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

VOL. V.—FOURTH SERIES.
JANUARY—JUNE, 1853.

"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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PLATES.

ERRATA IN VOL. IV.

P. 305, *for* last line *read*

$$\iiint dx\, dy\, dz \cdot \int_0^T c\, dt\; \epsilon^{\frac{-1}{J}\int_0^t \mu dt} = \iiint dx\, dy\, dz \int_0^t c\, dt \cdot \epsilon^{\frac{-1}{J}\int \mu dt}.$$

— 306, in line 4, before the last clause *insert*, and if the specific heat of any part of the body does not vary with its temperature,

— 306, *for* line 5 *read*

$$T = \epsilon^{\dfrac{\iiint \log(t+\alpha) c\, dx\, dy\, dz}{\iiint c\, dx\, dy\, dz}} - \alpha.$$

— 416, line 11 from bottom, *for* influences *read* instances.

ERRATUM IN VOL. V.

Page 111, line 15, *for*

$$F_B - F_C = \int_{Q_2}^{Q_2} \frac{d(Q + f(Q))}{Q} \quad read \quad F_C - F_B = \int_{Q_2}^{Q_1} \frac{d(Q + f(Q))}{Q}.$$

I. *On the Œconomical Production of Mechanical Effect from Chemical Forces.* By J. P. JOULE, *F.R.S. &c.**

PERHAPS the most important applications of dynamical theory are those which refer to the production of motive power from chemical and other actions. To point out the rules for constructing an engine which shall approach perfection as nearly as possible, and to determine the quantity of work which ought to be evolved by a perfect engine of any given class, are objects of the greatest consequence in the present state of society, and which have in fact been to a great extent already accomplished by the labours of those who have taken a correct view of the nature of heat. I intend on the present occasion to submit to the Society some of the laws which have been recently arrived at by Professor Thomson and myself, and to offer some hints as to the means of carrying out into practice the deductions of theory.

Engines which derive their power from the operation of chemical forces may be divided into three classes. The first class comprises those exquisite machines in which chemical forces operate by the mysterious intervention of life, whether in the animal or vegetable creation. The second class includes machines in which the chemical forces act through the intervention of electrical currents, as in the ordinary revolving electro-magnetic apparatus. The third comprises those engines in which the chemical forces act through the intervention of the heat they

produce; these, which may be termed thermo-dynamic engines, include steam-engines, air-engines, &c.

The process whereby muscular effort is developed in the living machine is, as might be expected, involved in great obscurity. Professor Magnus has endeavoured to prove that the oxygen inspired by an animal does not immediately enter into combination with the blood, but is mechanically conveyed by it to the capillary vessels within the muscles, where it combines with certain substances, converting them into carbonic acid and water. The carbonic acid, instead of oxygen, is then absorbed by the blood, and is discharged therefrom when it reaches the lungs. Taking this view, we may admit with Liebig, that at each effort of an animal a portion of muscular fibre unites with oxygen, and that the whole force of combination is converted by some mysterious process into muscular power, without any waste in the form of heat. This conclusion, which is confirmed by the experiments related in a joint memoir by Dr. Scoresby and myself, shows that the animal frame, though destined to fulfill so many other ends, is as an engine more perfect in the œconomy of *vis viva* than any human contrivance.

The electro-magnetic engine presents some features of similarity to the living machine, and approaches it in the large proportion of the chemical action which it is able to evolve as mechanical force. If we denote the intensity of current electricity when the engine is at rest by a, and the intensity of current when the engine is at work by b, the proportion of chemical force converted into motive force will be $\frac{a-b}{a}$, and the quantity wasted in the form of heat will be $\frac{b}{a}$. Now from my own experiments, I find that each grain of zinc consumed in a Daniell's battery will raise the temperature of a lb. of water $0°\cdot 1886$; and that the heat which can increase the temperature of a pound of water by one degree, is equal to the mechanical force which is able to raise a weight of 772 lbs. to the height of one foot, or according to the expression generally used, to 772 foot-pounds. Therefore the work developed by a grain of zinc consumed in a Daniell's battery is given by the equation

$$W = \frac{145 \cdot 6(a-b)}{a}.$$

We now come to the third class of engines, or those in which the chemical forces act through the intervention of heat. In the most important of these the immediate agent is the elasticity of vapour or permanently elastic fluids. In a very valuable paper on the dynamical theory of heat, Professor William Thomson

has demonstrated that if the heat evolved by compressing an elastic fluid be equivalent to the force absorbed in the compression, the proportion of heat converted into mechanical effect by any perfect thermo-dynamic engine will be equal to the range of temperature divided by the highest temperature from the absolute zero of temperature. Therefore, if in a perfect steam-engine a be the temperature of the boiler from the absolute zero, and b be the absolute temperature of the condenser, the fraction of the entire quantity of heat communicated to the boiler which will be converted into mechanical force, will be represented by $\dfrac{a-b}{a}$, which is analogous to the fraction representing the proportion of chemical force converted into mechanical effect in the electromagnetic engine*. The extreme simplicity of this very important deduction which Professor Thomson has drawn from the dynamical theory of heat, is of itself a strong argument in favour of that theory, even if it were not already established by decisive experiments.

Now, estimating the heat generated by the combustion of a grain of coal at $1°\!\cdot\!634$ per lb. of water, its absolute mechanical value will amount to $1261\!\cdot\!45$ foot-pounds; hence, according to Professor Thomson's formula, the work performed by any perfect thermo-dynamic engine will, for each grain of coal consumed, be represented by the equation

$$W = \frac{1261\cdot45 \; (a-b)}{a},$$

which applies, as before intimated, not only to air-engines, but also to those steam-engines in which the principle of expansion is carried to the utmost extent, providing always that no waste of power is allowed to take place in friction, and that the entire heat of combustion of the coal is conveyed to the boiler or air-receiver.

Professor Thomson was the first to point out the great advantages to be anticipated from the air-engine, in consequence of the extensive range of temperature which it may be made to possess; and in a paper communicated to the Royal Society soon afterwards, I described a very simple engine which fulfills the criterion of perfection according to Professor Thomson's formula. This engine consists of three parts, viz. a condensing air-pump, a receiver, and an expansion cylinder; the pump

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* Referring to this analogy, Professor Thomson writes as follows:—" I am inclined to think that an electric current circulating in a closed conductor *is heat*, and becomes capable of producing thermometric effects by being frittered down into smaller local circuits or ' molecular vortices.' "— Letter to the Author, dated March 31, 1852.

forces atmospheric air into the receiver, in the receiver its elasticity is increased by the application of heat, and then the air enters the expansion cylinder, of which the volume is to that of the pump as the absolute temperature of the air in the receiver is to that of the air entering it. The cylinder is furnished with expansion gear to shut off the air, when the same quantity has been expelled from the receiver as was forced into it by one stroke of the pump. By this disposition the air is expelled from the expansion cylinder at the atmospheric pressure, and at the absolute temperature corresponding with b in Professor Thomson's formula.

As an example of the above kind of air-engine, I will take one working in atmospheric air of 15 lbs. pressure on the square inch and 50° Fahr. I will suppose that the expansive action in the cylinder is to exist through three-fourths of its length. Then as the action of the compressing pump is the reverse of that of the cylinder, the piston of the former must traverse three-fourths of its length before the air is sufficiently compressed to enter the receiver by its own pressure. The temperature of the air entering the receiver, determined by

Poisson's equation $\dfrac{t'}{t} = \left(\dfrac{V}{V'}\right)^{k-1}$, will be 439°·59 Fahr., and its

pressure will be 105·92 lbs. on the square inch. Supposing now that the volume of the cylinder is to that of the pump as 4 to 3, the density of the air in the receiver to that forced into it by the pump must be as 3 to 4, in order to keep the quantity of air in the receiver constant. The temperature of the air in the receiver will also require to be kept at 739°·12 Fahr. in order to maintain the pressure of 105·92 lbs. on the square inch. The air entering the cylinder at the above pressure and temperature will escape from it at the end of the stroke at the atmospheric pressure, and at the temperature 219⅔°.

It will be remarked that there are two ranges of temperatures in the engine I have described, viz. that of the pump, and that of the cylinder. Owing, however, to the exact proportion which subsists between the two, the same result is arrived at by the application of Professor Thomson's formula to either of them. Taking, therefore, the range of the cylinder, and converting the temperatures of the air entering and discharged from the cylinder into the absolute temperatures from the real zero by adding to them 459°, we obtain for the work evolved by the consumption of a grain of coal,

$$W = \frac{1261 \cdot 45 \, (1198 \cdot 12 - 678 \cdot 66)}{1198 \cdot 12} = 546 \cdot 92 \text{ foot-pounds.}$$

In order to compare the foregoing result with the duty of a steam-engine approaching perfection as nearly as possible, I will admit that steam may be safely worked at a pressure of 14 atmospheres. The temperature of the boiler corresponding to that pressure will, according to the experiments of the French Academicians, be 387° Fahr. The temperature of the condenser might be kept at 80°. Reducing the above to temperatures reckoned from the absolute zero, we obtain for the work evolved by the combustion of each grain of coal,

$$W = \frac{1261 \cdot 45 \ (846 - 539)}{846} = 457 \cdot 76 \text{ foot-pounds.}$$

It would therefore appear, even in the extreme case which I have adduced, that the performance of the steam-engine is considerably inferior to that of the air-engine. The superiority of the latter would have been still more evident had I also taken an extreme case as an illustration of its œconomy. It must, moreover, be remarked that the heated air escaping from the engine at a temperature so high as $219\frac{2}{3}°$ might be made available in a variety of ways to increase still more the quantity of work evolved. A part of this heated air might also be employed in the furnaces instead of cold atmospheric air.

We may also hope eventually to realize the great advantage which would be secured to the air-engine by causing the air, in its passage from the pump to the cylinder, to come into contact with the fuel by the combustion of which its elasticity is to be increased. It appears to me that the air might pass through a number of air-tight chambers, each containing ignited fuel, and that whenever any one of the chambers required replenishing, its connexion with the engine might be cut off by means of proper valves, until by removing an air-tight lid or door the chamber could be filled again with fuel. By means of suitable valves, it would be easy to regulate the quantity of air passing through each chamber so as to keep its temperature uniform ; and by a separate pipe, furnished also with valves, by which the air could be carried from the pump to the upper part of the chambers without traversing the fuel, the engine man would be enabled to keep the temperatures of the chambers, as well as the velocity of the engine, under proper control.

II. *On the Mechanical Effect of Heat and of Chemical Forces.
By* W. J. Macquorn Rankine, *F.R.S.E. &c. In a Letter
to* J. P. Joule, *F.R.S. &c.**

59 St. Vincent Street, Glasgow,
December 11, 1852.

My dear Joule,

I RETURN you my best thanks for the papers you have
sent me.

In that on the " Œconomical production of Mechanical Effect
from Chemical Forces," I am struck in particular with the ana-
logy you have pointed out between the functions representing
the fractions of the motive power converted into mechanical effect
in the electro-dynamic and thermo-dynamic engines respectively,
both being capable of being expressed by the formula

$$\frac{a-b}{a}.$$

With respect to the nature of the functions a and b in the case
of electricity, it is evident that they are proportional, as you have
stated[†], to the strengths of the current measured when the
engine is at rest and in motion. In fact, ab being the total
power expended, b^2 is converted into heat, and $ab-b^2$ remains
for the useful effect.

As to the nature of these functions in the case of *heat*, they
are proportional to the absolute quantities of thermometric heat
contained in the body at the temperatures of heating and cooling
respectively.

If we make no definite hypothesis respecting the kind of *vis
viva* which constitutes thermometric heat, then a and b are func-
tions of the temperatures, the nature of which has to be deter-
mined empirically; and this view constitutes the basis of the
theory of Carnot as modified by Clausius and Thomson.

If we assume the principle (with Mayer) that the specific heat
of a perfect gas is constant at all temperatures, or the necessary
consequence of this assumption, that the heat developed by
compressing a perfect gas is the exact equivalent of the power
employed, then the functions a and b become simply proportional
to the absolute temperatures as measured by a perfect gas ther-
mometer. I see you have adopted this view in your investigations
on the air-engine.

If we adopt, respecting the nature of the *vis viva* which con-

* Communicated, with the permission of the Author, by Mr. J. P. Joule.
† " Experiments and Observations on the Mechanical Powers of Electro-
magnetism, Steam, and Horses," by Messrs. Scoresby and Joule, Philoso-
phical Magazine, S. 3. vol. xxviii. p. 451, 1846.

stitutes thermometric heat, the hypothesis which I have called that of molecular vortices, that is to say, if we suppose thermal elasticity to arise from the centrifugal force of molecular revolutions, I have shown that the functions a and b are proportional to the absolute temperatures diminished by a constant which is yet unknown, but which must be the same for all substances; being, in fact, the absolute temperature which corresponds to total privation of heat; in other words, the distance between the absolute zero of gaseous elasticity and the absolute zero of heat. This is expressed by the formula

$$\frac{a-b}{a} = \frac{\tau_a - \tau_b}{\tau_a - \kappa},$$

where τ_a and τ_b are the absolute temperatures, and κ the constant in question. If we assume that this constant is either null or inappreciably small compared with the temperatures at which machines usually work, we obtain the formula

$$\frac{\tau_a - \tau_b}{\tau_a},$$

which you have employed in your investigations.

With respect to the value of the constant κ, the only existing data which throw any light upon it are the experiments just published by Thomson and yourself on the heat produced by the friction of air; and even they, so far as they have yet gone, are sufficient to prove only that it is exceedingly small. When carried out on a larger scale (and especially with carbonic acid gas, for reasons which I shall point out), I have no doubt they will furnish the means of determining it exactly.

I am now deducing the theoretical results of these experiments according to the hypothesis of molecular vortices; and though my calculations are not yet complete, I think it desirable to let you know the results so far as I have gone.

The mechanical value of the heat which disappears in the first instance in expanding a substance from the volume V_1 to the volume V_2 at a *constant temperature,* is represented by

$$(\tau - \kappa) . \int_{V_1}^{V_2} \frac{dP}{d\tau} \, dV.$$

The power developed by the expansion, which in your experiments is reconverted into heat by friction, is

$$\int_{V_1}^{V_2} P \, dV.$$

So that if K_V be the dynamical specific heat of the body at constant volume, the final cooling effect is

$$-\Delta\tau = \frac{1}{K_V}\left\{ \tau\int_{V_1}^{V_2}\left(\frac{dP}{d\tau} - \frac{P}{\tau}\right)dV - \kappa\int_{V_1}^{V_2}\frac{dP}{d\tau}dV \right\}.$$

If the substance were a perfect gas, then

$$\frac{dP}{d\tau} = \frac{P}{\tau},$$

and the first integral would be null. But no existing gas is perfect, and $\frac{dP}{d\tau}$ is always greater than $\frac{P}{\tau}$. I have therefore, in the first instance, calculated for the pressures and temperatures in your experiments the values of the first term of the above formula (which represents, in fact, the heat expended in overcoming the cohesion of the gas) according to the formulæ which I deduced in 1849 from the experiments of M. Regnault; and so far as I have gone, the result has been that the first term of the above equation *always exceeds the actual cooling by a fraction of a degree.* These two quantities vary in a similar manner; that is to say, they both increase with increased pressure, and diminish rapidly with increased temperature.

The excess of the calculated cooling by cohesion above the actual cooling, if freed from the effects of errors, would represent the second term of the above equation, and afford the means of computing the value of the constant κ; but it is both too small and too irregular in its amount to serve for this purpose further than to show that κ has probably some value, but certainly a very small one.

I would now suggest to you the advantage of using carbonic acid gas in some future experiments, because it is a definite chemical compound, and therefore not subject to variation in its properties; and also because in it the effect of cohesion is greater than in atmospheric air, and capable of being calculated with greater precision. The formula representing the pressure of carbonic acid in terms of its volume and temperature is very simple, and agrees very closely with Regnault's experiments. It is of this form :

$$\frac{P}{P_0} = \frac{D}{D_0}\cdot\frac{\tau}{\tau_0} - \left(\frac{D}{D_0}\right)^2\cdot\left(\frac{a}{\tau} - b\right).$$

τ_0 is the absolute temperature of melting ice, D_0 the *theoretical* density in the state of perfect gas at the absolute temperature

τ_0 and pressure P_0; P, D, τ the actual pressure, density, and absolute temperature, and a and b two constants*.

The numerical value of the absolute temperature of melting ice is of much importance in researches on heat. It is usual to employ an approximate value deduced from the coefficient of expansion of air, such as 491° Fahr. or 273° Cent.; but on comparing a series of such approximate values (which are all too small) deduced from Regnault's experiments on air and carbonic acid at progressively diminishing pressures, it appears that as the density diminishes, those values tend towards

$$274°{\cdot}6 \text{ Cent.} = 494°{\cdot}28 \text{ Fahr.};$$

which is the true absolute temperature of melting ice, correct (as I believe) to a small fraction of a degree.

I may add, that the cooling effect of cohesion in carbonic acid gas, as calculated from the formulæ I have given, varies directly as the difference between the initial and final *densities*, and inversely as the following function of the absolute temperature:—

$$\frac{2a}{\tau^2} - \frac{b}{\tau}.$$

So that at a certain temperature it disappears, and a heating effect commences, which with increase of temperature gradually becomes insensible. The law for atmospheric air is much more complicated.

I beg you will excuse the great length of this letter, which has arisen from my anxiety to explain to you fully my reasons for recommending carbonic acid as a subject for experiments on the thermal effects of currents, and for adopting the value I have mentioned for the absolute temperature of melting ice.

Until further experiments are made, I think that for practical purposes we may neglect the constant κ.

<div align="center">I am, yours very truly,</div>

J. P. Joule, Esq.   W. J. Macquorn Rankine.

Dec. 24.—On completing the reduction of the experiments referred to above, I have found the results, especially where large quantities of air were employed, to agree better than I at first anticipated. They seem to indicate that the value of κ is somewhere about 2° Centigrade.—W. J. M. R.

* The details of this and similar formulæ are contained in the 21st, 25th, and 26th Articles of a paper "On the Centrifugal Theory of Elasticity," Philosophical Magazine, December 1851.

III. *On the Heat of Chemical Combination.*
By THOMAS WOODS, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,   Parsonstown, Dec. 13, 1852.

IN the last Number of this Magazine Dr. Andrews disputes my right to the first publication of the proof, that "the decomposition of a compound body occasions as much cold as the combination of its elements originally produced heat." He quotes a paper he published in this Magazine in 1844, and one in the Philosophical Transactions for 1848, wherein he *"assumes"* the truth of this proposition from the fact, that when one base displaces another, a quantity of heat is absorbed equivalent to that produced by the combination of the former. On this account he thinks he has a right by prior publication to the discovery.

Now if such an assumption can be looked on in this light, the merit of the discovery should, I believe, be given to Hess, because so far back at least as 1841 he applies the knowledge of heat lost by the withdrawal of a base to account for the circumstance that no heat is developed in double decomposition,—this is what he calls his principle of *thermo-neutrality.* But supposing that Dr. Andrews was the first to direct attention to this point, I must still be excused by him for saying, that, assuming the truth of an important and constantly operating principle from the agreement of one class of facts is not more than conjecture; it certainly cannot be said to be a proof. I must, however, say that, until Dr. Andrews directed my attention by his letter to the papers he there refers to, I was not aware that he or any other person had in any way alluded to the subject. If I had known of the papers, I would have quoted them in mine. I was only acquainted with the *results* of Dr. Andrews's very valuable labours. The theory I had formed of the cause of the heat of chemical combination, required me to prove that the absorption of heat by decomposition is equal to its production by combination, and this I attempted to do without knowing of, or being able to refer to, the experiments of others, my library being very limited. Through the kindness of the Earl of Rosse, whose library is just the reverse of mine, I have been since then enabled to supply my wants. I offer this excuse to Dr. Andrews and others for not having noticed what had been said on the subject previously to my paper; but at the same time I still maintain, after having made every reference to what others have done, there is nothing to prevent me saying, that, by right of prior publication, I am the first who proved the general prin-

ciple, that "the decomposition of a compound body occasions as much cold as the combination of the elements originally produced heat."

Mr. Joule, in the introduction to his paper "On the Heat disengaged in Chemical Combinations," published in this Magazine last July, after noticing that I had proved the definite production of cold by decomposition, adds, that previous to the year 1843 he had demonstrated "that the heat rendered latent in the electrolysis of water is at the expense of the heat which would otherwise have been evolved in a free state by the circuit." But his conclusions in no way interfere with mine, for he ascribes the absence of heat to a cause quite different from absorption by decomposition. He reasons thus, if I understand him rightly :—A current of a certain intensity passing through a conductor with resistance produces a certain amount of heat. The same intensity of current ought equally to affect water and a solid conductor; but he proves less heat is produced by it in water; and ascribes as the reason, that a certain amount of the intensity is used up in overcoming the obstruction offered by the decomposition, and the residue of the current is alone concerned in producing the heat. Now this explanation differs altogether from that which ascribes to the decomposition itself the absorption of the heat. The latter is the principle I endeavoured to establish in the paper published in this Magazine in October 1851, and by a similar process to mine, which Mr. Joule himself proves in the Number for July 1852.

If it be true that decomposition occasions as much cold as combination of the same elements produces heat, is not Mr. Joule's theory of the heat of combination being due to resistance to electric conduction disproved?

I have the honour to be, &c.,

Thomas Woods.

IV. *Note on Combinations.*
By *the* Rev. Thomas P. Kirkman, *A.M.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE *puzzle* of the fifteen young ladies now takes rank as a case of a mathematical *problem*, of no common interest, from the analysis developed in its treatment. Mr. Anstice has shown, in the Cambridge and Dublin Mathematical Journal, how this kind of question may be brought under the mastery of

algebra, and has given the rule for the like arrangement of $12n + 3$ symbols, whenever $6n + 1$ is a prime number. It thus appears, that all the results which are indicated by the analysis of Mr. Spottiswoode in this Journal of May last, and which were anticipated, rather than affirmed by him, are certainly true.

The arguments adduced by me (page 526, vol. iii.) to show that a similar arrangement of 21 and of 33 symbols is impossible, while they are, as I conceive, conclusive for those numbers, are not tenable for the case of $3n$, when n is any prime greater than 11; for the reason that the triads, required for such arrangement, are fewer than those which can be made with n things. I would beg leave to add the following to the theorems which I have already had your permission to enunciate.

If r *is a prime number,* $(r^a - 1):(r - 1)$ *symbols can be arranged in* $(r + 1)$-*plets till every duad is once and once only employed.*

If r, $(r^a - 1):(r - 1)$, $(r^b - 1):(r - 1)$, &c. *be prime numbers,*

$$(r + 1) . \left(\frac{r^a - 1}{r - 1} \right)^p . \left(\frac{r^b - 1}{r - 1} \right)^q . \ldots \left(\frac{r^c - 1}{r - 1} \right)^r \text{ symbols can be ar-}$$

ranged in $(r + 1)$-*plets till every duad has been once and once only employed, whatever be the number and the powers of the prime factors.*

Croft, near Warrington,
    December 3, 1852.

V. *Researches on the Conductibility of Minerals for Voltaic Electricity.* By Elie Wartmann, *Professor of Natural Philosophy in the Academy of Geneva*[*].

IN examining the conductibility of crystals, the author, as his title indicates, has made use of voltaic electricity, the effects produced being measured by a good galvanometer introduced into the circuit. If the mineral appeared to be an insulator, the intensity of the current was augmented until a battery consisting of ten large couples of Daniell was applied. In the contrary case the current was moderated to the required degree by means of a rheostat.

The purity of the minerals exercises a great influence on their conductibility; the author therefore employed, in all possible cases, well-determined crystals. All the experiments were made in winter, in a room whose temperature varied from 14° to 17° Cent., between which limits the conductibility of the substance is not sensibly altered. Every experiment was repeated several

times, and with several specimens, the surfaces having been previously purified with care.

Three hundred and nineteen species have been submitted to direct examination by the author. The comparison of his results with those of previous experimenters shows in general a satisfactory coincidence. Where divergences exhibited themselves, they are to be referred to the variety of structure resulting from difference of locality, and without doubt also to the fact of his having made use of voltaic instead of machine electricity.

The influences resulting from differences of structure are exhibited by the experiments of M. Riess on the conductibility of the sulphuret of antimony, native and artificial. These observations have been confirmed by those of Professor Faraday, and more recently by MM. Karsten and Munck of Rosenschöld, who have found among other peculiarities, that native crystals of realgar procured from Nagy-Ag are good conductors, while M. Hausmann ranks the substance among the semi-conductors, and M. Pelletier among the insulators. Sulphuret of zinc, in pieces or in powder, is a conductor or an insulator according as it is prepared in the dry or the humid way. Does not this explain the divergence of the results of M. Hausmann from those of Mr. Fox with reference to blende? Black sulphuret of mercury very pure conducts well, while cinnabar is a perfect insulator. The other sulphurets exhibit the same peculiarities.

The diversity of electric propagation, according as it comes from the pile or the machine, is rendered manifest by a crowd of experiments. MM. Hausmann and Henrici distinguish as semi-conductors a great number of substances, such as carbonate and sulphate of lime, which with current electricity are perfect insulators. The author has repeated the greater number of their experiments and found them correct. This remark will be sufficient to explain certain divergences which exhibit themselves between their results and those of Mr. Fox, as also the complete coincidence (if we except the sulphurets of bismuth and silver) of his results and those of the English physicist.

There are two aspects of the subject, the study of which would be very interesting, namely, the relative conductibilities of non-insulating minerals of the same dimensions and subjected to the same conditions; and the variation of this property in different directions through the same crystal. Unhappily difficulties almost insurmountable lie in the way of such an examination. The manner of contact with the surfaces at the entrance or the exit of the current can augment or diminish the deviation of the rheometer two or threefold. The author has made numerous experiments with an insulated platinum forceps, between which the crystal was squeezed by means of a vice. But the fraction of a revolution of the latter, more or less, caused such alterations

in the conductibility that no satisfactory result could be obtained*. The author tried to moisten the surfaces of the platinum with a conducting liquid, and in some cases the readings were by this treatment rendered more constant. But in most cases he only succeeded in polarizing the forceps, or in developing electro-chemical phænomena which masked the result sought after.

These obstacles became still more numerous in examining the variations of conductibility in the same mineral. The conducting substances which belong to other systems than the regular are not numerous. The crystals to be obtained are nearly always very small, and their texture rarely homogeneous. The natural faces are in some cases covered by an insulating crust, although of a fine polish. M. Wartmann has found some crystals perfect conductors, and others of the same appearance which arrested the most energetic currents, until by the continued friction of a very hard body the surface was abraded. Some very beautiful crystals of oxide of tin have proved themselves conductors along their edges, and here and there on their faces, but everywhere else they were insulators. Finally, the variable adherence of the surfaces of cleavage sometimes modifies the conductibility in the most capricious manner. These difficulties have obliged MM. Wiedemann and Senarmont to have recourse to the super-ficial distribution of machine electricity, and to determine in a manner less direct the sense of greater or less conduction.

The experiments which M. Wartmann made in connexion with this point were executed by passing a current simultaneously round two galvanometers of Ruhmkorff. The crystal was clasped in one direction between the jaws of a forceps, which latter was connected with one of the galvanometers; in the perpendicular direction it was clasped between a second pair of forceps which were connected with the other galvanometer. Multiplied experiments have shown that manganite (*acerdèse*) conducts much better parallel to the axis than in the perpendicular direction. Plates of sulphuret of molybdenum from Vallais exhibited the opposite de-portment, which appears to the author to be sufficiently pro-nounced in certain crystals of arseniate of iron also.

The results of M. Wartmann's investigation, similar in many respects to those of MM. Hausmann and Henrici, are as follows:—

1. The conducting minerals belong to five primitive crystalline types. None are found among the twelve species of the system represented by the oblique unsymmetrical prism.

2. Minerals present all intermediate degrees between perfect conductibility and perfect insulating power.

3. Native metals and their alloys are conductors.

* Might not the difficulty be combated by enclosing the crystal between two columns of mercury which should press uniformly against its surfaces, and from which wires might proceed to the galvanometer?—Ed.

4. Among the metallic oxides great differences of conductibility exist. Those which are opake and endowed with lustre, are in general better conductors than the others. M. Necker has noticed this circumstance.

5. The same remark applies to the metallic sulphurets.

6. The chlorides are in part conductors and in part insulators.

7. The same variety exists among the salts. The great majority of them are insulators.

8. The molecular state determines the conductibility or non-conductibility of the same substance. Diamond insulates, graphite conducts well.

9. Among the minerals of vegetable origin, the more perfect the carbonization the better the conductibility.

10. Among the conducting minerals which do not crystallize regularly, some present differences of conductibility when the direction of the current through the mass is varied.

Professor Wartmann has recently made some experiments with the electric light*, with a view of testing its applicability as a means of public illumination. To render the light constant, an instrument constructed by Duboscq for the adjustment of the coal points was made use of. With a pile of 50 couples of Bunsen he obtained a light intolerable to the naked eye; his assistant, indeed, by incautiously looking at the light, had his eyes inflamed to such a degree as to render it necessary to bathe them several times with warm water before they could be opened. In the first experiment the flame of a candle and that of a jet of gas cast very distinct shadows upon a screen. At a distance of 250 metres the light reflected from a mirror competed successfully with that of a jet of gas.

A second experiment was made under conditions still more decisive. In this experiment the descending rain and the absence of houses deprived the light of all auxiliary splendour. Nevertheless on concentrating its rays on a slightly divergent cone, by means of a concave glass mirror 0·4 of a metre in diameter, it was found that the light was equal to that of 300 large gas-jets at least. At 100 metres, notwithstanding the rain, it was possible to read distinctly. The experiment was made before the hydraulic engine-house in Geneva, and from the higher windows of the hotel and the neighbouring houses the persons taking part in the experiment could be recognized. The light was cast upon the façades of the quay, and its reflexion was

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* Note sur quelques Expériences faites avec le Fixateur Electrique, par M. le Prof. Elie Wartmann.—*Bibliothèque Universelle,* Aug. 1852.

seen at *Bursin sur Rolle,* a distance of 16,000 metres.   In public illumination, however, on account of the turnings of streets and lanes, it is necessary to multiply the illuminating points; and it has ever been an objection to the electric light, that its illumination can only be local.   The Professor has, however, assured himself that more than one pair of coal-points may be introduced into the self-same circuit, and a light intolerable to the eye obtained at each place of interruption.   Possibly by an extension of this principle, and the introduction of suitable commutators for opening and closing the circuit, the light may be made available for domestic purposes.   Not only has the light been found sufficient to produce a portrait on a Daguerreotype plate, but Prof. Wartmann has proved that even the image of the green portions may be obtained.

VI. *On the Optical Figures produced by the Disintegrated Surfaces of Crystals. By* Sir DAVID BREWSTER, *K.H., D.C.L., F.R.S.*[*]

[With Five Plates.]

THERE is no branch of natural science about which we know so little as that which relates to the structure of crystalline bodies.   By assuming the form of an integrant molecule, crystallographers have found no difficulty in building those geometrical solids which minerals and artificial crystals present to our observation.   They conceive that these molecules unite by their homologous sides in the formation of the primitive crystal, and by supposing that they arrange themselves in plates on the faces of that crystal, each plate successively diminishing in size by the abstraction of a certain number of these molecules in lines of a given direction,—all the secondary forms of the crystal may be easily deduced.

In place of employing, as Hauy has done, integrant molecules having the form of a tetrahedron, a triangular prism, and a parallelopiped, others have suggested the more philosophical idea of constructing crystals out of spheroidal elements, including, of course, the sphere by which the oblate passes into the prolate solid.   But in whatever way crystallographers shall succeed in accounting for the various secondary forms of crystals, they are then only on the threshold of their subject.   The real constitution of crystals would be still unknown; and though the examination of these bodies has been pretty diligently pursued, we can at this moment form no adequate idea of the complex and beautiful organization of these apparently simple structures.   The double refraction and pyro-electricity of crystals related to cer-

tain fixed points of their primitive forms ; and the phænomena of circular polarization in quartz and amethyst, connected with the plagiedral faces of the crystal, indicate remarkable peculiarities of structure ; and I have had occasion to show that all the properties comprehended under double refraction and polarization do not exist in the ultimate molecules of the body, but are wholly the result of those forces by which these molecules are combined. Structures still more complicated have been discovered by the analysis of polarized light; and in the complex formations of *Apophyllite* and *Analcime,* we witness the operation of laws resembling more those which regulate the structures of animal life than those which had previously been observed in crystalline formations.

The doubly refracting structure of crystals, or to use the language of the undulatory theory, the law according to which this structure permits the æther to be distributed in their interior, relative to one or more axes, becomes the index as well as the measure of certain changes of structure which in some cases arise during the process of crystallization. When the atoms approach each other in a pure and undisturbed solution, the crystal which they form will be a correct type of the species ; but if the solution has been exposed to agitation,—if its electrical condition has been changed,—if foreign matter, crystallized or uncrystallized, opake or transparent, coloured or uncoloured, amorphous or isomorphous with the crystal ;—if any such matter has been introduced into the solution, we may expect a crystal deviating from the type of perfect crystallization, in transparency, or colour, or density, or hardness, or refractive power, or in doubly refracting and polarizing structure. A very remarkable example of such changes I discovered long ago in *Chabasie.* When the crystal had begun to form, it possessed the structure of the perfect mineral, but the force of *positive* double refraction of each successive layer began to diminish till it wholly disappeared. The changes, however, did not stop here ; a *negative* doubly refracting structure commenced at the neutral line, and gradually increased till the crystal was completed. This singular effect I ascribed to the introduction of foreign matter between the integrant molecules of chabasie, which weakened their force of aggregation, and consequently the double refraction produced by the mutual compression which arises from that force. By pursuing the same idea, I have been recently led to discover the cause of the beautiful but perplexing phænomena of *dichroism,* and I hope to be able to lay before the Society an artificial combination in which the actual phænomena are reproduced.

Having thus briefly adverted to the present state of our knowledge of the interior constitution of crystals, I shall now proceed

to the proper subject of this paper, which is to describe the optical figures produced by the disintegrated surfaces of minerals and artificial crystals.   The disintegration by which these figures are developed, is produced by three causes :—

I. By the natural action of solvents on the mineral, either at the time of its formation or at some subsequent period in the bowels of the earth.

II. By the action of acids and other solvents upon the surfaces of perfect crystals ; and,

III. By mechanical abrasion.

I. The first examples of *Natural* disintegration which I met with were in *Brazil Topaz*.   In a great number of these topazes I observed cavities filled with a white pulverulent substance, which Berzelius, who analysed it at my request, found to be a sort of *marl*, consisting of *silex, alumina, lime,* and *water,* and which, as he remarks, would have formed a *zeolite* had it been crystallized.   Upon examining the sides of the cavities which contained this substance, I found that they were rough and irregular, as if they had been disintegrated by a solvent; and I observed the very same effect on the flat summits and pyramidal faces, but never on the faces of the prism.   As it was impracticable to apply the goniometer to the mensuration of the angles of the minute facets which the microscope rendered visible on these disintegrated surfaces, I thought of obtaining a general idea of their position by examining the manner in which they arranged the reflected images of a luminous point placed at a distance.   Upon making this experiment, I was surprised to see a beautiful optical figure, consisting of the most elegant curves of contrary flexure, studded with tufts of light, and arranged with the most perfect symmetry round the central image of the luminous point which is formed by those portions of the summit of the crystal which had escaped from the action of the solvent. This remarkable arrangement of the reflected light is shown in Plate I. fig. 1, where it consists of *three* curves of contrary flexure of the general form of *lemniscates,* having at the extremities of their greater axis two semicircular tufts of light, and at the extremities of their lesser axis two triangular tufts of light. These figures undergo considerable changes on different specimens, depending, as will afterwards be seen, either on the time during which the solvent has acted upon it, or upon its dissolving power :— but they never deviate from the general type ; and in the most imperfect and rough specimens, of which I have examined more than a hundred, it is easy to recognise the elements of the perfect figure.   One of these variations in the figure is shown in fig. 2, where the light of the inner curve is diffused over a nebu-

lous figure with a crescent at each end, and an elliptical space in the centre, from which the image of the candle or luminous point has wholly disappeared. Hence it appears that the whole of the original surface of the flat summit of the crystal has been removed by the action of the solvent, an effect which may be imitated, as we shall presently see, in artificial crystals. The nebulous expansion of which we have been treating has sometimes rectilineal branches at its extremities, and is sometimes filled up in the middle, where the image of the candle is distinctly seen. In other specimens, this nebulous portion is the only part that is visible. The angular magnitude of the figure varies greatly in different specimens, and also its distinctness and continuity. When the elementary facets are large, the outline of the figure is marked by separate images of the candle; and when these facets are very small, the luminous tracery is soft and nebulous, and sometimes shading off into coloured tints, like the fringes produced by the interference of common or polarized light.

The optical figures produced by the faces of the pyramid are less distinct and beautiful, but not less remarkable, than those which we have been describing. Upon faces inclined about 145° to the summit plane, and which seem to be those marked *s* by Hauy*, the strange figure shown at A, fig. 3, is seen; on the adjoining face the same figure reversed is seen as shown at B; on the next face is seen the figure C, the same as A; and on the next face again the figure D, the same as B. I have observed other figures on faces differently inclined to the axis, but they are not of sufficient distinctness to merit delineation.

Optical figures analogous to those seen in topaz may be observed in various other minerals, but it is very difficult to find specimens that have undergone disintegration on their surfaces.

On the cubical faces of a specimen of *White Fluor-spar* from Shaionce town, Illinois, U.S., sent to me by Professor Silliman, I have observed a figure consisting of *four* radiations, inclined 90° to each other, and having the bright central image entirely obliterated. On the octohedral surfaces of the common fluor-spar, the figure consists of *three* radiations, inclined 120° to each other†.

In a crystal of *Hornblende*, the four summit planes at each end of the prism give the figure of a small luminous circle, as shown in fig. 4, the central image being wholly obliterated. On the faces of the prism, which are not those of cleavage, the figure is a luminous rhomboid, as shown in fig. 5, with a nebulous image at each angle, and one in the centre, the shorter axis of the rhomboid coinciding with the axis of the prism. In some speci-

* Plate 44, fig. 1, &c., first edition.
† These three radiations are *ab, ac, bc* in fig. 12. See foot-note on p. 28.

mens the luminous lines uniting the four images at the angles are not developed.

In a specimen of *Axinite*, I observed the remarkable geometrical figure shown in fig. 6. It consisted of two images *a, b,* joined by a line of light, and each of them sending out, in opposite but parallel directions, luminous rectilineal branches *ac, bd.* The line *ab* is perpendicular to the edges of the prism, and *ac, bd* parallel to the sides of the reflecting face. On the opposite side of the prism the figure is reversed.

On the faces of the primitive cube of *Boracite,* the optical figure seen by reflexion is a rectangular luminous cross, with a central image, the radiations being perpendicular to the edges of the square faces. *Muriate of soda* that had begun to deliquesce in a humid atmosphere exhibits the same figure.

The faces of the octohedron of *oxidulated iron* gives six luminous radii, inclined 60° to each other; but each alternate image is stronger than the one adjacent to it.

On the rhomboidal faces of the dodecahedron,*Garnet* gives an optical figure like a St. Andrew's cross, the line bisecting the arms of the cross being perpendicular to the longer diagonal of the rhomboidal face.

The natural faces of a fine octohedral *Diamond* gave *three* luminous radiations, inclined 120° to each other; and the same figure was exhibited by the faces of a rough pyramid of *Amethyst,* and by some of the cleavage planes of *Oligist Iron-ore.*

As minerals with disintegrated surfaces are not to be found in mineralogical cabinets, owing to their being in general bad specimens, I have not been able to pursue this branch of the subject any further; but I have no doubt that, if I had such a copious supply of other minerals as I had of topaz, I should be able to find among them specimens of equal interest.

II. We come now to the second and the principal branch of the subject,—to describe the optical figures produced by the action of water, acids, and other solvents, upon the surfaces of perfect crystals, both natural and artificial.

The crystals which I have found to be best adapted for exhibiting the action of solvents in producing optical figures by reflexion, are *Alum, Fluor-spar,* and *Calcareous Spar.*

If we take a fine crystal of *Alum,* and look at the image of a candle reflected as perpendicularly as possible from one of the faces of the octohedron, it will appear perfectly distinct, and without any luminous appendages. If we now immerse it for an instant in water, and dry it quickly with a soft cloth, the reflected image will send out three luminous radiations, as shown in fig. 7. By a second immersion in the water, three small

images of the candle will be developed at 1, 2, 3; and by a little further action of the solvent, these images connect themselves with the central image S, by the radial lines 1S, 2S, 3S, inclined 110° to each other, and 30° to the principal radiations from S. By continuing the action, other three images start up at 4, 5, 6, but apparently without any radial connexion with S. The principal radiations aS, bS, cS begin at this period to grow faint between 4 and 1, 5 and 2, and 6 and 3. Another immersion of the crystal developes the images 7, 8, 9; and by continuing the action, the images 1, 2, 3 become the brightest, and the branches A, B, C become more like images at m, n, o. The central image S has now transferred almost all its light to the new images, and another immersion will make it disappear altogether, leaving the central part of the figure perfectly dark, as in fig. 8.

It is now obvious, that by repeated actions of the solvent we have removed the whole of the original surface of the crystal by which the central image S was formed, and have replaced it by a great number of facets, which reflect, in consequence of their various inclinations, the different portions of the geometrical image shown in fig. 8. If we carry the process of solution further, the figure will undergo successive changes, becoming larger and more discontinuous in its outline.

The beauty and regular development of these phænomena depend in some measure on the perfection of the original surface of the crystal, and greatly on the uniform temperature of the water, and the shortness of the period during which the crystal is immersed in it. The successive development of the figure may be pretty well seen upon an artificial surface of the octo-hedron of alum, provided it is nearly parallel to the original surface. When the inclination of the artificial face is consider-able, the optical figure loses its symmetry, and gradually passes into other figures produced by the other faces of the crystal towards which the artificial face is inclined.

On some occasions I have found the principal radiations united by a beautiful nebulous web of a triangular form (as shown in fig. 9).

All the figures above described may be seen by reflexion from the smallest portion of the face of the octohedron, and they are often more beautiful on one part than another. The principal radiations are shown in the figures as if they were seen from the centre of the triangular face, in which case they point to the angles; but in all other cases, the radiations are perpendicular to the opposite sides of the triangle.

If we expose any of the six square faces perpendicular to the three axes of the octohedron to the action of water in the manner already described, and examine the optical figure which it pro-

duces by reflexion, we shall see four rectangular radiations, as in fig. 10, each radiation being perpendicular to a side of the square, and consequently passing into one of the three radiations formed by each face of the primitive octohedron.   By successive actions, these *four* radiations become shorter towards the central image, which gradually grows fainter and sometimes disappears.

If the same experiment is made with the TWELVE faces formed on the *twelve* edges of the octohedron, we shall obtain a figure with *two* radiations, forming an oval line with the image of the candle in the middle of it.   This image becomes gradually nebulous and finally disappears, leaving a kind of elongated oval nebula, with a dark oval centre, as shown in fig. 11, where the line AB is perpendicular to the replaced edge, and parallel to an axis of the octohedron.   The *two* radiations A, B obviously pass into one of the *three* radiations given by the adjacent faces of the octohedron; and if we were to cut a great number of artificial faces variously inclined from that which gives the *two* radiations in fig. 11, to that which gives the *four* in fig. 10, we should observe fig. 11 gradually passing into fig. 7, and acquiring a third radiation, and fig. 7 passing into fig. 10, and acquiring a fourth radiation.

From the phænomena exhibited by *alum* I proceed to those produced by *fluor-spar*, a mineral having the same primitive form and cleavage.   Having immersed one of the faces of the octohedron for a few days in sulphuric acid, I obtained by reflexion the beautiful figure shown in fig. 12*.   The three principal radiations, A, B, C, with the luminous triangular centre, are first developed, and by continuing the action of the acid, *six* new images are produced at *e, f, g, h,* and *i, k,* connected by lines of light with the other part of the figure.   A continuance of the action developes six luminous curves proceeding from the images *ef, gh, ik,* as in fig. 13, having each a new image within their concavity.   Three insulated images appear also at *l, m, n,* distant 120° from each other, and 60° from the principal radiations.

When the faces of the cube formed by planes replacing the angles of the octohedron are acted upon by the acid, the beautiful figure shown in fig. 14 is produced, the half-moons at the four angles being more distinctly brought out in some cases than in others.

The mutual connexion of these figures will be seen in fig. 15, where the triangles represent the faces of the octohedral pyramid unfolded as it were, and the quadrangular figure the square base of the pyramid.

Among the variations of figure produced by the strength of

the acid, or the duration of its action, one of the most interesting is the one represented in fig. 16, where the three principal radiations are inclosed in a luminous equilateral triangle, having a bright image at each of its angular points. If we grind and polish the opposite surface of the octohedron, so as to have a parallel plate, we shall see fig. 16 much more brilliantly by transmitted light*. If we now expose this second surface to the action of the acid, we shall see the optical figure shown in fig. 17, which is fig. 16 inverted. The cause of this inversion is, that this second face is parallel to a face in the opposite pyramid of the octohedron, whose apex lies in an opposite direction to that of the face which gives fig. 16. If we now look through the two faces that have been acted upon by the acid, we shall see the beautiful luminous figure shown in fig. 18, each image produced by the one surface being converted into an optical figure by the second. When the figure produced by the first surface of the plate of spar has its simplest form of three radiations, the multiplied figure seen by transmission contains only the twelve bright images and the central image of fig. 18; but when it exhibits the more compound form of fig. 12 or 13, the transmitted figure becomes exceedingly complex. It is obvious that the figure shown in fig. 14 will not be altered by transmission through two surfaces. Its brilliancy, however, and distinctness will be increased.

In some specimens I have observed three beautiful luminous arches, *mn, mo, no,* as shown in fig. 12.

Upon the face of a cube of fluor-spar, which had been ground and smoothed, but not polished, before it was acted upon by dilute *muriatic acid,* I observed the appearance in fig. 19. The original image had entirely disappeared from the centre of the rounded square of light, and the interior of the square was filled up with a faint nebulous light of uniform intensity. The *eight* round images were equidistant and equally bright, and the perimeter of the square was brightest at its angles and the middle points of its sides.

After the specimen had been exposed for some time to the action of boiling muriatic acid, the face which had given fig. 19 now exhibited the remarkable phænomena shown in fig. 20. The nebulosity had almost disappeared from the interior of the square, and collected, as it were, in its centre. The brightest parts of the figure were the curved masses at the angles, the middle parts of the sides of the figure being exceedingly faint.

From the *tessular* I proceeded to the *rhombohedral* system of crystallization, and I employed calcareous spar and sulphate of potash in the inquiry.

* This is the case with all the optical figures previously described.

Having immersed a rhomb of calcareous spar in dilute *nitric acid,* four parts of water being added to one of acid, I observed the reflected figure from all the faces of the rhombohedron to have the form shown in fig. 21.   The obtuse angle of the crystal was in the direction CE, and the angle ACB was greater than 120°.   As the obtuse angle of the opposite face has an opposite direction, the figure which it gives by reflexion is the inverse of fig. 21 ; so that, by looking through the parallel faces, we obtain a figure with six luminous radiations.   By varying the strength of the acid, the time of its action, and taking the surfaces of different crystals, the figure undergoes remarkable changes ; but though two individual figures often occur between which no similarity exists, yet, by observing the transitions of a considerable number, we may trace the family likeness through them all.

The thin web of light AEB, BD, and DA, appear at an early stage of the action, but it is often wanting between A and B ; and by continuing the action, a radiation often appears at F, sometimes united, and sometimes not, with the centre C.   The radiation CD sometimes expands suddenly below C into a diverging brush of light, and in other cases it is often wholly wanting, as well as the triangular luminous centre C.   In this case we have only two luminous brushes, A, B, with a small central image at C, A and B being sometimes joined by bright light, and sometimes by a small arch of nebulous light, the centre of which was at C.

On the faces of three different crystals, a figure with *five* radiations, diverging at unequal angles, was produced.   Two of these were the radiations A, B, the third was the brush developed at E, and the *fourth* and *fifth* were formed by the division of CD into two branches.   Sometimes the whole of the central part of this five-rayed figure was wanting, leaving the expanded part of the radiation in the circumference of a sort of oval ring, which was sometimes luminous throughout, but studded with the five brushes of stronger light.

When the solvent was pretty strong muriatic acid and water, the figures have often a great similarity to those already described ; but in some cases they have the form of luminous shields of a triangular form, as shown in fig. 22.   The place of the central image is at C ; the brightest part of the figure is at E, with a reddish margin, and the next brightest at A and B.   In other crystals the lights A, B, E were connected with C by the radiations ; and in one case, where a weaker acid was used, E was elevated further above C, and a horizontal band of light passed below C to the sides AD, BD.

When strong vinegar was used as the solvent, I obtained the

figure shown in Pl. IV. fig. 23, the letters having the same indications as in the preceding figures.

I now proceeded to apply the solvents to the summit planes of the prism. My first experiment was made on an artificial face perpendicular to the axis. By the action of vinegar it gave the figure shown in fig. 24, which consists of three radiations, inclined 120° to each other, having its centre sometimes dark, and sometimes occupied with a small image. The rudiments of other three radiations, inclined 60° to the former, are distinctly visible; and beside a luminous circle circumscribing the whole, there are three non-concentric circular arches, similar to those seen in fluor-spar.

The very same figure, with the exception of the circle and the circular arches, was obtained from the action of dilute muriatic acid on the natural faces of the *chaux carbonatée basée* of Haüy.

Having ground and repolished the artificial summit which exhibited fig. 24, I exposed it to the action of dilute muriatic acid, when I was surprised to see it produce the strange figure shown in fig. 25. Although the symmetry of the figure is hostile to the idea that its shape might have been partly the effect of accident, yet I found it unaltered by repolishing, and again disintegrating the surface, and what is still more decisive, I obtained the very same effect twice from another crystal of calcareous spar*.

By placing the crystal which gave this remarkable figure in a stronger acid solution, it gave on both its faces the figure in fig. 26, the light of which is strongest in the circular arches. By continuing the action of the same acid, the three inclosed radiations disappear entirely, and what is still more singular, they reappeared by a further continuance of the action. The action being prolonged they again disappeared, the circular arches grew wider and more confused, till they filled up the space which they at first inclosed.

Another crystal of spar exhibited the very same series of successive changes which I have now described.

I now reground and polished the faces of both these specimens. When they were plunged into strong dilute acid, their disintegrated surfaces produced no figure; but by increasing the strength of the solution, the figures were developed as formerly.

In order to observe the effect produced upon faces that were not coincident either with the primary or secondary faces of the crystals, I ground down one of the acute solid angles, and replaced it by a plane inclined 71° to a face of the rhomb, the common section being parallel to the long diagonal of this last face. After being immersed in dilute nitric acid, it gave the

* The brightest part of the figure was *ab*, the part above *a* being faint.

strange branching figure shown in fig. 27, where *abc* forms the brightest portion.   I obtained the same figure with another crystal, but the parts *xy* were wanting, and *b* and *c* were continued through *a* to *m* and *n*.   The side *a* was directed to the obtuse angle of the rhomb.   With another crystal, in which the artificial face was inclined 104° instead of 71°, the figure shown in Pl. V. fig. 28 was produced.

My next experiments were made with *sulphate* of *potash*, a crystal which belongs to the rhombohedral system.   By the slightest action of water upon the flat summit of a hexagonal prism it produced six luminous images, symmetrically arranged round the central image, each image being opposite a side of the hexagon.   All these images were connected with the central image by a halo of fainter light.   The faces of the hexagonal prism produce the figure shown in fig. 29, the line AB being coincident with the axis of the prism.   By continuing the action, the branches C, D vanished, and the figure appeared as in fig. 30, the images being connected with a haze of light.

A more remarkable effect was produced with the faces of the truncated pyramid.   Three of the six faces produced the effect shown in fig. 31, while the other three alternate faces produced the same figures, but without the wings E, E.   Sometimes two images are seen below B.

My attention was now directed to the system of crystallization in which the base of the primitive crystal is a square.   Having immersed a fine crystal of *Faroe apophyllite* in dilute nitric acid, the summits of the prism were alone acted upon.   They produced a figure with *four* rectangular radiations directed to the angles of the summit, and four much shorter ones pointing to the sides of the square summit.   The four large rays appeared first connected with a luminous web, and the four small ones were subsequently developed.

The very same figure, but with some modifications, was produced by the action of water upon the summit of the square prism of sulphate of potash and copper.   The small radiations were produced last, as in apophyllite; but what is remarkable, they are directed to the angles, and not to the sides of the square face.   The extreme solubility of this salt renders it difficult to develope the figure distinctly.

From another crystal of the same class, *superacetate of copper and lime,* I obtained the beautiful figure shown in fig. 32, where the eight radiations are of equal length, and the images at their extremities connected by beautiful curves of light concave outwards.

I have made a great number of experiments with crystals belonging to the prismatic system, such as *sulphate of magnesia,*

borax, tartrate of potash and soda, sulphate of iron, and *sulphate of copper*; but though I have delineated many of the figures which they produce, and though some of them have considerable interest, I am not able to present the details in the form which I could wish. I expected to have been able to obtain interesting and definite results by subjecting the faces of a large class of minerals to the action of fluoric acid; but, in so far as my experiments went, I was disappointed. Dr. Fyfe, many years ago, exposed several crystals of quartz and amethyst, which I sent him for this purpose, to the action of fluoric acid, but the disintegration of the surfaces was such that they would not reflect any light at all. I have no doubt, however, that by weakening the action and carrying it on very slowly the desired effect will be produced.

During the preceding experiments I was led to observe, that different solvents had a tendency to produce different figures, and I confirmed the truth of the observation by many experiments. When muriatic acid, for example, acts upon *alum,* it produces a figure with six radiations, not unlike those of sulphate of potash, and by continuing the action, the central image vanishes. If in this state we immerse it in water, three of the radiations vanish, and it assumes the usual form. When again immersed in muriatic acid, the six images reappear. Diluted nitric acid has the same effect as muriatic acid; but diluted sulphuric acid gives such a form to the radiations, that their extremities are included within an equilateral triangle, the larger radiations pointing to the three angles, and the shorter ones to the three sides.

Diluted *alcohol,* though it acts feebly upon *alum,* produces a figure different from water and the acids. It gives a figure with three short radiations; and, by further dilution, the figure undergoes changes which give it a greater resemblance to the aqueous figure.

In order to retard or diminish the action of solvents upon highly soluble crystals, I conceived the idea of immersing them in solutions of the crystal of different degrees of strength. In making this experiment on alum, I took a crystal which gave the figure shown in fig. 8, and having immersed it in a saturated solution of alum for a single instant, I found that it had, as it were, seized the particles of alum in the solution, and replaced them in their proper position on the disintegrated face. By subsequent immersion the face repassed through all the stages at which it produced the phænomena shown in fig. 7, and finally became perfect, reflecting a single image of the candle. *The singular fact in this experiment is, the inconceivable rapidity with which the particles in the solution fly into their proper places upon*

the disintegrated surface, and become a permanent portion of the solid crystal.

In repeating and varying these experiments, I observed a number of curious facts, which it would be out of place here to describe. I immersed crystals of alum in saturated solutions of nitre and other salts, and observed many remarkable changes upon the figures which they produced. The changes take place principally upon the central parts of the figure, as shown in fig. 33, which represents one of the forms which a solution of *nitre* gave to the figure produced by *alum*; but in other cases the whole figure suffers a change. A crystal of *sulphate of potash*, which gave the hexangular radiations already described, produced the same figure, with twice the angular magnitude, when dipped for a few seconds in a saturated solution of nitre.

In consequence of having observed that the natural cleavage planes of crystals gave indications of regular optical figures, similar to those produced by solution, I was led to make some experiments on the effects of *mechanical abrasion,* as produced by coarse sandstone*, or by the action of a rasp or large-toothed file. Surfaces thus torn up produced, in a rude manner, the optical figure given by solution; but what was very remarkable, *the figure had a different position, or had the position which solution would have developed on the opposite face.* This is also true of the figures produced by natural cleavage planes, in which the separating surfaces have been slightly torn up.

It is scarcely necessary to observe, that the power of producing the optical figures described in this paper may be communicated to wax or isinglass, &c. The impressions on isinglass enable us to see the figure by transmitted light, and to observe its form and dimensions with greater accuracy.

Allerly, February 1, 1837.

VII. *On the Colours of a Jet of Steam and of the Atmosphere.* *By* Reuben Phillips, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I WAS not aware of the existence of the papers referred to by M. Clausius until I saw his letter in the last Number of the Magazine. Not having access to the foreign Journals, I am unable to compare, or perhaps to correct, the following by what M. Clausius has written.

---

* In fig. 12 the triangle *abc* is brought out by grinding on a coarse hone an octohedral surface of fluor-spar similarly situated to one on the natural face.

My reasons for concluding that clouds consist of minute drops, are as follows. Prof. Henry's experiments demonstrate that the film of a soap-bubble contracts on the included air with great force considering the thinness of the film*. Now the amount of condensation to which the enclosed air is subjected by the film is inversely as the diameter of the sphere, that is, if the thinness of the film is always the same. Supposing a little bubble to be formed in the air, like one of those imagined to form cloud, then, from the very small diameter of such a vesicle, the included air would be strongly condensed, perhaps to the extent of two or three atmospheres or more. Oxygen and nitrogen being soluble in water, and the solubility of gaseous matter in water increasing rapidly with the pressure, it follows that the particles of water will take up air at the interior surface of the film; and on these particles diffusing themselves, and so arriving at the outer surface of the film, they will liberate a quantity of air corresponding to the diminished pressure. So that from this species of exosmose alone, the bubble would soon become a homogeneous drop. I conclude, therefore, that vesicles so small as the particles which form clouds cannot long exist, if ever they are formed. With regard to the ease with which water takes up air under a high pressure, I may mention, that if water be shaken for a few minutes with air condensed to the extent of about six atmospheres, then on allowing the water to escape into the air by a quiet stream, and receiving it into a glass vessel, a considerable effervescence can be perceived.

Dr. Waller could find no evidence of vesicular structure by the aid of the microscope†.

I think the sky has an important action in producing the morning and evening red. Now the highest clouds are evidently situated below the region of the sky; and the blue colour of the sky deepens to an observer ascending a mountain. From this I conclude that the firmament consists of a distinct and very distant orb of the atmosphere, containing a great number of particles of water so small as to give a blue of the first order by reflexion. When the sun is near the horizon, its rays traverse a much greater thickness of this orb than at other times, and consequently the direct light of the sun becomes sensibly reddened by interference on transmission through the sky. The orb of the sky can, I think, hardly be regarded as being in a state of rest with respect to the earth, for it may be moved, and perhaps heaped up at times by winds; the sky may also be subject to tidal, and other motions caused by the expansions and contractions of a variable temperature, by which motions the curvature of the

* Phil. Mag. vol. xxvi. p. 541.
† Ibid. vol. xxviii. p. 99, and vol. xxix. p. 103.

firmament over some spots of the earth's surface may be changed, which would cause the rays of the sun at those places to traverse a greater or less length of the firmament. I hold, that if these motions are sufficiently extensive, they must produce sensible alterations in the intensity of the morning and evening red.

With regard to the action of clouds and mists in the lower regions of the atmosphere in producing the morning and evening red, I follow Professor Forbes and M. Clausius. I look upon the clouds as white, or nearly white reflectors, which only appear coloured in consequence of red light falling on them, or from being seen through a coloured mist. I cannot, however, come to a conclusion as to whether the usual sombre colours of the clouds are generally produced in the clouds themselves or in the clearer air; neither can I see any reason why, in accounting for the colours of the clouds, we must restrict ourselves to the consideration of those particles only which produce the first blue and red.

I am, Gentlemen,

Your most obedient Servant,

7 Prospect Place, Ball's Pond Road,   REUBEN PHILLIPS.
    December 11, 1852.

VIII. *On Copper Smelting.* By JAMES NAPIER, *F.C.S.**

[Continued from vol. iv. p. 465.]

WHEN the ore is sufficiently calcined, it is let down from the hearth into the *cubs* or vaults beneath by openings in the floor; water is added to the hot ore in the cubs until the whole is completely drenched; during the addition of the water much sulphuretted hydrogen is given off. The ore is then removed from the cubs to a yard, and there stored up until removed to the fusing-furnace. The oldest portion calcined is taken first, it being of considerable importance that the ore should lie for some time in the moist state after the calcining operation, in order that the metals present may become partially oxidized. It is found that a few days' exposure in this way not only facilitates the fusing, but produces a richer mat than if the ore were fused directly from the calciner.

An ore-fusing furnace is fitted up with a hopper upon the roof, into which the workman puts the charge of calcined ore, while the fusion of a preceding charge is going on. The ore is weighed out to the workman in hundredweights, and carried to the hoppers in boxes. The charge for an ordinary-sized fusing-furnace ranges—

* Communicated by the Author, who reserves to himself the copyright, any infringement whereof will invoke legal proceedings.—EDS.

From 25 to 30 cwt. of calcined ore.

From 7 to 9 cwt. of sharp slag from more advanced operations.

From 2 to 3 cwt. of cobbing.

If the ore is known to be stiff, a box of lime or fluor-spar is added; but if flux can be avoided, it is better.

When the charge is let down into the furnace, it is spread equably over the bottom, and the doors all closed, stopping every air-hole with clay, and the heat of the furnace increased as rapidly as possible. In the chimney immediately above and opposite the reverberator is a small hole, through which the workman looks and ascertains the condition of his heat. A similar hole in the door-plate looking into the hearth enables him to judge of the state and condition of the charge. This mode of ascertaining the state of the furnace requires some experience; but it is of great practical value, as it prevents the necessity of opening doors by which the furnace would become cooled.

After about five hours' firing, when the furnace has reached a white heat and all appearances are favourable, the door-plate is removed and a long iron rake passed through the fused contents to make sure that the whole is perfectly fused. This being the case, the workman begins the operation of *skimming*, that is, drawing off the scoriæ or slag, which from its less specific gravity floats on the surface of the mat, which is effected by means of a long rake, the scoriæ being drawn out at the front door. This operation requires considerable experience and care in drawing the rake along the surface of the metal, so as not to take out any of the metal with the slag. When the furnace is skimmed, the common practice is to let down a second charge of ore, fuse and skim in the same manner before tapping the furnace to let out the metal. The tap is in the side of the furnace, and is an aperture the size of a brick, extending from the surface of the sand bottom of the furnace upwards. It is filled up with a mixture of clay and sand. When the furnace is tapped, an opening is made a little above the charge, and then cut down until a small gutter is formed for the metal, which in flowing over cuts deeper, making its own way to the bottom of the aperture, and so emptying the furnace.

Generally the mat or *coarse* metal from the ore-furnace is tapped into a large pit of water which granulates it. These pits are from 6 to 8 feet deep, and from 4 to 5 feet square, into which a perforated box is lowered which receives the charge of metal, and which is raised by a crane or pulley, and the metal removed to a yard for the next operation. This mat is termed granulated *coarse* metal.

Whatever modifications or improvements may have been

adopted and practised by any of the smelters, up to the stage of coarse metal they are all the same; all modifications generally begin at the coarse metal. Before referring to any of these modifications, we will first inquire into the changes which have taken place in the operation described; and that these changes may be the better understood, we will take a small charge of the average quality and follow out the actual results of practice.

25 cwt. of calcined ore, containing in 100 parts,—

Copper	12·5
Iron	28·5
Sulphur	16·0
Silica	39·0
Oxygen, &c.	6
	100·0

5 cwt. of sharp slag from more advanced operations, composed of—

Oxide of iron	53
Oxide of copper	2
Silica	45
	100

2 cwt. of cobbing, composed mostly of old bottoms, containing—

Silica	75
Oxide of iron	15
Oxide of copper	10
	100

These on being fused together yielded about $9\frac{1}{2}$ cwt. of coarse metal and 18 cwt. of slag, there being always a loss upon the weight put in, owing to the volatilization of sulphur, water, &c. The composition of the coarse metal was—

Copper	31·4
Sulphur	27·3
Iron	41·3
	100·0

That of the slag, including the silica in pieces that were mixed, was—

Silica	71
Protoxide of iron	27
Lime, &c.	2
	100

These results correspond closely to the principles laid down in

the former articles. The coarse metal obtained by this fusion varies considerably in quantity and also in composition, depending upon the length of calcination, the quantity of iron and sulphur remaining, the heat of the furnace, &c. It is also liable to be affected by the flux when any is used. If that be fluorspar or lime, the coarse metal will be enriched; but if oxide of iron, which nevertheless is a preferable flux, the coarse metal will not be so rich in copper, a portion of the oxide of iron being reduced to the state of sulphuret. The qualities of oxide of iron used are the rich slags from iron cupola furnaces, and the scale and pickling dust obtained from the iron plates in tin-plate works, their composition being as under:—

	Slag.	Pickle dust.	Rolling scale.
Silica	25	4·0	6·4
Protoxide of iron . . .	75	95·5	93·6
Carbonaceous matters .	...	·5	...
	100	100·0	100·0

The following table of analyses of a few kinds of coarse metal will show its variable character:—

	Copper.	Sulphur.	Iron.
Coarse metal from fusing ores not calcined, using iron flux	17·0	30·0	53·0
Metal from similar ores calcined	27·0	32·5	30·5
Coarse metal from ordinary operations...	27·0	31·6	40·6
do. do.	21·1	33·2	45·5
do. do.	39·5	36·4	25·0
do. do.	29·5	32·8	38·9
do. do.	27·6	32·8	35·3
do. do.	31·5	27·6	35·0

Most of these contained small portions of other metals, and silica, which are not given. From the varying character of the mat, it is impossible to reduce its composition to any chemical formula; we think, however, that the copper and sulphur, after fusion, always exist as a subsulphuret; and the inconstancy in the composition of the coarse metal is in the relations of the sulphur and iron ranging from a bisulphuret to a protosulphuret, or more probably mixtures of these.

M. Le Play gives as the composition of the coarse metal,—

Copper	33·7
Iron	33·6
Sulphur	29·2
Other metals, &c. .	3·1
	99·6

and deduces from this the formula $3Cu^2S \, Fe^2 S^3 + 4FeS$, which

we think very probable ; although from the above table, and from many other analyses of coarse metal, almost every charge would require a different formula.

The slags skimmed from the charge are removed to an open yard, and when cool are broken up into small pieces and examined by the eye for small shots of metal which may have been drawn out by the rake, and which are laid aside to be returned to the fusing-furnace. The average quantity so returned from one charge is about 2 cwt., even with the most careful workman and best fused charges. The examination of the slags is generally the work of old men, which from long practice they do with astonishing accuracy ; nevertheless there are often slags with a small quantity of copper chemically diffused through the mass which cannot be detected by the eye, but which may be easily and quickly tested by the methods described under assaying, namely, taking a few test-bottles of equal size, containing each $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, and 1 grain of copper dissolved and converted into ammoniuret. A sample of the slag is ground fine, and 100 grains digested at a boiling heat in nitromuriatic acid for about 15 minutes ; a little water is added, and then ammonia in excess, after which the solution is filtered into a bottle of similar dimensions with the others and the colours compared. Should it be the same as the one containing $\frac{1}{4}$ grain of copper, then the slag has $\frac{1}{4}$ per cent., and so with the others. The careful testing of the slags in this way, apart from the picking out of the shots, becomes an important guide to the whole management of the works. Imperfect fusion, whether caused by bad coal, bad furnace, or bad workmen, never gives a perfect slag ; and that small causes often produce great and important results, we may illustrate by the following circumstance. In a work where the maximum quantity of copper in the slag allowed to be cast away was $\frac{1}{4}$ of a per cent., two furnaces working alongside each other at the same rate produced what was practically termed clean slag ; but the one contained seldom less than $\frac{1}{4}$ per cent., the other seldom above $\frac{1}{8}$ per cent. They fused 40 tons per week, producing each about 25 tons slag, being a weekly loss in copper in the one furnace over the other of nearly £3, equal to the wages of two first class men. The cause of the difference in this instance was in the furnace, an imperfection in the draught, which would not have been observed but for testing the slags.

Clean slag is and ought to be a primary object to the smelter ; but it often happens that a desire to increase the make of copper induces a heavy charge being put into the furnace, or the building of larger furnaces, which in all the instances we have seen where the slags were tried strictly by testing, proved of little or no benefit. There is a maximum size of furnace and charge

which cannot be increased without affecting the slag, and the length of time required for fusing. What ought to constitute the proper size of charge, or furnace, or length of time, it would be difficult to fix as a rule; but the following results, coming under our own observation, may give a better idea of the principles we are here stating.

1st. The furnace hearth measured $11\frac{1}{2}$ by 8 feet, had 25 cwt. calcined ore, 7 cwt. of slag and cobbing as the charge, and delivered 5 charges in the 24 hours, equal to 6 tons ore per day. The foul slag, with shots in it to be returned, averaged $2\frac{1}{2}$ cwt. each charge. The clean slag contained fully $\frac{5}{8}$ths per cent. copper. The mistake in these operations was allowing too little time; for fusing each charge it could not exceed 4 hours. Average of coal consumed per week 20 tons.

2nd. A furnace $12 \times 8\frac{1}{2}$ feet inside measure, charged with 31 cwt. of ore and 9 cwt. of slag and cobbing, delivered 4 charges in the 24 hours, equal to 6 tons ore per day, each charge taking about $5\frac{1}{2}$ hours time. The foul slags with shots averaged 2 cwt. per charge; the clean slags contained under $\frac{1}{4}$ per cent. copper. Coals consumed weekly 21 tons.

3rd. Furnace measured $13\frac{1}{2}$ by $8\frac{1}{2}$ feet inside, charged with 36 cwt. of ore and 9 cwt. of slag and cobbing, 4 charges in 24 hours, equal to 7 tons of ore per day. Average foul slags with shots 3 cwt. per charge; the clean slags contained $\frac{1}{4}$ per cent. Average coal consumed weekly 23 tons.

One furnace of the ordinary size, working four weeks, fusing each charge 31 cwt. ore and 9 cwt. of slag and cobbing, gave as an average $1\frac{1}{2}$ cwt. foul slag each charge, and slags thrown away containing ·28 per cent. copper. The same furnace working other four weeks, but fusing each day 36 cwt. of ore and 9 cwt. of foul slag, gave an average of $3\frac{1}{2}$ tons foul slags each charge, and the slags thrown away had ·38 per cent. copper. Here were two evils; more foul slag to be refused, and more copper cast away: the extra coal for the month was 5 tons. The loss here is about £7, the gain is 28 tons more ore fused, costing 5s. per ton, besides the drawback of the furnace being more liable to get out of repair.

These few examples will suffice to illustrate the principles of management. A great number might be enumerated where the apparent gain on one point was accompanied by losses on some other. We have seen ore-fusing furnaces measuring $19\frac{1}{2} \times 11\frac{1}{2}$, with a charge, including slag and cobbing, of 90 cwt., and fusing four of these in 24 hours; but the quantity of slags to be returned was so great, that the furnace, to fuse its own foul slags, which a furnace should, could only take in one-half the charge of ore, and consumed upwards of 30 tons of coal per week.

In some of the smelting-works certain modifications of the process described for the coarse metal are adopted. Instead of tapping the mat into a pit of water, it is let out into sand-beds, and when cool broken up and removed to another furnace, and fused along with the scoriæ or slag from the roasting furnace, containing about from 8 to 10 per cent. of copper. This enriches the coarse metal, and produces a mat termed occasionally *white metal,* of an average composition of—

Copper	.	.	.	.	.	40
Iron	.	.	.	.	.	30
Sulphur	.	.	.	.	.	28
Silica	.	.	.	.	.	2
						100

This metal is tapped into a pit of water in the same way as the coarse metal, and laid aside for calcination. This operation of fusing, added to an additional two or three hours of extra calcination of the granulated metal, saves an operation in a more advanced stage of the process. The œconomy of this modification is not so apparent as to cause its general adoption, although it would require no alteration of furnace. The advantages it is said to possess are greatly neutralized by disadvantages attending some of the after operations, which will be better judged of when these are described.

Another modification which we have had considerable experience in carrying out is the following. The coarse metal is tapped into sand-beds, broken up, and removed to another furnace of an extra large size, as it requires but a low heat to fuse. About 4 tons of the coarse metal are fused in this furnace, with 4 cwt. of salt cake (sulphate of soda), and 2 cwt. of coal. When in perfect fusion the whole is well stirred with an iron rabble, and the furnace immediately afterwards tapped into sand-moulds; as soon as the metal *sets* (becomes solid), and while red-hot, the *pigs* or ingots are thrown into a tank of water, which is caused to boil by the heat of the metal; at the same time the metal falls into an impalpable powder, similar to slaking lime. The soda salt, which is reduced or decomposed in the fusion, dissolves in the water, forming a strong alkaline ley, and the powder is thus boiled in a caustic alkali, by which means all impurities capable of being dissolved in hot alkaline leys are separated from the metal, such as oxides and sulphurets of tin, antimony, and arsenic. After the boiling has ceased for some time, the alkaline solution is let off and run into the horizontal culverts leading from the calcining and fusing-furnaces, where the water is evaporated, and a part of the soda recovered with any copper that may have been carried over, the water also assisting the conden-

sation of the fumes from these furnaces. A sample of this dried stuff from a culvert we have found to be composed of—

Silica	20·4
Alumina	11·5
Lime	2·8
Peroxide of iron . .	8·0
Oxide of copper . .	10·3
Oxide of antimony .	7·7
Oxide of tin . . .	1·0
Arsenic	·8
Sulphuric acid . .	21·5
Soda	8·7
Water	6·8
	99·5

The powder left in the tanks is washed by allowing clean water to pass through it for a time, it is then removed into a heap ready to be calcined.

The reactions taking place during these operations, we think, are as follows :—The sulphate of soda is reduced by the coal to the caustic state, some of it probably to the metallic condition. Two samples of different qualities taken before being put into the tank of water gave—

Copper	32·3	28·6
Iron	31·8	38·7
Sulphur	26·7	26·5
Silica	1·5	·5
Antimony, &c.	·9	·7
Carbonaceous matter	·4	·6
Sulphate and caustic soda, and loss	6·4	4·4
	100·0	100·0

When a piece of this coarse metal, after being fused with soda and cooled, is immersed in water, there is a rapid absorption of water with evolution of heat, and diffusion of a smell of sulphide of hydrogen, while the mass falls into powder. Immediately after this absorption, and before disintegrating, the piece of coarse metal when broken has a brown colour, which, when seen through a lens, is found to be caused by a reduction of the copper either to the state of metal or suboxide, probably from the presence of sodium, which decomposes the water, becoming converted into soda, while the hydrogen reduces the subsulphuret of copper. The powder soon turns black by exposure, and now contains oxide of copper and oxide of iron along with the sulphurets. The soda in the alkaline water in the tanks is in the caustic state, with a little undecomposed sulphate, and contains antimony

and tin in solution if any of these metals were present in the ore.

The sulphate of soda used in the above process should be of the best sort; that obtained in the manufacture of nitric acid from nitrate of soda is better than that procured from common salt in making soda ash. The former contains on an average about 99 per cent. of sulphate, while the latter does not contain more than 70 per cent., the remainder being common salt, the presence of which is deleterious.

The slag obtained from the fusion of the ores is an important consideration in a smelting establishment: by referring to the average composition of the ore, in connexion with the fact that all the iron and silica goes to form slag, it will be seen that upwards of one-half of the weight of the ores remains in the form of refuse; so that a small work, producing 30 tons metallic copper weekly, will yield not less than 120 tons of slag in that time, being 7000 tons annually; an important item, and costing a considerable sum to put out of the way.

Many plans have been suggested for applying these slags to useful purposes, but few of them have been acted upon. They are extensively used in the neighbourhood of Swansea for repairing roads; an application not the most suitable, as they produce uncomfortable dark roads at night, from the almost entire absence of reflected light.

Propositions have been made to use the scoria for building materials, which, however, requires two conditions: first, the moulding it into a shape suitable for building purposes; and secondly, annealing: being a kind of glass, unless annealed it is brittle. To mould the slag as it is skimmed, prevents its examination for copper and induces carelessness in skimming; risking a loss of copper, which is so much more important than a common building article: the moulding of the slags is therefore seldom practised. We have seen an old slag wall, three feet high, having in it copper sufficient to give the wall a value of 20s. a lineal yard for the copper alone.

Whatever proposition is made for using the slags, it should not interfere or have any connexion with the smelting operations, or the thorough examination of the slags for copper.

From the fact that the slags contain on an average 30 per cent. of iron, it has been suggested, and trials have been made, to manufacture iron from it. The great quantity of silica present, and the necessity of adding flux sufficient to combine with it before the iron can be obtained, is a commercial drawback to any such scheme; and even were this not the case, the iron obtained from the slags is very impure and generally contains other metals, and always copper. A sample of iron made from

slags that were rich in oxide of iron gave by analysis—

Iron	92·0
Copper	2·4
Sulphur	1·8
Insoluble matter . .	1·4
Nickel	·3
Manganese . . .	trace
Carbonaceous matters	1·3
	99·2

The slag obtained while making this iron was not exhausted of oxide of iron, although three equivalents of lime for the silica present were added as flux. Such iron is well-suited for the precipitation of copper from solutions, as described in a former article, and might be worth the attention of those who adopt such processes.

On the surface of slag we have occasionally met with a siliceous froth of a gray-white colour, and very light, owing to its porosity; its composition was found by analysis to be—

Silica	96·31
Oxide of iron . .	1·67
Lime	·55
Magnesia . . .	·52
Alumina	·45
	99·70

In the black glassy slags we occasionally meet with large pieces of a gray porous character resembling pumice, with an appearance as if other substances had filtered out, leaving a skeleton of silica, heavier, more tenacious, and harder than the froth referred to above. Its composition was—

Silica	97·5
Oxide of iron . . .	1·0
Sulphur	·8
Lime	·7
	100·0

[To be continued.]

IX. *On the Valuation of Life Contingencies by means of Tables of Single and Joint Lives.* By CHARLES JAMES HARGREAVE, *Esq., LL.D., F.R.S.*[*]

LET $a_1, a_2 \ldots a_n$ be the reciprocals of the roots of the expression $1 - s_1 x + s_2 x^2 - \ldots \pm s_{n-1}x^{n-1} \mp s_n x^n$, which call ϕx. We have then $\phi x = (1 - a_1 x)(1 - a_2 x) \ldots (1 - a_n x)$, from which the

[*] Communicated by the Author.

following series of values may be readily deduced, the Σ imply-
ing the sum of all the instances of the form placed under it, so
that each expression is a symmetrical function of $a_1, a_2 \ldots a_n$.

$$\phi(1) = (1-a_1)(1-a_2) \quad \ldots \quad (1-a_n) \,.\, \text{No. of terms, 1.}$$

$$-\phi'(1) = \Sigma a_1(1-a_2) \ldots \ldots (1-a_n) \,.\, \text{No. of terms, } n.$$

$$\frac{1}{2}\phi''(1) = \Sigma a_1 a_2 (1-a_3) \ldots \ldots (1-a_n) \,.\, \text{No. of terms, } n\,\frac{n-1}{2}.$$

$$-\frac{1}{2.3}\phi'''(1) = \Sigma a_1 a_2 a_3 (1-a_4) \ldots (1-a_n) \,.\, \text{No. of terms, } n\,\frac{n-1}{2}\,\frac{n-2}{3}.$$

$$\vdots \qquad\qquad \vdots \qquad\qquad\qquad \vdots$$

$$\pm\frac{1}{2.3..p}\phi^{(p)}(1) = \Sigma a_1 a_2 a_3 .. a_p(1-a_{p+1}) .. (1-a_n) \,.\, \text{No. of terms, } n\,\frac{n-1}{2} .. \frac{n-p+1}{p}.$$

$$\vdots \qquad\qquad \vdots \qquad\qquad\qquad \vdots$$

$$\pm\frac{1}{2.3..(n-1)}\phi^{(n-1)}(1) = \Sigma a_1 a_2 a_3 .. a_{n-1}(1-a_n)\,.\, \text{No. of terms, } n.$$

$$\mp\frac{1}{2.3..n}\phi^{(n)}(1) = a_1 a_2 a_3 \ldots a_n \qquad\qquad \,.\, \text{No. of terms, 1.}$$

The sign is positive when p is even, and negative when p is odd.
A single term of the pth expression contains p factors of the
form a, and $n-p$ factors of the form $1-a$; and the expression
itself is the sum of every term which can be so constructed.

Let there be n persons whose names are $A_1, A_2 .. A_n$; and let
a_p denote the probability that A_p will be living at any given future
date. Then by what precedes, we see that the probability that
at the given date there will be exactly p out of the set living,
(implying that exactly $n-p$ in number are dead), is

$$\pm\frac{1}{2.3..p}\phi^{(p)}(1).$$

Thus we have—

Probability that all are dead $= 1 - s_1 + s_2 - s_3 + \ldots$,

Probability that exactly one is living $= s_1 - 2s_2 + 3s_3 - 4s_4 + \ldots$,

Probability that exactly two are living $= s_2 - 3s_3 + 6s_4 - 10s_5 + ..$;

and so on until we arrive at the probability that exactly one is
dead, which is $s_{n-1} - ns_n$; and finally the probability that all
are living, which is s_n.

In like manner, we observe that the expression

$$\phi(1) - \phi'(1) + \frac{1}{2}\phi''(1) - \ldots \pm \frac{1}{2.3..p}\phi^{(p)}(1)$$

represents the probability that p persons or less (including none)
are living at the given date; and, omitting the first term, we
have the probability that p persons or less are living, excluding

the case of all dead. If $p = n$, the expression becomes $\phi(1-1)$ or $\phi(0)$ or 1, the event being certain.

By referring to the well-known process for obtaining the value of an annuity which depends upon the existence of any *status* of a contingent nature, the reader will without any difficulty at once perceive the correctness of the subjoined solution of the following problem.

Problem.—To determine the value of an annuity during the continuance of any *status* which can be made to depend upon the life or death of any number of persons out of the set $A_1, A_2 \ldots A_n$.

Mode of solution.—Find the probability (by the formulæ above given) that the status will be in existence after the lapse of any number of years; for $s_1, s_2 \ldots$ write their values $a_1 + a_2 + a_3 + \ldots$, $a_1 a_2 + a_1 a_3 + \ldots$; and if necessary, multiply the expression out, so as to get rid of all brackets; then for each small letter write the corresponding large letter; and the result is the value of the annuity in question expressed in values of annuities on single and joint lives, on the assumption that A_n means the value of an annuity on the life of A_n; $A_m A_n$ the value of an annuity on the joint lives of A_m and A_n; and so on for any number of persons. The most convenient notation is to consider these values as framed on the assumption that unity denotes the value of a present perpetuity; so that A_n means the ratio of the value of an annuity on the life of A_n to the value of a perpetuity, or the value of the annuity on the assumption that a perpetuity is 1. Calling A_n the value of such a *status*, the value of a perpetuity expectant on the determination or failure of the *status* is $1 - A_n$. The preceding rule, therefore, determines also the value of a perpetuity expectant on the failure of any *status* formed out of n lives, or payable only when the *status* does not exist.

The above mode of solution will be rendered plain by a few examples.

Let us take every contingency that can be made from the lives of A_1 and A_2. Here

$$\phi(1) = 1 - s_1 + s_2, \quad -\phi'(1) = s_1 - 2s_2, \quad \frac{1}{2}\phi''(1) = s_2,$$

which give the following probabilities and values :—

Probability that both are dead is $\phi(1)$ or $1 - (a_1 + a_2) + a_1 a_2$; Value of perpetuity expectant on death of survivor,

$$1 - (A_1 + A_2) + A_1 A_2;$$

Probability that one or both are living, $a_1 + a_2 - a_1 a_2$; Value of annuity for two lives and life of survivor, $A_1 + A_2 - A_1 A_2$. These two instances are complementary to each other.

Probability that one only is living is $-\phi'(1)$ or $a_1 + a_2 - 2a_1 a_2$;

Value of annuity for life of survivor expectant on the death of one of the two is $\Lambda_1 + \Lambda_2 - 2\Lambda_1\Lambda_2$; and value of present perpetuity, payment of which is suspended from death of first to death of second, is $1 - (\Lambda_1 + \Lambda_2) + 2\Lambda_1\Lambda_2$.

Probability that both are living is $\frac{1}{2}\phi''(1)$ or s_2 or a_1a_2; value of annuity on joint lives is $\Lambda_1\Lambda_2$; value of perpetuity expectant on death of one of the two is $1 - \Lambda_1\Lambda_2$.

Now let us take the contingencies from the lives of three persons, Λ_1, Λ_2, and Λ_3.   Here

$$\phi(1) = 1 - s_1 + s_2 - s_3; \quad -\phi'(1) = s_1 - 2s_2 + 3s_3; \quad \frac{1}{2}\phi''(1) = s_2 - 3s_3;$$
$$-\frac{1}{2.3}\phi'''(1) = s_3.$$

Probability that all are dead is $1 - s_1 + s_2 - s_3$; value of perpetuity expectant on death of last survivor is

$$1 - (\Lambda_1 + \Lambda_2 + \Lambda_3) + (\Lambda_1\Lambda_2 + \Lambda_1\Lambda_3 + \Lambda_2\Lambda_3) - \Lambda_1\Lambda_2\Lambda_3;$$

and, taking the complement, we have value of annuity for the lives and life of the survivor

$$\Lambda_1 + \Lambda_2 + \Lambda_3 - (\Lambda_1\Lambda_2 + \Lambda_1\Lambda_3 + \Lambda_2\Lambda_3) + \Lambda_1\Lambda_2\Lambda_3.$$

Probability that one only is living is $s_1 - 2s_2 + 3s_3$; and value of annuity for life of last survivor expectant upon the decease of both the others is

$$\Lambda_1 + \Lambda_2 + \Lambda_3 - 2(\Lambda_1\Lambda_2 + \Lambda_1\Lambda_3 + \Lambda_2\Lambda_3) + 3\Lambda_1\Lambda_2\Lambda_3.$$

Probability that one or none is living is $1 - s_2 + 2s_3$; therefore value of perpetuity expectant on the death of two of the three is $1 - (\Lambda_1\Lambda_2 + \Lambda_1\Lambda_3 + \Lambda_2\Lambda_3) + 2\Lambda_1\Lambda_2\Lambda_3$; and value of present annuity payable so long as two or more of the three are living is

$$\Lambda_1\Lambda_2 + \Lambda_1\Lambda_3 + \Lambda_2\Lambda_3 - 2\Lambda_1\Lambda_2\Lambda_3.$$

Probability that exactly two are living is $s_2 - 3s_3$; and value of annuity for joint life of the survivors expectant upon the decease of the first of the three is

$$\Lambda_1\Lambda_2 + \Lambda_1\Lambda_3 + \Lambda_2\Lambda_3 - 3\Lambda_1\Lambda_2\Lambda_3.$$

Probability that two or one are living is $s_1 - s_2$; so that value of annuity for life of the two survivors and the survivor of them expectant on the decease of the first of the three is

$$\Lambda_1 + \Lambda_2 + \Lambda_3 - (\Lambda_1\Lambda_2 + \Lambda_1\Lambda_3 + \Lambda_2\Lambda_3).$$

Probability that all are living is s_3; so that value of annuity for the joint lives of the three is $\Lambda_1\Lambda_2\Lambda_3$; and the complementary perpetuity is $1 - \Lambda_1\Lambda_2\Lambda_3$.

The above formulæ thus solve every possible contingency that

can be constructed out of a single set of existing lives, treating them always as a set, and abstaining, as we have done, from dividing the set into individuals; and the results are obtained by merely handling the formulæ without the necessity of entering into any consideration of the various contingencies that may arise. Among the more obvious results are the following, which I presume are well known. The value of a perpetuity expectant upon the death of the survivor of a set of lives is

$$1 - \Sigma(A) + \Sigma(AA) - \Sigma(AAA) + \ldots;$$

and the value of an annuity for their lives and the life of the survivor is

$$\Sigma(A) - \Sigma(AA) + \Sigma(AAA) - \ldots;$$

the former being, (perpetuity) $+$ (annuities for joint lives in even combinations) $-$ (annuities for single lives and joint lives in odd combinations); and the latter being, (annuities for single lives and joint lives in odd combinations) $-$ (annuities for joint lives in even combinations).

Let us now introduce another set of lives B_1, B_2 .. B_m, and construct a *status* out of them which is to coexist with the *status* constructed out of the former set. We have merely to find the probability of the new *status* being in force at a given time, and multiply it by the probability of the old *status*; and when the multiplications are performed, change the small letters into large ones as before. The same process may be extended to any number of coexistent *status*.

Thus, suppose we require the value of an annuity for the lives of A_1 and A_2, and the survivor commencing at the death of B_1; we have merely to multiply $a_1 + a_2 - a_1 a_2$ by $1 - b_1$; which gives, changing the letters,

$$A_1 + A_2 - A_1 A_2 - A_1 B_1 - A_2 B_1 + A_1 A_2 B_1.$$

The complexity of the *status* only increases the labour of working out the results, without in any way adding to the real difficulty of the problem. Thus, suppose we require the value of an annuity for the lives of A_1 and A_2 and the survivor of them, commencing at the death of such one of the set B_1, B_2 and B_3 as shall die second, with the proviso that it shall only be payable so long as there are either exactly two or exactly three of the set C_1, C_2, C_3 and C_4 living. We have to multiply together

$$a_1 + a_2 - a_1 a_2,$$
$$1 - (b_1 b_2 + b_1 b_3 + b_2 b_3) + 2 b_1 b_2 b_3,$$
$$c_1 c_2 + c_1 c_3 + c_1 c_4 + c_2 c_3 + c_2 c_4 + c_3 c_4 - 2(c_1 c_2 c_3 + c_1 c_2 c_4 + c_1 c_3 c_4 + c_2 c_3 c_4)$$
$$+ 2 c_1 c_2 c_3 c_4;$$

and after the multiplications, to change the letters into the corresponding names. The result would be given in this case in terms of no less than 165 annuities on joint lives.

The preceding formulæ may be adapted to the case of contingencies involving not merely lives, but also a fixed term of years, it being assumed that the term, as well as the lives, begins to run *in præsenti*. Thus, if T denote the value of an annuity for a term, and A_1T the value of an annuity for so much of the term as A_1 shall live, all the results above given remain applicable if we substitute T for one of the lives. Thus $A_1 + T - A_1T$ is the value of an annuity for the term and the life, whichever shall last the longest; $A_1 + T - 2A_1T$ is the value of an annuity for the residue of one period (the term or the life) after the other has expired. So $A_1 + A_2 + T - (A_1A_2 + A_1T + A_2T) + A_1A_2T$ is the value of an annuity for two lives and a term, whichever shall last the longest.

In these investigations A_n denotes the present value of an annuity which is payable at intervals of a year from the present time, provided the life be in existence on the day of payment; the complement of this, or $1 - A_n$, must therefore be understood as denoting a perpetuity expectant on the death of A_n, on the assumption that it commences in point of interest at the beginning of the year in which the life drops, or that its first payment takes place at the beginning of the year ensuing the fall of the life. Unless this assumption be made, the two added together will not constitute a present perpetuity. In like manner, an annuity commencing at the death of A_1 and continuing until the death of A_2 is represented by $A_2 - A_1A_2$, on the assumption that it begins in interest at the commencement of the year in which A_1 dies. So, when we represent by $M(1 - A_n)$ the value of a sum M payable on the death of A_n, we mean that it is payable at the commencement of the year of A_n's death; and if it be not payable until the end of that year, we must take a year's discount from it. The diminution of value occasioned by payment being postponed until the fall of a life or other *status* may be regarded as a particular mode of discounting the sum, the rate of discount depending on the nature of the *status*. Thus the value of M pounds payable at the death of the survivor of $A_1 A_2 .. A_n$ is $M(1 - \Sigma A + \Sigma(AA) - \Sigma(AAA) + ..)$; and the value of M pounds payable after the decease of n lives where they are not coexistent, but each life (or rather a life of that value) is nominated on the death of his predecessor, (or more strictly at the beginning of the year in which he dies), is

$$M(1 - A_1)(1 - A_2) \ldots (1 - A_n).$$

If all the lives have the same value P, the value of M pounds

payable after the death of n such lives, each life being nominated at the commencement of the year in which his predecessor died, is $M(1-P)^n$; from which we at once perceive that the present value of a series of payments of M pounds, one made now, another expectant on death of P, the next expectant on the death of another nominee P, and so on *ad infinitum*, is

$$M(1+(1-P)+(1-P)^2+ \ . \ .) \text{ or } \frac{M}{P};$$

and if such an interest be expectant on the fall of an existing life A_n, its value is $M(1-A_n) \div P$. Payments of this nature, which go by the name of fines, are in practice made payable at the end of the year in which the life falls, and the new life is not nominated until that time; this adds one year certain to the value of every life, so that $(1-A_n)$ becomes $v \ (1-A_n)$, and P becomes $1-(1-P)v$, v being $1 \div (1+r)$, where r is the rate of interest per pound per annum. These give as the present value of such a fine $M(1-A_n) \div (P+r)$, which is in substance the same formula as that adopted by Mr. Milne.

The reader will be cautious not to confound $(1-A_1)(1-A_2)$ with $1-(A_1+A_2)+A_1A_2$; the former is the value of a perpetuity expectant on the death of two successive lives, the latter is a perpetuity on the death of the survivor of two concurrent lives of the same respective values. If the former expression be multiplied out, it should be written thus : $1-(A_1+A_2)+A_1.A_2$, so as to show that the last term is a product, and not an expression of the value of joint lives. It is evident that in dealing with successive lives of different values, it is immaterial in what order they are taken.

The formulæ at the head of this paper may be applied not merely to lives, but to any other set of events, the individual probabilities of which are known. Thus if there are n events whose respective probabilities are $a_1, a_2 \ldots a_n$, then

$$\pm \phi^{(p)}(1) \div (2.3..p)$$

is the probability that exactly p will happen; and

$$\phi(1) - \phi'(1) + \frac{1}{2}\phi''(1) - \ldots \pm \frac{1}{2.3..p}\phi^{(p)}(1)$$

is the probability that p or less will happen; and so on:

Dublin, Dec. 18, 1852.

X. On the Temperatures of Conductors of Electrical Currents. By RICHARD ADIE, Esq., Liverpool*.

THE experiments which have led to the discussion in the pages of this Journal between Dr. Tyndall and myself on the subject of the reduction of temperatures by electricity, took their origin some nine years ago in an examination I made of the thermo-electric cross. I remember I found it a most difficult experiment to explain; and as Dr. Tyndall in his last communication in your December Number speaks of the mysterious action of the outlying arms of this cross, I will briefly detail the examination I then made; and by doing so I think I shall show that I have not omitted to vary the currents employed, but, on the contrary, have gone through the widest possible range of currents in order to establish the general law of the unequal heating effect of an electrical current. In the thermo-electric cross, this property of electricity heating bars unequally gives rise to an apparent refrigeration at its centre for electricity of every degree of tension; while M. Peltier, in his announcement of the production of cold by electricity, says that a feeble electrical current must be used to show it.

The annexed diagram represents the thermo-electric cross, where BD is a bar of antimony, CE a similar bar of bismuth, fastened in good metallic contact at their centres A. HB and CI are wires for connecting with a galvanic battery; EF and DG are similar wires for connecting with a galvanometer. When a current is circulated through the joint A in the direction BAC, a galvanometer in connexion with the terminal wires F and G is deflected as if the joint A was heated.
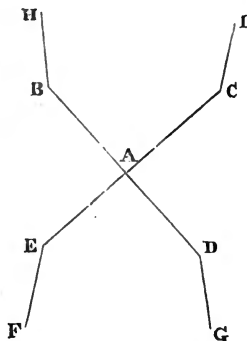
Again, when the current is circulated in the opposite direction CAB, the action on the galvanometer is the same as if the joint A was cooled. This result is constant for electrical currents varied in tension from feeble thermal sources up to powerful currents used in blasting gunpowder. The application of a thermometer to the joint A soon shows that it is heated slightly with the feeble currents and more distinctly with those of higher tension. According to M. Peltier, it should be heated by the currents of high tension, but refrigerated by those of low tension. The explanation of this apparently intricate experiment is, that the galvanometer is not then measuring

* Communicated by the Author.

a thermo-electric current developed at the joint A, but one developed at the joint C, the most highly heated portion of the bismuth conductor AC : for the unequal heating of bismuth conductors see Edinb. Journal, vol. xxxvii. p. 300.   The direction of this current is ACI, from left to right; in metallic contact with it there is another circuit AEF, also from left to right; now where there are two wires for conducting an electrical current offering different degrees of resistance to its passage, the electricity passes along that which offers the least obstruction ; by the path ACI it would require to pass through the battery, while by the path AEF it has a continuous metallic circuit to pass along through the galvanometer ; by this route it finds the least resistance to return to A, so that in the cross the same action on a galvanometer attached to the wires FG can be obtained from heating at C and developing a current moving in the direction ACI, as by cooling at A and generating a current in the direction AEF, both being from left to right.

In my experiments published in 1844, I mention that the nearest approach to the production of cold by electricity was attained with a bismuth and antimony couple, the bismuth bar projecting a little beyond the joint for the purpose of immersing it in water.   On passing a feeble current across the joint from the antimony to the bismuth, the water had its temperature raised 12°; then, on reversing its direction to make it cross the joint from the bismuth to the antimony, the temperature of the water was only raised 2°.   The following repetition of the experiment will show a remarkable effect on the temperatures of metallic conductors.   I cannot make the experiment go the length of producing cold, but I can show the power of an electrical current quickly to reduce the temperature of a heated joint to a point which is stable so long as the current remains unchanged.

In the annexed diagram AB is a bar of antimony, CD a bar of bismuth soldered to the antimony bar at B a little within the extremity of the bar at C.   At C the bulb of a small thermometer CE is applied in contact with a cavity in the point of the bar and enveloped with a little cotton wool, merely sufficient to cover the surface of the bulb.   When this apparatus had been handled, it
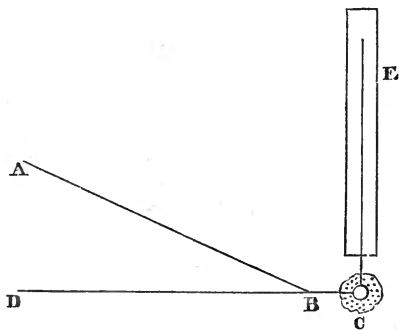
required a period of two hours for the temperature of the thermometer to coincide with that of the atmosphere. The time necessary for this purpose having elapsed, a current from a single pair was passed in the direction DBA ; the thermometer then rose one degree, and remained stationary there. To break the circuit without disturbing the apparatus, I withdrew the plates of the battery from their cell, when the thermometer quickly rose two degrees above where it had stood while the current was circulating, at which point it remained until it began to cool slowly down by radiation. But without waiting for this, the battery was restored to its cell, which had the effect of speedily reducing the temperature of the joint to within one degree of the surrounding atmosphere. This effect of raising the temperature of a joint two degrees by breaking contact and then depressing it by the electrical current, admitted of comparatively rapid repetition. I cannot but help viewing it as a beautiful experiment, which I should be glad if Messrs. Thomson and Tyndall would accept as a portion of the supposed reduction of temperatures by electricity. The elevation of the thermometer CE on breaking contact is what might be expected from the unequal heating effect of the electrical current ; for it is placed at the extremity of the bismuth bar, where the electricity maintains a lower temperature than in any other part; when the source of this inequality is removed, it is natural to expect that the heat will diffuse itself equally through all the parts in metallic contact and thus raise the thermometer. On the return of the electrical current the converse of this takes place, the electricity has the power to re-arrange the temperatures after the same manner that it had left them. I do not think that this experiment shows the absorption of heat by electricity, for there is a continual loss of heat going on by radiation from the surface of the metals ; and if the electricity has the power of concentrating all this loss on one point, it will account for the rapid fall of the thermometer at C.

To the *experimentum crucis* given by Dr. Tyndall, the only objection I have to take is the shortness of the time he allows for obtaining the normal temperatures. The late Mr. Troughton, in his account of his method for graduating astronomical circles, speaks of the time required to allow temperatures to settle after handling, and says twelve hours were not too long for his purpose. In thermo-electrical inquiries the same patience is necessary, and I have often had two hours to wait for the metals to cool. Should Dr. Tyndall be inclined to do me the honour of again experimenting, I think he may prove that he has been performing one of the same class as that I have met with, where a current of electricity has been shown to be able to re-arrange temperatures, and within certain limits to cool a heated joint at the time it is making the new arrangement.

I regret that Dr. Tyndall should have spoken of my views of these experimental inquiries as partial; one of the results obtained should be an answer to that charge, where I have had occasion to establish a general law, that electricity, of whatever tension, heats a conductor unequally. For the rest I am willing to abide his decision, or the decision of those who may come after him, in the matter of the supposed production of cold by electricity, for I feel confident that the onward progress of science never will establish such a law; while to M. Peltier, the originator of these inquiries, due honour will be done; and it will be remembered that when he was experimenting, he had not the benefit of any previous researches to guide him.

Although experiment is the only Cæsar to whom appeal can be made in a case of this kind, still there are advantages to be derived from looking at what may be expected from other known laws of heat and electricity. The admirable researches of Mr. Joule, which have appeared in the pages of this Magazine, show that the resistance to conduction and the heat developed by an electrical current are always in direct proportion; from which it may be inferred, that where there was no resistance there would be no heat; and again, if there was absorption of heat, the converse of resistance should be looked for; what kind of force in a thermo-electrical joint this can be was the difficulty I have felt in reflecting on the supposed fact of the absorption of heat by electricity circulating in conductors which resist its passage in a greater or less degree.

XI. *On the Direction of the Vibrations of the Luminiferous Æther in Plane-polarized Light.* By W. Haidinger[*].

Physical proof of the proposition that the vibrations of the luminiferous æther in plane-polarized light are perpendicular to the plane of polarization.

1. O *BSERVATION.*—Let a dichromatic uniaxal crystal be regarded in a direction perpendicular to the principal axis.

The line AA′ in the adjacent figure is the axis of the crystal, BB′ the direction along which the eye observes the crystal, CC′ the transverse direction perpendicular to the two others.
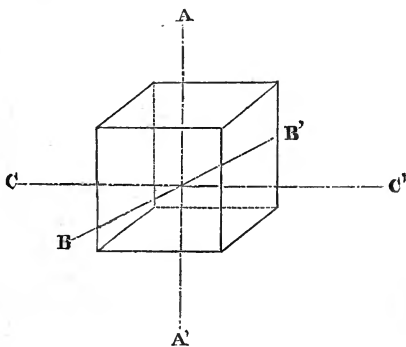
Let the crystal be regarded in ordinary light, and let the phænomena

exhibited be examined through the dichroscopic lens in all azimuths perpendicular to the axis. Of the *two images* of the dichroscopic lens, the ordinary one being polarized in the direction of both images, and the extraordinary one perpendicular to this direction, the images appear *differently* coloured by the two different tints of the dichromatic crystal.

The position of the eye and dichroscopic lens remaining unchanged, if the crystal be turned around the axis AA′ the phænomena remain the same: we observe the upper image O in the adjacent figure, polarized in the direction of the axis and of the transparent colour A, and the lower image E polarized perpendicular to the axis and possessing the other transparent colour of the crystal.
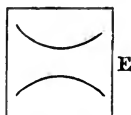
When the crystal is observed in the direction of the axis, the two images of the dichroscopic lens appear of the same colour, that of A, or of the ordinary image in the observation perpendicular to the axis.

2. *Proposition.*—*The vibrations of the æther are perpendicular to the plane of polarization.*

3. *Proof.*—In ordinary light the vibrations of the æther occur in all azimuths perpendicular to the direction of propagation. In plane-polarized light the vibrations are all made in one and the same plane.

In every observation perpendicular to the axis two rays of light appear, which are polarized at right angles to each other. For every single observation, therefore, we have two planes of vibration. In the homogeneous crystal each of these is distinguished by a peculiar colour. For all observations round about the axis we have one direction of vibration coinciding with the axis, and an infinite number of directions of vibration which lie in the plane perpendicular to the axis.

In the direction of the axis only *one* colour appears, but it is observed in all azimuths. To it evidently belong all vibrations which are perpendicular to the axis, and which exhibit the *same* colour when the line of view is perpendicular to the axis.

The *other colour* is not at all observed in the direction of the axis, but in all azimuths round the latter. The only vibrations which can give rise to it are those which take place in the direction of the axis. Observation shows that this colour is polarized perpendicular to the axis. It is, however, caused by vibrations in the direction of the axis, and consequently *the vibrations stand perpendicular to the plane of polarization,* which was to be proved.

The method of proof could also be conducted as follows:—
Let the three lines perpendicular to each other be AA′ the axis, BB′ the longitudinal diagonal, and CC′ the transverse diagonal.

B being the point of view, that colour which is observed about BC B′C′ alone, not in the direction of the axis, must have its vibrations *parallel* to AA′ or to CC′, certainly *not parallel* to BB′, for these would be longitudinal. The point of view being C, the vibrations would, it is equally certain, be parallel to AA′ or parallel to BB′, certainly *not parallel* to CC′, for these again would be longitudinal. To *one* colour, however, belongs only *one* direction of vibration ; BB′ and CC′ are both excluded, and AA′ remains as the *only possible* direction of vibration. This stands perpendicular to the plane BC B′C′. But this is polarized perpendicular to the axis, hence the direction of vibration is perpendicular to the plane of polarization.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 10, "EXPERIMENTS towards the construction of new forms
1852. of Instruments for the correction of Compass Errors
due to the presence of iron in ships ; with investigations on the nature of the attraction of Iron on the poles of Magnets." By Julius Roberts, Esq., Lieut. R.M. Artillery. Communicated by Capt. W. H. Smyth, R.N., For. Sec. R.S.

The object of the author's experiments and investigations is stated to have been the production of an instrument that would, under all variety of circumstances, give a correct (magnetic) meridian direction, or in some way indicate the amount of its own error ; and he considers that he has, at least partially, succeeded in the attainment of this object in the instrument described in this paper, and of which drawings accompany the communication.

In order to determine the nature of the action of a mass of iron on a magnetic needle, the author constructed needles with the magnetic bar wholly on one side of the central support, counterpoised by an arm carrying a weight on the other. In some of these the magnetic bar was straight, in others it was bent in the middle, either the upper or lower half being horizontal. He also constructed a compound needle of two such bent bars, suspended concentrically with two of their contrary poles remote, the other two poles being the one above and the other below the points of support. In order to give magnetic stability to the compound needle, an arc of soft iron was attached to the extremity of the counterpoising arm of each magnet, so as to be in close proximity, though not actually to touch the other. Attached to the counterpoise arms were the halves of a compass-card cut through the east and west points, and so adjusted that the edge of one was vertically over that of the other when the needles were in the same vertical plane, but crossed each other when the needles deviated from the same direction. The author considered that a mass of iron attracting each pole of this compound needle would cause the cards to cross each other, and thus give the

amount of deviation due to that mass, but found that, instead of the
cards crossing, the needles deviated in contrary directions so as to
remain in one straight line, as if they had been rigidly connected.

To obviate this, for the magnet having its unmarked end pointing
south was substituted one having its marked end in that direction,
but of less power than that of which the marked end pointed north ;
and two magnets rigidly connected in the same straight line, with
their marked ends remote, were balanced concentric with the other
two. The author considers that the result of this combination would
be, that this astatic bar would, by the repulsion between its poles
and those of the other magnets, take up a position at right angles
to the magnetic meridian, the other magnets pointing due north and
south.   Further, that the north pointing and south pointing ends of
the compound needle on the half cards being poles of the same name,
that is, both marked, any mass of iron which attracted one end
would also attract the other, and thus the half cards which they
carried would be caused to cross each other ; so that the whole
being properly adjusted, the deviations caused by the iron on the
two needles will be equal, and the error of deviation of either one
will be half the angle between the two. A figure of this complicated
compass accompanies the description.

In conclusion, the author states that the practicability of rendering
the instrument so simple and accurate as to be generally useful, de-
pends on experiments and investigations yet to be made ; and
then enters upon a calculation, from which, however, no definite
results are deduced.

June 17.—" On the Impregnation of the Ovum in the Amphibia
(Second Series revised), and on the direct agency of the Spermato-
zoon." By George Newport, F.R.S., F.L.S. &c.

The author remarks that, having in a former paper shown that the
spermatozoon alone is the impregnatory agent, he endeavoured in a
subsequent communication to the Royal Society, a report of which
is printed in the Proceedings for June 1851, to arrive at some con-
clusion as to the nature of its influence ; and, from the facts he was
then acquainted with, he announced the view that the spermatozoon
appeared to be the organ of a special form or condition of force in
the animal body.   At that time he had no evidence that the sperma-
tozoon penetrates into the coverings of the egg, as he had constantly
found it attached only to the surface.   Since then he has detected
it within the substance of these coverings, and sometimes even par-
tially imbedded in the vitelline membrane beneath them ; but he has
no evidence that it enters the vitelline cavity.   While therefore the
fact of penetration into the envelopes necessitates some revision of
the details of the view announced, he still regards the spermatozoon
as the organ of a special condition or form of force in the animal
structure.

He then proceeds to show the relative duration of vitality in the
spermatozoon and the egg, and points out that that of the former
is shorter than is usually supposed ; that at the temperature of
55° Fahr. it usually is lost in from three to four hours after removal
from the body into water ; but that at a lower temperature it is

retained longer, and that when the spermatic fluid has contained many undeveloped cells, and has been preserved in a temperature of 51° Fahr., it has fertilized at the end of twenty-four hours. The egg loses its fitness to be impregnated very soon after it is passed into water, usually within the first hour, owing chiefly to the endosmosis and expansion of its envelopes. But when retained within the body of the dead frog its vitality is preserved for twenty-four, and sometimes even for forty-eight hours, at a low temperature. He next shows that the results produced by the active vibratile spermatozoa on the dead egg are similar to those which are at first produced on the living one by solution of potass, viz. the yelk becomes shrivelled and contracted, and this result also occurs when decomposing spermatic fluid is applied to it. Having repeated the experiments formerly mentioned (Proceedings, p. 83), that the frog's egg may be fecundated by application of exceeding minute quantities of spermatozoa by means of the head, and even of the point of a small pin, to almost any part of its surface, he shows that there are some parts of the surface more, and some less, susceptible than others ; and that, in a series of careful experiments made with a view to test this fact, he found that when the egg is placed vertically, with the centre of the white surface uppermost, and the spermatozoa are applied to this part, and not allowed to flow over the sides of the egg, fecundation is then but rarely effected ; but that when the centre of the dark surface is uppermost, and the spermatozoon is applied to that part, fecundation of the egg is then almost invariably the result. A fact is also mentioned which is of some value in experiments on artificial impregnation. The chamber which is formed above the yelk in the fecundated egg, as described in the author's former paper, is commenced at the end of the first hour, by the contraction and depression of the upper or dark surface of the yelk ; and thus affords an early proof as to whether or not the egg has been fecundated. When no chamber is formed, it is certain that the egg has not been fecundated. But the chamber may be formed, and the yelk not undergo segmentation ; in which case fecundation has been only partial and incomplete. The motion of the spermatozoon in relation to its function is then examined, and the author states that he regards this motion as only the visible exponent of a peculiar power in the impregnating agent, and as essential to its function, and that it is associated with the material composition and structure of this body, the degree of procreative efficiency of which, he thinks, is indicated by the degree, or intensity of its motive power ; although he believes that some portion of the substance of the body of the spermatozoon is also communicated to the egg in fecundation.

The author then shows that having adopted a mode of examining the egg, beneath the microscope, at the time of the spermatozoa being supplied to it, different from that which he formerly employed, he has been enabled to detect the fact of penetration by the spermatozoon into the envelopes, and its arrival at the vitelline membrane, with great facility. Availing himself of the fact previously ascertained, that impregnation may be effected by the direct application of the spermatozoon by means of the pin's head or point, he put the

fact of penetration to a very positive test beneath the microscope, and found that the spermatozoon always penetrates at the parts to which it is applied, and at no other part of the egg, and that a short time afterwards it may be detected striking into the vitelline membrane, by its thicker or body portion, in a line with the point at which it has entered, and the centre of the yelk, and that he has usually found eggs so penetrated to have been fertilized and produce embryos. Further, that eggs in which no spermatozoa have been seen in contact with the yelk membrane, have usually been unfruitful, although numerous spermatozoa have been observed on their surface. During his experiments the author had an opportunity of examining some eggs which had been impregnated by the natural concurrence of the sexes, and then found that these most fully confirmed the results obtained by artificial impregnation. Spermatozoa were observed sticking into the vitelline membrane for many hours after the time at which the egg must have been fecundated; which the author believes must be within the *first half-hour*, and perhaps within the *first few minutes*; as he has sometimes found spermatozoa close to the vitelline membrane *within one minute* after they had been supplied to the egg. The spermatozoon invariably enters the egg with its thicker or body portion foremost, and passes onwards with a direct but slightly serpentine motion, in a centripetal direction to the vitelline membrane. A large proportion of the spermatozoa never enter the envelopes of the egg, if they happen to come into contact with them laterally, as is frequently the case; they then merely adhere to the surface, but do not fecundate. The greatest number penetrate when supplied to the egg within a few seconds after removal from the male body, and of the eggs from the body of the female. Some experiments are then detailed which the author states arose out of a communication made to him by Mr. Busk, F.R.S. The spermatozoa were narcotized by exposure for eight or ten minutes to the vapour of chloroform, and it was then found that when in this state, while perfectly motionless, as when dead, they did not impregnate. These experiments, in connection with others, seemed to show that the entrance of the spermatozoon into the egg is not the result of an endosmic action of the envelopes, but is that of the operation of a distinct power in the spermatozoon.

The nature of the influence of the spermatozoon is then examined. The author has endeavoured to put this to the test of experiment, *first*, by immersion of eggs, both before, and at the period of fecundation, and during the segmentation of the yelk, in solutions of potass; and *next*, by reducing the bodies of recently-obtained and perfectly active spermatozoa, to a fluid state, by trituration in a glass mortar, prepared for the purpose, and then applying the materials so obtained to the egg immediately it is expelled from the female, and before it has been in contact with water; and consequently at the time it is most susceptible of the fecundatory influence. The experiments by immersion in potass solution showed that the endosmic action of the egg envelopes is exceedingly rapid, as decomposition of the yelk was commenced in some within *three* minutes of the application of the solution. In very weak solution the result was

different, and appeared to be favourable to the action of the spermatozoon. The fluid obtained by trituration of the spermatozoa was applied to several sets of eggs, but no fecundation of the egg was effected by it; the yelks, however, became affected, being in some cases shrivelled and contracted, as when potass solution or decomposing seminal fluid is applied, thus showing that the substance of the broken down spermatozoa had passed to the yelk by endosmosis. These experiments were made at the same time with others made with portions of the same fluid which had not been triturated, and in which the spermatozoa were still active. In these instances fecundation was constantly effected, so that the conclusion deduced from these comparative trials was, that fecundation is not the result simply of the addition of the substance of the body of the spermatozoon to the egg, but primarily seems to be due to a force or dynamic power in the spermatozoon, which is lost when this body has ceased to give evidence of its retention of it, in its power of motion.

