptible trace of gold* was obtained.

Exp. XXV. Sample bought at Mr. H. Barnes's, 38 Long Acre (very white and clean). 5760 grs. reduced by heating in a Cornish crucible, gave of lead 3136 grs. Of this lead, cupelled 2000 grs., obtained a very minute globule of silver, which by parting with nitric acid left a *just perceptible trace* of gold.

Exp. XXVI. Repeated Exp. XXV. 5760 grs. reduced as before, gave of lead 2830 grs. Of this lead, 2000 grs. cupelled gave a very minute globule of silver, which, after parting with nitric acid, left a *just perceptible trace* of gold.

[To be continued.]

XIX. *On the Method of Symmetric Products. By* JAMES COCKLE, *M.A., of Trinity College, Cambridge; Barrister-at-Law of the Middle Temple**.

[Concluded from vol. v. p. 174.]

44. QUADRATICS furnish us with a function which, if not the strict analogue of those presented by the other equations, may yet be considered as corresponding to them.

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* Communicated by the Author.

45. Let

$$Y_1=y_1+zy_2,\quad Y_1^1=zy_1+y_2,$$

and, consequently,

$$Y_1^1+Y_1=\Sigma\,.\,Y=(z+1)(y_1+y_2)=(z+1)\Sigma\,.\,y\,;$$

then, if we make

$$z+1=0,$$

we have an equation analogous to that which occurs in the higher degrees, and the result

$$P^1=\pi^1{}_2(y_2)=Y_1Y_1^1=-Y^2{}_1=4y_1y_2-(y_1+y_2)^2$$

shows that Y_1 may be obtained by the extraction of a square root, and, $\Sigma\,.\,y$ being known, the quadratic is solved.

46. It may be well, before addressing ourselves to equations of the fifth degree, to illustrate the Method of Symmetric Products by an example in which U_n does not occur*. For this purpose let us take the cubic

$$x^3+ax^2+bx+c=0.$$

47. Guided by (4) we assume

$$y_1=(x_1-\xi)^{-1},\quad y_2=(x_2-\xi)^{-1},\quad y_3=(x_3-\xi)^{-1},$$

and consequently

$$Y_1=(x_1-\xi)^{-1}+\alpha_1(x_2-\xi)^{-1}+\beta_1(x_3-\xi)^{-1},$$
$$Y_2=(x_1-\xi)^{-1}+\alpha_2(x_2-\xi)^{-1}+\beta_2(x_3-\xi)^{-1}.$$

48. The conditions of symmetry are

$$1=\alpha_1\alpha_2=\beta_1\beta_2,$$
$$E=\Sigma\,.\,\alpha=\Sigma\,.\,\beta=\Sigma\,.\,\alpha_1\,\beta_2\dagger,$$
$$\pi_2(y_3)=Y_1Y_2=\Sigma\,.\,y^2+E\Sigma\,.\,y_1y_2,$$

and the expressions (α_1,α_2), (β_1,β_2) each involve the roots of

$$z^2-Ez+1=0.\quad.\quad.\quad.\quad.\quad.\quad.\quad(k)$$

49. From the identity

$$(\alpha_1+\alpha_2)(\beta_1+\beta_2)-(\alpha_1\beta_2+\alpha_2\beta_1)-(\alpha_1\alpha_2+\beta_1\beta_2)=0,$$

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* Systems consisting of two, three, four, . , p single equations may be respectively called dual, ternal, quaternal, . , p-al systems. That which the discussion of quintics has presented to us for solution is septuagintal and, in the language of my 'Analysis,' sexdecimary. We have only been able to solve 60 of the members of this 70-al 16-ary 4-ic system. The refractory equations have given rise to the function U_4 or Σ'. For my nomenclature of *order* I may cite the authority of Garnier (*Anal. Alg.* p. 121).

† From this, by excluding nugatory assumptions, we might obtain

$$\phi'(\beta)=\phi'(\beta^2)=\{\phi'(\beta)\}^2-2,\ \text{or}\ \phi'(\beta)=2\ \text{or}\ -1.$$

The two relations between the functions ϕ' which are expressed in (42) give rise respectively to the two equations which I have, as Question I. (1869) of the 'Diary' for 1854, proposed for simultaneous solution.

we, in terms of (48), deduce

$$E^2-E-2=0,$$

and find that -1 and 2 are the values of E.

50. The substitution of 2 for E leads to nugatory results, but that of -1 changes (k) into

$$z^2+z+1=0,$$

the roots of which are the unreal cube roots of 1. If we represent these quantities by α and α^2, the condition

$$1+\Sigma\,.\,\alpha_1\beta_2=0$$

points to the relations

$$\alpha_1=\beta_2=\alpha \text{ and } \alpha_2=\beta_1=\alpha^2.$$

51. Denoting the transformed equation in y by

$$y^3+q_1y^2+q_2y+q_3=0,$$

we shall find

$$Y_1Y_2=\pi_2(x_3-\xi)^{-1}=q_3^{-2}\{(a^2-3b)\xi^2+(ab-9c)\xi+(b^2-3ac)\},$$

and, if we determine ξ so as to make $\pi_2(y_3)$ vanish, the relations

$$Y_1 \text{ (or } Y_2)=0,\quad \Sigma\,.\,y=q_1,\quad \Sigma\,.\,y_1y_2=q_2$$

will give y and, consequently, x*.

52. The Method of Symmetric Products enables us to confine the attention to a greatly diminished number of functional

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* The reducing equation of (51) was first obtained by Bezout (Par. *Mém.* for 1762, p. 24). I subsequently (Camb. Math. Journ. for May 1841) was conducted to it by radically different considerations. Mr. Cayley (Camb. and Dub. Math. Journ. for May 1851) has, by giving a coefficient to x^3, generalized my process, and Mr. Rotherham (Eng. Journ. of Ed. for August 1853) has given a solution of a cubic, independently arrived at, but substantially identical with mine. The reducing equation is that which arises from making $\pi_2(y_3)$ vanish.

The intrinsic interest of Mr. Cayley's result is enhanced by its connexion with Dr. Boole's functions θ and θ', and with his own researches on hyperdeterminants. The generalized form of π_2 is the function employed by M. G. Eisenstein (*Crelle*, for 1844, vol. xxvii.), and adverted to by M. C. Hermite (Ib. for 1851, vol. xli.) as a "quadratic form" throwing light on the structure of cubic equations.

It is strange that the function π_2 should have been so much unnoticed. Its coefficients were present, and that too in a suggestive form, to Lagrange (Equations, p. 42) as they had been to La Fontaine (Ib. p. 141). Compare Waring (*Misc. Anal.* p. 22, line 7), Garnier (p. 115), Rutherford (Complete Solution, &c. p. 9), and Mech. Mag. lii. 229, where for M read —M.

Various *direct* solutions, including those of Tartaglia, Ivory, Graves, Waring and Rutherford, and two of my own, are adverted to in my 'Notes.' That of Waring (*Med. Alg.* p. 98) has been erroneously attributed to Laplace. Some assumptions suggested, though not for the same purpose, by Newton (see Fluxions, Lond. 1737, p. 23) may be compared with that of Bezout. Dr. Rutherford's solution, characterized by his usual skill, has a general resemblance to that of Bezout.

values. Thus, for cubics, we employ *two* only in place of the *six* which, when approached from first principles, Lagrange's process involves (Berl. Mem. for 1770, p. 144; Garnier, *Anal. Alg.* 2nd ed., Paris, 1814, p. 174; Young, p. 459). For biquadratics we have *three* instead of the *twenty-four* which (Garnier, pp. 184, 185; Young, pp. 464–6) are successively reduced to twelve and to six, the latter having certain relations one with another. In the theory of quintics we do not take *one hundred and twenty* values as our point of departure, but, starting from *four*, we are conducted to *twenty-four* as involved in the discussion.

53. Cardan's statement of Paciolo's views as to the impossibility of solving *cubics* is dealt with by Cossali (*Origine*, &c., vol. ii. pp. 96, 97). Tschirnhausen[*] seems to have had no doubt of the possibility of solving equations of any degree. Euler[†] did not despair of that of the fifth, notwithstanding obstacles which Waring[‡] deemed insuperable; nor did Bezout[§]. Lagrange (Berl. Mem. for 1770–71) showed the connexion of *all*[‖] solutions then known with the permutations of rational functions of the roots, and pointed out difficulties in the theory of the high equations. Sir W. R. Hamilton has proved that the relations among Lagrange's functions discovered by Badano do not lead to a solution of equations of the fifth degree. The formulæ of Wronski do not, in the opinion of Gergonne and Peacock, differ

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[*] See the first and second pages (204, 205) of Tschirnhausen's paper printed in the Leipsic *Acta Eruditorum* for 1683, and entitled *Methodus auferendi omnes terminos intermedios ex datá equatione.*

[†] See § 20 (pp. 230–1) of Euler's essay *De formis radicum æquationum cuiusque ordinis coniectatio* (Pet. Com. [for 1732–33] vol. vi.); see also § 37 (pp. 92, 93) of his essay *De resolutione æquationum cuiusvis gradus* (Pet. New Com. [for 1762–63] vol. ix.). Garnier (p. 228) points out very clearly the modification to which Euler subjected his original form of root.

[‡] See Waring's *Miscellanea Analytica*, &c. (Camb. 1762), p. 47; see also his *Meditationes Algebraicæ* (Camb. 1770), pp. 120, 121, 122. The researches of Bezout, alluded to at p. v. of the *Præfatio* of the latter work, appear in the Paris *Mémoires* for 1762 and 1765 (not 1764). In explanation of the remainder of the paragraph which contains the allusion, Waring's paper in the Philosophical Transactions for 1779 (pp. 86–104), not forgetting its introductory part, should be referred to. (Et vid. *Misc. An.* p. 44.) Lagrange appreciated Waring's abilities (Berl. Mem. for 1771 [published in 1773], p. 202).

[§] Bezout, *Mémoire sur plusieurs classes d'équations de tous les degrés, qui admettent une Solution algébrique* (Paris *Mémoires* for 1762 [published in 1764], pp. 17–52), arts. (3.), (6.), and (11.); see also his *Mémoire sur la résolution générale des équations de tous les degrés* (Ib. for 1765 [published in 1768], pp. 533–552). Compare p. 549 with Art. (85.) of Lagrange's discussion (Berl. Mem. for 1771, p. 187).

[‖] Lagrange's omission to notice T. Simpson's generalization of Ferrari's solution of biquadratic is scarcely an exception to this remark.

essentially from those of Lagrange. Abel (*Œuvres*, vol. ii. pp. 185–209) never desisted from the efforts commenced by Ruffini (Ib. p. 186). The joint argument of Abel and Sir W. R. Hamilton appears to have been assented to by Murphy (Equations, p. 77), but grave doubts respecting it have been thrown out by Dr. Peacock. Professor J. R. Young* inclines to their conclusion. In favour of the possibility of the solution we may cite Ivory (Equations, *Encycl. Brit.* 7th ed. vol. ix. p. 341), and Mr. G. B. Jerrard. Mr. Bronwin wavers in opinion. Poinsot regarded the question as involved in utter uncertainty (Preface to the 3rd ed. of Lagrange's Equations, p. xvi.). Vandermonde, whose theory of equations Lagrange (Equations, p. 272) considered as being, to some extent, more direct than his own, after some elaborate investigations, states that he is not in a condition to offer even a conjecture upon the possibility of the general solution (Par. *Mém.* for 1771, pp. 365–416; see Art. XXXIV. of his paper).

54. In these papers we have been conducted *à priori* to Lagrange's functions, as those which seem to fulfill the conditions of maximum symmetry. Equations of the fifth degree furnish us, not with a symmetric, but, with what, in the nomenclature of my Fragment on Multiplicity of Values†, may be termed an *epimetric* product.

55. Thus, x being the root of a general quintic, we have

$$\pi_4(x_5)=\sigma(x)+\epsilon(x),$$

where σ is symmetric and ϵ epimetric.

56. Let $s_1, s_2, ., s_6$ be the six values of $\epsilon(x)$. Then, this function being *hyposymmetric*, it may (Phil. Mag. Dec. 1858, p. 448, Art. XX.) be expressed in terms of x_5 and of four quantities t_1, t_2, t_3, t_4 connected with x by the relations

$$t_1=x_1-x_5, \quad t_2=x_2-x_5, \quad t_3=x_3-x_5, \quad t_4=x_4-x_5;$$

or may, in other words, be represented by

$$\epsilon_5(x)+\phi(x_5).$$

57. We have, consequently, six equations of the form (Ibid.)

$$s_\alpha=t_\alpha^2 t_\gamma t_\delta+t_\beta^2 t_\alpha t_\gamma+t_\gamma^2 t_\beta t_\delta+t_\delta^2 t_\alpha t_\beta+\phi(x_5);$$

and, if by means of five of these ϕ and t be eliminated from the remaining one, we obtain

$$\chi(s_1, s_2, ., s_6)=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (l)$$

58. Replacing $\alpha, \beta, \gamma, \delta$ by 1, 2, 3, 4 respectively, we find

$$s_1=t_1t_2t_3t_4(t_1t_2^{-1}+t_2t_4^{-1}+t_3t_1^{-1}+t_4t_3^{-1})+\phi(x_5).$$

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* Equations, pp. 468, 469; Mechanics' Magazine, vol. xlviii. pp. 101, 102; On the General Principles of Analysis, pp. 49–51.

† See Phil. Mag. for December 1853 (S. 4. vol. vi.), pp. 444–8.

59. The quantity between the first brackets is a pure epimetric function, whose structure is

$$a_r(t)=t_r, \quad e_1(t)=t_2^{-1}, \quad e_2(t)=t_4^{-1}, \quad e_3(t)=t_1^{-1}, \quad e_4(t)=t_3^{-1},$$

and for which (Ib. p. 446, Art. XI.)

$$\epsilon\psi(\epsilon)=\epsilon(\epsilon-1)=3\times 2=6,$$

a result which confirms our previous determination of the number of values of $\epsilon(x)$.

60. In the present state of the subject of equations of the fifth degree, I do not desire to have any remark of mine placed in a higher category than that of conjecture. I shall be satisfied if they have a sufficient degree of probability to merit further investigation.

61. It would, then, seem that the epimetric $\epsilon(x)$ is susceptible of finite algebraic evaluation; and this for two reasons.

62. First. The six quantities s are not independent, but are functions of the five quantities x. They do not, therefore, constitute the roots of a *general* equation of the sixth degree, and, being subject to the internal relation (l), their determination is facilitated. (Vide Poinsot, *loc. cit. sup.*)

63. Second. The product $\pi_4(x_5)$ is one of those 'critical' functions which, under their symmetric form, I have defined in (5).

64. We have, in fact,

$$\sigma(x)=c_4C_4+5\,.\,3^{-1}(3p_2^2-7p_1p_3+16p_4)*,$$

and, the unbracketed quantity being critical, (b) can only enter into $\epsilon(x)$ through the bracketed expression, which is rational. Hence the mode in which b is involved in s is such as to free the former quantity from radicality, and to indicate relations favourable to solution, or rather, perhaps, which account for its existence, supposing it to exist.

65. Let us now proceed to an equation in y, connected with

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* Replacing y by x in (5), it may be well, throughout the whole of this discussion, to consider the coefficient of p_n in C_n as unity, and, consequently, c', c_2, c_3, and c_4 as respectively equal to 4, -3, -8 and $-5^3 3^{-1}$. We shall then have the result given in (64), from which it appears that the R of (8) is not equal to U_4.

That result may be readily verified, for in Mr. Jerrard's last notation (Phil. Mag. for May 1853, p. 355) the relation of (38) becomes

$$P_4=\mathfrak{S}4-\mathfrak{S}1\,.\,3+\frac{1}{2}(\mathfrak{S}2^2)+\mathfrak{S}1^2\,.\,2\,.\,3-\frac{1}{24}\mathfrak{S}1^4;$$

and this, by the table at p. 33 of his 'Researches,' is equal to

$$p_1^4-5p_1^2p_2+5p_2^2+5p_1p_3-15p_4,$$

and the substitution of p for B in (41) will afford the means of completing the verification.

that in x by the relation

$$y_r = Q_0 + Q_1 x_r + Q_2 x_r^2 + Q_3 x_r^3 + Q_4 x_r^4,$$

which is substantially the most general transformation that can be adopted, and let us, in the first instance, make Q_0 vanish. The critical nature of π justifies us in this course.

66. We shall find

$$\pi_4(y_5) = Q_1^4\{\sigma(x) + \epsilon(x)\} + Q_1^3 Q_2\{\sigma^{\backprime}(x) + \epsilon^{\backprime}(x)\} + \ldots,$$

there being *six* relations of this form.

67. Although a want of hyposymmetry in $\epsilon^{\backprime}$, $\epsilon^{\backprime\backprime}$, &c. will probably complicate the process by which we arrive at the equations corresponding to (l), such equations exist in all cases. The argument of (63) will apply to $\epsilon^{\backprime}$, $\epsilon^{\backprime\backprime}$, &c.

68. The functions ϵ are, all of them, six-valued. The investigation of their properties will be aided by that of relations like those which follow, and which seem to indicate that epimetric and other unsymmetric expressions admit of systematic discussion.

69. Let Σ be the sign of symmetric and S of epimetric summation; so that, in (39), we may make

$$\Sigma' \,.\, y_1^2(y_2 y_5 + y_3 y_4) = u = S_1(y_1^2 y_2 y_3).$$

70. Change the y's within the brackets into their corresponding squares, and depress those without to the first power, and we have

$$\Sigma' \,.\, y_1(y_2^2 y_5^2 + y_3^2 y_4^2) = S_1(y_1 y_2^2 y_3^2).$$

71. We may take

$$S_1 = S_2\binom{12}{\cdot\,\cdot} = S_3\binom{13}{\cdot\,\cdot} = S_4\binom{14}{\cdot\,\cdot} = S_5\binom{15}{\cdot\,\cdot} = S_6\binom{34}{\cdot\,\cdot}$$

as giving the law of suffixes*.

72. The following relation, in which a singular case is given by $b=2a$, holds,

$$S_r \,.\, x_1^a x_2^b x_3^b = p_n^b S_r \,.\, x_1^{a-b} x_2^{-b} x_3^{-b}.$$

73. We also have, omitting identical suffixes,

$$S \,.\, x_1^3 x_2 x_3 - S \,.\, x_1 x_2^2 x_3^2 - \Sigma \,.\, x S \,.\, x_1^2 x_2 x_3$$
$$+ \Sigma \,.\, x_1^2 x_2^2 x_3 + \Sigma \,.\, x_1^2 x_2 x_3 x_4 = 0,$$

a result which admits of generalization, and connects epimetrics of various forms†.

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* $\binom{14}{25} = \binom{15}{\cdot\,\cdot}$, $\binom{12}{34} = \binom{14}{\cdot\,\cdot} = \binom{23}{\cdot\,\cdot}$, &c.

† The product of $S_1 \,.\, x_1^2 x_2 x_3$ into $S_1 \,.\, x_1 x_2^2 x_3^2$ is the six-valued function

$$\Sigma \,.\, x_1^3 x_2^3 x_3^3 + \Sigma \,.\, x_1^3 x_2^2 x_3^2 x_4 x_5 + \Sigma \,.\, x_1^3 x_2^2 x_3^2 x_4^2 +$$
$$\Sigma' \,.\, x_1^4(x_3^3 x_2 x_4 + x_5^3 x_2 x_3 + x_2^3 x_5 x_4 + x_4^3 x_3 x_5) +$$
$$\Sigma' \,.\, x_1^4(x_3^2 x_2^2 x_5 + x_5^2 x_3^2 x_4 + x_2^2 x_4^2 x_3 + x_4^2 x_5^2 x_2),$$

in which the sequences occur in known cycles. An outline of the history

74. Not to pursue this subject further, let us return to the theory of quintics, and consider the binomial form, in which all the roots are given by

$$y_r = \alpha^{r-1}\tau.$$

75. In this case we have

$$Y_1=0, \quad Y_2=5\tau, \quad Y_3=0, \quad Y_4=0.$$

76. For the form of De Moivre, we have

$$y=\alpha\tau_1+\alpha^4\tau_4,$$

and

$$Y_1=5\tau_4, \quad Y_2=5\tau_1, \quad Y_3=0, \quad Y_4=0.$$

77. For the first form of Euler, we have*

$$y=\alpha\tau_1+\alpha^2\tau_2 \text{ or } y=\alpha\tau_1+\alpha^3\tau_3,$$

and

$$Y_1=0, \quad Y_2=5\tau_1, \quad Y_3=5\tau_2 \text{ or } 0, \quad Y_4=0 \text{ or } 5\tau_3.$$

78. For a form obtainable by modifying an assumption of Bezout, we have†

$$y=\alpha\tau_1+\alpha^2\tau_2+\alpha^3\tau_3,$$

and

$$Y_1=0, \quad Y_2=5\tau_1, \quad Y_3=5\tau_2, \quad Y_4=5\tau_3.$$

79. This form is the first suggested by the Method of Symmetric Products. Its attainment would reduce the problem to the following 'Given a quintic with a known homogeneous linear relation ($Y=0$) existing among its roots, to find those roots ‡.'

80. Thus far I have traced this symmetric (or epimetric) method. The discussion of the problem of (79), of the determinability of $S(x_1^2x_2x_3)$, &c., and of the uses to which the disposable members of the series in Q can be put, belong to the general theory of equations of the fifth degree, under which I hope at a future time to reconsider them. In dealing with the

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of the theory of symmetric functions is given by Lagrange (Equations, p. 190), and tables of their values, up to the tenth degree inclusive, will be found at p. 374 of Vandermonde's *Mémoire sur la résolution des équations* referred to in (53), and published in the year 1774. Mr. Jerrard's 'Researches' and his recent investigations (Phil. Mag. for May and Supp. for June 1853) have given a great extension to the theory.

* Euler, *De resolutione*, &c. (§§ 39, 40, 41 and 42, pp. 94–6 of vol. ix. of the Pet. New Com. published in 1764). The paper *De formis*, &c. was published in 1738.

† By making Bezout's a (Par. *Mém.* for 1765, pp. 543, 544) vanish we should have the form of (78). For each of the forms of (75), (76), (77) and (78), $\pi_4(y_5)$ vanishes.

‡ On this part of the subject see my paper On Equations of the Fifth Degree at pp. 84–6 of the 'Diary' for 1848.

modified Bezoutian form of (78), the assumption

$$y=Q_1x+Q_2x^2$$

will, the functions ϵ being determinable, suffice for the evanescence of Y_1*.

4 Pump Court, Temple,
December 27, 1853.

XX. *On the Œconomy of the Heating or Cooling of Buildings by means of Currents of Air.* By Professor W. Thomson†.

If it be required to introduce a certain quantity of air at a stated temperature higher than that of the atmosphere into a building, it might at first sight appear that the utmost œconomy would be attained if all the heat produced by the combustion of the coals used were communicated to the air; and in fact the greatest œconomy that has yet been aimed at in heating air or any other substance, for any purpose whatever, has had this for its limit. If an engine be employed to pump in air for heating and ventilating a building (as is done in Queen's College, Belfast), all the waste heat of the engine, along with the heat of the fire not used in the engine, may be applied by suitable arrangements to warm the entering current of air; and even the heat actually converted into mechanical effect by the engine, will be reconverted into heat by the friction of the air in the passages, since the overcoming of resistance depending on this friction is the sole work done by the engine. It appears, therefore, that whether the engine be œconomical as a converter of heat into mechanical work or not, there would be perfect œconomy of the heat of the fire if all the heat escaping in any way from the engine, as well as all the residue from the fire, were applied to heating the air pumped in, and if none of this heat were allowed to

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* It will be seen, from my investigation at pp. 45, 46 of the Supplementary Number to vol. iii. of the Mathematician, that the equation

$$x^5+ax^3+bx+\frac{b^2}{3a}=0$$

admits of finite algebraic solution. I have discussed another solvable form of quintic at pp. 76, 77 of the 'Diary' for 1851. Euler has devoted §§ 44, 45, and 46 of his paper *De resolutione*, &c. to the consideration of another solvable form. Bezout has pointed out others (Par. *Mém.* for 1765, p. 544).

† Communicated by the author, having been read before the Glasgow Philosophical Society, November 15, 1852. Mathematical demonstrations of the results stated in this paper are published in the form of the solution of a problem in the Cambridge and Dublin Mathematical Journal, November 1853.

escape by conduction through the air passages. It is not my present object to determine how nearly in practice this degree of œconomy may be approximated to; but to point out how the limit which has hitherto appeared absolute may be surpassed, and a current of warm air at such a temperature as is convenient for heating and ventilating a building may be obtained mechanically, either by water power without any consumption of coals, or by means of a steam-engine driven by a fire burning actually less coals than are capable of generating by their combustion the required heat; and secondly, to show how, with similar mechanical means, currents of cold air, such as might undoubtedly be used with great advantage to health and comfort for cooling houses in tropical countries*, may be produced by motive power requiring (if derived from heat by means of steam-engines) the consumption of less coals perhaps than are used constantly for warming houses in this country.

In the mathematical investigation communicated with this paper, it is shown in the first place, according to the general principles of the dynamical theory of heat, that any substance may be heated thirty degrees (Fahr.) above the atmospheric temperature by means of a properly contrived machine, driven by an agent spending not more than about $\frac{1}{35}$th of energy of the heat thus communicated; and that a corresponding machine, or the same machine worked backwards, may be employed to produce cooling effects, requiring about the same expenditure of energy in working it to cool the same substance through a similar range of temperature. When a body is heated by such means, about $\frac{34}{35}$ths of the heat is drawn from surrounding objects, and $\frac{1}{35}$th is created by the action of the agent; and when a body is cooled by the corresponding process, the whole heat abstracted from it, together with a quantity created by the

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* The mode of action and apparatus proposed for this purpose differs from that proposed originally by Professor Piazzi Smyth for the same purpose, only in the use of an egress cylinder, by which the air is made to do work by its extra pressure and by expansion in passing from the reservoir to the locality where it is wanted, which not only saves a great proportion of the motive power that would be required were the air allowed simply to escape through a passage, regulated by a stopcock or otherwise, but is absolutely essential to the success of the project, as it has been demonstrated by Mr. Joule and the author of this communication, that the cold of expansion would be so nearly compensated by the heat generated by friction, when the air is allowed to rush out without doing work, as to give not a tenth of a degree of cooling effect in apparatus planned for 30 degrees. The use of an egress cylinder has (as the meeting was informed by Mr. Macquorn Rankine) recently been introduced into plans adopted by a committee of the British Association appointed to consider the practicability of Professor Piazzi Smyth's suggestion, with a view to recommending it to government for public buildings in India.

agent, equal to about $\frac{1}{35}$th of this amount, is given out to the surrounding objects.

A very good steam-engine converts about $\frac{1}{10}$th of the heat generated in its furnace into mechanical effect; and consequently, if employed to work a machine of the kind described, might raise a substance thirty degrees above the atmospheric temperature by the expenditure of only $\frac{10}{35}$ths, or $\frac{2}{7}$ths, that is, less than one-third of the coal that would be required to produce the same elevation of temperature with perfect œconomy in a direct process. If a water-wheel were employed, it would produce by means of the proposed machine the stated elevation of temperature, with the expenditure of $\frac{1}{35}$th of the work, which it would have to spend to produce the same heating effect by friction.

The machine by which such effects are to be produced must have the properties of a "perfect thermo-dynamic engine," and in practice would be either like a steam-engine, founded on the evaporation and recondensation of a liquid (perhaps some liquid of which the boiling-point is lower than that of water), or an air-engine of some kind. If the substance is to be heated or cooled by air, it will be convenient to choose this itself as the medium operated on in the machine. For carrying out the proposed object, including the discharge of the air into the locality where it is wanted, the following general plan was given as likely to be found practicable. Two cylinders, each provided with a piston, ports, valves, and expansion gearing, like a high-pressure double-acting steam-engine, are used; one of them to pass air from the atmosphere into a large receiver, and the other to remove air from this receiver and discharge into the locality where it is wanted. The first, or ingress cylinder, and the receiver should be kept with their contents as nearly as possible at the atmospheric temperature, and for this purpose ought to be of good conducting material, as thin as is consistent with the requisite strength, and formed so as to expose as much external surface as possible to the atmosphere, or still better, to a stream of water. The egress cylinder ought to be protected as much as possible from thermal communication with the atmosphere or surrounding objects. According as the air is to be heated or cooled, the pistons and valve gearing must be worked so as to keep the pressure in the receiver below or above that of the atmosphere. If the two cylinders be of equal dimensions, the arrangement when the air is to be heated would be as follows:—The two pistons working at the same rate, air is to be admitted freely from the atmosphere into the ingress cylinder, until a certain fraction of the stroke, depending on the heating effect required, is performed, then the entrance port is to be shut, so that during the remainder of the stroke the air may expand down to the pressure of the receiver, into which, by the opening of another valve, it is to be admitted in the reverse stroke; while the egress cylinder* is to draw air freely from the receiver though the whole of each stroke on one side or the other of its piston; and in the reverse strokes first to compress this air to the atmospheric pressure (and so heat it as required), and then discharge it into a pipe leading to the locality where it is to be used. If it be required to heat the air from 50° to 80° Fahr., the ratio of expansion to the whole stroke in the egress cylinder would be $\frac{18}{100}$, the pressure of the air in the receiver would be $\frac{82}{100}$ of that of the atmosphere (about 2·7 lbs. on the square inch below the atmospheric pressure), and the ratio of compression to the whole stroke in the egress cylinder would be $\frac{13}{100}$. If 1 lb. of air (or about 15½ cubic feet, at the stated temperature of 80°, and the mean atmospheric pressure) be to be delivered per second, the motive power required for working the machine would be ·283 of a horse power, were the action perfect, with no loss of effect, by friction, by loss of expansive power due to cooling in the ingress cylinder, or otherwise. If each cylinder be 4 feet in stroke, and 26·3 inches diameter, the pistons would have to be worked at 30 double strokes per minute.

On the other hand, if it be desired to cool air, either the ingress piston must be worked faster than the other, or the stroke of the other must be diminished, or the ingress cylinder must be larger, or an auxiliary ingress cylinder must be added. The last plan appears to be undoubtedly the best, as it will allow the two principal pistons to be worked stroke for stroke together, and consequently to be carried by one piston-rod, or connected by a simple lever, without the necessity of any variable connecting gearing, whether the machine be used for heating or for cooling air; all that is necessary to adapt it to the latter purpose, besides altering the valve gearing, being to set a small auxiliary piston to work beside the principal ingress cylinder, with which it is to have free communication at each end. If it were required to cool air from 80° to 50° Fahr., the auxiliary cylinder would be required to have its volume $\frac{1}{17}$th of that of each of the principal cylinders; and if its stroke be the same, its diameter would

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* In this case the egress cylinder acts merely as an air-pump, to draw air from the receiver and discharge it into the locality where it is wanted, and the valves required for this purpose might be ordinary self-acting pump-valves. A similar remark applies to the action of the ingress cylinder in the use of the apparatus for producing a cooling effect on the air transmitted, which will then be that of a compressing air-pump to force air from the atmosphere into the receiver. But in order that the same apparatus may be used for the double purpose of heating or cooling as may be required at different seasons, it will be convenient to have the valves of each cylinder worked mechanically, like those of a steam-engine.

therefore be a little less than a quarter of theirs. The valves would have to be altered to give compression in the ingress cylinder during the same fraction of the stroke as that required for expansion when the air is heated through the same range of temperature, and the valves of the egress cylinder would have to give the same proportion of expansion as is given of compression in the other case; and the pressure kept up in the receiver by the action of the pistons thus arranged would be $1\frac{18}{82}$ atmospheres, or about 3·2 lbs. on the square inch above the atmospheric pressure. The principal cylinders being of the same dimensions as those assumed above, and the quantity of air required being the same (1 lb. per second), the pistons would have to be worked at only 24·6 double strokes per minute instead of ·30, and the horse power required would be ·288, instead of as formerly ·283, when the same machine was used for giving a supply of heated air.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 68.]

Dec. 8. 1853.—Colonel Edward Sabine, R.A., V.P. and Treasurer, in the Chair.

THE following paper was read:—"On some of the Products of the Decomposition of Nitrotoluylic Acid." By Henry M. Noad, Ph.D., Lecturer on Chemistry at St. George's Hospital.

The author refers to a former memoir in which he described the mode of preparation and properties of two new organic acids, the analogues of benzoic and nitrobenzoic acids in the toluyl or immediately succeeding series, and to which the names of toluylic ($C_{16}\,H_8\,O_{44}$) and nitrotoluylic ($C_{16}\,H_7\,(NO_4)\,O_4$) acids were consequently given.

In the present paper he resumes the study of the action of nitric acid on cymol ($C_{20}\,H_{14}$), and describes first some unsuccessful attempts to procure from that oil the substitution compound $C_{80}\left\{\begin{matrix}H_{13}\\NO_4\end{matrix}\right\}$, from which, by the action of reducing agents, he had hoped to procure a new organic base homologous with aniline, toluidine, &c. He then investigates the products of the decomposition of his new nitrogen acid. He describes the preparation and properties of nitrotoluylamide $C_{16}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}O_4,\ NH_2$, and having succeeded, though by a rather tedious process, in obtaining this substance in some quantity, he studies the action of reducing agents on it. By the action of hydrosulphate of ammonia upon an aqueous solution of the amide, a crystalline substance was procured, which analysis proved to be homologous with the *carbamide—carbanilide* of Hofmann, and with the *anilo-urea* of Chancel. The study of its properties showed that it must be considered as the analogue of the latter, that it is the true urea of the toluyl series, being a well-defined organic base, forming a series of crystalline salts, of which the nitrate and oxalate were qualitatively examined. A synoptical view of these ureas is given, showing their relation with the urea type.

By the action of a boiling solution of caustic potash on toluyl urea ($C_2 H_5 (C_{14} H_7) N_2 O_2$), a new acid was procured, the analysis of which showed that it has three homologues in the benzoyl series, viz. *anthranilic acid, benzamic acid*, and *carbanilic acid*, all of which are represented by the formula $C_{14} H_7 NO_4$, the composition of the new acid being expressed by the formula ($C_{16} H_9 NO_4$).

The limited quantity of this acid at the author's disposal, and the great difficulty with which it was procured, did not enable him to decide positively with which of the above acids it corresponds, though its mode of formation would render it probable that it is the true analogue of *carbanilic acid*. The determination of this question is of some interest, inasmuch as should it prove to correspond to *anthranilic acid*, a road might through it be opened for the introduction of a series of new substances at present entirely wanting, namely, the proper homologues of *salicylic* acid and its derivatives. The author proposes to return to this subject, and he gives, in conclusion, a synoptical view of those corresponding members of the *benzoyl* and *toluyl* groups which in the present and former paper he has established.

Dec. 15.—Thomas Bell, Esq., V.P., in the Chair.

1. "On the Acidity, Sweetness, and Strength of Wine, Beer and Spirits." By H. Bence Jones, M.D., F.R.S.

(1.) The acidity of the different liquids was determined by means of a standard solution of caustic soda. The quantity of liquid neutralized was always equal in bulk to 1000 grs. of water at 60° F.

The acidity in different—

Sherrics	varied from	1·95 grs.	to	2·85 grs.	of caustic soda.
Madeira	,,	2·70	,,	3·60	,,
Port	,,	2·10	,,	2·55	,,
Claret	,,	2·55	,,	3·45	,,
Burgundy	,,	2·55	,,	4·05	,,
Champagne	,,	2·40	,,	3·15	,,
Rhine wine	,,	3·15	,,	3·60	,,
Moselle	,,	2·85	,,	4·50	,,
Brandy	,,	0·15	,,	0·60	,,
Rum	,,	0·15	,,	0·30	,,
Geneva	,,	0·07			,,
Whisky	,,	0·07			,,
Bitter ale	,,	0·90	,,	1·65	,,
Porter	,,	1·80	,,	2·10	,,
Stout	,,	1·35	,,	2·25	,,
Cider	,,	1·85	,,	3·90	,,

Hence the order in which these wines may be arranged, beginning with the least acid, is Sherry, Port, Champagne, Claret, Madeira, Burgundy, Rhine, Moselle.

(2.) The sugar was determined by means of Soleil's saccharometer, which at least gives the lowest limit to the amount of sugar.

The sweetness in different—

Sherries	varied from	4 grs.	to	18 grs.	in the ounce.
Madeira	„	6	„	20	„
Champagne	„	6	„	28	„
Port	„	16	„	34	„
Malmsy	„	56	„	66	„
Tokay	„	74			„
Samos	„	88			„
Paxarette	„	94			„

Claret, Burgundy, Rhine, and Moselle contained no sugar.

Hence the order in which these wines may be arranged, beginning with the driest, is—

Claret Burgundy Rhine Moselle
Sherry
Madeira
Champagne
Port
Malmsy
Tokay
Samos
Paxarette.

In a dietetic view, assuming that the sugar becomes acid, then the mean results as to the acidity of the different fluids examined, beginning with the least acid, is—

Geneva Whisky
Rum
Brandy
Claret
Burgundy
Rhine wine
Moselle
Sherry
Madeira
Champagne
Cider
Port
Porter
Stout
Malmsy Madeira
Ale
Tokay.

(3.) The alcohol was determined by means of the alcoholometer of M. Geisler of Bonn.

The strength of different samples of—

Port	varied from	20·7	per cent. to	23·2	per cent. by measure.
Sherry	„	15·4	„	24·7	„
Madeira	„	19·0	„	19·7	„
Marsala	„	19·9	„	21·1	„
Claret	„	9·1	„	11·1	„
Burgundy	„	10·1	„	13·2	„

Rhine wine	varied from	9·5	per cent. to	13·0	per cent. by measure.
Moselle	,,	8·7	,,	9·4	,,
Champagne	,,	14·1	,,	14·8	,,
Brandy	,,	50·4	,,	53·8	,,
Rum	,,	72·0	,,	77·1	,,
Geneva	,,	49·4			,,
Whisky	,,	59·3			,,
Cider	,,	5·4	,,	7·5	,,
Bitter ale	,,	6·6	,,	12·3	,,
Porter	,,	6·5	,,	7·0	,,
Stout	,,	6·5	,,	7·9	,,

The Burgundy and Claret have less alcohol than was found by Mr. Brande forty years ago in the wines he examined. The Sherry is now stronger, the Port is not so strong, the Marsala is weaker, the Rhine wine is the same strength, the Brandy is as strong as formerly; the Rum is nearly half as strong again; the Porter is stronger, and the Stout rather stronger than formerly.

Lastly, the specific gravity of each liquid was taken. As this however chiefly depends on the amount of alcohol and sugar present, and as these were directly determined, the specific gravity may be taken as a distant control on the amount of sugar present.

Thus, in those wines in which the amount of alcohol was the same, the specific gravity was found to vary with the amount of sugar found by the saccharometer.

The results of the analysis of each sample of wine, &c. is given in a series of tables, which do not admit of any abstract.

2. "On a New Method of propagating Plants." By E. J. Lowe, Esq., F.R.A.S., F.G.S. &c.

The author states that the experiment of a new method of propagating plants has been so successful, that he has taken the liberty of forwarding to the Royal Society this short paper upon the subject, for the guidance of those who are interested in the advance of horticulture.

It had occurred to him, that if a cutting of a plant were sealed at the base, so as to exclude the moisture of the soil from ascending the stem in injurious quantities, the method of striking cuttings of most species of plants would not be so precarious a process as at present; and accordingly some collodion was obtained in order to make the experiment.

With respect to this new process, he states, that immediately upon the cutting being severed from the parent stem, the collodion was applied to the wound, and then left a few seconds to dry, after which the cuttings were potted in the ordinary manner.

To test the value of this new process more effectually, duplicates of all the species experimented upon were at the same time similarly planted, without the collodion being applied to them.

Experiments were carried on in two different ways; one batch of cuttings being placed on a hot-bed, whilst a second batch was planted in the open ground, without even the protection of glass.

First Batch.—All of which were placed on a hot-bed on the 1st of September, and examined on the 1st of October:—

Stove Plants.

Number of cuttings with collodion applied.	Name of plant.	Number of cuttings which took root.	Number of cuttings without the application of collodion.	Number of cuttings which took root.
1	Ixora coccinea	1	1	0
1	Tacsonia manicata	1	1	1
3	Franciscea Hopeana	3	3	0
3	Franciscea Pohliana	3	3	0
2	Gloxinia Maria van Houtte	0	2	1
2	Begonia incarnata	2	2	1
8	Achimenes patens	7	8	6
2	Hoya bella	2	2	1
2	Rondeletia speciosa	2	2	1
2	Allamanda nerifolia	2	2	1

Greenhouse Plants.

Number of cuttings with collodion applied.	Name of plant.	Number of cuttings which took root.	Number of cuttings without the application of collodion.	Number of cuttings which took root.
6	Boronia serrulata	5	6	0
3	Polygala dalmaisiana	1	3	0
6	Polygala grandiflora	3	6	2
6	Verbena luna	6	6	6
1	Chorozema cordata	1	1	0
1	Epacris pallida	0	1	0
2	Leschenaultia formosa	2	2	1
1	Swainsonia astragalifolia	1	1	0
1	Swainsonia galegifolia	0	1	0
2	Abelia rupestris	2	2	0
4	Plectranthus concolor, picta	2	4	2

Second Batch.—Planted in the open ground on the 1st of September, and examined on the 1st of October:—

Hardy Plants.

Number of cuttings with collodion applied.	Name of plant.	Number of cuttings which took root.	Number of cuttings without the application of collodion.	Number of cuttings which took root.
12	Garrya elliptica	5	12	1
12	Erica vagana	7	12	4
18	Bupleurum longifolium	6	18	0
12	Laurus fœtens	10	12	7
6	Rose, Souvenir de la Malmaison	4	6	3
12	Taxus baccata, golden-leaved var.	8	12	4

	Total number of cuttings to which collodion was applied.	Number of cuttings which took root.	Total number of cuttings without the application of collodion.	Number of cuttings which took root.
First batch	59	46	59	23
Second batch	72	40	72	19

The experiment, the author considers, speaks for itself. Notwithstanding the season being too far advanced for the full benefit of the process to be thoroughly observed, still twice as many cuttings took root treated by the new method as had rooted by the old. The mortality in the open ground was increased by slugs having eaten off above the soil some of the cuttings; those thus damaged were examined after they had been in the ground a month, and it was found that the collodion was quite as sound as when first applied. It would therefore appear that the collodion seals the wound of the cutting, and protects it from the fatal effects of damp, until roots are prepared to force through the covering of gun-cotton. It is further stated, that the application of this solution has been found to be exceedingly beneficial in the pruning of such plants as *Euphorbia speciosa, Impatiens latifolia, Impatiens latifolia-alba, Hoya bella, Hoya imperialis*, &c., the cut branches being prevented from bleeding.

It is the author's intention next spring to follow out this experiment, in budding and grafting, as he considers that it will also be useful in this branch of horticulture.

Gutta-percha, dissolved in æther, was in some instances substituted to heal the wounds caused by pruning; yet owing to this solution not drying as rapidly as collodion, the first, and sometimes the second application was not sufficient.

The effect of these solutions upon cut flowers was very marked. Two branches were gathered as nearly alike as possible; to the flower-stalks of the one, collodion was applied. These flowers were placed in vases filled with water; those coated over with collodion began to fade in thirty-six hours, and many were quite dead in three days; whilst the flowers merely placed in water in the ordinary manner remained fresh and healthy. Those that faded soonest were *Reseda odorata* and *Tropæolum majus*, and those which were least affected were *Tagetes erecta* and *Senecio erubescens*.

Dec. 22, 1852.—Thomas Graham, Esq., V.P., in the Chair.

"On certain Properties of Square Numbers and other Quadratic Forms, with a Table by which all the odd numbers up to 9211 may be resolved into not exceeding four square numbers." By Sir Frederick Pollock, F.R.S. &c.

In examining the properties of the triangular numbers 0, 1, 3, 6, 10, &c., the author observed that every triangular number was composed of four triangular numbers, viz. three times a triangular number plus the one above it or below it; and he found that all the natural numbers in the interval between any *two* consecutive triangular numbers might be composed of four triangular numbers having the sum of their roots, or rather of the indices of their distances from the first term of the series constant, viz. the sum of the indices of the four triangular numbers which compose the first triangular number of the *two*.

Not being at that time aware of any law by which the series that fills up the intervals could be continued, he subsequently turned his attention to the square numbers as apparently presenting a greater variety of theorems. He observed that if any four square numbers, a^2, b^2, c^2, d^2, have their roots such, that, by making one or more positive and the rest negative, the sum of the roots may be equal to 1, then if the root or roots of which the sum is 1 less be each of them increased by 1, and the others or other be each diminished by 1, the sum of the squares of the roots thus increased or diminished will be $a^2+b^2+c^2+d^2+2$. This he found to be only a particular case of more general theorems.

Theorem A.—If the sum of the roots $a, b, c, d=2n-1$, and n be added to each of the less set, and subtracted from each of the greater, the *increase* in the sum of the squares of the new roots will be $2n$.

Theorem B.—If the sum of the roots $=2n+1$, and n be added to each of the less set and subtracted from each of the greater, the *diminution* in the sum of the squares of the new roots will be $2n$.

By means of these he shows—

Theorem C.—If any four squares be assumed which compose an odd number, these may be diminished till four squares are attained the sum of whose roots will equal 1.

By applying the first of these theorems to four roots, the sum of whose squares is an odd number, the author deduces, in a tabular form, the squares (four or less) which compose the odd numbers from 21 to 87; and remarks that there does not appear to be any limit to this mode of continuing to increase the sum of four squares by 2 each time. As, however, although this may render it probable that every odd number is composed of four, three, or two squares, it falls very short of a mathematical proof, unless it can be shown that the series can be continued by some inherent property belonging to it, he proceeds to examine the series, in order to ascertain what approach can be made to such a proof.

Adopting a method similar to that observed in the triangular numbers, the author forms what he terms the series of *Gradation*, by means of which the series of squares which compose the odd numbers may be advanced by steps or stages which increase regularly and obey a certain law, and at which this series is, as it were, commenced anew from roots of the form $n, n, n, n+1$, or $n-1, n, n, n$; the form of the sum of the squares of these roots being $4n^2 \pm 2n+1$, and the series of gradation 1, 3, 7, 13, 21, 31, 43, 57, 73, &c. On this principle a more extended table of the odd numbers resolved into squares (not exceeding four in number) is constructed. On this the author remarks that it is complete to the 96th odd number (191), that is, there are in this table square numbers which will form the odd numbers in succession, whose roots (some $+$, some $-$) $=1$; and therefore the expression $4n^2 \pm 2n+1$ up to $4n^2 \pm 2n+191$ may be divided into 4 or 3 squares, whatever be the value of n. The numbers in the table exactly fill up the interval between

$$47^2,\ 47^2,\ 47^2,\ 48^2=8931,$$

and

$$47^2,\ 48^2,\ 48^2,\ 48^2=9121,$$

whose difference $=190$, the difference between the first term and the last term in the table: it will therefore resolve into square numbers any odd number up to $9121+190=9211$.

With reference to the mode in which the intervals in the table may be filled up, the author states the following general theorems relating to the sums of three square numbers, by means of which the roots may be varied, and yet the sum of the squares remain the same.

Theorem D.—If any three terms of an arithmetical series, and omitting the 4th term, the three following terms be arranged thus,

$$\begin{array}{ccc} a+b, & a+2b, & a+6b, \\ a\ , & a+4b, & a+5b, \end{array}$$

the sum of the squares of each set of terms will be the same.

Theorem E.—If four numbers in arithmetical progression be placed thus,

$$\begin{array}{cc} a\ , & a+2b, \\ a+4b, & a+6b, \end{array}$$

and the sum of the 1st and 4th be divided into two parts whose difference shall be four times the arithmetic ratio, as $a+7b-(a-b)$, and the parts be placed with the terms, the greater with the less, and the less with the greater, thus,

$$\begin{array}{ccc} a\ , & a+2b, & a+7b, \\ a-b, & a+4b, & a+6b, \end{array}$$

the sum of the squares will be equal.

Theorem F.—Let two numbers which differ by $2n$ be placed thus:

$$\begin{array}{cc} a+n, & a+n, \\ a-n, & a-n, \end{array}$$

then if the sum of the four ($2a$) be divided so as to have the same difference ($2n$), and the parts be placed, the less with the greater, and the greater with the less, thus,

$$\begin{array}{ccc} a+n, & a+n, & 2a-n, \\ a-n, & a-n, & 2a+n, \end{array}$$

the sum of the squares shall be the same.

The author illustrates this part of the subject by deducing six forms of roots whose squares $=197$.

XXII. *Intelligence and Miscellaneous Articles.*

ON THE OCCURRENCE OF NICKEL AND COBALT IN SOME MINERAL SPRINGS, AND ON A METHOD FOR THEIR ISOLATION. BY OSSIAN HENRY.

MAZADE some time since stated that he had found in the chalybeate springs of Neyrac and its ochreous deposits, titanium, glucina, cobalt and nickel. In consequence of this statement, the author has tested several chalybeate waters for nickel and cobalt, and ascertained the presence of these two metals by the following process:—

To a large quantity of the water a slight excess of carbonate of soda is added; the fluid is then allowed to stand in the air until the iron is completely converted into oxide and deposited. The deposit of the spring itself may also be taken. These deposits are dissolved in muriatic acid, and evaporated to a certain degree for the removal of the glucina, titanium, sand and silica, when the solution generally contains only alumina, lime, magnesia, iron, manganese, nickel and cobalt.

To this solution carbonate of soda is again added until a precipitate is obtained, which is agitated in the air with a large quantity of water. It is then washed; and when it has become oxidized in the air, it is brought into contact with water which has been saturated with carbonic acid in an apparatus fitted for the purpose. This dissolves only the carbonates of nickel and cobalt, upon which sulphuretted hydrogen is passed through the solution, or hydrosulphuret of sodium added to it.

By this means the nickel and cobalt are separated, generally very slowly, in the form of sulphurets. The sulphurets are dissolved in nitromuriatic acid, precipitated by carbonate of soda, and treated in the manner proposed by Laugier for the detection of cobalt and nickel.—*Journ. de Pharm. et de Chim.*, 3rd ser. xxiv. p. 305.

ON THE INFLUENCE OF PRESSURE UPON THE FORMATION OF CHEMICAL COMPOUNDS. BY PROF. WÖHLER.

Hydrate of chlorine, which is immediately decomposed at ordinary temperatures and at the pressure of the atmosphere, remains for the most part undecomposed even at a summer heat when enclosed in hermetically-sealed tubes, under the pressure of the chlorine which is set free from a portion of it which undergoes decomposition. In such a tube, when plunged into water of a temperature of 86°–104° F., the hydrate of chlorine is decomposed, but becomes partially restored on its return to the ordinary temperature.

This decomposition is not prevented by the exclusion of the air under the pressure of chlorine gas of the tension of the atmosphere; under these circumstances the decomposition takes place as usual at any temperature above 32° F.

A tube in which hydrate of chlorine was hermetically sealed was exposed to the sun for a whole summer's day. It became fluid, but did not indicate decomposition of the water by the setting free of oxygen.

The author had already observed, that during the preparation of liquid sulphuretted hydrogen from sulphuret of hydrogen in hermetically-sealed tubes, colourless crystals are sometimes formed, which immediately disappear on the tube being opened.

In two tubes, in which sulphur, but no liquid sulphuretted hydrogen had separated, these crystals were formed in large quantity; they did not however make their appearance in a third tube, in which the persulphuret of hydrogen was enclosed together with concentrated muriatic acid. Hence the author concludes, that the crystalline compound, which is no doubt a hydrate of sulphuretted hydrogen, must be produced when a small quantity of water is enclosed with hydrate free from acid; the water then combines with the sulphuretted hydrogen under the pressure of the condensing sulphuretted hydrogen (17 atmospheres). Under this pressure it is permanent at ordinary temperatures. If the tube be heated in water to 86° F., the compound dissolves, and rapidly becomes fluid, returning to a solid state again on being cooled to the ordinary temperature.—*Ann. der Chem. und Pharm.*, lxxxv. p. 374.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1853.

Chiswick.—December 1. Overcast: fine: clear and frosty. 2—4. Dense fog. 5. Foggy: slight rain. 6, 7. Foggy: overcast. 8. Foggy: fine. 9. Fine: slight rain. 10, 11. Cloudy. 12. Hazy: uniformly overcast. 13. Foggy. 14. Overcast. 15. Snowing: clear and frosty. 16. Overcast: clear, with bright sun: severe frost at night. 17. Severe frost: overcast: frosty. 18. Clear: overcast. 19. Uniformly overcast: clear and fine: cloudy. 20. Hazy. 21. Densely clouded: boisterous at night. 22. Overcast. 23. Cloudy: clear. 24. Cloudy: clear and frosty. 25. Frosty: hazy: clear, with sharp frost at night. 26. Frosty: fine: frosty. 27. Clear and frosty: hazy: clear and frosty. 28. Clear and frosty: fine: severe frost at night. 29. Severe frost: clear and fine: frosty. 30. Snow-shower: frosty. 31. Very clear and frosty: partially overcast: sharp frost at night.

Mean temperature of the month	32°·49
Mean temperature of Dec. 1852	46 ·56
Mean temperature of Dec. for the last twenty-eight years	39 ·64
Average amount of rain in Dec.	1·5 inch.

Boston.—Dec. 1. Cloudy: rain A.M. 2. Fine. 3. Cloudy. 4, 5. Foggy. 6. Cloudy: rain A.M. 7. Foggy. 8. Fine. 9. Fine: rain P.M. 10. Fine: rain early A.M. 11—13. Cloudy. 14. Fine. 15. Snow and rain A.M. and P.M. 16. Cloudy: snow A.M. 17. Cloudy. 18, 19. Fine. 20. Cloudy: rain P.M. 21. Cloudy: rain A.M. and P.M. 22, 23. Cloudy: rain A.M. 24. Cloudy. 25. Foggy. 26. Foggy: rain and snow P.M. 27. Fine: snow P.M. 28. Snow A.M. and P.M. 29. Fine. 30. Fine: snow A.M. 31. Fine.

Sandwick Manse, Orkney.—Dec. 1. Hazy A.M.: rain P.M. 2. Damp A.M.: drops P.M. 3. Bright A.M.: cloudy, aurora P.M. 4. Clear A.M. and P.M. 5. Bright A.M.: clear, aurora P.M. 6. Clear A.M.: clear, aurora S. P.M. 7. Bright A.M.: frost, showers P.M. 8. Showers A.M. and P.M. 9. Showers A.M.: clear P.M. 10. Bright A.M.: fine P.M. 11. Frost A.M.: damp P.M. 12. Damp A.M.: showers P.M. 13. Bright A.M.: clear, A.M. lunar halo P.M. 14. Damp and P.M. 15. Cloudy A.M.: clear P.M. 16. Damp A.M.: clear P.M. 17. Clear, frost A.M. and P.M. 18. Bright, frost A.M.: clear, frost P.M. 19. Showers A.M.: clear P.M. 20. Showers A.M.: clear, frost P.M. 21. Clear, frost A.M.: clear, frost, aurora P.M. 22. Cloudy, frost A.M.: showers P.M. 23. Showers A.M.: showers, aurora P.M. 24. Cloudy A.M. and P.M. 25. Cloudy A.M.: sleet-showers P.M. 26. Hail-showers A.M.: snow-showers P.M. 27. Snow drift A.M. and P.M. 28. Snow-showers A.M.: snow, cloudy P.M. 29. Thaw: showers A.M. and P.M. 30, 31. Snow-drift A.M.: snow-showers P.M.

Mean temperature of Dec. for twenty-six previous years	41°·18
Mean temperature of this month	38 ·97
Mean temperature of Dec. 1852	40 ·74
Average quantity of rain in Dec. for thirteen previous years	4·13 inches.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1853. Dec.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·096	29·971	29·77	29·82	29·87	48	24	48	43	44½	se.	s.	......	....	·07	·03
2.	29·937	29·899	29·70	29·92	29·82	42	21	37	45	47	e.	sse.	sw.	....	·03	·22
3.	29·959	29·951	29·70	29·86	29·81	41	36	32	40½	44	e.	calm	ssw.	·02	....	·04
4.	29·964	29·904	29·72	29·73	29·70	43	35	37	42½	45	e.	sw.	sw.	....	....	....
5.	29·907	29·892	29·65	29·76	29·91	46	34	36·5	44	39½	e.	calm	ne.	·01	....	....
6.	30·060	29·930	29·72	30·11	30·13	45	36	40	39	34	ne.	nw.	......	....	·03	·12
☽ 7.	30·099	30·088	29·83	29·94	29·85	44	28	35	37	44	ne.	nw.	w.	....	....	....
8.	30·236	30·164	29·90	30·00	30·16	43	29	31	41	45	n.	calm	sw.	·01	....	·09
9.	30·336	30·328	30·10	30·31	30·35	51	34	31	46	47	ne.	nnw.	sw.	·02	....	·09
10.	30·283	30·119	30·04	30·35	30·35	41	30	37·5	45	37	ne.	nnw.	s.	....	·08	....
11.	30·070	29·995	29·82	30·26	30·17	34	29	34·5	38	42	se.	se.	sw.	....	....	....
12.	29·934	29·782	29·70	29·96	29·84	35	28	32	41	40	se.	s.	se.	....	....	·06
13.	29·579	29·333	29·35	29·71	29·76	46	38	36·5	38	34	e.	e.	ne.	....	....	·01
14.	29·299	29·197	29·04	29·73	29·79	43	25	40	43	43	ne.	ene.	se.	....	....	·09
○ 15.	29·244	29·196	29·00	29·82	29·79	38	27	32	41	40	ne.	ne.	nne.	·20	....	....
16.	29·526	29·324	29·10	29·67	29·71	33	7	32	42	41	n.	calm	s.	....	·32	·04
17.	29·698	29·647	29·36	29·75	29·75	36	22	33	34	34	n.	s.	ne.	....	....	....
18.	29·661	29·546	29·40	29·69	29·71	34	26	32	38	37	se.	sse.	e.	....	....	....
19.	29·605	29·483	29·35	29·80	30·04	35	28	32	38½	36	e.	e.	e.	....	....	·14
20.	29·843	29·724	29·54	30·17	30·34	37	34	35	40½	40	ne.	ese.	se.	....	....	·11
21.	29·995	29·969	29·78	30·38	30·48	36	33	36	34	36	ne.	ne.	......	....	·03	....
22.	30·005	29·075	29·92	30·47	30·28	37	29	36	37	44	ne.	ene.	nw.	....	·14	....
☾ 23.	29·921	29·793	29·63	30·17	30·30	41	29	38	40	41	n.	n.	ne.	·04	·04	·15
24.	30·227	30·065	29·90	30·32	30·34	38	24	36	43½	44	ne.	ene.	n.	....	....	·05
25.	30·201	30·199	29·94	30·25	30·13	36	14	34	43½	42	ne.	calm	nw.	....	....	....
26.	30·128	30·013	29·87	30·21	30·04	36	19	33	31½	32	se.	calm	nw.	....	....	·07
27.	29·972	29·873	29·70	30·08	30·17	35	22	22	28	31	n.	calm	n.	....	·02	....
28.	30·083	29·902	29·75	30·36	30·37	34	8	27	30½	32	n.	n.	nnw.	....	·12	·02
29.	30·291	30·161	30·08	29·93	29·67	32	15	14	40	37½	ne.	n.	nw.	....	·20	·34
● 30.	29·670	29·550	29·37	29·59	29·62	37	22	33	31	29	w.	nnw.	n.	....	....	....
31.	29·679	29·501	29·40	29·46	29·39	33	19	22	27	31	nw.	nnw.	nw.	....	·09	....
Mean.	29·919	29·792	29·65	29·986	29·988	39·03	25·96	33·4	38·80	39·14				0·30	1·17	1·67

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XXIII. *Reports on the Progress of the Physical Sciences.* *By* Dr. HIRST.

On the Application of Magnetic Induction to the determination of the Magnetic Inclination. *By* Prof. W. WEBER.

[Poggendorff's *Annalen*, vol. xc. p. 209.]

[With a Plate.]

IT is known that by the investigations of Gauss, observations on terrestrial magnetism have been raised from mere comparisons to actual measurements, which in point of precision are comparable to astronomical ones. The system of measurement here referred to has, however, been employed hitherto in observations on the horizontal elements of the earth's magnetism alone, the horizontal intensity and declination. It is true that these horizontal elements form in themselves a complete system, from which, as Gauss has shown, a complete determination of the earth's magnetism may be obtained without the aid of observations on the inclination. But it is also true that the attainment of this end would be facilitated by means of observations on the inclination, and hence the latter must not be disregarded. At present, however, such observations by no means possess that classic character which those on the horizontal elements have attained through the use of the magnetometer. The causes of their deficiency in this respect are essentially two: *First.* The action of the vertical magnetic force is not observed by itself, but in combination with the action of gravity on our ponderable needles. A separation of the effects due to each of the two forces is certainly possible, by demagnetizing the needle and thus obtaining several combinations of the two; but the determination of the vertical magnetic force thus obtained can never possess that degree of precision which a determination of the horizontal magnetic force now enjoys, inasmuch as the latter is deduced from effects produced by this force alone, and uninfluenced by any other forces. *Secondly*. The *magnetometric arrangements*, on which the possibility of *accurate* observation depends, cannot be applied to instruments for measuring the inclination, on account of the influence of friction which is inseparable from such instruments; and as long as this exists, the application of more accurate methods of observation would simply lead to deceptive results. These defects are so intimately connected with the conditions under which the action of the vertical component of the earth's magnetism on the magnetism of other bodies must be observed, that they cannot possibly be avoided; the most ingenious arrangement and combination of observations may serve to diminish the prejudicial consequences of these defects, but never to remove them.

Soon after Faraday's discovery of magnetic induction, Gauss drew attention to the possibility of investigating the earth's magnetism by observing its action on the electricity of bodies; and, acting on this suggestion, Weber, in 1837, constructed a new instrument for measuring the magnetic inclination, which he named the "*Induction-Inclinatorium*[*]." This instrument was completely free from the first of the above-mentioned defects, that is to say, no interference of the force of gravity took place, and hence no demagnetization was necessary to separate its effects from those of the vertical magnetic force. The second defect, however, was not removed; magnetometric arrangements could not yet be combined with this instrument so as to obtain accurate results, hence this new method was of no avail.

At length Weber has succeeded in removing this second defect also, and has discovered an arrangement whereby the *electric action* of the vertical as well as the horizontal component of the earth's magnetism may be measured, by the unifilar magnetometer, with a degree of precision of which hitherto the magnetometric determination of the horizontal elements alone could boast. For the practical solution of the problem, an accurate and complete investigation of the earth's magnetism, this extended application of the magnetometer to measurements of the inclination is of some importance.

A simple and general notion of Weber's new instrument may be obtained by a reference to the well-known construction of the *electro-magnetic telegraphs* now in use. These, it will be remembered, are of two kinds. In one the electric currents made use of

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[*] *Resultate aus den Beob. des magn. Vereins im Jahre* 1837, S. 81.

to give the signs, proceed from a *galvanic battery*; in the other they are induced in the electric conductor by means of *magnetic force*. Weber has constructed a telegraph of the latter kind, where the *earth's magnetic force* induces electric currents strong enough to be used for giving signs. For such induction, either the horizontal or the vertical component of the earth's magnetism may be used. Let us suppose the telegraph to be a so-called needle-telegraph (such as are used in England), in which the current gives the sign by passing through a helix and deflecting a needle suspended in the same from the magnetic meridian. It is evident that, by an exact comparison of the deflections, we may determine the *ratio of the intensities* of the electric currents respectively induced by the *vertical* and *horizontal* component of the earth's magnetic force; this ratio is the *tangent of the required inclination.* In constructing such a telegraph, therefore, it is necessary to obtain deflections of the needle which are capable of *accurate measurement*, and at the same time to exclude all *disturbing influences.* For the former purpose it is clear, that, in place of the common magnetic needle of the galvanometer, a magnetometer must be used. To exclude all disturbing influences it is necessary,—1, that no *commutation* of the conducting wires be employed in order to increase the deflection; 2, that the amount of deflection be independent of the velocity of the inductor's motion. For, in the first place, the circuit must remain *unchanged* if the observed deflections are to furnish an exact comparison of the forces by which they were produced; and in the second place, these forces must not be subject to variation, as would be the case if they were dependent upon the velocity of the inductor's motion. It will be shown that both conditions are fulfilled when the inductor's whole motion is limited to an *induction stroke*, capable of momentary execution, and strong enough to produce in the magnetometer a deflection capable of the finest measurement. Such a simple induction stroke consists in a semi-rotation of the inductor; no commutation in the connexion of the inductor wire with that of the galvanometer is here necessary, because during such a semi-rotation no change in the direction of the induced current takes place.

We proceed next, by help of the figures in Plate II., to give a description of this new instrument, to which Weber has given the name *induction-magnetometer*.

A transverse section of the inductor is shown at A, fig 1. The circumference of the cylinder on which the wire is coiled is 718·3 millims., its diameter is $aa = a'a'$, and its breadth $aa' = 120·05$ millims. The copper wire around this cylinder is 542296 millims. long, and has a mean sectional area of 4·145 square millims. It was covered first with wool and then with gutta percha, and so coiled round the cylinder as to form 605 coils arranged in eighteen strata. The circumference of the last encircling stratum is 1078·6 millims.

This induction coil is surrounded by a strong wooden frame, *bbbb*; at its extremity is a wooden pulley C, with two circular grooves, in which lie the wires connecting the inductor and galvanometer. To this frame two strong brass pivots, *d*, *d'*, are fixed. The two pivots are exactly cylindrical and of equal diameters, and rest on the Y-formed supports *e*, *e'*, fixed to the beam of the frame B, B'. Fig. 1 shows the induction coil in the position in which it can rotate around a horizontal axis. To test the horizontality of the axis of rotation, a level, *c*, is used; its setting is provided with two Y-formed feet, by means of which it can be placed on the two pivots *d*, *d'* which form the axis of rotation. At the end of one of the pivots, *d'*, is a brass ball, in which a conical-shaped cavity at *f* is turned. This ball serves to displace the induction coil, so that from being horizontal, its axis of rotation is brought into a vertical position. For instance, the level *c* being removed, the induction coil A can be raised by the pivot *d* when the sphere on the pivot *d'* sinks into a spherical cavity cut at *g* into the beam B'. When the axis of rotation is thus brought into a vertical position, as shown by the dotted lines in the figure, the raised pivot *d* fits into a Y-formed support *h*, fixed to the beam D of the framework, and is held there by the pressure of a spring; this pressure can be regulated by a screw. In this position the point of a screw, passing vertically through the beam B', fits into the conical-shaped cavity of the ball on the pivot *d'*; thus the induction coil can be raised so as to rotate freely on this point by merely turning the screw head at *k* under the beam. To test the verticality of the axis of rotation, a level is placed upon the level carrier *ll* which fits on to the end of the pivot *d*.

Fig. 2 represents the induction coil in its horizontal position, as seen from above. The inductor's axis (*i. e.* the axis of the cylinder on which the wire is coiled) is here perpendicular to the plane of the figure. Perpendicular to the frame to which the pivots *d*, *d'* are fixed, is a second frame *mm*, surrounding the induction coil, which carries two stout brass pins at *n*, *n*. On the rotation of the induction coil these pins strike against screws fixed into the beam of the framework, and thus at the end of each induction stroke the inductor's axis is held vertical. One of these pins; which during the rotation moves in the upper semicircle, strikes on these screws from above; the other, which moves in the lower semicircle, from below. These screws can be somewhat displaced in a vertical direction, and their position once found they can be fixed there. This position is determined by seeking the arc through which the induction coil must be moved, so that the induced currents may annihilate one another. The position of the induction coil which corresponds to the middle of this arc is the position of the brass pins at the moment when they should strike the screws. The two ends of the wire of the inductor are led to the clamps *p*, *p* and there fastened. From these clamps the connecting wires pass over the pulley C to the galvanometer.

Fig. 3 is a section of the multiplication coil together with the magnetic needle. The circumference of the cylindrical surface around which the wire is coiled is 1027·4 millims., its diameter is $aa = a'a'$, and its breadth $aa' = 225{\cdot}6$ millims. Around this cylindrical surface two copper wires, covered first with spun wool and then with gutta percha, were coiled side by side. Each wire is 992656 millims. long, and the mean sectional area of both wires taken together is 8·1682 square millims. Each of these wires forms 779 coils, arranged in twenty-five strata. The circumference of the last stratum is 1523·4 millims.

In the middle of the coil the magnetic needle NS is suspended on a prismatic bar, which can be moved along a cross-bar *bb* and screwed fast to it. Above the coil is another cross-bar, parallel to the lower one, and suspended from a fine wire; the two are connected at their extremities by two thin vertical connecting rods which pass outside the multiplication coil. To the upper cross-bar a mirror and torsion circle is affixed. Lastly, the space enclosed by the coil, in which the needle oscillates, is closed on both sides by covers, *cccc*.

Fig. 4 is a vertical section of the galvanometer in the direction of the magnetic meridian. Here the direction of the upper cross-bar *d*, as well as that of the lower cross-bar *b*, is perpendicular to the plane of the figure. Both are connected by thin vertical bars attached to their extremities before and behind the multiplier. The mirror *e* is connected with the cross-bar *d* by means of a Y-formed hook, and above the mirror, similarly connected, is the torsion circle hung on a thin wire. The case which encloses *d*, *e*, and *f* projects a little both before and behind the multiplier, and on both sides reaches down to the covers *cccc*, fig. 3; so that the connected space occupied by *d*, *e*, *f* and the coil is completely enclosed, with the exception of an opening through which the thin suspending wire passes. A parallel plate of glass is placed in the side of this case immediately before the mirror, so that the image of the scale may be observed in the mirror by a telescope. Lastly, the frame of the multiplier has a hexagonal form, and the undermost side of this hexagon rests upon a stone pedestal A.

Fig. 5 is a ground plan of the building, showing the relative situations of the inductor C, the galvanometer B, and the telescope and scale A. The distance between the galvanometer and inductor is so great, that the influence of the galvanometer needle on the inductor vanishes in comparison to that of the earth's magnetism. The inductor stands on three foot-screws *a*, *b*, *c*, which are used to give the axis of the inductor a perfectly *vertical* or *horizontal* position, according as the *horizontal* or *vertical* component of the earth's magnetic force is used for induction.

It was so contrived, that during the observations the person at the telescope could set the inductor in rotation by means of his foot; in the following observations, however, the induction strokes were made by an assistant. The two wires from the galvanometer and inductor passed over the telescope-stand close to the observer, where, by means of a clamp, the galvanometer wire could be made to return without passing over the inductor, or a continuous wire made to pass over both the galvanometer and inductor. As these contrivances, however, can be modified to suit the conveniences of the observatory and habits of the observer, a fuller description is unnecessary.

It should be here remarked, that, for inclination measurements alone, a galvanometer with so great a helix as the one above described is by no means necessary; it is only because the mean radius of the coils ought not to be less than 230 millimetres that so great a mass of copper (60 kilogrammes) was used for this galvanometer. But the great width of the coil, as well as its circular form, was not chosen on account of the inclination measurements to be made therewith, but because it was intended that the instrument should also serve for fixed standard measurements of the normal resistance in galvanic circuits, similar to the instrument described in the *Abhandlungen über elektrodynamische Maasbestimmungen* (Leipzig, 1852), ii., with which, however, such measurements of resistance could be made but once. Such measurements have actually been made with the present instrument; but for the results, as well as for the necessary theoretical elucidations, we must refer the reader to the original memoir.

For correct and convenient measurements of the inclination, which at present interests us, Weber remarks that all the dimensions of the galvanometer may without injury be diminished one-half; so that an eighth part of the wire (about 10 kilogrammes) would be sufficient. The accuracy of the inclination measurements would even be increased by this means, for the deflection of the needle would be thereby increased in the ratio of $1 : \sqrt{2}$, and might perhaps be doubled, if, instead of the circular form, the coil had an appropriate elliptical form, the axis of the needle coinciding with the major axis of the ellipse. Should the deviation of the needle be thus too great to be conveniently measured by the scale, only half the amount of wire (about 10 kilogrammes) need be employed for the inductor. This diminution of the mass of the inductor would have another advantage, the rotation of the inductor coil could be made with greater ease.

In former memoirs* Weber has shown the application of the induction stroke to delicate measurements, and has developed the rules according to which the deviations of the magnetic needle, produced by the induced electric currents, are *multiplied* in the galvanometer, as well as subjected the observations to mathematical calculation. It is not necessary to repeat these rules here; it will be sufficient to give the observations made in accordance with them, from which it may be decided whether, in this instrument for the measurement of the inclination, the necessary delicacy is combined with the intended simplification of labour, as it should be in order to be an essential improvement upon the best known inclination instruments.

The magnetometer and galvanometer being prepared, the position of the inductor was so regulated that,—1, its own axis (*i. e.* the axis of the cylindrical surface around which the wire was coiled) was horizontal, and parallel to the magnetic meridian; 2, that the axis around which it could be made to revolve 180° backwards and forwards, was perfectly *vertical*; this could be tested within a few seconds by means of the level. The magnetometer needle in the galvanometer was at the commencement in a state of perfect rest in the magnetic meridian. This could always be secured by the damping force of the galvanometer, which was increased by conneeting the two wires from the galvanometer to the inductor by means of a copper clamp, the galvanometer thus forming a circuit for itself. When the damping force was thus increased, two succeeding arcs of oscillation of the needle were in the ratio of 100 : 71; hence after thirty oscillations, or, as the time of an oscillation was eighteen seconds, in nine minutes, the amplitude was diminished 29,000 times by this damping force; that is to say, the needle was in a state of rest, no matter how great the original arcs of oscillation might have been. Before commencing observations, the clamp used to increase the damping power was removed, and then the *first* (positive) induction stroke given with the inductor. The needle being thus set in motion, left the magnetic meridian, and in nine seconds (half its

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* *Resultate aus den Beob. des magn. Ver. im Jahre* 1838 (Leipzig, 1839), and *Abhandlungen über elektrodynamische Maasbestimmungen* (Leipzig, Weidmann'sche *Buchhandlung*, 1852), 2te Abhandlung, Beilage C. S. 341*ff*.

time of oscillation) attained its maximum deflection to the east (or west). This was noted by the observer, after having marked down the needle's position when at rest. The needle now returned, and after nine seconds passed through the magnetic meridian. At this moment followed the *second* (negative) induction stroke, caused by turning the inductor back through 180°, whereby the needle was accelerated in its returning motion. After nine seconds, the needle, thus accelerated, attained its maximum western (or eastern) deflection, which was again noted, and so forth. No. 0 in the following table shows the original position of rest, Nos. 1 to 16 show the sixteen succeeding elongations observed as above described. These observations lasted scarcely five minutes.

Table I.

Göttingen, 1852. Aug. 3, $0^h\ 20^m$—$0^h\ 25^m$.

No.	Position of the needle in divisions of the scale at the observed elongation.	No.	Position of the needle in divisions of the scale at the observed elongation.
0	1236·2	9	1030·8
1	1195·0	10	1450·9
2	1313·8	11	1014·7
3	1131·1	12	1464·0
4	1367·4	13	1003·1
5	1085·6	14	1473·4
6	1405·3	15	996·9
7	1053·2	16	1479·8
8	1432·0		

After the last observation, the clamp for increasing the damping power of the galvanometer was immediately closed; and during the damping action the position of the inductor was changed, so that,—1, its own axis stood vertical; 2, the axis around which it was revolved 180° backwards and forwards was exactly *horizontal* and parallel to the magnetic meridian. This was effected by means of the level described in fig. 1. After removing the clamp for damping, the second series of observations was made in the same manner as the first. This series, however, was made four times at intervals of ten minutes, or as soon as the needle had again come to rest. After these four series of observations, during which the inductor's position was unchanged, followed the last series, in which the inductor had the same position as at first. The following table gives a summary of the six series of observations, which, including the time requisite for damping and changing the inductor's position, required $1^h\ 20^m$ to complete. The several series are denoted by the letters A, B, C, D, E and F.

Table II.

Göttingen, 1852. Aug. 3, $0^h\,20^m$—$1^h\,40^m$.

	Position of the needle at the observed elongations.					
No.	A.	B.	C.	D.	E.	F.
0	1236·2	1235·1	1234·4	1233·8	1233·7	1233·3
1	1195·0	1335·2	1334·9	1334·9	1333·8	1192·2
2	1313·8	1052·7	1052·0	1051·0	1050·1	1310·7
3	1131·1	1488·9	1488·3	1488·0	1487·0	1128·4
4	1367·4	922·8	921·9	920·9	920·1	1364·4
5	1085·6	1596·8	1597·5	1597·1	1595·9	1083·0
6	1405·3	830·4	829·7	828·7	827·9	1402·8
7	1053·2	1674·6	1674·6	1673·8	1672·6	1050·9
8	1432·0	764·9	764·0	762·9	762·0	1429·8
9	1030·8	1728·8	1728·9	1728·0	1726·9	1028·1
10	1450·9	717·9	717·0	715·9	715·2	1448·9
11	1014·7	1767·8	1768·0	1767·0	1766·1	1012·1
12	1464·0	684·3	684·0	683·0	682·1	1462·2
13	1003·1	1795·4	1795·4	1794·6	1793·9	1000·6
14	1473·4	661·0	660·6	659·8	659·0	1471·8
15	996·9	1815·0	1814·9	1814·2	1813·6	993·2
16	1479·8	644·3	644·2	643·0	642·4	1478·2

If in each series of observations the number corresponding to the position of rest opposite No. 0 be subtracted from the elongations opposite Nos. 1 to 16, the corresponding distances of elongation will be obtained, as shown in the following table. The signs, which are alternately positive and negative, are here neglected.

Table III.

Göttingen, 1852. Aug. 3, $0^h\,20^m$—$1^h\,40^m$.

	Distances of elongation.					
No.	A.	B.	C.	D.	E.	F.
1	41·2	100·1	100·5	101·1	100·1	41·1
2	77·6	182·4	182·4	182·8	183·6	77·4
3	105·1	253·8	253·9	254·2	253·3	104·9
4	131·2	312·3	312·5	312·9	313·6	131·1
5	150·6	361·7	363·1	363·3	362·2	150·3
6	169·1	404·7	404·7	405·1	405·8	169·5
7	183·0	439·5	440·2	440·0	438·9	182·4
8	195·8	470·2	470·4	470·9	471·7	196·5
9	205·4	493·7	494·5	494·2	493·2	205·2
10	214·7	517·2	517·4	517·9	518·5	215·6
11	221·5	532·7	533·6	533·2	532·4	221·2
12	227·8	550·8	550·4	550·8	551·6	228·9
13	233·1	560·3	561·0	560·8	560·2	232·7
14	237·2	574·1	573·8	574·0	574·7	238·5
15	239·3	579·9	580·5	580·4	579·9	240·1
16	243·6	590·8	590·2	590·8	591·3	244·9

In each of these series it will be seen that the distance of elongation increases, not uniformly, but according to a known law; it approaches a certain limit, from which the damping power, or the *decrementum logarithmicum*, may be easily calculated. Let a represent this limit, and $\log \frac{1}{\theta}$ the *decrementum logarithmicum*, then the distance of elongation No. $n = x_n = a(1-\theta^n)$, hence

$$a = \frac{x_n\, x_n}{2x_n - x_{2n}},$$

and

$$decrem.\ log. = \log\frac{1}{\theta} = \frac{1}{n}\log\frac{x_n}{x_{2n} - x_n}.$$

According to this formula, the *decrementum logarithmicum* calculated from the above observations is

$$\log\frac{1}{\theta} = 0{\cdot}075, \text{ nearly};$$

and the approximate limit for the series of observations A, F $=261{\cdot}7$; for the series of observations B, C, D, E$=627$.

The following corrections must be applied to the above values of the distances of elongation:—

1. The correction due to the influence of the oscillation of the needle at the commencement of the observations.

If $\pm e$ be the elongation of the needle immediately preceding the first induction stroke, then to the next following $\mp e\theta$ must be added, to the second $\pm e\theta^2$, to the third $\mp e\theta^3$. For the above observations, where $e=0$, this correction is unnecessary.

2. According to the laws of catoptrics, the observed distances of elongation are proportional to the tangents of double the angles of deflection; they must be reduced to values proportional to the sines of half the angles of elongation, *i. e.* to the deflecting forces. For this purpose the horizontal distance from the mirror to the scale must be known; in the present case this distance $r=3685$ divisions of the scale. If x be the observed distance of an elongation, then its reduced value is

$$= x - \frac{11}{32}\cdot\frac{x^3}{rr}.$$

After this reduction, a determination of the *tangent of the inclination* may be obtained from each observation in the columns B, C, D, E by dividing it by the mean of the observations in the columns A, F. The following table shows the results:—

Table IV.

No.	B.	C.	D.	E.
1	67° 40′ 30″	67° 45′ 27″	67° 52′ 37″	67° 40′ 39″
2	66 57 26	66 57 26	67 0 9	67 5 33
3	67 29 37	67 30 2	67 31 28	67 27 9
4	67 10 31	67 11 19	67 12 53	67 15 38
5	67 21 15	67 25 59	67 26 39	67 22 57
6	67 14 13	67 14 13	67 15 26	67 17 33
7	67 20 51	67 22 48	67 22 15	67 19 11
8	67 15 30	67 16 2	67 17 20	67 19 26
9	67 18 59	67 20 58	67 20 14	67 17 44
10	67 17 55	67 18 24	67 19 35	67 21 0
11	67 18 48	67 20 53	67 22 46	67 18 6
12	67 21 28	67 20 34	67 21 28	67 23 15
13	67 17 27	67 19 0	67 18 33	67 17 14
14	67 21 18	67 20 40	67 21 6	67 22 36
15	67 24 15	67 25 31	67 25 19	67 24 15
16	67 23 18	67 22 3	67 23 18	67 24 21

The most correct value of the tangent of the inclination is obtained from each of the series B, C, D, E by dividing its sum by the mean of the sums of the two series A, F. For this purpose the corrections already mentioned, instead of being made on each observation, may be made on their sums; viz.—

1. Instead of adding $\pm e\theta^n$ to each observation, $\pm e\frac{\theta(1-\theta^{16})}{1-\theta}$ may be added to the sum of the sixteen observations.

2. Instead of deducting $\frac{11}{32}\cdot\frac{x^3}{rr}$ from each observation, $\frac{11}{32}\cdot\frac{px^3}{rr}$ may be deducted from the sum of all the sixteen observations, where

$$p=\frac{16-3\theta\frac{1-\theta^{16}}{1-\theta}+3\theta^2\frac{1-\theta^{32}}{1-\theta^2}-\theta^3\frac{1-\theta^{48}}{1-\theta^3}}{\left(16-\theta\frac{1-\theta^{16}}{1-\theta}\right)^3}.$$

By this means the calculation will be simplified, inasmuch as the value of p is constant. If we represent the sums of the series A, B, C, D, E, F by the same letters, then after making this correction, we obtain

A=2873·05 B=6881·27
F=2877·15 C=6886·10
D=6889·36
E=6887·98

In this manner the following four determinations of the inclination I are obtained from the four series of observations B, C, D, E:—

Tan I.		I.
$\frac{6881{\cdot}27}{2875{\cdot}10}$	. . . .	67° 19′ 26″
$\frac{6886{\cdot}10}{2875{\cdot}10}$	. . . .	67 20 18
$\frac{6889{\cdot}36}{2875{\cdot}10}$	. . . .	67 20 53
$\frac{6887{\cdot}98}{2875{\cdot}10}$	. . . .	67 20 38

From the 2nd to the 12th of August observations were made and calculated in a similar manner four times a day, at 1^h, 7^h, 13^h, 19^h, by Prof. Weber, Dr. v. Quintus Icilius, M. Eisenlohr and Mr. Hansen. All these gentlemen were accustomed to observations with magnetometers, and the agreement in their several results proved that no other practice is required beyond that which is necessary for all other magnetometric observations.

The mean inclination deduced from all these observations was for

Aug. 7, 1852, 67° 19′ 43″.

For purposes of comparison it was found necessary to apply a small correction to this result, on account of a slight influence exerted by the magnetometer upon the inductor; the data for this correction were easy to determine, and after applying it, the inclination for

Aug. 7, 1852, was 67° 18′ 38″.

This result is next compared with others previously made in Göttingen by Humboldt, Forbes and Gauss. Their results were the following:—

Dec.	1805	. . .	69° 29′ 0″	Humboldt.
Sept.	1826	. . .	68 29 26	
July 1,	1837	. . .	67 47 0	Forbes.
...	...	. . .	67 53 30	
Oct. 8,	1841	. . .	67 42 43	Gauss.
June 21,	1842	. . .	67 39 39	

Hence, from Gauss's observations, the mean annual decrease of the inclination in Göttingen for the ten years between 1842 and 1852

$$=2' \, 9'';$$

and from those of Humboldt and Forbes, this mean decrease for the thirty-six years between 1806 and 1842

$$=3' \; 2''\cdot 3.$$

This result agrees with Hansteen's investigations on the observations made at other places in Europe, by showing that the yearly decrease of inclination in Göttingen has become less. If t be the number of the year, and I the inclination, we may set

$$I=67^\circ \; 23' \; 43''-122''\cdot 29(t-1850)+1''\cdot 337(t-1850)^2,$$

whence we obtain the following comparison of the observed with the calculated values :—

		Observed.	Calculated.	Difference.
Dec.	1805	69° 29′ 0″	69° 36′ 43″	−7′ 43″
Sept.	1826	68 29 26	68 23 17	+6 9
July 1,	1837	67 47 0	67 52 41	−5 41
...	...	67 53 30	67 52 41	+0 49
Oct. 8,	1841	67 42 43	67 42 0	+0 43
June 21,	1842	67 39 39	67 40 18	−0 39
Aug. 7,	1852	67 18 38	67 18 34	+0 4

According to this, the annual decrease of inclination at Göttingen during twenty-two or twenty-three years has diminished one minute; in round numbers, therefore, the annual decrease—

in 1828	was	3	minutes
... 1850	...	2	...
... 1873	will be	1	...
... 1895	...	0	...

Hence, in all probability, the inclination in Göttingen will continue to decrease until the year 1895, when it will attain a minimum of

$$66^\circ \; 37' \; 7'',$$

and from that time it will increase.

From this investigation, Weber concludes that the inclination instruments hitherto employed, being more portable, will continue to be used on journeys in preference to his own; but that in fixed observatories, where magnetometers are kept for other purposes, it will be found advantageous to apply induction to *observations on the inclination with the magnetometer*, inasmuch as a great simplification of labour and a uniformity in the treatment of all the three elements of the earth's magnetism is thereby attainable.

XXIV. *Additional Observations on the Anticlinal Line of the London and Hampshire Basins.* By P. J. Martin, *Esq., F.G.S.*

[Continued from vol. ii. p. 477.]

To Richard Taylor, Esq.

Dear Sir,

EVERYTHING I have ventured to publish on the geology of the Weald denudation having appeared in your Journal, I am naturally desirous you should give place to some short observations as an appendix to the series of papers you were so kind as to print for me two years ago.

In doing this, I shall avoid as much as possible all discussion of a controversial character; and I leave the appropriation of any discovery, or the merit of priority of observation, to those who think such matters worth contending for.

What I have now to say would have been advanced long ago, but I was given to understand that it was probable a meeting of the British Association would shortly take place at Brighton. In that hope I waited, prepared to discuss on the spot some of the most interesting questions bearing on the subject, and on the highly illustrative phænomena of that locality.

The workers in the field of inquiry offered by the anticlinal line of the London and Hampshire basins, and especially of that part of it which relates to the Weald denudation, seem pretty well agreed as to structural arrangement. Of the agents that have been at work in effecting the changes here exhibited, and of their *modus agendi*, there is still great difference of opinion; and of the phænomena of Drift, now engaging so much public attention, there exists, and perhaps will continue to exist for some time to come, much contrariety of sentiment. It is to this point I shall chiefly address myself. But as a preliminary step, I will first briefly recapitulate the arguments, or rather enumerate the natural appearances in favour of the necessary relation of the various phænomena of denudation, and with which I consider every kind of drift to be most intimately connected.

Let any man look at the left-hand corner of Mr. Greenough's map, and consider the surface arrangement of the immense area comprised in the elevation of this great anticlinal, and the still greater area of the countries which must necessarily have been fashioned by it. No man who does not take this periscopic view, and who cannot comprehend the phænomena here exhibited in their *totality*, is qualified justly to interpret any part of them. The key to the whole is in the conception of the contemporaneity of upheaval and denudation; not a piecemeal elevation of one subordinate anticlinal and another subordinate anticlinal, the excavation of one or of many valleys, or the accumulation of this or that bed of diluvium, to be accounted for by certain local arrangements. With this key in his mind, let the observer first consider the phænomena of anticlinal and synclinal contortions, the flexures and puckerings of strata struggling upwards under the restraint of lateral pressure, or rather lateral resistance and incumbency, such foldings and contortions most apparent in the great argillaceous formation of the Wealden *. Then the system of cross-fracture, first pointed out by myself and afterwards mathematically accounted for by Mr. Hopkins, as the result of one great and simultaneous act of upheaval. Thirdly, of the uniformity of watershed and drainage, first glanced at by Conybeare and Phillips, and afterwards enlarged on by myself, and insisted on as an evidence of the unity and entirety of at least the first great upburst of the inferior beds. Fourthly, of the uniform and irrefragable evidence of contemporaneous violent aqueous erosion to be observed over some hundreds of square miles of country; the prominence of stony, and the recession of more friable and otherwise more destructible surfaces; and especially the downcast and laceration of the escarpments of the outcropping secondary stony strata. And lastly, in the arrangement and the dispersion of drift along the axis of elevation, within the eroded valleys on either side, and within the basins or great synclinal depressions, of which the anticlinal forms the line of separation.

Without this key, and this comprehensive view of the general arrangement of the country from the chalk of the midland counties to the chalk of the Isle of Wight and the Boulonnais, the strictly geological features of this area remain a mystery, and all attempts at forming a just rationale of local phænomena must fail.

It is with this habit of viewing the south-east of England as the scene of great disturbance, as I before said, in its *totality*, that the phænomena of drift can only be studied with effect. It has been frequently asserted that the Weald has no drift. I have endeavoured, in my former communications to this Journal, to bring evidence in contradiction of this assertion. Sir Roderick Murchison, in his disquisition on "flint drift," published in the Journal of the Geological Society, soon after the promulgation of my opinions, lays much stress on the bare state of the rocky Hastings-sand districts; and although he finds some marks of a local accumulation of detrital matters, and describes with great accuracy the vicinal remarkable gravel-bed at Hever, he seems still to think the absence of flint-drift in the centre of the Weald strong presumptive evidence of an exceptional case. There is no

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* It is probably owing to the impressible nature of these beds that they are made the nucleus of the denudation, and the axis of elevation,—the point of least resistance, existing thereby.

drift nor detrital matter, save modern *alluvium* (I must still retain the use of that word), that is not, in antiquarian phrase, of the date "tempore denudationis;" and it appears to me that the absence of great accumulations of the relics of the higher strata on the lofty ridges and well-washed slopes of the "Forest Ridge" is just what might be expected. In my cursory description of the zones of drift as they mantle round the nucleus of the Weald, I have endeavoured to show, that exactly as we recede from the vicinity of the higher beds, their relics become correspondingly rare. The rounded gravel of the eocene, except here and there a stray pebble, as a general rule, disappears on the thoroughly denuded chalk downs. Entire flint nodules which abound there become rare, and are succeeded by angular and fragmentary in the greensand country. Here, in my subcretaceous zone, we find large accumulations of flint, mixed up with fragmentary chert and sandstone, with now and then a chalk pebble or an eocene pebble, all entangled in deep drifts of disintegrated sand-rock. Descending the escarpment of the lower greensand, we find in the place of flint and gravel, a thin sprinkling of the durable ironstone, and fragments of the chert, of the stratum next above, with concretional iron-rag or bog-iron of the Weald clay country, with here and there a small *trainée* (as Sir Roderick says) of angular and fuscous flints dyed by the iron imbibed from the soil. Of these *trainées* the bed at Hever approaches hard on the Hastings-sand country and the centre of the denudation; and I had the pleasure of showing another to Sir Roderick at Shipley, four miles south of Horsham. And since that time I have discovered another at Slinfold, three miles west of that town, in the remarkable longitudinal valley which skirts, if it does not lie in, the exact axis of the great anticlinal of the Forest Ridge. Here, near the Roman gate on the road between Stroud and Rudgwick, a bed of diluvium is to be found, with chert and ironstone, and a fair sprinkling of brown flints.

Agreeably to the foregoing rule of the gradual disappearance of the relics of the superior strata, the elevated rocks of Tunbridge Wells and the rest of the central districts, as before said, exhibit only here and there, scattered over the surface or turned up by the plough, water-worn fragments of sandstone impressed with Cyprides, fragments of the marble of the Weald clay, and finally the detritus of the Hastings sands and clays themselves. To satisfy himself that these are accumulated with all the characters of diluvium, let the observer take the Ordnance Map, mark the deep longitudinal valleys of the Rother and its tributaries, examine particularly the eastern slopes and spurs of the beautiful and picturesque eminences that there abound, the railway cuttings that traverse them, and the more fertile hop-gardens of that country, and he will be satisfied that drift of the real diluvial character is not wanting to this, the barest part of the Wealden.

The presence of these detrital masses, and the absence of the remains of the higher strata on these elevated ridges, ought to be received as corroborative proof of the community of character, and the one and indivisible nature of these drifts of denudation. It is to be remembered that thousands of feet of greensand, chalk, and tertiary beds have been cast off from these elevated ridges, and it is only wonderful that any fragments of their ruins are still to be found in their vicinity. I venture to reassert, then, my conviction, that a large and comprehensive view of the arrangement of drift in concentric zones is agreeable to nature and fact, from the patches of eocene to be found on the Hampshire chalk down to the lowest beds of Hastings and Winchelsea; with such exceptions only as tend to confirm the rule.

Of the absence of the usual mammal bones in the Wealden zone of drift, I infer but little, but that little is of the affirmative character. All the upper beds being removed, the greater the chance that with them would be removed the remains of the animals which died a natural death on the original surface of the country, or of the surrounding countries, or which perished in the catastrophe we contemplate.

Much remains to be said in reference to the local arrangements of drift; the involved and tumultuous admixture of sands and clays, the manufacture of brick-earths and loams on the spot on which they are found, and the *quasi* stratified beds* of sand and gravel, and of the boulder clays which have been swept into the great synclinals on either side; but it is most convenient to confine our attention at present to the central and most simple, as well as the most illustrative part of our subject. Of the fossil or diluvial wood, and trunks of trees *in situ* amongst the gravel beds of Surrey, below the chalk, spoken of by Sir R. Murchison (on the authority of Mr. Austen) in attestation of "a true terrestrial surface" after the commencement of the denuding æra, I cannot say that they do not exist; but I have looked into many gravel-pits there, and in the corresponding districts under the South Downs, and I have never seen any wood in drift which was not of the most modern de-

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* I believe that many of these diluvial beds contain organic remains derived from the latest tertiaries, broken up *tempore denudationis*. Such remains are supposed to be of pleiocene date, as assimilating to, or even identical with, existing species.

Specimens of this sort were shown to me by Mr. Mackie from the drift at Folkstone, and it was from some such semi-stratified deposit near Bognor, I suspect, that Lady Murchison took the shells spoken of by Sir Roderick. (Journ. Geol. Soc. vol. vii. p. 371.)

scription, such as would till lately have been called mere "alluvium." Carbonized trunks of trees are to be found in all the bogs and swamps, especially in the alluvium of the river-courses, as noticed in my early memoir on Western Sussex. On the banks of the Mole and the Wey, and of their affluents, I doubt not such prostrate and uprooted trees may be detected;—they are post-diluvial.

It remains that I should say something more of the well-known raised beach at Brighton, and of the parallel case at Sangatte, described by Mr. Prestwich. I have treated these, and I still consider them as belonging to the eocene æra, brought into view at these places by sea-section. I cannot but entertain the same opinion till similar appearances are pointed out at the same level *below the chalk*, and within the areas of denudation on either side of the channel. At the same time I am not prepared to insist that there were not intermissions in the operation of denuding forces; and that during some such lull these beaches may have been formed. I have not said, as Sir Roderick seems to think, that the denudation of the Weald was the work of one transient great act of elevation and flood; but of this I am quite assured, that the first great upburst gave the character of these forces, as evinced by the arrangements of anticlinals, the fissures which determined the disposition of valleys, the watershed and drainage. I believe also there is evidence to prove a long-continued season of oscillation, and perhaps of unremitting violent aqueous erosion contemporaneous therewith. This was succeeded by a sudden, and not a gradual retirement of these angry waters, and the season of tranquillity which has continued up to this time. I hope to be pardoned for insisting thus strongly on the unity of action, and the grandeur of proportions of these great changes, when it is considered that Buckland and some other of our best authorities have held similar sentiments; and that in working out the details of these and contemporaneous operations in their entirety, we bring forward the best tests of the bold generalizations of Elie de Beaumont.

The season seems to be at hand when we shall find less difficulty in readmitting the agency of catastrophic action into the elements of change. The able expositor of the geological phænomena of Patagonia has said, that any sudden operation which would account for the spread of the drift materials of that part of South America would disturb the relations of half a hemisphere. And why not? Are we not on the eve of believing that at no very remote period we were without any Gulf-stream, and that its establishment (and how could it be otherwise than suddenly established?) put an end to the "glacial period," and gave a temperate climate to western Europe? Are the *oser* beds of

Sweden accounted for on any known principles of slow accumulation? and can the drift which is so extensively spread over central and northern Europe be considered solely as the droppings of icebergs? "Verily," as it has been wittily said, "palaces are not built with a teaspoon, nor hospitals endowed with a pinch of snuff!"

Pulborough, Feb. 13, 1854.

XXV. *On the Date of the Discovery of the Optical Properties of Chrysammate of Potash. In a Letter from* Sir David Brewster *to* Professor Stokes*.

My dear Mr. Stokes,

PROFESSOR FISCHER has put into my hands this afternoon a copy of your interesting paper "On the Metallic Reflexion exhibited by certain Non-metallic Bodies," in which I observe the following passage:—

"In mentioning my own observations on safflower-red, Herapathite, &c., nothing is further from my wish than to neglect the priority of those to whom priority belongs. M. Haidinger had several years before discovered the phænomenon of the reflexion of differently coloured oppositely polarized pencils, which Sir David Brewster *shortly afterwards*, and independently, discovered in the case of chrysammate of potash."

My experiments were made in the end of 1842 and the beginning of 1843, with crystals of chrysammate of potash sent to me on the 2nd of December 1842 by Mr. Schunck, along with other seven new substances, aloetinate of potash, &c. Mr. Schunck directed my attention to the *green metallic lustre* of the salt, and to the redness of its solution, which I instantly examined.

I found it very difficult to work with such imperfect crystals, and I delayed publishing the imperfect results which I had obtained in the expectation of some time or other getting better crystals.

On the 1st of March, 1846, I read to the Literary and Philosophical Society of St. Andrew's all the observations inserted in my journal of experiments; and not being able to attend the Southampton meeting of the British Association, I made an abstract of what I had read at St. Andrew's, which is the paper you refer to as published in the Report of the Association.

In 1842 and 1843 I examined also various other substances, but I have not published any of the results which I obtained.

I enclose Mr. Schunck's letter, which will show you that I

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* Communicated by Professor Stokes.

had requested him in 1842 to send me for examination some of those remarkable substances.

Believe me to be,
Ever most truly yours,
D. Brewster.

St. Leonard's College, St. Andrew's,
January 20, 1854.

Note.—In using the expression "discovered" I followed the common practice of making a discovery date from its publication; it would have been more exact to have said "announced the discovery of."

Sir David Brewster's announcement of the discovery was referred by me to the date of the meeting of the British Association at Southampton, because I was not aware of the previous communication made to the Literary and Philosophical Society of St. Andrew's, nor could I well have been. I shall therefore, I hope, be held excused for not having alluded to the latter.

G. G. Stokes.

XXVI. *On the Mechanical Action of Heat.* *By* William John Macquorn Rankine, *C.E., F.R.SS. Lond. and Edinb. &c.*

[Continued from p. 122.]

Section IV. *Of the Mechanical Action of Steam, treated as a Perfect Gas, and the Power of the Steam-engine.*

(21.) In the present limited state of our experimental knowledge of the density of steam at pressures differing much from that of the atmosphere, it is desirable to ascertain whether any material error is likely to arise from treating it as a perfect gas. For this purpose the ratio of the volume of steam at 100° Centigrade, under the pressure of one atmosphere, to that of the water which produces it at 4°·1 Centigrade, as calculated theoretically on the supposition of steam being a perfect gas, is to be compared with the actual ratio*.

The weight of one volume of water at 4°·1 Centigrade being taken as unity, that of half a volume of oxygen at 0° Centigrade, under the pressure of one atmosphere, according to the experiments of M. Regnault, is

0·000714900
That of one volume of hydrogen . 0·000089578 . (36)
The sum being . . . . 0·000804478

The reciprocal of this sum being multiplied by $\frac{374·6}{274·6} = 1·364166$

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* The actual ratio is only known approximately.

(the ratio of dilatation of a perfect gas from 0° to 100° Centigrade), the result gives for the volume of steam of saturation at 100° Centigrade as compared with that of water at

$$4^{\circ}{\cdot}1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad 1695{\cdot}72, \quad . \quad . \quad . \quad (37)$$

and for its density . 0·00058972.

The agreement of those results with the known volume and density of steam is sufficiently close to show, that at pressures less than one atmosphere it may be regarded as a gas sensibly perfect; from which it may be concluded, that in the absence of more precise data, the errors arising from treating it as a perfect gas at such higher pressures as occur in practice, will not be of much importance*.

Representing, then, by v the volume of unity of weight of water at 4°·1 Centigrade, that of unity of weight of steam at any pressure and temperature will be given by the formula

$$V = \frac{1696 v \varpi}{(\tau)} \cdot \frac{\tau}{P}, \quad . \quad . \quad . \quad . \quad . \quad (38)$$

ϖ representing the number of units of weight per unit of area in the pressure of one atmosphere, and (τ) the absolute temperature at which the pressure of saturation is one atmosphere; being for the Centigrade scale 374°·6, and for Fahrenheit's scale 674°·28.

The mechanical action of unity of weight of steam at the temperature τ and pressure P, during its entrance into a cylinder, before it is permitted to expand, is represented by the product of its pressure and volume, or by

$$PV = \frac{1696 v \varpi}{(\tau)} \cdot \tau. \quad . \quad . \quad . \quad . \quad . \quad (39)$$

The coefficient $\frac{1696 v \varpi}{(\tau)}$ represents a certain depth of fall per degree of absolute temperature, and is the same with the coefficient $\frac{1}{CnM}$ already referred to. By taking the following values of the factors,

$v = 0{\cdot}016$ cubic foot per pound avoirdupois,
$\varpi = 2117$ pounds avoirdupois per square foot,

we find this coefficient to be

$$\left.\begin{array}{l}153{\cdot}35 \text{ feet} = 46{\cdot}74 \text{ metres per Centigrade degree,}\\ 85{\cdot}19 \text{ feet per degree of Fahrenheit;}\end{array}\right\} (40)$$

this determination may be considered correct to about $\frac{1}{1200}$ part.

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* These remarks are applicable to the calculation of the power of steam-engines only.

When French measures are used in the calculation, the following is the result:—

$$\left.\begin{aligned} v &= 1 \text{ cubic centimetre per gramme,} \\ \varpi &= 1033{\cdot}3 \text{ grammes per square centimetre,} \\ \frac{1}{\mathrm{C}n\mathrm{M}} &= 46{\cdot}78 \text{ metres per Centigrade degree,} \\ &= 153{\cdot}48 \text{ feet } \quad \ldots \quad \ldots \\ &\quad \text{or } 85{\cdot}27 \text{ feet per degree of Fahrenheit.} \end{aligned}\right\} \quad . \quad (41)$$

The difference, which is of no practical importance in calculating the power of the steam-engine, arises in the estimation of the density of liquid water.

(22.) Unit of weight of steam at saturation, of the elasticity P_1 and volume V_1 corresponding to the absolute temperature τ_1, being cut off from external sources of heat, it is now to be investigated what amount of power it will produce in expanding to a lower pressure P_2 and temperature τ_2.

It has already been shown at the end of the second section, that if vapour at saturation is allowed to expand, it requires a supply of heat from without to maintain it at the temperature of saturation, otherwise a portion of it must be liquefied to supply the heat required to expand the rest. Hence, when unity of weight of steam at saturation, at the pressure P_1 and volume V_1, expands to a lower pressure P, being cut off from external sonrces of heat, it will not occupy the entire volume V corresponding to that pressure, according to equation (38), but a less volume

$$S = mV,$$

where m represents the weight of water remaining in the gaseous state, the portion $1-m$ having been liquefied during the expansion of the remainder. The expansive action of the steam will therefore be represented by

$$\int_{V_1}^{S_2} d\mathrm{S} \,.\, \mathrm{P}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

The law of variation of the fraction m flows from the following considerations:—

Let δm represent the indefinitely small variation of m corresponding to the indefinitely small change of temperature $\delta\tau$; L, the latent heat of evaporation of unity of weight; K_S, as in equation (30), the specific heat of vapour at saturation, which is a negative coefficient varying with the temperature; then we must have

$$-\mathrm{L}\delta m = m\mathrm{K_S}\,\delta\tau, \text{ or } \frac{\delta m}{m} = -\frac{\mathrm{K_S}}{\mathrm{L}}\,\delta\tau,$$

in order that the heat produced by the liquefaction of δm may be equal to the heat required to expand m. Hence making, according to equation (30),

$$K_S\,\delta\tau=\mathfrak{k}\left(\delta\tau+N\frac{\tau}{V}\,\delta V\right),$$

and

$$\delta\tau=-\frac{\tau}{V}\,\delta V\,\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}-1},$$

we obtain

$$\frac{\delta m}{m}=-\frac{\mathfrak{k}\tau}{L}\left(N-\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}-1}\right)\frac{\delta V}{V}; \quad . \quad (43)$$

and denoting the coefficient of $\frac{\delta V}{V}$ by $-\nu$,

$$\left.\begin{aligned}
&\frac{d\log m}{d\log V}=-\nu;\ \frac{d\log S}{d\log V}=1-\nu;\\
&\text{and because}\\
&\frac{d\log V}{d\log P}=-1+\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}},\\
&\frac{d\log m}{d\log P}=+\nu\left(1-\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}}\right)\\
&\frac{d\log S}{d\log P}=-(1-\nu)\left(1-\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}}\right)=-\sigma.
\end{aligned}\right\} \quad . \quad . \quad (44)$$

As the mean temperature of the liquid thus produced more or less exceeds that of the remaining vapour, a small fraction of it will be reconverted into vapour if the expansion is carried on slowly enough; but its amount is so small, that to take it into account would needlessly complicate the calculation, without making it to any material extent more accurate.

(23.) The extreme complexity of the exponent σ, considered as a function of the pressure P, would render a general formula for the expansive action $\int P\,dS$ very cumbrous in its application. For practical purposes, it is sufficient to consider the exponent σ as constant during the expansion which takes place in any given engine, assigning it an average value suitable to the part of the scale of pressures in which the expansion takes place. For engines in which the steam is introduced at pressures not exceeding four atmospheres, I conceive that it will be sufficiently accurate to make

$$\sigma=\frac{6}{7};$$

while for engines in which the initial pressure lies between four and eight atmospheres, the suitable value is

$$\sigma=\frac{5}{6}.$$

The utmost error which can arise from using these exponents is about $\frac{1}{100}$ of the whole power of the engine, and that only in extreme cases.

Making, therefore,
$$P=P_1\left(\frac{S}{V_1}\right)^{-\frac{1}{\sigma}},$$

we obtain for the value of the expansive action of unity of weight of steam,

$$\left.\begin{aligned}\int_{V_1}^{S_2} dS\,.\,P &= P_1V_1\frac{\sigma}{1-\sigma}\left(1-\left(\frac{S_2}{V_1}\right)^{1-\frac{1}{\sigma}}\right)\\ &= P_1V_1\frac{\sigma}{1-\sigma}\left(1-s^{1-\frac{1}{\sigma}}\right),\end{aligned}\right\}\quad . \quad . \quad (45)$$

s being used to denote $\frac{S_2}{V_1}$, or the ratio of the volumes occupied by steam at the end and at the beginning of the expansion respectively.

A table to facilitate the computation is given in the sequel.

The gross mechanical action of unity of weight of steam on one side of the piston is found by adding to the above quantity the action of the steam before it begins to expand, or P_1V_1, and is therefore

$$P_1V_1\left(\frac{1}{1-\sigma}-\frac{\sigma}{1-\sigma}s^{1-\frac{1}{\sigma}}\right):\quad . \quad . \quad . \quad (46)$$

the values of the coefficients and exponent being

for initial pressures between	$\frac{1}{1-\sigma}$	$\frac{\sigma}{1-\sigma}$	$1-\frac{1}{\sigma}$
1 and 4 atmospheres . .	7	6	$-\frac{1}{6}$.
4 and 8 atmospheres . .	6	5	$-\frac{1}{5}$.

(24.) The following deductions have to be made from the gross action, in order to obtain the action effective in overcoming resistance.

First. For loss of power owing to a portion of the steam being employed in filling steam-passages, and the space called the *clearance* of the cylinder at one end. Let the bulk of steam so employed be the fraction cS_2 of the space filled by steam at the end of the expansion; then the loss of power from this cause is

$$P_1cS_2=csP_1V_1.$$

Secondly. For the pressure on the opposite side of the piston, of the steam which escapes into the condenser, or into the atmosphere, as the case may be. Let P_3 be the pressure of this steam; the deduction to be made for its action is

$$P_3S_2(1-c)=P_3V_1(1-c)s.$$

These deductions having been made, there is obtained for the effect of unity of weight of water evaporated,

$$V_1\left\{P_1\left(\frac{1}{1-\sigma}-\frac{\sigma}{1-\sigma}s^{1-\frac{1}{\sigma}}-cs\right)-P_3(1-c)s\right\}. \quad (47)$$

(25.) The effect of the engine in unity of time is found by multiplying the above quantity by the number of units of weight of water evaporated in unity of time.

If this number be denoted by W,

$$WS_2(1-c)=WV_1(1-c)s=Au \quad . \quad . \quad . \quad . \quad (48)$$

will represent the cubical space traversed by the piston in unity of time, A denoting the area of the piston, and u its mean velocity.

Now let the whole resistance to be overcome by the engine be reduced by the principles of statics to a certain equivalent pressure per unit of area of piston, and let this pressure be denoted by R. Then

$$RAu=RWV_1(1-c)s \quad . \quad . \quad . \quad . \quad . \quad (49)$$

expresses the effect of the engine in terms of the gross resistance.

We have now the means of calculating the circumstances attending the working of a steam-engine according to the principle of the conservation of *vis viva*, or, in other words, of the equality of power and effect, which regulates the action of all machines that move with a uniform or periodical velocity.

This principle was first applied to the steam-engine by the Count de Pambour; and accordingly, the formulæ which I am about to give only differ from those of his work in the expressions for the maximum pressure at a given temperature, and for the expansive action of the steam, which are results peculiar to the theory of this essay.

In the first place, the effect, as expressed in terms of the pressure, is to be equated to the effect as expressed in terms of the resistance, as follows:—

$$\left.\begin{aligned}RAu=RWV_1(1-c)s=WV_1\Big\{P_1\Big(\frac{1}{1-\sigma}-\frac{\sigma}{1-\sigma}s^{1-\frac{1}{\sigma}}-cs\Big)\\ -P_3(1-c)s\Big\}.\end{aligned}\right\}(50)$$

This is the fundamental equation of the action of the steam-engine, and corresponds with equation A of M. de Pambour's theory.

(26.) Dividing both sides of equation (50) by the space traversed by the piston in unity of time $WV_1(1-c)s$, and transferring the pressure of the waste steam, P_3, to the first side, we obtain this equation:—

$$R+P_3=P_1\frac{\frac{1}{1-\sigma}-\frac{\sigma}{1-\sigma}s^{1-\frac{1}{\sigma}}-cs}{(1-c)s}, \quad . \quad . \quad (51)$$

which gives the means of determining the pressure P_1 at which the steam must enter the cylinder, in order to overcome a given resistance and counter-pressure with a given expansion; or supposing the expansion s to be variable at pleasure, and the initial pressure P_1 fixed, the equation gives the means of finding, by approximation, the expansion best adapted to overcome a given resistance and counter-pressure.

The next step is to determine, from equations (XV.) of the Introduction and (38) of this section, the volume V_1 of unity of weight of steam corresponding to the maximum pressure P_1. Then equation (48) gives the space traversed by the piston in unity of time, which, being multiplied by the resistance R per unit of area of piston, gives the gross effect of the engine.

(27.) If, on the other hand, the space traversed by the piston in unity of time is fixed, equation (48) gives the means of determining, from the evaporating power of the boiler W, either the volume V_1 of unity of weight of steam required to work the engine at the given velocity with a given expansion, or the expansion s proper to enable steam of a given initial density to work the engine at the given velocity. The initial pressure P_1 being then determined from the volume V_1, the resistance which the engine is capable of overcoming with the given velocity is to be calculated by means of equation (51).

(28.) This calculation involves the determination of the pressure P_1 from the volume V_1 of unity of weight of steam at saturation, which can only be done by approximation. The following formula will be found useful for this purpose:—

$$P_1=\varpi\left(\frac{V_0}{V_1}\right)^{\frac{17}{16}}, \quad . \quad . \quad . \quad . \quad . \quad (52)$$

where ϖ represents the pressure of one atmosphere, V_0 the volume of steam of saturation at that pressure (being 1696 times the volume of water at 4°·1 Cent., or 27·166 cubic feet per pound avoirdupois), and V_1 the volume of steam of saturation at the pressure P_1. This formula is only applicable between the pressures of one and eight atmospheres; that is to say, when the volume of steam is not greater than 27 cubic feet per pound, nor less than 4, and the temperature not lower than 100° Centigrade, nor higher than 171° Centigrade (which correspond to 212° and 340° Fahrenheit).

The most convenient and expeditious mode, however, of computing the pressure from the volume, or *vice versâ*, is by interpolation from the table given in the sequel.

(29.) The resistance denoted by R may be divided into two parts; that which arises from the *useful work performed*, and that which is independent of it, being, in fact, the resistance of the engine when unloaded. Now it is evident that the maximum *useful* effect of the steam has been attained, as soon as it has expanded to a pressure which is in equilibrio with the pressure of the waste steam added to the resistance of the engine when unloaded; for any further expansion, though increasing the total effect, diminishes the useful effect. Therefore if we make

$$R = R' + f,$$

R' being the resistance arising from the useful work, and f the resistance of the engine when unloaded, both expressed in the form of pressure on the piston, the expansion corresponding to the maximum of useful effect will take place when

$$\left.\begin{array}{c} P_2 = P_3 + f, \\ \text{the corresponding ratio of expansion being} \\ s = \left(\dfrac{P_1}{P_3 + f}\right)^{\sigma}. \end{array}\right\} \quad . \quad . \quad . \quad (58)$$

The maximum useful effect with a given pressure on the safety-valve has been so fully discussed by M. de Pambour, that it is unnecessary to do more than to state that it takes place when the initial pressure in the cylinder is equal to that at the safety-valve; that is to say, when it and the useful resistance are the greatest that the safety-valve will permit.

(30.) Annexed is a table of the values of some of the quantities which enter into the preceding equations in the notation of the Count de Pambour's works.

Expression in the notation of this paper.	Equivalent expression in M. de Pambour's notation.
$R = R' + f$	$(1+\delta)r + f$
Au	av
W	$S \times$ weight of one cubic foot of water.
P_3	p
s	$\dfrac{l+c}{l'+c}$
c	$\dfrac{c}{l'+c}$.

(31.) As an illustration, I shall calculate the maximum useful effect of one pound, and of one cubic foot of water, in a Cornish double acting engine, in the circumstances taken by M. de Pambour as an example for that kind of engine; that is to say,—

	Per square ft.
Clearance one-twentieth of the stroke, or $c=\frac{1}{21}$,	
Resistance not depending on the useful load,	$f=72$ lbs.
Pressure of condensation, ...	$P_3=576$ lbs.

Consequently to give the maximum useful effect,

$$P_2=P_3+f \qquad =648 \text{ lbs.}$$

Total pressure of the steam when first admitted, $P_1=7200$ lbs.
Volume of 1lb. of steam $V_1=8{\cdot}7825$ cubic feet.
Therefore $P_1V_1=63234$ lbs. raised one foot.

$$\frac{P_1}{P_2}=\frac{7200}{648}, \text{ and consequently}$$

Expansion to produce the maximum useful effect $s=\left(\frac{P_1}{P_2}\right)^{\frac{6}{7}}=7{\cdot}877$.

Space traversed by the piston during the action of one pound of steam, $=V_1(1-c)s=65{\cdot}886$ cubic feet.

Gross effect of one pound of steam, in pounds raised one foot high,

$$=P_1V_1\left(7-6s^{-\frac{1}{6}}-\frac{s}{21}\right)-P_3V_1(1-c)s. \quad . \quad =112004$$

Deduct for resistance of engine when unloaded $fV_1(1-c)s$	$=$ 4744
Effect of one pound of steam in overcoming resistance depending on useful load . . .	107260
This being multiplied by $62\frac{1}{2}$, gives for the effect of one cubic foot of water evaporated, in pounds raised one foot	6,703,750

It is here necessary to observe, that M. de Pambour distinguishes the useful resistance into two parts, the resistance of the useful load independently of the engine, and the increase in the resistance of the engine, arising from the former resistance, and found by multiplying it by a constant fraction, which he calls δ. In calculating the net useful effect, he takes into account the former portion of the resistance only; consequently,

Net useful effect as defined by M. de Pambour

$$=\frac{\text{Gross effect } -fV_1(1-c)s}{1+\delta}. \quad . \quad . \quad . \quad . \quad . \quad (54)$$

The value of δ, for double acting steam-engines generally, is considered by M. de Pambour to be $\frac{1}{7}$; consequently, to reduce the effect of one cubic foot of water as calculated above to that which corresponds with his definition, we must deduct $\frac{1}{8}$, which leaves

5,865,781 lbs. raised one foot.

M. de Pambour's own calculation gives

6,277,560,

being too large by about one-fifteenth.

Explanation of Tables to be used in calculating the Pressure, Volume, and Mechanical Action of Steam, treated as a Perfect Gas.

(32.) The object of the first of the annexed tables is to facilitate the calculation of the volume of steam of saturation at a given pressure, of the pressure of steam of saturation at a given volume, and of its mechanical action at full pressure.

The pressures are expressed in pounds avoirdupois per square foot, and the volumes by the number of cubic feet occupied by one pound avoirdupois of steam, when considered as a perfect gas; those denominations being the most convenient for mechanical calculations in this country.

The columns to be used in determining the pressure from the volume, and *vice versâ*, are the third, fourth, sixth and seventh.

The third column contains the common logarithms of the pressures of steam of saturation for every fifth degree of the Centigrade thermometer from $-30°$ to $+260°$; that is to say, for every ninth degree of Fahrenheit's thermometer from $-22°$ to $+500°$.

The fourth column gives the differences of the successive terms of the third column.

The sixth column contains the common logarithms of the volume of one pound of steam of saturation corresponding to the same temperatures.

The seventh column contains the differences of the successive terms of the sixth column, which are negative; for the volumes diminish as the pressures increase.

By the ordinary method of taking proportional parts of the differences, the logarithms of the volumes corresponding to intermediate pressures, or the logarithms of the pressures corresponding to intermediate volumes, can be calculated with great precision. Thus, let $X+h$ be the logarithm of a pressure not found in the table, X being the next less logarithm which is found in the table; let Y be the logarithm of the volume corresponding to X, and $Y-k$ the logarithm of the volume corresponding to $X+h$; let H be the difference between X and the next greater logarithm in the table, as given in the fourth column, and K the corresponding difference in the seventh column; then by the proportion

$$H : K :: h : k,$$

either $Y-k$ may be found from $X+h$, or $X+h$ from $Y-k$.

In the fifth and eighth columns respectively are given the actual pressures and volumes corresponding to the logarithms in the third and sixth columns, to five places of figures.

In the ninth column are given the values of the quantity denoted by P_1V_1 in the formulæ, which represents the mechanical action of unity of weight of steam at full pressure, or before it has begun to expand, in raising an equal weight. Those values are expressed in feet, being the products of the pressures in the fifth column by the volumes in the eighth, and have been found by multiplying the absolute temperature in Centigrade degrees by 153·48 feet. Intermediate terms in this column, for a given pressure or a given volume, may be approximated to by the method of differences, the constant difference for 5° Centigrade being 767·4 feet; but it is more accurate to calculate them by taking the product of the pressure and volume.

When the pressure is given in other denominations, the following logarithms are to be added to its logarithm, in order to reduce it to pounds avoirdupois per square foot:—

For millimetres of mercury	0·44477
For inches of mercury	1·84960
For atmospheres of 760 millimetres . . .	3·32559
For atmospheres of 30 inches	3·32672
For kilogrammes on the square centimetre . .	3·31136
For kilogrammes on the circular centimetre .	3·41627
For kilogrammes on the square metre . . .	$\bar{1}$·31136
For pounds avoirdupois on the square inch .	2·15836
For pounds avoirdupois on the circular inch .	2·26327

To reduce the logarithm of the number of cubic metres occupied by one kilogramme to that of the number of cubic feet occupied by one pound avoirdupois, add 1·20463.

The logarithms are given to five places of decimals only, as a greater degree of precision is not attainable in calculations of this kind.

(33.) The second table is for the purpose of calculating the mechanical action of steam in expansive engines.

The first column contains values of the fraction of the entire capacity of the cylinder which is filled with steam before the expansion commences (being the quantity $\frac{1}{s}$ of the formulæ), for every hundredth part, from 1·00, or the whole cylinder, down to 0·10, or one-tenth.

If l be the entire length of stroke, l' the portion performed at full pressure, and c the fraction of the entire capacity of the cylinder allowed for clearance, then

$$\frac{l'}{l}=\frac{\frac{1}{s}-c}{1-c}, \text{ and } \frac{1}{s}=(1-c)\frac{l'}{l}+c.$$

The *entire capacity of the cylinder* is to be understood to include clearance at one end only.

The second column gives the reciprocals of the quantities in the first, or the values of the ratio of expansion s.

The third and fourth columns, headed Z, give the values of the quantity $\frac{1}{1-\sigma}-\frac{\sigma}{1-\sigma}s^{1-\frac{1}{\sigma}}$ of article 23, which represents the ratio of the entire gross action of the steam to its action at full pressure, without allowing for clearance. The third column is to be used for initial pressures of from one to four atmospheres; and the fourth for initial pressures of from four to eight atmospheres.

The deduction to be made from the quantity Z for clearance is cs, or the product of the fraction of the cylinder allowed for clearance by the ratio of expansion. Hence, to calculate from the tables the net mechanical action of unity of weight of steam, allowing for the counter-pressure of the waste steam P_3, as well as for clearance, we have the formula

$$P_1V_1(Z-cs)-P_3V_1(1-c)s,$$

being equivalent to the formula (47) of this paper.

Note.—The third, fourth, and fifth columns of the first of the following tables may be relied on as showing the pressures of steam and their logarithms with a degree of accuracy limited only by that of M. Regnault's experiments, from which the formula used in computing them was deduced.

On the other hand, the sixth, seventh, eighth, and ninth columns of the first table, relating to the volume of one pound of steam and its action at full pressure, are to be considered as merely sufficiently accurate, in the absence of more precise data, for the calculation of the power of steam-engines to an approximation near enough for practical purposes, and are not to be relied upon, in theoretical computations, above the temperature of about 60° Centigrade, or 140° Fahrenheit.

TABLE I.—Pressure and Volume of Steam, supposing it to be a perfect gas, and its Action at Full Pressure.

(1.) Temperature Fahrenheit.	(2.) Temperature Centigrade.	(3.) Logarithm of pressure in lbs. per square foot.	(4.) Differences.	(5.) Pressure in lbs. per square foot.	(6.) Logarithm of volume of one lb. of steam in cubic feet.	(7.) Differences.	(8.) Volume of one lb. of steam in cubic feet.	(9.) Action of a given weight of steam in raising an equal weight in feet, at full pressure $=P_1V_1$.
−22°	−30°	$\bar{1}$·99278	0·20563	0·9835	4·58173	0·19684	38171	37541
−13	−25	0·19841	0·19602	1·5791	4·38489	0·18741	24260	38309
− 4	−20	0·39443	0·18710	2·4799	4·19748	0·17865	15757	39076
+ 5	−15	0·58153	0·17864	3·8153	4·01883	0·17036	10443	39843
14	−10	0·76017	0·17085	5·7567	3·84847	0·16272	7054·6	40611
23	− 5	0·93102	0·16348	8·5314	3·68575	0·15550	4850·1	41378
32	0	1·09450	0·15661	12·431	3·53025	0·14877	3390·4	42146
41	+ 5	1·25111	0·15012	17·828	3·38148	0·14242	2407·0	42913
50	10	1·40123	0·14404	25·190	3·23906	0·13648	1734·0	43680
59	15	1·54527	0·13836	35·097	3·10258	0·13093	1266·4	44448
68	20	1·68363	0·13284	48·265	2·97165	0·12553	936·81	45215
77	25	1·81647	0·12780	65·535	2·84612	0·12061	701·65	45983
86	30	1·94127	0·12297	87·957	2·72551	0·11590	531·51	46750
95	35	2·06724	0·11842	116·75	2·60961	0·11146	407·01	47517
104	40	2·18566	0·11410	153·34	2·49815	0·10725	314·88	48285
113	45	2·29976	0·11002	199·42	2·39090	0·10328	245·98	49052
122	50	2·40978	0·10614	256·91	2·28762	0·09950	193·92	49820
131	55	2·51592	0·10247	328·04	2·18812	0·09593	154·21	50587
140	60	2·61839	0·09897	415·33	2·09219	0·09253	123·65	51354
149	65	2·71736	0·09566	521·63	1·99966	0·08931	99·922	52122
158	70	2·81302	0·09250	650·16	1·91035	0·08625	81·349	52889
167	75	2·90552	0·08953	804·49	1·82410	0·08336	66·696	53657
176	80	2·99505	0·08658	988·67	1·74074	0·08050	55·048	54424
185	85	3·08163	0·08388	1206·8	1·66024	0·07788	45·734	55191
194	90	3·16551	0·08129	1463·9	1·58236	0·07538	38·226	55959
203	95	3·24680	0·07879	1765·2	1·50698	0·07295	32·135	56726
212	100	3·32559	0·07640	2116·4	1·43403	0·07064	27·166	57494
221	105	3·40199	0·07415	2523·4	1·36339	0·06847	23·088	58261
230	110	3·47614	0·07196	2993·2	1·29492	0·06635	19·721	59028
239	115	3·54810	0·06988	3532·6	1·22857	0·06434	16·927	59796
248	120	3·61798	0·06788	4149·3	1·16423	0·06241	14·596	60563
257	125	3·68586	0·06597	4351·3	1·10182	0·06057	12·642	61331
266	130	3·75183	0·06414	5647·2	1·04125	0·05881	10·996	62098
275	135	3·81597	0·06238	6545·9	0·98244	0·05711	9·6037	62865
284	140	3·87835	0·06069	7557·0	0·92533	0·05548	8·4204	63633
293	145	3·93904	0·05907	8690·4	0·86985	0·05393	7·4105	64400
302	150	3·99811	0·05751	9956·6	0·81592	0·05242	6·5452	65168
311	155	4·05562	0·05601	11366	0·76350	0·05099	5·8010	65935
320	160	4·11163	0·05456	12931	0·71251	0·04959	5·1583	66702
329	165	4·16619	0·05319	14662	0·66292	0·04828	4·6017	67470
338	170	4·21938	0·05184	16572	0·61464	0·04698	4·1176	68237
347	175	4·27122	0·05056	18673	0·56766	0·04576	3·6954	69005
356	180	4·32178	0·04932	20979	0·52190	0·04457	3·3258	69772
365	185	4·37110	0·04812	23502	0·47733	0·04342	3·0014	70539
374	190	4·41922	0·04696	26256	0·43391	0·04231	2·7159	71307
383	195	4·46618	0·04586	29254	0·39160	0·04126	2·4638	72074
392	200	4·51204	0·04478	32512	0·35034	0·04023	2·2405	72842
401	205	4·55682	0·04375	36043	0·31011	0·03924	2·0423	73609
410	210	4·60057	0·04274	39863	0·27087	0·03829	1·8663	74376
419	215	4·64331	0·04176	43986	0·23258	0·03734	1·7084	75144
428	220	4·68507	0·04085	48425	0·19524	0·03649	1·5676	75911
437	225	4·72592	0·03994	53201	0·15875	0·03561	1·4413	76679
446	230	4·76586	0·03906	58326	0·12314	0·03478	1·3278	77446
455	235	4·80492	0·03819	63815	0·08836	0·03395	1·2256	78213
464	240	4·84311	0·03740	69680	0·05441	0·03320	1·1335	78981
473	245	4·88051	0·03660	75947	0·02121	0·03244	1·0501	79748
482	250	4·91711	0·03582	82625	$\bar{1}$·98877	0·03170	0·97447	80516
491	255	4·95293	0·03507	89728	$\bar{1}$·95707	0·03099	0·90588	81283
500	260	4·98800		97275	$\bar{1}$·92608		0·84349	82050

TABLE II.—Expansive Action of Steam.

(1.)	(2.)	(3.)	(4.)	(1.)	(2.)	(3.)	(4.)
Fraction of cylinder filled with steam at full pressure $=\frac{1}{s}$.	Ratio of expansion $=s$.	Coefficient of gross action $=Z$.		Fraction of cylinder filled with steam at full pressure $=\frac{1}{s}$.	Ratio of expansion $=s$.	Coefficient of gross action $=Z$.	
		Initial pressure one to four atmospheres.	Initial pressure four to eight atmospheres.			Initial pressure one to four atmospheres.	Initial pressure four to eight atmospheres.
1·00	1·000	1·000	1·000	·54	1·852	1·586	1·580
·99	1·010	1·010	1·010	·53	1·887	1·602	1·596
·98	1·020	1·020	1·020	·52	1·923	1·620	1·613
·97	1·031	1·030	1·030	·51	1·961	1·637	1·630
·96	1·042	1·041	1·041	·50	2·000	1·655	1·647
·95	1·053	1·051	1·051	·49	2·041	1·673	1·665
·94	1·064	1·062	1·062	·48	2·083	1·691	1·683
·93	1·075	1·072	1·072	·47	2·128	1·709	1·701
·92	1·087	1·083	1·083	·46	2·174	1·728	1·719
·91	1·099	1·094	1·093	·45	2·222	1·748	1·738
·90	1·111	1·104	1·104	·44	2·273	1·767	1·757
·89	1·124	1·115	1·115	·43	2·326	1·787	1·777
·88	1·136	1·126	1·126	·42	2·381	1·808	1·796
·87	1·149	1·138	1·137	·41	2·439	1·829	1·817
·86	1·163	1·149	1·149	·40	2·500	1·850	1·837
·85	1·176	1·160	1·160	·39	2·564	1·871	1·858
·84	1·190	1·172	1·171	·38	2·632	1·894	1·880
·83	1·205	1·183	1·183	·37	2·703	1·916	1·902
·82	1·220	1·195	1·195	·36	2·778	1·939	1·924
·81	1·235	1·207	1·206	·35	2·857	1·963	1·947
·80	1·250	1·219	1·218	·34	2·941	1·987	1·970
·79	1·266	1·231	1·230	·33	3·030	2·012	1·994
·78	1·282	1·243	1·242	·32	3·125	2·038	2·019
·77	1·299	1·256	1·255	·31	3·225	2·064	2·044
·76	1·316	1·268	1·267	·30	3·333	2·091	2·070
·75	1·333	1·281	1·280	·29	3·448	2·119	2·097
·74	1·351	1·294	1·292	·28	3·571	2·147	2·124
·73	1·370	1·307	1·305	·27	3·704	2·176	2·152
·72	1·389	1·320	1·318	·26	3·846	2·207	2·181
·71	1·408	1·333	1·331	·25	4·000	2·238	2·211
·70	1·429	1·346	1·344	·24	4·167	2·270	2·242
·69	1·449	1·360	1·358	·23	4·348	2·304	2·273
·68	1·471	1·374	1·371	·22	4·545	2·338	2·306
·67	1·493	1·387	1·385	·21	4·762	2·374	2·341
·66	1·515	1·401	1·399	·20	5·000	2·412	2·376
·65	1·538	1·416	1·413	·19	5·263	2·451	2·413
·64	1·563	1·430	1·427	·18	5·556	2·492	2·452
·63	1·587	1·445	1·441	·17	5·882	2·534	2·492
·62	1·613	1·459	1·456	·16	6·250	2·579	2·534
·61	1·640	1·474	1·471	·15	6·667	2·626	2·579
·60	1·667	1·490	1·486	·14	7·143	2·676	2·626
·59	1·695	1·505	1·501	·13	7·692	2·730	2·675
·58	1·724	1·521	1·516	·12	8·333	2·786	2·728
·57	1·754	1·537	1·532	·11	9·091	2·847	2·784
·56	1·786	1·553	1·547	·10	10·000	2·912	2·845
·55	1·818	1·569	1·563				

[To be continued.]

XXVII. *On some Acoustic Phænomena produced by the motion of Liquids through short Efflux Tubes.* *By* F. SAVART*.

THE memoir, an abstract of which we are about to give, is a posthumous one, and was presented to the Academy of Paris by Arago. Of the four paragraphs which the author intended the memoir to contain, the first only was completed.

When a liquid, such as water, issues through a cylindrical efflux-tube, it produces under certain circumstances musical tones of great intensity and of a peculiar quality, somewhat analogous to that of the human voice. The fact that this phænomenon has not been hitherto observed, arises from the peculiar circumstances under which alone an efflux tube can occasion such tones; for it is necessary, first, that its length should not differ much from its diameter; next, that the charges or heights of the issuing liquid should bear a certain proportion to this diameter; and lastly, that the diameter of the reservoir should not exceed a certain magnitude, dependent on the diameter of the efflux tube; in a word, it is only under particular circumstances that the effect can exhibit itself in all its beauty.

To obtain a general idea of the phænomenon, conceive one extremity of a glass tube, from 6 to 8 centimetres in diameter and 2 metres long, closed by means of a metallic plate, in the centre of which is a circular aperture whose diameter is equal to the thickness of the plate. After having fixed the tube in a vertical position and filling it with water, the orifice is opened, when it is observed that the efflux takes place periodically, and is accompanied by a tone which at the commencement is feeble and confused, but gradually acquires force as the charge diminishes, until it attains a certain limit, beyond which its intensity decreases, and in some cases it disappears altogether. But as the charge continues to sink, the tone soon regains force, becoming at the same time more grave, until at length it attains another maximum of intensity, after which it again becomes feebler in order to increase anew, with a still lower pitch, and so on. The number of these ventral segments, as they may be called, depends upon the diameter and height of the tube, as well as upon the diameter of the orifice.

From the above it evidently follows, that when a liquid issues through a short efflux tube, its whole mass is periodically subject to certain modifications, either in the velocity or the direction of its component threads.

The experimental analysis of these phænomena presents great difficulties, owing partly to their extremely short duration, par-

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* Abridged from the *Comptes Rendus* for August 1853, and communicated by Dr. Hirst.

ticularly when the diameter of the efflux tube is pretty large in relation to that of the tube which serves as reservoir; then, again, under like conditions, these phænomena do not rigorously reproduce themselves in successive experiments; and lastly, they are influenced by a number of small and almost inappreciable circumstances, such as the more or less perfect polish of the efflux tube, the purity of the liquid, the cleanness of the sides of the reservoir, &c.

The experiments of M. Savart, so far as recorded, are intended to illustrate,—

1. The influence of the charge.
2. That of the diameter of the efflux tube.
3. That of its height.
4. That of the diameter of the reservoir, supposing it first to be cylindrical and concentric with the efflux tube, then cylindrical and of small diameter at its inferior part, or cylindrical and of large diameter at its superior part.

1. *The influence of the Charge.*

The cylindrical efflux tubes consisted each of a simple plate of brass, in the centre of which was a circular aperture perpendicular to the two parallel faces; or at the centre of such a plate, a small cylindrical tube was soldered. Every precaution was taken that the plate in contact with the liquid and the efflux tube itself were perfectly clean and polished; that the latter was quite cylindrical, and exactly perpendicular to the polished face of the plate.

The tubes which served as reservoirs were of glass, their diameters varied from 30 to 165 millimetres; they were chosen as cylindrical as possible, and the extremity of each in contact with the plate was ground so that the plane of the latter was perpendicular to the axis of the cylinder. The plate and glass cylinder were joined by gum-mastic applied hot, care being taken that it did not penetrate into the interior of the glass tube, and that the axes of the efflux and glass tubes were coincident.

On experimenting, the glass tube was fixed in a vertical position by means of an immoveable support, the efflux tube was closed in order to fill the glass one with water, and after allowing the whole to rest quiet for a quarter of an hour, the efflux tube was opened, and the observations commenced. In all cases the experiments were repeated several times, until there was no longer any uncertainty either as to the tones produced, their intensity, or their corresponding charges.

The first experiment was made with a glass tube 2·66 metres in height and 0·041 metre in diameter, having an efflux tube whose height and diameter were each 2·15 millims. On observing the issuing jet during the several phases of the phænomenon, it was seen to swell out considerably whenever the tone attained its maximum intensity, and to contract every time this intensity decreased. In the first case the jet appeared to open, in the second to close.

As the charge diminished from 2·66 metres to 0·15 metre, the tone lowered through two octaves; but this fall was not gradual, it took place by fits, so that the whole series constituted a number of ventral segments, for each of which the tone was nearly constant, except when the charge was feeble. These ventral segments were separated by nodes, where the tone, without disappearing altogether, was much less intense, and reduced to a kind of rustling noise. At first this appeared to be incapable of analysis by the ear; nevertheless, on closer examination the noise appeared to be composed of two tones, one being that of the superior ventral segment, the other that of the inferior; from which it is evident that the nodes are the points where the state of vibration which constitutes a certain ventral segment becomes modified, in passing to that which constitutes the ventral segment immediately succeeding.

The number of such ventral segments decreases when the aperture is enlarged. In the above case there were fifteen, and the vessel was emptied in 315 seconds. In another experiment with the same glass tube, but with an efflux tube whose height was 5·456 millims., and diameter 5·4 millims., there were only four such ventral segments, and the vessel was emptied in 47 seconds. In the former experiment, the tone corresponding to the charge 2·66 metres was more than an octave higher than in the latter; the lowest audible tone in the latter experiment corresponded to the charge 0·57 millim. When the vessel emptied itself with great velocity, each ventral segment presented this peculiarity, that the tone after reaching its maximum intensity always rose a little higher in pitch, sometimes, indeed, a semitone.

By comparing the numbers of vibrations corresponding to each pressure with the square roots of the pressures themselves, the former were found to be in general proportional to the latter, or to the velocity of efflux. It is clear that this law cannot be rigorously exact for all pressures, because the tone remains almost invariable throughout each ventral segment; nevertheless its existence was on the whole quite manifest. The degree of exactitude may be seen from the following table:—

Charges.	Square roots of the charges.	Ratios.	Diameter of efflux tube, 2·15 millims.		Diameter of efflux tube, 5·5 millims.	
			Numbers of vibrations corresponding to the tone.	Ratios.	Numbers of vibrations corresponding to the tone.	Ratios.
Centims.						
108	10·4	1·48	3200	1·56	1365	1·48
70	8·4	1·20	2212	1·08	960	1·04
50	7·1	1·00	2048	1·00	921	1·00

The general influence of the charge upon the number of vibrations can be easily rendered sensible by inclining the tube during the efflux of the liquid. In this manner, by a gradual inclination from a vertical to a nearly horizontal position, the tone fell rapidly, whereas by raising it more towards the vertical, the tone rose higher and higher; and, what is worthy of remark, the differences in intensity were incomparably less now than when the tube was fixed. The tone could be easily rendered constant by inclining it gradually towards the vertical, according as the level of the water fell.

2. *Influence of the diameter of Efflux Tube.*

In a preceding memoir Savart demonstrated that when a liquid issues through a circular orifice in a thin plate, pulsations are produced whose number is proportional to the square root of the charge, and inversely proportional to the diameter of the orifice.

It has just been shown that with short efflux tubes the number of vibrations conforms to the first of these laws, and what follows will prove that the second law is also fulfilled.

The experiments were made with efflux tubes of various diameters, adapted successively to a tube 1·70 metre in height and 5 centimetres in diameter. From the following table it will be seen that the numbers of vibrations may be considered inversely proportional to the diameters of the efflux tubes.

		mm	mm	mm	mm
	Diameter of efflux tubes	2·15	4·65	5·40	9·30
	Ratios of these diameters	1·00	2·16	2·51	4·32
metre.					
Charge 1·08	Ratios of the numbers of vibrations...	4·50	2·00	1·92	1·00
... 0·70	... ...	4·32	2·00	1·88	1·00
... 0·50	... ...	4·27	2·13	1·94	1·00

But here also it is evident that this law can be but an approximation to the true nature of the phænomenon, inasmuch as the tone makes sudden changes, and rises always somewhat higher after each maximum of intensity has been passed.

3. *Influence of the height of Efflux Tube.*

The height of the efflux tube has a far greater influence on the production of tones than on their nature when produced. Thus, when the height of the efflux tube exceeds the double of its diameter, the production of tones may be said to be almost impossible, slight rustling noises are alone heard, and they, too, only when the charge is feeble. Similarly, when the height of the efflux tube is less than half its diameter, the efflux must be regarded as taking place through a thin plate, and consequently tones are impossible. Hence it is evident that the necessary vibratory motion can only occur within very narrow limits.

A series of experiments were made with a reservoir tube 1·70 metre in height and 5 centims. in diameter, to which efflux tubes, 5·4 millims. in diameter and of different heights, were successively adapted. One of these had a height of 2·726 millims., or nearly equal to half the diameter, and the efflux took place without a tone, as is always the case with thin plates. Another had a height of 10·9 millims., or nearly equal to double its diameter, and with it no tone at all was obtained. Between these extremes the results seemed to indicate, first, that the number of ventral segments diminishes when the height of the efflux tube is either greater or less than its diameter; secondly, that the charges corresponding to the first appearance of tone are more and more feeble as the height of the efflux tube becomes greater or less than its diameter; and lastly, that the tones acquire their greatest intensity when the height is equal to the diameter.

With regard to the influence of the height of the efflux tube upon the number of vibrations, although no certain law could be established, it was evident that the number was greater when the efflux tube was shorter, and it appeared probable that this number was inversely proportional to the height of the efflux tube.

By immersing the tube about 1 centimetre deep in water, and causing the efflux to take place into this liquid instead of into air, it was found that extremely short efflux tubes produced very intense tones, and this continued to be the case until the height was but one-fourth of the diameter. Indeed, tones continued to be produced far beyond this limit, even when the height did not exceed one-tenth of the diameter; in short, it was only when the efflux took place through orifices pierced through thin plates that the production of tones was no longer possible, or rather no longer perceptible by the ear; for Savart has shown that even then the efflux takes place with a velocity periodically variable.

With efflux tubes whose height was greater than their diameters, the only effect of causing the efflux to take place in water seemed to be a slight depression of tone, amounting to about half a semitone, which circumstance may be easily explained when we consider the greater resistance of the medium which the jet has to penetrate.

On repeating the experiments with efflux tubes whose diameters exceeded their heights, it often happened that different results were obtained. At such times a depression and elevation of the tone through an entire octave often occur. Hence it would appear that the threads of liquid no longer possess that stability in direction and velocity which was observed with longer efflux tubes. On this account the only conclusion drawn by Savart from this series of experiments is, that when the efflux takes place in the same medium, a state of sonorous vibration may exist for all orifices, from those in thin plates to those whose height does not exceed double their diameter.

4. *Influence of the diameter of the Reservoir Tube.*

In order to study the influence of the reservoir tube, only one efflux tube was used; its height and diameter were equal to 2·25 millims., and it was successively adapted to four glass tubes of different diameters. The experiments proved that the diameter of the tube had an influence on the number of ventral segments, which increased as the diameter of the tube was augmented, or as the vessel emptied itself more slowly.

Experiments were next made by fixing glass tubes, to which efflux tubes had been previously adapted, to a vessel of much larger diameter, in which the level of the liquid could either be kept constant or not, as required. In doing so, a glass tube was taken, provided with a certain efflux tube, and the positions of maximum intensity were determined. The tube was then cut of such a length, that when it was afterwards adapted perpendicularly to the bottom of a larger vessel, the charge corresponded to one of those which gave a maximum intensity. It was thus found that the tone was the same as the one corresponding to a like charge when a simple tube was employed; it acquired, however, a remarkable force when the level of the liquid was kept constant.

The experiments which verified the above remarks were made with efflux tubes 2·15 millims. and 5·4 millims. in diameter. With the former, the glass tubes were 62 centims. long, and varied in diameter from 17 to 63 millims.; these were fixed to the bottom of a wooden trough whose height was 50 centims., upper diameter 46·5 centims., and lower diameter 38 centims. The efflux tube, whose diameter was 5·4 millims., was adapted to a glass tube 0·05 metre in diameter and 1·23 metre in height, which was fixed to the centre of the plane circular base of a large copper vessel whose height was 0·12 metre and diameter 0·68 metre. In both cases the tones produced were exactly the same as those corresponding to the same heights of charge when simple tubes alone were used.

The same experiments were afterwards made with efflux tubes of larger diameter, the apparatus in other respects being unaltered; the agreement in tone between tubes of the same diameter throughout and tubes fixed to a much larger vessel, was found no longer to exist. This agreement is likewise destroyed when the height of the glass tube, in relation to that of the vessel placed above, is less considerable; which shows that the number of vibrations does not depend solely upon what takes place at the orifice itself, but that it depends partially, at least, on the motion of the liquid in the tube. This motion must be modified considerably during the passage of a liquid from a vessel of a large to one of a small capacity; for at the place of transition a contraction takes place which must necessarily have some influence on the velocity of the liquid in the smaller tube, so that the effect will be the same as a diminution of the charge.

Savart confirmed the above explanation by fixing a glass tube, with an efflux tube 4·65 millims. in diameter, to the copper vessel above described. The total charge was 1·06 metre. When the upper orifice of the glass tube was partially closed by placing a disc at the bottom of the copper vessel, a depression of tone took place; at first this depression was inconsiderable, but it was much increased when the disc covered half the orifice. This depression was evidently caused by a diminution of the velocity with which the liquid issued from the efflux tube.

XXVIII. *On the Mechanical Values of Distributions of Electricity, Magnetism and Galvanism.* *By* Professor W. Thomson*.

I. *Electricty at rest.*

To electrify an insulated conductor (a Leyden phial, for instance, or any mass of metal resting on supports of glass) in the ordinary way by means of an electrical machine, requires the expenditure of work in turning the machine. But inasmuch as part, obviously by far the greater part, of the work done in this operation goes to generate heat by means of friction, and of the small residue some, it may be a considerable proportion, is wasted in generating heat (electrical light being included in the

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* Communicated by the Author; having been read at a meeting of the Glasgow Philosophical Society, Jan. 26, 1853.

term) by the flashes, illuminated points, and sparks which accompany the transmission of the electricity from the glass of the machine where it is first excited to the conductor which receives it, the mechanical value of the electrification thus effected would be enormously over-estimated if it were regarded as equivalent to the work that has been spent. Notwithstanding, the mechanical value of any electrification of a conductor has a perfectly definite character, and may be calculated with ease in any particular case by means of formulæ demonstrated in this communication. The simplest case is that of a single conductor insulated at a distance from other conductors, or with only uninsulated conducting matter in its neighbourhood. In this case the mechanical value of the electrification of the conductor is equal to *half the square of the quantity of electricity multiplied by the capacity of the conductor**.

In any case whatever, the total mechanical value of all the distributions of electricity on any number of separate insulated conductors electrified with any quantities of electricity, is demonstrated by the author to be equal to half the sum of the products obtained by multiplying the "potential†" in each conductor by the quantity of electricity by which it is charged. Each term of this expression does not represent the independent value of the actual distribution on the conductor to which it corresponds, inasmuch as the "potential" in each depends on the presence of the others when they are near enough to exert any sensible mutual influence; but independent expressions of these independent values are readily obtained, although not in a form convenient for statement here; and the author proves that their sum is equal to the total value, as calculated by the preceding expression. When a conductor is discharged without other mechanically valuable effects being developed, as, for instance, when the knob of a Leyden phial is put in communication with the outside coating, or when a flash of lightning takes place, the heat is equal in mechanical value to the distribution of electricity lost. Hence, by what precedes, the amount of heat is proportional to the *square* of the quantities discharged, as was first demonstrated by Joule, in a communication to the Royal Society in 1840, although it had been announced by Sir W. Snow Harris

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* Electrical capacity is a term introduced by the author to signify the proportion of the quantity of electricity that the conductor would retain to that which it would communicate to a conducting ball of unit radius, insulated at a great distance from other conducting matter, if connected with it by means of a fine wire.

† A term first introduced by Green, which may be defined as the quantity of mechanical work that would have to be spent to bring a unit of electricity from a great distance up to the surface of the conductor, supposed to retain its distribution unaltered.

as an experimental result, to be simply proportional to the quantity. Mr. Joule's result has been verified by independent experimenters in France, Italy and Germany. The author pointed out other applications of his investigation, some of a practical kind, and others in the Mathematical Theory of Electricity. He mentioned, that although he had first arrived at the results in 1845, and used them in papers published in that year, the first explicit publication of the theorem regarding the mechanical value of the electrification of a conductor appears to be in 1847, in a paper entitled "Ueber die Erhaltung der Kraft," by Helmholtz.

II. *Magnetism.*

If a piece of soft iron be allowed to approach a magnet very slowly from a distant position, and be afterwards drawn away so rapidly, that, at the instant when it reaches its primitive position, where it is left at rest, it retains as yet sensibly unimpaired the magnetization it had acquired at the nearest position, a certain amount of work must have been finally expanded on the motion of the iron. For during the approach, the iron has only the magnetization due to the action of the magnet on it in its actual position at each instant; but at each instant of the time in which the iron is being drawn away, it has the whole magnetization due to the action of the magnet on it when it was at the nearest. Hence it is drawn away against more powerful forces of attraction by the magnet than those with which the magnet attracts it during its approach; from which it follows, that more work is spent in drawing the iron away than had been gained in letting it approach the magnet. The sole effect due to this excess of work is the magnetization which the iron carries away with it; and consequently, the mechanical value of this magnetization must be precisely equal to the mechanical value of the balance of work spent in producing it.

After a very short time has elapsed with the piece of soft iron at a great distance from the magnet, it will have lost, as is well known, all or nearly all the magnetization which it had acquired temporarily in the neighbourhood of the magnet; and in this short time some energy, equivalent to that of the magnetization lost, must have been produced. Mr. Joule's experiments show that this energy consists of heat, which is generated in the iron during demagnetization; and we infer the remarkable conclusion, that at the end of the process which has been described, or of any motion of a piece of soft iron in the neighbourhood of a magnet, from a certain position and back to the same, the iron will be as much warmer than it was at the beginning, as it would have been without any magnetic action, if it had received the heat that would be generated by the expenditure of the same amount of work on mere friction.

The same considerations are applicable to the magnetization of a piece of steel; with this difference, that, according to the hardness of the steel, the magnetization which it receives in the nearest position will be more or less permanent; and if there be any demagnetization after removal from the magnet, it will be much less complete than in the case of soft iron, and that heat will be necessarily generated both during the magnetization which takes place during the gradual approach, and in the subsequent demagnetization. Further, by putting together a number of pieces of steel, each separately magnetized, a complete magnet will be formed, of which the mechanical value will be equal to the sum of the mechanical values of its parts, increased or diminished by the amount of work spent or gained in bringing them together.

Upon the principles which have been explained, the author has investigated the mechanical value of any conceivable distribution of magnetism in any kind of substance. The result, which cannot be well expressed except in mathematical language, is as follows:—

$$\int\!\!\int\!\!\int \lambda q^2 dx\, dy\, dr + \frac{1}{8\pi}\int\!\!\int_{\infty}^{\infty}\!\!\int \mathrm{R}^2 dx\, dy\, dz,$$

where R denotes the resultant magnetic force at any internal or external point (x, y, z), q the intensity of magnetization at a point (x, y, z) of the magnet, and λ a quantity depending on the nature of the substance at this point.

The integral constituting the first term of this expression includes the whole of the magnetized substance, and expresses the sum of the separate mechanical values of the distributions in all the parts obtained by infinitely minute division along the lines of magnetization. The second term expresses the amount of work that would have to be spent to put these parts together, were they given separately, each with the exact magnetization that it is to have when in its place in the whole. If the substance be perfectly free in its susceptibility for magnetization or demagnetization, λ will be such a function of the inductive capacity, that if a ball of similar substance be placed in a magnetic field where the force is F, the intensity of the magnetization induced in it will be

$$\frac{\mathrm{F}}{2\lambda + \frac{4\pi}{3}}.$$

III. *Electricity in motion.*

If an electric current be excited in a conductor, and then left without electromotive force, it retains energy to produce heat, light, and other kinds of mechanical effect, and it lasts with diminishing, or it may be with alternately diminishing and increasing strength, before it finally ceases and electrical equilibrium is established; as is amply demonstrated by the experiments of Faraday and Henry, on the spark which takes place when a galvanic circuit is opened at any point, and by those of Weber, Helmholtz, and others on the electro-magnetic effects of varying currents. The object of the present communication is to show how the mechanical value of all the effects that a current in a close circuit can produce after the electromotive force ceases may be ascertained, by a determination founded on the known laws of electro-dynamic induction, of the mechanical value of the energy of a current of given strength circulating in a linear conductor (a bent wire, for instance) of any form. To do this, in the first place it may be remarked, that a current once instituted in a conductor, will circulate in it with diminishing strength after the electromotive force ceases, just as if the electricity had inertia, and will diminish in strength according to the same, or nearly the same, laws as a current of water or other fluid once set in motion and left without moving force in a pipe forming a closed circuit. But according to Faraday, who found that an electric circuit consisting of a wire doubled on itself, with the two parts close together, gives no sensible spark when suddenly opened compared to that given by an equal length of wire bent into a coil, it appears that the effects of ordinary *inertia* either do not exist for electricity in motion, or are but small compared with those which, in a suitable arrangement, are produced by the "induction of the current upon itself." In the present state of science it is only these effects that can be determined by a mathematical investigation; but the effects of electrical inertia, should it be found to exist, will be taken into account by adding a term of determinate form to the fully determined result of the present investigation, which expresses the mechanical value of a current in a linear conductor as far as it depends on the induction of the current on itself.

The general principle of the investigation is this; that if two conductors, with a current sustained in each by a constant electromotive force, be slowly moved towards one another, and there be a certain *gain of work* on the whole, by electro-dynamic force operating during the motion, there will be twice as much work as this spent by the electromotive forces (for instance, twice the equivalent of chemical action in the batteries, should the electromotive forces be chemical) over and above that which they would have had to spend in the same time, merely to keep up the currents, if the conductors had been at rest, because the electro-dynamic induction produced by the motion will augment the currents; while on the other hand, if the motion be such as to require the *expenditure* of work against electro-dynamic forces to produce it, there will be twice as much work saved off the action of the electromotive forces by currents being diminished during the motion. Hence the aggregate mechanical value of the currents in the two conductors when brought to rest will be increased in the one case by an amount equal to the work done by the mutual electro-dynamic forces in motion, and will be diminished by the corresponding amount in the other case. The same considerations are applicable to relative motions of two portions of the same linear conductor (supposed perfectly flexible). Hence it is concluded that the mechanical value of a current of given strength in a linear conductor of any form is determined by calculating the amount of work against electro-dynamic forces required to double it upon itself, while a current of constant strength is sustained in it. The mathematical problem thus presented leads to an expression for the required mechanical value consisting of two factors, of which one is determined according to the form and dimensions of the line of the conductor in any case, irrespectively of its section, and the other is the square of the strength of the current. If it be found necessary to take inertia into account, it will be done by adding to this expression a term consisting of two factors, of which one is directly proportional to the length of the conductor, and inversely proportional to the area of its section, and the other is the square of the strength of the current.

XXIX. *On Electric Induction—Associated cases of Current and Static Effects. By* Professor FARADAY, *D.C.L., F.R.S.*

[The following important paper is a verbal copy of an abstract of a lecture given by Professor Faraday at the Royal Institution on the evening of Friday, Jan. 20, of the present year.—EDS.]

CERTAIN phænomena that have presented themselves in the course of the extraordinary expansion which the works of the Electric Telegraph Company have undergone, appeared to me to offer remarkable illustrations of some fundamental principles of electricity, and strong confirmation of the truthfulness of the view which I put forth sixteen years ago, respecting the mutually dependent nature of induction, conduction, and insulation (Experimental Researches, 1318, &c.). I am deeply indebted to the Company, to the Gutta Percha Works, and to Mr. Latimer Clarke, for the facts; and also for the opportunity both of seeing and showing them well.

Copper wire is perfectly covered with gutta percha at the Company's works, the metal and the covering being in every part regular and concentric. The covered wire is usually made into half-mile lengths, the necessary junctions being effected by twisting or binding, and ultimately soldering; after which the place is covered with fine gutta percha, in such a manner as to make the coating as perfect there as elsewhere: the perfection of the whole operation is finally tried in the following striking manner by Mr. Statham, the manager of the works. The half-mile coils are suspended from the sides of barges floating in a canal, so that the coils are immersed in the water whilst the two ends of each coil rise into the air: as many as 200 coils are thus immersed at once; and when their ends are connected in series, one great length of 100 miles of submerged wire is produced, the two extremities of which can be brought into a room for experiment. An insulated voltaic battery of many pairs of zinc and copper, with dilute sulphuric acid, has one end connected with the earth, and the other, through a galvanometer, with either end of the submerged wire. Passing by the first effect, and continuing the contact, it is evident that the battery current can take advantage of the whole accumulated conduction or defective insulation in the 100 miles of gutta percha on the wire, and that whatever portion of electricity passes through to the water will be shown by the galvanometer. Now the battery is made one of intensity, in order to raise the character of the proof, and the galvanometer employed is of considerable delicacy; yet so high is the insulation, that the deflection is not more than 5°. As another test of the perfect state of the wire, when the two ends of the battery are connected with the two ends of the wire, there is a powerful current of electricity shown by a much coarser instrument; but when any one junction in the course of the 100 miles is separated, the current is stopped, and the leak or deficiency of insulation rendered as small as before. The perfection and condition of the wire may be judged of by these facts.

The 100 miles, by means of which I saw the phænomena, were thus good as to insulation. The copper wire was $\frac{1}{16}$th of an inch in diameter; the covered wire was $\frac{4}{16}$ths; some was a little less, being $\frac{7}{32}$nds in diameter; the gutta percha on the metal may therefore be considered as 0·1 of an inch in thickness. 100 miles of like covered wire in coils were heaped up on the floor of a dry warehouse and connected in one series, for comparison with that under water.

Consider now an insulated battery of 360 pairs of plates (4 × 3 inches) having one extremity to the earth; the water wire with both its insulated ends in the room, and a good earth discharge wire ready for the requisite communication: when the free battery end was placed in contact with the water wire and then removed, and afterwards a person touching the earth discharge touched also the wire, he received a powerful shock. The shock was rather that of a voltaic than of a Leyden battery; it occupied *time*, and by quick tapping touches could be divided into numerous small shocks; I obtained as many as forty sensible shocks from one charge of the wire. If *time* were allowed to intervene between the charge and discharge of the wire, the shock was less; but it was sensible after two, three, or four minutes, or even a longer period.

When, after the wire had been in contact with the battery, it was placed in contact with a Statham's fuse, it ignited the fuse (or even six fuses in succession) vividly; it could unite the fuse three or four seconds after separation from the battery. When, having been in contact with the battery, it was separated and placed in contact with a galvanometer, it affected the instrument very powerfully; it acted on it, though less powerfully, after the lapse of four or five minutes, and even affected it sensibly twenty or thirty minutes after it had been separated from the battery. When the insulated galvanometer was permanently attached to the end of the water wire, and the battery pole was brought in contact with the free end of the instrument, it was most instructive to see the great rush of electricity into the wire; yet after that was over, though the contact was continued, the deflection was not more than 5°, so high was the insulation. Then separating the battery from the galvanometer, and touching the latter with the earth wire, it was just as striking to see the electricity rush out of the wire, holding for a time the magnet of the instrument in the reverse direction to that due to the ingress or charge.

These effects were produced equally well with either pole of the battery or with either end of the wire; and whether the electric condition was conferred and withdrawn at the same end, or at the opposite ends of the 100 miles, made no difference in the results. An intensity battery was required, for reasons which will be very evident in the sequel. That employed was able to decompose only a very small quantity of water in a given time. A Grove's battery of eight or ten pair of plates, which would have far surpassed it in this respect, would have had scarcely a sensible power in affecting the wire.

When the 100 miles of wire in the air were experimented with in like manner, not the slightest signs of any of these effects were produced. There is reason, from principle, to believe that an infinitesimal result is obtainable, but as compared to the water wire the action was nothing. Yet the wire was equally well and better insulated, and as regarded a constant current, it was an equally good conductor. This point was ascertained by attaching the end of the water wire to one galvanometer, and the end of the air wire to another like instrument; the two other ends of the wires were fastened together, and to the earth contact; the two free galvanometer ends were fastened together, and to the free pole of the battery: in this manner the current was divided between the air and water wires, but the galvanometers were affected to precisely the same amount. To make the result more certain, these instruments were changed one for the other, but the deviations were still alike; so that the two wires conducted with equal facility.

The cause of the first results is, upon consideration, evident enough. In consequence of the perfection of the workmanship, a Leyden arrangement is produced upon a large scale; the copper wire becomes charged statically with that electricity which the pole of the battery connected with it can supply*; it acts by induction through the gutta percha (without which induction it could not itself become charged, Exp. Res. 1177), producing the opposite state on the surface of the water touching the gutta percha, which forms the outer coating of this curious arrangement. The gutta percha across which the induction occurs is only 0·1 of an inch thick, and the extent of the coating is enormous. The surface of the copper wire is nearly 8300 square feet, and the surface of the outer coating of water is four times that amount, or 33,000 square feet; hence the striking character of the results. The intensity of the static charge acquired is only equal to the intensity at the pole of the battery whence it is derived; but its quantity is enormous, because of the immense extent of the Leyden arrangement; and hence when the wire is separated from the battery and the charge employed, it has all the powers of a considerable voltaic current, and gives results which the best ordinary electric machines and Leyden arrangements cannot as yet approach.

That the air wire produces none of these effects is simply because there is no outer coating correspondent to the water, or only one so far removed as to allow of no sensible induction, and therefore the inner wire cannot become charged. In the air wire of the warehouse, the floor, walls, and ceiling of the place constituted the outer coating, and this was at a considerable distance; and in any case could only affect the outside portions

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* Davy, Elements of Chemical Philosophy, p. 154.

of the coils of wire. I understand that 100 miles of wire, stretched in a line through the air so as to have its whole extent opposed to earth, is equally inefficient in showing the effects, and there it must be the distance of the inductric and inducteous surfaces (1483.), combined with the lower specific inductive capacity of air, as compared with gutta percha, which causes the negative result. The phænomena altogether offer a beautiful case of the identity of static and dynamic electricity. The whole power of a considerable battery may in this way be worked off in separate portions, and measured out in units of static force, and yet be employed afterwards for any or every purpose of voltaic electricity.

I now proceed to further consequences of associated static and dynamic effects. Wires covered with gutta percha and then inclosed in tubes of lead or of iron, or buried in the earth, or sunk in the sea, exhibit the same phænomena as those described, the like static inductive action being in all these cases permitted by the conditions. Such subterraneous wires exist between London and Manchester; and when they are all connected together so as to make one series, offer above 1500 miles; which, as the duplications return to London, can be observed by one experimenter at intervals of about 400 miles, by the introduction of galvanometers at these returns. This wire, or the half or fourth of it, presented all the phænomena already described; the only difference was, that as the insulation was not so perfect, the charged condition fell more rapidly. Consider 750 miles of the wire in one length, a galvanometer *a* being at the beginning of the wire, a second galvanometer *b* in the middle, and a third *c* at the end; these three galvanometers being in the room with the experimenter, and the third *c* perfectly connected with the earth. On bringing the pole of the battery into contact with the wire through the galvanometer *a*, that instrument was instantly affected; after a sensible time *b* was affected, and after a still longer time *c*: when the whole 1500 miles were included, it required two seconds for the electric stream to reach the last instrument. Again; all the instruments being deflected (of course not equally, because of the electric leakage along the line), if the battery were cut off at *a*, that instrument instantly fell to zero; but *b* did not fall until a little while after; and *c* only after a still longer interval,—a current flowing on to the end of the wire whilst there was none flowing in at the beginning. Again; by a short touch of the battery pole against *a*, it could be deflected and could fall back into its neutral condition before the electric power had reached *b*; which in its turn would be for an instant affected, and then left neutral before the power had reached *c*; a wave of force having been sent into the wire which gradually travelled along it, and made itself evident at successive intervals of time in different parts of the wire. It was even possible, by adjusted touches of the battery, to have two simultaneous waves in the wire following each other, so that at the same moment that *c* was affected by the first wave, *a* or *b* was affected by the second; and there is no doubt that by the multiplication of instruments and close attention, four or five waves might be obtained at once.

If after making and breaking battery contact at *a*, *a* be immediately connected with the earth, then additional interesting effects occur. Part of the electricity which is in the wire will return, and passing through *a* will deflect it in the reverse direction; so that currents will flow out of both extremities of the wire in opposite directions, whilst no current is going into it from any source. Or if *a* be quickly put to the battery and then to the earth, it will show a current first entering into the wire, and then returning out of the wire at the same place, no sensible part of it ever travelling on to *b* or *c*.

When an air wire of equal extent is experimented with in like manner, no such effects as these are perceived; or if, guided by principle, the arrangements are such as to be searching, they are perceived only in a very slight degree, and disappear in comparison with the former gross results. The effect at the end of the very long air wire (or *c*) is in the smallest degree behind the effect at galvanometer *a*; and the accumulation of a charge in the wire is not sensible.

All these results as to *time*, &c. evidently depend upon the same condition as that which produced the former effect of static charge, namely, *lateral induction*; and are necessary consequences of the principles of conduction, insulation and induction, three terms which in their meaning are inseparable from each other (Exp. Res. 1320, 1326*, 1338, 1561, &c.). If we put a plate

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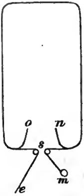
* 1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phænomena are produced. They appear to me to consist in an action of contiguous particles, dependent on the forces developed in electrical excitement: these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation*; and being in this state, the contiguous particles have a power or capability of communicating these forces, one to the other, by which they are lowered and discharge occurs. Every body appears to discharge (444 987); but the possession of this capability in a *greater or smaller degree* in different bodies makes them better or worse conductors, worse or better insulators: and both *induction* and *conduction* appear to be the same in their principle and action (1320.), except that in the latter an effect common to both is raised to the highest degree, whereas in the former it occurs in the best cases in only an almost insensible quantity.

of shell-lac upon a gold-leaf electrometer and a charged carrier (an insulated metal ball of two or three inches diameter) upon it, the electrometer is diverged; removing the carrier, this divergence instantly falls, this is *insulation* and *induction*: if we replace the shell-lac by metal, the carrier causes the leaves to diverge as before; but when removed, though after the shortest possible contact, the electroscope is left diverged,—this is *conduction*. If we employ a plate of spermaceti instead of the metal, and repeat the experiment, we find the divergence partly falls and partly remains, because the spermaceti insulates and also conducts, doing both imperfectly: but the shell-lac also conducts, as is shown if time be allowed; and the metal also obstructs conduction, and therefore insulates, as is shown by simple arrangements. For if a copper wire, 74 feet in length and $\frac{1}{12}$th of an inch in diameter, be insulated in the air, having its end *m* a metal ball; its end *e* connected with the earth, and the parts near *m* and *e* brought within half an inch of each other, as at *s*; then an ordinary Leyden jar being charged sufficiently, its outside connected with *e* and its inside with *m*, will give a charge to the wire, which instead of travelling wholly through it, though it be so excellent a conductor, will pass in large proportion through the air at *s*, as a bright spark; for with such a length of wire, the resistance in it is accumulated until it becomes as much, or perhaps even more, than that of the air, for electricity of such high intensity.



Admitting that such and similar experiments show that conduction through a wire is preceded by the act of induction (1338.), then all the phænomena presented by the submerged or subterranean wires are explained; and in their explanation confirm, as I think, the principles given. After Mr. Wheatstone had, in 1834, measured the velocity of a wave of electricity through a copper wire, and given it as 288,000 miles in a second, I said, in 1838, upon the strength of these principles (1333.), "that the velocity of discharge through the *same wire* may be greatly varied, by attending to the circumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force, which tension is charge and induction. So if the two ends of the wire in Professor Wheatstone's experiment were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before; and if these two plates were the inner and outer coating of a large jar, or a Leyden battery, then the retardation of that spark would be still greater." Now this is precisely the case of the submerged or subterraneous wires, except that instead of carrying their surfaces towards the inducteous coatings (1483.), the latter are brought near the former; in both cases the induction consequent upon charge, instead of being exerted almost entirely at the moment within the wire, is to a very large extent determined externally; and so the discharge or conduction being caused by a lower tension, therefore requires a longer time. Hence the reason why, with 1500 miles of subterraneous wire, the wave was two seconds in passing from end to end; whilst with the same length of air wire, the time was almost inappreciable.

With these lights it is interesting to look at the measured velocities of electricity in wires of metal, as given by different experimenters.

		Miles per second.
*	Wheatstone, in 1834, with copper wire made it	288,000
*	Walker, in America, with telegraph iron wire .	18,780
*	O'Mitchell, ditto ditto .	28,524
*	Fizeau and Gonnelle (copper wire)	112,680
*	Ditto (iron wire)	62,600
†	A.B.G. (copper) London and Brussels Telegraph	2,700
†	Ditto (copper) London and Edinburgh Telegraph	7,600

Here the difference in copper is seen by the first and sixth result to be above a hundred fold. It is further remarked in Liebig's report of Fizeau and Gonnelle's experiments, that the velocity is not proportional to the conductive capacity, and is independent of the thickness of the wire. All these circumstances and incompatibilities appear rapidly to vanish, as we recognize and take into consideration the lateral induction of the wire carrying the current. If the velocity of a brief electric discharge is to be ascertained in a given length of wire, the simple circumstances of the latter being twined round a frame in small space, or spread through the air through a large space, or adhering to walls, or lying on the ground, will make a difference in the results. And in regard to long circuits, such as those described, their conducting power cannot be understood whilst no reference is made to their lateral static induction, or

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* Liebig and Kopp's Report, 1850 (translated), p. 168.

† Athenæum, January 14, 1854, p. 54.

to the conditions of intensity and quantity which then come into play; especially in the case of short or intermitting currents, for then static and dynamic are continually passing into each other.

It has already been said, that the conducting power of the air and water wires are alike for a constant current. This is in perfect accordance with the principles and with the definite character of the electric force, whether in the static, or current, or transition state. When a voltaic current of a certain intensity is sent into a long water-wire, connected at the further extremity with the earth, part of the force is in the first instance occupied in raising a lateral induction round the wire, ultimately equal in intensity at the near end to the intensity of the battery stream, and decreasing gradually to the earth end, where it becomes nothing. Whilst this induction is rising, that within the wire amongst its particles is beneath what it would otherwise be; but as soon as the first has attained its maximum state, then that in the wire becomes proportionate to the battery intensity, and therefore equals that in the air wire, in which the same state is (because of the absence of lateral induction) almost instantly attained. Then of course they discharge alike, and therefore conduct alike.

A striking proof of the variation of the conduction of a wire by variation of its lateral static induction is given in the experiment proposed sixteen years ago (1333.). If, using a constant charged jar, the interval *s*, page 203, be adjusted so that the spark shall freely pass there (though it would not if a little wider), whilst the short connecting wires *n* and *o* are insulated in the air, the experiment may be repeated twenty times without a single failure; but if after that, *n* and *o* be connected with the inside and outside of an insulated Leyden jar, as described, the spark will never pass across *s*, but all the charge will go round the whole of the long wire. Why is this? The quantity of electricity is the same, the wire is the same, its resistance is the the same, and that of the air remains unaltered; but because the intensity is lowered, through the lateral induction momentarily allowed, it is never enough to strike across the air at *s*; and it is finally altogether occupied in the wire, which in a little longer time than before effects the whole discharge. M. Fizeau has applied the same expedient to the primary voltaic currents of Ruhmkorff's beautiful inducting apparatus with great advantage. He thereby reduces the intensity of these currents at the moment when it would be very disadvantageous, and gives us a striking instance of the advantage of viewing static and dynamic phænomena as the result of the same laws.

Mr. Clarke arranged a Bains's printing telegraph with three pens, so that it gave beautiful illustrations and records of facts like those stated: the pens are iron wires, under which a band of paper imbued with ferro-prussiate of potassa passes at a regular rate by clock-work; and thus regular lines of prussian blue are produced whenever a current is transmitted, and the time of the current is recorded. In the case to be described, the three lines were side by side, and about 0·1 of an inch apart. The pen *m* belonged to a circuit of only a few feet of wire and a separate battery; it told whenever the contact key was put down by the finger; the pen *n* was at the earth end of the long air wire, and the pen *o* at the earth end of the long subterraneous wire; and by arrangement, the key could be made to throw the electricity of the chief battery into either of these wires, simultaneously with the passage of the short circuit current through pen *m*. When pens *m* and *n* were in action, the *m* record was a regular line of equal thickness, showing by its length the actual time during which the electricity flowed into the wires; and the *n* record was an equally regular line, parallel to, and of equal length with the former, but the least degree behind it; thus indicating that the long air wire conveyed its electric current almost instantaneously to the further end. But when pens *m* and *o* were in action, the *o* line did not begin until some time after the *m* line, and it continued after the *m* line had ceased, *i. e.* after the *o* battery was cut off. Furthermore, it was faint at first, grew up to a maximum of intensity, continued at that as long as battery contact was continued, and then gradually diminished to nothing. Thus the record *o* showed that the wave of power took time in the water wire to reach the further extremity; by its first faintness, it showed that power was consumed in the exertion of lateral static induction along the wire; by the attainment of a maximum and the after equality, it showed when this induction had become proportionate to the intensity of the battery current; by its beginning to diminish, it showed when the battery current was cut off; and its prolongation and gradual diminution showed the time of the outflow of the static electricity laid up in the wire, and the consequent regular falling of the induction which had been as regularly raised.

With the pens *m* and *o*, the conversion of an intermitting into a continuous current could be beautifully shown; the earth wire, by the static induction which it permitted, acting in a manner analogous to the fly-wheel of a steam-engine or the air-spring of a pump. Thus, when the contact key was regularly but rapidly depressed and raised, the pen *m* made a series of short lines separated by intervals of equal length. After four or more of these had passed, then pen *o*, belonging to the subterraneous wire, began to make its mark, weak at first, then rising to a maximum, but always continuous. If the action of the contact key was less rapid, then alternate thickening and attenuations appeared in the *o* record; and if the introductions of the electric current at the one end of the earth wire were at still longer intervals, the records of action at the other end became entirely separated from each other. All showing most beautifully how the individual current or wave, once introduced into the wire, and never ceasing to go onward in its course, could be affected in its intensity, its time and other circumstances, by its partial occupation in static induction.

By other arrangements of the pens *n* and *o*, the near end of the subterraneous wire could be connected with the earth immediately after separation from the battery; and then the back flow of the electricity, and the time and manner thereof, were beautifully recorded; but I must refrain from detailing results which have already been described in principle.

Many variations of these experiments have been made and may be devised. Thus the ends of the insulated battery have been attached to the ends of the long subterraneous wire, and then the two halves of the wire have given back opposite return currents when connected with the earth. In such a case the wire is positive and negative at the two extremities, being permanently sustained by its length and the battery, in the same condition which is given to the short wire for a moment by the Leyden discharge (p. 203); or, for an extreme but like case, to a filament of shell-lac having its extremities charged positive and negative. Coulomb pointed out the difference of long and short as to the insulating or conducting power of such filaments, and like difference occurs with long and short metal wires.

The character of the phænomena described in this report induces me to refer to the terms *intensity* and *quantity* as applied to electricity, terms which I have had such frequent occasion to employ. These terms, or equivalents for them, cannot be dispensed with by those who study both the static and the dynamic relations of electricity; every current where there is resistance has the static element and induction involved in it, whilst every case of insulation has more or less of the dynamic element and conduction; and we have seen that with the same voltaic source, the same current in the same length of the same wire gives a different result as the intensity is made to vary, with variations of the induction around the wire. The idea of intensity, or the power of overcoming resistance, is as necessary to that of electricity, either static or current, as the idea of pressure is to steam in a boiler, or to air passing through apertures or tubes; and we must have language competent to express these conditions and these ideas. Furthermore, I have never found either of these terms lead to any mistakes regarding electrical action, or give rise to any false view of the character of electricity or its unity. I cannot find other terms of equally useful significance with these; or any, which, conveying the same ideas, are not liable to such misuse as these may be subject to. It would be affectation, therefore, in me to search about for other words; and besides that, the present subject has shown me more than ever their great value and peculiar advantage in electrical language.

Note.—The fuse referred to in page 199 is of the following nature:—Some copper wire was covered with sulphuretted gutta percha; after some months it was found that a film of sulphuret of copper was formed between the metal and the envelope; and further, that when half the gutta percha was cut away in any place, and then the copper wire removed for about a quarter of an inch, so as to remain connected only by the film of sulphuret adhering to the remaining gutta percha, an intensity battery could cause this sulphuret to enter into vivid ignition, and fire gunpowder with the utmost ease. The experiment was shown in the lecture-room, of firing gunpowder at the end of eight miles of single wire. Mr. Faraday reported that he had seen it fired through 100 miles of covered wire immersed in the canal by the use of this fuse.

XXX. *On the Homographic Transformation of a Surface of the Second Order into itself**. *By* A. Cayley, *Esq.*†

I PASS to the improper transformation. Sir W. R. Hamilton has given (in the note, p. 723 of his Lectures on Quaternions) the following theorem:—If there be a polygon of $2m$ sides inscribed in a surface of the second order, and $(2m-1)$ of the sides pass through given points, then will the $2m$th side constantly touch two cones circumscribed about the surface of the second order. The relation between the extremities of the $2m$th side is that of two points connected by the general improper transformation; in other words, if there be on a surface of the second order two points such that the line joining them touches two cones circumscribed about the surface of the second order, then the two points are as regards the transformation in question a pair of corresponding points, or simply a pair. But the relation between the two points of a pair may be expressed in a different and much more simple form. For greater clearness call the surface of the second order U, and the sections along which

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* See Phil. Mag. vol. vi. p. 326 (Nov. 1853).

† Communicated by the Author.

it is touched by the two cones, θ, ϕ; the cones themselves may, it is clear, be spoken of as the cones θ, ϕ. And let the two points be P, Q. The line PQ touches the two cones, it is therefore the line of intersection of the tangent plane through P to the cone θ, and the tangent plane through P to the cone ϕ. Let one of the generating lines through P meet the section θ in the point A, and the other of the generating lines through P meet the section ϕ in the point B. The tangent planes through P to the cones θ, ϕ respectively are nothing else than the tangent planes to the surface U at the points A, B respectively. We have therefore at these points two generating lines meeting in the point P; the other two generating lines at the points A, B meet in like manner in the point Q. Or P, Q are opposite angles of a skew quadrangle formed by four generating lines (or, what is the same thing, lying upon the surface of the second order), and having its other two angles, one of them on the section θ and the other on the section ϕ; and if we consider the side PA as belonging determinately to one or the other of the two systems of generating lines, then when P is given, the corresponding point Q is, it is clear, completely determined. What precedes may be recapitulated in the statement, that in the improper transformation of a surface of the second order into itself, we have, as corresponding points, the opposite angles of a skew quadrangle lying upon the surface, and having the other two opposite angles upon given plane sections of the surface. I may add, that attending only to the sections through the points of intersection of θ, ϕ, if the point P be situate anywhere in one of these sections, the point Q will be always situate in another of these sections, *i. e.* the sections correspond to each other in pairs; in particular, the sections θ, ϕ are corresponding sections, so also are the sections Θ, Φ (each of them two generating lines) made by tangent planes of the surface. Any three pairs of sections form an involution; the two sections which are the sibiconjugates of the involution are of course such, that, if the point P be situate in either of these sections, the corresponding point Q will be situate in the same section. It may be noticed that when the two sections θ, ϕ coincide, the line joining the corresponding points passes through a fixed point, viz. the pole of the plane of the coincident sections; in fact the lines PQ and AB are in every case reciprocal polars, and in the prsent case the line AB lies in a fixed plane, viz. the plane of the coincident sections, the line PQ passes therefore through the pole of this plane. This agrees with the remarks made in the first part of the present paper.

The analytical investigation in the case where the surface of the second order is represented under the form $xy-zw=0$ is so simple, that it is, I think, worth while to reproduce it here, although for several reasons I prefer exhibiting the final result in relation to the form $x^2+y^2+z^2+w^2=0$ of the equation of the surface of the second order. I consider then the surface $xy-zw=0$, and I take $(\alpha,\beta,\gamma,\delta)$, $(\alpha',\beta',\gamma',\delta')$ for the coordinates of the poles of the two sections θ, ϕ, and (x_1, y_1, z_1, w_1), (x_2, y_2, z_2, w_2) as the coordinates of the points P, Q. We have of course $x_1y_1-z_1w_1=0$, $x_2y_2-z_2w_2=0$. The generating lines through P are obtained by combining the equation $xy-zw=0$ of the surface with the equation $xy_1+yx_1-zw_1-wz_1=0$ of the tangent plane at P. Eliminating x from these equations, and replacing in the result x_1 by its value $\frac{z_1w_1}{y_1}$, we have the equation

$$(yz_1-zy_1)(yw_1-wy_1)=0.$$

We may if we please take $yz_1-zy_1=0$, $xy_1+yx_1-zw_1-wz_1=0$ as the equations of the line PA; this leads to

$$\left.\begin{array}{l} yz_1-zy_1=0 \\ xy_1+yx_1-zw_1-wz_1=0 \end{array}\right\} \quad \left.\begin{array}{l} yw_2-wy_2=0 \\ xy_2+yx_2-zw_2-wz_2=0 \end{array}\right\}$$

for the equations of the lines PA, QA respectively; and we have therefore the coordinates of the point A, coordinates which must satisfy the equation $\beta x+\alpha y-\delta z-\gamma w=0$ of the plane θ. This gives rise to the equation

$$y_2(\alpha y_1-\delta z_1)-w_2(\gamma y_1-\beta z_1)=0.$$

We have in like manner

$$\left.\begin{array}{l} yw_1-y_1w=0 \\ xy_1+yx_1-zw_1-wz_1=0 \end{array}\right\} \quad \left.\begin{array}{l} yz_2-zy_2=0 \\ xy_2+yx_2-zw_2-wz_2=0 \end{array}\right\}$$

for the equations of the lines PB, QB respectively; and we may thence find the coordinates of the point B, coordinates which must satisfy the equation $\beta'x+\alpha'y-\delta'z-\gamma'w=0$ of the plane ϕ. This gives rise to the equation

$$y_2(\alpha'y_1-\gamma'w_1)-z_2(\delta'y_1-\beta'w_1).$$

It is easy by means of these two equations and the equation $x_2y_2-z_2w_2=0$ to form the system

$$\begin{aligned} x_2&=(\alpha y_1-\delta z_1)(\alpha'y_1-\gamma'w_1) \\ y_2&=(\gamma y_1-\beta z_1)(\delta'y_1-\beta'w_1) \\ z_2&=(\gamma y_1-\beta z_1)(\alpha'y_1-\gamma'w_1) \\ w_2&=(\alpha y_1-\delta z_1)(\delta'y_1-\beta'w_1); \end{aligned}$$

or, effecting the multiplications and replacing z_1w_1 by x_1y_1, the values of x_2, y_2, z_2, w_2 contain the common factor y_1, which may be rejected. Also introducing on the left-hand sides the common factor MM′, where $M^2=\alpha\beta-\gamma\delta$, $M'^2=\alpha'\beta'-\gamma'\delta'$, the equations become

$$\begin{aligned}
\mathrm{MM}'x_2&=\gamma'\delta x_1+\alpha\alpha' y_1-\alpha'\delta z_1-\alpha\gamma' w_1\\
\mathrm{MM}'y_2&=\beta\beta' x_1+\gamma\delta' y_1-\beta\delta' z_1-\beta'\gamma w_1\\
\mathrm{MM}'z_2&=\beta\gamma' x_1+\gamma\alpha' y_1-\beta\alpha' z_1-\gamma\gamma' w_1\\
\mathrm{MM}'w_2&=\beta'\delta x_1+\alpha\delta' y_1-\delta\delta' z_1-\alpha\beta' w_1,
\end{aligned}$$

values which give identically $x_2y_2-z_2w_2=x_1y_1-z_1w_1$. Moreover, by forming the value of the determinant, it is easy to verify that the transformation is in fact an improper one. We have thus obtained the equations for the improper transformation of the surface $xy-zw=0$ into itself. By writing x_1+iy_1, x_1-iy_1 for x_1, y_1, &c., we have the following system of equations, in which (a, b, c, d), (a', b', c', d') represent, as before, the coordinates of the poles of the plane sections, and $\mathrm{M}^2=a^2+b^2+c^2+d^2$, $\mathrm{M}'^2=a'^2+b'^2+c'^2+d'^2$, viz. the system*

$$\begin{aligned}
\mathrm{MM}'x_2=&(aa'-bb'-cc'-dd')x_1+(ab'+a'b+cd'-c'd)y_1\\
&+(ac'+a'c+db'-d'b)z_1+(ad'+a'd+bc'-b'c)w_1\\
\mathrm{MM}'y_2=&(ab'+a'b-cd'+c'd)x_1+(-aa'+bb'-cc'-dd')y_1\\
&+(bc'+b'c-da'+d'a)z_1+(bd'+b'd-ac'+a'c)w\\
\mathrm{MM}'z_2=&(ac'+a'c-db'+d'b)x_1+(bc'+b'c-ad'+a'd)y_1\\
&+(-aa'-bb'+cc'-dd')z_1+(cd'+c'd-ba'+b'a)w_1\\
\mathrm{MM}'w_2=&(ad'+a'd-bc'+b'c)x_1+(bd'+b'd-ca'+c'a)y_1\\
&+(cd'+c'd-ab'+a'b)z_1+(-aa'-bb'-cc'+dd')w_1,
\end{aligned}$$

values which of course satisfy identically ${x_2}^2+{y_2}^2+{z_2}^2+{w_2}^2={x_1}^2+{y_1}^2+{z_1}^2+{w_1}^2$, and which belong to an improper transformation. We have thus obtained the improper transformation of the surface of the second order $x^2+y^2+z^2+w^2=0$ into itself.

Returning for a moment to the equations which belong to the surface $xy-zw=0$, it is easy to see that we may without loss of generality write $\alpha=\beta=\alpha'=\beta'=0$; the equations take then the very simple form

$$\mathrm{MM}'x_2=\gamma'\delta x_1,\quad \mathrm{MM}'y_2=\gamma\delta' y_1,\quad \mathrm{MM}'z_2=-\gamma\gamma' w_1,$$
$$\mathrm{MM}'w_2=-\delta\delta' z_1,$$

where $\mathrm{MM}'=\sqrt{-\gamma\delta}\sqrt{-\gamma'\delta'}$; and it thus becomes very easy to verify the geometrical interpretation of the formulæ.

It is necessary to remark, that, whenever the coordinates of

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* The system is very similar in form to, but is *essentially* different from, that which could be obtained from the theory of quaternions by writing

$$\mathrm{MM}'(w_2+ix_2+jy_2+kz_2)=$$
$$(d+ia+jb+kc)(w+ix+jy+kz)(d'+ia'+jb'+kc');$$

the last-mentioned transformation is, in fact, *proper*, and not *improper*.

the points Q are connected with the coordinates of the points P by means of the equations which belong to an improper transformation, the points P, Q have to each other the geometrical relation above mentioned, viz. there exist two plane sections θ, ϕ such that P, Q are the opposite angles of a skew quadrangle upon the surface, and having the other two opposite angles in the sections θ, ϕ respectively. Hence combining the theory with that of the proper transformation, we see that if A and B, B and C ... M and N are points corresponding to each other properly or improperly, then will N and A be points corresponding to each other, viz. properly or improperly, according as the number of the improper pairs in the series A and B, B and C ... M and N is even or odd; *i. e.* if all the sides but one of a polygon satisfy the geometrical conditions in virtue of which their extremities are pairs of corresponding points, the remaining side will satisfy the geometrical condition in virtue of which its extremities will be a pair of corresponding points, the pair being proper or improper according to the rule just explained.

I conclude with the remark, that we may by means of two plane sections of a surface of the second order obtain a proper transformation. For if the generating lines through P meet the sections θ, ϕ in the points A, B respectively, and the remaining generating lines through A, B respectively meet the sections ϕ, θ respectively in B$'$, A$'$, and the remaining generating lines through B$'$, A$'$ respectively meet in a point P$'$, then will P, P$'$ be a pair of corresponding points in a proper transformation. In fact the generating lines through P meeting the sections θ, ϕ in the points A, B respectively, and the remaining generating lines through A, B respectively meeting as before in the point Q, P and Q will correspond to each other improperly, and in like manner P$'$ and Q will correspond to each other improperly, *i. e.* P and P$'$ will correspond to each other properly. The relation between P, P$'$ may be expressed by saying that these points are opposite angles of the skew hexagon PAB$'$ P$'$A$'$B lying upon the surface, and having the opposite angles A, A$'$ in the section θ, and the opposite angles B, B$'$ in the section ϕ. It is, however, clear from what precedes, that the points P, P$'$ lie in a section passing through the points of intersection of θ, ϕ, *i. e.* that the proper transformation so obtained is not the general proper transformation.

2 Stone Buildings, Jan. 11, 1854.

XXXI. *On a particular case of Elliptic Integrals whose Parameters are Imaginary. By the* Rev. JAMES BOOTH, *LL.D., F.R.S. &c.*[*]

THE arc of a spherical parabola may be represented by the sum of two elliptic integrals of the third order, whose parameters are *imaginary* and *reciprocal*. The spherical conic section, whose principal arcs 2α and 2β are connected by the equation $\tan^2\alpha-\tan^2\beta=1$, is called the spherical parabola.

If we assume the expression given at page 32 of the Theory of Elliptic Integrals[†], for an arc of the spherical parabola, the focus being the pole, and ϕ the angle which the perpendicular arc from the focus, on the tangent arc of a great circle to the curve, makes with the principal transverse arc, γ being the *modular* angle, or the angle between the focus and the adjacent vertex,

$$\sigma=\sin\gamma\int\frac{d\phi}{\sqrt{1-\cos^2\gamma\sin^2\phi}}+\tan^{-1}\left[\frac{\sin\gamma\tan\phi}{\sqrt{1-\cos^2\gamma\sin^2\phi}}\right]. \quad (1)$$

Let $\cos\gamma=i$, $\sin\gamma=j$. Then differentiating the preceding equation, it will become, after some reductions,

$$\frac{d\sigma}{d\phi}=\frac{j[1-i^2\sin^2\phi+\cos^2\phi+j^2\sin^2\phi]}{[\cos^2\phi-i^2\sin^2\phi\cos^2\phi+j^2\sin^2\phi]\sqrt{1-i^2\sin^2\phi}}. \quad . \quad (2)$$

Now the numerator is equivalent to $2j(1-i^2\sin^2\phi)$, and the denominator may be written in the form $1-2i^2\sin^2\phi+i^2\sin^4\phi$. But $i^2=i^2(i^2+j^2)$, hence this last expression may be put under the form $1-2i^2\sin^2\phi+i^4\sin^4\phi+i^2j^2\sin^4\phi$. This expression is the sum of two squares. Resolving this sum into its constituent factors, we shall find

$$\frac{d\sigma}{d\phi}=\frac{2j(1-i^2\sin^2\phi)}{[1-i(i+j)\sqrt{-1})\sin^2\phi][1-i(i-j\sqrt{-1})\sin^2\phi]\sqrt{1-i^2\sin^2\phi}}. \quad (3)$$

This product may be resolved into the sum of two terms. Let

$$\frac{d\sigma}{d\phi}=\frac{\mathrm{A}}{[1-i(i+j\sqrt{-1})\sin^2\phi]\sqrt{1-i^2\sin^2\phi}}+\frac{\mathrm{B}}{[1-i(i-j\sqrt{-1})\sin^2\phi]\sqrt{1-i^2\sin^2\phi}}; \quad (4)$$

or reducing these expressions to a common denominator,

$$\frac{d}{d\phi}=\frac{(\mathrm{A}+\mathrm{B})-(\mathrm{A}+\mathrm{B})i^2\sin^2\phi+(\mathrm{A}-\mathrm{B})\sqrt{-1}ij\sin^2\phi}{[1-i(i+j\sqrt{-1})\sin^2\phi][1-i(i-j\sqrt{-1})\sin^2\phi]\sqrt{1-i^2\sin^2\phi}}. \quad (5)$$

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[*] Communicated by the Author.

[†] The Theory of Elliptic Integrals and the properties of Surfaces of the second order applied to the investigation of the motion of a body round a fixed point. 8vo. London, G. Bell. 1851.

Comparing this expression with (3), $A+B=2j$, $A-B=0$, or $A=j$, $B=j$. Whence integrating (4), we get

$$\sigma=j\int\frac{d\phi}{[1-i(i+j\sqrt{-1})\sin^2\phi]\sqrt{1-i^2\sin^2\phi}}+j\int\frac{d\phi}{[1-i(i-j\sqrt{-1})\sin^2\phi]\sqrt{1-i^2\sin\phi}}. \quad (6)$$

Hence σ, the arc of a spherical parabola, may be represented by the sum of two elliptic integrals whose parameters are imaginary.

Now if we multiply together the imaginary parameters $(i^2+ij\sqrt{-1})$ and $(i^2-ij\sqrt{-1})$, their product is i^2, or the parameters are reciprocal.

It is very remarkable, that although the spherical parabola is a spherical conic, the imaginary parameters satisfy the criterion of conjugation which belongs to the logarithmic form, and not that of the circular form. Let $m=i(i-j\sqrt{-1}), n=i(i+j\sqrt{-1})$. These values of m and n satisfy the equation of logarithmic conjugation $m+n-mn=i^2$, and not $m-n+mn=i^2$, the condition of circular conjugation.

The arc of a circle may in like manner be represented as the sum of two imaginary integrals of the logarithmic form, or by an imaginary arc of a parabola.

Let $x=\tan\theta$. Then $\theta=\int\frac{dx}{1+x^2}$. Hence, resolving the denominator into its factors, and reducing,

$$2\theta=\int\frac{dx}{1+\sqrt{-1}x}+\int\frac{dx}{1-\sqrt{-1}x}, \quad . \quad . \quad . \quad (7)$$

whence

$$\frac{\pi}{2}=\int_0^1\frac{dx}{1+\sqrt{-1}x}+\int_0^1\frac{dx}{1-\sqrt{-1}x}. \quad . \quad . \quad . \quad (8)$$

Multiply (7) by $\sqrt{-1}$; then integrating,

$$\left.\begin{aligned} &2\theta\sqrt{-1} \quad l(1+\sqrt{-1}x)-l(1-\sqrt{-1}x), \\ \text{or} \quad &4\theta\sqrt{-1}=l(1+\sqrt{-1}x)^2-l(1-\sqrt{-1}x)^2. \end{aligned}\right\} . \quad (9)$$

Let $x=1$, then $\theta=\frac{\pi}{4}$, whence (9) may be reduced to

$$\pi\sqrt{-1}=\log(-1);$$

a result we may otherwise obtain. As also $\frac{\pi}{2}\sqrt{-1}=\log\sqrt{-1}$.

We see, therefore, that the presence of the imaginary symbol $\sqrt{-1}$ indicates a transition from parabolic to circular arcs, or conversely. In a paper published in the Philosophical Transactions for 1852, it has been shown how the transition may be made from the properties of the circle to their analogues in the parabola.

The difference Δ between the arc of a parabola and its protangent may be expressed by an imaginary circular arc, meaning by *protangent* the projection of the radius vector on the tangent to the curve. As $\Delta=\int\frac{d\theta}{\cos\theta}$, let $\sin\theta=x$, then $\Delta=\int\frac{dx}{1-x^2}$, or $\Delta=\int\frac{dx}{1+(\sqrt{-1}x)^2}$, whence

$$\Delta=\frac{\tan^{-1}(\sqrt{-1}x)}{\sqrt{-1}}. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

This gives a new meaning to the symbol $\sqrt{-1}$. It does not merely signify a change of angular position through a right angle; it has a meaning much more extensive. It indicates, as we have shown[*], that a property which is imaginary relatively to the curve in question, whether it be a circle or a parabola, becomes real when transferred to the parabola or the circle. Thus imaginary logarithms represent real circular arcs; and De Moivre's theorem is an example of a property of imaginary circular arcs, which, when transferred to the parabola, becomes a real positive geometrical theorem. It would be easy to multiply examples, the above will suffice.

XXXII. *On the Colours of Mausite.* *By* W. HAIDINGER[†].

"THE hexagonal salt of iron $Fe^2O^3, SO^3+3(KO)SO^3+3H^2O$, which furnished me such interesting results with respect to the presence of basic water[‡], forms crystals which, perpendicular to their axes, allow *green* light, and parallel to their axes allow yellow or red-brown light (according to the length of the six-sided columns or tables) to pass through them. This salt, however, contains *no trace* of protoxide of iron, but only oxide of iron, sulphuric acid, potash and water (basic water and water of crystallization). Under certain circumstances, the same salt forms a crystalline powder of a beautiful greenfinch colour, a tint wherein yellow and green are both distinguishable."

Thus writes my respected friend Prof. Scheerer of Freiburg, in a letter addressed to me on the 10th of May, 1853, concerning the crystals whose properties of colour form the subject of the following remarks.

The above memoir, "Beiträge zur näheren Kenntniss des

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[*] Philosophical Transactions for 1852, part 2. p. 390; also see Cambridge and Dublin Mathematical Journal for 1853.

[†] Translated from Poggendorff's *Annalen*, vol. xc. p. 474; having been communicated to the Academy at Vienna, July 1853.)

[‡] Poggendorff's *Annalen*, vol. xi. p 73.

polymeren Isomorphismus," in Poggendorff's *Annalen*, as well as Gmelin's Handbook of Chemistry (vol. iii. p. 255), contain full descriptions of the above salt, which was first obtained by Maus*, and which Scheerer for brevity called Maus's salt†, a term which I have here adopted as the root of specific nomenclature and transformed into *Mausite*. The difficulty, almost impossibility, of forming a systematic chemical nomenclature will, I think, sufficiently justify the specific name I have proposed in the title.

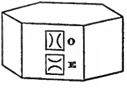
According to Scheerer, the composition of Mausite is—

Sulphuric acid	41·94
Peroxide of iron . . .	20·97
Potash	20·58
Basic water	9·43
Water of crystallization .	7·08
	100·00

Analyses were made some time ago by Maus, Anthon and W. Richter, and since then by R. Richter in Scheerer's laboratory. The latter analysed not only crystals which were obtained in Scheerer's laboratory, but other larger ones obtained in Schweizer's laboratory at Zurich, by W. Hilgard of Illinois.

According to a later account, crystals of Mausite may be obtained by simply placing a flat vessel containing a solution of common alum-iron in a room at the ordinary temperature, and allowing it there to evaporate. At length Prof. Scheerer sent me a number of small crystals, which, on an immediate examination, exhibit properties so unexpected, that I willingly take advantage of the last meeting in the academical year to communicate my observations to the Mathematical and Physical Class; even before I have had time, from a process of crystallization now in progress, to obtain larger specimens upon which, perhaps, observations may ultimately be made with greater precision.

In agreement with Scheerer's description, the crystals of Mausite are regular hexahedral prisms; those before me measure about half a line in every direction, though many of equal thickness and in perfectly parallel position group themselves to hexahedral tables which measure as much as three lines. Instead of the surface at the end, appears a quite flat quartzoid. The thickness being half a line, I distinguish the following tints according to the usual mineralogical nomenclature. Colour of the base O hyacinth-red. Colour of the axis E olive-green. The distinction in colour is uncommonly clear, and increases with the thickness.



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* Poggendorff's *Annalen*, vol. xi. p. 78.

† Ibid. p. 77.

It is true the lighter olive-green is sometimes rather more intense, more yellow, but instead of the red tint a perfectly black one appears; in the direction of its axis the crystal is opake. It would be possible to use plates of Mausite, about a line in thickness, as we use the best tourmaline if they were cut parallel to the axis, and placed between two polished glass plates. These small prisms and plates are not so thick, but the tints may be best compared through a dichroscopic lens, as shown in the above figure. When the thickness is less, however, the hyacinth becomes clearer, it passes into a liver-colour, which exhibits more and more yellow, and passing through an actual *olive-green*, indistinguishable from the colour of the axis in other specimens, attains a still clearer tint. If a crystal be split with a knife, and the splinter be magnified sixty times in order to examine it, the two yellow tints are found almost to coincide, except that the one is nearly colourless. Microscopes are now often constructed for polarized light. But when this is not the case, it suffices to hold a crystal of Iceland spar between the ocular and the eye. On the other hand, however, the above-mentioned broader, table-like groups of crystals are themselves so dark as to have reached the liver-coloured tint immediately preceding hyacinth-red. Hence the series of colours for the ordinary and extraordinary rays are identical,—white, olive-green, liver-colour, hyacinth-red, black. But the ordinary ray is more absorbed, the darker tints making their appearance at less thicknesses. At present the proportion of these thicknesses corresponding to the same tint for both rays, may be stated as being about 1 : 8 or 1 : 10, until future experiments on larger and more complete crystals shall have furnished numerical data; then, too, it may be decided whether the more absorbed ray is also most refracted, as required by the general law which embraces both Babinet's laws.

The actual object of the present communication, however, is to direct attention to an indubitable consequence of the above observations; that is, the proof of the fact that there are strong dichromatic crystals in which the differences of tint in different directions do not arise from absolute differences of colour, but depend upon the greater and less absorption of the light in passing through such crystals.

Certainly this difference arising from greater and less absorption could not account for the change of the dark or clear blue tint of Cordierite into a yellowish-white, or, in darker crystals, even to a honey-yellow tint; neither can the blackish-blue (ink-blue, purple), beautiful violet-blue, and asparagus-green colours of diaspore arise, one from the other, by greater or less absorption. Their theoretical consideration, in reference to the position and form of the colouring bodies, or to the general arrangement of the ultimate particles, is consequently much more difficult than with crystals such as Mausite. Here, notwithstanding the dichromatism, one body alone, peroxide of iron, suffices; for it is sufficient to assume that the particles are so arranged, that when a ray passes through the hexahedral prism from surface to surface, becoming thereby divided into two polarized perpendicular to one another, it meets a greater number of material points or particles with the vibrations of the ordinary ray perpendicular to the axis, than with those of the extraordinary ray in the direction of the axis.

Hence the differences of tint, even the olive-green, which in comparison with hyacinth-red appears still more green, present no difficulty to this explanation. They depend simply upon greater or less absorption. On this account, however, the colours of crystals which contain *peroxide of iron* in combination with oxalic acid and alkalies appear all the more enigmatical; for even when they are dichromatic, the most beautiful *grass-green* is coexistent only with quite pale, yellowish-white tints, which at furthest reach but to olive-green.

XXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 149.]

Dec. 22, 1853.—Thomas Graham, Esq., V.P., in the Chair.

THE following paper was read:—

"An Inquiry into some of the circumstances and principles which regulate the production of Pictures on the Retina of the Human Eye, with their measure of endurance, their Colours and Changes." By the Rev. W. Scoresby, F.R.S., Corresponding Member of the Institute of France, &c.

The investigations of the author embrace three distinct cases,—the case of achromatic pictures; that of coloured pictures of uncoloured objects, derived simply or mainly from the influence of light on the eye; and that of the spectra of coloured objects, together with certain applications of the results obtained to other optical characteristics, determinations or phænomena.

The general mode of experiment employed in these researches is described as "the viewing of illuminated objects with a steady fixed gaze at a special point, and then determining the impression on the retina by examining the images developed *with closed eyes*." The time of viewing the objects varied from a momentary glance up to half a minute, more rarely to a minute; and the mode of eliciting the impression was, primarily, by closing the eyelids into gentlest contact, whilst the head was kept unmoved, and the eyelids steady in their original direction. Thus performed, the experiment becomes very simple and manageable, and the results, various as they are in colour or depth of tint, are almost unfailingly elicited and often curious or beautiful.

Whilst the general result of viewing an illuminated object is the production of a clearly-defined picture on the retina, appearing in certain cases instantly, or more commonly, from 3 to 5 seconds after the eyes are closed,—the nature or quality of the picture, with its degree of endurance and changes, is found to present, under differences in the circumstances, an almost endless variety. Thus the results, it was found, might be varied by differences in the time of gazing on the object; by differences in the intensity of the external light, and by the partial or total exclusion of the light of the room from the eyelids; by alterations in the degree of compression of the eyelids; by the movement of the eyeballs during the time of observing the picture; as also by variations in the normal state of the eyes on commencing the experiments.

All these influencing circumstances had been made successively, or sometimes combinedly, the subjects of special investigation by the author; and ultimately, in most respects, he considered, so far as his own eyes might be deemed to yield *general* phænomena, with satisfactory or conclusive results. Various experiments had been made on the spectra derived from light reflected from opake objects in comparison with those elicited by light transmitted by transparent substances, both white and coloured; as also on the differences in the measure of endurance, the variety of their repetitions, and the phænomena of their changes in colour, of the pictures photographed within the eye, under curiously modified conditions.

The present communication, however, comprises only a part of these investigations, the first of the cases referred to at the outset, viz. inquiries respecting *colourless pictures* on the retina, derived from the viewing of objects under low or moderate degrees of light, or of pictures observed irrespective of chromatic effects.

1. As to the effect of *Time* in the viewing of an illuminated object, on the nature and permanency of the picture produced, it was found that, in favourable states of the eyes, a mere momentary glance (such as of a window viewed from the back of a room) was sufficient for producing a distinct negative picture of the illuminated aperture, with the cross bars of the window-frame, which, under certain changes, could be seen ordinarily for about 20 seconds, and under strong light, sometimes for an interval of a minute or two of time, if not more. But the impression from a continuous viewing of a window rather strongly illuminated, for a period of a minute, was very remarkable, the image remaining on the retina whilst the experimenter was breakfasting, and also engaged in writing, so as to be distinctly seen, on slightly closing the eyes, an hour afterwards, and, in another case which he particularly describes, after a lapse of 80 minutes.

2. Experiments on the effects of *quantity* or *intensity* of light, on the visual spectra derived from uncoloured objects, showed that

such spectra were yielded by extremely low degrees of illumination. The light, for instance, of the moon or stars thrown on a white linen blind, produced distinct negative pictures of the slightly illuminated aperture. Candlelight gave also negative pictures of white and black objects. Low illumination from transmitted solar light gave, in most cases, colourless pictures, appearing sometimes immediately on closing the eyes, as by a flash of light, or otherwise in 3 to 5 seconds in negative tints; these pictures, where the object had been viewed for some seconds, were found to fade away and subsequently reappear in less dark shades, sometimes with several such changes.

3. The changes in the optical spectra from the *partial* or *entire exclusion of light* from the closed eyelids were found to be very striking. No matter how this diminution or exclusion of light was effected,—whether by the thickening of the eyelids by compression, or turning the face away from the light, or interposing the hand or other opake substance betwixt the eyes and the light, or covering the face altogether,—the spectra assumed a new character as to light and shadow, ordinarily, but not in all cases, complementary to the tints originally observed. A total exclusion of the external light still left the picture clear and distinct, with a continuance, after occasional changes, little differing from that of other experiments.

4. This measure of fixidity of the spectra impressed on the retina led the author to some curious results in obtaining duplicate or *multiple pictures* of the same object. Thus, by gazing at a window, successively at different fixed points previously determined on, he multiplied the cross bars so as to produce a picture of a window with twice or quadruple the number of panes. A white statuette, viewed at different points in succession, whilst strongly illuminated, enabled the author to obtain double pictures in black or gray, associated according to the relation of the points gazed at, in unlimited variety. Or viewing the statuette from two positions differing in distance, he obtained images of different dimensions. Double images, too, were obtained by using the eyes separately; and also by looking at an object nearer to the eyes than the statuette, so that the lines of the axes might diverge at the distance of the statuette, thus beautifully elucidating one of the chief causes of the indistinctness of vision as to objects nearer to, or more remote from the eyes than that directly contemplated.

5. Complete pictures were also obtained by the *combination of parts* separately viewed, whilst various impressions, however incongruous, were combined into one picture. Thus parts of the statuette were viewed, under the adoption of a moveable screen, so as either to combine the separately-viewed portions rightly, or to transfer one part, such as the head, to either shoulder, or to adjust two heads in different positions. Separate impressions, also, of segments of the statuette were taken on the eyes singly, and these combined, accordingly as the same or different points of view were selected, into perfect or distorted pictures. The appearance of the parts of the

resulting spectrum, however, were not always synchronous portions, sometimes appearing and disappearing by separate or partial changes, like the effect of the dissolving views of the magic lantern.

6. Pictures, diagrams, printing, &c., were found, under due influence of light, to yield cognizable and sometimes vivid impressions on the retina. Diagrams in black and white, or chequered surfaces like that of a chess-board, gave very distinct pictures, always negative, the squares coming successively into view, beginning with the portion gazed at. The succession of changes, when the impression was strong, in this experiment was not a little curious, the perfect image of the chess-board after bursting into view, gradually fading altogether away, and then reviving, in less strong tints, in a series of repetitions.

Another curious, though anticipated result, the author also describes under this section,—the determination, by viewing the ocular spectra, of portions of diagrams or elements in pictorial or typographical surfaces, which had not been noticed in the act of gazing. Thus, particularly on viewing a line of printed figures at a particular point, without noticing those on either side, a considerable series, right and left, were so plainly depicted on the visual organ as to be easily known; whilst, in like manner, a point in a line of a printed placard being gazed at, the lines above and below came into view on closing the eyes, and could frequently be read.

Of certain general facts elicited by this first series of investigations, the author notices, that in viewing impressions on the retina with closed eyes, all the pictures appear to occupy a position *externally*, similar to the effect when the objects are directly seen; that the spectra derived from moderate or strong degrees of transmitted light have prevalently the character of transparency, and those from very low degrees, most ordinarily, of opacity; that although many of the spectral phænomena the author had observed were well known to be capable of elicitation in the ordinary form of the experiment with the eyes open, yet the series of phænomena, as a whole, could not be so elicited, nor was it possible by such form of experiment to analyse and compare the phænomena whilst in progress of change, which, in the form he had adopted, were usually exhibited as plainly as if the spectra were the real and immediate effects of ordinary direct vision; and that such is the precision and such the certainty with which the pictures are ordinarily developed, after duly viewing any illuminated object, that the expected result, so far as the eliciting of definite pictures is concerned, hardly ever fails.

Jan. 12, 1854.—The Lord Chief Baron, V.P., in the Chair.

A paper was read, entitled "On some New and Simple Methods of detecting Manganese in Natural and Artificial Compounds, and of obtaining its Combinations for œconomical or other uses." By Edmund Davy, Esq., F.R.S., M.R.I.A. &c.

In this paper the growing importance of manganese since its discovery, and its extensive distribution in Nature are noticed. Manganese is chiefly found combined with oxygen, but its oxides are commonly mixed with those of iron, and though different methods of separating them have been recommended, yet no very simple or unobjectionable test for manganese seems to be known. Two methods for detecting manganese are recommended, viz.—

1. The pure hydrated fixed alkalies, potash and soda, and especially potash. 2. Sulphur.

With regard to the first method. Though the compound *Chameleon mineral* made by strongly heating nitre or potash and peroxide of manganese together, has long been known, yet it appears hitherto to have escaped observation, that potash seems to be a more delicate test of manganese than any other known substance. The use of potash in this way is simple and easy; it is employed in solution; equal weights of the alkali and water form a fluid well-adapted for the purpose; different metals may be used in the form of slips on which to make the experiments, but a preference is given to silver foil, as it is less acted on by alkalies than platina, and is more readily cleaned. A slip of such foil, about two or three inches in length and half an inch wide, answers well. Solids, to be examined for manganese, are finely pulverized; fluids require no preparation; the smallest portion of either is mixed with a drop or part of a drop of the alkali on the foil and heated by a spirit-lamp (for many experiments a candle affords sufficient heat), when on boiling the alkali to dryness and raising the heat, the characteristic green manganate of potash will appear on the foil. The delicacy of the alkali as a test thus applied, will be obvious on using the most minute portions of manganese ores in fine powder, and the author's son, Dr. E. W. Davy, readily detected manganese in a single drop of a solution containing one grain of solid sulphate in ten thousand grains of water. The presence of other oxides do not appear to impair the efficacy of this test. A strong solution of hydrate of soda in water, used in a similar manner, affords an excellent test for manganese, little inferior in delicacy to potash, but the latter is shown to be preferable.

Carbonate of soda has long been regarded as one of the most delicate tests of manganese, especially if aided by a little nitrate or chlorate of potash, but that carbonate is much inferior as a test for manganese to potash or soda, requiring a far higher temperature to form the manganate of soda, and the aid of oxidizing substances, as nitre and chlorate of potash, which are quite unnecessary with those alkalies. Borax, too, in point of delicacy is not to be compared with the fixed alkalies as a test for manganese.

The author is of opinion that the fixed alkalies in solution and silver foil will form a valuable addition to the agents employed by the mineralogist and chemist in the examination of minerals, ores, &c.

2. *Sulphur.*—If a little flowers of sulphur be mixed with about its own bulk of the common peroxide of manganese, and exposed on a slip of platinum foil to a red heat, sesquioxide, sulphuret and sulphate of manganese will be formed, and by continuing the heat for a short time, an additional quantity of the sulphate will be produced from

the sulphuret. On treating the mass with water and filtering the fluid, a solution of sulphate of manganese will be obtained which will yield a white precipitate with the ferrocyanide of potassium, without a trace of iron.

Similar experiments may be made with any manganese ores, or with substances known or suspected to contain manganese. The quantity of materials operated on may be increased or diminished at pleasure; but if increased, the heat should be continued a little longer, to decompose any remaining sulphuret, and thus add to the quantity of sulphate formed. In the same way manganese was detected in some minerals in which it was known to exist, and in others in which it had not been previously found; likewise in soils and subsoils, in the ashes of coal and peat, in a number of pigments, and also in the ashes of different fabrics partially dyed brown by manganese.

Sulphate of manganese is formed, with sulphuret, when sulphurous acid gas is made by heating a mixture of peroxide of manganese and flowers of sulphur, even in close vessels. The sulphate may also be more readily obtained, in quantity, by simply boiling a solution of common green vitriol in water for about a quarter of an hour or upwards, in contact with an excess of sesquioxide of manganese in fine powder, till the solution affords a white precipitate with ferrocyanide of potassium.

Chloride of manganese may also be formed in a similar manner by boiling an aqueous solution of protochloride of iron with an excess of sesquioxide of manganese, or it may be made with greater facility by dissolving this oxide in the common muriatic acid of commerce, taking care that the oxide be present in excess.

The brown sesquioxide of manganese may be made, not only by means of sulphur, but more readily and better by mixing the common peroxide with about one-third of its weight of peat mould, sawdust or starch, and exposure to a red heat in an open crucible with occasional stirring for about a quarter of an hour, or until the oxide acquires a uniform brown colour.

The sulphate and chloride of manganese being extensively used in dyeing, calico-printing and other arts, and in making the compounds of manganese, the simple means stated of forming those salts, free from iron (it is presumed), are material improvements on the circuitous methods hitherto adopted.

ROYAL INSTITUTION OF GREAT BRITAIN.

Jan. 27, 1854.—On the Vibration and Tones produced by the Contact of Bodies having different Temperatures. By John Tyndall, Esq., Ph.D., F.R.S., Professor of Natural Philosophy, Royal Institution.

In the year 1805, M. Schwartz, an inspector of one of the smelting-works of Saxony, placed a cup-shaped mass of hot silver upon a cold anvil, and was surprised to find that musical tones proceeded from the mass. In the autumn of the same year, Professor Gilbert of Berlin visited the smelting-works and repeated the experiment. He observed that the sounds were accompanied by a quivering of the hot silver; and that when the vibrations ceased, the sound ceased also. Professor Gilbert merely stated the facts, and made no attempt to explain them.

In the year 1829, Mr. Arthur Trevelyan, being engaged in spreading pitch with a hot plastering-iron, and once observing that the iron was too hot for his purpose, he laid it slantingly against a block of lead which chanced to be at hand; a shrill note, which he compared to that of the chanter of the small Northumberland pipes, proceeded from the mass, and on nearer inspection he observed that the heated iron was in a state of vibration. He was induced by Dr. Reid of Edinburgh to pursue the subject, and the results of his numerous experiments were subsequently printed in the Transactions of the Royal Society of Edinburgh.

On the 1st of April, 1831, these singular sounds and vibrations formed the subject of a Friday evening lecture by Professor Faraday, at the Royal Institution. Professor Faraday expanded and further established the explanation of the sounds given by Mr. Trevelyan and Sir John Leslie. He referred them to the tapping of the hot mass against the cold one underneath it, the taps being in many cases sufficiently quick to produce a high musical note. The alternate expansion and contraction of the cold mass at the points where the hot rocker descends upon it, he regarded as the sustaining power of the vibrations. The superiority of lead he ascribed to its great expansibility, combined with its feeble power of conduction, which latter prevented the heat from being quickly diffused through the mass.

Professor J. D. Forbes of Edinburgh was present at this lecture, and not feeling satisfied with the explanation, undertook the further examination of the subject; his results are described in a highly ingenious paper communicated to the Royal Society of Edinburgh in 1833. He rejects the explanation supported by Professor Faraday, and refers the vibrations to "a new species of mechanical agency in heat"—a repulsion exercised by the heat itself on passing from a good conductor to a bad one. This conclusion is based upon a number of general laws established by Professor Forbes. If these laws be correct, then indeed a great step has been taken towards a knowledge of the intimate nature of heat itself, and this consideration was the lecturer's principal stimulus in resuming the examination of the subject.

He had already made some experiments, ignorant that the subject had been further treated by Seebeck, until informed of the fact by Professor Magnus of Berlin. On reading Seebeck's interesting paper, he found that many of the results which it was his intention to seek had been already obtained. The portion of the subject which remained untouched was, however, of sufficient interest to induce him to prosecute his original intention.

The general laws of Professor Forbes were submitted in succession to an experimental examination. The first of these laws affirms that "*the vibrations never take place between substances of the same nature.*" This the lecturer found to be generally the case when the hot rocker rested upon a *block*, or on the edge of a thick plate of the same metal; but the case was quite altered when a thin plate of metal was used. Thus, a copper rocker laid upon the edge of a penny-piece did not vibrate permanently; but when the coin was beaten out by a hammer, so as to present a thin, sharp edge, constant vibrations were obtained. A silver rocker resting on the edge of a half-crown refused to vibrate permanently, but on the edge of a sixpence continuous vibrations were obtained. An iron rocker on the edge of a dinner knife gave continuous vibrations. A flat brass rocker placed upon the points of two common brass pins, and having its handle suitably supported, gave distinct vibrations. In these experiments the plates and pins were fixed in a vice, and it was found that the thinner the plate within its limits of rigidity, the more certain and striking was the effect. Vibrations were thus obtained with iron on iron, copper on copper, brass on brass, zinc on zinc, silver on silver, tin on tin. The list might be extended, but the cases cited are sufficient to show that the proposition above cited cannot be regarded as expressing a "general law."

The second general law enunciated by Professor Forbes is, that "*both substances must be metallic.*" This is the law which first attracted the lecturer's attention. During the progress of a kindred inquiry, he had discovered that certain non-metallic bodies are endowed with powers of conduction far higher than has been hitherto supposed; and the thought occurred to him that such bodies might, by suitable treatment, be made to supply the place of metals in the production of vibrations. This anticipation was realized. Rockers of silver, copper and brass, placed upon the natural edge of a prism of rock-crystal, gave distinct tones; on the clean edge of a cube of fluor-spar the tones were still more musical; on a mass of rock-salt the vibrations were very forcible. There is scarcely a substance, metallic or non-metallic, on which vibrations can be obtained with greater ease and certainty than on rock-salt. In most cases a high temperature is necessary to the production of the tones, but in the case of rock-salt the temperature need not exceed that of the blood. A new and singular property is thus found to belong to this already remarkable substance. It is needless to enter into a full statement regarding the various minerals submitted to experiment. Upwards of twenty non-metallic substances had been examined by the lecturer, and distinct vibrations obtained with every one of them.

The number of exceptions here exhibited far exceeds that of the substances which are mentioned in the paper of Professor Forbes, and are, it was imagined, sufficient to show that the second general law is untenable.

The third general law states, that "*The vibrations take place* *with an intensity proportional (within certain limits) to the difference of the conducting powers of the metals for heat, the metal having the least conducting power being necessarily the coldest."* The evidence adduced against the first law appears to destroy this one also; for if the intensity of the vibrations be proportional to the difference of the conducting powers, then where there is no such difference there ought to be no vibrations. But it has been proved in half a dozen cases, that vibrations occur between different pieces of the same metal. The condition stated by Professor Forbes was, however, reversed. Silver stands at the head of conductors; a strip of the metal was fixed in a vice, and hot rockers of brass, copper, and iron were successively laid upon its edge: distinct vibrations were obtained with all of them. Vibrations were also obtained with a brass rocker which rested on the edge of a half-sovereign. These and other experiments show that it is not necessary that the worst conductor should be the cold metal, as affirmed in the third general law above quoted. Among the metals, antimony and bismuth were found perfectly inert by Professor Forbes; the lecturer, however, had obtained musical tones from both of these substances.

The superiority of lead as a cold block, Professor Faraday, as already stated, referred to its high expansibility, combined with its deficient conducting power. Against this notion, which he considers to be "an obvious oversight," Professor Forbes contends in an ingenious and apparently unanswerable manner. The vibrations, he urges, depend upon the difference of temperature existing between the rocker and the block; if the latter be a bad conductor and retain the heat at its surface, the tendency is to bring both the surfaces in contact to the same temperature, and thus to stop the vibration instead of exalting it. Further, the greater the quantity of heat transmitted from the rock to the block during contact, the greater must be the expansion; and hence, if the vibrations be due to this cause, the effect must be a maximum when the block is the best conductor possible. But Professor Forbes, in this argument, seems to have used the term expansion in two different senses. The expansion which produces the vibration is the sudden upheaval of the point where the hot rocker comes in contact with the cold mass underneath; but the expansion due to good conduction would be an expansion of the general mass. Imagine the conductive power of the block to be infinite, that is to say, that the heat imparted by the rocker is instantly diffused equally throughout the block; then, though the general expansion might be very great, the local expansion at the point of contact would be wanting, and no vibrations would be possible. The inevitable consequences of good conduction is to cause a sudden abstraction of the heat from the point of contact of the rocker with the substance underneath, and this the lecturer conceived to be the precise reason why Professor Forbes had failed to obtain vibrations when the cold metal was a good conductor. He made use of *blocks*, and the abstraction of heat from the place of contact by the circumjacent mass of metal was so sudden as to extinguish the local elevation on which the vibrations depend. In the experiments described by the lecturer, this abstraction was to a great extent avoided by reducing the metallic masses to thin laminæ; and thus the very experiments adduced by Professor Forbes against the theory supported by Professor Faraday appear, when duly considered, to be converted into strong corroborative proofs of the correctness of the views of the philosopher last mentioned.

XXXIV. *Intelligence and Miscellaneous Articles.*

ELECTRO-DEPOSITION OF ALUMINIUM AND SILICIUM. BY GEORGE GORE, ESQ.

To the Editors of the Philosophical Magazine and Journal.

8 Broad Street, Birmingham,
February 24, 1854.

GENTLEMEN,

ENCLOSED are two specimens of sheet copper, one coated with metallic aluminium and the other with silicium, by electro-deposition process; and if the following simple statement of the manner in which they were obtained is worthy of a place in your Magazine, I shall be happy to have it published.

To obtain the aluminium, I boiled an excess of dry hydrate of alumina in hydrochloric acid for one hour, then poured off the clear liquid, and added to it about one-sixth of its volume of water; in this mixture I placed an earthen porous vessel containing one measure of sulphuric acid to twelve measures of water, with a piece of amalgamated zinc plate in it. In the chloride of aluminium solution I immersed a piece of copper of the same amount of immersed metallic surface as that of the zinc, and connected it with the zinc by means of a copper wire, and set it aside for several hours; when on examining it, I found it coated with a lead-colour deposit of aluminium, which when burnished possessed the same degree of whiteness as platinum, and did not appear to tarnish readily by immersion in cold water or in the atmosphere, but was acted upon by sulphuric or nitric acids, either concentrated or dilute.

I found that if the apparatus was kept quite warm, and a copper plate much smaller than the zinc plate was used, the deposit appeared in a very short time, in several instances in less than half a minute. Also I found that if the chloride solution was not diluted with water, the deposit was equally, if not more rapid.

I have also succeeded in obtaining a quick deposit of aluminium in a less pure state by dissolving ordinary "pipe-clay" in boiling hydrochloric acid, and using the supernatant clear solution undiluted with water in the place of the before-mentioned liquid. A similar deposit of aluminium was also obtained from a strong aqueous solution of acetate of alumina; likewise from a saturated aqueous solution of ordinary "potash alum," but rather slowly; with each of the solutions named, the deposit was hastened by putting either one, two, or three small Smee's batteries in the circuit.

To obtain the deposit of silicium, I dissolved monosilicate of potash (formed by fusing together 1 part of silica with 2¼ parts of carbonate of potash) in water, in the proportion of 40 grs. to 1 oz. measure of water, proceeding in like manner as with the alumina solutions, the process being hastened by interposing one pair of small Smee's battery in the circuit. With a very slow and feeble action of the battery, the colour of the deposited metal was much whiter than that of the aluminium, closely approximating to that of silver; its other properties I have not yet had time to examine.

I remain,

Yours very truly,

George Gore.

EXPERIMENTS ON THE ARTIFICIAL PRODUCTION OF POLYCHROISM IN CRYSTALLIZED SUBSTANCES. BY M. DE SÉNARMONT.

In some researches upon crystallization which the author has pursued for several years, he has been led to study the absorption of light which takes place in coloured crystals, and the polychroism which accompanies it.

This singular property, which is possessed by many minerals and artificial products, consists essentially in the circumstance that the two luminous rays resulting from double refraction undergo in the interior of the crystal an unequal extinction in their colorific elements, so that a pencil of white natural light is separated on its emergence from the crystal into two pencils of different colours at the same time that they are polarized at a right angle.

It may be inquired whether such a phænomenon must be necessarily and exclusively caused by the coloration, either of the crystal itself, or of some other substance chemically combined with it; and whether it may not sometimes be the effect of two different and coexistent causes, as a birefractive power exerted by the crystalline matter itself, and an absorbent power exercised by some foreign colouring matter accidentally distributed in the interstices of the crystal, like the impurities which crystals derive from their mother-liquors. This question can only be decided by synthesis; it would be solved if we could succeed in introducing into crystalline salts all sorts of colouring matters, incapable of forming a chemical union with them, but capable of incorporating themselves by a sort of impregnation.

The problem, put in these terms, appears more simple than it really is. For dichroism, selecting different colours for suppression in each of the refracted rays, it is impossible that its cause can be quite independent of that which thus splits the luminous rays in crystalline refraction. The absorbent agent, whatever it may be, must, on the contrary, be connected and in some degree subordinated to the crystallization; and if it may reside in coloured non-crystalline particles, it is at least necessary that their arrangements should be continuous to a certain point with the crystalline medium, that they should be symmetrically arranged by their interposition amongst the essential materials of the molecular edifice, and so adapted to its structure as to participate in its regular arrangement.

The question, therefore, was to find colouring principles of sufficient tenuity to be, as it were, assimilated by the crystals during their formation, to become distributed almost molecularly in their interior without forming accumulations in any one portion of their substance; and it was necessary to find salts with a molecular tissue sufficiently loose to form regular and homogeneous crystals in strongly coloured, and consequently very impure mother-liquors, whilst nevertheless their formation was not accompanied by a sufficiently energetic eliminatory process for the total expulsion of all foreign matters; lastly, even when all these conditions were fulfilled, it still remained doubtful whether the production of polychroism would take place in this medium; for nothing proves it to be inherent in every kind of coloration, and its effective conditions are absolutely unknown.

The author now lays the following facts, the principal result of an immense number of experiments, before the Academy.

A colouring matter, disseminated continuously in the interior of a crystal between its laminæ of growth, but absolutely foreign to the substance, and capable of spontaneous elimination by simple recrystallization from pure water, may nevertheless communicate a property of polychroism and an energy of absorbent action, equal, if not superior, to those of natural coloured bodies, in which these properties are most distinctly manifested.

The author exhibited large crystals of nitrate of strontia formed in a tincture of logwood which had been rendered purple by a few drops of ammonia. The crystals thus acquired a colour like that of chrome-alum, and a sufficiently distinct polychroism to exhibit the following phænomena:—

1. Natural white light produces by transmission under certain incidences a red, and under others a blue or violet colour.

2. Observed with a doubly refractive prism, these crystals are resolved into two images, one red, the other deep violet, according to the thickness; and these images change colours by passing through the intermediate shades in proportion as the crystalline plate turns in its own plane.

3. Two similar transparent plates superposed in a parallel orientation permit the passage of a portion of the incident white light as a bundle of purple rays; superposed at right angles, they arrest it like the tourmalines, or at all events reduce it to a violet tint of such obscurity that it may be considered as destroyed.

4. Another phænomenon may also serve, if necessary, as a palpable demonstration of the intimate connexion established in this compound medium, between the absorption thus artificially produced and the natural birefractive properties.

From these crystals, perfectly homogeneous laminæ, slightly inclined towards the optical axes, may be separated; when a lamina of this kind is placed very close to the eye and illuminated by natural white light, there will be seen ultimately, in the direction of each of these axes, a brilliant orange spot traversed by a hyperbolic branch. These expand to the right and left of the principal section in the form of curved pencils, half violet and half dark blue, and divide the field of the lamina into two regions, in which the purple tints gradually become degraded on either side of their common limit.

The dark tufts interrupted by the luminous spot are also fringed towards the point with a little yellow and blue; this coloration is entirely local, and manifestly arises from the dispersion of the optical axes corresponding with the different colours. This dispersion is, in fact, pretty distinct in nitrate of strontia.

These appearances, perfectly characteristic of the polychroism of crystals with two optical axes, and absolutely identical with those first observed by Brewster in Cordierite, found by Haidinger in andalusite, and tolerably distinct in some varieties of epidote, are manifested with much greater splendour in the large laminæ which may readily be prepared with nitrate of strontia. In their natural state the colourless crystals exhibit nothing similar, and the optical axes only become visible by means of polarized light. Other suitable colouring matters and other crystallized salts produce analogous effects in various degrees.—*Comptes Rendus*, Jan. 23, 1854, p. 101.

ON ALUMINIUM AND ITS COMPOUNDS. BY M. DEVILLE.

It is known that Wöhler obtained the metal aluminium in the state of a powder by treating the chloride with potassium. By a suitable modification of Wöhler's process, the decomposition of the chloride of aluminium can be regulated so as to produce a temperature sufficient for the particles of the metal to agglomerate into globules. If the mass composed of the metal and chloride of sodium (sodium is preferable to potassium) is exposed to a bright red heat in a porcelain crucible, the excess of chloride of aluminium is expelled, and there is left a saline mass with an acid reaction in which are disseminated more or less large globules of perfectly pure aluminium.

This metal is as white as silver, and in the highest degree malleable and ductile. When wrought, however, it exhibits greater resistance, and its tenacity probably approaches that of iron. It is hardened by hammering, but reacquires its softness on being reheated. Its fusing-point differs but slightly from that of silver; its specific gravity $=2\cdot56$; it can be smelted and cast without being perceptibly oxidized; it is a good conductor of heat. It is not in the least affected by moist or dry air, does not tarnish, but remains bright by the side of zinc and tin freshly cut, which soon become dull. Sulphuretted hydrogen has no action upon it, cold water does not alter it, boiling water does not tarnish it. It is not acted upon by nitric acid, weak or strong, or by weak sulphuric acid, employed cold. Its true solvent is hydrochloric acid, with which it forms chloride of aluminium. Heated to redness in hydrochloric acid gas it furnishes dry volatile chloride of aluminium.