The author then proceeds to inquire whether these results do not justify our viewing the spermatozoon as the organ of a special form or condition of force in the animal structure? and states, as he has done on a former occasion (Proceedings, June 1851, p. 83), other grounds on which the hypothesis seems to be supported, pointing out that the spermatozoon, like muscle and nerve, has both general and special anatomical structure and special chemical composition; and that as we have been accustomed to regard the power of muscular contractility as a distinct force, or form of force, of the body,—the same view being held with regard to nerve, the properties of these two tissues being perfectly distinct from each other,—so it appears to be correct to view the property of the spermatic structure; which is not only perfectly distinct from either of these, but different from that of every other tissue in the organization, and is not exercised until the structure itself has been entirely separated from the body of which it originally formed a part.

" On the Functions of the Membrana Tympani, the Ossicles and Muscles of the Tympanum, and of the Eustachian Tube in the Human Ear, with an account of the Muscles of the Eustachian Tube and their action in different classes of Animals." By Joseph Toynbee, Esq., F.R.S. &c.

The author commences his paper by making some observations on the general arrangements of the *ossicula auditûs.* The malleus and incus being firmly connected together by ligaments, are considered as a single bone, forming an elastic arch, the anterior extremity of which is firmly attached to the Glasserian fissure, the posterior to the anterior part of the mastoid cells. This arch is kept steady by the actions of the tensor tympani. The movement of this arch is that of rotation; and it is effected by the tensor tympani muscle. When this muscle contracts, the lower part of the arch, consisting of the handle of the malleus and the long process of the incus, is drawn inwards; by this action the membrana tympani is rendered tense, and the stapes being pressed towards the cavity of the labyrinth, the fluid in the latter is compressed.

The anatomy and attachments of the *stapes* are next minutely described. The base of this bone, generally stated by writers on the physiology of the ear as being attached to the margin of the *fenestra ovalis* by a simple membrane, *ligamentum annulare baseos stapidis*, is shown to possess some points of considerable interest. Instead of a simple margin to which the ligament above noticed is fixed, the base of the stapes is stated to present a circumferential surface for articulation with the *fenestra ovalis*. This circumferential surface, larger at the anterior and posterior extremities than in the middle, is covered by articular cartilage. The surface of the *fenestra ovalis*, to which the circumference of the base of the stapes is applied, is somewhat larger than that of the stapes. It is smooth, has a very compact structure, and is not covered by cartilage.

The base of the stapes is attached to the *fenestra ovalis* by two ligaments. The inner or vestibular ligament passes from the inner margin of the *fenestra ovalis* to the inner margin of the circumferential surface of the base of the stapes. The outer one passes from the outer margin of the *fenestra ovalis* to the corresponding margin of the stapes. These two circular ligaments leave between them a space which may be considered as an articular cavity; this cavity always containing a sufficient quantity of fluid to lubricate the articular surfaces of the bones.

The movements of the stapes are of two kinds, one being produced by the action of the *tensor tympani,* the other by the *stapedius* muscle. If the *tensor tympani* muscle be drawn in the direction of its course, while the cavity of the vestibule has been exposed to view, the base of the stapes will be observed to be slightly projected towards the cavity of the vestibule, and it returns to its normal position as soon as the muscle is left quiescent: the ligaments above described appear to be the organs whereby the stapes is again drawn outwards. In this movement the stapes may therefore be described as passing to and fro within the *fenestra ovalis*, as a piston does in a cylinder. The *second* movement of which the base of the stapes partakes is one of rotation, and it is effected by the stapedius muscle. To show this movement, it is requisite to perform one or two careful experiments. The stapedius muscle being exposed in its canal, while the stapes is left undisturbed in the tympanic cavity which has been laid open, if the muscle be drawn in the direction of its course, the anterior crus of the stapes is observed to move slightly outwards and backwards. In what manner the base of the stapes is affected during this movement of the crura it is difficult to decide, but it would appear probable that its anterior part is drawn outwards from the cavity of the vestibule while the posterior part is pressed inwards, though to a less extent. That the action of the stapedius muscle is to relax the fluid of the labyrinth, is however shown by the following experiment. The tympanic cavity and stapedius muscle being exposed to view, a section is to be made through the cochlea, a small portion of the *scala vestibuli* being left continuous with the cavity of the vestibule. If the stapedius muscle be now drawn in the direction of its course, the fluid in the cut extremity of the *scala vestibuli* is observed to recede

slightly towards the vestibule, and it returns to its former position as soon as the stapedius muscle is left quiet. A second action of the *stapedius* muscle is to act as a laxator of the *membrana tympani*, and it thus appears that the *stapedius* muscle is the antagonist of the *tensor tympani*; and it seems to be brought into action during the act of listening, while the *tensor tympani*, on the contrary, contracts when the ear has to be protected from any loud vibrations.

The next part of the paper is devoted to the consideration of the functions of the *membrana tympani*; which, besides the one usually ascribed to it, viz. of receiving the sonorous vibrations from the air and of conducting them to the chain of bones and thence to the labyrinth, the author considers to be as follows :—

1. To act in conjunction with the ossicles and muscles of the tympanum as the analogue of the iris in the eye, and to exclude from reception by the labyrinth, of such strong vibrations as would be injurious to its integrity ; also, in exactly opposite circumstances, to receive the most faint undulations, which would not be perceived unless the *membrana tympani* were rendered less tense than is the case in ordinary circumstances. The former of the two duties is performed by the *tensor tympani* muscle, the latter by the *stapedius*. The *membrana tympani* is not only of use in preventing powerful sonorous vibrations from compressing too forcibly the expansion of the auditory nerve, but it also protects the labyrinth from the forcible pressure of air or of a foreign substance in the meatus, during a blow on the ear or the introduction of a solid body which presses against the *membrana tympani*.

2. The *membrana tympani* shuts out the air in the meatus from that in the tympanic cavity, and by this means an atmosphere of certain physical conditions is constantly kept in contact with the membranes between the tympanum and labyrinth.

The *second* part of the paper is devoted to the examination of the Eustachian tube in man and animals. Anatomists seem to have inferred that the Eustachian tubes in their natural state are constantly open, and that the air of the tympanic cavities is always continuous with that in the fauces. An examination of the guttural portion of the Eustachian tube in man and animals has led the author to conclude, that excepting during muscular effort, this orifice is always closed, and that the tympanum is a cavity distinct from the outer air. The agents whereby the Eustachian tubes are opened in the human subject, are the muscles of the palate, and it is by their action during the process of swallowing that the tubes are ordinarily opened. That the act of swallowing is the means whereby the Eustachian tubes are opened, is shown by some experiments of which the following may be cited. If the mouth and nose be closed during the act of swallowing, a sensation of fulness and distension is produced in the ears ; this arises from the air, which is slightly compressed in the fauces, passing into and filling the tympanic cavities ; upon removing the hand from the nose, it will be observed that this feeling of distension does not disappear, but remains until the act of deglutition is again performed, but while the nose is not closed ; in this experiment the Eustachian tubes were opened during each act

of swallowing; but during the first act, while they were open, air was forced into the cavity of the tympanum by the contraction of the muscles of the fauces and pharynx, and the orifices were again closed, and remained so until the second act of swallowing, which opened the tube and allowed the air to escape. That the act of deglutition opens the Eustachian tubes, was inferred also from the custom usually adopted of swallowing while the descent in a diving-bell is performed; by this act the condensed air is allowed to enter the tympanum and the sensation of pain and pressure in the ears is avoided. The author then proceeds to show that the *tensor* and *levator palati* are the muscles which are attached to and open the Eustachian tubes in man, and the mode in which they act is pointed out.

The *third* part of the paper is devoted to the examination of the Eustachian tube in animals; and the author arrives at the conclusion, that in Mammalia, Birds, and those reptiles having a tympanic cavity, the Eustachian tubes, as in man, are closed excepting during muscular effort. In some *mammalia* the muscles opening the tubes are, as in man, those belonging to the palate; in others the function is performed by the superior constrictor of the pharynx. In *birds* it is shown that there is a single membranous tube into which the two osseous tubes open; this membranous tube is situated between, and intimately adherent to, the inner surface of each pterygoid muscle, and by which muscles the tube is opened.

The conclusion respecting the influence of the closed Eustachian tubes to which the author arrives, is that the function of hearing is best carried on while the tympanum is a closed cavity; that the analogy usually cited as existing between the ordinary musical instrument, the drum and the tympanum, to the effect that in each it is requisite for the air within to communicate freely with the outer air, is not correct. The view that the sonorous vibrations of the air in a closed tympanic cavity are more effective in impressing the membrane of the *fenestra rotunda* than when it is open to the outer air, is strengthened by the performance of the following experiment with the tuning-fork. If this instrument be made to vibrate by striking it against a firm solid, and if the handle be then placed in contact with the head, the sound at first loud, gradually becomes fainter, and soon ceases to be heard; if at the moment that it has ceased to be heard, a finger be placed over the tragus of one ear, and firmly pressed so as to close the external meatus from the outer air, the sound of the tuning-fork is again heard, and continues to be heard for some seconds; thus showing that the sonorous undulations existing in the external meatus are not sufficiently powerful to affect sensibly the *membrana tympani* until they are wholly confined by the walls of the tube when closed.

The leading results arrived at in the paper are as follows :—

1. That a principal function of the *membrana tympani* muscles and ossicles of the tympanum, is to act as the analogue of the iris in the eye.

2. That the *tensor tympani* muscle, by drawing tense the *membrana tympani*, and by compressing at the same time the fluid in the

labyrinth, protects the ear from the injurious influence of very powerful vibrations.

3. The stapedius muscle, by slightly relaxing the *membrana tympani* and the fluid of the labyrinth, places the ear in a position to be influenced by vibrations of a most delicate character.

4. Another function of the *membrana tympani* is to form part of the resonant walls of the closed tympanic cavity.

5. The guttural orifices of the Eustachian tubes are closed, and the tympanic cavities do not communicate with the cavity of the fauces excepting during certain muscular actions.

6. In man and some mammalia the Eustachian tubes are opened by the muscles of the palate, in other animals by the superior constrictor of the pharynx.

7. In birds there is a membranous tube common to the two osseous Eustachian tubes, and this common tube is opened by the action of the internal pterygoid muscles.

8. For the function of hearing to be perfect, it is requisite that the tympanic cavity should be closed from the outer air.

" An Experimental Inquiry undertaken with the view of ascertaining whether any, and what signs of current Force are manifested during the organic process of Secretion in living animals " (continued). By H. F. Baxter, Esq. Communicated by R. B. Todd, M.D., F.R.S. &c.

The present communication is a continuation of a series of experiments, the first part of which was published in the Phil. Trans. for the year 1848. The object is to show that the changes which occur during the organic process of secretion in living animals are accompanied with the manifestation of current force ; and the principal facts upon which this conclusion is founded are the following :—

1st. It was found that when the electrodes of a galvanometer are brought into contact with the secreted product and the venous blood flowing from the same organ, an effect upon the needle is produced, indicating the venous blood to be *positive.* This fact was established in the liver, kidneys and mammary gland.

2ndly. The effect could not be referred to the heterogeneity of the fluids without assuming that the blood was *acid* and *combined* with the secreted product ; nor could it be referred entirely to *thermo-electric* effects, inasmuch as the current varied in each organ, and was capable of traversing a liquid conductor. The effects, however, may be partly due to *catalytic actions on the combining power of platinum* ; and this last supposition tended to confirm the opinion originally entertained by Wollaston, that the changes which occur during secretion are analogous to those which take place in the *decomposing* cell of a voltaic circle.

Without giving any definite opinion as to the lungs performing the office of a secreting organ, it was found, that when one electrode was in contact with the mucous surface, and the other in contact with the blood in the pulmonary veins, an effect occurred upon the needle indicating the blood (arterial) to be *positive.* This fact appears to afford some explanation of the failures of Muller, Pouillet and of the author in his early attempts to obtain evidence of current

force being manifested when a circuit was formed between an artery and a vein in the living animal.

"On a new Series of Organic Bodies containing Metals." By Dr. E. Frankland, Professor of Chemistry, Owen's College, Manchester. Communicated by B. C. Brodie, Esq., F.R.S.

The author communicates in this memoir the continuation of his researches, a preliminary announcement of which appeared several years ago, upon a new series of organic compounds closely allied to cacodyle in their composition and properties, and which, like that body, are formed by the union of the alcohol radicals with various metals, and are distinguished for their powerful electro-positive character. These remarkable compounds are procured by the action of heat or light upon their proximate constituents, and are thus distinguished from most other organic compounds of this nature by the manner of their formation. The author describes seven of these compounds.

Stanethylium.—When iodide of ethyle and metallic tin are exposed to the influence of heat or light, which is most conveniently done in sealed glass tubes, the tin gradually dissolves in the ethereal liquid, which finally solidifies to a mass of colourless crystals. A quantity of gas, comparatively very small, is generated at the same time. This gaseous product of the reaction proved, on analysis, to be a mixture of hydride of ethyle and olefiant gas, produced from the decomposition of iodide of ethyle by tin into iodide of tin and ethyle, which last is transformed at the moment of its liberation into the two gases just mentioned. The principal and most important reaction, however, consists in the direct union of tin with iodide of ethyle, giving rise to a crystalline body which is the iodide of a new organic radical, *stanethylium.*

By double decomposition the other compounds of stanethylium can be readily formed ; the author has prepared and described the oxide, chloride, bromide and sulphide of stanethylium ; these bodies exhibit a striking resemblance to the corresponding bi-compounds of tin, but are distinguished from them by a peculiar pungent and irritating odour resembling that of the volatile oil of mustard.

If a slip of zinc be immersed in a solution of chloride of stanethylium, dense oily drops soon form on the surface of the zinc, and finally collect at the bottom of the vessel. This oily liquid is the radical stanethylium, which possesses the following properties :—it exists at ordinary temperatures as a thick heavy oily liquid of a yellow or brownish-yellow colour, and an exceedingly pungent odour resembling that of its compounds, but much more powerful. It is insoluble in water, but soluble in alcohol and ether. At about 150° C. it enters into ebullition, but is simultaneously decomposed, metallic tin being deposited. In contact with air stanethylium rapidly absorbs oxygen, and is converted into oxide of stanethylium. Chloride, iodide and bromide of stanethylium are instantaneously formed by the action of chlorine, iodine and bromine, or their hydrogen acids respectively, upon stanethylium. The two first are in every respect identical with the salts above mentioned, and the identity of the bromide is further proved by an ultimate analysis. The

formula of stanethylium is $C_4 H_5 Sn$; that of the oxide $C_4 H_5 SnO$, and that of the bromide $C_4 H_5 Sn Br$. Stanethylium therefore perfectly resembles cacodyle in its reactions, combining directly with the electro-negative elements, and regenerating the compounds from which it has been derived.

Stanmethylium and stanamylium are formed when the iodides of methyle and amyle respectively are exposed to the action of light in contact with tin; their salts are isomorphous with those of stanethylium, but they have not yet been completely investigated.

Zincmethylium.—This radical is formed in an uncombined state when iodide of methyle and zinc are exposed to a temperature of about 150° C. in a sealed tube; the zinc gradually dissolves with an evolution of gas, whilst a mass of white crystals and a colourless mobile liquid refracting light strongly, occupy, after a few hours, the place of the original materials. In this reaction two distinct decompositions take place, viz. the decomposition of iodide of methyle by zinc with the production of iodide of zinc and liquid zincmethylium, and the decomposition of iodide of methyle by zinc with the formation of iodide of zinc and the gaseous radical methyle. The zincmethylium was obtained pure by distillation in an atmosphere of dry hydrogen. Its formula is $C_2 H_3 Zn$, and it possesses the following properties. It is a colourless, transparent and very mobile liquid, possessing a peculiar penetrating and insupportable odour, and boiling at a low temperature. Zincmethylium combines directly with oxygen, chlorine, iodine, &c., forming somewhat unstable compounds. Its affinity for oxygen is even more intense than that of potassium; in contact with atmospheric air it instantaneously ignites, burning with a beautiful greenish blue flame, and forming white clouds of oxide of zinc; in contact with pure oxygen it burns with explosion, and the presence of a small quantity of its vapour in combustible gases gives them the property of spontaneous inflammability in oxygen. Thrown into water, zincmethylium decomposes that liquid with the evolution of heat and light; when this action is moderated, the sole products of the decomposition are oxide of zinc and hydride of methyle.

The extraordinary affinity of zincmethylium for oxygen, its peculiar composition, and the facility with which it can be procured, cannot fail to cause its employment for a great variety of transformations in organic compounds; by its agency there is every probability that oxygen, chlorine, &c. can be replaced atom for atom by methyle, and thus entirely new series of organic compounds will be produced, and clearer views of the rational constitution of others be obtained.

The gaseous methyle formed simultaneously with zincmethylium is identical in composition and properties with the methyle derived from the electrolysis of acetic acid; it was mixed, however, with hydride of methyle generated by the decomposition of accompanying zincmethylium vapour by the water over which the gas was collected.

Zincethylium and zincamylium are homologous bodies formed by similar processes; their investigation is not yet completed.

Hydrargyromethylium.—The author has only yet studied the iodide

of this radical, which is formed by the action of sunlight upon iodide of methyle and metallic mercury. After an exposure of several days to sunlight, white crystals begin to form in the liquid, which finally solidifies to a white crystalline mass; ether dissolves out the new compound and deposits it perfectly pure by spontaneous evaporation.

Iodide of hydrargyromethylium ($C_2 H_3$ HgI) is a white solid, crystallizing in minute nacreous scales, which are insoluble in water, moderately soluble in alcohol, and very soluble in ether and iodide of methyle; it is slightly volatile at ordinary temperatures, and exhales a weak but peculiarly unpleasant odour, which leaves a nauseous taste upon the palate for several days. At 100° C. the volatility is much greater, and the crystals are rapidly dissipated at this temperature when exposed to a current of air. At 143° C. it fuses and sublimes without decomposition, condensing in brilliant and extremely thin crystalline plates. In contact with the fixed alkalies and ammonia it is converted into oxide of hydrargyromethylium, which is dissolved by an excess of all these reagents.

A corresponding compound containing amyle is formed, though with difficulty, under similar circumstances, but the attempts to form one containing ethyle have not yet been successful. Preliminary experiments have also been made with other metals, amongst which arsenic, antimony, chromium, iron, manganese and cadmium promise interesting results.

From a review of the composition and habits of all the organo-metallic bodies and their compounds at present known, the author is of opinion that the view most generally held respecting the constitution of cacodyle, according to which that radical is a conjugate compound consisting of arsenic conjugated with two atoms of methyle, and which view must, if true, be applied to all the organo-metallic bodies, is no longer tenable; and he contends that the behaviour of these bodies clearly indicates that they are compounds formed upon the type of the oxides of the respective metals, a portion of the oxygen being replaced by the several radicals, methyle, ethyle and amyle; the establishment of this new view of their constitution will remove these bodies from the class of organic radicals, and place them in the most intimate relation with ammonia and the bases of Wurtz, Hofmann and Paul Thenard; indeed the close analogy between stibethine and ammonia first suggested by Gerhardt, has been most satisfactorily demonstrated by the behaviour of stibethine with the haloid compounds of methyle and ethyle. Stibethine furnishes us therefore with a remarkable example of the law of symmetrical combination, and shows that the formation of a five-atom group from one containing three atoms can be effected by the assimilation of two atoms, either of the same or of opposite electro-chemical character: this remarkable circumstance suggests the following question. Is this behaviour common also to the corresponding compounds of arsenic, phosphorus and nitrogen, and can the position of each of the five atoms with which these elements respectively combine be occupied indifferently by an electro-negative or an electro-positive element? This question, so important for the advance of our knowledge of the organic bases and their congeners, cannot now long remain unanswered.

" On the Dentate Body of the Cerebellum." By William Brinton, M.D. Communicated by R. B. Todd, M.D., F.R.S. &c.

The corpus dentatum has generally been described and recognised as a wavy line or lamina of grey matter, which is seen in certain sections of the crus of the cerebellum, and contains fibres apparently derived from the restiform body, and the processus e cerebello ad testes. Reil's account, with some vague and conflicting details, gives it a more definitely tubular form, although he is apparently not certain of the continuity of its upper and lower layers posteriorly.

The author explains these somewhat varying descriptions by the physical characters of the tissues investigated, and by the condition —fresh or hardened in spirit—of the specimens examined by different anatomists.

He deduces the form and situation of the recent corpus dentatum by uniting numerous and successive sections made in the three directions of space*. Its arrangement with respect to the fibres of the cerebellum, cerebrum, medulla oblongata, and medulla spinalis, is chiefly deduced from examinations of specimens hardened in alcohol.

By these two methods he is led to the following conclusions, that each corpus dentatum forms a tubular investment to the extremity of the processus e cerebello ad testem; it is open towards the fourth ventricle, and is connected with the opposite body by a commissure of grey matter in its median line. While its interior exclusively receives the fibres of this cerebro-cerebellar peduncle, its exterior radiates fibres to the various lobes of the cerebellum, which fibres, at the bottom of each lobe-stem, become inseparably mixed with a bundle from the restiform body, and with another from the pons varolii.

Its *comparative anatomy* in mammalia corresponds with this view; its *minute anatomy* does not contradict it. And while the *physiological import* of this arrangement eludes all conjecture, the author has little doubt that its anatomical structure and relations are best comprehended in the formula which he would thus assign to it, viz. that of being the cerebro-cerebellar ganglion.

" Proof of a sensible difference between the Mercurial and Air-Thermometers from 0° to 100° C." By J. J. Waterston, Esq. Communicated by Colonel Sabine, R.A., Treas. V.P.R.S. &c.

This paper has reference to a former communication " On a General Law of Density in Saturated Vapours." In the present paper the author states that the formulæ that embrace MM. Dulong and Petit's four standard mean values of the relative expansion of air, mercury and glass, exhibit the temperature by the air-thermometer in advance of the mercurial thermometer, between 0° and 100° C. The amount of difference increases from 0° to 48°, and then diminishes to 100°; the maximum value being 0°·513. The most eminent modern authorities deny the existence of any such difference, or appear tacitly to admit that it is too small to be observed. For

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* Diagrams to this effect accompanied the paper.

this reason no correction was made on temperatures below 100° in Chart No. 2, where indeed it could hardly be perceptible. Although of little practical importance, this difference, if it exists, cannot safely be neglected in theoretical researches, inasmuch as the value of a degree of the mercurial thermometer must in such case be a variable quantity, differing in the ratio of 23 to 24 from 100° C. to 0°.

Having at last obtained satisfactory proof of the existence and amount of the correction between 0° and 100°, he has thought it of sufficient importance to give a detailed account of the method employed to extract the required evidence from M. Regnault's observations on the tension of low pressure steam.

As the law of density, illustrated in Chart No. 2, has clearly reference to the air-thermometer, if a series of observations were *perfectly* correct, they must *perfectly* exhibit this difference—if it really exists —when projected on the chart; because the divergence from the line that joins the points at 0° and 100° must exactly correspond with the correction required at the intermediate temperatures. In short, the line of density would appear as a curve slightly concave towards the axis, and if the proper correction were made on the temperatures, that curve would be converted into a straight line. This view is illustrated by a sketch, in which the curvature is purposely much exaggerated. In this a straight line is drawn, as the gradient of density, and in which the points range if the temperatures are by the air-thermometer. This line is inclined to the axis x of temperature, at an angle of which h is the cotangent (see 'Proceedings,' vol. vi. p. 98). At points in it corresponding to temperatures 50°, 60°, 70°, &c., straight lines are drawn parallel to the axis x; and at distances in these equal to the respective computed differences, straight lines are drawn at right angles to the axis x, and meeting the lines of constant pressure drawn through the corresponding points of the straight line which represents the gradient of density. The curved line passing through the points of intersection, is that in which the points of density range if laid off to the temperatures by the mercurial thermometer.

The author then states that the first attempt was made by obtaining the value of the constants g and h ('Proceedings,' vol. vi. p. 98) from the observation at 50° and 100°; then computing the intermediate tensions at 60°, 70°, &c., and comparing them with observation. The result is given in the following table:—

TABLE I.

50°	60°	70°	75°	80°	90°	100°	
91·98	148·52	232·22	287·27	353·06	523·71	760	Temp. by mercurial therm.
91·98	148·79	233·09	288·50	354·64	525·45	760	Computed tensions.
							Observed tensions.
0	+0·27	+0·87	+1·23	+1·58	+1·74	0	Difference.

The same operations were performed with temperatures corrected. The result is given in the following table:—

Table II.

50°	60°	70°	75°	80°	90°	100°	Temp. by merc. therm.
50°·512	60°·481	70°·413	75°·366	80°·310	90°·171	100	Temp. by air-therm.
91·98	149·03	233·30	288·60	354·60	525·09	760	Computed tension.
91·98	148·79	233·09	288·50	354·64	525·45	760	Observed tension.
0	−0·24	−0·21	−0·10	+0·04	+0·36	0	Difference.

It is remarked that the differences in the first table show a distinct curvature with reference to the chord; while in the second table the accordance with the straight line is as perfect as could be expected.

With the view of bringing these facts out into higher relief and presenting the deflection to the eye on a scale that should at once be relatively correct and very highly magnified, the author computed six values of h from the observations at 90° and 100°, at 80° and 100°, &c., without correcting the temperatures, by the formula

$$h = \frac{t_2 - t_1}{\overline{\left.\frac{p_2}{t_2}\right|^{\frac{1}{6}}} - \overline{\left.\frac{p_1}{t_1}\right|^{\frac{1}{6}}}}$$

These are given in the following table; they are quantities proportional to the cotangents of the inclination of the chords to the axis of temperature.

Table III.

50°	60°	70°	75°	80°	90°	111°·74	Temp. by merc. ther.
158·854	159·025	159·357	159·571	159·816	160·387	165·406	Values of h.
0	+0·171	+0·503	+0·717	+0·962	+1·533	+6·552	Diff. from h at 50°.

The differences in this table are progressively increasing, and their relation to each other is very nearly that of the corresponding differences of the inclination of the chords. They are represented by these inclinations in a figure, and, in order to render the divergence from a straight line more manifest, the scale taken is 10° angular measure to the unit of difference, the length of the chords corresponding with the intervals of temperature below 100°. Joining the extremities of the chords, a magnified view is obtained of the curve determined from observations with temperatures uncorrected. On this the author remarks, that if the temperatures required no correction, the points so determined would lie in a straight line, always taking for granted the integrity of the law of density and the perfect accuracy of the observations.

The next step was to perform the same computation with temperatures corrected. The resulting values of h are given in the following table :—

Table IV.

50°	60°	70°	75°	80°	90°	111°·74	Temp. by merc. ther.
56°·512	60°·481	70°·413	75°·366	80°·310	90°·171	111°·50	Temp. by air-therm.
157·121	156·983	157·006	157·053	157·155	157·428	161·738	Values of h.
0	−0·138	−0·115	−0·068	+0·024	+0·307	4·617	Diff. from h at 50°.

The difference between h at $50°$ in the two tables being $1°733$, a straight line is drawn from the point corresponding to $100°$, making an angle of $17°·33$, with the chord for $50°$ in the uncorrected temperatures; and lines are drawn from the same point making angles $1°·38$, $1°·15$, $0°·68$, &c. with this line, the intersections of which with the distances or chords corresponding to the temperatures, give the points which represent on the same magnified scale, the observations with the temperatures corrected. The author remarks that the line joining these points represents the *empirical* law of density, and that its relation to the standard right line for the temperature $50°$ is precisely what might be expected to subsist between the empirical and true curve of tension. It intersects that line—and *intersection*, not *contact*, is the character of empirical formulæ—at $50°$, $75°$, and $100°$, and at intermediate temperatures diverges from it to the extent of about $\frac{1}{40}$th of a degree at the maximum.

Thus, he states, M. Regnault's observations between $50°$ and $100°$ afford a distinct answer to the inquiry in the affirmative, and it seems no longer possible to doubt that there is a difference between the mercurial and air-thermometers below $100°$; and that its amount does not sensibly differ from the formulæ that embrace MM. Dulong and Petit's standard observations. He annexes these formulæ in a combined form adapted to the Centigrade scale.

$$t_m = \frac{B t_a}{A - t_a} - \frac{t_a^3}{C^3} - \frac{t_a}{D} \quad \dots \dots \dots \dots \dots (1)$$

t_a=temperature by air-thermometer .... $\log B = 3·7145723$
t_m=temperature by mercurial thermometer $A = 4539°·617$
                                       $\log C^3 = 6·43303$
                                         $\log D = 0·78587$

It would be more convenient if we could express t_a in terms of t_m, but this can only be done approximatively, as in the following:

$$t_a = \frac{B t_m}{A - t_m} - \frac{t_m^2}{C^3} - D. \quad \dots \dots \dots \dots \dots (2)$$

If greater accuracy is required, the rule is to find t_a from t_m by (2), then substitute it in (1), and compute t_m; this compared with the true value shows the alteration to be made in t_a to obtain *its* true value.

In conclusion the author observes, it might be expected, without reference to theory, that the curve deduced from the uncorrected temperatures should not show, in its continuation above 100, any abrupt divergence from its regular course; nevertheless from $100°$ to $111·74$ the direction of the chord shows such a break in the law of continuity, and which there appears no way of accounting for, unless by a fault in the observations above $100°$. Their divergence from the law of density is shown in the Chart.

XIII. *Intelligence and Miscellaneous Articles.*

ON THE PERIODIC RETURN OF THE MINIMUM OF SUN-SPOTS;
THE AGREEMENT BETWEEN THOSE PERIODS AND THE VARIA-
TIONS OF MAGNETIC DECLINATION. BY ROD. WOLF.

SINCE the time when the Academy was pleased to take an interest
in my observations establishing a relation between sun-spots and
terrestrial magnetism, I have continued the study of these phænomena,
and have examined at least four hundred volumes, in order to make
myself acquainted with all the observations of sun-spots. The result
is a memoir, which I shall shortly complete, the contents of which
appear to me of sufficient importance to warrant my presenting a
brief report of them. The memoir is divided into six sections, as
follows :—

In the first chapter it is proved, by means of the six different
epochs established by the minimum and maximum of sun-spots, that
the mean duration of sun-spots may be fixed at $11 \cdot 111 = 0 \cdot 038$ year,
so that nine periods are exactly equivalent to a century.

In the second chapter it is proved, that in each century the years
$0 \cdot 00$, $11 \cdot 11$, $22 \cdot 22$, $33 \cdot 33$, $44 \cdot 44$, $55 \cdot 56$, $66 \cdot 67$, $77 \cdot 78$, $88 \cdot 89$ cor-
respond to the minimum of sun-spots. The interval between the
minimum and the succeeding maximum varies. The mean is five
years.

The third chapter contains an enumeration of all the observations
of sun-spots from the time of Fabricius and Scheiner to Schwabe,
always placed parallel with my periods. The agreement is astonishing.

The fourth chapter establishes the remarkable analogies between
the sun-spots and the variable stars, from which it may be admissible
to infer an intimate connexion between these singular phænomena.

The fifth chapter demonstrates that my period of $11 \cdot 111$ years
coincides still more exactly with the variations of the magnetic de-
clination than the period of $10\frac{1}{3}$ years assumed by M. Lamont. The
magnetic variations even follow the sun-spots, not only in their
regular changes, but also in all their smaller irregularities; and I
think that this latter remark will suffice to prove this important
relation.

The sixth chapter treats of a comparison between the solar
period and the meteorological indications contained in a Zurich re-
gister for the years 1000–1800. The result is, in accordance with the
idea of Sir W. Herschel, that the years in which the spots are more
numerous are also drier and more fertile than others; the latter, on
the contrary, being moister and stormy. The aurora borealis and
earthquakes mentioned in that register predominate strikingly in the
years of sun-spots.—*Comptes Rendus*, vol. xxx. pp. 19, 705.

RESULT OF SOME EXPERIMENTS ON THE CHEMICAL ACTION OF
LIGHT. BY J. W. SLATER, ESQ.

These experiments were undertaken chiefly in order to examine
the law proposed by Grothuss, that substances are most readily de-
colorized by rays of light of a colour complementary to their own.
The solutions used for isolating the different rays were, bichromate
of potash for yellow, mixed chlorides of copper and iron for green,

ammonio-sulphate of copper for blue, sulphuric tincture of roses for
red, and water with a little nitric acid for white.   The vessels con-
taining these solutions stood on a shelf about 12 feet from the
ground, and had free sunshine through the day.   In the first place,
five test-tubes containing a strong solution of permanganate of pot-
ash were placed respectively in the five rays. The order of decom-
position was blue, red, white, green, yellow.   The two first were
nearly colourless on the third day, and on the seventh, when opened
and tested, contained no manganese in solution.   The white and
green were not entirely decomposed till the twenty-second day, and
the yellow after eight weeks still contained much permanganic acid.
In order to determine what was the effect of free contact with the
atmosphere upon actinic decompositions, two tubes containing solu-
tion of permanganate, the one sealed, the other open, were fixed in
phials of the ammonio-sulphate of copper, and cemented to the necks
so as to prevent the escape of ammoniacal fumes.   In about eight
hours the solution in the sealed tube had become colourless, whilst
the other retained a deep red tinge.   Peroxide of mercury exposed
to blue light in a sealed tube was much blackened in four days,
whilst a similar portion in an open tube was not affected. Periodide
of mercury (prepared by trituration) in sealed tubes and dry gave
the following order of action,—blue, red, white, green, yellow;
the action of the two latter rays being hardly perceptible, and pos-
sibly due to small portions of blue and red light which had not been
absorbed by the solutions.   In unsealed vessels the action was rather
less rapid.   Covered with water, the iodide was not affected by any
of the rays.   This salt is far more stable than is generally asserted.
The sample used in these experiments had been kept for ten years
in diffused light without any alteration, and even in the blue ray an
exposure of several weeks was required to produce any great effect.
Periodide obtained by precipitation seemed rather less stable.

Iodide of starch, perfectly dry, gave the following order:—
blue, red, white, yellow, green.   The decomposition under the two
latter rays was very slow and imperfect.   When moist, the iodide
was bleached far more rapidly than when dry ; more rapidly also in
open than in closed tubes. Perchloride of mercury was acted on as
follows :—blue, red, white, green.   The yellow ray caused no per-
ceptible formation of protochloride.   This substance, under the blue
ray, gives false results if not perfectly screened from ammoniacal
vapours. Peroxide of mercury gave blue, red (considerable), white,
green, yellow (very slight). Alcoholic tincture of the green colouring
matter of leaves was decomposed very rapidly, and almost simulta-
neously in all the rays ; apparent order, white, red, yellow, green,
blue.   Alcoholic sulphocyanide of iron gave—white, blue, yellow,
green, red.   To determine whether the actinic decomposition of
solutions was in any way influenced by their degree of concentra-
tion, seven sealed phials were placed in white light, each containing
40 parts by measure of concentrated aqueous solution of perchloride
of mercury.   To No. 2 had been previously added 20 volumes of
distilled water ; No. 3, 40 pts. ; No. 4, 80 pts. ; No. 5, 120 pts. ;
No. 6, 160 pts. ; No. 7, 330 pts. The decomposition was most rapid
in No. 6, then Nos. 5 and 4.   The statement that the blue ray de-

prives salts of their water of crystallization, could not be verified with oxalate of ammonia and ferrocyanide of potassium. Weighed portions of these salts, placed respectively under blue, white and green glasses, were found at the end of a month to have suffered no change in weight. It has been observed by Hunt, that a solution of bichromate of potash gives a greenish-yellow precipitate with sulphate of copper in the sunshine. This was found to take place almost equally in all the rays, whether in sealed or open vessels, though a little earlier in the former. The action was found more rapid and more complete in dilute than in concentrated solutions. The latter, after having ceased to deposit, give a further amount of precipitate if diluted. There is a considerable effervescence in the liquid, caused by the escape of oxygen gas. The precipitate is at first of a greenish-yellow, but becomes a yellowish-brown when dry, and shrinks greatly in bulk. Solutions of sulphate of copper and of bichromate, exposed separately to the sun, and then mixed in the dark, give the same precipitate; but if prepared in the dark, no perceptible precipitate is obtained, even on long standing. If the mixture is boiled, however, a precipitate appears. If the precipitate formed by the action of the sun is removed and the liquid boiled, a precipitate appears of a redder shade, probably identical with Bensch's chromate of copper. I have not as yet obtained a quantity sufficient for analysis, either of this or of the former precipitate. Whether the electric and hygrometric conditions of the atmosphere exert any influence upon actinic decompositions, I have not been able to determine with certainty.—*From the Chemical Gazette for Sept.* 1, 1852.

NOTE ON THE INDUCED CURRENTS PRODUCED BY THE TORSION OF IRON. BY G. WERTHEIM.

It has long been known that an iron wire submitted to the action of terrestrial magnetism becomes permanently magnetic when it is made to suffer a considerable and likewise permanent torsion. An attempt is made to explain this fact by saying that torsion acts in the same manner as any other mechanical impulse, that it facilitates the separation of the two magnetic fluids, and that at the same time it communicates to the iron a certain coercive force. This opinion however is founded upon incomplete observation; torsion acts in a manner which is altogether special in compelling the molecules to assume a spiral arrangement, and thus communicating to the substance itself the form which Ampère has assigned to the internal currents. Temporary torsion produces temporary magnetic effects, permanent torsion produces permanent magnetic effects, and these effects cannot be reproduced by any other mode of action of mechanical forces.

Temporary Effects.—A bar of iron, magnetized to saturation, is partially demagnetized at the moment it suffers a temporary torsion, and regains its full magnetic properties as soon as the torsion ceases; in other words, during the torsion it is traversed by an inverse current, and when the torsion ceases by a direct current, whatever may be the direction in which the torsion is effected.

I understand by saturation that state of magnetic equilibrium presented by a bar of iron which has acquired the full amount of mag-

netic properties of which it is susceptible under the influence of a given current, or which, after the interruption of the current, has already lost all the magnetic properties which it is unable to retain permanently ; so long as that state of equilibrium is not established, neither torsion nor the reverse act otherwise than any other mechanical shocks.

The experiment is made in the following manner :—A bar of soft iron, well annealed, 1 metre in length and 15 millims. in diameter, is fixed by one extremity, the other being placed in the centre of a wheel, by means of which it may be twisted in two directions. It carries two spirals, one of which is intended to receive the current of a single element of Daniel's battery, while the other serves as a spiral of induction : the latter is connected with a galvanometer. It is unnecessary to state that the two spirals must be sufficiently far apart that no direct induction can take place. When the current is set up, the needle turns 90° towards the right, the north pole is fixed and the south pole twisted ; when an inverse current is set up, the south pole of the bar is fixed and the north pole twisted, and the needle turns to the left. The following table will render the experiments easily intelligible :—

Torsion towards the right.	Detorsion.	Torsion towards the left.	Detorsion.	Remarks.
90° right	90° right	90° right	70° right	The bar magnetized, any other mechanical shock acts upon the needle in the same direction as torsion.
50 ...	45 ...	20 ...	35 ...	
3 left	50 ...	12 left	45 ...	The bar being magnetized to the highest degree, the application of any force which does not produce torsion leaves the needle at 0.
5 ...	35 ...	20 ...	45 ...	
20 ...	30 ...	20 ...	30 ...	
70 ...	60 left	90 ...	75 left	On reversing the current, the same phænomena are presented in an inverse direction.
50 ...	50 ...	5 right	50 ...	
12 ...	40 ...	14 ...	40 ...	
30 ...	42 ...	30 ...	40 ...	
90 right	20 right	90 ...	10 right	The current was interrupted ; the bar became demagnetized at first to saturation.
50 ...	0 ...	35 ...	15 ...	
20 ...	5 left	30 ...	15 ...	
20 ...	10 ...	12 ...	10 ...	

The coercive force of any kind of iron may be measured by the number of torsions necessary to make it reach the point of saturation.

Permanent Effects.—When a bar of iron or a bundle of iron wire has been magnetized by means of a violent permanent torsion, under the influence either of the terrestrial or any other current, it does not behave like an ordinary magnet. *When twisted or untwisted temporarily in the direction of the permanent torsion, magnetization is caused, or a direct current ; and twisting or untwisting in the contrary direction causes demagnetization, or an inverse current.*

This experiment may be easily made with two bundles of the same iron wire, which are suspended vertically, and twisted in such a way as to make of one a right-handed helix and of the other a left-handed one ; both have the north pole upwards and the south pole below. On their introduction into the spiral, the needle turns towards the right. But when, after having fastened the north pole of each

bundle, the south pole is temporarily twisted in the same direction, inverse currents are produced according as one or other of the two bundles are twisted :—

	Torsion to the right.	Detorsion.	Torsion to the left.	Detorsion.
Right-handed helix, which has originally been twisted from the right to the left	15 left	10 right	15 right	14 left
Left-handed bundle	15 right	15 left	15 left	15 right

Consequently it is only necessary to add to the apparatus a commutator, which reverses the direction of the current after each semi-oscillation, to obtain, by means of turning vibrations, a continuous current, which may be rendered very intense.

It appears to me that these facts will lead to the solution of important theoretical questions. I purpose to discuss them in a memoir upon the torsion of solid bodies in general, with which I have long been occupied.—*Comptes Rendus,* vol. xxxv. pp. 19, 702.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1852.

Chiswick.—November 1. Uniformly overcast : drizzly : slight rain. 2. Heavy rain : clear. 3. Clear : heavy rain : clear. 4. Foggy : large white clouds : slight rain. 5. Overcast : exceedingly fine : rain at night. 6. Rain : very fine : boisterous. 7. Cloudy and boisterous : rain : clear and windy. 8. Overcast. 9. Uniform haze : overcast : clear. 10. Overcast. 11. Drizzly : rain : excessively heavy rain at night. 12. Rain and boisterous throughout. 13. Uniformly overcast : foggy : drizzly. 14. Rain : very fine : extraordinary heavy rain. 16. Barometer very low : fine : rain at night. 17. Fine. 18. Densely clouded : fine : clear. 19. Foggy : overcast. 20. Dense fog : constant rain. 21. Rain. 22. Densely overcast. 23. Fine : rain at night. 24. Overcast. 25. Foggy : fine : rain. 26. Boisterous, with heavy rain. 27. Clear and fine. 28. Showery. 29,30. Overcast.

Mean temperature of the month .... 47°·38
Mean temperature of Nov. 1851 .... 35 ·86
Mean temperature of Nov. for the last twenty-six years ... 43 ·08
Average amount of rain in Nov. .... 2·30 inches.

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

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

Mean temperature of Nov. for twenty-five years .... 42°·60
Mean temperature of this month .... 41 ·52
Average quantity of rain in Nov. for six years .... 4·83 inches.

On the 11th from 6·50 till 7·10 P.M. very red aurora. A bow in the southern hemisphere, from which it proceeded to the zenith, particularly from the ends towards the east and west.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; at Boston; and by the Rev. C. Clouston, at Sandwick Manse, Orkney.

Days of Month. 1852. Nov.	Barometer — Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8½ p.m.	Thermometer — Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Wind — Chiswick 1 p.m.	Boston	Orkney, Sandwick	Rain — Chiswick	Boston	Orkney, Sandwick
1.	29·773	29·675	29·30	29·28	29·22	62	57	50	51	50	s.	se.	s.	·02	…	·05
2.	29·620	29·538	29·55	29·26	29·25	60	48	55	50	49	sw.	sse.	s.	·61	·14	·34
3.	29·748	29·658	29·20	29·27	29·47	56	32	58·5	48	42	sw.	wsw.	ssw.	·16	·26	·06
4.	29·840	29·689	29·40	29·53	29·65	60	44	42	47	44	s.	sw.	s.	·01	·05	…
☾ 5.	29·391	29·306	28·97	29·60	29·14	63	40	53	49	51½	s.	ese.	se.	·05	·10	·01
6.	29·843	29·737	29·32	29·22	29·39	58	52	43·5	46	44	sw.	ssw.	sse.	·05	·04	·66
7.	29·979	29·862	29·33	29·25	29·21	60	54	58	47	50½	sw.	s.	sw.	·15	·04	…
8.	30·146	30·048	29·46	29·47	29·79	62	56	55	47½	44	sw.	wsw.	nw.	…	·09	·14
9.	30·164	30·045	29·70	30·00	30·06	59	48	53	45	37	nw.	calm	nne.	·06	·06	·15
10.	30·032	29·937	29·60	30·09	30·09	50	42	43	41½	36	nw.	nw.	nne.	1·02	·29	·20
● 11.	29·706	29·408	29·37	29·94	29·90	54	48	41	39	34	s.	e.	…	·13	·93	·05
12.	29·558	29·487	29·25	29·97	29·97	49	44	46	37	34	e.	e.	nne.	·22	·04	·08
13.	29·496	29·412	29·20	29·79	29·74	48	44	43	39	33	e.	se.	n.	1·24	·11	·08
14.	29·362	29·225	29·05	29·66	29·60	54	46	42	32	37½	e.	s.	e.	·34	·43	·08
15.	29·071	28·989	28·70	29·43	29·28	58	49	48	34	44½	sw.	ese.	nne.	·12	·25	·62
16.	29·057	28·848	28·50	29·11	28·88	60	49	52	41	45	sw.	ssw.	se.	·11	·10	·11
17.	29·189	29·070	28·67	29·00	29·02	56	42	48	46	43	sw.	s.	wnw.	·11	·04	·11
☽ 18.	29·713	29·279	28·83	29·00	29·16	40	27	42	45	42	sw.	w.	…	·24	…	·56
19.	29·763	29·548	29·40	29·39	29·40	53	36	35·5	43	42	se.	sse.	s.	·35	…	…
20.	29·550	29·304	29·10	29·40	29·38	53	43	42·5	39	39	s.	sse.	e.	·10	·03	·08
21.	29·096	29·034	28·73	29·31	29·26	54	45	47	34	39	se.	e.	…	·04	·22	·22
22.	29·299	29·042	28·76	29·12	29·15	47	29	42	42	40½	e.	nw.	w.	·18	·48	…
23.	29·512	28·965	29·13	29·15	29·27	44	38	32·5	43	40½	sw.	nw.	nw.	…	…	·08
24.	29·872	29·266	29·04	29·53	29·81	47	27	32	38	38½	s.	n.	n.	·22	…	·22
25.	29·947	29·689	29·60	29·96	29·83	54	41	32	34	44	s.	s.	se.	·70	·47	·04
○ 26.	29·483	29·426	29·00	29·16	28·73	56	38	52	42	44	sw.	s.	ssw.	…	·05	·13
27.	29·832	29·692	29·26	29·16	29·23	49	39	42	42	42	w.	wsw.	ssw.	·08	…	·30
28.	29·670	29·450	29·10	29·21	29·38	51	28	42	34½	37	nw.	ssw.	nnw.	…	…	·12
29.	29·680	29·507	29·20	29·64	29·83	39	34	37	34½	34	n.	h.	nw.	…	·05	·94
30.	29·954	29·783	29·54	29·78	29·47	42	25	32	37	41	n.	n.	s.	·05	·05	·05
Mean.	29·644	29·463	29·17	29·456	29·452	53·26	41·50	44·9	41·83	41·21				6·20	4·32	5·18

XIV. *On the Refraction of Sound.* By C. Sondhauss[*].

DURING several years the thought has been present with me, that sonorous waves expanding in the air must, on passing into another medium in which they possess a different velocity of propagation, undergo a change similar to that suffered by waves of light; or in other words, that the rays of sound which fall obliquely on the limiting surface of both media must be deflected to or from the normal in a manner similar to the rays of light. To arrive at a decision upon this point, it was necessary to construct a lens-shaped body of such a nature as readily to take up the motion of the sonorous rays, and to concentrate them by refraction in a point, where the sound might be plainly heard, as it is in the focus of a concave mirror. A lens formed of a solid substance did not appear to me suitable for the purpose. The thought of forming a lens-shaped bladder from a thin membrane therefore occurred to me; this bladder, when filled with a gas of greater density than the air, for example with carbonic acid, might furnish a convex lens suitable for the refraction of sound.

As a first experiment, I made use of an air-balloon of gold-beater's skin, about a foot in diameter. After having filled it with carbonic acid I suspended it freely; fixed at a distance of about a foot from it a weak-ticking watch, and listened at the opposite side of the balloon, in the direction of its axis, to the ticking. As often as I made the experiment, I thought I heard the ticking for a distance of several feet behind the balloon, in

the direction of the axis, more plainly than at an equal or less distance in any other direction; the difference, however, did not seem to me to be sufficiently considerable to furnish a conclusive proof of the refraction of sound, or to justify the publication of the experiment. I next endeavoured to obtain a membranous convex lens instead of the balloon, and succeeded finally in constructing one of post-paper. I obtained, however, no result with it, probably because the spherical paper surfaces were too stiff to receive and transmit the weak undulations excited by the ticking of the watch.

Otherwise occupied, I let the experiment rest until after the discovery of collodion. By the kindness of M. Müller, apothecary in Breslau, who exhibits great skill in the preparation of collodion balloons, I was enabled to construct a large lens which was suited to the experiments intended. M. Müller prepared for this purpose a gigantic balloon; out of it two segments were cut and attached to a hoop of lead, so that the lens-shaped body thus obtained consists, when blown out, of a low cylinder, whose ends are spherical segments. The hoop of lead has a diameter of eleven and a third Paris inches, and a breadth of two and a quarter; one of the two spherical segments possesses a height of $2''$ $1'''\cdot5$, the other a height of $2''$ $2'''\cdot5$.

In order to fix this apparatus conveniently at two diametrically opposite points of the hoop, two lead tubes are soldered which serve as axes, and rest upon two wooden pillars, so that the lens can be turned round a horizontal axis and thus easily set in either a perpendicular or horizontal position; by lengthening or shortening the two wooden pillars the lens can be set either higher or lower. In the annexed woodcut the lens and its supports are represented; at D and E are two other lead tubes soldered to the hoop, and made use of to inflate the lens. I fill it from below, connecting D by means of an elastic tube with the carbonic acid apparatus, and permitting the air to issue through the upper orifice E. When the

lens is completely inflated, and the air driven out by the carbonic acid, the elastic tube is first removed from the under opening D and the tube closed with a good cork; the upper orifice is then tightly closed.
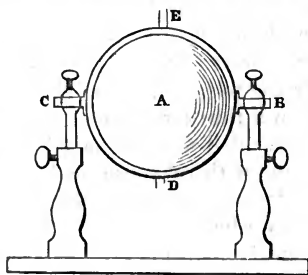
As no suitable unenclosed space was at my disposition, the observations were made in a room. In the first place, I observed

the sonorous waves which were generated by the ticking of a watch. The watch was placed at the same height as the centre of the lens, and the latter received upon its membranous surface the waves proceeding from the watch, and transmitted them through the carbonic acid to the air at the other side. In repeated experiments it was found that the ticking of the watch was distinctly audible at a point in the axis of the lens, while out of the axis, at an equal or even less distance, the ticking was either less audible or not at all to be heard. Inasmuch as a subjective error might readily creep into such experiments, I have not relied upon the judgement of my own ear alone, but have subjected the experiment to the judgement of others, who all found the phænomenon to be exactly as I have stated it. I can here call upon the testimony of such observers as Bunsen, Duflos, Frankenheim, Gebauer and Kirchhof, who have all been kind enough to attend my experiments. To arrive at a certain decision, the experiment was so modified, that, while the observer sat with closed eyes at one side of the lens and listened to the ticking of the watch at the other, the lens was alternately removed and brought into its place. It was thus found that the ticking ceased every time the lens was withdrawn, and immediately heard again as soon as the lens was replaced between the watch and the observer. When the hand was held before the watch so as to intercept the sonorous waves and prevent them from falling on the lens, the ticking was not heard, but was immediately audible when the hand was removed.

It would have been very interesting to me to determine the distance of the point of convergence of the rays of sound corresponding to different distances between the watch and the lens. The ear, however, is not suited to observations of this character; and I have scarcely ever been able to decide at what distance from the lens the ticking of the watch was loudest. The greater or less attention, the wearying of the observer, the irregular stroke of the watch, and the circumstance that in the neighbourhood the same tranquillity does not always reign, are sources of perpetual error in these observations; I will therefore for the present merely state generally, that the place where the ticking of the watch appeared to be most plainly heard moved to a greater distance from the lens when the watch was brought closer to the latter, and will introduce merely a single series of observations, in which I have endeavoured to determine to what distance behind the lens the ticking could be heard, when the watch was suspended at various distances.

Distance of the watch from the centre of the lens.		Distance of the ear from the centre of the lens.
ft.	in.	ft.
	8	10
1		10
1	3	9
1	6	8
2		6
2	9	4
3		3
3	6	2
4		2
5		2

In the two last observations the lens had become flaccid by the escape of carbonic acid, and was filled again by the blowing in of air. Hence the refraction is less than in the foregoing observations. I would remark, that, where the watch was brought more closely to the lens, so that its distance amounted to about one or two feet, its ticking was also audible immediately behind the lens, and from this forward to the distance given above. Where, however, the watch was placed at a distance of from four to five feet from the lens, I could hear no sound immediately behind the lens, but heard it plainly when the ear was placed at a distance of one or two feet from the latter. In the last four observations the ticking was heard most distinctly when the ear was $1'\ 3''$ distant from the centre of the lens.

The distance of the point of convergence for parallel rays of sound, or the focal distance of the collodion lens, could not, for the reasons before given, be obtained; but judging from the experiments hitherto made, it cannot be much beyond a foot. According to this, the index of refraction for sound in its passage from air into carbonic acid must be somewhat greater than the ratio of its velocities in these two media. If we assume, in accordance with the experiments of Dulong, the velocity of sound in air to be 333 metres, and in carbonic acid 261·6 metres, and regard the ratio of both, $n = 1 \cdot 272$, as the index of refraction of the carbonic acid lens, the radii of the two spherical collodion membranes amounting to about $8'' \cdot 5$, we obtain, according to the known formula for the focal distance of glass lenses,

$$\frac{1}{f} = (n-1)\left(\frac{1}{R} + \frac{1}{r}\right);$$

for the focal distance of parallel rays of sound refracted by my lens,

$$f = 15'' \cdot 6,$$

which value is too great. If, however, the exponent of refraction be taken at $\frac{4}{3}$ or 1·333, we obtain

$$f = 12''·7,$$

which coincides with the experiments.

I will take the liberty of mentioning in conclusion one or two other experiments which I have made with the sound-lens. Words and entire sentences spoken softly by a person standing at one side of the lens were distinctly understood by an observer whose ear was placed at a suitable distance in the axis at the other side; so that between these two persons a conversation might be held which would be scarcely, if at all, understood by those standing around. I have also made experiments with more intense sonorous waves, which are likewise refracted and brought to convergence by the lens. In the place of the watch a small organ-pipe was fixed, blown through, and its tone at the opposite side of the lens observed. Although the sonorous waves reflected from the walls of the chamber and reaching the ear of the observer exercised a disturbing influence, it was shown in the most decided manner that the tone of the pipe behind the lens was much stronger. The ear of the observer, whenever it was brought into the axial line, experienced a regular shock. The tone of the pipe undergoes by its passage through the lens no alteration as regards height and character.

Later Addition.

With regard to these experiments on the refraction of sound, I would further remark that I corroborated them afterwards in an objective manner. As source of sound I made use of an organ-pipe, and permitted the waves which were refracted and concentrated by the sound-lens to act upon the tin tube *abcd*. They enter the wide mouth *cd*, and excite tremors in a fine membrane drawn over *ab*, which are exhibited by the motion of sand strewn upon the membrane.
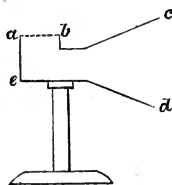
XV. *The Supernumerary Bows in the Rainbow arise from Interference.* By J. H. Pratt, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

1. THE question having been started, "Do the supernumerary bows in the rainbow arise from interference?"—not knowing any book where the doubt was resolved, I set to work to make the necessary calculations; and as they may be of interest to your readers, I forward them to you for publication, should you see fit to give them a place in your Magazine.

2. Let r = rad. of a drop of rain.

μ = index of refraction for water.

= 1·331 for red, 1·344 for violet rays.

ϕ = angle of incidence of a ray.

ϕ' = angle of refraction, $\sin \phi = \mu \sin \phi'$.

θ = angle of inclination of an incident ray with the same when it emerges after two refractions and one intermediate reflexion within the drop.

ϕ_1 and θ_1 the values of ϕ and θ corresponding to those rays which emerge parallel.

D = distance of drop of rain from the spectator.

3. Let the plane of the paper pass through the sun, the eye of the spectator, and the centre of any given drop of rain; and let (fig. 1) S*abc*E be the course of any ray from the sun, twice refracted and once internally reflected by the drop.

Fig. 1.
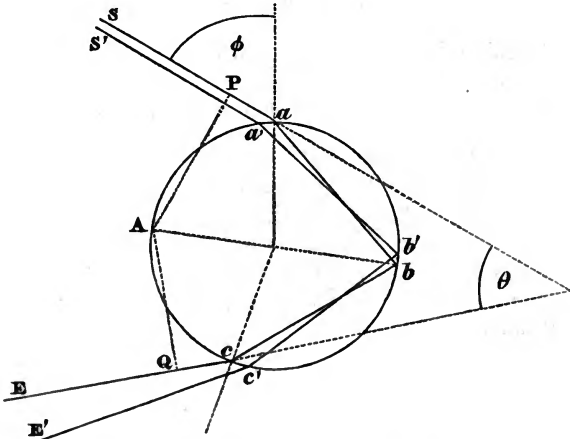
It is easily seen that $\theta = 4\phi' - 2\phi$; putting this value of ϕ' in $\sin \phi = \mu \sin \phi'$, we have

$$\sin \phi = \mu \sin \left(\frac{1}{4}\theta + \frac{1}{2}\phi \right). \quad . \quad . \quad . \quad . \quad (1)$$

4. When the rays emerge as well as enter parallel, θ remains constant for that pencil while ϕ varies; hence by differentiating (1) with respect to ϕ,

$$\cos \phi = \frac{1}{2}\mu \cos\left(\frac{1}{4}\theta + \frac{1}{2}\phi\right). \quad . \quad . \quad . \quad (2)$$

Let ϕ_1 and θ_1 be the values of ϕ and θ which satisfy these equations. Adding the squares of (1) and (2) and reducing, we have

$$\sin \phi_1 = \sqrt{\frac{4-\mu^2}{3}}.$$

Then (1) gives

$$\sin\left(\frac{1}{4}\theta_1 + \frac{1}{2}\phi_1\right) = \frac{1}{\mu}\sin\phi_1.$$

These two formulæ give

$$\phi_1 = 59° \ 32', \quad \theta_1 = 42° \ 24' \text{ for red rays };$$
$$\phi_1 = 58° \ 46', \quad \theta_1 = 40° \ 30' \text{ for violet rays.}$$

The breadth of the primary bow is therefore $= 1° \ 54'$.

5. In the following calculations only the red rays will be considered; and we shall find the following values useful:

$$\sin\frac{1}{2}\phi_1 = {\cdot}496469, \quad \sin\phi_1 = {\cdot}861924.$$
$$\tan\phi_1 = 1{\cdot}7, \quad \cos 2\phi_1 = -0{\cdot}485827.$$
$$\sin\theta_1 = {\cdot}674517.$$

6. The supernumerary bows are always seen close to the inner limit of the primary bow; and $\theta_1 - \theta$, or the angular distance of any point in them from the red of the primary bow, varies from $2°$ to $4°$, or reducing angles to arcs, from $\frac{1}{29}$th to $\frac{1}{15}$th part of the radius.

To find how θ and ϕ vary together for points within the limits of the supernumerary bows, let $\theta = \theta_1 - \beta$ and $\phi = \phi_1 - \alpha$; α and β will both be small. Substitute these values in (1),

$$\therefore \sin(\phi_1 - \alpha) = \mu \sin\left(\frac{1}{4}\theta_1 + \frac{1}{2}\phi_1 - \frac{1}{4}\beta - \frac{1}{2}\alpha\right).$$

Expand in powers of α and β, and reduce, observing that θ_1 and ϕ_1 satisfy equations (1) and (2); the result is

$$\alpha^2 \tan\phi_1 - \frac{\alpha^3}{3} = \beta + \frac{(2\alpha+\beta)^2}{16}\tan\phi_1 - \frac{(2\alpha+\beta)^3}{96}.$$

For a *first* approximation neglect β^2 and $\alpha^3 \ldots$, then

$$\beta = \frac{3}{4}\tan\phi_1 . \alpha^2 = \frac{5}{4}\alpha^2, \quad \because \tan\phi_1 = 1{\cdot}7.$$

For a *second* approximation retain α^3, but reject α^4, and therefore β^2; reduce and transpose, divide by the coefficient of α^2, and put $\tan\phi_1 = 1{\cdot}7$; then

$$\alpha^2 - \frac{1}{3}\alpha\beta - \frac{1}{5}\alpha^3 = \frac{4}{5}\beta,$$

or
$$\alpha^2 - \alpha\left(\frac{1}{3}\beta + \frac{1}{5}\alpha^2\right) = \frac{4}{5}\beta.$$

Put the first approximate value of α^2 in the second term,

$$\therefore \alpha^2 - \frac{1}{2}\alpha\beta = \frac{4}{5}\beta,$$

$$\therefore \left(\alpha - \frac{1}{4}\beta\right)^2 = \frac{4}{5}\beta, \;\; \because \beta^2 \text{ is neglected,}$$

$$\therefore \alpha = \pm\frac{2}{5}\sqrt{5\beta} + \frac{1}{4}\beta.$$

Call these two values α and $-\alpha'$.

7. This shows that for every value of β there are two values of α, one positive and the other negative, and nearly equal to each other; that is, there are always two values of ϕ, one less than ϕ_1 and the other greater, which give the same deviation $\theta_1 - \beta$; and therefore the corresponding rays emerge parallel to each other. It is the interference of these rays, which take different courses in the drop of rain and emerge parallel, which causes the supernumerary bows. This I proceed to show.

The manner in which these rays pass through the drop is shown in the accompanying figure (fig. 2). The dark line in the middle is the ray of least deviation, cE and c'E$'$ emerge parallel, and the pencils mix on reaching the eye.

Fig. 2.
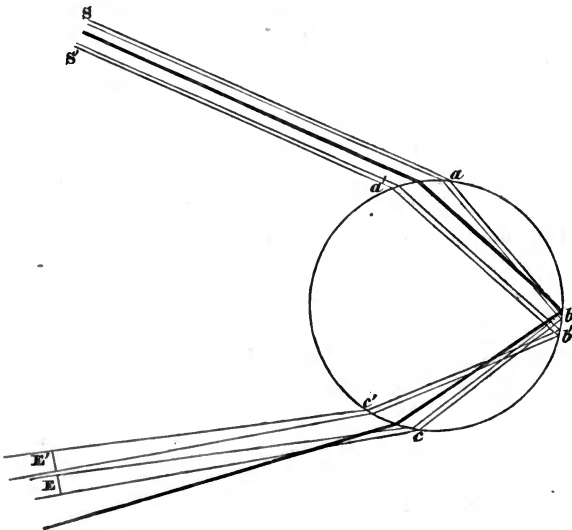
8. I will first find the intensities of these emergent rays, and compare them.

A cylindrical pencil of parallel rays falling on the drop of rain will emerge through an element of the surface of the drop, of dimensions different to those of the element through which it passed on entering; and the pencil will emerge, in the general case, not parallel, but diverging, both in the plane of the paper and at right angles to that plane. It will be necessary to calculate this in order to find the intensity of the emerging light.

9. And first, I will calculate the divergence at right angles to the paper.

Fig. 3.
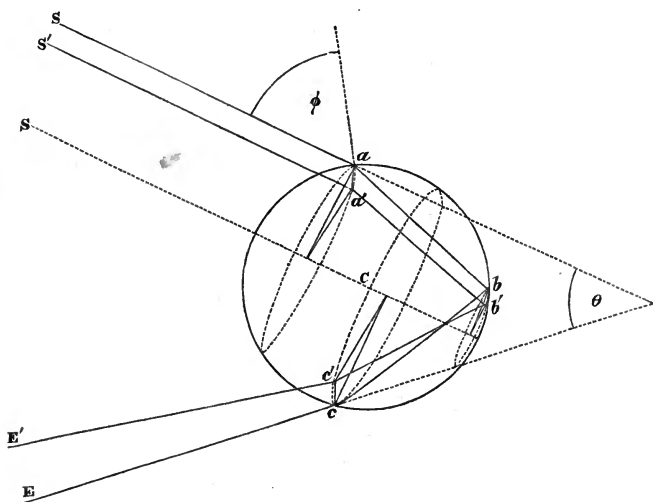
Let S*abc*E (fig. 3) be the course of a ray through the drop. Draw S*c* parallel to the incident rays through the centre of the drop. Let the figure revolve round S*c* through a very small angle, so that the above ray may assume the position of a new ray S'*a'b'c'*E'. Thus *aa'* is the breadth of the incident pencil measured at right angles to the paper, $=w$ suppose. Then the angle through which the plane revolves $= \dfrac{w}{r \sin \phi}$; and

$$cc' = \frac{w}{r \sin \phi} \times r \sin (\theta + \phi) = w \frac{\sin (\theta + \phi)}{\sin \phi}.$$

This is the width of the pencil when it is emerging. The rays *c*E, *c'*E' make, each of them in its own plane, an angle θ with

the intersection SC of the planes.   Hence these rays make with each other the angle $\sin \theta \times$ angle of inclination of the planes $= \dfrac{w}{r} \dfrac{\sin \theta}{\sin \phi}$; and therefore at a distance D from the drop, the rays are spread, at right angles to the paper, through the space

$$w \frac{\sin (\theta + \phi)}{\sin \phi} + w \frac{D}{r} \frac{\sin \theta}{\sin \phi}.$$

The first term may always be neglected with respect to the second.   For D must be an enormous number of multiples of r, the radius of a drop of rain; and the only case in which the first term might *appear* to be large and the second small is when $\phi = 0$, and therefore $\theta = 0$.   But in this case, (1) in art. 3 gives $4\phi = \mu\theta + 2\mu\phi$, and the above expression becomes

$$w \frac{4 - \mu}{\mu} + w \frac{D}{r} \cdot \frac{4 - 2\mu}{\mu},$$

and the first term is always extremely small in comparison of the second.   Hence the space through which the pencil spreads at right angles to the paper

$$= w \frac{D}{r} \frac{\sin \theta}{\sin \phi}.$$

10. Now I will calculate the divergence in the plane of the paper. (See fig. 1.)

Let SabcE and S$'a'b'c'$E$'$ be the course of two rays of the incident pencil in the plane of the paper.   The arc

$$abc = (2\pi - 4\phi')r = (2\pi - \theta - 2\phi)r,$$

$$\therefore \text{arc } abc - \text{arc } a'b'c' = \left(\frac{d\theta}{d\phi} + 2 \right) r\delta\phi,$$

$$\therefore cc' = cba - c'b'a' + aa' = \left(1 + \frac{d\theta}{d\phi} \right) r \cdot \delta\phi; \ \ \because aa' = -r\delta\phi;$$

and the width of the pencil at c

$$= cc' \cos \phi = \left(1 + \frac{d\theta}{d\phi} \right) r \cos \phi \cdot \delta\phi;$$

and the width at a (or w) $= r \cos \phi \cdot \delta\phi$;

$$\therefore \text{width at } c = w \left(1 + \frac{d\theta}{d\phi} \right).$$

The rays cE, c'E$'$ make an angle $\delta\theta$ with each other, or $\dfrac{d\theta}{d\phi} \cdot \dfrac{w}{r \cos \phi}$; and therefore at a distance D from the drop of rain, the pencil

spreads in the plane of the paper through the space

$$w\left(1+\frac{d\theta}{d\phi}\right)+w\frac{\mathrm{D}}{r}\frac{d\theta}{d\phi}\sec\phi.$$

This is reduced to w when the deviation is a minimum, and the pencil emerges parallel in the plane of the paper.

11. From the last two articles, it appears that the ratio of the spaces occupied by the emergent pencil at a distance D and by the incident pencil

$$=\frac{\mathrm{D}}{r}\frac{\sin\theta}{\sin\phi}\left\{1+\left(1+\frac{\mathrm{D}}{r}\sec\phi\right)\frac{d\theta}{d\phi}\right\}.$$

Put $\theta_1-\beta$ and $\phi_1-\alpha$ for θ and ϕ, and reduce, observing that θ_1 and ϕ_1 satisfy equations (1) and (2), and neglecting higher powers of α than the first, and therefore β, and we have the above ratio

$$=\frac{\mathrm{D}}{r}\frac{\sin\theta_1}{\sin\phi_1}\left\{1+\left(\frac{2+\sin^2\phi_1}{\sin 2\phi_1}+\frac{\mathrm{D}}{r}\frac{3\sin\phi_1}{1+\cos 2\phi_1}\right)\alpha\right\};$$

or, neglecting the first term within the second brackets as being extremely small compared with the next,

$$=\frac{\mathrm{D}}{r}\frac{\sin\theta_1}{\sin\phi_1}+\frac{\mathrm{D}^2}{r^2}\frac{3\sin\theta_1}{1+\cos 2\phi_1}\,\alpha,$$

or substituting the values given in art. 5,

$$=\frac{18}{23}\frac{\mathrm{D}}{r}+\frac{4\mathrm{D}^2}{r^2}\,\alpha.$$

12. The intensity of the light which comes to the eye varies as the reciprocal of this expression. A quantity of light is lost in the refractions and reflexion, which is some function of the angle of incidence: call it $\mathrm{F}(\phi_1-\alpha)$. Then the intensity of the light which comes to the eye

$$=\mathrm{F}(\phi_1-\alpha)\div\left(\frac{18}{23}\frac{\mathrm{D}}{r}+\frac{4\mathrm{D}^2}{r^2}\alpha\right).$$

When $\alpha=0$, that is for the primary bow itself, the intensity of the light

$$=\mathrm{F}(\phi_1)\cdot\frac{23}{18}\frac{r}{\mathrm{D}}.$$

But for the supernumerary bows, in which α is not less than $\frac{1}{6}$, the first term within the bracket is incomparably smaller than the second. Hence for these bows the intensity

$$=\mathrm{F}(\phi_1-\alpha)\frac{r^2}{4\mathrm{D}^2}\frac{1}{\alpha}.$$

Putting $-\alpha'$ for α, and disregarding the change of sign in the intensity because it arises only from the crossing of the rays within the drop, the intensity of the other pencil of light which emerges with the same angle of deviation as the one above considered, and mixes with it,

$$= \mathrm{F}(\phi_1 + \alpha') \frac{r^2}{4\mathrm{D}^2} \frac{1}{\alpha'}.$$

And therefore the ratio of the first to the second

$$= \frac{\mathrm{F}(\phi_1 - \alpha)}{\mathrm{F}(\phi_1 + \alpha')} \cdot \frac{\alpha'}{\alpha}.$$

As $\phi_1 - \alpha$ and $\phi_1 + \alpha'$ do not differ much (being about 54° $30'$ and 64° $30'$), I assume that the first factor of the above $=1$; it is, in fact, somewhat greater than unity. Hence the ratio of intensities

$$= \frac{\alpha'}{\alpha} = \frac{8 + \sqrt{5\beta}}{8 - \sqrt{5\beta}} = 1 + \frac{\sqrt{5\beta}}{4}$$

$= \dfrac{13}{12}$ nearly, close to the primary bow.

This ratio departs more and more from unity, though slowly, in passing from the inner limit of the primary bow. The factor put $=1$ departs from unity very rapidly; because as ϕ increases, the quantity of light lost by external reflexion increases rapidly.

From this it appears, that the two portions of light emerging with the same deviation are, for points within one or two degrees of the primary bow, of intensities sufficiently near each other to allow of their interfering; though, because the intensities are not exactly the same, the interference will not be total; but there will be rings of alternately strong and feeble intensity. As far, then, as *intensity of light* is concerned, the supernumerary bows may arise from interference. We must now see whether the *difference of paths* will agree with the same.

13. From A (see fig. 1) draw AP and AQ perpendicular to the incident and emergent rays. The space PabcQ, estimated by the motion of light *in air*, $= 2 \cdot \mathrm{P}a + 2\mu \cdot ab$,

$$= 4r \sin\left(\frac{1}{2}\phi + \frac{1}{4}\theta\right) \sin\left(\frac{1}{2}\phi - \frac{1}{4}\theta\right) + 4r\mu \cos\left(\frac{1}{2}\phi + \frac{1}{4}\theta\right)$$

$$= 2r \cos\frac{1}{4}\theta - 2r\cos\frac{1}{2}\phi + 4\mu r \cos\left(\frac{1}{2}\phi + \frac{1}{4}\theta\right).$$

Put, first $\phi_1 - \alpha$ and $\theta_1 - \beta$, and then $\phi_1 + \alpha'$ and $\theta_1 - \beta$ for ϕ and θ, and subtract and reduce, neglecting powers of α and α' higher than the first, and therefore β altogether: the result gives the *difference of paths* of the two streams of light which emerge

at the same angle $\theta_1 - \beta$,

$$= \left(2 \sin \phi_1 - \sin \frac{1}{2} \phi_1 \right) r \frac{\alpha - \alpha'}{4},$$

substituting from art. 5,

$$= 0\cdot153 \times r\beta.$$

Let λ be the length of an undulation of red light, and suppose the above interval is m of these undulations,

$$\therefore m \cdot \lambda = 0\cdot153 \times r\beta,$$

$$\therefore \beta = 13 \times \frac{m \cdot \lambda}{2r} ;$$

or turning arcs into angles,

$$= 745° \times \frac{m \cdot \lambda}{2r}.$$

Now $\lambda = 0\cdot0000266$ of an inch. And if *the diameter of the drop of rain equal* (for example) about 3000 of these undulations, that is, *about $\frac{1}{12}$th of an inch,* then

$$\beta = \frac{m°}{4} \text{ nearly} ;$$

or, putting $m = 1, 2, 3 \ldots$ successively, the angular distance of the successive red bows formed by the interference in question from the primary red bow will be

$$15', \quad 30', \quad 45', \quad 1°, \quad 1° 15' \ldots.$$

These bows will be of a diminishing intensity, and none of them will have anything like the intensity of the primary red bow, for which $m = 0$.

In the same way the other colours—the orange, yellow, green, blue, indigo, and violet—will have their sets of bows of interference at somewhat less and less intervals than those of the red. These bows of interference will not be visible in the space occupied by the primary bow, its intensity being so great as to drown their feebler light ; they may have the effect of somewhat, though perhaps almost insensibly, diluting the purity of the prismatic colours of the primary bow after the red, which colour alone will be absolutely undiluted. But within the violet of the primary bow these bows of interference will begin to show themselves : in this case $\beta = 2°$ and more : hence $m = 8$ and more, and the bows of interference of the various colours will begin to overlap, so as to produce the general effect of alternating red and green, as seen in Newton's rings. These are the prevailing colours in the supernumerary bows seen in the heavens ; and we may therefore conclude that they arise, as here described, from interference.

The distances of the bows of interference, it will be seen, de-

pend upon the size of the drops of rain, although the width of the primary bow is independent of this. The larger the drops of rain, the closer are the bows; and therefore under some circumstances the bows may be so close together, that at points within the primary bow, the mixture of colours may be such as to make the bows invisible. This may account for the fact, that the supernumerary bows are not always seen accompanying the primary bows.

N. W. Provinces, India,
    Sept. 18, 1852.

XVI. *On the Principles of Hydrodynamics.* *By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.*

[Concluded from vol. iv. p. 450.]

THE antecedent investigation of the free motion of a compressible fluid, led to the general law that the motion is symmetrically disposed about a rectilinear axis, along which a given state of the fluid as to density and velocity is propagated at a certain constant rate. An expression for the rate of propagation was obtained involving an unknown constant b^2, the value of which it is necessary to ascertain in order to calculate numerically the velocity of sound. I proceed now to show how that constant may be determined, and a solution be given to the following proposition.

Proposition XI. It is required to calculate numerically the rate of the propagation of motion in a given compressible fluid.

I have given a solution of this important question in the Philosophical Magazine for February 1849 (vol. xxxiv. p. 97), but as the reasoning there is not adequately exhibited, I propose to repeat it now in more detail. It has been shown that the free vibratory motion of a compressible fluid is symmetrical with respect to a rectilinear axis, and may be resolved into motions parallel and transverse to this axis.

The component parallel to the axis $= f\dfrac{d\phi}{dz}$.

The component transverse to the axis $= \phi\,\dfrac{df}{dr}$.

The condensation at any point $\quad = -\dfrac{f}{a^2}\cdot\dfrac{d\phi}{dt}$.

The value of the quantity f is given by the series

$$f = 1 - er^2 + \frac{e^2 r^4}{1^2.2^2} - \frac{e^3 r^6}{1^2.2^2.3^2} + \&c.,$$

e being substituted for $\dfrac{b^2}{4a^2}$. Hence at points for which $f=0$, the velocity parallel to the axis and the condensation are constantly equal to zero, and at points for which $\dfrac{df}{dr}=0$, the transverse velocity vanishes, and the condensation is a maximum. The equation $f=0$, it is well known, has an unlimited number of possible roots; and the equation $\dfrac{df}{dr}=0$, by the theory of equations, has a possible root intermediate to every two consecutive roots of the other equation. Consequently there will be an unlimited number of cylindrical surfaces in which the condensation is zero, and an equal number of intermediate surfaces of no transverse velocity. As a preliminary step towards the determination of the constant e, it is required to show that for very large values of r, the intervals between the cylindrical surfaces of no condensation are equal to each other, and to ascertain the value of the common interval. The following process appears to suffice for this purpose.

For the sake of convenience, substitute α^2 for er^2. Then the nth term of the above series is

$$\pm \frac{\alpha^{n-1}}{1^2 . 2^2 . 3^2 \ldots (n-1)^2}.$$

If this be greater than the $(n+1)$th, n^2 is greater than α^2, or n is greater than α; and if it be greater than the $(n-1)$th, α^2 is greater than $(n-1)^2$, or n is less than $\alpha+1$. Hence the greatest term is that indicated by the whole number next greater than α. If $\alpha=n$, the nth and $(n+1)$th terms are equal, and greater than any of the others. Now it may without difficulty be shown that the above series, after multiplying by $1^2 . 2^2 . 3^2 \ldots n^2$, may be expressed as follows:

$$\pm 1^2 . 2^2 . 3^2 \ldots n^2 f = + \frac{\alpha^{2n-4p-2} n^{4p} . \left\{ \alpha^2 - n^2 \left(1 - \dfrac{2p}{n}\right)^2 \right\}}{\left(1 - \dfrac{1}{n}\right)^{-2} . \left(1 - \dfrac{2}{n}\right)^{-2} \ldots \left(1 - \dfrac{2p-1}{n}\right)^{-2}}$$
$$+ \alpha^{2n-2} (\alpha^2 - n^2)$$
$$+ \frac{\alpha^{2n+4p-2} n^{-4p} . \left\{ \alpha^2 \left(1 + \dfrac{2p}{n}\right)^{-2} - n^2 \right\}}{\left(1 + \dfrac{1}{n}\right)^2 . \left(1 + \dfrac{2}{n}\right)^2 \ldots \left(1 + \dfrac{2p-1}{n}\right)^2},$$

where the middle term of the expression consists of the nth and $(n+1)$th terms of the series; the first term of the expression gives every pair of terms preceding the nth by substituting for p

the numbers 1, 2, 3, &c. to $\frac{n}{2}$ or $\frac{n-1}{2}$, according as n is even or odd; and the last term gives every pair following the $(n+1)$th by substituting for p, 1, 2, 3, &c. *ad infinitum*. Let n be a very large number, and suppose that $\alpha^2 = n^2$. Then we have

$$\pm 1^2 \cdot 2^2 \cdot 3^2 \ldots n^2 f = \frac{n^{2n} \cdot \left(1 - \left(1 - \frac{2p}{n}\right)^2\right)}{\left(1 - \frac{1}{n}\right)^{-2} \cdot \left(1 - \frac{2}{n}\right)^{-2} \ldots \left(1 - \frac{2p-1}{n}\right)^{-2}}$$

$$+ \frac{n^{2n} \cdot \left(\left(1 + \frac{2p}{n}\right)^{-2} - 1\right)}{\left(1 + \frac{1}{n}\right)^2 \cdot \left(1 + \frac{2}{n}\right)^2 \ldots \left(1 + \frac{2p-1}{n}\right)^2}.$$

If p be taken so that $2p-1$ is comparable with n, the factors

$$\left(1 - \frac{1}{n}\right)^2 \cdot \left(1 - \frac{2}{n}\right)^2 \ldots \left(1 - \frac{2p-1}{n}\right)^2$$

and

$$\left(1 + \frac{1}{n}\right)^{-2} \cdot \left(1 + \frac{2}{n}\right)^{-2} \ldots \left(1 + \frac{2p-1}{n}\right)^{-2}$$

become very small, and the terms which they multiply become inconsiderable with respect to those for which $2p-1$ is not comparable with n. Now for the latter terms the right-hand side of the above equality is satisfied, if quantities inferior by two degrees at least to the terms of the highest order be omitted, as will appear by expanding the factors

$$\left(1 - \frac{1}{n}\right)^{-2}, \quad \left(1 - \frac{2}{n}\right)^{-2}, \&c., \quad \left(1 - \frac{2p}{n}\right)^2,$$

and

$$\left(1 + \frac{1}{n}\right)^2, \quad \left(1 + \frac{2}{n}\right)^2, \&c., \quad \left(1 + \frac{2p}{n}\right)^{-2},$$

each to two terms. But we have seen above that if $\alpha^2 = n^2$, the nth and $(n+1)$th terms are the greatest. It hence appears that the value n^2 of α^2 causes the principal terms to vanish approximately. And as these terms cannot be made to vanish approximately by terms that are inconsiderable with respect to them, it follows that the value n of α approximates more and more to a root of the equation $f = 0$ in proportion as n is larger. By parity of reasoning $n+1$, $n+2$, &c. are roots of the same equation, n being indefinitely large. We may therefore conclude that the infinite roots of the equation $f = 0$ form an arithmetic series of which the common difference is unity.

By precisely analogous reasoning I have found that the value

$n(n-1)$ of α^2 satisfies to the same degree of approximation the principal terms of the equation $\frac{df}{dr}=0$, and consequently that $n-\frac{1}{2}$ approximates to a root of that equation in proportion as n is large. So $n+\frac{1}{2}$, $n+\frac{3}{2}$, &c. are roots, n being indefinitely great. Thus the infinite roots of $\frac{df}{dr}=0$ form an arithmetic series of which the common difference is unity, and are means between the infinite roots of $f=0$. If, therefore, r_1, r_2 be the radii of two consecutive cylindrical surfaces in which the condensation $=0$, or the transverse velocity $=0$, we shall have $\sqrt{e}(r_2-r_1)=1$; and putting D for r_2-r_1,

$$\sqrt{e}=\frac{1}{D}.$$

Now suppose a series of vibrations such that the condensation and velocity each varies as the function ϕ, or $m\sin\frac{2\pi}{\lambda}(z-a't+c)$, to be propagated along the rectilinear axis of the motion in the positive direction, and an exactly equal series to be propagated in the opposite direction, the possibility of the coexistence of two such series having been previously proved. The effect of the two series will be to form along the axis at equal intervals points of no condensation, and exactly intermediate to these, points of quiescence. Thus the motion along the axis will be similar to the transverse motion which we have just considered. Also the general expression for the transverse velocity, viz. $\phi\frac{df}{dr}$, proves that the transverse vibrations to which a single series gives rise will be executed in the same time as the direct vibrations along the axis. This will clearly be the case also when there are two opposite and equal series. Hence the interval between two consecutive points of quiescence on the axis must be equal to the interval between two consecutive surfaces of no transverse velocity. The former interval is $\frac{\lambda}{2}$, and the latter has been shown above to be $\frac{1}{\sqrt{e}}$. Hence

$$\frac{1}{\sqrt{e}}=\frac{\lambda}{2}.$$

But it has been proved that the velocity of propagation is

$$a\sqrt{1+\frac{e\lambda^2}{\pi^2}}.$$

Consequently, by substituting the above value of e,

$$\text{Velocity of propagation} = a \sqrt{1 + \frac{4}{\pi^2}}.$$

This value accords with the experimental determination of the velocity of sound. (See Phil. Mag. vol. xxxiv. p. 98.)

For the purpose of substantiating the above reasoning, I proceed to notice two objections which may be expected to be urged against it; one drawn from analytical, and the other from physical considerations.   First, it may be said, that when r is indefinitely large, the middle term of the equation

$$\frac{d^2f}{dr^2} + \frac{df}{rdr} + 4ef = 0$$

is incomparably less than the other two, and that the value of f is very approximately given by the solution of the equation

$$\frac{d^2f}{dr^2} + 4ef = 0,$$

viz. $f = c_1 \cos 2 \sqrt{e}(r + c_2)$.   According to this reasoning, the consecutive large values of r which cause f to vanish increase by the common difference $\dfrac{\pi}{2\sqrt{e}}$, and not by $\dfrac{1}{\sqrt{e}}$.   To this argument I reply, that the middle term of the above equation is not small compared with the others for *every* large value of r, in proof of which it is only necessary to remark, that for all the maximum values of that term $\dfrac{d^2f}{dr^2}$ is *equal* to $\dfrac{df}{rdr}$.   The form of the function or series for f depends therefore essentially on that middle term, however large r be taken, and is not even approximately obtained if that term be omitted.

Again, it may be urged that the excess of the observed velocity of sound above the value a is physically accounted for by the generation or absorption of heat consequent upon a sudden condensation or rarefaction of the air.   It is proved by experiments on air contained in closed spaces, that a sudden condensation raises the temperature and a sudden rarefaction depresses it. But such experiments seem to be inapplicable to the case of sudden changes of density in air which is not enclosed, and cannot be safely depended upon for the determination in that case of the effect on the temperature and elasticity of the air.   I am not aware of any experiment having been made by which any change of temperature or elasticity has been shown to accompany sudden changes of density in free air.   And even if it be admitted that a change of elasticity is produced in that manner, the observed velocity of sound is not accounted for without

making *pro hac vice* the hypothesis that the increment of elasticity varies at each instant from point to point, and is exactly proportional to the increment of density. A theory of this kind, resting on hypotheses, ought to have no weight against a course of reasoning which deduces the velocity of sound exclusively from hydrodynamical principles.

From what has now been proved, it appears that the velocity (V), so far as it is independent of any arbitrary disturbance, may for small motions be expressed by the function

$$mf \sin \frac{2\pi}{\lambda} (z \mp \kappa at + c),$$

the quantities m, λ, and c being altogether arbitrary. The numerical quantity κ is equal to $1 \cdot 185447$. Also from the equation

$$a^2 \sigma + f \frac{d\phi}{dt} = 0,$$

it follows that the condensation (σ) is at the same time expressed by the function

$$\pm \frac{\kappa m f}{a} \sin \frac{2\pi}{\lambda} (z \mp \kappa at + c).$$

Hence between V and σ we have the general relation

$$\kappa V = \pm a\sigma,$$

the + or − sign applying according as the propagation is in the positive or negative direction.

The application of the foregoing general results to particular cases of disturbance is to be made on the principle, that the initial disturbance and subsequent motion must be composed of parts, either finite, or indefinitely small, that conform to the circumstances of the motion that have been shown to be independent of all that is arbitrary. There are two distinct classes of disturbance of which a compressible fluid appears to be susceptible. In the one class no velocity is impressed initially, and the condensation through a space of arbitrary extent is made to be of arbitrary value at a given instant, after which the fluid is allowed to move freely. It may be conceived that the initial state results from a combination of the motions obtained in the foregoing investigation, which for distinction I shall call *normal* motions, the number of the sets of vibrations, the directions of their axes, and the values of the quantities m, λ, and c being completely at our disposal. The fluid being left to move freely after the first instant, it may be presumed that the values of m, λ and c, which belong initially to a given set of vibrations, remain unaltered. Although the value of the function f indicates that the transverse motion in a single set of vibrations extends inde-

finitely from the axis, it is possible that by a combination of different sets of vibrations with different values of the constants, the distant motions and condensations may be destroyed, and an initial disturbance of finite extent be composed of normal vibrations.

In the other class of disturbances the velocity is given at given positions in successive instants, and the condensation is to be inferred from the given circumstances of the disturbance. The fluid may be conceived to be set in motion by the surface of a solid, and to be either unlimited in extent, or to be confined within solid boundaries. The motion in such cases is *constrained*, and may be conceived to be composed of parts of the normal motions for which m, λ, and c are constant only for an indefinitely small time and through an indefinitely small space.

In the application of these principles to the undulatory theory of light, the initial disturbances appear to be of the first class, and not to be immediately or necessarily due to the motion of a solid; the motion contiguous to an axis of propagation, which is analytically distinguished by satisfying exactly the integrability of $udx + vdy + wdz$, is the exponent of a *ray* of light; and the direct vibrations, and the parts of the transverse vibrations remote from the axis, must be supposed to be incapable of producing the sensation of light.

Before proceeding to apply the above results to an instance of arbitrary disturbance of the fluid, the circumstances of uniform rectilinear propagation in cases of *constrained* motion are first to be investigated.

Proposition XII. To determine the relation between the velocity and the density, when uniform propagation takes place in a straight tube whose transverse section is of arbitrary magnitude, but everywhere indefinitely small.

Let V and ρ be the velocity and density of the fluid which passes the transverse section m at the distance z from the origin at the time t, and let V' and ρ' be the velocity and density of the fluid which at the same instant passes the transverse section m' situated in advance of the other by the interval δz. The increment of fluid between the two sections in the interval from $t - \dfrac{\delta t}{2}$ to $t + \dfrac{\delta t}{2}$ is

$$m\rho V\delta t - m'\rho'V'\delta t,$$

because the changes of ρV and $\rho'V'$ in the small interval δt may be supposed to be proportional to the time. Let the above increment become equal to the excess of the quantity of fluid at the time $t - \dfrac{\delta t}{2}$ in the element of length δz terminating at the section m, above the quantity of fluid at the same time in the

element between the sections m and m'. That is, neglecting terms involving $\delta t \, \delta z$ and δz^2, let

$$m\rho \mathrm{V} \delta t - m'\rho' \mathrm{V}' \delta t = m\rho \delta z - m'\rho' \delta z.$$

Then the mean density of the former element will have been transferred through the space δz in the time of δt, and $\dfrac{\delta z}{\delta t}$ is its rate of propagation, which ultimately applies to the density ρ. Hence by the above equation, passing from differences to differentials,

$$\frac{d \cdot m\rho \mathrm{V}}{dz} = \frac{d \cdot m\rho}{dz} \cdot \frac{\delta z}{\delta t}.$$

Now as m is of arbitrary value, the rate of propagation cannot be assumed to be uniform excepting so far as the changes of ρ and V are independent of the changes of m. Omitting, therefore, the differentiation with respect to m in the above equation, and supposing the rate of uniform propagation to be a', we have

$$\frac{d \cdot \rho \mathrm{V}}{dz} = a' \cdot \frac{d\rho}{dz},$$

and by integration,

$$\rho \mathrm{V} = a'(\rho - 1) + \chi(m, t),$$

the arbitrary function being a function of m as well as t, because the differentiation with respect to m was omitted. For small motions, putting $1 + \sigma$ for ρ and neglecting the product $\sigma \mathrm{V}$,

$$\mathrm{V} = a'\sigma + \chi(m, t).$$

The effect of the variation of m is taken into account by making a given *phase* of the condensation vary inversely as the transverse section of the tube. Hence if $\psi(z)$ be the transverse section at any distance z from the origin, the nature of the supposed kind of motion will be expressed generally by the equations

$$\mathrm{V} - \chi(m, t) = a'\sigma = \frac{\mu}{\psi(z)} \, \phi(z - a't + c).$$

It is to be remarked, that if $\chi(m, t) = 0$, the resulting equation between V and σ, viz. $\mathrm{V} = a'\sigma$, is different from that obtained for the case of unconstrained motion along an axis of propagation. In the latter case we had $\kappa^2 \mathrm{V} = \kappa a \sigma$, κa being the rate of propagation. The reason of the difference appears to be, that the transverse vibrations which take place in free motion are destroyed in constrained motion by an impressed transverse force, which alters the relation between the velocity and condensation without altering the rate of propagation.

Recurring now to the general equation

$$\frac{d\rho}{dt} + \frac{d \cdot \mathrm{V}\rho}{ds} + \mathrm{V}\rho\left(\frac{1}{\mathrm{R}} + \frac{1}{\mathrm{R}'}\right) = 0,$$

to the first approximation we have

$$\frac{dV}{Vds} + \frac{1}{R} + \frac{1}{R'} = -\frac{d\sigma}{Vdt}.$$

Supposing this equation to apply to constrained uniform recti-
linear propagation, we have $ds = dR = dR'$; and from the equa-
tions above, omitting the arbitrary quantity $\chi(m, t)$,

$$-\frac{d\sigma}{Vdt} = \frac{\phi'(s - a't + c)}{\phi(s - a't + c)}.$$

Hence by integration

$$V = \frac{\psi(t)}{RR'} \phi(s - a't + c).$$

Since by comparison with the foregoing equations $\dfrac{\psi(t)}{RR'}$ does not
contain the time, we must either have RR' an arbitrary function
of s and $\psi(t)$ a constant, which is the case of motion in a rigid
tube, or simply $\psi(t)$ a constant, which is the case of motion
constrained to be rectilinear, and at given positions to be in the
directions of the normals to a given surface, but in other respects
free.   For uniform propagation from a centre $R = R'$, and

$$V = a'\sigma = \frac{C}{R^2} \phi(R - a't + c).$$

Proposition XIII. To prove that the propagation of motion
in a rectilinear tube of arbitrary and indefinitely small trans-
verse section is *uniform,* and that the rate of propagation is the
same as that along a rectilinear axis of free motion.

In the first place it is to be remarked, that the condensation
and velocity may vary in a manner entirely arbitrary, both at a
given position in successive instants, and from point to point of
the axis of the tube at a given instant.   This follows from the
fundamental principle that the parts of the fluid may be momen-
tarily separated by an indefinitely thin partition transverse to the
motion, provided the condensations and velocities on the oppo-
site sides of the partition are equal.   Assuming, therefore, the
motion to be made up of parts of the normal motions, the quan-
tities m, λ, c cannot generally be considered constant except
through an indefinitely small space, or for an indefinitely small
time.   In fact, the general motion becomes identical with the
particular case, by making these quantities vary in a manner
analogous to the known process of passing from the general
integral to the particular solution of a common differential equa-
tion.   It is evident that the lines of motion in the tube are
either directed to centres, or to focal lines, situated on its axis,
the positions of which are given for given points of the tube,

but vary from one point to another. We have now to show that motion composed of the normal motions in the manner just stated, may satisfy this condition respecting the directions of the lines of motion.

In the case of free motion, we have

Velocity parallel to the axis $= mf \sin \dfrac{2\pi}{\lambda}(z \mp \kappa a t + c)$,

Velocity transverse to the axis $= -\dfrac{m\lambda}{2\pi} \cdot \dfrac{df}{dr} \cos \dfrac{2\pi}{\lambda}(z \mp \kappa a t + c)$.

Let $f = 1 - er^2$, r being very small, and let the direction of the motion, which, by reason of the assumed value of f, must pass through the axis of z, make an angle α with that axis. Then since $\dfrac{df}{dr} = -2er$, and $e\lambda^2 = 4$,

$$\alpha = \frac{4r}{\pi\lambda} \cot \frac{2\pi}{\lambda} (z \mp \kappa a t + c).$$

Now this equation is satisfied independently of the value of r, if $\lambda = k(z \mp \kappa a t + c)$ and $r = \alpha(z \mp \kappa a t + c)$, the constant k being determined by the equation

$$1 = \frac{4}{\pi k} \cot \frac{2\pi}{k}.$$

The directions of the motion at all points of a given section perpendicular to the axis will thus pass through a point of the axis whose distance from the section is the arbitrary quantity $z \mp \kappa a t + c$. By means of the above value of λ and the arbitrary quantity m, the circumstances of the motion in a rectilinear tube whose transverse section is everywhere a circle, may be satisfied at a given position in successive instants, or at successive positions at a given instant. It is worthy of remark that this reasoning has required the equation $e\lambda^2 = 4$.

If the motion be directed to focal lines, the value of f to be used is

$$f = 1 - 2e(x \cos \theta + y \sin \theta)^2,$$

θ being an arbitrary angle, and x and y very small. For it was previously shown that the differential equation which gives f is satisfied by the equation $f = 2\sqrt{e}(x \cos \theta + y \sin \theta)$, whatever be θ. The components of the motion parallel to the axes of coordinates being in this case

$$f\frac{d\phi}{dz}, \quad \phi\frac{df}{dx}, \quad \phi\frac{df}{dy},$$

the directions of the resultants at all points of a given transverse section may be made to pass through two focal lines in given positions by means of the additional arbitrary quantity θ. In

other respects the calculation is analogous to that exhibited above.

This proposition may also be viewed in another manner. Since for small values of r, $f = 1 - er^2 = \cos \sqrt{2e}r$ nearly, we have the approximate differential equation

$$\frac{d^2f}{dr^2} + 2ef = 0.$$

That is, for small values of r, f is given by a linear equation with constant coefficients, which is not the case without that limitation. We may therefore suppose the motion in a rigid tube of small transverse section to be made up of an infinite number of normal motions, the axes of which are always in fixed directions determined by the boundaries of the tube. The transverse motions will thus be destroyed, and the resulting motions may be in lines passing through centres, or through focal lines. It should here be remarked, that the composition of motion employed above has no reference to any *physical* circumstance, but is simply an *analytical* process for representing the motion, the number of components being supposed infinite, and that for this reason it was necessary to prove first that f was given by a linear equation with constant coefficients. Similarly, we found in a former proposition that the general value of f is given by the equation

$$f = \Sigma a \delta\theta (x \cos\theta + y \sin\theta),$$

the sum being taken from $\theta = 0$ to $\theta = 2\pi$, and the number of terms being infinite. But it must not be inferred that the normal motion is *physically* so composed.

From either of the two preceding modes of viewing the question we may infer, that as the motion is composed of parts of normal vibrations for which m, λ, and c may be regarded as constant through an indefinitely small space and for an indefinitely small time, the velocity of propagation is the constant κa, this quantity being independent of any arbitrary circumstances. We may not, however, conclude that the relation between the velocity and condensation is the same as in the normal vibrations. Conceive the axis of a set of normal vibrations to coincide with the axis of a cylindrical tube, and the transverse vibrations to be every moment destroyed by an impressed accelerative force equal and opposite to the effective transverse force. The free motion will thus be reduced to the motion in the rigid tube, the impressed force having the same effect as the reaction of the boundaries of the tube. This impressed force, being transverse, will not alter the rate of propagation, but the relation between the velocity and condensation will be changed, as appears from the reasoning in Proposition XII. In the equations obtained in that

proposition we have only to substitute κa for a', and we thus have generally

$$V - \chi(m, t) = \kappa a \sigma = \frac{\mu}{\psi'(z)} \phi(z - \kappa a t + c).$$

Corollary. The effective accelerative force of the fluid in the direction of the axis of the tube may be inferred from the foregoing results. By the effective accelerative force of the fluid is to be understood that part of the actual effective accelerative force which is independent of the *variations* of the arbitrary quantities $\chi(m, t)$ and $\dfrac{\mu}{\psi'(z)}$, or which is obtained by supposing these quantities constant through an indefinitely small space and for an indefinitely small time. Thus we have

$$\frac{dV}{dt} = - \frac{\mu \kappa a}{\psi'(z)} \phi'(z - \kappa a t + c) = - \kappa a \frac{dV}{dz}.$$

But it was found by the reasoning of Proposition XII., that if the variation of m be omitted, to the first approximation

$$\frac{dV}{dz} = \kappa a \frac{d\sigma}{dz}.$$

Hence

$$\kappa^2 a^2 . \frac{d\sigma}{dz} + \frac{dV}{dt} = 0.$$

It thus appears that the effective accelerative force of the fluid in a slender rigid tube is greater than that of fluid in free motion in the ratio of κ^2 to 1.

The following problem has been selected for solution, as being illustrative of various parts of the foregoing analytical theory.

Problem. The fluid is disturbed in such a manner that the velocity and condensation are everywhere and at all times functions of the distance from a fixed centre, and the velocity tends to or from the centre: it is required to determine the motion.

It will plainly be sufficient to consider the motion in a slender tube bounded by planes passing through the centre. The boundaries of the tube may be conceived to be rigid, and consequently, from what has been proved, the differential equations to be employed are the following:

$$\kappa^2 a^2 . \frac{d\sigma}{ds} + \frac{dV}{dt} = 0,$$

$$\frac{d\sigma}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

It is now permitted to introduce into these equations the condition that V and σ are functions of R the distance from the

centre, which condition could not be introduced without error into equations applicable only to *free* motion. The resulting differential equations and their solution will evidently be the same as those obtained in the ordinary mode of treating this problem, with the difference only that κa takes the place of a. Hence we have at once

$$V = \frac{f'(R - \kappa a t)}{R} + \frac{F'(R + \kappa a t)}{R}$$

$$- \frac{f(R - \kappa a t)}{R^2} - \frac{F(R + \kappa a t)}{R^2}$$

$$\kappa a \sigma = \frac{f'(R - \kappa a t)}{R} - \frac{F'(R + \kappa a t)}{R}.$$

It is important to remark that these equations, containing arbitrary functions, apply exclusively to the given arbitrary disturbance. For instance, when $t = 0$, let $V = 0$, and let the values of σ be given through an arbitrary extent along the tube. Then since

$$0 = R\big(f'(R) + F'(R)\big) - \big(f(R) + F(R)\big)$$

for every value of R through that extent, we must have

$$f(R) + F(R) = 0 \text{ and } f'(R) + F'(R) = 0.$$

Hence
$$\kappa a \sigma = \frac{2f'(R)}{R}.$$

The function $f'(R)$ is thus determined, and $F'(R) = -f'(R)$. If, therefore,

$$V_1 = \frac{f'(R)}{R} \text{ and } V_2 = -\frac{f'(R)}{R},$$

we have $V_1 + V_2 = 0$, and

$$\frac{\kappa a \sigma}{2} = V_1, \quad \frac{\kappa a \sigma}{2} = -V_2.$$

These two last equations show that the motion at the first instant resolves itself into equal motions, which satisfy the conditions of uniform propagation in opposite directions. The velocities $-\dfrac{f(R)}{R^2}$ and $-\dfrac{F(R)}{R^2}$, destroy each other at the first instant, and not being accompanied by condensation, give rise to no propagation. The velocity and condensation after the first instant at any point may be inferred from the uniformity of propagation, and from the law of variation according to the inverse square of R, as shown in Proposition XII.

Next let the velocity be given at a given distance h from the centre during an arbitrary interval, and let it be required to find the resulting condensation. As there is only one condition in

this case to be satisfied, one of the arbitrary functions must be made to disappear. Let the function $F=0$. Then

$$V = \frac{f'(h-\kappa at)}{h} - \frac{f(h-\kappa at)}{h^2}$$

$$\kappa a\sigma = \frac{f'(h-\kappa at)}{h}.$$

It is again to be remarked, that as these equations contain arbitrary functions, they can apply only to the arbitrary disturbance, the denominators having no reference to the law of variation of the velocity and condensation with the distance, but to the proportion of the two parts of which the velocity, each moment that it is impressed, consists. When V is given as a function of t, the two parts of the velocity are found by the solution of a common differential equation, of which the variables are the function f and the time t. I have given instances of this process in a paper on the Motion of a small Sphere in an elastic medium (Cambridge Philosophical Transactions, vol. vii. part 3). After thus finding f, and by consequence f', the initial condensation σ becomes a known function of t. The initial relation between V and σ is

$$V + \frac{f(h-\kappa at)}{h^2} = \kappa a\sigma,$$

which agrees with the general relation we have already obtained between the velocity and condensation when uniform propagation takes place in a rigid tube. The condensation σ and the part of the velocity $\dfrac{f'(h-\kappa at)}{h}$, are propagated, after the first instant of the generation of the condensation, with the uniform velocity κa, varying at the same time with the distance according to the law of the inverse square, as proved in Prop. XII. The other part of the velocity, $-\dfrac{f(h-\kappa at)}{h^2}$, being accompanied with no condensation, or at least with none of the order considered in this investigation, is transmitted instantaneously, varying with the distance inversely as the square of the distance as in an incompressible fluid, and existing only so long as the disturbance is going on. It would seem that the initial generation of condensation is not possible without this accompanying velocity, which is so much the less as the distance of the place of disturbance from the centre is greater. The proposed problem has thus been completely solved on the principles of this theory.

For the purpose of confirming the foregoing reasoning I proceed to give another solution, which to a certain extent is applicable to the same problem. It is evident that if an unlimited

number of normal vibrations be propagated equally in all directions, their axes passing through the centre, the resulting velocity and condensation will be functions of the distance from the centre, and the resulting motion will be in lines drawn through the centre. The summation of the effects of all these vibrations should, therefore, give a solution in accordance with that obtained above. But it must be remarked, as before, that as this composition of the motion is an analytical conception, and not a physical reality, the reasoning can only extend to cases in which $f = 1 - er^2$, and r is very small compared to λ. This being premised, take any point P on a line drawn in a fixed direction through O the centre, and let ON be the direction of any axis of vibration such that the $\angle PON = \alpha$, and the plane PON makes the angle θ with a fixed plane passing through OP. Draw PN perpendicular to ON, and let $OP = R$, $ON = z$, $PN = r$. The condensation produced at P by a single set of vibrations is $\dfrac{\kappa f}{a} \cdot \dfrac{d\phi}{dz}$, and the sum of the resolved parts of the direct and transverse motions in the direction OP is

$$f \frac{d\phi}{dz} \cos \alpha + \phi \frac{df}{dr} \sin \alpha.$$

The resolved parts perpendicular to OP destroy each other. The number of axes that pass through the element $z \sin \alpha \, d\theta \, . \, z \, d\alpha$ varies as the element directly and as z^2 inversely, and may therefore be assumed equal to $k \sin \alpha \, d\alpha \, d\theta$. Consequently we have to obtain the two integrals,

$$\frac{k\kappa}{a} \iint f \frac{d\phi}{dz} \sin \alpha \, d\alpha \, d\theta,$$

$$k \iint \left(f \frac{d\phi}{dz} \cos \alpha + \phi \frac{df}{dr} \sin \alpha \right) \sin \alpha \, d\alpha \, d\theta,$$

the integrations being taken from $\theta = 0$ to $\theta = 2\pi$, and from $\alpha = 0$ to $\alpha = \pi$. Now let

$$\phi = -\frac{\lambda m}{2\pi} \cos \frac{2\pi}{\lambda} (z - \kappa at + c),$$

so that

$$\frac{d\phi}{dz} = m \sin \frac{2\pi}{\lambda} (z - \kappa at + c).$$

Since, from what was said above, the first order of approximation is alone permitted, we must have $f = 1$, and

$$\frac{df}{dr} = -2er = -\frac{8r}{\lambda^2},$$

because $e\lambda^2 = 4$. Also $R \sin \alpha = r$, and $R \cos \alpha = z$. By making

these substitutions and performing the integrations, it will be found that

$$V = -\frac{km\kappa^2\lambda}{R} \cdot \left\{ \cos\frac{2\pi}{\lambda}(R - \kappa at + c) + \cos\frac{2\pi}{\lambda}(R + \kappa at + c) \right\}$$

$$+ \frac{km\kappa^2\lambda^2}{2\pi R^2} \cdot \left\{ \sin\frac{2\pi}{\lambda}(R - \kappa at + c) + \sin\frac{2\pi}{\lambda}(R + \kappa at + c) \right\}$$

$$\sigma = -\frac{km\kappa\lambda}{Ra} \cdot \left\{ \cos\frac{2\pi}{\lambda}(R - \kappa at + c) - \cos\frac{2\pi}{\lambda}(R + \kappa at + c) \right\}.$$

It will suffice to remark, that the previous general solution accords with this solution on giving a particular form to the arbitrary functions, and that the change of the relation between the velocity and condensation in passing from free to constrained motion is confirmed.

If the fluid were put in motion by a smooth solid sphere moving in it in a given manner, the velocity impressed at each point of the surface at any instant would be known. The directions of the impressed velocities determine the directions of the initial propagations, and as we found in the general consideration of the mutual action of the parts of the fluid no other than rectilinear propagation, it follows that the initial direction of propagation continues to all distances from the centre. The motion will therefore be the same for an indefinitely small portion of a wave, as when the disturbance is made at a given distance from a fixed centre; but the direction of propagation at a given point will be continually changing, and the motion of a given particle will be curvilinear, being directed to or from positions which the centre of the sphere had in successive instants. I see, therefore, no reason from these new researches to alter the solution of the problem of resistance to a small sphere in an elastic medium, which I have given in the paper already referred to, excepting that κa must be substituted for a. The effect of this change will be to diminish the condensation, and therefore the pressure, for a given velocity, in the ratio of 1 to κ; and the coefficient of buoyancy and resistance, instead of being 2, will be $1 + \frac{1}{\kappa}$, or 1·844, which accords well with observation.

The same principles would suffice to determine the motion when the disturbance is in the directions of the normals of any continuous surface, and is a function of the distance from the surface; and also to ascertain the effect of the motion of such a surface in a given manner in the fluid.

I have now completed the explanation of my hydrodynamical theory sufficiently to enable any mathematician who may regard it with favour, and who may have more time for these researches

than I can command, to pursue the subject further. Whilst I admit that I have had occasion from time to time to modify my first ideas, I must be allowed to express my conviction that these last results are in all essential respects correct, and that the propositions which I have succeeded in establishing will eventually be regarded as standard propositions in this department of applied mathematics. Not knowing to what extent they may at present receive the assent of mathematicians, I have preferred the Philosophical Magazine to any other medium of communication, for the purpose of affording an opportunity for any discussion or elucidation which the novelty of the views may call for. The obvious and important application of these hydrodynamical theorems in the undulatory theory of light I need not now insist upon.

Cambridge Observatory,
December 30, 1852.

XVII. *On the Restoration of Mechanical Energy from an unequally heated space.* By Prof. W. THOMSON*.

W̲HEN heat is diffused by conduction from one part to another of an unequally heated body, the body is put into such a state that it is *impossible* to derive as much mechanical effect of a non-thermal kind† from it as could have been derived from the body in its given state‡. Hence, if a body be given in an envelope impermeable to heat, with its different parts at different temperatures, a *dissipation* of mechanical energy within it, going on until the temperatures of all its parts become the same, can only be avoided by immediately restoring a portion of its mechanical energy from the state of heat, and equalizing the temperature of all its parts, wholly by the operation of perfect thermo-dynamic engines. Let T be the uniform tempe-

* Communicated by the Author.
† [Note added Jan. 14, 1853.] Instead of "mechanical effect of a non-thermal kind," I should have said simply *potential energy*, had I at the time of writing this paper learned the use of the admirable terms "potential" and "actual" introduced by Mr. Rankine in his paper "On the Transformation of Energy" (communicated to the Glasgow Philosophical Society at its last meeting, Jan. 5, and published in the present number of the Philosophical Magazine), to designate the two kinds of energy which I had previously distinguished by the inconvenient adjectives of "statical" and "dynamical." (See Proc. Roy. Soc. Edinb. Feb. 16. 1852; or Phil. Mag. Oct. 1852, p. 304.)
‡ Proceedings of the Royal Society of Edinburgh for Feb. 16, 1852, (p. 139), or p. 305 of the last Volume of this Journal. The formulæ given in that paper which have reference to the subject of the present communication, require corrections, which are indicated in "Errata" published in the last Number.

rature to which the body is brought by this process of restoration; let t be the temperature of the body in its given condition at any point xyz; and let $cdt \,.\, dx\, dy\, dz$ be the quantity of heat that an infinitely small element, $dx\, dy\, dz$, of the body must part with to go down in temperature from t to $t - dt$. Let us suppose that this quantity of heat enters a perfect thermodynamic engine, of which the hot part is at the temperature t, and the cold part at the temperature T. The quantity of work that will be derived from it will be

$$dx\, dy\, dz\, \mathrm{J}\,.\, cdt \,.\, (1 - \epsilon^{\frac{-1}{\mathrm{J}}\int_{\mathrm{T}}^{t}\mu dt}) \quad . \quad . \quad . \quad . \quad (a),$$

where μ denotes Carnot's function, and J the mechanical equivalent of the thermal unit (see Dynamical Theory of Heat, § 25, Trans. Royal Soc. Edinb. 1851, or Phil. Mag. Aug. 1852), and the part of it rejected as waste into the refrigerator at the temperature T will be

$$dx\, dy\, dz \,.\, cdt \,.\, \epsilon^{\frac{-1}{\mathrm{J}}\int_{\mathrm{T}}^{t}\mu dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad (b),$$

or

$$\frac{dx\, dy\, dz \,.\, cdt \,.\, \epsilon^{\frac{-1}{\mathrm{J}}\int_{0}^{t}\mu dt}}{\epsilon^{\frac{-1}{\mathrm{J}}\int_{0}^{\mathrm{T}}\mu dt}} \quad . \quad . \quad . \quad . \quad . \quad (c),$$

Hence the whole work obtained by lowering, by means of a perfect thermo-dynamic engine, the temperature of the element $dx\, dy\, dz$ from t to T is

$$dx\, dy\, dz \int_{\mathrm{T}}^{t} \mathrm{J}\,.\, cdt \,.\, (1 - \epsilon^{\frac{-1}{\mathrm{J}}\int_{\mathrm{T}}^{t}\mu dt}) \quad . \quad . \quad . \quad (d);$$

and the part of the heat taken from this element, which is rejected into the refrigerator, is

$$\frac{dx\, dy\, dz \int_{\mathrm{T}}^{t} cdt \,.\, \epsilon^{\frac{-1}{\mathrm{J}}\int_{0}^{t}\mu dt}}{\epsilon^{-\frac{1}{\mathrm{J}}\int_{0}^{\mathrm{T}}\mu dt}},$$

or

$$\frac{dx\, dy\, dz \left\{ \int_{0}^{t} cdt\; \epsilon^{\frac{-1}{\mathrm{J}}\int_{0}^{t}\mu dt} - \int_{0}^{\mathrm{T}} cdt\; \epsilon^{\frac{-1}{\mathrm{J}}\int_{0}^{t}\mu dt} \right\}}{\epsilon^{-\frac{1}{\mathrm{J}}\int_{0}^{\mathrm{T}}\mu dt}} \quad . \quad (e).$$

These expressions are of course equally applicable to parts of the body of which the temperatures are lower than T; and, without change of form, will express respectively (in virtue of the algebraic signs corresponding to any case in which t is $<$ T), (*d*) the quantity of work obtained by *raising* the temperature of an element $dx\, dy\, dz$ from t to T by heat drawn from a source at the temperature T by means of a thermo-dynamic engine, and (*e*) the

negative quantity of heat *given to* the part of the engine at the temperature T in this process. Now if such a process be completed for every part of the body without either taking or giving heat by communication with other bodies, the whole quantity of heat given to the refrigerators at T by the engines worked from all the parts of the body for which $t > T$, must be equal to the whole quantity given by the sources at T to the engines working to raise the temperature of those parts of the body for which $t < T$; or, algebraically, the sum of the quantities of heat given by all the engines to the parts of them at T must be 0, that is, according to (e),

$$\frac{\iiint dx \, dy \, dz \left\{ \int_0^t cdt \cdot \epsilon^{\frac{-1}{J}\int_0^t \mu dt} - \int_0^T cdt \cdot \epsilon^{\frac{-1}{J}\int_0^t \mu dt} \right\}}{\epsilon^{\frac{-1}{J}\int_0^T \mu dt}} = 0 \quad (1).$$

From this we deduce

$$\iiint dx \, dy \, dz \cdot \int_0^T cdt \cdot \epsilon^{-\frac{1}{J}\int_0^t \mu dt} = \iiint dx \, dy \, dz \int_0^t cdt \cdot \epsilon^{\frac{-1}{J}\int_0^t \mu dt} (2),$$

by which the value of T may be found. When T is determined, the whole work obtained in the process may be calculated by adding all the terms given by the expression (d); and thus, if W denote its value, we find

$$W = J \iiint dx \, dy \, dz \int_T^t cdt \cdot (1 - \epsilon^{\frac{-1}{J}\int_T^t \mu dt}) \quad . \quad (3).$$

The first member of the former of these equations may be put into a simpler form if we take Θ to denote the thermal capacity of the whole body at the temperature θ; a quantity which must generally be considered as a function of θ. Thus, if $c_\theta \, dx \, dy \, dz$ denote the thermal capacity of the portion $dx \, dy \, dz$ when at the temperature θ, we have

$$\iiint c_\theta dx \, dy \, dz = \Theta \quad . \quad . \quad . \quad . \quad . \quad (4);$$

and the equation becomes

$$\int_0^T \Theta d\theta \cdot \epsilon^{\frac{-1}{J}\int_0^\theta \mu dt} = \iiint dx \, dy \, dz \int_0^t cdt \cdot \epsilon^{\frac{-1}{J}\int_0^t \mu dt}. \quad (5).$$

In order that there may be data enough for solving the problem, the nature of the given body must be specified so that the value of c for each point xyz is known, not only for the given initial temperature t of that point, but for all other temperatures through which we have to suppose it to vary in this investigation; and Θ is therefore to be regarded as a known function of θ. Hence the value of the integral $\int_0^\theta \Theta d\theta \, \epsilon^{\frac{-1}{J}\int_0^{} \mu dt}$

may be regarded as a known function of θ, and may be tabulated for different values of this variable. The value of θ, for which this function is equal to the second member of equation (5), is the required quantity T.