It will be readily understood what important uses such a metal, which is white and unalterable like silver, which does not blacken in the air, is fusible, malleable, ductile and tenacious, and has in addition the singular property of being lighter than glass, may be turned to if it can be obtained readily. This I have every reason to believe will prove to be the case, for the chloride of aluminium is decomposed with remarkable ease by the common metals at an elevated temperature; and a reaction of this kind, which I am attempting to carry out on a large scale, will solve this question in a practical point of view.—*Comptes Rendus*, February 6, 1854.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1854.

Chiswick.—January 1. Clear and frosty: snowing: frosty. 2. Frosty throughout: severe frost at night. 3. Severe frost, with fog: overcast. 4. Overcast and cold: snow 7 inches deep. 5. Thick haze: rain and sleet: 2 inches additional depth of snow. 6. Hazy. 7. Heavy rain. 8. Clear and fine. 9. Rain: foggy: rain at night. 10, 11. Cloudy and cold. 12. Cloudy: slight rain. 13. Slight haze: clear. 14. Clear: hazy. 15. Foggy: cloudy. 16. Dense fog: overcast. 17. Cloudy. 18. Densely clouded: overcast. 19. Dense fog. 20. Rain: overcast. 21. Fine: frosty. 22. Frosty haze: clear and fine: overcast: frosty. 23. Foggy: very fine. 24. Cloudy and windy: rain: frosty. 25. Frosty: clear and fine: cloudy. 26. Fine: very clear. 27. Densely overcast: rain. 28. Slight rain: cloudy: clear. 29. Boisterous, with rain: overcast. 30. Cloudy and mild: clear. 31. Overcast: cloudy.

Mean temperature of the month	37°·88
Mean temperature of January 1853	40 ·41
Mean temperature of Jan. for the last twenty-eight years	36 ·68
Average amount of rain in Jan.	1·74 inch.

Boston.—Jan. 1. Fine. 2. Cloudy: snow A.M. and P.M. 3. Cloudy: thermometer at 8 A.M. 5°. 4. Stormy: snow-storm A.M. and P.M. 5. Cloudy. 6. Cloudy: snow A.M. 7. Cloudy: rain P.M. 8. Cloudy: rain A.M. 9—13. Cloudy. 14. Fine. 15. Cloudy: rain early A.M. 16—19. Cloudy. 20. Cloudy: rain P.M. 21. Cloudy. 22, 23. Fine. 24. Cloudy: rain A.M. 25. Fine. 26. Fine: rain A.M. 27. Cloudy. 28, 29. Cloudy: rain A.M. 30. Fine. 31. Cloudy.

Sandwick Manse, Orkney.—Jan. 1. Snow-drift A.M.: snow-showers P.M. 2. Bright A.M.: cloudy P.M. 3. Bright A.M.: snow-showers P.M. 4. Bright A.M.: clear P.M. 5. Bright A.M.: snow-showers P.M. 6. Bright A.M.: clear P.M. 7. Thaw A.M.: sleet and rain P.M. 8. Sleet-showers A.M. and P.M. 9. Bright, frost A.M.: cloudy P.M. 10. Bright, frost A.M.: clear P.M. 11. Clear, frost A.M.: clear P.M. 12. Snow-showers A.M. snow-drift, showers P.M. 13. Clear, fine A.M. cloudy P.M. 14. Cloudy A.M.: clear, frost P.M. 15. Bright A.M.: clear, aurora P.M. 16. Bright A.M.: rain P.M. 17. Cloudy A.M.: clear, aurora P.M. 18. Bright A.M.: cloudy, aurora P.M. 19. Bright A.M.: cloudy P.M. 20. Bright A.M.: cloudy, aurora P.M. 21, 22. Cloudy A.M. and P.M. 23. Clear A.M.: cloudy, aurora P.M. 24. Cloudy A.M.: clear P.M. 25. Cloudy A.M.: sleet-showers, lightning P.M. 26. Hail-showers A.M.: cloudy P.M. 27. Cloudy A.M.: showers P.M. 28. Showers A.M. and P.M. 29. Showers A.M.: showers, aurora P.M. 30. Drizzle A.M.: drizzle, aurora P.M. 31. Showers A.M.: cloudy P.M.

Mean temperature of Jan. for twenty-seven previous years	38°·46
Mean temperature of this month	36 ·47
Mean temperature of Jan. 1853	38 ·55
Average quantity of rain in Jan. for thirteen previous years	4·35 inches.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1854. Jan.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·427	29·378	29·16	29·35	29·22	32	17	22	28	28	sw.	nnw.	n.	....	....	....
2.	29·291	29·205	29·08	29·04	29·18	32	4	23·5	34	34	sw.	se.	nw.	....	....	·48
3.	29·316	29·132	29·22	29·32	29·54	32	28	21	34	32½	e.	se.	ne.	....	·15	·19
4.	28·940	28·912	28·97	29·57	29·53	32	31	31	23	27	se.	ese.	ne.	·86	....	....
5.	28·920	28·900	28·78	29·47	29·34	36	31	31	27	32	ne.	e.	ne.	·03	·20	·13
☽ 6.	29·104	28·989	28·82	29·27	29·29	38	28	30	27	23½	w.	e.	e.	·08	....	....
7.	29·158	28·851	28·93	29·22	29·25	42	37	32·5	33	34	s.	e.	e.	·33	·06	·50
8.	29·169	28·897	28·69	29·35	29·51	46	31	36	35	35	sw.	sw.	ene.	....	·12	·60
9.	29·119	29·096	29·00	29·61	29·74	40	37	30	34	30	e.	e.	ne.	·06	....	·61
10.	29·606	29·435	29·28	29·94	30·01	38	34	36	28½	26	n.	nnw.	se.	·02	....	·25
11.	29·896	29·833	29·66	30·00	29·89	38	31	34	31	30	n.	n.	ssw.	....	....	....
12.	29·833	29·659	29·59	29·62	29·52	40	29	33	33½	35	se.	s.	s.	·02	....	....
13.	29·656	29·596	29·43	29·42	29·42	42	28	31	37½	37	s.	ssw.	s.	·08	....	·30
○ 14.	29·873	29·707	29·40	29·48	29·56	45	26	33	37	33	sw.	wsw.	se.	·10	....	....
15.	29·715	29·624	29·45	29·56	29·62	40	32	34	37	36	se.	ese.	s.	....	·04	·08
16.	29·863	29·815	29·60	29·62	29·43	46	41	33	34	39	se.	s.	s.	·01	....	....
17.	30·076	29·928	29·63	29·43	29·54	48	45	44	40	42	sw.	ssw.	sw.	·01	....	·28
18.	30·151	30·134	29·79	29·64	29·78	48	35	48	44	40½	sw.	ssw.	s.	....	....	·03
19.	30·065	29·944	29·75	29·58	29·45	45	29	39	42	41	se.	s.	s.	....	....	....
20.	30·005	29·927	29·60	29·49	29·33	51	35	43	44½	41½	sw.	s.	s.	·01	....	....
21.	30·267	30·175	29·94	29·89	29·63	50	28	40	42	44½	s.	ssw.	s.	....	·03	·04
☾ 22.	30·107	29·978	29·66	29·80	29·62	49	24	40	44	44	sw.	ssw.	se.	....	....	·09
23.	30·089	30·028	29·77	29·73	29·23	50	36	34	39	42½	s.	ssw.	se.	....	....	....
24.	29·743	29·576	29·35	29·03	29·27	51	24	42	38½	39	sw.	s.	wsw.	·02	....	·21
25.	30·123	30·084	29·77	29·34	29·10	48	36	31	39	41	sw.	ssw.	wsw.	·06	·03	....
26.	30·536	30·331	29·90	29·47	30·06	50	26	36	35	41	sw.	sw.	w.	....	....	·23
27.	30·487	30·267	29·10	29·53	29·40	46	42	38·5	48	43	sw.	s.	sw.	·03	·17	·07
● 28.	30·172	30·095	29·75	29·65	29·46	49	29	45·5	39	39	sw.	w.	s.	·10	·04	·18
29.	29·934	29·808	29·37	28·88	29·30	53	48	48	42	39½	sw.	sw.	wsw.	·10	·08	·14
30.	30·141	30·111	29·70	29·48	29·35	52	45	52	41	41	sw.	sw.	w.	....	....	·18
31.	30·204	30·088	29·77	29·65	29·59	53	40	51·5	38½	40	w.	wsw.	wsw.	....	....	·13
Mean.	29·773	29·661	29·41	29·497	29·490	43·93	31·83	36·2	36·45	36·50				1·92	0·92	4·72

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[FOURTH SERIES.]

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XXXV. *On the Composition and Metamorphoses of some Sedimentary Rocks.* *By* T. S. Hunt, *of the Geological Commission of Canada*[*].

In the Report of Progress of the Geological Survey of Canada for 1851-52, I have described a peculiar metamorphosis of a portion of the Lower Silurian shales at St. Nicolas, on the south side of the St. Lawrence, near Quebec. The rocks there exposed have been minutely described by Mr. Logan; they belong to that division of the formation which is known as the Hudson River group, and consist of beds of greenish, quartzose sandstones, from one to three feet in thickness, interstratified with thin layers of greenish, bluish, or reddish shales. The colours of these are probably connected with different states of oxidation of the iron which they contain. The green colour is sometimes seen in small oblong rounded patches in the red shale, and when in a mass of the latter a layer of from half an inch to an inch of somewhat calcareous matter occurs, it is bounded above and below by a portion of green slate, sometimes no more than a quarter of an inch in thickness. In one section near Pointe Lévy, the green colour was seen following down a rift in the red slates across the stratification, and spreading irregularly on either side. Such modes of occurrence seem to indicate a deoxidation of the red slates by imbedded or infiltrating organic matters.

Mr. Logan had observed that an intrusion of trappean rocks at St. Nicolas has altered the shales, and converted them into a material resembling serpentine; and as his researches in the

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[*] Communicated by the Author.

Eastern Townships have shown that the serpentines of that metamorphic region belong to Lower Silurian strata, of which these rocks at St. Nicolas are the stratigraphical equivalent, it was hoped that a chemical examination of them, and a comparison between the altered and unaltered shales, would throw some light upon the nature of that widely-spread metamorphism, and upon the origin of serpentine. The results obtained were different from what was expected, but are not, however, without interest.

In the immediate vicinity of the intrusive rock thin layers of the shale are changed into a greenish translucent material, which is sometimes an inch in thickness. The surfaces of the sandstone in contact with the shale seem to have undergone a similar change, or at least to be covered with a film of the green translucent mineral, and they often exhibit a concretionary or mammillated surface. In one instance the thickening of a stratum of shale forms a mass several inches in diameter, which is earthy and opake within, but upon the surface next to the overlying rock assumes the serpentine-like character alluded to; and in a continuation of the layer, where it becomes thinner, the transformation is complete. In the interstratified sandstones, which are sometimes conglomerate, masses of the same green mineral are found; and it was also observed filling fissures in fragments of a bright red, and much indurated shale, which had fallen from a high part of the cliff.

This green mineral is found by chemical analysis to be distinct from serpentine, and is essentially a hydrous silicate of alumina, protoxide of iron and potash, with small portions of soda, lime and magnesia. Its greater density serves also to distinguish it from serpentine. It occurs in schistose masses with a granular texture, also botryoidal, with appearance of concentric structure, and a conchoidal fracture. Hardness 2·5 to 3·0. Density of the schistose specimens 2·68 to 2·71; of the botryoidal, 2·78. Lustre waxy, shining; streak white; powder unctuous; colour greenish-white, yellowish-green, olive-green, often mottled; translucent, subtranslucent. Very fragile, especially when moistened; sectile; the light-coloured portions resemble in appearance some specimens of indurated talc.

To distinguish this material, and to recall its resemblance to ophite or serpentine, I provisionally gave it the name of *parophite*, without, however, claiming for an amorphous product of the alteration of a sedimentary rock the rank of a distinct mineral species. I give in connexion with some analyses of this substance, one of the unaltered schist, which passes in a little distance into parophite. The colour of the schist is dark ash-gray, sometimes marked with red; the laminæ are somewhat curved, their surfaces glimmering and slightly unctuous; it is completely earthy, opake even upon the edges, and very soft, yielding with ease to the nail; the powder is not at all gritty.

The parophite loses water when exposed to heat and turns gray; it is but imperfectly decomposed by hydrochloric acid. The following analyses were made by fusion with carbonate of soda, and the alkalies were determined by decomposing a second portion with fluor-spar and sulphuric acid. Analyses I. and II. are of schistose parophite, III. of the botryoidal variety, and IV. of the earthy schist which passes into I.

	I.	II.	III.	IV.
Silica	48·60	48·42	49·13	48·10
Alumina	27·90	27·60	27·80	28·70
Protoxide of iron	5·67	4·50	5·90	4·80
Lime	1·51	2·80	3·80	2·10
Magnesia	2·20	1·80	1·40	1·41
Potash	5·30	5·02	5·67 by diff.	4·49
Soda	1·91	2·78		1·53
Water	7·40	6·88	6·30	8·40
	100·49	99·80	100·00	99·53

Traces of manganese were found in all of them.

These analyses show that the change consists only in a chemical union of the finely divided mixture which makes up the sedimentary rock. Prof. C. U. Shepard described some time since by the name of ***dysyntribite***, an aluminous silicate which is found in St. Lawrence County, New York, and had been mistaken for serpentine, which it resembles in colour, lustre, and general appearance; it has, according to him, a hardness of 3·5 to 4·0, and a density of 2·76 to 2·81. He gave the following analytical results:—silica, 47·68; alumina, 41·50; protoxide of iron, 5·48; water, 4·83; and traces of lime and magnesia; =99·49[*]. Notwithstanding the resemblance of this to the parophite of St. Nicolas, the differences in composition, particularly in the large amount of alumina and the absence of alkalies, according to the analyses of Prof. Shepard, were such that we supposed the rocks to be distinct.

The recent analyses of dysyntribite by J. Lawrence Smith and G. Brush have, however, shown that this substance, although of variable composition, contains a large amount of potash, and in many specimens approaches the parophite. They obtained from 44·7 to 46·7 per cent. of silica, and from 4·7 to 6·3 of water; while the alumina varied from 31 to 35 per cent., and the oxide

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[*] Report of the American Association for the Advancement of Science, vol. iv. p. 311.

of iron from 3 to 4. These specimens also contained, besides minute quantities of lime and magnesia, from 10·5 to 11·5 per cent. of alkalies in which the soda varied from a trace to 3·6 per cent. Other specimens afforded 8 to 12 per cent. of lime, 7 to 8 of magnesia, and from 4 to 6 per cent. of potash, with traces of soda; the proportions of silica, water, and oxide of iron being the same*.

According to Prof. Shepard, this material is often associated with the deposits of specular iron ore in the crystalline limestones and felspathic rocks, which in northern New York underlay the Lower Silurian strata, and are by Dr. Emmons of the New York Geological Survey, regarded, the limestones and iron included, as hypogene rocks. The researches of Mr. Logan upon the extension of these rocks into Canada, where they are very widely spread, have, however, shown them to be undoubtedly altered sedimentary deposits, leading us to infer in both cases a similar origin for these hydrous alkaline silicates.

The large amount of alkalies in the schists of St. Nicolas is worthy of notice, because the small quantity remaining in kaolin and some other clays seems to have given rise to an idea that such sedimentary rocks are generally deficient in alkaline ingredients. Another red slate from the Etchmin river, belonging to the same strata as the last, afforded the following analytical results:—

Silica	66·00
Alumina and protoxide of iron	24·60
Potash	3·67
Soda	2·22
Lime and magnesia	traces
Water	3·00
	99·49

Although more siliceous, it resembles that of St. Nicolas in the large proportion of alkalies and the predominance of potash. A glance at the first analyses will show that that slate under igneous action might produce a large amount of a potash felspar like orthoclase, together with an alumino-ferruginous augite; it is, in fact, the almost unchanged detritus of old felspathic and pyroxenic rocks, and is converted into similar rocks in the metamorphic region further south.

The following analyses were made for the purpose of determining the chemical composition of good roofing-slates, but are not without value to the chemical geologist. No. I. is from Kingsey in the Eastern Townships, and, like those already de-

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* American Journal of Science (2), vol. xvi. p. 50.

scribed, is Lower Silurian; it is purplish-blue in colour, completely opake, with a feeble glimmering lustre on its cleavage planes, which are nearly at right angles to the bedding density 2·884. No. II. is an Upper Silurian slate from Westbury, Eastern Townships; it is greenish-blue, with a silky surface, and is translucent on the edges; density 2·711. Both of these are excellent roofing-slates. No. III. is a Welsh roofing-slate closely resembling No. I. in appearance, with a density of 2·824; and No. IV. is from the quarries of Angers in France, very like No. II., but more translucent, with a pearly lustre on its cleavage surfaces, and somewhat talcose; density 2·882. The analyses were made by fusion with carbonate of soda, and the alkalies determined by decomposing a separate portion with a mixture of carbonate of lime and sal-ammoniac, after the method of Dr. J. Lawrence Smith.

	I.	II.	III.	IV.
Silica	54·80	65·85	60·50	57·00
Alumina	23·15	16·65	19·70	20·10
Protoxide of iron .	9·58	5·31	7·83	10·98
Lime	1·06	·59	1·12	1·23
Magnesia . . .	2·16	2·95	2·20	3·39
Potash	3·37	3·74	3·18	1·73
Soda	2·22	1·31	2·20	1·30
Water	3·90	3·10	3·30	4·40
	100·24	99·50	100·03	100·13

It is probable from the colour, that a part of the iron in I. and III. exists as peroxide; traces of manganese were detected in all of them. The last specimen had been exposed to the weather upon the roof of a house for nearly a century, and this may account for the smaller quantity of alkalies which it contains. When finely elutriated, these slates give to the water a distinctly alkaline reaction, and to such a solvent process is to be ascribed the deficiency of alkalies in the schist of St. Nicolas compared with the associated parophite, with which this reaction is much less distinct. In our Geological Report of last year, from which these analyses are taken, I have had occasion to point out the relation between these slates and the alkaline mineral springs which are so abundant in this region. These waters contain, besides alkaline chlorides, large portions of alkaline carbonates, with borates and silicates, the chlorides frequently constituting the smaller part of the solid contents. Some of these springs are further remarkable from the large proportion of potash which they contain, in one instance amounting to one-fourth of the alkaline salts present, when estimated as chlorides of potassium and sodium.

The post-pliocene clays of the St. Lawrence valley also contain a large proportion of alkalies; they are sometimes calcareous, and afford, besides carbonates of these bases, a considerable amount of lime and magnesia as silicates. The following results were obtained with a reddish-fawn coloured clay from the Rivière à la Graisse, Rigaud; it is impalpable, and remains a long time suspended in water. It yields to heated hydrochloric acid 12·95 per cent. of alumina and peroxide of iron, 3·97 of lime and 1·92 of magnesia; the amount of carbonic acid evolved is a little more than is required to form a carbonate with the lime thus obtained. Its complete analysis gave—

Silica (by difference)	50·81
Alumina	21·70
Peroxide of iron	5·60
Lime	5·32
Magnesia	2·62
Potash	2·85
Soda	2·61
Phosphoric acid	·74
Carbonic acid	3·25
Water	4·50
	100·00

Another clay from the vicinity of Montreal, which was mixed with 13·5 per cent. of siliceous sand and a little magnetic iron, and was free from earthy carbonates, afforded (the sand included) the following results:—

Silica (by difference)	65·53
Alumina	13·15
Peroxide of iron	8·50
Lime	1·73
Magnesia	1·14
Potash	1·76
Soda	2·35
Phosphoric acid	·54
Water and some organic matter	5·30
	100·00

The first of these clays, in the proportions of silica, alumina, lime and alkalies, approaches some of the lime felspars, and its metamorphosis might yield a mixture of andesite or labradorite, with hypersthene, constituting a rock similar to one which is very abundant in our older ante-Silurian strata, and may very well have been the source of this clay.

Montreal, Canada, Feb. 20, 1854.

XXXVI. *On the Mechanical Action of Heat.* *By* William John Macquorn Rankine, *C.E., F.R.SS. Lond. and Edinb. &c.*

[Concluded from p. 185.]

[With a Plate.]

On the Power and Œconomy of Single-acting Expansive Steam-engines, being a Supplement to the Fourth Section of a Paper 'On the Mechanical Action of Heat.'*

(34.) THE objects of this supplementary paper are twofold: *first,* to compare the results of the formulæ and tables relative to the power of the steam-engine, which have been deduced from the dynamical theory of heat, with those of experiments on the actual duty of a large Cornish engine at various rates of expansion; and *secondly,* to investigate and explain the method of determining the rate of expansion, and, consequently, the dimensions and proportions of a Cornish engine, which, with a given maximum pressure of steam in the cylinder at a given velocity, shall perform a given amount of work at the least possible pecuniary cost, taking into account the expense of fuel, and the interest of the capital required for the construction of the engine.

This problem is solved with the aid of the tables already printed, by drawing two straight lines on a diagram annexed to this paper (Plate III.).

The merit of first proposing the question of the œconomy of expansive engines in this definite shape belongs, I believe, to the Artizan Club, who have offered premiums for its solution; having done so (to use their own words) "with a view to enable those who, from their position, cannot take part in the discussions of the various scientific societies, to give the profession the benefit of their studies and experience." The 5th of April is the latest day fixed by them for receiving papers; and as this communication cannot possibly be read to a meeting before the 7th of April, nor published until some months afterwards, I trust I may feel confident that it will not be considered as interfering with their design.

Formulæ applicable to the Cornish Engine.

(35.) The equations of motion of the steam-engine in this and the original paper are the same in their general form with those of M. de Pambour. The differences consist in the expressions for the pressure and volume of steam, and for the mechanical effect of its expansion; the former of which were deduced from a formula suggested by peculiar hypothetical views, and the latter from the dynamical theory of heat.

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* Read to the Royal Society of Edinburgh, April 21, 1851.

Those equations are Nos. (50) and (51) of the original paper. I shall now express them in a form more convenient for practical use, the notation being as follows:—

Let A be the area of the piston.

l, the length of stroke.

n, the number of double strokes in unity of time.

c, the fraction of the total bulk of steam above the piston when down, allowed for clearance and for filling steam-passages; so that the total bulk of steam at the end of the effective stroke is

$$\frac{lA}{1-c}. \qquad (a)$$

l', the length of the portion of the stroke performed when the steam is cut off.

s, the ratio of expansion of the steam, so that

$$\left.\begin{aligned} \frac{1}{s} &= (1-c)\frac{l'}{l}+c \\ \frac{l'}{l} &= \frac{\frac{1}{s}-c}{1-c} \end{aligned}\right\} \qquad (b)$$

Let W be the weight of steam expended in unity of time.

P_1, the pressure at which it enters the cylinder.

V_1, the volume of unity of weight of steam at saturation at the pressure P_1, which may be found from Table I.*

F, the sum of all the resistances not depending on the useful load reduced to a pressure per unit of area of piston; whether arising from imperfect vacuum in the condenser, resistance of the air-pump, feed-pump, and cold water pump, friction, or any other cause.

R, the resistance arising from the *useful load*, reduced to a pressure per unit of area of piston.

Z, the ratio of the total action of steam working at the expansion s to its action without expansion. Values of this ratio are given in Table II.

Then the following are the two fundamental equations of the motion of the steam-engine, as comprehended in equation (50) of the original paper.

First. Equality of power and effect,

$$RAln = WV_1\{P_1(Z-cs)-F(1-c)s\}. \qquad (c)$$

Secondly. Equality of two expressions for the weight of steam

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* The volumes thus found (as has been already stated), though near enough the truth for practical purposes, are only approximately correct, having been computed on the assumption that steam is a perfect gas.

expended in unity of time,

$$W = \frac{Aln}{V_1(1-c)s}. \quad \ldots \ldots \quad (d)$$

From these two equations is deduced the following, expressing the ratio of the mean load on the piston to the initial pressure of the steam :—

$$\frac{R+F}{P_1} = \frac{Z-cs}{(1-c)s} \quad \ldots \ldots \quad (e)$$

being equivalent to equation (51).

In computing the effect of Cornish engines, these formulæ require to be modified, owing to the following circumstances.

The terms depending on the clearance c have been introduced into equations (c), (d), on the supposition that the steam employed in filling the space above the piston at the top of its stroke is lost, being allowed to escape into the condenser, without having effected any work; so that a weight of steam Wcs is wasted, and an amount of power $WV_1(P_1-F)cs$ lost, in unity of time. But in Cornish engines this is not the case; for by closing the equilibrium-valve at the proper point of the up or out-door stroke, nearly the whole quantity of steam necessary to fill the clearance and valve-boxes may be kept imprisoned above the piston so as to make the loss of power depending on it insensible in practice. This portion of steam is called a cushion, from its preventing a shock at the end of the up-stroke; and as Mr. Pole, in his valuable work on the Cornish engine, has observed, its alternate compression and expansion compensate each other, and have no effect on the duty of the engine. The proper moment of closing the equilibrium-valve is fixed by trial, which is, perhaps, the best way; but if it is to be fixed by theory, the following is the proper formula. Let l'' be the length of the portion of the up-stroke remaining to be performed after the equilibrium-valve has been closed, then

$$\frac{l''}{l} = \frac{c(s-1)}{1-c}. \quad \ldots \ldots \ldots \quad (f)$$

A slight deviation from this adjustment will produce little effect in practice if the fraction c is small*.

In forming the equations of motion, therefore, of the Cornish engine, we may, without material error in practice, omit the terms denoting a waste of steam and loss of power due to clearance and filling of steam-passages, and the results are the following :—

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* The same equation will serve to determine the proper moment for closing the exhaust-port in non-condensing engines, so as nearly to prevent loss of power from clearance.

Equation of effect and power in unity of time,

$$\text{Useful effect } E=RAln=WV_1\{P_1Z-F\}. \quad . \quad . \quad (57)$$

Weight of steam expended in unity of time,

$$W=\frac{Aln}{V_1 s}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

From those two fundamental equations the following are deduced:—

Ratio of mean load on piston to maximum pressure,

$$\frac{R+F}{P_1}=\frac{Z}{s}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (59)$$

Duty of unity of weight of steam,

$$\frac{E}{W}=V_1(P_1Z-F); \quad . \quad . \quad . \quad . \quad . \quad (60)$$

which, being multiplied by the number of units of weight of steam produced by a given weight of fuel, gives the duty of that weight of fuel.

Weight of steam expended per stroke,

$$\frac{W}{n}=\frac{Al}{V_1 s}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (61)$$

In fact, it is clear that if any five quantities out of the following seven be given, the other two may be determined by means of the equations—

$R+F$, the mean load on unit of area of piston.
P_1, the maximum pressure of steam in the cylinder.
s, the ratio of expansion.
W, the weight of steam produced in unity of time.
A, the area of the piston.
l, the length of stroke.
n, the number of strokes in unity of time.
The other quantities, E, V_1, Z, are functions of those seven.

Comparison of the Theory with Mr. Wicksteed's *Experiments.*

(36.) In order to test the practical value of this theory, I shall compare its results with those of the experiments which were made by Mr. Wicksteed on the large Cornish pumping-engine, built under the direction of that eminent engineer by Messrs. Harvey and West, for the East London Water-works at Old Ford, and which were published in 1841. The dimensions and structure of the engine, and the details of the experiments, are stated with such minuteness and precision, that there is none of that uncertainty respecting the circumstances of particular cases, which is the most frequent cause of failure in the attempt to apply theoretical principles to practice.

The engine was worked under a uniform load at five different rates of expansion successively. The number of strokes and the consumption of steam during each trial having been accurately registered, Mr. Wicksteed gives a table showing the weight of steam consumed per stroke for each of the five rates of expansion. I shall now compute the weight of steam per stroke theoretically, and compare the results.

Throughout these calculations I shall uniformly use the foot as the unit of length, the avoirdupois pound as that of weight, and the hour as that of time. Pressures are consequently expressed in pounds per square foot for the purpose of calculation; although in the table of experiments I have reduced them to pounds per square inch, as being the more familiar denomination.

The data respecting the dimensions and load of the engine, which are constant throughout the experiments, are the following:—

Area of piston . . . . . $A = 34\cdot854$ square feet.
Stroke . . . . $l = 10$ feet.
Cubic space traversed by piston during one down stroke . } $= Al = 348\cdot54$ cubic feet.
Clearance and valve-boxes . . . . $18\cdot00$
Sum . . . . . $366\cdot54$

Therefore, $c = 0\cdot05$

$\mathrm{R} =$ useful load of piston . $= 1597\cdot$ lbs. per square foot.
$\mathrm{F} =$ additional resistance . $= 266\cdot6$ ...
$\mathrm{R} + \mathrm{F} =$ total mean pressure on piston . . . . } $= 1863\cdot6$ ...

The mode of calculation is the following:—

Mr. Wicksteed states the fraction $\frac{l'}{l}$ of the stroke performed at full pressure in each experiment. From this the ratio of expansion s is computed by equation (*b*), giving in this case

$$\frac{1}{s} = 0\cdot95\frac{l'}{l} + 0\cdot05.$$

The value of Z corresponding to s is then found by means of the third column of Table II.; that column being selected because the initial pressures were all below four atmospheres. This affords the means of determining the initial pressure of the steam by equation (59), viz.

$$\mathrm{P}_1 = \frac{s}{\mathrm{Z}}(\mathrm{R}+\mathrm{F}) = 1863\cdot6\,\frac{s}{\mathrm{Z}}.$$

By using Table I. according to the directions prefixed to it, the volume of one pound of steam at the pressure P_1 in cubic feet is calculated, and thence by equation (60) the weight of steam per stroke, according to theory, which is compared with the weight as ascertained by experiment.

Further to illustrate the subject, the useful effect or duty of a pound of steam is computed according to the theory and the experiments respectively, and the results compared.

The following table exhibits the results:—

Comparison of the Theory with Mr. Wicksteed's *Experiments on the Cornish Pumping-engine at Old Ford.*

Nnmber of experiments.	Pressure in the boiler, lb. per square inch.	Steam cut off at $\frac{l'}{l}$ of the stroke.	Ratio of expansion s.	Maximum pressure in the cylinder, lb. per square inch.	Lb. of steam expended per stroke,		Difference.	Duty of one lb. of steam,		Difference.
					By theory.	By experiment.		By theory.	By experiment.	
								ft.-lbs.	ft.-lbs.	
B.	30·45	0·603	1·605	14·27	7·781	7·536	−0·245	71530	73860	+2330
C.	33·20	0·477	1·988	15·59	6·963	6·463	−0·500	79936	86123	+6187
D.	39·2	0·397	2·342	16·9	6·236	6·200	−0·036	89275	89776	+ 501
E.	41·2	0·352	2·605	17·89	5·905	5·985	+0·085	94258	93002	−1256
F.	45·7	0·313	2·882	18·93	5·626	5·470	−0·156	98940	101756	+2816
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)

This comparison sufficiently proves that the results of the theory are practically correct.

It is remarkable, that in every instance except one (experiment E) the experimental results show a somewhat less expenditure of steam per stroke, and a greater duty per pound of steam, than theory indicates. This is to be ascribed to the fact, that although the action of the steam is computed theoretically, on the assumption that during the expansion it is cut off from external sources of heat, yet it is not exactly so in practice; for the cylinder is surrounded with a jacket or casing communicating with the boiler, in which the temperature is much higher than the highest temperature in the cylinder, the pressure in the boiler being more than double the maximum pressure of the steam when working, as columns (2) and (5) show. There is, therefore, a portion of steam of whose amount no computation can be made, which circulates between the boiler and the jacket, serving to convey heat to the cylinder, and thus augment by a small quantity the action of the steam expended; and hence the formulæ almost always err on the safe side*.

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* Another, and perhaps a more important cause of the excess of the actual above the calculated performance of a pound of steam is the following. It has been proved experimentally by Messrs. Joule and Thomson in the case of air and other gases, and by Mr. C. W. Siemens in the case

Supposing one pound of the best Welsh coals to be capable (as found by Mr. Wicksteed) of evaporating 9·493 lbs. of water at the pressure in the boiler during the experiment F, then the duty of a Cornish bushel, or 94 lbs. of such coals, in the circumstances of that experiment would be—

By theory	88,288,000 ft. lbs.
By experiment . . .	90,801,000 ...
Difference . . .	+ 2,513,000 ...

Œconomy of Single-acting Expansive Engines.

(37.) By increasing the ratio of expansion in a Cornish engine, the quantity of steam required to perform a given duty is diminished; and the cost of fuel and of the boilers is lowered. But at the same time, as the cylinders and every part of the engine must be made larger to admit of a greater expansion, the cost of the engine is increased. It thus becomes a problem of maxima and minima to determine what ratio of expansion ought to be adopted under given circumstances, in order that the sum of the annual cost of fuel, and the interest of the capital employed in construction, may be the least possible as compared with the work done.

That this problem may admit of a definite solution, the following five quantities must be given :—

P_1, the initial pressure in the cylinder.

F, the resistance not depending on the useful load.

ln, the amount of the length of the effective strokes made in unity of time.

h, the annual cost of producing unity of weight of steam in unity of time, which consists of two parts; the price of fuel, and the interest of the cost of the boilers.

k, the interest of the cost of the engine, per unit of area of piston.

Hence the annual expenditure to be taken into consideration,

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of steam, that when an elastic fluid expands without performing work, by rushing through a narrow orifice, so that all the power developed by the expansion is expended in agitating the particles of the fluid, and extinguished by their friction, then the heat produced by that friction partially compensates for the heat which disappears during the expansion. Hence in a Cornish engine, when the steam is *wire-drawn* so as to be at a lower pressure in the cylinder at the commencement of the stroke than in the boiler, the friction of its particles causes its temperature, and therefore its volume in the cylinder, to be greater than those corresponding to saturation at the given pressure, upon which the calculations in the text are founded; and hence a somewhat less weight of steam than that computed by the formulæ suffices to perform a given amount of work.

reduced to unity of weight of steam, is

$$h+k\frac{\mathrm{A}}{\mathrm{W}}=h+k\frac{\mathrm{V}_1 s}{ln}.$$

And the useful effect of unity of weight of steam being

$$\mathrm{V}_1(\mathrm{P}_1\mathrm{Z}-\mathrm{F}s),$$

the problem is to determine the ratio of expansion s, so that

$$\frac{\mathrm{V}_1(\mathrm{P}_1\mathrm{Z}-\mathrm{F}s)}{h+k\frac{\mathrm{V}_1 s}{ln}}$$

shall be a maximum.

Dividing the numerator of this fraction by $\mathrm{V}_1\mathrm{P}_1$, and the denominator by $\frac{k\mathrm{V}_1}{ln}$, both of which are constants in this problem, we find that it will be solved by making the ratio

$$\frac{\mathrm{Z}-\frac{\mathrm{F}}{\mathrm{P}_1}s}{\frac{hln}{k\mathrm{V}_1}+s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (62)$$

a maximum.

The algebraical solution would be extremely complicated and tedious. The graphic solution, on the other hand, is very simple and rapid, and sufficiently accurate for all practical purposes, and I have therefore adopted it.

In the annexed diagram, Plate III. fig. 1, the axis of abscissæ, $-\mathrm{XO}+\mathrm{X}$, is graduated from O towards $+\mathrm{X}$ into divisions representing ratios of expansion, or values of s. The divisions of the axis of ordinates, OY, represent values of Z. The curve marked "locus of Z" is laid down from the third column of Table II. of the Appendix to the original paper, being applicable to initial pressures not exceeding four atmospheres.

Through the origin O draw a straight line BOA, at such an inclination to $-\mathrm{XO}+\mathrm{X}$ that its ordinates are represented by $\frac{\mathrm{F}}{\mathrm{P}_1}s$. Then the ordinates measured from this inclined line to the locus of Z represent the value of the numerator $\mathrm{Z}-\frac{\mathrm{F}}{\mathrm{P}_1}s$, of the ratio (62), corresponding to the various values of s.

Take a point at C on the line BOA, whose abscissa, measured along $\mathrm{O}-\mathrm{X}$, represents $-\frac{hnl}{k\mathrm{V}_1}$. Then the ordinates, measured from BOA, of any straight line drawn through C, vary proportionally to the denominator $\frac{hnl}{k\mathrm{V}_1}+s$ of the ratio (62).

Through the point C, therefore, draw a straight line CT, touching the locus of Z; then the ratio (62) is a maximum at the point of contact T, and the abscissa at that point represents the ratio of expansion required.

Example.

(38.) To exemplify this method, let us take the following data.

Greatest pressure in the cylinder $P_1=20$ lbs. per square inch, $=2880$ lbs. per square foot.

The corresponding value of V_1 is 20·248 cubic feet per pound of steam.

To obtain this initial pressure in the cylinder of a Cornish engine, it will be necessary to have a pressure of about 50 lbs. per square inch in the boiler.

F, resistance not depending on the useful load $=2$ lbs. per square inch, $=288$ lbs. per square foot, $=\frac{1}{10}P_1$.

ln, amount of down strokes, $=4800$ feet per hour; being the average speed found to answer best in practice.

To estimate h, the annual cost of producing one pound of steam per hour, I shall suppose that the engine works 6000 hours per annum; that the cost of fuel is one penny per 100 lbs. of steam*; that the cost of boiler for each pound of steam per hour is 0·016 ton, at £27, $=$ £0·432; and that the interest of capital is five per cent. per annum. Hence h is thus made up:—

Fuel for 6000 lbs. of steam at 0·01*d*. . .	£0·2500
Interest on £0·432, at 5 per cent. . . .	0·0216
	$h=$ £0·2716

Estimating the cost of the engine at £250 per square foot of piston, we find $k=5$ per cent. per annum on £250 $=$ £12·5, and

$$\frac{h}{k}=0·0217;\quad \frac{hln}{kV_1}=5·144.$$

The line BOA, then, is to be drawn so that its ordinates are $\frac{F}{P_1}s=\frac{1}{10}s$.

The point C is taken on this line, at $\frac{hln}{kV_1}=5·144$ divisions of the axis of abscissæ to the left of OY.

The tangent CT being drawn, is found to touch the locus of Z at 2·800 divisions to the right of OY.

Then $s=2·800$ is the ratio of expansion sought, corresponding to the greatest œconomy.

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* This estimate is made on the supposition that coals, capable of producing nine times their weight of steam, are worth about 16*s*. 9*d*. per ton.

If we make $c = 0{\cdot}05$, as in Mr. Wicksteed's engine, then the fraction of the stroke to be performed at full pressure is

$$\frac{l'}{l} = 0{\cdot}323,$$

being nearly the same as in experiment F.

The mean resistance of the useful load per square foot of piston is

$$R = \frac{Z}{s} P_1 - F = 1713{\cdot}6 \text{ lbs.}$$

The duty of one square foot of piston per hour,

$$Rln = 8{,}225{,}300 \text{ foot-lbs.}$$

And one horse-power being 1,980,000 foot-lbs. per hour, the real horse-power of the engine is

4·154 per square foot of piston.

The duty of one pound of steam is

$$RV_1 s = 97{,}154 \text{ foot-lbs.}$$

To give an example of a special case, let the duty to be performed be 198,000,000 foot-pounds per hour, being equal to 100 real horse-power, for 6000 hours per annum. This being called E, we find from the above data that the area of piston required is

$$A = \frac{E}{Rln} = 24{\cdot}072 \text{ square feet.}$$

The consumption of steam per hour is

$$W = \frac{E}{RV_1 s} = 2038 \text{ lbs.},$$

which requires $2038 \times 0{\cdot}016 = 32{\cdot}608$ tons of boilers.

The expenditure of steam per annum is

$$2038 \times 6000 = 12{,}228{,}000 \text{ lbs.}$$

Hence we have the following estimate:—

Cost of engine, 24·072 square feet of piston at £250	£6018·000
Cost of boilers, 32·608 tons at £27	880·416
Total capital expended	£6898·416
Interest at 5 per cent. per annum	£344·921
Cost of fuel per annum, 12,228,000 lbs. of steam at 0·01*d*.	509·500
Annual cost for interest and fuel	£854·421

I wish it to be understood that the rates I have adopted in the foregoing calculations, for interest, cost of fuel, and cost of construction, are not intended as estimates of their average amount, nor of their amount in any particular case, but are merely assumed in order to illustrate by a numerical example the rules laid down in the preceding article. It is of course the business of the engineer to ascertain those data with reference to the special situation and circumstances of the proposed work; and having done so, the method explained in this paper will enable him to determine the dimensions and ratio of expansion which ought to be adopted for the engine, in order that it may effect its duty with the greatest possible œconomy.

SECTION V. *On the Œconomy of Heat in Expansive Machines**.

(39.) A machine working by expansive power consists essentially of a portion of some substance to which heat is communicated, so as to expand it, at a higher temperature, being abstracted from it, so as to condense it to its original volume at a lower temperature. The quantity of heat given out by the substance is less than the quantity received; the difference disappearing as heat to appear in the form of expansive power.

The heat originally received by the working body may act in two ways; to raise its temperature, and to expand it. The heat given out may also act in two ways; to lower the temperature, and to contract the body. Now as the conversion of heat into expansive power arises from changes of volume only, and not from changes of temperature, it is obvious that the proportion of the heat received which is converted into expansive power will be the greatest possible, when the reception of heat, and its emission, each take place at a constant temperature.

(40.) Carnot was the first to assert the law, *that the ratio of the maximum mechanical effect to the whole heat expended in an expansive machine is a function solely of the two temperatures at which the heat is respectively received and emitted, and is independent of the nature of the working substance.* But his investigations not being based on the principle of the dynamical convertibility of heat, involve the fallacy that power can be produced out of nothing.

(41.) The merit of combining *Carnot's law,* as it is termed, with that of the convertibility of heat and power, belongs to M. Clausius and Professor William Thomson; and in the shape into which they have brought it, it may be stated thus:—

The maximum proportion of heat converted into expansive power by any machine is a function solely of the temperatures at which

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* Read to the Royal Society of Edinburgh, April 21, 1851.

heat is received and emitted by the working substance; which function, for each pair of temperatures, is the same for all substances in nature.

This law is laid down by M. Clausius, as it originally had been by Carnot, as an independent axiom; and I had at first doubts as to the soundness of the reasoning by which he maintained it. Having stated those doubts to Professor Thomson, I am indebted to him for having induced me to investigate the subject thoroughly; for although I have not yet seen his paper, nor become acquainted with the method by which he proves Carnot's law, I have received from him a statement of some of his more important results.

(**42.**) I have now come to the conclusions,—

First. *That* Carnot's *law is not an independent principle in the theory of heat, but is deducible, as a consequence, from the equations of the mutual conversion of heat and expansive power, as given in the first Section of this paper.*

Secondly. *That the function of the temperatures of reception and emission, which expresses the maximum ratio of the heat converted into power to the total heat received by the working body, is the ratio of the difference of those temperatures to the absolute temperature of reception diminished by the constant, which I have called* $\kappa = \mathrm{C}n\mu b$, and which must, as I have shown in the Introduction, be the same for all substances, in order that molecular equilibrium may be possible.

(43.) Let abscissæ, parallel to OX in the diagram, Pl. III. fig. 2, denote the volumes successively assumed by the working body; and ordinates, parallel to OY, the corresponding pressures. Let τ_1 be the constant absolute temperature at which the reception of heat by the body takes place; τ_0 the constant absolute temperature at which the emission of heat takes place. Let AB be a curve such that its ordinates denote the pressures, at the temperature of reception τ_1, corresponding to the volumes denoted by abscissæ. Let DC be a similar curve for the temperature of emission τ_0. Let AD and BC be two curves, expressing by their coordinates how the pressure and volume must vary in order that the body may change its temperature without receiving or emitting heat; the former corresponding to the most condensed, and the latter to the most expanded state of the body during the working of the machine.

The quantity of heat received or emitted during an operation on the body, involving indefinitely small variations of volume and temperature, is expressed by adding to equation (6) of Section IV. the heat due to change of temperature only in virtue of the real specific heat. We thus obtain the differential equation

$$\delta Q' + \delta Q = + \frac{\tau - \kappa}{CnM} \left\{ \delta V \left(\frac{1}{V} - \frac{dU}{dV} \right) - \delta\tau . \frac{dU}{d\tau} \right\}$$
$$+ \mathfrak{k}\delta\tau,$$

in which the positive sign denotes absorption, and the negative emission.

If we now put for $\frac{dU}{dV}, \frac{dU}{d\tau}$ their values according to equation (11.), we find

$$\delta Q' + \delta Q = + (\tau - \kappa) \frac{dP}{d\tau} . \delta V$$
$$+ \left\{ \mathfrak{k} + \frac{1}{CnM} \left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2} \right) + (\tau - \kappa) \frac{d}{d\tau} \int \frac{dP}{d\tau} dV \right\} \delta\tau. \quad (63)$$

The first term represents the variation of heat due to variation of volume only; the second, that due to variation of temperature. Let us now apply this equation to the cycle of operations undergone by the working body in an expansive machine, as denoted by the diagram.

First operation. The body, being at first at the volume V_A and pressure P_A, is made to expand by the communication of heat at the constant temperature τ_1, until it reaches the volume V_B and pressure P_B, AB being the locus of the pressures.

Here $\delta\tau = 0$; therefore the total heat received is

$$\left. \begin{aligned} H_1 &= (\tau_1 - \kappa) \int_{V_A}^{V_B} \frac{dP}{d\tau} dV \\ &= (\tau_1 - \kappa) \{ \phi(V_B, \tau_1) - \phi(V_A, \tau_1) \} \end{aligned} \right\} \quad . \quad . \quad . \quad (a)$$

Second operation. The body, being prevented from receiving or emitting heat, expands until it falls to the temperature τ_0, the locus of the pressures being the curve BC. During this operation the following condition must be fulfilled,

$$0 = \delta Q' + \delta Q ;$$

which, attending to the fact that V is now a function of τ, and transforming the integrals as before, gives the equation

$$0 = \mathfrak{k} + \frac{1}{CnM} \left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2} \right) + (\tau - \kappa) \left(\frac{d}{d\tau} + \frac{dV}{d\tau} \cdot \frac{d}{dV} \right) \phi(V, \tau).$$

This equation shows that

$$\phi(V_B, \tau_1) - \phi(V_C, \tau_0) = \psi(\tau_1, \tau_0). \quad . \quad . \quad . \quad (b)$$

Third operation. The body, by the abstraction of heat, is made to contract at the constant temperature τ_0, to the volume V_D and pressure P_D, which are such as to satisfy conditions epending on the fourth operation. CD is the locus of the pressures. The heat emitted is evidently

$$H_0=(\tau_0-\kappa)\{\phi(V_C,\tau_0)-\phi(V_D,\tau_0)\}. \quad . \quad . \quad (c)$$

Fourth operation. The body, being prevented from receiving or emitting heat, is compressed until it recovers its original temperature τ_1, volume V, and pressure P_A; the locus of the pressures being DA. During this operation, the same conditions must be fulfilled as in the second operation; therefore

$$\phi(V_A,\tau_1)-\phi(V_D,\tau_0)=\psi(\tau_1,\tau_0), \quad . \quad . \quad . \quad (d)$$

ψ being the same function as in equation (*b*).

By comparing equations (*b*) and (*d*), we obtain the relation which must subsist between the four volumes to which the body is successively brought in order that the maximum effect may be obtained from the heat. It is expressed by the equation

$$\phi(V_B,\tau_1)-\phi(V_A,\tau_1)=\phi(V_C,\tau_0)-\phi(V_D,\tau_0). \quad . \quad (64)$$

From this and equations (*a*) and (*c*), it appears that

$$\frac{H_0}{H_1}=\frac{\tau_0-\kappa}{\tau_1-\kappa}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (65)$$

That is to say, *when no heat is employed in producing variations of temperature, the ratio of the heat received to the heat emitted by the working body of an expansive machine is equal to that of the absolute temperatures of reception and emission, each diminished by the constant κ, which is the same for all substances.*

Hence let

$$\Pi=H_1-H_0$$

denote the maximum amount of power which can be obtained out of the total heat H_1 in an expansive machine working between the temperatures τ_1 and τ_0. Then

$$\frac{\Pi}{H_1}=\frac{\tau_1-\tau_0}{\tau_1-\kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (66)$$

being the law which has been enunciated in article 42, and which is deduced entirely from the principles already laid down in the Introduction and first Section of this paper.

The value of the constant κ is unknown; and the nearest approximation to accuracy which we can at present make is, to neglect it in calculation as being very small as compared with τ*.

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* Subsequent investigations, founded chiefly on the experiments of Messrs. Joule and Thomson on the thermic phænomena of currents of elastic fluids, have shown that the constant κ, as anticipated in the text, is actually very small, if not altogether inappreciable. Its approximate values, computed from these experiments, range from 0° to about 2° Centigrade; the discrepancies being too small to affect materially the computation of the power of engines.

(44.) This approximation having been adopted, I believe it will be found that the formula (66), although very different in appearance from that arrived at by Professor Thomson, gives nearly the same numerical results. For example: let the machine work between the temperatures 140° and 30° Centigrade; then $\tau_1=414^{\circ}{\cdot}6$, $\tau_0=304^{\circ}{\cdot}6$, and

$$\frac{\Pi}{\mathrm{H}_1}=0{\cdot}2653.$$

Professor Thomson has informed me, that for the same temperatures he finds this ratio to be 0·2713*.

(45.) To make a steam-engine work according to the conditions of maximum effect here laid down, the steam must enter the cylinder from the boiler without diminishing in pressure, and must be worked expansively down to the pressure and temperature of condensation. It must then be so far liquefied by conduction alone, that on the liquefaction being completed by compression, it may be restored to the temperature of the boiler by means of that compression alone. These conditions are unattainable in steam-engines as at present constructed, and different from those which form the basis of the formulæ and tables in the fourth Section of this paper; hence it is found, both by experiment and by calculation from those formulæ, that the proportion of the total heat converted into power in any possible steam-engine is less than that indicated by equation (66).

The annexed table illustrates this:—

Case.	Absolute temperature in the boiler = τ_1 Centigrade.	Absolute temperature in the condenser = τ_0 Centigrade.	Total heat expended in Centigrade degrees applied to liquid water.	Heat transformed into expansive power in Centigrade degrees applied to liquid water.	Proportion of heat rendered effective.	Maximum proportion according to Carnot's law.
Mr. Wicksteed's engine, experiment F, by calculation......	$135^{\circ}{\cdot}2+274^{\circ}{\cdot}6=409^{\circ}{\cdot}8$	$30^{\circ}\ \ +274^{\circ}{\cdot}6=304^{\circ}{\cdot}6$	$617^{\circ}{\cdot}7$	$71^{\circ}{\cdot}2$	0·1153	0·2567
Ditto by observation.	Ditto.	Ditto.	Ditto.	73·23	0·1185	Ditto.
(1)	(2)	(3)	(4)	(5)	(6)	(7)

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* From information which I have received from Professor Thomson subsequently to the completion of this paper, it appears that his formula becomes identical with the approximate formula here proposed, on making the function called by him $\mu=\frac{\mathrm{J}}{\tau}$, J being Joule's equivalent.

Mr. Joule also, some time since, arrived at this approximate formula in the particular case of a perfect gas.

The heat transformed into power, as given in the fifth column, has been reduced to Centigrade degrees in liquid water, by dividing the duty of a pound of steam by Mr. Joule's equivalent, 1389·6 feet per Centigrade degree.

In these examples, founded on the calculated and observed duty of Mr. Wicksteed's engine during experiment F, the actual ratio is less than half the maximum. This waste of heat is to be ascribed to the following causes:—

First. The expenditure of heat in raising the feed-water to the temperature of evaporation.

Secondly. The initial pressure in the cylinder is but 18·93 lbs. on the square inch, while that in the boiler is 45·7.

Thirdly. The expansive working of the steam, instead of being continued down to 30° Centigrade, the temperature of the condenser, stops at a much higher temperature, 74°·66. This is the most important cause of loss of power.

XXXVII. *On the Theory of Compound Colours.*

By Professor GRASSMANN*.

[With a Plate.]

IN the 87th volume of this journal†, Helmholtz published a series of observations, some of which were new and ingenious, and from which he comes to the conclusion, that the theory of compound colours, universally admitted since the time of Newton, is erroneous in its most essential points; and that there are properly only two prismatic colours, yellow and indigo, which when mixed give white. It may consequently not be regarded as superfluous to show that the Newtonian theory of compound colours is correct to a certain point, and especially that the opinion that every colour has its complementary colour, with which when mixed it gives white, is founded upon mathematically incontestable facts, so that this opinion must be regarded as one of the most tenable in physics. I will then show that the *positive* observations of Helmholtz, instead of tending to the subversion of this theory, serve rather partly to prove and partly to complete it.

For this purpose it will be necessary to analyse the impression of colour of which the eye is capable into its elements. The eye first of all distinguishes light as coloured and colourless. In colourless light (white, gray) it only distinguishes a greater or less *intensity*, and this may be mathematically determined.

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* From the *Annalen der Physik und Chemie*, 1853, p. 69.

† Philosophical Magazine, S. 4. vol. iv. p. 519.

In like manner we only distinguish the greater or less intensity of a homogeneous colour. But even for the difference of the individual homogeneous colours we have a mathematically determinable standard, which is furnished most perfectly by the duration of vibration corresponding to each colour; even in ordinary speech, this difference has been very suitably distinguished by the expression *tint*. Thus, in a homogeneous colour we can distinguish both its intensity and its tint. If now a homogeneous colour be mixed with colourless light, the impression of colour becomes weakened by this intermixture. Ordinary language is rich in terms by which this difference is characterized; the expressions, intense, deep, pale, dull, whitish, which are added to the names of colours, may serve as instances. The scientific terminology, which must be substituted for this popular nomenclature, arises spontaneously from what has been above stated, as every impression of colour of this kind may be analysed into three mathematically determinable elements,—*the tint, the intensity of the colour*, and the *intensity of the intermixed white*. The various tints form a continuous series of such a kind, that when we start from one colour of this series and proceed forward, we finally arrive at the original colour. One circumstance, however, must not be left unnoticed here, namely, the difficulty of obtaining homogeneous red light, which forms the transition between the violet and red of the ordinary spectrum, and which can only be produced by the prism under remarkably favourable circumstances (on a bright summer's noon)*. This outermost colour of the spectrum, which may be equally well regarded as extreme red or extreme violet, I will call *purple*. If now we regard any compounded light, the eye can distinguish in it only the three elements above mentioned; that is to say, every impression of light may be imitated by mixing a homogeneous colour of a certain intensity with colourless light of a certain intensity. Thus in every sensation of light we have to distinguish three things,—the intensity of the colour, the tint, and the intensity of the intermixed colourless light. It would be easy to make an apparatus by means of which we should be enabled to determine every colour according to these three elements. In order to give an idea of this, let us suppose two white screens of similar nature moveable upon a hinge, in such a manner that the white side of the screens shall be upon the outside of the angle formed at the hinge; let a divided circle be also provided to measure this angle. The coloured light to be examined is now allowed to fall upon one of these screens in

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* See Poggendorff's *Annalen*, vol. xiii. p. 441.

a plane perpendicular to the axis of rotation; on the other screen, and in the same plane, white light is suffered to fall in any direction; and in a direction perpendicular to the latter, homogeneous light; the light being so selected that it may have the same tint as the light to be examined. If now this latter screen be turned upon the hinge, any desired ratio of intensity may be given to the colourless and homogeneous light which is dispersed on all sides from this screen. If the first screen be then also turned upon the hinge, any degree of intensity less than that produced by light falling on it perpendicularly may be given to the light diffused from it. In this manner a position of the screens must necessarily be found, if the light falling upon the second be not too strong, in which both will produce the same impression upon an eye observing them both at the same time. Such an apparatus would therefore be sufficient to determine mathematically all the elements which may come under observation.

The above statement, that the eye can only distinguish these three elements directly, might indeed be doubted. And truly it might be difficult to prove it directly, since there always remains the possibility that one eye, by virtue of its peculiar organization, might perhaps discover differences invisible to another. However, for our purpose the fact is perfectly sufficient, that hitherto no observer has been able to mention another element determining the impression of colour; and moreover, in ordinary language, we only recognise these three elements in the description of this phænomenon, so that we may affirm with certainty that hitherto only these three elements have been observed in the impression of colour; in the following arguments we shall only go back to this assertion.

In the second place we assume, "that if one of two mingling lights be continuously altered (whilst the other remains unchanged), the impression of the mixed light also is continuously changed."

We say that an impression of light is continuously altered when the two intensities (that of the colour and that of the intermixed colourless light) are continuously changed; and where the tint also, supposing that the intensity is not zero, continuously changes. If the colour have no intensity, the light is colourless; and consequently by the continuous diminution of the intensity to zero, a tint may gradually pass over into another completely separated from it, if the intensity of the latter increase continuously from nothing upwards. It scarcely needs to be mentioned, that the case in which one or more of the elements determining the impression remain the same must be regarded as coming within the idea of continuity. Now as regards the continuous change of the tint, this will generally be produced by the continuous change of the duration of vibration determining this tint; with this difference, that the impression of colour of the extreme violet will continuously unite itself to that of the extreme red. In point of fact, the transition from violet to red through purple is just as continuous to the eye as that between any two other colours, although the limit has not yet been fixed by observation at which the same impression of colour is reproduced by a different duration of vibration. I will call the transition from red through orange, yellow, green, blue, violet, and purple back to red, the *positive*, and that in the reverse direction the *negative* transition. According to this, any coloured light A may continuously pass into another coloured light B in three different ways; namely, by the colour continuously assuming all the tints which lie between A and B in the positive transition, or by its passing through all those in the negative transition, or lastly, by the light becoming colourless once or several times during its transition. The principle of continuous transition which we have just developed must be regarded as perfectly established by experience, as a sudden spring in the phænomena would be apparent even in the most crude observations, and such a spring has not as yet been discovered.

From these propositions the following position may be derived with mathematical certainty:—

"To every colour belongs another homogeneous colour, which, when mixed with it, gives colourless light."

Proof. Let a be the tint of the given colour. Let it be assumed that there is no homogeneous colour, which, when mixed with this, furnishes colourless light; let any homogeneous colour whatever be taken possessing the tint of x and the intensity y. If now, whilst x remains constant, y be increased from nothing upwards until the intensity of the colour a disappears in comparison, the mixture will continuously change; and as, according to the assumption, it can never give colourless light, its tint will also change continuously; thus, as the mixture at first has the tint a, and finally the tint x, it has continuously passed from a to x. This transition may be either positive or negative; whether it be the one or the other will depend upon the tint x. If the tint x be supposed to differ infinitesimally from a, but on the side of positive transition, this transition will also be positive. For if it were negative, all tints besides those differing infinitesimally from a must be produced by the elevation of the intensity y, and therefore tints which are quite different from a; let y be an intensity by which a tint quite different from a is produced. Now it is clear that the colour, the tint of which is a and its intensity y, when mixed with a gives the tint a, whilst the colour which has the tint x and the intensity y furnishes quite a different tint; but these two colours mixed with a, with the same intensity y, have two infinitely close tints; that is to say, these two colours mixed with a pass continuously into one another, so that (according to the second proposition) the mixture must continuously change, as also its tint; but this should be quite different. Thus the supposition that the transition from a to x may be negative leads to a contradiction, that is, it must necessarily be positive. For the same reason, if x lies at an infinitely small distance from a towards the negative side, a negative transition from a to x will take place. If now the tint x be supposed to change continuously from a towards the positive side, so as to pass through the entire series of colours back to a, the corresponding transition of the mixture, which in each case is effected by the elevation of y, as it is at first positive and afterwards negative, must necessarily change its sign somewhere. Let a' be a tint in which this change occurs, so that before x reaches it the transition is positive, but as soon as it has passed it becomes negative. If the tint x passes continuously through the tint a', the tint of the mixture must continuously vary with every value of the intensity y, hence the whole of the tints which result from the increase of the intensity y lie extremely close together in both cases (when x lies at an infinitesimal distance from a' on the right or left side). This, however, is impossible, as some of them lie on the positive and the others on the negative transition from a to a'. Thus the supposition that there is no homogeneous colour, which, when mixed with a, furnishes white, leads to a contradiction; *i. e.* every colour has another homogeneous colour, which, when mixed with it, furnishes white. Q. E. D.

I have chosen the indirect form of proof, because in this manner the greatest possible exactness is most readily obtained without digression. Moreover, it is evident that this indirect form of proof carries with it the direct assertion that the colour a', at which the character of the transition changes, is the same which, when mixed with a, in any degree of intensity must give colourless light.

If now we test Helmholtz's experiments, we obtain from them, at least approximately, the colour which is capable of furnishing colourless light with any other given colour. For yellow, according to Helmholtz, this is indigo, a result which is by no means so divergent from the Newtonian theory of compound colours as it appears to be at first sight. Helmholtz has more exactly determined the two colours, which, according to him, furnish white; for the yellow lies between the lines D and E of Fraunhofer, and about three times as far from E as from D, the indigo extends from the middle of the space between the lines F and G almost to the line G, so that any indigo which lies between these limits furnishes white with any yellow which lies near the position above described. Comparison with the Newtonian theory of compound colours is rendered difficult by the fact, that the names of the colours as employed by various observers have not always the same sense, of which one may be very readily convinced by comparing the descriptions of the colours lying between the lines of Fraunhofer given in different handbooks and memoirs. Newton exactly describes the position of the limits between every two of his colours as they appeared in the spectrum of his prism; he also determined the mean refractive and dispersive indices of his prism; so that all the elements exist for determining the position of the Newtonian limits of the colours between Fraunhofer's lines as exactly as these determinations of Newton's will permit. On this principle, by a comparison of Fraunhofer's and Newton's measurements, and supposing that Newton's first red and last violet correspond with Fraunhofer's lines B and H, I have found that Newton's first orange (that is, the limit between red and orange) lies between the lines C and D at a proportionate distance of 7 : 6; his yellow commences between D and E, at a distance from the former line of about $\frac{1}{11}$th of the interval between D and E; his green commences also between D and E, at about $\frac{1}{11}$th of the interval between those two lines from the latter; his blue begins near F, at a distance of about $\frac{1}{14}$th of the interval FG from F; his indigo between F and G at a proportionate distance from those lines of 5 : 3; his violet commences at G. The assumption that the boundaries of the Newtonian spectrum coincide with the lines B and H is certainly somewhat arbitrary, but we arrive at the same result if we assume that the colours of mean refrangibility of Newton and Fraunhofer coincide. If we now construct Newton's coloured disc according to the rules given in his "Optics" (lib. i. pars 2, prop. vi.), and draw upon it the positions of the Fraunhofer lines as above given (Plate III. fig. 4), it appears that, according to the Newtonian theory, the yellow determined by Helmholtz furnishes white with an indigo which lies between the lines F and G of Fraunhofer, and which is distant from F and G in the proportion of 15 : 2. In the figure these colours are distinguished by the dotted line which unites them. This indigo therefore falls within the limits of colour, between which, according to Helmholtz, the complementary colours of yellow lie. Thus we see that this observation of Helmholtz agrees essentially with the result of Newton's investigations. As regards the other colours, Helmholtz denies the possibility of obtaining white by the mixture of two of them. But if we test any of his series of researches, as, for instance, that upon the mixture of red with the other colours, we shall always readily obtain the complementary colour. According to him, red gives with orange, yellow and green, the intermediate tones of colour which lie in this series, and therefore, according to our denomination, on the positive side of red. Thus, for example, according to Helmholtz, red mixed with green gives a *pale* yellow, which, when red predominates, passes over through orange into red; and when green prevails, passes through yellowish-green to green. In the same manner, red, with violet, indigo, and sky-blue, furnishes the intermediate tones of colour, which, according to us, lie on the negative side of red. According to Helmholtz, red mixed with azure-blue gives a *whitish* violet, which, when red predominates, passes into rose-colour and carmine. Thus, from the propositions proved above, the complementary colour of red must lie between green and azure-blue, and must therefore be a tint of bluish-green. Now Helmholtz says that the mixture of red with the greenish-blue tones produces a flesh-colour, but nothing is said as to how this flesh-colour passes over into bluish-green when this tint is in excess, although this must be the case. There is consequently a deficiency here. Moreover flesh-colour is nothing but a red mixed with much white, and it has no other conceivable transition to bluish-green, except by the gradual weakening of the red until it entirely disappears before the white, and the gradual production of bluish-green from this white (or gray); in short, the normal transition through colourless light takes place in this case. The same applies to the other series of experiments. The table of the complementary colours derived from them would be as follows:—

Yellow, Yellowish-green, Green, Bluish-green, Azure, Indigo,
Indigo, Violet, Purple, Red, Orange, Yellow,

in which the complementary colours stand one above another.

I have hitherto endeavoured to make as few assumptions as possible suffice. I will now, in order to introduce the main principle of compound colours, add a third assumption to the two preceding ones, namely,—

"That two colours, each of which has a constant tint and a constant intensity of the intermixed white, also give constant mixed colours, no matter of what homogeneous colours they may be composed."

This proposition also appears to be sufficiently proved by the preceding observations; for that coloured powders, when mixed, furnish results differing from those obtained when the light proceeding from them only is mixed, can form no objection, especially as Helmholtz has disclosed the cause of this difference.

Let *a* be a homogeneous colour, and *a'* that homogeneous colour which furnishes white when mixed with *a*. For the sake of clearness, let us suppose *a* and *a'* to be represented by two lines of equal length running from the same point in opposite directions (fig. 5). Let *b* also be a colour, which, when mixed with *a*, furnishes an equal quantity of white to that produced by its mixture with *a'*; and in order to express this equal relation of *b* to *a* and *a'*, let *b* be represented by another line perpendicular to *a* and *a'*. The intensity of the colour *b* is to be so chosen, that if *b'* be the colour which gives white with *b*, the intensity of the light resulting from this mixture may be equal to that of the light produced by the mixture of *a* and *a'*. This may be represented in the figure by making the line which expresses the colour *b* as long as those representing *a* and *a'*, whilst the complementary colour of *b* may be represented by the line *b'*, of equal length with *b*, but running in the opposite direction. We will suppose that, of the two colours *b* and *b'*, *b* is that which lies upon the side of positive transition from *a*. It is evident that if the colour *a* be given, *a'*, *b*, and *b'* may be found by observation. For instance, if *a* be yellow, *a'* is indigo; between *a* and *a'* on the side of positive transition lie the various tints of green and blue; greenish-yellow mixed with yellow (*a*) gives a very small, but with indigo (*a'*) a very considerable intermixture of white. Proceeding from greenish-yellow on the positive side, the intermixture of white will increase by mixture with yellow, and decrease by mixture with indigo. A tint will consequently lie on the course of the transition, which will furnish an equal quantity of white when mixed with yellow, as with indigo. This must be about green, so that *b* will be green and *b'* purple. Now it is evident, that, by the mixture of two of these four colours, all tints must be obtainable. These tints may be ascertained by observation for all degrees of intensity of the homogeneous colours *a* and *b*, *b* and *a'*, *a'* and *b'*, *b'* and *a*. Suppose the intensities of the two colours to be mixed to be signified by the length of the lines representing them, so that if a colour has the tone *a*, and its intensity is in the same proportion to that of *a* as *m* to 1, then that colour may be represented by a line having the same direction as *a*, but *m* times its length. Having represented in this manner the two colours geometrically, let us construct from these lines the ***geometrical sum***, that is, the diagonal of the parallelogram which has the two lines for its sides*, and assume that this sum or diagonal shall

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* The idea of this geometrical sum was first developed by Möbius in his *Mechanik des Himmels* (1843), and by myself in my *Ausdehnungslehre* (1844).

represent the colour of the mixture, its direction showing the tint, and its length the intensity of the colour.

This done, the tint and intensity of any mixture of colours may be found by mere construction. Thus it is only necessary to determine the lines which represent the tint and intensity of the mixed colours, and then to add these geometrically, that is, to compound them as forces, and the geometrical sum (the resultants of these forces) represents the tint and intensity of the mixture. This follows immediately, because the order in which the geometrical addition (or compounding of the forces) is effected is without influence upon the result. Thus the colours represented in conformity with the above determination by the lines a, b, a', b' may be taken as a basis; then let αa, when α is positive, represent a colour which has the tint a, and the intensity of which is in the same relation to that of a as α to 1; or if α be negative, a colour which possesses the tint of the complementary colour a', and the intensity of which is in the same relation to that of a' as α to 1. The same applies to the second colour b, and its complementary colour b'. Of the two colours e and e', the component colours of which are to be ascertained, let the first be represented by the mixture of the colours αa and βb, and the other by the mixture of the colours $\alpha' a$ and $\beta' b$, then (leaving the intermixture of white out of the question) the mixture of c and c' may be represented by the combination of the four colours αa, βb, $\alpha' a$, $\beta' b$. But αa mixed with $\alpha' a$ gives $(\alpha+\alpha')a$, and βb mixed with $\beta' b$ gives $(\beta+\beta')b$. Consequently the mixture of c and c' may also be represented by the mixture of the two colours $(\alpha+\alpha')a$ and $(\beta+\beta')b$. As, however, these latter have the original tints a, b or a', b', their mixture is represented by the geometrical sum of the lines, and consequently by the lines $(\alpha+\alpha')a+(\beta+\beta')b$, *i. e.* by $(\alpha a+\beta b)+(\alpha' a+\beta' b)$, *i. e.* by the geometrical sum of two lines, which, taken separately, represent the colours to be mixed.

This law, which follows of necessity from the three original assumptions, and which only requires a simple but complete series of observations for the determination of the series of colours, may also be expressed in another manner. Thus, if a circle be drawn round the origin of the lines, having a for its radius, and substituting for each line the point at which it strikes the periphery provided with a weight proportional to the length of the line, the mixed colour produced by two given colours may be ascertained in the following manner. Each of the colours is represented by a loaded point on the periphery, in such a manner that the radius belonging to it shows its tint, whilst the weight expresses the intensity, and determines the centre of gravity. A line drawn from the centre to the centre of gravity then represents the tint; and when multiplied by the sum of the weights, the intensity also. The identity of this determination with the preceding one is readily seen from the following construction of the centre of gravity pointed out in my *Ausdehnungslehre.* The centre of gravity of the points A, B, C . . . , to which are respectively attached the weights $\alpha, \beta, \gamma \ldots$, is ascertained by drawing from any point O the lines OA, OB, OC; these are multiplied respectively by $\alpha, \beta, \gamma \ldots$, and changed in the proportion of $1:\alpha$, $1:\beta$, $1:\gamma \ldots$ without altering their directions; from the lines thus obtained the geometrical sum is formed, and this divided by $\alpha+\beta+\gamma \ldots$; the terminal point of the line thus obtained is the desired centre of gravity.

Lastly, with regard to the intermixture of colourless light, another assumption is still necessary. The simplest method is to assume—

"That the total intensity of the mixture is the sum of the intensities of the lights mixed."

I understand under the term total intensity of light, the sum of the intensity of the colour, expressed as above, and the intensity of the intermixed white. The intensity of the white, as also of every single colour, is assumed proportional, not to the square of the amplitude, but to the amplitude itself; so that by the mixture of two white, or similarly coloured lights, the intensity of the mixture is the sum of the intensity of the mixed lights.

This fourth assumption is not to be regarded as so well founded as the three preceding, although on theoretical grounds it appears to be the most probable. In order to draw the conclusions from this hypothesis, we will suppose the intensity of the colour represented by the line a to be equal to 1, and assume that the various homogeneous colours, the intensity of which is 1, are represented by points on the periphery of a circle, so that, in conformity with what has been stated above, the weight of these points must also be supposed equal to 1. Now let A and B (fig. 6) be two points on the periphery, which consequently represent homogeneous colours of an intensity $=1$. Let the colours αA and βB be mixed, *i. e.* two homogeneous colours, whose intensities are α and β, and their tints A and B, then the sum of the intensities is $\alpha+\beta$. In order now to determine the colour of the mixture, we have, as above described, to find the centre of gravity of the points A and B, furnished with the weights α and β. Let this be C, the centre of the circle O; then if the radius of the circle be supposed $=1$, the intensity of the colour is $=(\alpha+\beta)$OC. Let the point at which OC, if produced, strikes the periphery be D, then the total intensity is $\alpha+\beta$, or, as the radius is supposed to be 1, $(\alpha+\beta)$OD. The total intensity, according to the supposition, will be equal to the intensity of the colour *plus* the intensity of the intermixed white; the latter is consequently $(\alpha+\beta)\mathrm{OD}-(\alpha+\beta)\mathrm{OC}=(\alpha+\beta)\mathrm{CD}$. The intensity of the intermixed white is therefore equal to the distance of the centre of gravity from the periphery, multiplied by the sum of the weights. From this it follows, further, that if the total mass be regarded as united in the centre of gravity, in which case, when furnished with such a weight, it is called the *geometrical sum** of the individual points with their attached weights, then every impression of light, according to its three elements, is accurately represented by a point with a certain weight attached to it. The direction in which this point lies with regard to the centre, or the point at which this direction, if followed, strikes the periphery, represents the tint; the weight of the point, the total intensity of the light; and the distance from the periphery multiplied by this weight, the intensity of the intermixed white. If by the depth of the colour of a light we understand the intensity of its colour, divided by the total intensity of the light, the depth of colour is represented by the simple recession of the point from the centre. If, then, two or more colours be represented in this manner, their mixture is completely represented by the geometrical sum of the loaded points denoting the individual colours. It will be seen that this rule, derived by a purely mathematical method from four sufficiently well-founded assumptions, agrees in its essential features with Newton's empirical rule. Nevertheless, the manner in which Newton distributes the homogeneous colours on the circumference of his discs requires a thorough revision, towards which only the first steps are made by the researches of Helmholtz. Not until sufficient light has been thrown upon this subject can we attempt the solution of the interesting question with regard to the law by which the vibrations belonging to the various colours combine to produce simple impressions of colour upon the nerves of the sensorium, a question upon the solution of which the idea of the various colours and of colourless light essentially depends.

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* See my *Ausdehnungslehre* and Möbius' *Barycentrischen Calcul.*

XXXVIII. *On Electro-dynamic Induction in Liquids.*

By Professor FARADAY, *F.R.S. &c.*[*]

To Prof. Aug. de la Rive, For. Mem. R.S. &c.

Royal Institution,
March 7, 1854.

MY DEAR FRIEND,

YOUR question, "whether I have ever succeeded in producing induction currents in other liquids than mercury or melted metals, as, for instance, in acid or saline solutions?" has led me to make a few experiments on the subject; for though I believed in the possibility of such currents, I had never obtained affirmative results: I have now procured them, and send you a description of the method pursued. A powerful electro-magnet of the horse-shoe form was associated with a Grove's battery of twenty pairs of plates. The poles of the magnet were upwards, their flat end faces being in the same horizontal plane; they are 3·5 inches square and above 6 inches apart. A cylindrical bar of soft iron, 8 inches long and 1·7 in diameter, was employed as a keeper or submagnet: the cylindrical form was adopted, first, because it best allowed of the formation of a fluid helix around it; and next, because when placed on the poles of the magnet, and the battery connexions made and broken, the magnet and also the keeper rises and falls through much larger variations of power, and far more rapidly than when a square or flat-faced keeper is employed; for the latter, if massive, has, as you know, the power of sustaining the magnetic conditions of the magnet in a very great degree when the battery connexion is broken. A fluid helix was formed round this keeper, having 12 convolutions and a total length of 7 feet; the fluid was only 0·25 of an inch in diameter, the object being to obtain a certain amount of intensity in the current by making the inductive excitement extend to all parts of that great length, rather than to produce a quantity current by largeness of diameter, *i. e.* by a shorter mass of fluid. This helix was easily constructed by the use of 8·5 feet of vulcanized caoutchouc tube, having an internal diameter of 0·25, and an external diameter of 0·5 of an inch: such a tube is sufficiently strong not to collapse when placed round the iron cylinder. The twelve convolutions occupied the interval of 6 inches, and two lengths of 9 inches each constituted the ends. This helix was easily and perfectly filled by holding it with its axes perpendicular, dipping the lower end into the fluid to be used, and withdrawing the air at the upper; then two long, clean, copper wires, 0·25 of an inch in diameter, were introduced at the ends, and being thrust forward until they reached the helix, were made secure by ligaments, and thus

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[*] From the *Bibliothèque Universelle de Genève.*

formed conductors between the fluid helix and the galvanometer: the whole was attached to a wooden frame so as to protect the helix from pressure or derangement when moved to and fro. The quantity of fluid contained in the helix was about 3 cubic inches in the length of 7 feet. The galvanometer was of copper wire, $\frac{1}{30}$th of an inch in diameter and 164 feet in length, occupying 310 convolutions; it was 18 feet from the magnet, and connected with the helix by thick wires dipping into cups of mercury. It was in the same horizontal plane with the magnetic poles, and very little affected by direct action from the latter.

A solution formed by mixing one volume of strong sulphuric acid and three of water was introduced into the helix tube, the iron keeper placed on the helix, and the whole adjusted on the magnetic poles in such a position, that the ends of the copper connectors in the tube were above the iron cylinder or keeper, and were advanced so far over it as to reach the perpendicular plane passing through its axis: in this position the lines of magnetic force had no tendency to excite an induced current through the metallic parts of the communication. The outer ends of the copper terminals were well connected together and the whole left for a time, so that any voltaic tendency due to the contact of the acid and copper might be diminished or exhausted: after that, the copper ends were separated, and the connexions with the galvanometer so adjusted, that they could be in an instant either interrupted, or completed, or crossed at the mercury cups. Being interrupted, the magnet was excited by the full force of the battery, and then the *direct* magnetic effect on the galvanometer was observed: the helix had been so arranged that any current induced in it should give a deflection in the contrary direction to that thus caused directly by the magnet, that the two effects might be the better separated. The battery was then disconnected, and when the reverse action was over, the galvanometer connexions were completed with the helix; this caused a deflection of only 2°, due to a voltaic current generated by the action of the acid in the helix on the copper ends: it showed that the connexion throughout was good; and being constant in power, caused a steady deflection, and was thus easily distinguished from the final result. Lastly, the battery was thrown into action upon the magnet, and immediately the galvanometer was deflected in one direction, and upon breaking battery contact it was deflected in the other direction, so that by a few alternations considerable swing could be imparted to the needles. They moved also in that particular manner often observed with induced currents, as if urged by an impact or push at the moment when the magnet was excited or lowered in force; and the motion was in the *reverse* direction to that produced by the mere direct action of the magnet. The effects were constant; when the communicating wires were crossed they again occurred, giving reverse actions at the galvanometer. Further proof that they were due to currents induced in the fluid helix was obtained by arranging one turn of a copper wire round the iron core or keeper in the same direction as that of the fluid helix, and using one pair of plates to excite the magnet; the induced current caused in the copper wire was much stronger than that obtained with the fluid, but it was always in the same direction.

After these experiments with the highly conducting solution, the helix was removed, the dilute acid poured out, a stream of water sent through the helix for some time, distilled water then introduced and allowed to remain in it a while, which being replaced by fresh distilled water, all things were restored to their places as before, and thus a helix of pure water submitted to experiment. The direct action of the magnet was the same as in the first instance, but there was no appearance of a voltaic current when the galvanometer communications were completed; nor were there any signs of an induced current upon throwing the magnet into and out of action. Pure water is too bad a conductor to give any sensible effects with a galvanometer and magnet of this sensibility and power.

I then dismissed the helix, but, placing the keeper on the magnetic poles, arranged a glass dish under it and filled the dish with the same acid solution as before; so that the liquid formed a horizontal fluid disc 6 inches in diameter nearly, an inch deep, and within 0·25 of an inch of the keeper; two long, clean, platina plates dipped into this acid on each side of the keeper and parallel to it, and were at least 5 inches apart from each other; these were first connected together for a time that any voltaic tendency might subside, and then arranged so as to be united with the galvanometer when requisite, as before. Here the induced currents were obtained as in the first instance, but not with the same degree of strength. Their direction was compared with that of the current induced in a single copper wire passed between the fluid and the keeper, the magnet being then excited by one cell, and was found to be the same. However, here the possibility exists of the current being in part or altogether excited upon the portions of the wire conductors connected with the platinum plates; for as their ends tend to go beneath the keeper, and so *into* the circuit of magnetic power formed by it and the magnet, they are subject to the lines of force in such a position as to have the induced current formed in them; and the induced current can obtain power enough to go through liquid, as I showed in 1831. But as the helix experiment is free from this objection, I do not doubt that a weak induced current occurred in the fluid in the dish also.

So I consider the excitement of induction currents in liquids not metallic as proved; and as far as I can judge, they are proportionate in strength to the conducting power of the body in which they are generated. In the dilute sulphuric acid they were of course stronger than they appeared by the deflection to be, because they had first to overcome the contrary deflection which the direct action of the magnet was able to produce; the sum of the two deflections, in fact, expressed the force of the induced current. Whether the conduction by virtue of which they occur is electrolytic in character or conduction proper I cannot say. The present phænomena do not aid to settle that question, because the induced current may exist by either the one or the other process. I believe that conduction proper exists, and that a very weak induction current may pass altogether by it, exerting for the time only a tendency to electrolysis, whilst a stronger current may pass, partly by it and partly by full electrolytic action.

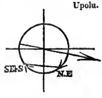
I am, my dear friend,
Ever most truly yours,
M. Faraday.

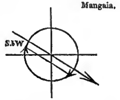
XXXIX. *On the Storm-tracks of the South Pacific Ocean. By* Thomas Dobson, *B.A., Head Master of the High School of Hobart Town, Van Diemen's Land.*

[With a Plate.]

THE law of the westerly progressive motion of tropical cyclones has now been well established in all the regions sufficiently frequented by large ships to afford several trustworthy observations of the same storm; as in the West Indian Seas, the South Indian Ocean, the Bay of Bengal, and the China Sea. For the South Pacific Ocean not a single storm-track is given in any work on cyclonology. The results of the researches that I have undertaken with a view to supply this deficiency have induced me to doubt the *generality* of the law of westerly progression, and to suspect the existence of an important anomaly in the region referred to; for while they do not present one exception to the law of rotation in the order N.E.S.W., they all tend to show that in the South Pacific Ocean cyclones travel at first *to the eastward*, and then towards the south-east and south, moving off finally towards the south-west. The mean storm-track of the South Pacific Ocean is therefore exactly the *reverse* of that of the South Indian Ocean, instead of being similar to it in position, as is always supposed by writers on cyclonology. The following is an abstract of the facts from which this conclusion has been deduced.

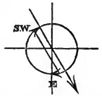
The Rev. W. Day of Hobart Town, in a journal kept by him at Upolu, one of the Navigators' Islands, describes a hurricane which occurred there on the 15th of December 1842. The wind set in from N.E., and got round to S.E. by S. This order of veering implies progression to the south of east. At 8 P.M. on the 16th of December, the hurricane reached H.M.S. Favourite in 21° S. 204° E., and lasted during the 17th and 18th. When off Mangaia on the 17th, the Favourite lost sails, boats, and one man. The wind shifted from N.E. to E.N.E. and to S. by W., so that the cyclone was now moving *towards the south-east.* In the Nautical Magazine, p. 425, volume for 1845, is given an extract from the log of the Favourite; and Captain Stokes has added many particulars of this hurricane in vol. ii. p. 441, of the Voyage of the Beagle; where, however, he erroneously supposes the progressive motion to be towards the south-west. (Track A in the chart, Plate III. fig. 3.)





The Sydney *Morning Herald* states that there was a hurricane at Upolu on the 16th of March, 1846. H.M.S. Juno, &c. were in the harbour. It blew hardest from N.W. A great many whalers came in to repair after the storm. The northern quadrant passed over Upolu.

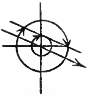
The Rev. A. Buzzacott has given me the particulars of what is called "the great hurricane" at Raratonga. On the 16th of March there was a furious gale from the *eastward.* At 1 A.M. of the 17th there was a *dead calm* for a few minutes, and then the wind shifted suddenly to the *south-west.* This implies the passage of the *central space* towards the *south-eastward.* At Mangaia, forty miles S.E. of Raratonga, the change of wind occurred at 10 A.M. of the 17th. At Raratonga the actual violence lasted from 9 P.M. of the 16th to 3 A.M. of the 17th. On board a vessel between Raratonga and Mangaia the barometer was lowest at 5 A.M. of the 17th. The barometrical fluctuations at Raratonga were—



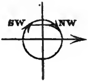
March 16, 10 A.M.	29·50	March 17, 1 A.M.	27·70
7 P.M.	29·00	2 A.M.	28·00
10 P.M.	28·50	4 A.M.	29·00
12 P.M.	28·00	6 A.M.	29·50

The track of this cyclone is marked B on the chart.

In the log of the missionary brig 'John Williams,' I find a notice of a hurricane which veered from N. to W. and then to S.W. in Jan. 1845, the vessel being thirty miles E.N.E. of Raratonga. The motion is still to the south-eastward. (Track C.)

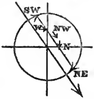


In Wilkes's Narrative of the American Exploring Expedition, vol. v. p. 19, is an account of a violent hurricane at Upolu on the 16th of December, 1840, in which the wind shifted from N.E. to S.W., and the motion was consequently to the S.E. The storm reached the Island of Tutuilah, S.E. of Upolu, on the next day (17th), the wind changing from N.W. to S.W. On the 1st of December a typhoon had raged near the Laughlin Islands in 9° S. and 154° E., but I possess no account of the changes of wind. (Track D on chart.)

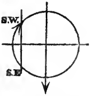


The Raratonga hurricane, described by Williams in his 'Missionary Enterprize,' which also devastated the Navigators' Islands, was undoubtedly one of the class of which the track has just been determined. At Raratonga the wind veered from eastward towards the west, and was therefore moving *towards the south.*

Lieut. Wilkes describes a hurricane at the Feejee Islands which lasted from the 22nd to the 25th of February 1840. It came on from N.E. and veered to N. and then to N.W., gradually hauling to the southward. The motion was therefore to *the south-eastward.*



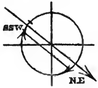
At midnight, on the 28th, the missionary brig 'Camden,' Captain Morgan, was in 31°S. 174° 7′ E., sailing eastward. During the 29th the hurricane passed over her, veering from S.E. to S.W. The centre therefore lay to the eastward of the ship, and was moving towards the south.



Lieut. Wilkes, at p. 381, vol. ii. of his 'Narrative,' has shown that the calm central area of a hurricane passed across the Bay of Islands, New Zealand, on the 1st of March, and that the cyclone was moving in a *south-west direction.* This is likewise confirmed by Mr. Piddington, Horn Book, p. 61. All the accounts coincide in showing that this was a hurricane of very unusual violence. (Track E.)

On the 10th of February, 1849, the barque Nimrod, from Sydney for China, was in 17° 38′ S. and 161° 26′ E., sailing N.W.

On the 13th, the centre of a hurricane, beginning at N.E. and ending at S.S.W., passed over the vessel, during which she lost her masts, &c. The progressive motion is towards the south-east.

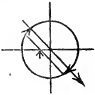


On the 15th of February, in 23° 38′ S. and 167° 40′ E., about fifty-eight miles S.S.E. of the Isle of Pines, the hurricane struck the brig Scamander, veering from E. to S. At 10 P.M. the brig was wrecked on the south reef of New Caledonia, and all hands except two drowned. (Track F.)

From the 18th to the 23rd of February, 1852, a cyclone was raging at New Caledonia and Amatam, the schooner Deborah was wrecked.—(*Sydney Morning Herald.*) On the 5th and 6th of March, the barque Lady Franklin was seventy miles S.E. of Norfolk Island. The *calm* passed her at 2 A.M. of the 6th. The wind veered from E.S.E. to W.N.W., indicating progression to the W. of S.

At Auckland, New Zealand, on the 6th of March, there was a furious storm, for the most part from E.N.E. The brig Maukin was wrecked, and H.M. surveying schooner parted both her anchors and drove on shore. (Track G.)

In Hunter's 'New South Wales' is described, by Governor King, a furious hurricane which devastated Norfolk Island in February, 1789. The wind began at N.E., veering round to E.S.E., and then to S. and moderating. Wind S.W. during the remainder of the month.



The motion is south-eastward, the central area passing to the eastward of the Island. (Track H.)

On comparing the curvature of the tracks thus determined with that of the celebrated Rodriguez hurricane (Track K), the nature of the anomaly will be apparent.

It is remarkable that the track marked E, which may be regarded as a mean of all the tracts determined, coincides with the Australian volcanic series, which, commencing about New

Guinea, passes between New Caledonia and the Navigators' Islands, and extends to the volcanoes of New Zealand.

It is obvious that the curvature of the storm-tracks of the South Pacific Ocean is of the same kind as that of the east coast of Australia; and the storm-tracks of the South Indian Ocean have likewise the same kind of curvature as the west coast of Australia. This relation holds equally in the case of the West Indian cyclones, and the coasts which they traverse. These observations may probably contain the germ of a law of progressive motion of cyclones *more general* than that of their initial westerly progression.

Hobart Town, April 25, 1853.

XL. *Improved construction of an Apparatus for the illustration of various phænomena of Rotating Bodies. By* Professor MAGNUS*.

[With a Plate.]

IN a paper of mine on the deviation of projectiles, I have drawn attention to a remarkable phænomenon which is exhibited during a body's rotation, and described an apparatus intended for the purpose of illustrating the motion of cylindro-conical projectiles, and of showing that the axis of a rotating body is fixed only under the condition that it is perfectly free, being, on the contrary, quite mobile when it is prevented from moving in one direction.

I have had this apparatus constructed in a modified form, which renders it more convenient for experiment, and at the same time alterations are introduced which render it possible to observe with this instrument the various remarkable phænomena exhibited by rotating bodies more completely than has hitherto been attainable.

The new construction is represented in figs. 1 and 2, Plate IV. AB and CD are two brass discs with thick edges, 3·8 inches in diameter, which having their axes *ab* and *cd* placed between points in the stirrups *abfg* and *cdhk*, are easily moveable. The stirrups are attached to the bar *mn*, which is held fast in the collar *os* by the screw *e*, and with this collar is free to move round the horizontal axis *qr* between two points, *q* and *r*, attached to the forked piece *pqr*. The forked piece is carried by the axis *vw*, which is pointed underneath. The entire apparatus, therefore, rests upon the point at *w*, and hence is free to move round a vertical, as also round a horizontal axis.

In order to check the one or the other of these motions, the piece of brass *pu* is so attached at *p* to the forked piece *pqr*, that

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* From Poggendorff's *Annalen* for February, 1854.

by means of the screw *z* it can be readily elevated, and pressed against the semicircular piece *xy* attached to the collar *os*. In this way it is possible to fix *mn* at any inclination to the horizon. When the screw *z* is loosened, the piece *pu* sinks by its own weight, and the motion is once more free.

In order to control the motion round the vertical axis, and besides this to accelerate it or to retard it, a wire *tu* is screwed into the piece *pu* at *u*.

From *m* and *n* depend the wires *ml* and *ni*, on which different weights, P, furnished with hooks, may be suspended.

A wooden handle EF, to which two strings of equal lengths are fastened at E and F, serves to set the discs AB and CD in rotation. By winding these strings round the axes of the discs either in the same or in opposite ways, it is possible to cause the discs to rotate either in the same or in opposite directions, and to impart to them velocities which are very nearly equal.

When both are caused to rotate in the same direction, so that they move as a single mass, and when the axis *mn* is perfectly free to move, the axis retains its direction either without weights, or when equal weights are placed at *l* and *i*. When on the one side a greater weight is placed than on the other, the apparatus moves round a vertical axis; and if the overweight be placed at *m*, the direction of rotation is opposed to what follows when the greater weight is placed at *n*. It is also evident that the direction of the motion is different according as the discs rotate in the one or the other direction. When, in respect to an observer situated in the production of *mn*, the motion is that of the hand of a clock, the weight being attached to that side of the apparatus which is nearest to the observer, then this side moves from the right of the observer to his left.

During this motion round a vertical axis, the inclination towards the horizon is scarcely sensibly changed, even when the one side sustains 2 lbs. or 1000 grms. more than the other. Accelerating the rotation round the vertical axis by carrying the rod *tu* round with the hand, the side on which the greater weight rests is elevated; whereas when the angular velocity of the apparatus is retarded, the same side sinks. Checking this completely, by holding fast the rod to *u*, the overweighted side falls as low as possible.

Permitting the apparatus weighted on one side, for example at *m*, to rotate freely round the vertical axis, and clamping it during its motion with the screw *z* in the exact inclination which it has assumed towards the horizon, the rotation round the vertical axis ceases, but begins again immediately, provided the discs AB and CD persist in their rotation, when the screw *z* is loosened. Care must, however, be taken in loosening the screw that the incipient rotation round the vertical axis be not checked, otherwise the weighted side of the apparatus falls immediately.

When this side falls as low as it can, that is, until one of the protuberances *x* or *y* strikes against the piece *pu*, the rotation round the vertical axis ceases, because the rotation round the horizontal axis is checked. If, however, the weighted side be raised with the hand, the rotation commences again round the vertical axis. In this case a peculiar motion generally sets in. While the apparatus rotates round the vertical axis, the weighted side moves up and down like a pendulum.

If instead of lifting this side the rod *tu* be carried round with the hand, and the entire apparatus thus turned round its vertical axis with a somewhat greater velocity than it would have turned of itself if the motion round the horizontal axis had not been checked, then the weighted side rises of itself, and the rotation round the vertical axis continues of itself, provided that the discs AB and CD rotate with sufficient velocity.

When the two discs AB and CD are impressed with equal velocities but in opposite directions, then the apparatus, although its motions are perfectly unhindered, is easily moveable, and the smallest surplus weight at *m* or *n* causes the overweighted side to sink immediately. If one disc only rotated, the entire apparatus would turn round the vertical axis *vw* in an opposite direction to that of the motion when both the discs rotate. Hence it is solicited in two opposite directions round this axis, and as the rotating forces are equal, they neutralize each other. In this way the two rotations round the vertical axis are checked. Hence the same consequence follows as if only a single disc rotated, or as if both moved in the same direction, the rod *tu* being held fast by the hand.

The whole of these experiments may be made without the use of weights. When the screw *e* is loosened, the rod *mn* may be moved back and forward in the collar *os*, so that the apparatus may assume the position given to it in fig 1. The weight of the disc CD acts then at the end of a longer arm than that of the other, AB, and in this way the same effect is obtained as if weights had been placed at *n*. The greater the difference of the two arms of the lever, the more quickly does the apparatus rotate.

By moving *mn* in the collar *os*, it is possible to make another experiment. If the distances of the rotating discs from the vertical axis *ow* be unequal, and the two discs be set in rotation with as nearly the same velocity as possible in opposite directions, then the apparatus is perfectly mobile. In like manner, when the shorter arm is so weighted as to establish an equilibrium, then the slightest weight, which while the discs are at rest is sufficient to destroy the equilibrium, destroys it also when the discs are in motion. From this it follows, which might also be inferred from other grounds, that the distance of the rotating mass from the vertical axis has no effect, or at least a remarkably small one, on the rotation of the apparatus.

As it appears convenient to distinguish this apparatus by a name, and inasmuch as it may be used for a great number of experiments on rotation, I would propose for it the name of *polytropium* or *polytrope.*

XLI. *On the Spheroidal State of Bodies.*

By Arthur H. Church, *Esq.*

To Dr. Tyndall, F.R.S. &c.

Royal College of Chemistry,
March 18, 1854.

Dear Sir,

THE successful method by which, in your last lecture, the existence of a space between water in the spheroidal state and the containing vessel was proved by the complete interruption that space offered to the passage of a galvanic current, has induced me to devise a few experiments on the subject.

I have to describe in the present communication, in the first place, some experiments I have just performed for the purpose of obtaining decisive evidence of the isolation of all bodies in the spheroidal state from the surfaces on which they roll; and in the second place, to offer a few suggestions as to the probable causes of the phænomena under consideration.

It was found by Boutigny, that if into a clean, red-hot platinum capsule acids and alkalies be placed, the acid and alkaline liquids will roll about, repelling one another violently. This, though an interesting example of the suspension of chemical affinity, does not prove the existence of a space between the platinum vessel and the spheroids.

The first experiment I have to mention was this:—I took a copper basin, three inches in diameter and rather more than half an inch deep, polished its concave surface, and covered it with a thin film of silver by the galvanic process. The plated basin was now brought to a very high temperature, and while thus heated, a few drops of a slightly alkaline solution of sulphide of sodium were poured into it. These drops instantly assumed the spheroidal form and rolled about, making, however, no mark or track upon the silver. The source of heat was now withdrawn: the temperature was soon so far reduced that the liquid exhibited its normal properties, the space between it and the silver no longer existed, and a black stain of sulphide of silver covered the dish.

Another instance of the assumption of the spheroidal state has been often noticed. It occurs when æther is placed on the surface of boiling water. Now, if a fixed inorganic acid be dissolved in the æther, and the water be coloured with litmus, no reddening of the latter will take place as long as the æther remains in the spheroidal state. The acidulated æther and the tinted water cannot, therefore, be in communication; they are separated by a film of air or of vapour.

I pass on to notice in as few words as possible the remainder of my experiments. I have remarked that in certain circumstances spheroidal globules form upon the surface of liquids during the processes of filtration and distillation. The phænomenon to which I refer is exhibited by many liquids, more frequently and conspicuously perhaps by those that are the more volatile. I have observed it with alcohol, water, aqueous and alcoholic solutions, syrup, with essential oils and many other organic substances. I have sometimes, however, found considerable difficulty in its reproduction, and will therefore describe in detail a method which is applicable in most cases for obtaining in this manner an example of the spheroidal state. We will employ a particular instance. If we take cymole, a hydrocarbon belonging to the benzole series, and half fill a bottle two or more inches in diameter with it, placing in the neck of the bottle a perforated cork through which passes a funnel-tube filled with cymole, and having a piece of sheet Indian-rubber stretched over its mouth, we shall find that on adjusting the funnel-tube till its lower extremity is rather less than half an inch from the surface of the liquid in the bottle, and on letting fall a drop of cymole from it, beautiful spheroidal globules will be formed and roll about for some time, scarcely diminishing in size. This experiment may be performed with great advantage if the cymole be warmed first. An ordinary funnel with a filter may be substituted for the funnel-tube, and will answer well if the lower aperture of the funnel has a diameter of about ·13 of an inch. The experiment may be repeated with other materials with similar effects. That the spheroids are not in contact with the surfaces on which they roll, may be proved by saturating the liquid in the tube with something that shall have a visible effect upon a substance dissolved in the liquid in the bottle. A beautiful illustration of this occurs when, under conditions similar to those before mentioned, we employ in the funnel-tube a solution of sugar containing sulphocyanide of potassium, and in the bottle a solution of sugar containing sesquichloride of iron; no red coloration takes place until the coalescence of the spheroids with the liquid beneath them. Many other chemical reactions may be made use of with similar results. It is very curious to see a solution of ferrocyanide of potassium floating upon a solution of sesquichloride of iron, while not a trace of Prussian blue is formed. These experiments must of course be recommenced whenever any union of the liquids employed has taken place. The cork spoken of above should have two perforations, one to admit the funnel-tube, and the other to allow the escape of air.

Is the employment of a volatile substance essential to the production of these phænomena? I imagined that this question might be answered by the following experiment. A dish of platinum might be heated strongly, and a drop of melted lead then placed upon it; now if the production of vapour from the substances employed were essential to the formation of the spheroidal state, the lead should at once dissolve and perforate the platinum; if, however, the spheroidal state occurs when two non-volatile substances are employed, the platinum vessel should not be perforated until its temperature has been considerably reduced. The experiment was tried with every precaution to prevent the oxidation of the lead and ensure an accurate result; a perforation of the platinum ensued the moment of placing the melted lead upon it. This proves that the production of vapour is essential to the occurrence of the spheroidal state; for it cannot be urged that that condition is never manifested when metals only are employed; for a drop of a volatile metal, mercury (melted mercury, we may say, in order to render its relationship to melted lead the more apparent), placed on an intensely heated surface of platinum instantly assumes the spheroidal form, and evaporating slowly, dances about in the vessel with peculiar movements. Upon thin, sonorous vessels of copper, &c., this movement takes place with such rapidity as to produce a musical tone of high pitch. I have sometimes seen globules of mercury and water rise to the height of six inches from the capsules in which they had been formed.

A word or two in conclusion, as an attempt at an explanation of the phænomena observed may not be out out of place.

Since a space always exists between the lower body and that in the spheroidal state, and since that state is not manifested by non-volatile substances, it seems reasonable to conclude that the vapour proceeding from those parts of the liquids nearest to the containing vessel or subjacent fluid tends to assist the internal molecular cohesive force of the drops in assuming and maintaining their spheroidal form. Other forces originated by the temperature may also be in operation.

I should premise that the solution of sulphide of sodium employed in my first experiment made a dark stain upon silver even in the cold. This renders the nullity of its action at a high temperature the more remarkable. It will be scarcely necessary to mention, that, for the successful performance of most of these experiments, considerable manipulative care is required.

If, in the further prosecution of my inquiries in this direction, I should discover anything worthy of notice, I hope to have the pleasure of communicating it to you.

I remain, dear Sir,
Truly yours,
Arthur H. Church.

XLII. *Proofs of two new Theorems relating to the Moon's Orbit.*

By Professor Challis*.

THE theorems which I propose to demonstrate are contained in a paper recently communicated to the Cambridge Philosophical Society. As I have reason to think that they are new, and of considerable importance in the lunar theory, I am desirous of giving them immediate publicity in the pages of the Philosophical Magazine. They may be enunciated as follows.

Theorem I. All small quantities of the second order being taken into account, the relation between the radius-vector and the time in the moon's orbit is the same as that in an orbit described by a body acted upon by a force tending to a fixed centre.

Theorem II. The eccentricity of the moon's orbit is a function of the ratio of her periodic time to the earth's periodic time, and the first approximation to its value is that ratio divided by the square root of 2.

Let the moon's place be referred to the plane of the ecliptic by rectangular coordinates, the origin being the earth's centre at rest, and the axis of x passing through the first point of Aries: let $\mu =$ the sum of the attractions of the sun and moon at the unit of distance, $m' =$ the sun's attraction at the unit of distance, and let r, r' be the radius-vectors of the moon and sun, and θ, θ' their true longitudes, at the time t reckoned from a given epoch. The following equations, including small quantities of the third order, may be readily obtained:—

$$\frac{d^2x}{dt^2} = -\left(\frac{\mu x}{r^3} - \frac{m'x}{2r'^3}\right) + \frac{3m'\rho}{2r'^3}\cos(\theta - 2\theta')$$

$$\frac{d^2y}{dt^2} = -\left(\frac{\mu y}{r^3} - \frac{m'y}{2r'^3}\right) - \frac{3m'\rho}{2r'^3}\sin(\theta - 2\theta')$$

$$\frac{d^2z}{dt^2} = -\left(\frac{\mu z}{r^3} - \frac{m'z}{2r'^3}\right) - \frac{3m'z}{2r'^3},$$

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* Communicated by the Author.

ρ being the projection of the moon's radius-vector on the plane of the ecliptic. Multiplying the equations by $2dx$, $2dy$ and $2dz$ respectively, adding, and putting V for the moon's velocity, we have

$$d\,.\,\mathrm{V}^2 = -\frac{2\mu dr}{r^2} + \frac{m'rdr}{r'^3} + \frac{3m'}{2r'^3}d\,.\,\rho^2\cos 2\overline{\theta-\theta'}$$
$$-\frac{3m'}{r'^3}(\rho^2 d\theta' \sin 2\overline{\theta-\theta'} + zdz).$$

It is proposed to integrate this equation so as to include all small quantities of the second order. That the approximation may apply to the case of the moon's motion, it will be assumed, in accordance with what is known by observation, that the true longitudes θ and θ' always differ from mean longitudes $nt+\alpha$ and $n't+\alpha'$ by small quantities. Hence $\frac{d\theta'}{d\theta}$ is a small quantity of the first order. Omitting, therefore, the terms in the above equation which involve $d\theta'$ and zdz, and putting for r' the sun's mean distance a', all small quantities of the second order are still retained, and we get by integration,

$$\mathrm{V}^2 + \mathrm{C} = \frac{2\mu}{r} + \frac{m'r^2}{2a'^3} + \frac{3m'\rho^2}{2a'^3}\cos 2\overline{\theta-\theta'}. \quad . \quad . \quad (\mathrm{A})$$

Again, small quantities of the second order being included,

$$x\frac{d^2y}{dt^2} - y\frac{d^2x}{dt^2} = -\frac{3m'\rho^2}{2a'^3}\sin 2\overline{\theta-\theta'}$$
$$x\frac{d^2z}{dt^2} - z\frac{d^2x}{dt^2} = 0 \qquad y\frac{d^2z}{dt^2} - z\frac{d^2y}{dt^2} = 0.$$

The first of these equations is equivalent to

$$d\,.\,\frac{\rho^2 d\theta}{dt} = -\frac{3m'\rho^2}{2a'^3}\sin 2\overline{\theta-\theta'}\,dt$$
$$= -\frac{3m'}{2a'^3}\sin 2\overline{\theta-\theta'}\,.\,\frac{\rho^2 d\theta}{dt}\cdot\frac{dt}{d\theta}dt$$

and

$$\sin 2\overline{\theta-\theta'}\,\frac{dt}{d\theta}\,dt = \frac{1}{n}\sin(2\overline{n-n'}t + 2\overline{\alpha-\alpha'})dt \text{ nearly.}$$

Hence by integrating, and neglecting all small quantities of a higher order than the second,

$$\frac{\rho^2 d\theta}{dt} = c_1 + \frac{3m'c_1}{4a'^3 n^2}\cos 2\overline{\theta-\theta'}.$$

Also we have

$$x\frac{dz}{dt} - z\frac{dx}{dt} = c_2 \qquad y\frac{dz}{dt} - z\frac{dy}{dt} = c_3.$$

Hence, if $d\phi$ be the small angle described by the radius-vector r, corresponding to the increment $d\theta$ of longitude in the time dt, and if $c_1{}^2+c_2{}^2+c_3{}^2=h^2$, and $\rho^2 n$ be put for c_1 in the small term, we have to the same approximation,

$$\frac{r^2 d\phi^2}{dt^2}=\frac{h^2}{r^2}+\frac{3m'\rho^2}{2a'^3}\cos 2\overline{\theta-\theta'}. \quad . \quad . \quad . \quad . \quad \text{(B)}$$

But

$$\mathrm{V}^2=\frac{dr^2}{dt^2}+\frac{r^2 d\phi^2}{dt^2}.$$

Hence, by the equation (A),

$$\frac{dr^2}{dt^2}+\frac{h^2}{r^2}-\frac{2\mu}{r}-\frac{m'r^2}{2a'^3}+\mathrm{C}=0. \quad . \quad . \quad . \quad . \quad \text{(C)}$$

This is the equation that would be obtained if a body were supposed to be acted upon by the central force $\frac{\mu}{r^2}-\frac{m'r}{2a'^3}$. Hence this equation proves Theorem I.

It may be remarked, that in the foregoing reasoning the inclination of the orbit to the plane of the ecliptic is taken into account, and that there is no limitation of the value of r excepting that its ratio to a' is a small quantity of the second order, the approximation in other respects proceeding solely on the hypothesis that the true longitudes of the sun and moon differ by small quantities from mean longitudes.

But it is evident from theoretical considerations, that on that hypothesis the moon's orbit cannot differ much from a circle, and it is known from common observation of her apparent diameter that this must be the case. Our approximation will consequently conform to the moon's motion, if the radius-vector be supposed to differ by a small quantity of the first order from a mean value a. If, therefore, a be put for r in the small term of the equation (C), it would seem that all small quantities of the second order are taken into account, and that we have to integrate the equation

$$\frac{dr^2}{dt^2}+\frac{h^2}{r^2}-\frac{2\mu}{r}-\frac{m'a}{2a'^3}+\mathrm{C}=0.$$

This integral would give the same relation between r and t as that in an orbit described by the central force $\frac{\mu}{r^2}$, the constant h being twice the area described by the radius-vector in the unit of time. And the equation (B) shows that h is also the *mean* value of twice the area described by the moon's radius-vector in the unit of time. Hence we must conclude that the mean distance and mean periodic time in this approximation to the moon's orbit are the same as those in an elliptic orbit described by the action of the central force $\frac{\mu}{r^2}$. Now as the first power of the disturbing force has been fully taken into account, it is impossible that this result can be true. Some step in the reasoning inconsistent with the hypothesis of the approximation must have been taken, which it is necessary to correct before we advance further.

After various fruitless attempts I discovered at length the following solution of this difficulty. Let it be supposed that the equation (C) contains the disturbing force as a *factor*. In that case, putting the equation under the form

$$dt = \frac{-rdr}{\sqrt{-\mathrm{C}r^2 + 2\mu r - h^2 + \frac{m'r^4}{2a'^3}}},$$

and substituting $a + \overline{r-a}$ for r, it is necessary to expand to the second power of $r-a$ in the small term, in order to embrace small quantities of the second order. This proceeding is legitimate, because it is only required that the value of r should be consistent with the hypothesis that the true longitude of the moon differs by a small quantity from a mean longitude. The approximation being conducted in this manner, the two equations (B) and (C) readily give by integration the following results, which include all small quantities of the second order:—

$$r = a + ae\cos\psi$$

$$\frac{\mathrm{C}^{\frac{3}{2}}}{\mu}\left(1 - \frac{5m'\mu^2}{2a'^3\mathrm{C}^3}\right)(t + \mathrm{T}) = \psi + e\sin\psi$$

$$\frac{a(1-e^2)}{r} = 1 + e\cos\left\{\left(1 - \frac{3m'\mu^2}{4a'^3\mathrm{C}^3}\right)(\theta + \gamma)\right\}$$

$$a = \frac{\mu}{\mathrm{C}} + \frac{m'\mu^2}{a'^3\mathrm{C}^4} \qquad e^2 = 1 - \frac{h^2\mathrm{C}}{\mu^2} + \frac{m'\mu^2}{2a'^3\mathrm{C}^3}$$

$$\left(\frac{m'\mu^2}{a'^3\mathrm{C}^3}\right)^{\frac{1}{2}} = \frac{\text{the moon's periodic time } (p)}{\text{the earth's periodic time } (\mathrm{P})}.$$

We have now to satisfy, by means of the arbitrary constants, the condition on which alone the above results can be obtained, viz. that the disturbing force is a factor of the equation (C). By substituting in that equation the values of r and $\frac{dr}{dt}$ given by the first and second of the above equations, it will appear that the condition is satisfied if the arbitrary constants h and C are such that $h^2C=\mu^2$. Hence it follows that

$$e=\frac{p}{\sqrt{2}P},$$

which proves Theorem II.

The third of the foregoing equations gives by a direct process the known value of the motion of the apse to the first approximation. The periodic time is found to be the same as that in an elliptic orbit whose mean distance is a, the force tending to the focus being $\mu\left(1-\frac{p^2}{2P^2}\right)$ at the unit of distance, which is otherwise known to be true.

The numerical value of $\frac{p}{\sqrt{2}P}$ for the moon is 0·0529, and the known eccentricity of her orbit is 0·0548. The difference 0·0019 is not more than might be expected from the degree of approximation embraced by the analysis. For Jupiter's four satellites the values of $\frac{p}{\sqrt{2}P}$ are 0·00029, 0·00058, 0·001168, and 0·002724. Respecting the eccentricities of their orbits, observation shows that those of the first and second are too small to be sensible, and that those of the third and fourth are only just sensible. The eccentricities of the orbits of the other satellites of the solar system are known with too little precision to admit of comparison with the theory, excepting that of Titan's orbit, which is stated to be 0·0293. But in this instance the large inclination of the orbit to the plane of Saturn's orbit forbids making a comparison.

The following considerations appear to me sufficient to prove that the eccentricity of the moon's orbit must be a function of the disturbing force. A straight line being drawn from the earth's centre in any direction in the plane of the moon's orbit, the radius-vector at the instants the moon passes this line in successive revolutions has different values. The fluctuations of value, which, as is known, do not exist in the elliptic theory, depend on the disturbing force in such a manner that the function by which they are expressed would vanish if the disturbing force were indefinitely small. The total fluctuations, in the case of a uniform apsidal motion, are the same in all directions, and take place about the same mean distance; and the difference between the extreme values of the radius-vector in any given direction is equal to the difference of the two apsidal distances, and therefore proportional to the eccentricity. Hence as the fluctuations of the radius-vector in a given direction depend on the disturbing force, it follows that the eccentricity is also a function of the disturbing force.

In opposition to these views, it will perhaps be urged that a satellite might be projected in such a manner that the eccentricity of the orbit it begins to describe would be different from that given by the above theorem. But without mathematical proof, which is altogether wanting, it cannot be asserted that the orbit would *retain* that eccentricity under the influence of a disturbing force, or that there would be a mean motion either of the radius-vector or of the apse. If such were the case, there would seem to be no reason for the failure of the method of integration which was applied in the first instance to the equation (C). In all probability, the motion in the case supposed would not settle down to a *permanent* state till the eccentricity of the orbit had reached the value which accords with the theorem.

If any hesitation to admit the truth of the theorem be felt, because none of the many eminent mathematicians who have treated of the lunar theory have arrived at the same conclusion, I may urge that by no previous method of investigation has the motion of the moon's apse been found in a *direct* manner. The indirect methods hitherto adopted to determine the motion of the apse precluded the possibility of arriving at any determination of the value of the eccentricity. It appears to me that I have indicated the initial steps of a new solution of the problem of the moon's motion, which it might be worth the while of those who have time for such researches to trace to its consequences.

Cambridge Observatory,
March 20, 1854.

XLIII. *On the Spheroidal State of Water in Steam-boilers.*

By A. Normandy, *Esq.*

To Dr. Tyndall, F.R.S. &c.

67 Judd Street, Brunswick Square,
March 20, 1854.

Dear Sir,

Convinced as I am that water frequently assumes the spheroidal state in boilers, in consequence of which these most important vessels, on which the very existence of the steam-engine, that is, of most of the comforts of civilized life, depend, are too often converted by mismanagement or ignorance into frighful engines of death and destruction, I venture now to trouble you with a few observations in addition to those which you were kind enough to listen to the other day, in the hope that, should the facts which I am about to relate appear to you as conclusive as they do to me, you may be induced, in your lectures on heat, to call once more the attention of the audience, should you deem fit to do so, to this, I believe, fruitful but almost unsuspected, or at any rate scarcely credited source of explosion.

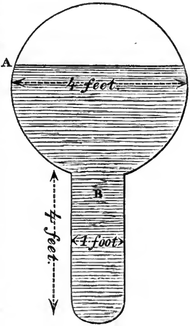
I have already had the honour to relate to you that I had seen the plates of a Cornish boiler, a quarter of an inch thick, become red-hot in the flue, although at the time the boiler contained its due quantity of water. It was a new boiler, about 18 feet long; and that the water had assumed therein the spheroidal state is proved by the fact, that a lead rivet, 1 inch in diameter, put in for safety was actually melted. It is, in fact, this rivet which led to the detection of the cause of the occurrence, and an investigation of the subject brought out the following details. The fireman or stoker was an ignorant mulatto, who having noticed the lead rivet in the boiler, had given it as his opinion that it would soon be melted by the fire; the possibility of such an accident having been denied by one of his fellow-workmen, a bet had taken place between them. The wager having thus been laid, the fireman set to keeping up a brisk, bright fire; in consequence of which, steam being generated exceedingly fast, *priming*, that is, a production of steam under the superincumbent water, took place, and, as a matter of course, the water, or a portion thereof, not being any longer in contact with the surface of the iron plate, the latter in a very short time became red-hot, and *the rivet melted*; for some time afterwards, however, the water being in the spheroidal state, the boiler remained apparently sound, that is, it did not leak, although it had an opening of an inch diameter left in consequence of the melting of the rivet; it is only shortly afterwards I was told, that the heat having diminished, torrents of water poured through the hole with almost explosive violence, emitting volumes of scalding steam as it came in contact with the fire on the grate.

This took place at the Wenlock Timber Saw-mills; and I can bear testimony to the above fact, and likewise to this circumstance, that when filled as usual with its normal quantity of water, the bottom of the boiler over the fire could, in the course of five or six minutes, be made red-hot at pleasure by brisk firing. In fact, having called at the above works, according to appointment, to meet the engineer there, my inquiry as to the then state of the boiler was answered by the foreman, in presence of his master, in these words:—"*The boiler is not red-hot now, but if you will step this way it will be made so immediately*;" and in spite of all remonstrance as to the danger of such an experiment, he came shortly afterwards to announce that the boiler was red-hot, and *I saw it* in that state in presence of the engineer, the master, his nephew, the foreman, and the fireman or stoker. The production of steam had been thereby spontaneously reduced, the bolt or lead rivet eventually melted, but not in my presence; for seeing the state of things, I speedily retreated with the engineer and the master, but almost reluctantly followed by the foreman, in whose somewhat jeering looks I could plainly read that he entertained but a poor idea of our pluck.