The solution of the problem may be put under a very simple form, if the thermal capacity of each part of the body be independent of the temperature, in the following manner. Let the temperature of the body be measured according to an absolute scale, founded on the values of Carnot's function, and expressed by the following equation,

$$t = \frac{J}{\mu} - \alpha \quad . \quad . \quad . \quad . \quad . \quad (6),$$

where α is a constant which might have any value, but ought to have for its value the reciprocal of the coefficient of compressibility of air, in order that the system of measuring temperature here adopted may agree approximately with that of the air-thermometer. Then we have

$$\epsilon^{-\frac{1}{J}\int_0^t \mu\, dt} = \frac{\alpha}{t+\alpha}, \text{ and } \epsilon^{-\frac{1}{J}\int_0^\theta \mu\, dt} = \frac{\alpha}{\theta+\alpha};$$

and since, according to the hypothesis that is now made, Θ is constant, the first member of equation (5) becomes simply $\Theta\alpha \log\dfrac{T+\alpha}{\alpha}$; and we have explicitly, for the value of T, the equation

$$\left.\begin{array}{c} T = \alpha\left\{\epsilon^{\frac{1}{\Theta}\iiint \log\frac{t+\alpha}{\alpha}\,.\,c\,dx\,dy\,dz} - 1\right\} \\[2mm] T = \epsilon^{\dfrac{\iiint \log(t+\alpha)\,.\,c\,dx\,dy\,dz}{\iiint c\,dx\,dy\,dz}} - \alpha\,; \end{array}\right\} \quad . \quad (7),$$

or

and equation (3) takes the simpler form,

$$W = J\iiint c\,dx\,dy\,dz\left(t - T - (T+\alpha)\log\frac{t+\alpha}{T+\alpha}\right) \quad . \quad (8).$$

If the given body be of infinite extent, and if the temperature of all parts of it have a uniform value, T, with the exception of a certain limited space of finite extent through which there is a given varied distribution of temperature, any of the equations (2), (5), or (7) leads to the result

$$T = T,$$

which might have been foreseen without analysis. In this case, then, equation (3) gives (in terms of a definite integral of which the elements vanish for all points at which t has the value T) the work that may be obtained by bringing the temperature of all the matter to T; and the same result is expressed more simply by (8) when the specific heat of all the matter in the space through which the initial distribution of temperature is non-uniform does not vary with the temperature.

1 Acton Square, Salford, Manchester,
    December 28, 1852.

XVIII. *On the General Law of the Transformation of Energy.*
  By WILLIAM JOHN MACQUORN RANKINE, *C.E., F.R.S.E.,*
  *F.R.S.S.A. &c.**

(1.) IN this investigation the term *energy* is used to compre-
hend every affection of substances which constitutes or
is commensurable with a power of producing change in opposi-
tion to resistance, and includes ordinary motion and mechanical
power, chemical action, heat, light, electricity, magnetism, and
all other powers, known or unknown, which are convertible or com-
mensurable with these. All conceivable forms of energy may be
distinguished into two kinds; actual or sensible, and potential
or latent.

Actual energy is a measurable, transferable, and transformable
affection of a substance, the presence of which causes the sub-
stance to tend to change its state in one or more respects; by
the occurrence of which changes, actual energy disappears, and
is replaced by

Potential energy, which is measured by the amount of a change
in the condition of a substance, and that of the tendency or force
whereby that change is produced (or, what is the same thing,
of the resistance overcome in producing it), taken jointly.

If the change whereby potential energy has been developed be
exactly reversed, then as the potential energy disappears, the
actual energy which had previously disappeared is reproduced.

The law of the conservation of energy is already known, viz.
that the sum of the actual and potential energies in the universe
is unchangeable.

The object of the present investigation is to find the law of
the *transformation of energy,* according to which all transforma-
tions of energy between the actual and potential states take place.

(2.) To reduce the problem to its simplest form, let us in the
first place consider the mutual transformation of one form only
of actual energy, and one form only of potential energy.

Let V denote one measurable state, condition, or mode of
existence of the substance under consideration, whose magnitude
increases when the kind of potential energy in question is de-
veloped.

Let U denote this potential energy.

Let P be the tendency or force whereby the state V tends to
increase, which is opposed by an equal resistance.

Then when the state V undergoes a small increase dV, the
potential energy developed or given out is

$$dU = PdV = \frac{dU}{dV}.dV, \left.\right\}$$

so that

$$P = \frac{dU}{dV}. \qquad \right\} \quad . \quad . \quad . \quad . \quad (A)$$

* Communicated by the Author, having been read to the Philosophical
Society of Glasgow, January 5, 1853.

Let Q denote the quantity, present in the substance, of the kind of actual energy under consideration. It is required to find how much of the potential energy of the kind U, developed by a small increase of the state V, is produced by transformation of actual energy of the kind Q, whose total quantity (to avoid complication of the problem) is supposed to be maintained constant by the communication of actual energy from external substances.

The quantity of potential energy of the form U which is produced by transformation from the actual form Q, is the effect of the presence of the total energy Q in the substance during the change dV.

To find this effect, let the total energy Q be conceived to be divided into an indefinite number of indefinitely small parts dQ, and let the effect of one be computed separately. To do this, let any one of the parts dQ be abstracted from the total energy Q; and let the effect of this be to diminish the development of power dU by the quantity d^2U. Then

$$\frac{d^2U}{dQ\,dV} \cdot dV\,dQ$$

is the effect, in development of potential energy, of the presence in the substance of the small portion dQ of actual energy; and as all the small portions of actual energy are similarly circumstanced, the potential energy which is developed by the change dV, in consequence of the presence of the whole actual energy Q, bears the same ratio to the whole energy Q which the above quantity bears to the portion dQ; that is to say, the conversion of energy from the actual form Q, to the potential form U during the change dV, is represented by

$$Q \cdot \frac{d^2U}{dQ\,dV} \cdot dV. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

(3.) Next let us suppose, that not only the state V varies, but the total quantity of energy Q also; and let us investigate what quantity of actual energy of the form Q must be communicated to the substance to produce simultaneously the variations dQ and dV.

First, there is the energy which remains in the actual form, directly producing the increase dQ in the total quantity Q.

Secondly, there is the energy (not yet determined) which may be transformed from the actual to some invisible potential form in consequence of the change dQ only. Let this be denoted by $L\,dQ$. The nature of this quantity may be best conceived by considering that the energy Q is itself a *state* of the substance, to effect a change in which a resistance L may have to be overcome.

Thirdly, there is the energy already determined and expressed

I 2

in the formula (1), which is transformed from the actual to the potential form in consequence of the change of state $d\mathrm{V}$.

The sum of those three quantities is as follows:

$$d.\mathrm{Q} = (1 + \mathrm{L})d\mathrm{Q} + \mathrm{Q}.\frac{d^2\mathrm{U}}{d\mathrm{Q}d\mathrm{V}}.d\mathrm{V}. \quad . \quad . \quad . \quad (2)$$

If from this expression be subtracted the potential energy developed, that is to say, given out in overcoming resistance,

$$\frac{d\mathrm{U}}{d\mathrm{V}}.d\mathrm{V},$$

the result will be the algebraical sum of the energies, actual and potential, acquired on the whole by the substance in passing from the total actual energy Q and state V to the total actual energy $\mathrm{Q} + d\mathrm{Q}$ and state $\mathrm{V} + d\mathrm{V}$; viz.

$$d\Psi = d.\mathrm{Q} - \frac{d\mathrm{U}}{d\mathrm{V}}.d\mathrm{V} = (1 + \mathrm{L})d\mathrm{Q} + \left(\mathrm{Q}\frac{d}{d\mathrm{Q}} - 1\right)\frac{d\mathrm{U}}{d\mathrm{V}}.d\mathrm{V}. \quad . \quad (\mathrm{B})$$

Now this quantity must be the same, whether the change $d\mathrm{Q}$ or the change $d\mathrm{V}$ be made first, or both simultaneously; otherwise by varying the order of making those changes, the sum of energy in the universe, actual and potential, might be changed, which is impossible. Therefore the above expression must be the complete differential of a function of the energy Q and state V; that is to say,

$$\frac{d\mathrm{L}}{d\mathrm{V}} = \frac{d}{d\mathrm{Q}}\left(\mathrm{Q}\frac{d}{d\mathrm{Q}} - 1\right)\frac{d\mathrm{U}}{d\mathrm{V}} = \mathrm{Q}\frac{d^2}{d\mathrm{Q}^2}.\frac{d\mathrm{U}}{d\mathrm{V}};$$

consequently

$$\mathrm{L} = f'.(\mathrm{Q}) + \mathrm{Q}.\frac{d^2\mathrm{U}}{d\mathrm{Q}^2},$$

$f'(\mathrm{Q})$ being a function of Q only, to be determined by experiment, of which $f(\mathrm{Q})$ is the primitive.

Thus we obtain, for the total energy, actual and potential, acquired by the substance, in consequence of the changes of total actual energy from Q to $\mathrm{Q} + d\mathrm{Q}$, and of state, from V to $\mathrm{V} + d\mathrm{V}$, the formula

$$\begin{aligned}
d\Psi = d.\mathrm{Q} - d.\mathrm{U} &= \left(1 + f'.(\mathrm{Q}) + \mathrm{Q}\frac{d^2\mathrm{U}}{d\mathrm{Q}^2}\right)d\mathrm{Q} + \left(\mathrm{Q}\frac{d}{d\mathrm{Q}} - 1\right)\frac{d\mathrm{U}}{d\mathrm{V}}.d\mathrm{V}\\
&= d.\left\{\mathrm{Q} + f(\mathrm{Q}) + \left(\mathrm{Q}\frac{d}{d\mathrm{Q}} - 1\right)\int'\frac{d\mathrm{U}}{d\mathrm{V}}.d\mathrm{V}\right\},
\end{aligned} \quad (3)$$

in which the symbol $d.\mathrm{U}$ is used to denote the total potential energy really developed; while $\int'\frac{d\mathrm{U}}{d\mathrm{V}}.d\mathrm{V}$ is a *partial integral* computed for each value of Q, as if that value were constant.

This equation is the complete expression of the *law of the transformation of energy,* from any one actual form Q to any one potential form U, developed by increase of the state V.

By analysing it, we find that, besides the simple increase of actual energy dQ, which retains its form, the substance receives

$\left(f'(Q) + Q\dfrac{d^2U}{dQ^2}\right)dQ$ of actual energy which disappears;

$Q\dfrac{d^2U}{dQ\,dV} \cdot dV$ which is directly transformed into potential

energy of the kind U;

and that it gives out the potential energy dU $=$ PdV.

(4.) The extension of these principles to any number of kinds of actual energy,

$$Q_\alpha, \quad Q_\beta, \quad Q_\gamma, \quad \&c. \ldots Q_\mu, \&c.,$$

and any number of kinds of potential energy,

$$U_a = \int P_a dV_a; \ U_b = \int P_b dV_b; \ U_c = \int P_c dV_c, \&c \ldots U_m = \int P_m dV_m \&c.,$$

developed by changes in the states or conditions

$$V_a, \quad V_b, \quad V_c, \quad \&c. \ldots V_m, \&c.,$$

leads to the following results :—

The quantity of any given kind of actual energy, Q_μ, which disappears, is

$$\left(f'(Q_\mu) + Q_\mu \Sigma \cdot \frac{d^2U}{dQ_\mu{}^2}\right)dQ_\mu,$$

the sum extending to all the forms of potential energy.

The quantity converted from any given actual form, Q_μ, to any given potential form U_m, is

$$Q_\mu \frac{d^2U_m}{dQ_\mu\,dV_m} \cdot dV_m.$$

Hence the algebraical sum of the energies acquired and given out by the substance is

$$d\Psi = \Sigma d.Q - \Sigma d.U = \Sigma.\left(1 + f'(Q) + Q.\Sigma \cdot \frac{d^2U}{dQ^2}\right)dQ + \Sigma$$

$$\left(\Sigma.Q\frac{d}{dQ} - 1\right)\frac{dU}{dV}\,dV = d.\left\{\Sigma Q + \Sigma f(Q) + \Sigma\Sigma \cdot Q\frac{d}{dQ}\right.$$

$$\left.\int\frac{dU}{dV} \cdot dV - \Sigma.\int\frac{dU}{dV}\,dV\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This equation is the complete expression of the *law of the mutual transformation of actual and potential energy of all possible kinds.*

The data requisite for its application to any physical problem

are, the nature and value of the function $f(Q)$ for each kind of actual energy of $\frac{dU}{dV}$ for each kind of potential energy, and of $\frac{d^2U}{dQdV}$ and $\frac{d^2U}{dQ^2}$ for each combination by pairs of an actual with a potential energy.

(5.) Abstract and metaphysical as the principles and reasoning of this paper may appear, they are of immediate practical utility.

When applied to the mutual transformation of visible motion with the power arising from attractive and repulsive forces, the equation 3 becomes identical.

It is in the theory of those forms of energy which manifest themselves to us only by their effects, such as heat and electricity, that this law becomes useful. It enables us, when power is produced by the consumption of one or more of those energies, to analyse the effect produced, to refer each portion to the kind of energy by which it is caused, and thus to determine how much of any given kind of energy must disappear in order to produce a given change in the condition of a substance.

An important consequence of the formula (1) is as follows:—
If the tendency, $\frac{dU}{dV}$, of the state V to increase is proportional simply to the actual energy present, Q, then

$$\frac{d^2U}{dQdV} = \frac{1}{Q}\cdot\frac{dU}{dV};$$

and consequently

$$Q\,\frac{d^2U}{dQdV}\cdot dV = dU;\quad .\quad .\quad .\quad .\quad .\quad (5)$$

that is to say, *when the tendency to the production of potential energy is simply proportional to the actual energy present, then is the actual energy converted into potential energy the exact equivalent of the whole potential energy produced.*

(6.) Another important consequence of the formulæ (1) and (3) is as follows:—If a substance be made to undergo a change of condition, and be brought back to its primitive condition by a process not the exact reverse of the former process, then will there be a certain amount of permanent conversion of energy between the actual and potential forms. That the permanent conversion from the actual to the potential form may be the greatest possible in proportion to the actual energy supplied from without, the changes of condition must be so regulated that none of the actual energy received or given out by the substance shall be employed in changing the total actual energy present in it, the whole being consumed or produced by transformation to or from the potential form.

To effect this, the following operations must be performed. The actual energy being maintained in the substance at the constant value Q_1, let it undergo a change of state from V_A to V_B. Then the actual energy supplied from without, which is all converted into potential energy, is

$$H_1 = Q_1 \cdot \frac{d}{dQ} \int_{V_A}^{V_B} \frac{dU}{dV} \, dV$$
$$= Q_1(F_B - F_A),$$

where F denotes the function $\dfrac{d}{dQ} \int \dfrac{dU}{dV} \, dV$.

Let the actual energy now be reduced to a lower amount, Q_2, entirely by transformation to the potential form, without transfer of actual energy to other substances. That this may be the case, we must have, according to equation (2),

$$0 = d \cdot Q = d\{Q + f(Q)\} + Q \, d \cdot F ;$$

or if F_C be the value of F at the end of the operation,

$$F_B - F_C = \int_{Q_2}^{Q_2} \frac{d(Q + f(Q))}{Q} . \quad . \quad . \quad . \quad (C)$$

Let F_D be a fourth value of F, such that
$F_A - F_D = F_B - F_C$; and consequently $F_C - F_D = F_B - F_A$. (D)
Then the substance having the actual energy Q_2 maintained constant, and its condition changed until F becomes F_D, the following quantity of energy must be retransformed from the potential to the actual state, and transferred to other substances :

$$H_2 = Q_2(F_C - F_D) = Q_2(F_B - F_A).$$

The substance being then brought back to its original condition, viz.

$$Q = Q_1 \text{ and } F = F_A,$$

without receiving or emitting actual energy, the following quantity of energy will at the end of the operation have been permanently transformed from the actual to the potential condition,

$$H_1 - H_2 = (Q_1 - Q_2)(F_B - F_A), \quad . \quad . \quad . \quad (6)$$

which bears the following proportion to the whole quantity of actual energy received by the substance from without,

$$\frac{H_1 - H_2}{H_1} = \frac{Q_1 - Q_2}{Q_1} ; \quad . \quad . \quad . \quad . \quad (7)$$

that is to say,—

The greatest quantity of energy which can be permanently converted from the actual to the potential state by causing a substance to undergo a cycle of changes, bears the same proportion to the whole actual energy communicated to the substance from without,

which the excess of the actual energy present in the substance during the reception of actual energy, above the actual energy present during the emission of actual energy, bears to the former of these two quantities.

This is the general law of the action of all possible machines, which work by the transformation of energy of all kinds, known or unknown.

Application to Heat.

(7.) Having described in detail the application of these principles to the theory of expansive heat, in the sixth section of a paper on the Mechanical Action of Heat, communicated to the Royal Society of Edinburgh, I shall here give merely an outline of the principal points of that application.

Expansive heat may be defined to be *a species of actual energy, the presence of which in a body gives it a tendency to expand.*

To adapt the preceding formulæ to this kind of energy, we must affix the following interpretation to the symbols.

Let Q denote the quantity of heat in a body, as measured by an equivalent quantity of mechanical power;

V the volume of the body, whose tendency to increase is represented by

P, the expansive pressure, in units of force per unit of surface.

Then $dU = P dV$ is the expansive power or potential energy developed by a small expansion dV; and during this expansion the following quantity of heat becomes latent, that is to say, is converted into expansive power, according to equation (1),

$$Q \cdot \frac{d^2 U}{dQ dV} \cdot dV = Q \cdot \frac{dP}{dQ} dV.$$

According to the principle laid down in the fifth article, if any substance exists in which the expansive pressure is simply proportional to the quantity of heat present, then the heat which disappears in expanding that substance is the exact equivalent of the power developed. In all known substances, however, even those in the gaseous state, the expansive pressure deviates from this law; and accordingly the difference

$$\left(Q \frac{dP}{dQ} - P \right) dV$$

represents the power expended in overcoming cohesive force, diminished by that which is produced by such elasticity as the body may possess independently of heat.

The recent experiments of Mr. Joule and Professor William Thomson on the thermic phænomena of currents of air, give values of this quantity under various circumstances as to temperature and density; and after calculating from formulæ deduced

from M. Regnault's experiments the effect of cohesive force, I have found that they indicate that perfect gases possess an elasticity, independent of heat, corresponding to that due to about $2°\cdot1$ of the Centigrade thermometer; that is to say, that the temperature of total privation of heat is about $2°\cdot1$ Centigrade above the absolute zero of a perfect gas-thermometer, or $272\frac{1}{2}°$ Centigrade below the freezing-point.

In applying the law of the efficiency of machines to the case of expansive heat, we must put the following interpretation on the symbols :—

Let Q_1 denote the total heat present in the expanding body during the period of receiving heat from without;

Q_2 the total heat during the period of emitting heat;

H_1 the whole heat received;

H_2 the whole heat emitted; so that

$H_1 - H_2$ is the useful effect of the engine, or the heat permanently converted into expansive power; then the proportion of the useful effect to the whole heat received is, as in equation (7),

$$\frac{H_1 - H_2}{H_1} = \frac{Q_1 - Q_2}{Q_1}.$$

If we admit the principle, which appears to me to be demonstrable, that the heat present in a body varies with temperature according to the same law for all substances, the above formula leads immediately to Carnot's law, as modified by Messrs. Clausius and Thomson. If we, further, adopt the hypothesis that expansive heat consists in vortices or eddies in atmospheres surrounding centres of molecular attraction, we are led to the conclusion, which is borne out by our present experimental knowledge so far as it extends, that the quantity of heat in a body is proportional simply to the temperature, as measured from the point of total privation of heat already mentioned. Let κ denote the position of this point on the thermometric scale; then the greatest proportion of heat which can be rendered effective by any expansive engine, receiving heat at the temperature τ_1 and emitting it at τ_2, is

$$\frac{\tau_1 - \tau_2}{\tau_1 - \kappa}, \qquad \cdots \cdots \cdots \quad (8)$$

being the formula which, in the fifth section of a paper on the Mechanical Action of Heat, I have deduced directly from the hypothesis above mentioned.

Application to Current Electricity.

(8.) In order to apply the general law of the transformation of energy and its consequences to current electricity, we must

consider in the first place how the actual energy present in a closed electric circuit is to be measured.

We know that when a closed electric circuit is not employed to produce any extraneous effect, such as induction, magnetism, or chemical analysis, its whole actual energy is expended in producing heat. We also know, through the experiments of Messrs. Riess and Joule, that the heat generated by an electric current in unity of time under such circumstances is proportional to the function called the *quantity* of the current multiplied by the function called the electromotive force. Hence we must adopt for the symbol Q the following signification,

$$Q = Mu, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where u is the quantity of the current, and M the electromotive force, measured in such a manner that their product shall represent the heat generated in unity of time in the circuit when no other effect is produced.

It is further known, that when no effect is produced but heat, the quantity of the current is equal to the electromotive force divided by a function called the resistance of the circuit; that is to say, under these circumstances

$$u = \frac{M}{R}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (E)$$

where R is that resistance; and that in all cases, whether other effects are produced or not, the heat generated is represented by the square of the quantity of the current multiplied by the resistance, or

$$H = Ru^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (F)$$

Chemical affinity constitutes a kind of potential energy, which is converted into the actual energy of electricity when substances combine. To determine the law of this transformation, we have the following facts :—The electromotive force depends on the nature of the substances which combine, and is moreover proportional to the number of surfaces in the circuit at which the combination takes place in the proper direction. Let n be this number, K a specific coefficient, then

$$M = Kn. \quad . \quad . \quad . \quad . \quad . \quad . \quad (G)$$

The quantity of the current depends also on the nature of the substances, and is proportional to the quantities of them which enter into combination at any one surface. Let z be the amount of compound formed in unity of time, k a specific coefficient; then

$$u = kz. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (H)$$

Consequently

$$Q = Mu = Kknz; \quad . \quad . \quad . \quad (10)$$

from which it appears, that if the principles which we have laid down as experimental data be rigorously correct, electrical actual energy and chemical energy are simply proportional to each other, and the amount of electrical actual energy produced in a circuit is the exact equivalent of the chemical potential energy which disappears.

Let us next examine the transformation of electrical actual energy into the potential energy of magnetism by means of soft iron; and for this purpose let a horse-shoe bar be magnetized by means of a coil encircling it, in which is a current of the energy

$$Q = Mu = Ru^2.$$

Let the keeper be at a fixed distance x from the bar, and let the amount of their mutual attraction be

$$P \cdot \phi x.$$

Then it is known by the experiments of Messrs. Joule, Lenz, and Jacobi, that within certain limits the coefficient P is proportional, for a given length and arrangement of coil, to the square of the quantity of the current, all other things being supposed constant; that is to say, it is simply proportional to the electrical actual energy; therefore, within those limits, the electrical actual energy which disappears in producing magnetic potential energy is the exact equivalent of the magnetic energy developed.

When the electric current exceeds a certain quantity as compared with the transverse section of the bar, Mr. Joule has proved that the magnetic attraction no longer increases in the exact ratio of the electrical energy, but more slowly. The conclusion to be drawn from this is somewhat remarkable. The total attraction between the bar and its keeper being proportional to P, the portion which causes electrical energy to disappear when it acts is proportional to $Q \dfrac{dP}{dQ}$, which being smaller than P, the difference, represented by $P - Q \dfrac{dP}{dQ}$, is the coefficient of a portion of magnetic attraction which acts in raising weights like that of a permanent magnet, without causing electrical actual energy to disappear. It is probable that the action of this portion of magnetism is accompanied by some peculiar variations in the molecular condition of the iron.

(9.) In order to apply the general law of the efficiency of machines to the mechanical action of electro-chemical energy, we have only to conceive for the purpose of this calculation that the reception and discharge of actual energy, which really take place at the same instant, are performed during alternate instants. Then Q_1 will represent the energy corresponding to the chemical action in the battery in unity of time, and Q_2 that corresponding

to the heat discharged in unity of time, and

$$\frac{Q_1 - Q_2}{Q_1} = \frac{M - Ru}{M}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (11)$$

the proportion of the total energy received which is converted into mechanical power.

This formula, deduced from an abstract principle by interpreting its symbols according to experimental data, agrees with results arrived at by Professor William Thomson and Mr. Joule from the special consideration of electro-chemical and magnetic forces.

If we take for granted (what is not absolutely certain) that the resistance of the circuit in an electro-magnetic engine is the same whether it is performing work or not, then let u_0 be the quantity of the current which would take place if the engine were not working, and we obtain the following value of the electromotive force,

$$M = Ru_0,$$

which reduces the above formula (11) to the following:

$$\frac{Q_1 - Q_2}{Q_1} = \frac{u_0 - u}{u_0}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (11A)$$

This is the formula given in a paper "On the Œconomical Production of Mechanical Effect from Chemical Forces" (Manchester Transactions, vol. x.), by Mr. Joule, who points out its analogy to the corresponding formula for heat. We have seen that they are particular cases of a universal principle.

To determine the mean quantity of current u in equation (11), we have the following data. Let v be the mean value of the function $\phi x \dfrac{dx}{dt}$, and

$$Pv$$

the mean amount of power developed by magnetic force in unity of time; then, m being a coefficient depending on the length and arrangement of coils round the bar, we have

$$P = mu^2,$$

and consequently

$$Pv = mu^2 v$$

is the electric energy converted into mechanical power in unity of time. Adding to this Ru^2, the quantity converted into heat, and equating the sum to the total production of electrical energy, we have

$$Mu = (R + mv)u^2,$$

and the quantity of the current is found to be

$$u = \frac{M}{R + mv}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (K)$$

This reduces equation (11) to the following,

$$\frac{Q_1 - Q_2}{Q_1} = 1 - \frac{R}{R + mv}; \quad \cdot \quad \cdot \quad \cdot \quad (12)$$

which shows that the ratio of the effect of an electro-magnetic engine, working by the induction of magnetism in a soft iron bar, to the power expended, approaches unity as the velocity increases without limit; but at the same time the actual performance diminishes indefinitely.

(10.) The foregoing examples illustrate the method of applying the general law of the transformation of energy to some of those forms with which we are most familiar, by interpreting the symbols according to the properties of the kinds of energy under consideration with which we are made acquainted by experiment. The examples are necessarily of a simple kind; but I purpose to apply the same principles to determine the laws of more complicated phænomena.

Glasgow, December 31, 1852.

XIX. *On Continued Fractions in Quaternions.* *By* Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S. &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland*[*].

[Continued from vol. iv. p. 303.]

4. THOSE who have acquired some familiarity with the interpretation of the results obtained by the Calculus of Quaternions, will have now little difficulty in seeing that the following geometrical theorems[†] are obtained from the consideration of the continued fraction $\rho_x = \left(\dfrac{\beta}{\alpha +}\right)^x \rho_0$, where $\alpha, \beta, \rho_0, \rho_x$ are real vectors, β being perpendicular to the other three, and the condition $\alpha^4 + 4\beta^2 > 0$ being satisfied.

Let c and d be two given points, and p an assumed point. Join dp, and draw cq perpendicular thereto, and towards a given hand, in the assumed plane cdp, so that the rectangle cq . dp may be equal to a given area. From the derived point q, as from a new assumed point, derive a new point r, by the same rule of construction. Again conceive that s is derived from r, and t from s, &c., by an indefinite repetition of the process. Then, *if the given area be less than half the square of the given*

* Communicated by the Author.
† These theorems are taken from art. 665 of the author's (as yet unpublished) Lectures on Quaternions.

line CD, and if a semicircle (towards the proper hand) be constructed on that line as diameter, it will be possible to inscribe a parallel chord AB, such that the given area shall be represented by the product of the diameter CD, and the distance of this chord therefrom. We may also conceive that B is nearer than A to C, so that ABCD is an uncrossed trapezium inscribed in a circle, and the angle ABC is obtuse. This construction being clearly understood, it becomes obvious, Ist, that because the given area is equal to each of the two rectangles, CA . DA and CB . DB while the angles in the semicircle are right, then, whether we begin by assuming the position of the point P to be at the corner A, or at the corner B, of the trapezium, every one of the derived points, Q, R, S, T, &c., will *coincide* with the position so assumed for P, however far the process of derivation may be continued. But I also say, IInd, that if *any other point* in the plane, *except these two fixed points*, A, B, be assumed for P, then not only will its *successive derivatives*, Q, R, S, T, . . be all *distinct* from it, and from each other, but they will *tend* successively and indefinitely to *coincide with that one of the two fixed points* which has been above named B. I add, IIIrd, that if, from any point T, distinct from A and from B, we go *back,* by an *inverse* process of derivation, to the *next preceding point* S of the recently considered series, and thence, by the same inverse law, to R, Q, P, &c., *this* process will produce an *indefinite tendency* to, and an *ultimate coincidence* with, the *other* of the two fixed points, namely, A. IVth. The common *law* of these two tendencies, direct and inverse, is contained in the formula

$$\frac{QB \cdot PA}{QA \cdot PB} = \frac{CB}{CA} = \text{constant};$$

which may be variously transformed, and in which the constant is independent of the position of P. Vth. The *alternate points,* P, R, T, &c., are all contained on one *common circular segment* APB; and the *other system of alternate points,* Q, S, &c., has for its locus *another circular segment,* AQB, on the *same fixed base,* AB. VIth. The *relation* between these *two segments* is expressed by this other formula, connecting the *angles* in them,

$$\text{APB} + \text{AQB} = \text{ACB};$$

the angles being here supposed to *change signs,* when their vertices cross the fixed line AB.

Observatory, December 30, 1852.

[To be continued.]

XX. *On the Conditions necessary and sufficient to be satisfied in order that a Function of any number of Variables may be linearly equivalent to a Function of any less number of Variables. By* J. J. Sylvester, *F.R.S.*[*]

IN the Cambridge and Dublin Mathematical Journal for November 1850, I defined an order as signifying any linear function of a given set of variables, and spoke of a general function of (n) variables as losing (r) orders when the relation between its coefficients is such that it is capable of being expressed as a function of ($n-r$) orders only. It will be highly convenient to preserve the same nomenclature for the purposes of the present investigation.

Dr. Otto Hesse, in a long memoir in Crelle's Journal, the contents of which have been described to me[†], but which I have not yet been able to procure, has given a rule for determining the analytical conditions for the loss of one order. I propose to give a more simple and comprehensive scheme of conditions than Professor Hesse appears to have discovered, applicable not to this case only, but to that of the loss of any number whatever of orders, and shall moreover show in what relation the substituted orders stand to the given variables.

Dr. Hesse's rule had been previously stated by me in the 4th section of my Calculus of Forms (Camb. and Dub. Math. Journ. May 1852[‡]) as applicable to the case of a general function of the 3rd degree of 3 variables becoming the representative of 3 right lines diverging from the same point, which is the case of a cubic function of 3 variables becoming a function of 2 linear functions of these variables, that is to say, losing one order: this, perhaps, might have been noticed in the Professor's memoir. I gave also another rule for the same case; but the true fundamental scheme of conditions about to be set forth will be seen to embrace as mere corollaries all such and such like rules, which in fact supply more or less arbitrary combinations of

[*] Communicated by the Author.

[†] A distinguished mathematical friend in Paris communicated to me with great admiration Professor Hesse's result over night. I ventured to affirm that, to one conversant with the calculus of forms, the problem could offer no manner of difficulty. An hour's quiet reflection in bed the following morning, or morning after, sufficed to disclose to me the true principle of the solution.

[‡] Vide vol. vii. p. 187. "When U represents a pencil of 3 rays meeting in a point, $\frac{dS}{da}=0$, $\frac{dS}{db}=0$, &c., and also therefore T$=0$" (S and T being the 2 Aronholdian invariants of U, and a, b, c, &c. the coefficients of U); "also in place of this system may be substituted the system obtained *by taking all the coefficients of the Hessian zero*."

the conditions, rather than the naked conditions themselves in their simple form and absolute totality.

I shall call the function to be dealt with U, and shall consider U to be a *homogeneous* * rational function of m dimensions in respect of x_1, x_2, ... x_n, and shall inquire what are the conditions which must obtain when U is capable of being expressed as a function of only $(n-r)$ orders, say l_1, l_2, ... l_{n-r}, each of which is of course a homogeneous linear function of the given (n) variables.

Let the term derivative of U be understood to mean any result obtained by differentiating U any number of times with respect to one or more of the variables x_1, x_2, ... x_n. The first derivatives will be of $(m-1)$ dimensions, the second derivatives of $(m-2)$ dimensions, and so on; and finally, the $(m-1)$th derivatives will be homogeneous linear functions of x_1, x_2, ... x_n. Suppose U to be expressible as a function of l_1, l_2, ... l_{n-r}. It is immediately obvious that the derivatives from the 1st to the $(m-1)$th inclusive will be all expressible as homogeneous functions of l_1, l_2, ... l_{n-r}, and vanish when these vanish. But this statement is in substance pleonastic; for by means of Euler's well-known law, any derivative of U, say K, may be expressed (to a numerical factor *près*) under the form of

$$x_1 \frac{dK}{dx_1} + x_2 \cdot \frac{dK}{dx_2} + \ldots + x_n \cdot \frac{dK}{dx_n};$$

and consequently, whenever the linear derivatives of U vanish, all the upper derivatives of U, including U itself, must vanish at the same time. The number of these linear derivatives, say ν, will be the number of terms in a homogeneous function of (n) variables of $(m-1)$ dimensions, that is to say,

$$\frac{n \cdot (n-1) \ldots (n-m+2)}{1 \cdot 2 \quad \ldots \quad (m-1)}.$$

Again, if all the ν linear derivatives vanish when the $(n-r)$ equations $l_1=0$, $l_2=0$, ... $l_{n-r}=0$ are satisfied, r being greater than zero, this can only happen by virtue of these ν derivatives being linear functions of $(n-r)$ of them. Now, conversely, I shall prove, that if it be true that all the linear derivatives of U are linear functions $(n-r)$ of them, then U may be expressed as a function of these $(n-r)$; and this rule, as will be immediately made apparent, will give the necessary and sufficient conditions for the loss of r orders in the most simple and complete

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* It is a common error to regard homogeneity of expression as merely a means for satisfying the desire for symmetry; the ground of its application and utility in analysis lies, in fact, much deeper; it is essentially a *method* and a *power*.

form by which they admit of being expressed. For the proof of the rule, only one additional remark has to be made in addition to that already made, of the vanishing of the linear derivatives necessarily implying the simultaneous evanescence of all the other derivatives; this additional remark being, that if the derivatives of any class, linear or otherwise, *quâ* one set of variables, become all zero, the derivatives of the same class, *quâ* any other set of variables linear functions of the first set and the same in number, will also become zero, for they are evidently expressible as linear functions of the first set.

Now let $d_1, d_2, \ldots d_{n-r}$ be any $(n-r)$ linear derivatives of U, of which all the other of the ν derivatives of this class are linear functions, so that they vanish when these $(n-r)$ vanish, and let U be expressed as a function of $(d_1, d_2, \ldots d_{n-r}; x_1, x_2 \ldots x_r)$. Then we may write

$$U = \phi_{m,0} + \phi_{m-1,1} + \phi_{m-2,2} + \ldots + \phi_{1,m-1} + \phi_{0,m},$$

where in general $\phi_{m-\epsilon,\epsilon}$ denotes a function homogeneous and of $m-\epsilon$ dimensions in respect to $d_1, d_2, \ldots d_{n-r}$, and homogeneous and of ϵ dimensions in respect to $x_1, x_2, \ldots x_r$. Now the linear derivatives of U all vanish when $d_1 = 0, d_2 = 0 \ldots d_{n-r} = 0$ for all values of $x_1, x_2, \ldots x_r$. Hence $U = 0$ on the same supposition, and hence $\phi_{0,m}$ is similarly zero. Also the first derivatives of U, *quâ* $d_1, d_2, \ldots d_{n-r}$, must vanish on the same supposition. Hence $\phi_{1,m-1}$ is identically zero; and so by taking the 2nd, 3rd, ... up to the $(m-1)$th or linear derivatives of U in respect to $d_1, d_2, \ldots d_{n-r}$, we find successively $\phi_{2,m-2}, \phi_{3,m-3}, \ldots \phi_{m-1,3}$ each identically zero, and consequently

$$U = \phi_{m,0} = \phi(d_1, d_2, \ldots d_{n-r}),$$

as was to be proved. To express the fact of the ν derivatives being linear functions of $(n-r)$ of them, form a rectangular matrix with the coefficients of the ν linear derivatives. This matrix will be n terms in breadth and ν terms in depth. Let $r = 1$: it is a direct consequence of the rule which has been established, that every full determinant consisting of a square (n) terms by (n) terms that can be formed out of this rectangular matrix must be zero: again, let $r = 2$; all the first minors, that is to say, all the determinants composed of squares $(n-1)$ terms by $(n-1)$ terms, must be zero, and so in general a loss of (r) orders will require that the $(r-1)$th minors shall all vanish; if $r = n$, the $(n-1)$th minors, *i. e.* the simple terms of the matrix which are all coefficients of U, must vanish, or in other words, when the function is of zero orders all the coefficients vanish (an obvious truism). Thus, then, we see that the true rule for the loss of one order in a polynomial of any degree is precisely the same as the well-known rule for the loss of one order in a qua-

dratic function; the speciality in the latter case consisting merely in the fact that ν being equal to n, the rectangular matrix becomes a square, and there is only one full determinant. Moreover, for any other value of (r) the above rule coincides with that given by me some time back in the Philosophical Magazine for the case of quadratic functions.

Professor Hesse's rule for finding conditions applicable to the loss of one order is, as I have already stated, a consequence of the more simple scheme of conditions above given. It consists in forming the determinant

$$\begin{vmatrix} \dfrac{d^2U}{dx_1{}^2}; & \dfrac{d^2U}{dx_1 . dx_2}; & \cdots & \dfrac{d^2U}{dx_1 . dx_n} \\[2ex] \dfrac{d^2U}{dx_2 . dx_1}; & \dfrac{d^2U}{dx_2 . dx_2}; & \cdots & \dfrac{d^2U}{dx_2 . dx_n} \\[1ex] \cdot \quad \cdot & \cdot \quad \cdot & \cdot \quad \cdot & \\[1ex] \dfrac{d^2U}{dx_n . dx_1}; & \dfrac{d^2U}{dx_n . dx_2}; & \cdots & \dfrac{d^2U}{dx_n . dx_n}, \end{vmatrix}$$

and equating the coefficients of this determinant fully developed separately to zero*. The attachment of the Professor to this particular form of covariant (I use the language of the calculus of forms) is readily intelligible, seeing the admirable application which he has made of it to the canonization of the cubic function of 3 variables, but it is really foreign to the nature of the present question; the coefficients of this covariant may easily be shown to be merely the full determinants of the $n \times \nu$ rectangular matrix above described, or linear functions of these said determinants with numerical coefficients. Hence the ground of its applicability.

Returning to the rule of the matrix, if we suppose the number of variables to be 2, and call the coefficients of U

$$a_0, \ na_1, \ n \cdot \frac{n-1}{2} a_2, \ \ldots a_n,$$

our rectangle becomes

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* A form capable of being so derived I have elsewhere termed (in compliment to M. Hesse) the Hessian of the function to which it appertains. This is the trivial name which is much needed on account of the frequent occurrence of the form, and has been adopted by Mr. Salmon in his admirable treatise on the higher plane curves. In systematic nomenclature it would be termed the discriminant of the quadratic emanant, or more briefly, the quadremanative discriminant. I have discovered quite recently that the long sought for symmetrical, and by far the most easy practical process for discovering the number of the real roots of an equation, is contained in, and may be deduced immediately from, a certain transformation of its Hessian!

$$\begin{vmatrix} a_0 & a_1 \\ a_1 & a_2 \\ a_2 & a_3 \\ \cdot \; \cdot \; \cdot \; \cdot \\ \cdot \; \cdot \; \cdot \; \cdot \\ a_{n-1} & a_n \end{vmatrix}$$

and the conditions become

$$a_0 \cdot a_2 - a_1^2 = 0 \qquad a_0 \; \cdot \; a_3 - a_1 \; \cdot \; a_2 = 0$$

$$a_1 \cdot a_3 - a_2^2 = 0 \qquad \cdot \; \cdot \; \cdot \; \cdot \; \cdot \; \cdot \; \cdot \; \cdot$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$a_{n-2} \cdot a_n - a_{n-1}^2 = 0 \qquad a_{n-3} \cdot a_n - a_{n-2} \cdot a_{n-1} = 0, \text{\&c.,}$$

all of which equations are obviously true (when the function loses an order, that is to say, becomes a perfect power) and are satisfied (special cases excepted) when any $(n-1)$ independent equations out of the entire number obtain; so that the number of conditions implied in the property to be represented is in exact conformity with the number of independent equations derived from the matrix, *i. e.* equations which, when satisfied, will in general cause all the rest to be satisfied. This conformity manifests itself also in the case of a quadratic function of n variables. But except in these two limiting (and, in an occult sense, reciprocal*) cases of a function of 2 variables of the nth degree, or of the degree 2 and n variables, this conformity in measure as the degree or number of variables rises, although it

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* There are frequent cases occurring in the calculus of forms of interchange between the degree of a function and the number of variables which it contains. Thus, to select a striking example (although one where the interchange is not exact), the theory of the real and imaginary roots or factors of a homogeneous function of 2 variables and of the nth degree may be shown to be immediately dependent upon the determination of the specific nature of a concomitant homogeneous function of the 2nd degree and of $(n-1)$ variables. For instance, if any ordinary algebraical equation of the 5th degree be given, a homogeneous quadratic function of 4 variables may be constructed, representing, consequently, a surface of the 2nd degree [the coefficients of which (as indeed is true whatever be the degree of the equation) will be quadratic functions of the coefficients of the given equation]; and such that, according as the surface so represented belongs to the class of 1°, impossible surfaces; 2°, the ellipsoid or hyperboloid of two sheets; 3°, the hyperboloid of one sheet; the given equation will have 5, 3, or only 1 real root! Moreover, an equality between two of the roots of the equation will be denoted by the loss of one order in the associated quadratic function; and so many orders altogether will be lost as there are independent equalities existing between the roots. An entirely new light is thus thrown on M. Sturm's theorem; and the number of real and imaginary roots in an equation is for the first time made to depend upon the signs of functions symmetrically constructed in respect to the two ends of the equation, which has long been felt as a desideratum.

must substantially continue to exist, becomes, and in an accelerated degree, less and less apparent.

Thus, take the simple case of a cubic function of 3 variables, and let us confine ourselves to the consideration of the conditions which must be satisfied when it loses a single order.   Let U be written out at length,

$$ax^3 + by^3 + cz^3 + 3hyz^2 + 3izx^2 + 3jxy^2 + 3h'y^2z + 3i'z^2x$$
$$+ 3j'x^2y + 6mxyz.$$

The matrix formed out of the coefficients of the linear derivatives becomes

$$\begin{vmatrix} a & j' & i \\ j & b & h' \\ i' & h & c \\ m & h' & h \\ i & m & i' \\ j' & j & m \end{vmatrix}$$

Now by the homoloidal law, if the terms in this rectangle were all unlike, the number of full determinants (3 terms by 3 terms) whose evanescence (except for special values) determines the evanescence of all the rest, should be $(6-3+1)\,(3-3+1)$, *i. e.* 4; but in the actual case, since the evanescence of all the full determinants is a necessary consequence of the function becoming a cubic function of 2 orders (*i. e.* breaking up into the product of 3 linear functions of x, y, z), and as this decomposability, as is well known, implies only the existence of 3 affirmative conditions, the four full determinants

$$\begin{array}{cccccccccccc} a & j' & i & & a & j' & i & & a & j' & i & & a & j' & i \\ j & b & h'\,; & & j & b & h'\,; & & j & b & h'\,; & & j & b & h'\,; \\ i' & h & c & & m & h' & h & & i & m & i' & & j' & j & m* \end{array}$$

which in the general case would be entirely independent, in this case cease to be so; and the vanishing of 3 of them must draw along with it by necessary implication (except for special values)

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* That is to say, a syzygetic relation must connect these four determinants.   I may as well here repeat, that when the vanishing of a set of (i) rational integral functions necessarily, and without cases of exception, implies the vanishing of another rational integral function, then this function is termed a syzygetic function of the others; and some power of it must be expressible under the form of a sum of (i) binary products of rational integral functions, one factor of each of which products must be one of the (i) given functions.   When the vanishing of all but one of a set of functions in general necessarily implies the vanishing of that one, but subject to cases of exception for specific values of the variables, then it can only be affirmed that the functions of the set are in syzygy; that is to say, that the sum of the products of each of them respectively by some rational integral function

the evanescence of the 4th, for thus only can the necessary conformity between the number of affirmative conditions and the number of unimplicated equations come to take effect. The clear and direct putting in evidence of this peculiar species of implication demands and deserves to be minutely considered; and as it must in part borrow its explanation from the very little yet known of syzygetic relations, so it must also throw new light on that great and important, but as yet unformed and scarcely more than nascent theory.

In conclusion, it is apparent from the demonstration above will be zero: the equation expressing this relation is termed a syzygetic equation.

Thus, if we take the 3 full determinants that can be formed out of the matrix

$$\begin{matrix} a & \alpha \\ b & \beta \\ c & \gamma, \end{matrix}$$

i. e. $a\beta - b\alpha$; $b\gamma - c\beta$; $c\alpha - a\gamma$,

these are in syzygy, for we can form the equation

$$c \times (a\beta - b\alpha) + a(b\gamma - c\beta) + b(c\alpha - a\gamma) = 0.$$

This, however, is not the only equation of the kind that can be formed, for

$$\gamma(a\beta - b\alpha) + \alpha(b\gamma - c\beta) + \beta(c\alpha - a\gamma) = 0$$

is also identically true. We see in this case that the evanescence of any 2 of the 3 functions $a\beta - b\alpha$; $b\gamma - c\beta$; $c\alpha - a\beta$ will in general imply the third, subject, however, to special cases of exception. Thus, if the 1st and 2nd vanish, the 3rd must vanish unless b and β *both* vanish; if the 2nd and 3rd vanish, the 1st must vanish unless c and γ both vanish; if the 3rd and 1st vanish, the second will vanish unless a and α both vanish. It will thus be seen that a peculiar species of *astricted* syzygy obtains between the 3 proposed functions, which enables us to affirm that *in general*, and except under *extra special* conditions, all three must vanish simultaneously. If 2 out of the 3 vanish, and the 3rd does not vanish, it is not merely (as might at the first blush of the theory of syzygy be conjectured) because some one other function vanishes in its place, but necessarily because a *plurality* of entirely independent functions (2 simple letters as it happens here) each separately vanish. Thus we see how all but one of a set of functions $\chi_1, \chi_2, \ldots \chi_n$ may *in general,* and yet *not universally,* necessarily vanish when all the rest vanish : to say that *one* syzygetic equation such as

$$\chi_1 \cdot \chi'_1 + \chi_2 \cdot \chi'_2 + \ldots + \chi_n \cdot \chi'_n = 0$$

obtains, is not enough to explain the circumstances of the case; the fact is, that several distinct systems of values of $\chi'_1, \chi'_2, \ldots \chi'_n$ will be found capable of satisfying the equation, so that each of the functions $\chi_1, \chi_2, \ldots \chi_n$ will have a *system* of syzygetic factors attached to it, and these unrelated, in the wide sense, that is, that if we take χ'_n, χ''_n, any two of the syzygetic factors attached to χ_n, they will *not be in syzygy* with $\chi_1, \chi_2, \ldots \chi_{n-1}$; so that when these $(n-1)$ functions vanish, the vanishing of χ'_n and χ''_n represents two distinct and completely independent conditions. Thus, in fine, the mutual implication of functions will in general denote the possibility of forming a *series* of syzygetic equations between them,—a remark, this, of no minor importance.

given, that when U, a function of (n) variables, becomes express-
ible as a function of $(n-r)$ orders, these orders may be taken
respectively any independent linear functions of the linear deri-
vatives of U, which remark completes the theory of functions,
subject to the loss of one or more orders. It is obvious (and I
am indebted to my esteemed friend Mr. Cayley for the remark),
that the conditions furnished as above by the $(m-1)$th, *i. e.*
linear derivatives, are identical with and may be more elegantly
replaced by those involved in the assertion of the existence of
linear relations between the 1st or $(m-1)$th degreed derivatives,
and we have then this very simple rule; *if ϕ, a function of
$x_1, x_2, \ldots x_n$, is expressible as a function of $n-r$ linear functions
of $x_1, x_2 \ldots x_n$, it is necessary and sufficient that (r) independent
linear relations shall exist between*

$$\frac{d\phi}{dx_1}, \quad \frac{d\phi}{dx_2}, \quad \cdots \quad \frac{d\phi}{dx_n}.$$

This rule itself also, it is evident, is capable of an independent
and immediate demonstration by means of integrating the partial
differential equation or equations by which it admits of being
expressed. The above theory may readily be extended to func-
tions of several systems of variables. Thus, for instance, the
determinant

$$\begin{vmatrix} a & b & c \\ a' & b' & c' \\ a'' & b'' & c'' \end{vmatrix}$$

vanishing will be indicative of the function

$$\left\{ \begin{array}{l} a\ xu + b\ xv + c\ xw \\ + a'\ yu + b'yv + c'yw \\ + a''zu + b''zv + c''zw \end{array} \right\}$$

being linearly equivalent to a function of the form

$$\left\{ \begin{array}{l} Ax'u' + Bx'v' \\ + Cy'u' + Dy'v' \end{array} \right\}$$

i. e. losing an order in respect of each of the two systems x, y, z;
u, v, w; and so in general.

7 New Square, Lincoln's-Inn,
    January 7, 1853.

XXI. *Analysis of Euclase.* By J. W. MALLET, *Ph.D.*[*]

AN analysis of 19·83 grs. of this mineral gave the following results reduced to 100 parts :—

		Atomic relations.
Silica	44·18	·950
Alumina . . .	31·87	·620
Glucina . . .	21·43	·564 ⎫ ·580
Peroxide of iron .	1·31	·016 ⎭
Peroxide of tin .	·35	
	99·14	

These numbers are not far from those of Berzelius, and correspond nearly to three atoms silica, two alumina, and two glucina, assuming the latter earth to be a sesquioxide, and hence we have the formula of the mineral $2G^2O^3, SiO^3 + 2(Al^2O^3, SiO^3)$; or, if alumina and glucina be isomorphous, $4(Al^2O^3 + G^2O^3), 3SiO^3$.

The fragments of crystals employed for the analysis were perfectly clear and transparent, of a very light mountain-green colour, and spec. grav. 3·036.

Particular care was taken in determining the presence and amount of the oxide of tin, the existence of which in this and some other hard silicates appears very difficult to account for, and in some degree connects itself with the question of the adoption of SiO^3 or SiO^2 as the formula for silicic acid.

XXII. *On some Facts connected with Geometrical Reasoning.* By JOHN POPE HENNESSY[†].

IN a late paper on some geometrical demonstrations, I alluded to the peculiar difficulty which has been found to exist when we attempt to give direct proof to certain theorems in elementary geometry; and I stated my belief that this difficulty arose from a defective premiss on which the proof depended. To discover the precise nature of such defect, I compared the definition of parallel lines, being one of those referred to, with the remaining definitions and axioms. The result arrived at by this comparison I venture to hope will somewhat elucidate the interesting question as to the origin of Euclid's *reductio ad absurdum*.

I found that all the definitions prefixed to the first book, which are used in the Elements, are, with one exception, affirmatives; the negative definition being that in which, according to my supposition, a defect existed. It is as follows :—

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[*] Communicated by the Author.
[†] Communicated by the Author.

" Parallel right lines are such as are in the same plane, and which, being produced continually in both directions, would never meet."

Adopting this definition, we are forced to give an indirect proof to the XXVIIth; but if we change its form to an affirmative, that proposition can be proved directly.

I also found, that of the twelve axioms one only was negative.

This led me immediately to discover a trivial, but extraordinary error, which the editors of Euclid* have promulgated in asserting that the first instance of indirect proof is to be found in the demonstration of the sixth proposition. The incorrectness of this assertion is easily shown in the following manner.

The so-called axiom in question is the tenth :—

" Two right lines cannot enclose a space."

This is used by Euclid in the concluding portion of the proof he gives to the fourth proposition. It is needless to state this proof at length, as I wish to call attention only to the following paragraph :—

" Since the extremities of the bases BC and EF coincide, these lines themselves must coincide; for if they did not, they would include a space (Xth ax.). Hence the sides BC and EF are equal."

By developing this argument, as we generally do in practice, the latent *reductio ad absurdum* becomes manifest.

Since the extremities of the bases BC and EF coincide, the lines themselves must coincide; if not, let EF fall above BC : according to the tenth axiom, two right lines cannot enclose a space, but here two right lines do enclose a space, which is absurd. In the same manner it can be shown that EF could not fall below BC; and as it cannot fall either above or below, it must fall on it.

If the hypothetical assumption, that one line lie above the other, was not adopted by Euclid, why does he call in the aid of the Xth axiom? And if he has made that assumption, his line of reasoning is analytical, his demonstration clearly *indirect*.

As in the preceding instance, the substitution of an affirmative premiss will permit us to give a direct proof to the fourth proposition. Before doing so I wish to refer to a collateral circumstance, the scholastic controversy about geometrical axioms, which I think has an important bearing on this investigation.

The most eminent metaphysicians have differed on the question, whether we should regard the definitions or the axioms as the *principles* in mathematical science. Mr. Locke was in favour of the former; he says, referring to the case of an individual who has learnt that the square of the hypothenuse in a right-

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* See any edition. Dr. Lardner's, p. 20; Mr. R. Potts's, p. 48.

angled triangle is equal to the sum of the squares of the two other sides, " the knowledge that 'the whole is equal to all its parts,' and 'if you take equals from equals the remainders will be equal,' helped him not, I presume, to this demonstration : and a man may, I think, pore long enough on these axioms without ever seeing one jot the more of mathematical truths*."

It is remarkable that Dr. Whewell†, one of the chief opponents of this view, should have cited in his arguments those axioms to which Mr. Locke did not refer, such as the X., XI. or XII. And M. D'Alembert ‡, whose opinions correspond with those in the Essay on Human Understanding, appears to exclude these three from the list. On this subject Mr. Dugald Stewart is perfectly clear; he advocates the doctrine that the axioms are not the principles of our reasoning, and he says, " In order to prevent cavil, it may be necessary for me to remark, that when I speak of mathematical axioms I have in view only such as are of the same description with the *first nine* of those which are prefixed to the Elements of Euclid§." This statement he subsequently reiterates.

I draw attention to these opinions for the purpose of showing that the substitution of affirmatives for negatives would be of importance in a philosophical point of view, apart from the fact of its being productive of synthetical reasoning, in permitting us to remove from the list of axioms those three which never should have been there, and about which much speculative and unsatisfactory discussion has taken place.

Having struck out the tenth and twelfth axioms, and, for the present, incorporated the eleventh with the tenth definition, the only substitutions required should be made in the definitions of parallel and straight lines.

In place of the XXXV. I would propose the following :—

Parallel lines are such, that if they meet a third right line, the two interior angles on the same side will be equal to two right angles.

And instead of the IV. definition :—Right lines are such that if they coincide in any two points one line must lie entirely in the other.

It may be urged as an objection against the first of these, that it is not as self-evident as the definition it professes to replace; but it should not be forgotten that Euclid uses *two* definitions ; and that the *one* here put forward, by which all the properties of

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* Essay on the Human Understanding, vol. i. p. 305.
† Philosophy of the Inductive Sciences, 2nd edit. vol. ii. p. 601.
‡ *Encyclopédie.* Discours préliminaire des éditeurs, xvi.
§ Philosophy of the Human Mind, vol. ii. pp. 40 and 527.

parallel lines may be deduced, is certainly as obvious as that to which he improperly gave the title of axiom.   The numerous attempts which have been made to supersede his theory have generally been met by two objections,—(1), that the subsequent demonstrations are much more complex, (2) and full of embarrassing *reductio ad absurdum.* These are put forward by Dr. Lardner very clearly; he says, " When you have once admitted Euclid's axiom, all his theorems flow from that and his definition, as the most simple and obvious inferences.   In other theories, after conceding an axiom much further removed from self-evidence than Euclid's, a labyrinth of complicated and indirect demonstration remains to be threaded, requiring much subtlety and attention to be assured that error and fallacy do not lurk in its mazes."

It will presently be seen that the demonstrations I propose to give are not open to either of those objections; whereas Euclid's, a fact which Dr. Lardner appears to have overlooked, are most decided specimens of indirect proof.

The definitions given by Euclid of a right line and plane surface are precisely the same, and have been objected to for many reasons, of which, I conceive, the most serious, that they cannot be used in his geometry, is equally applicable to those given by Archimedes and Plato.   Now M. Legendre has adopted the definition of a straight line given by the former, that it is the shortest distance between any two points; but he has not taken Archimedes' definition of a plane surface, which is exactly similar : in this case he has substituted for Euclid's the following, attributed to Hero :—"A plane surface is such that the right line joining every two points which can be assumed upon it lies entirely in the surface."   This definition has received the unanimous approbation of modern geometers, even those most conservative of Euclid's arrangement.   It is not this, however, which induced me to make a definition of right lines precisely similar; but the more important fact, that every other definition I have seen was either of no utility in geometric reasoning or of a negative form.   I cannot avoid citing the tacit acknowledgement that such a definition is better than Euclid's Xth axiom, which is given by Mr. Mill*, in tracing deductive truths to their original inductive foundation.   He proves the fifth proposition from first principles by six formulæ, the third of which is " straight lines having their extremities coincident coincide."   It is obvious that the simplicity and elegance of the proof in this case would have been interfered with if this definition has not been assumed.

* System of Logic, 2nd edit. vol. i. p. 286.

The following are the direct demonstrations of the twenty-seventh and twenty-ninth propositions, which flow from the definition of parallel lines I have given.

XXVII. If a line intersect two right lines and make the alternate angles equal to each other, these right lines are parallel. DCB and ECB are equal to two right angles, but the former is equal to ABC, therefore ABC and ECB are equal to two right angles; therefore (def.) AB and CD are parallel.
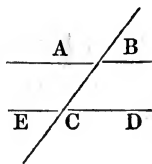
XXIX. If a right line intersect two parallel right lines it makes the alternate angles equal. (In the same figure) ABC and ECB are equal to DCB and ECB; therefore ABC and DCB are equal.

IV. Having assumed the definition which I suggest, a direct demonstration immediately occurs : this is so obvious that a proof is unnecessary.

The fact that this proposition is proved indirectly, and that such a form of proof only can be given to it, unless an alteration similar to what I have made be adopted, may point out a method of testing the accuracy of the very interesting conjecture which Mr. Sylvester has published on this subject. For example, if it is indispensable for the proof of the following theorem, I think we should assume such proof to be fundamentally indirect : the theorem is taken from Mr. Sylvester's paper which appeared in the Philosophical Magazine for November 1852, but according to his criterion should admit of the direct form :—"To prove that if from the middle of a circular arc two chords be drawn, and the nearer segments of these cut off by the line joining the end of the arc be equal, the remoter segments will also be equal." It is difficult to conceive any proof of this theorem which would not ultimately rest on the fourth proposition, and consequently rest on *reductio ad absurdum.* Such a proof, however, may be found to exist, although the writer of this paper has been unable to discover it. An opinion differing from Mr. Sylvester's, in a mathematical inquiry where certainty has not been arrived at, should be put forward with diffidence; still more so when that gentleman's conjecture has received, to a certain extent, the approval of Professor De Morgan *.

Queen's College, Cork,
    December 28, 1852.

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* See Phil. Mag. December 1852.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 66.]

Nov. 25, THE following papers were read:—
1852.　　1. "New solution of Kepler's Problem." By Professor P. A. Hansen.

It is well known how much labour has been bestowed by geometers on the solution of Kepler's Problem, and what complicated results have been obtained for the coefficients in the expression for the Equation of the Center. I have lately found a new solution of this problem, which differs strikingly from former solutions in this respect, that it leads to an unexpectedly simple law of coefficients. It is as follows:—

Let g be the mean anomaly;
ϕ be the angle of excentricity;
i be any positive integer number.

Put $\beta = \tan \frac{\phi}{2}$, $\mu = i \cos^2 \frac{\phi}{2}$,

$$P_i = 1 + \mu + \frac{\mu^2}{2} + \frac{\mu^3}{2 \cdot 3} + \&c. + \frac{\mu^i}{2 \cdot 3 \ldots i}; \qquad Q_1 = 1 - \mu,$$

$$P_{i+1} = P_i + \frac{\mu^{i+1}}{2 \cdot 3 \ldots i+1} \qquad\qquad Q_2 = Q_1 + \frac{\mu^2}{2},$$

&c.

$$Q_3 = Q_2 - \frac{\mu^3}{2 \cdot 3},$$

&c.

Then the Equation of the Center

$$= (1 - \beta^2) \Sigma_1^\infty \frac{2}{i} \left\{ P_i \cdot \beta_i + P_{i+1} \cdot Q_1 \cdot \beta^{i+2} + P_{i+2} \cdot Q_2 \cdot \beta^{i+4} + \&c. \right\} \sin ig.$$

The analysis which has led me to this, and the form of this result, are by no means peculiar to the Equation of the Center only, but apply to all functions which it is necessary to develope in series proceeding by sin ig or cos ig.

2. "An Experimental Inquiry undertaken with the view of ascertaining whether any, and what signs of Current Force are manifested during the organic process of Absorption (Lacteal) in living animals."—Part II. By H. F. Baxter, Esq.

In the experiments related in this paper, it is shown that when the electrodes of a galvanometer are brought into contact, one with the mucous membrane of the intestine, and the other with the chyle flowing from the lacteal of the same point, an effect upon the needle occurs indicating the chyle to be *positive*. The effects may be partly due to the changes which take place during *secretion*, the mesentery acting as a conducting body; this supposition however will not negative the conclusion that the effects may be attributed, in some measure at least, to the changes which occur during lacteal absorption.

3. "An Experimental Inquiry undertaken with the view of ascer-

taining whether any, and what signs of Current Force are manifested during the organic process of Assimilation in the Muscular and the Nervous Tissues in living animals."—Part III. By H. F. Baxter, Esq.

§ I. *On the existence of Current Force in the Muscular Tissues.*

After relating the conclusions arrived at by Matteucci in reference to the origin of the *muscular current*, the author endeavoured to obtain more direct evidence by forming a circuit between the muscular tissue and the venous blood; the effects however were but slight, they nevertheless indicated the *tissue* and the *venous* blood to be in opposite electric states. The results of the experiments tend to confirm the inferences of Matteucci.

§ II. *On the existence of Current Force in the Nervous Tissues.*

After referring to the results obtained by Pacinotti, Puccinotti Matteucci, and Du Bois Reymond, experiments are related in which it is shown that if one electrode be inserted into the substance of the brain, and the other be brought into contact with the blood flowing from the internal jugular vein, an effect upon the needle occurs indicating the blood to be *positive*; an effect also was easily obtained if the latter electrode was placed in contact with any other part of the animal, such as the muscles. Reasoning from the results obtained, combined with physiological evidence, it was considered that they tend to establish the conclusion that the effects are due to the changes which occur during the organic process of assimilation or nutrition.

4. " On the Theory of Waves." By Andrew John Robertson, Esq.

The author remarks that in the seventh volume of the Cambridge Philosophical Transactions, Mr. Earnshaw has a paper on the Mathematical Theory of the Waves of Translation, and that the objection to the theory therein given has been pointed out by Professor Stokes in a Report to the British Association, namely, that it requires a mathematically sudden generation and destruction of motion. Previously to his having an opportunity of reading Mr. Earnshaw's paper he had considered the subject in a manner entirely different (Phil. Mag. Dec. 1850, March 1851). The analysis he then employed was not however such as to lead to results sufficiently general, and he has in consequence now employed a higher analysis, in the application of which he acknowledges himself indebted to Mr. Earnshaw's paper for considerable assistance.

Taking the results deduced by Mr. Scott Russell from his experimental inquiry as the basis of his investigation, the author assumes, —1st, that the horizontal motion, produced by the passage of a wave, in every particle of any vertical column is the same; and 2nd, that the velocity of transmission is uniform. On these principles he deduces the value of c, the velocity of transmission,

$$c = (h \pm 2k)\sqrt{\frac{g}{h \pm k}};$$

in which h is the depth of undisturbed waters; $2k$ is the height of a positive and the depth of a negative wave; and g the accelerating

force of gravity. Hence it appears that the velocity is greater for a positive than for a negative wave in the same channel.

Comparing this result with Mr. Scott Russell's experiments, it appears that on fourteen experiments of positive waves, the total error is $3\frac{1}{2}$ per cent. on the sum of all the velocities; and that on sixteen experiments of negative waves, the error is scarcely 2 per cent. on the sum of all the velocities. The author infers that it may therefore be safely considered that the experiments bear out the theory, which shows that the positive and negative waves are phenomena of the same class, and not distinct, as maintained by Mr. Scott Russell and Mr. Earnshaw.

The next point considered is the horizontal motion of the individual particles occasioned by the passage of a wave. It is shown that the velocity of any particle is shown by the equation

$$u = \pm c \frac{2k}{h \pm 2k} \sin^2 q(ct - x),$$

in which $q = \frac{1}{c} \sqrt{\left(\frac{g}{2(h \pm k)} \cdot \frac{h \pm 2k}{h} \right)}.$

At the crest of the wave this becomes $\pm c \dfrac{2k}{h \pm 2k}$; and when the height of the wave is equal to the depth of the water, the velocity is $\dfrac{c}{2}$, or half the velocity of the wave itself. In a negative wave the motion of the particles is in the direction opposite to that in which the wave is moving.

According to Mr. Scott Russell's observations, a wave breaks when its height is equal to the depth of water, but the author considers that Mr. Russell did not succeed in producing a wave of a cusped form at once, and that it assumed that form and broke only when it travelled up a channel with a rising bottom. He further observes, that at the commencement and termination of the motion, the direction of the particle is vertical; under the crest of the wave it is horizontal; and that the path has an oval form, but evidently not an ellipse. He next deduces λ, the length of a wave, in terms of T, the period of the wave, or the time which elapses from the commencement to the termination of the motion of a particle, viz.

$$\lambda = c \mathrm{T} \sqrt{\frac{h}{h + 2k}};$$

on which equation he remarks, that setting aside the variation caused by a change in the value of k, both in that of c and in the radical, the length of the wave varies directly as T; and that we have thus an explanation of the observed fact, that a wave becomes gradually diminished in height and increased in length.

The author then enters on the subject of *Oscillating Waves.* He remarks that, in the wave of translation, it appears from the preceding investigation, that the horizontal and vertical motions of the particles commence and terminate together; and that consequently the particles left at rest when the wave has passed must continue in the same state, unless some fresh disturbance again set them in motion.

But if the vertical motion were at its greatest when the horizontal motion was destroyed, the particles would oscillate in both directions, and a disturbance once given would continue until friction or external force destroyed it. He further observes, that it has been shown by Mr. Airy (Encyc. Metrop.) that the motion of the particles in oscillating waves diminishes in a geometrical ratio as their distance below the surface increases, which result agrees with Mr. Scott Russell's observations; but that it is interesting to examine whether a series of oscillatory waves can exist in a channel of uniform breadth and depth, upon the supposition that the horizontal excursion of the particles is the same from the top to the bottom. From the analytical investigation of the author, it appears that there can exist in a channel of uniform breadth and depth a series of waves of the same magnitude and moving with the same velocity. In the case of rivers, the tide-wave appears to be of this description. If the effect of friction on the bottom be neglected, the horizontal velocity of the particles may be considered to be the same throughout the whole depth—and the motion is oscillatory, if that due to the stream be separated. But the length of the wave is very great in comparison with the depth, and the vertical motion is so slight, that it may be neglected altogether. On this supposition, which simplifies the investigation, the velocity for a wave of translation is

$$(h+2k)\sqrt{\frac{g}{h}};$$

and for an oscillating wave is $(h+k)\sqrt{\dfrac{g}{h}}$;

which differ but little from the results previously obtained, and are rather in excess, as was to be expected, when no part of the force has been expended in producing vertical motion.

Referring to the solution of this problem, given by Mr. Airy in the Encyclopædia Metropolitana, and the equation there arrived at, the author states that it is with great diffidence, and not until after the most careful examination, that he ventures to question the accuracy of this result. In a further investigation, he points out the source of this inaccuracy; and in conclusion observes, that it must be confessed to be unsatisfactory to point out an error without supplying the deficiency caused by it, but that the analytical difficulties are such, that he cannot, consistently with the attention required by other avocations, attempt, for the present at least, to continue an investigation which is interesting as a matter of science and useful in its application to hydraulic engineering.

Dec. 9.—The following papers were read.

1. " An Experimental Inquiry undertaken with the view of ascertaining whether any signs of Current Electricity are manifested in Plants during vegetation." By H. F. Baxter, Esq.

In the present communication the author has related the experimental results that he has arrived at, and which tend to show that electric currents exist in the leaves and in the roots or spongioles of plants. Becquerel and Wartmann have already proved that electric currents may be obtained in different parts of vegetables, but the object of the paper is to point out the connection of the currents in

the above-named organs with the vital or organic changes which take place in them.

In consequence of the secondary actions which occur at the electrodes some difficulty is experienced in ascertaining the true or normal result in the roots. Combining however the facts obtained by means of the galvanometer with analogical evidence, the author considers that they tend to establish the conclusion, that, during the changes which occur in the leaves and in the roots of plants, current electricity is manifested.

2. "On the relation of Cardioids to Ellipses." By Joseph Jopling, Esq.

The object of this communication is to point out the relation of Cardioids to Ellipses, and that the former as well as the latter are related to and deducible from the cone.

The author remarks that the motions of the common trammel show most beautifully the mechanical relation of ellipses and cardioids, and that they are thus reciprocals of each other; that an ellipse, as is well known, is a *plane section*, or a projection of a plane section of a cone upon any other plane, the limits being the circle and the right line; and a cardioid is also a projection from a cone; the difference being that the cardioid is obtained from a curved section, formed by the intersection of a sphere or other curved solid with a cone.

After referring to properties of the sections of cones by spheres depending on the magnitude of the vertical angles of the cone, the author states that these and many other new curves, their relations, and new properties of the cone and the sphere are made most clearly manifest, and numerous practical results are very readily obtained by the application of a double scale of sines to the rays of the cone—distributed equally on the plan—correspondingly on the elevation, and on the developed surface, or on any other projection of the cone.

He considers that it is of great importance that some method should be devised to give appropriate names to these new curves, especially those so evidently and intimately related to old ones. Thus the curved intersection of a cone and a sphere, from which the cardioid is projected on the base, and which has then the cusp turned symmetrically inwards, by another projection on a vertical plane gives a symmetrical line with the cusp turned outwards, having other distinct points of change in the curvature.

As ellipses are related to cardioids, so it is stated are hyperbolas in a similar way related to conchoids; conchoids to their mechanical reciprocals; and parabolas to cissoids; amongst the vast number of curves, any of which can conveniently be derived by scales practically from the cone.

By this method it is considered that not only projections of curved sections of cones, spheres, &c. are obtained with ease, but also by means of scales, ruled papers, hollow cones and diagrams, the plane sections of cones, and all projections from them are greatly facilitated.

In conclusion the author remarks, that if this subject can be entertained by the Royal Society, he will make copies of a series of diagrams to illustrate what he has here stated in reference to scales applied to cones to obtain cardioids, &c.

A paper was read, entitled " On the Solution of Urinary Calculi in dilute Saline Fluids, at the temperature of the body, by the aid of Electricity." By H. Bence Jones, M.D., F.R.S., Physician to St. George's Hospital.

In 1848 the author first attempted to dissolve calculi in a solution of nitrate of potash, by placing them at the same time between the electrodes of a galvanic battery; an effect was produced on the uric acid at the negative pole, but no very decided result was obtained. The experiments were resumed during the last summer.

The results with uric acid calculi may be thus arranged. The action was chiefly at the negative electrode.

Experiments.	Duration of experiment. h m	Strength and temperature of solution.		Power of Battery.		Result. grs.
1.	4	Saturated solution of nitre	212°	...	10 ...	0½ dissolved.
2.	6 5	¼ nitre, ¾ water	109	...	5 ...	11
3.	6 10	,, ,, ...	101	...	10 ...	14
4.	6 20	,, ,, ...	100	...	10 ...	16
5.	6 45	,, ,, ...	106	...	10 ...	12
6.	3 17	,, ,, ...	98	...	20 pair	27½

Oxalate of lime calculi were then tried.

1.	7	¼ nitre, ¾ water	90	...	5 ...	0½ dissolved.
2.	7	,, ,, ...	104	...	10 ...	2
3.	6 15	In sulphate of soda.........	101	...	10 ...	2
4.	5 45	In common salt	102	...	10 ...	1
5.	6 10	¼ nitre, ¾ water	108	...	20 ...	6
6.	3 19	{ ¼ nitre with phosphate of soda.................... }	110	...	20 ...	1 dissolved.
7.	3 15	{ ¼ nitre with bichromate of potash .... }	111	...	20 ...	2
8.	3 17	½ nitre	110	...	20 ...	2½
9.	2 50	¼ nitre......................	92	...	20 ...	2½
10.	3	,, ...	100	...	40 ...	5

Hence oxalate of lime calculi can be only very slowly dissolved in a solution of nitrate of potash, which acts far more energetically on uric acid calculi. The action is at least four times greater on uric acid calculi. Oxalates with urates, and oxalates with phosphates, were found to be far more rapidly acted on than oxalate of lime alone.

Phosphatic calculi were then taken. A piece of hard phosphate of lime gave

Experiments.	Duration of experiment. h m	Strength and temperature of solution.		Power of battery.		Result. grs.
1.	7¼	¼ nitre, ¾ water	102	...	10 pair	... 15
2. with fusible calculus	1 13	,, ,,	96	...	20	... 31

The action was chiefly at the positive electrode.

Experiments with marble.

1.	5½	¼ nitre	104	...	10	... 27½
2. ....,.......	6½	{ In sulphate of soda .... }	101	...	10	... 4½

From these experiments, then, the following conclusions are drawn:—From two to nine grains of uric acid calculus can be dissolved in an hour in a neutral dilute solution of nitre at the temperature of the body, whilst in the same time from two to twenty-five grains of phosphatic calculus can be dissolved. However, only from half a grain to two grains of oxalate of lime can be removed by the same

means in the same time. Still, if the stone consists of oxalate with urate, from one to two grains may be dissolved in an hour; and if it consists of oxalate with phosphate, from four and a half to five and a half grains can be taken away in that time.

These results may be obtained with calculi which have been long removed from the bladder, and have been dried at 212°.

MM. Prevost and Dumas in 1823 proposed to treat calculi by the galvanic pile. By means of the *mechanical action* of the mixed gases evolved by decomposing water, they found that phosphatic calculi might be disintegrated. The *chemical action* of the substances evolved by galvanic action on the surface of calculi was not determined by them.

Jan. 6, 1853.—A paper was read, entitled "On Molecular Influences. Sect. I. Transmission of Heat through Organic Structures." By John Tyndall, F.R.S.

In this paper the author has examined the influence exerted by the molecular structure of wood upon the passage of heat through the substance. Finding the usual modes of determining the conductivity of bodies inadequate to his purpose, he has been led to the construction of a new instrument which is capable of indicating very slight differences of transmissive power.

A cubical space is cut out of the centre of a rectangular slab of mahogany. The same slab holds a thermo-electric pair of bismuth and antimony, which are fixed in trenches cut out to receive them. The junction of the pair (which is of a V-shape) abuts from one of the faces of the cubical space just mentioned; the end of a wooden slider forms the opposite boundary of the cubical space, and against this end a platinum wire, bent several times up and down so as to form a kind of micrometer-grating, is laid and imbedded in the wood. A small projection of ivory abuts at each side of the bismuth and antimony junction, and from one projection to the other a thin membrane is drawn, thus enclosing a space in front of the junction, which is filled with mercury. Two similar projections jut at the sides of the micrometer-grating, and across from one projection to the other, a second membrane is stretched, thus enclosing another chamber in front of the wire. This chamber is also filled with mercury, and against the wire a thin plate of mica is cemented, thus preventing all contact between the two metals. From the free ends of the bismuth and antimony bars wires proceed to a delicate galvanometer.

The substances to be examined by this instrument are reduced to the cubical form and placed between the two membranes; the slider being brought closely up against the cube, the latter is clasped firmly between the rigid projections before-named. The membranes are pressed gently against the two opposite faces of the cube by the mercury behind, and thus a contact is secured which, as the mercury is not changed during an entire series of experiments, remains perfectly constant. This is a most important point in experiments of this nature, for when the conditions of contact vary in even a slight degree, comparable results are out of the question. This remark of course applies exclusively to an inquiry like the present,

where the object is to detect minute differences of molecular action. The protruding ends of the micrometer-wire are united to the poles of a small galvanic battery, and the wire is heated by the passage of the current; the heat is transmitted through the film of mica to the mass of mercury in front, which thus becomes the source of heat immediately applied to the face of the cube. The current is permitted to circulate through the bent wire for 60 seconds. During this time the heat passes from the face of the cube in contact with the source to the opposite face; the quantity transmitted to the opposite face at the end of a minute, will of course depend on the conductivity of the body in the given direction. This quantity is measured by its effect upon the galvanometer.

The temperature of the source will, of course, depend upon the amount of electricity transmitted through the bent wire, and to preserve this amount perfectly constant from day to day, a tangent galvanometer and rheostat are introduced into the voltaic circuit; a current which produces the invariable deflection of 35° is made use of to heat the wire. By this arrangement experiments which are separated from each other by long intervals of time are rendered strictly comparable.

In the manner above indicated, the author has submitted fifty-four different kinds of wood, both English and foreign, to examination. The cubes were taken so that four faces of each were parallel to the fibre, and the remaining two consequently perpendicular to it. Of the four parallel to the fibre, two opposite ones were parallel to the ligneous layers, and the other two perpendicular to them. The amount of heat transmitted in 60 seconds across the mass of each cube in these three directions, respectively, was determined in the way described, and the following law of action established:—

At all points except the centre of the tree, wood possesses three unequal axes of calorific conduction which are at right angles to each other. The first and greatest axis is parallel to the fibre; the second axis is perpendicular to the fibre, and to the annual layers of the wood; while the third and least axis is perpendicular to the fibres and parallel to the layers. It is observed that these axes of calorific conduction coincide in order of magnitude and in direction with the axes of elasticity discovered by Savart.

The author furthermore points out the existence of two other systems of axes in wood,—the axes of cohesion and the axes of fluid permeability, both of which coincide with the axes of calorific conduction.

Experiments have been made on the conductivity of various other bodies, and the non-conducting powers of the substances which enter into the composition of organic tissues is strikingly exhibited. From comparative experiments with quartz and some other substances, the author points out the influence which a mass of silica exposed to the sun's rays, as in the African deserts, must exert upon climate.

The paper concludes with experiments on some other organic structures: Tooth of Walrus, Tooth of Elephant, Whalebone, Rhinoceros's-horn, Cow's-horn; and which show how small is their trans-

missive power: that of sealing-wax, bees'-wax, sole-leather, glue, gutta-percha, India-rubber, filbert-kernel, almond-kernel, boiled ham-muscle, raw meat-muscle, appears to be unappreciable by the method described.

Nov. 12, 1852.—On a New Method of Computing the Perturbations of Planets. By M. Encke. Translated into English and illustrated with Notes by G. B. Airy, Esq., Astronomer Royal.

The main object of the author is to supply an easy and accurate method of computing the perturbations of the various small planets revolving between Mars and Jupiter, the number of which is constantly increasing from fresh discoveries. The method, however, is applicable to any of the other bodies of the planetary system.

Let t denote the time from which it is proposed to compute the action of the disturbing body. By a slight transformation the differential equations of the motion of the disturbed planet are reduced to the following form :—

$$\xi = \int\int \Big(\mathrm{X} + \frac{k^2}{r^{\circ 3}} (3\frac{x^\circ}{r^\circ} \delta r - \xi) \Big) dt^2$$

$$\eta = \int\int \Big(\mathrm{Y} + \frac{k^2}{r^{\circ 3}} (3\frac{y^\circ}{r^\circ} \delta r - \eta) \Big) dt^2$$

$$\zeta = \int\int \Big(\mathrm{Z} + \frac{k^2}{r^{\circ 3}} (3\frac{z^\circ}{r^\circ} \delta r - \zeta) \Big) dt^2,$$

x°, y°, z° being the elliptical coordinates of the disturbed planet, and r° its radius vector, as deducible from the elements of the orbit in which it was revolving at the instant t; X, Y, Z the resolved values of the disturbing force in the directions of corresponding rectangular axes; ξ, η, ζ the perturbations of x, y, z, counted from t; δr the perturbation of the radius vector; and k a constant quantity.

These equations are integrated by mechanical quadratures, the successive numerical values of the quantity under the sign of the double integral in each equation being corrected by a peculiar process, founded on the theory of finite differences.

The first step is to divide the period for which the perturbations are to be computed into a number of sufficiently small intervals of time, each equal to τ, and to calculate the values of X, Y, Z for the times $t - \frac{1}{2}\tau$, $t + \frac{1}{2}\tau$, $t + \frac{3}{2}\tau$, &c., using for this purpose the elliptical coordinates x°, y°, z° of the disturbed planet. As ξ, η, ζ, $\dfrac{d\xi}{dt}$, $\dfrac{d\eta}{dt}$, $\dfrac{d\zeta}{dt}$ are assumed severally equal to zero at the commencement of the period for which the perturbations are to be computed, the first three values of the quantities under the sign of the double integral $\Big($ or $\dfrac{d^2\xi}{dt^2}$, $\dfrac{d^2\eta}{dt^2}$, $\dfrac{d^2\zeta}{dt^2} \Big)$ are readily found with sufficient accuracy by an

approximative process, and from them the corresponding values of ξ, η, ζ are deduced. For all the subsequent values of ξ, η, ζ, the computation is uniform. The mode of procedure is this :—From the three last values of $\dfrac{d^2\xi}{dt^2}$ an approximate value of the same quantity for the following argument is readily deduced, and a provisional value of ξ is then found by means of the formula of integration $\xi = {}''\!f + \frac{1}{1^2}\dfrac{d^2\xi}{d\,t^2}$, $''\!f$ being a numerical quantity computed with the utmost facility by the aid of results already obtained. The corresponding values of η and ζ being found by a similar process, the three resulting numbers are employed in computing the second part of the expression under the integral sign in each of the fundamental equations. Finally, the numbers thus found, when applied to X, Y, Z, give the corrected values of $\dfrac{d^2\xi}{dt^2}$, $\dfrac{d^2\eta}{dt^2}$, $\dfrac{d^2\zeta}{dt^2}$, which in their turn lead by double summation to the true values of ξ, η, ζ. By a similar process the values of ξ, η, ζ for the next argument are ascertained, and so on in succession, the operation being rapidly carried on throughout the entire period for which the perturbations are to be computed.

In order to test the practical utility of this method, M. Encke compared its results with those obtained by the method of the variation of arbitrary constants. For this purpose the perturbations of Vesta by Jupiter from 1854, May 24, to 1855, October 7, were subjected to a double computation. The following table exhibits the values of the polar coordinates of the disturbed planet at the end of this period, as resulting from the use of both methods :—

	Variation of Constants.	New Method.
Longitude	335° 20′ 6″·4	335° 20′ 6″·6
Latitude	−5 40 42·3	−5 40 42·3
Log. Rad. Vector ...	0·3665032	0·3665042

M. Encke mentions as a proof of the easy application of this method, that in computing the perturbations of Vesta with given places of Jupiter for thirteen epochs, only eleven hours were occupied, notwithstanding that he was frequently interrupted in the course of the operation.

If extreme accuracy is required, the values of x, y, z, resulting from the application of ξ, η, ζ, to x°, y°, z°, may be employed in computing X, Y, Z, and then by repeating the operation we may obtain corrected values of ξ, η, ζ. This process, which gives the perturbations depending on the square of the disturbing force, may be repeated over and over again, until the assumed and resulting values of ξ, η, ζ finally agree. The method, however, is so rapidly exhaustive that this object is generally secured at the first repetition.

When the perturbations are large the method is still practicable, but it becomes necessary in this case to reduce in a corresponding degree the magnitude of the intervals of quadrature.

The following are some of the principal advantages attending this method of computation :—

1. It proceeds from the fundamental equations of motion by a

series of arithmetical operations of extreme simplicity, only a few analytical expressions of by no means a complicated character being employed in the computation.

2. It affords the means of immediately correcting the quantities employed in computing the disturbing forces for each successive interval, and thereby leads rapidly to results of great accuracy.

3. Since the perturbations of x, y, z are of the same order as those of the polar coordinates, they serve to afford unequivocal indication of the extent to which the intervals of quadrature ought to be diminished. It is well known that the method of the variation of constants may assign enormous perturbations to the elements, while at the same time the actual perturbations may be very insignificant.

The original researches of M. Encke on this subject are contained in Nos. 791, 792, and 814 of the *Astronomische Nachrichten*. It may be mentioned that a similar method had been already proposed by Mr. G. P. Bond, of Cambridge, U.S. M. Encke, however, did not become acquainted with this circumstance until he had completed his investigation.

Extract of a Letter from Professor Secchi to Mr. Grant, F.R.A.S.
"Rome, May 8, 1852.

" Allow me, dear Sir, to present you my most sincere thanks for the kind letter you addressed to me, and for the valuable present of your book, which was joined with it. I would have written to you sooner, but I waited a little while, to be able to inform you of some researches which I have undertaken on the distribution of heat at the surface of the sun, and on the constitution of the lunar geology, which I hope will be well received by you.

" As to the first subject, you know that it is generally admitted that light is brighter at the centre of the solar disc than at the edges. M. Fizeau proved that the same diminution subsisted for chemical radiation, and I obtained a confirmation of it in the Daguerreotypes of the sun taken during the last solar eclipse. But I had no proof that the fact would be the same with heat, although this was highly probable. I tried, therefore, to fill this blank, and have instituted a series of researches on the temperature of the different parts of the solar disc. I have been thus led to some interesting conclusions quite new to me ; but before exposing them, I think it necessary to say something on the method of conducting the experiments. A thermo-electric pile, belonging to a very nice Melloni's apparatus, was attached to the telescope of our equatoreal, and fixed on the prolongation of its optical axis at a distance of about 15^{cm} from the eye-piece : the image of the sun, magnified by the eye-piece itself, was thus projected on the pile, and had a diameter of 12^{cm} nearly. When a leaf of white paper was put in the place of the pile, the image was perfectly defined, and the spots neatly terminated. By a diaphragm, the aperture of which was 4^{mm} broad and 15^{mm} long, disposed parallel to the edge of the disc, a small portion of the sun's image, which was about $1'$ of arc broad and $4'$ long, was allowed to radiate on the pile.

The construction of the equatoreal enabled me to keep the same points of the disc steadily fixed on the pile, as far as it was necessary to obtain an invariable deflection of the needle of the galvanometer, and the position of the points of the sun, under examination, was readily deduced from the reading of the declination circle. There is no need to observe that all extraneous radiation was prevented by large screens, supported by the telescope itself, and the turning dome was only opened as far as it was necessary to introduce solar rays on the object-glass. The telescope has $1^m \cdot 25$ focal length, and 77^{mm} aperture.

"Now these are the results obtained in sundry experiments :— 1. The heat of the solar image is at the centre almost twice as great as at the borders. This is found to be true, examining the diameters both in right ascension and declination. 2. The maximum of temperature did not appear to be at the centre, but above it, in a point distant from it about 3′ of geocentric declination. Constructing graphically the curve of the intensity of heat, taking as *abscissæ* the parts of the sun's diameter, and as *ordinatæ* the intensities themselves, it appears that this curve (a kind of inverted parabola) is not symmetrically disposed about the axis of the ordinates, but a good deal inclined towards the upper edge. I subjoin some numbers which represent the intensity of heat in the parts of the diameter of the sun taken in minutes, + above and − below the centre of the image.

Positions on the diameter of the sun in declination	$+14'\cdot96$	$+11'\cdot32$	$+3'\cdot00$	$+1'\cdot32$	$-10'\cdot9$	$-14'\cdot88$
Relative intensity of heat	57·39	88·81	100·00	99·48	81·32	54·34

These are the results of eight series of experiments, none of which is found in contradiction with the others, and their separate numbers are very nearly the same, so that the fact seems to me completely ascertained. It is certainly curious that the maximum of heat corresponds with the position of the solar equator, as visible from the earth at the epoch of the experiment (20th, 21st, 22nd March). This leads naturally to the conclusion that the solar equatoreal regions must be hotter than the polar regions, as was suspected already from the more frequent appearance of the spots there. The conclusion seems perfectly accurate, even admitting a solar atmosphere, since the effect of this last should be to diminish symmetrically the radiation around the centre of the image; on the contrary, if the polar regions are less hot than the equatoreal, the intensity of heat should have been less in the lower part of the image, where the south pole of the sun was visible; and, consequently, the parts having equal distance from the centre of the image had a very different heliographical latitude, on account of the inclination of the solar axis to the ecliptic. From these principles only, the non-symmetry of the curve is accounted for. If *this alone* is the cause, the curve will be found symmetrical in the months of June and December, and reversed in September, since in the two former the equator passes through the centre of the

image, and in the last is below it. But it is not impossible that the two solar hemispheres should possess different temperatures, as seems to be the case on the earth, and is suspected in Mars. If this is the case, these researches will throw some light on the climatology of the earth itself; since the heat of the sun must be different, according as one or the other of its poles is turned towards the earth. Future experiments will resolve this question. With respect to the poles of the sun, I shall add here a conjecture on a fact recently discovered by Captain Sabine. The journal *Institut* relates that this gentleman has found that the deviation of the magnet from its mean position at the Cape of Good Hope is found to be in opposite directions at the epochs of the two equinoxes. Might this not be an effect of the solar magnetical polarity on the terrestrial magnetism? The fact deserves to be examined, if it takes place in our hemisphere, and in opposite directions. Coming again to the solar heat, I have found that spots seemed less hot than the rest; but as only small groups of them were visible, no singular fact or law can be stated from these observations. I shall conclude this account by noticing an odd historical coincidence, namely, that these observations were made in the same room where it is said F. Scheiner, the first who used a telescope mounted equatorially, made his observations of the sun. This room has been this year added to the observatory.

" I shall now briefly expose to you some considerations on the theory of lunar formations, which have been the subject of some discussions with our eminent geologist, Professor Ponzi, whom I gladly engaged in these researches. He is exceedingly well acquainted with the terrestrial volcanoes, both ancient and modern (being now occupied in the description of the Roman States), and with Italian geology; the conclusions to which we have arrived are these :—The lunar volcanic formations must be distributed into three classes, to which a fourth class may be added analogous to our Plutonian formations.

" The first class of the lunar volcanoes possesses a distinctive character; that the edges of the craters are almost completely obliterated, so that their border now is a continuation of the plane ground, in which they seem excavated, and a deep well only remains in the place of the ancient mouth of the volcano. Instances of this kind are very frequent near the south pole of the moon, and around the large spot Tycho; but Tycho itself does not belong to this class. The physiognomy of these craters nearly resembles our submarine volcanoes of the Monti Ciminii to the N.W. of Rome. The country around the craters of Bracciano, Bolsena and di Vico, is almost flat, and the old openings of the craters are now deep lakes. On this ground we are led to believe that even in the moon many subaqueous volcanoes existed. Another distinct character of these volcanoes of the first class is, that they are in a line, as if they burst from the cracks of the solid body of the crust produced by earlier formations; this is most striking in Arzahel, Purbach, Alphonsus, and many others, and they seem to follow the cracks made by the *soulèvement* which

raised Tycho, the lunar Apennines, &c. Some of the higher chains of lunar mountains are seen visibly parallel to the alignement of the craters : this fact also is like that which we observe on the earth ; indeed, the large Italian volcanic chain follows the line of the Apennines along this country.

" The second class of lunar volcanoes are those which have their outside edges elevated above the surrounding plane ; their form is generally regular, and not broken, as those of the preceding class, and the ground around them is elevated in a radiating disposition, as is visible around Tycho, Copernicus, Aristotle, &c. The regularity of their forms suggests that the ejected matter was not disturbed by the motion of waves, and, consequently, that they were atmospherical volcanoes, like those of the Monti Laziali, Albani, and Tusculani, at the south-east of Rome ; the want of breach in the craters seems to indicate that no lava, but only scoriæ and loose matters, have been ejected. The disposition of the soil around them suggests the opinion that they are of a comparatively later epoch, and formed after the crust of the satellite was pretty resistent, and was capable of being elevated all round by a great effort. It is singular, indeed, that this *radiation* of the soil around is found proportional to the magnitude of the central crater. The effect of this *soulèvement* extended sometimes to a prodigious distance, comparable to that of the Cordilleras of the Andes on the earth. The greater part of the craters of both the classes now described possesses an insulated rock inside, very seldom appearing (at least in commonly good telescopes) perforated. This bears great analogy with what we see in more than one place in the ancient volcanoes of the earth, where the erupting mouth has been stopped by a dome of trachytic matter as by a stump. Monte Venere, near Rome, is of this formation, and lies in the centre of an immense old crater.

" The third class of lunar craters is very small, and bears a great likeness with those called by geologists adventitious craters, and seems to be of a very late formation, the last efforts of the expiring volcanic force. They are irregularly scattered through all the moon, but occur more frequently at the borders or inside of the old demolished craters, although not concentric with them, and seem to have been produced after the large ones were completely closed, either by trachytic ejection or by becoming lakes. These small craters have very seldom rocks inside, or a flat bottom ; but their cavity is conical, and does not exceed in dimension our common volcanoes, which are yet active on the earth. From these facts and observations it appears, that volcanic action has gone on in the moon through all the same stages which it has gone and is going on in the earth, and is there probably completely extinguished, on account of the smaller mass of the moon, which has been cooled very rapidly. This rapidity of cooling, joined with the smaller gravity, may account for the great development of volcanism there, and comparatively fewer Plutonian formations. But extensive instances of this kind are not wanting ; the lunar Alps, the Apennines, the Ripheæ, &c., may represent this formation, surrounding vast basins, and

having modern volcanoes following the direction of the higher edges of their chains.   Professor Ponzi seems to think it unquestionable that water existed at the surface of the moon ; the fierce glare of the sunshine is not able to melt the ice there, which is probably at the temperature of the planetary spaces ; just as the sun at the surface of the earth is not able to melt our glaciers, which yet possess a certainly higher temperature.   Cold, and other unknown causes, may have absorbed and fixed all the atmosphere which anciently existed, as we see that the immense atmosphere which anciently surrounded the earth has been fixed by several chemical processes and reduced to its actual composition ; and it might be possible that this actually existing atmosphere of ours should be all solidified, either by cold or chemical processes, if the earth arrives at the same degree of cold which seems to have place on the moon.

" These are some of the conclusions to which we have arrived in our inquiry into the solar and lunar constitution ; they do not certainly belong to astronomy strictly taken, but a recreation from more serious occupations may be excused to astronomers dwelling in the pure climate of Italy.   I trust you will receive them as a token of my gratitude, and of interest for the work which you have published."

In a later letter Professor Secchi tells Captain Manners " that the observatory of the Collegio Romano is about to be furnished with a large refractor equatoreally mounted.   The refractor will have 9 inches aperture, and a focal length of 14 French feet." There is only a little hesitation at present as to the best mode of mounting this telescope.

Since the date of the preceding communication, Sig. Secchi has made the following interesting experiments on the heat emitted by different parts of the solar disc. It appeared from the former experiments that the heat increased from the margin of the solar disc to the centre ; but there was this peculiarity, that when it was measured in the direction of the axis of the sun's rotation, it attained its maximum intensity at a point situate about 3′ above the centre. At the time when the former experiments were made, the projection of the sun's equator upon his disc passed above his centre, at a distance agreeing almost exactly with the position of maximum temperature ; whence the conclusion naturally suggested itself to Sig. Secchi, that the equatoreal regions of the sun are intrinsically hotter than the other parts of his surface.   He justly remarked, that if this were true, it ought to follow that in the month of June, when the nodes of the solar equator pass through the earth, the upper and lower hemispheres should appear equally heated ; and on the other hand, in the month of September, when the projection of the solar equator attained its greatest depression, the point of maximum temperature should fall *below* the centre of the disc.   Experiments similar to those previously undertaken by him have served to confirm this view of the subject.   " In the month of August the upper hemisphere appeared a little hotter than the lower, but the differences,

always very small, were not unfrequently negative, In September *the lower hemisphere appeared decidedly the hotter of the two.* The maximum difference rather exceeded 2° (by the galvanometer), being almost identical with the result obtained in the month of March *in the opposite direction.* Moreover, this maximum occurred from the 14th to the 16th September, which also corresponds with the epoch of the greatest depression of the solar equator."

Sig. Secchi remarks, that the influence of the solar spots upon the temperature is very striking. Sometimes a spot which did not occupy more than $\frac{1}{100}$th the aperture of the pile, caused the temperature to fall 3°, or about $\frac{1}{15}$th of the whole intensity. He found, however, by isolating the spot with a diaphragm, that it exercises a sensible influence even in the obscure part.

XXIV. *Intelligence and Miscellaneous Articles.*

ON THE TEMPERATURES OF CONDUCTORS OF ELECTRICAL CURRENTS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I FEEL some difficulty in writing this note, and yet I think it requisite to say a closing word upon a subject which has already occupied too much of your space. It is nearly ten years since Mr. Adie made his experiments : would not a careful repetition of them at present show more wisdom upon his part than a reliance upon results which have such a mass of scientific evidence against them ? With regard to his theory of the metallic cross*, I have one request to make of Mr. Adie ; and that is, that he would kindly place his pencil on the point C, where he supposes the current to be generated, follow the course of the current, and get back to C, *without passing through the battery.* Some oversight must, I imagine, have occurred in Mr. Adie's dissection of the cross ; as the matter stands at present, I frankly confess my inability to understand it.

JOHN TYNDALL.

Queenwood, Jan. 1853.

REMARKS ON CHEMICAL AFFINITY. BY PROF. BUNSEN.

The force which produces or destroys chemical combination is influenced by a great variety of circumstances. Its effects are modified by the action of light, heat, electricity, and likewise by the relative masses of the bodies acting upon each other. It is moreover influenced by the state of aggregation of the bodies, and their contact with others of a different kind. Consequently, chemical affinity may be regarded as a function of all these various determining conditions ; and in order to ascertain the value of the force, it will be sufficient to determine mathematically the form of that function.

* Phil. Mag. for January 1853, p. 46.

The immortal Berthollet, to whom we are indebted for the doc-trine of chemical statics, was the first who considered the cause of chemical action under this point of view.   He was led theoretically to express the influence of masses in the form of a law which still retains the name he gave to it, and he endeavoured to explain by its aid the influence which the relative masses of the bodies acting upon each other exercise in determining the nature of the resulting compounds.   According to this law, when one body is brought into contact with two others of different kinds and weights, but capable of entering into chemical combination with it, the quan-tities of both being greater than would be required for such combi-nation, it is divided between the two in a proportion the same as that of their absolute affinities to their relative masses.   If, there-fore, we represent by A and B respectively, the masses of the two bodies employed in chemical excess, and by α and β the coefficients of the absolute affinities of each for the third body C, the quantities of A and B which unite with C, represented by a and b, will be to each other as $\alpha A : \beta B$.   The proportions in which A and B combine with C may then be found by means of the equation $\dfrac{\alpha}{\beta} = \dfrac{aB}{bA}$.

It appeared of great interest to study the value of this law, which is still unsupported by any decisive experiment.   The investigation has not entirely confirmed Berthollet's law, but has led to the dis-covery of another law, which appears to promise valuable results relating to the study of chemical affinity.   This law may be expressed by means of the following four propositions :—

1. When a body A is brought into contact with two or more others B, B', . . . in excess, and the whole placed in conditions most favour-able to their union, the body A takes from each of those B, B', . . . quantities which always bear a simple proportion to each other.   It follows from this, that when 1, 2, 3, 4, or more equivalents of one of these compounds are formed, 1, 2, 3, 4, or more of the other are also formed.

2. When in this way one equivalent of the compound A+B and one equivalent of the compound A+B' are formed, the mass of the body B may be increased in contact with B' up to a certain point without altering this chemical relation.   If, however, it is increased beyond a certain limit, the atomic relation, which was as 1 : 1, is sud-denly changed and becomes as 1 : 2, 1 : 3, 2 : 3, &c.   The mass of one of the bodies may then be increased again without altering the chemical relation, until having reached a second limit this relation between the bodies in contact is again changed.

3. When a body A, in acting upon an excess of the compound BC, reduces it and gives rise to the formation of the compound AB, while C is liberated and in its turn reacts upon the new compound resulting from the union of A with B, the portion of B+C which is reduced bears a simple atomic relation to that which is not reduced.

4. We may thus, in order to effect these reductions, increase the mass of one of the bodies in contact, without altering their chemical relation, up to a point at which, having reached a certain limit, it

passes suddenly into another state of equilibrium, in which, however, the bodies always remain in a simple proportion expressible in whole numbers.—*Comptes Rendus*, No. 25, December 1852.

RESEARCHES ON THE DENSITY OF SULPHUR.
BY CHARLES BRAME.

The author gives the final results of his recent labours in the following terms:—

1. The specific gravities of the different varieties of sulphur, or of its different molecular conditions hitherto adopted, do not agree together, not merely for the different forms, but likewise for each one in particular, and the limits of these discrepancies extend beyond those of any possible errors of observation. The differences are caused as much by foreign bodies present in the sulphur employed, as by the various circumstances under which any particular molecular form of sulphur had been obtained, or by the influence of physical agents upon it. The limits of variation appear to be comprised between 1·87 or 1·9319, the lowest density of the viscous sulphur, and 2·0757, the highest density which has been observed in native crystallized sulphur. However, it appears that the maximum density of sulphur may rise as high as 2·08 or 2·09, and perhaps even to 2·1.

2. The increase in the density of sulphur furnishes evidence of a persistent molecular motion in a body apparently solid. This motion is more or less slow or rapid, and proves that frequently the molecules of a body are only in a state of apparent rest.

3. The slight increase in the specific gravity does not always determine the perfect condensation of the material substance, if, as is perhaps the case, we do not know in which kind of sulphur the molecules are in a state of static equilibrium. It is however probable that the native crystallized sulphur and the old hard sulphur approximate most closely to this condition; the crystals formed by fusion come equally near, although they maintain a density a little lower than that of the preceding kinds.

4. The octohedral crystalline state does not appear to be the one towards which all the forms of sulphur always and necessarily tend. All artificially prepared sulphur consists of a mixture of crystalline sulphur and membranous sulphur, as has been detected by M. Ch. Deville and by myself*.

5. Every kind of sulphur in different molecular conditions really tends towards that form which is compact, amorphous or crystalline, transparent or opake. The utricular form and state of sulphur are the necessary consequence of the facts stated in this memoir and the conclusions of Scheerer, Marchand, Ch. Deville and myself, which refer to a correlation of the passage from one molecular condition to another, with an alteration in the specific heat, the crystalline form and the specific gravity. There is at the same time a

* In accordance with this fact, I believe that I shall be able to trace an analogy between several molecular forms of sulphur and those of glass, &c.

change in the volatility, the melting-point, divisibility, and the chemical properties, as I showed some years since.

6. The utricular condition may therefore continue for a considerable time under the other apparent forms of this body. Further, we may recognize in this circumstance the cause of the diversities of the physical and chemical properties which the different kinds of sulphur present.

7. The density of sulphur is one of the principal, variable, physical properties which are directly correlative with the persistence of the utricular form among the other apparent forms of sulphur.—*Comptes Rendus*, No. 21, November 1852, p. 748.

ON THE OCCURRENCE OF GOLD IN PENNSYLVANIA.
BY CHARLES M. WETHERILL, PH.D.

In the spring of 1851, an earth was given to me for examination, of which the locality was not exactly stated but which was said to have been taken not far from the city, in which gold was detected. The earth was said to have been obtained in digging a well. Several months later, while in Reading, I met with a notice in a German newspaper of that place, which stated that some time previously an earth had been found in digging a well, upon the land of Mr. Yoder, Franconia Township, Montgomery county, which proved, upon examination, to contain gold. I have no doubt but that this is the locality of the earth which I examined. Several rocks from the neighbourhood were analyzed, consisting of clay slate rock, ferruginous quartz, decayed in places, containing pyrites and magnetic oxide of iron-sand. In most of these gold was detected in traces. Some specimens contained no gold whatever. The earth from the well, which was more particularly examined, consisted of sand and gravel, coating in some places fragments of shale or other rock. A careful examination of these with the lens detected a rather thick spangle of gold adhering to the gravel, and a small rounded mass of a white malleable metal, which proved by a micro-chemical investigation upon half of it to be native tin, which occurs only, according to Dana, in small grayish-white metallic grains along with Siberian gold. It melted before the blowpipe, was oxidized by nitric acid, the resulting oxide being insoluble in tartaric acid, and dissolved slowly in HCl, with which solution HS gave the yellowish-brown precipitate $SnS + SnS^2$. This occurrence of native tin is strongly opposed to the supposition of fraud in the earth examined. Separating the rock and washing, gave a further quantity of very fine gold spangles, mingled with pyrites and magnetic oxide of iron, together with more spangles of native tin. One pound and a half of the original substance, from which these spangles were removed, after separation of the rocks and concentration by washing, was melted with twice its weight of litharge (previously tested for gold), and a small quantity of charcoal powder. The resulting button of lead was cupelled (adding to the lead the gold already found) and the silver treated with nitric acid, which left a coherent mass of gold weighing 0·006 grm. One hundred pounds of the

earth would therefore contain 0·4 grm. of gold, worth about twenty-six and a half cents.

During a stay at Reading, in the summer of 1851, I noticed a vein of decayed ferruginous quartz, very much resembling the auriferous quartz of North Carolina. It was uncovered in exploring the deposits of iron ore in Penn's Mount behind the city. I neglected at the time to secure specimens; and upon a second visit to the locality this spring to obtain a quantity for analysis, I found it covered. I obtained, however, from the vicinity a quartz rock, quartz and felspar mingled, and sand, which on analysis yielded an exceedingly minute quantity of a brownish powder after treating the silver button resulting from cupellation by nitric acid; but which was too minute from which to derive any definite conclusion as to the presence or absence of gold. A former pupil of mine, in an examination of the pyrites of the same locality, thought to have detected traces of gold. I have no doubt that a more careful examination of the rocks in the vicinity would yield affirmative results in an examination for this metal.—*From the Transactions of the American Philosophical Society.*

METEOROLOGICAL OBSERVATIONS FOR DEC. 1852.

Chiswick.—December 1. Overcast: rain. 2. Overcast. 3. Fine. 4, 5. Densely clouded. 6. Overcast. 7. Rain. 8. Foggy: rain: clear at night. 9. Clear and fine. 10. Low clouds: rain: densely overcast. 11. Exceedingly fine. 12. Fine: overcast: rain. 13. Rain: uniformly overcast. 14. Cloudy: clear at night: rain. 15. Rain: cloudy: clear. 16. Clear: fine, with sun: thunder, lightning and heavy rain at night. 17. Partially overcast: fine: boisterous, with lightning at night. 18. Clear and fine: extensive and sudden rise of barometer. 19. Slight rain: overcast. 20. Fine: densely clouded: clear at night. 21. Exceedingly fine. 22. Overcast: rain. 23. Overcast. 24. Rain: drizzly: overcast. 25. Very fine: overcast. 26. Fine: very fine: boisterous, at times quite a hurricane at night. 27. Very boisterous: large white clouds: clear at night. 28. Clear and fine. 29. Fine: overcast. 30. Overcast: fine: clear. 31. Very fine.

Mean temperature of the month .... 46°·54
Mean temperature of Dec. 1851 .... 38 ·88
Mean temperature of Dec. for the last twenty-six years ... 39 ·69
Average amount of rain in Dec. .... 1·52 inches.

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

Sandwick Manse, Orkney.—Dec. 1. Showers A.M. and P.M. 2. Showers A.M.: showers, aurora P.M. 3. Bright A.M.: cloudy, aurora P.M. 4. Damp A.M.: cloudy P.M. 5. Showers A.M.: showers, aurora P.M. 6. Rain A.M.: showers, aurora P.M. 7. Bright A.M.: clear, aurora P.M. 8. Cloudy A.M. and P.M. 9. Cloudy A.M.: clear P.M. 10. Rain A.M.: showers, aurora P.M. 11. Showers A.M.: rain P.M. 12. Drizzle A.M.: cloudy P.M. 13. Clear, frost A.M.: clear, aurora P.M. 14. Bright, frost A.M.: drizzle P.M. 15. Damp A.M.: clear P.M. 16. Clear, frost A.M.: drizzle P.M. 17. Sleet-showers A.M. and P.M. 18. Cloudy, frost A.M. and P.M. 19. Lightning, rain A.M.: cloudy P.M. 20. Cloudy A.M. and P.M. 21. Cloudy A.M.: snow-showers P.M. 22. Clear, frost A.M.: cloudy P.M. 23. Clear A.M. and P.M. 24. Cloudy A.M.: showers P.M. 25. Cloudy A.M.: sleet-showers P.M. 26. Bright A.M.: hazy P.M. 27. Cloudy A.M.: drizzle P.M. 28. Showers A.M.: fine P.M. 29. Cloudy A.M.: shower P.M. 30. Drops A.M.: shower P.M. 31.· Showers A.M.: showers, thunder and lightning P.M.

Mean temperature of Dec. for twenty-five years .... 41°·20
Mean temperature of this month .... 40 ·74
Average quantity of rain in Dec. for six previous years ...... 3 86 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. 1852. Dec.	Barometer Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Orkney Sandwick 9¼ a.m.	Orkney Sandwick 8¼ p.m.	Thermometer Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Orkney Sandwick 9¼ a.m.	Orkney Sandwick 8¼ p.m.	Wind Chiswick 1 p.m.	Boston	Orkney Sandwick	Rain Chiswick	Boston	Orkney Sandwick
1.	29·960	29·897	29·58	29·32	29·43	48	39	32	46	43	sw.	ssw.	ssw.	·05		·44
2.	29·936	29·838	29·44	29·40	29·48	48	35	38	42	42½	sw.	sw.	w.		·06	·43
3.	30·028	29·992	29·60	29·75	29·82	51	38	37	39	37	sw.	w.	sw.			·33
4. ☾	29·904	29·884	29·40	29·48	29·42	55	52	49	40¼	45	sw.	ssw.	ssw.		·04	·02
5.	29·897	29·827	29·37	29·51	29·56	55	49	52	45	46	sw.	wsw.	w.	·25		·16
6.	29·778	29·771	29·30	29·34	29·35	52	41	48	42½	44	sw.	w.	ssw.		·25	·25
7.	29·691	29·435	29·28	29·43	29·45	50	44	44	42½	41	s.	s.	se.	·19		·21
8.	29·298	29·241	28·94	29·48	29·35	49	37	45·5	43	44	sw.	s.	e.	·08	·20	
9.	29·613	29·525	29·10	29·42	29·48	53	45	40·5	41	40	sw.	w.	e.	·01	·05	·10
10.	29·583	29·537	29·10	28·98	28·96	54	49	48	46	45½	s.	s.	sw.	·03	·07	·30
11. ●	29·595	29·519	29·18	29·00	29·06	57	48	47	45	41	s.	s.	ne.	·20		·01
12.	29·505	29·429	29·05	29·28	29·33	54	40	47	39	37	s.	s.	n.	·12		·21
13.	29·473	29·383	28·93	29·52	29·51	54	43	44	36	34	s.	s.	calm	·25	·08	·01
14.	29·364	29·335	28·58	29·27	29·15	55	42	46	36	39	sw.	s.	nne.	·03		·07
15.	29·106	28·983	28·98	29·07	29·14	52	37	47	39	38	sw.	s.	nne.	·27	·16	·19
16.	29·412	29·151	28·63	29·24	29·17	52	45	39·5	36½	36½	sw.	w.	nne.	·01		·10
17.	29·259	29·146		29·11	29·58	51	34	45	35½	37	sw.	w.	n.		·12	·04
18. ☽	30·208	30·026	29·68	30·07	30·08	49	31	36	36	36	ne.	nnw.	ese.	·01		·05
19.	30·119	30·007	29·61	29·44	29·36	52	43	46	42	46½	s.	ssw.	w.		·05	·47
20.	29·999	29·771	29·35	29·36	29·68	56	37	49	43	41½	sw.	ssw.	nw.			
21.	30·053	29·925	29·58	29·85	29·90	51	34	46	36	34½	sw.	wsw.	nw.	·04		
22.	29·794	29·738	29·42	29·85	29·80	49	38	42	36	35	sw.	ese.	sse.	·03	·07	·03
23.	29·788	29·724	29·42	29·64	29·50	50	37	36·5	38	40	se.	se.	sse.	·09	·08	
24.	29·788	29·735	29·30	29·06	29·16	56	48	40	41½	42	sw.	sw.	sw.			·04
25. ○	29·733	29·659	29·25	29·00	29·16	52	41	46	40	40	w.	sw.	wsw.		·04	·20
26.	29·703	29·351	29·30	29·14	29·06	55	45	41	34	38	sw.	sw.	sse.	·29		·12
27.	29·729	29·032	28·50	28·00	28·38	54	36	52	44	44	sw.	wsw.	w.			
28.	29·789	29·525	29·16	28·78	29·15	54	26	35·5	44	44	sw.	wsw.	nw.		·16	·43
29.	29·700	29·654	29·43	29·46	28·92	54	47	35	43	41	sw.	s.	se.	·02		·41
30.	30·046	29·784	29·38	28·87	29·29	52	38	47	37	45½	sw.	sw.	w.		·02	·28
31.	30·040	30·003	29·70	29·40	29·24	50	44	37	44	46	s.	sw.	ssw.			·10
Mean.	29·738	29·607	29·24	29·307	29·361	52·38	40·71	43·1	40·66	40·82				1·97	1·45	5·04

XXV. *New Experiments on the Mean Density of the Earth.*
By F. REICH[*].

I OUGHT perhaps to apologise for returning to a subject which has been already submitted to so fundamental an examination, that it may seem superfluous to enter upon its further consideration. From observations made by me, and published in a short paper in 1838, the mean density of the earth was found to be almost exactly what Cavendish found it, viz. 5·45. According to a far more elaborate investigation by Baily it amounts to 5·66. Baily's experiments have been conducted with so much care, and are so very numerous, that the value of mine in comparison with them is evanescent. These, however, agree so well among themselves, as to render it probable that the difference between us is to be referred to the deficiency in point of number of my observations; and Baily himself has been kind enough to bestow considerable attention upon this difference, without, however, being able to ascertain its cause. The conjecture expressed by him, that I perhaps have omitted some less coincident observations, I must state to be not correct.

It may be easily conceived that I also have occupied myself in endeavouring to render an account of this difference; and I believe that I have discovered its origin to be attributable to two circumstances.

The first is the treatment of the results obtained immediately from observation, which treatment has been more correctly conducted by Baily than by me; this will be rendered most evident by an example. When, for instance, four observations were made,

the first and third with the mass in one position, and the second and fourth with the mass in the other position, I compared the mean of the former two with the mean of the latter; whereas Baily drew his result from the first and third compared with the second, and from the second and fourth compared with the third. If, when the mass is in a certain position, the arm of the torsion-balance always came to rest at the same place, both modes of calculation would lead to the same results; but the position of equilibrium perpetually changes, and, as a rule, towards the same side during a series of experiments; hence Baily's mode of observation is the most correct; and if the change in the position of equilibrium were proportional to the time, which is not exactly the case, it would furnish results totally free from error.   In the memoir, therefore, my former observations have been recalculated, and a final result of 5·49 obtained.

Another circumstance appears, on the other hand, to render it probable that the final result of Baily is somewhat too high.   In Table V. of his work the single series of observations are arranged according to the weight of the spheres made use of. Taking the arithmetic mean for each sphere, a more exact calculation with reference to the values of the single series being unnecessary for our purpose, the mean density of the earth is found to be—

<blockquote>
With the 2½-inch lead sphere . . . . 5·595
With the 2-inch lead sphere . . . . 5·634
With the 2½-inch hollow copper sphere . 5·658
With the 1½-inch platinum sphere . . 5·627
With the 2-inch zinc sphere . . . . 5·666
With the 2-inch glass sphere . . . . 5·768
With the 2-inch ivory sphere . . . . 5·775
With the copper arm alone . . . . 6·024
</blockquote>

We observe here that the result increases as the weight of the sphere diminishes, the sphere of platinum forming only a trifling exception to the general rule.   It is therefore probable that some correction has been inaccurately determined by Baily, which operates most sensibly in the case of the lighter spheres, leaving, however, a small error, on the same side, attached to the heaviest also.

The apparatus made use of by me was placed in a closed cellar; to preserve the wood-work from decay, it was removed to a room on the second floor, originally with the intention of preserving, but not of making use of it in experiments, as I found myself unable to take such anxious precautions against changes of temperature and other disturbances as Baily has taken.   But by availing myself of an artifice described by the latter, and first

proposed by Professor Forbes, I have been able in this new locality to make observations quite as regularly as before. The artifice consists in covering the case surrounding the torsion-balance within and without with a metallic coating, in my case with tin-foil. The advantage of such a metallic covering in observations with very mobile apparatus has been proved in other cases also (by MM. Regnault and Kohlrausch); it may perhaps be due to the lessening of the sensitiveness of the apparatus with regard to differences of temperature; or, which seems to me most probable, to the removal of every trace of electrical development from the apparatus.

Another modification of the apparatus resulted from the following out of my original idea to make all corrections as small as possible, or rather to do away with them altogether; so that, as far as practicable, we should have to deal with the simple attraction between the mass and the sphere alone. For this reason the thick wire which originally carried the mass, and the attraction between which and the sphere and arm was by no means inconsiderable, was omitted; instead of it the mass was laid upon a kind of rotating disc which surrounded the sphere and its case, so that when the position of the mass was altered, the attraction of no other body for the sphere suffered any change. In this case, however, experiments could no longer be made by observing the position of equilibrium of the arm at various distances between the mass and sphere; but the same procedure as that followed by Cavendish and Baily had to be adopted, the mass being suffered to attract the sphere, first from one side and then from the other.

With the same spheres as were formerly made use of, which were composed of tin with 10 per cent. of bismuth and a little lead, and weighed 484·19 grammes, and also with the same masses of lead which weighed 45031 grammes, three series of experiments were executed. I will here state the single results, and refer to the memoir itself for the more special indications of the experiments.

The first series was executed with a copper wire 0·5 millimetre thick and 2270 millimetres long, and gave—

5·5948	5·5933	5·4715*
5·4390	5·6216	5·2067*
5·7114	5·5470	5·7452
5·4406	5·5177	5·5737
5·5270	5·6880	5·6211
5·5587	5·6046	5·5334
5·3773	5·6149	5·6423
5·5237	5·5681	5·5237

Mean . . . 5·5519
Probable error 0·0152

M 2

The original numbers themselves suggest the idea that the two results marked thus * must be erroneous; omitting these from the calculation, we obtain as

Mean 5·5712 with a
Probable error of 0·0113.

The second series was executed with a copper wire 0·4 of a millimetre thick and 620 millimetres long. The first wire was too stiff to be stretched quite straight by the weight of the torsion-balance attached to it, and it was a natural conjecture, that the considerable alterations in the force of torsion, which pronounced themselves both by differences in the magnitude of the deflections and by differences in the times of vibration, might be due to the imperfect tension of the wire; this source of error being excluded by making use of a thinner wire, a better coincidence among the single results might be expected. The thinner wire must, however, be taken shorter than the former one, in order not to make the duration of a vibration inconveniently great. This hope was not fulfilled; for in the first series the time of a double vibration varied from 688·6 to 722·0 seconds, and in the second series from 497·0 to 512·6 seconds; the former of which corresponds to a change of the force of torsion in the ratio of 1 : 1·099, and the latter to a change of the same force in the ratio of 1 : 1·064; the results obtained in the second series are also somewhat more discrepant than those of the first series, for we found—

5·5953	5·7574	5·5770
5·7860	5·7442	5·5793
5·3127	5·6176	5·9935
5·5767	5·4817	5·6369
5·5471	5·5847	5·4581
5·5245	5·5157	5·6910
5·5734	5·7812	5·6806
5·5772	5·6016	5·6214

Mean . . . 5·6173
Probable error 0·0181

The third series was executed with a bifilar iron wire; the wires being 4·2 millimetres apart below, and 5 millimetres apart above, and still 2270 millimetres in length, and I hoped thereby to obtain more coincident results. This expectation seemed to be justified by the fact, that after one or two days the wire assumed an almost constant position, whereas a unifilar wire requires several months before the tendency to turn to the one side or the other is annulled. In this respect, therefore, the bifilar wire is decidedly to be preferred. But unfortunately the differences in the force of torsion were even more considerable

than before; the single observed times of vibration varied from 770·3 to 813·2 seconds, and hence the force of torsion in the ratio of 1 : 1·114.

This variability in the force of torsion of a bifilar or unifilar wire, which exhibits itself when the weight which imparts tension to the wire remains the same, diminishes in time, is independent of temperature, and which, it may be remarked, exhibited itself in Baily's experiments in a still greater degree, is well deserving of attention; for future observations it would be of interest to ascertain its cause, and whether it is not possible completely to annul it.

The bifilar wire gave for the mean density of the earth,—

5·3468	5·4957	5·5080
5·4487	5·7034	5·6469
5·7235	5·7936	5·6304
2·5102	5·7326	5·4411
5·5539	5·5248	5·3913
5·7192	5·7639	5·5886
5·7233	5·4751	5·7647
5·6360	5·5333	5·5282

Mean . . . 5·5910
Probable error 0·0169

With the bifilar wire, therefore, we have not succeeded in obtaining more coincident results.

From the three series we obtain as principal mean—

$$5·5832,$$

with a probable error of 0·0149.

A doubt very naturally suggests itself here, whether feeble magnetic or diamagnetic actions between the mass and the sphere might not interfere with the correctness of the results, without, however, subscribing to the view developed by Mr. G. Whitehead Hearn (Philosophical Transactions, 1847, part 2, p. 122).

To obtain certainty upon this point, I made a number of experiments with a diamagnetic sphere of bismuth and a magnetic sphere of iron.

The bismuth sphere gave—

5·6728	5·3939	5·7153
5·6853	5·7876	5·5063
5·1436	5·5032	5·3551
5·5033		

Mean 5·5266
with a probable error of 0·0402.

The iron sphere gave—

5·6269	5·8482	5·9636
5·7234	5·8071	5·3563
5·5645	5·6455	5·8719
5·6098	5·7025	5·5453

Mean 5·6887

with a probable error of 0·0312.

The smaller result obtained with the bismuth sphere can decide nothing in the present uncertainty; the result with the much more strongly magnetic iron sphere is, on the contrary, divergent enough to render it probable that a disturbing influence was exercised here; it may be conjectured that the magnetism of the iron sphere acted repulsively upon the diamagnetic mass of lead; hence, diminished the deflection of the arm due to gravitation, and gave too great a value.

It is at all events evident from this, that, in the principal experiments, where the sphere made use of was much more feebly diamagnetic than the sphere of bismuth, no sensible action of the kind spoken of could have been exerted.

Cavendish himself found, and the experiments of Baily and myself completely corroborate his observation, that it is not practicable to determine the time of oscillation of the torsion-balance, once for all, and then simply to ascertain the deflection caused by the approximated mass, inasmuch as on account of the changeableness of the force of torsion of the suspending wire spoken of above, correct results can only be obtained when the time of oscillation and the deflection are determined simultaneously. This is also the reason why experiments made by me at the suggestion of Professor Forbes, and which consisted in deducing the mean density of the earth from the observed times of vibration, first, when the attraction of the mass was permitted, as heretofore, to act at right angles to the direction of the arm, and secondly, parallel to this direction, led to no useful result. In the present arrangement of my apparatus, this might be readily accomplished by placing two masses of lead diametrically opposite to each other; these, however, being so situate, that in one instance the line joining their centres should be normal to the direction of the arm, and afterwards parallel to this direction. In this way the arm of the torsion-balance would not be at all deflected; but its time of oscillation would in the former case be increased, and in the latter diminished, and from the ratio of both it would be easy to deduce the result sought for.   With

reference to this point, and to certain unsuccessful experiments of the nature just described, additional information is contained in the memoir. If a sufficiently delicate torsion-balance of perfectly constant vibration-time could be constructed, the solution of the problem, in the way last mentioned, would not be without interest.

Finally, I have made use of the torsion-balance in diamagnetic experiments. As these, however, have been already partially published, and, as regards the different repulsion of rock crystal and calcareous spar in the direction of the axis and perpendicular to the latter, have been more successfully executed by Tyndall*, I refrain from mentioning them further here.

XXVI. *On a New Series of Organic Bodies containing Metals. By* Dr. E. Frankland, *F.C.S., Professor of Chemistry, Owen's College, Manchester†.*

UNDER the above title I described, more than three years ago, some preliminary experiments ‡ which proved the existence of certain organic compounds highly analogous to cacodyle, and, like that body, consisting of a metal, or in some cases phosphorus, associated with the groups $C^2 H^3$, $C^4 H^5$, $C^6 H^7$, &c., and possessing in most instances, highly remarkable powers of combination. I fixed the composition and studied some of the reactions of two of these bodies, to which the names Zincmethyle ($C^2 H^3 Zn$) and Zincæthyle ($C^4 H^5 Zn$) were provisionally assigned, besides giving methods for procuring similar compounds containing tin, arsenic and phosphorus, by acting upon the iodides of the alcohol radicals with these elements, and expressing a belief, founded upon the similarity of functions existing between hydrogen and the groups of the form $C^n H^{(n+1)}$, that most, if not the whole, of the compounds contained in the following series might be formed; those marked thus * being at that time already known.

Hydrogen series.	Methyle series.	Æthyle series.	Butyle series.	Valyle series.	Amyle series.	Phenyle series.
$Zn\ H$	$Zn\ C^2 H^3$*	$Zn\ C^4 H^5$*	$Zn\ C^6 H^7$	$Zn\ C^8 H^9$	$Zn\ C^{10} H^{11}$	$Zn\ C^{12} H^5$
$As\ H^2$*	$As(C^2 H^3)^2$*	$As(C^4 H^5)^2$	$As(C^6 H^7)^2$	$As(C^8 H^9)^2$	$As(C^{10} H^{11})^2$	$As(C^{12} H^5)^2$
$Sb\ H^3$*	$Sb(C^2 H^3)^3$	$Sb(C^4 H^5)^3$	$Sb(C^6 H^7)^3$	$Sb(C^8 H^9)^3$	$Sb(C^{10} H^{11})^3$	$Sb(C^{12} H^5)^3$
$P\ H^3$*	$P\ (C^2 H^3)^3$*	$P\ (C^4 H^5)^3$	$P\ (C^6 H^7)^3$	$P\ (C^8 H^9)^3$	$P\ (C^{10} H^{11})^3$	$P\ (C^{12} H^5)^3$

More recently Löwig and Schweitzer* have commenced labouring in the same field, and have filled up one of the gaps in the foregoing table by the formation of stibæthyle, $(Sb(C^4 H^5),)$ in acting upon iodide of æthyle with an alloy of antimony and potassium ; the same chemists state also the probable formation of similar compounds containing methyle and amyle in place of æthyle, and bismuth and phosphorus instead of antimony.

I have continued my researches upon the organo-metallic bodies formed as above described, and having succeeded in increasing the list by the addition of several new members, I propose, in a series of papers, of which this is the first, to lay before the Royal Society the results of my experiments on the formation of bodies of this class.

The agents which I have hitherto employed in the formation of these organo-metallic compounds are two, viz. heat and light ; in many cases either of these can be used, in others only one can be made to effect the desired combination, whilst more rarely the assistance of both appears to be essential.   In those experiments in which heat was employed the materials were subjected to its action in sealed glass tubes, about 12 inches long, and varying in diameter from half an inch to 1 inch, the thickness of the glass being about one-eighth of an inch†.   To preserve the gaseous products of the operation in a state of perfect purity for subsequent investigation, the tubes were well exhausted before being sealed ; they were then immersed to about half their depth in an oil-bath, and heated to the required temperature.   In cases where the influence of light was employed, the materials, confined in tubes of precisely similar dimensions, were exposed to the sun's rays, concentrated in most cases by an 18-inch parabolic reflector, near the focus of which the tubes were placed, either naked or surrounded by a solution of sulphate of copper to absorb the calorific rays.   By this arrangement the light and heat could be increased, diminished, or modified at pleasure, which was found very convenient in several of the operations.

Action of Tin upon Iodide of Æthyle.

When iodide of æthyle and metallic tin are exposed to the action of either heat or light, the tin gradually dissolves in the æthereal liquid, which finally solidifies to a mass of nearly colourless crystals.   This reaction is effected most conveniently by the action of light, an excess of tinfoil, cut into narrow slips,

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* *Annalen der Chemie und Pharmacie*, vol. lxxv. p. 315.

† A minute account of the construction and use of these tubes is given in the Journal of the Chemical Society, vol. ii. p. 265.

being employed: the sealed tubes containing these ingredients should be placed near the focus of a large parabolic reflector, the temperature being prevented, if necessary, from rising too high by immersing them in water or in a solution of sulphate of copper. The unconcentrated rays of the sun, or even diffused daylight, are quite sufficient to determine the formation of the crystalline body; but an exposure of several weeks, or even months, would be necessary for the completion of the change, which is effected by the use of the reflector in a few days of bright sunshine. The liquid gradually assumes a straw-yellow colour, but its solidification is prevented as long as possible at the end of the operation, by allowing the temperature to rise 20° or 30° C. above that of the atmosphere; thus nearly the whole of the iodide of æthyle becomes united with tin. When heat instead of light is employed to effect the combination, the tubes should not be more than half an inch in diameter; and to avoid the risk of explosion, should not be more than one-fourth filled with the materials: the combination takes place at about 180° C. The agency of heat is therefore much less convenient than that of light in the production of this reaction, which is also never so complete as when the latter agent is employed; I have satisfied myself, however, that the results are the same in both cases.

Examination of solid products.—The capillary extremities of the tubes in which the iodide of æthyle had been exposed to the action of tin, were broken off under sulphuretted water and beneath a jar filled with the same liquid*; the gases evolved were preserved for eudiometrical investigation. The crystalline product of the reaction was then withdrawn from the tubes, and after being exposed to a gentle heat for a few minutes to expel the iodide of æthyle that had escaped combination, was treated with alcohol, in which the crystals readily dissolved, leaving only a small residue of a bright red colour, which proved to be proto-iodide of tin. The filtered alcoholic solution was then placed over sulphuric acid *in vacuo*, where it soon deposited a large crop of long needle-like crystals, which, when freed from the mother-liquor, washed with a small quantity of dilute alcohol, dried between folds of bibulous paper, and finally over sulphuric acid *in vacuo*, yielded the following analytical results:—

I. 1·6806 grm., treated with aqueous solution of ammonia, was immediately decomposed, iodide of ammonium being formed, whilst the iodine in the original compound became, as I shall show below, replaced by oxygen; this oxide, which is almost absolutely insoluble in ammonia, collected on a filter and dried at 100° C., weighed ·7263 grm.; decomposed by boiling nitric acid it gave ·5811 grm. peroxide of tin. The ammoniacal solu-

* Journal of Chemical Society, vol. ii. p. 267.

tion, acidulated with nitric acid and precipitated by nitrate of silver, yielded 1·8418 grm. iodide of silver.   After precipitation of the excess of nitrate of silver by hydrochloric acid, sulphuretted hydrogen was passed through the solution, and the slight precipitate formed was washed, dried, ignited, and added to the above peroxide of tin, in the weight of which it is included.

II. 1·4254 grm., burnt with oxide of copper, 2 inches of metallic copper being placed in front of the combustion-tube, gave ·5858 grm. carbonic acid and ·2975 grm. water.

III. 1·2209 grm. gave 0·5008 grm. carbonic acid and ·2580 grm. water.

IV. 2·0980 grms., treated as described in No. I., yielded ·9218 grm. of the body produced by the action of solution of ammonia, which yielded ·7239 grm. peroxide of tin.   The ammoniacal solution, precipitated by nitrate of silver as in No. I., produced 2·2883 grms. iodide of silver.

V. ·9113 grm. gave ·3735 grm. carbonic acid and ·1908 grm. water*.

These numbers show that the crystalline body is a compound of one atom of æthyle, one atom of tin, and one atom of iodine. The formula $C^4 H^5 Sn I$ requires the following values :—

		Calculated.		Found.				
				I.	II.	III.	IV.	V.
C^4	.	24	11·18	...	11·21	11·19	...	11·18
H^5	.	5	2·33	...	2·32	2·35	...	2·33
Sn	.	58·82	27·40	27·18	...	...	27·12	
I	.	126·84	59·09	59·21	...	...	58·76	
			100·00					

For reasons described below, I propose to call this compound *iodide of stanæthylium.*

Iodide of stanæthylium crystallizes in transparent, slightly straw-coloured needles, which are right rectangular prisms, frequently one-twelfth of an inch broad and 2 or 3 inches in length. They are very soluble in æther and in boiling alcohol; less so in cold alcohol and in water; the watery solution is decomposed on boiling, oxide of stanæthylium being precipitated and hydriodic acid formed.   Iodide of stanæthylium fuses at 42° C., and boils at 240° C., undergoing at the same time partial decomposition : it possesses, at common temperatures, a peculiar pungent odour, somewhat resembling the volatile oil of mustard, and which irritates the eyes and lining membrane of the nose, causing a discharge which continues for several hours or even days, espe-

* The substance used in Nos. IV. and V. was produced by the action of light, that used in the other analyses by the agency of heat.

cially if the vapour from the heated iodide of stanæthylium be inhaled; yet this compound can scarcely be said to be volatile at common temperatures, since a few grains may be exposed to the air for several weeks without any appreciable loss of weight.

Oxide of Stanæthylium.—In contact with solutions of the alkalies, iodide of stanæthylium is immediately decomposed, oxide of stanæthylium and an alkaline iodide being formed; with solutions of potash and soda the oxide of stanæthylium dissolves in an excess of the precipitant, but is reprecipitated, unchanged, by cautious neutralization of the alkaline solution; with solution of ammonia the precipitated oxide remains undissolved on the addition of an excess of the alkali; a quantity of the oxide of stanæthylium, prepared in this latter manner, was heated for a few minutes with an excess of ammonia, thrown on a filter and washed with distilled water until all iodide of ammonium was removed. Submitted to analysis it yielded the following results:—

I. ·3497 grm., burnt with oxide of copper, gave ·3218 grm. carbonic acid and ·1630 grm. water.

II. ·7296 grm., decomposed by nitric acid and ignited, gave ·5778 grm. peroxide of tin.

III. ·9218 grm. gave, when similarly treated, ·7239 grm. peroxide of tin.

These numbers agree closely with the formula $C^4 H^5 SnO$, as is seen by the following comparison:—

				Calculated.		Found.		
						I.	II.	III.
C^4	.	.	:	24	25·05	25·09	...	...
H^5	.	.	.	5	5·22	5·18	...	...
Sn	.	.	.	58·82	61·39	...	62·25	61·73
O	.	.	.	8	8·34	...	...	...
					100·00			

Analyses Nos. I. and IV. of iodide of stanæthylium also clearly show the transformation of the iodide into the oxide of stanæthylium by ammonia; in analysis No. I. 1·6806 grm. of iodide of stanæthylium gave ·7263 grm. of oxide of stanæthylium, and in analysis No. IV. 2·0980 grms. of the iodide yielded ·9218 grm. of the oxide of stanæthylium. Hence

	Calculated.	Found.	
		I.	II.
100 parts of iodide of stanæthylium } yield of oxide of stanæthylium	44·64	43·22	43·93

The numbers obtained by experiment correspond sufficiently

well with the theoretical one, when it is considered that oxide of stanæthylium is not absolutely insoluble in excess of ammonia.

Oxide of stanæthylium presents the appearance of a somewhat cream-white amorphous powder, closely resembling peroxide of tin, but less heavy than that oxide; it has a peculiar though slight æthereal odour and a bitter taste; it is insoluble in water, alcohol and æther, but readily dissolves in solutions of acids and of the fixed alkalies; with acids it forms salts, which are, however, for the most part difficultly crystallizable; those with strong acids exhibit an acid reaction. The nitrate deflagrates when heated to about $120°$ C., and on the application of a higher heat becomes pure peroxide of tin. The salts of oxide of stanæthylium behave with reagents so nearly like the salts of peroxide of tin, that the two are very difficult to distinguish from each other.

Sulphuret of Stanæthylium.—When sulphuretted hydrogen is passed through an acid solution of a salt of stanæthylium, a cream-coloured precipitate falls, which is insoluble in dilute acids and ammonia, but soluble in concentrated hydrochloric acid, solutions of the fixed alkalies, and alkaline sulphurets; from its solutions in the fixed alkalies and alkaline sulphurets, it is reprecipitated, unchanged, on the addition of an acid. I have made no analyses of this body, but there is no doubt that its formula is $C^4 H^5 Sn S$, and that it is produced by the following reaction—

$$C^4 H^5 SnO + HS = C^4 H^5 SnS + HO.$$
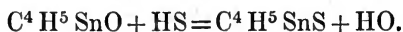
Sulphuret of stanæthylium presents the appearance of an amorphous cream-coloured powder, having a pungent and very nauseous smell, resembling decayed horse-radish: when heated it fuses, froths up and decomposes, emitting vapours of a most insupportable odour. Heated with nitric acid it is decomposed with the formation of peroxide of tin.

Chloride of Stanæthylium, $C^4 H^5 Sn Cl$. — This salt is best prepared by dissolving oxide of stanæthylium in dilute hydrochloric acid: on evaporation at a gentle heat or over sulphuric acid *in vacuo*, the chloride crystallizes out in long colourless needles, isomorphous with the iodide, which salt it also closely resembles in all its properties; it is however more volatile, and therefore emits a more intensely pungent and irritating odour than the iodide.

Stanæthylium.—When a strip of zinc is immersed in a solution of a salt of stanæthylium (a solution of the chloride of stanæthylium is the best for this purpose) it speedily becomes covered with dense oily drops of a yellow colour, which finally separate from the lower extremity of the zinc and accumulate at the bottom of the vessel; the formation of the oily liquid is much favoured by the application of a gentle heat. The yellow oil

was separated from the supernatant liquid by means of a pipette, and well washed with successive large portions of cold water; being then dried over chloride of calcium and submitted to analysis, it yielded the following results:—

I. ·3150 grm., burnt with oxide of copper and oxygen gas, gave ·3498 grm. carbonic acid and ·1757 grm. water.

These numbers correspond sufficiently with the formula $C^4 H^5 Sn$, when it is considered that the stanæthylium, as thus prepared, contains traces of undecomposed chloride of stanæthylium, which I did not succeed in removing by the most protracted washing; and as stanæthylium does not crystallize, and cannot be distilled without decomposition, I could not avail myself of these means of purification. The above formula requires the following numbers:—

				Calculated.	Found.
C^4	.	.	24	27·32	26·95
H^5	.	.	5	5·69	5·51
Sn	.	.	58·82	66·99	
				100·00	

The isolation of stanæthylium from its chloride by zinc is therefore expressed by the following simple equation:—

$$\left. \begin{array}{c} C^4 H^5 Sn\, Cl \\ Zn \end{array} \right\} = \left\{ \begin{array}{c} Zn\, Cl \\ C^4 H^5 Sn. \end{array} \right.$$
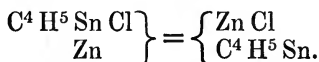
Stanæthylium exists at the ordinary atmospheric temperature, as a thick, heavy, oily liquid, of a yellow or brownish-yellow colour, and an exceedingly pungent odour, resembling that of its compounds, but much more powerful. It is insoluble in water, but soluble in alcohol and æther. At about 150° C. it enters into ebullition, a quantity of metallic tin is deposited, and a colourless liquid distils over, having a peculiar odour, containing a considerable quantity of tin, and exhibiting no tendency to combine with iodine or bromine: the composition and properties of this liquid I have not further ascertained; it possibly consists of or contains *binethide of tin* ($Sn(C^4 H^5)^2$). In contact with the air stanæthylium rapidly attracts oxygen and is converted into a white powder, which has all the properties of oxide of stanæthylium. Chloride, bromide, and iodide of stanæthylium are immediately formed by the action of chlorine, bromine and iodine, or their hydrogen acids respectively, upon stanæthylium; the first and third are in every respect identical with the salts above described. I have analytically examined the bromide prepared by adding an alcoholic solution of bromine to an alcoholic solution of stanæthylium until the colour of the bromine no longer disappears; by spontaneous evaporation the bromide

of stanæthylium is deposited in long white needles, which closely resemble, both in appearance and properties, the chloride of stanæthylium.   These crystals, pressed between folds of bibulous paper, and dried over sulphuric acid *in vacuo,* gave the following analytical results :—

·9730 grm., burnt with oxide of copper, yielded ·5108 grm. carbonic acid and ·2582 grm. water.

These numbers agree very closely with the formula $C^4 H^5 Sn Br$, as is seen from the following comparison :—

		Equivs.		Calculated.	Found.
Carbon	. .	4	24	14·30	14·32
Hydrogen	.	5	5	2·98	2·95
Tin	. . .	1	58·82	35·05	
Bromine	. .	1	80·00	47·67	
			167·82	100·00	

These results show that stanæthylium perfectly resembles cacodyle in its reactions, combining directly with the electronegative elements and regenerating the compounds from which it has been derived.

Examination of Gases.—The examination of the gases evolved on opening the tubes in which iodide of æthyle and tin had been submitted to the action of heat, and which were allowed to stand over sulphuretted water for twelve hours, yielded the following results.   Specific gravity :—

Weight of flask filled with gas . . 35·4712 grms.
Temperature of room . . . . . 20°·8 C.
Height of barometer . . . . . 761·2 mm.
Height of internal column of mercury 15·2 mm.
Temperature in balance case . . . 22°·6 C.
Weight of flask filled with dry air . 35·4703 grms.
Temperature in balance case . . . 22°·8 C.
Capacity of flask . . . . . . 140·50 cubic centims.

From these data the specific gravity was calculated to be 1·0384.

The remainder of the gas was submitted to eudiometrical analysis : the following numbers were obtained :—

I. In Short Eudiometer.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0° C. and 1 metre press.
Volume of gas used (dry) . . .	143·4	21°·0 C.	16·3 mm	760·5 mm	99·09
Volume after action of fuming SO^3 (dry)	122·5	18·2	37·0	754·7	82·42

II. In Combustion Eudiometer.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0° C. and 1 metre press.
			mm	mm	
Volume of gas used (moist) . .	110·3	18·1 C.	570·7	755·2	17·48
Volume after admission of O (moist)	383·6	18·3	274·7	755·7	167·31
Volume after explosion (moist) .	326·9	18·4	332·4	755·8	124·86
Volume after absorption of CO² (dry)	264·5	17·0	398·3	760·7	90·23
Volume after admission of H (dry)	592·5	17·0	77·9	761·6	381·33
Volume after explosion (moist) .	303·5	17·7	355·9	762·7	111·64

As the gas, left unabsorbed by fuming sulphuric acid, was soluble in about its own volume of alcohol, with the exception of a very small per-centage due to the nitrogen introduced by diffusion through the sulphuretted water, it could not contain either hydrogen or hydride of methyle; and the result of the above combustion proves that it is hydride of æthyle, for I have shown that 1 vol. of hydride of æthyle consumes 3·5 vols. oxygen, and generates 2 vols. carbonic acid, numbers which almost exactly correspond with those obtained.

17·48 vols. of the gas, containing 17·15 vols. of combustible gas and ·33 vol. of nitrogen, consumed 59·93 vols. oxygen and generated 34·63 vols. carbonic acid; hence

Volume of combustible gas.		Oxygen consumed.		Carbonic acid generated.
17·15		59·93		34·63
1	:	3·49	:	2·01

Further, the gas agrees in all its chemical and physical properties, with the hydride of æthyle prepared by the action of zinc upon iodide of æthyle in presence of water.

The composition of the gas absorbed by fuming sulphuric acid was determined by exploding a known volume of the original gas with excess of oxygen, and determining the quantities of oxygen consumed, and carbonic acid generated.

This determination gave the following numbers :—

III.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0°C. and 1 metre press.
Volume of gas used (moist)	122·0	19·7C.	563·0 mm	764·2 mm	20·95
Volume after admission of O (moist)	424·5	20·0	240·4	764·3	200·33
Volume after explosion (moist)	363·5	20·4	301·0	763·5	150·40
Volume after absorption of CO^2 (dry)	296·7	19·7	366·4	760·4	109·03
Volume after admission of H (dry)	683·1	21·7	4·0	760·8	478·89
Volume after explosion (moist)	368·6	21·8	293·3	761·2	153·09

Hence 20·95 vols. containing 20·62 vols. combustible gas, consumed 70·78 vols. oxygen, and generated 41·37 vols. carbonic acid : now as 20·62 vols. of this gas must contain, according to analyses Nos. I. and II., 17·10 vols. hydride of æthyle, which would consume 59·85 vols. oxygen and generate 34·20 vols. carbonic acid, it is evident that the volumes of oxygen consumed, and carbonic acid generated, by the gas absorbed by fuming sulphuric acid, must bear the following relation to each other :—

Volume of gas absorbable by fuming SO^3.		Oxygen consumed.		Carbonic acid generated.
3·52		10·93		7·17
1	:	3·10	:	2·03

The body removed by fuming sulphuric acid is therefore olefiant gas, 1 vol. of which consumes 3 vols. oxygen, and generates 2 vols carbonic acid, numbers which correspond sufficiently with those obtained in the above determination.

The last analysis can also be employed to control Nos. I. and II.; for if we represent the volume of nitrogen, contained in the original gas, by x, that of hydride of æthyle by y, and that of olefiant gas by z; and further, the volume of mixed gases, oxygen consumed, and carbonic acid generated, respectively by A, B and C, we have the following equations :—

$$x+y+z=A$$
$$\frac{7}{2}y+3z=B$$
$$2y+2z=C,$$

from which the following values for x, y and z are derived :—

$$x = \cdot 27$$
$$y = 17 \cdot 06$$
$$z = 3 \cdot 62$$
$$\overline{20 \cdot 95}$$

The per-centage composition of the gases evolved by the action of heat upon iodide of æthyle and tin is therefore the following:—

	I. and II.	III.
Hydride of æthyle . .	81·61	81·43
Olefiant gas	16·82	17·28
Nitrogen	1·57	1·29
	100·00	100·00

This result is also confirmed by the determination of the specific gravity of the gaseous mixture, as is seen from the following calculation :—

$$\text{C}^4\,\text{H}^5\,\text{H} \quad . \quad . \quad . \quad 81\cdot61 \times 1\cdot03652 = 84\cdot590$$
$$\text{C}^4\,\text{H}^4 \quad . \quad . \quad . \quad 16\cdot82 \times \cdot96742 = 16\cdot272$$
$$\text{N} \quad . \quad . \quad . \quad 1\cdot57 \times \cdot96740 = 1\cdot519$$

$$\frac{100\cdot00 \qquad\qquad 102\cdot381}{100} = 1\cdot02381$$

Specific gravity found by experiment . . $= 1 \cdot 0384$

The presence of hydride of æthyle and olefiant gas amongst the products of the action of heat upon iodide of æthyle and tin, shows that the combination of tin with iodide of æthyle is not the only reaction which takes place, but that a portion of the iodide of æthyle is also decomposed by the tin, with the production of iodide of tin and æthyle; the latter body being transformed at the moment of its liberation into hydride of æthyle and olefiant gas, a catalysis to which this radical is so prone,

$$\left. \begin{array}{c} 2\text{C}^4\,\text{H}^5\,\text{I} \\ 2\text{Sn} \end{array} \right\} = \left\{ \begin{array}{l} \text{C}^4\,\text{H}^5,\ \text{H} \\ \text{C}^4\,\text{H}^4 \\ 2\text{SnI} \end{array} \right.$$
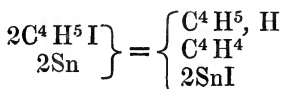
It was ascertained that protoiodide of tin was present amongst the solid products of the reaction.

The large excess of hydride of æthyle exhibited in the above analysis, may have been caused, either by the greater solubility of olefiant gas in iodide of æthyle (a further and considerable amount of gas being expelled from the tube by the application of a gentle heat), or by the presence of moisture in the materials, which would give rise to the formation of oxyiodide of tin and

hydride of æthyle,

$$\left.\begin{array}{c} C^4\,H^5\,I \\ HO \\ 2Sn \end{array}\right\} = \left\{\begin{array}{l} C^4\,H^5\,H \\ SnO + Sn\,I. \end{array}\right.$$
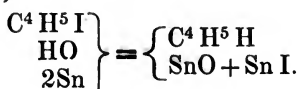
Both these causes probably contribute to produce the excess of hydride of æthyle; but the very small amount of gaseous products, compared with the solid ones, convinced me that the production of the former is only an accidental circumstance, which, however it may be interpreted, does not at all affect the principal reaction, viz. the formation of iodide of stanæthylium. The gases, evolved by the action of light upon iodide of æthyle and tin, are perfectly similar to those obtained by the action of heat.

Stanmethylium and stanamylium are formed when the iodides of methyle and amyle respectively are exposed to the action of light in contact with tin; their salts are isomorphous with those of stanæthylium; but I have not yet completed the investigation of these bodies.

[To be continued.]

XXVII. *On the Method of Symmetric Products.*   By JAMES COCKLE, *M.A., of Trinity College, Cambridge; Barrister-at-Law of the Middle Temple*.*

[Continued from vol. iv. p. 496.]

17. THE following processes, which supersede the formidable eliminations threatened by the conditions already arrived at, appear to suggest a general method of treating symmetrical equations.

18. From the terms in $y_2^3\,y_3$ and $y_2^2\,y_3^2$ respectively we obtain

$$\Sigma\,.\,\beta - \Sigma\,.\,\alpha_1\alpha_2\alpha_3\beta_4 = 0 \text{ and } \Sigma\,.\,\beta_1\beta_2 - \Sigma\,.\,\alpha_1\alpha_2\beta_3\beta_4 = 0,$$

or

$$\beta_1 + \beta_2 + \beta_3 + \beta_4 = \alpha_2\beta_1 + \alpha_1\beta_2 + \alpha_4\beta_3 + \alpha_3\beta_4$$

and

$$(\beta_1 + \beta_2)(\beta_3 + \beta_4) = (\alpha_2\beta_1 + \alpha_1\beta_2)(\alpha_4\beta_3 + \alpha_3\beta_4).$$

19. It follows from this that

$$(\beta_1 + \beta_2)(E - \beta_1 - \beta_2) = (\alpha_2\beta_1 + \alpha_1\beta_2)(E - \alpha_2\beta_1 - \alpha_1\beta_2),$$

and we find

$$\beta_1 + \beta_2 = \alpha_2\beta_1 + \alpha_1\beta_2 \text{ or } \alpha_4\beta_3 + \alpha_3\beta_4,$$
$$\beta_3 + \beta_4 = \alpha_4\beta_3 + \alpha_3\beta_4 \text{ or } \alpha_2\beta_1 + \alpha_1\beta_2.$$

Of these systems of values I, here at least, adopt the former and make

$$\beta_1 + \beta_2 = \alpha_2\beta_1 + \alpha_1\beta_2, \quad \beta_3 + \beta_4 = \alpha_4\beta_3 + \alpha_3\beta_4,$$

or $$\beta_1 = \alpha_1 \beta_2 \text{ and } \beta_3 = \alpha_3 \beta_4;$$

hence

$$\beta_1 \beta_2 \beta_3 \beta_4 = 1 = \alpha_2 \alpha_4 \beta_1{}^2 \beta_3{}^2, \text{ or } \beta_1{}^2 \beta_3{}^2 = \alpha_1 \alpha_3. \quad . \quad . \quad (d)$$

20. The equality of the coefficients of $y_2{}^3 y_3$ and $y_3{}^3 y_2$ is expressed by

$$\Sigma \cdot \alpha_1 \alpha_2 \alpha_3 \beta_4 - \Sigma \cdot \alpha_1 \beta_2 \beta_3 \beta_4 = 0,$$

or

$$(\alpha_2 + 1)\beta_1 + (\alpha_4 + 1)\beta_3 = \Sigma \cdot \frac{\alpha_1}{\beta_1} = (\alpha_1 + 1)\beta_1^{-1} + (\alpha_3 + 1)\beta_3^{-1}.$$

Multiplying the left of this equation by $\sqrt{\overline{\alpha_1 \alpha_3}}$, and the right by $\beta_1 \beta_3$, and slightly changing the form of the result, we obtain

$$\{(1 + \alpha_1^{-1})\beta_1 + (1 + \alpha_3^{-1})\beta_3\} \sqrt{\overline{\alpha_1 \alpha_3}} = (\alpha_1 + 1)\beta_3 + (\alpha_3 + 1)\beta_1.$$

Multiply the last equation into β_3 and β_1 successively. The respective results, combined with (d), give

$$\beta_1{}^2 = \alpha_1 \text{ and } \beta_3{}^2 = \alpha_3.$$

21. Now, from

$$\Sigma \cdot \beta_1{}^2 \beta_2{}^2 = \Sigma \cdot \alpha_1 \alpha_2 = F = \Sigma \cdot \beta_1 \beta_2,$$

we deduce

$$(\beta_1{}^2 + \beta_1^{-2})(\beta_3{}^2 + \beta_3^{-2}) = F - 2 = (\beta_1 + \beta_1^{-1})(\beta_3 + \beta_3^{-1}),$$

and hence arrive at

$$(F - 2)^2 = F - 2 + 2E + 4 = F + 2E + 2. \quad . \quad . \quad (e)$$

22. But we also have

$$E^2 = (\Sigma \cdot \beta)^2 = \Sigma \cdot \beta^2 + 2\Sigma \cdot \beta_1 \beta_2 = E + 2F; \quad . \quad (f)$$

and the elimination of F between (e) and (f) gives

$$E^4 - 2E^3 - 9E^2 + 2E + 8 = 0 = (E^2 - 1)(E^2 - 2E - 8),$$

where the values of E are $1, -1, -2, 4$. I here adopt -1, and from (f) find that 1 is the corresponding value of F.

23. These values reduce (c) to the form

$$z^4 + z^3 + z^2 + z + 1 = 0,$$

and the values of z are the unreal roots of

$$z^5 - 1 = 0,$$

of which, if α be one, the others are α^2, α^3 and α^4.

24. Let us now make

$$Y_1 = y_1 + \alpha \ y_2 + \alpha^2 y_3 + \alpha^3 y_4 + \alpha^4 y_5,$$
$$Y_2 = y_1 + \alpha^4 y_2 + \alpha^3 y_3 + \alpha^2 y_4 + \alpha \ y_5,$$
$$Y_3 = y_1 + \alpha^3 y_2 + \alpha \ y_3 + \alpha^4 y_4 + \alpha^2 y_5,$$
$$Y_4 = y_1 + \alpha^2 y_2 + \alpha^4 y_3 + \alpha \ y_4 + \alpha^3 y_5,$$

and

$$\pi_4(y_5) = \psi \cdot y^4 + \psi \cdot y_1^3 y_2 + \psi \cdot y_1^2 y_2^2 + \psi \cdot y_1^2 y_2 y_3 + \psi \cdot y_1 y_2 y_3 y_4,$$

and determine the functions ψ.

25. Denoting by $c\{\phi\{y\}\}$ the coefficient of $\phi\{y\}$ in π, we see that

$$c(y_r^4) = \alpha^{10} = 1,$$

and all the terms in y^4 are symmetric, and

$$\psi \cdot y^4 = \Sigma \cdot y^4. \quad . \quad . \quad . \quad . \quad . \quad (g)$$

26. Let η denote one and θ another of the quantities $\alpha, \beta, \gamma, \delta$, and let η_1 and θ_1 be respectively equal to α^m and α^n. Then

$$c(y_r^3 y_s) = \Sigma \cdot \eta_1 \eta_2 \eta_3 \theta_4 = \Sigma \cdot \eta_1^{-1} \theta_1$$
$$= \alpha^{m-n} + \alpha^{n-m} + \alpha^{3(m-n)} + \alpha^{3(n-m)} = -1,$$

and the terms of the form $y_r^3 y_s$ are symmetric, and

$$\psi \cdot y_1^3 y_2 = -\Sigma \cdot y_1^3 y_2. \quad . \quad . \quad . \quad . \quad (h)$$

27. Again :

$$c(y_r^2 y_s^2) = \Sigma \cdot \eta_1 \eta_2 \theta_3 \theta_4 = 2 + (\eta_2 \theta_1 + \eta_1 \theta_2)(\eta_4 \theta_3 + \eta_3 \theta_4)$$
$$= 2 + (\alpha^{n-m} + \alpha^{m-n})(\alpha^{3(n-m)} + \alpha^{3(m-n)}) = 1,$$

and the terms of the form $y_r^2 y_s^2$ are symmetric, and

$$\psi \cdot y_1^2 y_2^2 = \Sigma \cdot y_1^2 y_2^2. \quad . \quad . \quad . \quad . \quad . \quad (i)$$

28. I shall next consider $y_1 y_2 y_3 y_4$, of which the coefficient is

$$\Sigma \cdot \alpha_1 \{\beta_2(\gamma_3 + \gamma_4) + \beta_3(\gamma_2 + \gamma_4) + \beta_4(\gamma_2 + \gamma_3)\}.$$

Developing this and making the substitutions indicated by (24), we find

$$c(y_1 y_2 y_3 y_4) = 4 + 5(\alpha + \alpha^2 + \alpha^3 + \alpha^4) = -1.$$

29. If in the formulæ of (28) we replace α by α^2, α^3, and α^4 successively, we shall be conducted to respective results identical with the last. In other words,

$$c(y_1 y_2 y_3 y_4) = c(y_1 y_3 y_5 y_2) = c(y_1 y_4 y_2 y_5) = c(y_1 y_5 y_4 y_3) = -1.$$

30. In place of Y_r put $\alpha_r^{-1} Y_r$. The product will be unaltered in value, but there will arise changes indicated by the symbolic equation

$$\chi(y_1, y_2, y_3, y_4, y_5) = \chi(y_5, y_1, y_2, y_3, y_4).$$

We hence infer that

$$c(y_2 y_3 y_4 y_5) = c(y_1 y_2 y_3 y_4) = -1,$$

and, all the terms of this form being symmetric, that

$$\psi \cdot y_1 y_2 y_3 y_4 = -\Sigma \cdot y_1 y_2 y_3 y_4. \quad . \quad . \quad . \quad (j)$$

31. Lastly, let

$$c(y_\eta^2 y_r y_s) = (q, r, s) = (q, s, r).$$

Then
$$(1, r, s) = \Sigma \cdot \eta_2 \theta_1 = E^2 - \Sigma \cdot \eta_1 \theta_1,$$
and
$$(1, 2, 3) = (1, 2, 4) = (1, 3, 5) = (1, 4, 5) = 2,$$
and
$$(1, 2, 5) = (1, 3, 4) = -3.$$

32. Let
$$(\eta_2 \theta_1 + \eta_1 \theta_2)(\eta_3 + \eta_4) = \eta', \quad (\eta_4 \theta_3 + \eta_3 \theta_4)(\eta_1 + \eta_2) = \theta',$$
and, for a reason derived from (26), let
$$\eta' + \theta' = f(m, n) = f(r-1, s-1).$$

Then, placing the suffix of the square to the left,
$$(r, 1, s) = \Sigma \cdot \eta_1 \eta_2 (\theta_3 + \theta_4) = \Sigma \cdot \theta + f(m, n) = -1 + f(r-1, s-1).$$

33. Let
$$\phi(t) = \alpha^t + \alpha^{-t},$$
then
$$f(r-1, s-1) = \phi(s-r)\phi(3r-3) + \phi(3s-3r)\phi(r-1),$$
whence we find
$$f(1, 2) = -2, \quad f(1, 3) = 3 = f(1, 4).$$

34. The nature of α is such that
$$f(1, 2) = f(2, 4) = f(3, 1) = f(4, 3) = 3 - 5,$$
$$f(1, 3) = f(2, 1) = f(3, 4) = f(4, 2) = 3,$$
$$f(1, 4) = f(2, 3) = f(3, 2) = f(4, 1) = 3.$$

35. By the cycle of (30) we find
$$(q, r, s) = (q-1, r-1, s-1),$$
where, when zero occurs among the symbols, 5 is to be substituted for it. And we now see that

$(2, 3, 4) = (1, 2, 3), \quad (2, 3, 5) = (1, 2, 4), \quad (2, 4, 5) = (1, 3, 4),$
$(3, 2, 4) = (2, 1, 3), \quad (3, 2, 5) = (2, 1, 4), \quad (4, 2, 3) = (3, 1, 2),$
$(4, 2, 5) = (3, 1, 4), \quad (5, 2, 3) = (4, 1, 2), \quad (5, 2, 4) = (4, 1, 3).$

36. By putting $\beta_r^{-1} Y_r$ in place of Y_r in the original system, we arrive at
$$(q, r, s) = (q-2, r-2, s-2);$$
and, so, at
$$(3, 4, 5) = (1, 2, 3), \quad (4, 3, 5) = (2, 1, 3), \quad (5, 3, 4) = (3, 1, 2),$$
results also obtainable from (35) by a double reduction.

37. We are thus conducted to
$$\psi \cdot y_1^2 y_2 y_3 = 2\Sigma \cdot y_1^2 y_2 y_3 - 5\Sigma' \cdot y_1^2 (y_2 y_5 + y_3 y_4),$$
Σ' being such that each value of y^2 and of $y_1 y_2$ occurs in it once and once only. It has what may be termed a partial symmetry.

38. Combining the last equation with (g), (h), (i) and (j),

and making
$$E = -1, \quad F = 1, \quad G = 2, \quad H = -1,$$
the relation given in (10) becomes
$$P_4 = \Sigma \cdot y^4 - \Sigma \cdot y_1^3 y_2 + \Sigma \cdot y_1^2 y_2^2 + 2\Sigma \cdot y_1^2 y_2 y_3 - \Sigma \cdot y_1 y_2 y_3 y_4.$$

39. And we also have
$$\left.\begin{aligned}
U_4 = -5\Sigma' \cdot y_1^2(y_2 y_5 + y_3 y_4) &= -5\{ y_1^2(y_2 y_5 + y_3 y_4) \\
+ y_2^2(y_1 y_3 + y_4 y_5) &+ y_3^2(y_1 y_5 + y_2 y_4) + y_4^2(y_1 y_2 + y_3 y_5) \\
+ y_5^2(y_1 y_4 + y_2 y_3) \}.
\end{aligned}\right]$$

40. If we denote by S a symmetric, and by U an unsymmetric function, the equation
$$\pi_4(y_5) = S + U$$
is satisfied, not only by
$$S = P_4, \quad U = U_4,$$
but also by
$$S = P_4 - 5\Sigma \cdot y_1^2 y_2 y_3, \quad U = U_4 + 5\Sigma \cdot y_1^2 y_2 y_3,$$
and in other ways not necessary to be mentioned.

41. In effecting the transformation (*b*) of (9) by my process, the second condition
$$B'_3 = \psi(x, y, z) = 0$$
must be superseded by the equation
$$B_4 - \frac{2}{5} B_1 B_3 + \frac{3}{5^2} B_1^2 B_2 - \frac{3}{5^3} B_1^4 = 0,$$
the left of which is a critical function. Each of the functions Y and, consequently, their product is critical.

42. It does not appear to be possible to render $\pi_4(y_5)$ perfectly symmetric in the general equation. For, starting with the results of (20), we may convert
$$c(y_1 y_2^2 y_3) - c(y_1 y_2 y_3^2) = (2, 1, 3) - (3, 1, 2) = 0$$
into
$$\phi'(\lambda)\phi'(\sqrt{\mu}) + \phi'(\sqrt{\lambda})\phi'(\mu) - 2\phi'(\sqrt{\lambda})\phi'(\sqrt{\mu}) = 0,$$
where
$$\phi'(r) = r + r^{-1}.$$
But if we combine this relation with
$$c(y_1^3 y_2) = c(y_1^3 y_3) \text{ or } \phi'(\lambda) + \phi'(\mu) = \phi'(\sqrt{\lambda}) + \phi'(\sqrt{\mu}),$$
we shall be led to conditions which seem inconsistent with symmetry.

43. In line 8 of (4) P_n must be replaced by P_{n-1}, and in line 14 of (5) C_{n-1} must be put in place of C_n.

2 Pump Court, Temple,
    Dec. 24, 1852.

XXVIII. *On Copper Smelting.* By JAMES NAPIER, *F.C.S.*[*]
[Continued from p. 39.]
Calcination of the granulated coarse Metal.

THE next operation is the calcination of the granulated coarse metal; this is done in the same manner as the calcination of the ore, but the furnaces used are always single-bedded, and measure inside about 21 feet in length and 14 feet in width. The charge of metal covers the bottom of the hearth to the depth of about 4 inches, making about 4 tons, and is put in through the hoppers fitted upon the top of the furnace as described for the ore.

The coarse metal being easily fused, great care is required not to raise the heat of the furnace too high, otherwise the metal will cake, and by adhering to the bricks will prove prejudicial to both the calcination and the furnace. When the charge is let into the furnace, it is slowly brought to a visible red, the fire is gradually and cautiously increased until the metal acquires a bright red heat, which should take about fourteen hours. This temperature is continued until the charge has been altogether twenty-four hours in the furnace, when it is let down through the bottom into the *cubs* or vaults beneath.

Fineness of granulation ensures a more perfect calcination, a more extensive surface being exposed. As the heat requisite to drive off the sulphur does not penetrate deep into the solid granules, rendering the calcination of coarse metal tedious, the mass is turned over by means of iron paddles every two hours during the operation. At some works it is done every hour, which we consider preferable; but there is a far greater loss of heat, owing to the doors of the furnace being kept longer open in stirring every hour than every two hours, which neutralizes the advantage of the extra turning over.

After the charge has laid a little in the *cubs* a quantity of water is thrown upon it, and it is then allowed to cool. The addition of the water prevents dust, which flies off readily when dry, causing loss; and also effects a certain amount of oxidation, as a great quantity of sulphuretted hydrogen is given off, the metal assuming a black colour. When cold, the metal is removed from the cubs to a heap, from which it is taken to the fusing furnaces.

The chemical changes which take place during the calcination of the coarse metal are somewhat similar to those described in the case of the ore, but are not fully developed owing to the solid state of the granules. That sulphates are formed, we have had sufficient evidence by taking out small portions during the operation and testing; but the quantities are small.

The following analyses give a fair average of the results of calcining coarse metal :—

Metal put into calciner.		Metal after calcination.	
Copper	32	Copper	33
Iron	36	Iron	36
Sulphur	25	Sulphur	13
Insoluble matter .	7	Insoluble matter .	7
	100	Oxygen	11
			100

The following is given by Le Play as the composition of calcined coarse metal, which differs but little from the average of our experience :—

	Coarse metal put in.	After partial calcination.	Finished.
Copper	33·7	34·6	35·6
Iron	33·6	35·1	36·1
Sulphur	29·2	16·4	11·7
Other metals . .	2	1·5	1·6
Insoluble matter . .	1·1	1·1	1·2
Oxygen	...	11·3	13·8
	99·6	100·0	100·0

The interference, arising from the solid character of the granules, with the full development of the chemical reactions in the calcination, is completely got over by obtaining the coarse metal in the state of a powder, either by crushing it mechanically, or by the process referred to in the last article, namely, fusing with sulphate of soda and coals, and immersion in water. In calcining such powders, the reaction between the metal and the air or gases in the chamber or hearth go on without interruption, and enable us to define it experimentally. It is with powder produced by the last process to which our investigations have been mostly directed; but the results we find applicable not only to crushed powder, but to the granulated coarse metal, so far as the action can be developed consistent with their solid state.

The first series of experiments with the powder were made in a two-bedded calciner. The powder was obtained from a poor Cornish ore, and was not thoroughly washed. A sample was taken from the furnace every four hours; 100 grains were boiled in distilled water, and the quantity of sulphates ascertained; another portion was digested in nitromuriatic acid, and the copper, iron, sulphur, and insoluble matters determined; the sulphur in the sulphates being deducted from the whole to determine what was combined with the metal as sulphuret. The following table gives the results :—

Time.	Copper.	Iron.	Sulphur.	Sulphuric acid.	Copper in solution.	Iron in solution.	Insoluble matters.
Put in.	20·4	39·7	24·7	·9	......	......	4·5
4 hours.	18·9	37·9	18·3	1·6	·5	......	5·5
8 hours.	20·5	32·4	7·5	13·9	6·0	·4	9·1
12 hours.	19·3	35·4	7·8	13·7	7·6	·4	6·8
16 hours.	23·9	26·4	5·1	14·4	6·4	·6	11·7
20 hours.	18·4	29·5	2·1	21·6	13·0	·7	16·1
24 hours.	26·2	39·8	7·4	4·4	1	trace	8·5
28 hours.	23·4	39·1	2·6	4·1	1	trace	7·0
32 hours.	23·8	35·0	1·7	4·8	1·4	trace	10·5
36 hours.	26·8	40·5	1·0	5·5	1·3	trace	5·5
40 hours.	25·2	36·0	·5	4·1	1·0	trace	7·0
44 hours.	22·9	34·1	·5	3·7	·5	trace	14·0

During the last twelve hours the heat of the furnace was at a bright yellow, sufficient to fuse uncalcined coarse metal.

The following is a repetition of the same experiments with another charge of powder, the heat of the furnace being more intense than the last :—

Time.	Copper.	Iron.	Insoluble.	Sulphur.	Sulphuric acid.	Sulphate of copper.	Sulphate of iron.
Put in.	22·5	35·1	4·4	24			
4 hours.	22·6	35·6	10·8	10	11·4	4·4	1·4
8 hours.	28·0	30·7	2·4	15·4	6·6	2·6	1·0
12 hours.	26·0	36·0	3·2	17	2·4	1	trace
16 hours.	26·8	35·2	3·2	18	3·0	·8	·6
20 hours.	26·0	40·4	4·8	18·5	1·4	·7	·2
24 hours.	26·6	27·8	12·8	3·6	4·8	2·7	trace
28 hours.	26·8	30·5	12	......	5·8	3·4	trace
32 hours.	26·5	26·3	12	1·5	7·8	5	trace
36 hours.	27·2	36·0	3·2	4	3·7	2	trace
40 hours.	28·5	36·0	6	2·5	4·3	1·5	
44 hours.	28·8	36·5	4	·5	3·4	·5	

By casting the eye along the sulphur and sulphuric acid columns, the change taking place is very apparent and interesting. The rapidity of the formation of sulphuric acid at the commencement may be assisted by the powder being damp; nevertheless there seems to be an intermitting action between the sulphur as sulphurets and sulphuric acid, the oxide of iron probably playing a part in these reactions. It is also seen that it is much more difficult to decompose sulphate of copper by heat than sulphate of iron. It is remarkable how little sulphur is driven off while in the top bed, and how rapidly it is evolved when the powder enters the lower and hotter bed, so as in a few hours to decompose the whole of the sulphurets.

The following table contains the results of experiments made with a three-bed calciner, the heat passing from one bed over the other, the powder being put in at the top and let down through each

bed, as described for the ore, the charge being kept in each sixteen hours, stirred once every hour, and a sample taken out immediately after stirring from the same part of the furnace, and tested in the manner above described.

Time.	Copper.	Iron.	Insoluble.	Sulphur.	Sulphuric acid.	Sulphate of copper.	Sulphate of iron.
Put in.	30	37·4	3·6	26·1	trace		
1 hour.	29·2	37·4	3·6	23	2·4	1	·4
2 hours.	27·2	34·6	7·2	17·5	7·2	3·5	1·4
3 hours.	26·8	34·6	4·8	14·2	5·2	3	·4
4 hours.	28	18·6	16	6·4	16·4	9·0	·6
5 hours.	26	30·5	8·8	11·5	5·1	3·5	·4
6 hours.	27·2	27·6	4·8	1·0	22·3	13	2·0
7 hours.	24·8	28·8	5·2	4·3	18·5	11·6	·2
8 hours.	27·2	35·6	2·8	16	5·6	3·5	·2
9 hours.	26	36	4	20·7	1·7	·8	trace
10 hours.	26	37·4	2	13·3	6·8	3·5	·4
11 hours.	24·8	38·2	1·6	17·5	4·5	2·4	·4
12 hours.	25·6	29·1	9·6	16	4·2	3·0	·2
13 hours.	28	36	2·8	19·4	2·4	1·4	trace
14 hours.	26·4	34·5	8	14	3	1·2	
15 hours.	27·2	36	2	16	4·4	2·6	trace
16 hours.	28	29	13·2	9	5·7	4·5	trace
17 hours.	26·4	34·6	6·8	11·6	5·8	3	trace
18 hours.	26·8	32	10	14·3	3·6	2	trace
19 hours.	27·2	36	3·6	18	2·2	1·5	trace
20 hours.	24	38·5	5·2	20	1·5	1·4	
21 hours.	24	38	5·2	9·4	5·3	2·5	trace
22 hours.	31	34·3	10·4	7·4	3·9	1	trace
23 hours.	24·3	30·3	9	8·6	5·3	2·8	trace
24 hours.	27	36·1	4·6	6·5	5·0	3·1	
25 hours.	27·2	34·1	3·9	7·2	5·4	2·8	
26 hours.	25·0	31·9	3·8	9·6	5·2	3	
27 hours.	30·4	30·4	6·7	5·4	6·6	4	
28 hours.	25·2	28·9	4·0	6	6·7	3·3	
29 hours.	24	32·7	5·8	8	4·6	3	
30 hours.	25·2	34·7	5·7	4·2	4·2	2·5	
31 hours.	25·5	34·3	5·7	7·4	2·9	3·7	
32 hours.	26·5	34·1	10·6	4·5	6·4	4·2	
33 hours.	26	37·5	11·4	3·6	8·5	7·3	
34 hours.	25	33·3	11·2	3·5	5·2	2·8	
35 hours.	21·2	36	4·8	11·3	6·3	3·8	
36 hours.	22	32·2	11·9	12	4·6	3·1	
37 hours.	24·6	33	5·7	10·3	3·8	1·7	
38 hours.	28·4	31·4	11·3	3·1	2·1	·4	
39 hours.	27·6	30·5	8·5	2·0	2	·4	
40 hours.	24	33·3	11·3	·7	1·5	·2	
41 hours.	27·9	31·3	9·5	1·1	1·4	·2	
42 hours.	26	30·6	6·9	·5	·7	·2	
43 hours.	27	36	5·8	1·4	1·5	·2	
44 hours.	24·8	31·9	13·6	1·4	1·7	·9	
45 hours.	27	36	5·6	1·2	1·6	·2	
46 hours.	24	36·4	4·8	1·2	2·3	·2	
47 hours.	24·6	36	3·9	1	2	·2	
48 hours.	23	36·1	6·9	1·9	2·4	2	
49 hours.	25	34·3	7·5	......	3·6	2	
50 hours.	27·6	36	3·3	......	1·4	trace	
51 hours.	22·4	30·4	10·1	......	3·4	3	

We observe in the above table the same kind of reaction as was noticed in the calcination of the ore—a sudden evolution of sulphur, and then a gradual absorption continuing for several hours. The change of sulphates into sulphurets, and again into sulphates, &c., is also worthy of remark; and this action takes place to a certain extent even in the lowest bed. The peroxide of iron at one time gives off a portion of its oxygen and is converted into the magnetic oxide, as we have often ascertained; at other times it takes up oxygen and is converted into the red oxide. We have repeatedly found that, in the samples taken out when the testing showed the decomposition of the sulphates into sulphurets, the powder was full of granules, which when broken had a vitreous appearance, and a yellow bronze colour resembling rich copper pyrites.

The next table shows the changes and reactions in the powder when calcined in a three-bed calciner, the fire entering directly into each bed, as described for the ore at page 462 of the last volume; so that the fumes from the one bed do not pass over the other, in order to avoid the sulphur in the fumes from the lower beds being absorbed by the powder in the upper beds.

Time.	Copper.	Iron.	Insoluble.	Sulphur.	Sulphuric acid.	Sulphate of copper.	Sulphate of iron.
Put in.	23·6	40·4	2·6	30·4	3·1		
1 hour.	24·	41·2	0·7	33·7	1·0		
2 hours.	21·5	39·8	3·0	30·1	1·0	......	trace
3 hours.	22	42·8	5·7	15·0	15·8	5	4·2
4 hours.	26·4	42	2·6	25·1	1	trace	trace
5 hours.	25·6	38·6	3·1	27	1·7	......	1·1
6 hours.	23·6	37·8	3	20 5	·7		
7 hours.	22·5	39·2	2·8	22	4·3	......	2·2
8 hours.	20	37·8	3·8	25·0	1·7	......	trace
9 hours.	24	40·6	2·6	26	1·7		
10 hours.	23·2	36·9	2·9	28·6	·5		
11 hours.	24	39·2	3	27·6	·8		
12 hours.	22	39·4	3·5	20·6	12·0	3·6	4·6
13 hours.	21	36·4	2·8	18·6	7·6	2·3	2·8
14 hours.	23·4	36·4	3·7	22·5	4·4	·3	2 8
15 hours.	24·4	36·4	3·4	20·2	8·7	......	4·5
16 hours.	25	39·2	3·1	24·2	3·3	·3	1·7
17 hours.	24·6	39·2	2·3	24·3	5·1	·2	2·1
18 hours.	26·4	38·6	4·5	20·7	2·5	·2	1·4
19 hours.	25·6	37·8	3·1	21	8·7	......	4·5
20 hours.	21	39·2	4·1	21·4	2·9	......	·7
21 hours.	24·4	39·8	3·9	15·0	3·3	......	1·4
22 hours.	26·4	37	3·8	13	4·7	2	1
23 hours.	27·4	39·8	3·8	8·7	6·9	3·2	trace
24 hours.	25	39·2	5·6	9	4·7	1·7	trace
25 hours.	24·8	40·6	4·1	18·5	1·1	......	trace
26 hours.	25	39·8	4	9	4·1	1·7	trace
27 hours.	25·2	37·8	2·8	17·8	2·2	......	·8

Table (*continued*).

Time.	Copper.	Iron.	Insoluble.	Sulphur.	Sulphuric acid.	Sulphate of copper.	Sulphate of iron.
28 hours.	26·4	40·6	4·1	11·6	1·5	......	trace
29 hours.	27	40·3	3·9	21	·7	......	trace
30 hours.	24·8	42	4·5	7·8	2·5	1	trace
31 hours.	25·2	43·4	4·6	5·5	3	1·3	trace
32 hours.	26·4	40·6	4·7	10·3	2·9	·5	·5
33 hours.	26·4	39·4	4·4	7·8	2·5	1·3	trace
34 hours.	26·4	40·6	5	10·2	7·3	3·3	trace
35 hours.	26·8	40·9	4·4	7·5	10·5	7	·5
36 hours.	27	40·6	4	10·6	3·1	1·2	·5
37 hours.	26·8	38·9	4·3	6	3·6	1·3	trace
38 hours.	26	40	3·8	2	3·2	·7	trace
39 hours.	26	40·3	3·8	1·4	4·3	·7	trace
40 hours.	26	39·2	4	1·1	3·6	2	trace
41 hours.	25·5	39·1	4·2	2	6·2	3·3	
42 hours.	26	40	3·2	5·8	3·3	1·5	
43 hours.	25·6	42	3·9	·2	4 .	1·3	
44 hours.	28·8	36·4	10·7	1·3	2·9	1	
45 hours.	25·6	39·5	3	·6	2·5	·7	
46 hours.	25·6	37·2	4·3	5	3·1	1·2	
47 hours.	24·4	36·4	5·7	4·3	2·5	·7	
48 hours.	24	37·6	7	6	1·8	·4	
49 hours.	26·8	39·5	6·1	4·4	2	·2	

In this series of analyses the same reactions are exhibited, but the same objection applies to this kind of furnace when used for powder as for ore—the impracticability of regulating the heat in each bed as required. If it be increased in any one of the beds, the heat of the others is reduced so as to affect the process.

If we look at the results as the powder enters each bed, there is an appearance of regularity. In going into the second, it has lost 6 per cent. of sulphur in the first, or about one-fifth ; and on entering the lowest bed it has parted with two-thirds in all, or 14 per cent. in the second bed ; but looking at the actions and reactions hourly, and the sudden starts the evolution of sulphur occasionally takes, the progress of the calcination is much slower than it ought to be, and would be were there sufficient control over the heat.

The following table shows the results of experiments made in an ordinary one-bed calciner as described for ore, and as universally used for granulated coarse metal.

Time.	Copper.	Iron.	Insoluble.	Sulphur.	Sulphuric acid.	Sulphate of copper.	Sulphate of iron.
Put in.	20	39·2	2·6	28·9	·8		
1 hour.	21·2	38·6	2·7	27·4	1·6	......	trace
2 hours.	22·8	39·6	2·8	24	1·6	......	trace
3 hours.	20·2	36·4	2·7	20·4	1·4	trace	trace
4 hours.	23·6	39·2	2·8	22·5	·4	......	trace
5 hours.	25·8	37·8	2·9	16·7	1·4		
6 hours.	24·4	40·6	2·8	17·5	·7		
7 hours.	26	39·2	2·6	14·5	1·7	trace	
8 hours.	24·6	37·8	2·8	16·5	1·4		
9 hours.	25·2	37·8	4·5	12·2	1·4	trace	
10 hours.	27·6	38·6	4·3	12·7	1·9	trace	
11 hours.	25·2	38·8	5·4	10·7	1·4	trace	
12 hours.	25·2	37·5	5·2	10·3	1		
13 hours.	25	36·4	9·0	8	2·1	trace	
14 hours.	26·8	37·8	6·5	10	1·4	trace	
15 hours.	24·6	33·6	13·9	8·6	1·5	·3	
16 hours.	24·8	36·7	7·7	7	2·4	trace	
17 hours.	25	29·1	12·9	4·5	2·5	·3	
18 hours.	22	38·5	4·7	5·1	2·1	·4	trace
19 hours.	26·4	38·6	6·9	5·5	1·7	·4	trace
20 hours.	26·8	37	9·5	4	2·4	1·0	trace
21 hours.	25·4	36·8	6	2·3	3·1	·4	trace
22 hours.	24·6	40·6	4·2	4·4	1·7	trace	
23 hours.	24	39·6	5·2	·8	2·1	trace	
24 hours.	24	42	3·8	1·5	2·1	trace	
25 hours.	23·8	39·4	11·3	1·8	2·4	trace	
26 hours.	24·2	36·4	6·3	·5	2·7	trace	
27 hours.	24·6	37·8	8·6	......	1	trace	

The following is the result of another experiment made in a similar furnace, the amount of sulphur alone being ascertained, including that in the sulphates which were formed.

Time.	Put in.	1 hour.	2 hours.	3 hours.	4 hours.	5 hours.	6 hours.	7 hours.	8 hours.	9 hours.	10 hours.	11 hours.	12 hours.
Sulphur..	30·6	30·2	29·5	25·7	21·4	21·3	17·9	19·9	21·8	20·3	20·4	19·2	11·4

Time.	13 hours.	14 hours.	15 hours.	16 hours.	17 hours.	18 hours.	20 hours.	21 hours.	22 hours.	23 hours.	24 hours.	25 hours.
Sulphur ..	21·1	10·5	10·5	9·2	3·5	5·9	3·4	6·2	4·1	4·7	3·8	1·7

These results speak for themselves. Although the actions and reactions in the last trials seem still to partake of that intermitting character, they are not so great, owing to the air being less loaded with sulphur, and more speedily removed. Less sulphates are also formed, which when present retard calcination. In sixteen hours with the three-bed furnace there was only a loss of one-fifth of the sulphur; in the above experiments two-thirds were evolved in the same time.

In all these experiments the powder was moist when put into the furnace, which greatly facilitated the formation of sulphuric acid. A furnace was constructed with a close chamber on the top, in which the powder was dried previous to being let into the calcining chamber. The first trial made was with regulus crushed into powder; it contained when lowered into the calcining bed 16·5 per cent. of sulphur. Samples were taken out every four hours and tested for sulphur and sulphuric acid, with the following results :—

	4 hours.	8 hours.	12 hours.	16 hours.	20 hours.	24 hours.
16·5	12·6	3·6	3·8	5·5	4·2	3·5

There was no sulphuric acid formed. It will be observed that eight hours were as effective as the twenty-four hours. We have often met with similar results, no doubt arising from the want of a proper heat and a regular current of air. The next trial was with powder made as described, in the water-tanks; it contained when dry 22·6 per cent. of sulphur. Samples taken out every four hours and tested for sulphuric acid and sulphur gave—

		4 hours.	8 hours.	12 hours.	16 hours.	20 hours.	24 hours.	28 hours.
Sulphur	22·6	14·2	5·4	5·0	2·0	1·3	1·8	trace
Sulphuric acid...	1·4	1·5	2·4	2·4	2·1	1·7	·3	·2

Here we observe a regular evolution of sulphur; and the results of many trials besides those now given, have shown that, when the calcination was done with care, twenty hours were often as effective as the twenty-eight in the above table.

We are now in a condition to offer a few remarks upon the nature of the action going on during calcination. When the powder is put into the furnace moist, there is little change in the first and second hour but the evaporation of water, after which sulphur is rapidly disengaged, mostly in an uncombined state. The flame or current from the fire travels circuitously within the hearth, carrying in its course the sulphurous vapours, which are deposited as it passes over cooler portions of the bed, and sulphates of iron and copper are there formed; hence the slowness and irregularity of the evolution of sulphur. Samples taken from different parts of the hearth previous to stirring, have often given a difference of more than double the amount of sulphur.

Sulphurous acid in a dry atmosphere is not converted into sulphuric acid by taking more oxygen from the air; but when moisture is present, small quantities are formed; and in the presence of peroxide of iron the formation of sulphuric acid is rapid; hence at the early part of the calcination sulphates are abundant. The peroxide of iron is converted into the magnetic oxide, the acid combining with a portion of the protoxide of iron, and the quantity of sulphate of iron is always large before the heat of the furnace becomes high, and often before any sulphate of copper exists. When sulphate of iron is decomposed by the intensity of the heat, there is always a simultaneous formation of sulphate of copper, probably from a mere transfer of the acid, the iron being converted into the state of peroxide. When the heat of the furnace is too high for the existence of sulphate of iron, and no moisture present, we nevertheless think that the oxygen of the peroxide of iron converts the sulphurous acid into sulphuric acid, which simultaneously combines with the oxide of copper.

These experiments point to the conclusion, that to obtain rapid and complete calcination, the stuff must be in powder, and subjected to a free current of air with a regular and increasing heat, and that it might be effected in not more than five or six hours. Various plans have been proposed to effect such a rapid calcination, such as a jet of steam blown over the surface of the matters calcining, and a current of hot air blown over the chamber by a blast, &c.; all have their merits as to the principle required, but there are practical considerations that have to be borne in mind in these applications. Whether calcining ore, coarse metal or powder, a quantity of the stuff is in the state of fine dust, which even under the mildest draft is carried away into the flue or culvert, which must be avoided as much as possible. Every attempt to increase the draft, either by more open fires, air-holes or blast, is followed by a greater loss of calcining matters. We have seen an increased draft lessen the time of calcination to one half, but the increase in the loss of stuff was more than compensated for the saving of time; so that improved drafts and steam-jets or blast must be kept within certain limits, or accompanied with some invention to prevent escape of light particles, which we believe within reach of practice.

The following are analyses of stuff obtained from flues and culverts leading from the calcining furnaces.

The first is from the culvert leading from a coarse metal calciner: sample taken about 20 feet from the furnace. The substance was in the state of powder.

Insoluble in acids	50·4
Peroxide of iron	7·8
Oxide of copper	5·4
Sulphur	2·3
Sulphuric acid	5·4
Lime	11·7
Arsenic, tin, antimony, &c.	16·7
	99·7

Matter taken from the flue of a calciner used for powder close to the furnace before it entered the culvert, and where the deposit was greatest; this was in the form of a powder, and having been exposed had absorbed moisture.

Insoluble in acid	4·0
Oxide of copper	24·5
Protoxide of iron	22·5
Sulphuric acid	32·6
Soda	2·3
Lime	·3
Water	13·0
Antimony and arsenic	·6
	99·8

Some of the copper and iron were as sulphates. The next is from the culvert leading from the same powder calciner as the flue from which the samples last taken led from; taken about 28 feet from the furnace.

Insoluble in acids	42·8
Peroxide of iron	18·4
Oxide of copper	3·7
Sulphuric acid	7·5
Soda	8·0
Lime	1·6
Antimony and arsenic	13·6
Water	3·9
	99·5

The difference between this and that deposited in the flue of the same furnace is worthy of remark. In the former there is much sulphate of copper and iron, and but little of the more volatile matters; while in this last, taken at a greater distance from the furnace, where the heat is less, the more volatile matters are in greater quantity.

[To be continued.]

XXIX. *Mathematics and Mathematicians.   The Journals of the late* Reuben Burrow. *By* T. T. Wilkinson, *Esq., F.R.A.S.**

No. I.

OF all the non-academic geometers of the last half-century none appears to have been more distinguished than Mr. Reuben Burrow.   His name to mathematicians of the English school is almost as a household word; and as a geometer he may be said, without exaggeration, to have equalled any of his contemporaries, Dr. Stewart himself not excepted.   His early history is involved in much obscurity; nor had his career through life and his premature death been noticed beyond the immediate circle of his friends previously to the appearance of a posthumous "Memoir" of "that distinguished, elegant Geometer, and able Mathematician, Reuben Burrow," by his admirer the late J. H. Swale, in the Mechanics' Magazine for October 1850.   From this short sketch, which was no doubt originally prepared for Leybourn's Mathematical Repository, it appears that Mr. Burrow was born on the 30th of December 1747, at Hoberley near Shadwell, a village about five miles north-east of Leeds in Yorkshire.   His principal tutor in mathematics was Mr. Crooks, master of a commercial and mathematical school at Leeds, but in after life Mr. Burrow removed to Portsmouth and opened a school on his own account.   Subsequently we find him at Greenwich as Assistant Astronomer to Dr. Maskelyne, and in 1774 he was associated with him in Perthshire, instituting a series of observations and making surveys on the mountain Schiehallien, in order to determine its attraction.   He was afterwards appointed to the office of "Teacher of Mathematics at the Drawing Room in the Tower;" but in 1782, at the instance of Colonel Henry Watson, he relinquished all his engagements in this country and went to India, where he died at Buxor on the 7th of June 1792, in the 45th year of his age.

His connexion with "Thomas Carnan, in St. Paul's Church Yard, who dispossessed the Stationers' Company of the exclusive privilege of printing Almanacks, which they had unjustly monopolised for 170 years," and the contents of his papers in the noted Lady's and Gentleman's Diary, published by them in opposition to the supposed rights of the Stationers' Company, have already been noticed at considerable length in the Mechanics' Magazine, vol. li.; nor need we stay to enumerate his Essays on Friction, the Hindoo knowledge of the Binomial Theorem, &c., in the early volumes of the Asiatic Researches, since most of these have subsequently been transferred to the

* Communicated by the Author.

pages of our mathematical periodicals, and are well known to mathematicians as being the best dissertations on their respective subjects at that time extant.   Among his separate publications, the Restoration of the Treatise of Apollonius on Inclinations is acknowledged to be a most elegant and able work, and furnishes a remarkable contrast to the attempt of Dr. Horsley on the same subject; whilst his Theory of Projectiles in the same tract is no less distinguished for elegance and originality.   It does not appear, however, that his superiority in geometry in any way enabled him to subdue his natural irritability, for at various periods of his career he had differences with almost every person of eminence with whom he came in contact.   He was one of those " *whose special education was in advance of his general;* " and hence, as Mr. Swale remarks, although " his *heart* was good, his habits had been formed by casualty and the necessity of the moment rather than by design and the prudent hand of a master." His antipathy to Dr. Hutton makes its appearance in various portions of his journals, and also in his Diary ;—he differed in opinion from Dr. Maskelyne respecting the mode of conducting the observations at Schiehallien, and left his service about the close of the survey ; and a curious chapter of his correspondence with the Board of Ordnance and the Duke of Richmond respecting additional pay for " making a survey of the Essex and Sussex coasts in the year 1777," may be seen by a reference to pp. 324–329, vol. lv. of the Mechanics' Magazine.

Since the preparation of that paper by " Senex," several of Mr. Burrow's private journals have fallen into my possession. They contain many curious entries relative to his outfit, passage, and residence in India, with occasionally a slight sketch of some mathematician or mathematical inquiry ; and as there can scarcely exist a doubt respecting their authenticity and general correctness, I have thought it would not be uninteresting to the lovers of mathematical history if a portion of the most important of these were selected for preservation in the pages of this Magazine, accompanied by such remarks as may serve to render the extracts intelligible.

The first journal is endorsed " Reuben Burrow, No. 11, New Square in the Minories, London, 1775," and commences on Saturday, August 5, with a reference to a former volume, which, however, is not now known to exist.

" Sunday, August 6, 1775. I heard two or three days ago that Heath had been at law about an estate of £50 per annum and had got it. . . . . . Lloyd told me that Mr. Benjamin Robins was a middle-sized man very much marked with the small-pox."

Most probably the person here alluded to was the so-called

Captain Heath, for some time editor of the Ladies' Diary for the Stationers' Company, and subsequently of the Palladium on his own responsibility. His opposition to Simpson, Rollinson and Turner (afterwards editors of the Diary, the Mathematician, and the Mathematical Exercises, and who at times corrected his errors), was inveterate; nor was his violent language towards them at all justifiable under any circumstances. Its grossness ultimately led to his dismissal from the editorship of the Diary, which was immediately conferred upon Simpson. Mr. Emerson is said to have assisted Heath in his attacks upon these able mathematicians, in consequence of some loose expressions which one of them was reported to have uttered respecting Emerson's attainments in geometry. Mr. Robins is well known as the author of many able works on Gunnery, Fluxions, Fortification, &c., which formed the principal text-books on those subjects for a long period. He was a candidate on two occasions for a Professorship at the Royal Military Academy at Woolwich; "but the interest," says Dr. Hutton, "which Mr. Muller had with the Board of Ordnance carried the election in his own favour. On this disappointment, Mr. Robins, indignant at the affront, determined to show them, and the world, by his military publications, what sort of man he was that they had rejected." His personal appearance does not seem to have been hitherto described.

"Monday, August 7th. I called upon Mr. Robertson, and he told me that the Stationers' Company had allowed Hutton £100 a year on condition of his not making any Almanacks for any persons except themselves. *Hutton, by the bye, does not know how to make an Almanack.*

"I gave Mr. Keech, or rather he took, a copy of the Times of High Water at London Bridge.

"Mr. Brown, at the Tower, had *five* Tables this day, which I had calculated for his Book on Gunnery.

"Put these in the Diary.

"1. Some Theorems on Maxima and Minima.

"2. Method of finding the Time and Azimuth.

"3. Method of placing Sector in the Meridian.

"4. Lalande's Table of the Solar System.

"5. To find the length of a Parabolic Arc."

The Mr. Robertson here noticed was John Robertson, F.R.S., author of a Complete Treatise of Mensuration, Elements of Navigation, &c. He was successively Master of the Royal Mathematical School in Christ's Hospital, and Clerk and Librarian to the Royal Society. Mr. Joseph Keech was a clerk and attorney in the Lord Mayor's Court Office; and from some original correspondence between himself and the Revs. John Lawson and

William Crakelt, now in my possession, I gather that he was the editor of the mathematical department of the London Magazine, a periodical of considerable repute towards the close of the last century. His papers in the Mathematical Miscellany, the Geometrical Delights, &c., prove him to have been a mathematician and astronomer of no mean abilities. Mr. Hugh Brown was the translator of Euler's edition of Robins's Gunnery, which, with the assistance of Mr. Burrow, as is now evident, he improved considerably. The publication of the first number of Carnan's Lady's and Gentleman's Diary was now determined upon ; but of the articles named in the memorandum, only the second, third, and fifth were inserted. The places of the others were supplied by some valuable geometrical discussions, which are the more remarkable on account of their *systematic* character. Mr. Burrow in this respect set the example of attempting the classification of geometrical inquiries, which it would have been well to have followed out more fully than has been done in most of our mathematical periodicals.

"Tuesday, August 8. I called this morning on Major Watson and asked him about getting into the South Sea House, but he had no acquaintance with any of the Directors. I told him about the Stationers' Company having allowed Hutton £100 a year, and he said he would call on Mr. Hett to inform him that he had heard of a new publication which he thought would interfere with their Diary :—*this he intended as a trial whether they would think it worth their while to stop my mouth as well as Hutton's, and in the same manner.* I met poor John Clarke, of Lincoln, who gave me an enigma. He had given in a plan to Lord North of making a settlement in some part of South America, near the river Amazon, on some country unclaimed by any European power ; but he had got nothing for it, and was almost starving ; he had not even a shilling in the world. I told his case to Major Watson in order to interest him in it if I could, but he did not seem to take the notice of it I could have wished ; he said, however, he should be glad to see his plan of the American affair."

Major, afterwards Colonel Watson, of the Royal Engineers, was a pupil of the celebrated Thomas Simpson, and became possessed of all his papers after his decease. He appears to have been the steady friend of Mr. Burrow, whom he employed in measuring an arc of the meridian in India, an account of which was drawn up by Professor Dalby of the Royal Military College. The intended *hint* to the Stationers' Company is characteristic of the *motives* which led to the destruction of their lucrative monopoly, and confirms Mr. Carnan's stereotyped announcement on the successive title-pages of this now celebrated almanac. *Mr,*

Burrow, it seems, would have had no objection to £100 a year from the Stationers' Company ! Poor Clarke's lot has been too frequently realized by unpatronized students : his enigma occurs in the first number of the Diary, and he continued to write "from Lincoln" during the whole of the time that it remained under Mr. Burrow's management.

"Sunday, Aug. 13. Lent Mr. Keech, Harris's Arrangement and Ghetaldus.

"Tuesday, Aug. 22. I finished two very difficult Problems in the Maxima and Minima, and did the greatest part of the Problem of finding the length of a Parabolic Curve generally by the Method of Equidistant Ordinates; but as I had some doubt about the truth of it, I gave it up, intending to get Major Watson to write to Landen about it, or else to write myself to Mr. Crakelt. I then called upon Mr. Robertson, and found that Francis Maseres, Esq. had left his book upon the Negative Sign there as a present for me. I had a good deal of talk with Mr. Robertson, and staid supper. He told me that Mr. William Jones wrote that History of Logarithms prefixed to Dodson's Tables of the Antilogarithmic Canon, and that Dodson wrote such a confused and odd style that there was neither head nor tail in it. He (Robertson) drew up the examples himself. He also gave me the History of the Mathematical Repository as follows. Mr. Robertson having taught General Conway mathematics, who was then only a Colonel, after he was elected Member of Parliament he called on Mr. Robertson, and told him that since his place in the House hindered his further attendance to mathematical subjects he should give them up, but at the same time should be glad to have those papers which he had learnt copied over. Mr. Robertson not having either time or inclination to do this himself, applied to Mr. Dodson, who again employed another person to copy the papers, but at the same time took a copy for himself. Mr. Robertson did not know this for a long time, not until he began to think on the scheme of publishing a Mathematical Repository himself, the first volume of which was to contain Algebraical questions, and the second Geometrical. He proposed this to Dodson, who readily accepted the offer of joining with him, and afterwards mentioned the plan to Mr. Jones, who was opposed to the project on account of the probability that Robertson would publish some of the methods which Jones had taught him, and which he (Jones) might probably afterwards publish himself. Mr. Robertson on this set the affair aside; but Dodson went on with it, and the greatest part of the Questions in the first volume, at least 200 of them, were copied from Mr. Robertson's papers. . . . . Dr. Hooke almost starved himself to death. Mr. Jones told Mr. Robertson that

when a medical man of Hooke's acquaintance accidentally called upon him, he found him absolutely dying of want; and on his leaving the house he gave an old woman who waited upon Hooke some money to buy him a fowl and some wine. Hooke happened to see him do it; and when the gentleman was gone, he took the money from the woman and would not buy anything with it."

Mr. James Dodson was also a Master of the Mathematical School in Christ's Hospital, and published his Antilogarithmic Canon, "a work unique of its kind," in 1742;—the Mathematical Repository is comprised in three volumes, which were issued in 1748, 1753, and 1755 respectively. The first volume is almost wholly composed of algebraical questions, most of which have since found their way into our regular treatises; the volume of Geometrical Problems was abandoned, owing probably to the fact that Dodson had not paid much attention to the geometrical analysis of the ancients. There is enough of originality and ability, however, on the Doctrines of Series and Annuities in the second and third volumes, to maintain his character as an "ingenious and very industrious mathematician." After Mr. Jones's death most of his papers fell into the hands of Mr. Robertson, so that he had, no doubt, good authority for his statements respecting the History of Logarithms. Dr. Hooke's penurious habits have been noticed by Hutton; but since he was some years in a "wasting condition," owing to "increasing illness," his repugnance to proper diet may have arisen from this cause rather than from a desire to amass wealth.

"August 24. Mr. Brown had most of his Tables home, and he paid me three guineas in part. . . . . . I called at the Swan's Nest, where Keech, Sanderson and myself had a dispute about my questions on Maxima and Minima.

"August 25. Mr. Robertson sent for me to ask if I would undertake to calculate an Eclipse mentioned by Herodotus, which happened about the time of the Expedition of Xerxes to Greece, while he was at Sardis. It was for Dr. Priestley, but as Mr. Robertson was leaving town he had not time to do it, so I undertook it. Kepler mentions it in page 291 of his *Astronomia pars Opticæ*, but seems doubtful whether there was an Eclipse or not.

"Mr. Robertson told me that as Nourse wanted a new edition of Simpson's Fluxions, he would recommend me to correct the proof sheets. *Mem.* There is an error about a series in the second volume that Mr. Todd has corrected, and there are also some few errors mentioned in the Miscellaneous Tracts to be corrected. Also, if Nourse will allow any notes to be added, there is in the London Magazine a few *general* Theorems of

which Simpson has only given particular cases;—these are done by Mr. George Brown.

"August 26. Read about Eclipses in the following works.

"On Projection in Turner's Exercises.

"Some in Lalande's Abridgement.

"September 3. I did a couple of Questions which were proposed by a Quaker, who had requested Mr. Atkinson, of the Mayor's Court Office, to give them to Mr. Keech and myself.

" *Question* 1.

"There are two given points *d* and *n,* and *dqn* is a string of a given length fastened to the given points:—required the point *q* where a weight W would rest suspended from the cord.

" *Construction.*

"With the centre *d* and distance *d*B = the length of the cord, describe the arc AB; draw the horizontal line *dp,* and perpendicular to it draw *n*B cutting the arc in B:—join B*d,* and make the < B*nq* = < *d*B*n*; then *nq* cuts *d*B in the point of suspension required.
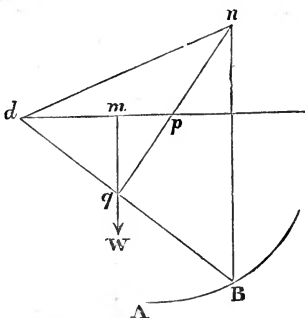
" *Demonstration.*

"*Lemma.* If the weight W be in equilibrio, it does not signify in what part of the line *nq,* the nail that fastens the cord, is put; but it is evident that when the nail falls in the horizontal line, as at *p,* then *dm* = *mp,* and consequently it must always be so.

"Now *qn* = *q*B, and ∴ < *dqn* = < *qn*B + < *qB n* = 2*qn*B, ∴ < *qn*B = $\frac{1}{2}$ < *dqn.* But *qm* is parallel to *n*B, and ∴ < *mqp* = *qn*B. Whence < *dqm* = < *mqp,* and consequently *dm* = *mp,* agreeably to the Lemma. Q. E. D.

" *Question* 2.

"What velocity must a ball suspended by a string have given to it so as to make it ascend to a horizontal position?

" *Solution.*

"Huyghens demonstrates that bodies falling from the same heights acquire the same velocities, and that pendulous bodies rise to the same heights from which they fall. Hence it is evi-

dent that the velocities which bodies must have to rise to *any* heights must be the same that the bodies would acquire by falling through the versed sines of the arcs they describe. Consequently for a ball suspended by a string to rise till it become horizontal, it must have a velocity at its lowest point equal to what it would acquire by falling through the length of the string of suspension.

" September 4. I called on Major Watson, who was just going out to Sion Hill to see Mr. Robinson, Lord North's Secretary, and he asked me to go with him ; accordingly we went together in a chaise, but he was not at home. We then went to Sir Charles Frederick's, but he was also from home. We then went to Pickett's at the Foundry near Blackfriars Bridge, and to Dr. Irwin's ; he who made salt-water fresh. I then went with Major Watson home and dined with him. After six o'clock I came to Mr. Todd's in Smithfield, and he and I went to see Mr. Wales ; we stayed supper, and he showed us the weapons, &c. from Otaheite and New Zealand.

" September 6. I called on Lord Charles Cavendish in the forenoon. He suspected that some one had wished to deter me from making calculations for him ; so after some conversation I was to make out a Table of Aberrations for him ; to finish the first Table about the Nutation and to calculate one month, viz. December 1769. In my way home I called on Mr. Robertson, but he was not come out of the country, so I left the result of the Eclipse with his daughter and dined there. On my way home I met Mr. Atkinson in Fetter Lane, who told me the Quaker was very well pleased with my solutions to his questions."

Mr. George Sanderson was a "tailor in Doctors Commons," a member and some time President of the Mathematical Society of Spitalfields, which was established by Mr. Joseph Middleton in 1717. He was one of the ablest geometers of the period, and his contributions to the Diaries and the London Magazine are both numerous and important. Whether there *was* an eclipse at the time mentioned by Herodotus appears to be still undetermined. The subject has been considered by some of our ablest astronomers, but their results are not conclusive. I have not been able to meet with Dr. Priestley's statements, on the authority of Mr. Burrow, nor is the nature of his conclusions anywhere stated in his journals :—an interesting summary of the principal results may be seen under the article 'Alyattes' in the first volume of the Penny Cyclopædia. Neither the Journals nor the Diary contains any further notice of the new edition of Simpson's Fluxions ; hence the probability is that Mr. Robertson was either unsuccessful in his application to Nourse, or that the edition was abandoned. The solutions of the Quaker's questions appear

worthy of preservation, inasmuch as the first is of some importance at the present time, and has found its way into several of our best treatises : a solution agreeing in most respects with that by Mr. Burrow is given by Professor Young in pp. 24, 25 of his useful work on the Elements of Mechanics. Mr. Thomas Todd was a " commission agent in West Smithfield," whence he occasionally wrote in the periodicals under the signature "Westsmithfieldiensis :" he appears to have delighted in a public dispute, and is principally remarkable for his protracted and somewhat violent controversies respecting the principles upon which the equation of payments in arithmetic ought to be based. Mr. William Wales is well known as the astronomer who accompanied Captain Cook in his second and third voyages. He was the author of several works on general literature, and also published a Restoration of Apollonius on Determinate Section in conjunction with his friend the Rev. John Lawson. An interesting series of letters which passed between these two able geometers, mostly relating to the Restoration of Apollonius, has recently fallen into my possession, from which it is evident that Mr. Wales was not inattentive to the study of pure geometry, even when engaged in making observations on the transit of Venus on the dreary shores of Hudson's Bay. Mr. Burrow appears to have completed Lord Charles Cavendish's requirements during the following month, for on "October 17" it is noted that he "called on Lord Cavendish and am to carry him the things copied out next Tuesday at 10 o'clock." This nobleman was the father of Henry Cavendish, Esq., whose name is so intimately connected with chemistry and the density of the earth ; but it is not probably so well known that the elder Cavendish interested himself so much with the niceties of physical astronomy.

Burnley, Lancashire,
January 13, 1853.

[To be continued.]

XXX. *On the Temperatures of Conductors of Electrical Currents. By* RICHARD ADIE, *Esq., Liverpool*[*].

IN my communication in the January Number of this Journal, I took occasion to show that galvanic currents had the power of rearranging the temperatures of the conductors after the cessation of the currents had allowed the temperatures to become equalized. I have now to return to the subject, to prove that the time required for an electrical current to alter the temperature of a joint is shorter than if the same joint is left to change by radiation only. I have also to offer evidence
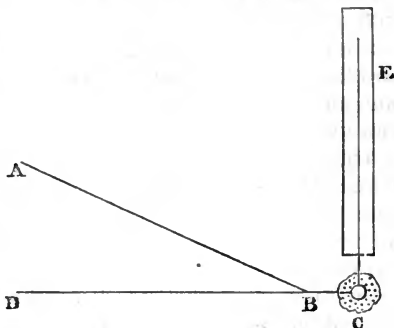
* Communicated by the Author.

of the influence of the temperature of the dilute acid solution used to excite the battery, over the temperature of the joint.

To save reference to the diagram of the thermo-electric couple used in the former experiments I repeat it here.

AB a bar of antimony.

DB a bar of bismuth soldered to the antimony at B.

C a thermometer-bulb applied to a cavity in the point of the bismuth bar and enveloped in a thin covering of cotton wool. A Smee's battery excited by a very weak acid solution was used to send galvanic electricity through the joint; and in order to get three uniform experiments where the temperature of the joint was raised in each case in like manner, the galvanic current was made to pass in the direction ABD, where it had to enter the bismuth at the joint and thus heat it. In from $6\frac{1}{2}$ to 7 minutes the thermometer CE rose 4°. The question for trial was the time the thermometer took to fall 4° when a current was passed through the joint in the direction to bring it down, and when left to cool of itself.

1st experiment. With the galvanic current reversed after heating, and passed in the direction DBA, where it escaped from the bismuth at the joint; the time occupied in cooling 4° was . . . . $6\frac{1}{2}$ minutes.

2nd experiment. With no galvanic current circulating, but with the wet plates of the battery in metallic contact with the couple DBA; the time occupied in cooling 4° was   12   ...

3rd experiment. The couple DBA disconnected from the wet plates; the time occupied in cooling 4° was 20   ...

These results clearly show the power of an electrical current for arranging temperatures; but they only relate to one joint at B, while the couple ABD, when connected with a Smee's battery, has five principal joints, besides minor ones in the copper, silver, zinc, and brass used in the construction of the battery. The chief joints are at A, B, D, and in the acid solution where in contact with the silver and zinc plates : there can be no doubt

that the galvanic current has at each of those joints a fixed temperature to sustain, the same as it has been found to have at B, and consequently the temperature of the different parts of the circuit must act and react on one another,—a point which I now sought to determine by experiment, for I could see that by means of a cold acid solution a joint might be kept for some time below the temperature of the atmosphere, and that if I could get a freezing mixture to form a battery, a joint might be greatly cooled by the electricity derived from its action; in other words, that galvanic batteries could furnish electrical currents at different normal temperatures.

In order to ascertain the effect of the temperature of the acidulated water on that of the joint, I made three experiments: the first with the solution, the apparatus, and atmosphere all of one temperature; the second with the solution colder than the atmosphere and apparatus; the third with the temperature of the solution above that of the air. The acid solution required being very weak, in order to improve its conducting power, it was nearly saturated with sulphate of zinc; and its volume was large in order that the temperature might not change too rapidly.

1st *Experiment.*

Temperature of atmosphere, solution and joint, at the commencement of the circulation of the current				46
...	after 15 minutes' action of	solution	. .	47
		joint	. . .	45·8
		atmosphere	.	46
...	... 30 ...	solution	. .	48
		joint	. . .	46
...	... 60 ...	solution	. .	50
		joint	. . .	46·2
...	... 110 ...	solution	. .	51
		joint	. . .	46·2

2nd *Experiment.*

Temperature of apparatus and atmosphere at the commencement of the experiment	. . .	45
...	of dilute acid solution	38
...	of joint after the current had circulated 15 minutes in the direction DBA . .	43·8

3rd *Experiment.*

Temperature of apparatus and atmosphere at the commencement of the experiment		46
...	of dilute acid solution	55
...	of joint after 15 minutes' action . . .	46·2

In the second experiment, where the joint was cooled 1°·2 below the temperature of the atmosphere by the electricity furnished by the action of a cold acid solution, on the interruption of the current the temperature of the joint rose to 46°, or one degree above that of the atmosphere. The second and third experiments I repeated many times in order to satisfy myself of their accuracy. In these experiments there is no effect on the temperature of the joint which may not be accounted for by the requirements of the battery for caloric, and the power which an electrical current possesses of arranging temperatures in a conductor composed of various materials. In the second experiment it may be held that the cold water at 38° would, without the aid of any galvanic current, reduce the temperature of the apparatus below that of the atmosphere : this is an effect distinct from the one noted ; for in the experiment in question there are portions of the metallic circuit intervening between the joint B and the cold solution which are above the temperature of the atmosphere ; for example, where the electricity enters the bismuth, the temperature for a current of this kind is 4° higher than where it quits it at B, or 2°·8 above the temperature of the atmosphere.

The normal temperature of the galvanic electricity derived from the action of a battery is a point which the heat of the solution and atmosphere cannot altogether govern ; for there are the chemical actions absorbing and evolving caloric, which must also have their influence. In the Smee's battery I think it extremely probable that the electricity comes from it at a temperature lower than that of the apparatus or atmosphere when these are uniform, as in the first experiment.

The following repetitions of the second experiment show that a cold water cell did not reduce the temperature like a cold acid solution.

		Pure water.	Acid and water.
Temperature of apparatus and atmosphere	.	45°	45°
...	solutions	38	38
...	joint after 15 minutes' action	44·8	43

The small reduction in the temperature of the joint in the pure water experiment is most probably due to the current given by the water. I would have preferred the experiment without this current, but to do so I must have destroyed a part of the metallic contact of the plates in the cold water with the joint. The difference, however, between the two experiments of 1°·8 is an ample proof of the galvanic current from a cold solution reducing the temperature of the joint, independently of the effect of conduction from the joint to the water.

On reflecting on the results of the experiments I have now submitted to your readers in connexion with this subject, the view I take of the supposed law of production of cold by electricity is, that it arises in the action of a battery where the heat developed by resistance to conduction is at its minimum; *so that the caloric wanted for the chemical actions going on is exhibited in the joint through the power of a galvanic current of bringing to a state of equilibrium the temperatures in its circuit.* In Smee's battery zinc is passing from a solid to a fluid state, and one of the elements of water is assuming a gaseous form.

XXXI. *On Systems in which Right and Left-handed Oscillations are not executed alike.* By M. A. Bravais[*].

THE remarkable rotation of the plane of oscillation of a pendulum observed by M. Foucault presents a similitude with the rotation of the plane of polarization of a homogeneous luminous ray, passing along the axis of a crystal of quartz, which appears to me to be worthy of the attention of physicists. The rotative phænomenon of quartz has hitherto had no analogue among determinate natural motions; but in the beautiful experiment of Foucault such an analogue has been discovered.

The only difference is, that in quartz, according to the theory of undulation, every molecule of æther situate in the axis of the ray imparts to the next following molecule its own motion in the direction of propagation, and returns to a state of rest, until a fresh shock sets it again in motion; while in the case of a pendulum the oscillating mass moves isolated and as a whole, without imparting its motion to neighbouring particles.

I propose examining the case where the oscillation is circular. Fresnel has shown that a circular vibration may be regarded as the equivalent of two plane vibrations of the same amplitude, but whose phases are 90° different. Starting from this, he has proved that if the planes of the two component vibrations rotate with a certain common velocity, the propagation of the corresponding circular motion will be altered, plus or minus, according as the rotation is right- or left-handed. From this he concluded that these two kinds of vibration cannot propagate themselves with the same velocity,—a result which he confirmed by direct experiment.

The same conclusion being applied to the lineal and very small vibrations of a pendulum, we observe that when the motion is circular, as in the case of the conical pendulum, the duration of a vibration cannot be the same when the motion is in the

same direction as the rotation of the plane in which the lineal vibrations are executed, that is from east to west, as when these directions are opposed to each other. In the former case the duration is shortened, in the latter case augmented. To the circular oscillation of a pendulum it is therefore necessary to apply a correction, the sign of which depends upon the sense in which the vibration is executed.

We arrive at the same result when we discuss *à priori* the effect of the rotatory motion of the earth upon the duration of the circular oscillation of a pendulum.

If t denote the duration of a *complete* lineal vibration of a pendulum (two seconds in the case of a seconds' pendulum), and T the duration of a sidereal day, then the angle of rotation of the earth during the time t, at the poles, is $2\pi \dfrac{t}{\mathrm{T}}$, and in the latitude λ it is $2\pi \dfrac{t}{\mathrm{T}} \sin \lambda$.

If the same pendulum describes a circle, the apparent rotation during the time t, when the direction of rotation is from east to west, is

$$2\pi + 2\pi \frac{t}{\mathrm{T}} \sin \lambda,$$

and when the direction is from west to east,

$$2\pi - 2\pi \frac{t}{\mathrm{T}} \sin \lambda.$$

The change of the angle by the rotation of the earth during the time T amounts therefore to $\pm 2\pi \sin \lambda$. The difference of phase between the two motions left and right, after twenty-four hours sidereal time, is therefore equal to the product of two complete oscillations into the sine of the latitude. From this it follows, that the conical seconds' pendulum, when it rotates from east to west at Paris, moves about three seconds quicker than when the rotation is from west to east, a quantity which far exceeds that which astronomers would think of neglecting.

This difference would be still greater in the case of long pendulums. With the pendulum of 11 metres suspended by M. Foucault in the Paris observatory, it would amount to at least ten seconds daily.

It is conceivable that this inequality might be proved in a direct manner, if two isochronous pendulums were set swinging simultaneously in opposite directions, so that their coincidences on one and the same diameter might be observed; for the rotation of this diameter must be the same as that of the plane of oscillation of a pendulum vibrating in a plane.

The oscillations being always assumed as very small, what

has been said above regarding circular oscillations may be applied to elliptical ones, and the resistance of the air may be neglected; but in practice it might be necessary to take both this and the amplitude of the vibrations into account.

After the foregoing remarks, it is to be hoped that the ability possessed by certain substances, particularly those operated on by electro-magnetism, to cause the plane of polarization to rotate, and to transmit the right- and left-handed æther vibrations with unequal velocities, will find a more complete explanation.

XXXII. *On* Mr. Cayley's *Impromptu Demonstration of the Rule for determining at sight the Degree of any symmetrical Function of the Roots of an Equation expressed in terms of the Coefficients.* *By* J. J. SYLVESTER, *F.R.S.**

FOR a considerable time past, among the few cultivators of the higher algebra, a proposition relative to the theory of the symmetrical functions of the roots of an equation has been in private circulation, which, to say nothing of the important applications of which it has been found susceptible to the calculus of forms, merits (by reason of its extreme simplicity), although, strange to say, it has, I believe, not yet obtained, a place in elementary treatises on algebra. The proposition alluded to I have reason to think first came to be observed in connexion with my well-known formulæ for Sturm's auxiliary functions in terms of the roots given in this Magazine. The theorem is briefly as follows. If a, b, c, &c. be the roots of an equation

$$x^n + p_1 x^{n-1} + p_2 . x^{n-2} + \&c. = 0,$$

any symmetric function such as $\Sigma a^\alpha . b^\beta . c^\gamma \ldots$, where $\alpha, \beta, \gamma \ldots$ are positive integers arranged according to the order of their magnitudes in a descending (or, to speak more strictly, nonascending) order, when expressed as a function of the coefficients, will be made up of terms of the form $p_1^{\theta_1} . p_2^{\theta_2} p_3^{\theta_3} \ldots p^{\theta_k}$, such that $\theta_1 + \theta_2 + \theta_3 + \ldots + \theta_k$ will be equal to (α) for some terms, but will for no term exceed (α); α being, as above described, that one of the indices $\alpha, \beta, \gamma \ldots$ which is not less than any of the others.

I had prepared, and indeed despatched, a somewhat elaborate proof of this theorem for the Cambridge and Dublin Mathematical Journal; but on proceeding to explain my method to Mr. Cayley, elicited from that sagacious analyst the following excellent impromptu, which I think too valuable to be lost; and as it is now a twelvemonth or two since our conversation on the

subject took place, and the author has not cared to put it on record, I feel myself under an obligation so to do, the more so as it entirely supersedes the comparatively inelegant demonstration of my own which I had previously intended to publish.

The method rests essentially on the following well-known theorem given by Euler relative to the partition of numbers; to wit, that the number of ways of breaking up a number n into parts is the same, whether we impose the condition that the number of parts in any partitionment shall not exceed (m), or that the magnitude of any one of the parts shall not exceed (m). Of this rule more hereafter—for the present to its application to the matter in hand.

Since a, b, c ... are the roots of $x^n + p_1 x^{n-1} + \ldots$, we have

$$p_1 = a + b + c + \ldots$$
$$p_2 = ab + ac + bc + \ldots$$
$$p_3 = abc + abd + acd + \ldots$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

Let $\alpha + \beta + \gamma + \ldots = n$, none of the quantities α, β, γ ... being greater than m, but α, β, γ ... being otherwise arbitrary and capable of becoming equal to any extent *inter se*. Also let $\lambda + \mu + \nu + \ldots = n$, the number of quantities λ, μ, ν, &c. being never greater than (m), but the quantities themselves being otherwise arbitrary, and being capable of becoming equal to any extent *inter se*. By Euler's rule the number of systems α, β, γ ... is the same as of the systems λ, μ, ν, ..., say P for each. For any system λ, μ, ν ..., we shall have $p_\lambda . p_\mu . p_\nu \ldots$, by virtue of the equations above written, expressible as the sum of terms of the form $\Sigma a^\alpha . b^\beta . c^\gamma \ldots$; it may easily be made ostensible, that *all* the combinations of α, β, γ ... subject to the above prescribed conditions must come into evidence by giving λ, μ, ν... all the variations of which they admit; but this is also immediately obvious indirectly from the consideration, that were it otherwise, linear relations would subsist between the different values of $p_\lambda . p_\mu . p_\nu . \ldots$, which is obviously absurd. Hence, then, we shall be able to express the P quantities of the form $p_\lambda . p_\mu \ldots$ by means of linear functions of the P quantities $\Sigma a^\alpha . b^\beta . c^\gamma \ldots$; and conversely, by solving the linear equations thus arising, the P quantities $\Sigma a^\alpha . b^\beta . c^\gamma \ldots$ may be expressed in terms of the quantities $p_\lambda . p_\mu \ldots$; consequently $\Sigma a^m . b^\beta . c^\gamma \ldots$, where (m) is greater or not less than any of the quantities β, γ ..., will be expressible by means of combinations $p_\lambda . p_\mu \ldots$, where the number of coefficients $p_\lambda . p_\mu \ldots$ (any number of which may become

identical) is for some of the combinations as great as, but for none of the combinations greater than (*m*), as was to be proved. It will of course be seen that, for the purposes of the demonstration above given, it would have been sufficient to have been able to assume that the number of partitions, when the greatest part is not allowed to exceed (*m*), is not *greater* than the number of partitions when the number of parts in any one partitionment does not exceed (*m*). The equality of these two numbers would then evince itself in the course of the demonstration as a consequence of this assumption.

A word now as to Euler's beautiful law upon which the above demonstration is based.

A corollary from it, obtained by subtracting the equation which it gives when the limiting number is taken (*m* − 1) from the equation which it gives when the limiting number is (*m*), will be the following proposition. The number of modes of partitioning (*n*) into (*m*) parts is equal to the number of modes of partitioning (*n*) into parts, one of which is always *m*, and the others (*m*) or less than (*m*). This proposition was mentioned to me by Mr. N. M. Ferrers*, whose demonstration of it (probably not different from that of Euler's for the other proposition, of which it may be viewed as a corollary) is so simple and instructive, that I am sure every logician will be delighted to meet with it here or elsewhere. It affords a most admirable example of that rather uncommon kind of reasoning whereby two abstract integers are proved to be equal indirectly, by showing that neither can be greater than the other.

If there be a group of A's and a group of B's, and every (A) can be shown to produce a (B), and every (B) can be shown to produce an (A), no matter whether the (A) producing a B is the same as, or different from, the A produced by that B, it is obvious that the number of A's cannot exceed that of the B's, nor of the B's that of the A's, and the two numbers will *therefore* be equal.

Take any such grouping as 3, 3, 2, 1, say A. This may be written as

$$1, \quad 1, \quad 1$$
$$1, \quad 1, \quad 1$$
$$1, \quad 1$$
$$1,$$

and by reading off the columns as lines, may be transformed into

---

* I learn from Mr. Ferrers that this theorem was brought under his cognizance through a Cambridge examination paper set by Mr. Adams of Neptune notability.

the group

$$1, \quad 1, \quad 1, \quad 1$$
$$1, \quad 1, \quad 1$$
$$1, \quad 1$$

i. e. 4, 3, 2, say B.

In A the number of parts is 4. In B the greatest part is 4; the others might be (although they happen not in this particular instance to be) 4, but cannot be greater than 4. And so every A in which the number of parts is 4 will give rise to a B in which 4 is one of the parts, and every other part is 4 or less, and evidently (although, as above remarked, this is immaterial to the demonstration) every such B gives reciprocally the same A from which it is itself derived; hence the number of A's and B's is equal. This is the theorem which, for the sake of distinction, I have called the Corollary to Euler's. Euler's own is proved by the same diagram; for if we define A as a grouping where the number of parts *does not exceed* 4, we get a definition of B as a grouping where the greatest part does not exceed 4, and so in general. We see that this theorem may be varied also by affirming that the number of ways in which n may be broken up, so that there shall never be less than (m) parts, is the same as the number of ways in which it may be broken up into parts, the greatest of which in any one way is not less than (m). So, again, a similar diagram makes it apparent, that if we break up each of i numbers into parts so that the sum of the greatest parts shall not exceed (or be less than) m, the number of ways in which this can be done will be the same as the number of ways in which these i numbers can be simultaneously partitioned so that the total number of parts in any simultaneous partitionment shall never exceed (or never be less than) m; and doubtless an extensive range of analogous general theorems relative to the partitioning of numbers may be struck out by aid of the same diagram, by no means easily demonstrable unless this simple mode of conversion happen to be thought of, but in that event becoming intuitively apparent. This mode of conversion is precisely that (only applied to a more general state of things) whereby, in elementary arithmetic, it is established that (m) times (n) is the same as (n) times (m). A consideration of the process by which the mind satisfies itself of the universality of this law, has been always sufficient to convince me of the absurdity of ascribing to an inductive process the capacity of the human mind for forming general ideas concerning necessary relations.

7 New Square, Lincoln's Inn,
    January 28, 1853.

XXXIII. *On some anomalous cases of Electrical Decomposition. By* W. R. Grove, *F.R.S. &c.**

WOLLASTON showed, in a well-known experiment†, that when Franklinic electricity from the prime conductor of an ordinary electrical machine was made to pass from fine points of gold wire sealed into glass tubes and immersed in water, both positive and negative pole gave mixed oxyhydrogen gas, the exact proportions of which are not given. This experiment has been subsequently discussed by Ritchie‡, Faraday§, and others, but differing as the decomposition in this case does from electrolysis by the voltaic battery, I am not aware that it has been repeated on a large scale, or that anything more than a mere verification of the fact has been attempted.

The apparatus constructed by M. Ruhmkorff, which I have described in a paper in the Philosophical Magazine for December last, p. 500, having given me the means of procuring electricity of tension in quantity far exceeding that of the best electrical machine, and having my attention directed to Wollaston's experiment by the wires sealed in glass tubes which I used for my recent experiments, I determined to make some experiments similar to Wollaston's, but with the spark from the secondary coil instead of that from the electrical machine. I select the following from amongst many experiments made with this apparatus.