In addition to this, I may say that in "waggon boilers," a shape than which none can be more unsafe, but which is now fortunately much less in use than in former years, bulging bumps are very often observed, which are certainly due to the pressure of the steam in the boiler upon the iron plates of the bottom, when by extravagant or incautious firing they have been made red-hot. Frequently, however, instead of bulgings or bumps being thus produced, a disruption of the boiler takes place, attended with more or less fatal consequences. Many such accidents have thus happened at Manchester, and particularly on Monday mornings.

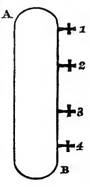
The following occurrence, related to me by my friend Mr. A. M. Perkins as having taken place *in his presence* in a boiler of a peculiar construction in which steam was generated by means of his hot-water pipes, is also well worth mentioning, as I think we may gather from it a correct explanation of the facts under consideration; and the rough sketch below, which represents a cross section of the boiler, will perhaps explain the matter better than I could do by writing.

The boiler had the dimensions indicated, and was 8 feet in length; it was filled with water to about A as usual, the safety-valve had an area of 4 inches. From such a boiler the whole of the water was completely driven off through the valve by violent *priming*; that is to say, steam was generated so fast by a hot water pipe in the limb B, that it completely lifted up the water and violently squirted it out of the boiler, as just related. In such a case, the water being heated by pipes, and not by contact with the heated surfaces of the boiler, no other mischief could result than that of priming: but suppose, on the contrary, such a boiler to have been heated in



the usual manner; there is no doubt that the steam generated in the limb B would, by lifting the superincumbent water, have soon permitted that part of the boiler to become red-hot, and thus have produced the curious phænomenon *revived* and *studied* by Boutigny d'Evreux, and named by him a fourth or spheroidal condition of matter, but to which, so far as refers to water, the late Jacob Perkins had, to my certain knowledge, called the attention of engineers and of savants more than twenty-five years ago (in fact, as far back as 1824).

Allow me to relate also the curious experiment of a distinguished civil engineer, Mr. Alexander Gordon, made several years ago, as related to me by Mr. A. M. Perkins.



AB is a cylinder provided with four try-cocks, 1, 2, 3, 4; water being introduced into the cylinder and heated therein, the cocks on being successively turned gave the following results; namely, steam issuing from 1, 2, 3, and water from 4; but after urging the fire, the above order was found to be completely reversed; that is to say, cock No. 1 emitted water, and cocks Nos. 2, 3, 4 steam.

These facts, in my humble judgement, appear to me to prove in a decided manner,—

1. That the heating of surfaces previous to the introduction of water is not necessary to produce the spheroidal state.

2. That many boiler explosions may be referable to that condition.

3. That all boilers which offer an extensive surface to the heat, that is to say, all boilers with internal flues, are pre-eminently liable to explosions from this cause.

Earthy deposits in all kinds of boilers are favourable to the production of this dangerous phænomenon.

I cannot terminate this informal letter without apologising for having so long trespassed on your valuable time and patience; but if a collection of facts be essential to the progress of science, I hope that you will indulgently receive this communication, and believe me,

Yours very faithfully,
A. Normandy.

XLIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 223.]

Jan. 12, 1854.—The Lord Chief Baron, V.P., in the Chair.

THE following was read :—

Supplement to a paper "On certain Properties of Square Numbers and other Quadratic Forms, with a Table, &c." By Sir Frederick Pollock, F.R.S. &c.

In the original draft of this paper there was a suggestion that all the terms of the series 1, 3, 7, 13. &c. [there called the Gradation-Series] possessed the property that was exhibited as belonging to the odd number 197. This was omitted in the copy from some doubt whether it was universally true. Since the paper was read that doubt has been removed, and it turns out that the property belongs not only to *all* the terms of the series 1, 3, 7, 13, &c., but to all odd numbers whatsoever. I am desirous to add to the paper this statement by way of supplement. The property referred to may be thus enunciated :—

Every odd number may be divided into square numbers (not exceeding 4) whose roots (positive or negative) will by their sum or difference [in some form of the roots] give every odd number from 1 to the greatest sum of the roots, which (of course) must always be an odd number.

Or the theorem may be stated in a purely algebraical form, thus :—If there be two equations

$$a^2+b^2+c^2+d^2=2n+1$$
$$a+b+c+d=2r+1,$$

a, b, c, d being each integral or zero, n and r being positive, and r a maximum; then if any positive integer r' (not greater than r) be assumed, it will always be possible to satisfy the pair of equations

$$w^2+x^2+y^2+z^2=2n+1$$
$$w+x+y+z=2r'+1$$

by integral values (positive, negative or zero) of w, x, y, z.

I hope shortly to communicate a proof of the above theorem, independent of any of the usual modes of proving that every odd number is composed of (not exceeding) four square numbers.

Note.—The differences of the roots of 197 were not fully stated in the paper, I add them here :—

197

has 7 forms of roots :—

Forms of roots.	
14, 1, 0, 0	
12, 7, 2, 0	3 ... 7......................17 21
12, 6, 4, 1	1, 3 ...... 9, 11, 13, 19, 21, 23,
11, 6, 6, 2	1, 3 ...... 9, 13, 21, 25,
10, 9, 4, 0	3, 5,.................. 15,................ 23
10, 6, 6, 5	3, 5, 7 .... 15, 17, ... 27.
9, 8, 6, 4	1, 3, 7, 11, 15, 19.............. 27.

Jan. 19.—Charles Wheatstone, Esq., V.P., in the Chair.

A paper was read, entitled "On the Geometrical Representation of the Expansive Action of Heat, and the Theory of Thermo-dynamic Engines." By W. J. Macquorn Rankine, F.R.S.S.L. & E. &c.

The author remarks, that if abscissæ be measured from an origin of rectangular coordinates, representing the volumes assumed by an elastic substance, and if ordinates, at right angles to those abscissæ, be taken to denote the corresponding expansive pressures exerted by the substance, then any succession of changes of pressure and volume may be represented geometrically by the coordinates of a curve. If such a curve have two extremities, the area included between the curve and the ordinates let fall from its extremities will represent (when positive) the expansive power given out by the substance during the process represented by the curve. Should the curve be closed, returning into itself, so as to denote a cycle of periodical changes of pressure and volume, then will the area, enclosed within the curve, represent (when positive) the expansive power given out during one cycle of changes. This area is positive when increase of volume takes place on the whole at greater pressures than diminution of volume. The area of such a closed curve represents also (when positive) the mechanical equivalent of the heat which permanently disappears, or is converted into expansive power, during a cycle of changes, for were it not so, the sum of energy in the universe would be changed, which is impossible.

As the principles of the expansive action of heat are capable of being presented to the mind more clearly by the aid of diagrams of energy than by means of words and symbols alone, such diagrams are applied, in the present paper, partly to the illustration and demonstration of propositions previously proved by other means, but chiefly to the solution of new questions, especially those relating to the theory of thermo-dynamic engines.

Throughout the whole of this paper, quantities of heat are expressed, not by units of temperature in an unit of weight of water, but by equivalent quantities of mechanical power, stated in foot-pounds according to the ratio established by Mr. Joule's experiments on friction (Phil. Trans. 1850), that is to say, 772 foot-pounds per degree of Fahrenheit, or 1389·6 foot-pounds per Centigrade degree, applied to one pound of liquid water at atmospheric temperatures.

A curve described on a diagram of energy, such that its ordinates represent the pressures of a homogeneous substance corresponding to various volumes of an unit of weight, while the total sensible or actual heat (Q) present in an unit of weight of the substance, is maintained at a constant value (Q_1), may be called the ***Isothermal Curve*** of Q_1 for the given substance. Its equation is

$$Q=Q_1.$$

If an unit of weight of a substance be allowed to expand, under a pressure equal to its own elasticity, without receiving or emitting heat, its actual heat will diminish during the expansion, and its pressure will diminish more rapidly than it would do if the actual heat were maintained constant. A curve whose coordinates represent this mode of variation of pressure and volume may be called a

Curve of no Transmission of Heat. For every such curve a certain function of pressure, volume and actual heat, called a *Thermo-dynamic Function* (F), has a constant value (F_A) proper to the particular curve under consideration; whose equation is therefore

$$F = F_A.$$

A curve whose coordinates represent the relation between pressure and volume when the substance is absolutely destitute of heat, may be called the *Curve of Absolute Cold.* It is at once an isothermal curve and a curve of no transmission, and is an asymptote to all the other curves of both those kinds, which approach it indefinitely as the substance expands without limit.

The whole theory of the expansive action of heat is comprehended in the geometrical properties and mutual relations of those two classes of curves; and all those properties and relations are the consequences of and are virtually expressed by the two following theorems:—

Theorem I.—*The mechanical equivalent of the heat absorbed or given out by a substance in passing from one given state as to pressure and volume to another given state, through a series of states represented by the coordinates of a given curve on a diagram of energy, is represented by the area included between the given curve and two curves of no transmission drawn from its extremities, and indefinitely prolonged in the direction representing increase of volume.*

Theorem II.—*If across any pair of curves of no transmission on a diagram of energy there be drawn any series of isothermal curves at intervals corresponding to equal differences of actual heat, the series of quadrilateral areas thus cut off from the space between the curves of no transmission will be all equal to each other.*

These two propositions are the geometrical representation of the application, to the particular case of heat and expansive power, of two axioms respecting Energy in the abstract, viz.—

Axiom I.—*The sum of energy in the Universe is unalterable.*

Axiom II.—*The effect, in causing transformation of energy, of the whole quantity of actual energy present in a substance, is the sum of the effects of all its parts.*

The application of these axioms to heat and expansive power involves the following

Definition.—*Expansive Heat is a species of actual Energy, the presence of which in a substance affects, and in general increases, its tendency to expand*;—

and this definition, arrived at by induction from experience, is the foundation of the theory of the expansive action of heat.

The first section of the paper is occupied chiefly with the demonstration of the first of the theorems quoted and its immediate consequences, which are applicable to all substances, homogeneous and heterogeneous.

The second section relates to the theory of the expansive action of heat in homogeneous substances.

From the second theorem above quoted, it is deduced, that the area of any quadrilateral bounded above and below by any two isothermal curves, and laterally by two curves of no transmission, is the product of the difference between the two quantities of actual heat proper to the isothermal curves, by the difference between the two thermo-dynamic functions proper to the curves of no transmission, being represented by an expression of this form,

$$(Q_1-Q_2)\,.\,(F_B-F_A).$$

While the area of a figure bounded above by the isothermal curve of Q_1, and laterally by the indefinitely-extended curves of no transmission corresponding to the thermo-dynamic functions F_A, F_B, is represented by

$$Q_1(F_B-F_A).$$

The area of a diagram of energy of any figure is calculated by conceiving it to be divided, by a network of isothermal curves and curves of no transmission, into an indefinite number of stripes or quadrilaterals, finding the area of each and adding them by summation or integration. By the aid of these principles various problems are solved.

In the third section the same principles are applied to determine the efficiency of thermo-dynamic engines worked by the expansion and contraction of permanent gases without and with the aid of *economisers* or *regenerators*. The *efficiency* of a thermo-dynamic engine is the proportion of the whole heat communicated to the working substance which is converted into motive power.

The *maximum theoretical efficiency* of a thermo-dynamic engine working between the limits of actual heat Q_1 and Q_2, whether without a regenerator or with a perfect regenerator, is expressed by the fraction

$$\frac{Q_1-Q_2}{Q_1}.$$

A theoretically perfect regenerator does not increase the maximum efficiency between given limits of actual heat, but merely enables that efficiency to be attained with a smaller extent of expansion, and consequently with a smaller engine.

The fourth section treats of the relation between actual heat and temperature, which must be known before the propositions of the preceding sections can be applied to actual substances. Existing experimental data are not yet adequate to the *exact* determination of this relation; but it is considered they are sufficient to show that a relation deduced by the author from the Hypothesis of Molecular Vortices (see Philosophical Magazine for December 1851, p. 510), is at least near enough to the truth for all purposes connected with the computation of the efficiency of thermo-dynamic engines. This relation is expressed by the formula

$$Q=\mathfrak{k}(T+T_0),$$

where T is temperature, measured from the melting-point of ice; T_0, the height of the melting-point of ice above the point of total privation of heat; and $\mathfrak{k}$, the mechanical value of the real specific heat of the substance. According to computations made in 1852 by the author from experiments by Messrs. Thomson and Joule, $T_0=272\frac{1}{2}°$ Centigrade$=490\frac{1}{2}°$ Fahrenheit, a value which may be considered sufficiently correct for practical purposes.

The maximum theoretical efficiency of every conceivable thermo-dynamic engine receiving heat at the temperature T_1, and giving out heat at the temperature T_2, is

$$\frac{Q_1-Q_2}{Q_1}=\frac{T_1-T_2}{T_1+T_0}.$$

The fourth section concludes with a system of formulæ, illustrated by numerical examples, for computing the power and efficiency of air-engines.

In the fifth section, the principles of the preceding sections are applied to aggregates consisting of heterogeneous substances, or of the same substance in different conditions, especially the aggregate of a liquid and its vapour; and the results are applied to the numerical computation of the theoretical efficiency of steam-engines.

Jan. 26.—The Rev. Baden Powell, V.P., in the Chair.

A paper was read, entitled "On the Vibrations and Tones produced by the contact of bodies having different Temperatures." By J. Tyndall, Esq., F.R.S. [An abstract of this paper was given in our last Number.]

ROYAL INSTITUTION OF GREAT BRITAIN.

March 3, 1854.—On certain Phænomena of Rotatory Motion. By the Rev. Baden Powell, M.A., V.P.R.S., F.R.A.S., F.G.S., Savilian Professor of Geometry, Oxford.

The mechanical principle of "the composition of Rotatory Motion," originally discovered by Frisi about 1750 (see *Frisius de Rotatione*, Op. ii. 134, 157, and *Cosmographia*, ii. 24), is equally simple in its nature, important and fertile in its consequences and applications, and susceptible of the easiest explanation and experimental illustration; yet it has been singularly lost sight of in the common elementary treatises. It is, indeed, discussed and applied in a mathematical form in Mr. Airy's Tract on Precession (Math. Tracts, p. 192, 2nd ed.); and the theorem is stated by Professor Playfair in his Outlines of Natural Philosophy (vol. i. p. 144), and its application explained (ib. vol. ii. p. 308). These, however, are not books of a popular kind, and the author is not aware of any mention of it in other English works. In a more abstract analytical form it has been discussed by several foreign mathematicians, especially by Poinsot, in a memoir read to the Academy of Sciences, May 19, 1834, but of which only an abstract was published; as well as by Poisson, in a paper in the *Journal de l'Ecole Polytechnique* (vol. xvi. p. 247).

The principle is involved in the explanation of several important phænomena, some of which are, in fact, mere direct instances of it; so that a simple experimental mode of exhibiting it would be eminently desirable; and several such have accordingly been devised which yet seem to have been but little generally known.

An ingenious instrument of the kind was contrived some years ago by Mr. H. Atkinson, a very brief account of which is given in the Astronomical Society's Notices, vol. i. p. 43, though so brief that it is difficult to collect what the precise mode of its action was, —but it seems somewhat complex.

A far more complete and instructive apparatus was invented by Bohnenberger, and described in Gilbert's *Annalen* (vol. lx. p. 60). It is also explained in some German elementary works. Attention has been more recently drawn to the subject by a highly interesting paper of Professor Magnus of Berlin (*Verhandlungen der Königl. Preuss. Akad.* 1852, translated in Taylor's Foreign Scientific Memoirs, N. S. part 3, p. 210), in which some remarkable applications of this apparatus are given; he also describes it (with a figure), and observes that the execution of it requires great delicacy and correctness of workmanship. Copies of this instrument have indeed been made in this country (one of which was exhibited through the kindness of Professor Wheatstone); but of these the author believes no description has ever appeared in English works, and they are certainly very little known, notwithstanding their manifest value to every lecturer: the essential parts are a sphere capable of rotating about an axis whose extremities rest in opposite points of a hoop which can turn on pivots *horizontally*, within another hoop turning on pivots about a *vertical* line.

In fact, the author of the present communication has long felt the want of such an apparatus for lecture illustration; and before he was aware of the existence of any of those just alluded to, had constructed one in a different form, and which is found to answer fully the purposes of illustration for which it is designed, without any nice workmanship or complex machinery. (See Astronomical Society's Notices, vol. xiii. pp. 221–248.)

Its object, like that of the instrument last mentioned, is to exhibit experimentally *the actual composition of rotations about two different axes impressed at once on the same body.*

The essential parts are merely a bar capable of rotating freely about one end of an axis (and loaded at its extremities to keep up the rotation), while the axis itself can turn about a point in its length near the end carrying the bar, upon a horizontal axis, capable of moving freely round a vertical pillar. At the lower end of the first axis is a weight which more than counterpoises the upper part.

If, then, there be no rotation in the bar about the first axis, the effect of the weight is to produce a rotation about the second alone, bringing down the first axis into a vertical position.

If now the first axis be held horizontally or obliquely, and a rotatory motion be given to the bar about it, on letting the axis go *we compound both rotations*; and the resulting effect is, that the weight will no longer bring the axis *down*, or alter its *inclination* at all, but will cause it to take a new position, or make the whole to turn round the vertical, in a direction *opposite* to that of the rotation.

Thus, although confessedly not new in principle, to make public an experimental illustration in so simple a form may not be without its use for a great majority of students.

Even the theoretical principle is capable of being stated in a way quite intelligible to those acquainted only with the very first rudiments of theoretical mechanics, presenting itself in close *analogy* to that well-known first principle, the composition of *rectilinear* motion.

As in this last case, if a body be in motion in one direction, and any cause tend to make it move in another, it will move in neither, but in an intermediate direction,—so we have the strictly analogous case in *rotatory* motion; *when a body is rotating about an axis, and any cause tends to make it rotate about another axis, it will not rotate about either, but about a new axis intermediate to the two.* Thus the result of compounding the two rotations will be, that the axis (carrying with it the rotating body) will simply take a new position, or will move in a direction determined by the nature of the impressed motions.

Professor Magnus, in the very able but rather prolix and obscurely written memoir before referred to, speaks (p. 223) of the consequences of such a law as evinced in the resulting rotations, but without any distinct or explicit statement of the essential theorem of the composition of rotatory motion. He gives, however, some singular and even paradoxical exemplifications of it. We may allude to one of these, which is capable of being put into a form at once *more simple*, and at the same time *more paradoxical*, than that in which he describes it. It consists in this: a wheel at one end of an axis and a weight at the other are suspended *in equilibrio*, which is of course unaltered, whether the wheel be at rest or in rotation; the weight is then slid so that the balance is *destroyed*; *now* if the wheel be set in rapid *rotation*, the *equilibrium is restored*. This is nothing but a simple case of the principle just stated, as shown by the author's apparatus.

Besides certain other cases traceable to a different cause, Professor Magnus's immediate object is to explan a curious observed anomaly in the motion of projectiles of an *elongated* form shot from *rifled* guns, and which consequently *rotate* about their axis while passing through the air in the direction of that axis.

He mentions the fact, that artillery experiments in different countries with rifled cannon and missiles of a cylindrical form with a conical apex, *always show a deviation of the point of the missile to the right, the rifle-spiral being right-handed.*

To explain the nature of this deviation was the object of special experiments on the part of the Prussian Artillery Commission, in which Professor Magnus assisted. The missiles were fired with low charges, so as to allow the motion to be accurately observed, and it was found that the axis remained sensibly in the direction of the tangent to the curved path, while the deviation to the right was always clearly marked. He observes that left-handed rifles have never been tried.

Professor Magnus, after some fruitless conjectures as to the cause, at length sought it in the principle of *the composition of rotatory motion*. He tried experimentally the effect of a current of air on a projectile of the form employed, by inserting such a body instead of the rotating sphere in Bohnenberger's apparatus, and observing the effect on it, first at rest and then in rotation, when the strong current of a blowing-machine was directed against the conical apex. When at *rest*, the current *elevated* the apex; owing to the form of the missile, the resistance acting not through the centre of gravity, but above it; when in *rotation* no *elevation* took place, but a *deviation* in the direction of the axis, in a direction opposite to that of rotation. To show the application of the principle in this case, he observes that the axis of the elongated projectile, which for an instant coincides with the *tangent* to its curved path, momentarily changes its direction, so that the front extremity or apex falls below its former position. Or, for a single instant it may be regarded as if locally at rest, but turning about its centre of gravity so as to depress the apex.

If the motion were simply in the direction of the axis, the *resistance of the air* would operate directly against it; but when the apex is continually tending to turn *downwards* from that line, the resistance acts against it partially *upwards*, and thus tends to raise the apex.

Thus, at a given instant, the elongated projectile may be represented by the rotating part of the apparatus just described.

When there is *no rotation*, the resistance of the air tending to raise the apex is represented by the weight at the lower end, which produces the same effect.

When a rapid *rotation* is communicated (suppose from left to right of the gunner), the result will be *no elevation of the apex*, but a *lateral* movement, or commencement of a rotation round the vertical —in astronomical language *retrograde*, if the former rotation be *direct*; but which, beginning from the opposite part of the circle, is, *relative to the operator*, towards the *right*.

The form of the projectile used in these experiments differs from that in the Minié rifle, in that the latter is hollow at its broader end, and thus the centre of gravity is thrown forward towards the apex. Hence, according to the same theory, the effect would probably here be to depress the apex, and therefore to give an opposite deviation; but it does not appear whether any such observations have been made; and in practice the effect would probably be quite insensible.

It occurred to the author that a very simple illustration of this deviation of rifle projectiles might be made by merely forming a sort of small arrow, whose head was composed of a cork, like a shuttlecock, but instead of the feathers, small card vanes inclined in the same direction round it, with a tail to balance it, and which thus in the mere act of throwing acquires a rotatory motion from the reaction of the air, to the right or left according as the vanes are inclined; and on trying this, there was always observed a deviation in the direction of the axis or point of the missile to the right or left accordingly, relative to the experimenter. It is, in fact, nearly impossible to throw such a body in a direction perfectly in one plane. The true deviation is, however, peculiarly liable to be disguised by the general resistance of the air on so light a missile, as well as by currents, &c. which it is not easy to guard against.

The well-known case of the *Boomerang* exhibits effects closely similar; for it is found that if so projected that its *rotation* is from left to right, its *deviation* will be in the *same direction*, and *vice versâ*; that is, supposing (as is the usual case) that its plane is inclined *upwards* from the operator:—if it be inclined *downwards*, the deviation is in the direction *opposite* to that of the rotation.

In the former case the reaction of the air against the flat surface of the missile would tend to increase its inclination *upwards*, in the latter *downwards*, with respect to the operator; and this in each case respectively would give the motion stated; as is easily seen on the principle, and by means of the apparatus, before described.

Thus it would follow, that this extraordinary instance of savage invention, which long ago puzzled inquirers, is simply a case (like the last) of "the composition of rotatory motion."

It should, however, be mentioned, that some experimentalists have entertained a different view of the cause of deviation in this instance.

Besides the results above stated, Professor Magnus (in the same memoir) mentions several other highly curious cases produced by certain modifications of the apparatus, but all referable to the same principles.

M. Fessel has also invented an apparatus (since called the gyroscope), an account of which is given with some remarks by Professor Plücker, and the editor in Poggendorff's *Annalen* (1853, Nos. 9 and 10), which, though apparently invented without any knowledge of Bohnenberger's apparatus, is a modification of it, referring to phænomena of the same kind as those of the equilibrium experiment mentioned at first.

This apparatus has been greatly improved upon by Professor Wheatstone, who has introduced other movements to include the conditions of rotation in different planes. One of these instruments was exhibited.

From these singular applications of a very simple mechanical truth, we may now turn to what is but another exemplification of the same thing, however apparently remote from those we have considered, and upon a far grander scale.

The phænomenon of the precession of equinoxes was known to Hipparchus, but no explanation of the fact was for ages imagined. Even Kepler, in the multiplicity of his hypothetical resources, could not succeed in devising anything plausible. The axis of the earth is slowly shifting its position, so that its pole points continually to a new part of the heavens—a new pole star—at the rate of about 50″ a year, and of course carries with it the point of intersection of the earth's equator with the ecliptic or plane of its orbit, at the same rate and in a direction opposite to that of its motion, or the order of the signs.

These phænomena remained wholly without explanation till Newton, led by the analogy of those disturbing forces on the orbit of a planet which cause its *nodes to regress*, showed that the same would occur in a *satellite* to the earth—in a *ring* of such satellites—in such a ring adhering to the equator, or the protuberant part of the terrestrial sphere, and thus that the equinoctial points would slowly regress. (See *Principia*, vol. i. p. 66, corr. 11–22.)

The more exact determination of quantitative results was reserved for Newton's successors, when a more powerful analysis had been applied by Euler, D'Alembert and others, to the full exposition of the theory, founded on general equations of motion; as since given in the writings of Laplace (*Méc. Cél.* liv. xiv. ch. 1), and Pontécoulant (*Théorie du Systéme du Monde*, liv. iv. ch. 5), which are necessary for including all the minuter variations detected by Bradley, and subsequent observers, showing the *nutation* of the axis, and the inequalities of precession due to the varying configurations of the attracting luminaries.

These higher mathematical views, though of course the most complete and systematic, are not the most direct or easy mode of explaining the subject to the student. Greater simplicity certainly characterizes the method adopted by Mr. Airy (in the tract before cited) of applying *directly* the theorem of the composition of rotatory motion, as doubtless Newton would have done had it been known to him. But here, as in so many other instances, the first explanation presented itself mixed up with more complex considerations; and as has been well observed, "simplicity is not always a fruit of the first growth."

To those not versed in the mathematical theory of all points in physical astronomy, the "*modus operandi*" of the precession, perhaps, usually seems the most paradoxical; and the explanations given in some of the best popular treatises are seldom found satisfactory, following as they do the letter of Newton's illustration, and omitting the *direct* introduction of the principle of composition, which, if only from what has been here offered, is at once seen to be easily capable of the most elementary explanation. Indeed it was from this consideration forcing itself on the mind of the author, in several courses of popular lectures on astronomy, that he was led to seek the means of experimental illustration above described; and which would more palpably imitate the phænomena to the eye, if, instead of the rotating *bar* a terrestrial *globe* be substituted (as in Bohnenberger's instrument)—for better illustration made protuberant at the equator—where the weight at the south pole acts the part of the sun's and moon's attraction, to pull down the protuberant matter of the spheroid at the equator if at *rest*, but when combined with the earth's *rotation* results in a transference of the position of its axis, or slow revolution of its pole round the pole of the ecliptic in a direction opposite to its rotation, carrying with it the equinoctial points, and causing the *signs* of the zodiac to shift backwards from their respective *constellations*.

It always affords a sort of intellectual surprise to perceive for the first time the application of some simple and familiar mechanical principle to the grand phænomena of astronomy; to see that it is but one and the same set of laws which governs the motions of matter on the earth and in the most distant regions of the heavens; to find the revolution of the apsides in a pendulum vibrating in ellipses, or the conservation of areas in a ball whirled round by a string suddenly shortened; or (as in the present case) to perceive a celestial phænomenon, vast in its relations both to time and space, and complex in its conditions, identified, as to its mechanical cause, with the rotatory movement of a little apparatus on the table before us, or to discover the precession of equinoxes in the deviation of a rifle or a boomerang. And the simple experimental elucidation of such phænomena and their laws will not be useless, as it tends to confirm in the mind of the student the great characteristic of the modern physical philosophy first asserted by Galileo, the identity of the causes of the celestial and terrestrial motions, and to aid and elevate our conception of those grand and simple principles according to which the whole machinery of the universe is so profoundly adjusted.

XLV. *Intelligence and Miscellaneous Articles.*

ON THE HEAT PRODUCED BY AN ELECTRIC DISCHARGE.

BY R. CLAUSIUS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN a paper by Prof. W. Thomson, in the March Number of the Philosophical Magazine, p. 193, the following statement occurs on the discovery of the important law, that the heat capable of being produced by an electric discharge is not simply proportional to the quantity of electricity employed, but to the square of the same, "as was first demonstrated by Joule in a communication to the Royal Society in 1840, although it had been announced by Sir W. Snow Harris as an experimental result, to be simply proportional to the quantity. Mr. Joule's result has been verified by independent observers in France, Italy and Germany." I may be allowed to observe on this subject, that the investigations of Riess, mentioned by me in my paper on the mechanical equivalent of an electric discharge, are of a much older date. The memoir in which Riess refuted the statement of Snow Harris, and proved to a certainty by numerous experiments that the heat which is produced by the discharge of a Leyden battery in any part of the connecting wire is represented by the formula $\frac{q^2}{s}$ const.,

in which q denotes the quantity of electricity used, and s the surface of the inner coating, is printed in Poggendorff's *Annalen* for March, 1837.

I am, Gentlemen,

Yours faithfully,

Berlin, March 19, 1854. R. CLAUSIUS.

ON SOME PECULIAR REDUCTIONS OF METALS IN THE HUMID WAY. BY PROF. WOHLER.

The following experiments were made for Prof. Wöhler by Hiller. The observation first made by Bucholz, that long crystals of metallic tin are formed when a rod of that metal is inserted in a solution of protochloride of tin, and the latter carefully overlaid with water, was first of all further tested. It appeared that, for the production of large crystals, the solution of chloride of tin must be acid. Of the tin immersed in the solution there was always more dissolved than was made up by that which crystallized. In one experiment the proportions were as 7 : 6.

These crystals are formed at the point of contact between the two fluids. If the solution be neutral, they appear below this in the solution of the protochloride, and remain bright.

Copper, inserted into a neutral solution of nitrate of copper, covers itself entirely with brownish-red crystals of protoxide of copper, and afterwards with sharp crystals of metallic copper. The copper is dissolved, especially at the point of contact of the fluids. The same phænomenon is produced, but in a less degree, with sulphate of copper. In solution of perchloride of copper, the copper is covered with crystals of the protochloride.

A rod of zinc, under similar circumstances, covers itself with gray granules of metallic zinc, especially at its lower end. In this case also the zinc is dissolved at the point of contact of the fluids.

Cadmium behaves in a similar manner in the solution of its nitrate; the reduced metal is more pulverulent, and therefore much more readily oxidized in the air than the reduced zinc.

Lead, in a solution of neutral nitrate or acetate of lead, furnishes small shining crystals of lead.

Bismuth precipitates the metal from a solution of protochloride of bismuth, if the latter has been overlaid first with muriatic acid, and afterwards with water.

On silver, immersed in a concentrated solution of nitrate of silver overlaid with water, metallic silver is deposited in a dendritic form, always originating from a few scattered points of the surface of the silver.—*Ann. der Chem. und Pharm.*, lxxxv. p. 253.

ON THE DECOMPOSITION OF WATER BY THE PILE. BY J. JAMIN.

M. Foucault has just published a note in which he shows that two voltameters traversed by the same current disengage different quantities of gas, if one contain acidulated water, with electrodes of platinum wire, and the other only pure water, with plates of a certain size for electrodes.

M. Foucault explains this phænomenon by supposing that fluids transmit electricity in two ways: by physical conductibility, which is effected without decomposition; and by chemical conductibility, which causes a separation of their elements. M. Foucault collected the gases in the same receiver, and did not endeavour to ascertain whether the passage of the current gave rise to any new chemical compound. The author has been occupied for several months in similar researches, and had ascertained the fact which M. Foucault has published. He now confirms it, but adds some experiments which prevent his admitting the theoretical consequences of M. Foucault.

In attentively studying the decomposition of water, it is evident the separation of its elements does not take place with the simplicity hitherto supposed. The double volume of hydrogen is scarcely ever obtained, and predominating quantities of either gas may be produced by alteration of circumstances, and especially by changing the extent of one of the electrodes.

Employing as a positive electrode a Wollaston wire, and as a negative a plate of 15 square centimetres, the author obtained five cubic centimetres of hydrogen and nine of oxygen, being in a proportion of 0·55 instead of 2 to 1. By changing the direction of the current so as to reverse the electrodes, and waiting for some hours to avoid errors of polarization, the volume of hydrogen collected was to that of oxygen as 9·3 to 1·0 instead of 2. These experiments, frequently repeated, did not always furnish equally decisive results, but they had always the same tendency; showing that electrodes with a large surface, whether positive or negative, disengage less gas than fine slender wires employed as the opposite electrodes in the same voltameters.

The experiment is more striking when two voltameters, arranged as in the first and second case, are placed in the same circuit. In one case there were—

H . .	929	1204
2O . .	1158	544
$\frac{H}{O}$ . .	1·60	4·42

If the two plates be put into one voltameter and the two wires into the other, without any change in the other arrangements, similar results will be obtained; the two plates produce but little gas, whilst the two wires furnish a large quantity. A voltameter may be formed with a permanent Wollaston wire, and with a plate of platinum or of some other metal which may be changed at pleasure; each plate will disengage (the intensity of the current being the same) very variable quantities of gas, whilst the wire produces a constant action; there is consequently complete independence between the two electrodes. These inequalities may be increased or diminished by very different circumstances; they become very remarkable in employing the platinum plates of a Grove's battery. The author thus formed two dissymmetrical voltameters, which only produced a disengagement of gas at the wires, but none from the plates; the first furnished nothing but hydrogen, the second only oxygen; by putting the two plates in the first vessel and the two wires in the second, and passing the current through them, the former allowed it to pass without decomposition, whilst the second produced a great disengagement of gas. This, therefore, has nothing to do with conductibility, but depends on a property peculiar to each electrode.

As one only of the elements of water is obtained by a dissymmetrical voltameter, it must be admitted that the one which is not disengaged must combine with the liquid, or become condensed on the plates. These two effects take place simultaneously.

To show that oxygenated water is produced when hydrogen alone is evolved, all that is necessary is to carry on the decomposition in a cold platinum crucible communicating with the positive pole, immersing the negative wire in the fluid which it contains. The author has found that the hydrogenated liquid produced by the evolution of oxygen from water possesses new properties.

The decomposition of water never takes place without an alteration of the electrodes, which is slow, but continuous. The positive electrode becomes yellow, and passes to orange; the negative acquires a violet colour. These tints gradually increase and blacken, so that it is evident that something is deposited upon the platinum. These deposits disappear in the air, especially when the electrodes are heated. The negative plate becomes clean in nitric acid, and absorbs gaseous oxygen; the positive plate is cleaned by deoxidizing liquids, and absorbs hydrogen. Lastly, if the two electrodes be immersed in acidulated water and united by a galvanometer, they give rise to a current of reaction which lasts several days. These properties acquired by the plates deserve particular attention, and the author thinks they are to be attributed to condensation of the two gases on their surfaces. This opinion is confirmed by the fact, that when the coloration has become very intense, the plates continue to disengage gas for a long time after the passage of the current has ceased.

To resume, the author thinks that the decomposition of water is not a simple phænomenon,—that electricity first of all produces the pure and simple separation of the elements of water, and afterwards physical or chemical actions, which, although difficult to observe, certainly exist. These are weak in ordinary cases, but are no longer to be neglected where electrodes of large dimensions are employed, and it is to these actions that the anomalies observed in the decomposition must be attributed.—*Comptes Rendus*, February 27, 1854, p. 390.

LITHOLOGIC STUDIES. BY C. SAINTE-CLAIRE DEVILLE.

What are the elements that should guide us in the natural classification of igneous rocks? Such is the problem which I have set before me in this first memoir. The very nature of the question, as it was put forward by M. Cordier in the important memoir which he published in 1815, led me necessarily to re-examine minerals from a particular point of view, that of the part which they take in the formation of rocks. In this study I must particularly dwell upon an important notion, first introduced into science by M. Elie de Beaumont*, the distinction which that geologist has established between matters formed by igneous fusion, or *eruptive matters in the manner of lava*, and matters volatilized or carried up in the molecular state, *eruptive matters in the manner of sulphur, sal-ammoniac*, &c. The natural divisions which may thus be established in minerals are based, as I show, at once on the characteristic properties of the minerals of each group, on their stratification, and on the laboratory experiments by which they have been reproduced.

In the latter part of my memoir I endeavour to indicate what advantage may be derived from the characteristic properties of minerals, in establishing the natural relations between rocks which are only aggregates of these various minerals.

The conditions of stratification which every good classification should indicate, lead me to inquire what ought to be understood by the words—*the age of an igneous rock*. I close this question, by which lithology enters into relation with stratigraphy, by showing that in each family of rocks the age is in immediate relation with the nature and abundance of the minerals formed in the manner of sulphur; so that we may say, that this kind of minerals plays in some sort, in the igneous rocks, the part of *characteristic fossils*.

The certainty or the probability of these conclusions resting entirely upon the facts brought together in my memoir, some of which are known, whilst others appear to me to be new, it will be readily understood, that, both from its nature and extent, the work which I lay before the Academy is scarcely susceptible of an analysis. I shall therefore confine myself, in concluding this note, to calling attention to some of the prominent points of my work, and which I

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* Des Emanations Volcaniques et Métallifères, *Bull. Soc. Géol. Fr.* 2nd ser. vol. iv.

hope will possess some interest, as they settle the chemical formula of several substances on which the opinions of scientific men do not agree.

To establish the normal composition of the minerals formed *in the manner of sulphur*, I suppose that wherever a certain proportion of fluorine or chlorine is met with in these minerals, these bodies only exist there still because the reaction which should have converted the chlorides and fluorides into oxides has only been imperfectly effected. But if we complete, by calculation, the substitution of oxygen for the chlorine and fluorine, converting the whole into oxides, we arrive at a simple formula.

For the topaz, these suppositions, already sufficiently justified by the discussion of the analyses of this mineral due to M. Forchhammer, have just been confirmed by some unpublished researchs of my brother and M. Fouqué. The topaz would thus be assimilated to a silicate of alumina, in which the oxygen of the base would be in relation to that of the acid in the proportion of 4 to 3.

When the mineral contains, together with fluorine, a body which, like silicium, and especially boron, is capable of being sublimed with it during calcination, we may, by taking the most probable hypothesis, restore to the mineral its primitive composition. I find an example of this in the tourmaline. It is well known that mineralogists are far from being agreed as to the formula which should be attributed to this substance. In his last supplement, M. Rammelsberg proposes three different formulæ, which have scarcely anything in common, to represent the different varieties of a mineral so homogeneous in its crystalline form and in its properties. I have no space here to discuss the numerous analyses of tourmaline contained in the same volume; but if we suppose that the loss by calcination, which varies from 1·8 to 3·8 per cent., is owing to a disengagement of fluoride of boron, and calculate from this datum and from the boron determined, the corresponding quantity of boracic acid, we obtain the following numbers for the oxygen of the four constituent elements of the mineral:—

	Protoxides.	Boracic acid.	Sesquioxides.	Silex.
Calculated . .	4·33	6·50	16·24	19·48
Found , . .	4·56	6·43	16·77	19·48

A remarkable concordance, which, I think, justifies us in attributing a single formula to all the varieties of tourmaline, according to which the oxygen would be distributed amongst the four elements in the following proportions:—

$$4:6:15:18.$$

This formula would certainly not apply to the last group of tourmalines of M. Rammelsberg, the tourmalines containing lithia. But the presence of this base may throw a light upon the causes of this anomaly; the lithia must in great part be carried off by the fluorine, and this supposition is confirmed by the researches of MM. H. Sainte-Claire Deville and Fouqué. Moreover, when the same hypothesis is applied to analyses of tourmalines which doubtless had not been previously exposed to a white heat, such as those mentioned in M. Dufrénoy's *Traité de Minéralogie*, although the oxygen of the protoxides always presents a slight deficiency, we obtain results very similar to the preceding*. Certain groups of minerals, although belonging to the class of those which are formed in the manner of lavas, are nevertheless capable of containing *in a state of mixture* quantities, sometimes considerable, of minerals formed in the manner of sulphur. It is thus that we may explain the presence of fluorine, boron, titanium, &c. in pyroxene, and especially in hornblende.

The following is an immediate consequence of this remark. The efforts which have been made by mineralogists to reduce to a simple formula the analyses of hornblende, in which the alumina may amount to 20·0, or be entirely wanting, are well known. The opinion generally adopted for want of a better is that of M. Bonsdorff, who, supposing that alumina is isomorphous with silica, represents the mineral pretty well by a silicate of protoxides, in which the proportions of the oxygen in the acid and in the base would be 9 : 4, instead of 8 : 4 as in pyroxene. But everything is explained and simplified if we suppose that the alumina is foreign to the normal composition of the mineral, and that it only exists there as an integral portion of a substance formed essentially in the manner of sulphur; a *spinellide*, that is to say, a body of the form $R^2O^3 RO$, like the spinels of iron or magnesia, protoxide of iron, &c., which are found mechanically mixed with hornblende, and consequently never alter its crystalline form.

In applying this hypothesis to a great number of analyses taken at random from amongst those contained in M. Rammelsberg's work, we find, after previously deducting the intermixed aluminate, that the oxygen of the silica is nearly equal to double that of the protoxides. The analyses which present a noticeable discrepancy are those of the hornblendes, which contain much magnesia, and this fact bears a relation to the difficulty which is experienced in determining this base exactly.

I wished my idea to be checked by analyses made in the laboratory of the Ecole Normale with the precision afforded by the methods recently introduced there. The following are the results furnished:—

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* I do not mean to say that the boron is necessarily in the form of boracic acid in the tourmaline, nor that fluorine may not be normally contained in it. This is a point that can only be settled by analyses in which these bodies shall have been very exactly determined. My hypothesis consists simply in considering fluorine as capable of substituting itself atomically for oxygen.

I have as yet arrived at nothing so clear as regards mica, but I have no doubt that the application of the same principle to complete analyses of this singular body will explain its anomalous composition; and it may be that the optical anomalies which M. de Sénarmont has simplified and defined so remarkably in his memoir, are only due to the variable proportions in which the mutual substitution of fluorine and oxygen takes place in it; it is a question which I propose, with my brother's assistance, to examine in a separate memoir.

	Contents per cent.		Proportion of the oxygen of the protoxides to that of the silica after the removal of the spinellide.
	Alumina.	Magnesia.	
White amphibole (Fremolite) from St. Gothard	1·49	25·00	1 : 2·02
Green amphibole (actinote) from St. Gothard	3·89	18·52	1 : 1·83
Black amphibole (hornblende) from Guadaloupe	15·48	14·37	1 : 2·00
			Average 1 : 1·95

I think, therefore, that the chemical formula of hornblende is exactly the same as that of pyroxene, and that we must consequently return to an opinion formerly maintained by M. G. Rose, but which he has since abandoned, attributing, I think, to the presence of the sesquioxide an influence which it should not have upon the normal constitution of the mineral. The *secondary* dimorphism of pyroxene and hornblende would also be sufficiently explained by the very peculiar gaseous medium in which the latter mineral appears to have been formed, and the curious facts noticed by M. G. Rose in *uralite* would favour this explanation.—*Comptes Rendus*, February 27, 1854, p. 390.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1854.

Chiswick.—February 1. Cloudy: rain. 2. Overcast: clear, with bright sun: frosty. 3. Frosty: dense fog. 4. Frosty: very fine: overcast. 5. Clear: cloudy. 6. Fine. 7. Cloudy: clear at night. 8. Clear: cold and dry: overcast. 9. Cloudy: rain. 10. Clear and frosty: cloudy and cold: clear. 11. Cloudy and fine: hazy. 12. Foggy: clear and fine. 13. Frosty: cold and dry: clear, with sharp frost at night. 14. Sharp frost: fine. 15. Overcast. 16. Cloudy: clear: overcast. 17. Cloudy. 18. Drifting snow: clear and cold: boisterous at night. 19. Clear and cold. 20. Overcast. 21. Clear and fine. 22. Fine: cloudy. 23. Clear. 24. Cloudy. 25, 26. Very clear. 27. Uniformly overcast: clear: overcast. 28. Fine.

Mean temperature of the month .... 37°·67
Mean temperature of February 1853 .... 32 ·53
Mean temperature of Feb. for the last twenty-eight years ... 39 ·13
Average amount of rain in Feb. .... 1·57 inch.

Boston.—Feb. 1. Cloudy. 2—4. Fine. 5—9. Cloudy. 10. Fine: snow A.M. 11. Cloudy. 12. Fine. 13. Cloudy. 14. Fine. 15, 16. Cloudy. 17. Cloudy: rain P.M. 18. Cloudy: stormy. 19. Fine. 20. Cloudy: rain A.M. and P.M. 21. Fine. 22. Fine: rain P.M. 23—26. Fine. 27, 28. Cloudy.

Sandwick Manse, Orkney.—Feb. 1. Showers A.M.: clear P.M. 2. Showers A.M. and P.M. 3. Cloudy A.M.: cloudy, aurora P.M. 4. Cloudy A.M. and P.M. 5. Sleet-showers A.M.: showers P.M. 6. Showers A.M. and P.M. 7. Sleet-showers A.M.: hail-showers P.M. 8. Hail-showers A.M. and P.M. 9. Snow-showers A.M.: clear P.M. 10. Cloudy A.M. and P.M. 11. Drizzle A.M.: cloudy P.M. 12. Cloudy A.M. and P.M. 13, 14. Drizzle A.M. and P.M. 15. Hail-showers A.M. and P.M. 16. Drizzle A.M. and P.M. 17. Snow-showers A.M. and P.M. 18. Snow-showers A.M.: hail-showers P.M. 19. Cloudy A.M. and P.M. 20. Showers A.M.: clear P.M. 21. Bright A.M.: cloudy P.M. 22. Drizzle A.M.: showers P.M. 23. Snow-showers A.M.: hail-showers P.M. 24. Showers A.M.: showers, aurora P.M. 25. Showers A.M.: aurora P.M. 26. Cloudy A.M. and P.M. 27. Rain A.M. and P.M. 28. Sleet-showers A.M.: showers P.M.

Mean temperature of Feb. for twenty-seven previous years ... 38°·20
Mean temperature of this month .... 39 ·20
Mean temperature of Feb. 1853 .... 33 ·74
Average quantity of rain in Feb. for thirteen previous years... 3·33 inches.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1854. Feb.	Max.	Min.		9¼ a.m.	8¾ p.m.	Max.	Min.		9¼ a.m.	8¾ p.m.						
1.	29·960	29·939	29·50	29·62	29·77	53	35	46	37	38½	sw.	sw.	wsw.	·42	....	·30
2.	30·274	30·119	29·86	29·95	30·03	44	22	33	41	45	ne.	calm	sw.	....	....	·14
3.	30·327	30·241	30·05	30·17	30·14	33	21	28	45	43	ne.	calm	s.	....	....	·06
☽ 4.	30·071	29·833	29·84	29·68	29·22	45	35	28·5	40	40	se.	se.	se.	·10	....	....
5.	29·937	29·869	29·60	29·38	29·06	44	40	34	39	48½	sw.	w.	sw.	....	....	·12
6.	30·081	30·081	29·58	29·32	29·29	56	50	51	44	41	w.	w.	w.	....	....	·24
7.	30·061	30·031	29·66	29·40	29·37	54	35	46	36	35	sw.	w.	w.	....	....	·33
8.	30·172	30·103	29·75	29·53	29·83	45	38	36	35½	35	nw.	w.	nw.	....	....	·24
9.	30·129	30·002	29·70	30·04	30·33	45	30	43	37	36½	nw.	nw.	nw.	·04	....	·12
10.	30·263	30·237	30·00	30·37	30·32	42	32	33	38	38	n.	nw.	sw.	·04	·02	·02
11.	30·273	30·185	30·00	30·10	30·15	40	26	34	38	30½	n.	nw.	nnw.	....	....	·07
12.	30·286	30·157	29·94	30·22	30·32	41	26	26	39	31	ne.	n.	w.	....	....	·09
○ 13.	30·611	30·502	30·24	30·36	30·30	38	17	35	44½	35	e.	nw.	w.	....	....	·11
14.	30·632	30·342	30·28	29·95	29·62	43	30	30	45	30½	sw.	w.	wnw.	·06	....	·13
15.	29·980	29·926	29·66	29·95	29·94	44	29	28	31½	32	ne.	nw.	nw.	....	....	·04
16.	30·025	30·005	29·76	29·66	29·34	42	34	33	37½	44	n.	nw.	nw.	....	....	·13
17.	29·769	29·404	29·35	28·89	28·84	48	26	43	34	29	sw.	w.	nw.	....	....	·19
18.	29·735	29·485	29·16	29·37	29·81	40	27	33	33½	35	nw.	n.	nw.	·08	·03	·07
19.	29·959	29·894	29·68	29·85	29·58	40	25	31	37½	39	n.	w.	sw.	....	....	·05
☾ 20.	30·296	29·908	29·63	29·54	29·96	52	28	38	42½	40½	w.	wsw.	w.	·02	·02	·08
21.	30·263	29·722	30·00	30·03	29·68	49	25	32·5	42	46½	ne.	n.	s.	....	·15	·02
22.	30·247	29·947	29·87	29·48	29·61	47	33	36	46½	39	sw.	ssw.	w.	·06	....	·23
23.	30·612	30·307	30·00	29·93	30·06	49	24	37	40	42	n.	w.	sw.	....	·06	·28
24.	30·502	30·291	30·06	29·46	29·62	47	35	36	48½	38	sw.	wsw.	w.	....	....	·14
25.	30·470	30·395	30·00	29·86	30·30	51	25	41	39½	35	nw.	w.	wnw.	....	....	·22
26.	30·543	30·475	30·20	30·35	29·97	50	23	35	40	43½	n.	n.	sw.	....	....	....
● 27.	30·415	30·321	30·00	29·75	29·69	53	32	40·5	36½	43	w.	wsw.	w.	....	....	·30
28.	30·517	30·285	29·90	29·85	30·04	53	19	44	30	44½	nw.	w.	w.	....	....	·38
Mean.	30·300	30·071	29·83	29·787	29·792	46·00	29·35	36·1	39·94	38·51				0·82	0·28	4·10

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MAY 1854.

XLVI. *Theory of the Electric Residue in the Leyden Jar.* *By* R. Kohlrausch*.

[With a Plate.]

THE solution of an important electrical problem rendered it necessary for me to seek the law of the formation of the electric residue in the Leyden jar. The sine-electrometer described in Poggendorff's *Annalen*, vol. lxxxviii. p. 497, was constructed for this purpose; and having, by means of it and the multiplier described in the present memoir, subjected the facts of the case to a more strict investigation (§ 1 to § 8), it soon appeared (§ 9) that the explanations of the phænomenon hitherto given could lay little claim to accuracy. Another cause for the production of the residue was therefore sought (§ 10), and the consequence was, that a numerical law was discovered (§ 11) which was applicable to practical purposes (§ 12).

The new hypothesis ought, indeed, to be subjected to strict criticism, for such hypotheses, on account of their wide applicability, have something attractive in them, and are therefore to be received with due caution. If, however, the explanation given in § 10 should be rejected, still the facts cited, and the correctness of the calculations, will not be questioned.

§ 1.

When a Leyden jar is charged, a constant diminution of the tension upon its knob is observed. If, in observing this, suitable electrometers be made use of, it will appear that the decrease shortly after charging is much more considerable than after some time has elapsed, and the conjecture arises that the phænomenon

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* From Poggendorff's *Annalen*, vol. xci. p. 56.

must be due to a cause different from the mere loss of the electricity in the surrounding air.

For the decision of this question, it is in the first place necessary to determine accurately the curve according to which the tension diminishes, more especially as this curve is to form the basis of conjectures regarding the existence of causes different from those hitherto assigned. Hence our first step is to make the observations necessary for the construction of such a curve.

If the observations are to be made with purity, care must be taken that the charging of the jar is of momentary duration, and not gradual, so that no uncertainty shall exist as to what occurs during the gradual augmentation of the electricity; an electrometer is also necessary, by means of which the tension may be observed from the moment of charging onwards. The first condition is secured by connecting a jar with another, or with a battery of jars, already charged; and the second is secured by the use of the sine-electrometer, applying the method described in vol. lxxxviii. p. 508 δ. of Poggendorff's *Annalen*.

In order to interrupt the pursuit of the real question as little as possible, I have deferred to Appendix I. the full description of the mode of observation for such a curve, and moreover for the one marked *a* in the table which follows, and state here the results merely; these were obtained with three different kinds of apparatus, *a*, *b*, *c*.

a. An ordinary Leyden jar with tinfoil coatings of about five-sixths of a square foot, and in the form of a sugar-glass.

b. A thick bottle with a narrow, short neck, usually applied to the holding of reagents. The neck was heated, and coated within and without with sealing-wax; the bottle was then filled up to the sealing-wax with mercury, and was surrounded outside by a layer of the same metal, 1 inch thick. To prevent the vessel from floating, it was held fast by means of screws in a cylindrical wooden trough.

c. A thick rectangular piece of a looking-glass had its metallic coating removed for 2 inches from the edge, and was coated on the other side with tinfoil; the glass rim on both sides was coated with sealing-wax. During the experiments the plate was laid with the tinfoil upon a sheet of metal, and to the other side, which was connected with the electrometer, electricity was communicated.

In all three cases, both of the bottles *a*, *b*, and the plate *c*, one of the coatings was carefully connected by a wire with the moist earth of a garden. In the columns marked *a*, *b*, *c*, the results of the observations are stated. In the column headed *t*, the time after charging is expressed in seconds. The column L_t contains the magnitude of the charge as given by the sine-electrometer.

Table *a*.

t.	L_t.
0	0·4742
18	0·4133
50	0·3896
110	0·3692
160	0·3516
215	0·3461
265	0·3373
330	0·3290
382	0·3223
450	0·3141
523	0·3080
577	0·3029
680	0·2951

Table *b*.

t.	L_t.
0	1·4968
5	1·4120
24	1·3221
59	1·2640
91	1·2256
114	1·2060
144	1·1826
188	1·1552
230	1·1326
282	1·1093
341	1·0854
406	1·0608
485	1·0354
573	1·0093
683	0·9823
804	0·9543
935	0·9254
1105	0·8954
1285	0·8643
1505	0·8317
1770	0·7977
2070	0·7622
2430	0·7247
2870	0·6851
3420	0·6429
4110	0·5977
4980	0·5486
5370	0·5266

Table *c*.

t.	L_t.
0	0·5559
6	0·5266
43	0·4918
71	0·4843
133	0·4677
193	0·4566
256	0·4446
328	0·4368
423	0·4268
531	0·4166
620	0·4097
715	0·4027
864	0·3918

The series of observations contained under *b* is represented graphically in fig. 2, Plate VI., by the line L_t. The curves of the two other series of observations are not drawn.

On the whole these curves present a similarity to parabolas, but they satisfy a constant parameter for a short distance only. It is seen, however, at first sight that they cannot be due to the mere loss of electricity in the air, as they are much too curved to be accounted for in this manner. Adhering to the results stated in *b*, and assuming that the loss of electricity is solely due to its dispersion in the air, the charge, which at first amounted to 1·4968 according to the arbitrary unit of the sine-electrometer, being reduced in 5370 seconds to 0·5266, the corresponding curve would be shown by the dotted line N, in which the stronger points are calculated*.

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* This is proved as follows:—Let L_0 be the charge of an insulated conductor at the commencement, then we have

$$dL_t = -\alpha L_t dt,$$

where α is a constant dependent on the state of the air.

Integrating from $t=0$ to $t=t$, we have

§ 2.

The question, to what other source than the loss of electricity in the air is the quick diminution of the tension of the knob of the Leyden jar at the commencement to be ascribed, finds a kind of answer in the fact, that after complete discharge has taken place, a short time having elapsed, a charge more or less considerable, of the same kind of electricity as that with which the jar was originally charged, makes its appearance. From this we may conclude that a portion of the electricity first communicated has concealed itself somewhere, so as not to contribute to the tension of the knob, and was prevented at the same time from being a sharer in the discharge; to this circumstance we might be inclined to refer the more speedy diminution of the tension than that which could be referred to the loss in the air.

Besides this, the following effect is observed. If a charge has remained in the jar for a considerable time, and a considerable portion of the electricity be taken from the jar, so that the tension becomes feebler by sudden springs, then immediately afterwards this tension is observed to sink very slowly, or sometimes to stand for a time and then sink, or, which is mostly the case, first rise for a time, then stand, and then sink finally.

Combining the facts of the last two paragraphs, we are compelled to divide the entire quantity of electricity, Q, in the jar into two portions, one of which, L, can be discharged, and which on this account may be called the *disposable charge*, and the other *r*, which is prevented from sharing in the discharge, and first makes its appearance after the removal or diminution of the

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$$\log \text{hyp} \frac{L_t}{L_0} = -\varkappa t.$$

If by observation we have

$$L_0 = 1{\cdot}4968 \text{ and } L_t = 0{\cdot}5266,$$

then

$$\varkappa = 0{\cdot}0001945;$$

and as, on the other hand,

$$\alpha_t = \alpha_0 e^{-\varkappa t},$$

we can readily calculate the value of the charges for every *t* between the beginning and the end, and lay down the curve represented by the dotted line. In this way we learn that for the times *t* the curve must possess the following ordinates 0.

t.	0.	*t*.	0.
59	1·4797	2070	1·0071
114	1·4640	3420	0·7696
573	1·3389	4110	0·6714
1105	1·2073	5370	0·5266

disposable charge, that is to say, becomes changed into a disposable charge.

This second portion, *r*, has been named the *residue*, and we will retain the term; and by the term *concealed residue*, we will denote the said portion while it remains inefficient in the jar, calling that portion which makes its appearance after the discharge the *restored residue*.

The observation of the restored residue is best accomplished by means of the sine-electrometer, by connecting the knob of the jar with the instrument immediately after the jar has been discharged. It is at once seen that the needle commences to diverge from the magnetic meridian; and by following the needle and reading off the deflections, we obtain a series of points sufficiently near to each other to permit of a curve being constructed which shall represent the augmentation of the charge. The ordinates of this curve increase most speedily at the commencement, so that in this case also the curve is not unlike a parabola: but the greater the loss of electricity in the air, the sooner does a point arrive where this increase passes over into a diminution; this point represents the moment when the exhibition of the residue is exactly balanced by the loss in the air. When the latter, on account of the nature of the jar, is very small, the augmentation continues for several hours; and for hours the charge appears to remain perfectly unchanged, the discharge requiring a number of days to effect it completely. When, however, the quantity lost in the air, or on account of imperfect insulation, is considerable as compared with the size of the jar, the curve, which ascends for a short time abruptly, is suddenly bent downwards. Drawings of such curves are given in fig. 2 ρ.

When the first residue has been discharged, a second small residual curve is observed, which proves that a concealed residue was still remaining; this can be repeated several times.

This repetition of the discharge is the only means of learning how much of a charge has changed itself into a concealed residue *r*. To reduce the loss of electricity during the process to a minimum, a magnetic needle of very small magnetic moment is necessary; and the observations must be made with small angles between the needle and the repelling arm. We first permit as much of the residue to return as can be observed with certainty, then discharge*, read off the angle, and repeat the process as long as a measurable residue remains. Although this process frequently continues for several hours, still, in comparison with the total residue, the loss of electricity is very small. The greater portion of the residue appears very quickly at the commencement of

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[*] It is essential here that the outer coating is perfectly discharged, for otherwise the deflection is illusory.

the operation, so that using small angles, several discharges are sometimes effected during the first minute. When we subsequently reduce the results obtained by making use of the small magnet and small deflections α, to the deflection of the strong needle and large deflections α, made use of in the observation of the original charge, and the sinking of the same, that is, in the construction of the tables a, b, and c, we obtain the residue (which, however, is still to be set free from the influence of the loss of electricity) equal to the sum $\rho+\rho'+\rho''+\ldots\ldots$ of the single residual discharges.

To the tables a, b and c, § 1, belong the corresponding ones a', b', c', in order to form the basis of a calculation. At the time when the last observation recorded in each of the first mentioned tables was made, the apparatus was completely discharged by connecting the inner coating of the jar for about half a second with the moist earth of the garden. At the same time the connecting wire was removed from between the jar and the electrometer, the new magnetic needle was suspended, and the new angle α arranged. The connexion between the jar and the instrument being again established, a second observer noted the times of the single discharges. These are set down in the columns t, and are reckoned in seconds, commencing from the moment of the first discharge. The numbers under ρ' denote the residues which again appeared.

Table a'.

t.	ρ'.
65	0·0443
153	0·0166
270	0·0158
535	0·0138
835	0·0109
1615	0·0109
3895	0·0109
5935	0·0075
Sum ...	0·1307

Table b'.

t.	ρ'.
255	0·1131
990	0·0763
2250	0·0565
5040	0·0538
8760	0·0375
13740	0·0301
37140	0·0207
73140	0·0107
Sum ...	0·3987

Table c'.

t.	ρ'.
34	0·0265
169	0·0101
288	0·0101
479	0·0101
794	0·0101
1234	0·0080
12034	0·0109
Sum ...	0·0858

§ 3.

The question now presents itself, what is it that we have actually measured by the sine-electrometer? According to my conviction, most certainly the same as we should have measured if at the same moments we had tested the same point of the knob. The electrometer in connexion with the interior coating of the jar is, indeed, nothing else than a continuation of the knob.

We have seen, however, in § 1, that the results obtained by the electrometer are by no means proportional to the quantity of electricity which was certainly present, but that, on the contrary, a portion of this electricity became in some measure latent, that is, was lost as far as the tension of the knob was concerned, and we referred the effect to the concealed residue which was formed. It might appear rash to assume without further consideration, that the portion which we saw returning as residue, *r*, after the discharge of the jar, was, during the charging of the jar, altogether without influence upon the tension of the electricity upon the knob. If the residue, as is usual, be referred to the gradual penetration of the glass to a certain depth by the electricity, then it would seem most natural to regard the total quantity of electricity in a jar in which a residue is formed neither as proportional to the tension on the knob of the jar, nor to the disposable charge, but to something made up of both. At all events we must, in the first place, ascertain how the matter really stands.

To decide this question, it is necessary to compare the same tension, measured electroscopically, first of a battery where the concealed residue is as small as possible, and secondly when it contains a large residue, with the disposable charge,—the latter, however, being measured in some other than an electroscopic way. In such a case we may make use either of the thermic or magnetic action of the current. The latter was the action chosen.

In the Appendix II. the small multiplying galvanometer is described which served for this purpose, its magnetic needle, furnished with a mirror and scale, being observed through a telescope like a magnetometer; and in the same place also the arrangement is described by which the inner and outer coatings of the battery, at the suitable moment determined by the sine-electrometer, are connected with the galvanometer wire for a time sufficient to allow the disposable charge to pass, but not sufficient to permit any measurable portion of the residue to make its appearance.

When the current of a Leyden jar or battery, retarded by the introduction of a liquid into the circuit, is sent through such a galvanometer, and when the discharge, as in the case before us, is accomplished in a small portion of a second, consequently in a very small fraction of the time of oscillation of the needle, the action of the current may be regarded as an instantaneous shock, which imparts suddenly to the needle a certain velocity of rotation, C. This velocity is proportional to the strength of the shock, and hence, in the present case, to the disposable charge sent through the galvanometer. Now W. Weber, in his Electro-dynamic Measurements, page 316, has given a formula, according to which, in the case of a galvanometer furnished with a damper, the velocity C may be determined from the first elongation x of the needle. The formula is

$$C = x \cdot \frac{\pi}{T} \cdot e^{\frac{\lambda}{\pi} \operatorname{arc\,tan} \frac{\pi}{\lambda}}$$

For the same galvanometer the time of oscillation T of the needle and the logarithmic decrement λ are constant, and hence also the magnitude of the disposable charge of the battery is proportional to the first elongation x of the needle; which elongation, on account of its smallness, may be regarded as proportional to the number of parts of the scale read off by the telescope. The observations were as follows.

A battery having about three square feet of coating, in which no residue existed, was charged, the sine-electrometer was introduced, and at the moment when the latter gave the charge 2·673 according to its own arbitrary unit, the discharge through the galvanometer was effected. The first elongation amounted to 44·0 parts of the scale. Between the charging and discharging a minute and a half intervened, and the concealed residue collected afterwards amounted to 0·14. The repetition of this experiment gave for the first elongation, in a case where the needle was not completely at rest, 43·8, and in a subsequent case 44·1 parts of the scale. In both of these last-mentioned cases the residue was not determined.

The highly charged battery now remained for fifty minutes without being discharged, at the end of which time the sine-electrometer showed the charge 2·673, and was then discharged. Three such experiments gave for the first elongation of the needle 44·0, 44·1, and 44·1 parts of the scale.

Thirdly, a strong charge was permitted to remain standing in the battery for 210 minutes, was then considerably weakened in order to bring the charge down to 2·673, and afterwards gave 44·0 parts of the scale for the first elongation. In a short time a concealed residue of 0·754 made its appearance, and the measurement of the remaining portion of the residue, which, according to estimation, would have given about twice the quantity, was interrupted, inasmuch as the experiment was sufficiently decisive. As therefore the concealed residue appeared to exert no influence whatever, it was not at all taken into account in the following experiments. Another battery was taken, and the action of its discharge under different circumstances ascertained. In the first column of the following little table are contained the deflections ϕ of the needle of the sine-electrometer; in the second the square root of the sine of this angle, which from the nature of the instrument is proportional to the charge, but in this case not reduced to the ordinary unit of the instrument; in the third and fourth columns, stands the observed first elongation E of the galvanometer needle in parts of the scale; in the fifth the number of parts of the scale calculated from the strongest charge, as they must have been if the results given by both instruments had completely coincided; and in the sixth column the difference *d*, given in parts of the charge.

ϕ.	$\sqrt{\sin\phi}$.	E.	Mean.	Calculated.	*d*.
1° 18′*	0·1505	10·8	10·8	10·85	−0·0046
8 45	0·3900	28 27·9 27·8 28	27·92	28·12	−0·0071
26 57	0·6732	48·5 48·5	48·5	48·53	−0·0006
79 15	0·9911	73 72·5	72·75	72·75	0

These experiments leave no doubt of the fact, that the disposable charge of the battery is truly given by the sine-electrometer, and hence that *the disposable charge of the battery is proportional to the tension of the electricity on the knob of the jar.* These experiments were made with Franklin's plate with the same result.

§ 4.

Having thus assured ourselves that the sine-electrometer enables us accurately to compare the disposable charges of the same jar with each other, the instrument may be applied to the measurement of these discharges if we assume one of them as unit; the electrometer has its own unit of measure. As stated in Poggendorff's *Annalen*, vol. lxxxviii. p. 502, it is the quantity of electricity which, when the repulsion of the magnetic needle by the repelling arm amounts to a certain small angle α, causes the needle to stand at right angles to the magnetic meridian; and to this unit all results of the instrument, when different needles and different angles of observation are made use of, are reduced. Let us choose, then, as unit, according to which the disposable charge of any jar is to be measured, the quantity of electricity which must be present in order to cause the electrometer to exhibit its own unit; then the numerical results of the latter serve immediately as the measure of the disposable charges for this particular jar†.

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* This charge consisted of a residue which had again made its appearance.

† It is here manifest that an absolute measure is not attained; for,

The curves obtained from the observations in the tables *a, b, c,* § 1, are therefore the actual representants of the disposable charges L_t present at the different times t in the apparatus, and the residues ρ' sought in the tables a', b', c', § 2, denote also quantities of electricity so circumstanced that a and a', b and b', and c and c', are referred to the same unit respectively*.

The first question which comes under our consideration is, whether means are at hand which will enable us to obtain from the observations a clear view as to the comparative quantities of the residues formed in the charged jar. For the sinking of the charge has, as we have observed, two causes; the loss of electricity in the air, and the formation of the residue. If the quantity of electricity Q_0 be imparted to the interior coating of the jar, which at the first moment shows the charge $L_0 = Q_0$, then at the end of the time t the quantity v_t is lost in the air†, and a concealed residue r_t is formed, so that the disposable charge at present is only

$$L_t = Q_0 - v_t - r_t;$$

while the quantity of electricity Q_t in the jar at the end of the time t is expressed by the equation

$$Q_t = L_t + r_t = Q_0 - v_t.$$

The observations gave us Q_0 and L_t directly, the latter at least for the times t at which the disposable charge was observed; and the question now is, whether we can determine v_t so that the residue for all observed times t may be given by the equation

$$r_t = Q_0 - (L_t + v_t).$$

This leads us to the construction of a residue-curve, the nature of which we shall be enabled to recognize; and thus the hope is

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according to what has been said, every distinct jar will have its own peculiar unit. If, therefore, the sine-electrometer is to be made use of to compare the charges of different jars, not the tensions on the knobs, but the quantities of electricity actually present, it is in the first place necessary to ascertain the ratio of the quantities assumed as units in the two jars. This can be found with certainty by first causing the electrometer to show the same divergence in both cases, and then discharging the jars through the multiplying galvanometer.

* More was not necessary for our present purpose. The three different units referred to in the tables *a, b, c* might indeed be reduced to each other. Two other tables besides those to be used subsequently must for this purpose be calculated, and the advantage did not appear to be worth this trouble.

† We intentionally avoid taking into account the loss of electricity by the imperfect insulation of the edge of the jar, because otherwise we could not set the loss as proportional to the charge L_t. The charge Q_0 should therefore be not taken greater than the validity of this assumption would permit of.

generated of our being able to arrive at a strict law, the coincidence of which with the observations will render probable the hypotheses on which the said law is founded.

Let us in the first place determine v_t.

In fig. 3 let *ab* represent the quantity of electricity $Q_0 = L_0$ communicated at the beginning; $b\,b'\,b''\,b''' \dots c$ the curves of the disposable charges L_t observed at numerous points, lying near each other, and referred to the abscissa *ad*, which is the representant of the time. This is reckoned from the moment when the jar was charged, and we have therefore after the times t_1, t_2, t_3, the disposable charges L_1, L_2, L_3. After the time T, which is expressed by the length *ad* in the figure, the jar will be discharged, consequently a quantity of electricity L_T corresponding to the line *dc* will have been removed.

At the end of the times $t' = da'$, $t'' = dd''$, $t''' = dd'''$, &c., the quantities ρ', ρ'', ρ''', &c., as concealed residues, make their appearance, the sum of which is

$$\rho' + \rho'' + \rho''' + \dots = \rho.$$

We know that we have recovered the quantity $L_t + \rho$ from the jar, and hence conclude that what is wanting to make up the original charge Q_0, has been lost in the air during the time of operation; calling this last portion V, we have

$$V = Q_0 - (L_T + \rho).$$

If in the figure $ef = \rho$, we have $fc = V$.

This total loss must now be distributed over the times in which it has taken place; more especially for the determination of r_t, it is necessary to inquire what portion v_t has been lost up to the particular times t, at which the disposable charge of the jar has been measured.

This charge is constantly proportional to the loss of electricity, consequently

$$dv_t = \alpha L_t dt;$$

and the integration of the expression to the right between the limits $t = 0$ and $t = t$ would, if we could perform it, and knew the value of α and the law of the curve L_t, at once give us the magnitude of v_t.

Now, in the first place, it is manifest that the constant α which has reference to the loss of electricity in the case of a Leyden jar, cannot be directly determined; secondly, that we have no hope of finding the law of the curve expressing the disposable charges, before we know the law of the residue in the jar. Inasmuch as v_t, L_t, and r_t are all dependent on each other, the solution of the problem appears only possible by finding at once the laws of all three curves. But assuming even that we possessed this gift of divination, we should nevertheless find the differential equation so complicated that its integration is scarcely to be thought of.

Under such circumstances we must make a virtue of necessity, and in seeking to determine v_t, we must take, instead of the actual law of the curve of the disposable electricity, the curve itself.

The expression
$$\int_0^t \mathrm{L}_t dt$$
is nothing else than the superficial area bounded by the curve, the abscissa, and the ordinates at the beginning and end of the time t, and we see immediately that the loss of electricity during the respective times
$$t_1-t_0,\ t_2-t_1,\ t_3-t_2,\ \&c.,$$
which latter are represented in the figure by the portions aa', $a'a''$, $a''a'''$, &c. of the abscissa, are proportional to the spaces
$$ab\,b'a'=\int_0^{t_1}\mathrm{L}_t dt=f_1;\quad a'b'b''a''=\int_{t_1}^{t_2}\mathrm{L}_t dt=f_2,\ \&c.$$
Denoting by F the total surface $abb'b''\ldots cd$, so that
$$\mathrm{F}=\int_0^{\mathrm{T}}\mathrm{L}_t dt=f_1+f_2+f_3+\ldots,$$
then we have $\alpha\mathrm{F}$ as the loss of electricity up to the first discharge of the battery. In like manner, the loss in the second operation is proportional to the surfaces
$$dg'd'=f';\quad d'g''d''=f'',\ \&c.$$

Denoting the entire sum of these spaces $f'+f''+f'''+\ldots$ by ϕ, then the total loss of electricity during the second operation will be expressed by $\alpha\phi$.

Hence as
$$\mathrm{V}=\alpha\mathrm{F}+\alpha\phi,$$
we obtain immediately
$$\alpha=\frac{\mathrm{V}}{\mathrm{F}+\phi}.$$

V is given to us by observation, and thus our knowledge of α depends solely on our knowledge of F and f.

If the measurements made at the times t_1, t_2, t_3, &c. lie so nearly together that the portion of curve which they limit may be regarded as a straight line, the surfaces f_1, f_2, &c. become trapeziums, and we have
$$f_1=\frac{\mathrm{L}_1-\mathrm{L}_0}{2}(t_1-t_0);\quad f_2=\frac{\mathrm{L}_2-\mathrm{L}_1}{2}(t_2-t_1),\ \&c.,$$

the sum F of which may be found, and will differ very little from

$$\int_0^{\mathrm{T}} \mathrm{L}_t dt.$$

The surfaces $f', f'', f''' \ldots$, which are bounded by the curves of the residues, may be regarded as triangles, so that we have

$$\phi = \frac{1}{2}\left[\rho'(t'-t^0) + \rho''(t''-t') + \ldots\right].$$

In this we obtain, instead of

$$v_t = \alpha \int_0^t \mathrm{L}_t dt,$$

the equation

$$v_t = \frac{\mathrm{V}}{\mathrm{F}+\phi} \cdot \Sigma^t{}_0(f),$$

where

$$\Sigma^t{}_0(f) = f_1 + f_2 + f_3 \ldots f_1,$$

which contains no unknown quantity, when for t is substituted the actual time of observation.

We can thus calculate for all observed disposable charges L_t, the loss v_t incurred up to the time of discharge, and have at the same time, according to the former equations, the corresponding concealed residues r_t and the total quantities Q_t which exist in the jar at the respective times. In the three following tables, a'', b'' and c'', the quantities so calculated are placed side by side with the former ones.

Table a''.

t.	L_t.	v_t.	Q_t.	r.
0	0·4742	0	0·4742	0
18	0·4133	0·0013	0·4729	0·0596
50	0·3896	0·0033	0·4709	0·0813
110	0·3692	0·0069	0·4673	0·0981
160	0·3516	0·0097	0·4645	0·1084
215	0·3461	0·0128	0·4614	0·1153
265	0·3373	0·0155	0·4587	0·1214
330	0·3290	0·0189	0·4553	0·1263
382	0·3223	0·0216	0·4526	0·1303
450	0·3141	0·0250	0·4492	0·1351
523	0·3080	0·0286	0·4456	0·1396
577	0·3029	0·0312	0·4430	0·1401
680	0·2951	0·0360	0·4382	0·1421

Table b''.

t.	L_t.	v_t.	Q_t.	r_t.
0	1·4968	0	1·4968	0
5	1·4120	0·0009	1·4959	0·0839
24	1·3221	0·0040	1·4982	0·1707
59	1·2640	0·0094	1·4874	0·2234
91	1·2256	0·0141	1·4827	0·2571
114	1·2060	0·0175	1·4793	0·2727
144	1·1826	0·0218	1·4750	0·2924
188	1·1552	0·0279	1·4689	0·3137
230	1·1326	0·0337	1·4631	0·3305
282	1·1093	0·0406	1·4562	0·3469
341	1·0854	0·0484	1·4484	0·3630
406	1·0608	0·0567	1·4401	0·3793
485	1·0354	0·0666	1·4302	0·3948
573	1·0093	0·0774	1·4194	0·4101
683	0·9823	0·0905	1·4063	0·4240
804	0·9543	0·1045	1·3923	0·4380
935	0·9254	0·1192	1·3776	0·4522
1105	0·8954	0·1377	1·3591	0·4637
1285	0·8643	0·1566	1·3402	0·4759
1505	0·8317	0·1789	1·3179	0·4862
1770	0·7977	0·2047	1·2921	0·4943
2070	0·7622	0·2327	1·2641	0·5019
2430	0·7247	0·2647	1·2321	0·5074
2870	0·6851	0·3018	1·1950	0·5099
3420	0·6429	0·3455	1·1513	0·5094
4110	0·5977	0·3967	1·1001	0·5024
4980	0·5486	0·4563	1·0405	0·4919
5370	0·5266	0·4814	1·0154	0·4888

Table c''.

t.	L_t.	v^t.	Q_t.	r_t.
0	0·5559	0	0·5559	0
6	0·5266	0·0005	0·5554	0·0288
43	0·4918	0·0034	0·5525	0·0607
71	0·4843	0·0055	0·5504	0·0661
133	0·4677	0·0101	0·5458	0·0781
193	0·4566	0·0144	0·5415	0·0849
256	0·4446	0·0188	0·5371	0·0925
328	0·4368	0·0237	0·5322	0·0954
423	0·4268	0·0301	0·5258	0·0990
531	0·4166	0·0372	0·5187	0·1021
620	0·4097	0·0429	0·5130	0·1033
715	0·4027	0·0489	0·5070	0·1043
864	0·3918	0·0581	0·4978	0·1060

To make these comparative quantities evident to the eye, the table b'' is represented graphically in fig. 2. Setting, for instance, by the observed L_t the corresponding v_t, then the points thus obtained determine the residue-curve denoted by r_t, the ordinates of which, however, have the line *be* as abscissa. Setting, on the contrary, by the observed L_t the corresponding r_t, we obtain a series of points, which, on being united, constitute the line Q_t or v_t, which is almost straight. The ordinates of this line, referred to *ad* as abscissa, represent the quantity Q_t of electricity present in the jar at the time t; and, when they are referred to *be*, they represent loss of electricity v_t.

§ 5.

The question now naturally presents itself, in what relation does the concealed residue of the same jar stand with respect to the magnitude of the charge first communicated; for it is soon evident that a stronger charge produces a greater residue. Whether the residue produced in equal times, but with different charges, be proportional to the strength of these charges, may be solved by ascertaining whether the sinking of the disposable charge follows the law of proportionality; for the air, retaining its quality, the electric loss from this source has this property. On this point a single decisive experiment may be cited.

The method described in Appendix I. of communicating to a jar instantaneously a known charge, may be easily so applied that the charge in one case is exactly ten times what it is in another case. This was done on two successive serene days, during which the warmed air of the room remained in a constant condition as regards the loss of electricity, a jar being chosen with which the loss was inconsiderable. The results are stated in the following small tables *d* and *e*. In table *d′* the results given in *d* are reduced to the times of table *e*.

Table *d*.		Table *e*.		Table *d′*.
t.	L_t.	t.	L_t.	L_t.
0	0·1406	0	1·4062	0·1406
20	0·1368	60	1·3586	0·1352
65	0·1349	120	1·3374	0·1334
135	0·1329	180	1 3220	0·1322
270	0·1308	300	1·3087	0·1305
390	0·1296	420	1·2962	0 1293
600	0·1274	540	1·2858	0·1280
720	0·1268	720	1·2737	0·1268
900	0·1255	900	1·2583	0·1255

The same experiment has been repeated several times with the same degree of coincidence.

We have thus arrived at the conclusion, *that with the same jar, the residues formed in the same time are proportional to the charge imparted at the commencement.* It must be left undecided whether the conclusion is strictly true for the strongest charges, as on account of the loss through imperfect insulation these cannot be examined in the manner which we have pursued.

Before we make the attempt to discover a formula for the calculation of the residue-curve, a few facts must be introduced which may be useful to enable us to form a clear and correct conception with regard to the residue, and thus prepare the way for the hypothesis upon which the formula is to be based.

[To be continued.]

XLVII. *On some Tubular Cavities in the Coralline Crag at Sudbourn and Gedgrave near Orford.* By Searles V. Wood, *Esq.*[*]

[With a Plate.]

It has been long observed that the surface of the chalk is more or less perforated with funnel-shaped depressions or "sand-pipes," as they are called in England, while they are known in France under the name of "puits naturels;" and in the year 1839, Sir Charles Lyell published a paper in the Philosophical Magazine, in which was brought under notice these sand-pipes, with an account of the manner of their supposed production. These pipes are therein described as varying in magnitude as well as in depth, and are also of various forms and inclinations, but are all of an angular or funnel shape, being larger at the upper part and generally terminating in a point: the mode by which it is therein supposed these excavations have been effected is by the operation of a chemical action upon the calcareous material of the mass of the chalk, the eroding action of acidulated waters, produced by the decomposition of vegetable matter, abstracting the carbonate of lime which had passed off into the atmosphere in the form of carbonic acid gas, and the depression or opening thus produced to have been gradually filled in by the weight of the superincumbent gravel. This hypothesis has been called in question by Mr. Trimmer, who in a paper read before the Geological Society in 1842, and published in their Proceedings, vol. iv. part 1. p. 6, and part 3. p. 482, 1844, has assigned the production of these depressions to the action of running waters which had eroded in furrows the surface of the chalk, and he considers their funnel-shaped appearance to be simply a transverse section of these gullies. It is, however, most probable that the surface of the calcareous rocks beneath the superficial gravel has been in various places acted upon both by a chemical agent as well as by mechanical action;

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[*] Communicated by the Author; having been read before the Geological Section of the British Association, at Ipswich, 1851.

and that the hypothesis proposed by Sir Charles Lyell is the true one for the funnel-shaped depressions, while there is also no doubt that the surface of the chalk has in some places been eroded as described by Mr. Trimmer.

The cavities immediately below the superficial soil on the eastern side of England might, I think, be classed under three distinct heads, as they are not only the presumed productions of different agents, but also the result of actions very dissimilar in their operations. The first are the well-known "*puits naturels*," which from their peculiar forms may for mere distinction be called *funnel pipes*; the second may be termed *gully pipes*; these, however, are not, strictly speaking, pipes at all, as they all terminate in a closed point; and the third more correctly *chimney pipes*.

The first or funnel pipes are more or less of an angular form, irregular in outline, and where it is possible to ascertain their full extent, they invariably terminate in a point. These are not only common on the surface of the chalk, but are very numerous, though generally small in size, upon the coralline crag; and in the parishes of Sudbourn and Gedgrave, wherever a section of this formation is visible, the whole surface is more or less excavated in this manner, and none that I could see exceeded in depth 3 or 4 feet, while 12 or 18 inches was the diameter of the upper part. The larger and deeper pipes of the chalk may be due to the erosive action having been prolonged through a much greater period; and in examining the contents of two or three of these excavations, which were filled with the superincumbent gravel, not a single fragment of shell could be observed, or any trace of carbonate of lime, which might have been the case had they resulted simply from the action of running waters; moreover, the edges of these depressions were too sharp in a material so loose and incoherent as is the coralline crag, and their positions too proximate for the probable action of any other agent than the slow and gradual process of a chemical abstraction acting from above.

The second class, or gully pipes, are described by Mr. Trimmer as having been formed by the action of running waters draining the surface of the chalk previous to its elevation above the sea, and the angular depressions visible in that rock, and now filled with gravel, are merely the transverse sections of these gullies; and he further states his having removed for a considerable space the superincumbent material and exposed the continuance of these depressions. Another form of excavation may also here be mentioned as being more in connexion with this last mechanical mode of operation, and is spoken of by Lieut. Newbold as occurring in southern India, wherein he describes the surface of the rock as eroded into cup-shaped cavities produced by the action of running waters, whose eddies or gyrations have by the trituration of sandy particles worn them into a subglobular form. These poteria or cup-shaped cavities are spoken of by Mr. Trimmer (p. 498) as having been observed by him on the surface of the chalk at Thorpe and Rackheath in Norfolk, as also in Kent: these he considers as incipient pipes, and they are presumed to be identical with those rock basins described by Lieut. Newbold in the Proceedings of the Geological Society, vol. iii. p. 702. Mr. Jukes also, in his account of Newfoundland, vol. ii. p. 138, mentions similar excavations close to Buchan's Island in the river Exploits, and particularizes two holes worn in the rock of a form somewhat singular, being perfectly perpendicular to the depth of 8 feet, with a diameter of about 2 feet; he says they were exactly circular, nearly full of water, and each contained a large stone and a quantity of sand at the bottom, and these were so produced by the whirling round of the stone from the rapidity of the current of water rushing over them.

The third class are what I propose to call chimney pipes, and are more especially the subject of the present notice; their form, as well as the mode by which it is presumed they were produced, being wholly different from the before-mentioned excavations, they appear to be deserving of a particular designation.

In a quarry of the coralline crag at Sudbourn near Orford, close to the gate which leads to the mansion of the Marquis of Hertford, are a number of these "pipes;" not less than nine were visible within the space of twenty yards when I visited the pit, all nearly of the same size, and excavated perpendicularly or nearly so, one only appearing to decline a little from that direction: the general dimensions of these pipes were about 18 or 20 inches in diameter. And in another pit in the parish of Gedgrave, at the distance of about a mile due south of this, may be seen also eleven more of these perforations, one of which is much larger than the others, with a somewhat different form (Plate V.)*.

The coralline crag in this locality consists of a sort of low table-land, bounded or rather cut off on the N.E. by the river Alde, and extending to the S.W. about five miles, where it is again cut out by the small river that runs up to Butley, having a transverse direction of about two miles and a half with a variable thickness; and a depth of 50 feet is reported to have been pierced through without coming to the clay. Wherever a section of this is visible, it appears to have formed on the upper part a complete coral bank, beneath which, as seen in the valleys

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* The Plate is from a sketch of some years date, and the pit at that part has since been worked, by which a considerable alteration is made.

where it has been denuded, is a deposit composed almost entirely of shells and their *débris;* while in the upper portion shells are rarely to be obtained, and only in the valleys corresponding with the present watersheds can this lower stratum be seen. The upper or coralline portion has here become somewhat indurated, and on some occasions has been employed as a building stone, the tower of Chillesford Church having been constructed principally of that material.

These chimney pipes appear to have perforated the entire thickness of the coralline crag deposit, at least it is so presumed, although I was unable to trace them to a greater depth than 12 or 14 feet from the surface, or immediately beneath the gravel to where they are lost in the talus at the bottom of the pit; but the crag at these localities has not probably more than 20 feet of thickness, as the water stands in the pit at that depth, indicating the presence of the clay beneath.

The most perfect of these tubes now remaining is one that presents rather more than a semicylindrical shape, but is emptied of its contents by the loss of the lateral supports: a small portion of another, the walls of which are as flat and nearly as smooth as the walls of a room, has been undermined so as to be visible from beneath; and through this aperture, which has a diameter of about 18 or 20 inches, a boy might readily pass. The large masses of Fascicularia and Theonoa, which in the ordinary surface of the crag stand out in prominent relief, were in these chimney pipes literally cut in halves, as if they had been produced by the mechanical operation of a boring implement.

From the cylindrical form of most of these tubes and their vertical position it is presumed they cannot be ranged in the second class, as it is quite impossible they can have been produced by running water; and their perfect regularity precludes any intimate connexion with the funnel-shaped pipes from the downward action of acidulated waters so removing the carbonate of lime, as in that case they must necessarily have an irregular outline. So large a number of them within so small an area militates against the probability of their having been produced by human agency, which the sight of a solitary one might have led to the belief; their great proximity would have rendered the repetition of such an operation useless, or at least very improbable, and the form of one of them being somewhat elliptical is an evidence against the probability of such a production.

That the erosion is effected by chemical action, and not by mechanical means, there is every reason to believe from the regularity of form and perfect smoothness of the sides of these cylinders; for in a mass of deposit composed of a homogeneous material, or nearly so, as regards its chemical components, though not all in the same state of compactness or density, the acid would act alike upon the more indurated as upon the less solid material, whereas a mechanical agent would be directed to the place of least resistance; and had the looser fragments then been removed the nodules of compact corals would have projected from the sides of the tubes. The whole aperture has a lining or coating upon its walls such as might be expected as the residuum adhering to the sides deposited by the evaporation of the carbonic acid gas: this covering or lining of the sides of the chimney is of an ochreous colour and of an aluminous composition, and may perhaps have been thicker than what now remains, as exposure to the rain and weather would leave only a small portion adhering to the crag.

One cause to which Sir Charles Lyell alludes regarding the possibility of his funnel-shaped tubes having been formed by the large tap roots of trees that had decayed away, and the aperture so left afterwards filled up by the falling in of the superincumbent gravel, could not be applicable as a cause for the production of these chimney pipes, which are much too regular in form to have been so produced.

In some parts of the coralline crag at Ramsholt I have met with several caves or openings, such as might have been produced by the subsidence or contraction of the clay beneath. One of these was so small as to be not more than a foot or 18 inches in height, such as would scarcely admit a man to crawl in, with a width only of a few feet: another somewhat larger, near the same locality, has been in former times used by smugglers as a place of retreat. These fractures seem to have been near the bottom of the crag, which at this place has a thickness of not more than 6 or 8 feet; the upper or more coralline portion has been washed off and the red crag is superimposed, while the London clay is visible immediately beneath, showing portions of all three deposits above the level of the river Deben. These cavities of a distant locality in the coralline crag are here mentioned as indicating the probability that similar fractures and openings exist in various other parts.

There is every reason to believe that these gravel-filled cavities were not eroded from above, either chemically or mechanically, but that the openings under notice were formed by the evolution and upward issue of acidulated gases. To produce this effect, it is necessary to suppose these tubes to have been connected with cavities or fissures in or through the clay beneath where the carbonic acid or other gases might have been elaborated. The regular and tranquil deposits of these older tertiary clay beds, as seen in England, give no indication whatever of any proximity to volcanic action even in its most subdued form; and the production of these gases must be accounted for, either by the supposition of their having risen from regions of considerable depth through the clay out of rocks of an igneous character beneath, or that some acid gas may have been formed in the clay itself by the decomposition of pyrites which is found in great abundance in that deposit; in either case it would be necessary, I should imagine, for cavities to have existed in the clay either for the passage of the gas or for its elaboration; and although no appearance exists of anything like disturbance in this neighbourhood, it may be remembered that the sea now approaches within a mile of the *locus in quo*, and all traces of such action may have been removed.

In speaking of the "sand-pipes" of the chalk, Sir Charles Lyell says, "If some of the largest pipes, of which the bottom has not been yet reached, be prolonged indefinitely downwards and connected with deep fissures, we may suppose that springs charged with carbonic acid rose up at some former period through the chalk and crag while these were still submerged. In proportion as the chalk was corroded, the incumbent substances would subside into the hollows thus formed, and the water would freely percolate, the matter thus intruding itself dissolving any calcareous ingredients which may be associated with it, and still continuing to widen the tube by corroding its walls."

Our chimney pipes were probably formed, as above stated, while the crag was beneath the sea, although I was unable to ascertain what were the true contents of these openings when first broken into. The form of one of these being of a funnel shape and opening outwards, would seem rather to imply a different mode of action from the other more perfectly formed cylinders in close proximity, bearing in that character a resemblance to the funnel-pipes of the general surface. I am inclined, however, to believe this tube is continued downwards, of a cylindrical form, and produced in the same manner, the enlargement of the upper part having been caused by some impediment to the ready and regular escape of the gas, retaining it near the surface, where it has in consequence exerted a more destructive force, the erosive power diminishing downwards, causing the angular inclination of the sides.

These pipes seem to differ from those of the Geysers in Iceland, or others that have been formed by water in various parts of the world in the immediate neighbourhood of volcanoes, inasmuch as from the fragile nature of the crag rock, the narrow partition between these pipes would have been ruptured by the upward action of water impelled through them with any degree of violence; and the more probable solution appears to be the quiet action of carbonic acid gas evolved in an upward direction; and the gas thus produced had its origin deep seated beneath the London clay, showing that volcanic force, although in a moderate degree, has exerted an influence in this locality since the deposition of the coralline crag bed. Whether these fumeroles or mofettes were formed only at the period immediately antecedent to the present epoch, is, perhaps, difficult to determine; they might have existed during the latter portion of the time when this bank was under formation, before it was ever elevated above the sea: I am inclined to attribute them to the time when the gravelly and sandy deposit of the red crag overlaid this coral bank before the face of the country was modified into its present form. They are, however, here brought forward to show the great probability that these calcareous rocks have been acted upon by gases rising from below as well as by acids in a downward direction; and although the surface of the chalk, as well as the crag, is corroded in most places, denoted by the funnel-shaped depressions beneath the superficial soil, there have not been recorded, that I am aware of, such marked evidences of upward action in any other rock in this country from the cretaceous period to the present time.

XLVIII. *On some early Experiments in Photography, being the substance of a Letter addressed to* Robert Hunt, *Esq. By the* Rev. J. B. Reade, *M.A., F.R.S.*

Stone Vicarage, Aylesbury,
February 13, 1854.

My dear Sir,

In giving you the information you require respecting my early researches in photography in 1836 and following years, I may assume that you are already aware, from my letter to Mr. Brayley of March 9, 1839, and published in the British Review for August, 1847, that the principal agents I employed, before Mr. Talbot's processes were known, were infusion of galls as an accelerator, and hyposulphite of soda as a fixer.

I have no doubt, though I have not a distinct recollection of the fact, that I was led to use the infusion of galls from my knowledge of the early experiments by Wedgwood. I was aware that he found *leather* more sensitive than *paper*; and it is highly probable that the tanning process, which might cause the silver solution to be more readily acted upon when applied to the leather, suggested my application of the tanning solution to paper.

In your own history of the photographic process, you say "the discovery of the extraordinary property of the gallic acid in increasing the sensibility of the iodide of silver was the most valuable of the numerous contributions which Mr. Talbot has made to the photographic art." It is nevertheless true, as stated by Sir David Brewster, that "the first public use of the infusion of nut-galls, which is an *essential element* in Mr. Talbot's patented process, is due to Mr. Reade," and in my letter to Mr. Brayley I attribute the sensitiveness of my process to the formation of a gallate or tannate of silver. I need scarcely say, that among various experiments I tried gallic and tannic acid in their pure state, both separately and mixed; but the colour of the pictures thus obtained with the solar microscope was at that time less pleasing to my eye, than the rich warm tone which the same acids produced when in their natural connexion with solutions of vegetable matter in the gall-nut. This organic combination, however, was more effective with the solar microscope than with the camera, though the lenses of my camera were five inches in diameter. It is probable enough that the richer tone was due to the greater energy of direct solar rays. In using the solar microscope, I employed a combination of lenses which produced a convergence of the luminous and photogenic rays, together with a dispersion of the calorific rays, and the consequent absence of all sensible heat enabled me to use Ross's cemented powers, and to make drawings of objects inclosed in Canada balsam, and of living animalcules in single drops of water. The method I employed was communicated to the Royal Society in December 1836, and a notice of it is contained in the 'Abstracts.'

You inform me that some persons doubt whether I really obtain *gallate of silver* when using an infusion of gall-nuts, and that one of Mr. Talbot's friends raises the question. It is sufficient to reply, that though gallic acid is largely formed by a long exposure of an infusion of gall-nuts to the atmosphere, as first proposed by Scheele, yet this acid does exist in the gall-nut in its natural state, and in a sufficient quantity to form gallate of silver as a photogenic agent; for M. Deyeux observes, that "when heat is very slowly applied to powdered gall-nuts, gallic acid sublimes from them, a part of which, when the process is conducted with great care, appears in the form of small white crystals." M. Fiedler also obtained gallic acid by mixing together a solution of gall-nuts and pure alumina, which latter combines with the tannin and leaves the gallic acid free in the solution; and this solution is found, on experiment, to produce very admirable pictures. But what is more to the point, Mr. Brayley, in explaining my process in his lectures, showed experimentally how gallate of silver was formed, and confirmed my view of the sensitiveness of the preparation. It is therefore certain that the use of gallate of silver as a photogenic agent had been made public in two lectures by Mr. Brayley at least two years before Mr. Talbot's patent was sealed.

I employed hyposulphite of soda as a fixer. Mr. Hodgson, an able practical chemist at Apothecaries' Hall, assisted me in the preparation of this salt, which at that time was probably not to be found, as an article of sale, in any chemist's shop in London. Sir John Herschel had previously announced the peculiar action of this preparation of soda on salts of silver, but I believe that I was the first to use it in the processes of photography. I also used iodide of potassium, as appears from my letter, as a fixer, and I employed it as well to form iodide of lead on glazed cards as an accelerator. Iodide of lead has of itself, as I form it, considerable photographic properties, and receives very fair impressions of plants, lace, and drawings when placed upon it, but with the addition of nitrate of silver and the infusion of galls the operation is perfect and instantaneous. Pictures thus taken were exhibited at the Royal Society before Mr. Talbot proposed his iodized paper. The microscopic photographs exhibited at Lord Northampton's in 1839 remained in his lordship's possession. I subsequently made drawings of sections of teeth; and one of them, a longitudinal section of a tooth of the *Lamna*, was copied on zinc by Mr. Lens Aldous for Owen's 'Odontography.' I may say this much as to my own approximation to an art, which has deservedly and by universal consent obtained the name of Talbotype.

Sir David Brewster, in his 'History of Photography,' passes immediately from the experiments of Wedgwood to those of Talbot; but the Transactions of the Royal Society, to which my friend Mr. Gravatt has directed my attention, will enable us to insert, if not a chapter, at least a very pregnant parenthesis. The Bakerian Lecture, in 1808, by Dr. Young, who never touched a subject without leaving his mark upon it, contains a highly interesting and original experiment on the photographic representation of the invisible chemical rays beyond the blue end of the spectrum. This experiment does not happen to be recorded in the first edition of your 'Researches on Light;' but no one will refer to it with greater pleasure than yourself, not only because it is the first photographic analysis of the spectrum, but also because it has the higher merit, even as it stands alone, of being the one sufficient fact which establishes the consummation so devoutly looked for, at the conclusion of your work, from the perservering accumulation of facts only; for it is in itself a simple and demonstrative proof, to use the words of Dr. Young, of the general law of interference, and, in your own words, "reconciles the chemical action of the photographic force, *energia*, with the undulatory theory of *light*." Dr. Young's experiment forms the conclusion of his lecture, and is given in the following terms:—"The existence of solar rays accompanying light more refrangible than the violet rays, and cognisable by their chemical effects, was first ascertained by Mr. Ritter; but Dr. Wollaston made the same experiments a very short time afterwards, without having been informed what had been done on the Continent. These rays appear to extend beyond the violet rays of the prismatic spectrum, through a space nearly equal to that which is occupied by the violet. In order to complete the comparison of their properties with those of visible light, I was desirous of examining the effect of their reflexion from a thin plate of air capable of producing the well-known rings of colours. For this purpose I formed an image of the rings, by means of the solar microscope, with the apparatus which I have described in the Journals of the Royal Institution, and I threw this image on paper dipped in a solution of nitrate of silver, placed at a distance of about nine inches from the microscope. In the course of an hour, portions of three dark rings were very distinctly visible, much smaller than the brightest rings of the coloured image, and coinciding very nearly in their dimensions with the rings of violet light that appeared upon the interposition of violet glass. I thought the dark rings were a little smaller than the violet rings, but the difference was not sufficiently great to be accurately ascertained; it might be as much as $\frac{1}{30}$th or $\frac{1}{40}$th of the diameters, but not greater. It is the less surprising that the difference should be so small, as the dimensions of the coloured rings do not by any means vary at the violet end of the spectrum so rapidly as at the red end. For performing this experiment with very great accuracy a heliostate would be necessary, since the motion of the sun causes a slight change in the place of the image, and *leather impregnated with the muriate of silver* would indicate the effect with greater delicacy. The experiment, however, in its present state is sufficient to complete the analogy of the invisible with the visible rays, and to show that they are equally liable to the general law (of interference), which is the principal subject of this paper."

It detracts nothing from the greatness of Dr. Young to say, that although the philosophy of this experiment is permanent truth, yet the spectral image of it soon faded away. Photography was not then, at the beginning of the century, an art as permanent as it is elegant and useful. Little was wanted to make it so, but it hung fire for nearly fifty years, till Talbot supplied that little.

I have just learnt from Admiral Smyth, that his friend Dr. Peacock, the Dean of Ely, has for the last seven years been engaged on a *life of Dr. Young*; and when the work appears, we shall have a more accurate knowledge of the man who was unquestionably the Newton of his day. Like his illustrious predecessor, he was a pioneer in the philosophy of light; and, as we have seen, by a single photographic experiment, overlooked hitherto by us all, has shown a perfect analogy between the undulations of the visible and invisible rays. Had he happened to head his chapter, as Wedgwood does, "On a method of taking Pictures by the agency of Light on Nitrate of Silver," his name and place would have been duly marked; but because theory, and not experiment, was the great point before him, the philosophical photographer is overlooked by the practical one.

Dr. Young's propositions are, that radiant light consists in undulations of the luminiferous æther, that light differs from heat only in the frequency of its undulations, that undulations less frequent than those of light produce heat, and that undulations more frequent than those of light produce chemical and photographic action,—all proved by experiments.

You close your own 'Researches on Light' by proposing the following questions as of the greatest importance for future investigation:—"Is *energia* absorbed by material bodies? Does it influence their internal constitution? Is it radiated from bodies in the dark, or at all concerned in the production of any of those changes which have been attributed to *dark rays*?"

Dr. Young's hypothesis seems to anticipate your questions, and almost to answer them in their order. He says, "All material bodies have an attraction for the *ætherial medium*,—by means of which it is accumulated within their substance,—and for a small space around them—in a state of greater density, but not of greater elasticity." (Bakerian Lecture, 1801.) Hence he considers material bodies to have within them latent light, latent heat, and latent chemical force, or "energia" (which is, in his opinion, a particular condition of the ætherial medium); that the luminous, calorific, and chemical phænomena are exhibited under two modifications,—the vibratory or permanent, and the undulatory or transient state; and that the forces which produce these several effects differ from each other only in the frequency of their undulations or vibrations.

Such are the conclusions at which the all-inquiring Dr. Young arrived in 1801, on a subject which in 1850 is proposed for our investigation. Well may Admiral Smyth say, "How strange it is that we are still but half acquainted with the results of his powerful mind!"

Of course I shall not quarrel with you if you do not accept his conclusions, *totidem verbis*, because, as I am aware, you see reasons for believing that light, or that agent which affects the organs of sight, is broadly distinguished from those rays which bring heat from its solar source, and both of these classes from those which produce, in the constitution of bodies, those singular changes which are more particularly the objects of your study. But Sir John Herschel has shown that, by certain artifices, even the extreme rays may be rendered visible; and Dr. Young, by an experiment most ingenious, and to his own mind, at least, most conclusive, has demonstrated the analogy of the invisible with the visible rays. I feel sure, therefore, that while adducing and discussing the proofs of your own theory, you will be glad to take the opportunity afforded by your second edition of placing Dr. Young's name in the niche which Fame has left empty.

Believe me to be, my dear Sir,
Very truly yours,
To Robert Hunt, Esq. J. B. READE.

XLIX. *Note on the "Enumeration of the Contacts of Lines and Surfaces of the Second Order."* By J. J. SYLVESTER, *M.A., F.R.S.**

IN the month of February, 1851, I gave in this Magazine an *à priori* and exhaustive process, founded upon the method of determinants, for determining every different kind of simple or collective contact capable of happening between lines and surfaces, and in general between all loci (whether intraspatial or extraspatial) of the second order. The question was shown to resolve itself into that of determining the number of singular relations capable of existing between two quadratic homogeneous functions of any given degree. My object in the paper referred to was actually to calculate the geometrical and analytical characters of these contents and singularities for intraspatial loci, *i. e.* loci representable by homogeneous quadratics of two, three, and four variables; but I incidentally appended a statement of the number of such for loci of five, six, seven, and eight variables, without, however, dwelling upon the means of representing the general law. This statement is, however, affected with certain inaccuracies of computation which will be presently pointed out.

It will be at once apparent, from an inspection of the principle of my method, that it remains equally applicable (*mutatis mutandis*) to the more general question of determining the relative singularities (in character and amount) of two functions, each linear in respect of two systems of variables $x_1, x_2, .., x_n$; $x'_1, x'_2, \ldots x'_n$; which species of functions degenerate into quadratic forms, when the two systems of variables become identical so as to coalesce into a single system. Some researches of Mr. Cayley into the autometamorphic substitutions of quadratic forms (meaning thereby the linear substitutions

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* Communicated by the Author.

which leave the form unaltered) required him to consider the nature of the singular relations capable of existing between two linear substitutions, which is precisely the question, differently stated, of the singular relations connecting two lineo-linear functions above adverted to; accordingly, I am indebted to Mr. Cayley for making an observation on the effect of my rule for finding such singularities, which leads to a most elegant formulization of the number of singularities in question, and which I proceed to introduce to the notice of my readers.

If U and V be two quadratic functions, each of n variables, and if we call D the discriminant of $U+\lambda V=D(\lambda)$, $D(\lambda)$ will be a function of λ of the nth degree. Now, first, I have observed that if any of these n roots be repeated any number of times, there will be a corresponding degree of singularity about one of the points of intersection of the loci represented by $U=0$, $V=0$; so that if the n roots of $D(\lambda)$ be made up of r_1 roots a_1, r_2 roots a_2, r_3 roots a_3, &c., there will be an *inclusive singularity* r_1 at one point, r_2 at another, r_3 at a third, and so on—by *inclusive singularity* meaning a number one unit greater than the index of singularity properly so termed; the inclusive-singularity at an *ordinary* intersection being called, 1, at a point of simple singularity, 2, of double singularity 3, and in general at a point of the $(r-1)$th degree of singularity, r.

Hence the total-inclusive singularity (which is an unit greater than the total-singularity, properly so called) may be broken up into as many partial heaps of inclusive-singularity as there are modes of decomposing n into integers. We may now confine our attention exclusively to the different modes in which a given amount of inclusive-singularity at a single point admits of subdivision into distinct species of singularity, for which I have given in my paper referred to the following rule:—The minor systems of determinants corresponding to the matrix of $U+\lambda V$ are to be considered in succession; and if (a) be any root of the complete determinant of the matrix occurring r times, every hypothesis is to be exhausted as regards the number of times in which $(\lambda-a)$ may be conceived to enter as a factor into each of the system of 1st minors, into each minor of the system of 2nd minors, into each minor of the system of 3rd minors, and so on; the number of such hypotheses being limited by the condition that, if *quoad* the root a, $(\lambda-a)^{k_1}$, $(\lambda-a)^{k_2}$, $(\lambda-a)^{k_3}$ be the greatest common factors respectively to three consecutive systems of minor determinants, k_1 must be not less than $2k_2-k_3$. Here steps in the beautiful observation of Mr. Cayley, that the question of assigning the different species of singularities respondent to the factor (a) supposed to occur (r) times, is, by virtue of the above condition, tantamount precisely to that of assigning the total number of decreasing* series of positive integers, commencing with a given number (r), subject to the condition that the second differences shall be all positive; which (he adds) calling the successive second differences δ, δ', δ'', &c., is tantamount to finding the number of ways that the equation $r=\delta+2\delta'+3\delta''+$ &c. admits of being solved by positive integers, which is obviously the same as the number of modes in which r admits of being decomposed into positive integer parts. Thus the idea of partition, which arises naturally in the first part of the process (that, namely, of the decomposition of the collective inclusive-singularity in every possible way into modes of distributive inclusive-singularity), reappears quite unexpectedly (it may almost be said miraculously), and as the result of an analytical transformation in the second part of the same.

It should be observed that the case of complete coincidence between U and V, which, supposing them to be functions of (n) variables, corresponds to the supposition of the same factor occurring respectively n times, $(n-1)$ times, $(n-2)$ times, &c. 2 times and 1 time in the complete determinant, the 1st minor system, the 2nd minor system, &c., the $(n-2)$th minor system and the $(n-1)$th minor system respectively, is here taken as the highest case of singularity; this and the case of non-singularity, which also adds a unit to the index of singularity, properly so called, will together make a difference of 2 units in the numbers given by me in the paper referred to, which numbers will accordingly be 3, 6, 14, &c., in lieu of 1, 2, 12†, &c. We are now enabled to give the following simple statement of the law for determining the total number of singularities which can exist between two quadratic forms of n variables (or if we like so to say, more generally between two linear substitution-systems of the nth order, viz. the number of the singularities (including absolute unrelatedness and entire coincidence within the purview of the term) is the index of double decomposition into parts of the number n. To raise up in the mind a clear conception of the idea of double decomposition, we may proceed as follows:—First. Suppose a state of things in which a body is supposed to be determined completely, provided that the number of molecules which it contains, and the different number of atoms in each molecule are given, the index of simple decomposition, *i. e.* of ordinary partitionment of the number of n, will be the number of different bodies which are capable of being formed out of n atoms. Now imagine that, for the complete determination of a

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* Such a series must, from its very nature, be *always* decreasing or increasing in the same direction.

† These numbers refer to quadratic homogeneous functions, containing respectively 2, 3, 4, &c. variables. For the case of functions containing but one variable there is no distinction between coincidence and unrelatedness, and the number of modes of relation is a single unit.

body, another step in the hierarchy of aggregation is to be taken into account, and that we must know for this purpose not only the number of molecules in the body and the number of atoms in each molecule, but also the number of monads in each atom; the number of bodies (differing by definition) capable of being formed out of n monads will then represent what I mean by the index of double decomposition of (or if we like so to say) to the modulus n. And it is obvious that this idea admits of indefinite extension, and that we may speak of the index of decomposition of any order of multiplicity (single, double, treble, &c.) of or to the modulus n.

For single decomposition it is well known and immediately obvious, that the indices to the successive moduli given by the rational numbers in regular progression will be the coefficients of x, x^2, x^3, &c. in the continued product,

$$(1-x)^{-1} . (1-x^2)^{-1} . (1-x^3)^{-1}, \text{ \&c. } \textit{ad inf.};$$

calling these n_1, n_2, n_3, &c., it is of course obvious, as Mr. Cayley has observed, that the indices of double decomposition to the same successive moduli will be the coefficients of the same arguments x, x^2, x^3, &c. in the continued product

$$(1-x)^{-n_1} . (1-x^2)^{-n_2} . (1-x^2)^{-n_3}, \text{ \&c. } \textit{ad inf.};$$

and by aid of this formula he has calculated (with extreme facility) the indices in question up to the modulus 11, and found that they form the series 1, 3, 6, 14, 27, 58, 111, 223, 424, 817, 1527, which accordingly is the series representing the number of singularities capable of existing between quadratic loci commencing with 1 and ending with 11 variables.

The values of $n_1, n_2, n_3, \ldots n_{11}$, &c. themselves are given in Euler's introduction, and are respectively

1, 2, 3, 5, 7, 11, 15, 22, 30, 42, 56, &c.,

which numbers will accordingly represent to their respectively corresponding moduli the number of *classes* of singularity, whether these classes be defined with reference to the different modes of distribution of the total collective singularity about different points, or with reference to the degree of the lowest system of minor determinants of the matrix to the determinant to $U + \lambda V$ having one or more factors in common, which latter is the mode of forming the classes adopted by me in the "Enumeration."

Let me be permitted to express the satisfaction which I have felt in finding this theory, which appeared to be doomed to hopeless oblivion, thus unexpectedly, after three years of interment, coming back to life, and at once filling a desired place in analytical researches pursued with apparently a totally different aim.

26 Lincoln's Inn Fields,
March 10, 1854.

L. *On the Chemical Composition of Recent and Fossil* Lingulæ, *and some other Shells. By* W. E. Logan, *F.R.S., and* T. S. Hunt*.

In the Report of Progress of the Geological Survey of Canada for 1851–52, we have mentioned the existence of small masses containing phosphate of lime, and having the characters of coprolites, which occur in several parts of the Lower Silurian rocks. In a bed of siliceous conglomerate towards the top of the calciferous sandstone, at the Lac des Allumettes, on the Ottawa, they are abundant in cylindrical and imitative shapes, sometimes an inch in diameter. The same material forms casts of the interior of a species of *Holopea* or *Pleurotomaria*, and often fills or completely encases the separated valves of a large species of *Lingula*, which Salter has referred to *L. parallela* of Phillips. The phosphatic matter is porous, friable, and of a chocolate-brown colour; it contains intermixed a large quantity of sand, and small pebbles of quartz are sometimes partly imbedded in it. The analysis of one specimen gave 36 per cent. of phosphate of lime, with 5 per cent. of carbonate and fluoride, besides some magnesia and oxide of iron, and 50 per cent. of siliceous sand.

Similar masses occur in the same formation at Grenville, and in the lower part of the Chazy limestone at Hawkesbury, in both cases containing fragments of *Lingula*. Those from the latter place are rounded in shape, and from one-fourth to one-half of an inch in diameter, blackish without, but yellowish-brown within, and having an earthy fracture; the analysis of one of them gave:—

Phosphate of lime (PO^5, 3CaO) . . .	44·70
Carbonate of lime	6·60
Carbonate of magnesia	4·76
Peroxide of iron and a trace of alumina .	8·60
Insoluble siliceous residue	27·90
Volatile matter	5·00
	97·56

From the colour it is probable that the iron exists as a carbonate. When heated in a tube, a strong odour like burning horn is perceived, accompanied by ammonia which reddens turmeric paper and gives white fumes with acetic acid, showing that a part at least of the volatile matter is of an animal nature. The specimens from Lac des Allumettes lose 1·7 per cent. by gentle ignition, with a like production of ammonia, and an odour

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* From Silliman's American Journal, vol. xvii. p. 235.

of animal matter: the same thing was observed with those from Grenville.

The existence in Lower Silurian rocks, of these masses, whose characters leave no doubt that they are coprolites, and whose chemical composition is like that of the excrements of creatures feeding upon vertebrate animals, led us to examine the shells of the *Lingulæ* always associated with these phosphatic bodies. The result has been that all the specimens yet examined consist chiefly of phosphate of lime; they dissolve readily with slight effervescence in hydrochloric acid, and the solution gives with ammonia a copious precipitate readily soluble in acetic acid, from which oxalic acid throws down lime. With a solution of molybdate of ammonia there is obtained a quantity of the characteristic yellow molybdo-phosphate, many times greater than the bulk of the shell.

We have thus examined *Lingula prima* and *L. antiqua* from the Potsdam sandstone, *L. parallela* from the calciferous, and a species somewhat resembling *L. quadrata* from the Trenton limestone. It was desirable to compare with these the shell of a recent species, and for this purpose fine specimens of the *Lingula ovalis*, of Reeve, from the Sandwich Islands, were furnished us by J. H. Redfield, Esq., of New York. The shell of this species had the same composition as the fossil ones; and the thick, green epidermis, which swelled up like horn when heated, gave a bulky white ash of phosphate of lime.

For a further analysis, the shell was boiled in water to remove all soluble matters, the soft parts still adherent were carefully detached, and the shell with its epidermis weighing ·186 grm., was calcined over a spirit-lamp. The brownish residue, weighing ·114 grm., readily dissolved with slight effervescence in dilute hydrochloric acid, leaving but a few light flakes of carbonaceous matter. Acetate of soda and perchloride of iron were added to the solution, which was boiled, and the precipitated basic salt separated by filtration and decomposed by hydrosulphuret of ammonia. The filtrate from the sulphuret of iron having been concentrated, the phosphoric acid was thrown down by ammonia with a magnesian salt; there was obtained ·070 grm. of pyrophosphate of magnesia, equal to ·044 of phosphoric acid, or ·0978 of phosphate of lime, PO^5, $3CaO$.

The lime was separated from the acetic filtrate as an oxalate, and gave ·108 of carbonate, equal to ·0605 of lime, being an excess of ·0075 over the amount required to form the phosphate, and corresponding to ·0134 of carbonate; the small amount of material did not permit us to determine whether a portion of the lime exists as fluoride. There was also obtained ·0032 of magnesia; the results from the calcined shell of *Lingula ovalis* are then as follows:—

Phosphate of lime . .	·0978	= 85·79	per cent.
Carbonate of lime . .	·0184	= 11·75	...
Magnesia	·0032	= 2·80	...
	·1144	= 100·34	

The proportion of phosphate of lime is that contained in human bones after their organic matter has been removed.

The texture of the ancient *Lingulæ* was observed to be unlike that of most other fossil shells, being more or less dark brown in colour, brilliant, almost opake, and not at all crystalline. These characters are also found in the allied genus *Orbicula*, and we therefore examined an undescribed species of it from the Trenton limestone, beautifully marked in a manner resembling *Conularia granulata*, and another large species, also undescribed, from the Upper Silurian; both of these consist chiefly of phosphate of lime; and the shell of a recent species, *O. lamellosa*, from Callao was found to be similar in composition. We have not yet been able to examine a specimen of the genus *Obolus*. The same dark colour and brilliancy were also remarked in the genus *Conularia*, and the shell of *C. trentonensis* proved on examination to be composed in like manner of phosphate.