The terminals of the secondary coil consisted of two wires of platinum $\frac{1}{60}$th of an inch in diameter sealed into glass tubes. One of these coated wires was prepared on purpose for these experiments; and having been carefully sealed into a glass tube, the extremity was ground on a hone until the section of wire formed one surface with the glass.

The other coated wire had been similarly prepared, but had been used for some time for the experiments in attenuated gases, given in the paper to which I have alluded; and the extremity of the wire was worn beneath the surface of the glass, a circumstance which proved of some little importance‖.

The tubes containing the platinum wires were curved in form

* Communicated by the Author.
† Phil. Trans. 1801, p. 431. ‡ *Ibid.* 1832, p. 282. § *Ibid.* 1833, p. 23.
‖ This result is important in another point of view than that relating to the experiments in this paper, viz. it shows the transmission of matter in electrical discharges, and that even in the aurora borealis experiment, or passage of the spark in highly rarefied gas, solid matter, even such as the dense substance platinum, is given off; thus favouring the theory that the spark is ignited matter, and rendering the hypothesis of a fluid unnecessary for its explanation.

something resembling the letter Z (see fig. 1), the extremities immersed in a porcelain capsule containing the liquid to be experimented on, a graduated test-tube filled with the liquid being inverted on the extremity of each.

Fig. 1.
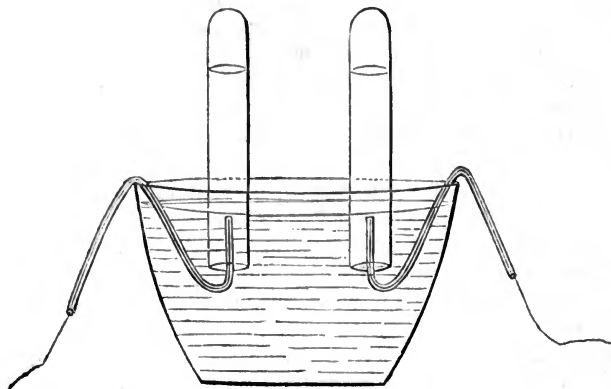
From the Z form of the terminals the extremities projected $\frac{3}{4}$ths of an inch into the test-tubes, so that no gas could possibly escape. At first I tried distilled water, which had been boiled immediately before the experiment to deprive it of air; I found, however, that from its want of conducting power, only an infinitesimal decomposition was produced; a few drops of sulphuric acid were added, and immediately a notable evolution of gas took place, a spark being visible at the end of the wire, which was worn beneath the surface of the glass whether this were positive or negative, and none on that which was even with the glass. This spark had a material influence on the relative quantity of gas given off by each terminal, the gas being always less from the spark-giving terminal than from that which did not yield a spark; and when both were constructed so as to give sparks, the quantities of gas were irregular, and bore no definite relation to each other. Thus in one experiment the gas from the negative was to that from the positive as 2·3 : 1 ; in another experiment the gas from the positive was to that from the negative as 2·5 : 1 ; frequently the quantities from each were nearly equal. It is obvious that when the wire was depressed below the surface of the glass, a film of gas must intervene between the wire and the liquid, across which film the spark passes. When the wire is even with the glass, the gas escapes more readily, and there are fewer or no sparks.

There is no doubt that as the bubble of gas forms on the point

of platinum, the spark recombines a portion of it, and the number of sparks, the size of the bubble preceding the spark, and the quantity recombined, would vary indefinitely in each different experiment, and even in the course of the same experiment.

I now give a selection of experiments from my note-book with the volumes and analyses of the gases evolved. The decomposition was arrested in each case when the larger volume of gas had reached to 0·3 c. i. The analyses were made by an eudiometer, which I have formerly described*, consisting of a loop of fine platinum wire sealed into a graduated glass tube, and heated or ignited by the voltaic battery :—

1st. The gas from the positive pole contracted to 0·4 of its original volume; the residue, mixed with half its volume of oxygen, detonated, leaving a minute residue, apparently nitrogen. The gas from negative pole, 2·3 times the volume of the positive contracted to 0·15 of its volume: the residue contracted very slightly on the addition of hydrogen.

2nd. Gas from positive, 2·5 volumes contracted to 0·7 of its volume.

From negative, 1 volume contracted to 0·4; the residues mixed contracted again to 0·7; and this residue, mixed with half its volume of oxygen, detonated, leaving only a very minute bubble.

3rd. Instead of two coated wires, one coated wire was employed, and the opposite terminal consisted of a strip of platinum foil, $\frac{1}{4}$ inch width and 4 inches length, exposing consequently, reckoning both sides, 2 square inches of surface.

This strip was attached to a platinum wire, which was sealed into a glass tube, the extremity of the platinum foil being $\frac{1}{2}$ an inch from the orifice of the tube, so that no gas could escape. (See fig. 2.)

Fig. 2.
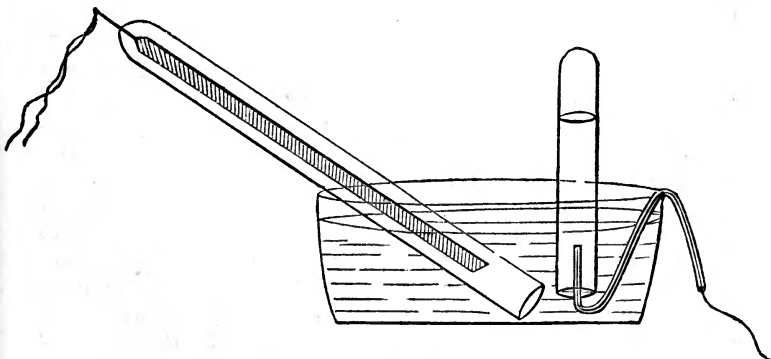
With the coated point positive, the platinum plate negative, 0·3 c. i. of gas was collected from the point; a minute bubble only appeared in the tube containing the strip or plate; the gas contracted in the eudiometer to 0·7 of its volume; the residue, mixed with half its volume of oxygen, detonated, leaving a very small residuum.

4th. The converse experiment, i. e. coated wire negative, plate positive, 0·3 c. i. of gas was collected from the negative coated wire, and, as in the former case, only a minute bubble appeared in the tube containing the plate, and far too minute for analysis; the gas from the negative point contracted to half its volume in the eudiometer; the residue was hydrogen as before.

5th. In the above experiments I had used from 2 to 3 oz. of distilled water, with a few drops of sulphuric acid added to improve its conducting power, without paying any particular attention to the exact quantity of acid which was mixed.

I now determined to try different degrees of dilution of the acid, and to note more accurately its proportion to the water. I first took the extreme case of dilution, using 3 oz. of distilled water, and merely touching it by a narrow glass rod which had been dipped into sulphuric acid.

The coated wire was the positive pole, the plate of platinum foil the negative; 0·3 c. i. was collected from the coated wire, and not a trace of gas was perceptible in the tube containing the plate. The gas contracted to one-half its volume in the eudiometer, and the residue, mixed with one-half its volume of oxygen, contracted to 0·3 of its original volume; the residue of this second contraction appeared to be nitrogen, and its greater quantity in this than in the previous experiment was doubtless due to the greater time which, from the inferior conducting power of the solution, was necessary for the decomposition, more air having thus entered into solution. In this experiment the liquid was exposed for twenty-four hours, during ten of which the discharges were kept up, while in the previous experiment less than an hour was sufficient, and the water had not cooled from its preliminary boiling before the experiment was completed.

6th. I next tried a strong solution of sulphuric acid, spec. grav. 1·23; the gas from the coated point, which was positive, contracted to 0·2 of its volume; the residue was hydrogen.

7th. I used a still stronger solution, spec. grav. 1·45, both points being coated; the volume from the negative pole was to that from the positive as 4·5 to 3·25; the gas from the negative pole contracted in the eudiometer to only 0·95 of its volume; the residue was pure *oxygen*; the gas from the positive pole contracted to 0·5; the residue was hydrogen. Here, to my surprise, there was, after the first contraction, a residuum of oxygen in the ratio of more than 2 volumes to 1 of the residuum

of hydrogen. The experiment was repeated with acid of spec. grav. 1·5; from the positive point 3·3 volumes were collected, from the negative 2·6; the gas from the negative pole contracted to 0·9 of its volume; the residuum was pure oxygen; that from the positive contracted to 0·4 of its volume, the residue being hydrogen.·

8th. I now tried a saturated solution of sulphate of copper with a few drops of sulphuric acid added; a remarkable difference was shown in the conducting power of this solution as compared with the dilute acid; the contact breaker of the primary coil gave very minute sparks, unattended with the usual snapping noise; it was much as if the terminals of the secondary coil had been united by a metallic connexion; and the superiority of the conducting power of the solution of sulphate of copper in this experiment over dilute sulphuric acid was far greater than is shown in ordinary electrolysis by the voltaic battery. The coated point was positive, the plate negative; the gas from the positive point detonated, leaving a very slight residue; but on the interior of the collecting tube, near the point, was a deposit of pulverulent metallic copper. On the negative plate there was no deposit, and no gas was given off from it. Here the copper deposit was evidently a reduction occasioned by the excess of hydrogen, but there was no symptom of any effect produced by any equivalent to it of oxygen.

9th. With a solution of caustic potash the coated point, which was positive, gave a gas which contracted to 0·32 of its volume, the residue being hydrogen; the plate, which was negative, gave off a mere trace of gas.

10th. Two plates were used as the terminals of the secondary coil in dilute sulphuric acid, instead of a plate and a point. No gas was given off from either, though the experiment was continued for several hours.

11th. I could detect no difference in the solutions employed before and after the experiment; but as the sulphuric acid might be expected to mask the effect of any oxygenated compound which might result from the absorption of oxygen, which took place in all these experiments except those where the strong solutions of sulphuric acid were employed, and as pure distilled water would give no effect, I tried the effect of the spark passed through aqueous vapour. Some distilled water was boiled and placed under the receiver of an air-pump, with a coated point and plate of platinum enclosed in a tube filled with the liquid, and inverted; the point and plate communicating respectively with the upper and under plate of the receiver, and thence brought into metallic communication with the ends of the secondary coil.

The receiver was then exhausted, and the vapour which had formed at the top of the tube soon dilated sufficiently to expose the point and plate; the spark was then passed across the vapour, and a permanent increase in volume of the vapour was soon detected. The experiment was continued for a week, the apparatus working five hours each day; at the end of that time much of the gas formed had escaped into the receiver, and on letting in the air the water rose in the tube until a bubble of gas of 0·03 c. i. remained in the top; this was analysed, and proved to be pure hydrogen.

On the interior of the tube was a dark pulverulent deposit, far too minute in quantity for analysis, but which had evidently proceeded from the platinum.

The only possible mode in which I can account for this experiment is, that this deposit consisted of an oxide or peroxide of platinum; and to account for it thus, it must be assumed that platinum will decompose water by abstracting its oxygen.

12th. I have endeavoured, by working for a long time upon a limited quantity of water very slightly acidulated with sulphuric acid, to detect some compound which might be formed by the oxygen which disappeared.

I placed in a small tube 0·15 c. i. of distilled water, touched with a rod which had been smeared with sulphuric acid. I continued working on this for a fortnight, averaging five hours a day; and as the liquid diminished by evaporation, I added distilled water which had been recently boiled. No change could be detected in the liquid at the expiration of this period. It gave an acid reaction with test-paper, precipitated chloride of barium, and showed no bleaching properties, which I looked for as a result of absorbed oxygen.

I have used the expressions positive and negative for the terminals of the secondary coil which bore this relation to each other upon breaking contact of the primary coil, and as far as I could ascertain by direct experiment, there was no spark or decomposition on making contact; but although from the time which the magnetic coil takes to acquire its magnetism no spark is visible with this apparatus on making contact, possibly some slight decomposition might take place on making contact; this, however, though it is well to notice the point, is immaterial to the consideration of the results detailed in this paper, as, if the currents were in alternate directions, the proportions of the gases would be equally exceptional and anomalous.

I have made many variations of the above experiments, but it would be tedious to detail them. The following are the general results :—

1st. With distilled water containing a small quantity of sul-

phuric acid, there is always a notable excess of *hydrogen* ; in one case the oxygen was only one-fifteenth of the whole volume of gas evolved. With solution of potash a similar result takes place.

2nd. When the quantity of acid is increased to the point at which the electrolytic power of the solution is the best, the excess of hydrogen is less; and probably if the exact point of the solution were attained, there would be no excess.

3rd. When the solution is much stronger than that which makes the best electrolyte, there is a notable excess of *oxygen*.

4th. When the electrodes exceed a certain size, water conducts without any apparent signs of decomposition.

On making a search for any previous experiments on this subject, I find that Faraday (Phil. Trans. 1834, p. 91) has observed that when a strong solution of sulphuric acid, formed by mixing two measures of oil of vitriol with one of water, was subjected to *ordinary* electrolysis, a remarkable disappearance of *oxygen* took place. The strength of the acid he then employed would be very nearly that employed by me in experiment 7 ; but in my experiment, when the solution is subjected to decomposition by the spark from the secondary coil, exactly the converse effect takes place, and there is a notable disappearance of *hydrogen*.

Faraday attributes the disappearance of the oxygen to the formation of peroxide of hydrogen ; but in those of my experiments where *oxygen* disappeared, this could hardly be the case, as the gases are eliminated at a point of ignition at which this compound would be decomposed, and in many of my experiments the liquid was kept very nearly at the boiling-point.

I abstain at present from expressing any opinion as to the cause of these singular anomalies, and hope to make a further communication on the subject when I can resume and complete some experiments which I have in view, but which it will be some time before I can undertake.

XXXIV. *On the Dependence of the Heating Power of the Galvanic Current on the nature of the surrounding Gas. By* R. CLAUSIUS.

WE take the following from a paper published by M. Clausius in the 12th Number of Poggendorff's Annalen for 1852. It shows the attention which the remarkable experiment of Grove (Phil. Mag. vol. xxvii. p. 445) has attracted in Germany, and also the different opinions entertained as to the cause of the observed phænomena. In the circuit of a voltaic battery Grove introduced two perfectly similar pieces of platinum wire, coiled into

a screw form and enclosed in two small glass tubes which were filled, the one with oxygen and the other with hydrogen; the tubes were immersed in two similar vessels containing equal quantities of water, which served as a calorimeter. When the circuit was established, the wire immersed in the oxygen became white-hot, while that in the hydrogen exhibited no visible redness. At the same time the heat yielded by the wires raised the temperature of the surrounding water, that in which the hydrogen tube was immersed being elevated from 60° to 70°, and that which surrounded the oxygen tube, from 60° to 81°.

In a similar manner Grove compared other gases with hydrogen, and found the following numbers, which have been reduced by M. Clausius, who takes the quantity of heat yielded in each experiment by the hydrogen as unit :—

Gases in which the wire was immersed	Nitrogen.	Oxygen.	Carbonic acid.	Olefiant gas.	Hydrogen.
Quantity of heat yielded............	2·26	2·10	1·90	1·57	1

In publishing a translation of the first communication of Grove upon this subject, Poggendorff expressed the opinion[*] that the cooling of a wire heated by galvanic electricity in different gases follows *mutatis mutandis* the same laws that Dulong and Petit have established in the case of bodies heated in an ordinary manner, and according to which hydrogen possesses the greatest cooling capacity. This manner of explanation is objected to by J. Müller in his " Report on the latest Progress of Physics," p. 396 ; the last-mentioned experiment being regarded by him as unmistakeably opposed to such an explanation. He says :—" This experiment proves decidedly that the feebler glowing of the wire in hydrogen, with an equal strength of current, is not to be referred to the circumstance that the hydrogen abstracts the heat most quickly from its wire, for in this case the water surrounding the hydrogen tube must be most speedily heated. All the circumstances point to the conclusion, that in the wire surrounded by hydrogen the actual quantity of heat produced is less than in the other wires." Müller closes his report with the statement, that in his opinion the experiment stands " completely isolated and unaccounted for."

M. Clausius, however, can find no such difficulty attached to the subject. The different quantities of heat yielded up by the wire in different gases, in connexion with the dependence of the quantity of heat developed by the current upon the resistance of the wire, appear to him to be quite sufficient to explain the phæ-

nomenon. Müller, he believes, has overlooked the dependence last mentioned. The concluding sentence above cited, " All the circumstances point to the conclusion," &c., is not at all disputed, but is rather a necessary consequence of the more speedy abstraction of heat by the hydrogen ; for the colder wire presents a less resistance, and hence, as the current is the same in both wires, a less quantity of heat must be developed in the wire, which is preserved cool by its surrounding gas, than in the other.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 140.]

Jan. 13, 1853. A PAPER was read, entitled " Description of some species of the extinct genus *Nesodon*." By Prof. Owen, F.R.S.
The author commences by referring to a genus of extinct herbivorous mammals which he had founded in 1836, on certain fossil remains discovered in Patagonia, and which, from the insular disposition of the enamel folds characteristic of the molar teeth, he had called *Nesodon*. Subsequent transmissions of fossils from the same part of South America, by their discoverer, Capt. Sulivan, R.N., now enabled the author to define four species of the genus. The first which he describes is founded on a considerable portion of the cranium and the lower jaw, with the teeth, and is called *Nesodon ovinus*. After the requisite osteological details and comparisons the author proceeds to describe the three incisors, the canine, and five molar teeth, which are present on each side of both upper and lower jaws, and then enters upon an inquiry as to the nature and homologies of the grinding teeth. The result is to show that the first four molars belong, with the incisors and canines, to the deciduous series, and that the fifth molar is the first true molar of the permanent series ; the germ of a second true molar was discovered behind this, in both the upper and the lower jaws, whence the author concludes that the *Nesodon ovinus* had the typical number of teeth when the permanent series was fully developed, viz. $i \frac{3-3}{3-3}$, $c \frac{1-1}{1-1}$, $p \frac{4-4}{4-4}$ $m \frac{3-3}{3-3} = 44$.
The structure of the grinding teeth proving the extinct animal to have been herbivorous, the number and kinds of teeth in the entire series show that it was ungulate. In this great natural series of mammalia the author next shows that the *Nesodon* had the nearest affinities to the odd-toed or Perissodactyle order amongst the existing species ; but certain modifications of structure, hitherto peculiar to the even-toed or Artiodactyle Ungulates, are repeated in the cranium of the *Nesodon* : more important marks of affinity are pointed out in the *Nesodon* to the *Toxodon* ; and both these extinct forms of South

American herbivores are shown to agree with each other in characters of greater value, derived from the osseous and dental systems, than any of those by which the *Nesodon* resembles either the Perissodac- tyle or Artiodactyle divisions of hoofed animals.

The genus *Nesodon* is characterized by the following modifications of the teeth, which in number and kind are according to the typical dental formula above given. *Incisors* trenchant, with long, slightly curved crowns, of limited growth : *canines* small, not exceeding in length the contiguous premolars. *Molars,* in the upper jaw, with long, curved, transversely compressed crowns, which contract as they penetrate the bone and ultimately develope fangs ; the outer side of the crown ridged, the inner side penetrated by two more or less complex folds of enamel, leaving insular patches on the worn crown : enamel thin. The *lower molars,* long, straight, and compressed ; divided by an external longitudinal indent into two unequal lobes, both penetrated at the inner side by a fold of enamel, which is com- plex in the hinder lobe. All the teeth have exserted crowns of equal height and arranged in an unbroken series. The bony palate is entire and extends back beyond the molars, the maxillaries and palatines forming the back part in equal proportions. A distinct articular cavity and eminence for the lower jaw ; the eminence long and concave transversely, short and convex longitudinally; a protu- berant post-glenoid process ; a strong and deep zygoma, the orbit and temporal fossa widely intercommunicating ; the premaxillaries join the nasals.

Of the genus presenting the above dental and osteal characters the author defines four species :—the first, about the size of a Llama, is the *Nesodon imbricatus* ; the second, of the size of a Zebra, is the *Nesodon Sulivani* ; the species to which belong the portions of skull, with the teeth, described in the present memoir, did not exceed the size of a large sheep, and is termed the *Nesodon ovinus* ; fourthly, a species of the size of a Rhinoceros, *Nesodon magnus,* is satisfactorily indicated by a grinder of the upper jaw. In conclusion, the author remarks, that the osteological characters defining the orders of hoofed quadrupeds, called *Proboscidia, Perissodactyla* and *Artiodactyla,* are associated with modifications of the soft parts of such importance, as not only to establish the principle of that ternary division of the great natural group of *Ungulata,* but to indicate that the known mo- difications of the skeleton of the extinct Toxodons and Nesodons of South America, in the degree in which they differ from the osteology of the already defined orders of *Ungulata,* must have been associated with concomitant modifications of other parts of their structure which would lead to their being placed in a distinct division, equal to the *Proboscidia* ; and, like that order, to be more nearly allied to the *Pe- rissodactyla* than the *Artiodactyla.* This new division of the *Ungulata* the author proposes to call *Toxodontia,* and he remarks that its dental and osteal characters, while they illustrate the close mutual affinities between the Nesodons and Toxodons, tend to dissipate much of the obscurity supposed to involve the true affinities of the *Toxodon,* and to reconcile the conflicting opinions as to the proper position of that genus in the mammalian class.

Jan. 20.—The following papers were read :—

1. " On the Extension of the value of the ratio of the Circumference of a circle to its Diameter." By William Rutherford, Esq., F.R.A.S.

The author, referring to a former communication on this subject, published in the Phil. Trans. 1841, states that, in the value of π here given to 208 places of decimals, there exists, in the latter part of one of the terms of the series for determining the value of $\tan^{-1}\frac{1}{99}$, a transposition of the figures of a recurring decimal, which vitiates a considerable number of the figures in the latter part of the value. This error had been detected in consequence of Professor Schumacher having observed that in the value of π which had been given him by M. Dase, who had calculated it to 200 places, from the formula $\frac{\pi}{4} = \tan^{-1}\frac{1}{2} + \tan^{-1}\frac{1}{5} + \tan^{-1}\frac{1}{8}$, the figures from the 153rd to the 200th differed entirely from those given by the author. The accuracy of M. Dase's result was confirmed by a double computation of Dr. Clausen of Dorpat, who deduced the value of π to 250 places of decimals, both by Machin's formula

$$\frac{\pi}{4} = 4\tan^{-1}\frac{1}{5} - \tan^{-1}\frac{1}{239},$$

and by the formula

$$\frac{\pi}{4} = 2\tan^{-1}\frac{1}{3} + \tan^{-1}\frac{1}{7};$$

and the author's result was shown to differ from the correct value by the periodic decimal $\cdot \dot{3}\dot{6}$.

Having been informed by Mr. W. Shanks of Houghton-le-Spring, that he had pushed his computation of the value of π to the extent of 318 decimals, the author resolved to extend his operations to upwards of 400 decimals. As Mr. Shanks had employed Machin's formula, the author resolved to make use of the same. At his request Mr. Shanks resumed his calculations, and has not only verified the author's value of π to 440 places of decimals, but has carried his own to the extent of 530 places. The author states that the values of $\tan^{-1}\frac{1}{5}$ and $\tan^{-1}\frac{1}{239}$, as well as the value of π, which are here subjoined, have been obtained by the independent computations of Mr. Shanks and himself, and that they both feel confident that these values are correct in every figure as far as 440 decimals.

$\tan^{-1}\frac{1}{5} =$ ·19739 55598 49880 75837 00497 65194 79029 34475 85103 78785
21015 17688 94024 10339 69978 24378 57326 97828 03728 80441
12628 11807 36913 60104 45647 98867 94239 35574 75654 95216
30327 00522 10747 00156 45015 56006 12861 85526 63325 73186
92806 64389 68c61 89528 40582 59311 24251 61329 73139 93397
11323 35378 21796 08417 66483 10525 47303 96657 25650 48887
81553 09384 29057 93116 95934 19285 18063 64919 69751 94017
08560 94952 73686 73738 50840 08123 67856 15800 93298 22514
02324 66755 49211 02670 45743 78815 47483 90799 7

$$\tan^{-1}\frac{1}{239} = \cdot 00418\ 40760\ 02074\ 72386\ 45382\ 14959\ 28545\ 27410\ 48065\ 30763$$

19508 27019 61288 71817 78341 42289 32737 82605 81362 29094
54975 45066 64448 63756 05245 83947 89311 86505 89221 28833
09280 08462 71962 33077 33759 47634 60331 84734 14570 33198
60154 54814 80599 24498 30211 46039 12539 49527 60779 68815
58881 27339 78533 46518 04574 25481 35867 46447 51979 10232
83097 70020 64652 82763 46532 96910 48183 86543 56078 91959
14512 32220 94463 68627 66155 20831 67964 26465 74655 11032
51034 35262 82445 12693 55670 49968 44452 47904 3

$$\pi = 3\ \cdot 14159\ 26535\ 89793\ 23846\ 26433\ 83279\ 50288\ 41971\ 69399\ 37510$$

58209 74944 59230 78164 06286 20899 86280 34825 34211 70679
82148 08651 32823 06647 09384 46095 50582 23172 53594 08128
48111 74502 84102 70193 85211 05559 64462 29489 54930 38196
44288 10975 66593 34461 28475 64823 37867 83165 27120 19091
45648 56692 34603 48610 45432 66482 13393 60726 02491 41273
72458 70066 06315 58817 48815 20920 96282 92540 91715 36436
78925 90360 01133 05305 48820 46652 13841 46951 94151 16094
33057 27036 57595 91953 09218 61173 81932 61179 3

Commencing at the 441st decimal place, Mr. Shanks' additional figures are as follow :—

$$\left(\tan^{-1}\frac{1}{5}\right)\ldots 78985\ 02007\ 52236\ 96837\ 96139\ 22783\ 54193\ 25572\ 23284\ 13846$$
47744 13529 09705 46512 24383 02697 56051 83775

$$\left(\tan^{-1}\frac{1}{239}\right)\ldots 33177\ 28393\ 07086\ 31401\ 93869\ 51950\ 37058\ 64107\ 70855\ 85540$$
45223 55388 14237 67708 36515 69182 52702 00228

$$(\pi)\ldots 31051\ 18548\ 07446\ 23799\ 62749\ 56735\ 18857\ 52724\ 89122\ 79381$$
83011 94912 98336 73362 44065 66430 86021 39488

In conclusion, the author states that Mr. Shanks has computed the value of the base of the Napierian system of logarithms as well as the values of the Napierian logarithms of 2, 3, 5 and 10, to the extent of 140 places of decimals.

2. "An Account of a Deep-sea Sounding in 7706 fathoms, in 36° 49′ South Latitude, and 37° 6′ West Longitude." By Captain Henry Mangles Denham, R.N., F.R.S.

This sounding was obtained on a calm day, October 30, 1852, in the course of the passage of H.M. ship Herald, from Rio de Janeiro to the Cape of Good Hope. The sounding-line was $\frac{1}{10}$th of an inch in diameter, laid into one length, and weighing, when dry, 1lb. for every hundred fathoms. Captain Denham received from Commodore McKeever of the United States Navy, commanding the Congress Frigate, a present of 15,000 fathoms of this line, 10,000 fathoms on one reel, and 5000 on another; and considers it to have been admirably adapted for the purpose for which it was made and to which it was applied. The plummet weighed 9lbs., and was 11·5 inches in length, and 1·7 inch in diameter. When 7706 fathoms had run off the reel the sea-bottom was reached. Captain Denham states that Lieut. Hutcheson and himself, in separate boats, with their own hands, drew the plummet up 50 fathoms several times, and after it had renewed its descent, it stopped, on each occasion, abruptly at the original mark to a fathom, and would not take an-

other turn off the reel. The velocity with which the line run out
was as follows :—

	h	m	s
The first 1000 fathoms in	0	27	15
1000 to 2000 fathoms in	0	39	40
2000 to 3000 fathoms in	0	48	10
3000 to 4000 fathoms in	1	13	39
4000 to 5000 fathoms in	1	27	06
5000 to 6000 fathoms in	1	45	25
6000 to 7000 fathoms in	1	49	15
7000 to 7706 fathoms in	1	14	15
	9	24	45

The whole time therefore taken by the plummet in descending
through 7706 fathoms, or nearly 7·7 geographical miles of 60 to the
degree, was 9ʰ 24ᵐ 45ˢ. The highest summits of the Himalaya, Dha-
walagiri and Kinchinginga, are little more than 28,000 feet, or 4·7
geographical miles above the sea. The sea-bottom has therefore
depths greatly exceeding the elevation of the highest pinnacle above
its surface.

The strength of the line tried before the sounding was found to
be equal to bear 72lbs. in air. The 7706 fathoms which ran out
weighed, when dry, 77lbs., exclusive of the plummet, 9lbs. Great
care was taken in the endeavour to bring the plummet again to the
surface to show the nature of the bottom, but, whilst carefully reel-
ing in, the line broke at 140 fathoms below the water-line, carrying
away a Six's thermometer which had been bent on at 3000 fathoms.

Jan. 27.—A letter was read giving an account of "An Explosive
Meteorite." By Francis Higginson, Esq., R.N.

The writer states that his attention having been aroused by the
highly electrical state of the atmosphere during a severe gale of wind,
he proceeded along the beach in the vicinity of Dover, at 2 A.M. on the
morning of Friday, the 17th of December 1852.

It had blown very hard during the night, the wind veering from
West to W.S.W., in occasional heavy squalls of rain and sleet,
accompanied at intervals by faint flashing scintillations, which at
first, being considered sheet lightning, were only noticed from their
unusual colour, a deep and sombre red. At about 4ʰ 50ᵐ A.M., how-
ever, these flashes constantly emerging from a dense, triangular and
very remarkable cloud in the S.E., which perceptibly increased in
size with great rapidity, he was induced to observe it with minute
attention. At 4ʰ 55ᵐ A.M., Greenwich mean time, the cloud had
assumed the form of a right-angled triangle, its hypothenuse, or
longest side, tending east and west. At this instant he first heard a
singular and extraordinary hissing sound in the air, not unlike that
of a passing shot, which, although at first not very loud, was yet
clearly distinguishable above the howling of the gale. At 5 A M. the
cloud had nearly doubled its original size, advancing steadily from
the S.E. in a N.W. direction, or from nearly dead to leeward, to-
wards the wind's eye; whilst the scintillations spoken of were

emitted with increased rapidity. He also then first perceived in the centre of the cloud, a dull, red, obscure nucleus, or fire-ball, apparently about half the diameter of the moon, having a tail five or six times that length, from which the flashes mentioned were sent forth, of surpassing brilliancy, as the meteor clearly descended with great velocity through the air, accompanied by a detonating, hurtling, hissing sound, impossible to describe, yet resembling that which precedes the shock of an earthquake. At three minutes past five o'clock A.M., the meteor having apparently spanned the Channel from S.E. to N.W., upon approaching the land—evidently throwing off portions of its substance as it passed through the atmosphere—the nucleus suddenly exploded with a report similar to a very heavy clap of thunder, giving out an intensely brilliant light, which rendered the minutest objects distinctly visible, although it rained violently and the sky was obscured by dark and threatening clouds. The dense body of the meteorite seemed to fall in the water about half a mile from the land, as indicated by a great volume of spray, which rose foaming in the distance.

Feb. 3. — The reading of Mr. Airy's paper, entitled "On the Eclipses of Agathocles, Thales and Xerxes," commenced at a former meeting, was resumed and concluded.

The author, after remarking that the calculations of distant eclipses made in the last century possess little value, proceeds to give the successive steps of improvement in the lunar theory as applicable to the computation of eclipses, and especially in the motion of the moon's node. The first great improvement was the introduction by Laplace of terms expressing a progressive change in the mean secular motions. With Bürg's tables, in which these changes were introduced, or with the same elements, Mr. Francis Baily and Mr. Ottmanns computed many eclipses in the search for that usually called the eclipse of Thales; and both these astronomers fixed upon the eclipse of B.C. 610, September 30, as the only one which could be reconciled with the account of Herodotus. Mr. Baily however subjoined a computation of the eclipse of Agathocles from the same elements, and found that this could not by any means be reconciled with the historical account; he inferred from this that some serious change in the theory is necessary, and that when it was introduced the eclipse of B.C. 610 might not be found to agree with history; but he thought it certain that no other eclipse could be adopted. The various values of the motion of the node adopted by different writers from different observations (principally total or annular eclipses) are then collected. Allusion is then made to the peculiar value of the eclipse of Stiklastad (brought to notice by Professor Hansteen), and which will be increased when the calculations shall have been made on unexceptional elements. The author then adverts to the great Reduction of the Greenwich Observations from 1750 to 1830, to Hansen's new inequalities, and to the numerical amounts of corrections of the principal elements. Then are given the coefficients of the change in secular value of mean motion of the mean of the moon's perigee, and of the moon's node, as found by

Laplace, Damoiseau, Plana, and Hansen; the principal change made by the latter writers from Laplace's values being in the motion of perigee.

The method of computation adopted by the author is then explained. He adopts the Greenwich mean motions and Damoiseau's coefficients for progressive change of secular mean motion. He then repeats the calculation with an arbitrary change of longitude of node; considering that, from the loose nature of the early Greenwich observations, this element is most likely to be in error, and that its errors will produce the greatest effect.

The author then discusses the account of the eclipse of Agathocles, B.C. 310, August 15. Adopting Alhowareah (under Cape Bon) as his landing-place in Africa, he states his belief that Agathocles sailed northward from Syracuse (a conjecture which he owes in the first instance to J. W. Bosanquet, Esq.), and was not far from the Straits of Messina. On the usual supposition of his sailing to the south, he would be near Cape Passaro.

On making the calculation with the Greenwich Elements unvaried, it is found that the eclipse would be total on the southern possible place of Agathocles, but not on the northern. The calculation being repeated with an arbitrary change in the place of the node, a graphical construction is employed to discover the numerical amount of the changes that must be made to satisfy the four following conditions:—1, the northern edge of the shadow touches the south station; 2, the northern edge touches the north station; 3, the southern edge touches the south station; 4, the southern edge touches the north station. If the south station be adopted, the change must lie between those of conditions 1 and 3; if the north, the change must lie between those of 2 and 4. The numerical values must be slightly increased for application to a more distant eclipse, as to that of Thales.

The eclipse of Thales is then considered. There appears to be no reason for connecting the locality (as Mr. Baily supposed) with the river Halys. The historical circumstances indicate with great probability that two large armies had met; and the question appears to be, in what part of Asia Minor is it likely that such bodies of troops would be collected. The author adverts to the form and passes of the mountains, and decides that the Median army entered most probably by the pass of Issus, or possibly by that of Melitene, and that the battle-field might be anywhere in the space bounded by Melitene, Issus, Iconium, Sardes, and Ancyra.

On calculating the eclipses which occurred for many years before and after B.C. 600, it appears that only the eclipse of B.C. 585, May 28, answers to the requisite conditions, and that it does so in a most satisfactory way. [This is the date adopted by the principal ancient chronologists; it would seem that it was first verified, by calculations founded on good elements, by J. R. Hind, Esq. during the preparation of this paper.] The path of the shadow which is most agreeable to the military and geographical circumstances, is one which implies a correction to the Greenwich Elements corresponding to that

which would make the eclipse of Agathocles nearly central over the northern station, and excludes the possibility of his passing by the southern route.

The author then adverts to the principal remaining causes of uncertainty in these conclusions, and points out the values of progressive change in the secular mean motions as peculiarly deserving investigation.

Allusion is then made to a record in the Persian poetical history, preserved by Sir John Malcolm, which appears to point to a total eclipse as occurring under similar circumstances in the province of Mazenderan. It appears however on calculation, that no total eclipse passed over Mazenderan, at least for many years, about the time in question.

The author then calls attention to the statement of Herodotus, that something like a total solar eclipse occurred when Xerxes was setting out from Sardes for his invasion of Greece. On calculation it appears impossible to explain this by a solar eclipse, and moreover the peculiar turn of the answer of the Magi to the inquiries of Xerxes would seem to be irreconcileable with a solar eclipse. The author thinks it most likely that the phenomenon really was the total eclipse of the moon which occurred on the morning of B.C. 479, March 14. If this were adopted, the date of the invasion of Greece must be brought down one year later than that given by the received chronology.

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ROYAL INSTITUTION OF GREAT BRITAIN.

Jan. 21, 1853.—Observations on the Magnetic Force. By Professor Faraday.

Inasmuch as the general considerations to be brought forward had respect to those great forces of the globe, exerted by it, both as a mass and through its particles, namely, magnetism and gravitation, the attention was first recalled briefly to certain relations and differences of the two which had been insisted upon on former occasions. Both can act at a distance, and doubtless at any distance, but whilst gravitation may be considered as simple and unpolar in its relations, magnetism is dual and polar. Hence *one* gravitating particle or system cannot be conceived to act by gravitation, as a particle or system, on itself; whereas a magnetic particle or system, because of the dual nature of its force, can have such a self-relation. Again, either polarity of the magnetic force can act either by attraction or repulsion; and not merely so, but the joint or *dual* action of a magnet can act also either by attraction or repulsion, as in the case of paramagnetic and diamagnetic bodies: the action of gravity is always that of attraction. As a further consequence of the difference in character of the powers, little or no doubt was entertained regarding the existence of physical lines of force* in the cases of dual powers, as electricity and magnetism; but in respect of gravi-

* Proceedings of the Royal Institution, June 11, 1852, p. 216; also Phil. Mag. S. 4. 1852, vol. iii. p. 401.

tation the conclusion did not seem so sure. As some further rela-
tions of the sun and the earth would have finally to be submitted,
the audience were reminded, by the use of Arago's idea, of the rela-
tive magnitude of the two; for, supposing that the centres of the
two globes were made to coincide, the sun's body would not only
extend as far as the moon, but nearly as far again, its bulk being
about seven times that of a globe which should be girdled by the
moon's orbit.

For the more careful study of the magnetic power a torsion-
balance had been constructed, which was now shown and its mode
of operation explained. The torsion wire was of hard drawn pla-
tinum, 24 inches in length, and of such diameter that 28·5 inches
weighed one grain. It was attached as usual to a torsion-head and
index. The horizontal beam was a small glass tube terminated at
the object end by a glass hook. The objects to be submitted to the
magnetic force were either cylinders of glass with a filament drawn
out from each, so as to make a long stiff hook for suspension from
the beam; or cylindrical bulbs of glass, of like shape, but larger
size, formed out of glass tube; or other matters. The fine tubular
extremities of the bulbs being opened, the way through was free
from end to end; the bulbs could then be filled with any fluid or
gas, and be re-submitted many times in succession to the magnetic
force. The source of power employed was at first a large electro-
magnet; but afterwards, in order to be certain of a constant power,
and for the advantage of allowing any length of time for the obser-
vations, the great magnet, constructed by M. Logeman upon the
principles developed by Dr. Elias (and which, weighing above 100
lbs., could support 430 lbs. according to the report of the Great
Exhibition Jury), was purchased by the Royal Institution and used
in the inquiries. The magnet was so arranged that the axis of
power was five inches below the level of the glass beam, the interval
being traversed by the suspension-filament or hook spoken of above.
The form and position of the terminations of soft iron are shown in
plan by the diagram upon a scale of $\frac{1}{10}$, and also the place of the
object. All this part is enclosed
in the box which belongs to, and
carries the torsion-balance, which
box is governed by six screws
fixed upon the magnet table; and
as both the box and the table have
lines and scales marked upon them,
it is easy to adjust the former on

the latter so that the beam shall be over and parallel to the line
a, e with the point of suspension over *c*; or, by moving the whole
box parallel to itself towards *m*, to give the point of suspension any
other distance from the angle *c*. As already said, the objects were
constructed with a suspension filament of such length as to make
them coincide in height with the angle in the magnetic field. When
suspended on the beam, they were counterpoised by a ring or rings
of lead on the further arm of the beam. These when required were
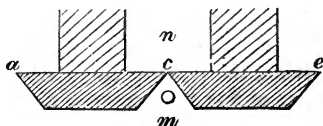
moved along the beam until the latter was horizontal; and that state was ascertained by a double arm support, which sustained the beam when out of use, brought it into a steady state when moving, and delivered it into a condition of freedom when required. The motion of the box to the right or left, so as to place the object in the middle of the magnetic angle, was given by two of the screws before spoken of; the motion to the given distance from *c*, by the other four.

Supposing the distance from *c* towards *m* to be adjusted to 0·6 of an inch, when the beam was loaded above, and no object before the magnet (the beam having been of course previously adjusted to its normal position, and the torsion-index placed at zero), it then remained to determine the return of the beam to its place when the object had been suspended on it and repelled: this was done in the following manner. A small plane reflector is fixed on the beam, near its middle part, under the point of suspension; a small telescope associated with a divided scale is placed about six feet from the reflector, and in such a position that when the beam is in its right place, a given degree in the scale coincides with the fine wire in the telescope. Of course the scale appears to pass by the wire as the beam itself moves, and with a double angular velocity, because of the reflexion. As it is easy to read to the fiftieth and even to the hundredth of an inch in this way, and as each degree occupies apparently 2·4 inches with the radius of 6 feet, so an angular motion, or difference of $\frac{1}{240}$th of a degree, could be observed; and as the radius of the arm of the beam carrying the object was 6 inches, such a quantity there would be less than $\frac{1}{2000}$th of an inch; *i. e.* the return of the beam to its first or normal position by the torsion force put on to counteract the repulsion, could be ascertained to within that amount. When an object was put on the adjusted beam, if diamagnetic it was repelled; and then, as the observer sat at the telescope, he, by means of a long handle, a wheel and pinion, put on torsion until the place of the beam was restored; and afterwards the amount of torsion read off on the graduated scale became the measure in degrees of the repulsive force exerted. At the time of real observations, the magnet, balance and telescope, were all fixed in a basement room, upon a stone floor. But it is unnecessary to describe here the numerous precautions required in relation to the time of an observation, the set of the suspension wire by a high torsion, the possible electricity of the object or beam by touch, the effect of feeble currents of air within the box, the shape of the object, the precaution against capillary action when fluids were employed as media, and other circumstances; or the use of certain stops, and the mode of procedure in the cases of paramagnetic action;—the object being at present to present only an intelligent view of the principles of action.

When a body is submitted to the power of a magnet, it is affected, as to the result, not merely by the magnet, but also by the medium surrounding it; and even if that medium be changed for a vacuum, the vacuum and the body still are in like relation to each other. In fact the result is always differential; any change in the medium

changes the action on the object, and there are abundance of sub-
stances which when surrounded by air are repelled, and when by
water are attracted, upon the approach of a magnet. When a cer-
tain small glass cylinder weighing only 66 grains was submitted on
the torsion-balance on the Logeman magnet surrounded by air, at
the distance of 0·5 of an inch from the axial line, it required 15° of
torsion to overcome the repulsive force and restore the object to its
place. When a vessel of water was put into the magnetic field, and
the experiment repeated, the cylinder being now in the water was
attracted, and 54°·5 of torsion were required to overcome this attrac-
tion at the given distance of 0·5. If the vessel had contained a
fluid exactly equal in diamagnetic power to the cylinder of glass,
neither attraction nor repulsion would have been exerted on the
latter, and therefore the torsion would have been 0°. Hence the
three bodies, air, glass (the especial specimen), and water, have their
relative force measured in relation to each other by the three expe-
rimental numbers 15°, 0°, and 54°·5. If other fluids are taken,
as oil, æther, &c., and employed as the media surrounding the *same
glass cylinder*, then the degrees of torsion obtained with each of
them respectively, shows its place in the magnetic series. It is the
principle of the hydrometer or of Archimedes in respect of gravity
applied in the case of the magnetic forces. If a different cylinder
be employed of another size or substance, or at a different distance,
the torsion numbers will be different, and the zero (given by the
cylinder) also different; but the media (with an exception to be
made hereafter) will have the same relation to each other as in the
former case. Therefore to bring all the experimental results into
one common relation, a Centigrade scale has been adopted bounded
by air and water at common temperatures, or 60° F. For this pur-
pose every separate series of results made under exactly the same
circumstances included air and water; and then all the results of
one series were multiplied by such a number as would convert the
difference between air and water into 100°; in this way the three
results given above become 21°·6, 0°, and 78°·4. By such a pro-
cess the magnetic intervals between the bodies are obtained on the
Centigrade scale, but the true zero is not as yet determined. Either
water, or air, or the glass, may be assumed as the zero, the intervals
not being in any way dependent upon that point, but the results
will then vary in expression thus :—

		$\overset{\circ}{0}$	$\overset{\circ}{21}\cdot6$	$\overset{\circ}{100}$
Air	. . . .	0	21·6	100
Glass	. .	21·6	0	78·4
Water	. .	100	78·4	0

all above the zero being paramagnetic, and all below diamagnetic in
relation to it. I have adopted a vacuum as the zero in the table of
results to be given hereafter.

In this manner it is evident that, upon principle, any solid, what-
ever its size, shape, or quality, may be included in the list, by its
subjection to a magnet in air and in water, or in fluids already
related to these : also that any fluids may be included by the use of

the *same* immersed solid body for them, air and water; and also that by using the same vessel, as, for instance, the same glass bulb, and filling it successively with various gases and fluids, including always air and water in each series, these included bodies may then have their results reduced and be entered upon the list. The following is a table of some substances estimated on the Centigrade scale; and though there are many points both of theory and practice yet to be wrought out, as regards the use of the torsion-balance described, so that the results can only be recorded as approximations, yet even now the average of three or four careful experiments, gives an expression for any particular substance under the same conditions of distance, power, &c. near upon and often within a degree of the place assigned to it. The powers are expressed for a distance of 0·6 of an inch from the magnetic axis of the magnet as arranged and described, and, of course, for *equal volumes* of the bodies mentioned. The extreme decimal places must not be taken as indicating accuracy, except as regards the record of the experiments: they are the results of calculation. Hydrogen, nitrogen, and perhaps some other of the bodies near zero, may ultimately turn out to be as a vacuum; it is evident that a very little oxygen would produce a difference, such as that which appears in nitrogen gas. The first solution of copper mentioned was colourless, and the second the same solution oxidized by simple agitation in a bottle with air, the copper, ammonia and water, being in both the same.

	c		o
Prot-ammo. of copper	134·23	Camphor	82·59
Per-ammo. of copper	119·83	Camphine	82·96
Oxygen	17·5	Linseed oil	85·56
Air	3·4	Olive oil	85·6
Olefiant gas	0·6	Wax	86·73
Nitrogen	0·3	Nitric acid	87·96
Vacuum	0·0	Water	96·6
Carbonic acid gas	0·0	Solution of ammonia	98·5
Hydrogen	0·1	Bisulphide of carbon	99·64
Ammonia gas	0·5	Sat. sol. nitre	100·08
Cyanogen	0·9	Sulphuric acid	104·47
A glass	18·2	Sulphur	118
Pure zinc	74·6	Chloride of arsenic	121·73
Æther	75·3	Fused borate lead	136·6
Alcohol absolute	78·7	Phosphorus	
Oil of lemons	80	Bismuth	1967·6

Plücker in his very valuable paper[*] has dealt with bodies which are amongst the highly paramagnetic substances, and his estimate of power is made for equal weights.

One great object in the construction of an instrument delicate as that described, was the investigation of certain points in the philosophy of magnetism; and amongst them especially that of the right application of the law of the inverse square of the distance as the

* Taylor's Scientific Memoirs, vol. v. pp. 713, 730.

universal law of magnetic action. Ordinary magnetic action may be divided into two kinds; that between magnets permanently magnetized and unchangeable in their condition, and that between bodies of which one is a permanent unchangeable magnet, and the other, having no magnetic state of its own, receives and retains its state only whilst in subjection to the first. The former kind of action appears in the most rigid and pure cases to be subject to that law; but it would be premature to assume beforehand, and without abundant sufficient evidence, that the same law applies in the second set of cases also; for a hasty assumption might be in opposition to the truth of nature, and therefore injurious to the progress of science, by the creation of a preconceived conclusion. We know not whether such bodies as oxygen, copper, water, bismuth, &c., owe their respective paramagnetic and diamagnetic relation to a greater or less facility of conduction in regard to the lines of magnetic force, or to something like a polarity of their particles or masses, or to some as yet unsuspected state; and there is little hope of our developing the true condition, and therefore the cause of magnetic action, if we assume beforehand the unproved law of action and reject the experiments that already bear upon it;—for Plücker has distinctly stated as the fact, that diamagnetic force increases more rapidly than magnetic force, when the power of the dominant magnet is increased; and such a fact is contrary to the law above enunciated. The following are further results in relation to this point.

When a body is submitted to the great unchanging Logeman magnet in air and in water, and the results are reduced to the Centigrade scale, the relation of the three substances remain the same for the same distance, but not for *different* distances. Thus when a given cylinder of flint-glass was submitted to the magnet surrounded by air and by water, at the distance of 0·3 of an inch, as already described, it proved to be diamagnetic in relation to both; and when the results were corrected to the Centigrade scale, and water made zero, it was 9°·1 below, or on the diamagnetic side of water. At the distance 0·4 of an inch it was 10°·6 below water: at the distance of 0·7 it was 12°·1 below water. When a more diamagnetic body, as heavy glass, was employed, the same result in a higher degree was obtained; for at the distance of 0·3 it was 37°·8 below water, and at that of 0·8 it was 48°·6 beneath it. Bismuth presented a still more striking case, though, as the volume of the substance was necessarily small, equal confidence cannot be placed in the exactitude of the numbers. The results are given below for the three substances, air being always 100° and water 0°; the first column of figures for each substance contains the distance* in tenths

* A given change of distance necessarily implies change in degree of force, and change in the forms of the lines of force; but it does not imply always the same amount of change. The forces are not the same at the same distance of 0·4 of an inch in opposite directions from the axial line towards *m* and *n* in the figure, p. 219, nor at any other equal moderate distance; and though by increase and diminution of distance the change is in the same direction, it is not in the same proportion. By fitly arranged terminations, it may be made to alter with extreme rapidity in one direction, and with extreme slowness or not at all in another.

of an inch from the axial line of the magnetic field, and the second, the place in Centigrade magnetic degrees below water.

Flint-Glass.	Heavy Glass.	Bismuth.
$0\cdot3 - 9\cdot1$	$0\cdot3 - 37\cdot8$	$0\cdot6 - 1871$
$0\cdot4 - 10\cdot6$	$0\cdot4 - 38\cdot6$	$1\cdot0 - 2734$
$0\cdot5 - 11\cdot1$	$0\cdot6 - 40\cdot0$	$1\cdot5 - 3626$
$0\cdot6 - 11\cdot2$	$0\cdot8 - 48\cdot6$	
$0\cdot7 - 12\cdot1$	$1\cdot0 - 51\cdot5$	
	$1\cdot2 - 65\cdot6$	

The result here is that the greater the distance of the diamagnetic bodies from the magnet, the more diamagnetic is it in relation to water, taking the interval between water and air as the standard : and it would further appear, if an opinion may be formed from so few experiments, that the more diamagnetic the body compared to air and water, the greater does this difference become. At first it was thought possible that the results might be due to some previous state induced upon the body, by its having been nearer to or further from the magnet ; but it was found that whether the progress of the experiments was from small to large distances, or the reverse ; or whether, at any given distance, the object was previous to the measurement held close up to the magnet or brought from a distance, the results were the same ;—no evidence of a temporary induced state could in any of these ways be found.

It does not follow from the experiments, if they should be sustained by future researches, that it is the glass or the bismuth only that changes in relation to the other two bodies. It may be the oxygen of the air that alters, or the water, or more probably all these bodies ; for if the result be a true and natural result in these cases, it is probably common to all substances. The great point is that the three bodies concerned, air, water, and the subject of the experiment, alter in the degree of their magnetic relations to *each other* ; at different given distances from the magnet the ratio of their magnetic power does not, according to the experiments, remain the same ; and if that result be confirmed, then it cannot be included by a law of action which is inversely as the square of the distance. A hydrometer floating in a fluid and subject to the gravity of the earth alone, would (other things being the same) stand at the same point, whether at the surface of the earth, or removed many diameters of the earth from it, because the action of gravity is inversely as the square of the distance ; but if we suppose the substance of the hydrometer and the fluid to differ magnetically, as water and bismuth does, and the earth to act as a magnet instead of by gravity, then the hydrometer would, according to the experiments, stand at a different point for different distances, and if so could not be subject to the former law.

The cause of this variation in the ratio of the substances one to another, if it be finally proved, has still to be searched out. It may depend in some manner upon the *forms* of the lines of magnetic force, which are different at different distances ; or not upon the

forms of the lines but the *amount* of power at the different distances ; or not upon the mere amount, but on the circumstance that in every case the body submitted to experiment has lines of *different degrees of force* passing through different parts of it (for however different the magnetic or diamagnetic conditions of a body and the fluid surrounding it, they would not move at all in relation to each other in a field of equal force) ; but whatever be the cause, it will be a concomitant of magnetic actions ; and therefore ought to be included in the results of any law by which it is supposed that these actions are governed.

It has not yet been noticed that these general results appear to be in direct opposition to those of Plücker, who finds that diamagnetic power increases more rapidly than magnetic power with increase of force. But such a circumstance, if both conclusions be accordant with facts, only shows that we have yet a great deal to learn about the physical nature of this force ; and we must not shut our eyes to the first feeble glimpses of these things, because they are inconsistent on both sides with our assumed laws of action ; but rather seize them, as hoping that they will give us the key to the truth of nature. Bodies when subject to the power of the magnet appear to acquire a new physical state, which varies with the distance or the power of the magnet. Each body may have its own rate of increase and decrease ; and that may be such as to connect the extreme effect of Plücker, amongst paramagnetic bodies on the one hand, and the extreme effects amongst diamagnetic bodies now described, on the other ; and when we understand all this rightly, we may see the apparent contradiction become harmony, though it may not conform to the law of the inverse square of the distance as we now try to apply it.

Plücker has already said, because of his observations regarding paramagnetic and diamagnetic force, that no correct list of magnetic substances can be given. The same consequence follows, though in a different direction from what has now been stated, and hence the reservation before made (p. 221). Still the former table is given as an approximation, and it may be useful for a time. Before leaving this first account of recent experimental researches, it may be as well to state that they are felt to be imperfect and may perhaps even be overturned ; but, that as such a result is not greatly anticipated, it was thought well to present them to the Members of the Royal Institution and the scientific world, if peradventure they might excite criticism and experimental examination, and so aid in advancing the cause of physical science.

On a former occasion * the existence of *physical* lines of force in relation to magnetism and electricity was inferred from the dual nature of these powers, and the necessity in all cases and at all times of a relation and dependence between the polarities of the magnet, or the positive and negative electrical surfaces. With respect to gravity a more hesitating opinion was expressed, because of the dif-

* Proceedings of the Royal Institution, p. 216.

ficulty of observing facts having any relation to *time*, and because two gravitating particles or masses did not seem to have any necessary dependence on each other, for the existence or excitement of their mutual power*. On the present occasion a passage was quoted from Newton which had since been discovered in his works, and which, showing that he was an unhesitating believer in physical lines of gravitating force, must from its nature rank him amongst those who sustain the physical nature of the lines of magnetic and electrical force; it is as follows, in words written to Bentley†: "That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

Finally, reference was made to Sabine's remarkable observation, sustained as it has been by Wolf, Gautier and others, of certain coincidences existing between the appearance of solar spots and the diurnal variation of the magnetism of the earth. Schwabe has been engaged in carefully observing the spots on the sun since the year 1826. He has found them gradually to increase in number and size from year to year, and then decrease; then again increase, again to decrease, and so on in a regular period of about ten years. The following is a part of his table‡ giving the years of the maxima and minima of spots:—

	Groups in the year.	Days of no spots.	Days of observation.
1826			
1828	225	0	282
1833	33	139	267
1837	333	0	168
1843	34	149	312
1848	330	0	278
1851			

Lamont (Dec. 1851)§ was induced by recent researches in atmospheric magnetism, to examine the daily magnetic variation in declination, and found that, as a whole, it increased and diminished, and then increased again, having a regular variation of about ten years: the year 1844 was given as having a minimum variation of

* Phil. Mag. S. 4. 1852, vol. iii. p. 403 (3246).
† Newton's Works, Horsley's edition, 4to, 1783, vol. iv. p. 438, or the Third Letter to Bentley.
‡ Humboldt's Cosmos, iii. p. 291, 292. *Bibliothèque Universelle*, 1852, xx. p. 184.
§ Poggendorff's *Annalen*, lxxxiv. p. 572. [Phil. Mag. June, 1852.]

6'·61, and the year 1848 as presenting a maximum variation of 11'·15.

Sabine* (March, 1852), in searching for periodical laws amongst the mean effects of the larger magnetic disturbances, found a simultaneous period of increase and decrease both at Hobarton and Toronto, on opposite sides of the globe; the minimum effect was in 1843, and the maximum effect in 1848, according therefore almost exactly with Lamont's observations at Munich. But, besides that, he pointed out the extraordinary circumstance that this similar variation of the daily magnetic declination is the same in length of period as that discovered by M. Schwabe for the solar spots; and still more, that the maxima and minima of these two most different phænomena coincide; for 1843 presents the least diurnal variation and the smallest number of solar spots, and 1848 the largest magnetic variation and the greatest number of solar observations. He has also observed that the same period of increase and decrease exists with the same epochs in the diurnal variation of the magnetic inclination of the earth's magnetic force in both hemispheres. The phænomenon is general both as regards all the magnetic elements, and in parts of the globe most distant from each other.

Gautier appears to have been struck with the same coincidence, but did not publish his idea until July 1852†. Wolf of Berne, who has sought far into the history of the sun spots, had the same thought, publishing it first at the end of July or beginning of August 1852‡. He endeavours to trace the general condition of the spots from the year 1600, and concludes that the true length of the period is 11·11 years. As it is impossible to conceive such a coincidence in the length of the period and the time of the maxima and minima of these two greatly differing phænomena, without believing in some relation of them to a common cause; so, the observation of such a coincidence at this moment ought to urge us more than ever into an earnest and vigorous investigation of the true and intimate nature of magnetism; by means of which we now have hopes of touching in a new direction, not merely this remarkable force of the earth, but even the like powers of the sun itself.

XXXVI. *Intelligence and Miscellaneous Articles.*

NOTICES OF THE SUPERPOSITION OF CERTAIN MINERALS IN SOME MINING DISTRICTS OF GERMANY§.

THE following interesting addition to our knowledge of the conditions which accompany some of the rarer minerals, forms an appendix to a translation of Mr. Henwood's inquiries on the same subject in the mines of Cornwall and Devon‖.

* Philosophical Transactions, 1852, p. 103.
† *Bibliothèque Universelle,* 1852, xx. 189.
‡ Proceedings of Natural Society of Berne, Nos. 245–247.
§ *Gangstudien oder Beiträge zur Kenntniss der Erzgänge, Herausgegeben von* B. Cotta, *Dr. Ph. und Professor der Geognosie in Freiberg,* vol. ii. Heft 1. p. 120.
‖ Lond. Edinb. and Dub. Phil. Mag. S. 3, vol. xxix. p. 359 (1846).

Mr. Surveyor Gümbel of St. Ingbert notices the following order of succession amongst the ingredients of the quicksilver veins which traverse the carboniferous formation of the Palatinate.

Mineral adjoining *wall* of lode.	Mineral resting on that in first column.	Mineral resting on that in second column.	Mineral resting on that in third column.	Mineral resting on that in fourth column.
Hornstone	Sulphate of barytes..	Iron pyrites	Cinnabar........	Native mercury.
Hornstone	Semi-opal..........	Quartz	Cinnabar........	Asphalt.
Hornstone	Fluor..............	Calc-spar	Quartz..........	Iron pyrites.
Iron pyrites	Cinnabar	Sulphate of barytes..	Quartz.	
Hornstone	Cinnabar	Native mercury		
Hornstone	Cinnabar.			
Sulphate of barytes	Cinnabar.			

Professor Breithaupt, who has long studied this subject, has observed the following superposition of minerals in the metalliferous veins of Saxony, Bohemia and Hanover :—

At Zinnwald in the Erzgebirge;—containing rock—Gneiss.

Quartz	Mica	Oxide of tin........	Fluor.	
Quartz	Mica	Wolfram	Tungstate of lime.	
Quartz	Mica	Galena	Sulphuret of tin.	
Quartz	Tungstate of lime ..	Fluor.		

At Altenberg in the Erzgebirge;—containing rock—Porphyry.

Quartz	Talc	Specular iron ore ..	Iron pyrites.	
Quartz	Specular iron ore ..	Fluor..............	Spathose iron ore.	
Quartz	Copper pyrites......	Sulphuret of bismuth		
Quartz	Sulphuret of bismuth			

At Ehrenfriedersdorf in the Erzgebirge;—containing rock—Mica-slate.

Quartz	Oxide of tin........	Topaz	Apatite	Fluor and quartz.
Quartz	Oxide of tin........	Wolfram	Blende..........	Iron pyrites.
Quartz	Oxide of tin........	Apatite	Gilbertite.	
Quartz	Wolfram .... {	Sulphuret of molyb-denum	} Fluor.	
Quartz	Mica	Apatite............	Herderite	Fluor.
Quartz	Wolfram	Arsenical pyrites.		
Wolfram ...... {	Sulphuret of molyb-denum	} Gilbertite	Tungstate of lime.	
Wolfram	Copper pyrites......	Tungstate of lime ..	Fluor.	

At Schlaggenwalde in Bohemia;—containing rock—Granite.

Quartz	Oxide of tin........	Wood arseniate of copper.		
Quartz	Fluor............ {			
Quartz	Cupreous manganese			
Quartz	Wolfram	Fluor.		
Oxide of tin	Topaz	Fluor.		
Oxide of tin	Tungstate of lime ..	Fluor.		

At Przibram in Bohemia;—containing rock—Grauwacke.

Spathose iron ore..	Blende	Spathose iron ore ..	Blende..........	Quartz.
Spathose iron ore {	Red iron ore (in form of spathose iron ore)	} Karbonspath.		

At Neu-dorf in the Harz;—containing rock—Clay-slate.

Spathose iron ore..	Galena	{ Antimonial gray copper ore.... }	Copper pyrites ..	Pearl-spar.
Spathose iron ore..	Quartz	Spathose iron ore ..	Galena .... {	Bournonite. Copper pyrites.

ON THE PRESENCE OF BORACIC ACID IN THE THERMAL WATERS CONTAINING ALKALINE SULPHURETS AT D'OLETTE (EASTERN PYRENEES). BY JULES BONIS.

The progress of analytical chemistry is continually making known to us the presence of substances in the water of springs, which may to a certain extent account for their effects either in agriculture or therapeutics. During the last few years a number of very important investigations have been made upon this subject, and it is only by means of the consideration of a great number of experiments and well-established facts that we have arrived at a knowledge of the true nature and mode of formation of mineral waters.

H. Rose has recently proposed a method for detecting the presence of very minute quantities of free or combined boracic acid in solution, by taking advantage of the reaction which it gives with turmeric paper.

The analogy which exists in certain respects between the waters of the Pyrenees and those of Tuscany induced me to seek for boracic acid in the former, and I selected the waters of Olette as being the type of sulphuretted waters discharged at the earth's surface with a high temperature. These waters issue from a quartzose and felspathic rock, which is decomposed under their influence, giving rise to products which I shall bring before the Academy upon a subsequent occasion.

In order to detect the presence of boracic acid in a mineral water, I evaporate a certain quantity in a porcelain capsule and acidulate the residue with hydrochloric acid, when if boracic acid is present it communicates to turmeric paper the characteristic red colour. Generally speaking, the simple coloration is not sufficient evidence of its presence ; and notwithstanding the opinion of H. Rose, so competent and always so conscientious an authority on subjects connected with analysis, I was desirous of convincing myself of the efficacy of this reagent by repeated trials. I first ascertained that water containing carbonates of potash and soda did not, when evaporated in a porcelain capsule, give the least red colour with turmeric paper, after the addition of hydrochloric acid ; I have obtained the same result with silicate of potash, sulphate of soda, sulphuret of sodium, and chloride of sodium. Finally, I made an artificial water having the same composition as that in which I suspected the presence of boracic acid ; and on operating with it in precisely the same manner as with the natural water, obtained a negative result.

For the purpose of bringing out the colour in a marked manner, I dip the paper into the liquid to be tested, previously acidulated ; I then dry it and repeat this operation several times, at last pouring over the paper some water faintly acidulated with hydrochloric acid, which deepens the colour. Characters drawn with acidulated water upon the paper thus prepared become perfectly legible.

The sensibility of this test is such that I was enabled to detect the presence of boracic acid in the waters of Olette by evaporating 500 cubic centimetres in a platinum capsule ; and to judge from the colour communicated to the paper, I should say that the quantity of borate is considerable. As to the possibility of extracting this acid

with advantage, I cannot offer an opinion until I have made a quantitative determination. It is probable that the acid exists in combination with soda, as in the waters occurring in China, Persia and Peru. In the water of the Tuscan lagoons, on the contrary, it is in a free state.

The alkalinity of thermal waters issuing from granitic rocks and containing silica in solution, frequently in considerable quantity, induces me to conjecture that they likewise contain boracic acid. I shall for the present limit myself to pointing out its existence in the waters of Olette, and on a future occasion will resume this subject. —*Comptes Rendus*, January 1853, p. 239.

ON THE PRESENCE OF BORACIC ACID IN THE MINERAL WATERS OF WIESBADEN AND AIX LA CHAPELLE.   BY R. FRESENIUS AND R. WILDENSTEIN.

It was not improbable that some thermal waters contain boracic acid as the waters of cold springs contain, besides iron, manganese and zinc, but hitherto there was no sufficiently delicate test for this acid, and in many cases it was not sought for in the analysis of thermal waters. Taking advantage of the process recommended by H. Rose, Professor Fresenius has succeeded in detecting boracic acid in the water of the "Kochbrunnen," at Wiesbaden, and this result induced Wildenstein, one of his pupils, to examine the water of the Kaiserquelle at Aix la Chapelle, which was also found to contain it. —*Journ. für Prakt. Chem.* vol. lv. pp. 163, 165.

ON THE PER-CENTAGE OF ZINC IN BRASS AND BRONZE, AND THE SEPARATION OF ZINC FROM COPPER.   BY M. BOBIERRE.

My researches on the composition of the alloys intended for the sheathing of vessels have led me to examine the chemical composition of the brass used for the navy. I soon found that the separation of zinc from copper presents numerous difficulties, and that the methods hitherto adopted are, if not insufficient, at least ill-calculated to furnish accurate results in all cases.

The analytical method which I am about to describe is based upon the known facts that zinc is volatile, and that its vapour is readily carried off by a current of hydrogen. The alloy of copper and zinc is exposed to a red heat for three-quarters of an hour or more in a small porcelain tray, and a current of hydrogen passed over it. By the application of this means in a great number of instances I have invariably obtained results possessing a remarkable degree of accuracy.

I have further ascertained that, under these circumstances, lead is not volatilized, so that the presence of this metal in a specimen of brass or bronze does not affect the accuracy of the results. Alloys of zinc and iron may be analysed very rapidly by means of the above process. —*Comptes Rendus*, January 1853, p. 224.

ON THE PRESENCE OF SUGAR IN THE URINE OF EPILEPTIC PATIENTS.   BY MM. MICHEA AND ALVARO REYNOSO.

That the urine passed by epileptics, after their attacks, contains sugar, is a fact which has been proved by a great number of experi-

ments. However, none of the usual tests are calculated to detect its presence in this excretion. Solution of potash, which is generally sufficient to indicate the presence of sugar in diabetic urine, produces no reaction with epileptic urine. The saccharometer is not of much value, being neither sufficiently sensitive nor convenient.

The most advantageous and decisive processes are fermentation and Barreswil's liquid. In order to obtain good and trustworthy results with the latter, several precautions are necessary before using it. The urine must be treated first with acetate of lead, which precipitates the other organic substances ; then with carbonate of soda, which removes the lead ; after concentrating the urine, Barreswil's test is added and the liquid boiled. Without these precautions the results are never decisive, and reactions are sometimes produced which would appear to indicate the presence of sugar when the urine does not contain a trace.—*Comptes Rendus*, January 1853, p. 230.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1853.

Chiswick.—January 1. Densely overcast. 2. Rain. 3. Fine. 4. Boisterous, with rain. 5. Fine : exceedingly fine in forenoon : clear at night. 6. Fine : cloudy and boisterous. 7. Rain : boisterous, with constant heavy rain. 8. Fine. 9. Fine, with bright sun. 10. Rain. 11. Clear : overcast. 12. Boisterous, with heavy showers. 13. Densely clouded : very fine : clear. 14. Clear : fine : overcast. 15. Rain : clear at night. 16. Fine : rain : hazy. 17. Cloudy : clear at night. 18. Fine : clear : frosty at night. 19. Uniformly overcast : drizzly : very boisterous at night. 20. Densely clouded and boisterous : rain. 21. Hazy : cloudy : showery. 22. Cloudy : clear and cold : hurricane, without rain at night. 23. Overcast and cold : clear. 24. Overcast throughout. 25. Foggy. 26. Foggy : fine : cloudy. 27. Fine : densely clouded : rain at night. 28. Overcast : cloudy. 29. Overcast throughout. 30. Overcast : rain at night. 31. Clear and frosty : very fine, with bright sun : foggy at night.

    Mean temperature of the month .... 40°·85
    Mean temperature of Jan. 1852 .... 39 ·66
    Mean temperature of Jan. for the last twenty-seven years ... 36 ·90
    Average amount of rain in Jan. .... 1·68 inch.

Boston.—Jan. 1. Cloudy. 2. Fine. 3—5. Cloudy. 6. Cloudy : rain p.m. 7. Cloudy. 8. Fine. 9. Cloudy. 10. Cloudy : rain a.m. 11, 12. Fine : rain p.m. 13, 14. Fine. 15. Rain a.m. 16. Fog : rain p.m. 17. Cloudy. 18. Fine. 19, 20. Fine : rain a.m. 21. Cloudy. 22. Fine : rain p.m. 23. Fine. 24. Cloudy : rain p.m. 25, 26. Cloudy. 27. Cloudy : rain p.m. 28, 29. Cloudy. 30, 31. Fine.

Sandwick Manse, Orkney.—Jan. 1. Clear a.m. : cloudy, lightning p.m. 2. Clear a.m. : clear, aurora, lightning p.m. 3. Cloudy a.m. : clear, aurora p.m. 4. Rain, cloudy a.m. : showers p.m. 5. Clear a.m. : sleet-showers p.m. 6. Cloudy a.m. : snowing, aurora p.m. 7. Cloudy a.m. : rainy, aurora p.m. 8. Showers a.m. : showers, aurora p.m. 9. Sleet-showers a.m. : hail, aurora p.m. 10. Showers a.m. : fine, aurora p.m. 11. Rain a.m. : rain, cloudy p.m. 12. Bright a.m. : clear, aurora p.m. 13. Bright a.m. : showers p.m. 14. Hail-showers a.m. : clear, aurora p.m. 15. Bright, frost a.m. : clear, frost p.m. 16. Frost a.m : cloudy, frost p.m. 17. Bright, frost a.m. : clear, frost p.m. 18. Cloudy, frost a.m. : showers p.m. 19. Cloudy a.m. : showers p.m. 20. Rain a.m. : clear, large halo p.m. 21. Cloudy a.m. : showers p.m. 22. Showers a.m. : hail-showers p.m. 23. Clear, frost a.m. : fine, clear p.m. 24. Clear, frost a.m. : frost, clear p.m. 25. Bright a.m. : clear p.m. 26. Showers a.m. : frost, clear p.m. 27. Bright a.m. : clear p.m. 28. Clear a.m. and p.m. 29. Bright a.m. : rain p.m. 30. Rain a.m. : showers, aurora p.m. 31. Bright a.m. : fine p.m.

    Mean temperature of Jan. for twenty-six previous years ...... 38°·60
    Mean temperature of this month .... 38 ·55
    Average quantity of rain in Jan. for seven years previous ...... 4·21 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. 1853. Jan.	Barometer					Thermometer					Wind			Rain		
	Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Chiswick 1 p.m.	Boston	Orkney, Sandwick	Chiswick	Boston	Orkney, Sandwick
1.	30·039	29·898	29·60	29·44	29·37	52	47	46	40½	42½	sw.	sw.	sw.	·03		·11
2. ☾	29·863	29·703	29·36	29·29	29·46	51	44	44	41	36½	sw.	s.	se.	·18		
3.	29·798	29·663	29·28	29·09	29·26	49	30	45	41	39	w.	wsw.	ssw.			·11
4.	29·657	29·463	29·24	28·77	28·98	52	38	45	50½	44	sw.	wsw.	w.	·12		·43
5.	29·716	29·629	29·18	28·98	29·18	52	30	46	39	39½	sw.	ssw.	ssw.	·01		·06
6.	29·656	29·481	29·22	29·27	29·02	49	32	39	37	37	sw.	s.	w.	·16		·30
7.	29·456	29·136	28·92	28·87	28·77	53	30	40	38½	39	se.	ssw.	w.		·20	·40
8. ●	29·527	29·404	29·09	28·82	28·86	49	32	35	36	38	s.	ssw.	ssw.	·34		·31
9.	29·740	29·680	29·30	28·89	29·24	50	35	34	40	41	w.	ssw.	e.			·37
10.	29·664	29·300	29·28	29·36	29·22	51	42	41	40	35	s.	wsw.	ssw.	·01		·28
11.	29·662	29·473	29·00	28·75	28·82	51	42	42·5	42	42	sw.	wsw.	sw.	·06	·15	·37
12.	29·568	29·412	29·10	29·10	29·11	53	38	45	42	37	sw.	calm	calm	·02	·13	
13.	29·454	29·382	29·94	29·13	29·33	48	35	38	35	38	w.	nw.	calm	·24	·12	·38
14.	29·776	29·672	29·33	29·63	29·73	47	34	34	38	31	sw.	nw.	e.			·04
15.	29·406	29·393	28·97	29·48	29·49	47	31	40	35	33	s.	ssw.	se.	·04		
16.	29·392	28·996	29·03	29·42	29·43	45	33	33	35½	34	nw.	sw.	s.		·12	
17. ☽	29·540	29·157	28·90	29·52	29·66	42	34	36	32	31	n.	sw.	sse.	·12	·12	·10
18.	29·943	29·762	29·52	29·63	29·44	44	26	32·5	37	40	sw.	w.	s.	·28	·40	·60
19.	29·957	29·799	29·60	29·48	28·87	54	47	50	39	49	sw.	n.	s.			·12
20.	29·714	29·569	29·12	28·74	29·39	55	37	42	44	34½	nw.	nw.	w.	·08	·05	·14
21.	29·463	29·360	29·06	29·00	29·12	50	33	32	40½	38	n.	sse.	n.	·05	·05	·01
22.	29·764	29·523	29·17	29·58	30·00	42	35	39	38½	35	e.	ne.	sse.	·17		
23.	30·122	29·970	29·77	30·18	29·90	43	31	34·5	34	35	e.	ne.	sse.	·06	·05	
24.	30·157	29·964	29·87	30·13	29·83	42	34	35	36	34	e.	ne.	e.		·05	·08
25. ○	29·803	29·647	29·50	29·77	30·05	42	32	36	39½	38½	ne.	calm	e.			·09
26.	29·737	29·651	29·36	29·91	29·87	40	26	36	38½	33½	n.	ssw.	calm	·04		·06
27.	29·791	29·739	29·50	29·98	29·87	40	36	40	36½	35	sw.	sw.	se.		·09	
28.	29·833	29·741	29·50	29·85	29·44	42	37	39	37	39	n.		s.			·62
29.	29·845	29·827	29·53	29·63	29·46	43	35	31	48	42			wsw.	·13		·14
30.	29·795	29·716	29·36	29·26	29·98	43	25		42	40			s.			
31.	30·204	30·086	29·76	29·77		47	24		44	42						
Mean.	29·743	29·583	29·33	29·381	29·431	47·35	34·35	38·7	39·27	37·83				2·14	1·41	5·12

XXXVII. *On Cavities in Amber containing Gases and Fluids.*
By Sir DAVID BREWSTER, *K.H., D.C.L., F.R.S., V.P.R.S.*
Edinb., and Associate of the Institute of France*.

HAVING recently had occasion to examine a great number
of Diamonds, and having discovered in by far the greater
number of them cavities of different shapes, round which the
substance of the diamond had been compressed when in a soft
state by the expansion of their gaseous contents, I was anxious
to re-examine some specimens of Amber in which I had observed
the same phænomena, and other specimens which were likely to
exhibit them.

The general character of the cavities in diamond was that of
extreme irregularity, and hence the sectors of polarized light and
the black cross which separated them partook of the same cha-
racter. In most of the cavities in amber, on the contrary, their
form was accurately spherical, and the polarizing structure which
surrounded them extremely perfect and beautiful. I have found,
however, numerous microscopic cavities which are very irregular,
and which, like those of the same minuteness in the diamond,
give regular sectors of polarized light. I have found also cavi-
ties of this kind existing in groups twelve or fifteen in number.

In a specimen of amber containing spherical cavities of various
magnitudes, and exhibiting around them the polarizing structure,
I have observed several cavities quite close to them without the
slightest trace of such a structure. The circumference of the
latter was encrusted with a sort of reddish powder, which had
probably been deposited by a fluid that had been carried off by
absorption.

* Communicated by the Author.

In my second paper " On the Refractive Power of the two new Fluids in Minerals," I have mentioned some specimens of amber, shown me by the late Mr. William Nicol, in which the inner surfaces of some of the cavities were rough, like finely-ground glass, and contained a fluid with a moveable vacuity[*].   Upon subsequently examining one of these specimens, I found that the apparent roughness of the cavity arose from its surface being covered with minute striæ parallel to one another.   Some of the cavities in the specimen were very irregular, while others were perfectly spherical ; the latter were *quite full* of fluid, a fact which became obvious by comparing the refraction at their boundaries with the refraction at the boundaries of the other cavities which had a vacuity in the fluid they contained.   Mr. Nicol was of opinion that the vacuity had increased in several of them, a result which could arise only from its absorption by the amber.

In a beautiful specimen of amber in the collection of the late Mr. Witham of Larkington, which I had occasion to examine, there were about eight spherical cavities like balls, all very close to each other ; and some of them so nearly in contact, that they were separated by a very thin film of amber. The fluid which they contained was a heterogeneous dark yellowish-brown substance. I opened one of the cavities in the presence of the late Dr. Edward Turner and Mr. Witham, and took out the fluid, which, to all of us, had the smell of soot. When placed upon a plate of glass it could be drawn along like the white of an egg, or the vitreous humour of the eye. When dried, it left a transparent matter like amber.   This matter had no double refraction. When exposed to heat under the blowpipe, it became of a fine *orange-red* colour.   It did not burn, but by a continuance of the heat it became black and disappeared.

In a very fine specimen of amber, belonging to the late W. G. Thomson, Esq., and of the size shown in the annexed diagram, there was a large oblong cavity *ab*, in which a fluid with its surface concave stood at the level *mn*. The fluid was so sluggish, or probably viscid, that in cold weather it required to be shaken to make it move to the end A by inverting it ; but in warm weather (64° Fahr.) it moved with great facility.

The extremity *b* of the cavity was so near the surface of the specimen, that I durst not make any experiments upon the fluid by heating it lest the
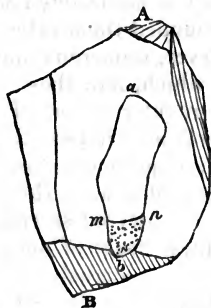
cavity should burst. I found, however, that it was not very expansible by heat; and that it was so evaporable, that at ordinary temperatures the sides of the cavity were covered with vapour, whose particles or small globules were about the $\frac{1}{100}$dth of an inch in diameter.

St. Leonard's College, St. Andrews,
    February 28, 1853.

XXXVIII. *Account of a remarkable Fluid Cavity in Topaz. By* Sir DAVID BREWSTER, *K.H., D.C.L., F.R.S., V.P.R.S. Edinb., and Associate of the Institute of France**.

THE remarkable fluid cavity in Topaz which I propose to describe is shown in the annexed diagram. The cavity itself is of a very irregular form; its greatest length, AB, being
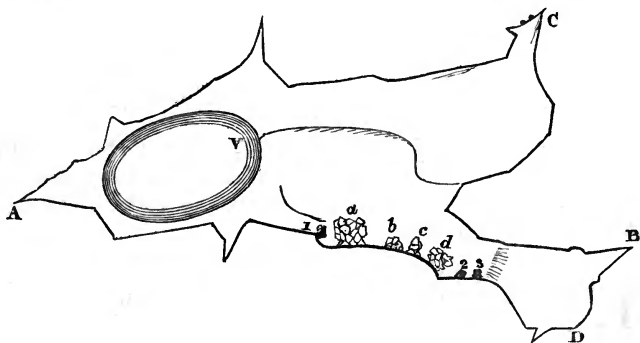
0·18 of an inch, and its greatest breadth, CD, 0·10 of an inch. It is filled with a fluid in which there is a large vacuity V. The fluid does not expand with heat, and is therefore quite different from either of the two new fluids which I discovered in Topaz and other minerals. The vacuity V does not change its place by holding the crystal in different positions, but by a violent jerk it may be moved to the other end of the cavity, and even broken into several separate vacuities. From the extreme sluggishness of its motion it is very probable that it has a considerable degree of viscidity; and that it is a fluid of low refractive power may be inferred from the refraction of light through the broken surface of one of the crystals in the cavity.

The cavity contains several crystals, as shown in the figure. Some of them have perfectly formed and brilliant facets, and are

* Communicated by the Author.

quite transparent; and all of them are moveable, descending pretty rapidly through the fluid when the specimen is inverted. One of the moveable crystals shown at a is a congeries of small crystals. The other transparent crystals are shown at b, c and d. Three little black fragments are shown at 1, 2 and 3. These three descend with much less velocity in the fluid than the transparent crystals, and have therefore a lower specific gravity. The fluid is not quite transparent, but has a slight degree of milkiness in its aspect; and it contains a good deal of a darkish flocculent matter, which descends through the fluid with a motion just perceptible. The sides of the cavity are lined with a sort of powdery deposit or thin crust. While some of the black fragments *descend* slowly in the fluid, as already stated, others *ascend*, a fact which I have never observed in the many hundreds of cavities of topaz which I have had occasion to examine. The milky colour of the fluid, the flocculent matter, and the powdery crust coexisting with the fluid*, are facts which I had never before observed.

St. Leonard's College, St. Andrews,
February 26, 1853.

XXXIX. *On Continued Fractions in Quaternions. By* Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S. &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

5. LET us now consider the continued fraction,

$$u_x = \left(\frac{\beta}{\alpha +} \right)^x u_0,$$

where u_0 and u_x are quaternions, and α, β are two rectangular vectors, connected by the relation,

$$\alpha^4 + 4\beta^2 = 0;$$

and, as a sufficient exemplification of the question, let it be supposed that α, β have the values

$$\alpha = i - k, \quad \beta = j.$$

It may easily be shown, by the rules of the present Calculus, that the expression,

$$u_1 = \frac{j}{i - k + u_0},$$

---

* In many cavities in topaz in which the fluid had disappeared I had found them lined with a crust. See Edinb. Trans. vol. x. pp. 22, 23.

gives the relations,

$$(u_1-k)^{-1} = k + i(u_0-k)^{-1}k,$$
$$S . (i \pm k)\{(u_1-k)^{-1} \pm (u_0-k)^{-1}\} = \mp 1,$$
$$(u_2-k)^{-1} - (u_0-k)^{-1} = k - i ;$$

and generally, by an indefinite repetition of the last process,

$$(u_{2n+x}-k)^{-1} - (u_x-k)^{-1} = n(k-i).$$

There is no difficulty in hence inferring that

$$\left(\frac{j}{i-k+}\right)^{\infty} u_0 = u_{\infty} = k,$$

whatever arbitrary *quaternion* (u_0) may be assumed as the original subject of the operation, which is thus indefinitely repeated. By assuming for this original operand a *vector* ρ_0 in the plane of *ik,* some geometrical* theorems arise, less general indeed in their import than the foregoing results respecting quaternions, yet perhaps not uninteresting, as belonging to a somewhat novel class, and coming fitly to be stated here, because they bear a sort of *limiting* relation to the results recently published in the Philosophical Magazine, as part of the present paper.

6. Let C and D be the extremities, and E the summit of a semicircle. Assume any point F in the same plane, and draw CQ perpendicular to DP, so that the rectangle CQ . DP may be equal to the given square CE². Then it is clear, Ist, that if the *hand* (or direction of rotation) be duly attended to, in thus drawing CQ ⊥ DP, the point Q will *coincide* with P, when the latter point P is so assumed as to coincide with the given summit E. But I say also, IInd, that if the point P be taken *anywhere else* in the same plane, and if, after deriving Q from it as above, we derive R from Q, &c., by repeating the same process, these *new* or derivative points Q, R, S, &c., will *tend,* successively

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* Note added during printing.—Since the foregoing communication was forwarded, I have perceived that the theorem VIII. of art. 6, which presented itself to me as an interpretation of the expression for $(u_1-k)^{-1}$, when $k=$CE, $i=$DE, $v_0=$CP, $u_1=$CQ, may be very simply proved by means of the two similar triangles, QEC, ECP″: and may then be employed to deduce *geometrically* all the other theorems of that article. (Each of these two triangles is similar to EP,C, if P, be on EP″, and PP, ∥ DC.) I see also that the lately published results of art. 4 may all be deduced geometrically, from the consideration of the two pairs of similar triangles, ADP, QCA, and BDP, QCB. These geometrical simplifications have only recently occurred to me ; but it may have been perceived that, on the present occasion, *geometry* has been employed merely to illustrate and exemplify the signification and validity of certain new symbolical expressions, and methods of calculation ; some account of which expressions and methods I hope to be permitted to continue. (March 15, 1853.)

and indefinitely, to coincide with the point E. I add, IIIrd, that if, from an arbitrarily assumed point s, we go *back*, on the same plan, to other points R, Q, P, &c., these new points, thus inversely derived, will *also* tend indefinitely to coincide with the same fixed summit E. IVth. The *alternate* points P, R, T, . . . are all contained on one common circular circumference; and the *other* alternate system of derived points Q, S, U, . . are all contained on *another circular locus*. Vth. These two new circles *touch* each other and the given semicircle at the given summit E; and their *centres* are *harmonic conjugates* with respect to the completed circle CED. (The same harmonic conjugation of the centres of the two loci might easily have been derived for the more general case considered in an earlier part of this paper, from the last formula of art. 4; I have found that it holds good also in another equally general case, hereafter to be considered, when the given area of the rectangle under CQ and DP is *greater* than the square on the quadrantal chord CE, in which case there *can* be *no convergence* to a limiting position, but there *may* be, under certain conditions, *circulation*.) VIth. If the chords PE, RE, TE, . . of the one circular locus, and also the chords QE, SE, UE, . . of the second locus, be prolonged through the point of contact E, so as to render the following rectangles equal to the given square or area,

$$\text{PE}\text{P}' = \text{RE}\text{R}' = \text{TE}\text{T}' = \ . \ . \ = \text{QE}\text{Q}' = \text{SE}\text{S}' = \text{UE}\text{U}' = \ . \ . \ = \text{CE}^2,$$

then not only will the points P′R′T′ . . be ranged on *one straight line*, and the points Q′S′U′ . . on another, but also the *intervals* P′R′, R′T′, . . Q′S′, S′U′, . . . will all be *equal* to each other and to the given diameter CD; and will have the same *direction* as that diameter. Thus the four points EPRT, or the four points EQSU, form what may be called an *harmonic group*, on the one or on the other circular locus: and if, as in some modern methods, the *directions* (and not merely the *lengths*) of lines be attended to, the chords EP, ER, ET, . . or EQ, ES, EU, . . . may be said to form, each set within the circle to which they belong, a species of *harmonical progression*. VIIth. The orthogonal projection of P′Q′, or Q′R′, &c., on CD, is equal in length and direction to the *half* of that given diameter. VIIIth. If P′P″ be so drawn as to be perpendicularly bisected by the common tangent to the three circles, the line P″Q′ will be equal in length and direction to the given quadrantal *chord* CE.

Observatory, February 19, 1853.

[To be continued.]

XL. *On a New Series of Organic Bodies containing Metals.
By* Dr. E. FRANKLAND, *F.C.S., Professor of Chemistry, Owen's
College, Manchester.*

[Continued from p. 170.]
Action of Zinc upon Iodide of Methyle.

WHEN iodide of methyle and zinc are exposed to a tem-
perature of about 150° C. in a sealed tube, the zinc
gradually dissolves with the evolution of gas, whilst a mass of
white crystals and a colourless mobile liquid, refracting light
strongly, occupy, after a few hours, the place of the original
materials. The gas evolved on breaking off the capillary extre-
mity of the previously exhausted decomposition tube, was col-
lected and preserved over sulphuretted water in the manner
already described : I will refer to this gas again under the name
of *α*. On cutting off the upper portion of the decomposition
tube and pouring cold distilled water upon the mobile liquid and
white mass of crystals just mentioned, a very violent action
ensued, and a column of flame several feet high shot up momen-
tarily from the mouth of the tube ; but the action soon became
more moderate, and a cork and gas-delivering tube being fitted
into the decomposition tube, the gas, after all atmospheric air
had been expelled, was collected and preserved in an apparatus
similar to that used for the gas *α*. I will call this second gas *β*.

Zincmethylium.—From a preliminary experiment, it was ascer-
tained that the gas evolved on opening the decomposition tube
possessed, before contact with water, a most insupportable and
very peculiar odour, and that, when ignited or brought in con-
tact with pure oxygen gas, it burnt with a greenish-blue flame,
producing dense white fumes : when a porcelain plate was held
in this flame, it immediately became coated with a jet black
deposit, surrounded by a white ring ; this black deposit dissolved
in dilute hydrochloric acid with evolution of hydrogen gas, and
the solution was found to contain chloride of zinc. Hence it was
evident that a volatile or gaseous compound of zinc was present
amongst the products of decomposition, and this was soon found
to reside in the mobile liquid above mentioned ; for on inverting
the tube and allowing a few drops of the liquid to escape, it
inflamed spontaneously the instant it came in contact with the
air, and produced, by its combustion, large quantities of oxide
of zinc. In order to obtain this liquid in a state of purity,
another tube was charged with iodide of methyle and excess of
zinc, and subjected to a heat of 150° or 160° C. until every
trace of iodide of methyle was decomposed. The drawn out
extremity of the tube being broken off, the included gas was
allowed to escape, and the liquid contents were then separated

from the solid ones by distillation at a gentle heat, in an atmosphere of dry hydrogen.   This was accomplished as shown in the following figure.
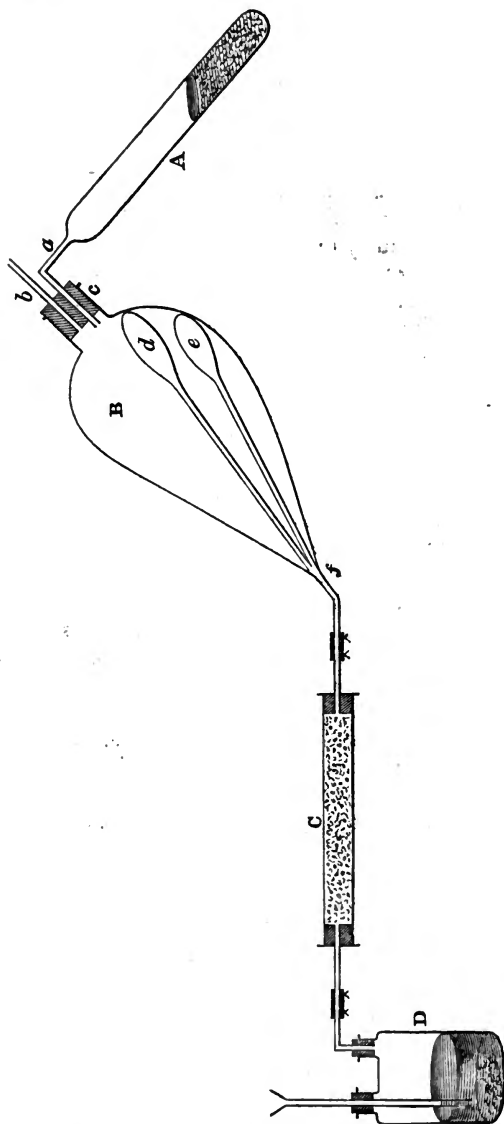
A is the decomposition tube bent at an obtuse angle at *a*, and

connected with the receiver B by the doubly perforated cork *c,* which also contains the small tube *b,* open at both ends. The receiver B is drawn out at *f* until its internal diameter is diminished to about $\frac{1}{20}$th of an inch, and this drawn out extremity is connected, by means of a caoutchouc joint, with the chloride of calcium tube C, which at its opposite extremity is in connexion with a hydrogen gas apparatus D. *d, e* are two small glass bulbs for preserving the condensed liquid. The apparatus being thus arranged, hydrogen is evolved in D, and becoming perfectly desiccated in passing through the chloride of calcium tube C, enters the receiver B at *f,* expelling the atmospheric air through the tube *b.* When the gas has thus streamed through the apparatus for at least a quarter of an hour, and every trace of air has been expelled from B and from the bulbs *d, e* by diffusion, the extremity of the tube *b* is hermetically sealed, at the same moment that the evolution of gas from D is interrupted. The drawn out extremity of the receiver B being then quickly sealed at *f;* B, *d* and *e* remain filled with pure dry hydrogen, and A with a mixture of gases free from oxygen, as any trace of this element, which might have penetrated there, would be instantaneously absorbed by its contents. B is then immersed to its neck in cold water, and a gentle heat cautiously applied to the whole length of A by means of a spirit-lamp. The mobile fluid in A soon enters into ebullition, and distils over into the receiver B; as soon as the distillation is finished and A become cold, its capillary extremity is fused off at *a* by means of a blowpipe, *a* remaining hermetically sealed. The receiver B is then removed from the water and dried; heat is applied to the side adjacent to the bulbs *d, e,* so as to expel a portion of the enclosed gas from their open ends at *f;* on subsequent cooling, a certain quantity of the liquid rises into these bulbs, which are alternately heated and cooled, until every trace of the liquid has not only entered them but passed entirely into their expanded portion, so as to leave the capillary limbs filled with hydrogen. It is of importance that the whole of the liquid should be forced to enter these bulbs, otherwise, on subsequently opening the mouth of the receiver, it inflames, causing the expulsion of the liquid from the bulbs, and thus rendering the experiment abortive. The cork *c* is then removed, and the bulbs *d, e* extracted as quickly as possible, the open capillary extremities being immediately sealed before the blowpipe. The bulbs, having been previously weighed, the increase denotes the weight of the included liquid. The residue in A was found scarcely to effervesce with water, and consisted of iodide of zinc mixed with the excess of metallic zinc employed.

I have fixed the composition of the liquid obtained as above

described, and proved it to be a compound of one atom of zinc and one atom of methyle, by the following experiments :—

I. One of the bulbs above mentioned was opened beneath an inverted receiver filled with recently boiled distilled water; its contents were rapidly resolved into hydrated oxide of zinc and a permanent gas, which last was submitted to eudiometrical examination ; the following results were obtained :—

The action of fuming sulphuric acid did not produce any diminution of volume.

The gas was nearly insoluble in absolute alcohol.

A eudiometrical combustion yielded the following results :—

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0°C. and 1 metre press.
Volume of gas used (moist) . .	122·5	18·7 C.	mm 602·2	mm 747·8	14·86
Volume after admission of O (moist)	287·9	18·7	418·4	747·6	84·39
Volume after explosion (moist) .	232·1	18·6	479·7	747·4	54·71
Volume after absorption of CO^2 (dry)	188·2	18·6	519·4	747·5	40·19
Volume after admission of H (dry)	549·4	18·4	162·7	747·6	301·04
Volume after explosion (moist) .	425·7	18·4	279·0	747·6	180·62

Volume of combustible gas.	Oxygen consumed.	CO^2 generated.
14·86	29·39	14·52
1 :	1·98 :	·98

In order to ascertain whether the gas was a single compound or a gaseous mixture, and also to determine its specific gravity, it was submitted to diffusion in an apparatus which I have already described* : the following results were obtained :—

I. In Diffusion Eudiometer.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0°C. and 1 metre press.
Volume of gas used (dry) . . .	173·0	19·0 C.	mm 20·1	mm 741·1	116·61
Volume after diffusion (dry) . .	144·3	19·2	46·3	740·8	93·63

* Quarterly Journal of Chemical Society, vol. ii. p. 283.

II. Estimation of Oxygen in residual gas.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0° C. and 1 metre press.
			mm	mm	
Volume of gas used (moist) . .	117·5	19·3 C.	53·1	740·6	75·44
Volume after absorption of O (dry)	101·2	17·0	69·3	742·8	64·16

III. Combustion of Gas remaining after absorption of Oxygen.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0° C. and 1 metre press.
			mm	mm	
Volume of gas used (moist) . .	155·9	17·3 C.	558·7	743·3	24·91
Volume after admission of O (moist)	302·7	17·7	399·0	744·0	93·78
Volume after explosion (moist) .	270·2	17·6	433·2	744·2	75·13
Volume after absorption of CO^2 (dry)	246·3	17·8	458·7	744·7	66·13
Volume after admission of H (dry)	522·3	17·9	182·4	745·1	275·80
Volume after explosion (moist) .	349·5	18·0	350·5	745·0	124·30

The gas remaining after diffusion and subsequent absorption of oxygen therefore contained in 24·91 vols. 15·63 vols. nitrogen and 9·28 vols. combustible gas, which last was a single gas and not a mixture, since it consumed the same amount of oxygen and generated the same amount of carbonic acid after as before diffusion :—

Volume of combustible gas.		Oxygen consumed.		CO^2 generated.
9·28		18·37		9·00
1	:	1·98	:	·97

Experiments Nos. I., II. and III., taken together, enable us to ascertain the volume of the gas which escaped and that of the air which entered during the diffusion experiment; these volumes are as follow :—

Volume of gas escaped . . .	86·95
Volume of air entered . . .	63·97

Hence, according to the well-known law of diffusion, the specific gravity of the gas must be ·5413.

The gas is therefore hydride of methyle (light carburetted hydrogen), 1 vol. of which consumes 2 vols. oxygen and generates 1 vol. carbonic acid, and the specific gravity of which is ·5528, numbers which correspond almost exactly with those obtained by experiment.

II. A glass jar, graduated in cubic centimetres, was filled with recently boiled distilled water, to which about twenty drops of sulphuric acid had been added, and inverted in a shallow glass dish containing the same liquid; the other bulb was then introduced into the inverted jar, and its capillary extremity broken off against the side of the vessel; the water now slowly gained access to the liquid in the bulb and steady decomposition ensued, the oxide of zinc dissolving as fast as formed, in the dilute sulphuric acid, and the hydride of methyle collecting in the inverted jar. When the decomposition was quite complete, the volume of gas was read off with the usual corrections for temperature and pressure, the graduated jar rinsed out and removed, and the solution of sulphate of zinc in the glass dish, after being evaporated to a smaller bulk, was treated with carbonate of potash and the zinc precipitated as basic carbonate, and weighed as oxide with the usual precautions: the following results were obtained:—

·3109 grm. gave ·2660 grm. oxide of zinc, and 138·15 cubic centimetres dry hydride of methyle, at 0°·C and 760 mm. pressure, equivalent to ·0930 grm.

These numbers agree sufficiently with those calculated from the formula $C^2 H^3 Zn$, when we consider that every trace of oxygen, which gained admission to the æthereal fluid before its decomposition, would diminish the volume of hydride of methyle, which would also be liable to further diminution, from the solvent action of the fluid over which it was determined:—

		Calculated.		Found.
1 equiv. of Methyle	.	15	31·56	29·91
1 equiv. of Zinc	. .	32·52	68·44	68·67
		47·52	100·00	98·58

This compound, for which I propose the name Zincmethylium, possesses the following properties:—It is a colourless, transparent and very mobile liquid, refracting light strongly, and possessing a peculiar penetrating and insupportable odour; it is very volatile, but I have not yet been able to determine its boiling-point with accuracy.

Zincmethylium combines directly with oxygen, chlorine, iodine, &c., forming somewhat unstable compounds, a description of

which I reserve for a future communication. Its affinity for oxygen is even more intense than that of potassium; in contact with atmospheric air it instantaneously ignites, burning with a beautiful greenish-blue flame, and forming white clouds of oxide of zinc; in contact with pure oxygen it burns with explosion, and the presence of a small quantity of its vapour in combustible gases gives them the property of spontaneous inflammability in oxygen. Thrown into water, zincmethylium decomposes that liquid with explosive violence and with the evolution of heat and light; when this action is moderated, so as to prevent any great rise of temperature, the sole products of the decomposition are oxide of zinc and hydride of methyle,
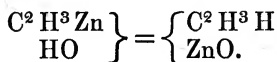
$$\left. \begin{array}{c} C^2\,H^3\,Zn \\ HO \end{array} \right\} = \left\{ \begin{array}{c} C^2\,H^3\,H \\ ZnO. \end{array} \right.$$

The extraordinary affinity of zincmethylium for oxygen, its peculiar composition, and the facility with which it can be procured, cannot fail to cause its employment for a great variety of transformations in organic compounds; by its agency there is every probability that we shall be able to replace oxygen, chlorine, &c., atom for atom, by methyle, and thus produce entirely new series of organic compounds, and obtain clearer views of the rational constitution of others. I intend to pursue this branch of the subject whilst studying the compounds of zincmethylium and the corresponding bodies containing æthyle and amyle.

Examination of the Gas α.—A quantity of this gas, after standing over sulphuretted water until all traces of iodide of methyle vapour had been absorbed, was transferred into a suitable flask for the determination of its specific gravity; the following numbers were obtained:—

Temperature of room	18°·6 C.
Height of barometer	754·2 mm.
Height of inner column of mercury .	15·2 mm.
Weight of flask and gas	35·4161 grms.
Temperature in balance case . . .	19°·6 C.
Weight of flask and dry air . . .	35·4500 grms.
Temperature in balance case . . .	20°·2 C.
Capacity of flask	140·51 cubic centims.

From these data the specific gravity was calculated to be ·79598

The eudiometrical analysis of the gas gave the following results:—

I. In Short Eudiometer.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0°C. and 1 metre press.
			mm	mm	
Volume of gas used (dry) . .	194·7	18·6 C.	2·1	754·2	137·06
Volume after action of fuming SO³ (dry)	194·8	18·7	2·2	753·5	137·03
Volume after removal of specimen for combustion (dry)	153·8	19·0	22·0	741·0	103·38
Volume after action of alcohol . .	82·0	18·7	7·0	741·4	53·72

II. In Combustion Eudiometer.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0°C. and 1 metre press.
			mm	mm	
Volume of gas used (moist) . .	101·5	18·9 C.	621·7	752·4	10·86
Volume after admission of O (moist)	388·1	18·6	311·7	752·0	154·20
Volume after explosion (moist) .	354·6	18·5	345·8	752·2	129·71
Volume after absorption of CO³ (dry)	321·1	18·3	376·9	752·0	112·88
Volume after admission of H (dry)	745·1	18·7	·8	751·1	523·19
Volume after explosion (moist) .	428·3	18·7	274·1	750·5	184·54

III.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0°C. and 1 metre press.
			mm	mm	
Volume of gas used (moist) . .	104·2	18·8 C.	619·2	750·5	11·23
Volume after admission of O (moist)	371·2	18·9	330·6	750·2	140·04
Volume after explosion (moist) .	335·0	18·9	367·7	750·2	114·76
Volume after absorption of CO² (dry)	299·0	18·2	401·3	750·3	97·83
Volume after admission of H (dry)	707·0	18·4	26·9	750·6	479·33
Volume after explosion (moist) .	431·6	18·7	273·4	749·8	185·96

Analysis No. I. proves the absence of all the members of the olefiant gas family, and also that the mixture consists of—

<div style="text-align:center">

Gas absorbable by alcohol . . . 48·04
Gas unabsorbable by alcohol . . 51·96
—————
100·00

</div>

The behaviour of the iodides of æthyle and amyle in contact with zinc*, led me to expect that the gaseous products of the decomposition of iodide of methyle by the same metal would consist of methyle, hydride of methyle, and the first member of the olefiant gas series, methylene; but in addition to the proof of the absence of this latter body afforded by the absence of all absorption by fuming sulphuric acid, analyses Nos. II. and III. demonstrate the impossibility of methylene being a constituent of the gaseous mixture; for on constructing three equations in which the volumes of methyle, hydride of methyle, and methylene are expressed, the value obtained for the last gas is invariably a small negative quantity. The volumes of methyle and hydride of methyle are readily found by the two following equations, in which the volume of combustible gas is represented by A, the contraction produced by explosion with excess of oxygen by B, and the volumes of methyle and hydride of methyle respectively, by x and y, the contraction produced by the explosion of methyle with excess of oxygen being 2·5 times its own volume, and that produced by the explosion of hydride of methyle twice its own volume :—

$$x + y = A,$$
$$\frac{5}{2}x + 2y = B.$$

The values of x and y may therefore be thus expressed :—

$$x = 2B - 4A,$$
$$y = 5A - 2B.$$

According to analysis No. II., 10·88 vols. of combustible gas produced a contraction, on explosion with oxygen, equal to 24·49 vols.; and in analysis No. III., 11·23 vols. of combustible gas produced a contraction, on explosion, equal to 25·28 vols. Hence, by the application of the foregoing equations, the per-centage composition of the gaseous mixture may be expressed as follows :—

	II.	III.	Mean.
Methyle . . .	50·18	50·22	50·20
Hydride of methyle	49·82	49·78	49·80
	100·00	100·00	100·00

* Journal of the Chemical Society, vol. ii. p. 265, and vol. iii. p. 30.

This result is confirmed by the action of alcohol in analysis No. I., and also by the determination of the specific gravity of the mixed gases, which agrees very closely with that deduced from the above numbers, as is seen from the following comparison :—

Methyle . . . $50 \cdot 20 \times 1 \cdot 0365 = 52 \cdot 0323$
Hydride of methyle $49 \cdot 80 \times \quad \cdot 5528 = 27 \cdot 5294$

$$\frac{100 \cdot 00 \qquad\qquad 79 \cdot 5617}{100} = \cdot 795617$$

Specific gravity found by experiment .   . $= \cdot 79598$

The origin of the hydride of methyle in the above gaseous mixture is readily perceived, when the volatility of zincmethylium and the method of collecting the gas are taken into consideration ; on opening the decomposition-tube beneath water, a copious effervescence was observed wherever the evolved gas came in contact with water ; and as this effervescence was accompanied by the formation of a flocculent precipitate of oxide of zinc, it could only be caused by the presence of the vapour of zincmethylium, which, on coming in contact with water, would be instantaneously decomposed into oxide of zinc and hydride of methyle.

I have not yet endeavoured to procure the methyle free from admixture with hydride of methyle, but have no doubt that, by collecting the gas as evolved from the decomposition-tube over mercury, and absorbing the zincmethylium vapour by dry iodine, the methyle would be left in a state of purity. It perfectly resembles in its properties, chemical and physical, the methyle procured by Kolbe from the electrolysis of acetic acid[*].

Examination of the Gas β.—This gas, evolved by the action of water upon the solid and liquid products of the decomposition of iodide of methyle by zinc, proved, as might have been anticipated, to be pure hydride of methyle, derived from the decomposition of the zincmethylium with which the crystalline residue of iodide of zinc was saturated. Its eudiometrical analysis yielded the following results :—

I. In Short Eudiometer.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0°C. and 1 metre press.
Volume of gas used (dry) . . .	168·9	18·6 C.	mm 3·6	mm 750·2	118·05
Volume after action of fuming SO³(dry)	169·5	18·7	3·7	749·8	118·35

[*] Journal of the Chemical Society, vol. ii. p. 173.

One volume of absolute alcohol, at 19° C. and 732·6 mm. pressure, absorbed ·175 vol. of this gas.

II. In Combustion Eudiometer.

	Observed volume.	Temp.	Difference of mercury level.	Barom.	Corrected vol. at 0° C. and 1 metre press.
			mm	mm	
Volume of gas used (moist) . .	126·7	18·9 C.	595·1	744·9	15·83
Volume after admission of O (moist)	344·9	18·9	357·7	744·8	119·64
Volume after explosion (moist) .	295·9	18·9	408·7	744·5	88·44
Volume after absorption of CO² (dry)	260·7	18·6	446·1	744·3	72·78
Volume after admission of H (dry)	705·7	18·5	24·6	741·3	473·66
Volume after explosion (moist) .	513·0	18·6	193·6	741·1	255·35

These results correspond almost exactly with those yielded by hydride of methyle, 1 vol. of which requires 2 vols. of oxygen for combustion, and generates 1 vol. of carbonic acid.

Volume of combustible gas.		Oxygen consumed.		CO² generated.
15·83		31·04		15·66
1	:	1·96	:	·99

By the action of zinc upon iodide of methyle, therefore, two distinct decompositions take place, viz. 1st, the decomposition of iodide of methyle by zinc with the production of iodide of zinc and methyle,

$$\left. \begin{array}{c} C^2\,H^3\,I \\ Zn \end{array} \right\} = \left\{ \begin{array}{l} C^2\,H^3 \\ Zn\,I, \end{array} \right.$$
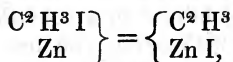
and 2nd, the decomposition of iodide of methyle by zinc, with the formation of iodide of zinc and zincmethylium,

$$\left. \begin{array}{c} C^2\,H^3\,I \\ 2Zn \end{array} \right\} = \left\{ \begin{array}{l} C^2\,H^3\,Zn \\ ZnI. \end{array} \right.$$
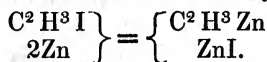
Zincæthylium.—This body is formed under precisely the same circumstances as zincmethylium, iodide of æthyle being substituted for iodide of methyle; it is a colourless and transparent liquid, refracting light strongly, and having a peculiar penetrating odour; it is less volatile than zincmethylium, and is not so readily prepared pure, owing to its retention of a small quantity of æthyle gas in solution; its affinities are also somewhat weaker than zincmethylium, and it only takes fire in air spontaneously

when exposed in considerable quantity. When allowed to absorb oxygen slowly, it forms a white amorphous oxide; it combines also directly with iodine, chlorine and bromine.   In contact with water it is instantaneously decomposed into oxide of zinc and hydride of æthyle,

$$\left. \begin{array}{c} C^4\,H^5\,Zn \\ HO \end{array} \right\} = \left\{ \begin{array}{c} C^4\,H^5,\,H \\ ZnO. \end{array} \right.$$
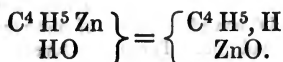
Its formula must therefore be $C^4\,H^5\,Zn$.   I reserve for a future communication the complete history of this and the following compound.

Zincamylium.—This body is generated when iodide of amyle is decomposed by zinc at the temperature of 180° C.   It is a colourless and transparent liquid which emits white fumes in contact with the air, but does not spontaneously inflame; it is decomposed in contact with water into oxide of zinc and hydride of amyle,

$$\left. \begin{array}{c} C^{10}\,H^{11}\,Zn \\ HO \end{array} \right\} = \left\{ \begin{array}{c} C^{10}\,H^{11}\,H \\ ZnO. \end{array} \right.$$
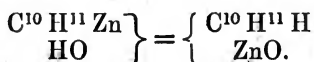
From this circumstance, and its analogy with zincmethylium, there can be no doubt that its formula is $C^{10}\,H^{11}\,Zn$.

Action of Mercury upon Iodide of Methyle in presence of Light.

When iodide of methyle is exposed to sunlight in contact with metallic mercury, it soon becomes coloured red from the separation of free iodine; after several hours' exposure this coloration disappears, and a small quantity of the yellow iodide of mercury subsides to the bottom of the liquid : after the action of sunlight for several days, the bulk of the mercury is observed to have considerably diminished, and white crystals begin to be deposited around the sides of the glass vessel : finally, after about a week's exposure, the liquid solidifies to a colourless crystalline mass : when this is digested with æther, the new compound dissolves, and is thus separated from metallic mercury, and from the small portion of iodide of mercury which is collaterally formed.   Only a very small quantity of gas is evolved during the formation of the white crystalline compound.   By spontaneous evaporation the æthereal solution solidifies to a mass of minute colourless crystalline scales : these, dried *in vacuo* and submitted to analysis, yielded the following numbers :—

I. ·3170 grm. dissolved in alcohol and treated with nitrate of silver, gave ·2142 grm. iodide of silver.

II. ·6205 grm. burnt with oxide of copper, gave ·0813 grm. carbonic acid, ·0505 grm. water, and ·5960 grm. protoiodide of mercury.   The iodide of mercury, a small portion of which was decomposed into metallic mercury and periodide, collected

as an incrustation at the front end of the combustion-tube, about a couple of inches of which had been left empty for this purpose, and projected from the furnace, the heat being so regulated that none of the iodide passed into the chloride of calcium tube, whilst none of the watery vapour condensed in the combustion-tube. When the analysis was concluded, the weight of the protoiodide of mercury, mixed with traces of periodide and metallic mercury, was determined by cutting off the part of the combustion-tube containing it, and ascertaining its weight before and after the iodide was removed. The numbers obtained agree very closely with the formula $C^2 H^3 HgI$, which requires the following values:—

	Calculated.		Found.	
			I.	II.
2 equivs. Carbon	12	3·51	...	3·57
3 equivs. Hydrogen	3	·88	...	·90
1 equiv. Mercury	200	58·51	...	⎫
1 equiv. Iodine	126·84	37·10	36·56	⎬ 96·05
	341·84	100·00		100·52

This compound is therefore evidently the iodide of a new organo-metallic body, consisting of one atom of methyle and one atom of mercury, and for which I propose the name *hydrargyromethylium*: it is formed by the direct union of one atom of mercury with one atom of iodide of methyle, under the influence of light,

$$\left.\begin{array}{c} C^2 H^3 I \\ Hg \end{array}\right\} = C^2 H^3 HgI.$$
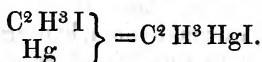
Iodide of hydrargyromethylium is a white solid, crystallizing in minute nacreous scales, which are insoluble in water, moderately soluble in alcohol, and very soluble in æther and iodide of methyle; by the spontaneous evaporation of these solutions the crystals are again deposited unchanged. Iodide of hydrargyromethylium is slightly volatile at ordinary temperatures, and exhales a weak but peculiarly unpleasant odour, which leaves a nauseous taste upon the palate for several days; at 100° C. the volatility is much greater, and the crystals are rapidly dissipated at this temperature when exposed to a current of air. At 143° C. it fuses and sublimes without decomposition, condensing in brilliant and extremely thin crystalline plates. In contact with the fixed alkalies and ammonia, it is converted into oxide of hydrargyromethylium, which is dissolved by excess of all these reagents; from these solutions sulphide of ammonium throws down sulphide of hydrargyromethylium as a slightly yellow flocculent precipitate of a peculiar and most insupportable odour.

I have not yet further examined the reactions of this remarkable body, nor have I attempted the isolation of the hydrargyromethylium.

A corresponding compound containing amyle is formed, though with difficulty, under similar circumstances, but I have not yet succeeded in producing one containing æthyle, the iodide of this radical yielding, as I have shown*, when exposed to sunlight in contact with mercury, iodide of mercury, and a mixture of æthyle, hydride of æthyle and olefiant gases.

I have also made some preliminary experiments with other metals, and find that most of them are capable of thus entering into combination with the organic groups, methyle, æthyle, and amyle; amongst those which thus combine under the influence of light most readily, and seem to promise the most interesting results, I may mention arsenic, antimony, chromium, iron, manganese and cadmium. I hope to have the honour of laying before the Royal Society, at an early period, the results of my experiments upon these compounds.

Imperfect as our knowledge of the organo-metallic bodies may yet appear, I am unwilling to close this memoir without directing attention to some peculiarities in the habits of these compounds, which promise at least to throw some light upon their rational constitution, if they do not lead to extensive modifications of our views respecting chemical compounds in general, and especially that interesting class termed conjugate compounds.

That stanæthylium, zincmethylium, hydrargyromethylium, &c. are perfectly analogous to cacodyle there can be no reasonable doubt, inasmuch as, like that body, they combine directly with the electro-negative metalloids forming true salts, from which, in most cases, and probably in all, the original group can be again separated unaltered, and therefore any view which may be taken of the new bodies must necessarily be extended to cacodyle. The discovery and isolation of this so-called organic radical by Bunsen was certainly one of the most important steps in the development of organic chemistry, and one, the influence of which upon our theoretical views of the constitution of certain classes of organic compounds, can scarcely be too highly estimated. It was impossible to consider the striking features in the behaviour of this body, without finding in them a most remarkable confirmation of the theory of organic radicals, as propounded by Berzelius and Liebig.

The formation of cacodyle, its habits, and the products of its decomposition, have for some time left no doubt of the existence

* Journal of the Chemical Society, vol. iii. p. 331.

of methyle ready formed in this body; and Kolbe*, in developing his views on the so-called conjugate compounds, has proposed to regard it as arsenic conjugated with two atoms of methyle $((C^2 H^3)^2 As)$. So long as cacodyle was an isolated example of an organo-metallic body, this view of its rational composition, harmonizing as it did so well with the facts elicited during the route of cacodyle through its various combinations and decompositions, could scarcely be contested; but now, since we have become acquainted with the properties and reactions of a considerable number of analogous bodies, circumstances arise which I consider militate greatly against this view, if they do not render it absolutely untenable. According to the theory of conjugate radicals just alluded to, cacodyle and its congeners, so far as they are at present known, would be thus represented :—

Cacodyle	$(C^2 H^3)^2$ As.
Oxide of cacodyle	$(C^2 H^3)^2$ AsO.
Cacodylic acid	$(C^2 H^3)^2$ AsO^3.
Stanmethylium	$(C^2 H^3)$ Sn.
Stanæthylium	$(C^4 H^5)$ Sn.
Oxide of stanæthylium	$(C^4 H^5)$ SnO.
Stanamylium	$(C^{10} H^{11})$ Sn.
Zincmethylium	$(C^2 H^3)$ Zn.
Zincæthylium	$(C^4 H^5)$ Zn.
Zincamylium	$(C^{10} H^{11})$ Zn.
Stibæthine (stibæthyle)	$(C^4 H^5)^3$ Sb.
Binoxide of stibæthine	$(C^4 H^5)^3$ SbO^2.
Oxide of stibmethylium	$(C^2 H^3)^4$ SbO.
Hydrargyromethylium	$(C^2 H^3)$ Hg.
Iodide of hydrargyromethylium .	$(C^2 H^3)$ HgI.

It is generally admitted, that when a body becomes conjugated, its essential chemical character is not altered by the presence of the conjunct: thus for instance, the series of acids $C^n H^n O^4$, formed by the conjunction of the radicals $C^n H^{(n+1)}$ with oxalic acid, have the same neutralizing power as the original oxalic acid; and, therefore, if we assume the organo-metallic bodies above mentioned to be metals conjugated with various hydrocarbons, we might reasonably expect that the chemical relations of the metal to oxygen, chlorine, sulphur, &c. would remain unchanged; a glance at the formulæ of these compounds will however suffice to show us that this is far from being the case: it is true that cacodyle forms protoxide of cacodyle and cacodylic acid, corresponding the one to a somewhat hypothetical protoxide of arsenic, which, if it exist, does not seem to possess any well-defined basic character, and the other to arsenious acid;

* Journal of the Chemical Society, vol. iii. p. 372.

but no compound corresponding to arsenic acid can be formed, and yet it cannot be urged that cacodylic acid is decomposed by the powerful reagents requisite to procure further oxidation, for concentrated nitric acid may be distilled from cacodylic acid without decomposition or oxidation in the slightest degree; the same anomaly presents itself even more strikingly in the case of stanæthylium, which, if we are to regard it as a conjugate radical, ought to combine with oxygen in two proportions at least, to form compounds corresponding to protoxide and peroxide of tin; now stanæthylium rapidly oxidizes when exposed to the air and is converted into pure protoxide, but this compound exhibits none of that powerful tendency to combine with an additional equivalent of oxygen, which is so characteristic of protoxide of tin; nay, it may even be boiled with dilute nitric acid without evincing any signs of oxidation: I have been quite unable to form any higher oxide than that described; it is only when the group is entirely broken up and the æthyle separated, that the tin can be induced to unite with another equivalent of oxygen. Stibæthyle also refuses to unite with more or less than two equivalents of oxygen, sulphur, iodine, &c., and thus forms compounds, which are not at all represented amongst the combinations of the simple metal antimony.

When the formulæ of inorganic chemical compounds are considered, even a superficial observer is struck with the general symmetry of their construction; the compounds of nitrogen, phosphorus, antimony and arsenic especially exhibit the tendency of these elements to form compounds containing 3 or 5 equivs. of other elements, and it is in these proportions that their affinities are best satisfied; thus in the ternal group we have NO^3, NH^3, NI^3, NS^3, PO^3, PH^3, PCl^3, SbO^3, SbH^3, $SbCl^3$, AsO^3, AsH^3, $AsCl^3$, &c.; and in the five-atom group NO^5, NH^4O, NH^4I, PO^5, PH^4I, &c. Without offering any hypothesis regarding the cause of this symmetrical grouping of atoms, it is sufficiently evident, from the examples just given, that such a tendency or law prevails, and that, no matter what the character of the uniting atoms may be, the combining power of the attracting element, if I may be allowed the term, is always satisfied by the same number of these atoms. It was probably a glimpse of the operation of this law amongst the more complex organic groups which led Laurent and Dumas to the enunciation of the theory of types; and had not those distinguished chemists extended their views beyond the point to which they were well supported by then existing facts,—had they not assumed, that the properties of an organic compound are dependent upon the position and not upon the nature of its single atoms, that theory would undoubtedly have contributed to the development of the science

to a still greater extent than it has already done; such an assumption could only have been made at a time when the data upon which it was founded were few and imperfect, and, as the study of the phænomena of substitution progressed, it gradually became untenable, and the fundamental principles of the electro-chemical theory again assumed their sway. The formation and examination of the organo-metallic bodies promise to assist in effecting a fusion of the two theories which have so long divided the opinions of chemists, and which have too hastily been considered irreconcileable; for, whilst it is evident that certain types of series of compounds exist, it is equally clear that the nature of the body derived from the original type is essentially dependent upon the electro-chemical character of its single atoms, and not merely upon the relative position of those atoms. Let us take, for instance, the compounds formed by zinc and antimony; by combination with 1 equiv. of oxygen the electro-positive quality of the zinc is nearly annihilated; it is only by the action of the highly oxidizing peroxide of hydrogen that the metal can be made to form a very unstable peroxide; but when zinc combines with 1 equiv. of methyle or æthyle, its positive quality, so far from being neutralized, is exalted by the addition of the positive group, and the compound now exhibits such intense affinity for the electro-negative elements as to give it the property of spontaneous inflammability. Teroxide of antimony has also little tendency to pass into a higher state of oxidation; but when its three atoms of oxygen are replaced by the electro-positive æthyle, as in stibæthine, that affinity is elevated to the intense degree which is so remarkable in this body.

Taking this view of the so-called conjugate organic radicals, and regarding the oxygen, sulphur, or chlorine compounds of each metal as the true molecular type of the organo-metallic bodies derived from it by the substitution of an organic group for oxygen, sulphur, &c., the anomalies above mentioned entirely disappear, and we have the following inorganic types and organo-metallic derivatives:—

Inorganic Types.		Organo-metallic Derivatives.	
$As \left\{ \begin{array}{c} S \\ S \end{array} \right\}$	. . .	$As \left\{ \begin{array}{c} C^2 H^3 \\ C^2 H^3 \end{array} \right\}$	Cacodyle.
$As \left\{ \begin{array}{c} O \\ O \\ O \end{array} \right\}$	. . .	$As \left\{ \begin{array}{c} C^2 H^3 \\ C^2 H^3 \\ O \end{array} \right\}$	Oxide of Cacodyle.
$As \left\{ \begin{array}{c} O \\ O \\ O \\ O \\ O \end{array} \right\}$	. . .	$As \left\{ \begin{array}{c} C^2 H^3 \\ C^2 H^3 \\ O \\ O \\ O \end{array} \right\}$	Cacodylic acid.

Inorganic Types.   Organo-metallic Derivatives.

Zn O . . .   Zn $(C^2 H^3)$ Zincmethylium.

$Zn \begin{Bmatrix} O \\ O_x \end{Bmatrix}$. . . $Zn \begin{Bmatrix} C^2 H^3 \\ O_x \end{Bmatrix}$ Oxide of Zincmethylium.

$Sb \begin{Bmatrix} O \\ O \\ O \end{Bmatrix}$. . . $Sb \begin{Bmatrix} C^4 H^5 \\ C^4 H^5 \\ C^4 H^5 \end{Bmatrix}$ Stibæthine.

$Sb \begin{Bmatrix} O \\ O \\ O \\ O \\ O \end{Bmatrix}$. . . $Sb \begin{Bmatrix} C^4 H^5 \\ C^4 H^5 \\ C^4 H^5 \\ O \\ O \end{Bmatrix}$ Binoxide of Stibæthine.

$Sb \begin{Bmatrix} O \\ O \\ O \\ O \\ O \end{Bmatrix}$. . . $Sb \begin{Bmatrix} C^4 H^5 \\ C^4 H^5 \\ C^4 H^5 \\ C^4 H^5 \\ O \end{Bmatrix}$ Oxide of Stibæthylium.

Sn O . . .   Sn $(C^4 H^5)$ Stanæthylium.

$Sn \begin{Bmatrix} O \\ O \end{Bmatrix}$. . . $Sn \begin{Bmatrix} C^4 H^5 \\ O \end{Bmatrix}$ Oxide of Stanæthylium.

$Hg \begin{Bmatrix} I \\ I \end{Bmatrix}$. . . $Hg \begin{Bmatrix} C^2 H^3 \\ I \end{Bmatrix}$ Iodide of Hydrargyro-
methylium.

The only compound which does not harmonize with this view is ethostibylic acid, to which Löwig assigns the formula $C^4 H^5 SbO^5$; but as that chemist has not yet fully investigated this compound, it is possible that further research may satisfactorily elucidate its apparently anomalous composition.

It is obvious that the establishment of this view of the constitution of the organo-metallic bodies will remove them from the class of organic radicals, and place them in the most intimate relation with ammonia and the bases of Wurtz, Hofmann and Paul Thenard; indeed, the close analogy existing between stibæthine and ammonia, first suggested by Gerhardt, has been most satisfactorily demonstrated by the behaviour of stibæthine with the haloid compounds of methyle and æthyle. Stibæthine furnishes us, therefore, with a remarkable example of the operation of the law of symmetrical combination above alluded to, and shows that the formation of a five-atom group from one containing three atoms can be effected by the assimilation of two atoms, either of the same or of opposite electro-chemical character: this remarkable circumstance suggests the following question:—Is this behaviour common also to the corresponding compounds of arsenic, phosphorus and nitrogen; and can the position of each of the five atoms, with which these elements respectively

combine, be occupied indifferently by an electro-negative or an electro-positive element? This question, so important for the advance of our knowledge of the organic bases and their congeners, cannot now long remain unanswered.

If the views which I have just ventured to suggest should be as well borne out by future researches as they are by the facts already known, they must occasion a profound change in the nomenclature of the extensive series of compounds affected by them: I have not, however, ventured to introduce this new system of nomenclature, even in the case of the new bodies described in this memoir, since hasty changes of this kind, unless absolutely necessary, are always to be deplored. In accordance with the suggested view of the constitution of the organo-metallic compounds, the following plan of nomenclature would probably be found most convenient.

Arsenic Compounds.

$(C^2 H^3)^2 As$ . . .	Bimethide of arsenic.
$(C^2 H^3)^2 AsO$ . .	Bimethoxide of arsenic.
$(C^2 H^3)^2 AsO^3$ . .	Bimetharsenic acid.
$(C^2 H^3)^2 AsO^3 + KO.$	Bimetharseniate of potash.

Zinc Compounds.

$(C^2 H^3) Zn$ . . .	Methide of zinc.
$(C^4 H^5) Zn$ . . .	Ethide of zinc.
$(C^{10} H^{11}) Zn$ . . .	Amylide of zinc.

Tin Compounds.

$(C^2 H^3) Sn$ . . .	Methide of tin.
$(C^2 H^3) SnI$ . . .	Methiodide of tin.
$(C^4 H^5) Sn$ . . .	Ethide of tin.
$(C^4 H^5) SnO$ . . .	Ethoxide of tin.
$(C^4 H^5) Sn Cl$ . .	Ethochloride of tin.
$(C^4 H^5) SnO SO^3$.	Sulphate of ethoxide of tin.
$(C^{10} H^{11}) Sn$ . . .	Amylide of tin.
$(C^{10} H^{11}) SnO$ . .	Amyloxide of tin.

Antimony Compounds.

$(C^2 H^3)^3 Sb$ . . .	Termethide of antimony.
$(C^2 H^3)^4 SbO$ . .	Quadromethoxide of antimony.
$(C^4 H^5)^3 Sb$ . . .	Terethide of antimony.
$(C^4 H^5)^3 Sb O^2$ . .	Terethobinoxide of antimony.

Mercury Compounds.

$(C^2 H^3) Hg$ . . .	Methide of mercury.
$(C^2 H^3) HgI$ . . .	Methiodide of mercury.

In naming the new bodies described in the present paper, I have, in conformity with the nomenclature of the organic bases, adopted the principle of employing the termination "*ium*" when the body unites with one equivalent of oxygen, chlorine, sulphur, &c., like ammonium, and the terminal "*ine*" when, like ammonia, it combines with two additional atoms.

XLI. *On Changes of the Sea-Level effected by existing Physical Causes during stated Periods of Time.* By ALFRED TYLOR, F.G.S.*

Introduction.

THE First Part of the ensuing paper is occupied with the details of the probable amount of the solid matter annually brought into the ocean by rivers and other agents, in suspension and solution; and the conclusion is arrived at, that the quantity of detritus thus distributed on the sea-bottom would displace enough water to cause an elevation of the ocean-level to the extent of at least 3 inches in 10,000 years.

In the Second Part an endeavour is made to compute the number of such periods of 10,000 years that must have elapsed during the accumulation of the immense mass of recent freshwater strata said to exist in the valley of the Mississippi.

The calculation as to the latter is made from the *data* collected by observers in America, of the extent of the deposit in question; and it is here supposed, first, that in former periods the *same* quantity of mud as at present has been annually carried into the Gulf of Mexico; and secondly, that the amount of sediment deposited on the delta and plains of the Mississippi does not exceed *one-tenth part* of the solid material which has been carried out (suspended in the water of the river) into distant parts of the Gulf of Mexico, or into the Atlantic Ocean itself.

From recent accounts by Mr. C. Ellet, of the United States, it appears that a column of fresh water, $1\frac{1}{4}$ mile wide and about 7 feet deep, is constantly entering the Gulf of Mexico at a speed of 2 to $2\frac{1}{2}$ miles per hour, and floats on the surface of a stratum of salt water, to which it partially communicates its own velocity. And below this a stratum of sea-water is found to be flowing in an opposite direction to that of the two strata of fresh and salt water above it. See figs. 1 and 2.

From the data submitted, it would appear that the accumulation of the alluvial deposit of the Mississippi must have occupied a great number of periods, during each of which an elevation of the sea-level of 3 inches may have occurred.

* Communicated by the Author.

Fig. 1. *Section of the Bar of the Mississippi.*

a, b, c, d, 1st, 2nd, 3rd, and 4th Bars of marine formation.
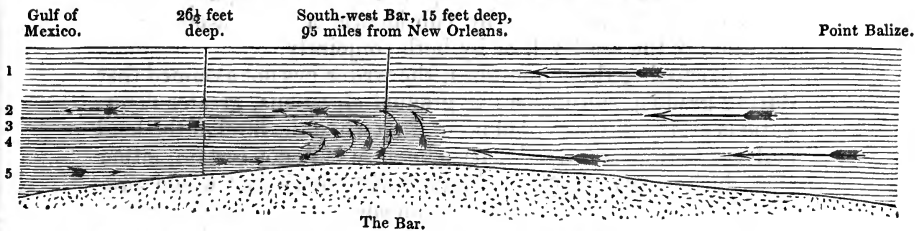
1. Stratum of fresh water, 7 feet deep, running per hour from 2 to $2\frac{4}{10}$ miles, and passing over the Bar without losing its velocity.

2. Stratum of salt water, 1 foot deep, running at the same speed as No. 1.
3. Ditto, running at 1 mile per hour.
4. Ditto, supposed to be stationary.
5. Ditto, flowing towards the Bar.

Fig. 2. *Theoretical Diagram of four successive Bars, with their accompanying fluviatile deposits.*

a, b, c, d, 1st, 2nd, 3rd, and 4th Bars of marine formation.

The general conclusion arrived at is, that the sea-level cannot be considered as stationary for practical geological purposes, since the operation of present physical causes would produce a considerable change in its height, even during the construction of a recent deposit like that in the valley of the Mississippi, which may be called small and local compared with those older forma-tions familiar to geological observers.

But the subsidence and elevation of the crust of the earth would be accompanied by alterations of the area of the sea-bed ; and the frequency of such movements would therefore furnish additional reasons for not considering the sea-level permanent for the lengthened periods requisite for the accumulation of sedimentary deposits of any magnitude.

In the Third Part of this paper an attempt is made to direct attention to the difficulty of finding any test by which to distin-guish strata gradually accumulated during a long-continued upward movement of the sea-level, from those strata formed on a sea-bottom slowly subsiding while the ocean-level was sta-tionary. In either case no change of depth of water may have occurred of sufficient importance to cause the removal of the Mollusca inhabiting the locality, and therefore the discovery of *the same species of organic remains from top to bottom of a thick deposit* is not an absolute proof (as has been supposed*) that

* "In formations from a few hundred to a thousand feet and upwards in thickness, the whole of which does actually belong to the same geological

gradual subsidence has occurred during that particular formation ; because the condition of equal depth of water during any deposit might be produced either by subsidence of the sea-bottom or elevation of the sea-level, or by both conjointly.

In discussing these questions, the writer has not assumed that during gradual subsidences or gradual elevations, greater denudations or depositions would occur than when the level of the land and sea-bottom was stationary ; because it is not certain, either that during such gentle oscillations the forces that would produce denudation are sensibly diminished or increased, or that the rocks which are brought within the reach of denuding forces are necessarily more easily worn away than those which were previously exposed to the same influences.

Part I.

It has long been acknowledged that the quantity of detritus annually carried into the ocean from various sources must displace an equal volume of water, and thus tend to raise the level of the sea.   Many years since it was estimated by an Italian that this change might amount to one foot in a thousand years. The general opinion on this subject has been, that the effects produced by the present supplies of detritus would be too minute to be perceptible, and on geological inquiries the ocean-level has been considered as permanent for all practical purposes*.   I here propose to offer the evidence of present denudation in certain countries where careful observations have been made, in order to show, that, if such rapid destruction of land occurs in most localities, then the operation of present physical causes must be amply sufficient to effect a perceptible alteration in the sea-level in a moderate space of time.

The mere consideration of the number of cubic feet of detritus annually removed from any tract of land by its rivers, does not produce so striking an impression upon the mind as the statement of how much the *mean surface level* of the district in ques-

age, and is therefore characterized by the same fossils, most curious and important results may be sometimes deduced if the position or relative heights at which the groups of fossils are imbedded be noted; and this is a point usually neglected.   For, thanks to the researches of Prof. E. Forbes, the depth of water under which a collection of shells lived can now be approximately told ; and thus the movement of the crust of the earth, whilst the strata including the shells were accumulating, can be inferred.

" For instance, if at the bottom of a cliff, say 800 feet in height, a set of shells are buried which must have lived under water only 50 or 100 feet in depth, it is clear that the bottom of the sea must have sunk to have allowed of the deposition of the 700 feet of superincumbent submarine strata ; subsequently the whole 800 feet must have been upraised." (Darwin.)

* Manfredi.   See Lyell's Principles, edit. 1850, p. 270 and 542.

tion would be reduced by such a removal. This information may be obtained by calculation from the published accounts of the quantity of mud annually abstracted from districts of known dimensions by their rivers. In this manner it is found that the Ganges would in about 1751 years, at its present annual rate, carry away from the land it drains (which is supposed to be about 400,000 square miles) as much detritus as would cover that area to the depth of one foot, as the following calculation will show :—

Thus, 27,870,400 (superficial feet in a mile) × 400,000 = 11,151,360,000,000, the number of superficial feet in the area of 400,000 square miles drained by the Ganges. The number of cubic feet of detritus discharged annually by that river is 6,368,677,400. (See Lyell's Principles.)

$\dfrac{6,368,677,400}{11,151,360,000,000} = \dfrac{1}{1751}$, consequently the reduction of the mean level of the Ganges district is $\dfrac{1}{1751}$ of a foot annually, or 1 foot in 1751 years.

6,368,677,440 cubic feet of mud discharged × 856 water to mud = 5,444,074,288,640 = the number of cubic feet of water annually discharged by the Ganges.

$\dfrac{5,444,074,288,640}{11,151,360,000,000} =$ about $\frac{1}{2}$ a foot, so that the mean annual discharge of water is equal to about 6 inches of rain on the whole area of 400,000 square miles.

The Mississippi, on the other hand, would occupy 9000 years at its present annual rate in reducing to the amount of one foot the mean surface-level of the district it drains, which is computed at eleven hundred thousand square miles. The result is obtained as follows :—

If 3,702,758,400 cubic feet of mud are annually carried down by the Mississippi (since the mud is to the water as 1 to 3000), 3,702,758,400 × 3000 = 11,108,275,200,000 = the number of cubic feet of water annually carried by the river into the Gulf of Mexico. The area of district drained by this river is stated at 1,100,000 square miles — 5280 × 5280 = 27,878,400 = the number of superficial feet in a mile — 27,878,400 × 1,100,000 = 30,666,240,000,000 = the number of superficial feet contained in the area of 1,100,000 square miles drained solely by the Mississippi.

$\dfrac{11,108,275,200,000}{30,666,240,000,000}$ foot $= \frac{1}{3}$ foot nearly. Consequently the water carried down by the river is equal to about 4 inches of rain over the surface of land drained.

If it be assumed that the levels of the rivers, lakes, and springs

are the same in this district at the same period of two consecutive years, the water sufficient to produce the above-mentioned 4 inches of the total of rain-fall upon the whole of this district must have been annually derived from clouds which have been charged with vapour in parts of the earth beyond the confines of the tract of country under consideration ; since, if the 4 inches of rain annually carried into the Gulf of Mexico were not replaced from foreign sources, the levels of the rivers, lakes, and springs must rapidly fall.

The estimate of denudation obtained from these countries may be incorrect when applied to other lands differing in altitude and receipt of rain. Besides, many rivers empty themselves into lakes and inland seas, and other extensive tracts are entirely without rain. Since there must thus be extensive districts which contribute no detritus whatever to rivers, I propose to *assume* that one-half the earth's surface only is drained by rivers flowing directly into the sea*, and that the average supply of detritus does not exceed that afforded by the district through which the Mississippi flows (a country where there are no very high mountains, and only a moderate quantity of rain).

The quantity of soluble salts annually carried into the ocean must amount to a very large volume, particularly as river-water always contains matter in solution, while it is only during two or three months of the year that alluvium in suspension is carried down in large quantities. The proportion of soluble salts in the water of the Thames is 17 to 70,000, or 1 to 4117 ; while the proportion of alluvium suspended in the water of the Mississippi is as 1 to 3000†.

The level of the land is as much reduced by what is carried away in solution, as if this were mud and sand removed in suspension ; and a submarine deposit formed from materials brought into the sea in solution will displace a volume of water equal to their former bulk ; and therefore, when the annual supply of soluble salts to the ocean does not exceed the quantity separated from solution, the same effect will be produced upon the sea-level by matter introduced, whether it be in solution or suspension. While the proportion of the land to the ocean remains as 1 to 3‡, it is evident that a reduction of 3 feet in the mean surface-level

* By reference to Johnston's Physical Atlas, the calculated proportion of land drained by rivers running into European lakes and inland seas may be seen.

† For the statistics of the Mississippi River see Sir Charles Lyell's Second Visit to the United States, edit. 1847, vol. ii. p. 249 to 253, and other places.

‡ M. Balbi shows (Atlas, Soc. Diff. Useful Knowledge, 1844) that the land on the globe equals 37,647,000 square geographical miles, the sea equals 110,875,000 square geographical miles.

Fig. 3. Diagram intended to express the effect upon the sea-level from a waste of $\frac{1}{9000}$dth of a foot per annum, over all land (from atmospheric and aqueous action), accompanied by a general waste of 136 feet of detritus from each foot of frontage (b) of coast-line (caused by the action of the sea), supposing there is one mile of coast-line to each 100 square miles of land, and to each 300 square miles of water.

Land.

Sea-level.

$\frac{1}{9000}$dth of a foot thick, 100 miles long.

300 miles.

Sea.
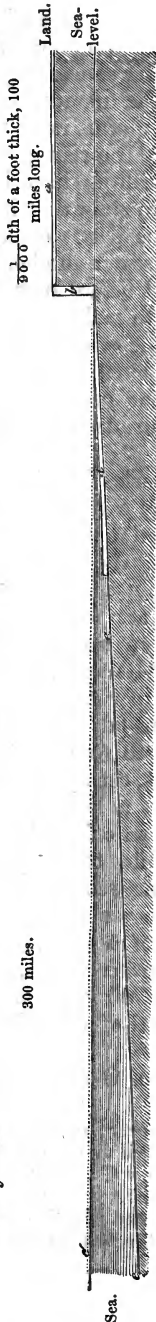
The horizontal dotted line indicates the rise of sea-level equal to $\frac{1}{10,000}$dth of a foot per ann.

of the land must take place by denudation before a volume of detritus would be conveyed into the sea sufficient to displace enough water to occasion an elevation of one foot on the ocean-level. (See fig. 3.)

There is great need of further information respecting the amount of sediment carried down by other rivers besides those mentioned; yet if the rate of denudation obtained from the statistics of the Ganges and Mississippi be any guide to what is occurring on the remainder of the globe, we cannot suppose that an *indefinite* time would be required for the performance of a denudation, which should reduce the mean surface-level of the land 3 feet, and raise that of the ocean 1 foot. It was during the contemplation of the changes of level that might have been produced by the operations of ordinary physical agents upon the surface of the earth, that Hutton was led to remark that it was not necessary to suppose the area of the land always maintained the same extent, but that from time to time new land would be formed by the elevatory movements of the sea-bottom to compensate for what had been carried into the ocean by the continued operations of rivers and breakers*. In speaking of the elevation of the sea-level, I only refer to the intervals between those movements of the land which might neutralize in an instant all that had been effected by the operation of rivers for immense periods of time.

It would add very much to the interest of this inquiry if any

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* "It is not necessary that the present land should be worn away and

proof could be brought forward of a recent gradual upward movement of the sea-level. This, however, would be difficult to observe*, on account of the rise in the water concealing the evidence of its former level, except just at the mouths of rivers, where the deposits of fluviatile alluvium might raise the land from time to time and keep it always above the rising waters.

The deposits situated at a few such localities have been described by the best observers, and I hope to show that in several cases there are appearances which might be partly explained by changes of the sea-level, but that a much greater number of cases and more certain evidence would be needed before such an event could be satisfactorily proved. I propose to make some remarks upon this point, after having submitted the evidence which has induced me to believe that the supply of detritus under present physical conditions is sufficient to raise the ocean level 3 or 4 inches in 10,000 years, provided no subsidence or elevation disturbed the result.

To this subject I now proceed. Sir Charles Lyell's published statements of the quantity of mud annually carried down by the Mississippi and Ganges appear to have been made with so much care, that they may be a better guide to the general rate of removal of soil by rivers than information obtained from a greater number of smaller rivers, which of course are more likely to be influenced by local circumstances. Eleven hundred thousand square miles of land are drained by the Mississippi†, which annually discharges a quantity of water equal in volume to 4 inches of rain, or about one-tenth of the total rain-fall over this entire surface, which forms one-fifth part of North America‡. From the mean of a great number of observations, the average quantity of alluvium suspended in the water appears to be 1 part in 3000. Consequently, as the water annually drawn off would cover an area of eleven hundred thousand square miles to the depth of 4 inches, the quantity of mud removed in the water (as measured at or near the mouth of the river) would cover the same extensive surface to the depth of $\frac{1}{3000}$dth part of 4 inches, or to the depth of $\frac{1}{9000}$dth part of a foot. Or, in other words, the Mississippi at its present rate would occupy 9000 years in carrying away detritus before the mean surface-level of one-fifth part of North America would be reduced 1 foot.

The Ganges discharges into the Indian Ocean a supply of

wasted exactly in proportion as new land shall appear; or conversely, that an equal proportion of new land should be produced as the old is made to disappear." (Hutton's Theory of the Earth, 1795, vol. i. p. 196.)

* See Darwin, Coral Reefs, &c., edit. 1851, p. 95.

† See art. Mississippi, Penny Cyclopædia, vol. xxv. p. 277.

‡ The total rain-fall of the United States is 39 inches between 24½° and 45° N. lat. (Berghaus and Johnston.)

water equal to about 6 inches of rain, or 400,000 square miles, or a much greater volume of water than the Mississippi pours into the Gulf of Mexico, taking into consideration the difference in size of the countries they drain.

The alluvium suspended in the waters of the Ganges* is as 1 to 858 by weight; consequently the detrital matter removed in suspension by the water in one year would cover the land from which it is derived to the depth of $\frac{1}{1751}$ of a foot; that is to say, the Ganges might pour out muddy water at its present rate for 1751 years before the mean level of 400,000 square miles would be reduced 1 foot in height. The great elevation of the Himalaya range, or possibly a greater rain-fall, may probably occasion the difference between the rates of denudation indicated by the Ganges and the Mississippi. As there are also parts of the earth's surface drained by rivers flowing into lakes and inland seas, and other tracts are entirely without rain, I propose to estimate (as before mentioned) that only half the land contributes detritus in suspension to rivers flowing directly into the sea†. If this area be annually reduced in level at the same rate as the district through which the Mississippi flows, then the mean level of the land on the globe would be reduced 3 feet in 54,000 years, and consequently the level of the ocean raised 1 foot in the same period by means of the detritus suspended in river-water poured into the ocean‡.

But in addition to the sediment carried down by means of rivers, we have also to take into consideration the amount of debris washed into the sea from cliffs during so long a period as that mentioned. It is difficult, however, to form any estimate of what this would annually amount to, for old maps and charts are hardly accurate enough to represent the waste of cliffs by breaker-action even within the last 100 years. Captain Washington has, however, published a report§ which gives an account of the encroachment of the sea at intervals on one part of the Suffolk coast. This will give a general idea of the contribution of detritus that may be obtained from some points of a coast-line. The following statements are collected from Captain Washington's Report on Harwich Harbour in 1844, from which also the figures 4, 5, 6, 7 are copied.

* See p. 261.

† The proportion of land without rain is about $\frac{1}{1200}$dth of the whole. Keith and Johnston say that nearly one-half the drainage-water of Europe and Asia falls into the Black and Caspian Seas. The proportion for Africa and America is not known.

‡ It is not improbable that the *solvent* powers of rain and river-water are as important agents in the removal of land as the agency above mentioned. Definite calculations on this subject remain to be made.

§ Tidal Harbours' Commission, First Report of 1845.
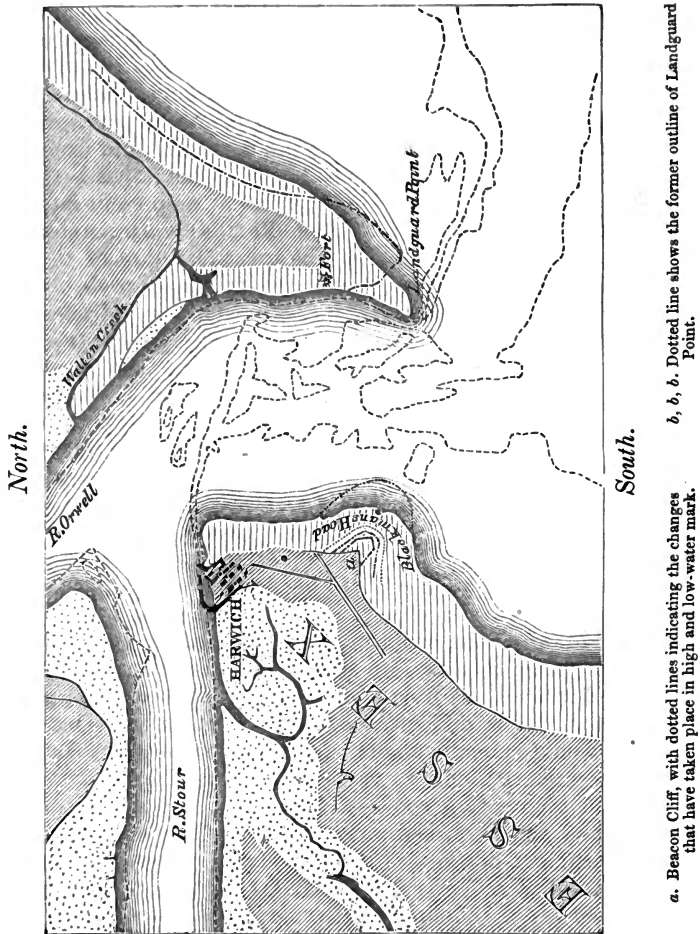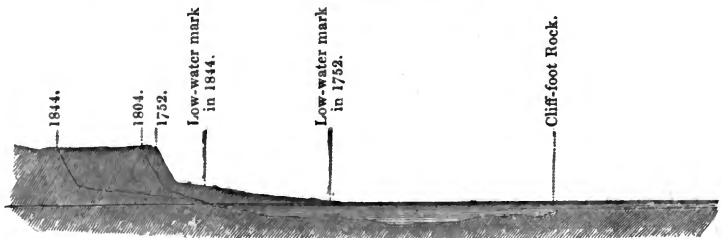
Fig. 4. *Map of Harwich.*

a. Beacon Cliff, with dotted lines indicating the changes that have taken place in high and low-water mark.

b, b, b. Dotted line shows the former outline of Landguard Point.

Fig. 5. *Section showing the Destruction of Beacon Cliff between 1752, 1804, and 1844.*

Figs. 6, 7. *Sections showing the Increase of Landguard Point between* 1804 *and* 1844.

Beach end in 1804.

a

Beach end in 1844.

a

a, a. Low-water level of ordinary springs.
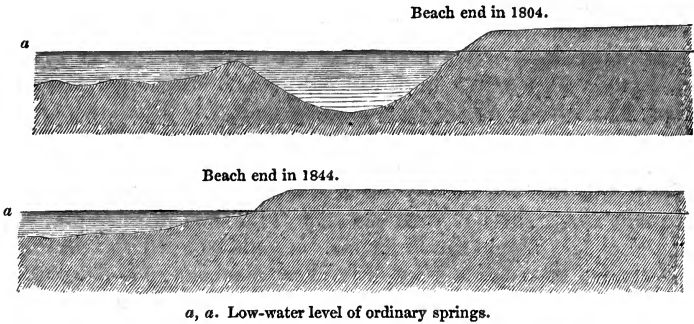
The cliff on the western side of the harbour is about 1 mile long and 40 feet high, and the encroachment of the sea appears to have been at the rate of 1 foot per annum between the years 1709 and 1756, so that the annual supply of detritus was equal to 40 cubic feet for each foot of frontage. Between 1756 and 1804 the advance increased to nearly 2 feet per annum; so that the annual removal of cliff amounted to nearly 80 cubic feet for each foot of frontage.

Between 1804 and 1844 the encroachment of the sea averaged 10 feet per annum, and the annual removal of detritus must have amounted to 400 cubic feet for each foot of frontage. It was during this latter period that extensive dredging for cement stone took place at the base of the cliff.

On the eastern side of the harbour events of an opposite character have occurred, for Landguard Point has gained 50 feet per annum in length during the last 30 years. The addition thus made to the land, and to the "littoral zone," presents an interesting example of the rapid accumulation of a local deposit under favourable circumstances. From the appearance of the beach, it would appear that the shingle and sand of which it is formed have been brought from the north, in which direction there are recorded instances of great destruction of land by storms during the last 300 years. The aspect, however, of much of the coast-line appears as if it had remained unaltered for a very long period, except in the manner Mr. R. A. C. Austen* alludes to when he remarks, "that although the sea for months together, and in places even for whole years, may not acquire

* Austen, Quart. Journ. Geol. Soc. vol. vi. p. 71–73; and De la Beche, Geol. Observer, 1851, p. 65.

any fresh spoil, yet there are few hours when its waters are unemployed in fashioning and abrading the materials already acquired." In considering the effect upon the sea-level caused by sand, mud, and pebbles washed in by the breakers, it is only necessary to regard those materials that may be brought in from cliffs above high-water mark; for the movement of sand and mud below high-water mark can produce no effect upon the sea-level, because the abstraction of these materials from one part of the shore is exactly balanced by their addition to some other part. For instance, some of the flint-pebbles which have contributed to the recent deposit at Landguard Point have been brought along shore a great distance from their original position on the cliff. These flints formed an addition to the sea-bed, and tended to raise its general level by displacing an amount of water equal to their bulk the moment they fell on the shore below high-water mark; and it is quite clear their subsequent movements, either beneath the waves or on the beach, could produce no further effect upon the sea-level, the spaces they occupied on one part of the coast being balanced by the vacancy left at some other. It is also evident that the beach at Landguard Point will go on extending so long as the fresh supplies of shingle and sand from the north exceed the removals southward.

In the same manner the continued supplies of pebbles from the westward enables the Chesil Bank to preserve its position. As soon, however, as any disturbing causes interrupt the supplies of new material, the sand and shingle beaches dependent upon them must soon disappear; and in fact the termination of every beach will be at that point where the waste and abrasion by breaker-action are balanced by the supply of pebbles and sand drifted from other places. Although it appears clear that only the detritus obtained from cliffs above high-water mark need be taken into calculation, yet I regret to find that scarcely any data of this kind exist, and therefore it is not possible to ascertain the probable effect upon the sea-level that is being produced by the detritus so derived. In the same manner the per-centage of soluble salts in the water of the few large rivers of which notes have been published has not been given separately from the percentage of matter in suspension, and therefore we are in ignorance of the supplies that are annually introduced into the ocean from the formation of submarine deposits from materials dissolved in the sea-water. When the rise in the sea-level from the effect of alluvium brought in suspension by rivers was being considered, I supposed that that cause alone might produce an elevation of 1 foot in 54,000 years; but in order to make some allowance for the similar effects that must be produced by the introduction into the ocean of materials from above high-water mark on coast-

lines* by breaker-action, and also by the formation of submarine deposits from materials which were brought into the ocean in solution, I now propose to consider that all these causes together might produce an elevation of the sea-level equal to 1 foot in 40,000 years, or 3 inches in 10,000 years.

Mr. Darwin has remarked, that "the knowledge of any result, which, with sufficient time allowed, can be produced by causes, though appearing infinitely improbable, is valuable to the geologist, for he by his creed deals with centuries and thousands of years as others do with minutes." For these reasons, even if, upon further investigation, it should be found that the true rise in the sea-level is much less than 3 inches in 10,000 years (in periods undisturbed by subsidences and elevation), yet it may still be an important element in accounting for those changes which we are now about to consider.

Part II.

Allusions have been already made to the difficulty of proving whether or not the sea-level had been *gradually* elevated, because the rise of the waters would conceal the evidence of their former height except just at the mouths of rivers, where deposits of fluviatile alluvium might raise the land from time to time and keep it above the waves. The recent strata formed at a few such localities have been described by the best observers; and while there are appearances in several cases which might be to some extent explained by the supposition of a gradual rise of the sea-level, yet no proof could be obtained without the concurrent testimony of a much greater number of instances than have yet been brought forward. Sufficient information, it appears, exists to show that the quantity of alluvium in the deltas of such rivers as the Mississippi, Ganges and Po, is so enormous, that the accumulation must have occupied a period of time during which it would not be possible to conceive the sea-level stationary.

Little progress could be made in an inquiry of this kind without clear views of the operations of rivers. The recent reports of engineers upon this subject supply an important link in the chain of evidence, and enable us to understand the laws which

* The rough estimation of the extent of coast-line, kindly supplied by Mr. A. K. Johnston (Nov. 1852), is as follows:—

	Nautical miles (60 to a degree).	English statute miles (69½ to a degree).
Europe	17,200	20,425
Asia	30,800	34,825
Africa	14,000	16,625
America	37,600	44,656
	99,600	116,531

govern the formation of alluvial plains along the lower parts of all river-courses.

The diagram (fig. 8) represents a section of 600 miles of North America, through the alluvial plains and delta of the Mississippi*, together with a section of the Gulf of Mexico, from a point 100 miles east of the Balize to the continent of South America. The sea-bottom is marked from the soundings on the Admiralty Chart, and the depth of the Mississippi and its fluviatile deposit are inserted from statistics collected by Sir C. Lyell†.

It will be seen that the level of the water in the Mississippi, near its junction with the Ohio, nearly 600 miles from the Gulf of Mexico, is 275 feet above that of the sea. The slope of the alluvial plains through which the river winds will therefore be less than 1 foot in 10,000.

The hills bordering the valley of the Mississippi are cut through in several places by the river, thereby exposing good sections of their component strata, consisting of alluvial deposits thought to be much more ancient than those we are about to consider.

An area of 16,000 square miles is occupied by the more modern alluvial formation between the head of the delta and the junction of the Ohio‡. It is supposed to be, in the average, 264 feet deep, and is from 30 to 80 miles wide. The true delta extends over 14,000 square miles, occupying a frontage of $2\frac{1}{4}$ degrees on the coast-line of the Gulf of Mexico, and extends 180 miles inland. At its southern extremity its surface is hardly above the level of high tides, but it rises gradually as it passes inland, and at New Orleans is nearly 10 feet above the sea-level.

A boring near Lake Pontchartain, of 600 feet, failed to penetrate the modern alluvium; and wherever excavations are made, the remains of trees are frequently found, apparently in the places where they grew, but now far below the sea-level. Sir Charles Lyell computes its average depth at 528 feet, and consequently nearly the whole of this modern deposit is below the sea-level, yet is supposed not to contain marine remains. The fall of the Mississippi during a course of 600 miles is shown by fig. 8; the depth of the channel varies from 80 to 200 feet until it approaches the Balize, where it shallows to 16 feet. The rise of the tide at this point is only 2 feet. The depth of the alluvial deposit below the river-channel is also indicated, together with

* For a most valuable detailed description of the physical geography, &c. of the Mississippi and Ohio valley, see Mr. C. Ellet's paper, Smithsonian Contributions, vol. ii. 1851.

† See note, p. 262.

‡ Lyell's Second Visit to the United States, 1849, vol. ii. pp. 146–152, 155, 169, 194, 195, 203, 243, &c.

Fig. 8. Diagram showing depth of the Delta (supposed, 600 feet); area 14,000 square miles; height of the river above the sea-level 275 feet at *; depth of river, supposed, 80 to 200 feet in this diagram, supposed to average 264 feet; area, 16,000 square miles.

Head of delta.

South America.

54 feet.

180 feet.

300 feet.

Gulf of Mexico.

c. Marine strata.

168 feet deep, New Orleans.

16 feet deep, Balize.

120 feet.

198 feet.

258 feet.

372 feet.

1380 feet.
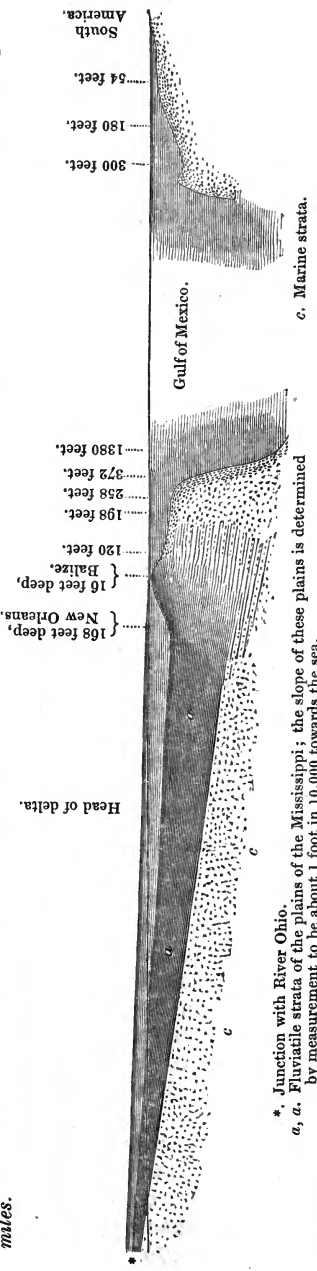
*, Junction with River Ohio.

a, a. Fluviatile strata of the plains of the Mississippi; the slope of these plains is determined by measurement to be about 1 foot in 10,000 towards the sea.

Direct distances :—Junction with Ohio to Balize, 580 miles. Head of Delta to Balize, 180 miles. New Orleans to Balize, 70 miles.

[Vertical scale 1 inch to 1000 feet. Horizontal scale 1 inch to 150 miles.]

Fig. 9. Transverse section of the Mississippi, where it is 1500 feet wide and 100 feet deep, running in the midst of an alluvial plain 50 miles wide. (This diagram shows the section of slow-flowing rivers in general.) Vertical scale 100 feet to the inch.
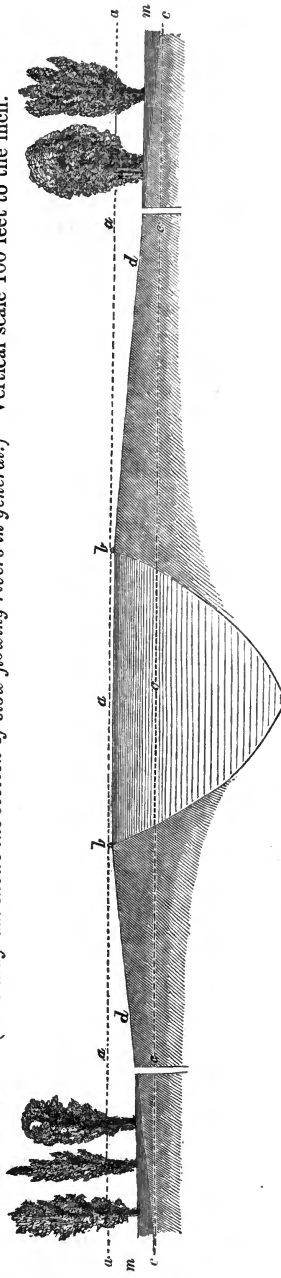
a, a. The level of water in the river during flood, which is 25 feet above the level of the distant marshes, *m, m.*
c, c. The level of water in the dry season.
b, b. Artificial banks or levées, 4 feet high.

d, d. The banks and plains.
m, m. Marshes, supplied with water by filtration from the river at all seasons of the year.

The fall of water is 2⅓ inches per mile. The whole body of water in the river must be in motion, so that even in flood-time only a small per-centage of the water and alluvium in the stream can escape over the banks.

the surface of the more ancient formation upon which the Mississippi has formed this great alluvial deposit, the bottom of which is now more than 500 feet below the present sea-level.

Mr. Charles Ellet, Jun., in a Report to the American Secretary at War, January 29, 1851, communicates the information from which the diagrams figs. 1 and 2 are constructed. See p. 259.

The theory of Mr. C. Ellet is, that the velocity of the stratum of fresh water (fig. 1) is communicated entirely to the underlying stratum, composed of salt water, partially to the next stratum 3, but not at all to stratum 4, which is stationary : stratum 5 is also marine, but it flows in an opposite direction to the rest, and restores the salt water which is carried away by the friction of the upper stratum, No. 1, against the surface of No. 2.

It is supposed that the rapid increase of deposit at the bar, fig. 1, arises from stratum No. 5 carrying mud to that point, where its velocity is partially neutralized by impinging against stratum No. 1.

From the following particulars of the deltas of the Ganges and Po, it would appear that they are similarly situated to the Mississippi. " An Artesian well at Fort William near Calcutta, in the year 1835, displayed at a depth of 50 feet a deposit of peat with a red-coloured wood similar to that now living.   At 120 feet clay and sand with pebbles were met with.   At the depth of 350 feet a freshwater tortoise and part of the humerus of a ruminant were found.   At 380 feet, clay with lacustrine shells was incumbent upon what appeared to be another dirt-bed or stratum of decayed wood.   At 400 feet they reached sand and shingle*."

In the delta of the Po, a well bored 400 feet failed to penetrate the modern alluvial deposit ; very near the bottom it pierced beds of peat, similar to those now forming.   The coarser particles of mud which have already passed the mouths of rivers may contribute to the marine or fluvio-marine deposits forming outside deltas ; but this can only be to a limited extent, as the great bulk of the mud is far too fine to settle near the coast. Little material could be obtained from cliffs along the sea coasts, but we have information of marine currents specially bringing sand and mud from other parts of the sea-bottom to the neighbourhood of deltas.   (See Mr. Ellet's observations.)

For these reasons, if the further examination of the deltas of the Mississippi and other rivers should lead to the discovery of some recent marine or fluvio-marine strata, it may turn out that such deposits have been more rapidly accumulated than the purely fluviatile beds with which they may be associated.   In estimating the age of deltas, allowance, however, ought to be made for such contingences, and also for their organic contents.

* Lyell, *loc. cit.* p. 248 ; and Principles, p. 267-270.

Let us now turn to fig. 9, which exhibits Sir Charles Lyell's transverse section of the channel and plains of the Mississippi, and at all points throughout a course of several hundred miles. The dotted lines are introduced to show the variation of the water-level in the wet and dry seasons. *b, b* represent the artificial Levée; *d, d* the banks and plains; *m, m* the swamps of the Mississippi. "The banks* are higher than the bottom of the swamps, because, when the river overflows, the coarser part of the sediment is deposited on the banks, where the speed of the current is first checked" (Lyell). The channel, however, is so wide and deep, that even if there were no artificial banks to prevent floods, the river would carry into the Gulf of Mexico the principal mass of the mud it had received with the water of its tributaries; for it is only for a short time in the year that the level of water in the river is above that of the adjoining plains. The swamps and the numerous lakes formed by deserted river-bends communicate at all times of the year with the main stream. In these places mud could be constantly deposited mingled with the remains of the vegetation which grows luxuriantly in the swamps. The only supply of inorganic matter for raising the level of the vast plains through which the river winds for hundreds of miles, must be the mud deposited upon them during the periodical floods. These are very much prevented by the artificial levée; but when they do occur, their force is augmented by the water being artificially dammed up.

"I have seen, says an eye-witness, when the banks of the Mississippi burst, the water rush through at the rate of ten miles an hour, sucking in flat boats and carrying them over a watery waste into a dense swamp forest" (Lyell). It would appear that the Mississippi differs in size and proportion more than in other respects from our rivers. For instance, when floods occur upon our own alluvial plains, they are most conspicuous at a distance from the stream which caused them, indicating that the parts of the plains nearest the banks are higher than those at a distance from it, and therefore that fig. 9 would also represent the *transverse section of slow rivers generally*. The similarity of the physical features presented by the lower parts of all rivers was particularly remarked by Hutton†.

It has been observed by engineers‡, that in all rivers in this

* There is a similar section of the Nile and its banks published in the fourth volume of the Quarterly Journal of the Geological Society, p. 344, but communicated by Lieut. Newbold in 1842.

† Theory of the Earth, vol. ii. p. 205–211.

‡ On this and the following points see First Report of the Tidal Harbours Commission, above referred to, which contains the opinions of our most celebrated engineers on the phænomena presented by tidal and other rivers.

country the large quantities of silt brought into them by winter freshets do not tend to choke the channels, but that, at that period of the year, former accumulations of deposit are actually removed by the force of the stream; and therefore, that although winter-freshets bring down silt with them, they carry into the sea a larger quantity than they have introduced into river channels. If it were allowable to assume that the unequal supply of water at different seasons of the year produces effects in the channel of the Mississippi similar to these just described on our own streams, the following account would represent the course of events. The diminution of the speed of the current of rivers assists the deposition of silt upon their beds, as much as its increased speed in the winter season favours its removal. The summer deposit, however thin it may be, cannot occur without contracting the sizes of the channel.

Winter-freshets following a sudden fall of rain would raise the water-level of rivers rapidly, and carry it above the banks before the augmented current has time to scour the river-channel and raise it to its former capacity. Accumulations of silt, small at any one place, must each raise the water a little above its proper level, and the point of overflow will be where the sum of these small elevations amounts to more than the height of the banks above last year's level. But floods leave a deposit of silt, &c. upon the banks they pass over, which increases the capacity of the channel; and until new deposit has again reduced the area of the stream below its proper size, inundations will not occur.

As each flood raises only the part of the bank it flows over, it is easy to see that the point of overflow will be changed from time to time; and every part of the alluvial plains through which a river flows will be visited in turn by floods, provided there are no artificial banks. These banks assist the scouring power of rivers in winter, because they retain more water in the river; but, on the other hand, silt that would have been carried over the banks is kept within the channel, and this may be the reason why the beds of all navigable rivers have become so much elevated during the historical period. The contraction of water-channels in summer, and their enlargement in winter, is thus directly traced to the unequal supply of rain at different periods of the year.

This being admitted, we have an explanation of the manner in which rivers may, by a succession of floods, build upon alluvial deposits along their courses, at the same time raising their beds in proportion to the height of their plains.

If river-channels were perfectly symmetrical in form, the identical sediment that had fallen in summer might be removed again in winter. It is, however, well known that river-channels are

deep on one side and shallow on the other. The principal deposit therefore takes place on the shallow or quiet side, and the principal removal occurs from the deep side where the current runs more quickly.

This may explain why the traveller on the Mississippi sees for hundreds of miles a caving bank on one side, and an advancing sand-bar on the other (Lyell). When the action of the river is also unequal on its two banks in different places along its course, a channel consisting of curves instead of straight lines must be produced. When each curve, however, had assumed the complete horse-shoe form, the water, by travelling round the outer circumference of the bend, will have its effective speed reduced to that on the inner or shallow side. The current would thus become more nearly equal in all parts of the channel, and necessarily the deposit likewise; and in winter it would have a nearly equal tendency to excavate the banks on both sides, which condition of equilibrium might last for some time.

Hutton, in 1795, has remarked, that there is evidence of denudation in every country where at any time of the year the streams carry off any particles of the superficial soil*. The Mississippi must derive its vast supplies of mud from thousands of such tributaries; for it could obtain them from no other source, unless we suppose it abstracts them from its own plains. Certainly in many places soil is being removed from one part or other of its plains; but an equal quantity must be added to some other part, for the river could not make a permanent inroad into its plains without enlarging its channel. This it does not do, or it would be able to carry off the winter-freshets without overflowing, and the present artificial bank would be unnecessary.

I have thus briefly referred to observations made by British engineers which may throw some light on the causes of periodical floods, and changes of channel in rivers, and also upon the formation of alluvial plains along their course. These questions need not further be entered into, because the limited growth of alluvial plains and deltas may be best illustrated by tracing the alteration in the mean level of a large part of North America that would be consequent upon a denudation sufficiently extensive to furnish the alluvium said to exist in the valley of the Mississippi. On the borders of the Gulf of Mexico at the present time marine strata are forming within a short distance of the fluviatile, and frequently alternate with them, because spaces of the sea-shore are enclosed by banks of river-mud and con-

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* Our clearest streams run muddy in a flood. The great causes, therefore, for the degradation of mountains never stop as long as there is water to run; although, as the heights of mountains diminish, the progress of their diminution may be more and more retarded. *Op. cit.* vol. ii. p. 205.

verted into lakes ordinarily communicating with the river, but sometimes with the sea after high tides.

The present marine or fluvio-marine deposits must be composed of mud that has passed the mouth of the river, or washed up by the sea, while the freshwater strata must be entirely formed from sand and mud carried over the river banks, or deposited on the bottom of lakes supplied by the stream before it enters the Gulf of Mexico.   An idea of the amount of denudation that has taken place in the interior of N. America might be either obtained from the extent of the marine deposits formed of mud that had passed the mouth of the river, or from that of the purely fluviatile and contemporaneous deposits formed from mud which had never entered the Gulf of Mexico.

But it is also necessary to estimate what proportion of the total quantity of mud brought down by the river is carried completely out to sea, compared to what is left either upon the marine or fluviatile portion of the delta.

Sir Charles Lyell has remarked, that the alluvium now remaining in the valley of the Mississippi can only represent a fragment of what has passed into the Gulf of Mexico; and this can readily be believed when we reflect upon the depth and breadth of the channel, and upon the short period of the year that the stream would throw any large quantity of mud into the plains even if there were no artificial banks.   We must also bear in mind that only the coarse mud could settle near the shore, for the finer particles could not deposit except in very deep water. For these reasons, even if the mud carried beyond the mouth of the river is only ten times the quantity left behind on the fluviatile portion of the delta and plains of the Mississippi, this amount of detritus could not be obtained without the mean level of one-fifth part of North America being reduced 100 feet by denudation affected by the action of rain, the atmosphere, and running water*.   But Hutton (vol. ii. p. 401) remarks, in 1795, that wherever any stream carried off particles of soil in its waters at any period of the year, it might be said that denudation was taking place in that country; yet he particularly observed that the waste of land was very unequal, being much more rapid in the elevated than in the more level parts of any district.   It is therefore possible that, during the reduction of the mean surface-level of the land drained by the Mississippi to the amount of 100 feet, some portions of the area might be lowered many times that amount, while other portions might suffer little, or be positively raised by the superposition of alluvial deposit.   We are, however,

* The data for calculating the annual quantity of detritus carried over the river's banks, in relation with that carried down to the sea, are very imperfect.   Further information on this subject is much needed.

informed by Sir Charles Lyell, that the Mississippi in one part of its course cuts through ancient fluviatile beds evidently antecedent to those recent deposits we have been considering. This formation is also stated to contain the remains of species of plants and animals now existing ; so that evidence is to be obtained in this district of still greater denudations (by these results) than those of which we have spoken, and which would produce changes on the surface of the earth since the introduction of the present fauna and flora of extent enough almost to realize Hutton's vision of mountains wasted away by the action of rain, the atmosphere, and running-water, and carried along river-courses into the ocean. It is not necessary to take an extreme view of this subject to gain the object we have in view, which is to show that, during the time occupied by the formation of the Mississippi delta, the sea-level might be perceptibly raised* by the agency of physical causes now in operation.

The reasons for supposing that a rise of 3 inches in each period of 10,000 years might occur, have been already discussed, and it only remains to state that, at the present rate of denudation, it would require five such periods to produce the quantity of detritus said to exist in the valley of the Mississippi ; while it would require fifty such periods to produce the requisite quantity of alluvium on the supposition that only one-tenth of the mud *in transitu* through the river was appropriated for the accumulation of its alluvial plains and delta. Under these circumstances it appears a legitimate conclusion, that the level of the sea cannot be considered permanent for all practical purposes when it may be shown that it might be disturbed by the operation of present causes during the period occupied by the construction of a single geological formation. Elevations and subsidences of the land or sea-bottom would also effect important changes in the height of the sea-level, sometimes counteracting and at others adding to the effects produced by the continuous operation of rivers, &c. The effects produced by these important causes would be an additional reason for not considering the sea-level permanent.

It is hardly necessary to add, that the continual waste of the earth's surface by the carrying of materials into the ocean by rivers and breakers particularly attracted the attention of Hutton. He considered† that this was counteracted by elevatory move-

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* This change of level may amount, under certain circumstances, to a great extent, but at the *lowest* calculation would be 15 feet.

† These remarks of Hutton are here introduced because he takes an entirely different view of this subject to that promulgated by Sir Charles Lyell, who considers that there has been always an excess of subsidence. (See Principles, 1850, p. 543.)

ments of the sea-bottom from time to time, but particularly mentions that it was not necessary to suppose that the dry land was equally extensive at all periods.   Since the fluctuation in the sea-level would be directly consequent upon the destruction of land arising from the operation of rain, the atmosphere, and running water on its surface, such changes would be in harmony with the spirit of the Huttonian theory.

Part III.

The average thickness of the deposit formed on the sea-bottom by the solid materials brought on to it from all sources has been estimated in the preceding part of the paper at 3 inches in 10,000 years, producing an elevation of that amount in the sea-level in the same period.   Some portion of the oceanic area may be supposed to receive no part of this supply, while other localities nearer the coast-line obtain a great deal more than the average. In the interval between these places, where the rate of deposit is extremely high, and those where it is extremely low, must lie an extensive tract of sea-bottom, where the accumulation of detritus does not much differ from the average rate, which we have supposed to be 3 inches in 10,000 years.   Such localities may be more extensive near those parts of the ocean-bottom which receive no supplies of detritus whatever, but they must stretch up to the coast-line in many places.   For instance, if it is supposed that a supply of 10 cubic feet of sand or mud is obtained from each foot of frontage of any coast-line, and distributed between high-water mark and 20 miles distant, it might raise the mean level of that portion of sea-bottom 1 foot in 10,000 years.

Rivers opening on to the shore might also bring down a still greater quantity of material; but although tides and currents are at work removing the sea-bed in one place and forming sedimentary strata in others from the old and new materials, there must everywhere be portions of every sea-bottom where the rate of deposit is intermediate between the highest and lowest, and may often not differ much from that of 3 inches in 10,000 years.   These portions of the great oceanic area, wherever they may be situated, are particularly interesting, because on them the accumulation of sedimentary deposit is taking place without any change in the depth of water, and yet without necessitating the supposition of gradual subsidence of the sea-bottom[*].   Even where deposits are taking place much faster

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[*] The effect of these causes on the general depth of the ocean would be of little importance in a geological point of view, except for *an extended period of time*, such as must have elapsed during the construction of a great serial group of strata.

than the mean rate, the variation in the depth of water would be proportionately less than if the sea-level had been permanent.

The limited supply of detritus derived from cliffs, and the wide distribution of that from rivers, renders it difficult to imagine any very extensive tract of sea-bottom where the rate of deposit derived exclusively from new materials should many times exceed the average. Even on areas where extreme cases of denudation and deposition occurred (in periods when the sea-bottom was unaffected by movements, subsidence and elevation), there would be many parts where the condition of depth would remain unaltered, because on them the rise in the sea-level would compensate the addition to the sea-bottom. Also if, in periods that are past, the supplies of detritus from rivers and cliffs were many times greater than at present, they must have caused proportionately greater fluctuation of the sea-level, and therefore under such circumstances there would also be parts of the oceanic area receiving deposit at the same rate that the sea was rising. There would thus have been opportunities for the accumulation of sedimentary rocks without any change taking place in the depth of the water they were formed in, during the intervals when the sea-bottom was undisturbed by subsidences and elevations. For these reasons, in examining the section of a marine formation containing throughout the remains of the same species of Mollusca, it would require independent evidence to determine whether the equal depth of water indicated by the organic remains had been preserved during the formation of the deposit by means of changes of the level of the sea-bottom, or that of the sea itself, or of both conjointly.

Great caution must also be requisite in judging of the time occupied in the formation of the older rocks from their mineral character, as the following description of passing events will also apply to periods that are long gone by.

Mr. Austen relates in one of his papers, that "with a continued gale from the west large areas of the dredging-grounds on the French coast became at times completely covered up by beds of fine marly sand, such as occurs in the offing, and which becomes so hard that the dredge and sounding-lead make no impression upon it: with the return of the sea to its usual condition, a few tides suffice to remove these accumulations[*]."

Mr. Deane, the submarine surveyor, also reported to the Institution of Civil Engineers, that the turn of the tide is felt as soon near the sea-bottom at a depth of 120 feet as it is at the surface; and he represents that the loose materials covering the Shambles Rocks are moved backwards and forwards with every tide.

[*] Quart. Journ. Geol. Soc. vol. vi. p. 79.

With these facts before us, what criterion can there be (even by estimating the sources of the detritus) for arriving at the minimum or maximum rate at which sands and marls become permanent additions to the sea-bed ? For the materials may present all the appearances of hasty accumulation, and yet the interval of time between the deposit of two strata of sand now contiguous may have been occupied by countless temporary deposits, as quickly brought and as quickly removed by the tide, and leaving no trace whatever of their existence. For the same reasons, we cannot be certain that in the valley of the Mississippi we have an unbroken sequence of fluviatile strata, in which the accumulations of one century form the base for those of the next, from the bottom to the top of the series; because there, as in marine formations, the deposits of one period may have been entirely removed in the next. It is therefore possible that many such movements may have occurred, and that the delta of the Mississippi may have occupied a longer period of time for its formation than could be computed from any data remaining. In the preceding part of the paper the conclusion was arrived at, without taking an extreme view of the rapidity with which the materials may have been collected for its deposition, that the work could not have been completed within a period for which the sea-level could be considered permanent*.

There must be, however, many rivers which are only able to afford very small supplies of mud to any alluvial formations, either from deriving their water from lakes or from countries with a very small rain-fall. During a period when the gradual elevation of the sea-level was not counteracted by the effects of more powerful causes, there would be conditions near the mouths of some rivers of this kind for the surface of their plains to be gradually elevated by the operation of winter floods at a rate somewhat similar to that of the sea-level. In this manner purely fluviatile deposits might be formed in the neighbourhood of the ocean, occupying positions similar to that represented in the lower part of the longitudinal section of the Mississippi, without the necessity of supposing any subsidence of the land. In the upper portions of such rivers, the periodical floods, assisted by the accumulation of terrestrial remains in the adjoining plains, would add stratum after stratum during periods when the surface of the country was unaffected by subterranean movements. It is probable that the rate of deposit might be accelerated in periods of subsidence ; but the manner in which rivers form plains along

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* It is hoped that in the course of a few years enough data will be forth-coming to determine more nearly the importance of this variation of level in a geological point of view.

their course in all countries under ordinary conditions, when no subsidence or elevation is occurring, was traced by Hutton.

Even if, in ancient periods, the rate of denudation was greater than at present, and the supplies of detritus to rivers more extensive, the fluctuations of the sea-level and the elevation of the beds and plains of rivers would have been proportionately greater. There would, therefore, still have existed some localities where the rate of the formation of alluvial plains near the sea kept pace with the elevation of the waters; so that, as at the present time, conditions would have existed for the accumulation of fluviatile strata containing terrestrial remains without any subsidence of the land. This is a subject, however, that must be further studied, more especially when its value is considered in relation to the great masses of fluviatile strata either of the Mississippi, the Ganges, the Nile, or the Po. For the above reasons it would be difficult to determine, when examining sections of thick fluviatile strata, whether these accumulations of detrital matter had been formed during subsidence of the land, or during the gradual elevation of the level of rivers and seas, arising from the continual operation of ordinary physical causes.

XLII. *Note on the Geometrical Representation of the Integral* $\int dx \div \sqrt{(x+a)(x+b)(x+c)}$. *By* ARTHUR CAYLEY[*].

THE equation of a conic passing through the points of intersection of the conics

$$x^2 + y^2 + z^2 = 0$$
$$ax^2 + by^2 + cz^2 = 0$$

is of the form

$$w(x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 = 0,$$

where w is an arbitrary parameter. Suppose that the conic touches a given line, we have for the determination of w a quadratic equation, the roots of which may be considered as parameters for determining the line in question. Let one of the values of w be considered as equal to a constant quantity k, the line is always a tangent to the conic

$$k \cdot (x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 = 0;$$

and taking $w = p$ for the other value of w, p is a parameter determining the particular tangent, or, what is the same thing, determining the point of contact of this tangent.

The equation of the tangent is easily seen to be

$$x \sqrt{b-c} \sqrt{a+k} \sqrt{a+p} + y \sqrt{c-a} \sqrt{b+k} \sqrt{b+p}$$
$$+ z \sqrt{a-b} \sqrt{c+k} \sqrt{c+p} = 0.$$

Suppose that the tangent meets the conic $x^2 + y^2 + z^2 = 0$ (which is of course the conic corresponding to $w = \infty$) in the points P, P′, and let θ, ∞ be the parameters of the point P, and θ', ∞ the parameters of the point P′, *i. e.* (repeating the definition of the terms) let the tangent at P of the conic $x^2 + y^2 + z^2 = 0$ be also touched by the conic $\theta(x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 = 0$, and similarly for θ'. The coordinates of the point P are given by the equations

$$x : y : z = \sqrt{b-c} \, \sqrt{a+\theta} : \sqrt{c-a} \, \sqrt{b+\theta} : \sqrt{a-b} \, \sqrt{c+\theta};$$

and substituting these values in the equation of the line PP′, we have

$$(b-c) \, \sqrt{a+k} \, \sqrt{a+p} \, \sqrt{a+\theta} + (c-a) \, \sqrt{b+k} \, \sqrt{b+p} \, \sqrt{b+\theta}$$
$$+ (a-b) \, \sqrt{c+k} \, \sqrt{c+p} \, \sqrt{c+\theta} = 0 \quad . \quad . \quad . \quad . \quad (*),$$

an equation connecting the quantities p, θ. To rationalize this equation, write

$$\sqrt{(a+k)(a+p)(a+\theta)} = \lambda + \mu a$$
$$\sqrt{(b+k)(b+p)(b+\theta)} = \lambda + \mu b$$
$$\sqrt{(c+k)(c+p)(c+\theta)} = \lambda + \mu c,$$

values which evidently satisfy the equation in question. Squaring these equations, we have equations from which λ^2, $\lambda\mu$, μ^2 may be linearly determined; and making the necessary reductions, we find

$$\lambda^2 = abc + kp\theta$$
$$-2\lambda\mu = bc + ca + ab - (p\theta + kp + k\theta)$$
$$\mu^2 = a + b + c + k + p + \theta.$$

Or, eliminating λ, μ,

$$\{bc + ca + ab - (p\theta + kp + k\theta)\}^2$$
$$-4(a + b + c + k + p + \theta)(abc + kp\theta) = 0 \quad (*)$$

which is the rational form of the former equation marked (*). It is clear from the symmetry of the formula, that the same equation would have been obtained by the elimination of L, M from the equations

$$\sqrt{(k+a)(k+b)(k+c)} = L + Mk,$$
$$\sqrt{(p+a)(p+b)(p+c)} = L + Mp,$$
$$\sqrt{(\theta+a)(\theta+b)(\theta+c)} = L + M\theta.$$

And it follows from Abel's theorem (but the result may be verified by means of Euler's fundamental integral in the theory of elliptic functions), that if

$$\Pi x = \int_{\infty}^{\cdot} \frac{dx}{\sqrt{(x+a)(x+b)(x+c)}},$$

then the algebraical equations (*) are equivalent to the transcendental equation

$$\pm \Pi(k) \pm \Pi(p) \pm \Pi \theta = 0;$$

the arbitrary constant which should have formed the second side of the equation having been determined by observing that the algebraical equation gives for $p = \theta$, $k = \infty$ a system of values, which, when the signs are properly chosen, satisfy the transcendental equation. In fact, arranging the rational algebraical equation according to the powers of k, it becomes

$$k^2(p-\theta)^2 \qquad\qquad (*)$$
$$-2k\{p\theta(p+\theta) + 2(a+b+c)p\theta + (bc+ca+ab)(p+\theta) + 2abc\}$$
$$+p^2\theta^2 - 2(bc+ca+ab)p\theta - 4abc(p+\theta)$$
$$+a^2+b^2+c^2-2bc-2ca-2ab = 0;$$

which proves the property in question, and is besides a very convenient form of the algebraical integral. The ambiguous signs in the transcendental integral are not of course arbitrary (indeed it has just been assumed that for $p = \theta$, Πp and $\Pi \theta$ are to be taken with opposite signs), but the discussion of the proper values to be given to the ambiguous signs would be at all events tedious, and must be passed over for the present.

It is proper to remark, that $\theta = p$ gives not only, as above supposed, $k = \infty$, but another value of k, which, however, corresponds to the transcendental equation

$$\pm \Pi k \pm 2\Pi p = 0.$$

The value in question is obviously

$$4k = \frac{p^4 - 2(bc+ca+ab)p^2 - 8abcp + a^2+b^2+c^2-2bc-2ca-2ab}{(p+a)(p+b)(p+c)}.$$

Consider, in general, a cubic function $ax^3 + 3bx^2y + 3cxy^2 + dy^3$, or, as I now write it in the theory of invariants, $(a, b, c, d)(x, y)^3$, the Hessian of this function is

$$\left(ac-b^2, \ \frac{1}{2}(ad-bc), \ bd-c^2\right)(x, y)^2,$$

and applying this formula to the function $(p+a)(p+b)(p+c)$, it is easy to write the equation last preceding in the form

$$4k = p - (a+b+c) - \frac{9 \text{ Hessian } \overline{(p+a}\ \overline{p+b}\ \overline{p+c)}}{(p+a)(p+b)(p+c)},$$

which is a formula for the duplication of the transcendent Πx.

Reverting now to the general transcendental equation

$$\pm \Pi(k) \pm \Pi(p) \pm \Pi(\theta) = 0,$$

we have in like manner

$$\pm \Pi(k) \pm \Pi(p) \pm \Pi(\theta) = 0.$$

And assuming a proper correspondence of the signs, the elimination of $\Pi(p)$ gives

$$\Pi\theta' - \Pi\theta = 2\Pi(k);$$

i. e. if the points P, P' upon the conic $x^2 + y^2 + z^2 = 0$ are such that their parameters θ, θ' satisfy this equation, the line PP' will be constantly a tangent to the conic

$$k(x^2 + y^2 + z^2) + (ax^2 + by^2 + cz^2) = 0.$$

Whence also, if the parameters k, k', k'' of the conics

$$k\ (x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 = 0$$
$$k'\ (x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2. \ = 0$$
$$k''(x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 = 0$$

satisfy the equation

$$\Pi k + \Pi k' + \Pi k'' = 0,$$

there are an infinity of triangles inscribed in the conic $x^2 + y^2 + z^2 = 0$, and the sides of which touch the last-mentioned three conics respectively.

Suppose $2\Pi k = \Pi\kappa$ (an equation the algebraic form of which has already been discussed), then

$$\Pi\theta' - \Pi\theta = \Pi(\kappa),$$

$\theta = \infty$ gives $\theta' = \kappa$; or, observing that $\theta = \infty$ corresponds to a point of intersection of the conics $x^2 + y^2 + z^2 = 0$, $ax^2 + by^2 + cz^2 = 0$, κ is the parameter of the point in which a tangent to the conic $k\ (x^2 + y^2 + z^2) + ax^2 + by^2 + cz^2 = 0$ at any one of its intersections with the conic $x^2 + y^2 + z^2 = 0$ meets the last-mentioned conic. Moreover, the algebraical relation between θ, θ' and κ (where, as before remarked, κ is a given function of k) is given by a preceding formula, and is simpler than that between θ, θ' and k.

The preceding investigations were, it is hardly necessary to remark, suggested by a well-known memoir of the late illustrious Jacobi, and contain, I think, the extension which he remarks it would be interesting to make of the principles in such memoir to a system of two conics. I propose reverting to the subject in a memoir to be entitled "Researches on the Porism of the in- and circumscribed triangle."

2 Stone Buildings, Feb. 16, 1853.

XLIII. *Analysis of a Siliceous Deposit from the hot Volcanic Springs of Taupo, New Zealand. By* J. W. MALLET, *Ph.D.**

THIS substance occurs as a porous but tolerably compact concretion, opake, and of a white colour slightly tinged with yellow, very tough and difficult to break, and intermediate in hardness between felspar and quartz.

Its specific gravity, taken in small fragments, is 2·031.

Digested in a cold solution of caustic potash it dissolves almost completely, though with great slowness.

It was analysed by fusion with carbonate of soda in the usual way, the portion employed being first dried at 212° in order to separate merely hygroscopic moisture.

By digesting the finely pulverized mineral in boiling water, I found on testing the water with nitrate of silver that some soluble compound of chlorine was present; and by employing a larger quantity of the siliceous sinter, having separated the chloride of silver and removed the excess of silver by muriatic acid, on evaporation to dryness the substance in combination with the chlorine was found to be sodium. Nothing but chloride of sodium had been dissolved out by the water; and by testing the solution of the portion thus washed obtained after fusion with an alkali, it appeared that all the chlorine existed as such, and had all been dissolved by the water.

The results of the analysis were the following :—

Silica	94·20
Alumina	1·58
Peroxide of iron . .	·17
Lime	trace
Chloride of sodium .	·85
Water	3·06
	99·86

If we deduct 1·53 per cent. of the silica as having been combined with alumina and oxide of iron, the relative proportions of silica and water in the mineral will be—

			Atom.
Silica	. .	92·67	1·993
Water	. .	3·06	·340

numbers which closely approximate to the formula $6SiO^3$, HO. This differs greatly from the composition of siliceous deposits from the Geysers of Iceland, as given by Damour and Forchhammer, the per-centage of water being about one-half of that indicated by their analyses.

* Communicated by the Author.

The present analysis also differs widely from one by Mr. R. Pattison[*] of an incrustation from the volcanic springs of New Zealand (but of which the particular locality is not given), as his specimen yielded—

Silica	77·35
Alumina	9·70
Peroxide of iron . .	3·72
Lime	1·74
Water	7·66
	100·17

The occurrence of chlorine in deposits of this character has not, as far as I am aware, been before noticed, though potash and soda in small quantities have been detected, existing, it seemed probable, in combination with the silica.   The existence of chloride of sodium, therefore, in this incrustation appears to be a point of some interest, though its bearing upon the chemical geology of these volcanic springs could not be considered without more distinct information as to the nature of the springs themselves, and the circumstances under which the incrustation was formed, than I have been able to obtain.

XLIV. *On a Geometrical Problem noticed by* Mr. Sylvester. *By* Thomas K. Abbott, *Esq.*[†]

IN answer to Mr. Sylvester's invitation, I beg to offer a direct demonstration of the theorem, that if from the middle a of a circular arc bc chords ad, ae be drawn whose remote segments md, ne are equal, the whole chords are so.   The rectangle $en \cdot na = cn \cdot nb$.   Add $an^2 = no^2 + oa^2$ (o being middle of bc); then $ea \cdot na = oc^2 + ao^2 = ac^2$.   Similarly for the other chord, $\therefore ea \cdot na = da \cdot ma$.   Bisect md in f and ne in g, add to both rectangles $mf^2 = ng^2$; then we have $af^2 = ag^2$, $\therefore af = ag$, $\therefore af + fd = ag + gc$. Q. E. D.

Now to show directly that if the bisectors of the base angles of a triangle be equal the sides are equal.   Let the reader form the figure thus : bd bisecting $< abc$, af bisecting $< bac$; let them meet in o, and let the external bisector of c meet the former in e and the latter in g.   Then we have the harmonic mean between ao, ag = that between bo, be, $\therefore$ producing be to e', so that $ee' = bo$ and ag to g', so that $gg' = ao$; and reciprocating the figure from the origin o, the points b, a, e', g' give a parallelogram circumscribing a circle ; and if through p, q the feet of perpendi-

culars from o on the sides adjacent to an acute angle we draw parallels to the sides (remembering that ea is perpendicular to ag, and eb to bg), then drawing $pohm$, $qokn$ perpendicular to these, and meeting the other two sides respectively in h and k, we find the harmonic mean between om, $op=2oh$; between on, $oq=2ok$ ($\because ee'=ob$). Also in triangle $bo.oe=ao.og=bo(oe'-bo)$. Hence producing on, om so that $oq'=oq$, and $op'=op$, also $oh'=2oh$; $ok'=2ok$. The points mn, $p'q'$ are in a circle in which $h'k'$ is polar of o and $oh'+2op'=ok'+2oq'$. Construct another figure where $p'z$ is parallel to oq' and $h'w$, and $q'y$ to op' and $k'x$, oc meeting hk in e; then we can show without difficulty (by considering $co^2-cq'^2$ and $ck^2-cq'^2$, &c.) that

$$(oh-ok)(xz+yw+2oe)=0, \quad \therefore oh=ok, \quad \therefore om=on, \quad \therefore og=oe,$$
$$oa=ob, \text{ &c.}$$

More easily as follows:—suppose that the angle b is not less than a, and describe a circle through adb cutting af in m; then the angles abm, dab are acute, and am is not greater than af. But also by hypothesis abm is not less than cab, and both are acute; $\therefore am$ is not less than bd or af; $\therefore d$ is equal to af, and $abc=bac$. Q. E. D.

XLV. *On the Mutual Attraction or Repulsion between two Electrified Spherical Conductors.* By Prof. W. Thomson[*].

IN a communication made to the British Association at Cambridge in 1845, I indicated a solution adapted for numerical calculation, of the problem of determining the mutual attraction between two electrified spherical conductors. A paper published in November of the same year in the first Number of the Cambridge and Dublin Mathematical Journal, contains a formula actually expressing the complete solution for the case of an insulated sphere and a non-insulated sphere of equal radius, and numerical results calculated for four different distances for the sake of comparison with experimental results which had been published by Mr. Snow Harris. The investigation by which I had arrived at this solution, which was equally applicable to the general problem of finding the attraction between any two electrified spherical conductors, has not hitherto been published; but it was communicated in July 1849 to M. Liouville, along with another very different method by which I had just succeeded in arriving at the same result, in a letter the substance of which constitutes the present communication. Formulæ marked (8) .... (18) in that letter expressed the details of the solution

according to the two methods. They are reproduced here in terms of the same notation, and with the same numbers affixed. The first-mentioned method is expressed by the formulæ (16), (17), (18), and the other by (8) . . . . (15). The formulæ marked with letters (a), (b), &c. in the present paper express details of which I had not preserved exact memoranda.

Let A and B designate the two spherical conductors; let a and b be their radii, respectively; and let c be the distance between their centres. Let them be charged with such quantities of electricity, that, when no other conductors and no excited electrics are near them, the values of the potential* within them may be u and v respectively.

The distribution of electricity on each surface may be determined with great facility by applying the " principle of successive influences " suggested by Murphy (Murphy's Electricity, Cambridge, 1833, p. 93), and determining the effect of each influence by the method of " electrical images," given in a paper entitled " Geometrical Investigations regarding Spherical Conductors†." The following statement shows as much as is required of the results of this investigation for our present purpose.

Let us imagine an electrical point containing a quantity of electricity equal to ua to be placed at the centre of A, and another vb at the centre of B. The image of the former in B will be $-\dfrac{b}{c}.ua$, at a point in the line joining the centres, and distant by $\dfrac{b^2}{c}$ from the centre of B. The image of this in A will be $\dfrac{a}{c-\dfrac{b^2}{c}}.ua$, in the same line, at a distance $\dfrac{a^2}{c-\dfrac{b^2}{c}}$ from the centre of A; the image of this point in B will be $\dfrac{-b}{c-\dfrac{a^2}{c-\dfrac{b^2}{c}}}\,ua$, at a distance $\dfrac{b^2}{c-\dfrac{a^2}{c-\dfrac{b^2}{c}}}$ from the centre of B; and so on: and in a similar manner we may derive a series of imaginary points from vb at

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* The potential at any point in the neighbourhood of, or within, an electrified body, is the quantity of work that would be required to bring a unit of positive electricity from an infinite distance to that point, if the given distribution of electricity were maintained unaltered. Since the electrical force vanishes at every point within a conductor, the potential is constant throughout its interior.

† Cambridge and Dublin Mathematical Journal, Feb. 1850, vol. v. p. 9.

the centre of B. To specify completely these two series of imaginary points, let p_1, p'_1, p_2, p'_2, p_3, p'_3, &c. denote the masses of the series of which the first is at the centre of A; and let f_1, f'_1, f_2, f'_2, &c. denote the distances of these points from the centres of A and B alternately; and, again, let q_1, q'_1, q_2, q'_2 . . . denote the masses, and g_1, g'_1, g_2, g'_2 . . . the distances of the successive points of the other series from the centres of B and A alternately. These quantities are determined by using the following equations, and giving n successively the values $1, 2, 3, ...$;

$$\left[\begin{matrix} f_1 = 0, & p_1 = ua \\ f'_n = \dfrac{b^2}{c-f_n}, & p'_n = -p_n\dfrac{f'_n}{b} \\ f_{n+1} = \dfrac{a^2}{c-f'_n}, & p_{n+1} = -p'_n\dfrac{f_{n+1}}{a} \end{matrix}\right] \left[\begin{matrix} g_1 = 0, & q_1 = vb \\ g'_n = \dfrac{a^2}{c-g_n}, & q'_n = -q_n\dfrac{g'_n}{a} \\ g_{n+1} = \dfrac{b^2}{c-g'_n}, & q_{n+1} = -q'_n\dfrac{g_{n+1}}{b} \end{matrix}\right] (8).$$

The two series of imaginary electrical points thus specified, would, if they existed, produce the same action in all space external to the spherical surfaces as the actual distributions of electricity do, those $(p_1, q'_1, p_2, q'_2, \&c.)$ which lie within the surface A producing the effect of the distribution on A, and the others $(q_1, p'_1, q_2, p'_2, \&c.)$, all within the surface B, the effect of the actual distribution on B. Hence the resultant force between the two partial groups is the same as the resultant force due to the mutual action between the actual distributions of electricity on the two conductors; and if this force, considered as positive or negative according as repulsion or attraction preponderates, be denoted by F, we have

$$F = \Sigma_{s=1}^{s=\infty} \Sigma_{t=1}^{t=\infty} \left\{ \frac{p_s q_t}{(c-f_s-g_t)^2} + \frac{p'_s q'_t}{(c-f'_s-g'_t)^2} + \frac{p_s p'_t}{(c-f_s-f'_t)^2} \right. $$
$$\left. + \frac{q_s q'_t}{(c-g_s-g'_t)^2} \right\} \quad . \quad . \quad (9),$$

where $\Sigma\Sigma$ denotes a double summation, with reference to all integral values of s and t. The following process reduces this double series to the form of a single infinite series, of which the successive terms may be successively calculated numerically in any particular case with great ease.

First, taking from (8) expressions for p_s and f_s in terms of inferior order, and for q_t and g_t in terms of higher order, and continuing the reduction successively, we have

$$\frac{p_s q_t}{c-f_s-g_t} = \frac{p'_{s-1}\dfrac{a}{c-f'_{s-1}}\cdot q'_t\dfrac{a}{g'_t}}{c-\dfrac{a^2}{c-f'_{s-1}}-\left(c-\dfrac{a^2}{g'_t}\right)} = \frac{p'_{s-1}q'_t}{c-f'_{s-1}-g_t'}$$

$$=\frac{p_{s-1}\dfrac{b}{c-f_{s-1}}\cdot q_{t+1}\dfrac{b}{g_{t+1}}}{c-\dfrac{b^2}{c-f_{s-1}}-\left(c-\dfrac{b^2}{g_{t+1}}\right)} = \frac{p_{s-1}q_{t+1}}{c-f_{s-1}-g_{t+1}};$$

and therefore

$$\frac{p_s q_t}{c-f_s-g_t} = \frac{p_{s-1}q_{t+1}}{c-f_{s-1}-g_{t+1}} = \frac{p_{s-2}q_{t+2}}{c-f_{s-2}-g_{t+2}}\cdots = \frac{p_1 q_{t+s-1}}{c-f_1-g_{t+s-1}} = -uq'_{t+s-1}$$

and

$$\frac{p'_s q'_t}{c-f'_s-g'_t} = -uq'_{t+s}.$$

Similarly, we find

$$\frac{p_s p'_t}{c-f_s-f'_t} = \frac{p_{s-1}p'_{t+1}}{c-f_{s+1}-f'_{t+1}} = \cdots = \frac{p_1 p'_{t+s-1}}{c-f'_{t+s-1}} = -up_{t+s},$$

and

$$\frac{q_s q'_t}{c-g_s-g'_t} = -vq_{t+s}.$$

Now $\dfrac{p'_n}{u} = \dfrac{q'_n}{v}$, and $\dfrac{p_n}{u}$ and $\dfrac{q_n}{v}$ are each independent of u and v; hence the following notation may be adopted conveniently:

$$\left.\begin{array}{ll} p'_n = -\dfrac{u}{S_n}, & q'_n = -\dfrac{v}{S_n} \\[2mm] p_n = \dfrac{u}{P_n}, & q_n = \dfrac{v}{Q_n} \end{array}\right\} \quad \cdots \quad (13).$$

Then, taking n to denote $t+s$ in the preceding equations, we have

$$\left.\begin{array}{l} \dfrac{p_{n-t}q_t}{c-f_{n-t}-g_t} = \dfrac{p'_{n-t-1}q'_t}{c-f'_{n-t-1}-g'_t} = \dfrac{uv}{S_{n-1}}; \\[3mm] \dfrac{p_{n-t}p'_t}{c-f_{n-t}-f'_t} = -\dfrac{u^2}{P_n}; \quad \dfrac{q_{n-t}q'_t}{c-g_{n-t}-g'_t} = -\dfrac{v^2}{Q_n} \end{array}\right\} \quad \cdots \, (14).$$

Hence we have

$$\frac{p_{n-t+1}q_t}{(c-f_{n-t}-g_t)^2} = \left(\frac{uv}{S_{n-1}}\right)^2 \frac{1}{p_{n-t}q_t} = \frac{uv}{S^2_n}\cdot P_{n-t+1}\, Q_t;$$

from which we conclude that

$$\Sigma_{s=1}^{t=\infty}\, \Sigma_{t=1}^{s=\infty}\, \frac{p_s q_t}{(c-f_s-g_t)^2} = \Sigma_{n=1}^{n=\infty}\, \frac{uv}{S^2_n}\, \Sigma_{t=1}^{t=n}\, (P_{n-t+1}Q_t);$$

and, by using this and transformations similarly obtained for the other parts of the expression for F, we obtain

$$F = \Sigma_{n=1}^{n=\infty} \left\{ \frac{uv}{S_n^2} \left[\Sigma_{t=1}^{t=n}(P_{n-t+1} Q_t) + \Sigma_{t=1}^{t=n-1}(S_{n-t} S_t) \right] \right.$$

$$\left. - \frac{u^2}{P_n^2} \left[\Sigma_{t=1}^{t=n-1}(P_{n-t} S_t) \right] - \frac{v^2}{Q_n^2} \left[\Sigma_{t=1}^{t=n-1}(Q_{n-t} S_t) \right] \right\} \quad (15).$$

The quantities P_n, Q_n, S_n which occur in this expression, may be determined successively for successive values of n in the following manner:—By substituting, in (8), for p_n, p'_n, q_n, q'_n their values by (13), and eliminating f_n, f'_n, g_n, g'_n, we find

$$\left. \begin{array}{l} cP_n = aS_{n-1} + bS_n, \quad cQ_n = bS_{n-1} + aS_n, \\ cS_{n-1} = bP_{n-1} + aP_n = aQ_{n-1} + bQ_n \end{array} \right\} \quad . \quad . \quad (a);$$

from which we derive

$$\left. \begin{array}{l} P_{n+1} = \dfrac{c^2 - a^2 - b^2}{ab} P_n - P_{n-1} \\[2mm] Q_{n+1} = \dfrac{c^2 - a^2 - b^2}{ab} Q_n - Q_{n-1} \\[2mm] S_{n-1} = \dfrac{c^2 - a^2 - b^2}{ab} S_n - S_{n-1} \end{array} \right\} \quad . \quad . \quad . \quad (b).$$

By giving n the values 1 and 2 in (13) and (8), we find

$$\left. \begin{array}{l} \left\{ \begin{array}{l} P_1 = \dfrac{1}{a}, \\[2mm] P_2 = \dfrac{a^2 - b^2}{a^2 b} = \dfrac{c^2 - a^2 - b^2}{ab} P_1 + \dfrac{1}{b}, \end{array} \right. \\[6mm] \left\{ \begin{array}{l} Q_1 = \dfrac{1}{b}, \\[2mm] Q_2 = \dfrac{c^2 - a^2}{ab^2} = \dfrac{c^2 - a^2 - b^2}{ab} Q_1 + \dfrac{1}{a}, \end{array} \right. \\[6mm] \left\{ \begin{array}{l} S_1 = \dfrac{c}{ab}, \\[2mm] S_2 = \dfrac{c^2 - a^2 - b^2}{ab} S_1 \end{array} \right. \end{array} \right\} \quad . \quad . \quad (c).$$

By these equations we have directly the values of the first two terms of each of the sets of quantities P_1, P_2, P_3, &c., Q_1, Q_2, Q_3, &c., and S_1, S_2, S_3, &c.; and the others may be calculated successively by the preceding equations.

The polynomials which constitute the numerators of the successive terms of the second member of (15) may also be calculated successively, by means of equations obtained in the follow-

ing manner. We have by (c), (b), and (a),

$$P_1Q_n + P_2Q_{n-1} + P_3Q_{n-2} + \&c. = \frac{1}{a}Q_n + \left(\frac{c^2-a^2-b^2}{ab}P_1 + \frac{1}{b}\right)Q_{n-1}$$

$$+ \left(\frac{c^2-a^2-b^2}{ab}P_2 - P_1\right)Q_{n-2} + \&c.$$

$$= \frac{c}{ab}S_{n-1} + \frac{c^2-a^2-b^2}{ab}(P_1Q_{n-1} + P_2Q_{n-2} + \&c.) - (P_1Q_{n-2} + P_2Q_{n-3} + \&c.);$$

and similarly we find

$$S_1S_{n-1} + S_2S_{n-2} + S_3S_{n-3} + \&c.$$

$$= \frac{c}{ab}S_{n-1} + \frac{c^2-a^2-b^2}{ab}(S_1S_{n-2} + S_2S_{n-3} + \&c.) - (S_1S_{n-3} + S_2S_{n-4} + \&c.);$$

$$S_1P_{n-1} + S_2P_{n-2} + S_3P_{n-3} + \&c.$$

$$= \frac{c}{ab}P_{n-1} + \frac{c^2-a^2-b^2}{ab}(S_1P_{n-2} + S_2P_{n-3} + \&c.) - (S_1P_{n-3} + S_2P_{n-4} + \&c.);$$

and

$$S_1Q_{n-1} + S_2Q_{n-2} + S_3Q_{n-3} + \&c.$$

$$= \frac{c}{ab}Q_{n-1} + \frac{c^2-a^2-b^2}{ab}(S_1Q_{n-2} + S_2Q_{n-3} + \&c.) - (S_1Q_{n-3} + S_2Q_{n-4} + \&c.).$$

Hence, if we put

$$\left.\begin{array}{l} \Sigma_{t=1}^{t=n}(P_{n-t}Q_t) + \Sigma_{t=1}^{t=n-1}(S_{n-t}S_t) = 2S'_n \\ \Sigma_{t=1}^{t=n-1}(P_{n-t}S_t) = P'_n, \\ \Sigma_{t=1}^{t=n-1}(Q_{n-t}S_t) = Q_n \end{array}\right\} \quad . \quad . \quad (e),$$

and

in terms of which notation the expression (15) for F becomes

$$\left.\begin{array}{l} F = 2uv\left(\dfrac{S'_1}{S^2_1} + \dfrac{S'_2}{S^2_2} + \dfrac{S'_3}{S^2_3} + \&c.\right) \\ -\left\{u^2\left(\dfrac{P'_1}{P^2_1} + \dfrac{P'_2}{P^2_2} + \dfrac{P'_3}{P^2_3} + \&c.\right) + v^2\left(\dfrac{Q'_1}{Q^2_1} + \dfrac{Q'_2}{Q^2_2} + \dfrac{Q'_3}{Q^2_3} + \&c.\right)\right\} \end{array}\right\}(f),$$

we have

$$\left.\begin{array}{l} S'_{n+1} = \dfrac{c^2-a^2-b^2}{ab}S'_n - \left(S'_{n-1} - \dfrac{c}{ab}S_n\right) \\ P'_{n+1} = \dfrac{c^2-a^2-b^2}{ab}P'_n - \left(P'_{n-1} - \dfrac{c}{ab}P_n\right) \\ Q'_{n+1} = \dfrac{c^2-a^2-b^2}{ab}Q'_n - \left(Q'_{n-1} - \dfrac{c}{ab}Q_n\right) \end{array}\right\} \quad . \quad . \quad (g).$$

Also we have directly from (e) and (c),

$$
\left.
\begin{array}{ll}
\mathrm{S}'_1 = \dfrac{1}{2}\dfrac{1}{ab}, & \mathrm{S}'_2 = \dfrac{1}{2}\dfrac{3c^2 - a^2 - b^2}{a^2 b^2} \\[2mm]
\mathrm{P}'_1 = 0, & \mathrm{P}'_2 = \dfrac{c}{a^2 b} \\[2mm]
\mathrm{Q}'_1 = 0, & \mathrm{Q}'_2 = \dfrac{c}{ab^2}
\end{array}
\right\}
\quad . \quad . \quad (h).
$$

These equations enable us to calculate successively the values of S'_1, S'_2, S'_3, &c., P'_1, P'_2, P'_3, &c., and Q'_1, Q'_2, Q'_3, &c., after the values of S_1, S_2, &c., P_1, P_2, &c., and Q_1, Q_2, &c. have been found.

The solution of (b) as equations of finite differences with reference to n, and the determination of the arbitrary constants of integration by (c), leads to general expressions for S_n, P_n, and Q_n; and by using these in (g), integrating the equations so obtained, and determining the arbitrary constants by means of (h), general expressions for S'_n, P'_n, and Q'_n are obtained. The expression for F may therefore be put into the form of an infinite series, with a finite expression for the general term. Further, the value of this series may be expressed, by means of analysis similar to that which Poisson has used for similar purposes, in terms of a definite integral. I do not, however, in the present communication give any of this analysis, except for the case of two spheres in contact which is discussed below, because, except for cases in which the spheres are very near one another, the series for F is rapidly convergent, and the terms of it may be successively calculated with great ease, by regular arithmetical processes, for any set of values of c, a, and b, by using first the equations (c), to calculate S_1, S_2, P_1, P_2, Q_1, Q_2; then (b) with the values 2, 3, &c. successively substituted for n, to calculate S_3, S_4, &c., and P_3, P_4, &c., and Q_3, Q_4, &c.; then (h) and (g) to calculate by a similar succession of processes, the values of S'_1, S'_2, S'_3, &c., P'_1, P'_2, P'_3, &c., and Q'_1, Q'_2, Q'_3, &c.

The following is the method, alluded to above, by which I first arrived at the solution of this problem in the year 1845.

The " mechanical value " of a distribution of electricity on a group of insulated conductors, may be easily shown to be equal to half the sum of the products obtained by multiplying the quantity of electricity on each conductor into the potential within it*.

---

* This proposition occurred to me in thinking over the demonstration which Gauss gave of the theorem *that a given quantity of matter may be distributed in one and only one way over a given surface so as to produce a given potential at every point of the surface,* and considering the mechanical signification of the function on the rendering of which a minimum that de-

Hence, if D and E denote the quantities of electricity on the two spheres in the present case, and if W denote the mechanical value of the distribution of electricity on them, we have

$$W = \frac{1}{2}(Du + Ev).$$

Now if the two spheres, kept insulated, be pushed towards one another, so as to diminish the distance between their centres from c to $c - dc$, the quantity of work that will have to be spent will be $F \cdot dc$, since F denotes the repulsive force against which this relative motion is affected. But the mechanical value of the distribution in the altered circumstances must be increased by an amount equal to the work spent in producing no other effect but this alteration. Hence $F \cdot dc = -dW$, and therefore

$$F = -\frac{1}{2}\frac{d(Du + Ev)}{dc} \quad \cdots \quad (16),$$

where u and v are to be considered as varying with c, and D and E as constants. Now, according to the notation expressed in (13), we have

$$\left.\begin{array}{l} \left(\dfrac{1}{P_1} + \dfrac{1}{P_2} + \&c.\right)u - \left(\dfrac{1}{S_1} + \dfrac{1}{S_2} + \&c.\right)v = D \\[2ex] -\left(\dfrac{1}{S_1} + \dfrac{1}{S_2} + \&c.\right)u + \left(\dfrac{1}{Q_1} + \dfrac{1}{Q_2} + \&c.\right)v = E \end{array}\right\} \quad (17).$$

Determining $\dfrac{du}{dc}$ and $\dfrac{dv}{dc}$ by the differentiation of these equations, and using the results in (16), we find

$$F = \frac{1}{2}\left\{ u^2 \frac{d}{dc}\left(\frac{1}{P_1} + \frac{1}{P_2} + \&c.\right) - 2uv\frac{d}{dc}\left(\frac{1}{S_1} + \frac{1}{S_2} + \&c.\right) \right.$$
$$\left. + v^2\frac{d}{dc}\left(\frac{1}{Q_1} + \frac{1}{Q_2} + \&c.\right)\right\} \quad (18).$$

This expression agrees perfectly with (f), given above; since, by differentiating the equations (b) and (c) with reference to c, we find that the quantities denoted above by S'_1, S'_2, S'_3, &c., P'_1, P'_2, P'_3, &c., Q'_1, Q'_2, Q'_3, &c., and expressed by the equations (g) and (h), are equal respectively to

$$\frac{1}{2}\frac{dS_1}{dc}, \quad \frac{1}{2}\frac{dS_2}{dc}, \quad \frac{1}{2}\frac{dS_3}{dc}, \&c., \quad \frac{1}{2}\frac{dP_1}{dc}, \quad \frac{1}{2}\frac{dP_2}{dc}, \quad \frac{1}{2}\frac{dP_3}{dc}, \&c.,$$
$$\frac{1}{2}\frac{dQ_1}{dc}, \quad \frac{1}{2}\frac{dQ_2}{dc}, \quad \frac{1}{2}\frac{dQ}{dc}, \&c.$$

monstration is founded. It was first published, I believe, by Helmholtz in 1847, in his treatise " Ueber die Erhaltung der Kraft," by the translation of which in the last number of the New Scientific Memoirs a great benefit has been conferred on the British scientific public.

The series (f) or (18) for F becomes divergent for the case of two spheres in contact, but the doubly infinite series from which this was derived in the first of the two investigations given above, is convergent when the terms are properly grouped together; and its sum may be expressed by means of a definite integral in the following manner.

Since the two spheres are in contact, the potentials within them must be equal, that is, we must have $u=v$. For the sake of simplicity, let us suppose the radii of the two spheres to be equal, and let each be taken as unity. Then we shall have $a=b=1$, and $c=2$; and the terms of doubly infinite series (9) in this case are easily expressed*, in very simple forms, by equations (8). Thus we find

$$
\mathrm{F}=v^2 \times \left[
\begin{aligned}
&\frac{1}{2^2} - \frac{1\cdot2}{3^2} + \frac{1\cdot3}{4^2} - \frac{1\cdot4}{5^2} + \frac{1\cdot5}{6^2} - \&c. \\
&- \frac{2\cdot1}{3^2} + \frac{2\cdot2}{4^2} - \frac{2\cdot3}{5^2} + \frac{2\cdot4}{6^2} - \&c. \\
&\quad + \frac{3\cdot1}{4^2} - \frac{3\cdot2}{5^2} + \frac{3\cdot3}{6^2} - \&c. \\
&\qquad - \frac{4\cdot1}{5^2} + \frac{4\cdot2}{6^2} - \&c. \\
&\qquad\quad + \frac{5\cdot1}{6^2} - \&c.
\end{aligned}
\right\} \quad . \quad (k)
$$

If we add the terms in the vertical columns, we find

$$
\mathrm{F}=v^2 \times \frac{1}{6}\left(\frac{1\cdot2\cdot3}{2^2} - \frac{2\cdot3\cdot4}{3^2} + \frac{3\cdot4\cdot5}{4^2} - \&c.\right),
$$

which is a diverging series, and is the same as we should have

---

* From equations (8) we find, in this case,

$$
f'_n = g'_n = \frac{2n-1}{2n}; \quad f_n = g_n = \frac{2n-2}{2n-1}
$$

$$
p'_n = q'_n = -\frac{v}{2n}; \quad p_n = q_n = \frac{v}{2n-1}.
$$

Hence

$$
\frac{p_s q_t}{(c-f_s-g_t)^2} = \frac{(2s-1)(2t-1)}{\{2(s+t)-2\}^2}
$$

$$
\frac{p'_s q'_t}{(c-f'_s-g'_t)^2} = \frac{2s\cdot2t}{\{2(s+t)\}^2}
$$

$$
\frac{p_s p'_t}{(c-f_s-f'_t)^2} = \frac{q_s q'_t}{(c-g_s-g'_t)} = -\frac{2t(2s-1)}{\{2(s+t)-1\}^2};
$$

and then, by (9), we obtain the expression for F in this particular case, given in the text.

found by using the form (f) or (18).   But if we add the terms in the horizontal lines, we find the following convergent series for F :—

$$F = v^2 \left\{ \int_0^1 \frac{\log\frac{1}{\theta} \cdot \theta d\theta}{(1+\theta)^2} - 2\int_0^1 \frac{\log\frac{1}{\theta} \cdot \theta^2 d\theta}{(1+\theta)^2} + 3\int_0^1 \frac{\log\frac{1}{\theta} \cdot \theta^3 d\theta}{(1+\theta)^2} - \&c. \right\}$$

Hence, since $(1+\theta)^{-2} = 1 - 2\theta + 3\theta^2 - \&c.$, we have

$$F = v^2 \int_0^1 \frac{\log\frac{1}{\theta} \cdot \theta d\theta}{(1+\theta)^4} \quad . \quad . \quad . \quad . \quad . \quad (l) ;$$

or, by actual integration,

$$F = v^2 \left[\frac{\log\frac{1}{\theta}}{(1+\theta)^3} \frac{3\theta^2 + \theta^3}{6} + \frac{1}{6} \log(1+\theta) - \frac{1}{6} \frac{\theta}{(1+\theta)^2} \right]_0^1$$

$$= v^2 \cdot \frac{1}{6} \times \left(\log 2 - \frac{1}{4} \right) = v^2 \cdot \frac{1}{6} \times (\cdot 69315 - \cdot 25)$$

$$= v^2 \times \cdot 073858.$$

The quantity of electricity on each sphere being equal to the sum of the masses of the imaginary series of points within it, is, according to the formulæ for p_1, q'_1, p_2, q'_2, &c.

$$v \left(1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \&c. \right), \text{ or } v \log 2.$$

Hence we have the following expression for the repulsion between the two spheres, in terms of Q the quantity of electricity on each,

$$F = Q^2 \cdot \frac{\frac{1}{6} \times (\log 2 - \frac{1}{4})}{(\log 2)^2}.$$

If x denote the distance at which two electrical points, containing quantities equal to the quantities on the two spheres, must be placed so as to repel one another with a force equal to the actual force of repulsion between the spheres, we have

$$\frac{(v \cdot \log 2)^2}{x^2} = F.$$

Using the value for F found above, we obtain

$$x = \frac{\log 2}{\sqrt{\left\{ \frac{1}{6} \times (\log 2 - \frac{1}{4}) \right\}}} = 2 \cdot 550.$$

If the electrical distributions on each surface were uniform, this distance would be equal to 2, the distance between the centres of the spheres; but it exceeds this amount, to the extent shown by the preceding result, because in reality the electrical density

on each conductor increases gradually from the point of contact to the remotest points of the two surfaces.

P.S. The calculation by the method shown in the preceding paper, of the various quantities required for determining the force between two *spheres of equal radii* (each unity), insulated with their centres at distances 2·1, 2·2, 2·3, &c. up to 4, has been undertaken, and is now nearly complete. I hope to be able to communicate the results for publication in the next or in an early number of the Magazine.

Glasgow College, March 21, 1853.

XLVI. *On a proposed Test of the Necessity of Indirect Proof in Geometrical Demonstrations, with Remarks on Methods of Demonstration.* By JAMES ADAMSON, *D.D.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

As you have in some late numbers of the Philosophical Magazine admitted the discussion of matters which are elementary in regard to geometry, but are of some interest in regard to the nature of geometrical argument, and as I have been called upon by peculiar circumstances to analyse that subject as a mode of intellectual education, I trust that you will permit me to offer to your notice a few of the conclusions to which I have been led.

Sir William Hamilton, Professor of Logic in the University of Edinburgh, has expressed himself as estimating mathematical science at no high value for such an end. It may perhaps be shown that less benefit than is attainable has been derived from it, not so much on account of the nature of the subject itself, as from the mode in which it has been treated.

In the Number of the Philosophical Magazine for December 1852, Mr. Sylvester has noticed a theorem regarding isosceles triangles, the discussion of which appears to have been attended with circumstances somewhat singular; and from his examination of it he has deduced a principle offered as a test for distinguishing those cases of geometrical argument in which the process is necessarily *indirect.* The circumstances remarked as singular are, that mathematicians of eminence had found the demonstration to be difficult, and that they had been able to establish its truth only by modes which are indirect. It was therefore taken by Mr. Sylvester as fitted to illustrate a suggestion by him, that the nature of the equations connected with such geometrical truths may determine the question whether the demonstrations admit, or do not admit, of direct modes of argument.

In regard to these positions, I would beg leave to observe, that, to a mind trained to correct modes of geometrical reasoning,

there could scarcely, I apprehend, have been found any difficulty
in the demonstration, and that there is no necessity for any *indi-
rectness* of procedure in it, according to the common apprehen-
sion of the term *indirect.*

Mr. Sylvester makes an inquiry also regarding a theorem,
which we may, if we choose, treat as subordinate to that above
alluded to.   The data are, that straight lines are drawn from the
middle of an arc, so that the portions of these lines intercepted
between the chord and the opposite part of the circumference
shall be equal.   The conclusion sought to be proved is, that the
portions of the lines between the chord and the arc are equal ;
or, which is the same thing, that the sums of both portions are
equal.   Now here are three positions, such that any one being
assumed as a portion of the hypothesis, the other two become
easy conclusions ; any case being *converse* to any other.   Now
it may be stated as a general rule, that a *converse* is most easily
and elegantly established by an indirect demonstration.   The
correct mode in such cases therefore is, to proceed *directly* with
that demonstration which is the most easy, and to take the others
indirectly as its converses.   These easier cases are determined by
this principle, viz. that the constituents, whether lines or angles,
whose relations are assumed as data, are, as to their position,
origin, or termination, connected with points, the relations of
which to others are settled by theorems already known.   When
the data are in regard to origin, position, or termination, other-
wise *dispersed,* the demonstration ought to follow as a converse
and be *indirect.*   But it is not necessarily indirect, or is so in
few cases.

The question put by Mr. Sylvester is, whether in the case
above mentioned this making of it to be a converse, or treating
it indirectly, can be avoided.   This is easily settled.   The position
assumed as hypothesis being, that the *more distant* portions of
the straight lines drawn from the middle of the arc are equal, it
will be seen at a glance that the chord of either half of the arc
is a mean proportional between the whole straight lines and their
nearer portions.   This necessarily and *directly* affords the con-
clusions, that these wholes and portions are equal each to each.

From this we can show *directly,* that if two triangles have
equal bases, equal vertical angles, and equal angle-bisectors at
that vertex, then the portions of their bases intercepted by the
angle-bisectors are equal each to each.   From this will follow
directly the conclusion, that if two angle-bisectors of a triangle
be equal, the triangle is isosceles, which is the theorem proposed.

If these conclusions be correct, it will follow that the test of
necessary indirectness in demonstration proposed by Mr. Syl-
vester is not applicable, as being founded on a case where that
property of geometrical argument does not necessarily exist.

· If elementary books of geometry were, like those of more modern sciences, accommodated to the character of the age, it could not, I presume, have happened that any one should have felt puzzled by such theorems as the above.   The general relations of the subjects considered would then be better understood. The theorem now discussed is only a modified instance of one more general, viz. that in any triangle the smaller angle has the larger bisector.   Both may be deductions from the following position, viz. that of all straight lines passing through the same point in the bisector of an angle, and limited by its constituents, that which has the smaller inclination to the bisector is the longer, and those that are equally inclined to the bisector are equal, and conversely, by direct demonstration, those which are equal are equally inclined.

From these truths we may derive the modified case relating to the isosceles triangle, either directly or indirectly ; the indirect mode being, as is the case generally when it is properly employed, the easiest and the most elegant.

<div align="center">[To be continued.]</div>

XLVII. *A Proof that all the Invariants* * *to a cubic Ternary Form are Rational Functions of* Aronhold's *Invariants and of a cognate theorem for biquadratic Binary Forms.   By* J. J. Sylvester, *F.R.S.*†

ALTHOUGH contrary to the order of exposition indicated in the title to this paper, I shall, as the simpler case, begin with establishing the theorem for a biquadratic form, say F in x, y.   Let

$$\mathrm{F} = ax^4 + 4bx^3y + 6cx^2y^2 + 4dxy^3 + ey^4$$
$$s = ae - 4bd + 3c^2$$
$$t = ace - ad^2 - c^3 - b^2e + 2bcd,$$

s and t are the two well-known invariants of F.   I propose to prove that there can exist no other invariants to F except such as are explicit rational functions of s and t.

Let F, by means of the substitution of $fx + gy$ for x, and

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* Communicated by the Author.

† A *Constant* in analysis is any quantity which in its own nature, or by the explicit conditions to which it is subjected, is incapable of change.   An *Invariant* is an expression apparently liable to change, but which, owing to certain compensations in the modifying tendencies impressed upon it, remains as a whole unaltered.   The former may be compared to a fixed point or system in mechanics; the latter to a point or system free to move, but kept at rest under the combined operation of contending forces.

$f'x + g'y$ for y, be made to take the form $f_1 = x^4 + y^4 + 6mx^2y^2$. Then by the characteristic property of invariants, if $\mathrm{I}(a, b, c, d, e)$ be any invariant to F of the degree q, we must have

$$\mathrm{I}(1, 0, m, 0, 1) = (fg' - f'g)^{2g}\, \mathrm{I}(a, b, c, d, e);$$

and it will be sufficient to prove that $\mathrm{I}(1, 0, m, 0, 1)$, or say more simply $\mathrm{I}(m)$, can only have the two radically distinct forms corresponding to s and t, *i. e.*

$$(s) = 1 - 3m^2 \text{ and } (t) = m - m^3,$$

any other admissible form of I being a rational explicit function of these two.