The similarity of composition in these genera is in accordance with the acute observations of Mr. Hall, who finds that *Conularia* is almost always associated with *Lingula* and *Orbicula*, and remarks that "these shells, so unlike in structure and habit, appear to have flourished under similar circumstances, and to have required the same kind of ocean bed or sediment" (Palæontology, vol. i. p. 101).

For the sake of comparison we have examined the following fossil shells; they have a common character, distinct from those already described, being lighter coloured, more translucent and granular in texture: *Atrypa extans*, *Leptæna alternata*, and *Orthis pectenella* from the Trenton limestone; *O. erratica* from the Hudson River group, and *Chonetes lata*? from the Upper Silurian, besides *Isotelus gigas*, and a species of *Cythere* from the Trenton. All of these consist of carbonate of lime, with only such traces of phosphate as are generally found in calcareous shells.

In the Report already quoted we have given a description of some phosphatic bodies which resemble the coprolites of the calciferous sandstone, and are found at Rivière Ouelle in thin layers of a conglomerate limestone, which is interstratified with red and green shales, and belongs to the top of the Hudson River group, or the base of the Oneida conglomerates. The phosphatic masses are very abundant, and rounded, flattened, or cylindrical in shape, and from one-eighth of an inch to an inch in diameter; they sometimes make up the larger part of the conglomerate. Iron pyrites in small globular masses occurs abundantly with them, often filling their interstices, but is not found elsewhere in the rock. These coprolites are finer grained and more compact than those from the Ottawa, and have a conchoidal fracture; their colour is bluish or brownish-black; the powder is ash-gray, becoming reddish after ignition. They have the hardness of calcite, and a density of 3·15. When heated they evolve ammonia with an animal odour, and with sulphuric acid give the reactions of fluorine. The quantitative analysis of one gave—

Phosphate of lime, $PO^5, 3CaO$ . .	40·34	per cent.
Carbonate of lime, with fluoride . .	5·14	...
Carbonate of magnesia	9·70	...
Peroxide of iron with a little alumina	12·62	...
Oxide of manganese	trace	...
Insoluble siliceous residue . . .	25·44	...
Volatile matter	2·13	...
	95·37	

The iron exists, in part at least, as carbonate, and its introduction in so large a quantity, giving colour and density to the coprolites, is doubtless connected with the formation of iron pyrites by the deoxidizing action of organic matters. The production of an equivalent of bisulphuret of iron from a neutral protosulphate of iron, which alone could exist in contact with limestone, must be attended with the elimination of an equivalent of protoxide of iron, for $2(SO^3 . FeO) - O^7 = Fe\,S^2 + FeO$.

It is remarkable that no traces of *Lingulæ* or any other shells have been detected with these coprolites. Thin sections of them are translucent, and under the microscope are seen to consist of a fine granular base, in which are imbedded numerous grains of quartz, and small siliceous spiculæ, like those of some sponges. In a bed of sandstone, associated with these conglomerates and slates at Rivière Ouelle, were found several hollow cylindrical bodies, resembling bones in appearance. The longest one is an inch and a half long, and one-fourth of an inch in diameter. It is hollow throughout, and had been entirely filled with the calcareous sandstone, in which it is imbedded, and whose disintegration has left the larger end exposed. The smaller extremity is cylindrical and thin, but it gradually enlarges from a thickening of the walls, and at the other end becomes externally somewhat triangulariform; the cavity remains nearly cylindrical, but the exposed surfaces are rough and irregular within.

The texture of these tubes is compact, their colour brownish-black, with a yellowish-brown translucency in thin layers. Analysis shows them to consist, like the coprolites, principally of phosphate of lime. One hundred parts gave—

Phosphate of lime	67·53
Carbonate of lime	4·35
Magnesia	1·65
Protoxide of iron	2·95
Insoluble siliceous sand	21·10
Volatile animal matter	2·15
	99·73

The microscopic examination of a section shows that the walls of the tube are homogeneous, unlike the coprolites, and that the siliceous sand in the analysis came from the sandstone which incrusted the rough interior of the fossil. The phosphate is finely granular, and retains no vestige of organic structure. The chemical composition and the remarkable shape of the specimens, however, leave little doubt of their osseous nature, unless we suppose them to be the remains of some hitherto unknown invertebrate animal, whose skeleton, like those of ***Lingula, Orbicula*** and ***Conularia,*** consisted of phosphate of lime, a composition hitherto supposed to be peculiar to vertebrate skeletons.

Montreal, Jan. 5, 1854.

LI. *Developments on the Porism of the In-and-circumscribed Polygon.* *By* A. Cayley, *Esq.*[*]

I PROPOSE to develope some particular cases of the theorems given in my paper, "Correction of two Theorems relating to the Porism of the in-and-circumscribed Polygon" (Phil. Mag. Nov. 1853). The two theorems are as follows.

Theorem. The condition that there may be inscribed in the conic $U=0$ an infinity of n-gons circumscribed about the conic $V=0$, depends upon the development in ascending powers of ξ of the square root of the discriminant of $\xi U+V$; viz. if this square root be

$$A+B\xi+C\xi^2+D\xi^3+E\xi^4+F\xi^5+G\xi^6+H\xi^7+\ldots,$$

then for $n=3, 5, 7$, &c. respectively, the conditions are

$$|\,C\,|=0,\qquad \begin{vmatrix} C, & D \\ D, & E \end{vmatrix}=0,\qquad \begin{vmatrix} C, & D, & E \\ D, & E, & F \\ E, & F, & G \end{vmatrix}=0, \text{ \&c.};$$

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[*] Communicated by the Author.

and for $n=4$, 6, 8, &c. respectively, the conditions are

$$|\,\mathrm{D}\,|=0,\qquad \begin{vmatrix}\mathrm{D},&\mathrm{E}\\ \mathrm{E},&\mathrm{F}\end{vmatrix}=0,\qquad \begin{vmatrix}\mathrm{D},&\mathrm{E},&\mathrm{F}\\ \mathrm{E},&\mathrm{F},&\mathrm{G}\\ \mathrm{F},&\mathrm{G},&\mathrm{H}\end{vmatrix}=0,\ \&\mathrm{c}.$$

Theorem. In the case where the conics are replaced by the two circles $x^2+y^2-\mathrm{R}^2=0$, $(x-a)^2+y^2-r^2=0$, then the discriminant, the square root of which gives the series

$$\mathrm{A}+\mathrm{B}\xi+\mathrm{C}\xi^2+\mathrm{D}\xi^3+\mathrm{E}\xi^4+\&\mathrm{c}.,$$

is

$$-(1+\xi)\{r^2+\xi(r^2+\mathrm{R}^2-a^2)+\xi^2\mathrm{R}^2\}.$$

Write for a moment

$$\mathrm{A}+\mathrm{B}\xi+\mathrm{C}\xi^2+\mathrm{D}\xi^3+\mathrm{E}\xi^4+\&\mathrm{c}.=\sqrt{(1+a\xi)(1+b\xi)(1+c\xi)},$$

then

$$\begin{aligned}
\mathrm{A}&=1\\
2\mathrm{B}&=a+b+c\\
-8\mathrm{C}&=a^2+b^2+c^2-2bc-2ca-2ab\\
16\mathrm{D}&=a^3+b^3+c^3-a^2(b+c)-b^2(c+a)-c^2(a+b)+2abc\\
-128\mathrm{E}&=5a^4+5b^4+5c^4\\
&\quad-4a^3(b+c)-4b^3(c+a)-4c^3(a+b)\\
&\quad+4a^2bc+4b^2ca+4c^2ab\\
&\quad-2b^2c^2-2c^2a^2-2a^2b^2\\
&\quad\&\mathrm{c}.
\end{aligned}$$

To adapt these to the case of the two circles, we have to write

$$r^2(1+a\xi)(1+b\xi)(1+c\xi)=(1+\xi)\{r^2+\xi(r^2+\mathrm{R}^2-a^2)+\xi^2\mathrm{R}^2\}.$$

and therefore

$$\begin{aligned}
c&=1\\
r^2(a+b)&=r^2+\mathrm{R}^2-a^2\\
r^2ab&=\mathrm{R}^2;
\end{aligned}$$

values which after some reductions give

$$\begin{aligned}
\mathrm{A}&=1\\
r^2 2\mathrm{B}&=2r^2+\mathrm{R}^2-a^2\\
-r^4 8\mathrm{C}&=(\mathrm{R}^2-a^2)^2-4\mathrm{R}^2r^2\\
r^6 16\mathrm{D}&=(\mathrm{R}^2-a^2)\{(\mathrm{R}^2-a^2)^2-2r^2(\mathrm{R}^2+a^2)\}\\
-r^8 128\mathrm{E}&=5(\mathrm{R}^2-a^2)^4\\
&\quad-8(\mathrm{R}^2-a^2)^2(\mathrm{R}^2+2r^2)r^2\\
&\quad+16a^4r^4.
\end{aligned}$$

Whence, also,

$$r^{12}1024(\mathrm{CE}-\mathrm{D}^2)=$$
$$\{5(\mathrm{R}^2-a^2)^4-8(\mathrm{R}^2-a^2)^2(\mathrm{R}^2+2r^2)r^2+16a^4r^4\}\{((\mathrm{R}^2-a^2)^2-4\mathrm{R}^2r^2)\}$$
$$-4\{(\mathrm{R}^2-a^2)^3-2(\mathrm{R}^2-a^2)(\mathrm{R}^2+a^2)r^2\}^2,$$

which after all reductions is

$$\begin{aligned}&(\mathrm{R}^2-a^2)^6\\&-12\mathrm{R}^2(\mathrm{R}^2-a^2)^4r^2\\&+16\mathrm{R}^2(\mathrm{R}^2+2a^2)(\mathrm{R}^2-a^2)^2r^4\\&-64\mathrm{R}^2a^4r^6.\end{aligned}$$

Hence the condition that there may be inscribed in the circle $x^2+y^2-\mathrm{R}^2=0$, and circumscribed about the circle $(x-a)^2+y^2-r^2=0$, an infinity of n-gons is for $n=3$, 4, 5 respectively, *i. e.* in the case of a triangle, a quadrangle and a pentagon, is as follows.

I. For the triangle, the relation is

$$(\mathrm{R}^2-a^2)^2-4\mathrm{R}^2r^2=0,$$

which is the completely rationalized form (the simple power of a radius being of course analytically a radical) of the well-known equation

$$a^2=\mathrm{R}^2-2\mathrm{R}r,$$

which expresses the relation between the radii R, r of the circumscribed and inscribed circles, and the distance a between their centres.

II. For the quadrangle, the relation is

$$(\mathrm{R}^2-a^2)^2-2r^2(\mathrm{R}^2+a^2)=0,$$

which may also be written

$$(\mathrm{R}+r+a)(\mathrm{R}+r-a)(\mathrm{R}-r+a)(\mathrm{R}-r-a)-r^4=0.$$

(Steiner, *Crelle*, vol. ii. p. 289.)

III. For the pentagon, the relation is

$$(\mathrm{R}^2-a^2)^6-12\mathrm{R}^2(\mathrm{R}^2-a^2)^4r^2+16\mathrm{R}^2(\mathrm{R}^2+2a^2)(\mathrm{R}^2-a^2)^2r^4$$
$$-64\mathrm{R}^2a^4r^6=0,$$

which may also be written

$$(\mathrm{R}^2-a^2)^2\{(\mathrm{R}^2-a^2)^2-4\mathrm{R}^2r^2\}^2-4\mathrm{R}^2r^2\{(\mathrm{R}^2-a^2)^2-4a^2r^2\}^2=0.$$

The equation may therefore be considered as the completely rationalized form of

$$(\mathrm{R}^2-a^2)^3+2\mathrm{R}(\mathrm{R}^2-a^2)^2r-4\mathrm{R}^2(\mathrm{R}^2-a^2)r^2-8\mathrm{R}a^2r^3=0.$$

This is, in fact, the form given by Fuss in his memoir "De Polygonis Symmetrice irregularibus circulo simul inscriptis et circumscriptis," *Nova Acta Petrop.* vol. xiii. pp. 166–189 (I quote from Jacobi's memoir, to be presently referred to). Fuss puts $\mathrm{R}+a=p$, $\mathrm{R}-a=q$, and he finds the equation

$$\frac{p^2q^2-r^2(p^2+q^2)}{r^2q^2-p^2(r^2+q^2)}=\pm\sqrt{\frac{q-r}{q+p}},$$

which, he remarks, is satisfied by $r=-p$ and $r=\frac{pq}{p+q}$, and that consequently the rationalized equation will divide by $p+r$ and $pq-r(p+q)$; and he finds, after the division,

$$p^3q^3+p^2q^2(p+q)r-pq(p+q)^2r^2-(p+q)(p-q)^2r^3=0,$$

which, restoring for p, q their values $\mathrm{R}+a$, $\mathrm{R}-a$, is the very equation above found.

The form given by Steiner (*Crelle*, vol. ii. p. 289) is

$$r(\mathrm{R}-a)=(\mathrm{R}+a)\sqrt{(\mathrm{R}-r+a)(\mathrm{R}-r-a)+(\mathrm{R}+a)}\sqrt{(\mathrm{R}-r-a)2\mathrm{R}},$$

which, putting p, q instead of $\mathrm{R}+a$, $\mathrm{R}-a$, is

$$qr=p\sqrt{(p-r)(q-r)}+p\sqrt{(q-r)(q+p)}.$$

And Jacobi has shown in his memoir, "Anwendung der elliptischen Transcendenten," &c., *Crelle*, vol. iii. p. 376, that the rationalized equation divides (like that of Fuss) by the factor $pq-(p+q)r$, and becomes by that means identical with the rational equation given by Fuss.

In the case of two concentric circles $a=0$, and putting for greater simplicity $\frac{\mathrm{R}^2}{r^2}=\mathrm{M}$, we have

$$\mathrm{A}+\mathrm{B}\xi+\mathrm{C}\xi^2+\mathrm{D}\xi^3+\mathrm{E}\xi^4+\&\text{c.}=(1+\xi)\sqrt{1+\mathrm{M}\xi}.$$

This is, in fact, the very formula which corresponds to the general case of two conics having double contact. For suppose that the polygon is inscribed in the conic $\mathrm{U}=0$, and circumscribed about the conic $\mathrm{U}+\mathrm{P}^2=0$, we have then to find the discriminant of $\xi\mathrm{U}+\mathrm{U}+\mathrm{P}^2$, *i. e.* of $(1+\xi)\mathrm{U}+\mathrm{P}^2$. Let K be the discriminant of U, and let F be what the polar reciprocal of U becomes when the variables are replaced by the coefficients of P, or, what is the same thing, let $-\mathrm{F}$ be the determinant obtained by bordering K (considered as a matrix) with the coefficients of P. The discriminant of $(1+\xi)\mathrm{U}+\mathrm{P}^2$ is $(1+\xi)^3\mathrm{K}+(1+\xi)^2\mathrm{F}$, *i. e.*

$$(1+\xi)^2\{\mathrm{K}(1+\xi)+\mathrm{F}\}=(\mathrm{K}+\mathrm{F})(1+\xi)^2(1+\mathrm{M}\xi),$$

where $\mathrm{M}=\frac{\mathrm{K}}{\mathrm{K}+\mathrm{F}}$; or, what is the same thing, M is the discriminant of U divided by the discriminant of $\mathrm{U}+\mathrm{P}^2$. And M having this meaning, the condition of there being inscribed in the conic $\mathrm{U}=0$ an infinity of n-gons circumscribed about the conic $\mathrm{U}+\mathrm{P}^2=0$, is found by means of the series

$$\mathrm{A}+\mathrm{B}\xi+\mathrm{C}\xi^2+\mathrm{D}\xi^3+\mathrm{E}\xi^4+\&\text{c.}=(1+\xi)\sqrt{1+\mathrm{M}\xi}.$$

We have, therefore,

$$A=1$$
$$2B=M+2$$
$$-8C=M^2-4M$$
$$16D=M^3-2M^2$$
$$-128E=5M^4-8M^3$$
&c.
$$1024(CE-D^2)=M^4(M^2-12M+16)$$
&c.

Hence for the triangle, quadrangle and pentagon, the conditions are—

I. For the triangle,

$$M+2=0.$$

II. For the quadrangle,

$$M-4=0.$$

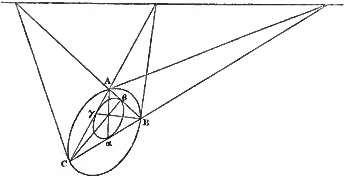
III. For the pentagon,

$$M^2-12M+16=0.$$

And so on.

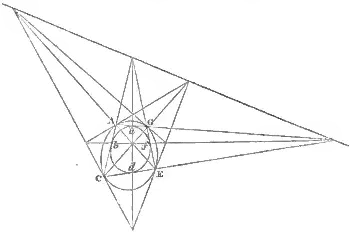
It is worth noticing, that, in the case of two conics having a 4-point contact, we have $F=0$, and consequently $M=1$. The discriminant is therefore $(1+\xi)^3$, and as this does not contain any variable parameter, the conics cannot be determined so that there may be for a given value of n (nor, indeed, for any value whatever of n) an infinity of n-gons inscribed in the one conic, and circumscribed about the other conic.

The geometrical properties of a triangle, &c. inscribed in a conic and circumscribed about another conic, these two conics having double contact with each other, are at once obtained from those of the system in which the two conics are replaced by concentric circles. Thus, in the case of a triangle, if ABC be the



triangle, and $\alpha\beta\gamma$ be the points of contact of the sides with the inscribed conic, then the tangents to the circumscribed conic at ABC meet the opposite sides BC, CA, AB in points lying in the chord of contact, the lines Aα, Bβ, Cγ meet in the pole of contact, and so on.

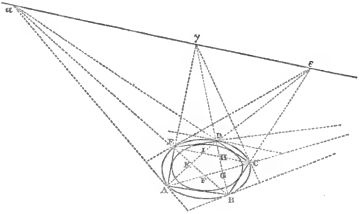
In the case of a quadrangle, if ACEG be the quadrangle, and *b*, *d*, *f*, *h* the points of contact with the inscribed conic, then



the tangents to the circumscribed conic at the pair of opposite angles A, E and the corresponding diagonal CG, and in like manner the tangents at the pair of opposite angles C, G and the corresponding diagonal AE, meet in the chord of contact. Again, the pairs of opposite sides AC, EG, and the line *dh* joining the points of contact of the other two sides with the inscribed conic, and the pairs of opposite sides AG, CE, and the line *bf* joining the pairs of contact of the other two sides with the inscribed conic, meet in the chord of contact. The diagonals AE, CG, and the lines *bf*, *dh* through the points of contact of pairs of opposite sides with the inscribed conic, meet in the pole of contact, &c.

The beautiful systems of 'focal relations' for regular polygons (in particular for the pentagon and the hexagon), given in Sir W. R. Hamilton's Lectures on Quaternions, Nos. 379–393, belong, it is clear, to polygons which are inscribed in and circumscribed about two conics having double contact with each other. In fact, the focus of a conic is a point such that the lines joining such point with the circular points at infinity (*i. e.* the points in which a circle is intersected by the line infinity) are tangents to the conic. In the case of two concentric circles, these are to be considered as touching in the circular points at infinity; and consequently, when the concentric circles are replaced by two conics having double contact, the circular points at infinity are replaced by the points of contact of the two conics.

Thus, in the figure (which is simply Sir W. R. Hamilton's figure 81 put into perspective), the system of relations



F, G(··)ABCI
G, H(··)BCDK
H, I (··)CDEF
I, K(··)DEAG
K, F(··)EABH

will mean, F, G(··)ABCI, that there is a conic inscribed in the quadrilateral ABCI such that the tangents to this conic through the points F and G pass two and two through the points of contact of the circumscribed and the inscribed conics, and similarly for the other relations of the system. As the figure is drawn, the tangents in question are of course (as the tangents through the foci in the case of the two concentric circles) imaginary.

2 Stone Buildings,
March 7, 1854.

LII. *Note on the Penetration of the Spermatozoon into the Interior of the Ovum.* *By* Martin Barry, *M.D., F.R.S.*[*]

THE fact that the spermatozoon penetrates into the interior of the ovum was published by myself in the Philosophical Transactions of the Royal Society of London for 1840. Further observations enabled me to record the same as established, in the Philosophical Transactions for 1843. And in the Edinburgh New Philosophical Journal for October 1843, will be found a drawing I gave of an ovum of the Rabbit containing spermatozoa —nine in number, with the statement that I once counted more than twenty in an ovum of this animal.

These observations were held by physiologists to have been a mistake, an opinion which lasted up to the year 1852, when Nelson confirmed them in ova of an Entozoon, Keber in ova of the freshwater Mussels *Unio* and *Anodonta*, and Newport in ova of the Frog. And the present note contains, I believe, the first announcement in this country of their having been confirmed in the Mammalia themselves. I have just received a letter from a friend in Germany informing me of a paper published by Prof. Bischoff, dated March 15, 1854[†], in which it is stated that spermatozoa have at length been seen within the *zona*, not only by R. Wagner, Henle, Baum, and Meissner, but even by Bischoff himself, who up to 1852 had considered my observations as "born of the imagination," but now candidly acknowledges that he had done me injustice, the mistake having been his own. And he remarks, that "Barry was certainly the first to see spermatozoa in the interior, not only of the mammiferous ovum, but of any ova." Professor Bischoff might have added, that for a dozen years Barry had a melancholy and not very enviable monopoly of the said observations. And it may now be mentioned that in this respect they resembled certain others, for confirmation of which I have always felt that I could afford to "bide my time." Thus I have at length received notice from a physiologist on the Continent of a work he is about to publish, containing a confirmation of my fact announced in Müller's *Archiv* for 1850, that cilia are composed of spirals.

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[*] Communicated by the Author.

[†] Entitled, "Bestätigung des von Dr. Newport bei den Batrachiern und Dr. Barry bei den Kaninchen behaupteten Eindringens der Spermatozoiden in das Ei."

LIII. *On the Heat produced by an Electric Discharge.*
By Professor W. Thomson.

To the Editors of the Philosophical Magazine and Journal.

2 College, Glasgow,
April 19, 1854.

Gentlemen,

It has been pointed out by M. Clausius, in a letter addressed to you and published in the last Number of your Magazine, that the first discovery of the true relation between the generation of heat in the discharge of a Leyden phial and the quantity of the previous charge is not, as I had stated it to be, due to Joule, but that it had been given in a paper published about three years earlier by Riess. I may be allowed to explain, that, in making the statement in question, I considered the law of that relation as an evident corollary from the great principle, that the whole heat generated in any discharge of electricity is exactly the equivalent in thermal energy for the mechanical value of the electrical charge which is lost*. There is no doubt who is the discoverer of *this*, and the originator, in your most valuable Magazine, of the theory of mechanical equivalence among the electric, chemical, magnetic, frictional, and pneumatic developments of energy, which has within the last two or three years attracted so many investigators.

The mere law, that the heat generated by the discharge of a Leyden phial or battery is proportional to the square of the quantity of electricity in the previous charge, is not, as I inadvertently stated, due to Joule; neither is it, as M. Clausius seems to suppose, due to Riess. Becquerel, I find, in his *Traité de l'Electricité* (vol. iii. p. 150, published in 1835, or two years earlier than the paper referred to by M. Clausius), enunciates it quite explicitly as having been established by "Cuthbertson and others, who had used electrometers in measuring the calorific action of the discharge of a battery." Mr. Joule, too, although in his first publication he only referred to the researches of Snow Harris which had recently appeared in the Philosophical Transactions, remarks in a subsequent paper (On the Heat disengaged in chemical Combinations, Phil. Mag. June 1852), "that Brooke and

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* The application of this principle to the discharge of a Leyden phial shows that the whole heat generated must be equal to $\frac{1}{J}\cdot\frac{1}{2}Q^2\frac{4\pi\tau}{IS}$, if J denote the mechanical equivalent of the thermal unit, Q the amount of the charge, τ the thickness of the glass, I its specific inductive capacity, and S the area of either side of the coated surface; a conclusion which wants no other experimental verification than such as may be considered desirable for verifying that $I\frac{S}{4\pi\tau}$ is the true expression for the capacity of a Leyden phial.

Cuthbertson found that the length of wire melted by an electrical battery varied nearly with the square of its charge;" and at the same time he refers to the researches of Riess on the calorific effects of frictional electricity, acknowledging their priority to his own researches on the heat generated by continuous electric currents.

I remain, Gentlemen,
Yours very faithfully,
William Thomson.

LIV. *On the Generation of Heat by Electricity.* *By* P. Riess.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Mr. William Thomson has stated, in the Philosophical Magazine for March 1854, p. 193, that the quantity of heat generated by the discharge of a Leyden jar is proportional to the square of the quantity of electricity, which was first proved in 1840 by Mr. Joule, in opposition to the results derived previously from the experiments of Sir W. Snow Harris. This statement is in all respects incorrect. Mr. Joule never proved the law above stated; but in a remark attached to his investigation on the heat of the voltaic current he gave utterance to the presumption, that under the necessary limitations the heat generated is proportional to the square of the charge of a given battery. (Phil. Mag. Oct. 1841, p. 265.) When this vague conjecture was first hazarded, a memoir of mine had been published for four years in which the conclusions of Harris were refuted and the law of the square of the current was proved (Pogg. *Ann.* March 1837, p. 341); and in two extensive inquiries I had already given the complete formula for the heat generated by the battery discharge, after having discovered the remarkable relation subsisting between the quantity of heat generated in a wire and the resistance of the said wire.

Such reclamations, which cannot be more painful to those who cause them than to those who find themselves compelled to make them, might be avoided if the reasonable demand were everywhere recognized, that an assertion of such a general character as that ventured by Mr. Thomson ought to be the consequence of a careful examination, not only of what has been done at home, but also of what has been published in other countries on the subject in question.

P. Riess.

Berlin, April 13, 1854.

LV. *On a Method for preserving the Sensitiveness of Collodion Plates for a considerable time.* *By* John Spiller *and* William Crookes[*].

THE extreme sensitiveness of collodion as compared with paper and other photographic surfaces renders this material invaluable in all cases where rapidity of action is desirable, but up to the present time its use has been greatly restricted by the necessity for preparing the plate and completing the whole of the manipulatory details within a comparatively short space of time, thus rendering this beautiful process practically inapplicable in all cases where the conveniences of a photographic laboratory are not at hand.

For some time past we have been investigating the causes which operate to prevent the excited plate retaining its efficiency for more than a few hours. It seemed highly probable that the permanent sensitiveness of the film was principally dependent on the retention of a moist surface; and if by any artificial means this end could be secured, the original sensitiveness of the film would be, for at least a reasonable time, preserved unimpaired.

The only attempts up to the present time to effect this object are, we believe, that of M. Girod[†], who proposes to enclose the sensitive collodion film between two plates of glass, with only so much of the exciting silver solution as might be retained by capillary attraction; and thus by retarding the evaporation of the water, to keep the surface moist, and consequently sensitive, for a longer period; and secondly, that of M. Gaudin[‡], who suggests the use of perfectly air-tight dark frames or boxes, in which a number of the wet plates could be arranged in a horizontal position and there kept until required. Besides these two methods, it is well known that the plate will remain excited for a considerable time if kept immersed in a solution of nitrate of silver; in fact, a glass bath in the camera has been often used in cases where the length of exposure was likely to be too prolonged to admit of the plate being placed in the ordinary slide.

Instead, however, of having recourse to a mechanical means for preventing the evaporation from the surface, we have endeavoured to avail ourselves of a chemical process, by the employment in the bath of substances having a powerful affinity for water; in the choice of these, however, we are necessarily limited to such as are neutral in constitution, and do not form insoluble compounds with silver. The nitrates and acetates, especially

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[*] Communicated by the Authors.

[†] Journ. Phot. Soc. No. 9.

[‡] Ibid. No. 10.

the former, seemed most convenient for our purpose on account of their general deliquescent nature, and for our first experiments we selected the nitrates of lime, magnesia, and zinc, as most promising of success. These agents were at first tried in the above-mentioned order; but from a few preliminary trials we were inclined to give the preference to the zinc salt, and having obtained such satisfactory results with its use, are induced to communicate them at once rather than withhold them until our investigation of the other compounds shall have been completed. At first we endeavoured to add the nitrate of zinc direct to the exciting bath, but the quantity required to prevent so large an amount of nitrate of silver from crystallizing out on the plate rendered the solution too dense to work with.

The following process can be recommended as having proved perfectly successful in our hands; we do not doubt that with more general use it may be considerably modified and improved, but at present we have rather contented ourselves with establishing the broad principle with such detail only as will suffice to ensure good results, and to leave to a future period the consideration of those minor points which only a long experience can develope.

The plate, coated with collodion (that which we employ contains iodide, bromide, and chloride of ammonium, in about equal proportions), is made sensitive by immersion in the ordinary solution of nitrate of silver (30 grains to the ounce), and after remaining there for the usual time, is transferred to a second solution of the following composition:—

Nitrate of zinc (fused) . . .	2 ounces.
Nitrate of silver	35 grains.
Water	6 ounces.

The plate must be left in this bath until the zinc solution has thoroughly penetrated the film (we have found five minutes amply sufficient for this purpose, although a much longer time is of no consequence); it should then be taken out, allowed to drain upright on blotting-paper until all the surface moisture has been absorbed (about half an hour), and then put by until required. The nitrate of zinc, which is still retained on the plate, is sufficient to keep it moist for any length of time, and we see no theoretical or practical reason why its sensitiveness should not be retained as long: experiments on this point are in progress; at present, however, we have only subjected them to the trial of about a week, although at the end of that period they were hardly deteriorated in any appreciable degree. It is not necessary that the exposure in the camera should be immediately followed by the development, as this latter process can be deferred to any convenient opportunity provided it be within the week. Previous to development, the plate should be allowed to remain for a few seconds in the original 30-grain silver-bath, then removed and developed with either pyrogallic acid or a protosalt of iron, and afterwards fixed, &c. in the usual manner.

The advantages of this process can scarcely be overrated. Besides the facility it affords of working in the open air without any cumbrous apparatus, photography may now be applied in cases where it would have been hitherto impracticable, owing to the feebleness of the light, *e. g.* badly illuminated interiors, natural caverns, &c.; if necessary, the exposure could be protracted for a week, or possibly much longer, and the deficiency of daylight compensated for by the employment of the electric or other artificial light. It will also be found useful where the plate must be kept ready excited, but the exact moment of exposure may depend upon possible contingencies rather than on the will of the operator, or in cases where it would be impracticable to prepare the plate just before exposure; for these reasons it might prove a valuable adjunct on the eve of a naval or military engagement, for accurately recording the positions of the forces.

A small proportion of nitrate of zinc added to the ordinary nitrate of silver bath in no way interferes with its action, and might obviate the inconvenience sometimes felt during hot weather in photographic rooms, of the film becoming partially dry before exposure. If added in a still smaller proportion to the silver solution used for exciting the ordinary Talbotype paper* (without the employment of gallic acid), it is very probable that its sensitiveness may be preserved during a much longer period than is generally possible. As far as our experiments have gone, they tend to confirm this supposition; but at present we can hardly speak more confidently on this point, as it is still under investigation.

There are, no doubt, many other substances which might equally well answer the purpose of nitrate of zinc; besides those already mentioned, the nitrates of cadmium, manganese, and perhaps also those of copper, nickel, and cobalt might be found serviceable. Glycerine at first seemed to promise very good results, but the principal difficulty was the necessary impurity of the commercial product, in consequence of its being obtained from the exhausted leys of the soap boilers.

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* This addition of nitrate of zinc to the silver solution in the *Talbotype* process was suggested, we have been informed, some time back by the French, but not with reference to its keeping properties—only as an accelerator.

LVI. *Additional Directions and Improvements in the Process for the Manufacture of Artificial Tourmalines.* *By* WILLIAM BIRD HERAPATH, *Esq., M.D. Lond.**

IN the last November Number of this Journal the author gave a formula for the production of these crystals of large size, so as to be available for optical purposes.

That formula succeeds very well when the temperature of the atmosphere in the apartment is 65°, and likely to be maintained at this point for three or four hours, falling gradually to 45° Fahr. in the course of the night. Under these circumstances it is rarely the case that some large plates are not discovered in the morning. These should be at once secured or they will re-dissolve or sink, and thus be lost.

When the temperature does not fall below 50° or 55° Fahr. a different formula is necessary. Less spirit must then be employed, in order to render the compound less soluble in the menstruum.

Formula No. 2 may now be used most successfully.

(2) Take of disulphate of quinine 100 grains,
... acetic acid (1·042) 4 fluid ounces,
... rectified spirit (0·837) 1 fluid ounce,
... spirituous solution of iodine 1 fluid drachm.

The crystals are produced more quickly by this formula than by the previous one; and in three or four hours must be removed, or the mother-liquid will quickly redissolve them. Many of the larger crystals become too heavy to float any longer and fall to the bottom of the flask, from whence they are not easily removed; still by the exercise of patience and some ingenuity they may be recovered, and fully repay for the trouble of cutting off the top of the flask, &c., by producing a field wholly devoid of violet light upon crossing two plates, especially when examined by daylight.

Formula No. 3.—I have recently succeeded in making these crystals of large size, by replacing the spirit of wine in No. 2 formula, by an equal quantity of hyponitric æther (sweet nitre). There is some advantage in using this solvent, as the crystals remain even for a week floating on the surface, without showing the least appearance of disintegration.

It is frequently the case in this experiment, that the free iodine disappears very readily; the solution becoming nearly colourless—under these circumstances, the compound does not form. If the temperature employed be too elevated, this is almost certain to occur; of course it is only necessary to add a

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* Communicated by the Author.

second quantity of iodine solution, so that the liquid may retain its dark sherry colour during the cooling.

After making the addition of the tincture of iodine in either of the previous formulæ, it is necessary to continue the heat of the spirit lamp for a short time, to dissolve all the cinnamon-brown iodide of quinine first deposited, so that the solution shall become perfectly clear dark-sherry wine colour, and then to filter the solution rapidly through white porous paper into a *perfectly clean* wide-mouthed Florence flask, matrass, or beaker, and to attend to the following conditions:—

1st. It is well to have at least six flasks half-filled with solutions made by the above formulæ, so as to ensure the production of foliaceous plates in some one or more of them; according to the state of the atmospheric temperature, as previously explained.

2ndly. It is more convenient to set these at work as nearly simultaneously as possible, and to watch them well during the next three or four hours, especially if formula No. 2 be employed, so that the produce may be caught at the most favourable moment. If any foreign bodies, as hair, dust, or filaments of paper, are floating in the solutions, the broad plates do not form, as those particles act as nidi for the crystals, and fatally hasten the process of deposition, so that the produce is small and confused.

3rdly. It rarely or never happens that we obtain these large plates at the first attempt; it is often necessary to redissolve and crystallize two, three or four times before we find any large enough for optical purposes; the reason is that the process of crystallization is at first much too confused and rapid to allow of the requisite arrangement of the particles or prisms, so as to constitute the broad compound optical leaflets.

4thly. It is always desirable not to raise the temperature to the boiling-point when redissolving, as spirit and iodine are both then lost, and the formative power of the mother-liquid becomes impaired and at length completely lost, in consequence of the relative proportions of acid and spirit being so altered by continuëd distillation of the latter.

5thly. It is always better to add four or five drops of the tincture of iodine after redissolving, in order to produce an atmosphere of iodine vapour above the liquid; this in condensing starts an early surface crystallization, whilst the mother-liquid is sufficiently charged with the compound.

6thly. These compound foliaceous plates having "crenated edges" consist of prisms arranged side by side in various forms, but frequently producing discs of considerable size, the component atoms of which are all arranged in the same optical direction; but at other times crystallization appears to start from a central point, and then the foliaceous laminæ (still consisting of adherent component prisms or sections of rhombic plates) radiate like the petals of a flower; under these circumstances the *corolla*-shaped disc would be useless; but either *petal*, if large enough, would be serviceable, as in the latter all the atoms would be in the same relative plane with regard to polarized light; consequently it is necessary to select for mounting the *most uniform* plates, and not always the largest clusters, for these would have a "macled" appearance, and produce a beam of light polarized in more than one plane, and therefore creating confused results in the polariscope.

Having obtained these crystals by one of the above formulæ, and caught one upon the glass circle as before described, it is better to submit it to a process of washing (before drying) so as to remove the mother-liquid and get rid of the interference of crystals of sulphate of quinine deposited either beneath or upon the plate; in my last paper, "cold distilled water imbued with iodine" was recommended for this purpose.

However, as the effect I then ascribed to iodine is by no means certain, and as it sometimes also injures the plate by depositing the brown iodide of quinine upon its surface, it is far better to employ a saucer or evaporating basin full of a saturated solution of Herapathite in a mixture of distilled water, and one-eighth of its bulk of acetic acid for this purpose. The acetic acid greatly hinders the tangential flying off of the crystalline plates from the glass discs upon *our slowly* and *very cautiously immersing them in a horizontal position* beneath the surface of the water; and if this should occur, the plate is frequently broken; should it float off, it must be recaught upon the glass disc as before, and then be cautiously dried by absorbing all moisture by bibulous paper as before, and drying it as rapidly as may be consistent with safety by exposure to air at 50° or 60°.

If it be considered desirable, it may be exposed for a short time to the vapour of iodine; this is best accomplished by means of solid iodine—the tincture was once recommended, but the alcoholic vapour renders it brittle, a defect which is not found with the solid substance.

I have been recently occupied, in conjunction with my friend Mr. Thwaites, in making some experiments upon different media for mounting these crystals. Those principally employed have been iodized almond oil and pure elaine, also slightly charged with iodine, as well as glycerine similarly iodized. The mode of using these is to prepare a saturated solution of iodine in either medium, then dilute it with four times the quantity of pure medium for use. Having placed the circular disc on the table, with a very minute drop of either substance on its centre, invert the plate carrying the crystal (this being downwards) upon the oil, elaine, &c., press together slightly, and then if too much medium has not been used, the plates are just kept apart by the interposed fluid. The edges are then surrounded or painted with a solution of gum-arabic, slightly toughened by the addition of a little isinglass or gelatine. This is a very rapid mode of mounting, but not certain or safe; sometimes the edging of gum cracks, air is admitted, the definition of the plate is lost, and its brilliancy for the polariscope much damaged. Oil and elaine appear to dissolve and disintegrate the crystals.

I therefore infinitely prefer the ætherial and iodized Canada balsam.

Crystals thus mounted keep without change or disintegration; some of the author's plates have been so prepared for months and are *still perfect,* whilst others mounted very recently in iodized oil, or elaine, or glycerine, have shown symptoms of destruction in as many weeks or days even. I have some which are quite as good as ever after *three years'* wear, these were mounted in fluid Canada balsam. When disintegration occurs, it appears to be greatly influenced by imperfection in the washing process, for those plates which have been mounted with crystals of sulphate of quinine beneath them certainly break up and dissect themselves with great rapidity; it appears to be produced by some unknown molecular action going on, and a rearrangement of the atomic constituents. The plates appear to become henpecked by minute parallelogrammatic holes; these begin at the edges and go onwards through the disc, rendering the plate useless.

The ætherialized Canada balsam has no action on the plates; however, it is perfect as a mounting fluid; it dries very readily too, a few days' exposure to air of 60° or 70° being alone sufficient for this purpose.

As turpentine has no solvent power over Herapathite, even at a boiling temperature, fluid Canada balssm may be employed; I therefore was in error in ascribing the disintegration to that medium. I have since found that the crystals *must be washed* to render them secure for mounting in it; the only objection to this medium is, that it dries so slowly.

The object of using the iodized compound is to give a black tone to the field; this it certainly accomplishes; the periodized crystals, when examined *by daylight,* are black as midnight, even if the plates are no thicker than gold-leaf, but an intense lamp or gaslight appears to pass through them with a violet hue, perhaps more inclining to brown-red; this is an unpolarized ray, and it cannot be obstructed by tourmaline or Nichol's prism; it requires four thicknesses of Herapathite (all parallel) to stop this last portion of the beam; it is doubtful whether the effect of periodizing the plate is lasting—I think not.

When it is absolutely necessary to obtain a perfectly black field with a total stoppage of all the incident rays (upon "crossing" the two crystals), it is much the better plan to employ a thicker plate of this substance: such a crystal will be generally found in the flask at the bottom of the mother-fluid. There is more trouble requisite in obtaining perfect plates, free from all intervening crystals, but the experimenter is generally repaid in the end by the perfection of the polarizing medium.

When the selenite stage is employed, the thinner and violet-coloured crystals are far preferable to those which give a black tone to the field; as the colours are more brilliant, and the flood of transmitted light much greater, so that we are enabled to use a less illuminating power. I am not in the habit of using an achromatic condenser with my polarizing apparatus, which probably accounts for some discrepancies in the results of observations made by different experimenters upon the same crystalline plates: those crystals which will transmit the violet rays, when strongly illuminated by gas or lamp light, will not do so when the instrument is used in daylight, or with a plane instead of a concave mirror, and without the achromatic condenser.

If it be necessary to obtain a most decidedly black field, the violet rays may be readily absorbed by interposing a thin plate of sulphate of copper beneath the polarizing plate of Herapathite and the source of illuminating power.

I have recently employed a plate of this substance, one-twentieth of an inch thick, cut on a hone, polished and mounted between two plates of thin glass in Canada balsam, as a means of correcting the defects of the thinner plates of these new tourmalines*—this substance possessing the power of absorbing the violet rays of the spectrum in a pre-eminent degree. In order to succeed in this experiment, it is necessary that the sulphate of copper should be inclined at a certain angle to the plane of primitive polarization, as it is a substance possessing two neutral axes, or planes of no-depolarizing power, the position of which may be easily found, and their direction marked upon the support, so that the intervening plate may be always inserted at the angle of its greatest activity. Professor Stokes has lately, in a letter to me, suggested the employment of a glass laden with the oxide of copper as a means of attaining the same end. Having, therefore, prepared a boracic glass, coloured by the black oxide of copper, I have used it effectually as an absorbent medium for counteracting the violet-red colour of the polarized

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* A solution of the sulphate or nitrate of copper in water will equally succeed in producing a black field.

beam. But although it offers great and manifest advantages when the new tourmalines are crossed at right angles, yet, upon revolving the superior crystal, and therefore bringing the two plates into a parallel position, we have a blue colour in the field, which must assuredly alter the colours of depolarizing media: it is, however, a very agreeable light to work by, as the intense yellow of gaslight is much mellowed down and counteracted by it. This corrective medium would be inadmissible when the selenite stage is employed, as the tints would be materially changed by its absorptive agency.

The mode of making this glass is simply to dry powdered biborate of soda in a crucible by the heat of an ordinary fire; again reduce the effloresced mass to powder, and mix it with a small quantity of the oxide of copper, such as is generally used in organic analysis; then introduce the mixture into a platinum crucible, and with a steady, long-continued heat, thoroughly vitrify it, pour it out upon a flat slate, clear metallic or Wedgewood-ware surface, and press it while still soft into a flattened plate. Upon cooling, a portion must be quickly ground down upon a hone, polished, and then mounted in Canada balsam between glass; the unmounted boracic glass may be kept for any length of time in turpentine without change, but in the air it effloresces, and becomes opake and useless.

I have been asked for a process by which the quinine may be recovered from the mother-solutions and from Herapathite itself. The following will be found very successful.

It is merely necessary to boil the mother-liquids with the Herapathite crystals, add a little solution of some soluble sulphuret, say potassium or ammonium, to convert the iodine into hydriodic acid, evaporate to dryness so as to expel the spirit, acetic acid, by a water-bath; redissolve the remaining salt in just sufficient boiling distilled water, filter, and set aside to crystallize; filter when cold, and dry the crystals by expression, and by a gentle heat.

The mother-liquid contains acetate and some sulphate of quinine; add to it, when cold, an alkali in solution, potassa, soda, or ammonia, or the carbonates of these; collect the separated alkaloid on a filter, and dissolve it in boiling water acidulated with sulphuric acid; *if this be not in excess*, the disulphate of quinine crystallizes on cooling, and the loss is very slight*.

32 Old Market Street, Bristol.

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* This disulphate may be again employed for the manufacture of the crystals; so may the neutral sulphate of quinine obtained by the first part of the process.

LVII. *Notice on Barometrical, Thermometrical, and Hygrometrical Clocks. By* Sir David Brewster, *K.H., D.C.L., F.R.S., V.P.R.S. Edin., and Associate of the Institute of France*[*].

IN the Number of this Journal for December 1853, Mr. Macquorn Rankine has described a Barometric Pendulum, and has referred to the history of this class of instruments.

About the year 1810 or 1811, I proposed, in the articles 'Atmospherical Clock and Barometer,' published in the Edinburgh Encyclopædia[†], the construction of *barometrical, thermometrical,* and *hygrometrical pendulums* for registering the indications of such instruments. As I was prevented by more interesting pursuits from constructing any of these clocks, I should not have thought of claiming any priority in proposing them; but I owe it to Mr. Babbage to state, that about 1820, without knowing of my suggestions, he actually constructed a *barometrical clock* and sent to me a paper on the subject, which he declined to have published, in consequence, I believe, of my having anticipated him in the idea. If I recollect rightly, Mr. Babbage not only made observations with his barometrical clock, but it was proposed by some of the influential members of the Royal Society to erect one of them in their apartments.

LVIII. *Poceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 291.]

Feb. 23, 1854.—The Rev. Baden Powell, V.P., in the Chair.

THE following communications were read :—

1. A paper entitled, "Continuation of the subject of a paper read Dec. 22, 1853, the supplement to which was read Jan. 12, 1854, by Sir Frederick Pollock, &c.; with a proof of Fermat's first and second Theorems of the Polygonal Numbers, viz. that every odd number is composed of four square numbers or less, and of three triangular numbers or less." By Sir Frederick Pollock, M.A., F.R.S. &c.

The object of this paper is in the first instance to prove the truth of a theorem stated in the supplement to a former paper, viz. "that every odd number can be divided into four squares (zero being considered an even square) the algebraic sum of whose roots (in some form or other) will equal 1, 3, 5, 7, &c. up to the greatest possible sum of the roots." The paper also contains a proof, that if every odd number $2n+1$ can be divided into four square numbers, the algebraic sum of whose roots is equal to 1, then any number n is composed of not exceeding three triangular numbers.

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[*] Communicated by the Author.

[†] Vol. iii. pp. 57 and 294.

The general statement of the method of proof may be made thus: two theorems are introduced which connect every odd number with the gradation series, 1, 3, 7, 13, &c., of which the general term is $n+n^2+1$ or $4p^2\pm 2p+1$ (that is, the double of a triangular number $+1$), each term of which series can be resolved into four squares, the algebraic sum of the roots of which, $p, p, p, p+1$, or $p-1, p, p, p$ may manifestly be $=1$. By these theorems it is shown that every odd number is divisible into four squares, having roots capable of forming as the sum of the roots 1, 3, 5, 7, &c. up to the greatest possible sum of the roots.

As the four square numbers which compose an odd number must obviously be three of them even and one odd, or three odd and one even, the differences of the roots among themselves must be the first odd and the third even, or *vice versâ*; and therefore these roots must have the sum of the first and third differences an odd number; the middle difference may be either odd or even.

The first of the theorems referred to, called by the author "Theorem P," is in substance this:—

Let r, s, t, v be the roots the squares of which compose any odd number N, such that $r+s+t+v=1$, and let each of these roots be increased by m; then $r+m, s+m, t+m, v+m$ will be the roots of the odd number $N+2m(2m+1)$; and $m-r, m-s, m-t, m-v$ the roots of the odd number $N+2m(2m-1)$; the sum of the roots in the first case being $4m+1$, and in the second $4m-1$. So that giving to m successively the values 0, 1, 2, 3, &c. in the general form $N+2m(2m\pm 1)$, a series will be formed in which the sums of the roots will be 1, 3, 5, 7, 9, &c., and the sums of their squares N, $N+2.1.1$, $N+2.1.3$, $N+2.2.3$, $N+2.2.5$, $N+2.3.5$, $N+2.3.7$, $N+2.4.7$, &c.; or N, $N+1.2$, $N+2.3$, $N+3.4$, $N+4.5$, $N+5.6$, $N+6.7$, $N+7.8$, &c. So that if p be the distance of any odd number in this series from N, the number will be $N+p(p+1)$, and the sum of its roots will be $2p+1$.

The conclusions to be drawn from this theorem are then stated:—

1. The greatest sum of the roots of the squares into which any odd number can be divided may be obtained: for let $2n+1$ be any odd number, and $2p+1$ the odd number to which the algebraic sum of its roots is required to be equal; then if p is such that $p(p+1)$ is less than $2n+1$, the number $2n+1$ can be resolved into squares the sum of whose roots is $2p+1$; otherwise it cannot.

2. The form of the roots of $2n+1$ may be found of which the algebraic sum is any possible odd number $2p+1$ except 1, provided all the odd numbers less than $2n+1$ possess the property of having the algebraic sum of their roots $=1$. For if from $2n+1$, $p(p+1)$ be taken, there will remain an odd number (N in Theorem P) such that, according to the condition stated, the algebraic sum of its roots $=1$; and in the series of roots and odd numbers formed from these roots according to theorem P, p terms from N will be found the number $2n+1$ composed of squares the algebraic sum of whose roots is $2p+1$.

It thus appears that any odd number $2n+1$ can be divided into squares the sum of whose roots will equal 3, 5, 7, &c. (any possible odd number except 1) if the odd numbers below it can be divided into squares the sum of whose roots $=1$; and if it can be shown that its roots in some form will equal 1, then the theorem M will be true for that number and for every number below it.

This is illustrated by an example, and then another theorem, called "Theorem Q," is stated. In this a series of roots and odd numbers is formed by making the 1st and 3rd differences of the roots constant, but reversed every alternate term, and increasing or diminishing the middle difference by 1 each term;—or the middle difference is made constant and the 1st and 3rd vary. The sums of the roots thus become constant in every term of the series, but the sums of the squares of the roots increase, as in theorem P, by the even numbers 2, 4, 6, 8, &c., so that the increase at any number of terms p is $p(p+1)$, or the double of a triangular number.

By the application of these theorems to a variety of examples, it is shown how any odd number may be composed of four squares, such that the algebraic sum of their roots may equal 1.

The theorems P and Q, it is considered, connect every odd number with every other odd number, so as to make it impossible if one odd number be composed of four squares, but that every other odd number should likewise be so. It is pointed out in what manner every possible combination of numbers which can furnish the differences of the roots of any squares, not exceeding four, which can make an odd number, and the sum of which roots $=1$, can be derived from the gradation series, that is from $4p^2 \pm 2p+1$. The combined effect of the theorems P and Q is therefore to prove that every odd number must be composed of not exceeding four square numbers.

The author goes on to show that every number is composed of not exceeding three triangular numbers, by proving that if every odd number $2n+1$ can be divided into four square numbers the sum of whose roots $=1$, then n will be composed of not exceeding three triangular numbers. This is done by taking the differences of the roots of $2n+1$, the algebraic sum of which roots is one, and diminishing the middle difference by theorem Q until it reaches a number nearest to half the sum of the first and third differences. The difference between $2n+1$ and the number thus obtained will be the double of a triangular number $=2T$. By the next step, the extreme differences are reduced until they are of the form m, $m+1$; and the difference between $2n+1-2T$ and the number thus obtained will again be the double of a triangular number $=2T'$. The differences last obtained give the double of a triangular number, $+1=2T''+1$. So that we find $2n+1=2T+2T'+2T''+1$. Consequently $n=$ the sum of three triangular numbers, if all the three operations be necessary; if not, to two or one triangular number only.

2. The first part of a paper "On a class of Differential Equations, including those which occur in Dynamical Problems." By W. F. Donkin, M.A., F.R.S., F.R.A.S., Savilian Professor of Astronomy in the University of Oxford.

This paper is intended to contain a discussion of some properties of a class of simultaneous differential equations of the first order, including as a particular case the form (which again includes the dynamical equations),

$$x'_i=\frac{dZ}{dy_i},\quad y'_i=-\frac{dZ}{dx_i}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{I.})$$

where $x_1 \dots x_n$, $y_1 \dots y_n$ are two sets of n variables each, and accents denote total differentiation with respect to the independent variable t; Z being any function of x_1 &c., y_1 &c., which may also contain t explicitly. The part now laid before the Society is limited to the consideration of the above form.

After deducing from known properties of functional determinants a general theorem to be used afterwards, the author establishes the following propositions.

If $x_1 \dots x_n$ be n variables connected with n other variables $y_1 \dots y_n$ by n equations of the form $y_i=\frac{dX}{dx_i}$ (X being a given function of $x_1 \dots x_n$); then the equations obtained by solving these algebraically, so as to express $x_1 \dots x_n$ in terms of $y_1 \dots y_n$, will also be of the form $x_i=\frac{dY}{dy_i}$; where Y is a function of $y_1 \dots y_n$, which may be defined by the equation

$$Y=-(X)+(x_1)y_1+\dots+(x_n)y_n,$$

in which the brackets indicate that the terms within them are to be expressed as functions of $y_1 \dots y_n$. Moreover, if p be any other quantity contained explicitly in X (besides the variables $x_1 \dots x_n$), the following relation will subsist; namely,

$$\frac{dX}{dp}+\frac{dY}{dp}=0,$$

the differentiation in each case being performed only so far as p appears *explicitly* in the function.

It is then shown that if X contain explicitly, besides $x_1 \dots x_n$, the n constants $a_1, a_2, \dots a_n$, and the variable t, and if the $2n$ variables $x_1 \dots x_n, y_1 \dots y_n$, be determined as functions of t by the system of $2n$ equations,

$$\frac{dX}{dx_i}=y_i,\quad \frac{dX}{da_i}=b_i, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{II.})$$

where $b_1 \dots b_n$ are n other constants, the elimination of the $2n$ constants from these equations and their differentials with respect to t, leads to the system of differential equations (I.), if for Z be put the result of substituting in $-\frac{dX}{dt}$ the values of the $2n$ constants in terms of the variables. The equations expressing the $2n$ constants in terms of the variables may be considered as the $2n$ integrals of the system (I.).

The author employs the symbol $[p, q]$ in a sense similar to that in which Poisson and others have employed (p, q), namely, as an abbreviation for $\Sigma_i\left(\frac{dp}{dy_i}\frac{dq}{dx_i}-\frac{dp}{dx_i}\frac{dq}{dy_i}\right)$; and he shows that if p, q

represent any two of the $2n$ constants a_1 &c., b_1 &c., then $[p, q]$ is either $=1$ or $=0$, according as p, q are a *conjugate* pair a_i, b_i, or not.

Next it is shown that if a_1, a_2, ...a_n represent any functions of $2nn$ variables x_1 ... x_n, y_1 ... y_n, satisfying identically the $\frac{n(n-1)}{2}$ conditions $[a_i, a_j]=0$, then if by means of the n given equations expressing a_1, &c. in terms of the variables, the set $y_1, y_2 \dots y_n$ be expressed as functions of $x_1 \dots x_n$, $a_1 \dots a_n$, the $\frac{n(n-1)}{2}$ relations $\frac{dy_i}{dx_j}=\frac{dy_j}{dx_i}$ will be identically satisfied; in other words, the expression for $y_1 \dots y_n$ will be the partial differential coefficients of a function of $x_1 \dots x_n$.

Hence it easily follows, that if any n integrals $a_1 \dots a_n$ of the system (1.) be given, which satisfy the conditions $[a_i, a_j]=0$, a "Principal Function" X can always be found, from which the remaining integrals of the system may be deduced by means of the second set of equations (11.).

The relation in which these investigations stand to the discovery of Sir W. R. Hamilton (as improved and completed by Jacobi) is pointed out. And it is shown that the system of n differential equations of the second order

$$\left(\frac{dW}{dx'_i}\right)'=\frac{dW}{dx_i}$$

(to which Lagrange had reduced the dynamical equations, and which Sir W. Hamilton had transformed into the system (I.) by a process depending upon the circumstance that, in dynamical problems, W contains x'_1, x'_2, ... x'_n only in the form of a homogeneous function) may, by means of the theorems established at the beginning of the paper, be reduced to the form (I.) without assuming anything as to the form of W, which may be any function whatever of $x_1 \dots x_n$, $x'_1 \dots x'_n$, and t.

The $2n$ integrals of the system (I.), obtained in the way above explained, being shown to satisfy the conditions

$$[a_i, b_i]=1, \quad [a_i, a_j]=[a_i, b_j]=[b_i, b_j]=0,$$

it is proposed to call them "normal integrals," and the constants a_1 &c., b_1 &c. "normal elements," any pair a_i, b_i being called *conjugate*.

In the second section, the author gives a simplified demonstration of Poisson's theorem (extended to the general system (I.)), that if f, g be any two integrals, $[f, g]$ is constant. The preceding principles are then exemplified by application to the problems of the motion of a material point under the action of a central force, and the rotation of a solid body about a fixed point.

In each case three integrals, c_1, c_2, c_3, are taken, satisfying the three conditions $[c_2, c_3]=0$, $[c_3, c_1]=0$, $[c_1, c_2]=0$; the first being the integral of *vis viva*, and the other two being derived from the integrals expressing the conservation of areas. In the former problem the "principal function" is then found with great ease, and the remaining integrals deduced. The set of "normal elements" thus obtained coincide with those given by Jacobi (in a memoir in Crelle's Journal, vol. xvii.). In the problem of rotation, the algebraical solution of the three assumed integrals for y_1, y_2, y_3 depends upon that of an equation of the fourth degree. It is therefore impracticable to exhibit the principal function in an explicit form. In this respect the result arrived at resembles that obtained by Mr. Cayley in a totally different way; Mr. Cayley having shown that the solution of the problem is reducible to quadratures, assuming the algebraical solution of a certain system of equations of the same form as those to which the author of the present investigation is conducted. (Camb. and Dub. Math. Journ. vol. i. p. 172.)

Methods are then indicated by which, when one system of "normal elements" is given, other systems may be found.

The practical value of "normal solutions" of the system (I.) depends chiefly upon the simplicity of the corresponding formulæ for the variation of elements, the theory of which is intended to form part of the subject of the following sections.

March 2.—Professor Graham, V.P., in the Chair.

The following Papers were read:—

1. "On the Growth of Land Shells." By E. J. Lowe, Esq., F.G.S., F.R.A.S. &c.

Perhaps the following observations on the growth of land shells may contain sufficient information to prove interesting to the Royal Society. Before describing them, however, a few introductory remarks will be necessary. Every individual experimented upon has been kept in confinement since the day it was hatched. Each species has been placed in a separate box (filled with soil to the depth of three inches), and care has been taken to feed the Mollusca every other day, the food chiefly consisting of the leaves of the lettuce and cabbage. In very dry weather the soil has been moistened with rain-water about once a week; in the box containing ***Helix pomatia*** small lumps of chalk have been mixed with the soil.

The species experimented upon were:—

Helix aspersa
— caperata
— hispida
— nemoralis
— pomatia
— rotundata
— virgata

Zonites cellarius
— lucidus
— nitidulus
— radiatulus
Bulimus obscurus
Clausilia nigricans
Pupa umbilicata

The facts arrived at are,—

1st. The shells of Helicidæ increase but little for a considerable period, never arriving at maturity before the animal has *once* become dormant.

2nd. Shells do not grow whilst the animal itself remains dormant.

3rd. The growth of shells is very rapid when it does take place.

4th. Most species bury themselves in the ground to increase the dimensions of their shells.

First Experiment with Helix pomatia.

A specimen of this species having deposited thirteen eggs which were hatched during the first week of August 1852, six of the young ones were deposited in a box (having a lace cover) placed in the shade. The young Helices were regularly fed every other day until the beginning of December, when they buried themselves in the soil for winter; up to this period they had gradually increased in dimensions to the size of *Helix hispida*. From December until April the soil was kept dry, the box being placed in the cellar. On the 1st of April they were replaced in the garden, the soil having previously been copiously watered. On the 3rd of April the young ones appeared on the surface, being *no larger* in size than they were in December, and although regularly fed up to the 20th of June they scarcely increased, not being perceptibly larger in size than they were in December. However, on the 20th of June five of them disappeared, having buried themselves (with the mouth of the shell *downwards*) in the soil; on the 30th of June they reappeared, having in ten days grown so rapidly as at this time to become equal in size to *Helix pisana*. They again buried themselves on the 15th of July and reappeared on the 1st of August, having again increased in size. From this date they did not apparently become any larger, and on the 2nd of November food was withheld for the winter, and at the present time (February 14th) they are in a dormant state. Probably they will arrive at maturity by July or August next. The sixth specimen did not bury itself until the 15th of August.

Second Experiment with Helix aspersa.

A pair of *Helix aspersa* having been procured in the act of copulation on the 19th of May 1852, they were placed in confinement. Each individual deposited about 70 eggs, which began to hatch on the 20th of June: these young ones grew but little during the summer. They buried themselves in the soil on the 10th of October, coming again to the surface on the 5th of April, *not having grown during the winter*. In May they buried themselves (with their *heads downwards* as with *Helix pomatia*, in winter they and other species buried themselves with the *head upwards*), appearing again in a week *double the size*; this process was carried on at about fortnightly intervals until July the 18th, when they were almost fully grown. It is worthy of remark that this species, as well as *Helix pomatia* and *Helix nemoralis*, and probably other of the Helicæ, form an operculum at the aperture, after which they retire considerably within the shell, and form a second (much thinner), behind which they rest during the winter.

It would be swelling this paper too much to describe all the observations in full; it will perhaps therefore be considered sufficient to remark that the process of growth *within the ground* takes place with *Helix nemoralis, Helix virgata, Helix caperata*, and *Helix hispida*. *Helix rotundata* burrows into decayed wood to increase the size of its shell. *Zonites radiatulus* appears to remain on decaying blades of grass; whilst *Pupa umbilicata, Clausilia nigricans* and *Bulimus obscurus* bury *their heads only* to increase their shells. With respect to *Zonites cellarius, Zonites lucidus*, and *Zonites nitidulus*, it was not satisfactorily ascertained whether their heads were buried during the process of growth.

Observatory, Beeston, E. J. Lowe.
1854, February 14th.

2. "Note on the Decomposition of Sulphuric Acid by Pentachloride of Phosphorus." By Alexander Williamson, Ph.D., F.C.S., Professor of Practical Chemistry in University College.

Chemists have long been aware of the fact that some acids unite with bases in one proportion only, others in two or more proportions. Thus a given quantity of nitric acid forms with what is termed its equivalent of potash, a definite nitrate of potash; if less than this equivalent quantity of potash were added to the nitric acid, the product would be a mechanical mixture of the same nitrate of potash with uncombined nitric acid; if more than the equivalent of potash were added, the excess of alkali would remain uncombined. Sulphuric acid, on the other hand, is capable of forming two compounds with potash, and it depends upon the proportions in which the two substances are brought together whether the neutral or acid sulphate is formed.

The number of compounds which an acid forms with one base is now considered as indicating its atomic weight. The weights of sulphuric and nitric acids which are respectively susceptible of neutralizing the same quantity of potash are termed *equivalent*, but these are by no means the same as their *atomic* weights. Sixty-three parts of nitric acid (nitrate of water) contains the same quantity of hydrogen as forty-five parts of sulphuric acid, and when they are neutralized by potash the whole of this hydrogen is removed and replaced by potassium; and if neither of the acids could combine in any other proportion with potash, their atomic weights would be the same as their equivalent weights. But sulphuric acid also forms a potash compound in which half of its hydrogen is replaced by potassium, the other half remaining in the compound, whereas the smallest particles of nitric acid either exchange the whole or none of their hydrogen for potassium.

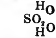
This fact is expressed in the simplest possible manner by the statement that the smallest indivisible particles of sulphuric acid contain two atoms of hydrogen, whilst those of nitric acid only contain one. Thus it is, that whereas the equivalent weights of the two acids are the quantities which contain the same amount of basic hydrogen, their atomic weights must be in the proportion of two equivalents of sulphuric to one of nitric acid. The simplest expression for an atom of nitric acid being empirically $NO_3\,H$, we shall accordingly represent an atom of sulphuric acid by the formula SO_4H_2. In like manner, an atom of common phosphoric acid, being tribasic, is expressed empirically by the formula PO_4H_3. The labours of Messrs. Laurent and Gerhardt greatly contributed to the establishment of these results, which are uncontroverted.

We have hitherto been accustomed to resort very freely to imaginary distinctions of form and arrangement of matter to explain the differences of properties; but of late years an opposite tendency has arisen, and chemists have felt the necessity of reducing their language and ideas to simpler and more consistent forms. This necessity was first felt in the most complex, *i. e.* the so-called organic part of chemistry. But the simplifications thus introduced have proved to be equally applicable to the inorganic part of the science; and their introduction is calculated to disengage, for the consideration of substantial differences of composition, the attention which has hitherto been absorbed by imaginary distinctions of form. Being unable to express the constitution of compounds without some formal artifice, we shall be able to see and compare their substantial differences most easily when all unnecessary variations of those formal artifices are eliminated. The success of this operation of course depends on our finding one form sufficiently general to replace the special and limited forms now employed.

In some papers published in the Journal of the Chemical Society two or three years ago, I endeavoured to show that the constitution of salts may be reduced to the type of water; that acids and bases being, truly, acid salts and basic salts, are perfectly conformable to the same principle; and that, amongst other things, the difference between monobasic and bibasic acids, &c. admits of a simple and easy explanation by it. The leading propositions in those papers have been adopted by several eminent chemists in this country and in France; and M. Gerhardt speedily enriched science with a series of brilliant and striking illustrations of their truth. As regards the constitution of bibasic acids, M. Gerhardt's results were, however, at variance with that theory; and he was led to represent them by formulæ equally inconsistent with his own previous views on the subject. I believe that this discrepancy is satisfactorily removed by the facts I have the honour of submitting to the consideration of the Society.

An atom of nitric acid, being eminently monobasic, is, as we have already shown, represented in the monobasic type $\begin{matrix}H\\H\end{matrix}O$ by the formula $\begin{matrix}(NO_2)\\H\end{matrix}O$, in which peroxide of nitrogen (NO_2) replaces one atom of hydrogen. In like manner, hydrate of potash $\left(\begin{matrix}H\\K\end{matrix}O\right)$ is obtained by replacing one atom of hydrogen in the type by its equivalent of potassium; and nitrate of potash $\left(\begin{matrix}NO_2\\K\end{matrix}O\right)$ by a simultaneous substitution of *one* atom of hydrogen by peroxide of nitrogen, the *other* by potassium. Sulphuric acid is formed from two atoms of water $\begin{matrix}\begin{matrix}H\\H\end{matrix}O\\\begin{matrix}H\\H\end{matrix}O\end{matrix}$; one of hydrogen from each is removed, and the two replaced by the indivisible radical SO_2. The series

Sulphuric acid.



$SO_2{}^{HO}_{HO}$,

Acid sulphate of potash.

$SO_2{}^{HO}_{KO}$,

Neutral sulphate of potash.

$SO_2{}^{KO}_{KO}$

explains itself.

Chemists have long known how to remove the basylous constituents H, K, &c. of these salts, and to replace them by others. But it is only recently that they have learnt to remove the chlorous radicals SO_2, NO_2, &c. in a similar manner. To obtain the chloride of potassium from its sulphate, it is sufficient to bring the latter into liquid contact with chloride of barium; but the same reagent would be powerless for the preparation of the chlorides of the radicals SO_2 or NO_2.

M. Cahours has shown us a reagent (the pentachloride of phosphorus) which is capable of forming from a great number of monobasic acids the chlorides of the acid radicals. Whilst extending our knowledge of the action of the body on monobasic and organic acids, and preparing numerous compounds of their radicals with one atom of chlorine, M. Gerhardt examined also the nature of its action upon bibasic acids and their compounds; and states that it consists of two successive phases, first, the liberation of the anhydrous acid, secondly, the substitution of two atoms of chlorine for one of oxygen in that anhydrous acid. These facts, if correct, would be unfavourable to the above view of the constitution of sulphuric and the other bibasic acids; and M. Gerhardt adopted accordingly the old formulæ, representing in their composition an atom of water ready-formed, SO_3H_2O.

Confining my remarks for the present to the case of sulphuric acid, whose decomposition is doubtless typical of that of other bibasic acids, I may state as the result of numerous experiments with the most varied proportions of pentachloride and acid, performed on a scale of considerable magnitude, that the first action of the pentachloride consists in removing one atom of hydrogen and one of oxygen (empirically peroxide of hydrogen) from the acid, putting in an atom of chlorine in their place and forming the compound $SO_2{}^{HO}_{Cl}$, which is strictly intermediate between the hydrated acid and the final product SO_2Cl_2 formed by a repetition of the same process of substitution of chlorine for peroxide of hydrogen. The existence and formation of this body, which we may call chloro-hydrated sulphuric acid, furnishes the most direct evidence of the truth of the notion, that the bibasic character of sulphuric acid is owing to the fact of one atom of its radical SO_2 replacing or (to use the customary expression) being equivalent to two atoms of hydrogen. Had this radical been divisible like an equivalent quantity of a monobasic acid, we should have obtained a *mixture*, not a *compound* of the chloride with the hydrate,—or, at least, the products of decomposition of that mixture.

Chloro-hydrated sulphuric acid boils at 145° Cent., distilling without decomposition. The intensity of its action upon water varies according to the manner in which the two bodies are brought together. When poured rapidly into a large quantity of cold water, a portion of it sinks to the bottom, and only gradually dissolves as a mixture of hydrochloric and sulphuric acids. When a small quantity of water is added to the compound, the same decomposition takes place with explosive violence. The acid dissolves chloride of sodium on the application of a gentle heat with evolution of hydrochloric acid, giving rise to a compound of the formula $SO_2{}^{Na\,O}_{Cl}$. When poured upon pieces of melted nitre at the atmospheric temperature, an effervescence takes place with evolution of a colourless vapour which possesses in a striking degree the odour of aqua regia. This vapour may be dissolved in various liquids, and when decomposed by water, yields nitric and hydrochloric acids. It is doubtless chloro-nitric acid, $NO_2\,Cl$. In like manner the chlorides of other inorganic acid radicals may be obtained, as from chlorates, perchlorates, sulphites, &c., but of these and other reactions I beg leave to defer any further account until the experiments now in hand are more advanced.

From the general resemblance of properties and identity of boiling-point of the chloro-hydrate with a compound discovered by Rose, and described by that eminent chemist as possessing empirically the composition $S_2\,O_5\,Cl_2$, I was led to suspect that the two might in reality be identical, which of course would require the addition of the elements of water to Rose's formula, and several experiments I have performed afford strong confirmation of that identity. The same compound is obtained by the action of dry hydrochloric acid on anhydrous sulphuric acid; and finally, I may mention that Mr. Railton obtained a small quantity of the same substance some weeks ago in my laboratory by the action of platinum-black at a high temperature on an imperfectly dried mixture of chlorine and sulphurous acid.

As regards the successive transformations effected in the pentachloride, I have observed the formation of Wurtz's oxychloride (the tribasic chloro-phosphoric acid ($PO\,Cl_3$)), and also of a compound boiling above 145°, probably $PO_2\,Cl$. Hydrated phosphoric acid is always found unless the amount of pentachloride added is very great.

March 9.—Thomas Bell, Esq., V.P., in the Chair.

The following paper was read:—

"On a new and more correct method of determining the Angle of Aperture of Microscopic Object-Glasses." By William S. Gillett, Esq., M.A.

The very large apertures assigned to the more recent microscopic object-glasses drew the author's attention some time since to the importance of testing the accuracy of the method employed to determine their amount.

With this object in view he began with the consideration that the central pencil was alone to be regarded, and that the marginal rays of this were the true limits of the angle of aperture, and that consequently the rays of all oblique pencils were to be excluded, as these might cross at a point not coincident with the principal focus, and being measured separately might form an angle (apparently of aperture) not coinciding of course with the true one, although perhaps not differing from it in amount. A short description of the usual method of measuring these angles will suffice to show what claim it has to confidence in these respects.

The microscope, with the object-glass to be examined and an ordinary eye-piece, is used as a telescope, and a light placed at some distance is commonly made an object to define the limit of the field of view, the image of which is formed near the back surface of the posterior combination, and the diffused light of this image, as seen through the eye-piece, is the indication that a pencil of light is admitted, whether central or oblique. Sometimes by an additional glass the eye-piece is made an erecting one capable of bringing the image into focus. This adds much to the convenience, but not to the correctness of the method. Thus the conditions of the microscopic object-glass are reversed, the principal focus being transferred from the front to the back, and the rays estimated are those of the extreme oblique pencils, which may or may not pass through the point of the principal focus of the glass when used for the microscope.

The importance of this in the illumination of objects immediately suggested itself; and the author obtained a further proof by another experiment bearing directly upon this point. A blackened wire was placed under a microscope at the focal point, with an object-glass of considerable power and aperture, the wire covering the field with the eye-piece used. The field was then illuminated with an achromatic condenser, the field of illumination exceeding, as it usually does, that of the microscope. As was expected, the oblique rays which passed on both sides of the wire prevented its blackness from being seen (this becoming of a milky-grey), until the field of illumination was reduced to the extent of that of the microscope, when it immediately assumed to the eye its natural blackness. This reminded the author of a beautiful illustration given by Professor Faraday some years since at the Royal Institution, of the effect of glare produced by placing white muslin blackened in parts before a white paper printed in large letters; with the white muslin in front, the letters were scarcely visible, while through the blackened parts they resumed their natural appearance. These experiments suggested the new method adopted, which may be briefly stated as follows:—

The microscope of which the object-glass is to be examined is placed horizontally and centred by an object placed in the focus. Next, there is substituted in place of the eye-piece, a hollow cone with an aperture at its summit. Light passing through this aperture is made to form an image of it in the principal focus of the object-glass, in the place of the original object. On this image a horizontally placed examining microscope is then directed, which traverses as the radius of a graduated circle, having its centre corresponding with the place of the original object, and therefore with the image to be viewed; and the angle of aperture is measured by the arc passed through between two extreme positions, in the usual manner. The method is further explained in the paper by a figure and description of the apparatus, which was itself exhibited in the Library after the meeting.

March 16.—Charles Wheatstone, Esq., V.P., in the Chair.

The following paper was read:—"On some new Compounds of Phenyle." By A. Williamson, Ph.D., F.C.S., Professor of Practical Chemistry in University College.

This communication contains a notice of some of the results obtained in an investigation of Carbolic Acid or Hydrated Oxide of Phenyle, conducted, under the author's superintendence, by Mr. Scrugham in the Analytical Laboratory of University College.

Referring to the substitution products obtained by Laurent from hydrate of phenyle by the action of chlorine and bromine, as well as to its combination with acids prepared by that chemist in conjunction with Gerhardt, the author states that the substance which they conceived to be chloride of phenyle has been found by Mr. Scrugham to be a mixture of two compounds.

As regards the preparation of hydrate of phenyle from the creosote of coal-tar, it is observed that the numerous fractional distillations by which it is usually isolated may be abridged by crystallization; for if creosote, having the boiling-point between 186° and 188° Cent., be left for some time in contact with a few crystals of the pure hydrate, it deposits a considerable quantity of beautiful colourless needles, which, when separated from the mother-liquid, distil at 184° Cent., and condense in the neck of the retort into a solid mass of pure hydrate of phenyle.

When pentachloride of phosphorus is added to hydrate of phenyle, the action is at first very energetic, hydrochloric acid being evolved, and the mixture becoming hot; but after a time the addition of fresh portions of pentachloride produces no perceptible action, unless the mixture be heated. Oxychloride of phosphorus is formed, as well as a neutral oily body, which is insoluble in aqueous potash at the common temperature, but soluble with decomposition in boiling potash. This oily compound would, from its mode of formation, be naturally supposed to be the chloride of phenyle, and it has been so considered by some distinguished chemists. It may, however, be separated by distillation into two perfectly definite and distinct bodies, one of which boils at 136° Cent., the other at a temperature above the range of mercurial thermometers. The former of these is a colourless mobile liquid, possessing a fragrant smell, not unlike that of bitter almonds. The latter is a more consistent inodorous liquid, which solidifies at a low temperature into a mass of colourless crystals. The liquid having the boiling-point of 136° is nothing else than the *chloride of phenyle*. The crystalline body is the *phosphate of phenyle*, one of the most beautiful products in organic chemistry. In the liquid state it is slightly yellow by transmitted light, and it reflects the more refrangible rays with a fine opalescent appearance, due no doubt to the so-called epipolic refraction. The epipolic rays visible by ordinary daylight on and at some depth below its surface, are of a fine violet tint, differing decidedly from the blue colour exhibited by disulphate of quinine in like circumstances. The flame of sulphur does not bring out this effect more strongly than the diffused light of the sun.

Phosphate of phenyle dissolves in strong nitric acid with evolution of considerable heat, and the solution gives out nitrous fumes on ebullition. A heavy yellow oil is precipitated by water from this solution, and collects in drops which ultimately solidify, and their solidification is, singularly enough, accelerated by *hot* water, by reason of its more quickly dissolving out the nitric acid which at first holds the solid body in solution. *Nitrophosphate of phenyle* is an acid, and forms with potash a beautiful crystalline salt.

An alcoholic solution of phosphate of phenyle decomposes acetate of potash on ebullition. After the alcohol is distilled off, the temperature of the mixture rises rapidly on the application of further heat, and a limpid oleaginous substance, having a very peculiar odour, distils over, which possesses the composition of *acetate of phenyle*. This compound boils at 190° Cent.; it is heavier than water, and very slightly soluble in that liquid. It dissolves with decomposition in boiling potash.

Cyanide of phenyle is obtained by the action of the phosphate on cyanide of potassium. It is decomposed by boiling potash with evolution of ammonia.

Terchloride of phosphorus, when distilled with hydrate of phenyle, seems to act at first similarly to the pentachloride, but the phosphite of phenyle formed is decomposed by heat; and among the products of distillation is found a body boiling at 80° Cent., and possessing all the properties of *benzine*, i. e. *hydruret of phenyle*.

The formation of the *iodide of phenyle* is necessarily attended with some difficulty, owing to the circumstance of phosphorus not combining with more than three equivalents of iodine. Its boiling-point is 190° Cent.

Mr. Scrugham has had reason to confirm the statements of Laurent and Gerhardt respecting the *benzoate of phenyle*, and has prepared that compound in considerable quantities by the action of chloride of benzoyle on phenylate of potash. Chloride and phosphate of phenyle could not be made to react on benzoate of potash.

Chloride of cuminyle reacts with violence on phenylate of potash, with formation of *cuminate of phenyle*, a compound analogous to the benzoate.

Chloride of phenyle was heated with phenylate of sodium, with a view to the formation of *oxide of phenyle*, and there is no doubt that this compound was formed by the reaction, as the correlative product, chloride of sodium, was detected. But a further account of this and other reactions is deferred until the experimental investigation is more advanced.

Specimens of most of the compounds mentioned were exhibited.

ROYAL INSTITUTION OF GREAT BRITAIN.

March 31, 1854.—On Chemical Affinity among Substances in Solution. By John Hall Gladstone, Esq., Ph.D., F.R.S.

An historical sketch of the development of the ideas of chemists concerning "affinity" was first given. The dogma of Hippocrates that "like combines only with like," was shown to be superseded by the view of Glauber and others, that unlike substances combine most readily; and that where two bodies have an affinity *for* one another, it is a sign that they have *no* affinity *with* one another. The views of Newton and Boyle in reference to the different degrees of strength of affinity were then considered, and particular attention was directed to the doctrine of Bergmann, that when a decomposition takes place by means of the greater elective attraction of a third body, that decomposition is complete. In opposition to this, Berthollet contended that in all such cases of composition or decomposition there takes place a partition of the base, or subject of the combination, between the two bodies whose actions are opposed; and that the proportions of this partition are determined, not solely by the difference of energy in the affinities, but also by the difference of the quantities of the bodies—by their physical condition,—and by that of the combinations capable of being generated. These views did not meet with a favourable reception at the time of their promulgation; and the attention of chemists had been drawn away from the subject until within these last few years, when Malaguti, Bunsen, Debus, and Williamson have published investigations bearing upon the point. The lecturer then stated, that before any of these papers had appeared, he had been thinking of and performing some experiments upon the subject in question, and that he was still continuing them.

After a few experiments illustrative of "chemical combination" and of "elective affinity," others were introduced to show how easily this latter phænomenon was affected by circumstances. Thus ammonia will displace alumina from a solution of the sulphate, but on the other hand, alumina will displace ammonia when heated with the solid sulphate of that volatile base; whilst if solutions of chloride of aluminium and sulphate of ammonia be mixed and evaporated, crystals of the double sulphate, ammonia-alum, will appear. There were on the table two white salts; the one had been carbonate of baryta, but by boiling with excess of sulphate of potash, it had been converted into the sulphate; the other had been sulphate of baryta, but by long-continued boiling with much carbonate of potash, it had suffered the opposite change into the carbonate. The lecturer then stated that so great is the influence exerted by these various circumstances, that some have doubted whether there be a true "elective affinity;" he however believed that after making every allowance for known causes there is still a residuary phænomenon to which that name is the most appropriate. Allowing then, with Bergmann, that relative degrees of affinity exist, the question arises:— Is Berthollet's law also correct? It is very difficult to arrive at a satisfactory answer, since it is almost impossible to eliminate other influences. Several reactions, however, were mentioned as tending to show that there is some truth in the law:—for instance, the solution of gold in hydrochloric acid upon the addition of nitrate of potash. The experiments of Bunsen on mixtures of carbonic oxide and hydrogen, exploded with a quantity of oxygen insufficient for complete combustion; and those of Debus on the precipitation of mixed hydrates of lime and baryta by carbonic acid were explained; as also the remarkable fact noticed by both, that the resulting products were always in certain atomic proportions to one another. But in both these cases the first products of the chemical action are removed at once from the field: it is quite another case when they remain free to act and react on one another. Supposing they all remain in solution, the requisite is fulfilled; but how are we to know what has then taken place? Malaguti thought to obtain an indication of this by mixing the aqueous solutions of two salts, one of which is soluble in alcohol, and the other is insoluble, and then pouring them into very strong alcohol, and analyzing the salts immediately thrown down. His results are tabulated; they are valuable, but to some extent open to objection, on account of the disturbing influence of the alcohol. Some observations of Professor Graham, and others of Professor Williamson, as yet unpublished, were then spoken of, and the Lecturer proceeded to describe his own endeavours to arrive at a knowledge of the intimate constitution of a mixture of salts in solution by observing their physical properties, especially colour.

If solutions of one equivalent of nitrate of iron, and a triple equivalent of sulphocyanide of potassium be mixed, a blood-red colour results owing to the formation of sulphocyanide of the sesquioxide of iron; the question arises—Has all the iron left the nitric acid to unite itself with the sulphocyanogen? It has not; for on the addition of equivalent after equivalent of sulphocyanide of potassium, a deeper red is constantly obtained. The arrangement by which this deepening of colour was quantitatively determined was explained, and imitated on the lecture table. The result was that even up to 375 equivalents, a regular increase was observed to take place, more rapidly at first than afterwards, which was exhibited to the eye by the results being projected as a curve. Again, as in the mixture of equal equivalents of the two salts, some iron still remains in combination with the nitric acid, a portion of the potassium must still remain united to the sulphocyanogen. Accordingly the addition of more iron salt also gives a deeper colour. The curve expressing the results of this experiment was a regular continuation of the curve formerly mentioned; and neither of them exhibited any of those sudden transitions which the experiments of Bunsen and Debus present. Diagrams exhibiting curves of the gallate and meconate of iron were also exhibited. Various experiments were then performed, showing the alteration in the resulting colour upon any change of any of the elements in the primary experiment; for instance, the substitution of other acids for the nitric acid. or of other bases for the potash. On the addition of a colourless salt to a coloured one, there results a diminution of the colour greater than the mere dilution would have produced, as was exemplified in the cases of the red sulphocyanide of iron mixed with sulphate of potash, and of the scarlet bromide of gold mixed with chloride of potassium. The lecturer accordingly drew the conclusion that when two salts mix without precipitation or volatilization, the acids and bases frequently, if not universally, arrange themselves according to some definite proportion; and that this depends on the relative quantity of the two salts, as well as upon the proper affinities of the substances composing them. He was unable then to enter upon the influence of heat, or of dilution in certain cases, or to add any remarks connected with double salts, or with other metals, or upon certain practical applications of these views in chemical and physiological science.

The fact that we very frequently find the double decomposition of a salt to be *complete*, the *whole* of one of its constituents being precipitated, was shown to be easily explained on the principles of Berthollet. Thus, for instance, when chromate of potash and nitrate of silver are mixed, at the first moment a division will take place producing four salts, but one of these—the chromate of silver—is thrown down at once as a precipitate, and thus put out of the field of action. Another division of the acids with the bases must take place, producing of course more of the insoluble chromate, and so on, till at length the whole of the silver is removed. And that this is really what does take place is rendered almost certain by the fact that wherever by an interchange of acids and bases a precipitate can be produced, that precipitate does form; and, if the substance be perfectly insoluble, the whole is thrown down; this occurring in opposition to all rules of "affinity," and to all tables that Bergmann, or any other chemist, ever did or could construct. The volatility of one of the products acts in the same manner as insolubility, as is exemplified in the decomposition of carbonates by any other acid. Crystallization also is but another phase of the same phænomenon. An experiment was exhibited in illustration of this. Dilute solutions of nitrate of lime, and sulphate of soda, were mixed at the ordinary temperature without producing any separation of solid matter; but they were so proportioned that upon heating the mixture, the crystallization of some sulphate of lime was determined, and when once this had commenced, it progressed rapidly: resembling in that respect the ordinary phænomena of precipitation. If in a double decomposition a far larger quantity of a sparingly soluble salt be produced at the first moment than the water can dissolve, the crystals will be formed rapidly and will accordingly be very small in size; but should there be formed at once only just sufficient to determine a separation in the solid form, the crystals will grow gradually, and will often attain a large size. This was exemplified on the mixture of nitrate of silver with the sulphates of copper and of potash respectively.

It is possible that the law of Berthollet may not be universally applicable; yet the present advanced state of science shows that not only is there, as Bergmann insisted, a true chemical affinity, that is, a preference of one substance to combine with a certain other substance instead of a third, but, in a great number of instances at least, this substance will combine with both according to certain proportions, whenever the whole of the affinities can be brought into play at the same time.

April 7.—On Silica and some of its applications to the Arts. By the Rev. J. Barlow, M.A., F.R.S., Vice-President, and Sec. R.I.

Silica is one of the most abundant substances known. Quartz, common sand, &c., flint, chalcedony, opal, &c,, and a variety of sand described by Mr. J. T. Way*, may respectively be taken as examples of crystallized and uncrystallized silica. Under all these forms silica is capable of combining with bases as an acid. Heat is however essentially necessary to effect this combination, a combination of which all the well-known silicates, whether natural, as felspar, mica, clay, &c., or artificial, as glass, slags, &c., are the results. The common forms of insoluble glass are produced by the union of silica with more than one base. But, when combined with an alkaline base only, silica forms a soluble glass, the degree of solubility of which depends on the proportion which the silicic acid bears to this alkaline base . . . . This soluble silicated alkali (or water-glass) may be prepared by various processes. If sand be used, 15 parts of fine sand, thoroughly incorporated with 8 parts of carbonate of soda, or with ten of carbonate of potash, and one of charcoal fused in a furnace, will produce a silicated alkali which is soluble in boiling water. Messrs. Ransomes obtained this silicated alkali by dissolving broken flints in a solution of caustic alkali at a temperature of 300° Fahr. And more recently, Mr. Way has observed that the sand which he has described will combine with caustic alkali at boiling heat, also producing a water-glass.

This water-glass has been applied to several important purposes, three of which were specially noticed.

I. *To protect Building-stones from decay.*—The stone surfaces of buildings, by being exposed to the action of the atmosphere, become liable to disintegration from various causes. Moisture is absorbed into their pores. The tendency of their particles to separate, in consequence of expansion and contraction, produced by alternation of temperature, is thus increased. Sulphurous acid is always present in the atmosphere of coal-burning cities, and cannot but corrode the calcareous and magnesian ingredients of oolites and dolomites. It is true that good stone resists these sources of injury for an indefinite time, but such a material is rarely obtained. As a preventive of destruction, whether arising from physical or chemical causes, it has been proposed to saturate the surfaces of the stones with a solution of the water-glass.

It is well known that the affinity of silica for alkali is so feeble that it may be separated from this base by the weakest acids, even by carbonic acid. According to the expectation of those who recommend the silification of stone, the carbonic acid of the atmo-

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* Quarterly Journal of Chemical Society, July 1, 1853, and Journal of Royal Agricultural Society, vol. xiv. part 1.

sphere will set the silica free from the water-glass, and the silica, thus separated, will be deposited within the pores and around the particles of the stone. The points of contact of these particles will thus be enlarged, and a sort of glazing of insoluble silica will be formed, sufficient to protect the stone against the effects of moisture, &c. This cause of protection applies chiefly to sand-stones. But wherever carbonate of lime or carbonate of magnesia enters notably into the composition of the building-stone, then an additional chemical action, also protective of the stone, is expected to take place between these carbonates and the water-glass. Kuhlmann remarks, "Toutes les fois que l'on met en contact un sel insoluble avec la dissolution d'un sel dont l'acide peut former avec la base du sel insoluble un sel plus insoluble encore, il y a échange; mais le plus souvent cet échange n'est que partiel [*]." In consequence of this "partial exchange" an insoluble salt of lime may be looked for whenever a solution of water-glass is made to act on the carbonate of lime or carbonate of magnesia existing in oolitic or dolomitic building-stones.

This expectation, however, has not been altogether sanctioned by experiment. A gentleman, eminently conversant with building materials[†], immersed a piece of Caen-stone in a solution of silicate of potash in the month of January 1849. This fragment, together with a portion of the block from which it had been separated, was placed on the roof of a building in order that it might be fully exposed to the action of atmosphere and climate. After five years the silicated and the unsilicated specimens were found to be both in the same condition, both being equally corroded. These specimens were exhibited in the Theatre of the Institution. But whatever ultimate results may ensue from this process, the immediate effects on the stone are remarkable. Two portions of Caen-stone were exhibited, one of which had been soaked in a solution of water-glass two months before. The surface of the unsilicated specimen was soft, readily abraded when brushed with water, and its calcareous ingredients dissolved in a weak solution of sulphurous acid. The silicated surface, on the other hand, was perceptibly hard, and resisted the action of water and of dilute acid when similarly applied[‡].

II. Another proposed use of the water-glass is that of *hardening cements, mortar*, &c., so as to render them impermeable by water.

Fourteen years since Anthon[§] of Prague proposed several applications of the water-glass. Among others he suggested the rendering mortars water-proof. He also suggests that this substance might

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[*] Expériences Chimiques et Agronomiques, p. 120.

[†] Charles H. Smith, Esq., one of the authors of the 'Report on the Selection of Stone for the Building of the New Houses of Parliament.'

[‡] Silliman's American Journal, January, 1854, contains a notice of the application of the water-glass to the decaying surfaces in the Cathedral of Nôtre Dame in Paris.

[§] Neuere Mittheilungen über die Nutzanwendung des Wasser-Glases, 1840. This subject has also been fully treated by Kuhlmann in his 'Mémoire sur l'Intervention de la potasse ou de la soude dans la formation des chaux hydrauliques,' &c., 1841.—*Expériences Chimiques et Agronomiques.*

be beneficially employed as a substitute for size in white-washing and staining walls. It was demonstrated by several experiments that carbonate of lime, mixed up with a weak solution of water-glass, and applied as a whitewash to surfaces, was not washed off by sponging with water, and that common whitewash, laid on in the usual manner with size, was rendered equally adhesive when washed over with water-glass.

III. *The Stereochrome of* Fuchs.—The formation of an insoluble cement by means of the water-glass, whenever the carbonic acid of the atmosphere acts on this substance, or whenever it is brought in contact with a lime-salt, has been applied by Fuchs to a most important purpose. The stereochrome is essentially the process of fresco secco[*] invested with the capability of receiving and perpetuating works of the highest artistic character, and which may be executed on a vast scale. Fuchs's method is as follows[†]:—

"Clean and washed quartz-sand is mixed with the smallest quantity of lime which will enable the plasterer to place it on the wall. The surface is then taken off with an iron scraper, in order to remove the layer formed in contact with the atmosphere; the wall being still moist during this operation. The wall is then allowed to dry; after drying it is just in the state in which it could be rubbed off by the finger. The wall has now to be *fixed, i. e.* moistened with water-glass[‡]. [An important point is not to use too much water-glass in moistening the wall.] This operation is usually performed with a brush. The wall must be left in such a condition as to be capable of receiving colours when afterwards painted on. If, as frequently happens, the wall has been too strongly fixed, the surface has to be removed with pumice and to be fixed again. Being fixed in this manner the wall is suffered to dry. Before the painter begins, he moistens the part on which he purposes to work with distilled water, squirted on by a syringe. He then paints: if he wishes to repaint any part, he moistens again. As soon as the picture is finished, it is syringed over with water-glass. After the wall is dry, the syringing is continued as long as a wet sponge can remove any of the colour. An efflorescence of carbonate of soda sometimes appears on the picture soon after its completion. This may either be removed by syringing with water, or may be left to the action of the atmosphere." Not to dwell on the obvious advantages possessed by the stereochrome over the real fresco, (such as its admitting of being retouched and its dispensing with joinings), it appears that damp and atmospheric influences, notoriously destructive of real fresco, do not injure pictures executed by this process.

[*] Vide Eastlake's Materials for a History of Oil Painting, p. 142.

[†] These particulars were obtained by Dr. Hofmann from Mr. Echter. A stereochromic picture by Echter and a sample of the water-glass as prepared in Munich were also exhibited by Dr. Hofmann.

[‡] The composition of the specimen was—

	per cent.
Silica	23·21
Soda	8·90
Potash	2·52

[The specific gravity of the solution 3·81.]

The following crucial experiment was made on one of these pictures. It was suspended for twelve months in the open air, under the principal chimney of the New Museum at Berlin; "during that time it was exposed to sunshine, mist, snow, and rain," and nevertheless "retained its full brilliancy of colour."

The stereochrome has been adopted on a grand scale by Kaulbach in decorating the interior of the great national edifice at Berlin already alluded to. These decorations are now in progress, and will consist of historical pictures (the dimensions of which are 21 feet in height and 24¾ in width), single colossal figures, friezes, arabesques, chiaro scuro, &c. On the effect of the three finished pictures, it has been remarked by one whose opinion is entitled to respect, that they have all the brilliancy and vigour of oil paintings, while there is the absence of that dazzling confusion which new oil paintings are apt to present, unless they are viewed in one direction, which the spectator has to seek for.

Mr. A. Church has suggested that if the surface of oolitic stones (such as Caen-stone) is found to be protected by the process already described, it might be used, as a natural *intonaco*, to receive coloured designs, &c. for exterior decorations; the painting would then be cemented to the stone by the action of the water-glass.

Mr. Church has also executed designs of leaves on a sort of terra cotta, prepared from a variety of Way's silica rock, consisting of 75 parts clay and 25 of soluble silica. This surface, after being hardened by heat, is very well adapted for receiving colours in the first instance, and for retaining them after silication.

LIX. *Intelligence and Miscellaneous Articles.*

ON A NEW METEORITE FROM NEW MEXICO.

BY DR. F. A. GENTH.

I AM indebted to Prof. Joseph Henry, Secretary of the Smithsonian Institution, for a small piece of an interesting meteorite from New Mexico. It was labelled "native iron," and is said to occur there in large quantities. Fortunately it was just sufficient for an examination, the results of which I here give. There is no doubt that the mineral is of meteoric, and not of telluric origin.

It is very crytalline, and shows a distinct octahedral cleavage. Its colour is iron-gray, its lustre metallic. Quite ductile. Spec. grav. (at 18° Cels.)=8·130.

Dissolves readily in diluted nitric acid, leaving a small quantity of insoluble residue, which, however, was also slowly dissolved by strong nitric acid or aqua regia, but still more easily by fusion with bisulphate of potash.

The methods used for its analysis were the following:—In analysis I. the meteorite was dissolved in strong nitric acid; nickel and cobalt were separated from iron by carbonate of baryta; nickel and cobalt were separated by hydrocyanic acid, potash, and oxide of mercury.

In analysis II. the meteorite was dissolved in diluted nitric acid and the residue filtered off on a weighed filter. In the filtrate, iron was separated from cobalt and nickel by addition of a sufficient quantity of acetate of potash, in order to convert the nitrates into acetates, and evaporation to dryness in a water-bath. The dry mass was boiled with water and filtered. From the filtrate, which contained the whole quantity of oxides of cobalt and nickel, these were precipitated by caustic potash. The precipitate of sesquioxide of iron was redissolved in hydrochloric acid and precipitated by ammonia. This method gives excellent results if used with care; the only objection might be, that the sesquioxide of iron thus separated is difficult to filter.

The insoluble residue was ignited and fused with bisulphate of potash. On treating the fused mass with water, a white substance of the appearance of titanic acid remained, which hydrochloric acid slowly dissolved. This substance and sesquioxide of iron were precipitated by ammonia, and from the filtrate, oxide of nickel separated as usual. The precipitate was weighed, dissolved in hydrochloric acid, and the iron precipitated by sulphide of ammonium after the addition of tartaric acid and ammonia. From the sulphide of iron the iron was determined as usual. From the filtrate, the other substance remained after the tartaric acid was destroyed by heat. It was, however, a very small quantity, and only sufficient for *one* blowpipe reaction. The borax bead gave in the inner flame an enamel of a bluish colour. I therefore believe that it is titanic acid, though the reactions somewhat differ.

The insoluble residue seems to be a combination of iron, nickel, and titanium. It contains *no* cobalt. Neither part of the meteorite contained carbon, sulphur, phosphorus or tin.

	I.	II.
Iron..........	=96·17	95·92
Nickel........	= 3·07	3·57
Cobalt........	= 0·42	
Insoluble......	= ..	0·57
	99·66	100·00

The insoluble part consisted of a steel-coloured powder in microscopic crystals, which showed three-sided planes. Its composition is—

Iron............ =55·07 per cent.
Nickel.......... =28·78 ..
? Titanium ...... =16·15 ..
100·00

It is remarkable that the elements in the insoluble part are in the following ratio:— Fe : Ni : ? Ti=6 : 3 : 2.

—Silliman's *American Journal*, March 1854, p. 239.

ON M. FOUCAULT'S PENDULUM EXPERIMENTS*.

Your Committee having undertaken to make this experiment with as much care as possible, have the honour to submit the following report on the results obtained by them.

Extracted from the Second Report of the Special Committee of the ry and Historical Society of Canada.

A carefully turned spherical ball of lead, 5·2 inches in diameter and weighing about 17 lbs., was employed as the weight, and suspended in the passage of the Quebec Music Hall, where a height of 60 feet was obtained. This weight was suspended by a fine steel wire, on one end of which a fine screw was turned, by means of which the wire was fastened to the ball from which the pendulum was suspended.

The following were the arrangements adopted by your Committee at the point of suspension. A small spherical ball of brass was ground into a hemisphere in a plate of the same metal. A hole was drilled through the centre of the hemisphere for the wire, and sufficiently large to allow the pendulum to vibrate in the required arc without coming into contact with the plate. The wire was screwed into the ball of suspension.

In order to start the pendulum for the experiments, a cotton thread was passed round the ball and tied over two pins in a heavy moveable block. When the weight secured in this manner had been brought to a state of rest, the thread was fired with a taper, and the pendulum commenced vibrating, the thread falling to the ground. A circle, 10 feet in diameter, was described on the floor from a centre under the point of suspension, and graduated into degrees, by which the progress of the plane of vibration was measured.

The first series of observations recorded in the tables were made on the 14th, 15th and 16th; and the second series on the 19th and 20th of May, 1853.

The first series of observations gives the angle actually moved through in $47^{h}\ 18^{m}$ (after applying the correction for the progression of the apse due to elliptic motion) only $1°\ 56'$ less than that calculated. The second series gives an error of $2°\ 2'$ in $23^{h}\ 10^{m}$. These errors may be represented in time by about 10 and 12 minutes; and your Committee consider that these experiments agree so nearly with calculation as to be strong corroborative evidence of the correctness of the theory, that the time taken by the plane of vibration to perform a complete revolution varies approximately as the time of the latitude.

It may not here be out of place to give an explanation of the accompanying tables. Columns (1) and (2) refer to the times of observation; (3) denotes the nature of the ellipse described by the pendulum, showing, if there be no elliptic motion, or if elliptic motion, whether it is progressing or retarding; (4) shows the azimuthal angle observed; (5) the angle moved through, and (6) the time between the observations; (9) is the angle calculated; (7) is the difference between (5) and (9); (8) is the angle corrected for elliptic motion; and (10) is the difference between (8) and (9).

Your Committee have great satisfaction in submitting the results of the different experiments. In some instances they have varied considerably from the calculated angles; but in all these the fact that the pendulum had acquired an elliptic motion, would seem to point to that circumstance as the chief cause of disturbance, while in most of the experiments in which there was no elliptic motion, the angles, as nearly as could be measured, were equal to those calculated by theory. The whole respectfully submitted.

(Signed) A. Noble, Lieut. R.A. and V.P.
W. Darling Campbell.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.		
Date.	From	To	Ellipse.	Azimuthal angle observed.	Angle moved through.	Difference of time.	Difference between 7 and 9.	Corrected angle.	Calculated angle.	Difference between 8 and 9.	Remarks.	
	h m	h m		° ′	° ′	h m	° ′	° ′	° ′	° ′		
13	10 36	11 53	R.	14 45	14 45	1 17·0	+0 30	....	14 14·1	+0 30	Started N. and S. maj. and min. axis not measured.	
	12 3	2 0·30	N.	35 45	21 0	1 56·30	−0 28·5	....	21 28·4	−0 28·5		
	2 0·30	3 36	N.	53 20	17 35	1 36	+0 8·3	....	17 26·7	+0 8·3		
	3 36	4 6	N.	58 40	5 20	0 30·1	−0 8·9	....	5 28·9	−0 8·9		
	4 17	5 17	R.	71 0	10 0	1 0	−0 57·7	10 50	10 57·7	−0 7·7		
	5 21	6 21	R.	84 4	10 10	1 0	−0 47·9	11 0	10 57·7	+0 2·1		
	6 24	7 36	R.	98 30	12 30	1 12	−0 38·1	13 0	13 8·9	−0 8·9		
	7 52	8 53	N.	113 0	11 0	1 1·0	−0 8	....	11 8	−0 8		
	8 53	11 10	...	136 10	23 10	2 17	−1 50	....	25 1	−1 50	No elliptic motion recorded.	
	11 20	4 34	N.	193 0	56 50	5 14	−0 32	....	57 22	−0 30		
14	4 34	7 22	P.	226 0	33 0	3 48	+2 45	32 20	30 16	+2 4		
	7 46	0 30	N.	261 0	31 0	2 44	+1 46	....	29 55·4	+1 46		
	10 30	10 50	N.	264 30	3 30	0 20	−0 8	....	3 38	−0 8		
	10 57	12 57	R.	288 0	21 30	2 0	−0 25·8	21 52·8	21 55·8	−0 3	Ellipse slightly retarding.	
	1 11	3 11	R.	309 56	19 56	2 0	−2 0	20 56	21 55·8	−0 59·8		
	3 24	5 38	R.	332 30	20 30	2 14	−3 56·4	21 35	24 28·6	−2 53·6		
	5 26	9 45	R.	18 30	40 30	3 49	−1 21·2	41 25	41 51·2	−0 26·2		
	10 0	12 0	R.	43 5	21 40	2 0	−0 15·8	21 55·8	21 55·8			
15	12 0	2 0	P.	66 0	22 55	2 0	+1 0	23 1	21 55·8	+0 45		
	2 12	6 12	R.	110 30	42 31	4 0	−1 21·6	43 51·6	43 51·6			
	6 33	11 53	N.	174 10	59 10	5 20	+0 42·5	....	58 27·5	+0 42·5		
						47 18·30	−8 8·2			−1 56·8		
											Max. axis.	Min. axis.
19	4 11	7 11	R.	30 44	30 46	3 0	−2 10	30 49·5	32 53	−2 3·5	15″	$\frac{1}{8}$
	7 50	9 45	R.	53 14	22 34	1 55	+1 33	22 51·3	21 1	+1 50·3	22	$\frac{1}{4}$
	9 55	10 44	N.	62 12	8 58	0 48	+0 1	8 58	8 57	+0 1		
	10 44	1 1	R.	86 35	24 23	2 17	−0 39	24 44	25 2	−0 17·9	15	$\frac{1}{4}$
20	1 29	2 59	R.	100 50	14 15	1 30	−2 11	15 29	16 24	−0 57	30	1
	3 8	6 8	R.	129 0	28 10	3 0	−4 43	29 24	32 53	−3 29	15	1
	6 20	8 50	R.	151 30	22 30	2 30	−4 54	23 29·2	27 24	−3 54·8	18	$\frac{4}{5}$
	9 20	11 20	R.	172 20	20 55	2 0	−1 0	21 32·8	21 55	−0 22·2	23	$\frac{1}{2}$
	11 34	2 43	P.	209 40	37 20	3 9	+2 54	36 52·8	34 30	+2 22·8	14	$\frac{3}{5}$
	2 55	5 55	P.	248 0	38 20	3 0	+5 27	37 40·5	32 53	+4 47·5	16	$\frac{1}{2}$
						23 10	−5 45			−2 2·3		

ON THE SUPPOSED CONVERSION OF AMMONIA INTO NITRIC ACID IN THE ANIMAL ORGANISM. BY CHRISTIAN JAFFÉ.

A series of experiments have recently been published by Dr. Bence Jones, from which it is inferred, that after introducing into the stomach a large quantity of ammoniacal compounds, nitric acid appears in the urine as a product of their oxidation.

From the extreme apparent improbability that such an oxidation could be effected in the animal organism, and at the instigation of Prof. Lehmann, the author has repeated the experiments upon which this inference is founded; and has come to the conclusion, that the method which Dr. Bence Jones adopted for detecting the presence of nitric acid in the urine is altogether inapplicable, and consequently that the inference that ammonia is oxidized within the organism is perfectly unfounded.

The method consists in distilling the somewhat concentrated urine with concentrated sulphuric acid until about one-half or two-thirds have passed over, treating the distillate with carbonate of potash, evaporating and testing the residue for nitric acid by means of starch, iodide of potassium and dilute hydrochloric acid, or with indigo.

The author believes that this process is inapplicable, because, even admitting the presence of nitric acid, it would appear impossible to distil over this acid in the presence of urea, or indeed any other organic substance. In order to decide this point, the author added a few drops of nitric acid to normal urine, concentrated by evaporation, and then distilled with sulphuric acid. The distillate, treated with starch and iodide of potassium, gave a reaction, which, although readily accounted for by subsequent experiments, at first led the author to form the erroneous opinion that a distillation of undecomposed nitric acid from urine actually took place. Normal urine, distilled with concentrated sulphuric acid, gave a liquid which became intensely blue when treated with starch and iodide of potassium, while at the same time it gave no indication of nitric acid with protosulphate of iron. It soon became evident that this reaction was altogether owing to the presence of sulphurous acid, formed by the action of the sulphuric acid upon the organic substances, and which, as is well known, causes a separation of iodine. It thus becomes easily intelligible why Dr. Bence Jones so seldom obtained this reaction in normal urine, as he saturated the distillate with carbonate of potash, and evaporated.

The presence of sulphurous acid in the distillate, and the dependence of the reaction upon it, was demonstrated in the following manner:—

In the first instance, normal urine was distilled without any addition of acid; and, as was to be expected, the distillate gave no reaction. Normal urine was then distilled with sulphuric acid; the distillate gave a reaction with starch and iodide of potassium; when tested with protochloride of tin for sulphurous acid, it acquired a yellow colour, but even on boiling no precipitate was formed, a result which might fairly be attributed to the small quantity of sulphurous acid. In order to prove more decisively that the reaction was owing to this small quantity of sulphurous acid, normal urine was distilled with phosphoric instead of sulphuric acid; and the distillate when tested did not give the slightest trace of a reaction. Lastly, when a distillate which at first gave the reaction was allowed to stand twenty-four fours, it no longer gave it, but the presence of sulphuric acid could be distinctly ascertained.

Thus then it appears that an oxidation of ammonia to nitric acid in the animal organism has not by any means been proved by Dr. Bence Jones's experiments. It remained, therefore, necessary to ascertain by some other means whether such a formation of nitric acid really took place. For this purpose, concentrated urine, mixed with a few drops of nitric acid, was distilled with phosphoric acid; and the distillate did not give the slightest reaction either with starch and iodide of potassium or with protosulphate of iron.

After these results, the author considers it unnecessary to enter more fully upon the consideration of the various deductions from Dr. Bence Jones's experiments; such, for instance, as the assumed formation of nitric acid from urea introduced into the stomach, especially as Frerich has already proved that this is always attended by an increase in the quantity of urea in the urine.—*Journ. für Prakt. Chem.*, June 1853.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1854.

Chiswick.—March 1. Frosty: very fine: clear, with sharp frost at night. 2. Frosty: very fine: hazy. 3. Very dense fog: foggy: partially overcast: frosty. 4. Frosty, with slight fog: overcast. 5. Overcast: clear: dense fog. 6. Dense fog: foggy throughout. 7. Foggy: fine: overcast. 8. Cloudy: overcast: clear. 9. Overcast: very fine: overcast. 10. Cloudy: slight rain. 11. Fine: clear. 12. Slight fog: very fine. 13. Clear and fine. 14. Cloudy: slight rain. 15. Foggy: cloudy. 16. Cloudy and fine. 17. Clear throughout: frosty at night. 18. Slight haze: rain. 19. Rain: overcast. 20. Clear and cold. 21, 22. Fine. 23. Cloudy and cold. 24, 25. Overcast. 26. Cloudy. 27. Overcast: very fine. 28. Cloudy: very fine. 29. Clear: very fine. 30. Overcast: fine. 31. Clear throughout.

Mean temperature of the month	42°·54
Mean temperature of March 1853	37 ·41
Mean temperature of March for the last twenty-eight years.	42 ·23
Average amount of rain in March	1·36 inch.

Boston.—March 1—3. Fine. 4, 5. Cloudy. 6, 7. Foggy. 8, 9. Cloudy. 10. Fine. 11. Cloudy. 12, 13. Fine. 14. Cloudy: rain A.M. 15. Fine. 16. Cloudy: rain A.M. 17. Fine: rain A.M. and P.M. 18. Cloudy. 19. Fine. 20—26. Cloudy. 27. Fine: rain A.M. 28. Cloudy. 29. Fine. 30. Cloudy. 31. Fine.

Sandwick Manse, Orkney.—March 1. Cloudy A.M. and P.M. 2. Bright A.M.: cloudy P.M. 3. Clear, fine A.M.: clear, aurora P.M. 4. Clear, fine A.M.: clear P.M. 5. Bright A.M.: drizzle P.M. 6, 7. Cloudy A.M. and P.M. 8. Bright A.M.: clear P.M. 9. Rain A.M.: cloudy P.M. 10. Bright A.M.: showers P.M. 11. Rain A.M.: cloudy P.M. 12. Bright A.M.: clear P.M. 13. Bright A.M.: cloudy P.M. 14. Cloudy A.M.: fine P.M. 15. Clear A.M.: cloudy P.M. 16. Cloudy A.M.: clear P.M. 17. Cloudy A.M.: drops P.M. 18. Cloudy A.M.: showers P.M. 19, 20. Cloudy A.M.: clear, aurora P.M. 21. Bright A.M.: cloudy, aurora P.M. 22. Cloudy A.M. and P.M. 23. Cloudy A.M.: clear P.M. 24. Cloudy A.M. and P.M. 25. Cloudy A.M.: clear, aurora P.M. 26. Bright A.M.: clear, aurora P.M. 27. Showers A.M.: cloudy P.M. 28. Clear A.M.: cloudy, hazy P.M. 29. Cloudy A.M.: cloudy, drizzle P.M. 30. Clear A.M. and P.M. 31. Clear A.M.: cloudy P.M.

Mean temperature of March for twenty-seven previous years.	40°·37
Mean temperature of this month	45 ·14
Mean temperature of March 1853	38 ·24
Average quantity of rain in March for thirteen previous years	2·59 inches.

The mean temperature of this month is higher than that of any March for the last twenty-seven years, and the barometer on the 4th was as high as on any day during the same period except on two, viz. 27th December 1840, when it was 30·72, and on 1st February 1841, when it was 30·76.

On the 30th, about 8 o'clock P.M., a comet with a long tail was seen N.W. about 7° above the horizon, and remained visible to the naked eye, and attracting attention for an hour and a half, but has not been seen since then, as the sky was too cloudy for several evenings.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1854. March.	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·702	30·677	30·34	30·16	30·30	54	18	34	47½	45	sw.	w.	s.	....	....	·03
2.	30·668	30·627	30·36	30·24	30·23	55	18	32	46	46	s.	s.	sw.	....	....	....
3.	30·628	30·509	30·32	30·42	30·61	54	18	30	44	43	e.	calm	w.	....	....	....
4.	30·752	30·705	30·44	30·71	30·60	50	33	42·5	44	42	e.	e.	w.	....	....	....
5.	30·714	30·570	30·37	30·52	30·46	45	20	36	46	45	se.	w.	w.	....	....	....
☽ 6.	30·636	30·437	30·20	30·36	30·19	44	24	28	47	40	e.	w.	sse.	....	....	·07
7.	30·424	30·377	30·10	30·05	29·83	50	44	30	47	46½	sw.	wsw.	s.	....	....	·10
8.	30·332	30·285	29·93	29·69	29·79	55	46	50	46½	45	sw.	wsw.	sw.	....	....	....
9.	30·258	30·195	29·83	29·60	29·48	62	47	52	48½	50	sw.	wsw.	sw.	....	....	·33
10.	30·101	29·991	29·56	29·35	29·52	56	40	55	44	41	sw.	ssw.	w.	·01	....	·19
11.	30·140	30·101	29·70	29·41	29·71	61	27	50	48	47	sw.	sw.	sw.	....	....	·53
12.	30·061	29·924	29·68	29·60	29·61	63	27	50	47	46	s.	sw.	s.	....	....	....
13.	29·960	29·881	29·60	29·57	29·22	64	43	43	47	48	sw.	ssw.	s.	....	....	....
○ 14.	30·019	29·851	29·47	29·45	29·75	64	27	49	47½	40	sw.	s.	se.	·02	....	....
15.	30·157	30·128	29·80	29·83	29·63	57	44	39	45	46	s.	s.	se.	·01	·06	....
16.	30·231	30·070	29·65	29·66	29·98	58	25	50	46	41	sw.	ssw.	sw.	·01	....	·09
17.	30·529	30·396	30·10	30·16	30·01	53	23	36	43½	44½	n.	e.	se.	....	·07	....
18.	30·212	29·957	29·80	29·75	29·97	53	35	43	46	41½	sw.	w.	se.	·19	·23	·09
19.	30·141	29·907	29·76	30·39	30·52	45	31	42	45½	37½	e.	ne.	n.	·15	....	....
20.	30·358	30·278	30·07	30·58	30·58	45	31	38	45	40½	ne.	e.	n.	·01	....	....
☾ 21.	30·404	30·321	30·06	30·61	30·61	49	27	40	43	43	ne.	ne.	wnw.	....	....	....
22.	30·498	30·419	30·20	30·52	30·44	49	42	42	45½	45	ne.	n.	wnw.	....	....	....
23.	30·399	30·349	30·10	30·40	30·37	52	23	44	46½	42	nw.	n.	wnw.	....	....	....
24.	30·271	30·203	30·00	30·29	30·01	48	23	41·5	47	45	ne.	n.	nw.	....	....	....
25.	30·106	29·954	29·72	29·81	29·82	48	40	42	44	37	nw.	w.	ne.	....	....	·06
26.	30·061	29·876	29·53	29·83	29·92	56	36	47	46	42½	nw.	nw.	w.	·02	....	....
27.	30·199	30·192	29·85	29·69	29·93	56	28	40	47	46	w.	sw.	w.	....	·06	·07
● 28.	30·379	30·295	29·93	30·00	29·97	61	28	51	50	46½	nw.	n.	wsw.	....	....	....
29.	30·387	30·312	29·94	29·81	29·81	60	31	49	53½	47	w.	......	sw.	....	....	....
30.	30·294	30·212	29·87	29·90	29·79	58	28	51	48½	44	sw.	ssw.	w.	....	....	·04
31.	30·401	30·354	30·00	29·98	29·83	57	29	45	46	48	w.	nnw.	w.	....	....	·05
Mean.	30·336	30·237	29·94	30·010	30·015	54·25	30·83	42·6	46·38	43·91				0·42	0·42	1 65

THE
LONDON, EDINBURGH AND DUBLIN
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[FOURTH SERIES.]

JUNE 1854.

LX. *On a new and simple Method of determining the amount of Urea in the Urinary Secretion. By* EDMUND W. DAVY, *A.B., M.B.T.C.D., Lecturer on Chemistry in the Carmichael School of Medicine, &c., Dublin*[*].

UREA has long been regarded with much interest by scientific men, on account of its physiological and chemical relations. It represents one of the last stages of a series of metamorphoses or changes which nitrogenous matter undergoes in the animal œconomy, and is the form under which the detritus of pre-existing nitrogenous tissues which have become effete, principally pass from the system. This interesting organic base, urea, is not only formed during the exercise of the vital functions in man and some of the higher animals, but is also produced during the chemical decomposition of a number of substances containing nitrogen; and the chemist can now obtain it in any quantity by artificial means, and thus imitate one of the most important results of the chemistry of life.

In reference to medicine, urea is not without some practical interest, as it is well known that during various diseased conditions of the system the quantity of urea eliminated from the blood by the action of the kidneys and excreted in the urine is occasionally subject to great variation, and some ready means of ascertaining its quantity in that secretion might frequently aid the physician in forming his diagnosis of certain diseases.

Different means of effecting this object have from time to time been proposed; but all the methods hitherto recommended, with the exception, perhaps, of Baron Liebig's recent one, require for

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[*] Communicated by the Author.

their execution much time and trouble, and in some cases complicated and expensive apparatus; and though capable in experienced hands of yielding tolerably accurate results, would in the case of those not much practised in chemical manipulation, and perhaps unaware of the many sources of error to be guarded against, give anything but correct results, and are therefore inapplicable to the greater number of those desirous of a quick and easy method of determining the quantity of urea in urine.

The method I propose is one of extreme simplicity, and can be performed by almost any one in a very few minutes, and is capable of yielding results sufficiently accurate for all practical purposes. It is founded on the fact I have recently observed, that urea is very readily decomposed by the chlorides, or rather hypochlorites of soda, potash or lime; and its constituent nitrogen is evolved in the gaseous state, and from the quantity of gas evolved I estimate the amount of urea present.

After trying different means of carrying out that fact with a view of making it available to determine the quantity of urea in urine, I found that the following very simple one seemed to answer the purpose completely. I take a strong glass tube, about 12 or 14 inches long, closed at one end, and its open extremity ground smooth, and having the bore not larger than the thumb can conveniently cover. This I fill more than a third full of mercury, and afterwards pour in carefully a measured quantity of urine to be examined, which may be from a quarter of a drachm to a drachm or upwards, according to the capacity of the tube; then holding the tube in one hand near its open extremity, and having the thumb in readiness to cover the aperture, I quickly fill it completely full with a solution of the hypochlorite of soda (taking care not to overflow the tube), and then instantly cover the opening tightly with the thumb, and having rapidly inverted the tube once or twice to mix the urine with the hypochlorite, I finally open the tube under a saturated solution of common salt in water, contained in a steady cup or small mortar. The mercury then flows out and the solution of salt takes its place, and the mixture of urine and hypochlorite being lighter than the solution of salt, will remain in the upper part of the tube, and will therefore be prevented from descending and mixing with the fluid in the cup. A rapid disengagement of minute globules of gas soon takes place in the mixture in the upper part of the tube, and the gas is there retained and collected. The tube is then left in the upright position till there is no further appearance of minute globules of gas being formed, the time being dependent on the strength of the hypochlorite and the quantity of urea present; but the decomposition is generally completed in from three to four hours; it may, however, be left much longer, even for a day if convenient, and having set the experiment going, it requires no further attention; and when the decomposition is completed, it is only necessary to measure the quantity of gas produced by transferring it into a graduated tube or measure.

I have generally used a graduated tube in the first instance, as it saves the trouble of transferring the gas and incurring the risk of losing some of it in the process. That which I would recommend as being convenient for this purpose is a stout tube having a bore of half an inch in diameter, and capable of holding from 2 to 3 cubic inches. A tube having this bore and about 14 inches in length, will hold $2\frac{1}{2}$ cubic inches, which will be quite large enough. Each cubic inch of it should be divided into tenths and hundredths of a part of a cubic inch.

It is scarcely necessary to remark, that in cases where great accuracy is required, due attention must be paid to the temperature and atmospheric pressure, and certain corrections made if these should deviate from the usual standards of comparison at the time of reading off the volume of the gas; but in most cases sufficiently near approximations to accuracy may be obtained without reference to those particulars. From a number of experiments, I have ascertained that the quantity of gas evolved from different amounts of urea, treated in the way I have just described, very closely approximates to the quantity of nitrogen gas which should be furnished from the urea by calculation. This will be seen from the following, taken from many experiments.

The fifth part of a grain of urea should furnish by calculation 0·3098 parts of a cubic inch of nitrogen gas at 60° F. and 30′ bar.; the same quantity of urea treated as described furnished in one experiment 0·3001, and in another 0·3069 parts of a cubic inch of gas at the same temperature and pressure; which shows that the calculated quantity of nitrogen differs from the amount of nitrogen gas obtained by only a few thousandths of a part of a cubic inch. I may observe that I was obliged to operate on such small quantities of urea, on account of the graduated tube I had at the time being only of one cubic inch capacity.

Seeing, then, that the quantity of gas evolved agrees so very closely with the calculated amount of nitrogen present in a certain quantity of urea, I take the calculated amount as being the more correct; and knowing the relation that exists between a certain quantity of urea and nitrogen, I can from the quantity of gas evolved in any case easily calculate the amount of urea present by the simple process of rule of three. Thus the fifth part of a grain, or 0·2 of a grain of urea, gives by calculation 0·3098 parts of a cubic inch of nitrogen gas. Then 0·3098 : the volume of gas found : : 0·2 : to the required quantity of urea; or multiplying the first and third terms by 5, we have 1·549 cubic inch of gas representing one grain of urea, which is a simpler proportion. Using these data, I made several comparative experiments on different samples of urine with my method and that of Baron Liebig's, which I believe is considered one of the most accurate of the methods of determining urea in urine at present known. The following are the results of three comparative experiments on different samples of urine, using the same with each method.

Amount of urea in grains and parts of a grain in one fluid ounce of urine.

	Liebig's.	New method.
1st experiment . .	3·680	3·712
2nd experiment . .	5·328	5·472
3rd experiment . .	4·976	4·976

In the first and second experiments the quantity of common salt present in the urine was taken into account, as it is found to increase to a slight degree the apparent quantity of urea in the urine by Liebig's method. In the third this was not taken into consideration, and the quantity of urea was compared with the mean of two results obtained by my method. These experiments show how very closely the results obtained by the new method agree with those by Baron Liebig's, and therefore show the correctness of the former method.

I prefer the hypochlorite of soda to that of potash as a decomposing agent, because the soda salt is an article of our pharmacopœia under the name "Sodæ chlorinatæ liquor," and therefore can be so easily procured; whereas the potash salt, not being used in medicine, would require to be specially made for the purpose. As to the hypochlorite of lime, I do not think it so effectual, and it has the disadvantage of soiling the sides of the graduated tube by the carbonate of lime formed in the reaction.

In reference to the quantity of hypochlorite of soda to be employed, it should always be used in excess; and I think that about five or six times the volume of the urine employed would be found generally to be quite sufficient, and ensure there being an excess of the hypochlorite. The amount required may be easily determined also by direct experiment, by adding to a certain quantity of urine to be examined in a glass a measured quantity of the hypochlorite, and leaving it for a short time till the evolution of gas is nearly over; then if, on the addition of more of the hypochlorite, the effervescence is renewed, it shows that there was not enough of the decomposing liquor first employed, and more must be added from time to time till no further evolution of gas is produced; and the quantity of hypochlorite used to arrive at this point indicates the amount necessary. I found by experiment that one grain of urea requires somewhere about half a fluid ounce of the ordinary sodæ chlorinatæ liquor for its complete decomposition. The amount of mercury employed requires some little attention. It should, as a general rule, be never less than the volume of gas produced; for if the volume of gas evolved is more than that of the mercury used, it will be more than that of the solution of salt, and therefore some of the mixture of urine and hypochlorite will be forced out of the tube before it is completely decomposed, and consequently some of the gas will be lost; so that if this occurs, we must repeat the experiment, using either a larger quantity of mercury if our tube will allow, or diminishing the quantity of urine employed.

It might be supposed on first sight that this method would be liable to the following source of error, viz. that some of the gas would be evolved and lost during the pouring in of the hypochlorite; but this is not the case, as several seconds elapse before there is any apparent reaction or evolution of gas on mixing the hypochlorite with the urine, and there is therefore full time to perform the experiment without any loss of the gas. I have also ascertained that the acid reaction of the urine does not affect my method. I should observe that this new method, like all the others known, is not perfectly free from some slight sources of error; the principal one being, that ammonia, if it exists in the urine, gives rise to nitrogen gas, and therefore increases the apparent amount of urea; but the same objection holds equally in Liebig's and Ragsky's methods, which are perhaps the two most accurate at present known. Uric acid also is similarly affected by the hypochlorite; but it and ammonia ordinarily occur in such small proportion in urine, that the error produced from these substances would be but trifling, and is partly corrected by taking the calculated quantity of nitrogen, which is, as I have shown, something more than that obtained from a certain quantity of urea by direct experiment.

In cases where ammonia or uric acid occurs in more than ordinary quantity, these substances must be separated by the usual means employed before having recourse to my method.

I should think that gently heating the urine with a certain quantity of baryta water as long as the odour of ammonia is disengaged, and then filtering the solution, as recommended by Liebig, for the separation of ammonia before applying his method (see the Quarterly Journal of the Chemical Society, vol. vi. p. 30), would effect the object very easily, and separate not only the ammonia, but also the greater part, if not all of the uric acid present.

There is one other source of error which may arise, and which can be easily avoided. It is the following: that if a solution of the hypochlorite of soda alone, or standing over mercury, be exposed to the light for several days, it will very gradually evolve a minute quantity of oxygen, which shows that in determining urea we should not allow the experiment to go on for too long a time; but if left for a day, or even two, it will scarcely make any appreciable effect on the quantity of gas evolved in testing for urea.

The reaction which appears to take place in the process seems to be the following. The hypochlorite of soda acting on the urea gives rise to the formation of carbonic acid, water, and chloride of sodium, together with the evolution of nitrogen gas. Thus

$$\overset{\text{Urea.}}{C^2.\,H^4.\,N^2.\,O^2.} + \overset{\text{3 Hypochlorite soda.}}{3(Cl,\dot{O}\,.\,NaO)} = 2CO^2 + 4HO + 3Cl.Na. + N^2.$$

The nitrogen is evolved and the carbonic acid is absorbed by some of the hypochlorite of soda in excess, for I find that this salt absorbs carbonic acid very quickly without evolving any other gas; and I failed in several experiments to detect the smallest portion of carbonic acid in the gas produced by acting on urea, though I have always noticed the presence of a very minute quantity of oxygen in the nitrogen gas. These appear from my experiments to be the changes produced; but this part of the subject I have not as yet minutely examined, and my experiments have hitherto been made on healthy urine; I have, however, ascertained that several of the substances found in urine during disease, as for example, sugar, albumen, bile, and excess of urinary colouring matter, produce scarcely any effect on the results obtained by this new method of determining the quantity of urea in the urinary secretion.

LXI. *Account of Experiments to determine the Velocities of the Rifle Bullets commonly used. By the* Rev. SAMUEL HAUGHTON[*].

THE following experiments were made for the purpose of ascertaining the reason of the alleged inferiority of the belted spherical bullet, used with the two-grooved rifle, as compared with elongated bullets of different kinds. The guns compared are the following:—

1. A two-grooved rifle; length 31·50 inches; diameter 0·66 inch; one turn in 4 feet.
2. The regulation Minié rifle; length 39 inches; diameter 0·69 inch.

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[*] Communicated by the Author.

3. Police carbine; length 28·75 inches; diameter 0·66 inch.

With these guns were used the following bullets:—

Two-groove rifle.—1. A Minié bullet, provided with two projections corresponding to the grooves of the rifle, without 'culot;' weight 697 grs. 2. A sugarloaf bullet, fired point foremost; weight 669·75 grs. 3. A belted, spherical bullet; weight 482 grs.

Minié rifle.—The regulation Minié bullet, with 'culot;' weight 744 grs.

Carbine.—Spherical bullet; weight 391 grs.

The method employed to determine the velocity of the bullets was Robins' ballistic pendulum; and the same quantity of the best gunpowder (40 grs.) was employed with each gun and bullet.

For the erection of the pendulum, and most efficient assistance afforded in the conduct of the experiments, I am indebted to Mr. Joseph Harris, of the firm of Truloch and Son, Dawson Street, Dublin, without whose aid I should have been unable to bring these experiments to a successful issue.

I shall first give the details of the experiments, and then mention the principal deductions which may be obtained from them.

The formula used in calculating the velocity is the following*:

$$v=\frac{gTa}{\varpi fc}\times nb, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where v = velocity of bullet in feet per second.
T = time of oscillation of pendulum.
a = distance of centre of gravity from axis of suspension.
ϖ = ratio of circumference of a circle to its diameter.
f = distance from axis of gun attached to pendulum to axis of suspension.
c = distance from axis of suspension to point of attachment of tape, by which the recoil is measured.
n = ratio of weight of pendulum to weight of bullet.
b = chord of arc of recoil, measured by tape.

The two-groove rifle barrel being firmly strapped with iron plates to the pendulum, the constants of the pendulum were carefully determined, and were as follows:—

$g=32{\cdot}195$ feet. $\varpi=3{\cdot}14159$ Weight of pend. $=36{\cdot}75$ lbs.
$T=1{\cdot}29$ sec. $f=75{\cdot}25$ in.
$a=57{\cdot}39$ in. $c=78{\cdot}25$ in.

From these data we obtain from (1)

$$v=0{\cdot}12894\times nb. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The following tables contain the results of the experiments

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* Poisson, *Traité de Mécanique*, vol. ii. p. 119.

made on the recoil of the two-groove rifle with the three bullets already described.

Table I.—Minié Bullet.

No.	*n*.	*b*.	*v*.
		in.	ft.
1.	369	17·50	833
2.	……	18·25	869
3.	……	17·25	821
4.	……	18·50	881
5.	……	18·00	857
6.	……	17·25	821

Mean velocity = 847 feet per second.

Mean quantity of motion measured in avoirdupois pounds moving through 1 foot per second = 84·33 lbs.

Table II.—Sugarloaf Bullet.

No.	*n*.	*b*.	*v*.
		in.	ft.
1.	384	17·50	866·2
2.	……	17·00	841·5
3.	……	17·37	859·8
4.	……	17·75	878·6
5.	……	17·62	872·3

Mean velocity = 863·7 feet.

Mean quantity of motion = 82·63 lbs.

Table III.—Belted Bullet.

No.	*n*.	*b*.	*v*.
		in.	ft.
1.	533	14·75	1013·3
2.	……	15·37	1055·9
3.	……	14·75	1013·3
4.	……	15·12	1038·7
5.	……	14·37	987·2

Mean velocity = 1021·68.

Mean quantity of motion = 70·39 lbs.

The Minié regulation-rifle barrel having been attached to the pendulum, formula (1) was calculated with the following constants, and the results are given in Table IV.

The carbine barrel was then attached to the pendulum and the recoil observed. The results are contained in Table V.

$g=32{\cdot}195$ feet. Weight of pend. and Minié barrel $=56{\cdot}50$ lbs.
$T=1{\cdot}29$ sec. Weight of pend. and carbine barrel $=55{\cdot}25$ lbs.
$a=61{\cdot}75$ in.
$\varpi=3{\cdot}14159$.
$f=74$ in.
$c=77$ in.

From these constants we find

$$v=0{\cdot}14326\times nb. \qquad (3)$$

Table IV.—Minié Regulation Rifle.

No.	n.	b.	v.
		in.	ft.
1.	531	12·25	931·90
2.	......	11·50	874·85
3.	......	12·12	922·39
4.	......	12·12	922·39
5.	......	11·75	893·86

Mean velocity $=909{\cdot}08$ feet.
Mean quantity of motion $=96{\cdot}63$ lbs.

Table V.—Carbine.

No.	n.	b.	v.
		in.	ft.
1.	989	9·00	1275·21
2.	......	9·12	1292·92
3.	......	8·75	1239·78
4.	......	8·62	1222·07

Mean velocity $=1257{\cdot}49$ feet.
Mean quantity of motion $=70{\cdot}24$ lbs.

If we assume that the force developed by the explosion of the powder, diminished by the friction of the barrel, is constant, it is easy to deduce the following expression for the velocity

$$v=Q\times\sqrt{\frac{s}{m}}, \qquad (4)$$

in which Q denotes a constant depending on the quantity of powder and diameter of the rifle, s the length of the barrel, and m the weight of the bullet.

Taking the velocity of the belted bullet, 1021·7 feet, as our datum, and calculating the velocities of the others from (4), we find

Table VI.—Theoretical and observed Velocities.

	Calculated.	Observed.	Difference.
	ft.	ft.	ft.
Minié bullet in two-groove rifle...	849·0	847·0	+ 2·0
Sugarloaf..............................	866·8	863·7	+ 3·1
Regulation Minié	915·0	909·08	+ 5·92
Carbine bullet........................	1083·7	1257·49	−173·79

The agreement of these results is very striking in the case of the rifles, and proves the truth of equation (4); and the disagreement in the case of the carbine proves, as might be expected, that the force of the powder is greater in the smooth bore than in the rifle. From the preceding results we may assert with confidence, that the velocity with which a bullet is propelled from a rifle by a given charge of powder depends mainly on the *weight of the bullet* and the *length of the barrel*, varying *inversely* as the square root of the former, and *directly* as the square root of the latter*.

The following experiments were made to ascertain the resistance of the air to bullets of different figures and weights. The bullets were fired at 80 feet distance, from the two-groove rifle into the pendulum, and the velocities calculated from formula (1).

The constants of the pendulum were—

$g=32{\cdot}195$ feet. $\varpi=3{\cdot}14159$.
$T=1{\cdot}29$ sec. $c=77$ in.
$a=60$ in. Weight of pend. after Exprs. $=51{\cdot}20$ lbs.

Table VII.—Minié Bullet at 80 feet.

No.	*n.*	*b.*	*f.*	*v.*
		in.	in.	ft.
1.	501	11·75	72·50	836·42
2.	502	11·87	71·00	864·54
3.	503	11·12	72·00	800·25
4.	504	11·00	69·00	827·68
5.	505	11·25	71·00	824·28
6.	507	11·37	69·00	860·61

Mean velocity $=835{\cdot}62$ feet.
Mean quantity of motion $=83{\cdot}22$ lbs.

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* The former of these laws was proved by Mr. Hutton to hold for smooth bore guns of large size, but the latter did not hold true for his experiments. I suppose the reason it is nearer the truth in rifles is on account of the increased friction in the latter.

Table VIII.—Sugarloaf Bullet at 80 feet.

No.	*n.*	*b.*	*f.*	*v.*
		in.	in.	ft.
1.	516	11·50	71·00	860·95
2.	517	11·37	71·00	852·87
3.	518	11·25	71·00	845·50
4.	519	10·62	67·75	838·05
5.	520	11·12	69·00	863·27

Mean velocity = 852·13 feet.
Mean quantity of motion = 81·53 lbs.

Table IX.—Belted Bullet at 80 feet.

No.	*n.*	*b.*	*f.*	*v.*
		in.	in.	ft.
1.	731	8·62	71·00	912·13
2.	732	8·25	69·00	901·58
3.	734	8·62	69·00	944·59
4.	735	7·62	66·00	874·15
5.	736	7·75	67·00	876·99

Mean velocity = 901·88 feet.
Mean quantity of motion = 62·23 lbs.

Collecting the preceding results into one table, we obtain—

Table X.

	Velocity at muzzle.	Velocity at 80 feet.	Quantity of motion at muzzle.	Quantity of motion at 80 feet.
	ft.	ft.	lbs.	lbs.
Minié bullet (two-groove).	847	835·62	84·33	83·22
Minié bullet (regulation)...	909 08	....	96·63	
Sugarloaf bullet	863·7	852·13	82·63	81·53
Belted bullet	1021·68	901·88	70·39	62·23
Carbine bullet..............	1257·49	....	70·24	

From this table it appears—

1st. That the quantity of motion communicated by a given quantity of powder to the Minié bullet, discharged from the regulation rifle, is greater than the quantity of motion possessed by any of the other bullets; this result being due partly to the greater weight of the bullet, and partly to the greater length of the rifle.

2nd. That the quantity of motion communicated to the belted bullet, discharged from the two-groove or Brunswick rifle, is less than that possessed by the other rifle bullets, this result being due to the lesser weight of the belted bullet.

3rd. That the quantity of motion communicated to the carbine bullet is equal to that possessed by the belted rifle bullet, although the carbine is shorter and its bullet lighter; this result being due to the greater friction of the bullet in the rifle barrel.

4th. That in traversing 80 feet of still air, the quantity of motion of the Minié bullet is diminished by $\frac{1}{76}$th; of the sugar-loaf bullet, by $\frac{1}{73}$th; and of the belted bullet, by $\frac{1}{8\cdot6}$th; the remarkable inferiority of the belted bullet being principally due to its shape, which appears to have been contrived so as to cause the maximum amount of resistance to its passage through the air.

5th. That the large stock of Brunswick two-groove rifles constructed for the use of the British rifle service, might be made as useful as the regulation Minié rifles, by adapting to them a bullet of the proper weight, shaped like the Minié bullet, provided with two projections at the side to fit the grooves of the rifle, and used with or without the iron 'culot' of the French bullets.

The length of barrel of the Brunswick rifle is 30 inches, and the size of bore is 0·704 inch. Calculating from these data the weight of the ball which should be used with this rifle in order to produce the same quantity of motion as in the Minié regulation rifle, I find it to be 967 grs., or 7¼ balls to the pound. If Minié balls of this weight were constructed to suit the bore of the Brunswick rifle, and provided with projections or wings to fit the grooves, they would be as efficient as the regulation rifles of 39 inches in length.

Trinity College, Dublin,
May 12, 1854.

LXII. *On Subterraneous Electro-telegraph Wires.* *By* Professor Faraday, *F.R.S. &c.*

To the Editors of the Philosophical Magazine and Journal.

Royal Institution,
April 28, 1854.

Gentlemen,

A COMMUNICATION has been just brought to my notice on some remarkable phænomena presented by subterraneous electro-telegraph wires observed and described by M. Werner Siemens of Berlin, in a communication bearing date April 15, 1850. They are the same phænomena as those shown to me by Mr. Latimer Clarke, and used in my communication (inserted in your Magazine for March 1854, p. 197) as illustrations of the truth of my ancient views of the nature of insulation, induction and conduction. It is only justice that I should refer to them; and I think they are so interesting, that you will be willing to reprint the account, very slightly abbreviated, which I send you; the effects are produced with wires covered with gutta percha and laid in the earth.

"A very remarkable phænomenon is constantly observed on long, well-insulated telegraphic lines. Suppose one extremity, B, of the wire be insulated, and the other, A, be connected with one pole of a battery of which the other touches the earth; at the instant of communication a brief current is observed in the near parts of the wire in the same direction as the instantaneous current which would exist if the extremity B were connected with the earth; on lines of perfect insulation no trace of this current remains. Suddenly replacing, through the action of a commutator, the battery by an earth conductor, a second instantaneous current is obtained of an intensity nearly equal to the first, but in the inverse direction. Finally, breaking the communication of A with the battery and also the earth, so as to insulate this extremity, and uniting the end B at the same instant with the ground, an instantaneous current is observed nearly equal in intensity to the former, and this time in the same direction as the first, *i. e.* as the continuous current of the battery. This last experiment can only be made on a double subterranean line having the two extremities A and B at the same station. One might at first sight suppose these phænomena to be due to secondary polarities developed on the wire, but many facts oppose such a conclusion. 1. The phænomena are more striking as the wire is better insulated. 2. The currents are much more brief than those due to secondary polarities. 3. Their intensity is proportional to the force of the battery, and independent of the intensity of any derived current that may occur in consequence of imperfect insulation; it follows that the intensity of the instantaneous currents can greatly surpass the maximum intensity which secondary currents in the same circuit could acquire. 4. Finally, the intensity of the instantaneous currents is proportional to the length of the wire, whilst an inverse relation ought to occur if the currents were due to secondary polarities.

"The phænomena are easily comprehended if we recall the beautiful experiment by which Volta furnished the most striking proof of the identity of galvanism and electricity. He showed that on communicating one of the ends of his pile with the earth, and the other with the interior of a non-insulated Leyden battery, the battery was charged in an instant of time to a degree proportional to the force of the pile. At the same time an instantaneous current was observed in the conductor between the pile and the battery, which, according to Ritter, had all the properties of an ordinary current. Now it is evident that the subterraneous wire with its insulating covering may be assimilated exactly to an immense Leyden battery: the glass of the jars represents the gutta percha; the internal coating is the surface of the copper wire; the external surface is the moistened earth. To form an idea of the capacity of this new kind of battery, we have only to remember that the surface of the wire is equal to 7 square metres per kilometre. Making such a wire communicate by one of its ends with a pile, of which the other extremity is in contact with the earth, whilst the other extremity of the wire is insulated, must cause the wire to take a charge, of the same character and tension as that of the pole of the pile touched by it:—that is what came to pass in the first of the instantaneous currents described. In Volta's experiment, on breaking the communication between the pole and the battery and connecting the two coatings of the latter by a conductor, an ordinary discharge was obtained:—to this discharge correspond the two instantaneous currents which are observed in opposite directions at the two extremities of the charged wire, on communicating their extremities with the earth, to the exclusion of the pile. It will be understood, also, that the first instantaneous current, namely, that which is connected with the charge of the wire, ought to be equally produced, though of a lower intensity, even when the other extremity of the wire is in communication with the earth. The instantaneous current then precedes the continuous current, or, if the statement be preferred, is added to it at the first moment. This instantaneous current has an intensity much greater than that of the continuous current; doubtless because in the act of charging the wire, the electricity in going to the different points of the wire passes through paths so much the shorter as the points to be charged are nearer to the pile."

The above is from the *Annales de Chimie*, 1850, vol. xxix. p. 398, &c.

I am, Gentlemen,
Your very faithful Servant,
M. Faraday.

LXIII. *Note on some Experiments on Rotatory Motion.*

By the Rev. Baden Powell, *M.A., V.P.R.S. &c.*

To the Editors of the Philosophical Magazine and Journal.

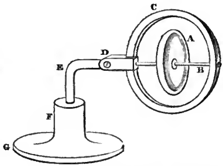
Gentlemen,

OBSERVING in a late Number some account of Professor Magnus's experiments, I conceive it may not be uninteresting to your readers to give the few following particulars relative to the closely-allied experiments of M. Fessel, and the discussions which have taken place respecting them, more especially as they will form an appropriate introduction to the beautiful researches on the subject which Professor Wheatstone has very recently brought before the Royal Society.

In Poggendorff's *Annalen*, Sept. 1853, No. 9, Prof. Plücker has given some account of the rotatory machine of M. Fessel. Notwithstanding the luminous theoretical researches on the subject in the papers of MM. Poinsot and Poisson, he observes that experimental illustration of it is still desirable, and mentions that M. Fessel, "formerly teacher in the provincial school of industry, now by the kindness or unkindness of circumstances led to exercise his art as a mechanician," has constructed an apparatus, which, from the way in which it presents the phænomena to the eye, excites surprise even in those who are acquainted with the principle.

The instrument will be understood at once from the sketch annexed.

The disc A, loaded round its circumference, can be spun rapidly on the axis B, whose ends rest in the ring C, itself joined by a hinge at D to the axis bent at a right angle at E, the vertical part of which turns freely in the tube F, supported on the stand G.



The observed phænomenon is this. When the disc is set in rapid rotation about B, the whole begins also to revolve bodily round the vertical axis E, and this is easily seen to be due to the action of gravitation tending to bring down the ring C about D.

M. Fessel adds, in a note, an account of the way in which he accidentally discovered the principle, by rotating a heavy wheel on one end of an axis. And M. Plücker proposed to him to modify the construction by attaching the ring C to one end of a bar carrying a sliding counterpoise, and capable of revolving about a pivot; and this, he says, answered admirably.

"The apparatus thus improved," Prof. Plücker continues, "gave the previous phænomena, but at the same time others immediately appeared.

"If the disc rotated in a vertical plane, then the axis moved itself round towards the opposite direction in the horizontal plane. If the first rotatory motion gradually lost, the second gained. If the second motion was quickened by an extraneous force, then the ring with the disc appeared to become *lighter*, since it rose up; on the contrary, by a retardation of the horizontal movement, it seemed to become heavier, or sank. With the same rotatory velocity of the disc, the horizontal rotation increased or diminished according as the force of gravitation was increased or diminished; as, for example, if the rotating disc be made of soft iron, it rises or sinks if we precede or follow it with a magnet, thereby accelerating or retarding the horizontal revolution."

He also points out the analogy of this apparatus to that of Bonenberger. In the *Annalen*, No. 10 (Oct. 1853), the editor adds "one more word on the Fessel rotatory machine," in which he confirms the account of its inventor by his own experience of its use, and adds an explanation of its principle, as he observes, for the sake of those who may wish to understand it without going into calculation.

The general nature of this explanation may be stated thus: the disc being supposed to rotate in a vertical plane, or the axis B horizontal; then at any point in its circumference the motion in the direction of the tangent may be conceived resolved into a horizontal and a vertical component, the direction of motion in each being of course opposed at opposite parts of the circle.

Now if the ring C be free to be acted on by gravitation, or the axis B tend to become inclined, or the rotation to take place in an oblique plane, the horizontal components will not change their direction, but the vertical components will; and will no longer have their direction in the plane of the disc: they will consequently cause it to change its plane, or exert a lateral force, and thus give the whole a motion of revolution round the vertical axis E. But while this takes place, the tendency to inclination of the axis B is counteracted, and the opposing lateral effects of the horizontal components result in the rotation of the disc round its horizontal axis in a vertical plane.

M. Fessel has added a small slider below D, which, when pushed out, supports the ring C against gravitation; and in this case there is no revolution round the vertical. M. Poggendorff also observes, that if the rotatory part of the instrument be balanced about a point near D, either on a pivot, or suspended by a string with a counterpoise, the same results are more satisfactorily exhibited.

He observes that the same principles apply to the explanation of Prof. Magnus's remarkable experiment with two discs rotating at opposite ends of a suspended axis. (See his paper translated in Taylor's Foreign Scientific Memoirs, Part III. May 1853, p. 229.)

Finally, the author observes with regard to the application of the Bonenberger apparatus which he had first pointed out (*Annalen*, June 1851, p. 308), " M. Person has lately made the just remark (*Comptes Rendus*, 1852, Sept. 27 and Oct. 18), that in order to withdraw it completely from the influence of the rotation of the earth, the apparatus must be placed with its principal axis parallel to the earth's axis; since (in strictness) the motion of the whole apparatus round an axis, which does not coincide with one of the three axes contained in it, must alter the rotation of the ball."

And he suggests, that in the experiment the whole should be furnished with the means of rapid rotation about a vertical axis, and so that the rotatory axis of the ball might have any required position given to it with respect to the new axis.

LXIV. *The Attraction of Ellipsoids considered geometrically. By* MATTHEW COLLINS, *B.A., Senior Moderator in Mathematics and Physics, and Bishop Law's Mathematical Prizeman, Trin. Coll. Dublin; Professor of Mathematics in the Mechanics' Institute, Liverpool**.

THE attraction of an ellipsoid A on a point P on its surface, or within it in a direction perpendicular to one of its principal planes B, is proportional to the distance of the attracted point P from that principal plane.

1. When P is on the surface. Draw PP' a chord of A perpendicular to B, and through P and P' draw planes parallel to B cutting the principal axis CC' perpendicularly in p and p'; then describe through p and p' an ellipsoid a, concentric, similar, and similarly placed to A, and its attraction on p will be equal to the attraction of A on P in a direction perpendicular to B.

For through PP' draw two planes E, F containing a very small angle, and through pp' draw two planes e, f parallel to the former; then let a cone of revolution, whose axis is PP' and vertex P cut E, F along the straight lines PE, PE', PF, PF', and let another such cone very close to the former, and having the same axis and vertex, also cut E, F along $\mathrm{PE}_{\prime}$ $\mathrm{PE}'_{\prime}$ $\mathrm{PF}_{\prime}$ $\mathrm{PF}'_{\prime}$, and through p draw, in the planes e, f, the straight lines pe pe' pf pf' and $pe_{\prime}$ $pe'_{\prime}$ $pf_{\prime}$ $pf'_{\prime}$ respectively parallel to the foregoing; then as the sections of the two similar ellipsoids A and a by the parallel planes E, e are necessarily similar ellipses, and as the chord PP' parallel to an axis (CC') of the greater ellipse is $=$ to the homologous axis pp' of the less, ∴ by Airy's Tract on the Figure of the Earth, Props. 2 and 3, $\mathrm{PE}+\mathrm{PE}'=pe+pe'$; and ∴ by Airy's 4th

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* Communicated by the Author.

proposition, the sum of the attractions exerted on P along PP′ by the two small pyramids $\mathrm{PEFE_{\prime}F_{\prime\prime}}$, $\mathrm{PE'F'E'_{\prime}F'_{\prime}}$ is equal to the sum of the attractions exerted on p along pp' by the two *corresponding* small pyramids $pefe_{\prime}f_{\prime\prime}$, $pe'f'e'_{\prime}f'_{\prime\prime}$ since the solid angles of the four pyramids are all equal to each other: and since there are obviously as many pairs of pyramids in the double wedge PP′E FE′F′ as there are *corresponding* pairs of pyramids (whose solid <s are also equal to those of the former) in the double wedge $pp'efe'f'$, and as, moreover, each double wedge of A has a corresponding double wedge of a, ∴ the whole attraction of a on p is equal to the attraction of A on P along PP′; but since a is similar to A, the attraction of a on p : attraction of A on C : : $pp'(=\mathrm{PP'}) : \mathrm{CC'}$ (*Principia*, Prop. 87, Cor. 1, Book 1); and so the attraction of A on P perpendicular to B, which was proved equal to attraction of a on p, is $\therefore = \frac{\frac{1}{2}\mathrm{PP'}}{\frac{1}{2}\mathrm{CC'}} \times$ attraction of A on C, which, since A and CC′ are constant, $\propto \frac{1}{2}\mathrm{PP'}$, which is the distance of P from B.

2. The general equation of surfaces of the second order being $\phi(xyz)=\mathrm{A}+\mathrm{B}x+\mathrm{C}y+\mathrm{D}z+\mathrm{E}x^2+\&c.=0$, the equation of the diametral plane bisecting all chords parallel to the straight line $x=mz$ and $y=nz$ is known to be $md_x\phi+nd_y\phi+d_z\phi=0$, which, on account of not containing the absolute constant term A, indicates that if *any* straight line ABB′A′ cuts two surfaces of the second order whose equations differ only in the constant terms, the intercepts AB, A′B′ will be equal, since the chords AA′, BB′ are bisected in the same point by the diametral plane conjugate to it, which plane is the same for both surfaces. Now the equations of two ellipsoids which are concentric, similar, and similarly placed, differ only in the absolute constant terms, and ∴ the intercepts AB, A′B′ of *any* straight line ABB′A′ cutting two such ellipsoids are equal; and hence it follows that a shell or couch bounded by two concentric, similar, and similary placed ellipsoidal surfaces, exerts no attraction on a point P situated anywhere within it, or on its interior surface. See Airy's Tract on the Figure of the Earth, Prop. 12, or the *Principia*, Prop. 70, and Prop. 91, Cor. 3, Book 1.

3. When P is within the ellipsoid A, we have then only to describe through P another ellipsoid A′, concentric, similar, and similarly situated to A; and since the shell between A and A′ exerts no attraction on P, as was just proved, ∴ the whole attraction of A on P is the same as that of A′ on P: then, as in 1, draw PP′ a chord of A′ perpendicular to B, and through P and P′ draw planes parallel to B cutting the principal axis CC′ perpendicularly in p and p', and then describe through p and p' another ellipsoid a, concentric, similar, and similarly placed to A or A′; and by 1, the attraction of A (or A′) on P perpendi-

cularly to B is equal to the whole attraction of a on p, and $\therefore = \frac{\frac{1}{2}\text{PP}'}{\frac{1}{2}\text{CC}'} \times$ attraction of A on C, which, as already observed, $\propto \frac{1}{2}\text{PP}'$, which is the distance of P (now supposed within A) from B.

4. The preceding proposition shows that the attraction of an ellipsoid on any point on its surface, or within it, can be got at once from the attraction of the same ellipsoid on a point placed at the extremity of an axis; and this latter attraction is found and reduced to elliptic functions as follows.

Let O be the centre, and $\text{OA}=a$, $\text{OB}=b$, $\text{OC}=c$ the semiaxes, and let the attracted point C be the vertex, and CO the axis of a cone of revolution D whose semiangle is θ, and let $\theta+d\theta$ be the semiangle of another such cone E very close to D, and having the same vertex and axis, and let O be the common vertex of two other cones D′ and E′ parallel to D and E. Conceive the portion of the ellipsoid between D and E to be divided into elementary pyramids by planes passing through CO; let f be the length of a side of one of these little pyramids, which is a chord of the ellipsoid and a side of D; and let g be the parallel side of D′, which is a radius of the ellipsoid; and let f' and g' be the projections of f and g upon c, and let ω be the small angle between two consecutive f^{s} (or g^{s}); then the attraction of the little pyramid whose side is f on its vertex C is $=f\omega\,.\,d\theta$ (see Airy's Tract on the Figure of the Earth, Prop. 4), and $\therefore$ its component along CO is $=f'\omega\,.\,d\theta$, $\therefore =2c\times\frac{f'}{2c}\omega\,.\,d\theta$; but $\frac{f'}{2c}=\frac{g'^2}{c^2}$, $\therefore$ said component $=\frac{2}{c}g'^2\omega\,.\,d\theta$, $\therefore =\frac{4}{c}\times\frac{1}{2}g^2\cos^2\theta\omega\,.\,d\theta$; now $\frac{1}{2}g^2\omega$ is the area on D′ included between the two consecutive g^{s}, and the sum of all such elements is the entire surface of D′ which we shall still name D′; $\therefore$ the attraction of the slice of the ellipsoid between D and E upon C along CO is $=\frac{4\text{D}'\cos^2\theta\,.\,d\theta}{c}$. Now the projection of D′ on the plane of ab is obviously an ellipse whose area D″ is $=\text{D}'\sin\theta$; let r and r' be the sides of D′ (radii of the ellipsoid) lying in the planes of ca and cb, then the semiaxes of D″ are plainly the projections of r and r', or $r\sin\theta$ and $r'\sin\theta$; and $\therefore$ $\text{D}''=\pi rr'\sin^2\theta$, and so $\text{D}'=\frac{\text{D}''}{\sin\theta}$, $\therefore =\pi rr'\sin\theta$; and so the attraction of the slice on C along CO is

$$=\frac{4\pi}{c}rr'\cos^2\theta\sin\theta\,.\,d\theta.$$

Now $$\frac{1}{r^2}=\frac{\cos^2\theta}{c^2}+\frac{\sin^2\theta}{a^2}\text{ and }\frac{1}{r'^2}=\frac{\cos^2\theta}{c^2}+\frac{\sin^2\theta}{b^2},$$

and ∴ the differential of the required attraction on C is

$$\frac{4\pi abc\cos^2\theta\sin\theta\,.\,d\theta}{(c^2\sin^2\theta+a^2\cos^2\theta)^{\frac{1}{2}}(c^2\sin^2\theta+b^2\cos^2\theta)^{\frac{1}{2}}};$$

by putting $\cos\theta=u$, or $\tan^2\theta=v$, this expression, given by Poisson (*Mécanique*, vol. i. p. 190), becomes transformed into

$$\frac{4\pi abcu^2du}{(c^2+\overline{a^2-c^2}\,.\,u^2)^{\frac{1}{2}}(c^2+\overline{b^2-c^2}\,.\,u^2)^{\frac{1}{2}}},$$

or

$$\frac{2\pi c\,.\,dv}{(1+v)\sqrt{(1+v)\left(1+\frac{c^2}{a^2}v\right)\left(1+\frac{c^2}{b^2}v\right)}}.$$

5. Now supposing $a>b>c$, let OA′ and OB′ portions of OA and OB be the semiaxes of the focal ellipse whose plane is perpendicular to OC, then $\mathrm{OA}'^2=a^2-c^2$ and $\mathrm{OB}'^2=b^2-c^2$; let OQ be perpendicular to the tangent PTQ, which touches this ellipse in T and meets OA′ at the point P; let $<\mathrm{OPT}=\phi$, and

$$\rho^2=c^2+(b^2-c^2)u^2=c^2+\mathrm{OB}'^2\cos^2\theta$$

and

$$\rho'^2=c^2+(a^2-c^2)u^2=c^2+\mathrm{OA}'^2\cos^2\theta;$$

and if the point P be taken such that $\frac{\mathrm{OP}}{\mathrm{OA}'}=\frac{\rho}{c}$ or $\mathrm{OP}=\frac{\mathrm{OA}'}{c}\rho$, then the equation

$$\mathrm{OA}'^2\cos^2\mathrm{A}'\mathrm{OQ}+\mathrm{OB}'^2\cos^2\mathrm{B}'\mathrm{OQ}=\mathrm{OQ}^2;$$

$\therefore\ =\mathrm{OP}^2\sin^2\phi$ gives

$$(a^2-c^2)\sin^2\phi+(b^2-c^2)\cos^2\phi=\frac{a^2-c^2}{c^2}(c^2+\overline{b^2-c^2}\,.\,\cos^2\theta)\sin^2\phi,$$

which gives

$$(a^2-c^2)\cos^2\theta=\cos^2\phi(c^2+\overline{a^2-c^2}\,.\,\cos^2\theta),$$

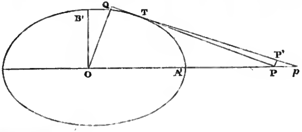
i. e. $\mathrm{OA}'^2\cos^2\theta=\rho^2\cos^2\phi$, and $\therefore\cos\phi=\frac{\mathrm{OA}'}{\rho'}\cos\theta$; and since

$$\mathrm{OP}=\frac{\mathrm{OA}'}{c}\rho=\frac{\mathrm{OA}'}{c}(c^2+\mathrm{OB}'^2\cos^2\theta)^{\frac{1}{2}},$$

$$\therefore\ \mathrm{P}p=d\,.\,\mathrm{OP}=\frac{\mathrm{OA}'}{c}(c^2+\mathrm{OB}'^2\cos^2\theta)^{-\frac{1}{2}}\times\mathrm{OB}'^2\cos\theta\sin\theta\,.\,d\theta$$

$$=\frac{\mathrm{OA}'\,.\,\mathrm{OB}'^2}{c\rho}\cos\theta\sin\theta\,.\,d\theta.$$

Let PP′ be perpendicular to the consecutive tangent from p,



then $p\mathrm{P}' = \mathrm{P}p \cos\phi$;

$$\therefore \; = \frac{\mathrm{OA}'^9 \,.\, \mathrm{OB}'^2}{c\rho\rho'} \cos^2\theta \sin\theta \,.\, d\theta :$$

but the attraction of the slice of the ellipsoid between D and E on C along CO was found above

$$= \frac{4\pi abc \cos^2\theta \sin\theta \,.\, d\theta}{\rho\rho'}.$$

And, moreover, as $\mathrm{P}'p$ is obviously $= d \,.\, \mathrm{B'TP}$,

$$\therefore \; = d(\mathrm{B'TP} - \text{const } \mathrm{B'TA'}),$$

i. e. $= d(\mathrm{TP} - \text{arc TA}')$; $\therefore$ the attraction of the slice on C is

$$= \frac{4\pi abc^2}{\mathrm{OA}'^2 \,.\, \mathrm{OB}'^2} \times p\mathrm{P}' = \frac{4\pi abc^2}{(a^2-c^2)(b^2-c^2} \, d \,.\, (\mathrm{TP} - \text{arc TA}') ;$$

and $\therefore$ the attraction of the whole ellipsoid on C is

$$= \frac{4\pi abc^2}{(a^2-c^2)(b^2-c^2)} \times (\mathrm{T_{\prime}P_{\prime}} - \text{arc } \mathrm{T_{\prime}A'}),$$

where $\mathrm{P}_{\prime}$ and $\mathrm{T}_{\prime}$ denote the ultimate positions of P and T corresponding to $\theta = 0$; and since by construction

$$\mathrm{OP} = \frac{\mathrm{OA}'}{c} \rho = \frac{\mathrm{OA}'}{c} (c^2 + \overline{b^2 - c^2} \,.\, \cos^2\theta)^{\frac{1}{2}},$$

$\therefore$ when $\theta = 0$, $\mathrm{OP}_{\prime} = \frac{b}{c}\,\mathrm{OA}'$, and

$$\therefore \; \mathrm{P_{\prime}T_{\prime}} = \frac{a(b'^2 - c^2)}{bc} = \frac{a^2(b^2 - c^2)}{abc}.$$

6. Hence, also, the differential of the ellipsoid's attraction on B, *i. e.* the attraction on B along BO of a slice of the ellipsoid comprised between two cones of revolution whose vertex is B and axis BO, and semiangles are θ and $\theta + d\theta$, is

$$= \frac{4\pi abc \cos^2\theta \sin\theta \,.\, d\theta}{(b^2 \sin^2\theta + c^2 \cos^2\theta)^{\frac{1}{2}} (b^2 \sin^2\theta + a^2 \cos^2\theta)^{\frac{1}{2}}}$$

$$= \frac{4\pi abcu^2 \,.\, du}{(b^2 - \overline{b^2 - c^2} \,.\, u^2)^{\frac{1}{2}} (b^2 + \overline{a^2 - b^2} \,.\, u^2)^{\frac{1}{2}}};$$

where, as before, $u = \cos\theta$. Now to represent this geometrically, let OA'' and OC' portions of OA, OC be the semiaxes of the focal hyperbola whose plane is perpendicular to OB, then $OA''^2 = a^2 - b^2$ and $OC'^2 = c^2 - b^2 = -(b^2 - c^2) = -OB'^2$. And putting now $\rho^2 = b^2 - (b^2 - c^2)\cos^2\theta$ and $\rho'^2 = b^2 + (a^2 - b^2)\cos^2\theta$, and taking the point P on the primary semiaxis OA'', so that $\frac{OP}{OA''} = \frac{\rho}{b}$. So that as $\rho < b$, P will lie between O and A''; then drawing PT touching the hyperbola in T, we find, as before, *mutatis mutandis*, the whole attraction of the ellipsoid on B

$$= \frac{4\pi ab^2c}{(a^2 - b^2)(b^2 - c^2)} \times (T_{,}P_{,} - \text{arc } T_{,}A'')\,;$$

$P_{,}$ and $T_{,}$ being now, as before, the ultimate positions of P and T corresponding to $\theta = 0$. So that, as before, $OP_{,} = \frac{c}{b} OA''$, and $\therefore P_{,}T_{,} = \frac{a^2(b^2 - c^2)}{abc}$ *exactly* the same as before.

The whole attraction on A cannot be similarly represented, because there is no real focal conic perpendicular to OA; but the equation $\frac{A}{a} + \frac{B}{b} + \frac{C}{c} = 4\pi\rho$, discovered by the late ingenious Professor MacCullagh, will then serve to find A; where ρ denotes the density, and A, B, C denote the whole attractions of the ellipsoid on the points A, B, C.

7. Let a, b, c be the semiaxes of a homogeneous fluid ellipsoid, and A, B, C the attractions on points at the ends of a, b, c, caused partly by the ellipsoid's own attractions on its parts, and partly by the centrifugal force of revolution about an axis $(2c)$, or by the action of an extraneous force directed towards its centre, and $\propto$ distance from the centre, then the ellipsoid will preserve its shape if $Aa = Bb = Cc$.

For then the whole forces acting on *any* point xyz of the mass in directions parallel to a, b, c will obviously be $\frac{Ax}{a}$, $\frac{By}{b}$, and $\frac{Cz}{c}$; and dividing these by $Aa = Bb = Cc$, they are as $\frac{x}{a^2}$, $\frac{y}{b^2}$, and $\frac{z}{c^2}$; but when the point xyz is on the surface, these last are as the cosines of the angles that the normal at the point xyz makes with the axes, as is evident from the equation $\left(\frac{xx'}{a^2} + \frac{yy'}{b^2} + \frac{zz'}{c^2} = 1\right)$ of the tangent plane. Thus the components of the force acting on the point xyz at the surface are as the cosines of the angles that the normal at this point makes with the axis, $\therefore$ the direction of the resultant force coincides with the normal or perpendicular to the surface itself, which is the condition necessary for equilibrium.

The general formula $dp=\rho(\mathrm{X}dx+\mathrm{Y}dy+\mathrm{Z}dz)$ obviously becomes in this case

$$dp=-\rho\left(\frac{\mathrm{A}x}{a}dx+\frac{\mathrm{B}y}{b}dy+\frac{\mathrm{C}z}{c}dz\right)=-\rho\mathrm{A}a\left(\frac{xdx}{a^2}+\frac{ydy}{b^2}+\frac{zdz}{c^2}\right),$$

and $\therefore\ p=\mathrm{C}'-\frac{\rho\mathrm{A}a}{2}\left(\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}\right)$ at the surface $p=0$, and $\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1$, $\therefore\ \mathrm{C}'=\frac{\rho\mathrm{A}a}{2}$, and $\therefore$ the pressure at any point xyz of the mass is $=\frac{\rho\mathrm{A}a}{2}\left(1-\frac{x^2}{a^2}-\frac{y^2}{b^2}-\frac{z^2}{c^2}\right)$, which at the centre, where $x=y=z=0$, becomes $\frac{\rho\mathrm{A}a}{2}$; and shows, moreover, that the surface of *any* ellipsoid concentric, similar, and similarly placed to the given one, is a *surface de niveau*, on every part of which the pressure is the same; and since A, B, C are obviously $\propto\rho$, $\therefore$ the pressure at any given point of the mass $\propto\rho^2$. See the *Principia*, Prop. 20, Book 3; McLaurin's *De Causa Phys. flux et reflux Maris*, Prop. 1; Airy's Tract on the Figure of the Earth, Props. 14, 15 and 16.

8. Let R and r be the radii of two homogeneous concentric globes, A and a the attractions of each on a point on the surface of the other, then $\frac{\mathrm{A}}{\mathrm{R}^2}=\frac{a}{r^2}$, whatever be the law of attraction as a function of the distance.

For let O be their common centre, OrR a radius meeting them at r and R, bc a chord of the less parallel to OR; produce Ob, Oc to meet the large globe's surface at B, C, then BC will be parallel to bc or to OR; and if b describe any little figure b' on the surface of r, it is evident B will describe a similar figure B$'$ on the surface of R; and the areas S, s of the normal sections of the cylinders C and c simultaneously described by BC, bc will obviously be to each other as $\mathrm{B}':b'$; $\therefore$ as $\mathrm{R}^2:r^2$. Now by Euclid (Prop. 4, Book 1) $\mathrm{B}r=b\mathrm{R}$ and $\mathrm{C}r=c\mathrm{R}$; $\therefore$ by Airy's Tract on the Figure of the Earth, Prop. 18 (generalized), attraction of cylinder C on the point r along rO : attraction of cylinder c on point R along RO :: S : s :: $\mathrm{R}^2:r^2$; and as this fixed proportion holds true for each *corresponding* pair of cylinders, $\therefore$ by taking their sums we shall still have $\mathrm{A}:a::\mathrm{R}^2:r^2$. See Poisson's *Mécanique*, vol. i. p. 201.

Mechanics' Institute, Liverpool,
April 19, 1854.

LXV. *On the Theory of Groups, as depending on the Symbolic Equation* $\theta^n=1$.—Part II.* *By* A. Cayley, *Esq.*†

IMAGINE the symbols

$$L, M, N, \ldots$$

such that L being any symbol of the system,

$$L^{-1}L, L^{-1}M, L^{-1}N, \ldots$$

is the *group*

$$1, \alpha, \beta, \ldots$$

Then, in the first place, M being any other symbol of the system, $M^{-1}L$, $M^{-1}M$, $M^{-1}N$, .. will be the same group $1, \alpha, \beta$ .. In fact, the system L, M, N .. may be written L, $L\alpha$, $L\beta$ ..; and if *e. g.* $M=L\alpha$, $N=L\beta$, then

$$M^{-1}N=(L\alpha)^{-1}L\beta=\alpha^{-1}L^{-1}L\beta=\alpha^{-1}\beta,$$

which belongs to the group $1, \alpha, \beta$ ..

Next it may be shown that

$$LL^{-1}, ML^{-1}, NL^{-1}, \ldots$$

is a group, although not in general the same group as $1, \alpha, \beta$ .. In fact, writing $M=L\alpha$, $N=L\beta$, &c., the symbols just written down are

$$LL^{-1}, L\alpha L^{-1}, L\beta L^{-1}, \ldots$$

and we have *e. g.* $L\alpha L^{-1}.L\beta L^{-1}=L\alpha\beta L^{-1}=L\gamma L^{-1}$, where γ belongs to the group $1, \alpha, \beta$.

The system L, M, N .. may be termed a group-holding system, or simply a holder; and with reference to the two groups to which it gives rise, may be said to hold on the nearer side the group $L^{-1}L, L^{-1}M, L^{-1}N$ .., and to hold on the further side the group LL^{-1}, LM^{-1}, LN^{-1} .. Suppose that these groups are one and the same group $1, \alpha, \beta$.., the system L, M, N .. is in this case termed a symmetrical holder, and in reference to the last-mentioned group is said to hold such group symmetrically. It is evident that the symmetrical holder L, M, N .. may be expressed indifferently and at pleasure in either of the two forms L, $L\alpha$, $L\beta$ .. and L, αL, βL; *i. e.* we may say that the group is convertible with any symbol L of the holder, and that the group operating upon, or operated upon, by a symbol L of the holder produces the holder. We may also

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* See January Number, p. 40.

† Communicated by the Author.

say that the holder operated upon by, or operating upon, a symbol α of the group reproduces the holder.

Suppose now that the group

$$1,\ \alpha,\ \beta,\ \gamma,\ \delta,\ \epsilon,\ \zeta\,..$$

can be divided into a series of symmetrical holders of the smaller group

$$1,\ \alpha,\ \beta\,,..$$

The former group is said to be a multiple of the latter group, and the latter group to be a submultiple of the former group. Thus considering the two different forms of a group of six, and first the form

$$1,\ \alpha,\ \alpha^2,\ \gamma,\ \gamma\alpha,\ \gamma\alpha^2\ (\alpha^3=1,\ \gamma^2=1,\ \alpha\gamma=\gamma\alpha),$$

the group of six is a multiple of the group of three, $1, \alpha, \alpha^2$ (in fact, $1, \alpha, \alpha^2$ and $\gamma, \gamma\alpha, \gamma\alpha^2$ are each of them a symmetrical holder of the group $1, \alpha, \alpha^2$); and so in like manner the group of six is a multiple of the group of two, $1, \gamma$ (in fact, $1, \gamma$ and $\alpha, \alpha\gamma$, and $\alpha, \alpha^2\gamma$ are each a symmetrical holder of the group $1, \gamma$). There would not, in a case such as the one in question, be any harm in speaking of the group of six as the product of the two groups $1, \alpha, \alpha^2$ and $1, \gamma$, but upon the whole it is, I think, better to dispense with the expression.

Considering, secondly, the other form of a group of six, viz.

$$1,\ \alpha,\ \alpha^2,\ \gamma,\ \gamma\alpha,\ \gamma\alpha^2\ (\alpha^3=1,\ \gamma^2=1,\ \alpha\gamma=\gamma\alpha^2).$$

Here the group of six is a multiple of the group of three, $1, \alpha, \alpha^2$ (in fact, as before, $1, \alpha, \alpha^2$ and $\gamma, \gamma\alpha, \gamma\alpha^2$ are each a symmetrical holder of the group $1, \alpha, \alpha^2$, since, as regards $\gamma, \gamma\alpha, \gamma\alpha^2$, we have $(\gamma, \gamma\alpha, \gamma\alpha^2)=\gamma(1, \alpha, \alpha^2)=(1, \alpha^2, \alpha)\gamma$). But the group of six is not a multiple of any group of two whatever; in fact, besides the group $1, \gamma$ itself, there is not any symmetrical holder of this group $1, \gamma$; and so, in like manner, with respect to the other groups of two, $1, \gamma\alpha$ and $1, \gamma\alpha^2$. The group of three, $1, \alpha, \alpha^2$, is therefore, in the present case, the only submultiple of the group of six.

It may be remarked, that if there be any number of symmetrical holders of the same group, $1, \alpha, \beta, ..$ then any one of these holders bears to the aggregate of the holders a relation such as the submultiple of a group bears to such group: it is proper to notice that the aggregate of the holders is not of necessity itself a holder.

LXVI. *On the Cartesian Barometer.*

By William Roxburgh, *M.D.*[*]

SOON after the discovery of the variations in height of the barometer, Descartes proposed the following mode of rendering them more conspicuous, almost as much so as they are in one filled with water alone. He suggested that two tubes should be joined to the opposite ends of a short wide cylinder so as to form one straight tube, which, being closed at one end, was to be filled with pure water and mercury in such proportions as to allow of the two fluids at all pressures meeting in the cylinder. In this, the Cartesian barometer, the pressure of the atmosphere is balanced by the water and mercury conjointly, but the variations of pressure are indicated chiefly by movements of the water, as the level of the mercury varies little in consequence of the large area of the cylinder. The movements of the water and mercury are to each other inversely as the areas of the tube and cylinder. The scale is that of the common barometer enlarged as in the wheel barometer; when, therefore, the movements are said to amount to so many hundredths of an inch, it is to be understood as meaning that they are equal in value to that height of mercury. The scale can be enlarged so as to render movements of $\frac{1}{100}$dth of an inch visible to the unassisted eye.

The only records of this instrument that I have seen, state that the air contained in the water is given off when the pressure is removed, and so renders its indications incorrect; also that this imperfection is irremediable. This depression, amounting in one year, in my first experiment, to only ·02 of an inch, has led me to suppose that the depression which caused the plan to be set aside was owing to the force of vapour, which was not so well understood at that time as at present; and as many variations of pressure are easily seen in this barometer, which would escape notice in the mercurial one, and if not attended to give rise to error, I think it will prove a valuable addition to a standard barometer, though never a substitute for one.

In hopes of getting rid of the air, and of lessening the correction required for the force of vapour, I tried several fluids in place of pure water. Among these was oil of turpentine; this caused a rapid evolution of gas and blackening of the mercury, and depressing the column several inches in a few minutes. A saturated solution of muriate of soda seemed at first more successful, but in a short time the column became depressed, and this depression continuing to increase at a regular rate, the tube

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[*] Communicated by the Author.

was emptied, when it was found that the salt having crystallized between the mercury and the glass, had so allowed the air to enter.

A solution of muriate of lime, not being crystallizable, was next tried; and this seems to stand best, as yet having sunk in 2½ years only ·03 of an inch, the greater part of this depression having occurred in the first few months, giving rise to the surmise that the air which has caused it was left in at the time of filling, and has not crept in since. The addition of the salt to the water, besides removing to a great extent the air, has the effect of diminishing the correction required for the force of vapour; the last named solution has its boiling-point at 234° F., and, as has been shown by experiments, the tension of vapour from water and watery solutions of salts is the same at an equal number of degrees below their boiling-points, the correction to be applied is lessened to that of pure water 22° lower than the observed temperature. This correction, which is to be added, and that for the expansion of the fluids, which is to be subtracted, thus nearly neutralizing each other at low temperatures, I have applied by means of a moveable scale, in the same way as is used in the sympiesometer. Among the slighter variations shown by this barometer, may be mentioned the oscillations during a gale of wind; these are quite as conspicuous in this barometer as they were observed by Professor Daniell in the water barometer, amounting frequently to 0·03, and once to 0·4 of an inch; they vary in duration from 5 to 7 seconds; they begin with a short, quick rise, followed by a slower and much greater descent, and then a return to the point of rest, which is much nearer the top than the bottom of the oscillation. Previously to a gale of wind, the column descends by jerks and with irregular rapidity; but on one occasion, on which no wind followed for two days, the column fell without the slightest jerk more than half an inch; there was, however, a heavy and long-continued fall of rain. During heavy and sudden showers the column rises, and falls again on the cessation of the shower; on one occasion the rise was ·02 of an inch. In a room with a fire, with door and window shut, the column is lower than when the window is open; the difference is usually ·005, but with a good fire ·01 of an inch. The last two causes are very likely to give rise to error, and the better the barometer the greater will be the error.

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LXVII. *Theory of the Electric Residue in the Leyden Jar.* *By* R. Kohlrausch.

[Continued from p. 320.]

§ 6.

It is clear the question may arise, whether the cause of the residue does not reside in the uncoated part of the jar or rim, inasmuch as a part of the electricity may move about between the glass and the varnish.

It appears plausible, that if such a movement occurred, it would do so gradually and in tolerable quantity, for the electricity pushed on to the rim on the insulated side would cause the opposite electricity to follow it up the other side, and condensation might take place as well on the insulator as under the coatings. At any rate the decision of this question does not appear superfluous.

If the rim of the jar actually influences the residue, then this influence ought to be more considerable, in proportion to the total charge, not only when the rim is broader (for it might be assumed that the movement was never very extensive), but particularly when the periphery of the coating is greater in comparison to its superficial area. Hence if, with an equal area of coating, the rim be reduced to a minimum, the residue ought to be reduced also.

The neck of the bottle which was described in § 1, under *b*, and to which the tables *b*, *b'*, *b''* belong, had a circumference of 5·7 centims., and an interior surface of 11·4 square centims., whilst the part of the interior surface of the same bottle which was covered with mercury contained 275 square centims.

The metallic coating of the Franklin's plate, which was described under *c* in § 1, and to which the tables *c*, *c'*, and *c''* belong, formed a rectangle whose sides measured 15 and 25 centims., whose periphery therefore was 80 centims., and area 375 square centims., whilst the surface of the uncoated rim amounted to 296 square centims.

If now we compare the tables *b''* and *c''* of § 4, we shall find that in 864 seconds, and with a primitive charge of 0·5559, the Franklin's plate had concealed the residue 0·1060; whilst the bottle *b* in 935 seconds, and with a primitive charge of 1·4968, produced the residue 0·4522, which is equivalent to a residue of about 0·4445 in 864 seconds, *i. e.* in the same time as the Franklin's plate. Hence, notwithstanding the unequal proportions of rim, the residue amounted to 0·297 of the original charge in the bottle, and 0·190, in the Franklin's plate. The proportion will be about the same if we compare these residues with the quantities of electricity Q_t which were present in both cases after 864 seconds. The bottle had a residue equal to 0·321 of this quantity, the plate 0·213. The whole hypothesis, however, is destroyed the moment we consider that according to the above almost half as much electricity ought at this time to be collected on the neck of the bottle, whose area is 11·4 square centims., as on the 275 square centims. of the whole interior coating; a proportion which will be still more unreasonable if we consider the state of the bottle at its final discharge after the lapse of 5370 seconds.

So much is clear, therefore,—*the rim, if not entirely without, has at any rate a very small influence on the residue.*

§ 7.

In reference to this production of a residue, an influence has also been ascribed to the cement with which the coating of tinfoil is usually attached to the glass. Without entering into theoretical considerations as to whether this influence must not in every case be a very secondary one, we shall here mention a few facts merely.

In the Franklin's plate *c*, the sine-electrometer was connected with the metallic coating, which rests immediately on the glass without the interposition of any cement. If we consider the tables *a''*, *b''*, and *c''*, and reduce the numbers of the two last to the time 680, *i. e.* when the jar *a*, coated internally and externally with tinfoil, was discharged, the metallic coating certainly appears to possess an advantage over this jar *a*, as well as over the bottle *b*, filled with mercury; for in 680 seconds the proportion of the concealed residue to the charge imparted at the commencement is

a.	*b.*	*c.*
0·297,	0·281,	0·187,

and the proportion of the same to the total quantity Q_t, which was still present after 680 seconds, is

a.	*b.*	*c.*
0·324,	0·299,	0·204.

According to this, the jar *a*, with the cement, furnished the greatest residue, and that with the metallic coating the least.

Apart from the fact that this difference may be attributed to the kinds of glass and to their thicknesses, the following experiment is particularly worthy of notice.

Before the bottle *b*, filled with mercury, was used for the examination of the residue, it had been already filled with acidulated water and examined, after having previously been washed out with a solution of caustic potash, in order that the surface might be completely moistened. If analogous to the influence of cements, one would expect that in this case the proportions of the residues would be different from those when, between the surfaces of the mercury and the glass, a thin and dry stratum of air must have been interposed; for it may be here remarked, that before pouring in the well-dried mercury, the glass had been washed with distilled water, heated, and sucked dry by means of a glass tube. No difference however was observable in these two cases; the residues formed in the same time had just the same magnitude, as may be seen from the following numbers, which refer to a case where, at both times, the charge at first imparted had been allowed to stand for 606 seconds:—

	L_0.	L_{606}.	Collected residue.
Water . .	1·49	1·03	0·30
Mercury . .	1·49	1·05	0·29

Here, indeed, the mercury appears to have the advantage, though the circumstance may probably be attributed to the great difficulty of constructing two Leyden jars with exactly equal magnitudes of coating, the one with an adhering, and the other with a non-adhering liquid.

The question as to the influence of the cement would be answered most decidedly by coating a glass plate on both sides with amalgam, and after examination replacing it by tinfoil. It will, we think, be difficult to find any measurable difference.

§ 8.

The magnitude of the residue appears to depend most essentially upon the thickness of the glass, the thicker glass producing the greater residue. The experiment about to be cited on this point is not quite decisive, for the amount of residue, of course, depends very much upon the kind of glass; and the two bottles which were compared should, on this account, have been blown from the same mass of glass, and cooled in exactly the same manner. Although this was not the case, yet the interesting result which was obtained may here be mentioned.

The mean thickness of glass of the bottle *b*, so often referred to, was 2·7 millims. This was found from the absolute and specific gravities, as well as from direct measurements. The interior surface, so far as it can be considered as coating, has been already stated to contain 230 square centims.

Another bottle, *e*, of thin glass had a mean thickness of 0·82 millims., and an interior surface of 230 square centims.

Both bottles were converted into Leyden jars by filling, and surrounding them with acidulated water; and by the methods described in Appendix I. and II., it was found that the quantities of electricity in these bottles *b* and *e*, at equal tensions on their knobs, had the proportion 508 : 1303.

To both jars a charge was momentarily imparted, which was immediately indicated by the sine-electrometer as 1·4941; and on discharging the jars after an interval of 9 minutes, the residues 0·3052 and 0·1180 were collected.

The proportion of the mean densities of electricity in the two bottles *b* and *e*, if they possess equal tensions at their knobs, is

$$\frac{508}{275} : \frac{1303}{230}, \text{ or } 1 : 3{\cdot}07.$$

The proportion of the quantities of electricity concealed as residues in *b* and *e* is

$$508 \times 0{\cdot}3052 : 1303 \times 0{\cdot}1180, \text{ or } 1 : 0{\cdot}991.$$

We see, therefore, *that the jar, which, compared with a second, has three times the thickness of glass, produces the same quantity of electricity as residue, although the mean density is three times less.*

Exactly similar phænomena were observed with two Franklin's plates of unequal thicknesses.

§ 9.

For a moment let us subject the usual explanation of the electric residue to a stricter examination.

According to it, the pressure of the electricity imparted to the one coating, and the strong attraction of the opposite electricity on the other coating, gradually force a part of the same into the glass. Either the whole of the electricity which thus penetrates the glass, or only the part which lies deepest, is prevented from sharing in the discharge, and gradually makes its appearance afterwards; for although the mutual attraction between the electricities in the glass has not ceased, the reappearance of the same may be accounted for, without attributing a repulsive force to the substance of the glass, by the changed conditions of equilibrium after the discharge, and particularly by the cessation of the pressure of the imparted electricity on the corresponding coating.

This explanation, however, necessarily involves the assumption, that the potential of the electricity which remains in the glass after discharging is zero for every point within the insulated coating. This results from the following conclusions:—

Let us suppose the insulated coating to have received a positive charge: then, according to hypothesis, there will be a stratum of positive electricity in the glass under it, and a stratum of negative electricity on the other side. The whole or a part of the electricities which have thus penetrated the glass have not been able to participate in the discharge; and their motion within the insulator is so slow, that for the short duration of a discharge they may be considered as motionless. Notwithstanding the quickness of the discharge, however, a state of equilibrium must establish itself, because, during the same, both coatings have been connected by conductors with the earth. If, therefore, the potential of the electricity which has remained in the glass be not zero for a point on the coating formerly charged with positive electricity, *i. e.* should the actions of these electrical masses on the said point not annul one another, then electricity, from some source or other, must have accumulated on the conducting parts of the system, in this instance on the coatings, in order to have produced equilibrium with that in the glass. As to the nature of this latent electricity, as it is called, it is evident, in Franklin's plate for instance, that negative electricity will now be present on the side of the coating which is turned towards the glass, *i. e.* which is next to the positive stratum in the glass, and which had a positive charge imparted to it at the commencement. For inasmuch as a so-called excess of electricity was at first present on this coating, it cannot be assumed that the electricity which has here penetrated the glass is less than that on the other side, and consequently the conclusion is forced upon us, that the action of this positive stratum on its adjacent coating will be greater than that exerted on it by the more distant negative stratum. This unequal action can only be met by assuming a suitable quantity of negative electricity on the inner side of one of the two coatings. As it would be without meaning, however, to place this quantity, a greater in this case, on the side of the glass which is occupied by negative electricity, it must be conceived as being present on the other side, *i. e.* under the coating which was formerly positive.

On the other hand, however, it is evident, if this state of things had actually existed, that the residue capable of being collected on the reinsulated side would but be equal to the difference of the positive electricity within the glass and the latent, negative electricity under the coating. But experiment shows that the collected residue approaches more to an equality with the loss of disposable charge, the less the electricity which is lost in the air, that is to say, the shorter the time during which the plate is allowed to remain charged; so that there can be no doubt that these two quantities would be exactly equal could all loss of electricity be avoided.

Thus it appears to be proved that the potential of the electricity, which during the discharge remained in the glass, must be assumed as zero in reference to the interior of the coating under examination; an assumption which, from the commencement, appears requisite on account of the proportionality existing between the disposable charge and the tension on the knob of the jar.

Further, the following facts ought to be considered:—

1. If after discharging the Franklin's plate it be turned upside down, the sine-electrometer being connected with the coating formerly in connexion with the earth, whilst the side which before was positive is now connected with the earth, a residue of negative electricity will be obtained.

2. If a Franklin's plate, with exactly equal sides, had equal and opposite charges imparted to them, then the quantities which penetrate the glass would certainly be exactly equal, in reference to arrangement and magnitude. In such a case, however, the potential before referred to would certainly not be zero. But how little this state must differ from that which ensues when, with a thin plate, the one coating remains connected with the earth! And, bearing fact 1 in mind, is it not probable that the residue would be obtained with exactly equal and oppositely charged sides?

3. Why, with equal charge of coating, does the thicker Franklin's plate give the greater residue?

4. Why does not the residue continually increase the longer the jar remains charged, or why does it approach a definite maximum dependent upon the magnitude of the charge?

These are points which after some consideration cause legitimate doubts concerning the truth of the whole hypothesis which regards the electricity as penetrating the glass; and in any case they authorize the attempt here made to substitute another hypothesis which will solve all these problems. Departing, therefore, from the usual method of representation, let us seek something new.

§ 10.

In this investigation it will be best, for the present, entirely to neglect the electricity lost in the air, which although unavoidable is still accidental, and to deduce the phænomena of the residue, in their purity, from the facts already recorded, so as to arrive at certain conclusions about them. The phænomenon presents itself in the following manner:—

A quantity Q of electricity is imparted to the insulated coating of the Franklin's plate, whereby a certain state of equilibrium is established. This state of equilibrium next gradually changes, so that a part r of Q enters into a new state, in which it can no longer act on the electroscopic tension; and this part r increases, at first quickly, but afterwards more and more slowly, towards a certain limit R, which it only reaches asymptotically. For the same jar this R is a definite part of Q, so that we have $R=pQ$. Exactly the same part r, too, is prevented from sharing in the discharge; but as soon as $Q-r$ has been discharged, it immediately begins to convert itself into disposable charge, and the conversion, becoming slower and slower, advances towards a certain limit, where we have again a certain part $r'=pr$, which has no electroscopic action, and cannot be discharged.

Instead of considering the part r of the charge (which we have called the concealed residue) as, according to the old hypothesis, placed in a kind of prison which it cannot leave, either personally or with respect to its action, we are still free to assume that it is placed underneath the coating, or, in other words, on the surface of the glass, though subject to an influence formed after, and indeed by the charge imparted to the plate. In order to correspond to the phænomena, the magnitude of this new influence must be a function not only of Q, but also of the time; for after imparting the charge to the jar, it must gradually come into existence, increase to a limit dependent upon Q, and finally, after withdrawing the disposable charge, gradually decrease.

To the inquiry, what this something may be which can exert an influence on the electricity, the only answer compatible with the present state of science is, that this something must again be electricity. As to where it resides, the fact that it continues when both coatings are in connexion with the earth, suggests the assumption that its residence is in the insulator, *i. e.* the glass itself. Lastly, it may be asked, how is it possible that this something, this electricity within the glass, can detain positive electricity under the one coating, and negative under the other? We may reply, that the fact of opposite sides having opposite actions shows that the glass has assumed a polarity; and finally, if, for the sake of greater clearness, we regard similar conditions, *e. g.* the *magnetic moment*, as represented by Gauss in his *Intensitas*, &c., the indefinite notion will be immediately converted into a definite one.

The definition of magnetic moment, as given by Gauss on the 13th page of his celebrated memoir under the hypothesis of a magnetic fluid, can be applied *verbatim* to the electric fluid of every body, so that the explanations of most electrical phænomena might be based upon the electric moment thus defined. Clearly, however, a new expression would be thus unnecessarily introduced for that which, under the terms *electromotive force* and *potential*, has already received its name in science. This expression would be particularly inappropriate for conductors, in which the electric moment will be changed by every external influence, whereas under the magnetic moment of a magnet the idea of stability is associated, or at any rate the idea that within pretty large limits the magnetic moment is essentially independent of external influences. Nevertheless, for the condition and mode of action of a body in whose interior the electric fluids are unequally distributed, and are only capable of extremely slow motion or of none at all, we have no better term, inasmuch as the condition itself is hypothetical, and hence it may not be deemed inappropriate to introduce the expression "*electric moment.*"

Let us conceive the possibility of the neutral electricity within an insulator being by some cause or other so separated, that

a. under the surface on the one side a stratum of positive, and on the opposite side a stratum of negative electricity formed itself; or that

b. a series of such alternate parallel strata were arranged behind each other; or that

c. in every ultimate particle the separation took place in the same direction, but that the electricity could not pass from one particle to another; and if we now further assume that such a state can exist after the cause which produced this separation of the electricities has ceased to act, then such an insulator could exercise an action, *i. e.* an electromotive force, on an external point.

d. A fourth condition is also conceivable; for instance, in every particle of the glass the electricities may be naturally separated, but in general without external action (for exactly the same reason as the molecular currents in soft iron are so, that is, because their own condition of equilibrium does not permit it); but, through some influence or other, the several particles, together with the fixed electricities clinging to them, may be so turned more or less in the same direction, that in this case also the sum of the distances of all positive particles from any plane may be different from the sum of the distances of all negative particles from the same. In reference to some one plane, the difference of these sums may, under certain circumstances, be greatest; and in reference to this plane, or to a normal erected on it, the electric moment of the body will be a maximum, and the normal may be called an electric axis.

By way of example, let us suppose a glass plate with its principal faces *g* and *h* symmetrically placed between two parallel surfaces, *e. g.* metallic plates, which we will call G and H, and which we will suppose to be charged with opposite electricities, then one of the four states *a*, *b*, *c*, or *d* may be conceived gradually to take place in the glass. Through the electromotive force of these metallic plates on the interior of the glass, the natural electricities might there be separated, so that, if G be positive, the negative particles in the glass will now lie nearer to G than the positive ones. We will, for the present, assume that no electricity has passed over immediately from the plates to the glass surfaces; let them now be withdrawn, then the glass will possess an electric moment, whose axis, if we thus call the direction of greatest action, is normal to the surfaces *g* and *h* of the glass, and it will therefore exert an independent electromotive force on any point without these surfaces; so that if a positively electric particle be now presented to the side *g*, where the positive plate G formerly stood, it will be attracted, if a negative it will be repelled: on the other side, *h*, the opposite of this will take place. If we replace the plate G, which has in the mean time been discharged, the potential of the electricities within the glass cannot be zero in reference to a point within the metallic plate; on the contrary, the electricities in the latter will be separated, so that to restore equilibrium positive electricity will continue to pass towards the side turned to the glass, and negative to flow away (supposing the plate not to be insulated), until the potential of the total electricity in reference to the interior of the metallic plate has become zero. Thus on the plate G a quantity of positive electricity, distributed in a determinate manner, is now rendered latent. If, instead of the above, we had replaced the plate H after discharging it and connecting it with the earth, then an exactly opposite condition would have been produced, *i. e.* negative electricity would have been rendered latent on H. If G, however, be first placed in the manner above explained, so that on it the distribution necessary to produce equilibrium can establish itself, then this equilibrium will no longer exist if H be placed in its old position; for to the action of the glass plate *g* is now added the similar action of the negative electricity collected on H; hence it is clear, that, if G and H are connected with the earth, more electricity must be now present on the sides turned towards the glass than when each plate singly stood near the glass, in order that the potential of the whole electricity, in reference to every point within the metallic plates, may be zero.

On the whole, it is clear that nothing will be altered if the plates are brought into contact with the glass; but if so, then we have a Franklin's plate which is charged and incapable of being discharged. The attraction of the electric moment in the glass is at the same time substituted for the pressure of the free electricity on the exterior surface of the coating in a commonly charged jar, which free electricity determines the discharge. Although it is not here asserted that the distribution of the electricities on the coatings of a plate charged by an electric moment of the glass is the same as that on the coatings of a plate charged in the usual manner, it is nevertheless evident that some such distribution causing equilibrium is possible.

If we now insulate one of the coatings, and impart to it a fresh quantity of the same kind of electricity as it already possesses, so that on the other side a corresponding quantity of the opposite kind of electricity will be rendered latent, then these new electricities will distribute themselves as if the plate had not been charged. The principle of statics is here applicable, according to which, if a system of forces are in equilibrium, they will still remain so when another system, itself in equilibrium, is added to the former. Hence the tension, at any point of the insulated coating, will be proportional to the quantity of electricity newly imparted, and the latter only will constitute the disposable charge.

It is only necessary to invert the process of reasoning here given, in order to see that the phænomena of the electric residue are contained in what has been said.

At the commencement, let everything about the Franklin's plate be non-electric. Next, let a quantity Q of positive electricity be imparted to the insulated coating, whereby a certain distribution on both coatings will ensue. An electric moment will now gradually establish itself in the glass, which, by its reaction, will produce a new distribution in such a manner that Q will be divided into two parts, belonging to two different systems of equilibrium, and consequently superposed upon one another. The quantity r which must be present underneath the insulated coating, in order that the newly created action of the electric moment in the glass may be held in equilibrium, is so withdrawn from Q that only the quantity $Q-r=L$ can distribute itself over the coating in the form of disposable charge as at the commencement, and only this quantity can be discharged.

Before long we shall assume a cause for the slow production of the electric moment, and why it can but reach a certain maximum, $r=pQ$, dependent upon the quantity of electricity Q. At present thus much is clear: in our representation of the phænomenon we must be prepared to admit, not only that the disposable part L of the charge, but also that the electromotive force of the whole quantity Q or $L+r$ of electricity on the surfaces of the glass has an influence on the formation of the electric moment, for there is no reason why in this respect one part should be inactive. We attribute the same cause to the slow disappearance of the electric moment as to its slow formation.

In this manner we may easily explain why, after discharging the plate, the concealed residue r continues to convert itself again into disposable charge until the part r' of it which still remains has attained its maximum *pr*. In a similar manner it may be shown why, when a part of the charge is suddenly withdrawn, the disposable charge either sinks more slowly than before, or stands, or increases according to the proportion which the residue already formed bears to the quantity of electricity which still remains.

Hitherto we have neglected all loss of electricity. In practice, where such a loss always occurs, the residue cannot, of course, attain any fixed maximum, and the condition of the plate at the time *t* will depend upon the proportion existing between the velocity with which the residue is formed and that of the loss of electricity in the air. From this point of view all the phænomena may be explained, although, it is true, such explanations will first acquire the requisite amount of certainty when the law of the residue-curve in § 11 shall have been deduced.

We have yet to discuss the cause which generates in the glass a kind of resistance to the influence of the external electricity, or, in other words, to the formation of the electric residue. For this purpose we must dive still deeper into the sea of hypothesis; and although this may appear hazardous when we reflect that we know nothing either of the essential nature of electricity or of the law of molecular forces, yet such an attempt may be permissible provided we confine ourselves to perfectly definite ideas, and obtain a result from our speculations.

Two essentially different suppositions may be made as to the slow formation of the residue, or rather of the electric moment which causes it:—

1. Electricity actually passes from one particle of glass to another, without, however, being able to break through the surfaces of the glass in order to combine with the opposite electricities there situated; so that a condition will ensue exactly the opposite of that before assumed for the penetration of the glass by the external electricities. After the discharge, the electricities in the glass combine in the same manner in which they were separated. Then the slowness of the formation of a residue may be attributed to the great difficulty of motion for the electricity in the interior of a bad conductor; the limit of the moment, however, to the circumstance, that within the glass, and on the coatings, such a distribution has at length taken place, that the potential of the whole electricity, in reference to every point within the glass, has become constant. The question, whether the possibility of such a distribution can be proved or not, must remain an open one. Although in this hypothesis the assumption of a quite peculiar force opposed to the electricity in the surface appears strange, it might still be admitted if everything else fitted well, for the surface is quite different from the interior.

It would be difficult, however, by this method of representation, to show why the thicker plate should furnish the greater residue. Let us therefore consider the second.

2. Electricity does not pass from one particle of glass to another, but either separates itself on each particle, as in the case of magnets, or was already separated on these particles, and the latter, together with the electricities clinging to them, are turned. In the first hypothesis, a force must be attributed to the several particles of glass, which only permits the separation, and the corresponding motion, to take place gradually, and which resists it the more the longer it is prolonged, so that the limit of the residue has herein its cause. Although in this case the molecular forces might play the part of resistance, inasmuch as with the separation of the electricities a simultaneous displacement of the atoms might be connected, yet it is clear that a more natural explanation of the slow change of condition, and of the maximum results from the last hypothesis, *i. e.* that the atoms are turned, and that the molecular forces, as in bends, torsions, &c., resist this turning. And if we consider the phænomena of elasticity as a whole, we shall soon find a circumstance which has great similarity with the one here mentioned, and which in its way stands single. This is the so-called elastic secondary action, which has been made known to us by W. Weber*, through his researches on silken threads (the only complete research which exists on this subject).

Probably all bodies possess this elastic secondary action; at least glass certainly does so†. The phænomenon may be thus characterized:—

When the form of a body is changed through the influence of some external force, without however surpassing the limits of perfect elasticity, the new state of equilibrium within the body, and hence also the change of form, will require time to establish itself; will, in fact, establish itself asymptotically after an indefinitely long period. At first the form changes quickly, afterwards very slowly. The subsequent slow change has been designated the "elastic secondary action," although the latter, as W. Weber remarks, is separated by no natural limit from the quicker change, for the velocity of the change of form does not anywhere decrease discontinuously. If the external action which produced the change of form cease, the body will assume the same series of forms, only in inverse order and with an opposite velocity; only after a long time (indefinitely long) will it perfectly recover its original form. In these wonderful phænomena, where the velocity of the moved mass bears no proportion to the

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* *De fili bombycini vi elastica.* Gottingæ, 1841.

† Poggendorff's *Annalen*, vol. lxxii. p. 393.

moving force, it is, to use an image, as if, whilst the resistance to the attack increased each moment, it became tired out by the duration of the same, so that gradually it became actually exhausted.

In a graphic representation, if we take the time calculated from the moment when the external influence commenced as abscissa, and the magnitudes of the changes of form as ordinates, then to different bodies differently shaped curves will correspond. Whilst with steel the curve, rising at first almost perpendicularly, will then appear bent almost at right angles and have a pretty sharp corner, we find that Weber's curve for the silken thread rises much more slowly, and approaches its asymptote at a greater inclination; and lastly, if we bear in mind the properties of a spiral coil of wax, the tapers cut from which, when placed on our Christmas trees, grew always crooked again, there can be no longer a doubt that the curve in question possesses but a gradual curvature, and rises quickly but for a very short period of time.

According to this, if no loss of electricity took place, there is nothing in the form of the residue curve (R, Plate VI. fig. 2), which was produced in the bottle *b*, that is contradictory to the hypothesis which regards the molecular forces of the glass as the cause of the slow formation of the residue and of its limit; inasmuch as they, in this case as well as in that of elasticity, permit but a slow realization of any changes in the state of equilibrium.

Although, it must be confessed, this explanation by means of a mechanical, resisting force rests ultimately on only one analogous action, we nevertheless abide by this manner of representation, on account of the difficulty there is to find a parallel case for the peculiar phænomenon of so slow a motion in comparison to the acting forces.

By this manner of representing the electric moment of the glass, where throughout the interior of the same the electricity on every particle is brought into a different position, we can now explain why the thicker plate furnishes a greater residue. In order to see this, however, we must again make a small digression.

The question is virtually this. We have two thin, insulated, metallic plates, which, being charged equally with opposite electricities, are placed parallel and at a very small distance from one another in comparison to their own dimensions. Will the action of these plates on a point between them decrease very much when the distance between them is increased, *e. g.* doubled, but still remains very small in comparison to their dimensions?

Here, where a strict calculation appears inadmissible, a few indications may serve to decide the question.

Let a plane, circular surface with the radius R be conceived, charged everywhere with electricity of the same density, so that the unit of surface contains the quantity e. At a distance a from the plane, and in the normal erected in the centre of the circle, conceive the quantity e' of electricity to be placed; required the action, *i. e.* the electromotive force exerted on this point, in the direction of the normal, by the whole electricity on the circular surface.

An element of surface, which is at the distance r from the centre, and makes an angle ϕ with any radius assumed as fixed, can be expressed by $rdrd\phi$; and, neglecting the sign, the action of the same on e' in the direction of the normal is

$$\frac{ee'ardrd\phi}{(a^2+r^2)^{\frac{3}{2}}}.$$

Integrating this, first between the limits $\phi=0$ and $\phi=2\pi$, and then between the limits $r=0$ and $r=\mathrm{R}$, it becomes

$$2ee'a\pi\left(\frac{1}{a}-\frac{1}{\sqrt{a^2+\mathrm{R}^2}}\right).$$

If we here set $\mathrm{R}=\infty$, we obtain the following theorem:— An indefinite plane, charged uniformly with electricity, attracts or repels an electric particle e' with the same force, $2\pi ee'$, whatever may be its finite distance from the same.

If R be not infinite, and $a=m\mathrm{R}$, the formula becomes

$$2ee'\pi\frac{\sqrt{m+1}-m}{\sqrt{m+1}}.$$

In order to see then what influence the distance of the point from the plane of the circle has, we select for m the numerical values 0·1 and 0·01. Then for the electric action we have the numbers

$$2ee'\pi\,.\,0{\cdot}9046 \text{ and } 2ee'\pi\,.\,0{\cdot}9901.$$

We see, therefore, that so long as the distance is small in comparison to the radius, the electric action is almost independent of the distance; for instance, when the distance is increased ten times, the action is only one-tenth of what it was before.

On the other hand, we may conclude that the action exerted on e' is principally due to the parts of the plane which lie nearest it. For example, if a remains the same, whilst for R we set at one time $10a$, and then $100a$, we obtain for the second action scarcely one-tenth more than for the first.

These numbers show, that if the electricities were uniformly distributed over the coatings of a Franklin's plate, and if, whilst their quantity remained the same, the thickness of the glass were increased, the electric moment would also be augmented, because more particles of glass would now be acted upon. But the electricity actually is distributed almost uniformly over a great portion of the Franklin's plate; and near the rim, where this is not the case, the principal action on the points in the glass must be ascribed to the neighbouring parts of the coatings, so that the conclusions before made will be approximately true*. Herefrom we learn, not only that with the same quantity of charge a greater electric moment is produced with the thicker glass, but also that at different places the action on the glass will be different; so that the manner in which the concealed residue produced by the electric moment disposes itself, cannot be very different from the distribution of the electricity which is produced underneath the coating by the disposable charge.

What has here been said of the Franklin's plate may, with modifications suited to their forms, be on the whole repeated of the common Leyden jar; hence it would appear that the phænomena of the electric residue may be completely explained by the electric moment, particularly under the hypothesis that the change in the glass affects its individual particles only.

[To be continued.]

LXVIII. *On the Voltaic Decomposition of Water.* *By* A. CONNELL, *F.R.S.E., Professor of Chemistry in the University of St. Andrews*†.

I HAVE learned during the last few months, from that excellent French periodical the *Cosmos*, that some experiments lately made by M. Leon Foucault and others, showing a difference in the amount of hydrogen evolved from two negative poles placed in water by the same galvanic current under certain varieties of circumstances, particularly where the current passed through acidulated water and distilled water, have been attracting a good deal of attention, and that the appearances have been thought by M. Foucault and some others to lead to some

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* The above must not be confounded with the fact, that the thinner the glass is, the stronger the charge which the same constant source of electricity will impart to the Franklin's plate. Here, as in the condenser, the theorem holds, that if the distances between the plates are very small in comparison to their magnitude, their charges are almost inversely proportional to their distances asunder; a result which agrees very beautifully with the calculations of Clausius in his able memoir "On the distribution of electricity on a single, very thin plate, and on the two coatings of a Franklin's plate" (Poggendorff's *Annalen*, vol. lxxxvi. p. 198, x.). What we have considered above is a quite different question to the one, how much electricity must be present on plates, at different distances asunder, in order that its potential, in reference to their interior, may everywhere be exactly the same.

† Communicated by the Author.

restriction of Mr. Faraday's beautiful law of the definite decomposing agency of the galvanic current. It may not perhaps be altogether superfluous to state, that it is now nearly fourteen years since I read to the Chemical Section of the British Scientific Association at Glasgow a notice of experiments to precisely the same effect, although from which assuredly no inference was drawn affecting Mr. Faraday's law; and which notice was afterwards inserted, in January 1841, in the 18th volume of this Journal, page 49*. My object at that time was to illustrate the voltaic decomposition of alcohol, by showing that the quantity of hydrogen liberated from the negative pole in acidulated water was the same as that liberated from the negative pole in alcohol, having its conducting power improved by the solution in it of minute quantities of potash or of potassium; my inference from the whole experiments on the subject being, that it was the water of the alcohol, considered as a hydrate which suffered voltaic decomposition. To obtain this comparative illustration of that view, it was shown that certain particulars required to be attended to, and that two circumstances seemed to modify the result: *first,* the absorption of hydrogen by the fluid, which hydrogen entered into the constitution of the secondary products of oxidation formed at the positive pole; and *secondly,* that when the conducting power of the liquid diminished, the galvanic action diminished also, "a result," I added, "which may be imitated *by passing the same current through distilled water, and water acidulated with sulphuric acid, when the hydrogen liberated from the pure water will be found to be notably less than that from the well-conducting fluid.*" Here, then, we have *precisely the same experiment* made and published about fourteen years ago, which is now brought forward as new by M. L. Foucault†.

On looking back to my notes of the experiment here referred to, I find more than one taken down, and all to the same effect. The following I find amongst them. Two tubes with platinum wires terminated by platinum foils, sealed hermetically in their closed extremities, were filled with distilled water and inverted in a small evaporating basin also containing distilled water, and secured in that position. Two other similar tubes were filled with distilled water acidulated with $\frac{1}{12}$th of sulphuric acid, and inverted in another evaporating basin containing acidulated water. Connexion was then made by a wire between the wires of two of the tubes, one in each basin; and the wires of the other two tubes were connected respectively with the poles of a Cruik-

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* In the eighth line of that page for *rigorous* read *vigorous.*

† M. Foucault's notice was read to the Academy of Sciences, Feb. 20, 1854. *Cosmos,* vol. iv. p. 249.

shank's battery of 36 pairs of 4-inch plates, charged with water containing $\frac{1}{33}$rd of a mixture of two measures sulphuric acid and one measure nitric acid. From the bad conducting power of the distilled water which formed a part of the circuit, the action was slight. During the first half-hour similar minute quantities of hydrogen were collected at the two negative poles. After $8\frac{3}{4}$ hours' action, a decided difference in the quantities had become apparent, there having been evolved from the negative pole in the distilled water 0·04 cubic inch, and from the negative pole in the acidulated water 0·07 cubic inch. After 24 hours, the amounts were respectively 0·105 cubic inch and 0·2 cubic inch, *i. e.* twice as much from the acidulated water as from the distilled water.

The only inference which I attempted to draw from such experiments was, that the bad conducting power of the distilled water interfered with the full electrolytic action. I confess that it never once occurred to me that any inference could be drawn from them, trenching upon the law of definite voltaic chemical agency; nor does it yet appear to me that there is any foundation for such an inference*. If M. Foucault's views be well founded, that it follows from this and other experiments to which he refers, that liquids have a conducting power independently of suffering decomposition, and that in experiments such as the above, a part of the current passes by this proper conduction, how is it to be expected that the full decomposing effect should be produced? It would be a violation of the law of definite agency if the same amount of electrolytic action ensued, when a part of the current passed by proper conduction, as when the whole current passed in virtue of decomposing agency. There is scarcely any law in science which does not present exceptions and limitations, till we come to be able to ascertain the true causes of such restrictions, when they by degrees return within the limits of the general proposition. The histories of the relation between atomic weights and specific heats, of isomorphism, and even of gravitation itself, afford illustrations of this principle.

St. Andrews, May 8, 1854.

LXIX. *On the Generation of Heat by Electricity.* *By* P. RIESS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I REGRET exceedingly to find that Mr. Thomson, in the Philosophical Magazine for May, page 347, while admitting one error, has fallen into a new and more serious one, which I

[* On reconsidering the subject our correspondent may, perhaps, find reason to change his opinion.—ED.]

cannot allow to pass unnoticed. Mr. Thomson no longer ascribes the discovery of the law, that the quantity of heat generated by the Leyden jar is proportional to the square of the quantity of electricity, to Mr. Joule, but claims it for Cuthbertson, from whose experiments, he asserts, M. Becquerel had "quite explicitly enunciated" the law in 1835. I regret that the remark of M. Becquerel has not been given completely, for if this had been done I should have been spared the present communication. The place in the original reads as follows:—"Cuthbertson et autres physiciens qui ont mesuré avec des électromètres l'action calorifique produite par la décharge d'une batterie, ont trouvé qu'elle croissait à peu pres comme la carré des charges des batteries pour certaines longueurs des fils. Cette loi varie en outre suivant l'épaisseur des jarres qui composent la batterie. Celles qui sont épaisses ont une puissance de *fusion* moins grande." It is not necessary to be acquainted with the experiments of Cuthbertson to see, from the tenour of the above, that M. Becquerel speaks of *fusion*, and of fusion only. That, however, by the well-known fusion experiments of Cuthbertson and others, which I myself have discussed in a memoir upon the same subject (Poggendorff's *Annalen*, vol. lxv. p. 497, translated in the Scientific Memoirs), the law in question has been proved, will scarcely be asserted by any man, even though he should not be acquainted with the fact that in the case of fusion, besides the heating, another electric action comes into play.

I have the honour to be,
Your obedient Servant,
Berlin, May 11, 1854. P. RIESS.

LXX. *On certain questions relating to the Moon's Orbit.* *By* Professor CHALLIS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I BEG of you the favour to make public the following statement on a matter which nearly concerns my scientific credit.

In the communication which I made to the April Number of the Philosophical Magazine on two new Theorems relating to the Moon's Orbit, I stated that a paper containing the proofs of these theorems had been read before the Cambridge Philosophical Society. Having become aware that hesitation was felt on the part of the Council of the Society to order the printing of the paper, on account of an adverse opinion formed of it by one or both of the gentlemen to whom it was referred, I at once withdrew it, with the view of relieving the Council from the necessity of coming to a decision, and because I considered that my communication to the Philosophical Magazine sufficiently secured my priority in the discovery of the remarkable theorems which I had succeeded in demonstrating. After I had signified my withdrawal, one of the reporters submitted to me of his own accord the reasons which induced him to form an unfavourable opinion of the paper, in perfect confidence that these reasons would convince me of my errors. The arguments are mainly directed against the truth of the new equation

$$\frac{dr^2}{dt^2}+\frac{h^2}{r^2}-\frac{2\mu}{r}-\frac{m'r^2}{2a'^3}+\mathrm{C}=0$$

(see Phil. Mag. for April, p. 280), which is the principal feature of my paper, and with which the theorems stand or fall. Now what distinctly marks the character of these arguments is, that *they contain not the slightest allusion to the reasoning by which the equation is established.* They are all drawn from *extraneous* considerations, or from *supposed consequences* of the equation, and are all such as I would readily answer if they were advanced publicly.

In fact, the proof of the equation is too simple and straightforward to admit of any question. In support of this assertion, I appeal to the reasoning given in my communication to the Philosophical Magazine; and I may add, that in an examination for the award of Dr. Smith's prizes held in January last, I proposed this equation for the candidates to investigate, when it was impossible they could have known of its existence till the examination paper was placed before them, and I received a complete proof of it. This circumstance suffices to show that it requires for its demonstration nothing but mathematical principles and processes commonly received and taught.

I now invite the reporter on my paper to discuss with me in this Journal (anonymously if he pleases), the important and interesting questions in the Lunar Theory raised by my communication to the April Number.

I am, Gentlemen,
Your obedient Servant,
J. Challis.

Cambridge Observatory,
May 22, 1854.

LXXI. *Note on a Formula by aid of which and of a table of single entry the continued product of any set of numbers (or at least a given constant multiple thereof) may be effected by additions and subtractions only without the use of Logarithms.* By J. J. Sylvester, *M.A., F.R.S.**

Introduction.

THE remark to which this note refers is not new; it has been well observed somewhere in Gergonne's *Annales* (Mr. Cayley being my informant), that by aid of the formula

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* Communicated by the Author.

$4ab=(a+b)^2-(a-b)^2$ the question of finding the product of two numbers is virtually reduced to a process of addition and subtraction, and of finding the values of two squares out of a table of squares. If the two factors a and b are both even or both odd, the formula ought to be changed into

$$ab=\left(\frac{a+b}{2}\right)^2-\left(\frac{a-b}{2}\right)^2;$$

if one of them is odd and the other even, we may employ the formula

$$ab=\left(\frac{a+b-1}{2}\right)^2-\left(\frac{a-b+1}{2}\right)^2+a.$$

So, again, for the product of three numbers, there exists the analogous formula

$$6abc=(a+b+c)^3-(a+b-c)^3-(b+c-a)^3-(c+a-b)^3.$$

Object of the Paper.

The object of this brief note is to exhibit and demonstrate the generalization of the above formulæ, *i. e.* to express the product of any n quantities $a_1, a_2, {}_3, \ldots a_n$ under the form of the sum of powers of simple linear functions of $a_1, a_2, \ldots a_n$. This may be done as follows.

General Formula.

Let $\theta_1, \theta_2, \theta_3, \ldots \theta_n$

be disjunctively equal to

$$1,\ 2,\ 3, \ldots n,$$

then

$$\begin{aligned}&(2\,.\,4\,.\,6\ldots 2n)(a_1\,.\,a_2\ldots a_n)\\ &=(a_{\theta_1}+a_{\theta_2}+a_{\theta_3}+\ \ldots\ +a_{\theta_n})^n-\Sigma(-a_{\theta_1}+a_{\theta_2}+\ \ldots\ +a_{\theta_n})^n\\ &+\Sigma(-a_{\theta_1}-a_{\theta_2}+a_{\theta_3}+\ \ldots\ +a_{\theta_n})^n+\&c.\\ &+(-)^n(-a_{\theta_1}-a_{\theta_2}-\ \ldots\ -a_{\theta_n})^n,\end{aligned}$$

which I call the principal equation.

Demonstration of the principal Equation.

Let $\phi_1, \phi_2, \phi_3, \ldots \phi_{n-1}$

be disjunctively equal to

$$1,\ 2,\ 3, \ldots (n-1),$$

then it is easily seen that

$$(a_{\theta_1}+a_{\theta_2}+\dots+a_{\theta_n})^n=(a_{\phi_1}+a_{\phi_2}+\dots+a_{\phi_{n-1}}+a_n)^n$$

$$\Sigma(-a_{\theta_1}+a_{\theta_2}+\dots+a_{\theta_n})^n=\Sigma(a_{\phi_1}+a_{\phi_2}+\dots+a_{\phi_{n-1}}-a_n)^n$$
$$+\Sigma(-a_{\phi_1}+a_{\phi_2}+\dots+a_{\phi_{n-1}}+a_n)^n$$

$$\Sigma(-a_{\theta_1}-a_{\theta_2}+\dots+a_{\theta_n})^n=\Sigma(-a_{\phi_1}+a_{\phi_2}+\dots+a_{\phi_{n-1}}-a_n)^n$$
$$+\Sigma(-a_{\phi_1}-a_{\phi_2}+\dots+a_{\phi_{n-1}}+a_n)^n$$

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

$$\Sigma(-a_{\theta_1}-a_{\theta_2}\dots-a_{\theta_{n-1}}+a_{\theta_n})^n=\Sigma(-a_{\phi_1}-a_{\phi_2}\dots-a_{\phi_{n-2}}+a_{\phi_{n-1}}-a_n)^n$$
$$+(-a_{\phi_1}-a_{\phi_2}\dots-a_{\phi_{n-1}}+a_n)^n$$

$$(-a_{\theta_1}-a_{\theta_2}\dots-a_{\theta_{n-1}}-a_{\theta_n})^n=(-a_{\phi_1}-a_{\phi_2}\dots-a_{\phi_{n-1}}-a_n)^n.$$

Hence it is apparent that when $a_n=0$, the right-hand side of the so-called principal equation spontaneously vanishes; it will therefore always contain a_n as a factor, and by parity of reasoning it will contain every one of the quantities $a_1, a_2, \dots a_n$ as a factor, and will consequently be equal to the product $a_1, a_2, \dots a_n$ multiplied by a numerical factor, which by making $a_1, a_2, \dots a_n$ each equal to unity, is readily seen to be

$$2^n\times(1.2.3\dots n)$$

(2^n being the sum of the numbers of terms in the $(n+1)$ groups); or if we please so to say, to $2.4.6\dots(2n)$. Q. E. D.

Conclusion.

If n is odd and be called $2m+1$, we have

$$4.6.8\dots(2n)a_1.a_2\dots a_n$$
$$=(a_{\theta_1}+a_{\theta_2}+\dots+a_{\theta_n})^n-\Sigma(-a_{\theta_1}+a_{\theta_2}+\dots+a_{\theta_n})^n$$
$$+\Sigma(-a_{\theta_1}-a_{\theta_2}+a_{\theta_3}+\dots+a_{\theta_n})^n\mp\&c.$$
$$+(-)^m(-a_{\theta_1}-a_{\theta_2}\dots-a_{\theta_m}+a_{\theta_{m+1}}+a_{\theta_{m+2}}+\dots+a_{\theta_n})^n;$$

and if n be even and be called $2m$, we have

$$4.6.8\dots(2n)(a_1.a_2\dots a_n)$$
$$=(a_{\theta_1}+a_{\theta_2}+\dots+a_{\theta_n})^n-\Sigma(-a_{\theta_1}+a_{\theta_2}+\dots+a_{\theta_n})^n$$
$$+\Sigma(-a_{\theta_1}-a_{\theta_2}+a_{\theta_3}+\dots+a_{\theta_n})^n\mp\&c.$$
$$+\tfrac{1}{2}(-)^m\Sigma(-a_{\theta_1}-a_{\theta_2}\dots-a_{\theta_m}+a_{\theta_{m+1}}+a_{\theta_{m+2}}+\dots+a_{\theta_n})^n;$$

where, it should be observed, that the last term is made up of integer parts, notwithstanding the presence of the factor $\frac{1}{2}$, which factor may be construed as only serving to denote that, of any pair of complementary linear functions of those which enter into this term, such as

$$-a_{q_1}-a_{q_2}\dots-a_{q_m}+a_{q_{m+1}}+a_{q_{m+2}}+\dots+a_{q_n}$$

and

$$-a_{q_{m+1}}-a_{q_{m+2}}\dots-a_{q_n}+a_{q_1}+a_{q_2}+\dots+a_{q_m},$$

one only is to be retained. The entire term is of course made up exclusively of such pairs.

Corollary.

If $\mathrm{R}(a_1, a_2, \dots a_n)$ denote any symmetrical algebraic function whatever of $a_1, a_2, \dots a_n$,

$$\Sigma_n^{\overset{0}{i}}\,\Sigma_{\nu_i}^{\overset{0}{\theta}}\,(-)^i\mathrm{R}(-a_{\theta_1},\ -a_{\theta_2},\dots-a_{\theta_i},\ a_{\theta_{i+1}},\ a_{\theta_{i+2}},\dots a_{\theta_n})$$

will contain $a_1 \,.\, a_2 \,.\, a_3 \dots a_n$ as a factor. In this formula ν_i denotes the number of combinations of n things taken i and i together.

26 Lincoln's Inn Fields,
March 8, 1854.

Postscript.

In constructing a table of single entry for appplying the formula

$$4ab=(a+b)^2-(a-b)^2,$$

i. e.

$$ab=\frac{1}{4}(a+b)^2-\frac{1}{4}(a-b)^2,$$

it is only necessary to retain the integer part of the quarters of the squares of all the numbers from 2 to the sum of the highest of the values of a and b to which the application of the table is proposed to be restricted, because the fractional parts of $\left(\frac{a+b}{2}\right)^2$ and $\left(\frac{a-b}{2}\right)^2$ will always destroy one another. A table for the multiplication of a ternary set of factors by means of the formula

$$abc=\frac{1}{24}(a+b+c)^3-\frac{1}{24}(a+b-c)^3-\frac{1}{24}(a-b+c)^3$$
$$-\frac{1}{24}(-a+b+c)^3,$$

will imply the registration of the values of the 24th parts of all numbers up to the highest value of $(a+b+c)$, and it becomes a question of some practical interest to determine in what way the fractional remainders of these 24th parts are to be dealt with.

The formula last written may give rise to either of the two subjoined cases, according as the numbers a, b, c correspond or not to the lengths of a possible triangle, viz.

$$(1) \qquad abc=\frac{1}{24}N_1^3-\frac{1}{24}N_2^3-\frac{1}{24}N_3^3-\frac{1}{24}N_4^3,$$

or

$$(2) \qquad abc=\frac{1}{24}N_1^3+\frac{1}{24}N_2^3-\frac{1}{24}N_3^3-\frac{1}{24}N_4^3,$$

the quantities N_1, N_2, N_3, N_4 being all supposed to represent positive integers.

A very little consideration will show, that if we neglect fractions in the table there may be entailed an error of 2, 1, 0, or -1. Whether the error is, on the one hand, an error of an even order (viz. 0 or 2), or, on the other hand, of an odd order (viz. 1 or -1), would be at once obvious by looking to see whether the formula, after neglecting the fractions, gave an odd result when the result ought to be odd, and an even result when the result ought to be even, or *vice versâ*. And the nature of the result as to whether it *ought* to be odd or even could be immediately inferred from observing whether a, b, c were or were not all of them odd numbers. But there would still remain an ambiguity in the correction to be applied in either case, arising from the doubt whether it should be zero or 2 in the one case, or whether it should be $+1$ or -1 in the other case.

This ambiguity might of course be removed by inserting in the table employed the first decimal place of $\frac{N^3}{24}$, and increasing the decimal part in the final result to unity, or lowering it to zero, according as its value might be greater or less than $\frac{1}{2}$; and it would be easy to ascertain the limits within which the decimal digit in the result must lie, and the range of values (of which 5 is one) from which it is excluded. The same end may, however, be gained much more elegantly and expeditiously, and by a method more closely analogous to that employed for the evolution of binary products, by the intervention of a very simple expedient.

The cubic residues in respect to the modulus 24 are easily verified to be as follows: 0, 1, 3, 5, 7, 8, 9, 11, 13, 15, 16, 17, 19, 21, 23. Let the tabular value of $\frac{N^3}{24}$ be made $\left[\frac{N^3}{24}\right]+K_N$, where $\left[\frac{N^3}{24}\right]$ means the integer part of the quantity within the brackets, and K_N may have any one of the three values 0, $\frac{1}{2}$, 1, viz.

$K_N=0$ when the remainder of N^3 to the divisor 24 is 0, 1, 3, or 5;

$K_N = \frac{1}{2}$ when the said remainder is 7, 8, 9, 11, 13, 15, 16 or 17; and

$K_N = 1$ when the remainder is 19, 21 or 23;

and let $\left[\frac{N^3}{24}\right] + K_N$ be called the cubic respondent to N, and be denoted by R(N);

and let the exact value of $\frac{N^3}{24}$ be called R′(N).

Let

$$R'(a+b+c) - R'(a+b-c) - R'(a-b+c) - R'(-a+b+c)$$
$$= R(a+b+c) - R(a+b-c) - R(a-b+c) - R(-a+b+c) + \Delta.$$

If in general we write $R'(n) - R(n) = E(n)$, Δ must be of one or the other of the two forms

$$E(n_1) - E(n_2) - E(n_3) - E(n_4),$$

or

$$E(n_1) + E(n_2) - E(n_3) - E(n_4),$$

where n_1, n_2, n_3, n_4 are supposed to be all positive integers. Now it is easily seen that $E(n)$ always lies within the limits $\pm\frac{5}{24}$; that is to say, it may reach up to $\frac{5}{24}$ or down to $-\frac{5}{24}$, but can never transgress these values in either direction. Hence it is obvious that Δ, which is made up of four terms, each of the form $E(n)$, can never be so great as $+1$ or so small as -1, and consequently Δ can only have one of the three values $+\frac{1}{2}$, 0, $-\frac{1}{2}$.

Hence, then, we may work with the tabular cubic respondents in lieu of the exact cubic respondents; if the result is an integer, it is good without any correction; if it is a fraction, $\frac{1}{2}$ must be added to, or taken away from it. And to ascertain which of these processes is to be applied, it is only necessary to consider whether the three factors to be multiplied are or not all of them odd.

In practically constructing a table of cubic respondents, it would not be necessary actually to insert the fraction $\frac{1}{2}$ in any case; a dot over, or a stroke through the last integer, would serve to denote that this fraction was to be understood.

A table of quadratic respondents (*i. e.* of the integer parts of the fourths of the square numbers) up to the base 20,000, has been actually constructed and published by a M. Antoine Voisin, under the title "Tables des Multiplications ou Logarithmes de Nombres entiers depuis 1 jusqu'à 20,000, au moyen desquelles on peut multiplier tous les nombres qui n'excèdent pas 20,000 par 20,000," &c. 12mo. à Paris, Firmin Didot, 1817. A copy of this is in Mr. J. T. Graves's valuable mathematical library at Cheltenham.

By logarithms the author intends the same quantities as I term respondents, certainly a less objectionable and safer term to employ. There appears to be an error in the title in affirming that any two numbers, not separately exceeding 20,000, may be multiplied by aid of these tables, as the sum of the two factors ought not to exceed 20,000. Mr. Peter Gray, so favourably known to an important section of the public as the author of many useful tables, has informed me that Major Shortredd, now in India, has computed a table of quadratic respondents extending to the argument 200,000, which he is taking measures to have published. Such tables would be very useful to computers, as they would serve for the multiplication of any two numbers whatever not containing more than five figures each. I should like to see a table of cubic respondents up to 30,000 appended to this work*.

26 Lincoln's Inn Fields,
May 12, 1854.

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* The best practical mode of using and arranging such a table I find, after much thought and consideration, would be as follows. It is easy to add two quantities and subtract their sum from a third by a single operation. If, then, a, b, c are the three numbers whose product it is required to find, they should be written under one another; and against (a) should be set the value of $a-b-c$; against (b), that of $b-a-c$; and against (c), that of $c-a-b$; under these three last results should be written the value of $a+b+c$; of the three former, two at least must be, all *may* be negative; their values arithmetically expressed will be of the form $\bar{\text{K}}(10{,}000)+\text{N}$, where K is 0, 1 or 2. In order that the final process of combining the 4 cubes may be made purely additive, the tables should show the values of $(10{,}000)^3$ less the respondent to $\text{K}(10{,}000)-\text{N}$, when K is 1 or 2 for all values of N from 1 to 9999. These complements to the respondents of the simple or augmented complements of N may be termed respectively the simply and doubly affected respondents of N, but in using the tables no distinction need be drawn between the respondents and the affected respondents. The arrangement of the tables will be as follows. In each page there will be a column for the arguments, which will extend from 1 to 9999, and five other columns containing respondents and bearing respectively for their headings the numbers $\bar{2}$, $\bar{1}$, 0, 1, 2. The four quantities formed by addition, or by addition and subtraction, from a, b, c, will all be of the form $\text{K}\,\nu_1\,\nu_2\,\nu_3\,\nu_4$ ($\nu_1\,\nu_2\,\nu_3\,\nu_4$ denoting respectively some one or other of the digits from 0 to 9), and K being one of the five symbols $\bar{2}$, $\bar{1}$, 0, 1, 2, the value corresponding to $\nu_1\,\nu_2\,\nu_3\,\nu_4$ will then be sought for in its proper column (according to the value of the guiding figure K), and the sum of the four values so found will be taken (the last figure to the left, which will be 2 or 3, being rejected). This result, affected, if necessary, with the proper correction of $\pm\frac{1}{2}$, will express the value of $a\times b\times c$.

LXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 371.]

March 23, 1854.—Colonel Sabine, R.A. and V.P., in the Chair.

THE following paper was read :—" Note on an indication of depth of Primæval Seas, afforded by the remains of colour in Fossil Testacea." By Edward Forbes, F.R.S., Pres. G.S. &c.

When engaged in the investigation of the bathymetrical distribution of existing mollusks, the author found that not only did the colour of their shells cease to be strongly marked at considerable depths, but also that well-defined patterns were, with very few and slight exceptions, presented only by testacea inhabiting the littoral, circumlittoral and median zones. In the Mediterranean only one in eighteen of the shells taken from below 100 fathoms exhibited any markings of colour, and even the few that did so, were questionable inhabitants of those depths. Between 35 and 55 fathoms, the proportion of marked to plain shells was rather less than one in three, and between the sea-margin and 2 fathoms the striped or mottled species exceeded one-half of the total number.

In our own seas the author observes that testacea taken from below 100 fathoms, even when they were individuals of species vividly striped or banded in shallower zones, are quite white or colourless. Between 60 and 80 fathoms, striping and banding are rarely presented by our shells, especially in the northern provinces; and from 50 fathoms shallow-wards, colours and patterns are well marked.

The relation of these arrangements of colour to the degrees of light penetrating the different zones of depth, is a subject well worthy of minute inquiry, and has not yet been investigated by natural philosophers.

The purpose in this brief notice is not, however, to pursue this kind of research, but to put on record an application of our knowledge of the fact that vivid patterns are not presented by testacea living below certain depths, to the indication of the depth, within certain limits, of palæozoic seas, through an examination of the traces of colour afforded by fossil remains of testacea.

Although their original colour is very rarely exhibited by fossil shells, occasionally we meet with specimens in which, owing probably to organic differences in the minute structure of the coloured and colourless portions of the shell, the pattern of the original painting is clearly distinguished from the ground tint. Not a few examples are found in Mesozoic as well as in Tertiary strata, but in all the instances on record, the association of species, mostly closely allied to existing types, and the habits of the animals of the genera to which they belong, are such as to prevent our having much difficulty about ascertaining the probable bathymetrical zone of the sea in which they lived.

But in palæozoic strata the general assemblage of articulate, molluscan and radiate forms is so different from any now existing with which we can compare it, and so few species of generic types still remaining are presented for our guidance, that in many instances we can scarcely venture to infer with safety the original bathymetrical zone of a deposit from its fossil contents. Consequently any fact that will help us in elucidating this point becomes of considerable importance.