It may be shown* that the parameter m in f will have six different values and no more.   In the first place, if we write ιx for x (ι meaning $\sqrt{-1}$), it is obvious that m becomes $-m$.   Again, let $x + \iota y$ and $x - \iota y$ be substituted in place of x and y respectively; then calling (f) the value assumed by F, when this substitution is made,

$$\begin{aligned}
(f) &= (x + \iota y)^4 + (x - \iota y)^4 + 6m(x^2 + y^2)^2 \\
&= (2 + 6m)(x^4 + y^4) + (-12 + \iota 2m)x^2 y^2 \\
&= (2 + 6m)\left\{ x^4 + y^4 + 6\frac{-\iota + m}{\iota + 3m}x^2 y^2 \right\}.
\end{aligned}$$

Hence if we write

$$\frac{\iota}{(2 + 6m)^{\frac{1}{4}}} x + \frac{\iota}{(2 + 6m)^{\frac{1}{4}}} y \text{ for } x,$$

and

$$\frac{\iota}{(2 + 6m)^{\frac{1}{4}}} x - \frac{\iota}{(2 + 6m)^{\frac{1}{4}}} y \text{ for } y,$$

and call what f_1 becomes after these substitutions f_2,

$$f_2 = \{ x^4 + y^4 + 6\gamma(m)x^2 y^2 \},$$

$\gamma(m)$ denoting $\dfrac{-1 + m}{1 + 3m}$.

In like manner, by writing in f_2

$$\frac{1}{\{2 + 6\gamma(m)\}^{\frac{1}{4}}} x + \frac{\iota}{((2 + 6\gamma(m))^{\frac{1}{4}}} y \text{ for } x$$

and

$$\frac{1}{\{2 + 6\gamma(m)\}^{\frac{1}{4}}} x - \frac{\iota}{\{2 + 6\gamma(m)\}^{\frac{1}{4}}} y \text{ for } y,$$

we obtain

$$f_3 = x^4 + y^4 + 6\gamma^2(m)x^2 y^2,$$

* See addendum.

where
$$\gamma^2 m = \frac{-1 + \dfrac{-1 + m}{1 + 3m}}{1 + 3\dfrac{-1 + m}{1 + 3m}} = \frac{-2 - 2m}{-2 + 6m} = \frac{-1 - m}{-1 + 3m};$$

$\gamma(m)$ is a periodic function of m of the third order, for we find
$$\gamma^3(m) = \gamma^2(\gamma m) = \frac{-(1 + 3m) - (-1 + m)}{-(1 + 3m) + 3(-1 + m)} = m.$$

It will of course be observed, also, that
$$\gamma^2 m = -\gamma(-m) \text{ and } \gamma m = -\gamma^2(-m).$$

Hence
$$(-\gamma)(-\gamma)m = -\gamma^3(-m) = m(-\gamma^2)(-\gamma^2)m = -\gamma^3(-m) = m.$$

So that, in fact, the six values of the parameter are
$$m, \quad \gamma m, \quad \gamma^2 m$$
$$-m, \quad -\gamma m, \quad -\gamma^2 m,$$

forming two cycles, having the remarkable property that the terms in the same cycle are periodic functions of the third order of one another, and each term in one cycle is a periodic function of the second order of every term in the other cycle.

The modulus of substitution for passing from f_1 to f_2, *i. e.* the square of the determinant

$$\begin{vmatrix} \dfrac{1}{(2+6m)^{\frac{1}{4}}} & \dfrac{\iota}{(2+6m)^{\frac{1}{4}}} \\ \dfrac{1}{(2+6m)^{\frac{1}{4}}} & \dfrac{-\iota}{(2+6m)^{\frac{1}{4}}} \end{vmatrix} \text{ is } \frac{(-2\iota)^2}{(2+6m)}, \quad i. e. \quad \frac{-2}{1+3m}.$$

So that if $I(m)$ be the value of any invariant of the degree q, corresponding to the form f_1, and consequently $I\left(\dfrac{m-1}{1+3m}\right)$ the same for f_2, we must have
$$I(m) = \left(\frac{1+3m}{-2}\right)^q . I\left(\frac{m-1}{1+3m}\right).$$

In like manner, by means of f_3 it may be shown that we must have the further equation
$$Im = \left(\frac{1-3m}{-2}\right)^q . I\left(\frac{m+1}{1-3m}\right).$$

These equations are easily verified for the values of (s) and (t). Thus

$$(s) = 1 + 3m^2 = \frac{(1+3m)^2}{4}\left\{1 + 3\left(\frac{m-1}{3m+1}\right)^2\right\}$$
$$= \frac{(1-3m)^2}{4}\left\{1 + 3\left(\frac{m+1}{1-3m}\right)^2\right\}$$

$$(t) = m - m^3 = -\frac{(1+3m)^3}{8}\left\{\frac{m-1}{3m+1} - \left(\frac{m-1}{3m+1}\right)^3\right\}$$
$$= -\frac{(1-3m)^3}{8}\left\{\frac{m+1}{1-3m} - \left(\frac{m+1}{-3m}\right)^3\right\};$$

and it is moreover obvious, that the values of (s) and (t) might have been found *à priori* by means of these functional equations.

The essential point of inference for my present purpose from the equations above which are of the form

$$\mathrm{I}m = \mathrm{H} \times \mathrm{I}\left(\frac{m-1}{3m+1}\right) = \mathrm{K} \times \mathrm{I}\left(\frac{m+1}{1-3m}\right)$$

is this, that if $\mathrm{I}(m)$ contain any power of m, say m^t, it must also contain $(m-1)^t$ and $(m+1)^t$; in a word, $(m^3 - m)^t$, which, by the way, it may be noticed is t^t. Now, if possible, let there be any invariant $\mathrm{I}_q(m)$ of the qth degree in (m) which is not a rational function of (s) and (t). If we make $2x + 3y = q$, as many integer solutions as exist of this equation (in which zero values of x and y are admissible), so many functions of the form $(s)^x \cdot (t)^y$ may be formed of the degree (q) in (m), and all of them of course invariantive functions.

As regards the general nature of any invariantive function in (m), since the change of x into $-x$ in $x^4 + y^4 + 6mx^2y^2$ introduces no change into the invariant if q be even, but changes the sign if q be odd, it follows that $\mathrm{I}^q(m)$ is of the form $\phi(m)^2$ when q is even, and of the form $m\phi(m^2)$ when q is odd.

Let μ be the number of solutions of the equation in integers above written. Then, by linearly combining all the different values of $(s)^x \cdot (t)^y$ with $\mathrm{I}_q(m)$, it is obvious that we may form a new invariant, say I'_q, in which the μ first occurring powers of m will be wanting, *i. e.* in which the indices $0, 2, 4, \ldots (2\mu - 2)$ will be wanting when q is even, and $1, 3, 5, \ldots 2\mu - 1$ when q is odd. Hence in the former case the new invariant will contain $m^{2\mu}$, and in the latter case $m^{2\mu+1}$; and therefore, by virtue of what has been shown already, I'_q will contain $(m^3 - m)^{2\mu}$ in the one case and $(m^3 - m)^{2\mu+1}$ in the other.

1. Let $q = 6i$, or $6i + 2$, or $6i + 4$; then $\mu = i + 1$; and therefore $(m^3 - m)^{2i+2}$, which is of the degree $6i + 6$ in (m), is contained as a factor in I which is of the degree (q) only, a quantity less than $6i + 6$, which is absurd.

Again, 2nd. Let $q = 6i + 1$, then $\mu = i$; and $(m^3 - m)^{2\mu+1}$ is of the degree $6i + 3$ in (m), and is contained as a factor in I, which is of the degree $6i + 1$, which is again absurd.

So, 3rd. If $q = 6i + 5$, $\mu = i + 1$; and the factor $(m^3 - m)^{2\mu+1}$ is of the degree $6i + 9$, which is still more absurd.

Finally, if $q = 6i + 3$, $\mu = i + 1$; and the factor becomes $(m^3 - m)^{6i+3}$, *i. e.* $(m^3 - m)^q$, and consequently the entire value of I' is $(t)^q$; but I differs only from I' by linear combinations of powers of (s) and (t). Hence I on this supposition cannot be

anything else but a rational function of (s) and (t), and thus the theorem is completely demonstrated.

It may for a moment be objected, that we have been dealing only with a particular form $x^4 + 6mx^2y^2 + y^4$, instead of the general form $ax^4 + 4bx^3y + 6cx^2y^4 + 4dxy^3 + ey^4$; but the latter is always reducible to the former by means of a definite linear substitution; and if we call the modulus of the substitution [*i. e.* the square of the determinant formed by the coefficients of substitution] M, to every general invariant I_q of the qth degree, to the latter corresponds a partial form (I_q) of invariant to the former, such that

$$I_q = \frac{1}{M^q} \cdot (I_q) \, ;$$

and consequently, since every (I) is a rational function of (s) and (t), so must every I be the same of s and t; unless, indeed, it were possible to have $I_{q'} = \frac{1}{M^{q'}} . (I^q)$, q' being different from and greater than q: but if this were the case, since $I_q = \frac{1}{M^q}(I_q)$ a power of M, the modulus would necessarily be an invariant; but in passing from $x^4 + y^4 + 6mx^2y^2$ to $x^4 + y^4 + 6\gamma(m)x^2y^2$, $1 + 3m$ becomes the modulus, which we know is not an invariant; hence the proposition is completely established for the case of the biquadratic function $(x, y)^4$*.

[To be continued.]

XLVIII. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

Feb, 11, 1853. ON the influence of Material Aggregation upon the manifestations of Force, by John Tyndall, Esq., Ph.D.
There are no two words with which we are more familiar than *matter* and *force*. The system of the universe embraces two things,— an object acted upon, and an agent by which it is acted upon ;—the object we call matter, and the agent we call force. Matter, in certain aspects, may be regarded as the vehicle of force; thus the luminiferous æther is the vehicle or medium by which the pulsations of the sun are transmitted to our organs of vision. Or to take a plainer case ; if we set a number of billiard balls in a row and impart a shock to one end of the series, in the direction of its length, we

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* I have made a tacit assumption throughout the foregoing demonstration (which is, however, capable of an easy proof), viz. that if any fractional function of the coefficients of any form be invariantive, the numerator and denominator must be separately invariants.

know what takes place; the last ball will fly away, the intervening balls having served for the transmission of the shock from one end of the series to the other. Or we might refer to the conduction of heat. If, for example, it be required to transmit heat from the fire to a point at some distance from the fire, this may be effected by means of a conducting body—by the poker for instance : thrusting one end of the poker into the fire it becomes heated, the heat makes its way through the mass, and finally manifests itself at the other end. Let us endeavour to get a distinct idea of what we here call heat; let us first picture it to ourselves as an agent apart from the mass of the conductor, making its way among the particles of the latter, jumping from atom to atom, and thus converting them into a kind of stepping-stones to assist its progress. It is a probable conclusion, even had we not a single experiment to support it, that the mode of transmission must, in some measure, depend upon the manner in which those little molecular stepping-stones are arranged. But we need not confine ourselves to the material theory of heat. Assuming the hypothesis which is now gaining ground, that heat, instead of being an agent apart from ordinary matter, consists in a motion of the material particles; the conclusion is equally probable that the transmission of the motion must be influenced by the manner in which the particles are arranged. Does experimental science furnish us with any corroboration of this inference? It does. More than twenty years ago MM. De la Rive and De Candolle proved that heat is transmitted through wood with a velocity almost twice as great along the fibre as across it. This result has been recently expanded, and it has been proved that this substance possesses three axes of calorific conduction; the first and greatest axis being parallel to the fibre; the second axis perpendicular to the fibre and to the ligneous layers; while the third axis, which marks the direction in which the greatest resistance is offered to the passage of the heat, is perpendicular to the fibre and parallel to the layers.

But it is the modification of the magnetic force by the peculiarities of aggregation, which forms the subject of the evening's lecture. What has been stated regarding heat applies with equal force to magnetism. The observed magnetic phænomena are of a composite character. The action of a magnetic mass is the resultant action of its molecules, and will be influenced by the manner in which they are aggregated. The fundamental phænomena of magnetism are too well known to render it necessary to dwell upon them for an instant. A small bar of iron was suspended in the magnetic field; it set its length parallel to the line joining the poles. Should we be justified from this experiment in concluding that a magnetic mass will always set its longest dimension axial? No. A second magnetic bar, equal in size to the former, was suspended between the poles; it set its length at right angles to the line joining the poles. Whence this deportment? We find the reason of it in the mechanical structure of the bar : it is composed of magnetic plates, transverse to its length; these plates set from pole to pole, and hence the length of the bar equatorial. But let us proceed

from this coarse experiment to one more delicate, where nature herself has imposed the conditions of aggregation. A plate taken from a mass of shale, picked up a few weeks ago in the coal district of Blackburn, was suspended between the poles; although strongly magnetic, it set its longest dimension at right angles to the line joining the poles. This deportment was at once explained by reference to the structure of the mass: it also, though apparently compact, was composed of layers transverse to its length; these layers set from pole to pole, and hence the length equatorial. Let us ascend to a case still more refined. A crystal of sulphate of nickel was suspended between the poles, and on exciting the magnet a certain determinate position was taken up by the crystal. The substance was magnetic, still its shortest dimension set from pole to pole. The crystal was removed from the magnetic field and the edge of a penknife placed along the line which set axial; a slight pressure split the crystal and disclosed two beautiful surfaces of cleavage. The crystal could in this way be cloven into an indefinite number of magnetic layers; these layers set from pole to pole, and hence the longest dimension, which was perpendicular to the layers, equatorial. Comparing all these experiments,—ascending from the gross case where the laminæ were plates of iron stuck together by wax, to that in which they were crystalline, the inference appears unavoidable, that the unanimity of deportment exhibited is the product of a common cause; and that the results are due to the peculiarities of material aggregation.

The beautiful researches of Plücker in this domain of science are well known. Plücker's first experiment was made with a plate of tourmaline. Suspended in the magnetic field with the axis of the crystal vertical, it set its length from pole to pole, like an ordinary magnetic body. Suspended with the axis horizontal, on exciting the magnet, Plücker found to his astonishment that the largest dimension set equatorial. Let us see whether we cannot obtain this deportment otherwise. Suspending the piece of shale already made use of, so that its laminæ were horizontal, on exciting the magnet the longest horizontal dimension of the plate set axial: moving the point of suspension 90° so that the laminæ were vertical, on exciting the magnet the length of the plate set equatorial. In the magnetic field the deportment of the crystal was perfectly undistinguishable from that of the shale. But it may be retorted that tourmaline possesses no such laminæ as those possessed by the shale: true—nor is it necessary that it should do so. A number of plates, bars, and discs, formed artificially from magnetic dust, exhibited a deportment precisely similar to the tourmaline,—suspended from one point they set their lengths axial, suspended from another point the lengths set equatorial. Let us now turn to what may be called the complementary actions exhibited by diamagnetic bodies. A homogeneous diamagnetic bar sets its length equatorial. But bars were exhibited composed of transverse diamagnetic laminæ which set their lengths axial. This experiment is complementary to that of the shale, &c.; the magnetic laminæ set

axial, the diamagnetic equatorial; and by attention to this the
magnetic body is made to behave like a homogeneous diamagnetic
body, and the diamagnetic body like a homogeneous magnetic
body.　Diamagnetic bars and discs were also examined, and a
deportment precisely complementary to that of the magnetic bars
and discs was exhibited.　A magnetic disc set its thickness from
pole to pole and consequently its horizontal diameter equatorial; a
diamagnetic disc set its thickness equatorial and its horizontal
diameter from pole to pole.　Two bodies of the same exterior form
and of the same colour, were suspended simultaneously in the fields
of two electro-magnets, and both the latter were excited by the
same current; the eye could detect no difference of deportment.
Both bodies possessed the shape of calcareous spar, and both set
the crystallographic axis equatorial.　One body however was com-
posed of wax, while the other was a true crystal.　In the same
way a crystal of carbonate of iron exhibited a deportment precisely
the same as that of a model formed of magnetic dust.　The explana-
tion of these phænomena may be given in a few words.　In the con-
struction of the models, the magnetic or diamagnetic dust was formed
into a kind of dough and pressed between two glass plates; the
same process was applied to the wax; and it is a universal law,
that in diamagnetic bodies the line along which the density of the
mass has been increased by compression, sets equatorial, and in mag-
netic bodies axial.　A reference to this principle will instantly render
plain all the experiments we have described.　In those cases where
the same artificial bar set at one time axial and at another time
equatorial, the deportment depended on the circumstance whether
the line of compression was vertical or horizontal.　When vertical
its directive power was annulled, and the action was determined by
the exterior form of the body; but when horizontal, its directive
action came into play and determined the position of the mass.
The magnetic bar, for example, suspended with its line of pressure
vertical, set axial, but with its line of pressure horizontal, it set
equatorial; for the pressure was exerted at right angles to its length.
This action is so general that it is difficult to find a body so perfectly
homogeneous as not to exhibit it in some degree.　Ipecacuanha
lozenges and Carlisle biscuits were suspended in the magnetic field,
and exhibited a most striking directive action.　The materials in
both cases were diamagnetic; but owing to the pressure exerted in
their formation their largest horizontal dimensions set from pole
to pole, the line of compression being equatorial.

Let us endeavour to arrive at the precise logical import of these
experiments.　Let us suppose that before ever a crystal had been
suspended in the magnetic field, we were acquainted with the fact
that a slight change of density in any direction is accompanied by
such modifications of the magnetic force as those above described:
—that we know that flour, bran, soap, shale, magnetic dust, diamag-
netic dust, &c., all exhibited this directive action,—that it is in fact
a universal law of matter; and then let us imagine some fortunate
experimenter hanging a crystal between the poles and observing a

deportment in every respect similar. Would not the analogy of the case at once flash upon him? Would he not regard this deportment as a beautiful, but still special example of that all-pervading law with which he was previously acquainted? Would he not congratulate himself on the possibility thus opened to him of searching out the mysteries of crystalline structure, and rendering apparent to his mental eye the manner in which the molecules are aggregated together? He would never have assumed the existence of forces altogether new to account for the observed actions; much less would he have affirmed that they were wholly independent of magnetism or diamagnetism; for he would know beforehand that the modification of these forces by the peculiarities of aggregation was the exact thing calculated to produce the phænomena. But magnecrystallic action was discovered when its universality was unknown; and hence its discoverer was led to regard it as something unique. A great temptation lay in his way: years before, a magnet, now present, had twisted a ray of light, and thus suggested a connexion between light and magnetism. What wonder then if this unifying instinct, this yearning to find the mystic bond which unites these forces, this prediction of the human mind that all the forces of nature are but branches of a common root,—what wonder, I say, if it jumped its bounds and cried "I have it!" too soon? For a long time the optic axis, and it alone, was chargeable with these phænomena,—phænomena which it was now hoped there would be little difficulty in referring to their proper cause, and regarding as examples of the modification of force by the peculiarities of aggregation.

The Lecturer then pointed out the bearing of the described results upon the problem of the diurnal range of the magnetic needle. Professor Faraday had referred the matter to the modification of atmospheric magnetism by the sun's rays[*]. That an effect was produced here could not for a moment be doubted, but the precise extent of this effect was still an open question. The discovery of a decimal period by Lamont threw a great difficulty in the way of any theory which would refer the diurnal range to thermic action; and the difficulty was greatly increased by the observation of Col. Sabine, who connected Lamont's discovery with that of Schwabe regarding the solar spots. But whatever the result of future inquiries as to the direct magnetism of the sun may be, no theory which proposes to exhaust the subject can afford to omit the mediate operation of the sun by his heat; not however confining it to the atmosphere, but extending it also to the earth's solid crust. Let us look once more to our experiments. The line of greatest density is that of strongest magnetic power. The body operated upon by the magnet is itself a magnet, and it is an experimental fact, that it is a stronger magnet along the line of greater density than along any other line. If instead of increasing the density in one direction we increase it in all directions, we thereby augment the general magnetic power of the

body. Anything therefore which tends to increase density increases magnetic power; and whatever diminishes density diminishes magnetic power also. Knowing this, the conclusion is inevitable, that the local action of the sun upon the earth's crust must influence, in some degree, the resultant effect. The action here meant is wholly different from that hitherto speculated on, and which had reference to the generation of thermo-electric currents which affect the needle. The simple mechanical change of density is what is meant. It is a true cause, and no complete theory can omit taking it into account.

The Lecturer then proceeded to remark on the influence of geological changes upon the earth as a magnet, and concluded as follows:

" This evening's discourse is, in some measure, connected with this locality; and thinking thus, I am led to inquire wherein the true value of a scientific discovery consists ? Not in its immediate results alone, but in the prospect which it opens to intellectual activity, in the hopes which it excites, in the vigour which it awakens. The discovery which led to the results brought before you to night was of this character. *That* magnet was the physical birth-place of these results; and if they possess any value they are to be regarded as the returning crumbs of that bread which in 1846 was cast so liberally upon the waters. I rejoice, Ladies and Gentlemen, in the opportunity here afforded me of offering my tribute to the greatest worker of the age, and of laying some of the blossoms of that prolific tree which he planted, at the feet of the great discoverer of diamagnetism."

XLIX. *Intelligence and Miscellaneous Articles.*

PECULIARITY OBSERVED IN A LUMINOUS ARCH.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

MAY I inquire through your Journal whether an appearance similar to a discharge of shooting stars has been ever observed by others in the body of a luminous arch ? I once witnessed such a spectacle; and having never met with any notice of a similar one, am inclined to consider it to have been singular, and therefore not an unfit subject for record. If otherwise, I stand corrected.

It is now several years since I saw this phænomenon. On the 20th of February 1849, about 10 P.M., when looking from an eastern window I observed a very splendid arch in the heavens. There had been one the preceding evening about 9 o'clock. The apex of both was situated some degrees south of the zenith, the direction being, as usual, nearly at right angles to the magnetic meridian. From some rough notes I have discovered, I find that it shot up from the eastward past the southern region of the Great Bear, ascending to the Twins (then just culminated), whence it descended between the stars Bellatrix and Aldebaran, and through Orion's shield down-

wards; probably reaching the regions of Eridanus, for intervening buildings hid its termination westwards from my view. While gazing at it, I was astonished to see a portion of the eastern limb, at a height of about 45° or 50°, suddenly change its character and aspect, and, for an extent of perhaps five degrees, exhibit the spectacle of a crowd of minute meteors rushing and commingling with one another,—each individual, so far as the eye could detect from the rapidity and confusion of their motions, precisely resembling an ordinary shooting star, having an apparent nucleus and a luminous train following it. This sight lasted, it may be, nearly a minute; and I have since regretted that I did not apply a 30-inch refractor, which was standing in the room at the time, to have marked the appearance through a magnifying medium; but I had scarcely time. Such is a brief statement of the fact. I shall be glad if it should afford any interest to those who may have made meteorological phænomena the more peculiar object of their attention.

I remain, Gentlemen,

Your very obedient Servant,

W. Pringle.

Edinburgh, Feb. 14, 1853.

P.S. The portion of the luminous bow occupied by the apparition of the minute cometary bodies was strictly confined in breadth to that of the arch. There was an obvious motion of the luminous matter of the arch itself in a direction from east to west, resembling a tremulous stream. The cometary projections followed the same course, while they lasted—the space they occupied being filled up, on their disappearance, with the usual luminous appearance.—W. P.

ON THE PERIODS OF REVOLUTION OF THE SATELLITES OF JUPITER AND SATURN. BY A. QUETELET.

By comparing together the periods of revolution and the distances of the satellites of Jupiter and Saturn, several very simple relations have been discovered, which are described in the different treatises on astronomy. There are however two that have not hitherto been noticed until pointed out by M. le Baron Behr. The first relates to the eight satellites of Saturn which may be supposed to form two groups, one consisting of the four inner, the other of the four exterior satellites. With regard to the first group, there is the following passage in the fourth volume of Humboldt's 'Cosmos':—

"Between the first four satellites a remarkable relation of commensurability in the periods of revolution presents itself. The period of the third satellite, Tethys, is double that of the first, Mimus; that of the fourth, Dione, double that of the second, Enceladus. The closeness of this relation extends to $\frac{1}{800}$ of the

longer periods. This unnoticed result was communicated to me by Sir J. Herschel in a letter as long back as 1845."

With regard to the second group, the period of revolution of the seventh satellite, Hyperion, is five times that of the fifth, Rhea; and the period of the eighth, Japhet, five times that of the sixth, Titan.

These results will be best shown by the following table containing the elements of comparison :—

Satellites.	Periods of revolution.	Reduced numbers.
1. Mimus	0ʺ943	M = 0·943
2. Enceladus	1·370	E = 1·370
3. Tethys	1·888	$\frac{1}{2}$ T = 0·944
4. Dione..........	2·789	$\frac{1}{2}$ D = 1·370
5. Rhea	4·517	R = 4·517
6. Titan	15·945	T = 15·945
7. Hyperion	22·500?........	$\frac{1}{5}$ H = 4·500
8. Japhet	79·330	$\frac{1}{5}$ J = 15·866

Of the satellites of Jupiter, it is known that the period of revolution of the first is about one-half that of the second, which is itself again one-half the period of the third satellite. M. Behr also points out that the period of revolution of the fourth satellite is double that of the third, plus $\frac{4}{5}$ of the difference between the periods of the second and the first.

Satellites.	Periods of revolution.	Reduced numbers.
1. .... a =	1ʺ769	$2a$ = 3·538
2. .... b =	3·551	b = 3·551
3. .... c =	7·155	$\frac{1}{2} c$ = 3·577
4. .... d =	16·699 ....$2c + \frac{4}{5}(b - a)$	= 16·686

Proc. Acad. Roy. de Belgique.

ON THE DISTRIBUTION OF GOLD.

My dear Francis,

As considerable interest is felt at the present time concerning the diffusion of gold over the earth's surface, it may be interesting to the readers of the Philosophical Magazine to be informed of the results of an investigation of that subject, which is now being carried on at the Government School of Mines. So far as that investigation has extended, I may state that a sensible and visible amount of gold has been extracted from every variety of British and foreign lead, as well as every specimen of litharge, minium, white lead, and acetate of lead, which have been examined. It has also been extracted in very sensible proportion from commercial bismuth. Between thirty and forty determinations have already been made.

The details of every determination will be shortly published.

I have great pleasure in acknowledging the services of my assistant Mr. R. Smith, to whom the investigation has been entrusted.

The examination is now being extended to a great variety of native minerals. I remain, my dear Francis,

Yours very truly, JOHN PERCY.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1853.

Chiswick.—February 1, 2. Very dense fog. 3. Overcast: cloudy. 4. Cloudy: rain. 5. Densely overcast. 6. Cloudy. 7. Fine: overcast. 8. Uniformly overcast. 9. Foggy: dusky haze: foggy: overcast: rain. 10. Cold rain: drizzly: clear. 11. Few snow-flakes. 12. Cloudy. 13. Overcast throughout: frosty. 14. Overcast: sharp frost. 15. Frosty: overcast: frosty. 16. Frosty: fine: clear. 17. Clear: slight snow. 18. Frosty, with brisk wind: snow-shower occasionally. 19. Dry and frosty: clear: frosty. 20. Frosty: large white clouds: clear and frosty. 21. Clear and frosty: fine: clear, with sharp frost at night. 22. Frosty: rain: overcast. 23. Drizzly: fine: clear and boisterous at night. 24. Clear and frosty: overcast: rain at night. 25. Cloudy: overcast: rain at night. 26. Boisterous, with rain. 27. Overcast: clear and frosty. 28. Clear and frosty: cold dry air: clear and frosty at night.

Mean temperature of the month .... 32°·53
Mean temperature of Feb. 1852 .... 38 ·72
Mean temperature of Feb. for the last twenty-seven years... 40 ·06
Average amount of rain in Feb. .... 1·62 inch.

Boston.—Feb. 1. Foggy. 2, 3. Cloudy. 4. Cloudy: rain A.M. and P.M. 5, 6. Cloudy. 7. Cloudy: rain P.M. 8. Cloudy. 9. Cloudy: rain P.M. 10. Cloudy: rain and snow A.M. and P.M. 11. Cloudy: snow A.M. and P.M. 12. Fine: snow A.M. and P.M. 13. Fine: snow A.M. 14. Cloudy: snow A.M. 15, 16. Fine. 17. Cloudy: snow A.M. 18. Fine: snow A.M. 19. Fine. 20. Cloudy: snow A.M. 21. Fine. 22. Cloudy: snow A.M. and rain P.M. 23. Fine: snow P.M. 24, 25. Fine. 26, 27. Cloudy: snow P.M. 28. Cloudy.

Sandwick Manse, Orkney.—Feb. 1. Bright A.M.: fine, cloudy, aurora P.M. 2. Hazy A.M.: cloudy P.M. 3. Rain A.M. and P.M. 4. Rain A.M.: clear, aurora P.M. 5. Clear, frost A.M.: cloudy P.M. 6. Cloudy A.M.: showers, aurora P.M. 7. Bright A.M.: clear, frost P.M. 8. Clear, frost A.M. and P.M. 9. Rain A.M.: cloudy P.M. 10. Bright, frost A.M.: snow-showers P.M. 11. Snow-showers A.M.: snow-drift P.M. 12. Snow-showers A.M.: cloudy P.M. 13. Snow A.M.: clear P.M. 14. Snow, clear A.M.: clear, aurora P.M. 15. Sleet-showers A.M.: cloudy P.M. 16. Snow-showers A.M.: clear P.M. 17. Snow-drift A.M. and P.M. 18. Deep snow A.M.: snow-showers P.M. 19. Snow-showers A.M.: snow-drift P.M. 20. Snow-showers A.M. and P.M. 21. Cloudy A.M.: thaw, showers P.M. 22. Frost A.M.: cloudy, thaw P.M. 23. Snow-drift A.M.: snow-showers P.M. 24. Snow, cloudy A.M.: snow-drift P.M. 25. Snow-showers A.M.: snow, cloudy P.M. 26. Snow-showers A.M. and P.M. 27. Clear A.M.: fine, aurora P.M. 28. Snow, cloudy A.M.: clear, aurora P.M.

Mean temperature of Feb. for twenty-six previous years ...... 38°·44
Mean temperature of this month .... 33 ·74
Average quantity of rain in Feb. for seven previous years...... 4·23 inches.
All marked as *rain* after 11th is melted snow, viz. 1·77 inch.

The snow-storm began on the night of the 10th and continued till the end of February, being the most severe remembered for twenty-six years at least, except in 1838, when the snow continued all the month and all the previous January, except the first eight days. It has also been one of the coldest months during that time, the only one decidedly colder being February 1838, when the mean temperature was 31°·31. Some instances occurred of the snow being rolled up in hollow fluted cylinders, as formerly observed here and described in February 1847 *, but on a much smaller scale.

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* See our Number for April 1847.

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. 1853. Feb.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick. Max.	Min.	Boston. 8½ a.m.	Orkney, Sandwick. 9¼ a.m.	8¼ p.m.	Chiswick. Max.	Min.	Boston. 8½ a.m.	Orkney, Sandwick. 9¼ a.m.	8¼ p.m.	Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
☽ 1.	30·192	30·160	29·88	29·92	29·97	32	28	29	41½	41½	n.	calm	sse.	...	...	...
2.	30·120	29·962	29·82	29·88	29·67	42	28	33·5	41	41	ne.	se.	sse.	...	...	...
3.	29·715	29·535	29·40	29·61	29·67	39	34	34	40	38½	se.	sse.	sse.	...	...	·28
4.	29·788	29·521	29·30	29·90	30·04	41	31	36	40	32	nw.	se.	calm	...	·04	·32
5.	29·878	29·710	29·50	30·02	29·78	42	32	36	34	37	se.	ne.	s.	·17	...	...
6.	29·848	29·685	29·53	29·42	29·40	43	34	36	34	37	ne.	wnw.	w.	...	·20	·06
7.	29·521	29·348	29·20	29·32	29·28	45	26	34·5	39	31	s.	s.	sse.	·01	...	·02
● 8.	29·230	29·052	29·00	29·25	29·33	40	34	34·5	34	30½	se.	sse.	e.	...	·20	...
9.	29·015	28·978	29·08	29·44	29·53	40	34	36	32	36½	e.	se.	n.	...	...	·04
10.	29·324	29·081	28·90	29·66	29·73	37	28	36	37	28	ne.	ne.	ne.	·02	...	·01
11.	29·368	29·362	29·13	29·86	29·92	36	27	30·5	35	25	n.	ne.	ne.	·06	·16	...
12.	29·494	29·388	29·20	29·68	29·94	34	27	30	26	28	ne.	ne.	nne.	...	·18	·03
13.	29·632	29·584	29·38	29·86	30·00	34	26	28	22	31	e.	n.	ne.	...	...	...
14.	29·818	29·798	29·00	30·00	29·80	32	18	25	33	33	ne.	nw.	nne.	...	·02	·06
15.	29·818	29·796	29·50	29·94	30·10	35	25	27	34½	35	nw.	nnw.	n.	...	·10	·07
☾ 16.	29·820	29·784	29·52	30·02	30·05	38	25	27	36	33	n.	nnw.	n.	...	·07	·07
17.	29·746	29·531	29·46	29·88	29·79	35	25	16·5	32	33	nw.	n.	n.	...	...	...
18.	29·530	29·494	29·24	29·65	29·56	36	16	19	31	28	n.	n.	calm	...	...	...
19.	29·710	29·550	29·38	29·82	29·92	34	19	26·5	34	26	ne.	n.	n.	...	...	...
20.	29·802	29·444	29·60	30·07	30·15	36	23	33	35	30	n.	nnw.	nw.	...	...	...
21.	30·094	30·050	29·77	30·06	29·82	38	19	34	34½	37½	nw.	w.	nw.	...	...	·46
22.	30·010	29·870	29·63	29·93	29·77	38	30	30·5	34½	36	sw.	w.	n.	·08	·12	·20
○ 23.	29·629	29·540	29·30	29·48	29·67	43	25	34	36½	34	w.	nw.	nw.	·02	·15	·05
24.	29·754	29·305	29·45	29·55	29·55	40	32	34	30	32	nw.	nw.	nw.	·09	...	·14
25.	29·732	29·456	29·26	29·67	29·34	40	29	33·5	37	32½	n.	sw.	se.	·15	·07	·18
26.	29·182	29·086	29·05	29·44	29·48	44	30	34	34	32	w.	sw.	n.	·09	·09	·06
27.	29·752	29·441	28·74	29·70	29·85	39	22	34	32½	29	n.	n.	n.	...	·12	·20
28.	29·900	29·809	29·27	29·70	29·64	40	24	32	39	34	nw.	n.	n.	...	·03	·25
Mean.	29·693	29·547	29·33	29·742	29·745	38·32	26·75	30·8	34·60	32·89				2·46	1·61	2·50

L. *On the Relations between the Atomic Weights of analogous Elements.* By J. H. GLADSTONE, *Ph.D.**

CHEMISTS who have turned their attention to the series of numbers representing the atomic weights of the elementary bodies, have frequently remarked curious relations between them. It is between similar elements that these numerical relations occur ; and to such an extent is this the case, that Berzelius, after mentioning numerous instances, says, "We see that bodies which present the same properties up to a certain point have certain relations between their atomic weights†."

To illustrate this statement, to show the extent of its truth, and to draw certain analogical inferences tending to the proper understanding of such a fact, are the objects of this communication.

The following is a list of the atomic weights arranged from the lowest to the highest, and thus without any reference to chemical relationship. The numbers adopted are those given in the last volume of Liebig's *Jahresbericht.*

1	Hydrogen.	14	Nitrogen.
4·7	Glucinum.	16	Sulphur.
6	Carbon.	19	Fluorine.
6·5	Lithium.	20	Calcium.
8	Oxygen.	21·3	Silicon.
10·9	Boron.	22·4	Zirconium.
12	Magnesium.	23	Sodium.
13·7	Aluminium.	25	Titanium.

* Communicated by the Author.
† *Traité de Chimie*, vol. iv.

26·7	Chromium.		58	Tin.
27·6	Manganese.		59·6	Thorinum.
28	Iron.		60	Uranium.
29·5	Cobalt.		64·2	Tellurium.
29·6	Nickel.		68·5	Barium.
31	Phosphorus.		68·6	Vanadium.
31·7	Copper.		75	Arsenic.
32·6	Zinc.		80	Bromine.
35·5	Chlorine.		92	Tungsten.
39·2	Potassium.		98·7	Platinum.
39·5	Selenium.		99	Iridium.
43·8	Strontium.		99·6	Osmium.
46	Molybdenum.		100	Mercury.
47	Cerium.		103·7	Lead.
47	Lanthanium.		108·1	Silver.
50	Didymium.		127·1	Iodine.
52·2	Rhodium.		129	Antimony.
52·2	Ruthenium.		184	Tantalum.
53·3	Palladium.		197	Gold.
56	Cadmium.		208	Bismuth.

If we glance at this list we notice some peculiarities, but no very striking ones. We might ask, for instance, Why should there be so many elements congregated about No. 28; and, again, about 52 ? Why should there be only one atomic weight between 80 and 99, and then a group of four ?

The following letter, kindly sent me by Professor De Morgan, will give the data for calculating the probabilities of this. I introduce it on account of its applicability, not only to this particular case, but to others which will occur in these observations.

<div align="right">" Univ. Coll., Lond., Dec. 18, 1852.</div>

" DEAR SIR,—The following, though but an imperfect view of the whole question, will be enough, I think, for your purpose. I send formula and all, that who likes may verify it.

" If there be n numbers, each of which may be drawn at any trial, and all equally likely, and if the following denominations be used,

$$P = \left(1 - \frac{1}{n}\right)^m$$

$$Q = m \cdot \left(1 - \frac{1}{n}\right)^{m-1} \cdot \frac{1}{n}$$

$$R = m\frac{m-1}{2}\left(1 - \frac{1}{n}\right)^{m-2}\frac{1}{n^2}$$

$$S = m\frac{m-1}{2}\frac{m-2}{3}\left(1 - \frac{1}{n}\right)^{m-3}\frac{1}{n^3}, \&c.$$

Then, speaking of one assigned number, the chance that that number shall not appear in m trials, is P; that it shall appear once and once only, is Q; twice and twice only, is R; and so on. Further, the chance that it shall appear once or more is $1 - P$. That it shall appear twice or more, the chance is $1 - (P + Q)$. Three times or more, $1 - (P + Q + R)$; and so on.

"For calculation,

$$Q = \frac{mP}{n-1}$$

$$R = \frac{(m-1)Q}{2(n-1)}$$

$$S = \frac{(m-2)R}{3(n-1)};$$

and so on.

"Let there be 100 numbers, and 60 trials to be made. I find

$$P = \cdot 54716$$
$$Q = \cdot 33161$$
$$R = \cdot 09881$$
$$S = \cdot 01929$$
$$T = \cdot 00278$$
$$U = \cdot 00031$$
$$\overline{P + Q + R + S + T + U} = \cdot 99996$$
$$1 \cdot 00000$$

$\cdot 00004$. Chance of six or more of a given number.

"It is then 99996 to 4, or 24999 to 1, against the appearance of a predicted number six or more times.

"Now suppose the question to be what is the chance that *some one* number, not named, shall occur six or more times; that is, either the one named in the last case, or some other? This is a much more complicated question, but it is certain that 100 times the chance in the last is too great. Now $\cdot 00004 \times 100 = \cdot 004$, which is too much decidedly. Consequently 996 to 4, or 249 to 1, are too small odds to lay against the appearance of some one number six or more trials.

"That is, you may lay more than 250 to 1 that of all the numbers, *no one* will occur six or more times in 60 trials.

"I am, dear Sir,

"Yours faithfully,

"*Dr. Gladstone.*"   "A. De Morgan."

Reverting to the list of elements given above, we certainly

find no recurrence of certain numbers, or other peculiarity, sufficiently striking to warrant us in drawing any inference; but let us arrange the elements according to their chemical relations, and the case will be entirely altered. Any arrangement of the elements is attended with difficulty: I shall not attempt to form one of my own, as it would be open to the objection that my mind had been biassed by dwelling upon the numerical relations; but I shall adopt that given in Gmelin's Handbook of Chemistry at the commencement of vol. ii.

O	N	H
F Cl Br I		L Na K
S Se Te		Mg Ca Sr Ba
P As Sb		G Er Y Tr Ce Di La
C B Si		Zr Th Al
Ti Ta Nb Pe W		Sn Cd Zn
Mo V Cr	U	Mn Co Ni Fe
Bi	Pb Ag	Hg Cu
Os Ru	Ir R	Pt Pd Au

Even here many elements are grouped together which have but a faint chemical resemblance. Thus tin has little in common with cadmium and zinc, or mercury with copper; fluorine is very different from the other halogens; magnesium can scarcely rank with the metals of the alkaline earths; whilst late researches have shown the strict isomorphism of chromium with manganese or iron.

If we substitute the equivalent numbers in this arrangement, the slightest glance will make us acquainted with many remarkable resemblances. Decimals are omitted for the sake of brevity.

8	14	1
19, 35, 80, 127		6, 23, 39
16, 39, 64		12, 20, 44, 68
31, 75, 129		5, —, —, —, 47, 50, 47
6, 11, 21		22, 60, 14
25, 184, —, —, 92		58, 56, 33
46, 69, 27	60,	28, 29, 30, 28
208, 104,	108,	100, 32
100, 52, 99,	52,	99, 53, 197.

Looking more closely into this arrangement of numbers, we shall find the observation of Berzelius borne out in every instance, but one, of a well-defined chemical group.

These numerical relations are of three kinds. The atomic weights of analogous elements may be the same; or may be in multiple proportion; or may differ by certain increments.

Of the first class we remark the strictly analogous metals,—chromium 26·7, manganese 27·6, iron 28, cobalt 29·5, and nickel 29·6. Then a double group of the platinum ore metals:—palladium 53·3, rhodium, 52·2, and ruthenium 52·2; and also platinum 98·7, iridium 99, and osmium 99·6. We are tempted to add to this—mercury 100. Again, in the mineral cerite we find together—cerium 47, lanthanium 47, and didymium 50. It has been remarked, not only that the metals of each of these groups have similar properties and weights, but that they are found associated together in nature. The question has often been put,—Would more accurate determinations show these atomic weights to be not nearly but exactly the same? It may be doubted. Yet it ought to be remembered that these numbers are the actual results of experiment, and are not controled by any theory, as is always the case with organic compounds.

As to the second class of numerical relations among atomic weights, namely, multiple proportions, Who has failed to remark that the platinum group has double the atomic weight of the palladium group, and that gold 197 is again the double of platinum? These two pairs have frequently been noticed—boron 10·9, and silicon 21·3; oxygen 8, and sulphur 16. We now come to a large group, those metals whose oxides principally affect an acid character, being also insoluble in water. The highest of these in weight is tantalum 184; half 184 is 92—the equivalent of tungsten; half 92 is 46—the equivalent of molybdenum; and half 46 is 23—just below the recognized equivalent of titanium 25*. Three times 23 is 69: now 68·6 is the equivalent of vanadium, being intermediate between tungsten and molybdenum. Tin has certain claims to be grouped along with the same elements; its equivalent is 58: now 57·5 would be two and a half times 23, and intermediate between molybdenum and vanadium. Taking 11·5 as the basis number of this series, we have—

			Received equiv.
Titanium	.	. $2 \times 11·5 = 23$	25
Molybdenum	.	$4 \times 11·5 = 46$	46
Tin .	.	. $5 \times 11·5 = 57·5$	58
Vanadium	.	. $6 \times 11·5 = 69$	68·6
Tungsten	.	. $8 \times 11·5 = 92$	92
Tantalum	.	. $16 \times 11·5 = 184$	184

* 23·6 according to Mosander.

Silicon is certainly very similar to the first of this group, titanium, but its atomic number 14·2 (reckoning silica to be SiO^2) is unconformable.

These metals, whose numbers have a multiple relationship, are not remarkable for being found together in nature.

The third kind of relationship is where an element, having properties intermediate between those of two other elements, has the intermediate atomic weight. Four instances of this have been noticed[*]. We observe all four groups in the arrangement of Gmelin given above. They are—

The metals of the alkalies :—

Lithium, 6·5.   Sodium, 23.   Potassium, 39·2.

The metals of the alkaline earths :—

Calcium, 20.   Strontium, 43·8.   Barium, 68·5.

The halogens :—

Chlorine, 35·5.   Bromine, 80.   Iodine, 127·1.

Sulphur and its congeners :—

Sulphur, 16.   Selenium, 39·5.   Tellurium, 64·2.

The members of the last two groups generally occur together in nature.

There are certain analogies which may perhaps lead us to some understanding of these facts.

First, in the case where there is the same atomic weight. If the allotropism of an element were carried through all its compounds, we should have what occurs in the iron and similar series. The only partial instances of this which I remember are the sulphides of phosphorus, as remarked by Berzelius, and silicic acid, the two conditions of which bear a striking analogy to the two allotropic forms of the elementary silicon itself.

Secondly, in the case of multiple atomic weights, is there not something analogous to the polymerizing of which we have many examples in organic chemistry? or to the modifications of a metal, such as mercury, where we have 100 or 200 parts combining with 1 equivalent of chlorine to form a salt? the difference in this latter case being, that when we regenerate the metal, from whatever source, it is always the same mercury.

Thirdly, in the case where an element of intermediate properties has an intermediate atomic weight. I regard this as strictly analogous to the series of homologous bodies so common in organic chemistry. My meaning may be best explained perhaps by a reference to the quasi-metals, or compound hydrogens. We have hydrogen 1, methyle 15, æthyle 29, amyle 71 : the com

* See Gmelin's Handbook of Chemistry, part 1. Dumas also brought forward some speculations on these groups at the Ipswich Meeting of the British Association.

pounds of these bodies differ progressively in properties—the boiling-point for instance—and they occur together in the processes of their preparation. Methyle is intermediate in chemical characters between hydrogen and æthyle, and has the intermediate atomic weight $\frac{1+29}{2} = 15$. If we did not know in what respect the one quasi-metal differed from another, we should have a series of bodies precisely analogous to the metals of the alkalies: but we do know it; we know that methyle is hydrogen plus a certain increment $C^2 H^2$; æthyle is hydrogen plus twice $C^2 H^2$, &c. The general expression for any such homologous series is, taking x as the increment,—

$$a; \quad a+x; \quad a+2x; \quad a+3x, \&c.$$

This will equally apply to any other cases of the addition of increments—the conjugate organic acids, such as formo-benzoic acid, or the series phosphoric acid PO^5, 71, azophosphoric acid $P^2 NO^5$, 116, and deutazophosphoric acid $P^3 N^2 O^5$, 161. Now it is precisely in like manner that I regard these series of elements: I view sodium as lithium plus a certain increment; in fact, $Na = L + x$, and $K = L + 2x$. Lithium is here the starting-point, the "hydrogen" of the series; and so in like manner are calcium, chlorine, and sulphur. We do not know what the increments are, but we know their atomic weights.

In the lithium series it is   16·3
    ...   calcium   ...   24·2
    ...   chlorine   ...   45·8
    ...   sulphur   ...   24·1

It is remarkable that the increments of the calcium and sulphur series are the same in weight—24, and that the increment of the lithium series should be almost exactly two-thirds of that number.

Why should this numerical relation always give us triads? As yet we have no instance of a fourth member of one of these series, unless indeed we view the titanium series in this light; but the advance of science may furnish us with such instances if my theory of increments be correct. Whether any element not contained in these groups have the atomic weight of some other plus a certain increment, cannot be known until we have a third member of the series to prove the fact. Thus we can only speculate upon the curious circumstance, that zinc and cadmium—two similar metals occurring together in nature—have atomic weights differing almost by the remarkable number 24. Zn $32·6 + 23·4 = 56$ at. wt. of Cd.

I believe that, with one exception, every well-defined group of elements has been considered under these three classes. That exception is in the case of arsenic and antimony, which are unquestionably analogous, phosphorus being also closely allied to them : the three are placed together in Gmelin's arrangement, but I see no ratio between their numbers 31, 75, 129. Schrœtter's reduction of the equivalent of phosphorus has prevented our considering antimony, 129, as four times that element. It is, however, double tellurium, 64·2, which is in some respects an analogous body.

There are several elements, such as bismuth, which have no very evident analogues; and of others which are similar we cannot speak, because we are ignorant of their atomic weights. These are pelopium and niobium, and yttrium erbium and terbium.

Alumina is usually classed with the earths; but its compounds are strictly isomorphous with those of the sesquioxides of the iron group. The equivalent of aluminium, 13·7, happens to be half of theirs. Again, glucina is certainly an analogous earth; viewing it also as a sesquioxide, the equivalent of glucinum is 7, which is half that of aluminium.

Some of the properties of lead would ally it to the family of the alkaline earths, but there is no apparent numerical relation : in other points it resembles silver, with which it is so generally found; their respective equivalents are 103·7 and 108·1, but neither the resemblance of chemical properties nor of number is very close. I would rather not consider it an instance of the general law.

Whatever may be thought of some of the speculations towards the close of this paper, these numerical relations are indisputable facts. That we should frequently find relations between 56 numbers drawn at hazard from a range little exceeding 100 might be predicted from the laws of probability, but that this should be to a considerable extent coincident with chemical relationship is not probable. Still more is it against all probability that, by mere chance, whenever, with one exception, close analogy of properties exists, there exists also numerical relationship; and although we cannot now see the precise reason of this, we can scarcely imagine that the intimate constitution of these related elementary bodies will long remain an unfruitful field of investigation.

LI. *On Continued Fractions in Quaternions.* By Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S. &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

7. THE geometrical theorems stated in recent articles of this paper, although perhaps not inelegant, cannot pretend to be important : indeed a hint has been given (in a note) of a quite elementary way, in which they may be geometrically demonstrated. But I think that the analytical process, by which I was led to the formulæ of art. 5, whereof the geometrical statements of art. 6 are in part an interpretation, may deserve to be considered with attention, on account of the novelty of the *method* employed : and especially for the examples which it supplies of calculation with *biquaternions*.

8. After obtaining the result already published in this Magazine (compare the number for May, 1852), for a certain continued fraction in quaternions, namely that if

$$u_x = \left(\frac{b}{a+}\right)^x c, \quad \ldots \ldots \ldots \quad (1)*$$

then †

$$\frac{u_x - u''}{u_x - u'} = u''^x \left(\frac{c - u''}{c - u'}\right) u'^{-x}, \quad \ldots \ldots \quad (2)$$

u', u'' being roots of the quadratic equation

$$u^2 + ua = b ; \quad \ldots \ldots \ldots \ldots \quad (3)$$

and after hence deducing the theorem (given in page 303 of the Philosophical Magazine for October, 1852), that for the case of *real quaternions,* and of *unequal tensors,*

$$u_\infty = u'', \text{ if } Tu'' < Tu' ; \quad \ldots \ldots \ldots \quad (4)$$

it was obvious, as a particular application, that by changing a, b, c, u', u'', u_x to $\alpha, \beta, \rho_0, \rho', \rho'', \rho_x$, and by supposing these last to be six *real vectors,* among which β is perpendicular to all the rest, I might write

$$\frac{\rho_x - \rho''}{\rho_x - \rho'} = \rho''^x \frac{\rho_0 - \rho''}{\rho_0 - \rho'} \rho'^{-x} ; \quad \ldots \ldots \ldots \quad (5)$$

and ultimately,

$$\rho_\infty = \rho'', \text{ if } T\rho'' < T\rho', \quad \ldots \ldots \ldots \quad (6)$$

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* The numbering of the equations commences here anew.

† This result holds good also in ordinary algebra, and even in *arithmetic* : but in applying it to quaternions, the *order of the factors* must be attended to.

the vectors ρ', ρ'' being roots of the quadratic,

$$\rho^2 + \rho\alpha = \beta. \quad \ldots \quad \ldots \quad \ldots \quad (7)$$

This last equation gave, by taking separately the scalar and vector parts,

$$\rho^2 + S \cdot \rho\alpha = 0 ; \quad \ldots \quad \ldots \quad \ldots \quad (8)$$

$$V \cdot \rho\alpha = \beta ; \quad \ldots \quad \ldots \quad \ldots \quad (9)$$

whereof the former (8) expressed that ρ terminated on a *spheric surface,* passing through the origin, and having the vector $-\alpha$ for its diameter; while the latter (9) expressed that ρ terminated on a *right line,* which was drawn through the extremity of the vector $\beta\alpha^{-1}$, in a direction parallel to that diameter. Thus (9) gave, by the rules of the present calculus,

$$\rho = \beta\alpha^{-1} + x\alpha, \quad \rho^2 = -\beta^2\alpha^{-2} + x^2\alpha^2, \quad S \cdot \rho\alpha = x\alpha^2 ; \quad (10)$$

and therefore, by (8), I had the ordinary quadratic equation,

$$x^2 + x = \beta^2\alpha^{-4}, \text{ or } (2x+1)^2\alpha^4 = \alpha^4 + 4\beta^2 > 0, \quad (11)$$

as in art. 4 (Phil. Mag. for February, 1853): the two values of the vector ρ, which answer to the two values of the scalar coefficient x, being here supposed to be geometrically real and unequal; or the right line (9) being supposed to meet the spheric surface (8), in two distinct and real points, A, B. Hence by assuming

$$\rho' = \text{CA}, \quad \rho'' = \text{CB}, \quad \rho_0 = \text{CP}, \rho_1 = \text{CQ}, \quad \alpha = \text{DC}, \beta = \text{CA} \cdot \text{DA}, \quad (12)$$

I was conducted with the greatest ease to the theorems of the last-cited article.

9. But in the case of art. 5, namely when

$$\alpha^4 + 4\beta^2 = 0, \quad \ldots \quad \ldots \quad \ldots \quad (13)$$

and when consequently

$$x = -\frac{1}{2}, \quad \rho'' = \rho' = \beta\alpha^{-1} - \frac{1}{2}\alpha, \quad (14)$$

the *equality* of the *two roots* of the quadratic (11) in x, or of the two real and vector roots of the equation (7) in ρ, appeared to reduce the formula (5) to an *identity*: and the simple process of the article last cited did not immediately occur to me. I therefore had recourse to certain *imaginary* or purely *symbolical solutions,* of that quadratic equation (7), or rather of the following, by which we may here conveniently replace it,

$$u^2 + u(i-k) = j ; \quad \ldots \quad \ldots \quad \ldots \quad (15)$$

the continued fraction to be studied being now,

$$u_* = \left(\frac{j}{i-k+}\right)^x u_0, \quad \ldots \quad \ldots \quad (16)$$

where ijk are the usual symbols of this calculus, and u_0 may denote any arbitrarily assumed quaternion. By an application

of a general process (described in art. 649 of my unpublished Lectures on Quaternions), I found that the quadratic (15) might be symbolically satisfied by the two following *imaginary quaternions*, or *biquaternion expressions* :

$$u' = -i - h(1-j)\,; \quad u'' = -i + h(1-j)\,; \quad . \quad . \quad (17)$$

where h is used as a temporary and abridged symbol for the *old and ordinary imaginary* of common algebra, denoted usually by $\sqrt{-1}$, and regarded as being always a *free* or *commutative factor* in any multiplication : so that

$$h^2 = -1, \quad hi = ih, \quad hj = jh, \quad hk = kh, \quad . \quad . \quad (18)$$

although $ji = -ij$, &c. In fact the first of these expressions (17) gives,

$$\begin{aligned}
u'(u'+i-k) &= \{i + h(1-j)\}\{k + h(1-j)\} \\
&= ik + h\{i(1-j) + (1-j)k\} + h^2(1-j)^2 \\
&= -j + h(i-k+k-i) + h^2(1-2j-1) \\
&= -j + 0h + 2j = j\,; \quad . \quad . \quad . \quad . \quad . \quad (19)
\end{aligned}$$

and the second expression (17) gives, in like manner,

$$u''(u''+i-k) = j\,: \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

so that, without entering at present into any account of the process which enabled me to *find* the biquaternions (17), it has been now *proved, à posteriori,* by actual substitution, that those expressions do in fact *symbolically satisfy* the quadratic equation (15). And because they are *unequal roots* of that equation, as differing by the sign of h, I saw that they might be employed in the general formula (2), without being liable to the practical objection that lay against the employment of the two *real* but *equal* roots, ρ', ρ'', of the equation (7).

10. Introducing therefore into the formula (2), or into the following, which is a transformation thereof,

$$\frac{u_x - u''}{u_x - u'} = \frac{u''^x(u_0 - u'')}{u'^x(u_0 - u')}, \quad . \quad . \quad . \quad . \quad (21)$$

the values (17), or these which are equivalent,

$$u' = -h(1-j-hi), \quad u'' = h(1-j+hi)\,; \quad . \quad . \quad (22)$$

and observing that

$$(j \pm hi)^2 = j^2 \pm h(ji+ij) - i^2 = 0, \quad . \quad . \quad . \quad (23)$$

and that therefore*

$$(1 - j \mp hi)^x = 1 - xj \mp xhi\,; \quad . \quad . \quad . \quad . \quad (24)$$

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* More generally, with these rules of combination of the symbols $hijk$, if f be any algebraic function, and f' the derived function,

$$f(1 + tj \pm thi) = f(1) + tf'(1)(j \pm hi)\,;$$

because $(tj \pm thi)^2 = 0$, if t be any scalar coefficient.

we see that

$$\frac{u_x+i-h(1-j)}{u_x+i+h(1-j)}=(-1)^x\frac{\text{A}_x-h\,\text{B}_x}{\text{A}_x+h\,\text{B}_x}, \quad . \quad . \quad (25)$$

where A_x, B_x are *two real quaternions,* namely,

$$\left.\begin{array}{c}\text{A}_x=(1-xj)(u_0+i)+xi(1-j),\\ \text{B}_x=(1-xj)(1-j)-xi(u_0+i)\,;\end{array}\right\} \quad . \quad (26)$$

or, as we may also write them,

$$\left.\begin{array}{c}\text{A}_x=(1-xj)(u_0-k)+i+k,\\ \text{B}_x=-xi(u_0-k)+1-j.\end{array}\right\} \quad . \quad (27)$$

In this manner I found it possible to *eliminate the symbol h,* or to return from imaginary to real quaternions; and so perceived that

$$\frac{u_{2n}+i}{1-j}=\frac{\text{A}_{2n}}{\text{B}_{2n}}; \quad \frac{u_{2n+1}+i}{1-j}=-\frac{\text{B}_{2n+1}}{\text{A}_{2n+1}}. \quad (28)$$

Both of these two last formulæ agree in giving, as a limit,

$$\frac{u_\infty+i}{1-j}=\frac{j}{i}=\frac{-i}{j}=+k\,; \quad . \quad . \quad (29)$$

and therefore (as in art. 5),

$$\left(\frac{j}{i-k+}\right)^\infty u_0=u_\infty=-i+k(1-j)=k, \quad (30)$$

whatever *real* quaternion may be assumed for u_0. This last restriction becomes *here* necessary, from the generality of the analysis employed: because, for the very reason that u', u'' are admitted as being at least *symbolical* (or imaginary) *roots* of the equation (15), therefore we must here say that

$$\text{if } u_0=u', \text{ then } u_x=u', u_\infty=u'\,; \quad . \quad . \quad (31)$$

and in like manner,

$$\text{if } u_0=u'', \text{ then } u_x=u'', u_\infty=u''. \quad . \quad . \quad (32)$$

11. By the first of the two real quaternion equations (28), we have,

$$u_{2n}-k=-i-k+\text{A}_{2n}\,\text{B}_{2n}^{-1}(1-j)\,; \quad . \quad . \quad (33)$$

but also, by the latter of the two values (27),

$$\text{B}_{2n}^{-1}(1-j)=\{(1-j)^{-1}\text{B}_{2n}\}^{-1}=\left(\frac{1+j}{2}\cdot\text{B}_{2n}\right)^{-1}$$

$$=\{1+n(k-i)(u_0-k)\}^{-1}\,; \quad . \quad . \quad (34)$$

again, by the former of the same two values (27),

$$\text{A}_{2n}-(k+i)\{1+n(k-i)(u_0-k)\}$$

$$=\text{A}_{2n}-(k+i)+2nj(u_0-k)=u_0-k\,; \quad (35)$$

therefore

$$u_{2n} - k = (u_0 - k)\{1 + n(k - i)(u_0 - k)\}^{-1}$$
$$= \{(u_0 - k)^{-1} + n(k - i)\}^{-1}; \quad . \quad . \quad (36)$$

or more simply,

$$(u_{2n} - k)^{-1} - (u_0 - k)^{-1} = n(k - i). \quad . \quad . \quad (37)$$

It was in this way that I was originally led to the formula of art. 5, namely,

$$(u_{2n+x} - k)^{-1} - (u_x - k)^{-1} = n(k - i); \quad . \quad . \quad (38)$$

but having once come to *see* that this result held good, it was easy then to pass to a much more simple *proof*, such as that given in the last-cited article, which was entirely independent of the imaginary symbol here called *h*, and employed only *real quaternions.*

12. It may be regarded as still more remarkable, that the *same real results* are obtained, when we *combine a real root with an imaginary one*, instead of combining *two* real roots or *two* imaginary ones. Thus the quadratic* equation (15) has *one* root, namely *k*, which must be considered as *real* in this theory, whether by contrast to the symbol *h* (or to the old imaginary of algebra), or because in the geometrical interpretation it is constructed by a *real line*, namely by the chord CE drawn to the point of *contact* E of the spheric surface (8) with the right line (9), under the condition (13); α and β being then for convenience replaced, as in art. 5, by the more special symbols $i - k$ and j. Now if we adopt this *real root k* as the value of *u'*, but retain the second of the two imaginary or biquaternion roots (17), as being still the expression for *u''*, the numerators of the formula (21) will remain unchanged, but the denominators will be altered; and instead of (25) we shall have this other formula,

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* An equation of the *n*th dimension in quaternions has generally n^4 *roots*, real or imaginary; because it may be generally resolved into a system of *four* ordinary and algebraical equations, which are *each* of the *n*th degree. However, it is shown in my Lectures that for the particular form (3), $u^2 + ua = b$ (or $q^2 = qa + b$), which occurs in the present investigation, *only six* (out of the sixteen) roots are *finite*; and that of these six, *two* are generally *real*, and *four imaginary*. In the particular case of the equation (15), *k* is by this theory a *quadruple root*, representing at once two real and two imaginary solutions, which have all become *equal* to each other, by the vanishing of certain radicals. Thus there remain in this case *only three distinct roots* of the quadratic (15), namely the one real root *k*, and the two imaginary roots (17): and what appears to me remarkable in the analysis of the present article 12, although otherwise exemplified in my Lectures, is the *mixture of these two classes* of solution of an equation in quaternions, a root of one kind being *combined* with a root of the other kind, so as to conduct to a correct determination of the value of a certain continued fraction, regarded as a real quaternion, which admits (as in art. 6) of being geometrically interpreted.

$$\frac{u_x + i - h(1-j)}{u_x - k} = \frac{h^x(\mathbf{A}_x - h\mathbf{B}_x)}{k^x(u_0 - k)}, \quad . \quad . \quad . \quad (39)$$

with the significations (26) or (27) of $\mathbf{A}_x$, $\mathbf{B}_x$, and therefore with the relations (34) (35). Observing that

$$h^{2n} = (-1)^n = k^{2n}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

we find that the formula (39), by comparing separately the real and imaginary parts, in the two cases of x even and x odd, gives these four others, not involving the symbol h:

$$\frac{u_{2n} + i}{u_{2n} - k} = \frac{\mathbf{A}_{2n}}{u_0 - k}; \quad \frac{1-j}{u_{2n} - k} = \frac{\mathbf{B}_{2n}}{u_0 - k}; \quad . \quad . \quad . \quad (41)$$

$$\frac{u_{2n+1} + i}{u_{2n+1} - k} = \frac{\mathbf{B}_{2n+1}}{k(u_0 - k)}; \quad \frac{1-j}{u_{2n+1} - k} = \frac{-\mathbf{A}_{2n+1}}{k(u_0 - k)}; \quad . \quad (42)$$

of which the consistency with (28) is evident, and which are found to agree in all other respects with conclusions otherwise obtained. Thus all these different processes of calculation conduct to consistent and interpretable results, although the method of the present article appears to depart even more than those of former ones from the ordinary analogies of algebra.

Observatory of T. C. D.
    March 17, 1853.

[To be continued.]

LII. *On the Subjectivity of certain Classes in Ethnology.* *By* R. G. LATHAM, *M.D.*[*]

TO the investigator who believes in the unity of the human species, whether he be a proper ethnologist, or a zoologist in the more current signification of the term, the phænomena exhibited by the numerous families of mankind supply nine-tenths of the *data* for that part of natural history which deals with *varieties* as subordinate to, and as different from, *species*. The history of domestic animals in comprehensiveness and complexity yields to the history of the domesticator. Compare upon this point such a work as Cuvier's on the Races of Dogs, with Dr. Prichard's Natural History of Man. The mere difference in bulk of volume is a rough measure of the difference in the magnitude of the subjects. Even if the dog were as ubiquitous as man, and consequently as much exposed to the influence of latitude and altitude, there would still be wanting to the evolution of canine varieties the manifold and multiform influences of civilization. The name of these is *legion*; whilst the extent to which they rival the more material agencies of climate and nutrition is getting, day by day,

more generally admitted by the best and most competent inquirers. Forms as extreme as any that can be found within the pale of the same species are to be found within that of the species *Homo*. Transitions as gradual as those between any varieties elsewhere are also to be found. In summing up the value of the *data* supplied by man towards the *natural history of varieties,* it may be said that they are those of a species which has its geographical distribution everywhere, and a moral as well as a physical series of characteristics. Surely if the question under notice be a question that must be studied inductively, Man gives us the field for our induction.

Before I come to the special point of the present notice, and to the explanation of its somewhat enigmatical heading, I must further define the sort of doctrine embodied in what I have called the belief of the unity of our species. I do not call the upholder of the developmental doctrine a believer of this kind. His views —whether right or wrong—are at variance with the current ideas attached to the word species. Neither do I identify with the recognition of single species the hypothesis of a multiplicity of protoplasts, *so long as they are distributed over several geographical centres.* The essential element to the idea of a single species is a single geographical centre. For this, the simplest form of the protoplast community is a single pair.

All this is mere definition and illustration. The doctrine itself may be either right or wrong. I pass no opinion upon it. I assume it for the present; since I wish to criticize certain terms and doctrines which have grown up under the belief in it, and to show, that, from one point of view, they are faulty, from another legitimate.

It will simplify the question if we lay out of our account altogether the *islands* of the earth's surface, limiting ourselves to the populations of the continent. Here the area is *continuous,* and we cannot but suppose the stream of population by which its several portions were occupied to have been *continuous* also. In this case a population spreads from a centre like circles on a still piece of water. Now, if so, *all changes must have been gradual, and all extreme forms must have passed into each other by means of a series of transitional ones.*

It is clear that such forms, when submitted to arrangement and classification, will not come out in any definite and well-marked groups, like the groups that constitute what is currently called species. On the contrary, they will run into each other, with equivocal points of contact, and indistinct lines of demarcation; so that discrimination will be difficult, if not impracticable. If practicable, however, it will be effected by having recourse to certain typical forms, around which such as approximate most

closely can most accurately and conveniently be grouped. When this is done, the more distant outliers will be distributed over the debateable ground of an equivocal frontier. To recapitulate: varieties as opposed to species imply transitional forms, whilst transitional forms preclude definite lines of demarcation.

Yet what is the actual classification of the varieties of mankind, and what is the current nomenclature? To say the least, it is very like that of the species of a genus. Blumenbach's Mongolian, Blumenbach's Caucasian, Blumenbach's Æthiopian,— where do we find the patent evidence that these are the names of varieties rather than species? Nowhere. The practical proof of a clear consciousness on the part of a writer that he is classifying *varieties* rather than *species*, is the care he takes to guard his reader against mistaking the one for the other, and the attention he bestows on the transition from one type to another. Who has ever spent much ethnology on this? So far from learned men having done this, they have introduced a new and lax term —*race*. This means something which is neither a variety nor yet a species—a *tertium quid*. In what way it differs from the other denominations has yet to be shown.

Now if it be believed (and this belief is assumed) that the varieties of mankind are *varieties of a species* only, and if it cannot be denied that the nomenclature and classification of ethnologists is the nomenclature and classification of men investigating the *species of a genus*, what is to be done? Are species to be admitted, or is the nomenclature to be abandoned? The present remarks are made with the view of showing that the adoption of either alternative would be inconsiderate, and that the existing nomenclature, even when founded upon the assumption of broad and trenchant lines of demarcation between varieties which (*ex vi termini*) ought to graduate into each other, is far from being indefensible.

Man conquers man, and occupant displaces occupant on the earth's surface. By this means forms and varieties which once existed become extinct. The more this extinction takes place, the greater is the obliteration of those transitional and intermediate forms which connect extreme types; and the greater this obliteration, the stronger the lines of demarcation between geographically contiguous families. Hence a variational modification of a group of individuals simulates a difference of species; forms which were once wide apart being brought into juxtaposition by means of the annihilation of the intervening transitions. Hence what we of the nineteenth century,—ethnologists, politicians, naturalists, and the like—behold in the way of groups, classes, tribes, families, or what not, is beholden to a great extent under the guise of *species*; although they may not be so in reality,

and although they might not have been so had we been witnesses to that earlier condition of things when one variety graduated into another and the integrity of the chain of likeness was intact. This explains the term *subjectivity.* A group is sharply defined simply because we know it in its state of definitude; a state of definitude which has been brought about by displacement and obliteration of transitional forms.

The geographical distribution of the different ethnological divisions supplies a full and sufficient confirmation of this view. I say "full and sufficient," because it cannot be said that *all* our groups are subjective, *all* brought about by displacement and obliteration. Some are due to simple isolation; and this is the reason why the question was simplified by the omission of all the *insular* populations. As a general rule, however, the *more definite the class, the greater the displacement;* displacement which we sometimes know to have taken place on historical evidence, and displacement which we sometimes have to infer. In thus inferring it, the language is the chief test. The greater the area over which it is spoken with but little or no variation of dialect, the more recent the extension of the population that speak it. Such, at least, is the *primâ facie* view.

A brief sketch of the chief details that thus verify the position of the text is all that can now be given.

1. The populations of South-eastern Asia, Mongol in physiognomy and monosyllabic in speech, have always been considered to form a large and natural, though not always a primary, group. Two-thirds of its area, and the whole of its frontier north of the Himalayas, is formed by the Chinese and Tibetans alone. These differ considerably from each other, but more from the Turks, Mongols, and Tongusians around. In the mountainous parts of the Assam frontier and the Burmese empire, each valley has its separate dialect. Yet these graduate into each other.

2. Central Asia and Siberia are occupied by four great groups, the populations allied to the Turk, the populations allied to the Mongol, the populations allied to the Mantshu, and the populations allied to the Finns. These are pretty definitely distinguished from each other, as well as from the Chinese and Tibetans. Each covers a vast area, an area, which, either from history or inference, we are certain is far wider at present than it was originally. They have encroached on each, and on the populations around, till they meet with families equally encroaching in the direction of China and Tibet. This it is that makes the families which are called *Turanian* and *Monosyllabic* natural groups. They are cut off, more or less, from each other and from other populations by the displacement of groups originally more or

less transitional.   The typical populations of the centre spread them at the expense of the sub-typicals of the periphery until the extremes meet.

3. The circumpolar populations supply similar illustrations. Beginning with Scandinavia, the Lap stands in remarkable contrast with the Norwegian of Norway, and the Swede of Sweden. Why is this? Because the Northman represents a population originally German,—a population which, however much it may have graduated into the type of the most southern congeners of the Lap, is now brought into contact with a very different member of that stock.

4. This phænomenon repeats itself in the arctic portions of America, where the Algonkin and Loucheux Indians (Indians of the true American type) come in geographical contact, and in physiological contrast, with the Eskimo.   Consequently along the Loucheux and Algonkin frontiers the line of demarcation between the Eskimo and the Red Indian (currently so-called) is abrupt and trenchant.   Elsewhere, as along the coast of the Pacific, the two classes of population graduate into each other.

5. The African family is eminently isolated.   It is, however, just along the point of contact between Africa and Asia that the displacements have been at a *maximum*.   The three vast families of the Berbers, the Arabs and the Persians, cannot but have obliterated something (perhaps *much*) in the way of transition.

6. The Bushmen and Hottentots are other instances of extreme contrast, *i. e.* when compared with the Amakosah Caffres. Yet the contrast is only at its height in those parts where the proof of Caffre encroachment is clearest.   In the parts east of Wallfisch Bay—traversed by Mr. Galton—the lines of difference are much less striking.

Such are some of the instances that illustrate what may be called the " subjectivity of ethnological groups,"—a term which greatly helps to reconcile two apparently conflicting habits, viz. that of thinking with the advocates of the unity of the human species, and employing the nomenclature of their opponents.

LIII. *On a New Test for Nitric Acid and the Nitrates.   By* Edmund William Davy, *A.B., M.B., T.C.D., Lecturer on Chemistry in the Carmichael School of Medicine, &c., Dublin**.

THE want of a simple and satisfactory test for minute quantities of nitric acid and its salts has long been felt by those engaged in chemical research; for though a number of methods, more or less delicate, have from time to time been proposed,

* Communicated by the Author.

most of them are objectionable, being inconclusive if taken alone, inapplicable in many cases, or requiring the aid of expensive alkaloids not easily procured. The test I propose appears to me to be free from those objections, and accomplishes its object with considerable facility; it depends on the formation of that interesting class of salts lately discovered by Dr. Playfair, the *Nitroprussides,* and the beautiful and characteristic effect produced on them by an alkaline sulphuret. I have found that the nitro-prussides may be formed (under certain circumstances) when only very minute quantities of nitric acid or its salts are present. The method I adopt is as follows :—To the solid or liquid supposed to contain nitric acid, in its free or combined state, add a few drops of a strong aqueous solution of ferrocyanide of potassium (yellow prussiate of potash), then some pure muriatic acid, mixing the ingredients well together both before and after the addition of the acid, and gradually raise the temperature of the mixture to about 160° Fahr., or in some cases it may be necessary to raise it a little higher; let it cool, and then neutralize with carbonate of soda or potash, a slight excess of either being of no consequence. Filter if there is much precipitate, and finally add to the fluid a drop or two of a solution of either sulphuret of ammonium, sodium or potassium, when if nitric acid were present, a fine purple or violet colour will be produced, which, however, is not permanent, but soon disappears. In using this test certain precautions are necessary, viz. 1st, much dilution of the materials must be avoided, as the acid is required to be tolerably strong to produce the necessary reaction. The muriatic acid I have used is of spec. grav. 1·15; if a much weaker acid be employed, the results will not be satisfactory. 2nd. If the nitric acid or nitrate be in very minute quantity, the mixture of it with the ferrocyanide and acid (already referred to) should be allowed to cool to the ordinary temperature before the alkaline carbonate is added; much excess of this last substance should be avoided, as at a temperature of about 100° it begins to decompose the nitroprusside, on the formation of which the test depends.

In proof of the delicacy of this test, I may remark I have detected by its means the nitric acid in the one two-hundredth part of a grain of nitre; and with one hundredth part of a grain the effect is very striking. Its efficacy, too, seems scarcely impaired by the presence of a number of substances, whether of mineral, vegetable, or animal origin, as by it I readily detected the presence of nitric acid in mortars taken from old buildings; also where that acid or its salts were added in small quantity to soils, sulphuric and muriatic acid, tea, porter, milk, &c.

I may further add, that there is generally a striking difference

between the cases where no nitric acid is present, and where it occurs, when the substance is heated with muriatic acid and the ferrocyanide. When nitric acid is not present, the mixture quickly becomes of a blue colour; but where that acid exists, it first becomes of a yellowish-green, then of an olive or dark brown; but these indications alone are not sufficient to prove the presence or absence of nitric acid, until afterwards confirmed by the action of an alkaline sulphuret. In heating the mixture, the temperature stated should be maintained for a few moments till it ceases to acquire a darker shade, thereby indicating that all the nitroprusside is formed.

Sulphuric acid may be substituted for muriatic in using this test; but I prefer the latter, as being more easily procured pure, and as strong sulphuric acid aided by heat will decompose the nitroprussides.

This test may also admit of application to nitrous acid and the nitrites; but as those compounds are comparatively unimportant, I will not now enter on this subject.

LIV. *On a proposed Test of the Necessity of Indirect Proof in Geometrical Demonstrations, with Remarks on Methods of Demonstration.* By JAMES ADAMSON, *D.D.*

IT does not seem difficult to discover a characteristic capable of determining what are the character, relation, and effect of truths of different orders employed in reasoning. All truths so employed, in a definite series of arguments, are separable into two divisions, according as they are, or are not, primarily made use of as hypotheses. We are perfectly sure that that proposition is essential, which is either singly modified to become a conclusion, or which is combined with something else to constitute a conclusion. Any other truths expressible in general terms, can be introduced only to facilitate the transition from the one form of statement, or the hypothesis, to the other form or conclusion. They therefore are auxiliary only, and the employment of them individually will be contingent on the form of argument which may be adopted.

Names employed to designate such orders of truths ought to classify distinctly things so different. No truth of the essential or hypothesis class should rank among those of the individually non-essential, or auxiliary class. The distinction does not rest on a proposition being more or less certain, or more or less obvious, but on their well-defined dissimilarity in use and value. This is dependent on a difference in generality of character; for

that which is only auxiliary in one science, may be auxiliary in others also. *Axiom* is the title for such auxiliary truths, and it ought to be restricted to them alone.

With a correct nomenclature, therefore, the question whether axioms in geometry are essential as the foundations of its theorems, is easily settled. They enter into no hypothesis, and are therefore not essential, but are auxiliary only. The relations expressed by them necessarily occur, but the expressions of them in general terms, which make them to be axioms, are non-essential. In fact those truths to which the name is generally applied, are corollaries from definitions of general terms. Their existence as axioms depends therefore on the existence of such general terms. There are tongues in which these are scanty. There may be tongues in which there are no such words as *equal, whole, part,* &c., and yet thought would follow its laws of analysis or proof; and demonstrations, though less or more cumbrous, would be as strict and as conclusive as they are in languages more prolific in terms.

Their auxiliary character, and their exclusion from being hypotheses, ought to be the only circumstances determining the character of axioms. To describe them as being " self-evident truths," is indefinite, and useless. Whether their truth be more or less easily perceived, depends not on their character, but on the mind, and on its consciousness of the signification of terms. Controversies, therefore, as to whether one proposition assumed to be an axiom be more or less clearly evident than another, are futile. All truths of this auxiliary character have a common nature, and deserve a common name. This common nature is most fitly indicated by the term *axiomic.* The proposition that " half the sum added to half the difference makes the greater of two magnitudes," does not differ *in kind* from this other—that " if equals be added to equals the sums are equal." With these also the following are identical *in kind,* notwithstanding their greater complexity—" If there be three magnitudes such that any two are together greater than the third, then half the sum of the three is greater than any one of them,"—or " if, in a series of continued proportionals, any term is equal to the difference of the two which precede it, then also any term is equal to the sum of the two which follow it:" and so in regard to multitudes of others. All are of the same order, and ought to come under the same name.

It is evident that whatever may be the subject of a science, truths of this class *may* become auxiliary in reasonings relating to these subjects. They are therefore not concrete, but abstract. Hence axioms in geometry are algebraic truths, or are true of any concrete magnitudes, as space, force, money, sound, &c.

Nothing purely geometric can enter among them.   Axioms in fact may be either corollaries or theorems.   In the former case, they may need no demonstration, or may be termed self-evident; in the latter case, they may, in some instances, to some minds, and in some instances, to all minds, require demonstration; but this does not affect their essential nature, nor the relations they bear to the subjects of specific sciences.

Everything, except their application, ought to be eliminated from the reasonings of specific sciences; and if requiring demonstration, they should be treated symbolically, or algebraically.   Geometry therefore has not assumed its proper logical form until we have relieved its demonstrations from all those portions of them which constitute reasonings applicable not purely to geometric, but abstractly to all modes of magnitude. These steps in reasoning should be provided for by axiomic theorems, just as they would be in sciences relating to force, money, sound, &c.   This would contribute greatly to clearness and elegance in demonstration.   No finer instance of such effects can perhaps be found than in the applications of Euclid's criterion of proportionality, which, under a general law, makes relations of number or of quotient, in regard to all kinds of quantity, to be deducible from relations of excess or difference.   Thus the same rule is applicable to every mode of geometrical magnitude.   An interesting example may perhaps be found also, in the use of the following theorem and its converse.   If $A : B = C : D$; then $A^2 : A^2 - B^2 = A^2 - C^2 : (A \pm D)^2 - (B \pm C)^2$, whence $(A + D)^2 - (B + C)^2 = (A - D)^2 - (B - C)^2$.   If we employ *opposite* terms to indicate the *extremes* and *means* of a proportion, then by using that word along with the others *adjacent* and *alternate*, we can exhibit these results in various very beautifully symmetrical forms of expression.   From the relation between the ordinates to the axis and the segments of the axis, in conic sections, we deduce instantly by the above theorem or its results, the conclusions, that the ratio of the distances from a point in the curve to the focus and to the directrix is constant; and that the sum or the difference of the lines to the foci is constant.   The complex proportions generally used for these purposes, may be considered as replaced by the steps of reasoning in the theorem; which are obvious.

Since axioms ought not to introduce hypotheses, we see at once what is the real place and office of geometrical definitions. In fact, as men do in all other sciences, and as they generally endeavour, though sometimes unconsciously, to do in geometry, definitions must be made the foundation of all conclusions.   Properties of objects are necessarily derived from their nature, either when assumed for consideration individually, or in combination.

Postulates are therefore inadmissible, except they be of such a character as determines the whole processes of the science; such as, that things defined or proved may be exemplified, or that objects may be conceived to be superimposed, &c.   Any properties becoming the foundation of argument must belong to the nature of the things argued about, and must be found in definitions, or in conclusions from them.

A definition ought to express a single property of the thing defined.   To object to a definition that it contains something which may be proved, is futile; for any property may become a conclusion, if some other be assumed as the hypothesis.   But a definition must not contain both a hypothesis and a conclusion, as is the case with some proposed definitions of parallelism, such as, their equidistance at indefinite points, or their common inclination to indefinite straight lines.

Things ought to be considered in the first instance individually, and as they are in themselves, without involving them in combinations.   Common sense or sound logic, therefore, requires that when, in constructing definitions, we find more properties than one of an object offered to our choice, we should prefer that which, assumed as a hypothesis, affords the shortest and clearest arguments.   Thus there are three properties characterizing straightness of line.   Now if we assume as the defining property *minimum* distance, expressed in the formula " shorter than any other between two points," we establish at once,— 1st, the property of *singleness;* 2nd, that of *coincidence.*   We cannot attain the same result, of thus instantly proving all, if we assume either of the others as the definition.

When the hypothesis of a proposition is single, it can have but one converse statement.   Such propositions ought perhaps to be termed *corollaries.*   A normal *theorem* is a proposition in which the hypothesis is twofold.   Its demonstration is the logical combination of these two truths.   There are therefore three truths so related, that two being assumed as hypothesis, the other follows as conclusion.   Hence such a theorem has two converse forms.   Now it is obvious that when the hypothesis announced is, strictly and solely, that which is used in the argument, and the conclusion announced is, strictly and solely, that which is gained by the combination, all converses are necessarily true. All apparent exceptions to this arise from incorrect statements of hypotheses, or of conclusions.   This logical position may therefore legitimately be assumed as rendering the demonstration of converses unnecessary.   Nevertheless the proof of a converse is always to be sought for, as completing the train of argument and determining the real nature of the hypothesis and conclusion, as they are involved in the argument.

The *use* of a converse statement is very important. It becomes a criterion of the presence or existence in given circumstances of the property announced as the original hypothesis, or, which is the same thing, it shows that either property is found where the other is, and *nowhere else*. If in a series of theorems converses follow in immediate sequence, those to which they bear this relation, their demonstrations will generally be indirect. Their *criterion* character renders this advisable, even where by means of preparatory theorems it might be avoided. In deducing conclusions from definitions, at the commencement of a series of arguments, the mind's process is ultimately the dismissal of an alternative, or is primarily indirect; *ex. gr.* if angles are equal their constituents will coincide; for *if not*, there is a contradiction. Evoking a contradiction will always be found to be the seal of certainty on the mind's convictions, for certainty is the impossibility of disbelieving.

Oppositeness of conditions must necessarily be distinguished from converseness of statement. Where such opposition gives origin to two theorems, each may have its converse, and all may require demonstration.

This is exemplified when we have equality among constituents in the hypothesis leading to equality among other constituents in the conclusion, combined with a definite relation of inequality in the data leading to a similar relation in the conclusion. Such cases we have in the relations of the sides and angles of different forms of triangle.

If, however, the relations of inequality are indefinite, though there may be four statements, two demonstrations only are needful. Take for instance the direct affirmative: " Rectangular parallelograms have equal diagonals; " to that we may have the direct negative; " Non-rectangular parallelograms have unequal diagonals," without determining the inequality. There will be, of course, a converse affirmative, and a converse negative. Now we shall find that if we demonstrate any two which are either converse to each other, or which are both direct, or both converse, affording four possible combinations, we shall have ample criteria, from any one of these dualisms, to determine all cases; while the other two possible arrangements do not serve that purpose. It is therefore sufficient in such instances to prove the direct affirmative and its converse. This is the proceeding which the mutual relation of such truths always suggests.

If we assume as the defining characteristic of proportion that property which consists in the equality of fractions or of quotients, we do not find this criterion to be readily applicable to geometrical magnitudes. Euclid's criterion, in his definition, is easily and elegantly applicable to them, for a reason already

stated. It is therefore of importance that the latter be derived from the former by rigid argument. The question being, whether ratios are equal or not equal, there will be four statements, by which relations between the two properties may be indicated.

1. In regard to four proportionals, equimultiples of alternate terms, when compared adjacently, have symmetrical relations as to magnitude.

2. In regard to four non-proportionals, equimultiples of alternate terms, when compared adjacently, have non-symmetrical relations as to magnitude.

3. Four magnitudes, of which the alternate equimultiples, when compared adjacently, have symmetrical relations as to magnitude, are proportionals.

4. Four magnitudes, of which the alternate equimultiples, when compared adjacently, have non-symmetrical relations as to magnitude, are not proportionals.

Of these four statements, two are enough to afford criteria both of proportionals and of non-proportionals. But to serve this end, we must, as has been already stated, take either, two which are converse to each other, or else those which are both affirmative, or else those which are both negative. To take the second and third, or the first and fourth, will not suit.

The character of a definition is such, that its direct and converse statements are both applicable. Hence Euclid's criterion of proportion includes both the first and third. When, however, we treat all as theorems dependent on equality of fractions, the case is different. If we so treat the first and fourth only, we have a test of non-proportionals in the fourth, but we have no test of proportionals at all. Neither can therefore be made to suit the case—given—*the relation of the equimultiples,* to infer—*proportionality.* The first is useless from the nature of its hypothesis, and the last from the nature of its conclusion. A misconception of this matter has prevailed generally, which it is as well to remove. It is found in Peacock's Algebra, vol. i. p. 175 ; the demonstration of the fourth in that work, moreover, is correct only for commensurable quantities ; and the attempt to remedy this will require other provisions, introducing greater complexity of reasoning. But the whole end is attained by proving the first and third, which are converse to each other.

From the equality of two fractions, to deduce the defining property of Euclid, offers no difficulty. The converse statement may be managed as follows :—If there be two homogeneous magnitudes C and D, and we take of the latter any multiple, such as nD, then may C + C + C, &c. be so taken that mC shall be

greater than nD; but $(m-1)$C shall be not greater than nD, whether the magnitudes be commensurable or not.

Again, if there be two magnitudes D and F, and a third magnitude C such that when equimultiples are taken of D and F it is found that no multiple of C can exceed either of the equimultiples nD or nF, without also exceeding the other, then D is equal to F. For if either of them as F be the greater, or F = D + Q, then substituting D + Q for F, and assuming n such that nQ is greater than C, we must have mC greater than nD + C when mC is greater than nD, whatever m may be; or in all cases $(m-1)$C must be greater than nD when mC is greater than nD, which is absurd. Hence these quantities are equal.

If we now assume A, B, C, D, in conformity with Euclid's definition, as our hypothesis, or have mA and mC such that in regard to nB and nD each always exceeds its adjacent multiple, when the other exceeds its adjacent multiple, then it is easy to show that A : B = C : D, in the sense of the adjacent terms forming equal fractions; for if a greater or smaller quantity were substituted for any of them, such as D, so as to form a proportion, then the application of the principle demonstrated above would show that this involved a contradiction.

This appears to be the natural and proper mode of making Euclid's criterion to be a consequence of the principle now adopted, as discriminating equality of ratios. It is evident that no other relation of the multiples needs to be introduced into the statement of the principle, except that of *contemporaneous excess* in magnitude; and it may therefore be expressed in the simpler form, that if we take equimultiples of alternate magnitudes, and find that the multiples of the first and third must each exceed its adjacent one, when the other exceeds its adjacent one, then these magnitudes are proportionals.

It will be easily seen that the effect of the converse statement is, as such ought always to be, restrictive or exclusive. It makes the relation of the multiples to be a test determining what *alone* can be proportionals, and renders unnecessary any other criterion of non-proportionals.

N.B. I may be in regard to this subject allowed to suggest the following statement, as a useful epitome of various truths :—
" In a proportion the sums or the differences of adjacent terms are as alternate terms, and the sums or the differences of alternate terms are as adjacent terms."

LV. *Were the Ancient Egyptians acquainted with Nitric Acid?*
By Thornton J. Herapath, *Esq.**

IN the Supplement Number of the Philosophical Magazine
for July last, my father published a paper on " Early Egyp-
tian Chemistry," in which he gave a short account of some expe-
riments he had made on certain hieroglyphical marks or letters
that had been discovered on the wrappers of a mummy which was
recently unrolled by Mr. Nash at this Institution; and he then
stated it was his belief that the ancient Egyptians were acquainted
with a marking-fluid containing nitrate of silver for its basis,
and were also familiar with the use of nitric acid. A short time
afterwards, Mr. Denham Smith, in a reply to this letter, took
exception to my father's views on the subject; and whilst admit-
ting the interesting nature of the discovery that had been made of
an argentine solution having been employed some three thousand
years ago as 'marking-ink,' totally dissented from the conclusions
that had been founded on it, inasmuch, he said, as there was no evi-
dence to prove that the Egyptians were even acquainted with the
art of distillation. He also hazarded the opinion, unsupported,
however, by direct evidence, that the marking-fluid in question
was prepared—if I understand him aright—by dissolving either
the chloride or oxide of silver in an ammoniacal solution, and
consequently without the intervention of any acid whatever. As
the question in dispute is one of considerable interest in a
scientific point of view, I determined to subject the marks to a
fresh examination. After making several preliminary experi-
ments, I found that no results of any value could be obtained by
the ordinary modes of analysis, and was therefore compelled, as
a last resource, to call in the assistance of the microscope.

Upon examining some of the fibres of the bandages that were
stained by the argentine ink, I found them to present a very
peculiar appearance. The ends nearest the corroded parts of the
fabric were stained of a deep brown or black; but for some little
distance beyond the blackened portion the fibre was coloured of
a pale yellow hue, as if, during the decomposition of the silver
salt that had been employed as 'marking-ink,' the nitrogenous
constituents of the tissue had been partially acted upon and con-
verted into xanthoproteic acid.

Between several of the fibres, and in many places adhering to
their external surfaces, were found small particles of some organic
tissue, which were coloured of a still deeper yellow than the
fibres themselves. On subjecting some of the latter to the action
of strong *liquor ammoniæ*, the yellow portions, particularly the

* Read before the Bristol Microscopical Society, and now communicated
by the Author.

altered intercellular substance, acquired a still darker hue, whilst those, on the contrary, which were stained by the reduced silver became somewhat paler, as if a portion of the argentine film were soluble in that menstruum; though it should be observed, that no further effect was produced on the stain even by a protracted digestion in ammonia. Now, upon making comparative experiments with a piece of the linen wrapper that had been recently 'marked' in the usual way with a solution of nitrate of silver, the fibres were found to present a very similar appearance to that before described; the only difference being that the yellow coloration was not quite so distinct, and more of the silver-stain was removed by the solution of ammonia; whereas on the other hand, fibres that had been stained by solutions of chloride and oxide of silver in ammonia were uniformly coloured of a dark brown or black, and exhibited no trace of yellow coloration.

These results, conjoined with the corroded state of the linen in the vicinity of the marks, justify us, I think, in concluding that the ancient Egyptians were really acquainted with nitric acid; and, as my father has already suggested, employed the nitrate of silver as a marking-fluid. In what manner, however, they prepared the acid, whether by the distillation of the nitrate of soda or nitrate of potash with oil of vitriol, or by one of the processes adopted by the alchemists, I will not at present attempt to decide, but will leave the problem to be solved at some future period, when the researches of antiquarians shall have offered us further evidence on the subject.

Mansion House, Old Park, Bristol,
      March 27, 1853.

LVI. *On Ground Ice found in the Beds of Running Streams.*
      By RICHARD ADIE, *Esq., Liverpool*.*

FOR the information of those who may not have turned their attention to this subject, it may be desirable that I preface my remarks by stating that ground ice is the name given to soft snow, like accumulations of ice, found only in the beds of running streams. In the Thames the watermen of the last century were familiar with ground ice, and it was then made the subject of accurate descriptions, but at that time no one attempted to account for its appearance; this has been reserved for a comparatively recent period, when Dr. Farquharson, M. Arago, and Dr. John Davy have made the causes of the appearance of ice in an unnatural position under water the object of their study. The theories they have offered for explaining the formation of ground ice are based on the supposition that the ice is formed

      * Communicated by the Author.

in the place where it is found. I had occasion to submit the methods of accounting for ground ice by Dr. Farquharson and M. Arago to my friend Mr. James Elliot, then resident on the banks of the river Teviot; he there tested the theories of the formation of ground ice by the facts presented to him through a careful examination of the bed of the river, and came to the conclusion that they did not agree well with one another. After a lapse of some time I submitted to him a new explanation of the phænomenon of the appearance of ground ice, which was based on details given in Mr. Knight and Mr. Eisdale's published papers on the subject*, and also from the valuable evidence supplied by Mr. Elliot against the theories in question. The theory I gave of the formation of ground ice was, *that it was all formed at the surface, being the coldest part of a running stream,* submerged by the motion of the current, then carried along until entangled by some plant or asperity in the bed of the stream. An icy nucleus once begun, the trite illustration of a snow-ball rolling down a hill describes what follows in a river at 32° temperature, having minute particles of ice mingled with the water, the process of accumulation, however, being inverted; for in the stream, the nucleus which corresponds to the snow-ball remains stationary, while the icy particles corresponding to the snow are rolled over it by the motion of the river. When the ice is lodged in a plant moored to the ground by a slender stem, the accumulation assumes a spherical form, from the twisting and turning of the plant in the current; but where the asperities are fast to the bottom, the ice spreads over the bed of the stream after the manner of a covering of snow on rough ground. The velocity of the water has an influence on the nature of the particles of ice which form the accumulation; in slow running streams, like those in the vicinity of Liverpool, it consists of thin scales of nearly one-quarter of an inch superficies; pieces of this size interlocked with one another have under water a transparent appearance, so that to a casual observer they are easily overlooked even when in large quantities in the bed of the river; where the water runs swiftly, the particles of ice become much smaller, and when collected together assume the appearance described by Mr. Knight, of the most silver whiteness.

In the theories offered to account for the formation of ground ice in the part of the stream where it is found, the motion of the water is stated to prevent the formation of ice at the surface, and the particles on the ground being nearly in a state of rest the water freezes there. This view proceeds on the assumption that motion in water is a preventive to freezing, which is only true in a

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* Abstracts from these will be found under article Ground Ice, Penny Cyclopædia.

qualified sense. When water has different temperatures at different depths, for example, 35° at the bottom and 31° at the surface, then it is easy to see that motion, by mixing the water at 35° with the surface-water, must retard its freezing ; and this is the state of our lakes, ponds, &c. But where the water from rolling along the bed of a stream has one uniform temperature of, say 31°·5, I apprehend the motion favours the formation of ice. In the experiment of cooling down still water below the freezing-point, a rapid congelation is found to follow the slightest motion.

The soft snow-like nature of ground ice is evidence in favour of its being not formed *in situ*; for if ice be formed by artificial means at the bottom of a vessel of water, it is of the hardest description; I can see no reason why, in the bed of a stream where the motion of the water is at a minimum, it should differ from that formed by the artificial process. In this country, where the frosts are never of very long duration, ground ice is in the form of a soft incohesive mass; but in localities on the continent, where the winter is steady and long, ground ice assumes a firmer compact form ; for the same reason that newly-fallen snow, a loose mass ready to be driven about by every wind, in process of time gradually passes into a state of firm ice, as has been shown in the modern researches on glacier ice. Another circumstance against the supposition that ground ice is formed where it is found is, that in places favourable for its reception the quantity found in the bed of the stream under a depth of one to two feet of water is often greater than the quantity of surface-ice on a like superficies of still water in the same neighbourhood. Supposing the ice to be formed on the bottom, the superincumbent body of water must act as a great retardation. The effect of a light arch over a canal in checking the freezing of the water underneath illustrates this. The quantity of ice in the river-bed should thus be much less than the surface-ice in the neighbourhood for a corresponding superficies, which is often not the case.

The streams best suited for exhibiting the phænomenon of ground ice are those which drain open exposed tracts of country with few trees or tile-drains. From 10 to 12 miles north of Liverpool, at Altcar, there is a flat country of this character: the land is drained by a series of open slow running ditches ; a portion of the water of these ditches is carried by a stream, locally known by the name of the Brook, into the river Alt. The velocity of the water varies from 2 to 3 miles per hour, depth 2 feet. On the 11th and 12th of February 1853, the district was visited by a severe frost, temperature 21°, with a sharp wind; on the morning of the 13th I examined the locality, when I found the bed abundantly covered with ground ice. The temperature of

the water was 31°·9, air 35°. The ice consisted of small thin plates interwoven with one another in every possible manner; where the water was not more than 2 feet deep, and the current ran at 2½ miles an hour, the ice spread over the bed of the stream to a depth of from 2 to 4 inches. At the edges on the surface there was not much ice; in thickness it was about ¾ of an inch at the edge, and thinning off quickly to a shell edge within 1 foot from the side of the bank. On the ditches in the neighbourhood the ice was about 1 inch thick, which at once broke when an attempt was made to stand upon it. In the forenoon of the 13th the sun shone with considerable power; this had the effect of producing a sufficient increment of temperature to detach the ice from the bed of the stream, and when I left the Brook the ground ice had begun to rise rapidly. I have observed that changes of this kind are very sudden; in half an hour a large portion of the ground ice will often disappear; it rises to the surface and floats away with the current. It often gets obstructed in its passage down, when the surface of the stream becomes choked with loose floating ice. In some places inundations follow, which render these accumulations a source of anxiety to the neighbouring proprietors.

I have at intervals of several years examined this locality for ground ice when the state of the weather seemed to be favourable for its formation, and rarely been disappointed. One or two nights of sharp frost with wind appears to produce it. The view that I take of the mode of its production is, that the wind acting on the ditches in the exposed flat district soon cools them down to 32°, and at the beginning of the frost forms scales of ice on their surface, which are carried along by a slow current till they reach the Brook; here the velocity of the water increases, and the light particles of ice are mixed up with the water by the current and get entangled by asperities in the bed of the stream, consisting chiefly of stones and portions of plants. In some parts where the stream runs slowly there is no ground ice; but if there be a short space of it with a more rapid current, there the accumulation of ice is found in plenty.

The frost continued from the 13th of February onward; some days later I again examined the bed of the Brook, and found nearly as much ground ice as on the first occasion; it now wore a gray aspect, from minute particles of clay and sand lodged amongst it by the current of water. Under two light stone arches over the rivulet I found it; under one of them, where the bed of the stream and the velocity of the water were favourable for the accumulation of this kind of ice, it existed in the greatest possible abundance. This observation recalled to my recollection a remark made by Mr. Elliot, while discussing M. Arago's theory of the

formation of ground ice, that in the Teviot he often found it in plenty in localities where the stream appeared to be well-shaded by trees. The fact of ground ice being found in such places at once shows that it never could have been produced there, for to form surface-ice in small quantity under an arch requires a more intense frost than we had had. The temperature of the stream where the ice was found under the arch was 32°, atmosphere 36°·5; the thickness of the ice on exposed ditches measured a little under 2 inches.

Wherever there are running streams readily cooled down to 32°, there can be no doubt about ground ice being found, but this temperature is essential; for if the water of the stream rise only a small portion of a degree above the freezing-point, the ground ice soon disappears.

While examining the waters in the vicinity of Altcar, I have often tried to estimate the relative power of a stream and of a stagnant water in producing ice. The Brook appears to come so soon to 32°, that I incline to the opinion that more ice is made on it than on an adjacent surface of still water. In the present frost there appeared much less ice on it, but then the sun at this season is daily of sufficient power to loosen large quantities from the edges which are seen continually passing down to the sea. This process has the effect of exposing every evening a fresh surface of water to the most favourable action of the sky, where ice must be formed more rapidly than in still waters already covered over with thick ice.

Subaqueous ice owes its preservation to the current of ice-cold water which the stream keeps continually pouring upon it. On the occasions when I have found it in the Brook, the frost had not penetrated the soil or stagnant water deeper than 2 inches; ice could not at such a time remain in still water, at 2 feet below the surface. In the situation I have described, where the subaqueous ice had a covering of a stone arch in addition to the water above it, the necessity of an active current of water to prevent its melting was still more apparent. During a former season, when examining the ice in the bed of the Brook, I tried the temperature of the clay and sand over which the water rolled, and found it to agree very closely with that of the water. This, however, was only for a small depth, for during frosts the temperature increased rapidly downwards for the first few feet. A running stream is to some extent an exceptional case to the rule, that in frosty weather the temperature increases quickly as we descend; for in the stream the force of gravity acts so as to keep bottom and surface-water continually changing their respective places. On this peculiarity of a river must mainly rest the view which I have advocated, *that ground ice consists of spiculæ*

formed in the coldest part of a river and transported to their place of rest by the action of the current.

In the neighbourhood of St. Petersburg, in the Neva, ground ice is found; when the surface of the stream has a covering of ice and snow 3 feet thick, it is described to have the usual loose snow-like texture, which to my mind is the best evidence that the stream has brought it there. But where the icy spiculæ were formed, and when they were lodged, are questions that require a local inspection before an attempt could be made to answer them. In the basin drained by the Neva, consisting of lakes, swamps, and mountain ranges on its confines, these spiculæ may have had a long journey from the interior before they reached their place of rest, the five months' winter to which the region is subjected affording ample time for the transit.

In the Tay, near Perth, it has been noticed that ground ice makes its appearance before surface-ice; and when the latter has become strong enough to walk on, the subaqueous ice may be viewed through the transparent surface-ice. I have no doubt that the same thing occurs in the Brook near Liverpool, when the winter is sufficiently severe to cover over its surface with ice; an effect which I have not witnessed, although residents in the neighbourhood inform me that it has taken place. The circumstance of the early appearance of the ground ice during a frost adds a confirmation to the view that the ice is lodged, not formed, in the bed of the stream.

LVII. *On Copper Smelting.* By James Napier, *F.C.S.*[*]
[Continued from p. 184.]

Fusing calcined coarse Metal.

THE next operation is fusing the calcined coarse metal. The charge for an ordinary-sized furnace of 8 feet by 13 feet is—

> 25 cwt. of calcined metal,
> 5 to 7 cwt. slags from the *roasters,*
> 2 to 3 cwt. of *cobbing.*

In this mixture the oxide of iron is in excess in relation to the silica, and it is therefore much more easily fused than the ore; but the reactions which take place are similar; the silica and oxide of iron combine to form slag, which floats upon the surface of the mat and has to be skimmed off, after which the mat is tapped out into sand-moulds. Two charges are generally fused before the metal is tapped out. This mat is termed *blue metal,* from its being of a slate-blue colour; the scoria is termed *sharp*

[*] Communicated by the Author, who reserves to himself the copyright, any infringement whereof will invoke legal proceedings.—Eds.

slag, from its containing an excess of oxide of iron, and being consequently used as a flux for fusing the ore.

The blue metal varies considerably in composition, according to the state of the calcination of the coarse metal. The following analyses of several kinds will make this variation manifest:—

	Low quality.	Average quality.	Good quality.	Superior quality.
Copper	55	58·8	63·2	65·8
Sulphur	23	20·5	21·8	22·6
Iron	18	11·6	8·6	9·1
Silica	3	4·2	4·8	1·8
Antimony, tin, &c.	1	1·3	·8	·7
Oxygen and loss .	...	3·6	·8	
	100	100·0	100·0	100·0

Comparing the results of this table with the quantity of sulphur in the calcined coarse metal, some sulphur appears to have been evolved during the fusion, but the quantity given off is very small. The reason of the blue metal being so rich in copper is owing to the rich slags added before fusing, which contain no sulphur.

Blue metal is often full of small specks of reduced copper, and small cavities lined with fine fibrous metallic copper, growing out like moss; these fibres are often not less than an inch in length. This is appropriately termed *copper moss;* it occurs generally where the metal has come into contact with moisture, which probably not only produces the cavities, but effects the reduction of the metal. The composition of this moss given by Le Play is—

Copper	98·2
Iron	·4
Nickel	·6
Mixed carbonaceous matters .	·2
	99·4

It has sometimes a brassy appearance, and at other times a crimson-red tint. A sample of each gave by analysis—

	Brass colour.	Red colour.
Copper	98·5	99·0
Sulphur	·4	·4
Tin, antimony, &c.	1·0	·5
Iron	trace	trace
	99·9	99·9

The purity of the copper is remarkable at this stage of the

operation. We have seen moss upon blue metal from the most impure ores equally as good as that from the best.

When blue metal is put into water immediately after setting, it assumes a red-brown colour. When this is examined with a lens, the colour is seen to be caused by the reduction of copper in fine grains, not only over the surface but through the mass, bearing out the supposition that the copper moss may be produced by moisture.

Copper moss occasionally occurs in some of the other operations, but it is most common in the blue metal.

The sharp slags obtained with the blue metal are also of varied composition. The following three analyses give a fair average of their qualities :—

Copper	1·5	1·9	2·0
Silica	40·2	35·8	36·6
Protoxide of iron	54·4	56·4	57·5
Lime	1·3	1·6	·9
Oxides of tin, antimony, &c.	·7	1·3	1·0
	98·1	97·0	98·0

Some of the iron in these analyses was in a higher state of oxidation than protoxide.

By referring to the table of equivalents of flux for silica, p. 455, vol. iv., the value of these sharp slags will be apparent, and also the proper proportions in which they should be added to an ore. Taking the mean of these analyses, we have—

Silica	37·5
Oxide of iron . . .	56·1
Lime	1·3
Other oxides . . .	1·0
	95·9

not including the copper. Then, as 46 silica requires 36 oxide of iron, the 37·5 will take 30 of the oxide of iron, leaving upwards of 27 per cent. of these sharp slags available as flux.

Several modifications have been introduced at this stage of the operation. Some smelters, instead of tapping the mat into sand-beds to make blue metal, tap it into deep pits of water in the same manner as we have described for coarse metal, granulating it for the purpose of giving it another calcination. When this is done, the granulated mat is termed *fine metal*, in contradistinction to coarse metal.

The calcination of the fine metal is conducted in the same manner as that of the coarse metal, and the chemical changes which take place in the furnace are the same. When the calcined fine metal is fused, some refinery slags rich in copper are

added. The produce of this fusion is about three-fourths mat and one-fourth slag, consisting of—

Copper 7
Oxide of iron . . . 64
Silica 24

The mat or metal will be described further on. This calcination and fusing replaces a roasting of 24 hours; and where the process of selecting is required, it is considered preferable.

When oxides and carbonates, such as the Australian ores, are on hand, they are generally fused with the calcined coarse metal, by which means a double advantage is obtained; the excess of oxide of iron in the calcined metal fluxes the silica of the ore which has little iron, and the copper in the ore is converted into subsulphuret, a condition necessary for reduction by the present method of smelting. The produce of this fusion is a mat termed *pimpled* metal, from its having small rough pimples on the surface of the ingots. The average composition of this metal is—

Copper 78
Sulphur 18
Iron 2
Silica 2
          ———
          100

When this metal is produced by the admixture of Australian ores, it has generally a redder colour than when obtained by fusing calcined fine metal, or roasting blue metal; it also contains a little more iron, but the reddish tint is caused by reduced copper diffused through it.

The slags from this process are stiff, and generally contain copper; the following is their average composition :—

Copper . . . . . 3·5
Silica . . . . . 60·4
Oxide of iron . . . 36·1
          ———
          100·0

These slags are fused with sulphuret ores, to obtain the copper from them.

It will be remarked, that the mixing of the Australian ores advances the mat obtained from the Cornish ores twenty-four hours; but it must also be borne in mind that the copper in the Australian ores is put back at least thirty-six hours by converting it into sulphuret.

Roasting.

The next operation to be described is that termed roasting; a process which, in chemical books, is generally identified with

calcining, but is distinct both in its mode and objects. The roasting furnace has already been described, page 346, vol. iv. The charge for an ordinary-sized furnace is about three tons. When the metal is brought to fusion, the air-holes of the furnace are all opened and a free current allowed to pass over the surface of the fused mass; the heat of the fire is then regulated so as to keep the charge in a sort of semifluid state. When blue metal is roasted, the action which occurs between the air and charge is similar to that which takes place in the calciner—the sulphur is oxidized and passes off as sulphurous acid; the iron with a little copper also oxidizes, forming scoria, which from time to time is skimmed off. These slags vary in composition according to the stage of the operation at which they are skimmed. At the beginning of the operation their average composition is—

Oxide of copper	8
Oxide of iron	49
Silica	37
Sulphur	3
Other oxides	3
	100

Those produced at the end of this process, just previous to the commencement of the true chemical reactions which occur in the process of roasting, furnished the following result :—

Oxide of copper	15
Oxide of iron	43
Silica	33
Sulphur	2
Oxygen and oxides	7
	100

This operation is termed the first roasting; the quantity of scoria from it amounts to about one-third of the weight of the metal put in. As this operation is usually continued twenty-four hours, the quality of the metal obtained varies exceedingly, and is distinguished by some peculiarity of appearance, as *pimpled metal, white metal,* or *regulus,* &c. The following analyses of a few of these varieties will illustrate our remarks :—

					By Le Play.
Copper	75·0	75	73	78	77·5
Sulphur	20·0	18	20	18	20·1
Iron	·7	4	5	2	2·2
Other metals, &c.	4·3	3	2	2	
	100·0	100	100	100	99·8

It will be observed that these compounds have the same general character, the principal differences consisting in the

quantity of iron and other metals; and until all the iron is removed, there is no proper roasting reaction. When this change is effected before the twenty-four hours allowed for the operation expires, the mat will be in the condition termed *sponge regulus*, and a quantity of copper reduced to the metallic state, which will be found at the bottom of the ingots or pigs, which are termed *copper bottoms*. This sponge regulus is composed as follows :—

					Le Play.
Copper	. .	80·5	79·5	80·0	81·1
Sulphur	. .	18·8	19·5	18·5	18·5
Iron	. .	·4	·5	·7	·2
		99·7	99·5	99·2	99·8

This composition is still very close to the formula $Cu\,2S$. It will be seen that the results obtained by roasting the blue metal are similar to those described when fusing calcined fine metal, and also by mixing calcined coarse metal with carbonates; so that whatever modification be adopted, they all meet at the stage of sponge regulus.

The next process is what is termed *roasting proper*, where the chemical reactions are different from those that take place in calcining, and where the copper is reduced to the metallic state.

About three tons of the regulus described is put into a roasting-furnace and fused as stated above. When the air-holes are all opened, so that a free current is allowed to pass over the surface, a brisk effervescence ensues over the surface of the fluid mass, much sulphurous acid is evolved, and a small quantity of scoria is formed, which is carefully removed before it accumulates. This scoria generally contains pieces of metallic copper. The average composition of these slags is—

Oxide of copper	. .	45
Oxide of iron	. .	25
Silica	28
Sulphur	2
		100

The chemical reactions which take place during this effervescence may be explained thus : if we take 1 equivalent of sub-sulphuret of copper, which is composed of 16 sulphur and 64 copper, and 2 equivalents of oxide of copper, which is composed of 16 oxygen and 64 copper—incidentally equal weights,—mix them together in a crucible and expose them to a heat sufficient to melt copper, the whole of the copper will be reduced, and the sulphur evolved as sulphurous acid,

$$Cu\,2S + 2CuO = SO^2 + 4Cu.$$
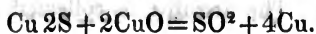
The reactions which occur in the process of roasting are the same. The oxygen of the air combines in the first place with a portion of the sulphur, forming sulphurous acid. A portion of the copper is also oxidized, and instantly reacts upon another portion of the subsulphuret, reducing the metal, as shown above. The process is a very beautiful one, and exhibits a nice adaptation of principles to practice. The sponge regulus has a specific gravity of 5, the reduced copper about 8; so that the copper sinks to the bottom where it is protected, and a new surface of regulus becomes exposed to the action of the air.

If the ore was pure, or if no *select* copper is required, the operation of roasting is continued until the whole of the copper is reduced, when it is tapped out into sand-moulds, forming *coarse copper, bed copper, pimpled copper,* or *blistered copper,* according to quality. The term *coarse copper* is applied occasionally to all these kinds except the blistered. If the ingot sets with contraction with a smooth hollow surface, it is termed *bed,* and generally indicates the presence of other metals, as tin. When the surface of the ingot is covered with pimples, it is termed *pimpled* copper, and indicates the presence of sulphur. When covered with large scales or blisters of oxide of copper, it is termed *blistered;* this only takes place when the copper is good and ready for refining. The following analyses of several of these kinds will illustrate our remarks :—

	Coarse copper.						Blistered copper.		
Copper	95·6	92·5	90·0	93·4	94·8	89·4	97·5	98·0	98·5
Iron	·3	1·2	1·4	2·4	2·0	2·	·7	·5	·8
Sulphur	·4	2·5	1·5	·6	·6	2·4	·2	·3	·1
Silica	·2	·4	2·6	·7	·3	2·4			
Tin and antimony.	2·1	2·	·3	·5	1·1	1·3	1·0	·7	
Lead	......	......	......	·5	......	......	......		
Oxygen and loss...	1·4	1·4	4·2	2·9	1·2	2·5	·6	·5	·6
	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0

All of these contained suboxide of copper dissolved in the metallic copper.

When the regulus contains other metals, such as tin, antimony, &c., the first portion of copper reduced reacts upon the sulphurets of these metals, reducing them, and the copper passes again into sulphuret; hence the first portion of reduced metal will contain the foreign metals. When making select copper, the roasting is carried on until about one-fourth of the copper in the regulus is reduced; the furnace is then tapped, and the reduced metal is obtained at the bottom of the first and second ingots or pigs, as *copper bottoms* which contain most of the metallic impurities. The regulus is collected and again roasted

which produces the purest metal the ordinary process of smelting can give; it is termed *best select.*

The bottoms are collected, and if not very impure, are roasted to oxidate the foreign metals, and an inferior quality of copper made from it termed *tile.* The composition of *copper bottoms* varies exceedingly. The following analysis is of a fair sample:—

Copper	74·0
Tin	13·8
Antimony	4·5
Lead	·8
Iron	2·5
Sulphur	3·9
	99·5

The following is from Le Play, but we have seldom met with any so impure; and with such bottoms, the regulus above would not be pure enough to make *select*:—

Copper	66·2
Tin	28·4
Iron	2·7
Arsenic	2·0
Sulphur	trace
	99·3

Copper bottoms are occasionally found at the bottom of the first pig in roasting blue metal; these are more impure than when selected ore is used, and is no doubt the sort given by Le Play. They are also found in fusing calcined coarse metal with Australian ores, and in fusing calcined fine metal. When best select is to be made from Cornish ores, the last process is considered best. The method of selecting is sometimes practised with inferior ores to obtain *tough cake* copper.