Traces of colouring are rarely presented by palæozoic fossils, and the author knows of few examples in which they have been noticed. Professor Phillips, in his 'Geology of Yorkshire,' represents the carboniferous species, *Pleurotomaria flammigera* (i. e. *carinata*) and *conica*, as marked with colour, and Sowerby has figured such markings in *P. carinata* and *P. rotundata*. In the excellent monograph of the carboniferous fossils of Belgium, by Professor De Koninck of Liège, indications of pattern-colouring are faintly shown in the figures of *Solarium pentangulatum*, and distinctly in those of *Pleurotomaria carinata* and *Patella solaris*.

In the cabinets of the Geological Survey of Great Britain are some finely-preserved fossils from the carboniferous limestone of Parkhill, near Longnor in Derbyshire. Among these are several that present unmistakeable pattern-markings, evidently derived from the original colouring. They are—

Pleurotomaria carinata and *conica*, showing wavy blotches, resembling the colouring of many recent *Trochidæ*.

An undescribed *Trochus*, showing a spiral band of colour.

Metoptoma pileus, and

Patella ? retrorsa, both with radiating stripes, such as are presented by numerous existing *Patellidæ*.

Natica plicistria, with broad mottled bands.

Aviculo-pecten, a large unnamed species, with spotty markings on the ribs in the manner of many existing *Pectines*.

Aviculo-pecten sublobatus, Ph. ? Beautifully marked with radiating, well-defined stripes, varying in each individual, and resembling the patterns presented by those recent *Aviculæ* that inhabit shallows and moderate depths.

Aviculo-pecten intercostatus and *elongatus* also exhibit markings.

Spirifer decorus and *Orthis resupinata*, show fine radiating white lines.

Terebratula hastata, with radiating stripes.

The analogy of any existing forms that can be compared with those enumerated, would lead to the conclusion that the markings in these instances are characteristic of mollusks living in a less depth of water than 50 fathoms. In the case of the *Terebratula*, which belongs to a genus the majority of whose living representatives inhabit deep water, it may be noticed that all the living species exhibiting striped shells are exceptions to the rule, and come from shallow water.

There are many circumstances which warrant us to suspect that the carboniferous mountain limestone of most regions was a deposit in shallow water. The facts now adduced materially strengthen this inference.

In the British Museum there is a beautifully spotted example of a Devonian *Terebratula*, brought by Sir John Richardson from Boreal America.

Specimens of the *Turbo rupestris*, from the Lower Silurian Limestone of the Chair of Kildare near Dublin, exhibit appearances that seem to indicate spiral bands of colour.

March 30.—Thomas Bell, Esq., V.P., in the Chair.

The following paper was read:—Note on the Melting-point and Transformations of Sulphur." By B. C. Brodie, Esq., F.R.S.

In the treatises of chemistry where the results of different observers are collected, various statements will be found as to the melting-point of sulphur. The numbers given in Gmelin's Chemistry vary from 104°·5 C. to 112°·2 C., but of five chemists cited, no two agree as to this apparently simple fact. There is evidently some peculiarity about this melting-point which is the cause of these anomalous results. In some experiments on allotropic substances, in which I have been engaged, I had occasion to submit this question to a more searching inquiry than it had hitherto received, in which I have discovered the cause of these discrepancies. In the present note I will briefly give the results at which I have arrived, reserving the details for a further and more full communication.

The melting-point of sulphur varies according to its allotropic condition. This condition is readily altered by heat, and invariably, without peculiar precautions, by melting. Hence the temperature at which sulphur melts is different from that at which it will solidify, or at which, having been melted, it will melt again.

The melting-point of the octohedral sulphur, as crystallized from the bisulphide of carbon, is 114°·5 C. But from the facility with which this sulphur, when heated even below its melting-point, passes into the sulphur of the oblique system, this fact may readily be overlooked. When this sulphur, in the state of fine powder, is heated even for the shortest time between 100° and 114°·5, this change cannot be avoided. For the transformation of large crystals a longer time is required. At a certain point the crystal becomes opake, and is often broken in pieces at the moment of the change. When in such a crystal this change has either entirely or partially taken place, the melting-point will be above 114°·5. The minute crystals of sulphur from alcohol, which are so extremely thin that their angles cannot be measured, have this melting-point of 114°·5, which fixes the system to which the crystals belong. The crystals of sulphur from benzole (rectified coal naphtha) melt also at 114°·5. The crystals from alcohol are very minute, consequently so readily transformed, that they presented anomalies which led me to doubt whether sulphur of both forms did not exist among them. I answered this question by dividing a certain number of carefully selected crystals, and taking the melting-point of the two halves of the same crystal. I found that these melting-points in many cases did not correspond, which would have been the case if the anomalies had arisen from the different nature of the crystals. Sulphur which has been melted at 114°·5, and of which the temperature has not been raised above 115°, remains, on solidification, perfectly transparent for any length of time. Heated beyond this point, it becomes, on cooling, more or less opake.

When sulphur has been converted by heating for a sufficient length of time, in the manner above mentioned, between 100° and 114°·5, it acquires a fixed melting-point of 120° C. This is the melting-point of the oblique prismatic sulphur. If sulphur thus converted be carefully melted so as to raise the temperature as little as possible above the melting-point, no sensible difference will be observed between the point of melting and of solidification. To obtain this fixed melting-point of 120°, care must be taken that the transformation of the sulphur has been thoroughly effected. If this be not done, it may melt at any point between 114°·5 and 120°. If, however, the temperature of the melted sulphur be raised above its melting-point of 120°, the point of solidification will be altered, and may lie even below the first melting-point of 114°·5 *. The point of solidification is in this case not fixed, but depends upon the temperature to which the sulphur is raised and upon the mode in which it is cooled. It has varied in my experiments from 118° to as low as 111°. When the melting-point of the sulphur, thus solidified, is taken, it will begin to melt at about the temperature of solidification. The cause of this anomaly is evident. When the temperature of sulphur is raised above 120°, a transformation into the viscid form instantly commences, so that the sulphur is a mixture of the two varieties, and the melting-point varies according to the proportion in which these two varieties are mixed. It varies inversely with the temperature to which the sulphur is raised, so that the presence of the viscid sulphur lowers the point of solidification. There is, however, a limit beyond which the melting-point is not affected by this admixture. I made the experiment of pouring sulphur, heated to its boiling-point, into water of different temperatures, and of taking the melting-point of the sulphur when it had become hard. Five different preparations, which, when extracted with bisulphide of carbon, gave each a different quantity of insoluble sulphur, coincided in the melting-point of about 112°. This sulphur, before melting, becomes transparent, and passes again into the viscid or elastic condition.

The sulphur which is insoluble in bisulphide of carbon, and which is prepared by extracting the hardened viscid sulphur with that reagent, has a melting-point considerably above 120°, but which I have not been able to determine with precision.

I had placed in a water-bath, at 100°, tubes containing fragments of the three definite varieties of sulphur. After a short time, on examining the tubes, I found the insoluble sulphur, which I have

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* This has been observed by Person, who states that if sulphur be heated above 150° its melting-point is lowered to about 112° or 110°. He says, that when heated with care, the thermometer will remain constant during crystallization, at 115°. I have not found this correct.—Ann. de Chemie, vol. xxi. p. 323.

stated to have such a high melting-point, distinctly melted. The octohedral sulphur had become opake and rounded at the edges, the other was unaltered in appearance. Further inquiry convinced me that the cause of the melting of the insoluble sulphur was, that it had passed into another modification, and that this conversion was attended with evolution of heat sufficient to melt the sulphur. The insoluble sulphur thus converted remains transparent, and is perfectly soluble in bisulphide of carbon.

It is stated in chemical treatises that the opacity which on solidification comes over the melted sulphur, is due to the transformation of the oblique prismatic into the octohedral sulphur, and the consequent disruption of the crystal. To this cause also is attributed the evolution of heat which has been observed in solid sulphur immediately after cooling. There are, however, no sufficient grounds for this view, and some of the observations which I have given are decidedly adverse to it. 1. The change readily takes place, even at temperatures at which sulphur becomes opake, in the opposite direction, namely, from the octohedron to the oblique prism. 2. The melting-point of the opake sulphur coincides too nearly with its point of solidification for it to be supposed that this change in it has taken place. On extracting melted sulphur which had become opake with bisulphide of carbon, I have constantly found present traces of insoluble matter, even where the greatest precaution had been taken to avoid elevation of temperature; and this opacity appears to me to be due to the hardening of the viscid sulphur, and the consequent deposition of opake matter in the pores of the crystals, which is quite sufficient to account for it. It remains to ascertain the cause of the evolution of the heat. On this point also I will offer a suggestion. It is well known that the appearance of opacity is delayed by pouring the sulphur into cold water, and that the sulphur thus formed is at first viscid and transparent, and only after a time becomes solid and opake. The received view, I believe, is that the hard sulphur thus formed is the solid form of the viscid sulphur, in the same sense as ice is the solid form of water. It appears to me more probable that these two sulphurs stand in a different relation, and that the change which takes place on solidification is an allotropic transformation of the viscid sulphur into the insoluble sulphur and one of the other modifications. In the case of sulphur gradually cooled this change takes place with rapidity, and, like other similar transformations, is attended with a sensible evolution of heat. Where the sulphur is *tempered* the change takes place very slowly, and the heat evolved is not perceived. This view is confirmed by a fact which I have discovered, namely, that the viscid sulphur possesses another solid form. I have found that when sulphur, melted at a high temperature, is suddenly exposed to intense cold—the cold of solid carbonic acid and æther—the sulphur formed is not viscid, but solid, hard, and perfectly transparent. When the temperature is allowed to rise to that of the air, the sulphur becomes soft and elastic. It is probable that this is the true solid form of the viscid sulphur.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. vi. p. 73.]

Nov. 14, 1853.—A paper was read by Mr. Dobson on the Theory of Cyclones. See Philosophical Magazine, vol. vi. p. 438.

Also, on the Storm-tracks of the South Pacific Ocean. See Philosophical Magazine, vol. vii. p. 268.

A communication was made by Mr. C. C. Babington on the use that has been made of the mode of growth to distinguish nearly allied Species.

Nov. 28.—A paper was read by Mr. Wedgwood on the Geometry of the first three books of Euclid, synthetically demonstrated from premises consisting exclusively of definitions.

In a treatise* published by the author a few years ago, definitions founded on relations of direction were indicated as exhibiting the ultimate analysis of the conceptions of straightness and parallelism in lines, and of planeness in surface; and in proof of the adequacy of these definitions as the basis of a complete system of geometry without the aid of axioms or any other assumption whatever, they were employed in demonstrating the principal propositions necessary to place the student on the ground occupied by the definitions and axioms of the ordinary system. If the basis thus built in underneath the old foundations of the science had been complete in every nook and corner, nothing more would have been required in order to rest the entire demonstration on the single principle of definitions. So long, however, as any step in the process, however subordinate, was left to be supplied by others, there always would be room for suspicion that the assumption in reasoning which was speciously plastered over in one place might be secretly undermining the system in another. The reform, moreover, of the premises in geometry is a problem on which such an infinity of thought has been spent, and to which so many answers, more or less plausible, have been offered, that nothing short of a complete exposition of a consistent scheme of demonstration can be expected to carry conviction in the validity of a fresh solution. The object of the present paper is accordingly to complete the task undertaken in the foregoing publication by a formal statement of the other definitions required in connexion with those of straight and parallel lines and plane surface, and by a rigid demonstration from these premises of the steps intervening between those and the premises of the ordinary system; and in additional proof of the fundamental character of the proposed analysis, the demonstration is carried on through the geometry of the three first books of Euclid by direct reasoning, without resort to the comparatively unsatisfactory method of *ex absurdo* proof, which, although equally conclusive as to the necessity of the result, yet always leaves a hankering in the mind for an answer why the case must be as the demonstration shows that it cannot avoid being.

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* The Principles of Geometrical Demonstration deduced from the original conception of Space and Form. Taylor and Walton. 1844.

In the execution of the foregoing plan, the whole of the problems of Euclid are omitted as irrelevant to the demonstration of the other propositions. The grounds on which they were adopted in the system of Euclid appear to be these. It frequently happens that it is necessary in the course of demonstration to make some new construction not included in the figure which forms the original subject of the proposition, and it was evidently thought that the geometer would not in strictness be entitled to take such a step until he had demonstrated the means of executing it with exactitude. The student was accordingly in the postulates put in possession of a ruler and a pair of compasses; and wherever any additional construction was required in the proof of a proposition, a problem was premised, showing the means by which the construction might be made by the aid of those implements.

But it should be recollected that the figure by which the demonstration is commonly accompanied is not the actual subject of the reasoning, but a mere illustration to aid the imagination and the memory, the exactitude of which is matter of comparative indifference. Moreover, the principle on which the problems are introduced is not consistently carried out to its legitimate conclusion even in Euclid. There is no difference in the reasoning between the figure which forms the original subject of the proposition, and the additional construction which is made in the course of demonstration; and therefore if it were necessary for the validity of the conclusion to demonstrate the means of executing the latter figure, it would be equally necessary in the case of the former. The student would not be entitled to move a step in the demonstration of the equality of two triangles having two sides and the included angle equal, until he had been taught how to construct two such triangles, and consequently how to describe an angle equal to a given angle. The demonstration in Euclid begins with perfect legitimacy. "Let ABC, DEF be two triangles in such and such conditions," without the necessity of indicating the means by which those conditions may be mechanically executed, or indeed of their possibility of actual existence; and it may with equal legitimacy proceed to exemplify in like manner any further construction which may be found necessary in the course of demonstration.

The question of motion has commonly been considered so essentially distinct from that of position, that all reference to the former subject has rigorously been excluded from the field of geometrical inquiry. But the position of every point must ultimately be determined by motion from points antecedently known, and to the incidents of motion we should accordingly look for the original source of the relations of position. Now motion (in as far as it influences position) admits of variation in two ways; viz. in the direction of the motion at each indivisible instant of time, and in the length of the track accomplished in a finite period; whence it has been said by Sir John Herschel that space (which is primarily known as the receptacle of motion) is reducible in ultimate analysis to distance and direction.

The relations of extent are simply those of equal, greater, and less, with respect to which it will be necessary only to define the test by which they are respectively to be demonstrated in concrete figure. The relations of direction are of a much more complicated nature. The different phases of this elementary attribute of motion are distinguished, not, like those of colour, by a permanent character independently cognizable in each individual, but more like musical notes, by their relative position on a peculiar scale which may be made to rest on any individual as an arbitrary basis.

The scale by which directions are compared is founded on the elementary relations of opposition and transverseness. In whatever direction we suppose ourselves to be traversing space, we recognize the possibility of returning to the same position from whence we set out by motion in a different direction, the relation of which to the original is that of opposition; or the two may be classed together as the positive and negative modifications of a common direction.

Again, if we fix our thoughts upon any given direction, we find a series of others in each of which it is possible to traverse space without advance in the original direction or in the one opposed to it. The directions so marked out by negation of progress in a certain direction are said to be *transverse* to the *normal* or direction in which no progress is made by the observer while advancing in the direction of any of the transverse series. If now we start afresh from any of the individuals of the latter series, it will be found that the series includes the opposite direction, as well as one direction and its opposite transverse to the former two. Every other individual of the series will be recognized as partaking in different proportions of the nature of these coordinates, or transverse directions, adopted as the basis of the scale. In other words, it will be found that distance in any intermediate direction is essentially composed of distance in the direction of each of the coordinates in different proportions, varying from all of the one and none of the other, to all of the latter and none of the former, with every modification arising from taking each of the coordinates in both a positive and a negative sense.

In like manner, as each intermediate direction is transverse to the original normal, a secondary series of directions with a different normal will arise from the combination of these coordinates in every proportion, and the whole expanse of space around the observer will be recognized as consisting of distance in every possible combination of proportions in the direction of three coordinates, of which the first may be taken at pleasure in space, the second may be identified with any of the series transverse to the first coordinate, and the third will be the single direction transverse to each of the former two. Within the sphere of three directions so related to each other we are entirely shut in. Whatever may be the particular direction in which the coordinates be laid, we can conceive no fourth direction essentially differing in nature from the former three, and therefore can conceive no possible direction which cannot be derived from some combination of three coordinates, or in which a given distance cannot be resolved into equivalent distances in the direction of the three coordinates.

We have thus in the relations of transverseness and opposition, and in the conception of intermediate directions arising from the combination of transverse coordinates in different proportions, a uniform scale by which, when applied to known directions in space, the position of any other direction may be accurately defined independent (it must be observed) of any reference to the notion of angular magnitude, of which as yet no mention has been made.

When two directions only are known in a system, they must be considered as members of the series transverse to a common normal; and one of the two being identified with the first coordinate of the scale, the position of the second will be completely determined by the proportion in which it partakes of the nature of the second coordinate or transverse direction of the series.

The directions commonly adopted as the basis of the scale, are the up and down, fore and aft, and right and left lines marked out (in any given position of the observer in a system) by the constitution of his bodily frame; and thus (in any given position of our bodies) a particular direction is defined in our thoughts by the proportion in which it partakes of the nature of those coordinates, that is to say, by the proportion in which distance in the direction in question is essentially composed of distance up or down, distance to the front or rear, and of distance to the right or left.

For the sake of simplifying the question, we will now confine our thoughts to motion in a plane surface, or to directions having reference to two transverse coordinates. Now although, in the actual apprehension of a figured system, the observer must be supposed to traverse the entire outline, and thus continually to change his place, yet he must be capable of doing so without rotation on his own axis, as he would otherwise acquire no notion of the configuration of his track in the external system. He will accordingly carry with him throughout the fundamental conceptions of front and back, right and left, and by reference to these coordinates will be able to compare and to identify directions in any part of the system.

It is in virtue of this complex scheme of relation between directions, that we are enabled to conceive the possibility of reaching the same point by different tracks from a common starting-point. We are indeed so much in the habit of thinking of points as marked out by physical phænomena (as by the letters in a geometrical illustration), that it is by no means obvious where the difficulty of the conception lies. But it must be remembered that points in geometry are distinguished solely by position, while the position of a given point is determined by the nature of the track by which it is reached from a point antecedently known. It is plain, therefore, that there would be no means of identifying points attained by tracks differing in any respect from each other, if the precise combination of distance and direction by which they were respectively attained were the ultimate test of their position. But now the knowledge of the fundamental scheme of relationship above explained makes us regard the space traversed in each successive instant of time in the track by which the position of a point is determined (and consequently the whole space traversed in the entire track), as equivalent to a certain distance in the direction of each of the two coordinates of the scale. The aggregate character (in respect of distance and direction) of the space traversed in different tracks (by which the position of the terminal points is governed) will thus be made to depend on the aggregate distance advanced in the direction of the two coordinates, a question to be tried by simple superposition. When the distance advanced in the direction of each coordinate is the same, the positions finally attained will be recognized as identical, and the points will coincide whatever may be the amount of intermediate divergence in the tracks by which they have actually been reached.

From the same principle it may be shown, that a straight line may be drawn from a given point to any other point in space. Because the space traversed in the track by which the second point must be supposed to have been determined, will be equivalent in distance and direction to a certain distance in each of the two standard directions of the system. Now inasmuch as the series of directions intermediate between any pair of transverse directions includes individuals partaking in every conceivable proportion of the nature of both the transverse directions between which they lie, it will always be possible to select one of the series a certain distance in which will be equivalent to given distances in each of the two transverse directions, and therefore the distances in the direction of the coordinates of the system under consideration, into which the space traversed in the original track has been resolved, may again be exchanged for an equivalent distance in a single direction duly related to each of the coordinates; in other words, the same position may be attained by motion in a single continuous direction as by a track of any other description, or what amounts to the same thing, a straight line may be drawn from a given point to a point determined by a track of any other description.

As soon as a straight line is known as lying in a single continuous direction, it becomes the most obvious means of marking the direction so exhibited throughout a finite extent of line. The series of directions transverse to a given normal may then be represented by two straight lines crossing each other at right angles, and an indefinite number of other straight lines diverging from the point of intersection, and dividing the plane surface round that point into as many parts as there are diverging lines. If now we take two of these lines, like the hands of a clock, and suppose one to remain fixed while the other revolves from left to right, it will pass successively through all the directions intermediate between left and front, while the quantity of plane surface intercepted between the hands abutting on the point of intersection will continually increase as the difference in their direction becomes greater, or in proportion as distance in the direction of the moveable hand contains a greater proportion of distance in the direction transverse to that of the fixed one. Thus we are taught a new mode of estimating the relation between the direction of straight lines diverging from a common point; not by a proportion which addresses itself to the understanding merely, but by a quantity admitting of measurement by bodily comparison, viz. by the quantity of plane surface intercepted between the diverging lines and abutting on the point of intersection, or by the magnitude of the included angle.

Professor Challis gave an account of a luminous appearance observed at the time of the perihelion passage of Klinkerfue's comet.

Professor Stokes read a paper on the Optical properties of Light reflected from Crystals of Permanganate of Potash. The substance of this paper is embodied in a paper on the Metallic Reflexion exhibited by certain Non-metallic Bodies, published in the Philosophical Magazine, vol. vi. p. 393.

Dec. 12.—Professor Fisher read the first part of a paper, entitled "Researches, Physiological and Pathological, on the Development of the Vertebral System."

After having explained what he meant by the term *vertebral system*, he stated (and he illustrated what he described by drawings) that the spinal marrow, at a particular stage of growth of the human embryon, exhibits indications of segmental development corresponding to that of the spinal column; that is to say, that each of its halves offers on its external surface a series of symmetrical spaces defined by transverse lines, each of which spaces corresponds to the roots of a single spinal nerve; and again, that each half presents in its internal structure, a double series, one anterior, the other posterior, of symmetrieal areas, two of which appeared to equal in extent one of the external spaces just spoken of. Professor Fisher also stated that the spinal marrow offers, at the period of development in question, several other peculiarities, some of them bearing likewise a segmental character; but he reserved a detailed description of them for a future communication.

Feb. 27, 1854.—A paper was read by Professor Challis, entitled "A direct Method of obtaining by Analysis the mean motions of the apse and node of the Moon's Orbit." See Philosophical Magazine, vol. vii. p. 278.

Also a paper by Mr. J. B. Phear on some parts of the Geology of Suffolk, particularly with reference to the Valley of the Gipping.

The deposits which constitute what is often termed the glacial formation, but which the present state of our knowledge hardly allows us to designate by a name significant of a common origin, present so much confusion to the inquirer, and impose upon him so much laborious research by the extent and the unconnected character of their distribution, that they have hitherto met with less attention than their importance deserves.

The county of Suffolk seems to be a district where a portion of these deposits is manifested with more than usual distinctness, and is capable of being studied with comparative facility. The county is separated from Norfolk on the north by the well-marked valleys of the Ouse and the Waveney, is bounded on the east and south by the sea and valley of the Stour, and is bordered by chalk uplands on the north-west; the whole central portion is thickly covered with a mass of blue drift-clay, cut into abrupt undulations by a network of valleys. This clay is totally without any symptom of stratification, and is full of fragments of all rocks of the secondary period, including specimens of granite and other igneous rocks.

Wells sunk in different parts of the county show this drift-clay to have a thickness varying from 200 feet to a few inches; it seems to thin off from the northern and western parts of the county towards the coast, and only exists in the shape of outliers beyond a line passing through Sudbury, Hadleigh, Bramford, Woodbridge, and Saxmundham; a line, it may be remarked, nearly coinciding with the edge both of the London clay and of the crag, and approximately passing through the heads of the tidal estuaries of the Orwell, Deben, Ore and Alde. The clay is almost universally underlaid by an unfossiliferous sand; and there is reason to conjecture that this sand, of a prevailing red colour, passes out beyond the just-mentioned line, and covers in many places the surface of the strip of land between it and the sea.

A detailed examination of the Gipping valley reveals a well-marked and connected line of sand cliffs fringing it, and its Coddenham tributary in particular, at a high level on both sides; the sand is generally pure white, though often red, horizontally stratified and capped with an unrolled gravel, which evidently owed its existence to the quiet washing away of the drift-clay from its insoluble contents. Above Needham Market the valley is channelled in drift-clay, but between Needham and Bramford it is cut through chalk; and it should be remarked, that the line of sand-hills does not extend up the valley with any great distinctness beyond the chalk. The phænomena seen at Creeting are not consistent with this sand lying beneath the drift-clay; and the inference is, that it constitutes the remains of an estuary deposit formed in the valley subsequent to its excavation in the drift clay.

All the other streams west of the Gipping have chalk for their floor during the middle part of their course, thus manifesting the existence of a ridge of chalk running beneath the drift accumulations nearly due west and east from Sudbury to Bramford. Disturbances evidenced in this ridge, and perhaps due to its elevation, are partaken of by the London clay and crag deposits which overlie it on the east and south.

In Norfolk the drift-clay attains a greater thickness than in Suffolk, and towards the north of the county is overlaid by a sand and gravel formation which may be appropriately termed upper drift. The gradual disappearance of this towards the south, together with the thinning away and final extinction of the drift-clay in the same direction, point to a region of greater denuding activity; it may be an interesting question whether such denudation be in any degree connected with the upheaval of the before-mentioned chalk ridge, or again, whether the sands of the Gipping valley bear any relation to the upper drift of Norfolk.

March 13.—A paper was read by Prof. Challis on the Eccentricity of the Moon's Orbit; supplement to a former communication on the mean motions of the Apse and Node. See the former paper, Phil. Mag. vol. vii. p. 278.

Also a paper by Mr. J. Clerk Maxwell on the Transformation of Surfaces by Bending.

The kind of transformation here considered is that in which a surface changes its form without extension or contraction of any of its parts. Such a process may be called bending or development. The most obvious case is that in which the surface is originally a plane, and becomes, by bending, one of the class called "developable surfaces." Surfaces generated by straight lines, which do not ultimately intersect, may also be bent about these straight lines as axes. In this way they may be transformed into surfaces whose generating lines are parallel to a given plane, just as the former class are transformed into planes.

In both these cases, the bending round one straight line of the system is quite independent of that round any other; but in those which follow, the bending at one point influences that at every other point. The case of a surface of revolution bent symmetrically with respect to the axis is taken as an example.

The remainder of the paper contains an elementary investigation of the conditions of bending of a surface of any form.

The surface is considered as the limit of the inscribed polyhedron when the number of the sides is increased and their size diminished indefinitely.

A method is then given by which a polyhedron with triangular facets may be inscribed in any surface; and it is shown, that when a certain condition is fulfilled, the triangles unite in pairs so as to form a polyhedron with quadrilateral facets. The edges of this polyhedron form two intersecting systems of polygons, which become in the limit curves of double curvature; and when the condition referred to is satisfied, the two systems of curves are said to be "conjugate" to one another.

The solid angle formed by four facets which meet in a point is then considered, and in this way a "measure of curvature" of the surface at that point is obtained.

It is then shown that if there be two surfaces, one of which has been developed from the other, one, and only one, pair of systems of corresponding lines can be drawn on the two surfaces so as to be conjugate to each other on both surfaces. This pair of systems completely determines the nature of the transformation, and is called a double system of "lines of bending." By means of these lines the most general cases are reduced to that of the quadrilateral polyhedron. The condition to be fulfilled at every point of the surface during bending is deduced from the consideration of one solid angle of the polyhedron. It is found that the product of the principal radii of curvature is constant.

By considering the angles of the four edges which meet in a point, we obtain certain conditions, which must be satisfied by the lines of bending in order that any bending may be possible. If one of these conditions be satisfied, an infinitesimal amount of bending may take place, after which the system of lines must be altered that the bending may continue. Such lines of bending are in continual motion over the surface during bending, and may be called "instantaneous lines of bending." When a second condition is satisfied, a finite amount of bending may take place about the same system of lines. Such a system may be called a "permanent system of lines of bending."

Every conception required by the problem is thus rendered perfectly definite and intelligible, and the difficulties of further investigation are entirely analytical. No attempt has been made to overcome these, as the elementary considerations previously employed would soon become too complicated to be of any use.

For the analytical treatment of the subject the reader is referred to the following memoirs :—

1. "Disquisitiones generales circa superficies curvas," by M. C. F. Gauss (1827).—*Comm. Recentiores Gott.* vol. vi.; and in Monge's "Application de l'Analyse à la Géométrie," edit. 1850.
2. "Sur un Théorème de M. Gauss, &c.," par J. Liouville.—*Liouville's Journal*, 1847.
3. "Démonstration d'un Théorème de M. Gauss," par M. J. Bertrand.—*Liouville's Journal*, 1848.
4. "Démonstration d'un Théorème," Note de M. Diguet.—*Liouville's Journal*, 1848.
5. "Sur le même Théorème," par M. Puiseux.—*Liouville's Journal*, 1848.

And two notes appended by M. Liouville to his edition of Monge.

March 28.—Prof. Miller gave an account of the relation between the physical characters and form of crystals of the oblique system as established by the observations of Mitscherlich, Neumann, De Senarmont, Wiedemann and Ångström.

A paper was read by Prof. De Morgan on some Points in the theory of differential equations.

1. The words *primordinal*, *biordinal*, &c. are used in abbreviation of the phrases 'of the first order,' 'of the second order,' &c.

The symbol for a differential coefficient, U_x for $\frac{dU}{dx}$, &c., long used by the author, is thus extended. By $U_{x|p,q}$ is meant $dU : dx$ with reference to x as contained in p and q, as well as explicitly. Thus $U_{x|p,q}$ means $U_x + U_p p_x + U_q q_x$; and $U_{x|y}$ means $U_x + U_y y'$.

Differentiations are sometimes expressed thus: $d_x U = U_x\, dx$, $d_{x,y} U = U_x dx + U_y dy$.

When it is only requisite to express functional relation, without specification of form, $(x, y, z) = 0$ or $z = (x, y)$ may signify an equation between x, y, and z. A letter may be used as its own functional symbol: thus $u = u(x, y, z)$ may signify that u is a function of x, y, z. And in 'for u write $u(x, y, z)$' there is a convenient abbreviation of 'for u substitute its value in terms of x, y, z.'

2. When, as so often happens, a variable enters under relations

which destroy the effect of its variation upon the *form* of differential coefficients, it is called *self-compensating*. Thus $\phi(x, y, a)=0$, $\phi_a(x, y, a)=0$, contain the self-compensating variable a. Similarly, when $\phi(x, y, a, b)=0$ is accompanied by $\phi_a da+\phi_b db=0$, a and b are mutually compensative, and primordinally. The addition of

$$\phi_{a(x|y)}\, da+\phi_{b(x|y)}\, db=0$$

makes a and b biordinally compensative.

3. When a finite change in x makes an infinite change in y, it makes an infinite change in $y' : y$, in $y'' : y'$, &c. When *either* or both P and Q become infinite, P : Q and $\mathrm{P}_x : \mathrm{Q}_x$ are both nothing, both finite and equal, or both infinite; provided that the infinite form is produced by *substitution for x*. If $u=(v, w, \ldots)$, any relation which makes u_v infinite either makes u_w infinite, or is independent of w. And if $u_v=\infty$ be produced by a relation containing v, then $u_v dv+u_w dw+\ldots=0$ and $u_{vw} dv+u_{vw} dv+\ldots=0$ are relations of identical meaning.

4. From the last it follows that U=const. is solved by making any factor of $d\mathrm{U}$ either 0 or ∞. In $d\mathrm{U}=\mathrm{M}(\mathrm{P}dx+\mathrm{Q}dy)$, singular solutions are obtained, as is known, from $\mathrm{M}=\infty$: it ought to be asked whether M=0 does not give singular *exceptions*, that is, cases in which U=const. arises otherwise than from $\mathrm{P}+\mathrm{Q}y'=0$. It is found more convenient to treat these cases without actual separation of the factor; that is, from $d\mathrm{U}=\mathrm{U}_x dx+\mathrm{U}_y dy$.

5. In a former paper, the author insisted on the arbitrary functions which enter the intermediate primitives: maintaining, for example, that the primordinal of $y''=0$ is $\phi(y', xy'-y)=0$, for any form of ϕ. Lagrange, he has since found, notices this extension, and rejects it, because it leads to $y'=a$, $xy'-y=b$, as necessary consequences of its ordinary solution. Mr. De Morgan maintains his opinion, and observes that Lagrange's reason would make it imperative to reject one of the two, $y'=a$, $xy'-y=b$, since either is the necessary consequence of the other.

6. In order to avoid the ambiguous use of the word *singular*, a singular solution is defined as any one which, by the mode of obtaining it, cannot have the ordinal number of constants: it is further styled *intraneous* or *extraneous*, according as it is or is not a case of the general solution. If $y=\psi(x, a)$ or $a=\mathrm{A}(x, y)$ give $y'=\chi(x, y)$, then $d\mathrm{A}=\mathrm{A}_y(y'-\chi)dx$ and $\chi=-\mathrm{A}_x : \mathrm{A}_y$ are identical equations. Every relation which satisfies $\mathrm{A}_y=\infty$ *is* a solution, and a singular solution; except possibly, relations of the form $x=$ const., which must always be examined apart. Also, $\mathrm{A}_y=\infty$ is identical with $\psi_a=0$. There can exist no solutions whatsoever except those which are contained in A=const., $\mathrm{A}_y=\infty$, and (possibly) $x=$const.

Again, $\chi_y=(\log\psi_a)_x$. Of this equation the author has found neither notice nor use: supposing it to have ever been given, he holds it most remarkable that it has not become common as the mode of connecting the two well-known and widely used tests of singular solution. It easily shows that $\chi_y=\infty$ contains all extraneous solutions, and all intraneous solutions which (as often happens) can be also obtained by making a a function of x. It also easily gives a conclusion arrived at by the author in his last paper, namely, that when $\chi_y=\infty$ is satisfied and not $y'=\chi$, it follows that $\chi_x+\chi_y\chi$ is infinite.

7. The author gives his own version of the demonstration of a theorem of M. Cauchy, for distinguishing extraneous and intraneous solutions. If $y=\mathrm{P}$, P being a given function of x, satisfy $y'=\chi(x, y)$, that is, if P' and $\chi(x, \mathrm{P})$ be identical, then $y=\mathrm{P}$ is an extraneous or intraneous solution of $y'=\chi(x, y)$, according as

$$\int_{\mathrm{P}}^{\mathrm{P}+\beta} \frac{dy}{\chi(x, y)-\chi(x, \mathrm{P})}$$

(x being constant) is finite or infinite for small values of β. This theorem has attracted little notice in this country: the author believes it to be fully demonstrated, and considers it one of the most remarkable accessions of this century to the theory of differential equations.

8. It is observed that the validity of the extraneous solution may depend upon the interpretation of the sign of equality by which $\mathrm{A}=\mathrm{B}$ is held satisfied when both sides are 0, or both infinite, even though $\mathrm{A}:\mathrm{B}=1$ is not satisfied. Thus $y'=2\sqrt{y}$ or $y=(x+a)^2$, has the extraneous solution $y=0$, which, however, is not a solution if by $y'=2\sqrt{y}$ we understand in *all* cases $y' : \sqrt{y}=2$.

9. The common mode of obtaining the singular solution of a biordinal (by combining $\phi(x, y, a, b)=0$, $d_{a,b}\phi=0$, $d_{a,b}\phi_{x|y}=0$) though sufficiently general, is never *shown* to be so.

Let $y=\psi(x, a, b)$, combined with $y'=\psi_x$, give $a=\mathrm{A}(x, y, y')$, $b=\mathrm{B}(x, y, y')$, from either of which follows $y''=\chi(x, y, y')$. The most general primordinal is $f(\mathrm{A}, \mathrm{B})=0$, f being arbitrary. Any given curve, $y=\varpi x$, may be made to solve this for some form of f; but, generally speaking, this solution will be *extraneous*. For A and B are so related that every intraneous solution makes A and B constant. And any primordinal equation whatever may in an infinite number of ways be thrown into the form $f(\mathrm{A}, \mathrm{B})=0$, so that the intraneous solutions shall make A and B constant.

(Given $y=\varpi x$, required a key to all the primordinals of which it is a singular solution. Take *any* equation $y=\psi(x, a, b)$, eliminate x between $a=\mathrm{A}(x, \varpi x, \varpi' x)$ and $b=\mathrm{B}(x, \varpi x, \varpi' x)$, and write $\mathrm{A}(x, y, y')$ and $\mathrm{B}(x, y, y')$ for a and b in the result.)

The equations $d\mathrm{A}=\mathrm{A}_{y'}(y''-\chi)dx$, $d\mathrm{B}=\mathrm{B}_{y'}(y''-\chi)dx$ are identically true. And $\mathrm{A}_{y'}=\infty$, or any relation which satisfies it, is a singular primordinal of $y''=\chi$, whenever it is a primordinal at all; that is, when y' appears in it. When $\mathrm{A}_{y'}=\infty$ is satisfied by a relation void of y', that relation is not necessarily a solution. The ordinary solutions of $\mathrm{A}_{y'}=\infty$ are solutions of $y''=\chi$; but not (necessarily) the singular solutions. The singular solutions of a relation which makes $\mathrm{A}_{y'}=\infty$ may make $\mathrm{A}_{y'}$ finite.

Comparing A and B with ψ, we have

$$\mathrm{A}_{y'}=-\frac{\psi_b}{\psi_a\psi_{bx}-\psi_b\psi_{ax}}, \quad \mathrm{B}_{y'}=\frac{\psi_a}{\psi_a\psi_{bx}-\psi_b\psi_{ax}},$$

$$\chi_{y'}=\{\log(\psi_a\psi_{bx}-\psi_b\psi_{ax})\}_x.$$

From these are obtained results in complete analogy with those for primordinal equations. But when $\psi_a\psi_{bx}-\psi_b\psi_{ax}=0$, the usual criterion of singular solution, is made valid by $\psi_a=0$, $\psi_b=0$, a singular primitive of the singular primordinal may be obtained, which does not necessarily satisfy $y''=\chi$.

10. Similar forms are given for *triordinal* equations. In noticing the manner in which the equations of the general theory may be easily expressed by what are called *determinants*, Mr. De Morgan expresses his admiration of the system, and his sense of the important services rendered by those who have laid its foundations. But he refuses to employ the word *determinant* in the sense proposed, on account of its not expressing any *distinctive* property of these functions. Until those who have a better right to give a name provide themselves with a *distinctive* one, he intends to call them *eliminants*.

The forms connected with $y=\psi(x, a, b)$ may be easily translated into others derived from $\phi(x, y, a, b)=0$. But the formula which connects $\chi_{y'}$ with ϕ is as follows:—

$$\chi_{y'}=\left\{\log\left(\frac{\phi_a\phi_{bx|y}-\phi_b\phi_{ax|y}}{\phi_y^2}\right)\right\}_{x|y}+\frac{\phi_a\phi_{by}-\phi_b\phi_{ay}}{\phi_a\phi_{bx|y}-\phi_b\phi_{ax|y}}\cdot\chi,$$

where by $U_{x|y}$ is meant $U_x+U_y y'$, even when U is a function of y'. Thus $(xy'-y)_{x|y}$, as here used, is 0.

11. The following idea of reciprocal polarity has been presented by M. Druckenmüller (as cited from Crelle's Journal by Mr. Boole), and, independently, by Professor Boole; it occurred to the author of this paper before he had seen the researches of either. If there be equations involving $m+n$ variables, and if, determining a point by fixing m of the variables, a curve be determined by giving all possible values to the remaining n (*point* and *curve* being here merely names of objects determined), we may say that the (m)-point is the pole of the (n)-curve. Similarly, we may make each (n)-point the pole of an (m)-curve. And all the points of any curve have polar curves which contain the pole of that curve. If the two sets of variables be severally made primordinally compensative, the general properties which arise are easy extensions of the well-known theory of reciprocal polars. Let (x, y) and (a, b) be two points; the polar property of $x^2+y^2=ax+by$ contains the direct and converse property of the angle in a semicircle. If $\phi(x, y, a, b)$ be the *modular equation*, and if x, y and a, b be compensative, any element (x, y, y') of *any* (x, y)-curve to the pole (a, b) determines an element (a, b, b') of an (a, b)-curve to the pole (x, y). These curves are *reciprocal polars*. In the common system, the modular equation is linear with respect to both pairs of coordinates, and the locus of those poles which lie in their polar straight lines is a conic section, to which the polars are tangents.

12. The method of transforming differential equations, given by the author in his last paper, is precisely the reference of the curves sought to their reciprocal polars, the modular equation being taken at pleasure. Mr. De Morgan now proposes to call it the method of *polar transformation*. Let $\phi(x, y, a, b)=0$ be the modular equation,

and let $\phi_x+\phi_y y'=0$, $\phi_a+\phi_b b'=0$, b' being $db:da$. Hence

$$a=\mathrm{A}(x,y,y'),\ b=\mathrm{B}(x,y,y');\quad x=\mathrm{X}(a,b,b'),\ y=\mathrm{Y}(a,b,b')$$
$$b'=\mathrm{B}_{y'}\div\mathrm{A}_{y'}\qquad\qquad y'=\mathrm{Y}_{b'}\div\mathrm{X}_{b'};$$

the biordinal factors, $y''-\chi(x,y,y')$, $b''-\alpha(a,b,b')$, disappearing from b' and y'. Hence b' depends on x,y,y'. Similarly, b'' depends x,y,y',y'', &c., and similarly for y',y'', &c. If in $f(x,y,y',y'',\&c.)=0$ we substitute for x,y,y', &c. in terms of a,b,b', &c., the two equations belong to polar reciprocals. If either can be integrated, the integration of the other depends on elimination: thus if the equation in a,b, &c. can be integrated, the solution of the equation in x,y is obtained by eliminating a and b between the integral obtained and $x=\mathrm{X}$, $y=\mathrm{Y}$.

13. There are two reciprocal biordinal equations belonging to the modular equation $\phi(x,y,a,b)=0$; $y''=\chi$ when a and b are constant, $b''=\alpha$ when x and y are constant. The two have the same condition of singular solution; for $\mathrm{A}_{y'}\phi_b=\mathrm{X}_{b'}\phi_y$. Let this be $\sigma(x,y,a,b)=0$, when cleared of y' or b'. The following table exhibits the relations of the double system:—

$$\begin{tikzcd}[column sep=small]
 & & & \phi(x,y,a,b)=0 \arrow[dll, dash] \arrow[drr, dash] & & & \\
\sigma(x,y,a,b)=0 \arrow[d, dash] \arrow[dr, dash] \arrow[drr, dash] & \phi_x+\phi_y y'=0 \arrow[dl, dash] \arrow[d, dash] \arrow[dr, dash] & & & & \phi_a+\phi_b b'=0 \arrow[dl, dash] \arrow[d, dash] \arrow[dr, dash] & \sigma(x,y,a,b)=0 \arrow[dll, dash] \arrow[dl, dash] \arrow[d, dash] \\
y'=\varpi(x,y) \arrow[d, dash] & a=\mathrm{A}(x,y,y') \arrow[d, dash] & b=\mathrm{B}(x,y,y') \arrow[dl, dash] & & x=\mathrm{X}(a,b,b') \arrow[d, dash] & y=\mathrm{Y}(a,b,b') \arrow[dl, dash] & b'=\lambda(a,b) \arrow[d, dash] \\
y=\Pi(x,\mathrm{C}) & y''=\chi(x,y,y') & & & b''=\alpha(a,b,b') & & b=\Lambda(a,\mathrm{Z}).
\end{tikzcd}$$

Eliminate a and b between $\phi=0$, $\sigma=0$, $\phi_{x|y}=0$, and we have $y'=\varpi$, $y=\Pi$, the singular primordinal and primitive of $y''=\chi$; those of $b''=\alpha$ are obtained by eliminating x and y from $\phi=0$, $\sigma=0$, $\phi_{x|y}=0$. There is a relation involved between C and Z, the constants of integration. For each value of C, $y=\Pi$ is the xy-curve which touches all in $\phi(x,y,a,\Lambda)=0$, for the corresponding value of Z and all values of a. The same of Z, $b=\Lambda$, and $\phi(x,\Pi,a,b)=0$. The contacts are of the second order, and $y=\Pi$, $b=\Lambda$, are polar reciprocals for corresponding values of C and Z. But the singular primitives of $y'=\varpi$ and $b'=\lambda$ are not necessarily reciprocals: when this does happen, their contacts with primitives are of the third order.

14. When a surface is described by one set of curves, as in the surface obtained by eliminating a from $\phi(x,y,z,a)=0$, $\psi(x,y,z,a)=0$, it is proposed to call it a *shaded* surface, and the curves *lines of shading*. The equation $f(x,y,z,y',z')=0$, y and z being functions of x, cannot, generally, belong to any family of surfaces in an unrestricted sense; that is, it cannot be always true of a point moving in any way upon a surface. Such a supposition would be equivalent to imagining a surface *every* point of which has the primordinal character of the vertex of a cone. But it may belong to any surface, properly shaded, or to any mode of shading, if the proper surface be chosen.

15. Two equations of the form $y=\Phi(x,a,b,c)$ $z=\Psi(x,a,b,c)$, give one, and only one, primordinal of the form $f(x,y,z,y',z')=0$.

Assume any surface $\omega(x, y, z)=0$; by this, and compensative relations between a, b, c, another pair of primitives may be found. But the primitives obtained from $\omega=0$ do not *shade* this surface, except in cases determined by two relations between the constants. Again, making a, b, c compensative, without any assumed surface, we find one equation of the form $(a, b, c, a', b')=0$, any primitives of which lead to other primitive forms for $f=0$. Each of the second primitives has contact of the first order with one family of curves from among the original primitives; and all ordinary primitives are found, in an infinite number of ways, among the connecting curves of others. There is a singular solution, a curve of contact to all primitives, when $\Phi_a=0$, $\Psi_a=0$, &c. can all be satisfied at once.

Since $y=\Phi$, $z=\Psi$, give a primordinal equation independent of constants, the polar reciprocal properties of curves in space are of a restricted form. Every surface dictates another surface, and a mode of shading *both*, so that each line of shading on either surface is the polar reciprocal of a line on the other.

16. The conversion of constants into compensative variables may give restricted solutions, as in the ordinary case of two variables, and every other in which the constants are converted into separately self-compensating variables. When these variables are made collectively compensating, and the equations permit elimination of the original variables, ordinary differential equations may be produced, the integration of which may, after substitution, give primitives of the same form as those from which they came. But when the original variables cannot be eliminated, arbitrary relations may be required, in number enough to eliminate the differentials of the new variables: in this case arbitrary functions enter the primitives finally deduced. Of this last case one instance is Lagrange's transition from a primitive of a primordinal partial equation having two constants to the complete primitive of that equation.

17. A biordinal partial equation may be produced from

$$U(x, y, z, a, b, c, e, h)=0$$

by eliminating the five constants between $U=0$ and the five results of primordinal and biordinal differentiation. But it is not true that every form of $U=0$ leads to one biordinal equation only; many forms lead to an infinite number. Two attempts to procure other primitives by making a, b, &c. compensative variables, end in two different forms of result. First, when all the resulting equations are required to be integrable, by introduction of a proper factor, the success of the process requires the integral of two partial equations, one primordinal and one biordinal, between four variables. Secondly, when no such condition is required, the result is another form involving five constants.

18. A primordinal partial equation belongs to a family of surfaces of which one is determined by any given curve through which it is to be drawn. A biordinal equation belongs to an infinite number of families; and a distinct conception of the conditions which select an individual surface is best formed by an extension of the following kind. A curve on a surface is analogous to a point on a curve: two curves being drawn on a surface, the analogue of the chord joining

two points on a curve is the developable surface (or surfaces) drawn through the two curves. The developable surface which touches the given surface in a curve (and not the tangent plane) is the analogue of the line which touches a curve in a point. A biordinal equation being given, one surface satisfying it is selected by a curve through which that surface is to pass, and a developable surface passing through that curve which the surface is to touch.

19, 20. The restrictions under which two arbitrary forms must enter, in order that a biordinal partial equation may exist independent of these functions, are wholly unknown. The case which is fully analogous to a biordinal of two variables, is of the most limited character. Ampère has noticed this: Mr. De Morgan was led to it by an examination of the polar properties of $\phi(x, y, z, a, b, c)=0$. This equation leads to $a=A$, $b=B$, $c=C$, where A, B, C are functions of x, y, z, p, q. The primordinal $f(A, B, C)=0$ is satisfied by $\phi=0$, subject to $\phi(a, b, c)=0$, and leads to a biordinal, independent of f, of the form

$$Q+Rr+Ss+Tt+U(s^2-rt)=0,$$

in which Q, R, &c. are not wholly independent of each other.

If the pole (a, b, c) move along a certain curve, the polar surface must touch a certain surface in one of the lines of a certain shading. That is, every *abc*-curve has a shaded surface, which is its polar reciprocal: and every line of shading of that surface has another surface for its polar reciprocal, shaded by lines of which the original *abc*-curve is one. And every surface has a reciprocal surface such that for each point on one there is a point on the other; and the point on one surface being taken, the polar surface of that point touches the other surface in the other point.

The singular solutions of the two biordinals derived from

$$\phi(x, y, z, a, b, c)=0$$

by means of x, y, z and of a, b, c, are connected by relations analogous to those already seen in the case of two variables. In fact, there is perfect coincidence and coextension between the properties of the general equation $y''=\chi(x, y, y')$ and a particular species of the equation $Q+Rr+Ss+Tt+U(s^2-rt)=0$. It is proposed to call this species the *polar biordinal*.

21. The general method of transforming partial equations, given in the last paper, is the investigation of the class of surfaces contained under a given equation by reference of them to their polar reciprocals, any convenient modular equation $\phi(x, y, z, a, b, c)$ being made the means of transformation.

22. The following notation is proposed for eliminants. The components being A_p, A_q, &c., B_p, &c., the eliminants are (A_p), (AB_{pq}), (ABC_{pqr}), &c.; the components being A, A′, &c. B, &c., the eliminants are (A°), $(AB^{\circ\,\prime})$, $(ABC^{\circ\,\prime\,\prime\prime})$, &c. Thus

$$(A_p)=A_p$$

$$(AB_{pq})=A_p(B_q)-B_p(A_q)$$

$$(ABC_{pqr})=A_p(BC_{qr})+B_p(CA_{qr})+C_p(AB_{qr})$$

$$(ABCD_{pqrs})=A_p(BCD_{qrs})-B_p(CDA_{qrs})+C_p(DAB_{qrs})-D_p(ABC_{qrs}).$$

and so on. Some slight investigation of properties is made, to exhibit the notation.

The following rule is suggested to determine, in any complicated case, whether the number of contiguous interchanges by which one arrangement of letters is converted into another shall be odd or even. This is an important matter in the theory of eliminants, though very complicated instances may seldom occur in practice. Write down one arrangement under the other, and, beginning at one letter in one line, mark the companion letter in the other line, pass on to that companion in the first line, mark its companion, and so on, until we arrive at a letter already marked. Call this sequence a *chain*, each mark being one *link*. Having formed one chain, begin at a letter not yet used, and form another; and so on until every letter has been used. Then, according as the number of chains with *even* links is odd or even, the number of interchanges of contiguous letters required is odd or even. For example, the two arrangements being

A B C D E F G H I J K L M N O P Q
H M O G Q B K L J P F C I N A D E
1 2 1 2 3 2 2 1 2 2 2 1 2 4 1 2 3.

Under A is H, under H is L, under L is C, under C is O, under O is A, already taken: the first chain has five links, the second is found to have nine, the third two, the fourth one. The number having even links is one, an odd number; hence an odd number of contiguous interchanges converts the first arrangement into the second.

23. The following is the method of ascertaining whether the biordinal equation

$$Q+Rr+Ss+Tt+U(s^2-rt)=0 \quad . \quad . \quad . \quad . \quad (1)$$

possesses a primordinal of the form $f(x, y, z, p, q)=0$, containing an arbitrary function. Considering x, y, z, p, q as five independent variables, integrate, by common methods, the equations

$$U\left(\frac{dv}{dx}+p\frac{dv}{dz}\right)+T\frac{dv}{dp}-\frac{k}{1+k}S\frac{dv}{dq}=0$$

$$U\left(\frac{dv}{dy}+q\frac{dv}{dz}\right)+R\frac{dv}{dq}-\frac{1}{1+k}S\frac{dv}{dp}=0,$$

k being one of the roots of $kS^2=(1+k)^2(RT+QU)$. If a common solution $v=A$ can be found, then $A=\text{const.}$ is a primordinal of (1).

If two common solutions, A and B, can be found, then $B=\varpi A$ is a primordinal, ϖ being arbitrary. But though in this case $A=\text{const.}$ and $B=\text{const.}$ are solutions, they cannot *coexist*, unless the values of k be equal, or unless $S^2=4(RT+QU)$. This last equation is one condition of polarity; and if, when satisfied, we find *three* (and there cannot be more) common solutions, A, B, C, inexpressible in terms of each other, then $f(A, B, C)=0$ is the most general primordinal, any two forms of it may coexist, or even any three, which amount to $A=\text{const.}$, $B=\text{const.}$, $C=\text{const.}$ Elimination of p and q between these last equations gives $\phi(x, y, z, a, b, c)=0$, the modular equation. And the general solution of (1) is found by assuming b and c in terms of a, and then making a a self-compensating variable.

24. The paper is concluded by some remarks on notation.

In an appendix to the preceding paper, read to the Society on the 1st of May, 1854, Mr. De Morgan points out an error committed by M. Cauchy in a very remarkable theorem, of which his enunciation is as follows.

Let ϕx be a function which can be developed in integer powers of x. Let $r(\cos\theta + \sin\theta \,.\, \sqrt{-1})$, r being positive, be any one of the roots of $\phi x = \infty$ or of $\phi' x = \infty$. Then the development of ϕx is convergent from $x=0$ up to $x =$ the least value of r.

M. Cauchy stipulates that the function shall be continuous; but he defines a function to be continuous so long as it remains finite, and receives only infinitely small increments from infinitely small accessions to the variable. It is then obviously impossible that the above theorem should be universally true. Were it so, it would follow that the development of $(1+x)^{\frac{3}{2}}$ is convergent for all finite values of x, whereas it is well known that this development becomes divergent when x is greater than unity. The error of M. Cauchy's demonstration (which contains a valuable method for establishing a large class of definite integrals) is the assumption that if an infinite number of convergent series of the form $a + bx + cx^2 + \ldots$, all with one value of x but different values of $a, b, c, \ldots$, be added together, the sum divided by the number of series is also a convergent series. This assumption is not universally true.

Mr. De Morgan takes a totally different line of demonstration, and establishes the following theorems.

If $r(\cos\theta + \sin\theta \,.\, \sqrt{-1})$, r being positive, represent a root of any one of the equations $\phi x = \infty$, $\phi' x = \infty$, $\phi'' x = \infty, \ldots$ then the development of ϕx in powers of x is always convergent from $x=0$ up to $x =$ the least value of r, and divergent for all greater values of x.

If the development have all its coefficients positive, or if all beyond an assignable coefficient be positive, the least value of r is obtained from a real and positive root.

If the signs of the development be, or finally become, recurring cycles, with l in each cycle, the least value of r is obtained from a root in which $\cos\theta + \sin\theta \,.\, \sqrt{-1}$ is one of the lth roots of unity. If no such cycle be finally established, $\cos\theta + \sin\theta \,.\, \sqrt{-1}$ may have a value of θ which is incommensurable with the right angle.

M. Cauchy has established from his own theorem (the want of sufficient statement of conditions not affecting this particular case) the necessity of the observed fact, that the developments produced by Lagrange's theorem for the development of implied functions always give, when convergent, the least of the real values which are implied.

LXXIII. *Intelligence and Miscellaneous Articles.*

ON THE PRODUCTION OF OZONE BY THE DECOMPOSITION OF WATER AT LOW TEMPERATURES. BY M. GORET.

IN some experiments in which I employed a voltameter cooled in a mixture of ice and common salt, I observed that the gas evolved, which was to be carried through drying tubes, attacked and quickly ate through the caoutchouc tubes uniting the different pieces of the apparatus. When the voltameter was not cooled, the caoutchouc completely retained the gas. It appeared to me that this corrosive action must be due to the presence of a larger quantity of ozone when the decomposition of water is effected at a low temperature.

I endeavoured to ascertain this quantity in the following manner. Ozone, like chlorine, possesses the property of converting arsenious acid into arsenic acid. If, then, we employ a standard solution of arsenious acid, of such a strength that it requires 1 litre of chlorine to convert the whole of the arsenious acid in 1 litre of the solution into arsenic acid, by passing the gas evolved by the pile through 50 cubic centimetres of this solution, the ozone contained in the gas will effect the conversion of a certain portion of the arsenious acid. For the determination of the quantity which has undergone this change, it is sufficient to compare the quantity of hypochlorite of lime required to complete the oxidation of the arsenious acid into which the gas has been passed, with the quantity necessary to convert the whole of the arsenious acid contained in 50 cubic centimetres of the normal solution into arsenic acid.

Let N be the number of cubic centimetres of a certain solution of hypochlorite of lime required for the conversion of 50 cubic centimetres of the normal fluid into arsenic acid, a change which is indicated by the decoloration of a drop of indigo.

Let N′ be the number of cubic centimetres of the same solution of hypochlorite of lime required to produce the decoloration of a drop of indigo in 50 cubic centimetres of the solution of arsenious acid previously partially oxidized by the action of the ozone.

Then the quantity of ozone which has been absorbed will have produced the same effect as x cubic centimetres of chlorine,

$$N : N-N' : : 50 \text{ cubic centims.} : x;$$

and if we suppose that 1 cubic centimetre of ozone is equivalent to 2 cubic centimetres of chlorine, $\frac{x}{2}$ will express the number of cubic centimetres of ozone.

But in order to ascertain the proportion of the quantity of ozone to the quantity of oxygen evolved, the volume of detonating gas produced must be measured. With this view I employed two voltameters traversed by the same electrical current. One was furnished with a conducting tube, which conveyed the gas to a gauged receiver placed on the water-trough. As the two voltameters disengage, at all events, very nearly the same quantity of gas, the proportion of oxygen produced by the other apparatus may be very well ascertained. The gas evolved by the second voltameter was conveyed by a glass tube to the bottom of a test-tube containing the 50 cubic centimetres of the solution of arsenious acid; this glass tube was recurved at its extremity, and the bubbles of gas escaping from it were received in a funnel immersed in the fluid. The narrow portion of this funnel was also recurved, so as to compel the gas to pass twice through the arsenious acid.

Notwithstanding the adoption of this method to effect a more complete absorption, the gas which had passed still possessed an odour of ozone, and there is reason to believe that a considerable portion escaped the action of the arsenious acid. The results here given are consequently by no means maximum determinations.

The liquid placed in the voltameters was pure sulphuric acid diluted with six times its volume of water, except in two experiments, when chromic acid was employed. In the first experiments the voltameter was composed of a rather small bottle, so that it rapidly became heated by the passage of the current, and it was difficult to keep it at a low temperature. It was afterwards replaced by a larger vessel.

One or two experiments were made without cooling the voltameter, in the rest it was surrounded by a mixture of ice and common salt. The results are given in the following table:—

Sulphuric Acid diluted with Water.

	Size of voltameter.	Duration of evolution.	N.	N′.	Volume of oxygen evolved.	Proportion of ozone to oxygen.	Temperature.
		h m	D	D	cc		
1	Small	1 52	127·5	127·0	666 about.	insensible	Without cooling.
2	Small	2 45	128·5	128·8	1500 ...	„	„
3	Small	1 0	128·0	126·1	731·64	0·00032	But little cooled.
4	Small	3 55	128·1	121·2	1461·16	0·00092	Little cooled.
5	A little larger.	„	84·0	74·0	1263·16	0·00236	Cooled with ice and salt.
6	A little larger.	„	84·0	72·0	1166·89	0·00351	
7	Small	2 5	102·1	84·3	1488·471	0·00293	The temperature was still above 32° F. at the end of exp.
8	Large	0 50	102·5	87·7	737·47	0·00489	+20° F. at end of experiment.
9	Large	2 15	102·8	95·1	1445·5	0·00129	Without cooling; temperature at commencement 43° F.
10	Large	2 55	202·25	157·3	1451·48	0·00383	Temperature at commencement 7° F; 21° at close.
	Chromic Acid diluted with Water.						
11	Small	4 15	101·3	97·0	1462·37	0·000758	Cooled.
12	Small	3 35	101·8	99·0	1444·43	0·000306	Without cooling.

—*Comptes Rendus*, March 6, 1854, p. 445.

ON THE SEPARATION OF NICKEL FROM COBALT.

BY PROF. LIEBIG.

The mixture of the two oxides is dissolved in hydrocyanic acid and potash; the solution is then heated for half an hour on the water-bath in an open dish, or, what is better, boiled in a flask. Cyanide of cobalt and potassium and protocyanide of nickel and potassium are produced. Mercury added to the solution throws down all the nickel in the form of oxide, forming cyanide of mercury, whilst the cobalt compound remains unchanged.

Or it may be treated as just described, but instead of adding mercury, the fluid may be allowed to cool, and supersaturated when cold with chlorine; caustic potash or soda is then added in such proportion, that as the protocyanide of nickel separates, it may be again dissolved. At last the nickel is completely separated in the form of black peroxide; but the cobalt compound is not altered by the chlorine, and the nickel thus separated is free of cobalt.—*Ann. der Chem. und Pharm.*, lxxxvii. p. 128.

ON THE PREPARATION OF HYDROFERROCYANIC ACID.

BY PROF. LIEBIG.

If equal volumes of a cold saturated solution of cyanide of potassium and fuming muriatic acid free from iron be mixed (the latter being gradually added to the former), pure snow-white hydroferrocyanic acid is precipitated. When dried upon a tile, it dissolves readily in alcohol, and may be obtained in crystals, free from muriatic acid, by treatment with æther and allowing it to stand.—*Ann. der Chem. und Pharm.*, lxxxvii. p. 127.

OBSERVATIONS ON THE NOMENCLATURE OF THE METALS CONTAINED IN COLUMBITE AND TANTALITE. BY PROF. A. CONNELL.

In 1801 Mr. Hatchett announced the discovery of a new metallic substance, contained as an oxygen acid combined with oxide of iron in an undescribed heavy black mineral from Massachussetts. To this new metal Mr. Hatchett gave the name of *columbium*, and the ore in which he found it has usually in this country been called columbite. A year afterwards Ekeberg announced a new metal which he called *tantalum*, in two Swedish minerals, which he distinguished by the names of tantalite and yttrotantalite.

A few years afterwards, Dr. Wollaston conceived that he had succeeded in establishing that columbium and tantalum are identical; and this view was tacitly acquiesced in by the greater portion of the chemical public for many years, the metal and its ores usually obtaining in this country the names of columbium and columbite, and on the Continent the names of tantalum, and tantalite and yttrotantalite. A mineral was also discovered at Bodenmais, which was held to contain this same metal.

This state of things continued till about 1846, when M. H. Rose of Berlin published a series of researches on the ores from these different localities, from which, so far as I can understand the matter, he drew the following conclusions: first, that the metal in the Swedish tantalite is a distinct metal, with its peculiar oxygen acid and other combinations, and for this metal the name of tantalum may be with great propriety reserved, being the metal discovered by Ekeberg, and by him called *tantalum*; secondly, that in the Bodenmais and American minerals two metals are contained, which M. Rose proposed to distinguish by the names of Niobium and Pelopium, the latter being supposed to be nearly allied to tantalum, but the former quite distinct in its characters*.

This view of Rose has more or less prevailed for the last eight years; although I confess it had always occurred to me, and occasionally I spoke out the view, that Mr. Hatchett's memory had been rather hardly dealt with, since M. Rose had left him entirely out of view, although truly the first discoverer of the first known of these metals and minerals.

When cerium was ascertained not to be a pure metal, but to contain lanthanium and didymium mixed with it, no one thought of dropping entirely the name of cerium. It still belongs to an acknowledged metal, and the rights of its discoverers are unimpaired.

Precisely the same observation applies in regard to yttria and the new oxides of erbium and terbium.

Other examples of the same kind might be quoted.

Now, on the authority of such precedents, when it was thought to be ascertained that the American columbite and the analogous Bodenmais mineral did not contain *one* new metal only, but at least two, justice seems to have required that the name of columbium should have been reserved for the more abundant of these two, just as the names of cerium and of yttrium have been preserved.

But how much more strongly does such a view hold good *now*, when it has been announced by M. Rose that the American and Bodenmais mineral contain only *one metal*, and for this metal he actually proposes the name of niobium†? Does it not follow very clearly that this metal ought to have the name of *columbium*? M. Rose has now come to the same conclusion at which Mr. Hatchett arrived fifty years ago, when he announced that one new metal, to which he gave the name of columbium, existed in the American mineral columbite. If the countrymen of the latter most distinguished analytical chemist have any sense of justice or regard for the memory of an eminent man—one with whom I am proud to say I had a slight acquaintance, and from whom I received some kindness—they will now unite for the future in support of his just right not to be forgotten and entirely laid aside in this matter. There cannot be a better opportunity than the present for taking this step.

I am very far from wishing to overlook the important researches of M. Rose on this, as on so very many other interesting topics, and we shall always feel grateful for his further investigations regarding

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* See Chemical Gazette, vol. iv. p. 349.

† See Chemical Gazette, vol. xii. p. 149.

columbium and its various oxides and other combinations. But we ought not to overlook what was done before him.

The matter is now reduced to a very simple issue.

We have *columbium* in the American and Bodenmais columbites, and probably now in some other minerals.

We have *tantalum* in Swedish tantalite and yttrotantalite, and probably in some other minerals.

Of all courses, one of the most ill-advised seems that followed by some English chemists, of giving occasionally the name of columbium to tantalum, which, as I understand the matter, is now quite ascertained to be different from any of the other metals. This course can only lead to confusion. Tantalum is not columbium.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1854.

Chiswick.—April 1, 2. Very fine. 3. Fine: clear: frosty. 4. Slight haze: cloudless: very clear. 5. Very fine. 6. Foggy: very fine. 7—9. Very fine. 10. Foggy: cold haze. 11. Hazy: fine: clear. 12. Cold haze: very dry air: partially overcast. 13. Dry haze: fine: clear. 14, 15. Very fine. 16. Quite clear: very fine: overcast. 17. Light clouds: fine: clear. 18. Slight haze: fine: very clear. 19. Slight haze: cloudless. 20. Very fine. 21. Cloudy. 22. Uniformly overcast: drizzling rain. 23. Cloudy and cold, with dry air: boisterous: very clear. 24. Cloudless: masses of white clouds formed in the forenoon: excessively dry air: very clear: severe and destructive black frost at night. 25. Light clouds: very dry air: clear. 26. Overcast. 27. Showery. 28. Cloudy and cold: very clear. 29. Rain: cloudy: clear. 30. Rain: cloudy: overcast.—The frost on the 24th was more severe than in any April for at least thirty previous years.

Mean temperature of the month	47°·53
Mean temperature of April 1853	45 ·44
Mean temperature of April for the last twenty-eight years	47 ·16
Average amount of rain in April	1·64 inch.

Boston.—April 1—7. Fine. 8. Cloudy. 9—14. Fine. 15. Cloudy. 16—20. Fine. 21. Rain A.M. 22. Rain A.M. and P.M. 23—26. Cloudy. 27. Rain A.M. and P.M., with thunder and lightning. 28. Rain A.M. 29. Cloudy: rain A.M. 30. Cloudy: rain P.M.

Sandwick Manse, Orkney.—April 1. Rain A.M.: rain, clear P.M. 2. Clear A.M.: showers P.M. 3. Showers A.M.: rain P.M. 4. Bright A.M.: cloudy P.M. 5. Showers A.M.: cloudy P.M. 6. Drizzle A.M.: clear P.M. 7. Bright A.M.: cloudy P.M. 8. Bright A.M.: showers P.M. 9. Bright A.M.: cloudy P.M. 10. Bright A.M.: showers P.M. 11. Bright A.M.: clear, aurora P.M. 12. Clear A.M. and P.M. 13. Fine A.M.: clear P.M. 14. Fine, clear A.M.: clear, aurora P.M. 15. Fog A.M.: damp P.M. 16. Cloudy A.M.: clear P.M. 17. Clear, fine A.M. and P.M. 18, 19. Clear A.M.: clear, aurora P.M. 20. Clear, fine A.M.: clear, fine, aurora P.M. 21. Clear, fine A.M.: cloudy P.M. 22. Cloudy A.M. and P.M. 23. Snow-showers A.M.: cloudy P.M. 24. Cloudy A.M.: cloudy, solar halo P.M. 25. Bright A.M.: clear P.M. 26. Damp A.M.: rain P.M. 27. Showers A.M. and P.M. 28. Snow-showers A.M. and P.M. 29. Clear A.M.: rain P.M. 30. Showers A.M.: showers, clear P.M.

Mean temperature of April for twenty-seven previous years	43°·43
Mean temperature of April 1853	44 ·49
Mean temperature of this month	44 ·68
Average quantity of rain in April for thirteen previous years	1·86 inch.

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

Days of Month.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
1854. April.	Max.	Min.		9¼ a.m.	8¾ p.m.	Max.	Min.		9¼ a.m.	8¾ p.m.						
1.	30·290	30·139	29·87	29·82	29·81	71	30	45	49	44½	sw.	wsw.	wsw.	....	....	·07
2.	30·409	30·250	30·05	30·12	30·13	62	32	46	46	38½	e.	ese.	w.	....	....	·12
3.	30·496	30·457	30·09	30·23	30·15	58	26	46·5	42½	45	n.	n.	w.	....	....	·04
4.	30·527	30·384	30·12	30·04	29·88	61	28	45·5	47½	47	w.	wnw.	sw.	....	....	·07
☽ 5.	30·371	30·342	29·95	29·98	30·14	65	27	46	44	44	sw.	w.	wsw.	....	....	·13
6.	30·441	30·308	29·99	29·78	29·98	67	28	47	48	42	w.	w.	wnw.	....	....	·06
7.	30·392	30·344	29·98	30·11	30·09	69	33	52	44	45½	w.	nw.	ssw.	....	....	·02
8.	30·229	30·058	29·85	29·86	29·72	69	29	50	48	46	s.	sw.	w.	....	....	·05
9.	30·184	30·125	29·77	30·00	30·13	65	31	49	47	46½	ne.	nnw.	wsw.	....	....	·01
10.	30·214	30·130	29·88	30·11	30·19	54	30	47	48½	46	e.	e.	w.	....	....	....
11.	30·201	30·110	29·83	30·30	30·46	64	37	46	47	38	ne.	ene.	ese.	....	....	·05
12.	30·356	30·320	30·05	30·48	30·40	60	44	47	45½	42	e.	ne.	se.	·01	....	....
○ 13.	30·346	30·250	30·07	30·40	30·34	61	26	48	50	42½	e.	e.	ese.	....	....	....
14.	30·171	30·053	29·85	30·23	30·14	66	29	49	49½	42½	e.	e.	se.	....	....	....
15.	30·027	29·990	29·70	30·09	30·17	67	26	48	49	46	ne.	n.	n.	·01	....	....
16.	30·195	30·103	29·82	30·26	30·24	64	40	47	46½	42	ne.	n.	se.	....	....	....
17.	30·234	30·206	29·94	30·22	30·21	59	32	48	51	45	e.	ene.	sse.	....	....	....
18.	30·130	30·024	29·87	30·18	30·12	67	41	47	50	45	e.	e.	se.	....	....	....
19.	29·968	29·868	29·67	30·07	30·00	77	44	52	47	44	se.	e.	se.	....	....	....
☾ 20.	29·750	29·566	29·47	29·89	29·82	75	51	55	53	46	e.	e.	calm	....	....	....
21.	29·486	29·367	29·20	29·63	29·76	70	41	51	49½	47½	s.	ene.	nne.	·01	....	....
22.	29·639	29·341	29·14	29·86	30·03	49	39	45	42½	38	n.	ne.	n.	·06	·13	....
23.	30·111	29·864	29·65	30·21	30·37	47	31	44	37½	37	ne.	ne.	n.	....	·05	·04
24.	30·416	30·299	30·00	30·39	30·39	51	18	43	43	43½	ne.	nne.	w.	....	....	....
25.	30·427	30·374	30·10	30·38	30·42	51	39	39·5	48	44½	n.	n.	w.	....	....	....
26.	30·391	30·302	30·06	30·23	29·89	56	35	50	49	46	ne.	n.	w.	....	....	....
● 27.	30·002	29·802	29·53	29·76	29·95	54	36	51·5	44	41	nw.	nw.	nnw.	·13	·02	·36
28.	29·913	29·840	29·55	29·96	29·74	53	37	42·5	39	37½	n.	n.	n.	....	·32	·03
29.	29·699	29·637	29·36	29·71	29·32	53	29	43·5	41½	37	n.	n.	s.	·04	·16	·07
30.	29·528	29·295	29·04	28·89	28·83	52	46	46	44	40½	sw.	w.	ssw.	·04	·07	·30
Mean.	30·151	30·038	29·78	30·039	30·027	61·23	33·83	47·2	46·36	43·01				0·30	0·75	1·42

THE

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SUPPLEMENT TO VOL. VII. FOURTH SERIES.

LXXIV. *On the Theory of Chances developed in* Professor Boole's "*Laws of Thought.*" *By* HENRY WILBRAHAM, *M.A., Fellow of Trinity College, Cambridge*[*].

SOME communications having already within the last few months appeared in the Philosophical Magazine, by Mr. Cayley and Professor Boole, relating to the subjects treated in the work lately published by the latter on the Laws of Thought, it may be considered not out of place to publish in the pages of the same Magazine the following observations on the theory of chances developed in that work.

The object of this paper is to show that Professor Boole does in the greater number of questions relating to chances solvable by his method (or at least in those which are most difficult to treat by other methods), tacitly assume certain conditions expressible by algebraical equations, over and above the conditions expressed by the data of the problem, and to show how these assumed conditions may be algebraically expressed.

When no condition among the chances of the simple events, but only the absolute chances of those several simple events are given, the reasoning of Chapter XVII. of Prof. Boole's book shows that it is assumed that the events are independent, *i. e.* that the event A is as likely to happen in one state of circumstances as regards the remaining events as in another; for instance, that A is as likely to happen if B happen and C do not, as it is if B and C both happen or both fail; and this assumption is implicitly introduced in the logical method of working the problem. It is an assumption easily expressed by an algebraical equation or system of equations. For instance, take the most simple case,—there are two events A and B, the chance of A happening is a, of B, b: what is the chance of A and B both happening? There are four possible cases; viz. (1) both happening, (2) B happening without A, (3) A without B, (4) both

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[*] Communicated by the Author.

failing. Let the chances of these four contingencies be respectively $\theta, \lambda, \mu, \phi$. To determine these we have the equations

$$\theta+\lambda+\mu+\phi=1, \quad \theta+\mu=a, \quad \theta+\lambda=b.$$

Another equation is given in Professor Boole's assumption that A is as likely to happen if B happen as it is if B fail, viz.

$$\frac{\theta}{\theta+\lambda}=\frac{\mu}{\mu+\phi} \text{ or } \frac{\theta}{\mu}=\frac{\lambda}{\phi}.$$

The same equation is given by the condition that B is as likely to happen if A happen as if it fail. These four equations determine the values of $\theta, \lambda, \mu, \phi$. Again, suppose three simple events A, B, C, the chances of which are a, b, c. There are here eight possible cases, (θ) A, B, C all happening, (λ) B and C but not A, (μ) A and C not B, (ν) A and B not C, (ρ) A not B or C, (σ) B not A or C, (τ) C not A or B, (ϕ) all failing. Denoting the chances of these several contingencies by the Greek letters prefixed to them, we have the equations

$$\begin{aligned}\theta+\lambda+\mu+\nu+\rho+\sigma+\tau+\phi&=1\\ \theta+\mu+\nu+\rho&=a\\ \theta+\lambda+\nu+\sigma&=b\\ \theta+\lambda+\mu+\tau&=c.\end{aligned}$$

Professor Boole's assumption of the independence of the simple events completes the system of equations necessary to determine the unknown quantities. It gives the equations

$$\frac{\theta}{\lambda}=\frac{\mu}{\tau}=\frac{\nu}{\sigma}=\frac{\rho}{\phi}, \quad \frac{\theta}{\mu}=\frac{\lambda}{\tau}=\frac{\nu}{\rho}=\frac{\sigma}{\phi}, \quad \frac{\theta}{\nu}=\frac{\lambda}{\sigma}=\frac{\mu}{\rho}=\frac{\tau}{\phi},$$

which comprise, in fact, four independent equations, from which, together with the first four, the unknown quantities may be determined algebraically.

That Professor Boole's method does in such cases as the two just mentioned tacitly make the assumptions stated, is evident as well *à posteriori* as *à priori*. For, in the first case, if we seek to find by the Professor's logical equations the chance of A and B both occurring, we find it to be ab, that of B and not A $(1-a)b$, and so on, which necessarily imply the condition I have stated to be assumed. So in the second case, we should find the chance of A, B, C all happening to be abc, that of B and C but not A $(1-a)bc$, and so on, which imply the four additional assumed conditions.

Now let us pass to the cases where certain conditions among the chances of the several events are given. In the first question which I have stated, where there are only two simple events A and B, suppose there to be another given relation among the chances besides the absolute chances of the simple events A and B. The new *given* condition appears now to supersede and take the place of the previously *assumed* condition; and with this new condition combined with the three former equations among θ, λ, μ, ϕ, the problem is easily solvable by algebra. In the second question, where there are three simple events A, B and C, suppose there to be one other given relation among the chances. This new condition certainly does to some extent supersede those previously assumed; and it appears to me that Professor Boole's reasoning would lead one to suppose that the former assumptions are entirely banished from the problem, and no others except the said newly given condition assumed in their stead. The fact, however, is that in this case certain additional assumptions are made, otherwise the problem would be indeterminate. The nature of these assumptions, which are different from the assumptions made when no condition besides the absolute chances of the simple events is given, will perhaps be better seen from the following discussion of an example than from any general reasoning. I shall adopt in it the same assumptions as are made in Professor Boole's method, but work it out without the aid of his logical equations. Any question which can be solved by the logical method may also be treated in this manner.

The chances of three events A, B, and C are a, b, c respectively, and the chance of all three happening together is m; what is the chance of A occurring without B?

Suppose A, B and C, and a further event S, to be four simple events mutually independent, the absolute chances of which are respectively x, y, z and s. We suppose for the present no connexion to exist between the original simple events A, B and C, and the subsidiary event S. There will be altogether sixteen possible mutually exclusive compound events, the chances of which (since the simple events are independent) are as follows:—

(δ) $xyzs$,
(ϵ) $(1-x)yzs$,
(θ) $x(1-y)zs$,
(ι) $xy(1-z)s$,
(κ) $xyz(1-s)$,
(λ) $(1-x)(1-y)zs$
(μ) $(1-x)y(1-z)s$,
(ν) $(1-x)yz(1-s)$,
(ϖ) $x(1-y)(1-z)s$,
(ρ) $x(1-y)z(1-s)$,
(τ) $xy(1-z)(1-s)$,
(υ) $x(1-y)(1-z)(1-s)$,
(ϕ) $(1-x)y(1-z)(1-s)$,
(χ) $(1-x)(1-y)z(1-s)$,
(ψ) $(1-x)(1-y)(1-z)s$,
(ω) $(1-x)(1-y)(1-z)(1-s)$.

Let us now make an assumption with respect to the subsidiary event S, viz. that it is never observed except in conjunction with the three other events, and is always observed to happen if they concur. Consequently those of the above sixteen compound events which represent S occurring while any one or more of the other three events do not occur, and which represent A, B, C all to occur without S occurring, must be considered as beyond the range of our observation. This does not contradict the former assumption of the mutual independence of the four simple events; for we do not by this last supposition say that such compound events are impossible, nor do we make any new assumption as to the probability of their occurrence, but only that, as they are beyond the limits of our observation, we have nothing to do with them. The events, therefore, which come within our circle of observation are those marked respectively δ, ν, ρ, τ, υ, ϕ, χ, ω; and the absolute chance that any event which may occur is an event within the range of our observation is

$$\begin{aligned} &xyzs+(1-x)yz(1-s)+x(1-y)z(1-s)+xy(1-z)(1-s)\\ &\quad+x(1-y)(1-z)(1-s)+(1-x)y(1-z)(1-s)\\ &\quad+(1-x)(1-y)z(1-s)+(1-x)(1-y)(1-z)(1-s),\end{aligned}$$

which is similar to the quantity called V in Professor Boole's book.

I must here observe that x, y, and z are not the same as the given quantities a, b and c; for the latter represent the chances of A, B, and C respectively occurring, provided that the event is one which comes within our range of observation, whereas x, y, and z represent the absolute chances of the same events whether the event be or be not within that range.

Of the eight events δ, ν, ρ, τ, υ, ϕ, χ, ω which compose V, four, viz. δ, ρ, τ, and υ, imply the occurrence of A. Consequently the chance that if the event be within our range of observation A will occur, is the sum of the chances of these last four events divided by the sum of the chances of the eight. This will be equal to the given chance a. Hence

$$\frac{xyzs+\{(1-y)z+y(1-z)+(1-y)(1-z)\}x(1-s)}{\mathrm{V}}=a.$$

So also

$$\frac{xyzs+\{(1-x)2+x(1-z)+(1-x)(1-z)\}y(1-s)}{\mathrm{V}}=b.$$

$$\frac{xyzs+\{(1-y)z+y(1-z)+(1-y)(1-z)\}z(1-s)}{\mathrm{V}}=c.$$

Also as the event S always in cases within our range of observation occurs conjointly with A, B and C, the chance of S occurring and that of A, B, and C all occurring are the same, and equal to m. Therefore

$$\frac{xyzs}{\mathrm{V}}=m.$$

Out of the events represented by V there are two, ϖ and υ, which imply that A occurs but not B; consequently the chance of A occurring but not B, which is the required chance and may be called $u, = \frac{x(1-y)(1-s)}{V}$. From these five equations x, y, z, s may be eliminated, and there remains an equation which gives u. Or the values of x, y, z, and s may be found from the first four equations, and thence the value of any function of them is known.

This method of solution is almost identical with Professor Boole's. The assumptions are the same in both, and they differ only in my examining as above the import of each step taken separately. Representing the chances of the sixteen separate compound events by the Greek letters prefixed to them, the condition that the four events A, B, C, and S are mutually independent is equivalent to the following relations among δ, ϵ, &c.

$$\frac{\delta}{\epsilon}=\frac{\theta}{\lambda}=\frac{\iota}{\mu}=\frac{\kappa}{\nu}=\frac{\varpi}{\psi}=\frac{\rho}{\chi}=\frac{\tau}{\phi}=\frac{\upsilon}{\omega}$$

$$\frac{\delta}{\theta}=\frac{\epsilon}{\lambda}=\frac{\iota}{\varpi}=\frac{\kappa}{\rho}=\frac{\mu}{\psi}=\frac{\nu}{\chi}=\frac{\tau}{\upsilon}=\frac{\phi}{\omega}$$

$$\frac{\delta}{\iota}=\frac{\epsilon}{\mu}=\frac{\theta}{\varpi}=\frac{\kappa}{\tau}=\frac{\lambda}{\psi}=\frac{\nu}{\phi}=\frac{\rho}{\upsilon}=\frac{\chi}{\omega}$$

$$\frac{\delta}{\kappa}=\frac{\epsilon}{\nu}=\frac{\theta}{\rho}=\frac{\iota}{\tau}=\frac{\lambda}{\chi}=\frac{\mu}{\phi}=\frac{\varpi}{\upsilon}=\frac{\psi}{\omega}.$$

These are reducible to eleven independent equations, viz. the seven in the first line and $\frac{\delta}{\theta}=\frac{\iota}{\varpi}=\frac{\kappa}{\rho}=\frac{\tau}{\upsilon}$, and $\frac{\delta}{\iota}=\frac{\kappa}{\tau}$. The statement that the four simple events are independent is only a concise way of stating that these eleven equations are assumed to hold good. The assumption of these eleven is equivalent to saying that δ, ϵ, &c. are proportional to $xyzs$, $(1-x)yzs$, &c.

We have taken S to represent a simple event of which the absolute chance is s, not to represent the concurrence of A, B and C; and when eight out of the sixteen compound events were struck out as implying the concurrence of the events which we know to be incompatible, we did not make S identical with A, B, and C concurring, but we only say that the cases in which S is accompanied with the absence of A, B and C, or of any of them, are beyond our universe of observation. The truth of the eleven assumed equations is supposed throughout the problem; for if at any point we were to discard or deny them, all conclusions based on them would fall to the ground.

It may naturally be asked, how comes it that when there were given only the chances of the events A, B and C, we assumed the independence of only *three* events, which is equivalent to assuming *four* equations; and when an additional datum was given, viz. the chance m of the concurrence of A, B and C, we assumed the independence of *four* events A, B, C and S, which implies *eleven* equations, whereas we might have expected that one assumed equation less than before would have been requisite? The answer is, that though all the eleven equations have been stated to be assumed, only some of them are in the actual working of the problem necessary assumptions. It is sufficient that the eleven equations should be true so far as they affect the relations among the eight contingencies in the compound event represented by V. It will be found that three only out of the eleven give such relations; and upon the assumptions comprised in these last three equations rests the truth of the solution. The three equations are $\frac{\upsilon}{\omega}=\frac{\tau}{\phi}=\frac{\rho}{\chi}$, and $\frac{\phi}{\omega}=\frac{\nu}{\chi}$. The other eight equations, though not contradictory to the data, are not essential to the solution, and need not have been assumed. If these three conditions had been inserted in the data of the problem, it might have been solved by a simple algebraical process without introducing the subsidiary event S.

This assumption of the independence of the simple events made directly in the solution I have given of the last question, is, as I have said, tacitly made in the logical solutions of the questions given in Professor Boole's book. In Proposition I. of Chapter XVII. the events represented by x, y, &c. are by hypothesis independent. In other words, the equations of condition implied by that independence (in number, 1 if there be 2 events x, y, 4 if there be 3 events, 2^n-n-1 if there be n events) are assumed to subsist among the compound events, which are combinations of the simple events x, y, &c. The theorem is proved and proveable only on this assumption. This proposition is assumed in Prop. II., and forms the basis of the application of the logical equations to questions of chances. In Prop. II. p. 261, the question is of this nature; given that, whenever it be known that the event which will happen will belong to a certain group of events represented by V, the chance of x happening is p, of y, q, &c.; required the absolute probabilities of x, y, ... when we have no such previous knowledge. As in the solution in the book, Prop. I. is in the outset assumed with regard to x, y, ..., the conditions of Prop. I. are assumed, and one of these is that x, y, ... are "simple unconditioned events," which (page 258) implies that they are independent. Consequently x, y, ... are in Prop. II. assumed to be independent. How this can be reconciled with Professor Boole's statement with regard to a particular example of the proposition that his reasoning "does not require that the drawings of a white and marble ball should be independent in virtue of the physical constitution of the balls; that the assumption of their independence is indeed involved in the solution, but does not rest upon any prior assumption as to the nature of the balls, and their relations or freedom from relations, of form, colour, structure, &c." (page 262), I am at a loss to understand. It would appear that its being involved in the solution proves that it must rest on a prior assumption, and that the prior assumption in this case is that the simple events are subject to the results of Prop. I.

When additional conditions, that the chance of a combination of events $\phi_1(x, y, \dots)$ is m, that of $\phi_2(x, y, \dots)$ is n, and so on, are given, and consequently subsidiary events s, t are introduced, the question becomes this:—given that $x, y, \dots s, t, \dots$ are independent events, and that if it be known that the event which will happen will belong to a certain group of events selected out of the whole number of possible combinations, in which s does not happen except in conjunction with $\phi_1(x, y, \dots)$, nor $\phi_1(x, y, \dots)$ except in conjunction with s, and so on with respect to t and $\phi_2(x, y, \dots)$, &c., the chances of $x, y, \dots$ are $p, q, \dots$ and of $s, t, \dots$ are $m, n, \dots$; required the absolute probabilities of $x, y, \dots$ when we have no such previous knowledge; or more usually, required the probability that out of the same group of events as before the event will be some definite combination of $x, y, \dots$

The independence of the events $x, y, \dots s, t, \dots$ is, as before, assumed in the assumption of the results of Prop. I. Nevertheless Professor Boole says (page 264) that the events denoted by s, t, &c., whose probabilities are given, have such probabilities not as *independent events*, but as events subject to a certain condition V. He seems throughout to consider V as a condition which does always obtain, and consequently that the chance of any event inconsistent with it is 0, and therefore he ignores the previously assumed independence of the simple events which is inconsistent with such a supposition, instead of considering V as a condition which, if it obtain, the chances of $x, y, \dots$ are as given in the data of the problem.

I will now take the first problem of Chap. XX. p. 321, which is the question treated of by Mr. Cayley in a paper in the Philosophical Magazine of last October, which elicited an answer from Professor Boole in a succeeding Number of the same Magazine, and work it out in the same manner as I have done a former question on Professor Boole's assumptions. The question is,—the probabilities of two causes A_1 and A_2 are c_1 and c_2 respectively; the probability that if A_1 happen E will happen is p_1, that if A_2 happen E will happen is p_2. E cannot happen if neither A_1 nor A_2 happen. Required the probability of E.

I will first, however, examine what result can be arrived at without making any assumption. Let ξ be the chance of A_1 and A_2 both happening and being followed by E, ξ' that of their both happening but not followed by E, η and η' the chances of A_2 happening without A_1, according as it is followed and not followed by E, ζ and ζ' those of A_1 happening without A_2 according as it is followed or not by E, and σ' the chance of neither A_1 nor A_2 happening, and E of course not happening. The data of the problem give the equations

$$\xi+\xi'+\eta+\eta'+\zeta+\zeta'+\sigma'=1$$

$$\xi+\xi'+\zeta+\zeta'=c_1$$

$$\xi+\xi'+\eta+\eta'=c_2$$

$$\xi+\zeta=c_1p_1$$

$$\xi+\eta=c_2p_2\,;$$

the chance (u) of E happening $=\xi+\eta+\zeta=c_1p_1+c_2p_2-\xi$, where ξ is necessarily less than either c_1p_1 or c_2p_2. We can get no further in the solution without further assumptions or data, having only six equations from which to eliminate seven unknown quantities. Without such the question is indeterminate.

Now, to adopt Professor Boole's assumptions, let x, y, z be the chances of A_1, A_2, and E respectively, and s, t those of two subsidiary events; x, y, s, t are assumed to be mutually independent events; consequently the chances of the sixteen mutually exclusive contingencies formed by combinations of these four simple events will be

(δ)	$xyst$,	(ϖ)	$x(1-y)(1-s)t$,
(ϵ)	$(1-x)yst$,	(ρ)	$x(1-y)s(1-t)$,
(θ)	$x(1-y)st$,	(τ)	$xy(1-s)(1-t)$,
(ι)	$xy(1-s)t$,	(υ)	$x(1-y)(1-s)(1-t)$,
(κ)	$xys(1-t)$,	(ϕ)	$(1-x)y(1-s)(1-t)$,
(λ)	$(1-x)(1-y)st$,	(χ)	$(1-x)(1-y)s(1-t)$,
(μ)	$(1-x)y(1-s)t$,	(ψ)	$(1-x)(1-y)(1-s)t$,
(ν)	$(1-x)ys(1-t)$,	(ω)	$(1-x)(1-y)(1-s)(1-t)$.

The relations among these sixteen events implied by the independence of the four simple events are, as before, eleven in number. As the events represented by s and t in all cases within our range of observation are concomitant with the concurrence of A_1 and E, and of A_2 and E respectively, the events represented by ϵ, θ, ι, κ, λ, ν, χ, ϖ, ψ must be struck out, being inconsistent with such concomitance, and consequently the aggregate event V comprises only the events δ, μ, ρ, τ, υ, ϕ, ω.

Of the eleven equations given by the independence of the simple events, only two involve merely terms comprised in V, and consequently those two are the only necessary assumptions. The truth of the remaining nine is immaterial to the question. The two which affect the terms in V only are

$$\frac{\delta}{\mu}=\frac{\rho}{\omega}, \text{ and } \frac{\tau}{\upsilon}=\frac{\phi}{\omega}.$$

As the events represented by s and xz are concomitant, and also those represented by t and yz, the event δ is equivalent to A_1, A_2 and E all happening, μ to A_2 and E not A_1, ρ to A_1 E not A_2, τ to A_1 A_2 not E, υ to A_1 not A_2 or E, ϕ to A_2 not A_1 or E, ω to neither A_1 A_2 nor E. Consequently the two equations assumed by Professor Boole in virtue of the method he employs are

$$\frac{\text{Prob. of } A_1, A_2, \text{and E all happening}}{\text{Prob. not } A_1,\ A_2,\ \text{E}}=\frac{\text{Prob. } A_1,\ \text{not } A_2,\ \text{E}}{\text{Prob. not } A_1, \text{ not } A_2, \text{ not E}},$$

and

$$\frac{\text{Prob } A_1,\ A_2,\ \text{not E}}{\text{Prob. not } A_1,\ A_2,\ \text{not E}}=\frac{\text{Prob. } A_1,\ \text{not } A_2,\ \text{not E}}{\text{Prob. not } A_1,\ \text{not } A_2,\ \text{not E}}.$$

These two conditions being assumed, it is easy by common algebra to determine the question; for, besides the six equations given, as I said before, in the data, we have the two

$$\frac{\xi}{\eta}=\frac{\zeta}{\sigma'} \text{ and } \frac{\xi'}{\eta'}=\frac{\zeta'}{\sigma'}.$$

From the first five and these last two it is easy to eliminate ξ', η, η', ζ, ζ', and σ', leaving a quadratic in ξ; and in this the value $c_1p_1+c_2p_2-u$ must be substituted for ξ, giving a quadratic to determine u similar to that found by Professor Boole.

The second of these two assumed equations, though perfectly arbitrary, is perhaps not an unreasonable one. It asserts that in those cases in which E does not occur, the relation of independence subsists between A_1 and A_2; that is, that provided E do not occur, A_1 is as likely to happen if A_2 happen as if A_2 fail. I do not see, however, that it is a more reasonable or probable hypothesis than others that might be framed; for instance, than those assumed by Mr. Cayley in his memoir in this Magazine. But the first of these equations appears to me not only arbitrary but eminently anomalous. In the form in which it stands as a relation among the chances of A_1, A_2 and E, no one, I should think, can contend that it is either deduced from the data of the problem, or that the mind by the operation of any law of thought recognizes it as a necessary or most reasonable assumption. Neither can it be said that the mutual independence of the events

A_1, A_2, and the other two represented by s and t (from which assumed independence the two equations are derived), is either a datum of the problem or a condition necessarily recognized by the mind; the absurdity of this is shown enough by the fact, that the latter two are purely imaginary events.

Mr. Cayley's solution is, in fact, as follows: he introduces the subsidiary quantities λ_1, λ_2 determined by the equations

$$p_1=\lambda_1+(1-\lambda_1)\lambda_2c_2$$
$$p_2=\lambda_2+(1-\lambda_2)\lambda_1c_1,$$

and finds u by the equation

$$u=\lambda_1c_1+\lambda_2c_2-\lambda_1\lambda_2c_1c_2.$$

In the preceding notation, and according to the meaning which Mr. Cayley attaches to the subsidiary quantities $\lambda_1\lambda_2$, we have

$$\begin{aligned}\xi&=c_1c_2(\lambda_1+\lambda_2-\lambda_1\lambda_2)\\ \xi'&=c_1c_2(1-\lambda_1)(1-\lambda_2)\\ \eta&=c_2(1-c_1)\lambda_2\\ \eta'&=c_2(1-c_1)(1-\lambda_2)\\ \zeta&=c_1(1-c_2)\lambda_1\\ \zeta'&=c_1(1-c_2)(1-\lambda_1)\\ \sigma'&=(1-c_1)(1-c_2);\end{aligned}$$

values which, combined with the equations for the determination of λ_1, λ_2, satisfy, as they should do, the fundamental system of relations between ξ, ξ', η, η', ζ, ζ', σ'. But the equations last written down give also

$$\sigma'\xi'=\eta'\zeta'$$
$$\sigma'(\xi+\xi')=(\eta+\eta')(\zeta+\zeta');$$

or, as they may also be written,

$$\frac{\xi'}{\eta'}=\frac{\zeta'}{\sigma'}$$
$$\frac{\xi+\xi'}{\eta+\eta'}=\frac{\zeta+\zeta'}{\sigma'};$$

i. e.

$$\frac{\text{Prob. } A_1, A_2, \text{ not } E}{\text{Prob. not } A_1, A_2, \text{ not } E}=\frac{\text{Prob. } A_1, \text{ not } A_2, \text{ not } E}{\text{Prob. not } A_1, \text{ not } A_2, \text{ not } E}$$

and

$$\frac{\text{Prob. } A_1, A_2}{\text{Prob. not } A_1, A_2}=\frac{\text{Prob. } A_1, \text{ not } A_2}{\text{Prob. not } A_1, \text{ not } A_2};$$

which are the assumptions made in Mr. Cayley's solution; it is clear that they amount to this, viz. that the events A_1, A_2 are treated as independent; first, in the case in which E does *not* happen; secondly, in the case where it is not observed whether E does or does not happen.

Though the data of the problem, together with the equations derived from the assumed independence of the simple events, are always enough to determine the unknown quantities x, y, &c., and consequently to determine the chances of the compound events represented above by the Greek letters δ, ϵ, &c., there are cases in which the required chance cannot be exactly expressed in a series of the terms δ, ϵ, &c. In these cases the problem remains indeterminate, notwithstanding the assumptions. Of this nature are Examples 1, 4, 7 of Chapter XVIII. In Ex. 1, for instance, the absolute chances of the four events there represented by ux, $u(1-x)$, $(1-u)x$, $(1-u)(1-x)$ may be found, but the chance of the required event cannot be expressed in a series of these chances, for it comprises all cases which come under the event ux, but only part, an unknown part, of those which come under $(1-u)(1-x)$.

What, now, is the practical value of Professor Boole's logical method as applied to the theory of chances? In cases determinable by ordinary algebraical processes, his book gives a systematic and uniform method of solving the questions, though very commonly a longer one than we should otherwise use; at least it appears to me that the really determinate problems solved in the book, as 2 and 3 of Chap. XVIII., might be more shortly solved without the logical equations. In these cases the originally assumed independence of the simple events is unnecessary, none of the equations implied thereby consisting wholly of terms comprised in V. The disadvantage of Professor Boole's method in such cases is, that it does not show us whether the problem is really determinate or requires further assumptions,—whether, in fact, the assumptions made are necessary or not. On the other hand, in cases not determinable by ordinary algebra, his system is this; he takes a general indeterminate problem, applies to it particular assumptions not definitely stated in his book, but which may be shown, as I have done, to be implied in his method, and with these assumptions solves it; that is to say, he solves a particular determinate case of an indeterminate problem, while his book may mislead the reader by making him suppose that it is the general problem which is being treated of. The question arises, Is the particular case thus solved a peculiarly valuable one, or one more worthy than any other of being solved? It is clearly not an assumption which must in all cases be true; nor is it one which, without knowing the connexion among the simple events, we can suppose more likely than any other to represent that connexion; for if we examined the assumptions as applied to any particular problem (as has been done with reference to the problem last discussed), we should find them to be such as do not strike us as particularly applicable, as was the case with that problem. If, being in ignorance what system of assumptions ought to be made to render the problem determinate, we were to wish to give a definite answer to the problem, it might be in the following form: ascertain the chance of the required event happening on any one system of assumptions, and the chance of that system representing the true connexion among the simple events, and multiply the values of these chances together; the sum of a series of these products comprising every possible system of assumptions would be the true chance of the event. But Professor Boole's method evidently does not attempt to solve any question of this nature. It would seem that though Professor Boole gives a uniform and eminently elegant method of solving a class of cases of such indeterminate problems, that class is not one of much practical application.

LXXV. *Theory of the Electric Residue in the Leyden Jar.*

By R. Kohlrausch.

[Concluded from p. 426.]

§ 11.

We will now endeavour to obtain an equation for the residue-curve by help of the principles stated in the foregoing paragraph.

The charge Q_0 being suddenly imparted to the jar, generates an electric moment *m*, which increases with the time, and, in order to re-establish an equilibrium between the action of the charge Q_0 and a contrary action which has been elicited in the glass, approaches a certain limit M, proportional to Q_0. In order to bring this electric moment into calculation, we must select some unit by which to measure it. Let the unit of moment be that which can detain a residue equal to unity; the latter unit being a certain quantity of electricity, indeed the same quantity according to which Q_0 is measured. As the moment, however, may be assumed proportional to the residue, instead of the former we shall substitute the latter, which is its effect, and say the primitive charge Q_0 has produced the residue r_t in the time *t*, which, in order to restore the equilibrium, must increase to R, so that then

$$R = pQ_0,$$

where *p* is a constant.

If, however, the primitive charge Q_0 continually suffers a loss through the air, so that at the time t it amounts only to Q_t, then the ideal state of equilibrium towards which the glass strives at the time t will no longer necessitate the residue pQ_0, but merely pQ_t. If an actual concealed residue r_t has been formed during this time, then the distance from the state of equilibrium is $pQ_t - r_t$.

Now this distance will continually decrease; whence its differential must be made negative, and the velocity of its change, or

$$\frac{d(pQ_t - r_t)}{dt},$$

is evidently greater the greater the distance which still exists; hence it might be set proportional to some function of the same.

If, by way of trial, we write

$$\frac{d(pQ_t - r_t)}{dt} = -b(pQ_t - r_t)^n,$$

we find by integration that the constants, among which p (as may easily be seen from the observations) has not a very extensive field of variation, cannot be determined so as sufficiently to satisfy the observations.

The velocity in the change of the distance from the state of equilibrium, however, is not in reality merely a function of this distance, as may be seen from the following fact, the mention of which has been hitherto purposely postponed.

If the residue r be generated by a strong charge in a very short time, and afterwards the same residue r be generated by a weak charge in a longer time, the distances from the state of equilibrium on discharging the jar will still in both cases be equal. Notwithstanding this, the former residue sooner converts itself into disposable charge than the latter. Thus we see that the said velocity in the change does not depend solely upon the magnitude of the distance from the state of equilibrium, but also upon the time during which the strain has continued. Owing to the secondary action, similar phænomena would probably be observed in elasticity if suitable bodies could be investigated. If a piece of wood were considerably bent for a short time, or only bent a little but for a long time, it may easily be conceived that, when released, an equal departure from the original form might be manifested in both cases, whilst the times which would be required completely to regain that form might be very different*.

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* It should here be mentioned, that W. Weber, in his research on the elastic force of a silken thread, has obtained an equation for the curve of elastic secondary action which is exactly analogous to the above, and does not contain a particular function of the time. That which we have called

On the right-hand side of the differential equation, therefore, we place a power t^m of the time, and as, by trial, it was found that the exponent n on this side must necessarily be unity*, we have

$$\text{I.}\quad \frac{d(pQ_t-r_t)}{dt}=-bt^m(pQ_t-r_t).$$

Integrating between the limits $t=0$ and $t=t$, and bearing in mind that when $t=0$, $r_t=0$ and $Q_t=0$, we have

$$\text{II.}\quad \log\frac{pQ_t-r_t}{pQ_0}=-\frac{b}{m+1}t^{m+1},$$

and

$$\text{III.}\quad r_t=p\left(Q_t-Q_0e^{-\frac{b}{m+1}t^{m+1}}\right).$$

Provided the principles from which this equation has been deduced are correct, and proper values be given to the constants p, m and b, we may calculate residues which ought to agree pretty well with the observed ones as recorded in § 4, Tables a'', b'' and c''.

It is not difficult to find approximate values for these constants. In the first place let us determine p. When the experiment has already continued for a considerable time, the state of equilibrium will be nearly reached, that is to say, the residue will not differ much from the limit which it is possible for it to reach with the charge then present. Thus approximately we shall have

$$r_t=pQ_t,$$

and p will not be much greater than $\frac{r_t}{Q_t}$. For example, the last determination in the Table b'' gives

$$\frac{r_t}{Q_t}=\frac{0{\cdot}4888}{1{\cdot}0154},$$

and we may assure ourselves that the value $p=0{\cdot}5$ is not far

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pQ_t-r_t in the above, he has represented by x, and hence, according to him,

$$dx=-bx^m\,dt.$$

An attempt to construct the curve from the equation

$$dx=-bxt^m\,dt$$

gave me less correct results. This attempt, however, might possibly have been more successful had the constants been otherwise determined. Without repeating the calculation I will not venture to decide the point.

* It would lead us too far to explain this, nor is it necessary, seeing that it does not interest us to know what equations are inapplicable.

from being correct. Again, if t' and t'' be two of the times of observation, and the corresponding Q_t and r_t be written with the same accents, we deduce from equation II.

$$m = \frac{\log \left\{ \dfrac{\log \left(\dfrac{pQ_{t'} - r_{t'}}{pQ_0} \right)}{\log \left(\dfrac{pQ_{t''} - r_{t''}}{pQ_0} \right)} \right\}}{\log t' - \log t''} - 1,$$

and

$$b = -\frac{m+1}{t^{m+1}} \log \text{nat} \left(\frac{pQ_t - r_t}{pQ_0} \right);$$

so that from two observations, properly chosen, approximate values for these magnitudes may be immediately found.

With these approximate values for the constants p, b and m, those of the several r_t may be calculated, which even now will be found to agree pretty well with the observed values; the correction of the constants may then be determined according to the method of minimum squares.

The values which we obtain in this manner from the Table a'' are,—

$$p = 0{\cdot}4289\,; \quad b = 0{\cdot}0397\,; \quad m = -0{\cdot}5744.$$

If we examine the values of these constants for the Tables b'' and c'', we soon notice that the constant m, and hence also the function of the time which was introduced into the equation, differs so little for the three observed curves, which have reference respectively to a common cylindrical jar with tinfoil coatings, to a narrow-necked bottle filled with mercury, and to a Franklin's plate, that it is evidently a number common to each of these pieces of apparatus. If, therefore, from the above value of m we determine the two other constants which belong to the observations in the Tables b'' and c'', we obtain the three following equations for the calculation of the residues, as they are given in the three Tables a'', b'' and c'':—

$$r_t = 0{\cdot}4289 \left[Q_t - Q_0 e^{-\frac{0{\cdot}0397}{0{\cdot}4255} \cdot t^{0{\cdot}4255}} \right],$$

$$r_t = 0{\cdot}5794 \left[Q_t - Q_0 e^{-\frac{0{\cdot}02237}{0{\cdot}4255} \cdot t^{0{\cdot}4255}} \right],$$

$$r_t = 0{\cdot}2562 \left[Q_t - Q_0 e^{-\frac{0{\cdot}0446}{0{\cdot}4255} \cdot t^{0{\cdot}4255}} \right].$$

Herein the values of Q_t, corresponding to the times t, are to be taken from the tables. In the following Tables a''', b''' and c''', the values of r_t, thus calculated, may be compared with the observed values:—

Table a'''.

t.	r_t calculated.	r_t observed.
0	0	0
18	0·0556	0·0596
50	0·0782	0·0813
110	0·0988	0·0981
160	0·1090	0·1084
215	0·1169	0·1153
265	0·1224	0·1214
330	0·1278	0·1263
382	0·1313	0·1303
450	0·1349	0·1351
523	0·1380	0·1396
577	0·1398	0·1401
680	0·1426	0·1421

Table b'''.

t.	r_t calculated.	r_t observed.
0	0	0
5	0·0872	0·0839
24	0·1619	0·1707
59	0·2180	0·2234
91	0·2544	0·2571
114	0·2738	0·2727
144	0·2949	0·2924
188	0·3199	0·3137
230	0·3379	0·3305
282	0·3592	0·3469
341	0·3782	0·3630
406	0·3947	0·3793
485	0·4119	0·3948
573	0·4273	0·4101
683	0·4432	0·4246
804	0·4569	0·4380
935	0·4688	0·4522
1105	0·4809	0·4637
1285	0·4902	0·4759
1505	0·4983	0·4862
1770	0·5051	0·4943
2070	0·5091	0·5019
2430	0·5108	0·5074
2870	0·5099	0·5099
3420	0·5053	0·5094
4110	0·4961	0·5024
4980	0·4839	0·4919
5370	0·4749	0·4888

Table c'''.

t.	r_t calculated.	r_t observed.
0	0	0
6	0·0285	0·0288
43	0·0567	0·0607
71	0·0661	0·0661
133	0·0783	0·0781
193	0·0855	0·0849
256	0·0907	0·0925
328	0·0971	0·0954
423	0·0990	0·0990
531	0·1016	0·1021
620	0·1032	0·1033
715	0·1045	0·1043
864	0·1052	0·1060

Whilst the calculated values in a''' and c''' are sometimes greater and sometimes less than the observed ones, a more regular deviation is observed in b'''; on the whole the calculated curve is a little more curved than the observed one. As the correspondence would not be improved by basing the calculation of these curves on values of the constants deducible, by the method of minimum squares, from all three, we must conclude that the observed curve contains errors. In fact, it was not immediately deduced from observation, but was obtained in § 4, by bringing the loss of electricity into calculation; and it is evident that this method will be the more uncertain the longer the observation lasted. In the present case, however, the first charge of the bottle was observed for an hour and a half, and the collection of the residue lasted twenty minutes; so that a correspondence between calculation and observation, so great as is here manifested, is rather to be wondered at, and places the certainty of the method in a clear light.

§ 12.

With respect to the meaning of the three constants p, b and m, b is found to be connected with the resistance which the glass opposes to the external influence of the electricity, so that its magnitude may be different for different kinds of glass, whilst m has reference to the time in which this resistance is gradually overcome. The circumstance that m has the same magnitude for the different glasses of the differently-shaped charging apparatus, seems to indicate that the function of the time, which differs little from the square root, was justly introduced into the formula, and that the resistance is a mechanical one, proceeding from the molecular forces on each particle of glass, as in the analogous case of elastic secondary action. The number p expresses what part of the charge which is then present could be detained by the electric moment of the glass if the state of equilibrium were attained. The magnitude of p depends, therefore, on b, and at the same time on the thickness of the glass. As the relation between p and b is still unknown, it does not appear possible at present to obtain the equation of the residue with only two constants.

Whatever opinion may be entertained with respect to these constants, or even to what has here been termed an electric moment, this much is at least certain, the electric residue can be calculated from the equation III. As soon as, for any particular jar, the constants shall have been determined according to § 11, we can give an equation for its disposable charge, which, if not strictly expressive of the precise law, will at any rate secure an approximation sufficiently correct for practical purposes. Practice, for example, may demand the calculation of the disposable charge, which, in a given time after a known charge had been imparted to the jar, was employed for some purpose or other, without being able directly to observe its magnitude. On the other hand, the magnitude of the disposable charge being known, that of the original charge, which was suddenly imparted to a jar at a certain earlier period, may be required. We will deduce the equation, and at the same time consider a particular example, from the data furnished by the jar a, and given in the Tables a, a', a'' and a'''.

According to the calculation of these observations, we have (see Table a''', and the calculation of Table a'' given in Appendix III.),—

$$Q_0=0{\cdot}4742\,;\quad V=0{\cdot}0409\,;\quad F=228{\cdot}086\,;\quad \phi=30{\cdot}767\,;$$
$$T=680\,;\quad p=0{\cdot}4289\,;\quad b=0{\cdot}0397\,;\quad m=-0{\cdot}5744.$$

Herein T is the time at which the jar was first discharged.

The line v_t, which in Plate VI. fig. 2, represents the curve of the loss of electricity, does not differ much from a right line, although the corresponding observations lasted an hour and a half. In the present case, where the observations lasted only eleven minutes, the curve will differ much less from a right line, so that we shall incur little error by assuming the loss to be proportional to the time.

If the loss during the whole operation was V, then up to the time T, when the jar was first discharged, it would be

$$V\cdot\frac{F}{F+\phi},$$

and hence, approximately,

$$v_t=\frac{t}{T}\cdot V\cdot\frac{F}{F+\phi}.$$

If, in another experiment, the primitive charge had been Q_0' instead of Q_0, but the condition of the atmosphere the same, then would

$$v_t'=\frac{t}{T}\cdot V\cdot\frac{F}{F+\phi}\cdot\frac{Q_0'}{Q_0}.$$

This expression, however, requires a correction if the condition of the atmosphere with respect to the loss of electricity be different. To this end the loss of electricity which is sustained by any insulated freely placed body must be determined by means of the torsion balance. Let μ represent this loss in the original observations on the jar to which the Tables a and a' and the unaccented letters Q_0, V, F, ϕ refer; and μ' the loss in the experiment now under consideration, then

$$v_t'=\frac{\mu'}{\mu}\cdot\frac{t}{T}\cdot V\cdot\frac{F}{F+\phi}\cdot\frac{Q_0'}{Q_0}.$$

Further,

$$L_t'=Q_0'-r_t'-v_t',$$

therefore

$$L_t'=Q_0'-p\left(Q_t'-Q_0'e^{-\frac{b}{m+1}t^{m+1}}\right)-v_t'.$$

If for Q_t' we substitute its value $Q_0'-v_t'$, and for v_t' the above expression, we have

$$\text{IV.}\quad L_t'=Q_0'\Big[1-p\left(1-e^{-\frac{b}{m+1}t^{m+1}}\right)-(1-p)\frac{\mu'}{\mu}\cdot\frac{t}{T}\,V\cdot\frac{F}{F+\phi}\cdot\frac{1}{Q_0}\Big].$$

Hence if the charge Q_0' which was originally imparted be known, L_t' can be found; or if the latter be given, Q_0' can be found.

In order to show how far this formula agrees with the observations, we will calculate from it the Table a of § 1, whereby, of course, $\mu' = \mu$ and $Q_0' = Q_0$:—

Table a^{IV}.

t.	L_t calculated.	L_t observed.
0	0·4742	0·4742
18	0·4184	0·4133
50	0·3939	0·3896
110	0·3700	0·3692
160	0·3571	0·3516
215	0·3462	0·3461
265	0·3381	0·3373
330	0·3291	0·3290
382	0·3230	0·3223
450	0·3158	0·3141
523	0·3090	0·3080
577	0·3044	0·3029
680	0·2964	0·2951

The deviation is here so small, that this approximate formula may be regarded as a very suitable substitute for the true equation of the disposable charge which has not yet been discovered.

If in any practical application a few minutes only are concerned, and if on the whole the jar suffers little loss, the equation

$$L_t = pQ_0 \left(1 - e^{-\frac{b}{m+1} t^{m+1}}\right),$$

followed by a few reductions, will be sufficiently correct.

The curve for the reappearing residue remains yet to be determined by calculation. Attempts have been made to this effect, and the formulæ deduced from the same principles completely fulfill the conditions. As it is not probable, however, that an application of these calculations will be made in practice, we withhold them, more especially as the subject has already occupied too much space.

Appendix I.

A battery of five jars (Plate VI. fig. 1), each containing a square foot of coating, stood in its box. At some distance was the single jar, F, which was to be examined; the outer coatings of these six jars were carefully connected with each other, and a wire led from them to the moist earth in the garden. In order to charge the single jar by means of the battery, a thick wire b, to which two thin spring-wires c and d were soldered, was fastened with shell-lac to a lever a; when this lever fell, the two spring-wires connected, for a very short time, the inner coating of the battery and the knob of the single jar. The conducting wire of the sine-electrometer S, which was 3 feet long, was connected with this jar by letting one extremity into a shallow cavity made in its knob. As long as the jar contains no electricity, the magnetic needle of the instrument stands in the meridian, making an acute angle α with a revolving metallic arm. At the moment of imparting a charge to the jar, the electricity would be carried to the needle and arm, and the former, being forcibly repelled from the latter, would be set in violent oscillation; but before the needle came to rest, and before the angle of observation α could be again produced by turning the arm, exactly the most important time for observation would be past. The object therefore was to bring the electrometer into such a condition, before any electricity was imparted to the jar, that observations might commence from the moment when, by falling, the lever effected the charge. This is not difficult if we know beforehand the angle ϕ through which the magnetic needle will be deflected from the meridian by electric repulsion, in order that it may make an angle α with the repelling arm, in other words, provided we already know what charge the jar will receive. Consequently if *mn* (fig. 1) is the position of the magnetic needle in the meridian, *ab* the line of vision which by means of reflexion is placed at right angles to the needle, and *pq* the arm which is connected in a fixed manner with the line of vision, the above object will be effected by turning the latter, and with it the arm, through an angle ϕ, and then, by means of a neighbouring galvanic current, deflecting the needle until it is again in its proper place, *i. e.* perpendicular to the line of vision. To this end the current is made to traverse a multiplying coil M, and moderated in the requisite manner by means of a rheostat or rheochord R. At the moment when, by the fall of the lever, electricity is conveyed from the battery to the single jar, the knob of shell-lac *e* breaks the circuit. The mechanism necessary thereto is very simple. One pole of the circuit is led to the strong wire *h* fixed to a board, the other to a spring-wire *g* (in the foreground of the drawing), which being properly bent requires only to be hooked on to *h* in order to close the current. When the knob of shell-lac strikes the end *f* of the wire *g*, it immediately interrupts the current, whose deflecting force will now be replaced by the repulsive force of the electricity which has entered the electrometer.

In order to know beforehand what charge the jar under examination will receive from the battery, the experimenter must already know in what proportion the electricity divides itself between these two bodies. In Appendix II. a better method for determining this proportion will be given; for the present the following method may suffice, which, for reasons there given, will always admit of sufficient exactitude when the whole coating of the battery is great in comparison to that of the jar. It consists simply in determining the quantity of electricity which will be withdrawn from the battery by the jar.

Before the beginning of the experiment whose description we have commenced, the sine-electrometer was connected with the battery of five jars, and the latter charged. After properly adjusting the needle of the instrument it was moved a little backwards, *i. e.* to an angle somewhat too small, and which amounted to 17° 52′, and allowed to remain, until, by the gradual loss of electricity in the air, the image in the mirror coincided exactly with the mark. At this moment the lever was allowed to fall by releasing a hook *i* attached to a string; a part of the electricity passed over from the battery to the single jar, and the electrometer, which was again adjusted with the least possible loss of time, in 15 seconds, showed a deviation of 12° 13′. The square roots of the sines of these angles are proportional to the charges Q and Q′ of the battery before and after the charge was imparted to the single jar. The latter charge amounts to $Q-Q'$, and the proportion between this and the quantity Q which was at first in the battery, that is to say, the quotient $\frac{Q-Q'}{Q}$, was calculated at 0·1695. This method was often repeated, and gave the following results:—

Deflection of the magnetic needle,		$\frac{Q-Q'}{Q}$.
For Q.	For Q′.	
17° 52′	12° 13′	0·1695
12 13	8 29	0·1651
8 29	5 27	0·1617
24 57	17 3	0·1662
11 55	8 17	0·1647
Mean ……	……	0·1654

In Appendix II. the cause of the difference which still exists between the several determinations of the quotient, amounting almost to 5 per cent., will be further discussed. As, however, the value of this quotient, according to the more trustworthy method there adopted, is 0·1647, or very near the above, we may set

$$\frac{Q-Q'}{Q}=0{\cdot}165.$$

We now know, therefore, that if at first the battery has a charge Q, this will sink to

$$Q'=Q(1-0{\cdot}165)=0{\cdot}835\ Q$$

at the moment when the single jar is charged. At the same time, however, we know that at the moment of transferring the electricity, the single jar will show a tension on the knob equal to that of the battery itself; hence, also, that the indication of the electrometer will be the same at this moment whether it be connected with the battery or with the single jar, for the quantity of electricity which this instrument withdraws from the battery always vanishes in comparison to the quantity in the latter.

To return to the experiment, therefore, the following method was pursued.

A second electrometer S′ was placed near the battery, though at so great a distance from the first S that their needles did not irritate one another. Both were at first connected with the battery of five jars, and the latter charged. By the help of an assistant both instruments were adjusted at the same moment. The electrometer S showed thereby a deflection of 18° 49′. The angle of deflection in the instrument S′ was of no importance, it being intended to remain undisturbed, so as, on subsequently charging the battery, to indicate the moment when the electrometer S, if the latter were still connected with the former, would show an angle of deviation equal to 18° 49′, or in other words, when the charge in the battery amounted to $Q = \sqrt{\sin 18° 49'}$.

These preparations being made and everything discharged, the instrument S was on another day connected with the single jar, and by means of the galvanic current deflected to 13°. This is the angle of deflection which would be produced if the charge $Q = \sqrt{\sin 18° 49'}$ in the battery spread itself over the single jar, and corresponds to the value of x in the equation

$$Q' = \sqrt{\sin x} = 0{\cdot}835 \sqrt{\sin 18° 49'}.$$

The battery was now charged again, the charge, which was at first too great, weakened by contact with a rod of dry fir-wood until the electrometer S′ showed almost the requisite deflection, and then the whole allowed to remain until, by loss of electricity, exactly the former charge reappeared. At this moment, which was communicated by an assistant, the falling lever substituted the deflecting force of the electricity which was transferred to the electrometer S for that of the galvanic current. The disturbance of the needle was scarcely perceptible at first, but it soon moved quickly towards the meridian, so that a new and correct readjustment could only be made after 18 seconds*, when the angle of deflection was 9° 50′.

An assistant noted the time when the first charge was imparted to the jar, as well as the moments whenever the sine-electrometer was readjusted, he also wrote down the correspond-

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* In this case the loss of so much time was due to bad manipulation. In other observations the readjustment was often completed in 5 seconds.

ing angles of deflection as the latter were dictated to him. The following series of observations thus resulted, which in the third column, under the title *charge*, contains the square roots of the sines of the angles of deflection.

Time in seconds after imparting the first charge.	Angle of deflection in the sine-electrometer.	Charge.
0	13° 0′	0·4742
18	9 50	0·4133
50	8 44	0·3896
110	7 50	0·3692
160	7 17	0·3561
215	6 53	0·3461
265	6 32	0·3373
330	6 13	0·3290
382	5 58	0·3223
450	5 40	0·3141
523	5 27	0·3080
577	5 16	0·3029
680	5 0	0·2951

APPENDIX II.

Description and use of the Multiplier.

A very strong copper damper of an elliptical form was surrounded by 1000 coils of good copper wire about 0·25 millim. thick, covered with silk, and coated with collodion; the bed of every coil was also well saturated with collodion. The ends of the wires were led to clamping screws, which were cemented with shell-lac into the same board that carried the multiplier. In the elliptical opening of the damper, which was 8·5 centims. long and 3·3 centims. high, a very strong cylindrical steel magnet 6 centims. in length and 1 centim. in diameter was suspended by means of a cocoon thread, stirrup and fork. The fork on which the magnet hung carried a mirror, in order that observations might be made in magnetometer fashion. The whole was well enclosed in a case provided with glass windows, so that the needle could not be disturbed by currents of air. The damping action was so strong, that oscillations of one degree elongation were quieted in one minute.

In using it for our purpose, one end of the multiplying coil was connected by an intermediate wire with the outer coating of the battery, which, besides this, was in good connexion with the earth. The other end of the multiplying coil was connected with the wire of a lever similar to that drawn in Plate VI. fig. 1 *a*, though between them a glass tube filled with water, and not too short, was interposed. The discharge which deflected the magnet was effected by the fall of the lever. The velocity with which the lever descended could be regulated by the height of its fall, or by a moveable weight made to act against cords of vulcanized caoutchouc. Without doubt a greater stability in the indications of this instrument would be obtained by using a heavier magnet, which would involve, however, a small increase in the other dimensions. Even with these dimensions the multiplier is an excellent electrometer for currents of discharge, provided it be properly retarded when very strong charges are employed.

Such an apparatus may be used with advantage to determine the proportion in which electricity has divided itself between two Leyden jars. A sine-electrometer is connected with one jar only, and as soon as the former indicates a certain deflection, the latter is discharged through the multiplier. A second jar is then connected with the first, and both are simultaneously discharged as soon as the sine-electrometer has attained its former position. If in the first case the needle of the multiplier be deflected to an angle α, and in the second case to $\alpha+\beta$, then by equal tension on the knobs, the quantities of electricity in the two jars will be as α to β. In general, instead of the angles α and β, the number of scale-divisions which are deflected into the field of the telescope may be taken.

In Appendix I. we have described a method for finding the proportion in which the charge divides itself between two jars by means of the sine-electrometer alone. This method suffers from the disadvantage of the charge in the battery being changeable. When a part of the charge is transferred to the single jar, some time must elapse before the electrometer can be readjusted for measurement. At first the charge imparted to the jar appears on this account too great, because during this time, besides the loss of electricity in the air, a residue will also have formed itself; afterwards it appears too small, because the residue already formed is too great to be detained by the remaining charge in the battery, and as a consequence it is partly set at liberty before the electrometer can be readjusted. The present method is free from such disadvantages. In order to show its exactitude, we will give the numbers which determine the proportion in which the charge distributed itself between the jar F mentioned in Appendix I., and the battery B of five jars.

Deflection of the needle in the multiplier in scale divisions.

	By B alone.		By B and F connected.
	66·3		79·5
	66·5		79·6
	66·5		79·4
	66·2		79·2
Mean	66·4		79·8
		Mean	79·5

Let $Q' = 66{\cdot}4$ and $Q = 79{\cdot}5$, then

$$\frac{Q-Q'}{Q} = \frac{13{\cdot}1}{79{\cdot}5} = 0{\cdot}1647.$$

APPENDIX III.

In order to calculate how the loss of electricity distributes itself over the several times of observation, a few indications which have reference to the calculation of the Table a'' will suffice.

The loss V is the difference between the primitive charge and that which we know to have been withdrawn. According to the Table *a*, the primitive charge was 0·4742; at the first discharge 0·2951 was withdrawn; then, according to Table a', the collected residues amounted to 0·1307, to which we may add 0·0075, equal to the last residue, as the quantity which probably still remained in the jar but could not be collected. Then is

$$V = 0{\cdot}4742 - (0{\cdot}2951 + 0{\cdot}1382) = 0{\cdot}0409.$$

The superficial area F enclosed by the curve of disposable charge is

$$\begin{aligned} F &= f_1 + f_2 + f_3 + \&\text{c.}\dots \\ &= \tfrac{1}{2}[(0{\cdot}4742 + 0{\cdot}4133)18 + (0{\cdot}4133 + 0{\cdot}3896)(50-18) + \dots] \\ &= 228{\cdot}086. \end{aligned}$$

The area ϕ enclosed by the curve of collected residues is

$$\begin{aligned} \phi &= f' + f'' + f''' + \&\text{c.}\dots \\ &= \tfrac{1}{2}[0{\cdot}0443 \,.\, 65 + 0{\cdot}0166 \,.\, (153-65) + \dots\dots] \\ &= 30{\cdot}767. \end{aligned}$$

Hence the constant for the loss of electricity is

$$\alpha = \frac{V}{F+\phi} = \frac{0{\cdot}0409}{258{\cdot}863} = 0{\cdot}000158.$$

Everything else is immediately given.

Marburg, Oct. 1853.

LXXVI. *On a new Electro-Magnetic Engine invented by* M. Marié Davy. *By* M. BECQUEREL*.

ATTEMPTS have been made for the last twenty years, to construct machines in which the magnetic property imparted to soft iron by the electric current, should be employed as a motive power; but the electromotive machines hitherto brought forward have been far from presenting any œconomical advantages over steam-engines.

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* From the *Comptes Rendus*, for May 15, 1854, p. 853.

Any electro-magnetic engine must be composed essentially of a series of electro-magnets of soft iron, of armatures also of soft iron, or arranged as electro-magnets, with various adjuncts, for the transmission of the electricity furnished by a battery or electro-magnetic machine, and of a commutator or breaker, for the purpose of producing a continuous circular or backward and forward motion.

In the machines hitherto constructed, these various parts do not combine all the conditions desirable for making use of all the power set in action; a cheap, constant, and powerful source of electricity does not yet exist; the soft iron, never being pure or perfectly malleable, retains for a longer or shorter period after each interruption a portion of the magnetization which had been communicated to it by the current; the primitive current and the extra current produce contrary effects, causing a mutual injury; and the commutators often present alterations when the circuit is closed.

M. Jacobi, moreover, who has carefully studied the subject of the practical employment of electro-magnetic engines, has arrived at this result,—that the mechanical effect or amount of work, considering the expenses necessary to keep them in action, is far inferior to that of the other motive powers in use. But this does not set the question at rest; for, if we succeed in discovering sources of electricity more œconomical and powerful than those at present in use, and in avoiding a portion of the inconveniences already mentioned, electricity and magnetism may take their place with heat as motive forces.

These considerations show that all researches having for their object the removal of some of the difficulties encountered in the employment of electricity as a motive power, should be received favourably; and the memoir recently presented by M. Marié Davy to the Academy contains some new views worthy of attention, as will be seen from the following report.

M. Marié thought, and with reason, that, in order to obtain the maximum of effect in electro-magnetic engines, the electro-magnets and the armatures must act up to the point of contact, seeing that the electro-magnetic force, as he found by calculation and experiment, decreases so rapidly with the distance, that in employing two electro-magnets, when these are brought together from a distance to the point of contact, they develope an amount of work in such a manner that five-sixths are produced in the last millimetre, and the half of the remainder in the last but one; when the second electro-magnet is replaced by an armature of soft iron, three-fourths of the quantity of work are produced in the last millimetre through which the armature passes, and more than half the remainder in the last but one.

In most of the rotatory electro-magnetic machines hitherto constructed, the moveable armatures pass rapidly before the fixed electro-magnets, following a line perpendicular to the axis, without coming into contact; thus the entire amount of work that might be obtained is not made use of. We must, however, refer to the fact, that M. Froment, who has paid much attention to electro-magnetic motor apparatus, has constructed a machine in which an interior wheel, furnished with armatures of soft iron, revolves upon the terminal faces of the fixed electro-magnets, so as to make use of the magnetic attraction even up to the point of contact of the magnetized surfaces; but this arrangement produces, during the action of the machine, a series of shocks or concussions, which are opposed to the construction of a powerful machine upon this model.

M. Marié makes the moveable electro-magnets or armatures revolve in such a manner as to approach the fixed electro-magnets in the direction of the axis up to the point of contact, without any shock. Upon this principle are constructed the two electromotive machines described in his note, one of which has a continuous rotatory, the other an oscillatory motion. We shall only refer to the former apparatus, of which he has prepared a model, which has worked in our presence.

This machine consists of sixty-three electro-magnets arranged at equal distances round a circle of wood, furnished internally with a circle of copper. All the electro-magnets have their axes directed towards the centre of the wheel, and their surface coincides with the concave surface of the copper circle.

In the interior of this large wheel there are two others, of which the radius is one-third of that of the former; these are also furnished with a circle of copper, and bear each twenty-one equidistant electro-magnets, of which the axes are directed towards the centre, and the polar surfaces coincide with the concave surface of the copper wheels; these little wheels can then revolve, without slipping, in the interior of the large wheel, and carry round by their movement the axle of the machine, which corresponds with the axis of the large wheel. The moveable electro-magnets come successively in contact with the fixed electro-magnets. The large and small wheels are furnished with teeth for the maintenance of the coincidence, when this is once established.

The machine is also provided with various pieces of apparatus for putting each of the electro-magnets successively in communication with the battery, and giving a different magnetization to the two electro-magnets at the moment when they act upon each other.

M. Marié has made a change which appears advantageous, by replacing the internal wheels by others, which, instead of bearing electro-magnets, are surrounded by a ring of soft iron, which forms the armature; the moveable portion is thus rendered lighter, and the teeth are rendered unnecessary. It is this modification of the machine that we have seen in action.

The circular electro-magnets of M. Nicklès will here find an interesting application; and, at our suggestion, M. Marié proposes to make some experiments with this addition, which will enable him to augment the power of the machine without increasing the expense.

The construction of the machine is somewhat affected by the inexperience of the maker, so that it required a battery of twenty-four Bunsen's elements to produce $\frac{1}{23}$ horse-power. But, according to M. Marié's calculations, one of the same energy, or perhaps even one of less intensity, would be sufficient, with a machine of large size, to produce 300 times the power, seeing that the friction would not increase in the same proportion as the force of the machine; the means of electrical communication not being changed, and the power produced by the attraction of the magnets being capable of multiplication in a great degree, by making use of electro-magnets formed of large cylinders of soft iron. The model was constructed with a view to show the relations between the effect calculated from the magnetic force developed in the electro-magnet and the actual force produced. The proportion was as 4 to 3, which is already a very close approximation, considering the numerous imperfections resulting from the bad construction of the machine.

LXXVII. *On some Extensions of Quaternions*[*]. *By* Sir William Rowan Hamilton, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the French Institute, Hon. or Corr. Member of several other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland*[†].

[1.] CONCEIVE that in the polynomial expressions,

$$\left.\begin{aligned} \mathrm{P} &= \iota_0 x_0 + \iota_1 x_1 + \ldots + \iota_n x_n = \Sigma \iota x, \\ \mathrm{P}' &= \iota_0 x'_0 + \iota_1 x'_1 + \ldots + \iota_n x'_n = \Sigma \iota x', \\ \mathrm{P}'' &= \iota_0 x''_0 + \iota_1 x''_1 + \ldots + \iota_n x''_n = \Sigma \iota x'', \end{aligned}\right\} \quad . \quad . \quad (1)$$

the symbols $x_0 \ldots x_n$, which we shall call the *constituents* of the polynome P, and in like manner that the constituents $x_0' \ldots x'_n$

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[*] See the work entitled, "Lectures on Quaternions," by the present writer. (Hodges and Smith, Dublin, 1853.)

[†] Communicated by the Author.

of P', and $x''_0 \ldots x''_n$ of P'', are subject to all the usual rules of algebra, and to no others; but that the other symbols, $\iota_0 \ldots \iota_n$, by which those constituents of each polynome are here symbolically multiplied, are not all subject to all those usual rules: and that, on the contrary, these latter symbols are subject, *as a system*, to some *peculiar laws*, of comparison and combination. More especially, let us conceive, in the first place, that these $n+1$ symbols, of the form ι_f, are and must remain unconnected with each other by any *linear* relation, with ordinary algebraical coefficients; whence it will follow that an *equality* between any *two* polynomial expressions of the present class requires that *all* their *corresponding constituents* should be *separately* equal, or that

$$\text{if } P' = P, \text{ then } x'_0 = x_0,\ x'_1 = x_1, \ldots x'_n = x_n: \quad . \quad . \quad (2)$$

and therefore, in particular, that the *evanescence* of any *one* such polynome P requires the vanishing of *each* constituent separately; so that

$$\text{if } P = 0, \text{ then } x_0 = 0,\ x_1 = 0, \ldots x_n = 0. \quad . \quad . \quad . \quad (3)$$

In the second place, we shall suppose that all the usual rules of *addition* and *subtraction* extend to these new polynomes, and to their terms; and that the symbols ι, like the symbols x, are *distributive* in their operation; whence it will follow that

$$P' \pm P = \iota_0(x'_0 \pm x_0) + \ldots + \iota_n(x'_n \pm x_n), \quad . \quad . \quad (4)$$

or that

$$\Sigma\iota x' \pm \Sigma\iota x = \Sigma\iota(x' \pm x): \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and as a further connexion with common algebra, we shall conceive that each separate symbol of the form ι may combine commutatively as a factor with each of the form x, and with every other algebraic quantity, so that $\iota x = x\iota$, and that therefore the polynome P may be thus written,

$$P = x_0\iota_0 + x_1\iota_1 + \ldots + x_n\iota_n = \Sigma x\iota. \quad . \quad . \quad . \quad (6)$$

But, third, *instead* of supposing that the symbols ι combine thus in general *commutatively, among themselves*, as factors or as operators, we shall *distinguish generally* between the two *inverted* (*or opposite*) *products*, $\iota\iota'$ and $\iota'\iota$, or $\iota_f\iota_g$ and $\iota_g\iota_f$; and shall conceive that all the $(n+1)^2$ *binary* products $(\iota\iota')$, *including squares* $(\iota^2 = \iota\iota)$, of the $n+1$ symbols ι, are *defined* as being each equal to a certain *given* or originally *assumed polynome*, of the general form (1), by $(n+1)^2$ equations of the following type,

$$\iota_f\iota_g = (fg0)\iota_0 + (fg1)\iota_1 + \ldots + (fgh)\iota_h + \ldots + (fgn)\iota_n; \quad . \quad (7)$$

the $(n+1)^3$ coefficients, or constituents, of the form (fgh), which we shall call the "constants of multiplication," being so many given, or assumed, algebraic constants, of which some may vanish, and which we do not here suppose to satisfy *generally* the relation, $(fgh) = (gfh)$. And thus the *product of any two given polynomes*, P and P′, of the form (1), combined in a *given order* as factors, becomes equal to a *third given polynome*, P″, of the same general form,

$$P'' = PP' = \Sigma x_f \iota_f \, . \, \Sigma x'_g \iota_g = \Sigma x''_h \iota_h \, ; \quad . \quad . \quad . \quad (8)$$

the summations extending still from 0 to n, and the constituent x''_h of the product admitting of being thus expressed:

$$x''_h = \Sigma (fgh) x_f x'_g . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

As regards the subjection of the symbols ι to the *associative* law of multiplication, expressed by the formula,

$$\iota \, . \, \iota' \iota'' = \iota \iota' \, . \, \iota'',$$

we shall make no supposition at present.

[2.] As a first simplification of the foregoing very general* conception, let it be now supposed that

$$\iota_0 = 1 \, ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

the n other symbols, $\iota_1, \iota_2, \ldots \iota_n$, being thus the only ones which are not subject to all the ordinary rules of algebra. Then because

$$\iota_0 \iota_g = \iota_g, \quad \iota_f \iota_0 = \iota_f, \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

it will follow that if either of the two indices f or g be $=0$, the constant of multiplication (fgh) is either $=1$, or $=0$, according as h is equal or unequal to the other of those two indices; and we may write,

$$(0fh) = (f0h) = 0, \text{ if } h \gtrless f \, ; \quad . \quad . \quad . \quad . \quad (12)$$

$$(0ff) = (f0f) = 1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

With this simplification, the *number* of the arbitrary or disposable constants of the form (fgh), which are not thus known already to have the value 0 or 1, is reduced from $(n+1)^3$ to $(n+1)n^2$; because we may now suppose that f and g are each > 0, or that they vary only from 1 to n. For we may write,

$$P = p + \varpi, \quad P' = p' + \varpi', \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where

$$\left. \begin{aligned} p &= \iota_0 x_0 = x_0, \quad \varpi = \iota_1 x_1 + \ldots + \iota_f x_f + \ldots + \iota_n x_n, \\ p' &= \iota_0 x'_0 = x'_0, \quad \varpi' = \iota_1 x'_1 + \ldots + \iota_g x'_g + \ldots + \iota_n x'_n \, ; \end{aligned} \right\} \quad (15)$$

and then, by observing that p and p' are symbols of the usual and algebraical kind, shall have this expression for the product of two polynomes:

$$P'' = PP' = (p + \varpi)(p' + \varpi') = pp' + p\varpi' + p'\varpi + \varpi\varpi' \, ; \quad . \quad (16)$$

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* Some account of a connected conception respecting *Sets*, considered as including Quaternions, may be found in the Preface to the Lectures already cited.

where the last term, or partial product, $\varpi\varpi'$, is now the only one for which any peculiar rules are required.

[3.] When the polynome P has thus been decomposed into two parts, p and ϖ, of which the one (p) is subject to all the usual rules of algebraical calculation, but the other (ϖ) to peculiar rules; and when these parts are thus in such a sense *heterogeneous*, that an *equation* between two such polynomes resolves itself immediately into *two* separate equations, one between parts of the one kind, and the other between parts of the other kind; so that

$$\text{if } \mathrm{P}=\mathrm{P}', \text{ or } p+\varpi=p'+\varpi', \text{ then } p=p', \text{ and } \varpi=\varpi'\,; \quad . \quad (17)$$

we shall call the former part (p) the *scalar part*, or simply THE SCALAR, of the polynome P, and shall denote it, as such, by the symbol S . P, or SP; and we shall call the latter part (ϖ) the *vector part*, or simply THE VECTOR, of the same polynome, and shall denote this other part by the symbol V . P, or VP: these *names* (scalar and vector), and these *characteristics* (S and V), being here adopted as an extension of the phraseology and notation of the Calculus of Quaternions*, in which such scalars and vectors receive useful geometrical interpretations. From the same calculus we shall here borrow also the conception and the sign of *conjugation*; and shall say that any two polynomes (such as those represented by $p+\varpi$ and $p-\varpi$) are CONJUGATE, if they have *equal scalars* (p), but *opposite vectors* ($\pm\varpi$): and if either of these two polynomes be denoted by P, then the symbol K . P, or KP, shall be employed to represent the other; K being thus used (as in quaternions) as the *characteristic of conjunction*. With these notations, and with the recent significations of p and ϖ,

$$p=\mathrm{S}(p+\varpi),\ \ \varpi=\mathrm{V}(p+\varpi),\ \ p-\varpi=\mathrm{K}(p+\varpi)\,; \quad . \quad (18)$$

or, writing P and P′ for $p+\varpi$ and $p-\varpi$,

$$\mathrm{P}'=\mathrm{KP}, \text{ if } \mathrm{SP}'=\mathrm{SP}, \text{ and } \mathrm{VP}'=-\mathrm{VP}\,; \quad . \ . \ . \ . \quad (19)$$

and generally, for any polynome P, of the kind here considered,

$$\mathrm{P}=\mathrm{SP}+\mathrm{VP},\ \ \mathrm{KP}=\mathrm{SP}-\mathrm{VP}. \quad . \ . \ . \ . \ . \ . \ . \quad (20)$$

We may also propose to call the n symbols $\iota_1 \ldots \iota_n$ by the general name of VECTOR-UNITS, as the symbol ι_0 has been equated in (10) to the SCALAR-UNIT, or to 1; and may call that equation (10) the UNIT-LAW, or more fully, the *law of the primary unit*.

[4.] Already, from these few definitions and notations, a variety of symbolical consequences can be deduced, which have indeed already occurred in the Calculus of Quaternions, but which are here taken with enlarged significations, and without reference to interpretation in geometry. For example, in the general equations (20), we may *abstract from the operand*, that

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* See Lectures, *passim*.

is, from the polynome P, and may write more briefly (as in quaternions),

$$1=S+V,\ K=S-V; \quad . \quad . \quad . \quad . \quad . \quad (21)$$

whence

$$S=\tfrac{1}{2}(1+K),\ V=\tfrac{1}{2}(1-K); \quad . \quad . \quad . \quad (22)$$

or more fully,

$$SP=\tfrac{1}{2}(P+P'),\ VP=\tfrac{1}{2}(P-P'),\ \text{if } P'=KP. \quad . \quad (23)$$

Again, since (with the recent meanings of p and ϖ),

$$\left.\begin{array}{l} Sp=p,\ Vp=0,\ Kp=p,\ S\varpi=0,\ V\varpi=\varpi,\ K\varpi=-\varpi, \\ S(p-\varpi)=p,\ V(p-\varpi)=-\varpi,\ K(p-\varpi)=p+\varpi, \end{array}\right\} (24)$$

we may write

$$\left.\begin{array}{l} SSP=SP,\ VSP=0=SVP,\ VVP=VP, \\ SKP=SP=KSP,\ VKP=-VP=KVP,\ KKP=P; \end{array}\right\} . \quad (25)$$

or more concisely,

$$\left.\begin{array}{l} S^2=S,\ VS=SV=0,\ V^2=V, \\ SK=KS=S,\ VK=KV=-V,\ K^2=1. \end{array}\right\} \quad . \quad . \quad (26)$$

The operations S, V, K are evidently *distributive*,

$$S\Sigma=\Sigma S,\ V\Sigma=\Sigma V,\ K\Sigma=\Sigma K; \quad . \quad . \quad . \quad . \quad . \quad (27)$$

and hence it is permitted to multiply together any two of the equations (21) (22), or to square any one of them, as if S, V, K were ordinary algebraical symbols, and the results must be found to be consistent with those equations themselves, and with the relations (26). Thus, squaring and multiplying the equations (21), we obtain

$$\left.\begin{array}{l} 1^2=(S+V)^2=S^2+V^2+2SV=S+V=1, \\ K^2=(S-V)^2=S^2+V^2-2SV=S+V=1, \\ 1K=(S+V)(S-V)=S^2-V^2=S-V=K; \end{array}\right\} . \quad . \quad (28)$$

and the equations (22) give similarly,

$$\left.\begin{array}{l} S^2=\tfrac{1}{4}(1+K)^2=\tfrac{1}{4}(1+K^2+2K)=\tfrac{1}{2}(1+K)=S; \\ V^2=\tfrac{1}{4}(1-K)^2=\tfrac{1}{4}(1+K^2-2K)=\tfrac{1}{2}(1-K)=V; \\ SV=VS=\tfrac{1}{4}(1+K)(1-K)=\tfrac{1}{4}(1-K^2)=\tfrac{1}{4}(1-1)=0. \end{array}\right\} (29)$$

Again, if we multiply (22) by K, we get

$$\left.\begin{array}{l} KS=\tfrac{1}{2}K(1+K)=\tfrac{1}{2}(K+K^2)=\tfrac{1}{2}(K+1)=S, \\ KV=\tfrac{1}{2}K(1-K)=\tfrac{1}{2}(K-K^2)=\tfrac{1}{2}(K-1)=-V; \end{array}\right\} . \quad (30)$$

all which results are seen to be symbolically true, and other verifications of this sort may easily be derived, among which the following may be not unworthy of notice:

$$(S\pm V)^{2m}=1,\ (S\pm V)^{2m+1}=S\pm V,\ \left(\frac{1\pm K}{2}\right)^m=\frac{1\pm K}{2}, \quad . \quad (31)$$

where m is any positive whole number.

[5.] As a second simplification of the general conception of polynomes of the form (1), which will tend to render the laws of their operations on each other still more analogous to those of the quaternions, let it be now conceived that the choice of the "constants of multiplication," (fgh), is restricted by the following condition, which may be called the "Law of Conjugation:"

$$\mathrm{K}\,.\,\iota\,\iota' = \iota'\,\iota, \text{ or } \mathrm{K}\,.\,\iota_f\,\iota_g = \iota_g\,\iota_f\,; \quad . \quad . \quad . \quad . \quad (32)$$

namely the condition that "opposite (or inverted) products of any two of the n symbols $\iota_1, \ldots \iota_n$, shall always be conjugate polynomes." The indices f and g being still supposed to be each > 0, the constants of multiplication (fgh), which had remained arbitrary and disposable in [2.], after that first simplification which consisted in supposing $\iota_0 = 1$, come now to be still further reduced in number, from $(n+1)n^2$ to $\frac{1}{2}n(n^2+1)$. For we have now, by operating with S on the equation (32), the following formula of relation between those constants,

$$(fg0) = (gf0)\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

and by comparing coefficients of ι_h, this other formula is obtained,

$$-(fgh) = (gfh), \text{ if } h > 0\,; \quad . \quad . \quad . \quad (34)$$

whence

$$(ffh) = 0, \text{ if } h > 0. \quad . \quad . \quad . \quad . \quad . \quad (35)$$

Writing, for conciseness,

$$(fg0) = (fg), \quad (ff) = (f), \quad . \quad . \quad . \quad (36)$$

the *squares*, ι^2, of the *n vector-units* ι, will thus reduce themselves to so many constant *scalars*,

$$\iota_1^2 = (1), \quad \iota_2^2 = (2), \ldots \iota_f^2 = (f), \ldots \iota_n^2 = (n)\,; \quad . \quad (37)$$

and besides these, we shall have $(n+1) \times \dfrac{n(n-1)}{2} = \frac{1}{2}(n^3-n)$ *other* scalars, as constants of multiplication; namely the constituents (fgh) of the polynomial expansions of all the binary products, $\iota\,\iota'$ or $\iota_f\,\iota_g$, of unequal vector-units, taken in any one selected *order*, for instance so that $g > f$; it being unnecessary now, on account of the formulæ of relation (33) (34), to attend *also* to the *opposite* order of the two factors, if the object be merely to determine the *number* of the independent constants, which number is thus found to be $n + \frac{1}{2}(n^3-n) = \frac{1}{2}(n^3+n)$, as above stated. Such then is the number of the constants of multiplication, including n of the form (f), and $\frac{1}{2}n(n-1)$ of the form (fg), besides others of the form (fgh), which remain still arbitrary, or disposable, after satisfying, first, the *Unit-Law*, $\iota_0 = 1$, and second, the *Law of Conjugation*, $\mathrm{K}\,.\,\iota\,\iota' = \iota'\,\iota$.

[6.] From this law of conjugation, (32), several general consequences follow. For, *first*, we see from it that "the *square* of every *vector* is a *scalar,*" which may be thus expanded:

$$\left.\begin{aligned}\varpi^2=(\iota_1x_1+\,..\,\iota_nx_n)^2=(1)x_1{}^2+(2)x_2{}^2+\,..\,+(n)x_n{}^2\\ +2(12)x_1x_2+2(13)x_1x_3+\,..\,+2(fg)x_fx_g+\,..\,;\end{aligned}\right\} \quad . \quad (38)$$

that is, more briefly,

$$(\Sigma\iota x)^2=\Sigma(e)x_e{}^2+2\Sigma(fg)x_fx_g, \quad . \quad . \quad . \quad . \quad (39)$$

the summations extending to values of the indices > 0, and g being $> f$. In the *second* place, and more generally, "*inverted products of any two vectors* are equal to *conjugate polynomes;*" or in symbols,

$$\varpi'\varpi=\mathrm{K}\,.\,\varpi\varpi', \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

whatever two vectors may be denoted by ϖ and ϖ'. In fact, these two products have (according to the definition [3.] of conjugates) one *common scalar part*, but *opposite vector parts*,

$$\left.\begin{aligned}\mathrm{S}\,.\,\varpi'\varpi=\mathrm{S}\,.\,\varpi\varpi'=\Sigma(e)x_ex'_e+\Sigma(fg)(x_fx'_g+x_gx'_f)\,;\\ -\mathrm{V}\,.\,\varpi'\varpi=\mathrm{V}\,.\,\varpi\varpi'=\Sigma(fgh)(x_fx'_g-x_gx'_f)\iota_h:\end{aligned}\right\} \quad (41)$$

whence also we may write, as in quaternions,

$$\mathrm{S}\,.\,\varpi\varpi'=\tfrac{1}{2}(\varpi\varpi'+\varpi'\varpi),\ \mathrm{V}\,.\,\varpi\varpi'=\tfrac{1}{2}(\varpi\varpi'-\varpi'\varpi). \quad . \quad (42)$$

And, *thirdly*, the result (40) may be still further generalized as follows:—"The *conjugate of the product of any two polynomes* is equal to the *product of their conjugates*, taken in an *inverted order;*" or in symbols,

$$\mathrm{K}\,.\,\mathrm{PP}'=\mathrm{KP}'\,.\,\mathrm{KP}. \quad . \quad . \quad . \quad . \quad . \quad (43)$$

In fact, we have now, by (16), (24), (27) and (40),

$$\begin{aligned}\mathrm{KP}''=\mathrm{K}\,.\,\mathrm{PP}'&=\mathrm{K}\,.\,(p+\varpi)(p'+\varpi')\\ &=\mathrm{K}(pp'+p\varpi'+p'\varpi+\varpi\varpi')\\ &=pp'-p\varpi'-p'\varpi+\varpi'\varpi\\ &=(p'-\varpi')(p-\varpi)=\mathrm{KP}'\,.\,\mathrm{KP}, \quad . \quad . \quad (44)\end{aligned}$$

as asserted in (43). It follows also, *fourthly*, that "the product of any two conjugate polynomes is a *scalar*, independent of their order, and equal to the difference of the squares of the scalar and vector parts of either of them;" for,

$$\text{if } \mathrm{P}'=\mathrm{KP}, \text{ then } \mathrm{PP}'=(p+\varpi)(p-\varpi)=p^2-\varpi^2; \quad . \quad (45)$$

where ϖ^2 is, by (38) or (39), a scalar. And if we agree to call the *square root* (taken with a suitable sign) of this scalar product of two conjugate polynomes, P and KP, the common TENSOR of each, and to denote it by the symbol TP; if also we give the name of VERSOR to the quotient of *a polynome divided by its own tensor*, and denote this quotient by the symbol UP: we shall then be able to establish several *general formulæ*, as extensions from the theory of quaternions. For we shall have

$$\mathrm{TP}=\mathrm{TKP}=\sqrt{(\mathrm{PKP})}=\{(\mathrm{SP})^2-(\mathrm{VP})^2\}^{\frac{1}{2}}; \quad . \quad . \quad (46)$$

$$\mathrm{T}(p\pm\varpi)=(p^2-\varpi^2)^{\frac{1}{2}};\ \mathrm{T}p=(p^2)^{\frac{1}{2}},\ \mathrm{T}\varpi=(-\varpi^2)^{\frac{1}{2}}; \quad (47)$$

$$\mathrm{UP}=\frac{\mathrm{P}}{\sqrt{(\mathrm{PKP})}},\ \mathrm{U}(p\pm\varpi)=\frac{p\pm\varpi}{(p^2-\varpi^2)^{\frac{1}{2}}}; \quad . \quad . \quad . \quad (48)$$

$$\mathrm{P}=\mathrm{TP}\,.\,\mathrm{UP}=\mathrm{UP}\,.\,\mathrm{TP}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (49)$$

$$\mathrm{TUP}=\mathrm{UTP}=1;\ \mathrm{TTP}=\mathrm{TP},\ \mathrm{UUP}=\mathrm{UP}: \quad . \quad . \quad (50).$$

with some other connected equations. But, although the chief *terms* (such as scalar, vector, conjugate, tensor, versor), and the main *notations* answering thereto (namely S, V, K, T, U), of the calculus of quaternions, along with several general *formulæ* resulting, come thus to receive extended significations, as applying to certain *polynomial* expressions which involve n vector-units, and for which as many as $\frac{1}{2}(n^3+n)$ constants of multiplication are still left arbitrary and disposable; yet it must be observed, that we have not hitherto established any *modular property* of either of the two functions, which have been called above the *tensor* and *versor* of a polynome; nor any *associative law*, for the multiplication of three such polynomes together.

Observatory of Trinity College, Dublin,
June 6, 1854.

[To be continued.]

LXXVIII. *Description and Analysis of two Mineral Species.* By T. S. Hunt, *of the Geological Commission of Canada*[*].

FOR the specimen which afforded the minerals here described I am indebted to the kindness of Professor Williamson of Queen's College, Kingston, C.W., to whom it was given by Dr. James Wilson of Perth. The locality assigned to it is the second lot of the ninth concession of the township of Bathurst, and it is probably derived from the crystalline limestone of that region. It consists of a white, massive pyroxene or diopside, with small crystals of silvery-gray mica, prisms of bluish-green apatite, portions of copper pyrites, and of a milk-white cleavable calcite, together with a rose-red species, having in its aspect some resemblance to a common variety of Wollastonite. It occurs massive, with cleavages which indicate an oblique system of crystallization; according to Prof. E. C. Chapman of the University of Toronto, who has examined a specimen of the mineral in the

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[*] From the Report of the Survey for 1852–53; communicated by the Author.

collection of the Canadian Institute, the cleavage prism is apparently right rhomboidal; the inclination of M : T = 110° to 115°. The cleavages with M and P are perfect, and easily obtained, giving to the mass a fibrous aspect; with T the cleavage is imperfect. Hardness, 3·5; density, 2·765 to 2·776. Lustre vitreous, shining, occasionally pearly on the cleavage surfaces. Colour, rose-red to peach-blossom red; subtranslucent; fracture uneven.

Before the blowpipe it becomes white and opake from the loss of water, and fuses easily with intumescence into a white enamel. When pulverized and boiled with hydrochloric acid, it is partly decomposed with separation of pulverulent silica. A qualitative analysis showed the presence of silica, lime, magnesia and alumina, with a little iron and traces of manganese, besides a large quantity of potash. The results of three quantitative analyses are subjoined; the earthy ingredients were determined by fusion with carbonate of soda; the alkalies in the first analysis were separated by a method for which I am indebted to Dr. A. A. Hayes of Boston. It consists in fusing the pulverized mineral for half an hour with one part of pure lime and four parts of chloride of calcium in a platinum crucible; the fusion was effected over a spirit-lamp. When cold, the mass was digested with water, and the solution then contained all the alkalies as chlorides, mixed with chloride of calcium. The lime-salt was decomposed by carbonate of ammonia, and the sal-ammoniac separated by sublimation from the evaporated residue, leaving the alkaline chlorides in a state of purity. The whole amount of water in the mineral is only expelled by a heat which produces incipient fusion. The small portion of silica, which in the ordinary process of analysis adheres to the alumina, was not separated except in the third of the following analyses :—

	I.	II.	III.
Silica	42·90	43·00	43·55
Alumina	28·10	27·80	27·94
Oxides of iron and manganese	...	·70	·20
Lime	6·94	6·72	6·50
Magnesia	3·99	3·83	3·81
Potash	8·27	8·27	8·37
Soda	·95	·95	1·45
Water	9·00	9·40	8·61
	100·15	100·67	100·43

As this interesting mineral appears to constitute a new species, I have named it *Wilsonite* after its discoverer, Dr. Wilson, who has long been known as a zealous student of the mineralogy of his district. It is to be wished that further examinations may detect distinct crystals of the mineral; a single imperfect one only was found in the calcite; small masses of the species are often completely imbedded in the accompanying pyroxene. This pyroxene is massive, crystalline, and affords cleavage prisms having the ordinary angles of the species; in a crevice it was observed in small crystals. Hardness=6·5; density 3·186 to 3·192. Lustre vitreous, pearly on the cleavage surfaces; colourless or grayish-white; translucent to semitransparent; fracture uneven, subconchoidal. Before the blowpipe the grayish portions become colourless; it retains its transparency and lustre, and fuses with some difficulty, and with intumescence, into a colourless glass. The results of two analyses were as follows:—

Silica	51·50	50·90
Alumina	6·15	6·77
Peroxide of iron	·35	
Lime	23·80	23·74
Magnesia	17·69	18·14
Loss by ignition	1·10	·90
	100·59	100·45

This pyroxene is peculiar from the amount of alumina, which has not hitherto been observed in any considerable quantity except in the dark-coloured ferruginous varieties. The alumina in these is supposed to replace a portion of silica, and admitting a similar relation in the present variety, we have the ordinary formula of pyroxene. The silica of the first analysis $(\mathrm{Si\,O^3}=45{\cdot}3)$ corresponds to 27·28, and the alumina to 2·87 of oxygen =30·05; while the oxygen of the other constituents, including the water, amounts to 14·95. The ratio of 30·05 : 14·95 is very nearly 2 : 1, conducing to the formula $(\mathrm{Si\,O^3, Al^2\,O^3})^2, (\mathrm{MO})^3$.

LXXIX. *On Fraxinine, the crystallizable principle in the Bark of the* Fraxinus excelsior, *or Common Ash* *By* John Stenhouse, *LL.D., F.R.S.*[*]

It is stated in most systems of chemistry, such for instance as Löwig's, Liebig's, &c., on the authority of Messrs. Keller, Herberger and Buchner, that the bark of the *Fraxinus excelsior* contains a neutral, crystallizable, bitter principle, to which they have given the name of fraxinine.

Their mode of extracting this principle was to treat an infusion of ash-bark, so long as a precipitate fell, first with neutral, and then with basic acetate of lead. The whole was then thrown upon a filter, and the clear liquid which passed through was treated with a current of sulphuretted hydrogen till all excess of

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[*] Communicated by the Author.

lead was removed. The aqueous solution, when free from sulphide of lead, on being sufficiently concentrated, yielded crystals of fraxinine on standing for a few days in a cool place. I operated on 3 lbs. of ash-bark exactly in the way just described. The impure crystals obtained from the aqueous solution were dried with blotting-paper, and when crystallized out of spirits of wine, were colourless. They had lost their bitter, and had acquired a sweetish taste. They had all the characteristics of mannite; and when they were subjected to analysis the following were the results:—

0·273 grm. substance, dried at 212° F., gave 0·194 grm. of water, and 0·389 grm. of carbonic acid.

	Calculated.		Found.
$C^{12} =$	72	39·5	38·897
$H^{14} =$	14	7·7	7·882
$O^{12} =$	96	52·8	53·221
	182	100·0	100·000

The so-called fraxinine therefore is merely mannite. The reason why previous experimenters had mistaken it for a new bitter principle was simply this, that they had not freed it entirely from adhering impurities.

LXXX. *On the Uniform Motion of Heat in Homogeneous Solid Bodies, and its connexion with the Mathematical Theory of Electricity**. *By* Professor WILLIAM THOMSON.

[SINCE the following article was written†, the writer finds that most of his ideas have been anticipated by M. Chasles in two memoirs in the *Journal de Mathématique*; the first in

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* This paper first appeared anonymously in the Cambridge Mathematical Journal in February, 1842. The text is reprinted without alteration or addition. All the foot notes are of the present date (March 1854). The general conclusions established in it show that the laws of distribution of electric or magnetic force in any case whatever must be identical with the laws of distribution of the lines of motion of heat in certain perfectly defined circumstances. With developments and applications contained in a subsequent paper on the Elementary Laws of Statical Electricity (Cambridge and Dublin Mathem. Journ. Nov. 1845), they constitute a full theory of the characteristics of lines of force, which have been so admirably investigated experimentally by Faraday, and complete the analogy with the theory of the conduction of heat, of which such terms as "conducting power for lines of force" (Exp. Res. §§ 2797–2802) involve the idea.

† This preliminary notice was written some months later than the text which follows, and was communicated to the editor of the journal to be prefixed to the paper, which had been in his hands since the month of September, 1841. The ideas in which the author had ascertained he had been anticipated by M. Chasles, were those by which he was led to the determi-

vol. iii., on the Determination of the Value of a certain Definite Integral; and the second, in vol. v., on a new Method of Determining the Attraction of an Ellipsoid on a Point without it. In the latter of these mémoires, M. Chasles refers to a paper, by himself, in the twenty-fifth *cahier* of the *Journal de l'Ecole Polytechnique*, in which it is probable there are still further anticipations, though the writer of the present article has not had access to so late a volume of the latter Journal. Since, however, most of his methods are very different from those of M. Chasles, which are nearly entirely geometrical, the following article may not be uninteresting to some readers.]

If an infinite homogeneous solid be submitted to the action of certain constant sources of heat, the stationary temperature at any point will vary according to its position; and through every point there will be a surface, over the whole extent of which the temperature is constant, which is therefore called an *isothermal* surface. In this paper the case will be considered in which these surfaces are finite, and consequently closed.

It is obvious that the temperature of any point without a given isothermal surface depends merely on the form and temperature of the surface being independent of the actual sources of heat by which this temperature is produced, provided there

nation of the attraction of an ellipsoid given in the latter part of the paper. He found soon afterwards that he was anticipated by the same author in an enunciation of the general theorems regarding attraction; still later he found that both an enunciation and demonstration of the same general theorems had been given by Gauss, whose paper appeared shortly after M. Chasles' enunciations; and after all, he found that these theorems had been discovered and published in the most complete and general manner, with rich applications to the theories of electricity and magnetism, more than ten years previously, by Green! It was not until early in 1845 that the author, after having inquired for it in vain for several years, in consequence of an obscure allusion to it in one of Murphy's papers, was fortunate enough to meet with a copy of the remarkable paper ("An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism," by George Green, Nottingham, 1828) in which this great advance in physical mathematics was first made. It is worth remarking, that, referring to Green as the originator of the term, Murphy gives a mistaken definition of "potential." It appears highly probable that he may never have had access to Green's essay at all, and that this is the explanation of the fact (of which any other explanation is scarcely conceivable), that in his Treatise on Electricity (Murphy's Electricity, Cambridge, 1833) he makes no allusion whatever to Green's discoveries, and gives a theory in no respect pushed beyond what had been done by Poisson. All the general theorems on attraction which Green and the other writers referred to, demonstrated by various purely mathematical processes, are seen as axiomatic truths in approaching the subject by the way laid down in the paper which is now republished. The analogy with the conduction of heat on which these views are founded, has not, so far as the author is aware, been noticed by any other writer.

are no sources without the surface. The temperature of an external point is consequently the same as if all the sources were distributed over this surface in such a manner as to produce the given constant temperature. Hence we may consider the temperature of any point without the isothermal surface as the sum of the temperatures due to certain constant sources of heat distributed over that surface.

To find the temperature produced by a single source of heat, let r be the distance of any point from it, and let v be the temperature at that point. Then, since the temperature is the same for all points situated at the same distance from the source, it is readily shown that v is determined by the equation

$$-r^2\frac{dv}{dr}=\mathrm{A}.$$

Dividing both members by r^2, and integrating, we have

$$v=\frac{\mathrm{A}}{r}+\mathrm{C}.$$

Now let us suppose that the natural temperature of the solid, or the temperature at an infinite distance from the source, is zero; then we shall have $\mathrm{C}=0$, and consequently

$$v=\frac{\mathrm{A}}{r}. \qquad \text{. . . . . (1)}$$

Hence that part of the temperature of a point without an isothermal surface which is due to the sources of heat situated on any element, $d\omega_1{}^2$, of the surface, is $\frac{\rho_1 d\omega_1{}^2}{r_1}$, where r_1 is the distance from the element to that point, and ρ_1 a quantity measuring the intensity of the sources of heat at different parts of the surface. Hence, the supposition being still made that there are no sources of heat without the surface, if v be the temperature at the external point, we have

$$v=\iint\frac{\rho_1 d\omega_1{}^2}{r_1}, \qquad \text{. . . . . (2)}$$

the integrals being extended over the whole surface. The quantity ρ_1 must be determined by the condition

$$v=v_1 \qquad \text{. . . . . (3)}$$

for any point in the surface, v_1 being a given constant temperature.

Let us now consider what will be the temperature of a point within the surface, supposing all the sources of heat by which the surface is retained at the temperature v_1 to be distributed over it. Since there are no sources in the interior of the surface, it follows that as much heat must flow out from the interior across the surface, as flows into the interior from the sources of heat at the surface. Hence the total flux of heat from the original surface to an adjacent isothermal surface in the interior is nothing. Hence also the flux of heat from this latter surface to an adjacent isothermal surface in its interior must be nothing; and so on through the whole of the body within the original surface. Hence the temperature in the interior is constant, and equal to v_1, and therefore, for points at the surface, or within it, we have

$$\iint \frac{\rho_1 d\omega_1{}^2}{r_1} = v_1. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Now, if we suppose the surface to be covered with an attractive medium, whose density at different points is proportional to ρ_1, $-\frac{d}{dx}\iint \frac{\rho_1 d\omega_1{}^2}{r_1}$ will be the attraction, in the direction of the axis of x, on a point whose rectangular coordinates are x, y, z. Hence it follows that the attraction of this medium on a point within the surface is nothing, and consequently ρ_1 is proportional to the intensity of electricity in a state of equilibrium on the surface, the attraction of electricity in a state of equilibrium being nothing on an interior point. Since at the surface the value of $\iint \frac{\rho_1 d\omega_1{}^2}{r_1}$ is constant, and since on that account its value within the surface is constant also, it follows, that if the attractive force on a point at the surface is perpendicular to the surface, the attraction on a point within the surface is nothing. Hence the sole condition of equilibrium of electricity distributed over the surface of a body is, that it must be so distributed that the attraction on a point at the surface oppositely electrified may be perpendicular to the surface.

Since at any of the isothermal surfaces v is constant, it follows that $-\frac{dv}{dn}$, where n is the length of a curve which cuts all the surfaces perpendicularly, measured from a fixed point to the point attracted, is the total attraction on the latter point; and that this attraction is in a tangent to the curve n, or in a normal to the isothermal surface passing through the point. For the same reason, also, if ρ_1 represent a flux of heat, and not an electrical intensity, $-\frac{dv}{dn}$ will be the total flux of heat at the variable extremity of n; and the direction of this flux will be along n, or perpendicular to the isothermal surface. Hence if a surface of an infinite solid be retained at a constant temperature, and if a conducting body bounded by a similar surface be electrified, the flux of heat at any point in the first case will be proportional to the attraction on an electrical point similarly situated in the second; and the direction of the flux will correspond to that of the attraction.

Let $-\frac{dv_1}{dn_1}$ be the external value of $-\frac{dv}{dn}$ at the original surface, or the attraction on a point without it, and indefinitely near it. Now this attraction is composed of two parts; one, the attraction of the adjacent element of the surface, and the other the attraction of all the rest of the surface. Hence, calling the former of these a, and the latter b, we have

$$-\frac{dv_1}{dn_1}=a+b.$$

Now, since the adjacent element of the surface may be taken as infinitely larger, in its linear dimensions, than the distance from it of the point attracted, its attraction will be the same as that of an infinite plane, of the density ρ_1. Hence a is independent of the distance of the point from the surface, and is equal to $2\pi\rho_1$. Hence

$$-\frac{dv_1}{dn_1}=2\pi\rho_1+b.$$

Now, for a point within the surface, the attraction of the adjacent element will be the same, but in a contrary direction; and the attraction of the rest of the surface will be the same, and in the same direction. Hence the attraction on a point within the surface and indefinitely near it is $-2\pi\rho_1+b$; and consequently, since this is equal to nothing, we must have $b=2\pi\rho_1$, and therefore

$$-\frac{dv_1}{dn_1}=4\pi\rho_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Hence ρ_1 is equal to the total flux of heat at any point of the surface divided by 4π.

It also follows, that if the attraction of matter spread over the surface be nothing on an interior point, the attraction on an exterior point indefinitely near the surface is perpendicular to the surface, and equal to the density of the matter at the part of the surface adjacent to that point multiplied by 4π.

If v be the temperature at any isothermal surface, and ρ the intensity of the sources at any point of this surface which would be necessary to sustain the temperature v, we have, by (5),

$$-\frac{dv}{dn}=4\pi\rho,$$

which equation holds, whatever be the manner in which the actual sources of heat are arranged, whether over an isothermal surface or not; and the temperature produced in an external point by the former sources is the same as that produced by the latter. Also the total flux of heat across the isothermal surface, whose temperature is v, is equal to the total flux of heat from the actual sources. From this, and from what has been proved above, it follows that if a surface be described round a conducting or non-conducting electrified body, so that the attraction on points situated on this surface may be everywhere perpendicular to it, and if the electricity be removed from the original body and distributed in equilibrium over this surface, its intensity at any point will be equal to the attraction of the original body on that point, divided by 4π, and its attraction on any point without it will be equal to the attraction of the original body on the same point*.

If we call E the total expenditure of heat, or the whole flux across any isothermal surface, we have, obviously,

$$E = -\iint \frac{dv_1}{dn_1} d\omega_1{}^2.$$

Now this quantity should be equal to the sum of the expenditure of heat from all the sources. To verify this, we must, in the first place, find the expenditure of a single source. Now the temperature produced by a single source is, by (1), $v=\frac{A}{r}$, and hence the expenditure is obviously equal to $-\frac{dv}{dr}\times 4\pi r^2$, or to $4\pi A$. If $A=\rho_1 d\omega_1{}^2$, this becomes $4\pi\rho_1 d\omega_1{}^2$. Hence the total expenditure is $\iint 4\pi\rho_1 d\omega_1{}^2$, or $-\iint\frac{dv_1}{dn_1}d\omega_1{}^2$, which agrees with the expression found above.

The following is an example of the application of these principles.

Uniform Motion of Heat in an Ellipsoid.

The principles established above afford an easy method of determining the isothermal surfaces, and the corresponding temperatures in the case in which the original isothermal surface is an ellipsoid.

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* After having established this remarkable theorem in the manner shown in the text, the author attempted to prove it by direct integration, but only succeeded in doing so upwards of a year later, when he obtained the demonstration published in a paper, "Propositions in the Theory of Attraction" (Camb. Math. Journ. Nov. 1842), which appeared almost contemporaneously with a paper by M. Sturm in Liouville's Journal, containing the same demonstration; exactly the same demonstration, as the author afterwards (in 1845) found, had been given fourteen years earlier by Green.

The first step is to find ρ_1, which is proportional to the quantity of matter at any point in the surface of an ellipsoid, when the matter is so distributed that the attraction on a point within the ellipsoid is nothing. Now the attraction of a shell, bounded by two concentric similar ellipsoids, on a point within it is nothing if the shell be infinitely thin; and its attraction will be the same as that of matter distributed over the surface of one of the ellipsoids, in such a manner that the quantity on a given infinitely small area, at any point, is proportional to the thickness of the shell at the same point. Let a_1, b_1, c_1 be the semi-axes of one of the ellipsoids; $a_1+\delta a_1$, $b_1+\delta b_1$, $c_1+\delta c_1$ those of the other. Let also p_1 be the perpendicular from the centre to the tangent plane at any point on the first ellipsoid, and $p_1+\delta p_1$ the perpendicular from the centre to the tangent plane at a point similarly situated on the second; then δp_1 is the thickness of the shell, since the two ellipsoids being similar, the tangent planes at the points similarly situated on their surfaces are parallel. Also, on account of their similarity,

$$\frac{\delta a_1}{a_1}=\frac{\delta b_1}{b_1}=\frac{\delta c_1}{c_1}=\frac{\delta p_1}{p_1},$$

and consequently the thickness of the shell is proportional to p_1. Hence we have, by (5),

$$-\frac{1}{4\pi}\frac{dv_1}{dn_1}=\rho_1=k_1 p_1, \quad . \quad . \quad . \quad . \quad . \quad (a)$$

where k_1 is a constant to be determined by the condition $v=v_1$ at the surface of the ellipsoid.

To find the equation of the isothermal surface at which the temperature is v_1+dv_1, let $-dv_1=\mathrm{C}$ in (a). Then we have $k_1 p_1 dn_1=\frac{\mathrm{C}}{4\pi}$, or $p_1 dn_1=\theta_1$, where θ_1 is an infinitely small constant quantity; and the required equation will be the equation of the surface traced by the extremity of the line dn_1, drawn externally perpendicular to the ellipsoid. Let x', y', z' be the coordinates of any point in that surface, and x, y, z those of the corresponding point in the ellipsoid. Then, calling α_1, β_1, γ_1 the angles which a normal to the ellipsoid at the point whose coordinates are x, y, z makes with these coordinates, and supposing the axes of x, y, z to coincide with the axes of the ellipsoid $2a_1$, $2b_1$, $2c_1$ respectively, we have

$$x'-x=dn_1\cos\alpha_1=\frac{\frac{x}{a_1^2}dn_1}{\sqrt{\left(\frac{x^2}{a_1^4}+\frac{y^2}{b_1^4}+\frac{z^2}{c_1^4}\right)}}=\frac{x}{a_1^2}p_1 dn_1=\frac{x}{a_1^2}\theta_1.$$

Or $x'-x=\frac{x'}{a_1^2}\theta_1$, since θ_1 is infinitely small, and therefore also $x'-x$; whence

$$x=x'\left(1-\frac{\theta_1}{a_1^2}\right)=\frac{x'}{1+\frac{\theta_1}{a_1^2}}.$$

In a similar manner we should find

$$y=\frac{y'}{1+\frac{\theta_1}{b_1^2}},\text{ and }\quad z=\frac{z'}{1+\frac{\theta_1}{c_1^2}}.$$

But

$$\frac{x^2}{a_1^2}+\frac{y^2}{b_1^2}+\frac{z^2}{c_1^2}=1,$$

and hence we have

$$\frac{x'^2}{a_1^2\left(1+\frac{\theta_1}{a_1^2}\right)^2}+\frac{y'^2}{b_1^2\left(1+\frac{\theta_1}{b_1^2}\right)^2}+\frac{z'^2}{c_1^2\left(1+\frac{\theta_1}{c_1^2}\right)^2}=1,$$

or

$$\frac{x'^2}{a_1^2+2\theta_1}+\frac{y'^2}{b_1^2+2\theta_1}+\frac{z'^2}{c_1^2+2\theta_1}=1,$$

for the equation to the isothermal surface whose temperature is v_1+dv_1, and which is therefore an ellipsoid described from the same foci as the original isothermal ellipsoid. In exactly the same manner it might be shown, that the isothermal surface whose temperature is $v_1+dv_1+dv'_1$ is an ellipsoid having the same foci as the ellipsoid whose temperature is v_1+dv_1, and consequently as the original ellipsoid also. By continuing this process it may be proved that all the isothermal surfaces are ellipsoids, having the same foci as the original one.

From the form of the equation found above for the isothermal ellipsoid whose temperature is v_1+dv_1, it follows that θ_1 or p_1dn_1 is $=a_1da_1$, where da_1 is the increment of a_1 corresponding to the increment dn_1 of n_1. Hence if a be one of the semiaxes of an ellipsoid, $a+da$ the corresponding semiaxis of another ellipsoid having the same foci, dn the thickness at any point of the shell bounded by the two ellipsoids, and p the perpendicular from the centre to the plane touching either ellipsoid at the same point, we have

$$\frac{dn}{da}=\frac{a}{p}. \quad \ldots\ldots\ldots \quad (b)$$

All that remains to be done is to find the temperature at the surface of any given ellipsoid having the same foci as the given ellipsoid. For this purpose, let us first find the value of $-\frac{dv}{dn}$ at any point in the surface of the isothermal ellipsoid whose semiaxes are a, b, c. Now we have from (a)

$$-\frac{dv}{dn}=4\pi kp,$$

where k is constant for any point in the surface of the isothermal ellipsoid under consideration, and determined by the condition that the whole flux of heat across this surface must be equal to the whole flux across the surface of the original ellipsoid. Now the first of these quantities is equal to $4\pi k\iint pd\omega^2$ ($d\omega^2$ being an element of the surface), or to $4\pi\frac{ka}{\delta a}\iint\delta pd\omega^2$, since $\frac{\delta a}{a}=\frac{\delta p}{p}$. But $\iint\delta pd\omega^2$ is equal to the volume of a shell bounded by two similar ellipsoids, whose semiaxes are a, b, c and $a+\delta a$, $b+\delta b$, $c+\delta c$, and is therefore readily shown to be equal to $4\pi\frac{\delta a}{a}abc$. Hence $4\pi\frac{ka}{\delta a}\iint\delta pd\omega^2$, or $4\pi k\iint pd\omega^2$ is equal to $4^2\pi^2 k\,abc$. In a similar manner, we have for the flux of heat across the original isothermal surface $4^2\pi^2k_1a_1b_1c_1$, and therefore

$$4^2\pi^2kabc=4^2\pi^2k_1a_1b_1c_1,$$

which gives

$$k=k_1\frac{a_1b_1c_1}{abc}.$$

Hence we have

$$-\frac{dv}{dn}=4\pi k_1\frac{a_1b_1c_1}{abc}p. \quad . \quad . \quad . \quad . \quad . \quad (c)$$

The value of v may be found by integrating this equation. To effect this, since a, b, c are the semiaxes of an ellipsoid passing through the variable extremity of n, and having the same foci as the original ellipsoid, whose axes are a_1, b_1, c_1, we have

$$a^2-a_1^2=b^2-b_1^2=c^2-c_1^2,$$

which gives

$$\left.\begin{array}{l} b^2=a^2-f^2 \\ c^2=a^2-g^2 \\ \text{where}\quad f^2=a_1^2-b_1^2,\quad g^2=a_1^2-c_1^2 \end{array}\right\}. \quad . \quad . \quad . \quad . \quad . \quad (d)$$

Hence (c) becomes

$$-\frac{dv}{dn}=4\pi k_1\frac{a_1b_1c_1p}{a\sqrt{(a^2-f^2)}\sqrt{(a^2-g^2)}}.$$

Now by (*b*) $dn=\frac{ada}{p}$, and hence

$$dv=-4\pi k_1\frac{a_1b_1c_1da}{\sqrt{(a^2-f^2)}\sqrt{(a^2-g^2)}}.$$

Integrating this, we have

$$v=-4\pi k_1a_1b_1c_1\int\frac{da}{\sqrt{(a^2-f^2)}\sqrt{(a^2-g^2)}}+\mathrm{C}.\qquad(e)$$

The two constants, k_1 and C, must be determined by the condition $v=v_1$ when $a=a_1$, and $v=0$ when $a=\infty$; the latter of which must be fulfilled, in order that the expression found for v may be equal to $\iint\frac{k_1p_1d\omega_1^2}{r_1}$.

To reduce the expression for v to an elliptic function, let us assume

$$\left.\begin{array}{l}a\ =f\operatorname{cosec}\phi\\ a_1=f\operatorname{cosec}\phi_1\end{array}\right\},\quad.\ .\ .\ .\ .\ .\ .\quad(f)$$

which we may do with propriety if f be the greater of the two quantities f and g, since a is always greater than either of them, as we see from (*d*). On this assumption, equation (*e*) becomes

$$v=\frac{4\pi k_1a_1b_1c_1}{f}\int_0^\phi\frac{d\phi}{\sqrt{(1-c'^2\sin^2\phi)}}+\mathrm{C}=\frac{4\pi k_1a_1b_1c_1}{f}\mathrm{F}_{c'}\phi+\mathrm{C},$$

where

$$c'=\frac{g}{f}.\quad.\ .\ .\ .\ .\ .\ .\ .\ .\quad(g)$$

Determining from this the values of C and k_1 by the conditions mentioned above, we find $\mathrm{C}=0$, and

$$k_1=\frac{fv_1}{4\pi a_1b_1c_1\mathrm{F}_{c'}\phi_1};\quad.\ .\ .\ .\ .\quad(h)$$

hence the expression for v becomes

$$v=v_1\frac{\mathrm{F}_{c'}\phi}{\mathrm{F}_{c'}\phi_1}.\quad.\ .\ .\ .\ .\ .\ .\ .\ .\quad(k)$$

The results which have been obtained may be stated as follows:—

If, in an infinite solid, the surface of an ellipsoid be retained at a constant temperature, the temperature of any point in the solid will be the same as that of any other point in the surface of an ellipsoid described from the same foci, and passing through that point; and the flux of heat at any point in the surface of this ellipsoid will be proportional to the perpendicular from the centre to a plane touching it at the point, and inversely proportional to the volume of the ellipsoid.

This case of the uniform motion of heat was first solved by

Lamé, in his memoir on Isothermal Surfaces, in Liouville's *Journal de Mathématique*, vol. ii. p. 147, by showing that a series of isothermal surfaces of the second order will satisfy the equation

$$\frac{d^2v}{dx^2}+\frac{d^2v}{dy^2}+\frac{d^2v}{dz^2}=0,$$

provided they are all described from the same foci. The value which he finds for v agrees with (e), and he finds for the flux of heat at any point the expression

$$\frac{\mathrm{KA}}{\sqrt{(\mu^2-\nu^2)}\sqrt{(\mu^2-\rho^2)}};$$

or, according to the notation which we have employed,

$$\frac{4\pi k_1 a_1 b_1 c_1}{\sqrt{(a^2-\nu^2)}\sqrt{(a^2-\rho^2)}},$$

where ν is the greater real semiaxis of the hyperboloid of one sheet, and ρ the real semiaxis of the hyperboloid of two sheets, described from the same foci as the original ellipsoid, and passing through the point considered. Hence a^2, ν^2, ρ^2 are the three roots of the equation

$$\frac{x^2}{u}+\frac{y^2}{u-f^2}+\frac{z^2}{u-g^2}=1,$$

or

$$u^3-(f^2+g^2+x^2+y^2+z^2)u^2+\{f^2g^2+(f^2+g^2)x^2+g^2y^2+f^2z^2\}$$
$$u-f^2g^2x^2=0.$$

Hence

$$a^2\nu^2\rho^2=f^2g^2x^2,$$

and

$$a^2\nu^2+a^2\rho^2+\nu^2\rho^2=f^2g^2+(f^2+g^2)x^2+g^2y^2+f^2z^2.$$

Therefore

$$(a^2-\nu^2)(a^2-\rho^2)=a^4-a^2\nu^2-a^2\rho^2-\nu^2\rho^2+\frac{2a^2\nu^2\rho^2}{a^2}$$
$$=a^4-\{f^2g^2+(f^2+g^2)x^2+g^2y^2+f^2z^2\}+2f^2g^2\frac{x^2}{a^2}$$
$$=a^4-(a^2-b^2)(a^2-c^2)-(2a^2-b^2-c^2)x^2-(a^2-c^2)y^2-(a^2-b^2)z^2$$
$$+2(a^2-b^2)(a^2-c^2)\frac{x^2}{a^2}$$
$$=a^4-(a^2-b^2)(a^2-c^2)-(b^2+c^2)x^2-(a^2-c^2)y^2-(a^2-b^2)z^2$$
$$+2b^2c^2\left(1-\frac{y^2}{b^2}-\frac{z^2}{c^2}\right)$$
$$=a^4-(a^2-b^2)(a^2-c^2)-(b^2+c^2)x^2-(a^2+c^2)y^2-(a^2+b^2)z^2+2b^2c^2$$
$$=a^2b^2+a^2c^2+b^2c^2-\{(b^2+c^2)x^2+(a^2+c^2)y^2+(a^2+b^2)z^2\};$$

which is readily shewn, by substituting for $a^2b^2+a^2c^2+b^2c^2$ its equal $(a^2b^2+a^2c^2+b^2c^2)\left(\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}\right)$, to be equal to $\frac{a^2b^2c^2}{p^2}$. Hence the expression for $-\frac{dv}{dn}$, given above, becomes

$$-\frac{dv}{dn}=4\pi k_1\frac{a_1b_1c_1}{abc}p,$$

which agrees with (*c*).

Attraction of a Homogeneous Ellipsoid on a Point within or without it.

If in (*c*) we put $k_1=\frac{da_1}{a_1}$, the value of $-\frac{dv}{dn}$ at any point will be the attraction on that point of a shell bounded by two similar concentric ellipsoids, whose semiaxes are

$$a_1,\ a_1\sqrt{(1-e^2)},\ a_1\sqrt{(1-e'^2)},$$

and

$$a_1+da_1,\ (a_1+da_1)\sqrt{(1-e^2)},\ (a_1+da_1)\sqrt{(1-e'^2)},$$

where

$$\left.\begin{aligned} &a^2-b^2=a_1^{\,2}-b_1^{\,2}=a_1^{\,2}e^2 \\ \text{and}\quad &a^2-c^2=a_1^{\,2}-c_1^{\,2}=a_1^{\,2}e'^2 \end{aligned}\right\}, \qquad . \quad . \quad . \quad . \quad (1)$$

the density of the shell being unity. Now this attraction is in a normal drawn through the point attracted to the surface of the ellipsoid whose semiaxes are a, b, c. If we call α, β, γ the angles which this normal makes with the coordinates x, y, z of the point attracted, we have

$$\cos\alpha=\frac{\frac{x}{a^2}}{\sqrt{\left(\frac{x^2}{a^4}+\frac{y^2}{b^4}+\frac{z^2}{c^4}\right)}}=\frac{px}{a^2},$$

and similarly,

$$\cos\beta=\frac{py}{b^2},\quad \cos\gamma=\frac{pz}{c^2}.$$

Hence, calling dA, dB, dC the components of the attraction parallel to the axes of coordinates, we have, from (*c*),

$$\left.\begin{aligned} dA&=4\pi x\frac{b_1c_1}{a^3bc}p^2da_1 \\ dB&=4\pi y\frac{b_1c_1}{ab^3c}p^2da_1 \\ dC&=4\pi z\frac{b_1c_1}{abc^3}p^2da_1 \end{aligned}\right\}. \quad . \quad . \quad . \quad . \quad . \quad (2$$

The integrals of these expressions, between the limits $a_1=0$ and $a_1=a'_1$, are the components of the attraction of an ellipsoid whose semiaxes are a'_1, b'_1, c'_1, or a'_1, $a'_1\sqrt{(1-e^2)}$, $a'_1\sqrt{(1-e'^2)}$ on the point (x, y, z). Now by (1) we may express each of the quantities b, c, b_1, c_1 in terms of a and a_1, and the equation

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1, \text{ or } \frac{x^2}{a^2}+\frac{y^2}{a^2-e^2a_1^{\,2}}+\frac{z^2}{a^2-e'^2a_1^{\,2}}=1 \quad . \quad (3)$$

enables us to express either of the quantities a, a_1 in terms of the other. The simplest way, however, to integrate equations (2) will be to express each in terms of a third quantity,

$$u=\frac{a_1}{a}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Eliminating a from (3) by means of this quantity, we have

$$a_1^{\,2}=u^2x^2+\frac{y^2}{u^{-2}-e^2}+\frac{z^2}{u^{-2}-e'^2}.$$

Hence

$$a_1da_1=\left\{ux^2+\frac{u^{-3}y^2}{(u^{-2}-e^2)^2}+\frac{u^{-3}z^2}{(u^{-2}-e'^2)^2}\right\}du$$

$$=\left(\frac{x^2}{a^4}+\frac{y^2}{b^4}+\frac{z^2}{c^4}\right)a_1^{\,4}u^{-3}du=a_1^{\,4}p^{-2}u^{-3}du.$$

Also from (4) we have $a=\frac{a_1}{u}$; from which we find, by (1),

$$b=\frac{a_1}{u}\sqrt{(1-e^2u^2)}, \quad c=\frac{a_1}{u}\sqrt{(1-e'^2u^2)}.$$

By (1) also, $b_1=a_1\sqrt{(1-e^2)}$, $c_1=a_1\sqrt{(1-e'^2)}$. Making these substitutions in (2) and integrating, we have, calling a' the value of a when $a_1=a'_1$,

$$\left.\begin{aligned}
\mathrm{A}&=4\pi x\sqrt{(1-e^2)}\sqrt{(1-e'^2)}\int_0^{\frac{a'_1}{a'}}\frac{u^2du}{\sqrt{(1-e^2u^2)}\sqrt{(1-e'^2u^2)}}\\
\mathrm{B}&=4\pi y\sqrt{(1-e^2)}\sqrt{(1-e'^2)}\int_0^{\frac{a'_1}{a'}}\frac{u^2du}{(1-e^2u^2)^{\frac{3}{2}}(1-e'^2u^2)^{\frac{1}{2}}}\\
\mathrm{C}&=4\pi z\sqrt{(1-e^2)}\sqrt{(1-e'^2)}\int_0^{\frac{a'_1}{a'}}\frac{u^2du}{(1-e^2u^2)^{\frac{1}{2}}(1-e'^2u^2)^{\frac{3}{2}}}
\end{aligned}\right\} . (5)$$

If the point attracted be within the ellipsoid, the attraction of all the similar concentric shells without the point will be nothing, and hence the superior limit of u will be the value of $\frac{a_1}{a}$ at the surface of an ellipsoid similar to the given one, and passing through the point attracted.

Now in this case $a_1=a$, since a is one of the semiaxes of an ellipsoid passing through the point attracted, and having the same foci as another ellipsoid (passing through the same point) whose corresponding semiaxis is a_1. Hence, for an interior point, we have

$$\left.\begin{aligned} A&=4\pi x\sqrt{(1-e^2)}\sqrt{(1-e'^2)}\int_0^1\frac{u^2du}{\sqrt{(1-e^2u^2)}\sqrt{(1-e'^2u^2)}} \\ B&=4\pi y\sqrt{(1-e^2)}\sqrt{(1-e'^2)}\int_0^1\frac{u^2du}{(1-e^2u^2)^{\frac{3}{2}}(1-e'^2u^2)^{\frac{1}{2}}} \\ C&=4\pi z\sqrt{(1-e^2)}\sqrt{(1-e'^2)}\int_0^1\frac{u^2du}{(1-e^2u^2)^{\frac{1}{2}}(1-e'^2u^2)^{\frac{3}{2}}} \end{aligned}\right\}. \quad (6)$$

These are the known expressions for the attraction of an ellipsoid on a point within it. Equations (5) agree with the expressions given in the supplement to liv. v. of Pontécoulant's *Théorie Analytique du Système du Monde*, where they are found by direct integration by a method discovered by Poisson. They may also be readily deduced from equations (6) by Ivory's theorem. Or, on the other hand, by a comparison of them, after reducing the limits of the integrals to 0 and 1, by substituting $\frac{a'_1}{a'}v$ for u, with equation (6), Ivory's theorem may be readily demonstrated.

LXXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 441.]

March 30, 1854.—Thomas Bell, Esq., V.P., in the Chair.

THE following paper was read :—" On the Structure and Affinities of *Trigonocarpon* (a fossil fruit of the Coal-measures). By Joseph D. Hooker, M.D., F.R.S.

Having been for some time engaged in examining the structure and affinities of some fossil fruits of the coal formation, included under the name *Trigonocarpon*, and the progress which I am enabled to make being extremely slow (owing to the difficulty of procuring good specimens), I am induced to lay before the Royal Society such results as I have arrived at, for publication in their Proceedings (if thought worthy of that honour). The details and illustrations of the subject will, when complete, be offered to the Geological Society of London.

My attention has for many years been directed to the genus *Trigonocarpon*; as, from the period of my earliest acquaintance with the flora of the carboniferous epoch, I have felt assured, that botanically, this was the most interesting and important fossil which it contained in any great abundance, and that until the affinities of this were determined, the real nature of the flora in question could never be regarded as even approximately ascertained.

In the first place, *Trigonocarpon* is so abundant throughout the coal-measures, that in certain localities some species may be procured by the bushel; nor is there any part of the formation in which they do not occur, except the underclays and limestone. The sandstone, ironstones, shales and coal itself, all contain them.

Secondly. The symmetry in form and size which many of them display, the regularity of the sculpturing on their surfaces, and various other points, suggested their belonging to a class of highly organized vegetables.

Thirdly. The fact of our being wholly unacquainted with the organs of fructification belonging to the exogenous vegetation, which also abounds in the coal formation, coupled with the assumed highly organized nature of *Trigonocarpon*, favoured the assumption that these might throw light upon one another, and seemed to afford a legitimate basis upon which to proceed, should I ever procure specimens of *Trigonocarpon* displaying structure, which I had long hoped to do.

It is, however, only since my return from India that I have been so fortunate as to obtain good specimens, and for these I am indebted to my friend Mr. Binney of Manchester, who has himself thrown much light upon the vegetation of the coal epoch, and whose exertions indeed have alone enabled me to prosecute the subject; since he has not only placed his whole collection of Trigonocarpons at my disposal, but has shared with me the trouble and expense of their preparation for study. All the specimens were found imbedded in a very tough and hard black-band or clay ironstone, full of fragments of vegetable matter, and which appears originally to have been a fine tenacious clay.

The individual Trigonocarpons are exposed by breaking this rock, and are invariably so intimately adherent to the matrix as to be fractured with it. A great many of these lumps of ironstone, containing partially exposed Trigonocarpons, have been sliced by a lapidary in the usual manner, and excessively thin sections taken on slips of glass. The sections were made necessarily very much at random, but as nearly as possible parallel, or at right angles to the long diameter of the fruit. Five of the specimens thus operated upon have proved instructive, presenting the same appearances, and all being intelligible, and referable to one highly developed type of plants. As, however, the term highly developed may appear ambiguous, especially with reference to a higher or lower degree in the scale of vegetable life, I may mention that by this term I mean to imply that there are in the fruit of *Trigonocarpon* extensive modifications of elementary organs, for the purpose of their adaptation to special functions, and that these modifications are as great, and the adaptation as special, as any to be found amongst analogous fruits in the existing vegetable world.

Thus, I find that the integuments of the fruit of *Trigonocarpon* are each of them a special highly organized structure; they are modifications of the several coats of one ovule, and indeed of the same number of integuments as now prevail in the ovules of living plants.

The number, structure and superposition of these, are strongly indicative of the Trigonocarpons having belonged to that large section of existing coniferous plants, which bear fleshy, solitary fruits, and not cones; and they so strongly resemble the various parts of the fruit of the Chinese genus *Salisburia*, that, in the present state of our knowledge, it appears legitimate to assume their relationship to it. In all the five specimens alluded to, there are more or less perfect evidences of four distinct integuments, and of a large cavity, which is in all filled with carbonate of lime and magnesia; these minerals, I presume, having replaced the albumen and embryo of the seed.

The general form of the perfect fruit is an elongated ovoid (rather larger than a hazel nut), of which the broader or lower end presents the point of attachment, while the upper or smaller end is produced into a straight, conical, truncated rostrum or beak, which is perforated by a straight longitudinal canal. The exterior integument is very thick and cellular, and was no doubt once fleshy; it alone is produced beyond the seed and forms the beak; its apex I assume to have been that of the primine of the ovule, and its cavity the exostome. The second coat appears to have been much thinner, but hard and woody or bony; it is impervious at the apex; is also ovoid, and sessile by its broad base within the outer integument, with which it is perhaps adherent everywhere except at the apex. This is marked by three angles or ridges, and being that alone which (owing to its hard nature) commonly remains in the fossil state, has suggested the name of *Trigonocarpon.* Within this are the third and fourth coats, both of which are very delicate membranes; one appears to have been in close apposition with the inner wall of the second integument, and the other to have surrounded the albumen. These are now separated both from one another, and from the inner wall of the cavity, by the shrinking of the contents of the latter, and the subsequent infiltration of water charged with mineral matter. I may remark, however, that these two membranes may be due to the separation of one into two plates, in which case the original one was formed of several layers of cells. Hitherto I have not been able to trace any organized structure within the cavity of the fruit, and its real nature therefore remains doubtful. It is only from the strong resemblance, in structure, appearance and superposition, which these integuments present to those of Taxoid Coniferæ, that I assume their probable relationship. *Salisburia*, especially, has the same ovoid fruit, sessile by its broader end, and its outer coat is perfectly analogous, being thick, fleshy, and perforated at its apex by a longitudinal canal (the exostome of the ovule); within this is a perfectly similar, woody, two or three angled, impervious integument, forming the nut. This again is lined with one very delicate membrane, and contains a mass of albumen covered with a second similar membrane. A marked analogy is presented to the European botanist by the fruit of the Yew, which has the same integuments though somewhat modified; the outer, fleshy coat in the Yew is however a cup-shaped receptacle, and not drawn up over the nut so as to leave only a small canal at the top, as in *Salisburia* and *Trigonocarpon.*

The nut also does not adhere to the fleshy cup except below its middle. The internal structure is the same in all three.

Such are the main facts which I have been able satisfactorily to establish. There are many others yet to be worked out, especially those connected with the individual tissues of which those bodies are composed; and it is particularly to be borne in mind that the discovery of some structure indicative of albumen or embryo, is absolutely essential to the complete establishment of the affinity I have suggested.

It must not be overlooked, that the characters through which I have attempted to establish an affinity between *Trigonocarpon* and Coniferæ are equally common to the fruits of Cycadeæ; and in connexion with this subject I may remark, that M. Brongniart* has referred the genus *Noggerathia*, which is also found in the coal-measures, to that natural order, together with some associated organs which are probably Trigonocarpons in a mutilated state. The leaves of *Noggerathia* are, however, alone known, and Dr. Lindley, when figuring those of one species (Lindley and Hutton, Fossil Flora, 28, 29), pointed out their great resemblance to those of *Salisburia*, thus affording collateral evidence of the view I have been led to adopt from an examination of the fruit alone.

April 6.—Thomas Graham, Esq., V.P., in the Chair.

The following communication was read:—"On a new Series of Sulphuretted Acids." By Dr. August Kekulé.

Adopting the idea that the series of organic compounds of which sulphuretted hydrogen is the type, corresponds in every respect with the series of which water is the type, I concluded that not only mercaptans and neutral sulphides which correspond to the alcohols and æthers, but also compounds corresponding to the acids, anhydrous acids and æthers of acids might be produced; I therefore endeavoured to obtain reactions which would enable me to replace oxygen in the compounds of the latter series by sulphur.

Such reactions are produced by the compounds of sulphur with phosphorus—the tersulphide ($P^2 S^3$) and the pentasulphide ($P^2 S^5$)—which are easily obtained by fusing together amorphous phosphorus and sulphur in an atmosphere of carbonic acid; no explosion takes place, although the combination is attended with a very violent action.

Experiment has proved that these combinations of sulphur and phosphorus act on the members of the series of water in the same manner (although less violently) as the corresponding compounds of chlorine and phosphorus;—however, with this difference, that by using the chlorine compounds the product is resolved into *two* groups of atoms, while by using the sulphur compounds there is obtained only *one* group; a peculiarity, which, according to the bibasic nature of sulphur, must have been expected. By acting on these compounds of sulphur and phosphorus with water one atom of sulphu-

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* *Annales des Sciences Naturelles*, 2nd Series, vol. v. p. 52.

retted hydrogen is obtained, while the chlorides give two atoms of hydrochloric acid,

$$6\left.\begin{matrix}H\\H\end{matrix}\right\}O+P^2S=3\left.\begin{matrix}H\\H\end{matrix}\right\}S+2PO^3H^3$$

$$6\left.\begin{matrix}H\\H\end{matrix}\right\}O+2PCl^3=6HCl+2PO^3H^3.$$

Similar reactions are observed with organic compounds belonging to the series of water with the formation of phosphorous and phosphoric acids respectively, or a conjugate acid. By acting in this way, the following series of sulphuretted organic compounds is obtained, by the side of which are placed for comparison the products formed by the action of the chlorides of phosphorus on the same substances.

Sulphuretted Hydrogen. $\left.\begin{matrix}H\\H\end{matrix}\right\}S.$	Hydrochloric Acid. $2HCl.$
Mercaptan. $\left.\begin{matrix}C^2H^5\\H\end{matrix}\right\}S.$	Chloride of Æthyle+Hydrochloric Acid. $C^2H^5Cl+HCl.$
Sulphide of Æthyle. $\left.\begin{matrix}C^2H^5\\C^2H^5\end{matrix}\right\}S.$	Chloride of Æthyle. $2C^2H^5Cl.$
Othyl-Hydrosulphuric Acid. $\left.\begin{matrix}C^2H^3O\\H\end{matrix}\right\}S.$	Chloride of Othyle+Hydrochloric Acid. $C^2H^3O,Cl+HCl.$
Othyl-Sulphide of Othyle. $\left.\begin{matrix}C^2H^3O\\C^2H^3O\end{matrix}\right\}S.$	Chloride of Othyle. $2C^2H^3O,Cl.$
Othyl-Sulphide of Æthyle. $\left.\begin{matrix}C^2H^3O\\C^2H^5\end{matrix}\right\}S.$	Chloride of Othyle+Chloride of Æthyle. $C^2H^3O,Cl+C^2H^5Cl.$

Mercaptan is obtained by the action of tersulphide or pentasulphide of phosphorus on alcohol with extreme facility. Sulphide of æthyle may also be prepared by acting on æther in a similar manner.

Thiacetic Acid,—Sulphuretted Acetic Acid,—has been obtained by me by acting on monohydrated acetic acid with tersulphide of phosphorus. It is a colourless liquid, boiling at about 93° C., and has a peculiar odour resembling sulphuretted hydrogen and acetic acid. It dissolves potassium in the cold and zinc on heating with the evolution of hydrogen, and gives with lead a salt less soluble than the ordinary acetate, so that it gives a precipitate with acetate of lead. By recrystallization from water or alcohol, the lead salt is obtained in fine silky needles, which, though quite colourless at first, are rapidly decomposed (whether in solution or in the solid form) with the formation of sulphuret of lead.

By analysis I found the lead salt contained—

Lead 58·8 per cent. Theory requires 58·0 per cent.

The acid contained—

Sulphur 41·3 per cent. Theory requires 42·1 per cent.

Thiacetic acid is also formed in small quantity and by secondary action, by distilling pentasulphide of phosphorus with fused acetate of soda. Pentachloride of phosphorus gives a violent reaction with thiacetic acid, yielding sulphochloride of phosphorus, chloride of othyle, and hydrochloric acid,

$$\left.\begin{array}{c} C^2H^3O \\ H \end{array}\right\} S + PCl^5 = C^2H^3OCl + HCl + PSCl^3.$$

Thiacetate of Othyle.—Sulphide of Othyle.—Anhydrous Sulphuretted Acetic Acid.—Pentasulphide of phosphorus acts but very feebly upon anhydrous acetic acid in the cold, but on heating a violent reaction takes place. By distilling the product, the anhydrous acid is obtained in the form of a colourless liquid, boiling at about 121° C., and having an odour greatly resembling sulphuretted acetic acid. On mixing with water it falls to the bottom, without, at first, suffering any change; on standing, however, it is slowly dissolved and decomposed into sulphuretted acetic acid and ordinary acetic acid. This change takes place much more rapidly on heating,

$$\left.\begin{array}{c} C^2H^3O \\ C^2H^3O \end{array}\right\} S + \left.\begin{array}{c} H \\ H \end{array}\right\} O = \left.\begin{array}{c} C^2H^3O \\ H \end{array}\right\} S + \left.\begin{array}{c} C^2H^3O \\ H \end{array}\right\} O.$$

It appears that anhydrous sulphuretted acetic acid is also produced by acting on the othyle-sulphide of lead with chloride of othyle, at all events chloride of lead is formed. Chloride of benzoyle gives with the lead salt a similar reaction, and it is probable that an intermediate sulphuretted acid is formed, having the formula

$$\left.\begin{array}{c} C^2H^3O \\ C^7H^5O \end{array}\right\} S.$$

Thiacetate of Æthyle.—Sulphuretted Acetic Æther.—This compound may be prepared by the action of pentasulphide of phosphorus on acetic æther. It is a liquid lighter than water, and possesses an odour resembling acetic æther and sulphuretted hydrogen. It boils at about 80° C.

It will be seen that the action of tersulphide and pentasulphide of phosphorus above described produces sulphuretted organic compounds by substituting sulphur for oxygen. The compounds obtained in this way may also be formed by replacing one or two atoms of hydrogen in sulphuretted hydrogen (H^2S), or one or two atoms of metal in sulphide of potassium (K^2S), or in sulphide of hydrogen and potassium (KHS), by organic radicals. Mercaptan, and the sulphides of alcohol radicals have, in fact, been long obtained in this manner.

The formation of a sulphuretted compound containing an acid radical has been observed by Gerhardt by acting on sulphide of lead with chloride of othyle. I have not made many experiments of this kind, but I have observed that chloride of benzoyle is not decomposed by sulphuretted hydrogen, while it (as well as chloride of othyle) gives a reaction with sulphide of hydrogen and potassium yielding chloride of potassium.

I am continuing these researches, and believe the above reactions will furnish many new compounds, and will tend to complete our knowledge of some of those organic and inorganic compounds now known.

April 27, 1854.—The Earl of Rosse, President, in the Chair.

The following papers were read:—

1. "On the Changes produced in the Blood by the administration of Cod-liver Oil and Cocoa-nut Oil." By Theophilus Thompson, M.D., F.R.S.

The author has found that during the administration of cod-liver oil to phthisical patients their blood grew richer in red corpuscles, and he refers to a previous observation of Dr. Franz Simon to the same effect. The use of almond-oil and of olive-oil was not followed by any remedial effect, but from cocoa-nut oil results were obtained almost as decided as from the oil of the liver of the Cod, and the author believes it may turn out to be a useful substitute. The oil employed was a pure cocoa oleine, obtained by pressure from crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast from the *Copperah* or dried cocoa-nut kernel, and refined by being treated with an alkali and then repeatedly washed with distilled water. It burns with a faint blue flame, showing a comparatively small proportion of carbon, and is undrying.

The analysis of the blood was conducted by Mr. Dugald Campbell. The whole quantity abstracted having been weighed, the coagulum was drained on bibulous paper for four or five hours, weighed and divided into two portions. One portion was weighed and then dried in a water-oven, to determine the water. The other was macerated in cold water until it became colourless, then moderately dried and digested with æther and alcohol to remove fat, and finally dried completely and weighed as fibrine. From the respective weights of the fibrine and the dry clot that of the corpuscles was calculated. The following were the results observed in seven different individuals affected with phthisis in different stages of advancement:—

		Red corpuscles.	Fibrine.
First stage, before the use of cod-liver oil	Female	129·26	4·52
	Male	116·53	13·57
First stage, after the use of cod-liver oil	Female	136·47	5·00
	Male	141·53	4·70
Third stage, after the use of cod-liver oil	Male	138·74	2·23
Third stage, after the use of cocoa-nut oil......	Male	139·95	2·31
	Male	144·94	4·61

2. "On a Property of Numbers." By the Rev. James Booth, LL.D., F.R.S. &c.

I know not whether the following property of numbers has been made public.

A number of six places, consisting of a repetition of a period of any three figures, is divisible by the prime numbers 7, 11 and 13. Thus 376376, 459459, 301301 are so divisible.

A number N of six places may be thus written:—

$$N = 100.000\,a + 10.000\,b + 1000\,c + 100\,d + 10\,e + f,$$

which, when divided by 7, will give a quotient q and a remainder $5a+4b+6c+2d+3e+f$.

Now if $d=a$, $e=b$, $f=c$, this remainder may be written $7(a+b+c)$, which is divisible by 7, whatever be the values of a, b, c.

In like manner if a number of six places be divided by 13, the remainder will be

$4a+3b+12c+9d+10e+f$; and, as before, if $d=a$, $e=b$, $f=c$, the remainder may be written $13(a+b+c)$, which is divisible by 13, whatever be the value of a, b and c.

In the same way it may be shown that a number of this kind is divisible by 11.

When the first figure of the period is 0, and the second any whatever i and j, the number is $0ij0ij = ij0ij$; or any number of five places, the first two and the last two being the same, while the middle place is 0, is divisible by 7, 11 and 13. Thus 34034, 14014 are so divisible.

When the first two places are 0, the number may be written $00i00i = i00i$, or any number of four places, the first and last figures being the same, while the two middle places are 0, is divisible by 7, 11 and 13. Thus 5005, 8008 are so divisible.

Like properties may be found for 17, 19, 23, but the periods are longer. The prime divisor being $2n+1$, it is manifest the number of places in the period cannot exceed, however it may fall short of n.

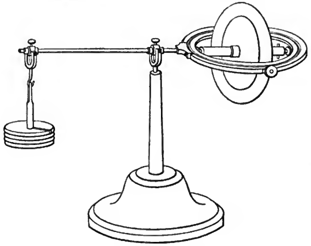
Thus when the divisor is 17, the number of places in the period is eight.

3. "On Fessel's Gyroscope." By C. Wheatstone, Esq., F.R.S.

Since the announcement of M. Foucault's beautiful experiment which has afforded us a new mechanical proof of the rotation of the earth on its axis, the phænomena of rotary motion have received renewed attention, and many ingenious instruments have been contrived to exhibit and to explain them. One of the most instructive of these is the Gyroscope invented by M. Fessel of Cologne, described in its earlier form in Poggendorff's *Annalen* for September 1853, and which, with some improvements by Prof. Plücker and some further modifications suggested by myself, I take the present opportunity of bringing before the Royal Society.

It is thus constructed: a beam is capable of moving freely round a horizontal axis which is itself moveable round a vertical axis, so that the beam may move in any direction round a fixed point; at one end of the beam is fixed a horizontal ring which carries a heavy disc, the axis of rotation of which is in a line with the beam; at the opposite extremity is a shifting weight by means of which the equilibrium of the beam may be established or disturbed at pleasure.

If the beam be brought into equilibrium, and the disc be rapidly rotated, by means of a thread unrolled from its axis, it will be seen that the beam has no tendency to displace itself in any direction.



The Gyroscope.

Not so, however, if the equilibrium be in any way disturbed; on moving the weight towards the centre of the beam, thus causing the *disc* to preponderate, it will be observed that if the disc rotates from right to left the beam will move round the vertical axis also from right to left; and if the motion of the disc be reversed the rotation of the beam will be reversed also. On causing the *equipoise* to preponderate contrary effects will take place. The velocity of the rotation of the beam round the vertical axis increases in proportion to the disturbance of the equilibrium. It will also be observed that, notwithstanding the increased or diminished action of gravity on the disc, its axis of rotation always preserves the same inclination to the vertical axis at which it has been originally placed. The effect produced is a seeming paradox. When the equilibrium is disturbed while the disc is at rest, the beam being placed in any other position than the vertical, gravity acts so as to turn it round a horizontal axis; but when the disc is in motion the usual effect of gravity disappears, and there is substituted for it a continued rotation round a vertical axis, that is, round an axis perpendicular to the plane which contains the axes of the two original rotations.

A similar composition of forces takes place when the disc is caused to rotate while the equilibrium of the beam is maintained, by impressing on the beam a rotation round the vertical axis. When the disc rotates from right to left, the slightest pressure tending to produce rotation round the vertical axis in the same direction, causes the end of the beam carrying the disc to ascend, and a pressure in the opposite direction causes it to descend, that is, the beam is constrained to move round a horizontal axis perpendicular to the vertical plane which contains the two axes of impressed rotation, a case exactly analogous to the preceding. The beam ascends and descends in like manner, after rotation has spontaneously taken place round the vertical axis in consequence of the equilibrium being disturbed, whenever this rotation is any how accelerated or retarded; the disc rotating from right to left and its weight predominating, the rotation round the vertical axis is from left to right; accelerating the latter motion will cause the disc to descend, and retarding it will occasion it to ascend.

As the centre of gravity of the beam is below its point of suspension, even when equipoised it is in perfect equilibrium only when it is horizontal, consequently, if it be elevated above or depressed below this position it will endeavour to resume it, tending to produce in the two cases rotations in opposite directions round a horizontal axis; the rotation of the disc combined with this tendency gives rise, as in the other cases I have mentioned, to a continued rotation round the vertical axis. If the disc rotate from right to left, and the end of the beam carrying it be elevated above the horizontal position, the rotation round the vertical axis will be from right to left; if, on the contrary, the same end of the beam be depressed below the horizontal position, that rotation will be from left to right.

In all the experiments above mentioned the axis of the rotating disc has remained in the prolongation of the beam, but, by means of an internal ring moveable round a line perpendicular thereto, this axis may be placed at any inclination and at any azimuth with respect to it. Very obvious considerations show that the inclination of this axis should produce no difference in the character of the effects but merely in their intensity, since in any inclined position of the disc its rotation is resolvable into two others, one perpendicular to the beam, and the other, which is incapable of producing any effect, in a plane containing it. When the axis of the rotating disc is vertical and at right angles to the beam, no rotation on the vertical axis ought to take place in any case; but, contrary to this expectation, although the beam be horizontal and in perfect equilibrium, a motion round the vertical axis results, which is in opposite directions according as one or the other end of the axis of the disc is uppermost. It is, however, easy to see that this rotation is not owing to the same cause which gives rise to the phænomena hitherto considered, for whether it be accelerated or retarded no change is produced in the horizontal position of the beam; it is, in fact, occasioned by the friction of the pivots of rotation dragging the beam into a corresponding motion. Attention to this extraneous cause of rotation will explain numerous anomalies which present themselves in many of the instruments contrived to exemplify the phænomena of combined rotary motions. It is one of the advantages of Fessel's apparatus that the phænomena may be exhibited in their more important phases without being affected by this source of error.

We may form a clearer conception of these phænomena by first considering some simpler facts which do not appear to me to have been hitherto sufficiently attended to. For this purpose let the system of rings carrying the disc be removed from the rest of the apparatus, and by unfastening the tightening screw let the inner ring be allowed to move freely within the outer. Having set the disc in rapid rotation, hold the outer ring at the extremities of the diameter which is in the plane in which the axis of motion of the disc is free to move, then giving to the outer ring a tendency to rotation round that diameter, it will be observed that, in whatever position the axis is, it will fly to place itself in the fixed axis thus determined, and rotation will take place round it in the same direction. Considerable resistance is felt so long as the moveable axis is changing its position, but when once it coincides with the fixed axis the rotation of the external ring round its diameter is effected with facility. A slight alternate motion of the outer ring, tending to give to it rotations in opposite directions, will occasion a continued rotation of the moveable axis. The same result takes place when an endeavour is made to rotate the outer ring round an axis perpendicular to its plane. In all cases when the axis of the rotating disc is free to move in a plane, and the outer ring is constrained to rotate round a line in this plane, the moveable axis will place itself so as to coincide with that line, and so that the disc shall rotate in the same direction as the ring; if the fixed axis be in a different plane the moveable axis will assume permanently that position in its plane which approaches nearest to the former. The moveable axis is thus apparently attracted towards the fixed axis if the rotations are in the same direction, and repelled from it if the rotations are in opposite directions.

In the experiments just described the free and constrained axes of rotation intersect, but in Fessel's apparatus they are distant from each other. In the latter case the rule must be thus modified, that the free axis of rotation tends to place itself *parallel* to the constrained axis of rotation, or to as near a position thereto as possible. By this principle all the results manifested are easily explained. The beam being in equilibrium, a motion impressed on it round the vertical axis causes it to ascend or descend, because the axis of the rotating disc tends to place itself parallel to the vertical axis of rotation and so that the disc rotates in the impressed direction. When the equilibrium of the beam is destroyed, gravity tends to make it rotate round a horizontal axis; the axis of the disc endeavours to place itself parallel with that axis, but both being unchangeably at right angles to each other, the tendency to place itself there gives rise to a continued rotation. Other results with this apparatus, to which I have not yet adverted, are similarly explained. Fix the outer ring horizontally and loosen the inner ring, keeping them both however in the same plane; then, on moving the beam round the vertical axis, the axis of the rotating disc will immediately fly to place itself parallel thereto, with rotation of the disc in the impressed direction. The rings being placed in the vertical plane, the same result will take place if the beam be moved in a vertical plane, *i. e.* round a horizontal axis.

The following additional experiments may be made with the rings detached from the apparatus. The results are necessary consequences of what has been previously explained :—

1. Suspend, by means of a string, the outer ring at the extremity of a diameter perpendicular to the axis of the inner ring; and, having loosened the latter, place it at right angles to the former. On causing the disc to rotate, its axis will retain its original position; but if the slightest effort be made to turn the outer ring round the vertical line, the axis of the rotating disc will instantly fly into this position, and the disc will move in the same direction as that of the impressed rotation.

2. The horizontality of the loose inner ring being restored, if a weight be suspended from either end of the axis of the disc, that axis will, while it preserves its horizontal or any inclined position, revolve round the vertical line; the direction of the motion will change if either the weight be applied to the opposite end of the axis or the disc rotate in the opposite direction. If this rotation be arrested, gravity will immediately cause the weighted end of the axis to descend.

3. Clamp the rings together either in the same plane or at right angles to each other, and fasten a string, in the first case, at the extremity of a diameter coinciding with the axis of the inner ring, and in the latter case at the extremity of a diameter perpendicular thereto. Having set the disc spinning, if a rotation round the vertical line be given to the system the axis of the disc will ascend, carrying with it the disc and rings notwithstanding their weight, and, even when the impressed rotation has ceased to act, will continue to rotate in the same direction until the motion of the disc ceases.

In this note I have purposely avoided entering into the mathematical theory of the phænomena, my intention having been solely to describe the apparatus exhibited and to give an intelligible account of its effects. Those who wish to investigate the subject more profoundly, will find the best guide in the Astronomer Royal's essay on Precession and Nutation published in his Mathematical Tracts.

LXXXII. *Intelligence and Miscellaneous Articles.*

SECOND NOTE ON THE DECOMPOSITION OF WATER BY THE PILE. BY M. JAMIN.

IN the last communication[*] which I laid before the Academy, I announced that I was in possession of some new facts; I now bring forward one of them:—

I decompose water in a voltameter constructed in a particular manner, collect the hydrogen in a graduated receiver, and transfer this to a vessel filled with water, either pure or containing a few drops of nitric acid.

I place in the same vessel an exactly similar receiver, containing the same quantity of hydrogen gas disengaged by the action of sulphuric acid upon zinc. This done, I cut a platinum wire into two exactly equal parts, heat them in a spirit-lamp, and place them in the two receivers so as to establish a metallic communication between the gas

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[*] Phil. Mag., p. 298 of the present volume.

and the liquid. The volume of the gas disengaged by zinc undergoes no change in the course of several days; the volume of the gas produced by the pile diminishes rapidly. The following are the numbers furnished by the experiment:—

$4^h\,20^m$	$4^h\,25^m$	$4^h\,40^m$	$5^h\,5^m$	$5^h\,55^m$	12^h
84	80	77	70	66	50

The action was afterwards prolonged indefinitely, but the remainder of the gas was no longer absorbable; the gas is consequently composed of an absorbable portion, and of a portion which behaves like ordinary hydrogen. The absorbable portion sometimes amounted to three-fourths of the total volume, but most frequently it was less than half; occasionally no absorption took place.

To ascertain the relation existing between the formation of this absorbable gas and the current, I measured the intensity (of the latter) with a tangent compass, and reduced the results to the unities of time and intensity. I ascertained that the volume of hydrogen may attain a maximum or remain at a lower value; in the former case it was not absorbable, in the latter, absorption took place. We may say, therefore, that in order to disengage a volume of absorbable gas, we must employ more electricity than in the production of a similar volume of ordinary gas.

These facts may be interpreted in two ways; we may suppose that the hydrogen disengaged by the pile is produced in a peculiar molecular condition whilst taking the gaseous form under the influence of the electric current, nearly as in the case of the oxygen which is electrified and modified at the positive pole. This conclusion is the only possible one if the gases disengaged be chemically pure.

But if, contrary to the generally received opinion, the gas collected at the negative pole be a mixture of the two constituent elements of water, we should have in contact two bodies which may combine under the influence of the platinum wire to reconstitute water.

Further experiments must decide the question, but whatever solution may be arrived at, the fact is important, as from it we shall derive the knowledge either of a new state of hydrogen, or of a mode of decomposition of water by the pile, in which both gases are disengaged at the same time.—*Comptes Rendus*, March 6, 1854, p. 443.

HANSEN'S ELECTRO-MAGNETIC ENGRAVING MACHINE.

This machine is somewhat on the principle of the well-known planing machine. The drawing to be copied and the plate to be engraved are placed side by side, on the moveable table or lid of the machine; a pointer or feeler is so connected, by means of a horizontal bar, with a graver, that when the bar is moved, the drawing to be copied passes under the feeler, and the plate to be engraved passes in a corresponding manner under the graver. It is obvious that in this condition of things, a continuous line would be cut on the plate, and, a lateral motion being given to the bed, a series of such lines would be cut parallel to and touching each other, the feeler of course passing in a corresponding manner over the drawing. If, then, a means could be devised for causing the graver to act only when the point of the feeler passed over a portion of the drawing, it is clear we should get a plate engraved, line for line, with the object to be copied. This is accomplished by placing the graver under the control of two electro-magnets, acting alternately, the one to draw the graver from the plate, the other to press it down on it. The coil enveloping one of these magnets is in connexion with the feeler, which is made of metal. The drawing is made on a metallic or conducting surface, with a rosined ink or some other non-conducting substance. An electric current is then established, so that when the feeler rests on the metallic surface, it passes through the coils of the magnet, and causes it to lift the graver from the plate to be engraved. As soon as the feeler reaches the drawing, and passes over the non-conducting ink, the current of electricity is broken, and the magnet ceases to act, and by a self-acting mechanical arrangement the current is at the same time diverted through the coils of the second magnet, which then acts powerfully and presses the graver down. This operation being repeated until the feeler has passed in parallel lines over the whole of the drawing, a plate is obtained engraved to a uniform depth, with a fac-simile of the drawing. From this a type-metal cast is taken, which, being a reverse in all respects of the engraved plate, is at once fitted for use as a block for surface printing.—*Journal of the Society of Arts*; and *Athenæum*, June 17, 1854.

ON THE FORCE WHICH GOVERNS CHEMICAL ACTIONS.

BY M. WITTWER.

My object is to show that the force which governs chemical actions is completely analogous to those which rule the actions of light, heat, electricity and magnetism.

Physicists determine the force of an electric current by the magnetic energy excited or by the deviation of the magnetic needle. This deviation and Melloni's pile afterwards furnished them with the means of measuring radiating heat. In my turn, I think it possible, by the assistance of light and heat, to obtain the means of measuring chemical force, whether we have disengagement of light and heat in a combination, or an absorption of these matters in consequence of the separation of the chemical components. Bodies are either in a state of stable equilibrium, when their separation requires a continued application of light, &c., as for instance, chlorine gas absorbed by water; or in a state of unstable equilibrium, when they only require the instantaneous application of these agents to modify their condition (as for example, the explosive compounds); chemical reactions may therefore be expressed as functions of light, heat, &c. To arrive at the law which rules affinity when an atom of A combines with several atoms of B, I admit the three following laws:—

1. Two heterogeneous bodies exercise upon each other a chemical action which decreases as the inverse square of the distance.

2. The reciprocal action of two homogeneous bodies is of the same nature.

3. When an atom of A combines with several atoms of B, the latter arrange themselves in such a manner that the reciprocal action becomes a minimum.

When in a combination of two heterogeneous bodies the reciprocal action is represented by α, and the reciprocal action of the homogeneous atoms by β, taking n and m to signify the coefficients derived from the number of atoms B, r for the distance, and $f(w)$ for some function of heat, &c., we arrive at the following equation:—

$$\frac{m\alpha+n\beta}{r^2}=f(w).$$

As regards heat, I find *à posteriori*, that r is equal to the product of the mass of the combination and the specific heat; $f(w)$ is equal to the elevation of temperature. As to light, I have abstained from determining $f(w)$, because the fundamental experiments are still wanting.

I could only make use of *three* experiments to check the correctness of my calculation, because all others at present known only furnish two numbers of atoms of B in a series; or it was necessary to determine the latent heat by calculation, for which purpose the data are still wanting. The three series of which I have been able to make use are, the combinations of sulphuric acid with water determined by Favre and Silbermann, and those of hydrate of potash with arsenic and phosphoric acids determined by Graham.

To my great satisfaction I find that these experiments agree, as nearly as could be desired, with the formula which I have employed. I obtained the relative numbers which express the affinity of the bodies which were experimented upon. The affinity between the alkali and the arsenic acid is nearly equal to the affinity between this base and phosphoric acid, but the distance between the atoms of the base and those of the acid is 1 for arseniate of potash to $\sqrt{2}$ for the phosphate of potash. The affinity between sulphuric acid and water in comparison to that of these two salts is only as 208 : 860.—*Comptes Rendus*, April 17, 1854, p. 750.

NOTE ON THE MAGNETISM OF LIQUIDS. BY M. QUET.

M. Plücker's method of ascertaining whether a liquid be magnetic or diamagnetic does not always furnish very well-marked results, especially with diamagnetic fluids. It appears to me that this may be effected in a simple and direct manner by the following experiment.

A slender glass tube containing a long column of the fluid to be tried is placed between the poles of one of M. Ruhmkorff's electromagnets; it is placed perpendicularly to the line of the poles in an horizontal direction, and the commencement of the liquid index is brought very close to the polar pieces. As soon as the electromagnet is set in action by a galvanic current, if the column of fluid be magnetic, it advances the whole length of the polar pieces, and quickly attains a position of permanent magnetic equilibrium. When the galvanic current is stopped, the index returns and recovers its original position. The magnetic attraction is in this case shown by a very sensible effect, that is to say, by a displacement of four or five centimetres. This is supposing that the column of fluid is of sufficient length to produce the maximum of action, but very short columns may be employed.

When a diamagnetic fluid is to be tested, and the experiment is arranged as just described, the index is repelled by the electro-magnet; but in this case the movement is less energetic, and the index only moves to a short distance, as the diamagnetic action is weakened in proportion to the distance of the liquid. If it be desired to give the experiment a very marked character, and even as distinct as for magnetic attraction, the index is to be brought between the polar pieces so as to cover their whole length; then, as soon as the electric current passes, the liquid is strongly repelled, and does not stop until it has passed the polar pieces to a greater or less distance. This movement, which may be produced to the extent of four or five centimetres, can leave no doubt as to the consequences of the experiment.

The sensibility of this kind of magnetoscope is sufficient to enable one to recognize the diamagnetic property of water, even when employing only a single Bunsen's element of middling energy.

With eight Bunsen's elements, a solution of chloride of manganese remains in a position of permanent magnetic equilibrium, even when the tube is made to slide rapidly in the direction of its axis.

If the pressure of the air be increased on one of the extremities of the fluid column, the index retrogrades between the polar pieces, and it may thus be brought to a position of unstable equilibrium. The excess of pressure which I have employed to produce this effect was about $\frac{1}{140}$th of an atmosphere. This number shows the magnetic energy of the chloride of manganese, and at the same time that the magnetoscope may be readily converted into an apparatus of measurement.

As a last instance of the sensibility of this method, I will mention that the solution of chloride of manganese diluted with one hundred times its volume of water still retains appreciable magnetic properties; but if it be diluted with twice this quantity, the magnetism of the salt is concealed, the diamagnetism of the water becoming predominant,

Theory shows that the elevation of a fluid in a capillary tube is in an inverse ratio to its weight. Hence we may believe that magnets may exert an appreciable influence upon the capillary elevation of liquids. This I have verified. One of the branches of a capillary siphon is placed between the poles of an electro-magnet; the capillary surface of a magnetic liquid is brought a little below the polar pieces and the electric current is then passed; the capillary surface immediately rises to a new position of equilibrium. With a diamagnetic fluid, of which the capillary surface is brought to a level with the poles, we get a depression of the column.—*Comptes Rendus*, March 20, 1854, p. 562.

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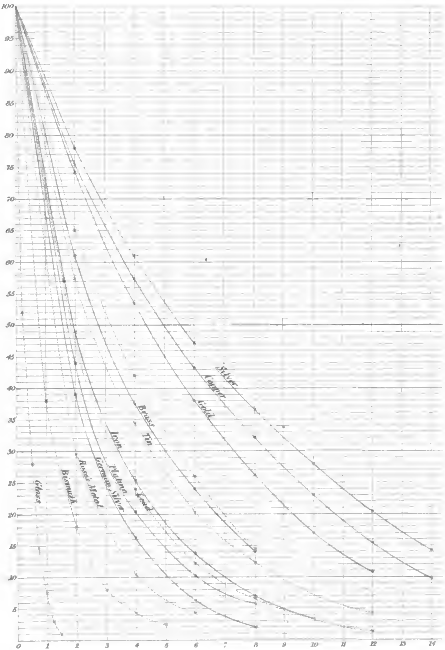
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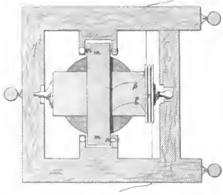

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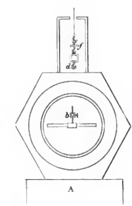

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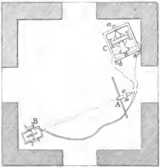

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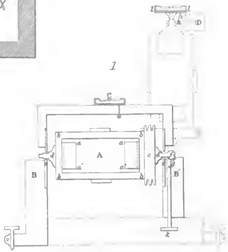

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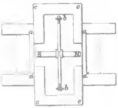

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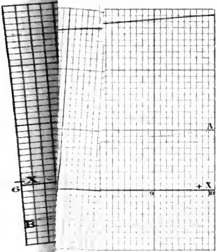

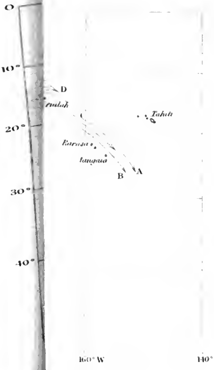

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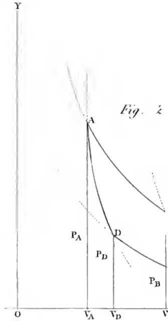

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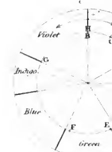

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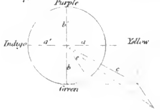

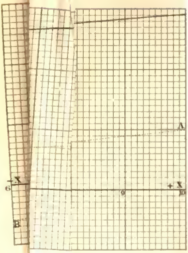

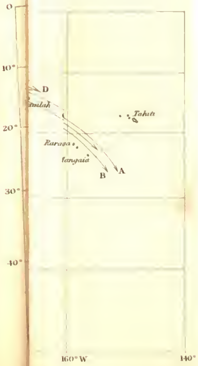

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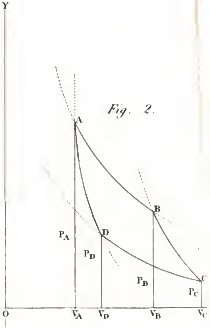

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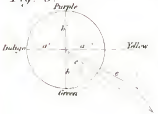

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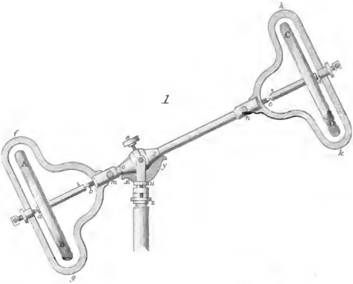

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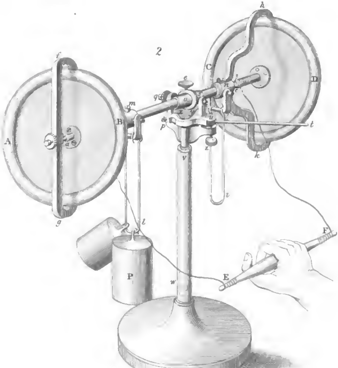

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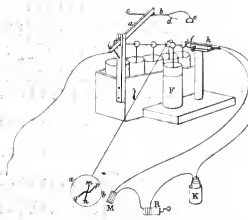

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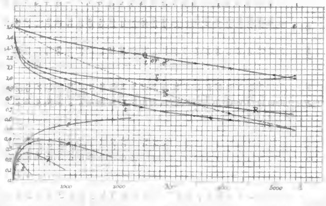

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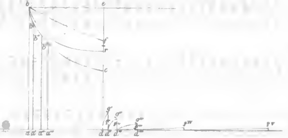

J.Basire, sc.