The next operation the copper is subjected to depends in a great measure upon the quality of the copper obtained from the roasting operation. If it be blistered, nothing further is required but refining; but if coarse copper, it must have another roasting. In some smelting-works it is put into the refining-furnace and there roasted before refining; in other works a separate furnace is used for this operation, in which it is brought to the quality of blister. The separate furnace is preferable, as it preserves the refining-furnace from being contaminated with impure metal, a prevalent source of annoyance. This roasting is done in the same manner as described for the regulus. About four tons of coarse copper form a charge. The high heat required to fuse the copper facilitates the scorifying of the alloy. The action may be defined thus:—the copper is oxidized by the air, which oxide

dissolves easily in the melted metal, and parts with its oxygen again to the alloy, forming scoria, which differs entirely from the other scoriæ, being composed principally of the silicate, oxide and metallic copper, with the oxides of the other metals. It forms upon the surface in tough irregular masses, which, when cooled, are covered over with crystals of oxide of tin resembling threads of glass, the composition of which is stated by Mr. Cameron to be four equivalents of oxygen to one of tin SnO^4 (Chemical Gazette for 1851, page 125). The slags contain generally about 60 per cent. of copper. The metal is freed considerably from its metallic impurities by this roasting.

The reader will not have failed to remark that the reduction and purifying of the copper are effected without the aid of carbonaceous matters of any sort, contrary to what is stated in all chemical books in reference to the reduction of copper; indeed the addition of carbonaceous matters would be detrimental.

During the roasting of copper there is a considerable loss of metal, owing to a portion of the oxide being carried away by the draft of the furnace in the form of fine powder. We have seen, where no culvert was connected with the furnace, and proper care not taken, the roof of the house round the chimney covered with suboxide of copper. Where culverts are used, this is nearly all saved; when obtained in the culverts, it is generally combined with silica and other matters. A portion fused as a cake from a culvert gave—

Silica and clay	60·2
Protoxide of iron	16·4
Sulphur	1·6
Lime	1·3
Tin and antimony	4·0
Oxide of copper	15·6
	99·1

A portion formed upon the inside of a stack as a fused incrustation, gave—

Insoluble	38·6
Oxide of copper	27·0
Sulphur	3·5
Sulphuric acid	3·4
Tin and antimony	4·5
Lead	2·8
Protoxide of iron	16·4
Lime	1·6
Manganese	1·3
Arsenic	·8
	99·9

The slags from this roasting are subjected to another operation. They are fused with a poor and highly sulphurous ore; the sulphur and iron of the ore combine with a portion of the copper in the slags, and form a rich coarse metal, having an impure alloy as *bottoms*, termed white metal, a highly crystalline alloy, the general composition of which is represented by the following analyses:—

	I.	II.
Copper	44·4	55
Tin	22·0	23
Antimony	2·3	3
Iron	28·0	17
Sulphur	3·1	2
	99·8	100

This alloy is refined by fusing it with an earthy mineral, composed principally of protoxide of iron and silica, containing a little copper and tin, which produces a compact alloy approaching speculum metal of an average composition of—

Copper	63
Tin	27
Antimony	5
Iron	4
Sulphur	1
	100

This white metal is used for making buttons and such articles as are cast, and is sold for little more than the value of the copper it contains. Any process that would separate the tin easily from the copper, and make it marketable, would be of importance to the smelter.

[To be continued.]

LVIII. *On a Method of Transforming Equations by means of Symmetric Functions.* By G. B. JERRARD*.

Introduction.

1. IN my Mathematical Researches (p. 20), I have shown that if the roots of the equations

$$x^m + A_1 x^{m-1} + A_2 x^{m-2} + \ldots + A_m = 0,$$
$$y^m + A'_1 y^{m-1} + A'_2 y^{m-2} + \ldots + A'_m = 0,$$

be so related that

$$y = Px^\alpha + Qx^\beta + Rx^\gamma + \ldots + Lx^\lambda;$$

* Communicated by the Author.

or rather that

$$y_1 = Px_1^\alpha + Qx_1^\beta + Rx_1^\gamma + \ldots + Lx_1^\lambda,$$

$$y_2 = Px_2^\alpha + Qx_2^\beta + Rx_2^\gamma + \ldots + Lx_2^\lambda,$$

$$\cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot$$

$$y_m = Px_m^\alpha + Qx_m^\beta + Rx_m^\gamma + \ldots + Lx_m^\lambda;$$

where $x_1, x_2, \ldots x_m$ are the roots of $x^m + A_1 x^{m-1} + \ldots = 0$, and $y_1, y_2, \ldots y_m$ of $y^m + A'_1 y^{m-1} + \ldots = 0$; the equation in y will take the form

$$y^m$$

$$- \mathfrak{S} . (\alpha P + \beta Q + \gamma R + \ldots + \lambda L) y^{m-1}$$

$$+ \frac{1}{1 . 2} \mathfrak{S} . (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^2 y^{m-2}$$

$$- \frac{1}{1 . 2 . 3} \mathfrak{S} . (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^3 y^{m-3}$$

$$\cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot$$

$$(-1)^m \frac{1}{1 . 2 \ldots m} \mathfrak{S} . (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^m = 0, \quad (a)$$

$\alpha, \beta, \gamma, \ldots \lambda$ being any integers. I have here substituted $\mathfrak{S}$ for $\int$, thinking it desirable that the letter $\int$ should be set apart as the symbol of integration.

The coefficients of this equation are, as I proceed to show, merely condensed modes of expression formed, in accordance with a well-known method of notation, by subjecting $\mathfrak{S}$, a symbol of operation to the same laws, with certain obvious limitations, as would obtain if $\mathfrak{S}$ were a symbol of quantity. Thus by

$$\mathfrak{S} . (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)$$

is represented

$$\mathfrak{S}\alpha \; P + \mathfrak{S}\beta \; Q + \mathfrak{S}\gamma \; R + \ldots + \mathfrak{S}\lambda \; L;$$

$\mathfrak{S}\alpha, \mathfrak{S}\beta, \ldots \mathfrak{S}\lambda$ being the sums of the αth, βth, .. λth powers respectively of the m quantities $x_1, x_2, \ldots x_m$. And, in general, if we expand

$$(\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^n$$

in precisely the same manner as we should do if we were operating on an ordinary algebraical expression, and then apply the symbol $\mathfrak{S}$ to each term, bearing in mind that $\alpha, \beta, \gamma, \ldots \lambda$ are the sole elements of the functions thus characterized by $\mathfrak{S}$, we shall arrive at the expression indicated by

$$\mathfrak{S} . (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^n.$$

The expansion in question will accordingly consist of as many terms of the form

$$\frac{1 . 2 \ldots n}{1 . 2 \ldots a \times 1 . 2 \ldots b \times \ldots \times 1 . 2 \ldots l} \; \mathfrak{S}\alpha^a \beta^b \ldots \lambda^l \; P^a Q^b \ldots L^l$$

as there are different solutions, in positive integers, of the equation

$$a + b + \ldots + l = n.$$

$\mathfrak{S}\alpha^a\beta^b\ldots\lambda^l$ is the symmetric function of which $x_1^a . x_2^a \ldots x^a \times x_{1'}^\beta, x_{2'}^\beta, \ldots x_b^\beta \times \ldots$ is a term. As for the number of terms in $\mathfrak{S}\alpha^a\beta^b\ldots\lambda^l$, it is wholly unaffected by the equalization of the elements of that function, $\alpha, \alpha, \ldots \beta, \beta, \ldots \lambda, \lambda, \ldots$. For example, if we take the very simple function of the 2nd order $\mathfrak{S}\alpha\beta$, supposing it to be symmetric with respect to two quantities t and u, we shall have

$$\mathfrak{S}\alpha\beta = t^\alpha u^\beta + u^\alpha t^\beta ;$$

from which, on putting $\beta = \alpha$, there will be derived

$$\mathfrak{S}\alpha^2 = t^\alpha u^\alpha + u^\alpha t^\alpha.$$

$\mathfrak{S}\alpha^2$ is thus composed of as many terms as $\mathfrak{S}\alpha\beta$. And a similar mode of derivation is supposed to extend to every order in the present system of symmetric functions*.

2. It is not difficult to demonstrate the truth of equation (a).

In effect, from a well-known property of equations, the coefficient of y^{m-n} in the transformed equation in y will be at once seen to be expressible by

$$(-1)^n \frac{1}{1.2 \ldots n} \Sigma y_1 y_2 y_3 \ldots y_n ;$$

if we use the symbol Σ in an equally extended sense with $\mathfrak{S}$, that is to say, if we suppose $\Sigma y_1 y_2 y_3 \ldots y_n$ to be derivable from $\Sigma y_1^a y_2^b y_3^c \ldots y_n^l$ without any diminution in the number of its terms, on taking $a = b = c = \ldots = l = 1$.

But, according to equation (a), the coefficient of y^{m-n} in the transformed equation will be

$$(-1)^n \frac{1}{1.2 \ldots n} \mathfrak{S} . (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^n ;$$

n being any integer less than $m + 1$.

Nothing therefore remains but to show, that if

$$y_\tau = P x_\tau^\alpha + Q x_\tau^\beta + R x_\tau^\gamma + \ldots + L x_\tau^\lambda,$$

where τ may have any one indifferently of the m values $1, 2, 3 \ldots m$

---

* In the systems of symmetric functions hitherto in use among mathematicians, $t^\alpha u^\alpha$, not $t^\alpha u^\alpha + u^\alpha t^\alpha$, would in the case we have been considering have the same characteristic as $t^\alpha u^\beta + u^\alpha t^\beta$. But had we thus *insulated* those symmetric functions in which there are equal elements, we should not have arrived at a theorem in which the separation of the symbol $\mathfrak{S}$ from its subjects could have taken place.

assigned to it, there will result

$$\Sigma y_1 y_2 y_3 \ldots y_n = \mathfrak{S} \cdot (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^n;$$

a theorem, the truth of which is admitted at the present day*.

3. Thus we perceive that in the problem of the transformation of equations, however vast it may be, we shall merely have to consider functions of the class

$$\mathfrak{S} \cdot (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^n,$$

if we take the very simple function of the 2nd order $\mathfrak{S} x_1^\alpha$, suppose.

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* The proof which I gave of it in my Mathematical Researches about twenty years ago was nearly as follows :—

Let us, in the first place, suppose that

$$R = 0, \ldots L = 0.$$

The expression for $\Sigma y_1 y_2 \ldots y_n$ will thus become

$$\Sigma (P x_1^\alpha + Q x_1^\beta)(P x_2^\alpha + Q x_2^\beta) \ldots (P x_n^\alpha + Q x_n^\beta).$$

This function is, we perceive, of n dimensions relatively to P and Q ; we see also that an αth power of each of the n quantities $x_1, x_2, \ldots x_n$ is successively joined to P, and a βth power to Q ; hence we are led to conclude that

$$\Sigma (P x_1^\alpha + Q x_1^\beta)(P x_2^\alpha + Q x_2^\beta) \ldots (P x_n^\alpha + Q x_n^\beta)$$
$$= v_0 \mathfrak{S} \alpha^n P^n + v_1 \mathfrak{S} \alpha^{n-1} \beta P^{n-1} Q$$
$$+ v_2 \mathfrak{S} \alpha^{n-2} \beta^2 P^{n-2} Q^2 + \ldots + v_n \mathfrak{S} \beta^n Q^n;$$

$v_0, v_1, v_9, \ldots v_n$ being certain constant but unknown quantities.

Now in order to determine these, let

$$\alpha = 0, \quad \beta = 0 ;$$

we shall then have

$$\Sigma (P + Q)^n x_1^0 x_2^0 \ldots x_n^0 =$$
$$\mathfrak{S} 0^n (v_0 P^n + v_1 P^{n-1} Q + v_2 P^{n-2} Q^2 + \ldots + v_n Q^n).$$

And since, in general,

$$\Sigma (P + Q)^n x_1^0 x_2^0 \ldots x_n^0 = (P + Q)^n \Sigma x_1^0 x_2^0 \ldots x_n^0$$
$$= (P + Q)^n \mathfrak{S} 0^n,$$

there will result, on making the requisite substitution,

$$(P + Q)^n = v_0 P^n + v_1 P^{n-1} Q + v_2 P^{n-2} Q^2 + \ldots + v_n Q^n.$$

Thus $v_0, v_1, v_2, \ldots v_n$ are respectively equal to $1, n, \dfrac{n(n-1)}{1 \cdot 2}, \ldots 1,$ the coefficients of the development of $(P + Q)^n$.

If, therefore, we introduce the equation of definition

$$\mathfrak{S} \cdot (\alpha P + \beta Q)^n = \mathfrak{S} \alpha^n P^n + n \mathfrak{S} \alpha^{n-1} \beta P^{n-1} Q$$
$$+ \frac{n(n-1)}{1 \cdot 2} \mathfrak{S} \alpha^{n-2} \beta^2 P^{n-2} Q^2 + \ldots + \mathfrak{S} \beta^n Q^n,$$

we shall finally obtain

$$\Sigma (P x_1^\alpha + Q x_1^\beta)(P x_2^\alpha + Q x_2^\beta) \ldots (P x_n^\alpha + Q x_n^\beta) = \mathfrak{S} \cdot (\alpha P + \beta Q)^n.$$

And from this we can ascend without difficulty to the general form in which R, ... L are undetermined and arbitrary.

the development of which can be effected with all the facility which attaches to the use of the multinomial theorem. If, for example, we wish to expand the function in question according to the descending powers of Q, we shall instantly find

$$\mathfrak{S} \cdot (\alpha P + \beta Q + \gamma R + \ldots + \lambda L)^n =$$
$$\mathfrak{S}\beta^n \ Q^n +$$
$$n\mathfrak{S}\beta^{n-1} \cdot (\alpha P + \gamma R + \ldots + \lambda L)Q^{n-1} +$$
$$\frac{n(n-1)}{1.2} \mathfrak{S}\beta^{n-2} \cdot (\alpha P + \gamma R + \ldots + \lambda L)^2 Q^{n-2} +$$
$$+ \mathfrak{S} \cdot (\alpha P + \gamma R + \ldots + \lambda L)^n.$$

I shall now, pursuing the method given in my Mathematical Researches, examine anew some of the consequences which flow from equation (*a*).

PROBLEM I.—*To take away the second, third, and fourth terms at once from the general equation of the* m*th degree.*

4. It appears from equation (*a*), that in order that A'_1, A'_2, A'_3, the coefficients of the 2nd, 3rd, and 4th terms of the transformed equation in y, may vanish simultaneously when

$$y = P + Q + Rx^2 + \ldots + Lx^\lambda,$$

we must find such values of P, Q, R, .. L as will satisfy the equations

$$\mathfrak{S} \cdot (0P + 1Q + 2R + \ldots + \lambda L) = 0,$$
$$\mathfrak{S} \cdot (0P + 1Q + 2R + \ldots + \lambda L)^2 = 0,$$
$$\mathfrak{S} \cdot (0P + 1Q + 2R + \ldots + \lambda L)^3 = 0.$$

Now we know from the theory of elimination, that in fulfilling these three conditions we shall, in general, be conducted to a final equation of $1 . 2 . 3$ or 6 dimensions. Here, then, we seem to be stopped. But when the series for y rises to the fourth power of x, the difficulty may be eluded in the following manner.

Mode of solution when $\lambda = 4$.

5. Since in the equation for x we are permitted to assume $A_1 = 0$, $A_2 = 0$, it is clear that $\mathfrak{S}1$ and $\mathfrak{S}1^2$ may both of them be made to vanish. Hence on considering that the first two of the equations of condition may, when $\lambda = 4$, take the forms

$$\mathfrak{S}1 \ Q +$$
$$\mathfrak{S} \cdot (0P + 2R + 3S + 4T) = 0,$$

and
$$\mathfrak{S}1^2 \ Q^2 +$$
$$2\mathfrak{S}1 \cdot (0P + 2R + 3S + 4T)Q +$$
$$\mathfrak{S} \cdot (0P + 2R + 3S + 4T)^2 = 0,$$

we shall perceive that Q may be completely detached from both these equations if we determine P, R, S, T so that

$$\mathfrak{S} . (0P + 2R + 3S + 4T) = 0,$$
$$\mathfrak{S}1 . (0P + 2R + 3S + 4T) = 0,$$
$$\mathfrak{S} . (0P + 2R + 3S + 4T)^2 = 0,$$

where the product of the numbers which mark the dimensions relatively to P, R, S, T is only $1 . 1 . 2$ or 2.

In this way, without resolving any equation of a higher degree than the second, we shall have

$$A'_1 = 0Q + 0,$$
$$A'_2 = 0Q^2 + 0Q + 0.$$

And Q, which as yet, therefore, is wholly undetermined, may now satisfy the cubic equation

$$\mathfrak{S} . (0P + 1Q + 2R + 3S + 4T)^3 = 0;$$

or rather

$$\mathfrak{S}1^3 \ Q^3 +$$
$$3\mathfrak{S}1^2 . (0P + 2R + 3S + 4T)Q^2 +$$
$$3\mathfrak{S}1 . (0P + 2R + 3S + 4T)^2 Q +$$
$$\mathfrak{S} . (0P + 2R + 3S + 4T)^3 = 0;$$

thus fulfilling the third and last condition

$$A'_3 = 0.$$

6. It may easily be shown that P, Q, R, S, T, y will none of them assume the form $\dfrac{G}{(1-1)H}$, where G and H are integral functions of the $(m-2)$ arbitrary coefficients $A_3, A_4, .. A_m$.

Returning to the equations in P, R, S, T, we see that the first of them will be reducible to

$$\mathfrak{S} . (0P + 3S + 4T) = 0,$$

since $\mathfrak{S}2 = 0$. We may also perceive that the second equation of the group will become

$$\mathfrak{S} . (3R + 4S + 5T) = 0.$$

For, since in general $\mathfrak{S}\tau v = \mathfrak{S}\tau \mathfrak{S}v - \mathfrak{S}(\tau + v)$, it is evident that the coefficients of P, R, S, T, in

$$\mathfrak{S}1 . (0P + 2R + 3S + 4T)$$

may all of them be derived from the expression

$$\mathfrak{S}1\mathfrak{S}v - \mathfrak{S}(1 + v),$$

on taking v successively equal to $0, 2, 3, 4$; whence

$$\mathfrak{S}1 . (0P + 2R + 3S + 4T) = -\mathfrak{S} . (3R + 4S + 5T),$$

$\mathfrak{S}1$ being equal to zero.

Accordingly we shall have

$$P = -\frac{1}{\mathfrak{S}0}\, \mathfrak{S} . (3S + 4T),$$

$$R = -\frac{1}{\mathfrak{S}3}\, \mathfrak{S} . (4S + 5T).$$

We conclude, therefore, since we may assume $T = 1$, that neither P nor R will be of the form $\dfrac{G}{(1-1)H}$, unless S be of that form.

Substituting now these expressions for P and R in the equation

$$\mathfrak{S} . (0P + 2R + 3S + 4T)^2 = 0,$$

the first member of which is an integral function of P, R, S, T, we shall obviously be conducted to a quadratic equation in S with determinate coefficients. This equation I shall represent by

$$\alpha S^2 + 2\beta ST + \gamma T^2,$$

α, β, γ being certain rational functions of $A_3, A_4, \ldots A_m$. The expression for S will consequently be

$$S = \frac{-\beta \pm \sqrt{\beta^2 - \alpha\gamma}}{\alpha}\, T.$$

Unless, then, α be equal to zero, S will not take the form $\dfrac{G}{(1-1)H}$.

Now since, in order to obtain α, we need not consider the whole development of the function $\mathfrak{S} . (0P + 2R + 3S + 4T)^2$, but only that part of it which is affected with S^2, it is clear that if we assume

$$P = pS + p',$$
$$R = rS + r',$$

assigning to p, p', r, r' such values as are deducible from the expressions previously found for P and R, we shall obtain α by merely writing p and r for P and R respectively, 1 for S, and suppressing the term 4T. Hence

$$\alpha = \mathfrak{S} . (0p + 2r + 3s)^2,$$

where

$$p = -\frac{\mathfrak{S}3}{\mathfrak{S}0}, \quad r = -\frac{\mathfrak{S}4}{\mathfrak{S}3}, \quad s = 1.$$

Further, if we expand $\mathfrak{S} . (0p + 2r + 3s)^2$ according to the descending powers of s, the expression for α will become

$$\mathfrak{S}3^2\, s^2 + 2\mathfrak{S}3 . (0p + 2r)s + \mathfrak{S} . (0p + 2r)^2 ;$$

or since $\mathfrak{S}0 = m$, $\mathfrak{S}2 = 0$,

$$\{(\mathfrak{S}3)^2 - \mathfrak{S}6\}s^2 +$$
$$2\{(m-1)\mathfrak{S}3\, p + (-\mathfrak{S}5)r\}s +$$
$$m(m-1)p^2 + (-\mathfrak{S}4)r^2 ;$$

from which, on eliminating p, r, s, there will finally result

$$\alpha = \frac{1}{m} (\mathfrak{S}3)^2 + 2 \frac{\mathfrak{S}4\mathfrak{S}5}{\mathfrak{S}3} - \frac{(\mathfrak{S}4)^3}{(\mathfrak{S}3)^2} - \mathfrak{S}6 ;$$

α cannot therefore vanish without inducing a relation among the coefficients A_3, A_4, ... A_m.

Having thus shown that S will not assume the form $\dfrac{G}{(1-1)H}$, it immediately follows that not only P and R, but also Q and y will in general be determinate in value.

I proceed now to show how the problem may be solved when the series for y does not extend beyond the third power of x.

Mode of solution when $\lambda = 3$.

7. Here $L = S$. The equations of condition will accordingly become

$$\left. \begin{array}{l} \mathfrak{S} . (0P + 1Q + 2R + 3S) = 0, \\ \mathfrak{S} . (0P + 1Q + 2R + 3S)^2 = 0, \\ \mathfrak{S} . (0P + 1Q + 2R + 3S)^3 = 0. \end{array} \right\} \quad . \quad . \quad . \quad (e)$$

Assuming, as before, $A_1 = 0$, $A_2 = 0$, let

$$\mathfrak{S}1 . (0P + 2R + 3S) = \mathfrak{D} * ;$$

then, on eliminating P and R, the second of the equations (e) will present itself in the form

$$2\mathfrak{D}Q + \alpha S^2 + 2b\mathfrak{D}S + c\mathfrak{D}^2 = 0 ; \quad . \quad . \quad . \quad (e'_2)$$

in which $\mathfrak{D}$ is indeterminate, and b, c are rational functions of A_3, A_4, ... A_m, the coefficients of the equation in x.

It may be proved very readily that both b and c will in general be different from zero.

In effect, if we observe that

$$\mathfrak{S} . (0P + 1Q + 2R + 3S)^2 =$$
$$0Q^2 + 2\mathfrak{D}Q + \mathfrak{S} . (0P + 2R + 3S)^2,$$

we shall find, if $P = pS + p_l\mathfrak{D}$, $R = rS + r_l\mathfrak{D}$,

$$\alpha = \mathfrak{S} . (0p + 2r + 3s)^2,$$
$$b = \mathfrak{S} . (0p + 2r + 3s)(0p_l + 2r_l),$$
$$c = \mathfrak{S} . (0p_l + 2r_l)^2 ;$$

p, r, s being already known, and p_l, r_l being respectively equal to 0, $-\dfrac{1}{\mathfrak{S}3}$.

Whence it appears, that, unless certain assignable relations exist among the coefficients of the equation in x, both b and c will be composed of finite non-evanescent factors.

* $\mathfrak{D}$ is the Hebrew letter Mēm.

Reverting to equation (e_2'), let us now take

$$Q = \mu\mathfrak{D} + \sigma S + q,$$

and we shall have, on eliminating Q,

$$2\mathfrak{D}q + \alpha S^2 + 2b'\mathfrak{D}S + c'\mathfrak{D}^2 = 0 ; \quad \ldots \quad (e_2'')$$

an equation which is of the same form indeed as the preceding one designated by (e'_2), but which involves two additional indeterminate quantities μ and σ. In effect

$$b' = b + \sigma, \quad c' = c + 2\mu.$$

We see, then, that equation (e_2'') may be reduced to the binomial form

$$2\mathfrak{D}q + \alpha S^2 = 0, \quad \ldots \quad \ldots \quad (e_2''')$$

by assigning such values to σ and μ as will make b' and c' vanish.

Again, if we eliminate P, R, and Q from the third of the equations (e), we shall arrive at an homogeneous equation of the third degree relatively to $\mathfrak{D}$, q, and S,

$$F(\mathfrak{D}, q, S)^3 = 0, \quad \ldots \quad \ldots \quad \ldots \quad (e'_3)$$

where F is expressive of a rational and integral function.

It only therefore remains to satisfy the simultaneous equations (e_2'''), (e'_3).

Now if, assuming $S = 1$, we designate by

$$\mathfrak{D}^6 + B_1\mathfrak{D}^5 + B_2\mathfrak{D}^4 + \ldots + B_6 = 0,$$

the final equation in $\mathfrak{D}$, and by

$$q^6 + C_1 q^5 + C_2 q^4 + \ldots + C_6 = 0,$$

that in q; we may without difficulty perceive that

$$B_n = C_n,$$

n being equal to any number in the series 1, 2, 3, 4, 5, 6.

For let

$$q^{-1} = \dot{q}, \quad -\frac{2}{\alpha}\mathfrak{D} = \dot{\mathfrak{D}};$$

then, since by equation (e_2''')

$$\dot{q} = \dot{\mathfrak{D}},$$

or rather

$$\dot{q}_n = \dot{\mathfrak{D}}_n,$$

it follows that the first members of the equations

$$\dot{q}^6 + \frac{C_5}{C_6}\dot{q}^5 + \frac{C_4}{C_6}\dot{q}^4 + \ldots + \frac{C_0}{C_6} = 0,$$

$$\dot{\mathfrak{D}}^6 - \frac{2B_1}{\alpha}\dot{\mathfrak{D}}^5 + \frac{2^2 B_2}{\alpha^2}\dot{\mathfrak{D}}^4 - \ldots + \frac{2^6 B_6}{\alpha^6} = 0,$$

must be identical. Now in order that the corresponding coefficients in these equations may be equal, they must all of them be

comprised in the expression

$$\frac{C_\nu}{C_6} = \left(\frac{-2}{\alpha}\right)^{6-\nu} B_{6-\nu},$$

on taking ν successively equal to 5, 4, 3, 2, 0. But $\supset$ and q are involved symmetrically in equation (e_2'''). There must accordingly exist a parallel system of conditions derivable from the equation

$$\frac{B_\nu}{B_6} = \left(\frac{-2}{\alpha}\right)^{6-\nu} C_{6-\nu}.$$

Hence it is manifest that the roots of the final equation in $\supset$ or q will be expressible by

$$q_1, q_2, q_3, \quad \supset_1, \supset_2, \supset_3,$$

while

$$q_n \supset_n = -\frac{1}{2}\alpha;$$

and that consequently, P, Q, R may all of them be determined by resolving equations of the first, second, and third degrees[*].

Long Stratton, Norfolk,
    January 14, 1853.

                        [To be continued.]

LIX. *On the Intensity and Quantity of Electric Currents.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

PERHAPS at the present time a translation of the terms intensity and quantity of electricity into their equivalents in the theory of resistances may not be less acceptable to some of your readers than it has been to me. I shall be glad if the following remarks appear to you of sufficient value for insertion in the Philosophical Magazine.

I am, your obedient Servant,

                        H. BENCE JONES.

Suppose that a hydro-electric and a thermo-electric current produce the same deflection of the same magnetic needle, and that an equal length of wire is then introduced into both circuits. The hydro-electric current and the additional wire may be so selected that no change in the effect of the current on the needle will become apparent, whereas the thermo-electric current will be considerably weakened.

Again, suppose that instead of the wire, a voltameter is intro-

duced into both circuits. By suitable arrangements the hydro-electric current may electrolyse a large quantity of water in a given time, whereas the thermo-electric current will not evolve a single bubble of gas, however long the action be continued.

Perfectly similar phænomena may be observed on comparing two hydro-electric currents, one of which arises from a battery of a great many small elements, the other from a battery of only a few large cells. The electro-magnetic and the electro-chemical effect of both may be exactly the same, or even the latter may prove to be the stronger when thus tested; and yet on submitting the human body to the former current it may experience a powerful shock, whereas on submitting it to the latter one there will not be the slightest perception of pain.

Puzzled by these contradictory results, electricians had recourse to the hypothesis of two distinct qualities, which they assumed to belong to every current, namely *intensity* and *quantity*. They admitted that the electro-magnetic and the electro-chemical action of the current gave the measure of the so-called quantity of electricity, and the amount of specific power to overcome any obstacle brought into its path (in the above-mentioned experiments, the additional length of wire, the voltameter or the human body) was designated by the term intensity. Thus it was believed that two currents of equal quantity might possess different degrees of intensity, and the reverse; just as two rays of light of the same colour may differ in their intensity.

By arbitrarily assuming in each particular case that the two qualities of the current were blended together in different proportions, it was easy to explain the differences which were observed when different currents were examined in the same circumstances, or the same current in different circumstances. The greatest intensity, and, in proportion to it, the least quantity, were generally ascribed to the currents of the electrical machine and of Zamboni's pile. On the other hand, the least intensity, and, in proportion to it, the greatest quantity, were ascribed to thermo-electric currents. In the hydro-electric and magneto-electric currents the quantity of electricity was said to increase with the size of the elements of the battery, or with the thickness of the wire of the coils; its intensity, on the contrary, was said to increase with the number of the elements put into action, or with the number of the windings of the coil of wire.

It need hardly be observed, that in the above-mentioned experiments the thermo-electric current and the current produced by the battery, consisting of a few large cells, play the part of currents possessing little intensity and great quantity; whilst the current of a battery of a great many small elements is supposed to be endowed with great intensity and little quantity.

In Ohm's theory such suppositions are not required.

According to this theory, two currents can only differ in one respect, that is, in their strength ; and this strength is measured by their effect on the needle, and by their electrolytic action during a unit of time.

The strength of the current in Ohm's theory is also called intensity, and it is supposed to be proportional to the quantity of electricity which passes through any cross section of the circuit during the unit of time.

This meaning of the word intensity, and the previous use of the same word, must be carefully distinguished. Ohm's so-called intensity agrees rather with the meaning of the term quantity, as above used, than with that of intensity.

According to Ohm, the intensity or strength of a current depends upon two circumstances. First, it is directly proportional to the electromotive force which gives rise to the current, and it is indifferent whether this arises from chemical action or from any other cause; secondly, it is inversely proportional to the resistance in the whole circuit, this resistance being the sum of the resistances in all parts of the circuit*. Hence the greater the resistance of the circuit already is, the less will the intensity of the current be diminished by any given increase of the resistance.

By means of this simple view it is easy to explain all the phæ-nomena for which the hypothesis of different proportions in the intensity and quantity of the currents was assumed. To make this more easily understood, it is convenient to divide the resistance of every circuit into two parts.

One part of the resistance belongs to that part of the circuit in which the current is generated, that is, within the battery itself. As this part of the circuit is indispensably necessary to the production of the current, the corresponding part of the resistance may therefore be called the internal or necessary resistance. The other part of the resistance belongs to that part of the circuit which is not essential to the production of the current, and which is generally considered as acted upon by the current; as, for example, the galvanometer wire or the volta-meter which may be introduced into the circuit of the above-mentioned battery. This part of the resistance may be called the external or accidental resistance.

By comparing the necessary resistance in different kinds of circuits, it may be easily ascertained that all those currents to which a considerable intensity in proportion to the quantity has been attributed, arise from *sources* of great necessary resistance. Indeed in the electrical machine the necessary resistance is almost

* See Phil. Mag. S. 4. vol. iii. p. 324.

infinite. In Zamboni's pile it is extremely great. In hydro-electric and magneto-electric circuits, intended to give so-called intense currents, the number of the elements or of the windings of the coil must be augmented, the necessary resistance of course increasing in the same proportion.

On the contrary, all those currents to which a moderate or a small intensity, in proportion to the quantity, has been attributed, arise from *sources* of small necessary resistance. This is the case, for example, with thermo-electric currents. And in order to increase the quantity of hydro-electric, or magneto-electric currents in proportion to their intensity, the size of the cells in the first case, or the diameter of the wire in the other case, must be increased.

If two circuits are taken of very unequal necessary resistances with electromotive forces differing in the same proportion as the necessary resistances, whilst the accidental (external) resistances are imperceptible or also proportional to the necessary resistances, then the intensity (strength) of the currents in these two circuits will be exactly the same, and will remain so as long as the electromotive force, the necessary and the accidental resistances, vary in the same proportion. This is exemplified in the case first mentioned, in which a hydro-electric and a thermo-electric current produce the same effect on the magnetic needle. This occurs notwithstanding the greater necessary resistance of the hydro-electric circuit, because of its greater electromotive force. That the electromotive force is greater is easily demonstrated by making the currents from the two sources oppose one another in the same circuit, when the current of the hydro-electric source will be paramount. It is evident that in this case the sum of the resistances which each current has to overcome is the same.

Thus, according to Ohm's theory, two currents, although produced by sources of different electromotive force, may be quite identical in their action, provided the resistances of the two circuits bear to each other the same proportion as the two electromotive forces. It will, however, be found that an immense difference exists in two such circuits whenever the proportion of the resistances is changed, or whenever an additional resistance is added to both circuits. When the necessary resistance of one of the circuits is very large, a considerable additional resistance may be added to it without any great change in the whole sum of the resistances being produced; accordingly the current will not experience any considerable loss of intensity, the magnetic needle will be deflected almost as much as it was before, and a conducting body introduced into the circuit will be acted upon with energy. In short, the current arising from a source of great necessary resistance will seem to overcome easily the

additional resistance brought into its path.   It will appear to be an intense current according to the older mode of expression.

On the other hand, when the same conducting body is introduced into a circuit of small necessary resistance, the whole sum of the resistances of the circuit may be greatly increased by the addition of the fresh resistance to the original necessary resistance; and as a consequence, the current will be diminished in the same proportion.   The magnetic needle will return to the zero-point, and the remaining current will produce no effect on the conducting body.   In short, the current arising from a source of small necessary resistance will appear unable to overcome the additional resistance brought into its path.   We have here a current of small intensity according to the older mode of expression.

If it be now granted that Ohm's theory gives an explanation of the phænomena which heretofore have been explained by the hypothesis of two different qualities in the electric current, then as it is one of the first and most important principles of inductive science not to assume two causes for any effect when one is sufficient, and not to assume without the most urgent necessity the existence of new qualities, it appears desirable to lay aside the older hypothesis of intensity and quantity, and to adopt instead Ohm's theory of sources of electricity having different degrees of necessary resistance.

LX. *A Proof that all the Invariants to a cubic Ternary Form are Rational Functions of* Aronhold's *Invariants and of a cognate theorem for biquadratic Binary Forms.   By* J. J. SYLVESTER, *F.R.S.*

NOW let us proceed to Aronhold's famous S and T, the invariants to the general cubic function $(x, y, z)^3$, forms equally dear to the analyst and geometer. (Vide Mr. Salmon's Higher Plane Curves *passim*.)

The method will be precisely the same as that applied to s and t*.   We commence with the canonical form

$$x^3 + y^3 + z^3 + 6m\,xyz.$$

On substituting $x+y+z$, $x+\rho y+\rho^2 z$, $x+\rho^2 y+\rho z$ for x, y, z, where ρ is the cube root of unity, the above quantity takes the form

$$(3+6m)(x^3+y^3+z^3+6\beta(m)\,.\,xyz),$$

where

$$\beta(m) = \frac{186-18m}{6(3+6m)} = \frac{1-m}{1+2m},$$

---

* The s is Mr. Cayley's property, the t belongs to Professor Boole, having been by him imparted, in the infancy of the theory, to Mr. Cayley, by whom it was first given to the world, at least in its character as an Invariant.

a periodic function in (m) of the second order only for

$$\beta^2 m = \frac{1+2m-1+m}{1+2m+2-2m} = m.$$

But if we write for x in the original form ρx, it becomes

$$x^3 + y^3 + z^3 + 6\rho m xyz;$$

and if for x we write $\rho^2 x$, it becomes

$$x^3 + y^3 + z^3 + 6\rho^2 m xyz.$$

Hence we can by linear substitutions obtain from $x^3 + y^3 + z^3 + 6mxyz$ the three additional forms

$$x^3 + y^3 + z^3 + 6\beta(m)xyz,$$
$$x^3 + y^3 + z^3 + 6\gamma(m)xyz,$$
$$x^3 + y^3 + z^3 + 6\delta(m)xyz,$$

where

$$\beta(m) = \frac{1-m}{1+2m}, \quad \gamma(m) = \rho^2 \frac{1-\rho m}{1+2\rho m} = \frac{\rho^2 - m}{1+2\rho m},$$

$$\delta(m) = \rho \frac{1-\rho^2 m}{1+2\rho^2 m} = \frac{\rho - m}{1+2\rho^2 m}.$$

In all, there will be twelve values of m forming three remarkable compound cycles,

$$m, \quad \beta(m), \quad \gamma(m), \quad \delta(m),$$
$$\rho m, \quad \rho\beta(m), \quad \rho\gamma(m), \quad \rho\delta(m),$$
$$\rho^2 m, \quad \rho^2\beta(m), \quad \rho^2\gamma(m), \quad \rho^2\delta(m).$$

It would be beside my present object to seek to develope fully the functional relations in which the several terms of these cycles stand to one another: the interesting relations

$$\beta^2(m) = \gamma^2(m) = \delta^2(m) = m$$
$$\beta\gamma(m) = \gamma\beta(m) = \delta(m)$$
$$\gamma\delta(m) = \delta\gamma(m) = \beta(m)$$
$$\delta\beta(m) = \beta\delta(m) = \gamma(m)$$

have been already stated by me in another place (Camb. and Dub. Math. Journ., March 1851*).

The (S) of the canonical form corresponding to the S of the general form is $m - m^4$; and the (T) corresponding to the T of the general form is $1 - 20m^3 - 8m^6$. (See my Calculus of Forms, Camb. and Dub. Math Journ., Feb. 1852.) It is my object to show that any other invariant (I) to the canonical form must be a rational function of S and T.

In the first place, I observe that every invariant to any function of an odd degree i of any odd number q of variables must

be of even dimensions; for if the degree of the dimensions be (q), and D the determinant of the coefficients of substitution, the invariant to the transform becomes the original invariant affected with a factor $D^{\frac{iq}{\rho}}$, where $\frac{iq}{\rho}$ must be an even integer, since otherwise the sign of this multiplier would be equivocal and indeterminable; hence when i and ρ are both odd, q must be even. Thus, then, $I(m)$ in the case before us must be an even-degreed function of m. Moreover, since the change of x into ρx converts m into ρm, and $I_q(m)$ into $\rho^q(m)$ [for D becomes ρ when x, y, z become ρx, y, z], $I_q(m)$ must be of the form $\phi(m^3)$, $m^2\phi(m)^3$, $m^3\phi(m)^3$, according as the index (q) is of the form $6i$, $6i+2$, $6i+4$.

By precisely the same reasoning as was applied to the preceding case of (s) and (t), we see that any invariant of m which contains m^c must also contain $(1-m)^c$, $(1-\rho m)^c$, $(1-\rho^2 m)^c$, *i. e.* must contain $(m-m^4)^c$, which in fact is $(S)^c$. If, now, we consider any invariant of the qth degree in (m) $I(m)$, and suppose it to be other than a rational function of (S) and (T), and if we take (μ) to denote the number of the solutions of $4x+6y=q$, it will follow that we may form an invariant $I'(m)$, which, when q is of the form $12i$ or $12i+6$, will contain m, and consequently $(m-m^4)^{3\mu+2}$ as a factor; and in like manner when q is of the form $12i+2$ or $12i+8$, will contain $(m-m^4)^{3\mu+2}$ as a factor; and when q is of the form $12i+4$ or $12i+10$ will contain $(m-m^4)^{3\mu+1}$ as a factor. Now

when
$$q=12i \qquad \mu=i+1$$
$$q=12i+6 \quad \mu=i+1;$$
when
$$q=12i+2 \quad \mu=i,$$
$$q=12i+8 \quad \mu=i+1;$$
when
$$q=12i+10 \quad \mu=i+1$$
$$q=12i+4 \quad \mu=i+1.$$

Hence the factors dividing I_q in these several cases will be of the respective degrees

$12i+12$, $12i+12$; $12i+8$, $12i+12$; $12i+16$, $12i+16$;

corresponding to (q), being of the several values

$12i$, $12i+6$; $12i+2$, $12i+8$; $12i+10$, $12i+4$;

which is clearly impossible. This proves the theorem in question (the passage being made from the canonical to the general form, as in the former part of this investigation), to wit, that S and T form what I have elsewhere termed a fundamental scale of inva-

riants to the cubic ternary form, entering as the exclusive ingredients into every other invariant that can be derived from such form.

A word of warning is necessary before I lay down my pen; that there can be only two algebraically *independent* invariants to $(x, y)^4$ or $(x, y, z)^3$, is an immediate consequence of the canonical form of each having but one parameter; so in general there can be at most but $(n-2)$ absolutely independent invariants of $(x, y)^n$; but the point established in the preceding investigation goes to show that there can exist no other invariants than such as are *rational* functions of s and t in the one case, and S and T in the other. I shall take some other occasion to establish a similar conclusion for the forms $(x, y)^5$ and $(x, y)^6$.

I have shown that there exist three invariants to the one of the degrees 4, 8, 12, and four to the other of the degrees 2, 4, 6, 10; and I shall demonstrate that any other invariant to either form must be a rational function of those above stated. For the cubic form $(x, y)^3$ we know that there is but one invariant, viz. its discriminant. Thus, then, for $n = 3, n = 4, n = 5, n = 6$ the number of absolutely independent invariants is $n - 2$, and the number of linearly independent invariants is no greater. But this result is by no means generally true. It may be proved by means of a great law of reciprocity* which I myself originated, but unfor-

* The theorem of reciprocity alluded to in the text is the following:—
If to any function $(x, y)^n$ there exists an invariant of the order m in the coefficients, then to $(x, y)^m$ there exists an invariant of the order (n) in the coefficients; or more generally, which is M. Hermite's addition, if to any system of functions $(x, y)^{n_1}, (x, y)^{n_2}, \ldots (x, y)^{n_t}$ there exists an invariant of the several dimensions $m_1, m_2, \ldots m_t$ in the respective sets of coefficients, then conversely to a system $(x, y)^{m_1}, (x, y)^{m_2}, \ldots (x, y)^{m_t}$ there exists an invariant of the dimensions $n_1, n_2, \ldots n_t$ in the respective sets of coefficients.

I had previously shown in this Magazine that Mr. Cayley's formulæ for finding the number of biquadratic invariants to any function $(x, y)^n$, given in that remarkable paper of his on linear transformations in the Cambridge and Dublin Mathematical Journal, where first dawned upon the world the clear and full-formed idea of invariants (the most original and important infused into analysis since the discovery of fluxions), could be expressed by means of the number of solutions of the equation in integers $2x + 3y = n$, the square of the quadratic invariant (which only exists for even values of n) counting for one in the fundamental biquadratic scale; this is of course a direct consequence, through the law of reciprocity, of the fundamental scale to $(x, y)^4$ consisting of a quadratic and a cubic invariant. My discovery of the fundamental scale of invariants to $(x, y)^5$ and $(x, y)^6$ now enables us, through the same law of reciprocity, to express the number of distinct Quintic and Sextic invariants to $(x, y)^n$, viz. as being the number of integer solutions of $x + 2y + 3z = \dfrac{n}{4}$ in the one case, and of $x + 2y + 3z + 5t = \dfrac{n}{2}$ in the other.

tunately threw aside, and which M. Hermite has since demonstrated, that there are more than five linearly independent invariants to $(x, y)^7$, and more than ten, in fact twelve at least, to $(x, y)^{12}$; that is to say, it is impossible in the latter case to find ten of which all the rest shall be rational functions, although an algebraical equation connects any 11. So, again, if we take a *system* of two cubic equations, there are only five absolutely independent invariants; but there are not less than seven linearly independent fundamental invariants, of which any other invariant must be a rational function. In fact, if we take for our two cubics

$$U = ax^3 + 3bx^2y + 3cxy^2 + dy^3$$
$$V = \alpha x^3 + 3\beta x^2 y + 3\gamma xy^2 + \delta y^3,$$

the (5) coefficients of the powers of λ in the discriminant of $U + \lambda V$, each of which is of four dimensions in the two sets of coefficients combined, are all invariants of the system; but there will be besides two more, one of which is a Combinant of six dimensions, being the resultant of U and V; the other is a Combinant of two dimensions only, viz. $a\delta - 3b\gamma + 3c\beta - d\alpha$. These seven together form the fundamental constituent scale.

The two last-mentioned may be expressed algebraically (by the introduction of square roots) as functions of the other five, but of course not as rational functions of the same. My attention was more particularly called to the search of a proof of the completeness of the Aronholdian system of invariants, by an inquiry as to the possibility of rigidly demonstrating that there could exist no others not made up of these, addressed to me in the spring of last year by one of the most gifted geometers of this or any other country. A morning or two after the inquiry reached me, in a walk before breakfast by the side of the ornamental water in St. James's Park (a time and place by no means, according to my experience, unfavourable to the inspirations of the Analytic muse), I had the satisfaction of falling upon the rather *piquant* demonstration above given, which essentially rests upon a principle, requiring no harder exercise of faith than the concession of the impossibility of a greater being contained in or proceeding out of a less.

7 New Square, Lincoln's Inn,
        March 1853.

Erratum.

In the first part of this paper given last month there is an error of calculation (not, however, affecting the result of the reasoning) in the last paragraph of page 302. The cases of

$q = 6i + 5$ and $q = 6i + 3$ should be combined, and the two concluding paragraphs in page 302 will then read as follows:—

If $q = 6i + 5$ or $= 6i + 3$, $\mu = i + 1$, and the factor $(m^3 - m)^{2\mu+1}$ is of the degree $6i + 9$, *i. e.* in each case greater than (q), which is absurd.

Addendum.

On the nature of the three Cycles of four terms each which contain the twelve values of the parameter to the canonical form of a cubic function of three variables.

The equations given in the text show that each term in any one cycle is a periodic function of the *second* order of each other term in the same cycle. Moreover, it may be shown that each term in any one cycle is a periodic function of the *third* order of every term in either of the other two cycles; a sort of relation between the cycles taken *per se*, and with one another precisely the inverse of what obtains (as already shown) for the two cycles of three terms, each containing the six values of the parameter to the biquadratic function of two variables. For as regards that case, it was shown in the first part of this paper that the terms in the same cycle are periodic functions of the third order of one another, and of the second order of each of those not in the same cycle with themselves.

If we make

$$m = A \qquad \frac{1-m}{1+2m} = B \qquad \frac{\rho^2 - m}{1 + 2\rho m} = C \qquad \frac{\rho - m}{1 + 2\rho^2 m} = D$$

$$\rho A = A' \qquad \rho B = B' \qquad \rho C = C' \qquad \rho D = D'$$

$$\rho^2 A = A'' \qquad \rho^2 B = B'' \qquad \rho^2 C = C'' \qquad \rho^2 D = D''.$$

The following table will exhibit all the ternary periods that can be formed between the terms of the several cycles:—

(1) A B′ D″	(4) B A′ C″	(7) C A′ D″	(10) D A′ B″
(2) A C′ B″	(5) B C′ D″	(8) C B′ A″	(11) D B′ C″
(3) A D′ C″	(6) B D′ A″	(9) C D′ B″	(12) D C′ A″.

For instance, as an example of the meaning of the table, take line (8), viz. C, B′, A″. This indicates that A″ is formed from B′ and C from A″ in the same way as B′ from C, and of course A″ from C in the same way as C from B′ and B′ from A″, &c. By means of this table it will easily be seen that a term in each of two cycles being given, the term in the third which forms with the given two a ternary period may immediately be assigned.

The remarks which I have to add on the nature of the equations for finding the parameter (m), as well for $(x, y)^4$ as for $(x, y, z)^3$, will be given hereafter.

LXI. *On some supposed Crystals of Trona.* By HENRY JAMES
BROOKE, *Esq., F.R.S. &c. With an Analysis by* Mr. ALLAN
DICK, *Metallurgical Laboratory, School of Mines**.

I WAS favoured some months since by Mr. Bollaert with some
crystals of this form, which were found
with glauberite and hayesine at Tarapaca.
On comparing the angle of the edges *ab* with
that of the faces *r'l* of Trona, as given at p. 598
of the new edition of Phillips's Mineralogy,
the two angles were found to agree so closely
as to lead to the conclusion that the Tarapaca crystals might be
trona. They have, however, since been analysed at the School of
Mines, and are found to be thenardite. The faces, also, *cc', dd'*
have been measured by Professor Miller, and the angles found to
agree very nearly with those of corresponding faces of thenardite.
No faces, however, which would truncate the edges *ab* of the Tara-
paca crystals have yet been observed on crystals of thenardite; but
as the indices of such faces would be 530, their occurrence is not
improbable. Whether this agreement of angle between particular
faces on crystals belonging to two different systems of crystal-
lization, of which a few other instances have been noticed, is
merely accidental, or is the result of some at present unknown
law, is a matter which requires further investigation.
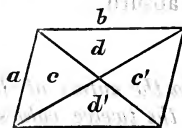
Analysis.

2·73 grs. were dissolved in water acidulated with hydrochloric
acid. Insoluble siliceous matter was separated by filtration,
which after ignition weighed 0·06 gr.

Chloride of barium was added to the filtrate. The precipitate
of sulphate of baryta weighed 4·39.

The excess of baryta in the filtrate was removed by carbonate
of ammonia, and the soda determined as chloride of sodium, of
which the weight was 2·18.

1·27 gr. heated in a tube gave only a trace of water, but a
violet vapour was evolved which condensed into minute crystals
of iodine, which furnished the characteristic reaction with starch †.

Neither nitric acid, lime nor magnesia were present in sensible
quantity.

Results tabulated :—

		Oxygen.
Soda	42·37	10·83
Sulphuric acid .	55·11	32·98
Insoluble residue	2·19	
	99·67	

Hence the formula is NaO, SO³.

* Communicated by the Authors.

[† The evolution of iodine by heat is interesting. Various saline products
from the same district have been examined in the metallurgical laboratory
by Mr. Dick, and all found to yield traces of iodine. The peculiar odour
evolved by hayesine is probably due to the slow evolution of iodine by
exposure to the atmosphere. It seems not unlikely that a supply of iodine
may be obtained from the above-mentioned products.—J. PERCY.]

LXII. *Notices respecting New Books.*

The Principles of Mechanical Philosophy applied to Industrial Mechanics. By THOMAS TATE, F.R.A.S. London: Longman, Brown, Green and Longmans. 1853.

A BOOK containing 342 pages so usefully filled it has rarely been our lot to peruse. The matter is well-chosen, well-arranged, and treated in a manner which proves how thoroughly competent the author is to execute the task which he has undertaken. A considerable portion of the work is devoted to questions of the highest practical importance. It is hardly generous to take exception to a single passage of so excellent a volume, yet we cannot help thinking, that, in reference to the loss of work alluded to in page 187, Mr. Tate is unconsciously transmitting an opinion belonging to an epoch of science which he has evidently left behind him. Even here, however, he might retort that the fallacy of the said opinion is not yet universally acknowledged. The exercises appended to each section of the book cannot fail to enhance its value to both teacher and student.

LXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 10, THE following papers were read:—
1853.   1. " On the determination of the Mean Temperature of every day in the year, as deduced from the Observations taken at the Royal Observatory, Greenwich, in the Years from 1814 to 1851." By James Glaisher, Esq., F.R.S.

This paper has for its object the determination of the true distribution of heat over the year, and is based upon an extensive series of observations taken at the Royal Observatory during thirty-eight years.

In order to obtain a correct determination of the mean daily temperature of each month, necessary to the proposed object, the author at the commencement of his memoir explains how the entire series of observations has been divided into groups, according to the recorded times of observation, for the purpose of applying the necessary corrections calculated from his tables of Diurnal Range, published in the Phil. Trans. for 1848. Having carefully explained his method of arranging and testing his data, and providing for exceptional days, upon which but few observations were recorded, the author gives the results in twelve separate tables, which exhibit the mean daily temperatures of every month in each of the thirty-eight years. In a note to the table for each month are given:—1. The mean temperature of the coldest day of that month, with the day of the month and the year, from 1814 to 1851; 2. the mean temperature of the hottest day of that month, with the day of the month and year, and the extreme difference of mean temperature of two days in that month; 3. The day of the month on which the mean temperature was subjected to the greatest change, with the minimum and maximum mean temperatures, the year of the minimum and of the maximum; 4. the day of the month on which the mean temperature was subjected to the least change, with the minimum and maximum mean temperatures, the year of the minimum and of the maximum. These results are embodied in the opposite table:—

Month	Mean temperature of coldest day from 1814 to 1831.	Day of month.	Year.	Mean temperature of hottest day,	Day of month.	Year.	Extreme difference of mean temp. of two days in the month.	Day of month on which mean temp. subjected to greatest change.	Minimum mean temperature.	Year.	Maximum mean temperature.	Year.	Difference.	Day of month on which mean temp. subjected to least change.	Minimum mean temperature.	Year.	Maximum mean temperature.	Year.	Difference.
January	10.7	20	1838	52.7	24	1834	42.0	20	10.7	1838	48.0	1828	37.3	6	26.1	1841	46.7	1844	20.6
February	12.6	9	1816	55.0	9	1831	42.4	9	12.6	1816	55.0	1831	42.4	20	26.2	1845	48.0	1850	21.8
March	22.1	13	1845	58.6	31	1815	36.5	16	25.2	1845	54.3	1828	29.1	29	36.9	1816	53.8	1830	16.9
April	27.8	1	1836	63.2	25 26	1821	35.4	3 4	28.9 29.5	1839	60.7 61.3	1848	31.8	22	38.0	1837	55.9	1826	17.9
May	36.2	3	1832	72.4	15	1833	36.2	15	42.2	1839	72.4	1833	30.2	2	44.4	1850	59.8	1838	15.4
June	45.0	7	1814	76.1	13	1818	31.1	25	45.1	1835	71.5	1820	26.4	16	52.6	1850	69.4	1846	16.8
July	47.7	20	1836	79.1	15	1825	31.4	18	52.3	1816	78.2	1825	25.9	10	54.4	1835	67.8	1845	13.4
August	43.2	31	1833	75.3	1	1825	32.1	20	47.0	1839	73.3	1826	26.3	9	56.4	1848	68.9	1842	12.5
September	40.7	28	1824	73.5	2	1824	32.8	1	44.5	1816	71.7	1824	27.2	23	47.1	1845	60.9	1846	13.8
October	28.4	29	1836	64.5	5	1834	36.1	29	28.4	1836	55.5	1847	27.1	4	45.0	1836	61.7	1837	16.7
November	23.4	24	1836	59.7	2	1834	36.3	24	23.4	1816	53.9	1846	30.5	4	35.1	1820	52.2	1834	17.1
December	18.4	24	1830	54.9	8	1848	36.5	25	18.6	1830	53.1	1824	34.5	4	31.1	1814	50.8	1836	19.7

The author then treats of the method adopted to deduce the most probable true mean temperature due to every day in the year; and concludes his paper by observing that there are periods of some duration which are very remarkable on account of the difficulty of assigning a physical cause for the anomalies apparent in the mean temperature. Starting from the lowest temperature, in January, it increases till the beginning of March, when, between the 3rd and 10th, not only is the increase checked, but there is a remarkable depression of temperature. After the 10th, the increase proceeds and is very rapid towards the end of April and the beginning of May; this rapid increase is rather suddenly checked, and followed by a period of cold towards the middle of May: this period is very marked. As remarkable a depression of temperature at this time of the year seems to have taken place in France, having been noted in Paris and at various localities, some situated near the coast; but it does not appear that the equally remarkable rise at the end of April has been noted. After the middle of May the numbers steadily increase till the 5th of July, when they attain their maximum value. The decline in the temperature towards the end of July is followed by an increase at the beginning of August, after which the decline of temperature is very regular till towards the end of November, when a sudden and considerable increase of temperature takes place; after this the curve declines to its lowest point on the 8th of January.

2. " On the periodic and non-periodic variations of Temperature at Toronto in Canada from 1841 to 1852 inclusive." By Colonel Edward Sabine, R.A., Treasurer and Vice-President of the Royal Society.

The principal object of this communication is to make known the non-periodic variations of temperature for every day in the twelve years, from 1841 to 1852 inclusive, at Toronto in Canada. The non-periodic variations are those differences of the temperature from its mean or normal state which remain after all the known periodical variations are allowed for, and are such as are generally accompanied by peculiarities of wind or of other meteorological circumstances. Recent investigations have led to the inference that opposite conditions of weather prevail simultaneously in the same parallels of latitude under different meridians, and that in particular Europe and America usually present such an opposition, so that a severe winter here corresponds to a mild one there, and *vice versâ*; and recent theories of the distribution of heat on the surface of the globe profess to furnish the explanation. To place the facts on indisputable ground, it is requisite that a comparison should be made of unexceptionable records of the non-periodic variations in Europe and America, continued for a sufficient time to afford a proper basis for inductive generalisation. Toronto, from its latitude 43° 40' N. and inland situation, is well suited to supply such a comparison with stations in the middle parts of Europe where similar records have been kept; and the twelve years embraced by the observations, viz. 1841 to 1852, have been years of unusual meteorological activity in Europe.

Details are given in the commencement of the paper showing the care bestowed on the examination of the thermometer employed, with a standard divided " *à l'échelle arbitraire,*" by the method of M. Regnault; as well as the precautions adopted for its fair exposure, and for its protection from rain and radiation. The observations were made by the non-commissioned officers of the detachment of the Royal Artillery employed in the duties of the observatory.

The period of twelve years comprises two series, in one of which the thermometer was observed hourly, and in the other less frequently, each observation in the second series receiving however a correction to the mean temperature of the day furnished for every hour and every day of the year by the first series. The two series, each of six years, are separately discussed; from the first series equations are derived from the mean monthly temperatures by the method suggested by Bessel (Astron. Nach. No. 136), whereby the most probable values of the temperature, on every day and every hour, are computed corresponding to the whole body of the observations. These the author regards as approximate normal values, and by comparing with them the actual daily temperatures,—which in the first six years are the means on each day of twenty-four equidistant observations, and in the second six years the means of all the observations made on each day, each observation having been corrected for the hour in the manner described,—the non-periodic variations for every day in the year are obtained and are given in a table.

From the approximate normal temperatures the author has represented in a Plate the phenomena of the temperature at Toronto, according to a method which, if applied to the different meteorological elements and in different localities, might, he thinks, materially facilitate their intercomparison. This method, in which three variables are represented, one being dependent on the other two, is essentially the same that has been long used in magnetic maps, and in the ordinary isothermal maps; from which latter however it differs in this respect, that, whereas in the ordinary isothermal maps the two variables on which the variation of temperature is dependent are the geographical latitude and longitude, in the present case the two variables are the hour of the day and the day of the year. The variation of temperature is here referred therefore to *time* and not to *space*; a distinction which the author proposes to convey by employing the term Chrono-Isothermals, as applicable to lines of this description. From the delineation in the Plate, and from the tables contained in the paper, many characteristic and some peculiar features of the climate and meteorology of the part of the North American Continent in which Toronto is situated, are readily perceivable. Several instances are pointed out; amongst these may be noticed the peculiar anomaly of the North American winter, which is very conspicuous in the Plate; and the absolute as well as relative *variability* of the temperature at different seasons of the year, exhibited by means of a numerical index analogous to the probable error of the arithmetical mean of a number of partial results, and deduced in a similar manner from the differences of individual years, months, and days, from

their mean values: whence it appears, in respect to the annual temperature, for example, that in any particular year there is an equal probability that its mean temperature will fall within the limits of 43°·8 and 44°·6, as that it will exceed those limits on either side.

Finally, the author has shown the " Thermic Anomaly" (as it has been recently termed) of the monthly and annual temperatures at Toronto by comparison with the normal temperatures computed by Dove (Verbreitung der Wärme, 1852), for the parallel of 43° 40' N. from 36 equidistant points on the parallel; from which comparison it appears that after allowance has been made for the elevation above the sea (342 feet), every month of the year is colder than the normal temperature of the same month in the same parallel; that the thermic anomaly reaches its extreme in February, when it exceeds 10° of Fahrenheit; and that on the average of the whole year it is little less than 6°.

Feb. 17.—A paper was read, entitled " On the Muscles which open the Eustachian Tube." By Joseph Toynbee, M.D., F.R.S.

The author commences by alluding to the opinion generally held by anatomists, viz. that the guttural orifice of the Eustachian tube is always open, and that the air in the tympanum is constantly continuous with that in the cavity of the fauces. An examination of the guttural orifice of the tube in man and other animals has led the author to conclude, that, except during muscular action, this orifice is always closed, and that the tympanum forms a cavity distinct and isolated from the outer air. The muscles which open the Eustachian tube in man are the tensor and levator palati, and it is by their action during the process of deglutition that the tubes are ordinarily opened. That the act of swallowing is the means whereby the Eustachian tubes are opened, is shown by some experiments of which the following may be cited. If the mouth and nose be closed during the act of swallowing the saliva, a sensation of fulness or distension is produced in the ears; this sensation arises from the air, which is slightly compressed in the fauces, passing into and distending the tympanic cavities: upon removing the hand from the nose, it will be observed that this feeling of pressure in the ears does not disappear, but it remains until the act of deglutition is again performed while the nose is not closed. In this experiment the Eustachian tubes were opened during each act of deglutition; during the first act, while they were open, air was forced into the cavity of the tympanum by the contraction of the muscles of the fauces and pharynx, and the guttural orifices of the tubes remained closed until the second act of swallowing, which opened the tubes and allowed the air to escape. That the act of deglutition opens the Eustachian tubes, was inferred also from the custom usually adopted of swallowing while the descent in a diving-bell is performed; by this act the condensed air is allowed to enter the tympanum, and the sensation of pain and pressure in the ears is removed or entirely avoided.

The author gives an account of the Eustachian tube and its muscles in Mammalia, Birds and Reptiles. In some mammalia the muscles opening the tubes appertain, as in man, to the palate, in others this function is performed by the superior constrictor muscles

of the pharynx. In Birds it is shown that there is a single membranous tube into which the two osseous tubes open; this membranous tube is situated between and is intimately adherent to the inner surface of each pterygoid muscle, and by these muscles the tube is opened. The conclusion to which the author arrives respecting the influence of the closed Eustachian tubes is, that the function of hearing is best carried on while the tympanum is a closed cavity, and that the analogy usually cited as existing between the ordinary musical instrument, the drum and the tympanum, to the effect, that in each it is requisite for the air within to communicate freely with the outer air, is not correct. On the contrary, the author shows that no displacement of the air is requisite for the propagation of sonorous undulations, and that were the Eustachian tubes constantly open, these undulations would extend into the cavity of the fauces, there to be absorbed by the thick and soft mucous membrane, instead of being confined to the tympanic cavity, the walls of which are so peculiarly well adapted to the production of resonance, in order that they shall be concentrated upon the labyrinth.

In corroboration of the above views the author states, that in cases of deafness dependent simply upon an aperture in the membrana tympani, whereby the sonorous undulations are permitted to escape into the external meatus, the power of hearing has been greatly improved by the use of an artificial membrana tympani made of vulcanized india-rubber or gutta-percha, which is so applied as again to render the tympanum a closed cavity.

Feb. 24.—A paper was read, entitled "On Periodical Laws in the larger Magnetic Disturbances." By Captain Younghusband, R.A., F.R.S.

In this communication the author has arranged, in tables, the disturbances of the magnetic declination at St. Helena and the Cape of Good Hope, for the purpose of exhibiting the systematic laws by which those phenomena are regulated, which were long described as irregular variations, because they were of occasional and apparently uncertain occurrence.

The frequency of the disturbances, and their amount, whether viewed separately as easterly or westerly movements, or as general abnormal variations (easterly and westerly being taken together), is shown to be dependent upon the hour of the day, the period of the year, and upon the year of observation. This dependence upon the year of observation affords additional testimony of a periodical variation in the magnitude of magnetic changes of the same character as that which has been found to exist at other places, and which has been considered to be coincident with variations of the solar spots.

The disturbances of larger amount only are noticed; those observations which differed by 2·5 scale divisions (1'·8 in arc at St. Helena, and 1'·9 in arc at the Cape) and upwards, from the normal place, were separated from the others and the values of the differences taken; there were therefore two series of figures to be dealt with, viz. the number of disturbances, and the aggregate amount of disturbance. These were separated into disturbances of the north

end of the magnet towards the east and towards the west, and the effect of each considered separately.

The periodical character of disturbances at St. Helena and the Cape in a cycle of years is indicated insofar as the limited extent of the observations would permit; sufficient however to point to the year 1843 as that of least disturbance at these two places, by showing a regular decrease from the previous years, and an increase in every succeeding year of observation. Though the hourly observations were discontinued before 1848, the year which Colonel Sabine has shown to be that of periodical maximum, as 1843 was that of minimum magnetic activity at Toronto and Hobarton, the observations now discussed are shown to be quite consistent with this period, and thus tend to establish it as a general law of magnetic phenomena. In the aggregate of each year the disturbances towards the west are shown to preponderate over those towards the east, both at St. Helena and the Cape of Good Hope; a similar preponderance of westerly over easterly has been found in every year of observation at Hobarton, but at Toronto the easterly disturbances exceeded the westerly both in number and amount in every year.

Arranging the disturbances into the several *months* of their occurrence, the greatest disturbance is found to occur in January and the least in June at St. Helena and the Cape of Good Hope; the same months being those of greatest and least disturbance at Hobarton, whereas at Toronto, *both* January and June are months of minimum disturbance, the maxima disturbance occurring there in April and September.

From this identity of the epoch of greatest and least disturbance, —at St. Helena, where the months of January and June are not those of opposite seasons, viewed either with respect to the sun's extreme altitude or to extreme periods of temperature,—at the Cape, situated in S. latitude 33° 56′,—and at Hobarton in S. latitude 42° 52′,—and contrasting this identity with a different law at Toronto in N. latitude 43° 39′, the author infers that the principal causes which produce an annual period of disturbance are not dependent upon local seasons. It is likewise pointed out that about the period of the equinoxes there is a tendency to maximum disturbances at all the stations, producing absolute maxima at Toronto, faintly but systematically indicated at the other stations.

The westerly disturbances were found to exceed the easterly in every month in the year at St. Helena and the Cape, which agrees with the results deduced from the Hobarton observations, while it appears from the observations at Toronto that the easterly disturbances exceeded the westerly in every month. The average value of a westerly disturbance is greater than that of an easterly in every month at St. Helena and the Cape of Good Hope. The disturbances at Hobarton again coincide with this result; and in a slight and less perfectly marked degree, Toronto has the same peculiarity.

Arranging the disturbances into the several *hours* of their occurrence, the hours of the day are found to be those of greatest disturbance in a very considerable degree; the sum of the ratios, during the twelve hours of the day, being about seven times as great as the

sum of those in the twelve hours of the night at St. Helena, and about 2·6 times as great at the Cape of Good Hope; while at Hobarton the sum of the twelve night ratios slightly exceeded the day; at Toronto the excess was larger, viz. as 1·3 to 1. The laws of easterly and westerly disturbances, in relation to the local hours, are then examined separately. At St. Helena and the Cape, the easterly day-disturbances exceed the easterly night-disturbances, and the westerly day-disturbances exceed the westerly night-disturbances. These results are compared with those at Toronto and Hobarton.

At St. Helena, although but comparatively few disturbances occur during the night hours, those disturbances are almost all westerly (183 disturbances, in all, occurred in nine night hours during five years, of which 174 were westerly and but nine easterly). In the day hours the westerly only *slightly* exceed the easterly disturbances. At the Cape, the westerly excess is less in the night and greater in the day than at St. Helena, and the night excess much greater than the day excess.

At St. Helena, the fact of the disturbances being more frequent in the day than in the night is consistent in every month of the year; this appears worthy of remark when it is remembered that at St. Helena the curve of the diurnal variation of the declination is precisely reversed at two opposite periods of the year; in one case corresponding to the curve of diurnal variation in middle northern latitudes, and in the other to that in middle southern latitudes.

The mean effect of the disturbances which have been separated as described, and which comprise all of largest magnitude, is a *constant westerly* effect at every hour both at St. Helena and the Cape of Good Hope, acting more energetically in the night than in the day. At Toronto the mean effect is westerly in the day and easterly in the night; at Hobarton, easterly in the day and westerly in the night.

ROYAL INSTITUTION OF GREAT BRITAIN.

Jan. 28, 1853.—" On Gerhardt's discovery of Anhydrous Organic Acids," by Professor Williamson.

The discovery by M. Gerhardt of a number of anhydrous organic acids has thrown so much light on one of the most important questions of chemical philosophy, that it constitutes one of the most remarkable illustrations of the manner in which the rich materials of organic chemistry may be brought to bear on the explanation of the phænomena of chemical action and the laws of chemical combination.

It is not unworthy of remark, that the bodies prepared by Gerhardt had for some years past been supposed to exist ready formed in combination with water and other bases, and that the chief objection to that supposition was founded on the circumstance of their never having been separated from such combination, and presented in an isolated form. In fact, Gerhardt has supplied the very link in the chain, which was expected to constitute evidence for a familiar theory of the constitution of salts. But the process by which

the result was attained is even more important than the result itself, and has led to our drawing from that result a conclusion different from that which was generally expected. Chemistry aims at discovering the nature of that action by which substances of opposite properties undergo those remarkable changes which we call chemical combination; and it naturally follows from this view of its objects, that chemical science is more advanced by the discovery of a new *process* than by the discovery of a new *substance*; and its theories are more immediately affected by the nature of a process of change than by any physical fact, such as the existence of a peculiar body or class of bodies. Thus it is that the method of isolating the anhydrous organic acids has afforded evidence of a new view of the constitution of acids and salts.

A few words may serve to give an idea of the previous state of the question.

Compounds of oxygen acids were supposed to consist of the anhydrous acid united with an oxide. Thus hydrated sulphuric acid was represented as containing the anhydrous group SO^3 plus an atom of water, H^2O; and in the saturation of this hydrated acid by a base such as potash, it was conceived that this oxide replaced the water. The existence of anhydrous sulphuric acid in an isolated state, and the fact that it so readily combines with water, was urged as an argument in favour of this theory; and the same holds good with phosphoric, carbonic, sulphurous, lactic, nitrous, and even (according to the recent discovery of Dessaignes) nitric acid.

However simple this view might appear, and however satisfactory it might be in explaining those cases of combination for which it was specially intended, chemists soon became acquainted with bodies perfectly analogous in their general properties to the oxygen acids, and producing by their action upon bases similar effects, but which, from the fact of their containing no oxygen, could not possibly be conceived as made up of water and an anhydrous acid. For instance, hydrochloric acid was proved, both analytically and synthetically, to be composed of nothing but chlorine and hydrogen; and when it combines with potash, the hydrogen is found to leave the chlorine, whilst potassium takes its place.

Being desirous of simplifying as far as possible their views of these phænomena, and of extending the same explanation to all like cases, certain chemists were led to imagine a new mode of representing the constitution and reactions of oxygen acids, which had the advantage of connecting the two classes of analogous reactions by the same theory. This consisted in conceiving, that in the formation of a hydrated acid, a compound radical is produced in combination with hydrogen; so that hydrated sulphuric acid is the hydrogen compound of SO^4, in the same way as hydrochloric acid is the hydrogen compound of chlorine. There were many arguments in favour of this view, amongst which the most prominent was derived from the fact, that when a salt of the one class, as chloride of potassium, decomposes a salt of the other, as sulphate of silver, the result is exactly in conformity with what must occur on the supposition of

the compound radical; and in like manner, the electrolytic decomposition of a sulphate moves the group SO^4 to the positive pole, where it either combines with a metal or undergoes decomposition.

One of the strongest arguments against the view that the oxygen acids contain water, is afforded by the results of recent researches (especially of MM. Laurent and Gerhardt) on the atomic weight of acids. Those chemists have rendered more definite and exact than they had been before our ideas on the distinctions between monobasic, bibasic and tribasic acids, and have clearly established that the correct expression of the atom of nitric acid must be such as contains half as much hydrogen as is contained in 1 atom of water (inasmuch as water is bibasic, and nitric acid monobasic). Of course this proportion may be as well established by doubling the atomic weight of water as by halving that of hydrated nitric acid; but either way it is clear that hydrated nitric acid cannot *contain* water.

Such was the position of the question, when an English chemist proved that the formation of æther from alcohol (which was considered chemically as the hydrate of æther) does not consist in a separation of two already-formed compounds, but in a substitution of hydrogen by the organic radical æthyle. A similar fact M. Gerhardt has proved respecting a great number of organic acids, by preparing bodies which stand to them in the same relation as æther does to alcohol.

The researches of M. Cahours had led to the discovery of a series of bodies necessary for Gerhardt's process. These were obtained by the action of pentachloride of phosphorus on various hydrated organic acids, and consisted of chlorine combined with the oxygenized radical of the acid. Thus from benzoic acid was prepared the chloride of benzoile, $C^7 H^5 OCl$, and the corresponding bodies from cuminic, cinnamic, and various other acids. Gerhardt has since made by the same process the body $C^2 H^3 OCl$, which is the chloride of the radical of acetic acid, called *othyle*. Now on bringing any one of these chlorides in contact with the potassium salt of the corresponding acid, the chemical force of combination between chlorine and potassium induced the decomposition.

These results can be most simply stated in the form adopted by M. Gerhardt, the discoverer, which consists in comparing the composition of these bodies with that of water, from which they are formed by the substitution of one or both atoms of hydrogen by organic radicals.

Thus water being represented by the formula $\begin{matrix} H \\ H \end{matrix}O$, acetic acid is formed from it by the action of chloride of othyle, $C^2 H^3 OCl$, which forms $\begin{matrix} C^2 H^3 O \\ H \end{matrix}O + HCl$, *i.e.* hydrated acetic acid and hydrochloric acid. If a second atom of chloride of othyle is made to act upon this acetic acid, or better upon the acetate of potash, $\begin{matrix} C^2 H^3 O \\ K \end{matrix}O$, we get, besides chloride of potassium, a compound, $\begin{matrix} C^2 H^3 O \\ C^2 H^3 O \end{matrix}O$, which,

when compared to the original type, may be considered as water having both its atoms of hydrogen replaced by the radical othyle, $C^2 H^5 O$. This compound is the anhydrous acetic acid, which might be called the acetate of othyle, inasmuch as that radical has, in the formation of the compound, taken the place of the basic potassium in the acetate of potash.

In like manner the anhydrous benzoic acid, $\genfrac{}{}{0pt}{}{C^7 H^5 O}{C^7 H^5 O}O$, was made by the action of the chloride of benzoile, $C^3 H^5 OCl$, on the benzoate of potash. It is a crystalline body, perfectly neutral to test-paper, scarcely soluble in water, readily soluble in alcohol and æther. On continued boiling with water, it is converted into hydrated benzoic acid, 1 atom of the anhydride with 1 atom of water forming 2 atoms of the hydrated acid by an interchange of hydrogen and benzoile. Besides several of these anhydrous acids, Gerhardt has prepared some intermediate acids, analogous to the intermediate æthers, by combining two different radicals in the same group. Thus chloride of benzoile with cuminate of potash, $\genfrac{}{}{0pt}{}{C^{10} H^{11} O}{K}O$, formed cuminate of benzoile, or benzocuminic acid, $\genfrac{}{}{0pt}{}{C^{10} H^{11} O}{C^7 H^5 O}O$; and in like manner several other intermediate acids were prepared.

In conclusion, the lecturer alluded to a feature of the development of the human mind in scientific research, which is strikingly illustrated by the substance and form of these results, and of which instances are probably to be found in the history of many others. The explanation of the above reactions consists in a combination of two modes of reasoning, which were developed by different schools, and for many years were used independently of one another. Gerhardt, to whose researches and writings some important steps in the doctrine of types are owing, formerly believed the truths which he saw from that point of view to be incompatible with the idea of radicals; but he now joins those chemists who find in each of these notions a necessary and most natural complement to the other.

May we not hope that such may be the result in other cases of difference of opinion on scientific questions, which the progress of knowledge will show to have been owing to the incompleteness and one-sidedness of each view, rather than to anything absolutely erroneous in either?

LXIV. *Intelligence and Miscellaneous Articles.*

ON THE CHEMICAL COMBINATIONS RESULTING FROM THE CONTACT OF SOLIDS WITH LIQUIDS IN VIRTUE OF FEEBLE ACTIONS.

BY M. BECQUEREL.

THE study of the chemical phænomena resulting from the contact of solids with liquids in virtue of feeble actions, whether they proceed from double decomposition, with or without the accompaniment of electrical forces, or from simple contact action, and indeed whatever may be their origin, still occupies my attention as regards their

relation to those phænomena which are produced solely by electro-chemical forces. The new results which I now make known, testify to the fecundity of the methods of investigation by means of which we may determine the play of affinities in the contact of solids with liquids.

Silica and Quartz (Si O³).—Silica generally occurs in the form of quartz, insoluble in alkalies, and sometimes in a hydrated state soluble in alkalies, as in opal, and the deposits formed by mineral waters. We are indebted to M. Ebelmen for some interesting researches on the artificial production of hydrated silica in a hard transparent state, like rock crystal, by exposing silicic æther to the action of moist atmospheric air; this silica has the formula $2SiO^3 + 3HO$, and its density is 1·770. These conditions are obviously not those under which silica has been formed naturally.

On the other hand, M. de Senarmont has shown that by heating under pressure a solution of silica, in water saturated with carbonic acid, or in weak hydrochloric acid, to 392° or 572° Fahr., anhydrous silica is obtained in the form of quartz, and insoluble in all reagents except hydrofluoric acid. It is probable that quartz has actually been formed in this way in nature, for the conditions are such as may co-exist at a certain depth below the surface of the earth.

Further, M. Fremy has recently succeeded in obtaining solid hydrated silica by decomposing sulphuret of silicium by water, and under conditions which may coexist naturally. Mr. Cross has, by means of a voltaic battery consisting of a great number of couples weakly charged, slowly decomposed a solution of silica in potash or hydrofluoric acid, in such a manner as to obtain at one of the poles, according to the chemical relation of the silica in the compound, prismatic crystals of quartz with pyramids at their summits. This mode of production may not be one which takes place naturally, but the fact is deserving of notice.

I have endeavoured to ascertain whether it may not be possible, by means of the method of double decomposition, which has already enabled me to obtain malachite (hydrated bibasic carbonate of copper) and a number of other compounds, to produce quartz and hydrated silica. For this purpose I introduced into a flask, furnished with a loosely fitting cork, a very weak solution of silicate of potash (about half a litre), with a number of plates of sulphate of lime. Carbonic acid gradually penetrated in the flask, giving rise to the formation of carbonate of potash, which immediately acted upon the sulphate of lime, producing sulphate of potash and carbonate of lime, which crystallized, while silica was precipitated sufficiently hard to scratch glass, and forming granules or plates of 5 or 6 centimetres superficies. These plates showed a tendency to assume a coralloid structure; the silica of which they consisted contained 12 per cent. of water. Silica was likewise deposited at the bottom of the flask in very delicate particles, having the same hardness as the above, which when treated with a hot solution of potash, left at the bottom of the capsule transparent lamellæ, which gave, between two Nichol's prisms, the colours presented by crystals causing double refraction,

as I was assured by M. G. Rose. These lamellæ therefore resemble quartz.

These facts show that silica is precipitated in the different states I have mentioned, at the instant that the carbonic acid of the atmospheric air which enters the flask combines with the potash holding the silica in solution, and that at the same time the carbonate of potash formed reacts upon and decomposes the sulphate of lime. The effects produced must be attributed to this double reaction.

Double Carbonate of Copper and Soda (NaO, CuO, $2CO^2$).—I have in a previous memoir* stated that bibasic carbonate of copper, (malachite) $2CuO$, CO^2, $2HO$, may be obtained by immersing a fragment of limestone covered with crystallized subnitrate of copper, $4CuO$ NO^5, $3HO$, into a dilute solution of bicarbonate of soda, and that if the action is allowed to go on until the whole of the subnitrate has been converted into bibasic carbonate, the latter is itself decomposed with production of a double carbonate of copper and soda in very minute crystals of a clear blue colour, which adhere firmly to the chalk. At the time that I wrote that memoir I commenced several experiments, with the intention of leaving them to spontaneous action during several months. Last November, on taking the fragments of limestone covered with subnitrate of copper out of the solution of bicarbonate of soda, in which they had been placed six months previously, I found the bibasic carbonate for the most part decomposed and converted into double carbonate of copper and soda, in beautiful microscopic crystals, having the form of right rhombic prisms with truncated summits.

Bibasic Arseniate of Lime and Ammonia.—When a fragment of limestone or chalk is left for some months in contact with a saturated solution of arseniate of ammonia in considerable excess, after the lapse of a certain time, very perfect transparent crystals are perceived upon its surface; in a preparation which was made at the commencement of 1852, these crystals are more than a centimetre on each side. Their form is an oblique rhomboidal prism. One of them presents very distinct modifications at the edges, indicating a double truncation, constituting a pyramid with four planes at each of the bases. The crystals effloresce very rapidly in the air, losing ammonia and water; the analysis, which I was unable to repeat from want of material, appeared to show that they were a double arseniate of lime and ammonia with a large quantity of water of crystallization.

It was to be expected that the other earthy bases—baryta, strontia, magnesia, &c.—would behave in the same manner as lime in the reaction of their carbonates upon arseniate of ammonia, and I therefore made some arrangements for the purpose of verifying this conjecture; after a few days I recognized upon the surface of the carbonates acicular crystals which could not be other than double compounds of arseniate of ammonia and the arseniates of the earthy bases. When a sufficient quantity of these crystals are formed, I shall analyse them in order to determine the composition of this new series of compounds.

* Phil. Mag. Ser. 4. vol. iii. p. 235.

I must here mention a circumstance which may not be without some interest for the geologist. Mineralogists are not agreed as to the composition of the arseniate of lime, formerly called pharmacolite, which would appear to lead to the opinion that several natural compounds of lime and arsenic acid exist. M. Dufrenoy is, for this reason, inclined to the opinion that this compound is a product of the decomposition of arseniferous minerals, and that it frequently contains an excess of base and hygrometric water which entirely alter the proportions. I will here add, that all the natural crystals are effloresced, and present the appearance of the crystals of double arseniate of lime and ammonia which have lost their ammonia and a part of their water of crystallization when they have been exposed to the atmosphere; however, the composition of these two kinds of crystals is not the same; the natural arseniate, irrespective of the water, is composed of 2 equivs. of lime and 5 equivs. of arsenic acid, or of 1 equiv. of the former and 2 equivs. of the latter, while the arseniate of lime, as I have prepared it, contains 2 equivs. of lime and 1 equiv. of arsenic acid. During the decomposition of the metallic arseniates it may happen that alkaline arseniates are formed, which reacting upon carbonate of lime, give rise to the formation of arseniate of lime.

The double arseniate of lime and ammonia, and consequently the arseniate of lime, may likewise be obtained by digesting, in a solution of arseniate of ammonia (10°), plates of sulphate of lime; after some months, acicular crystals are seen upon the surface of the mineral, which, according to all appearances, have the same chemical composition as the double arseniate above described.

In conclusion, I will mention some preparations which are not without interest. When a calcareous mineral, coated with subnitrate of copper, is allowed to react upon a saturated solution of neutral or bichromate of potash, there are obtained, in the first place, chromate of copper in microscopic crystals of a clear green colour, and secondly, a chromate of the same base also, in microscopic crystals, but of a brownish red colour.

Crystallized cyanide of copper may be obtained in the same manner; it is of a red colour.—*Comptes Rendus,* January 1853, 209.

ON THE TEMPERATURE PRODUCED BY THE COMBUSTION OF CARBON IN THE AIR. BY H. DEVILLE.

It is well known that near the tuyères of blast furnaces, a very elevated temperature is developed, which M. Ebelmen considers to be equal to the melting-point of platinum. Some experiments made in the course of an investigation altogether different, have led me to believe that the heat developed during the combustion of carbon is capable of producing effects much more energetic and comparable with those obtained by means of a mixture of hydrogen and oxygen. Thus, by a suitable arrangement of the furnace, and with the proper kind of carbon, it is possible to melt and even to volatilize platinum and to melt pure silica*. These results, and the simplicity of the

* M. Deville exhibited to the Academy a crucible made from platinum

means by which they may be obtained, have convinced me that they will become useful to the chemist and manufacturer; I have therefore decided upon submitting to the Academy the details of the operation, which, I trust, will not be found unworthy of attention.

The apparatus which I employ is a simple furnace 30 centimètres high and 18 centimètres diameter, supported upon a plate of cast-iron pierced with holes arranged in a circle 5 centimètres from the centre. This is placed in communication with the bellows of a portable forge.

The best kind of crucibles melt down at the temperature in question to a perfectly liquid glass, and for a substitute I was obliged to have recourse to pieces of well-burnt lime, which may easily be brought into the shape of thick crucibles. Their covers are likewise made of lime. M. Berthier observed that hydraulic limes were readily fused at a high temperature, and I have found that perfectly pure lime very frequently agglutinated. It is therefore indispensable to employ a somewhat porous lime, slightly siliceous, which will only become compact at the most elevated temperatures.

With regard to the combustible, it must be very porous and in a state of very fine division; and I should add that I succeeded only when I made use of the residue of the imperfect combustion of coal, the clinkers mixed with cinders which fall from the grate of the heating apparatus and still at the *École Normale*, passed through a wire sieve. With coal of the best quality, in very small particles, the effects are much more feeble and do not differ from those which have already been obtained.

This extremely elevated temperature is developed with such rapidity that in a few minutes it reaches its maximum. But it does not extend beyond a small distance upwards, carbonic oxide being formed higher up with a considerable reduction of temperature, and the production of a very long and feebly heated flame. M. Ebelmen has perfectly explained this phænomenon, the cause of which is now generally known.—*Comptes Rendus*, November 1852.

NOTE ON THE REPRODUCTION OF ENGRAVINGS AND DRAWINGS BY MEANS OF THE VAPOUR OF IODINE. BY M. NIEPCE DE SAINT VICTOR.

In 1847 the author published a memoir on the action of different vapours, and amongst others of that of iodine. He stated that the vapour of iodine attached itself to the black portions of an engraving to the exclusion of the whites, so that the picture could be reproduced on paper sized with starch, or on glass coated with this substance; in this manner designs were produced, but they could not be rendered very permanent.

The author now proposes to render them unalterable by the following processes:—If a design obtained on starched paper or glass

melted in lime, a crucible cover upon which were numerous globules of platinum which had been volatilized, and a specimen of pure silica melted in graphite.

in the manner described by him in 1847 *, be plunged into a solution of nitrate of silver, it will disappear; if the paper or glass be now exposed to the light for a few seconds, the iodide of silver, into which the iodide of amidone which formed the primitive design has been converted, being much more sensitive than the nitrate of silver with which the rest of the surface is imbued, is acted upon much more rapidly; if the paper or glass be then dipped into a solution of gallic acid, the design is immediately reproduced, and it is then treated with hyposulphite exactly as is done with photographic pictures. By this process the design becomes as permanent as these latter, and it will probably be adopted in many cases.

M. Bayard has just made another application of the vapour of iodine; after exposing the engraving to the vapour of iodine, he applies it upon a glass prepared with sensitive albumen, so as to form a negative picture, with which he then takes positive impressions on paper in the usual manner. In this manner he has obtained beautiful reproductions of very old engravings without any distortion of the image.—*Comptes Rendus*, March 28, 1853, p. 581.

REMARKS ON THE STRUCTURAL CONDITIONS OF IRON.
BY T. R. V. FUCHS.

The difference in physical characters presented by the several kinds of iron is generally attributed to the presence of a variety of substances, among which carbon is considered the most important. It is contained in all kinds of iron, almost always accompanied by silicon, which perhaps exercises the same influence. Raw iron contains the largest quantity of carbon, bar iron the least, and steel is in some sort intermediate between the two; but the quantity of carbon does not in any case bear a constant proportion to the iron, nor are these three kinds of iron separated from each other by any definite limits. These two facts are sufficient to show that the carbon cannot be in a state of very intimate combination with the iron, and there are no sufficient grounds for assuming that the different conditions of this metal are determined solely by the quantities of carbon contained in it. The numerous, and in many respects valuable analyses of iron have served only to prove the truth of the above remark. Upon the gratuitous assumption that the varying per-centage of carbon is the cause of the differences in character of iron, attention has been too exclusively devoted to this point, while another, and perhaps more essential one, the crystalline structure, has been overlooked.

Fuchs expresses his conviction that iron is a dimorphous substance, that there are, in fact, two species (varieties) of iron,—the tesseral and the rhombohedral. He considers it as proved that malleable iron belongs to the tesseral system; and if any doubt still exists, it may be inferred from analogy that such is the case, inasmuch as all other malleable metals possess crystalline forms belonging to this system.

The crystalline form of raw iron has not been ascertained with so much certainty, but Fuchs considers it highly probable that it be-

* See Phil. Mag. Ser. 3. vol. xxxii. p. 206.

longs to the rhombohedral system, because it comes within the class of perfectly brittle metals, the crystalline forms of which, as far as we are acquainted with them, are rhombohedral.

But the difference between malleable and cast iron does not consist merely in the crystalline structure, which may be open to doubt, but likewise in the physical characters, and to some extent in the chemical behaviour, for instance the cohesion, hardness, resistance to fracture, fusibility, oxidizability, solubility in acids, &c. He is of opinion that these circumstances alone would justify the inference that there is a specific difference between malleable and cast iron, which he compares with those presented by the modifications of sulphur, phosphorus, arsenious acid, by glass and Reaumur's porcelain.

Finally, with regard to steel, Fuchs is of opinion that it is an alloy of tesseral and rhombohedral iron. The per-centage of carbon which it contains varies from 0·625 (Gay-Lussac) to 1·9 (Karsten). It cannot therefore be regarded as a definite and constant compound. It differs from other alloys in the circumstance that its characters may suffer considerable alteration without an accompanying addition or loss of substance, as in the hardening and softening of steel, changes which Fuchs supposes to be the result of an internal and alternating metamorphosis, by which the relative proportion of the two species of iron is altered. Thus, according to his views, in hardened steel the rhombohedral preponderates over the tesseral iron, and the reverse in soft steel. Very hard steel would, therefore, from the very small proportion of tesseral iron, approximate closely to cast iron; and this conjecture is favoured by the low specific gravity of hardened steel. By the process of tempering, the proportion of tesseral iron in steel would increase with the temperature. The two kinds of iron in steel may be regarded as in a state of constant mutual tension, which may perhaps be the reason why steel retains permanently communicated magnetism, while malleable iron does not.

An experiment of Schafhäutl's* would appear to favour the above views. He submitted a piece of a razor-blade to the action of tolerably strong hydrochloric acid for several days, at the end of which time it was found to have been very unequally attacked. When washed, dried and broken in a mortar, it furnished fragments, some of which could be powdered, while others were malleable.

With regard to the important and much-discussed question of the alteration of malleable iron when exposed to continuous vibration, concussion or torsion, in consequence of which it acquires a granular fracture, Fuchs admits that such an alteration takes place even in the best-worked metal, but does not altogether agree with the explanation usually offered for it, viz. the gradual assumption of a crystalline texture; and is of opinion that it consists in the passage of the iron from a fibrous crystalline state to a granular crystalline state, a change in the aggregation, not an essential metamorphosis. When iron passes from the fibrous into the granular texture, the cohesion of the molecules is lessened; and by their aggregation into

* Prechtl's *Technologischer Encyclopädie, Abhandlung über den Stahl,* vol. xv. p. 377.

rounded groups, a heap of distinct particles is produced, which may be compared with what mineralogists call granular minerals. The continuity of the mass is thus to some extent destroyed, inasmuch as these granular particles only adhere together more or less, and consequently the greater the size and number of these particles the greater is the diminution in tenacity. According to the statement of Kohn, the original condition of iron thus altered cannot be restored by heating to redness and forging, but only by exposure to a welding heat; and Fuchs considers this a sufficient proof that this alteration of iron consists in a breaking up of the continuity of the mass. The restoration of this continuity requires that the granular iron should, by exposure to a welding heat, be rendered amorphous, when the cohesive force again becomes active, a condition which in the case of most other bodies obtains only when they are liquid.— *Schweizerisches Gewerbeblatt,* September 1852.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1853.

Chiswick.—March 1. Snowing : sleet : clear and frosty. 2. Uniformly over-cast : fine : overcast : slight frost. 3. Clear : fine : cloudy : clear and frosty. 4. fine : slight rain. 5. Rain. 6. Cloudy : slight rain. 7. Rain : foggy. 8. Uniformly overcast : rain : foggy at night. 9. Foggy : fine. 10. Overcast : fine : clear. 11. Foggy : fine : slight fog. 12. Dense fog : fine : foggy. 13. Slight fog : very fine : rain at night. 14. Rain : heavy rain : clear and frosty. 15. Hoar-frost : large masses of snow-like clouds : very fine : overcast. 16. Uniform haze : cloudy. 17. Densely overcast : clear and frosty at night. 18. Overcast : frosty. 19. Clear : dry air : frosty. 20. Fine : cloudy : frosty. 21. Slight snow : overcast : clear and frosty. 22. Cloudy and cold : clear and frosty. 23. Snow-showers occasionally. 24. Clear. 25. Clear : cloudy : clear and frosty. 26. Quite clear : fine : hazy. 27. Overcast. 28. Cloudy. 29. Foggy : dry cold haze. 30. Dry haze : very fine. 31. Very fine : rain.

Mean temperature of the month .... 37°·41
Mean temperature of March 1852 .... 39 ·62
Mean temperature of March for the last twenty-seven years 42 ·52
Average amount of rain in March .... 1·40 inch.

Boston.—March 1. Cloudy : snow A.M. 2—5. Cloudy. 6. Cloudy : rain P.M. 7, 8. Cloudy. 9. Fine. 10. Cloudy. 11. Fine. 12. Cloudy. 13. Fine. 14. Cloudy : rain A.M., and rain and snow P.M. 15. Fine. 16. Cloudy : rain A.M. and snow P.M. 17. Cloudy : snow P.M. 18. Fine. 19. Fine : snow A.M. 20. Fine. 21. Cloudy. 22. Cloudy : snow A.M. and P.M. 23. Cloudy. 24. Fine. 25. Cloudy : snow A.M. and P.M. 26. Fine. 27. Cloudy : rain A.M. and P.M. 28—31. Fine.

Sandwick Manse, Orkney.—March 1. Cloudy A.M. : showers, thaw P.M. 2. Showers A.M. : showers, thaw P.M. 3. Fine, frost A.M. : clear, aurora P.M. 4. Fine, thaw A.M. : damp P.M. 5. Fine, clear A.M. : cloudy P.M. 6. Rain A.M. : clear, aurora P.M. 7. Bright A.M. : clear, S. aurora P.M. 8. Cloudy A.M. : clear, aurora P.M. 9. Cloudy A.M. : cloudy, aurora P.M. 10. Cloudy A.M. : clear, aurora P.M. 11. Bright A.M. : clear, aurora P.M. 12. Hazy A.M. : clear, aurora P.M. 13. Bright A.M. : hazy P.M. 14. Hazy A.M. and P.M. 15. Snow-showers A.M. : sleet-showers P.M. 16. Snow-showers A.M. : cloudy P.M. 17. Clear, frost A.M. : clear, aurora P.M. 18. Clear, frost A.M. : snow-showers P.M. 19. Snow-showers A.M. : clear, frost P.M. 20, 21. Snow-showers A.M. and P.M. 22, 23. Clear, frost A.M. and P.M. 24. Clear, snow A.M. : clear P.M. 25. Snow-showers A.M. : clear, frost P.M. 26. Drops A.M. : rain P.M. 27. Bright A.M. : fine P.M. 28. Bright A.M. : fine, aurora P.M. 29. Clear A.M. : fine P.M. 30. Bright A.M. : clear P.M. 31. Bright A.M. : cloudy P.M.

Mean temperature of March for twenty-six previous years ... 40°·46
Mean temperature of this month .... 38 ·24
Average quantity of rain in March for seven previous years . 2·30 inches.
·83 of that marked as rain is melted snow.

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. 1853. March.	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.
	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	29·696	29·653	29·36	29·67	29·29	39	24	34	31	39	sw.	s.	nw.	·27	...	·08
2.	29·503	29·389	29·15	29·50	29·82	45	27	36	38	36	nw.	n.	se.	·10	·04	·15
3.	30·087	29·840	29·66	29·99	29·96	40	21	32	32	33	ne.	n.	s.	...	...	·05
4.	30·113	29·982	29·80	29·84	29·71	46	35	34	41	38	s.	w.	s.	·02	...	...
5.	29·840	29·838	29·55	29·66	29·64	49	39	39	42	42½	sw.	s.	sse.	·12	...	...
6.	29·897	29·862	29·57	29·43	29·61	55	44	41	43	41	sw.	s.	w.	·02	...	·19
7.	29·937	29·917	29·60	29·77	29·76	54	34	46	42	38	sw.	n.	se.	·11	·05	·22
8.	30·043	29·932	29·60	29·60	29·77	50	29	36	42	40½	sw.	s.	ssw.	·03	...	...
9.	30·158	30·141	29·80	29·68	29·68	56	36	41	45	44½	s.	s.	sse.	...	...	·11
10.	30·191	30·151	29·79	29·52	29·11	55	27	41	41½	41	sw.	ese.	ssw.	...	...	·02
11.	30·173	30·083	29·87	30·17	30·16	56	35	39	45	40	e.	e.	sse.	...	...	...
12.	30·030	29·906	29·73	30·02	29·86	56	28	39	41½	39	e.	e.	se.	...	...	...
13.	29·823	29·622	29·50	29·81	29·74	61	37	36	41	40½	s.	e.	se.	·36	...	...
14.	29·627	29·515	29·18	29·73	29·79	50	25	45	41½	39	sw.	ese.	ese.	·18	...	·06
15.	29·615	29·525	29·27	29·87	29·94	52	32	34	36	33	sw.	s.	e.	...	·43	·02
16.	29·631	29·594	29·33	29·99	30·00	41	31	41	35½	32	ne.	ene.	e.	...	·02	...
17.	29·890	29·829	29·56	30·03	30·09	33	22	34	31½	28½	ne.	e.	e.	...	·17	·02
18.	30·101	29·928	29·70	30·23	30·19	34	20	33	33½	31	e.	ene.	n.	...	·04	...
19.	30·150	30·132	29·87	30·11	30·10	40	20	30	40	31	n.	n.	...	...	...	·02
20.	30·116	29·946	29·76	29·98	29·94	44	22	34	39	30½	w.	nnw.	ene.	...	·07	·10
21.	29·829	29·764	29·56	30·06	30·16	42	23	30	33½	32	s.	n.	n.	·01	...	·06
22.	29·887	29·859	29·67	30·15	30·16	42	24	35	36	30	n.	n.	...	...	...	·12
23.	29·855	29·829	29·67	30·08	29·99	40	23	33	36	33	ne.	ne.	s.	...	·11	·05
24.	29·843	29·891	29·57	29·94	29·98	41	17	32	41	35	n.	n.	...	·01	...	·09
25.	29·916	29·847	29·66	29·92	29·92	41	18	33	40½	32	ne.	n.	s.	·06	...	·06
26.	30·025	29·966	29·75	29·80	29·57	44	17	28	37	41½	ne.	wnw.	w.	...	·04	·17
27.	30·063	29·999	29·72	29·86	30·02	50	27	39	42½	39	w.	w.	s.	·04	...	·22
28.	30·150	30·129	29·90	30·05	30·10	48	21	33	45	41½	e.	n.	w.	...	·12	...
29.	30·141	29·995	29·90	30·06	29·96	48	26	34	47	41	e.	s.	s.	...	...	...
30.	29·831	29·691	29·61	29·80	29·70	58	29	35	44	38	e.	s.	se.	...	...	...
31.	29·704	29·422	29·40	29·63	29·46	57	40	43	43	43	s.	s.	se.	·19	...	...
Mean.	29·931	29·844	29·61	29·854	29·844	47·32	27·51	36·2	39·59	36·90				1·48	1·09	1·79

LXV. *On Transient Electric Currents.* *By* Prof. WILLIAM THOMSON*.

THE object of this communication is to determine the motion of electricity at any instant after an electrified conductor, of given capacity, charged initially with a given quantity of electricity, is put in connexion with the earth by means of a wire or other linear conductor of given form and resisting power.   This linear conductor, which, to distinguish it from the other or principal conductor, will be called the discharger, is supposed to be of such small electrical capacity that the whole quantity of free electricity in it at any instant during the discharge is excessively small compared with the original charge of the principal conductor.   Now any difference that can exist in the strength of the current at any instant in different parts of the discharger must produce accumulations of free electricity in the discharger itself, and therefore must be very small compared with the actual strength of the current depending on the discharge of the principal conductor.   The strength of the current throughout the discharger will therefore be considered as the same at each instant, and, being measured by the quantity of electricity discharged per second, will be denoted by γ.   Again, the conducting property and extent of surface of the principal conductor, and the resistance of the discharger, will be considered as so related that the potential throughout the principal conductor is uniform at each instant.   Hence if q denote the quantity of electricity which the principal conductor possesses at any time t,

we have

$$\gamma = -\frac{dq}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

Now, if C denote the electrical capacity of the principal conductor, that is, the quantity of electricity which it takes to make the potential within it unity, the mechanical value or the " potential energy " of the distribution of a quantity q upon it is $\frac{1}{2}\frac{q^2}{C}$. As this diminishes from the commencement of the discharge, and varies during the whole period of the discharge, corresponding mechanical effects must be produced in the discharger according to the general law of " *vis viva*," or of the preservation of mechanical energy. The mechanical effects in the discharger are of two kinds,—first, the excitation or alteration of electrical motion ; secondly, the generation of heat. To estimate the first of these, it is necessary to know the mechanical value or the " actual energy " of an electrical current of given strength established and left without electromotive force in the discharger. In investigations which I have made towards a mechanical theory of electro-magnetic induction, I have found that the mechanical value of a current in a closed linear conductor is equal to the quantity of work that would have to be spent against the mutual electromagnetic forces between its parts in bending it from its actual shape into any other shape, while a current of constant strength is sustained in it by an external electromotive force, together with the mechanical value of the current in the conductor thus altered. According to Faraday's experiments (Experimental Researches, § 1090, &c.), it appears that the actual energy of a current in a linear conductor doubled upon itself throughout its whole extent, is either nothing, or such as to produce no sensible spark when the circuit is suddenly opened at any point ; that is, that what can be obviously interpreted as inertia of electricity either does not exist, or produces but insensible effects compared to those which have been attributed to the " induction of a current upon itself." According to these views, the actual energy of an electric current of given strength in a given closed linear conductor would be determined analytically by calculating the amount of work against mutual electro-magnetic actions required to double it upon itself throughout its whole extent ; but it may be that a more complete knowledge of the circumstances will show a term depending on electrical inertia which must be added to the quantity determined in that way to give the entire mechanical value of the current. However this may be, and whether the linear conductor be open or closed, it is obvious that the actual energy of a current established in it and left without electromotive force must be proportional to the square of the strength of the

current, and this is all that is required to be known for the present investigation. Let then $\frac{1}{2}A\gamma^2$ denote the actual energy of a current of strength, γ, in the linear conductor which serves for discharger in the arrangement which forms the subject of the present investigation, A being a constant which may be called the electrodynamic capacity of the discharger. The work spent in exciting electrical motion during the time dt will be

$$d\left(\frac{1}{2}A\gamma^2\right).$$

Again, the work done in generating heat in the same time is, according to Joule's law,

$$k\gamma^2 dt,$$

if k denote the "galvanic resistance" of the discharger, or the mechanical equivalent of the heat generated in it, in the unit of time by a current of unit strength[*]. Now the loss of potential energy from the principal conductor, in the time dt, being $-d\left(\frac{1}{2}\frac{q^2}{C}\right)$, is entirely spent in producing these effects; and therefore

$$-d\left(\frac{1}{2}\frac{q^2}{C}\right) = d\left(\frac{1}{2}A\gamma^2\right) + k\gamma^2 dt \quad . \quad . \quad . \quad . \quad (2).$$

This equation and (1), with the conditions

$$q = Q, \text{ and } \gamma = 0, \text{ when } t = 0 \quad . \quad . \quad . \quad . \quad (3),$$

are sufficient for the determination of q and γ for any value of t, that is, for the complete solution of the problem.

By (1) we have

$$-d\left(\frac{1}{2}\frac{q^2}{C}\right) = \frac{q}{C}\gamma dt,$$

and (2) becomes

$$\frac{q}{C}\gamma dt = A\gamma d\gamma + k\gamma^2 dt,$$

from which we find

$$q = C\left(A\frac{d\gamma}{dt} + k\gamma\right) \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Substituting for γ its value by (1), we obtain

$$\frac{d^2q}{dt^2} + \frac{k}{A}\frac{dq}{dt} + \frac{1}{CA}q = 0 \quad . \quad . \quad . \quad . \quad (5).$$

The general solution of this equation is

$$q = K\epsilon^{\rho t} + K'\epsilon^{\rho' t},$$

* See a paper entitled "Application of the Principle of Mechanical Effect to the Measurement of Electromotive Forces and Galvanic Resistances," Phil. Mag. Dec. 1851.

where ρ and ρ' are the roots of the equation

$$x^2 + \frac{k}{A}x + \frac{1}{CA} = 0.$$

Using equations (3) and (1) to determine the arbitrary constants K and K', and to derive an expression for γ, we obtain a complete solution of the problem which is expressed most conveniently by one or other of the following sets of formulæ, according as ρ and ρ' are real or imaginary:—

$$\left. \begin{aligned} q &= \frac{Q}{2\alpha A} \epsilon^{-\frac{k}{2A}t} \left\{ \left(\alpha A + \frac{k}{2}\right)\epsilon^{\alpha t} + \left(\alpha A - \frac{k}{2}\right)\epsilon^{-\alpha t} \right\} \\ \gamma &= \frac{Q}{2\alpha AC} \epsilon^{-\frac{k}{2A}t} \left\{ \epsilon^{\alpha t} - \epsilon^{-\alpha t} \right\} \end{aligned} \right\} \quad . \quad (6),$$

where

$$\alpha = \left(\frac{k^2}{4A^2} - \frac{1}{CA} \right)^{\frac{1}{2}},$$

$$\left. \begin{aligned} q &= \frac{Q}{\alpha' A} \epsilon^{-\frac{k}{2A}t} \left\{ \alpha' A \cos(\alpha' t) + \frac{k}{2}\sin(\alpha' t) \right\} \\ \gamma &= \frac{Q}{\alpha' AC} \epsilon^{-\frac{k}{2A}t} \cdot \sin(\alpha' t) \end{aligned} \right\} \quad . \quad (7).$$

where

$$\alpha' = \left(\frac{1}{CA} - \frac{k^2}{4A^2} \right)^{\frac{1}{2}}.$$

Among numerous other beautiful applications of his "electro-dynamometer," Weber has shown a method of determining what he calls the "duration"* of a transient electric current. In accordance with the terms he uses, the duration, and the *mean strength* of a transient current may be defined respectively as the duration and the strength that a uniform current must have to produce the same effects on the electro-dynamometer and on an ordinary galvanometer; so that if T and Γ denote the duration and the mean strength of a current, of which the actual strength at any instant is γ, we have

$$\left. \begin{aligned} T &= \frac{\left\{ \int_0^\infty \gamma \, dt \right\}^2}{\int_0^\infty \gamma^2 \, dt} \\ \Gamma &= \frac{\int_0^\infty \gamma^2 \, dt}{\int_0^\infty \gamma \, dt} \end{aligned} \right\} \quad . \quad . \quad . \quad (8);$$

* "Bestimmung der Dauer momentaner Ströme mit dem Dynamometer nebst Anwendung auf physiologische Versuche," § 13 of Weber's *Electro-dynamische Maasbestimmungen*, Leipsic 1846.

since the electro-dynamometer indicates the value of $\int_0^\infty \gamma^2 dt$, and the ordinary galvanometer that of $\int_0^\infty \gamma dt$. If for γ we use the expression in either (6) or (7), we find

$$\int_0^\infty \gamma^2 dt = \frac{1}{2} \frac{Q^2}{kC}, \quad \cdots \quad \cdots \quad (9),$$

as might have been foreseen, independently of the complete solution, by considering that, as the heat generated in the discharger is the sole final effect produced by the discharge, the mechanical value of the whole heat generated, or $\int_0^\infty k\gamma^2 dt$, must be equal $\frac{1}{2} \frac{Q^2}{C}$, the mechanical value of the primitive charge, and that k has been assumed to have a constant value during the discharge. Again, we derive from (1) and (3),

$$\int_0^\infty \gamma dt = Q \quad \cdots \quad \cdots \quad (10).$$

Hence in the present case the expressions for the duration and mean strength of the current become

$$\left.\begin{array}{c} T = 2kC \\ \Gamma = \dfrac{Q}{2kC} \end{array}\right\} \quad \cdots \quad \cdots \quad (11).$$

We conclude that the " duration " of the discharge is proportional to the capacity of the principal conductor, and to the resistance of the discharger; and that it is independent of the quantity of electricity in the primitive charge, and of the electro-dynamical capacity (denoted above by A) of the discharger. The only doubtful assumption involved in the preceding investigation is that of the constancy of k during the discharge. Joule's experiments show that the value of k remains unchanged for the same metallic conductor kept at the same temperature, whatever be the strength of the current passing through it; but that it would be increased by any elevation of temperature, whether produced by the current itself or by any other source of heat, since an elevation of temperature always increases the galvanic resistance of a metal. When large quantities of electricity are discharged, or when the discharger is a very fine wire, great augmentations and diminutions may therefore take place in the value of k, and therefore the solution obtained above is not applicable to such cases. If, however, k denote the mean resistance of the discharger during the discharge, that is, a quantity such that

$$k \int_0^\infty \gamma^2 dt = \int_0^\infty \kappa \gamma^2 dt \quad \cdots \quad \cdots \quad (12),$$

where κ denotes the actual resistance at any instant of the discharge, the last equations (11) become merely the expressions for the elements determined by Weber from observations by means of the two instruments, and they are therefore applicable to all cases.

In the experiments described by Weber, the discharger consisted of a wet cord of various lengths, and all the wire of the electrodynamometer and the ordinary galvanometer. The "durations" of the discharge in different cases were found to be nearly proportional to the length of the wet cord, and equal to ·0851 sec., or about $\frac{1}{12}$th of a second, when the length of the cord was 2 metres. As the principal resistance must undoubtedly have been in the wet cord, we may infer from equations (11) and (12) that the mean resistances in all the different discharges must have been nearly proportional to its lengths. In some of the experiments the length was only $\frac{1}{4}$ of a metre, and the value of T was about ·0095 of a second. Hence the current in the string must have been about eight times as intense as when the duration was $\frac{1}{12}$, since the quantities of electricity discharged were nearly equal in the different cases. We conclude that the intensity of the current cannot have materially affected the resisting power of the cord; probably not nearly so much as inevitable differences arising from accidental circumstances in the different experiments. Hence, although nothing is known with certainty regarding the non-electrolytic resistance of liquid conductors in general, it is probable that the whole resistance of the discharger in Weber's experiments must have been nearly independent of the strength of the current at each instant; and we may therefore consider the general solution expressed above by (6) or (7) as at least approximately applicable to these cases.

The two forms (6) and (7) of the solution of the general problem indicate two kinds of discharge presenting very remarkable distinguishing characteristics. Thus in all cases in which $\frac{k^2}{4A^2}$ exceeds $\frac{1}{CA}$, the exponentials in (6) are all real; and the solution expressed by these equations shows that the quantity of electricity on the principal conductor diminishes continuously, and that the discharging current commences and gradually increases in strength up to a time given by the equation

$$-\left(\frac{k}{2A}-\alpha\right)\epsilon^{-\left(\frac{k}{2A}-\alpha\right)t}+\left(\frac{k}{2A}+\alpha\right)\epsilon^{-\left(\frac{k}{2A}+\alpha\right)t}=0,$$

or

$$t=\frac{1}{2\left(\frac{k^2}{4A^2}-\frac{1}{CA}\right)^{\frac{1}{2}}}\log\frac{\frac{k}{2A}+\left(\frac{k^2}{4A^2}-\frac{1}{CA}\right)^{\frac{1}{2}}}{\frac{k}{2A}-\left(\frac{k^2}{4A^2}-\frac{1}{CA}\right)^{\frac{1}{2}}}\quad . \quad (13);$$

after which it diminishes gradually, and, as well as the quantity of electricity on the principal conductor, becomes nothing when $t = \infty$. On the other hand, when $\frac{1}{CA}$ exceeds $\frac{k^2}{4A^2}$, the exponentials and trigonometrical functions in (7) are all real; and the solution expressed by these equations shows that the principal conductor loses its charge, becomes charged with a less quantity of the contrary kind of electricity, becomes again discharged, and after that charged with a still less quantity of the same kind of electricity as at first, and so on for an infinite number of times before equilibrium is established. The times at which the charge of either kind of electricity on the principal conductor is a maximum, being those at which γ vanishes, are the roots of the equation $\sin(\alpha' t) = 0$, and therefore follow successively from the commencement at equal intervals $\frac{\pi}{\alpha'}$. The quantities constituting the successive maximum charges are

$$Q, \quad -Qe^{-\frac{k\pi}{2A\alpha'}}, \quad Qe^{-\frac{2k\pi}{2A\alpha'}}, \quad -Qe^{-\frac{3k\pi}{2A\alpha'}}, \&c. \quad . \quad (14);$$

each being less in absolute magnitude than that which precedes it in the ratio of $1 : e^{\frac{k\pi}{2A\alpha'}}$, and of the opposite kind. The strength of current will be a maximum in either direction when $\frac{d\gamma}{dt} = 0$, or when

$$\frac{k}{2A} \sin(\alpha' t) = \alpha' \cos(\alpha' t) ;$$

and therefore if T_1, T_2, &c. denote the successive times when this is the case, measured from the commencement of the discharge, and γ_1, γ_2, &c. the corresponding maximum values of the strength of the current, and if θ denote the acute angle satisfying the equation $\tan \theta = \frac{2A\alpha'}{k}$, we have

$$T_1 = \frac{\theta}{\left(\frac{1}{CA} - \frac{k^2}{4A^2}\right)^{\frac{1}{2}}}, \quad T_2 = \frac{\theta + \pi}{\left(\frac{1}{CA} - \frac{k^2}{4A^2}\right)^{\frac{1}{2}}}, \quad T_3 = \frac{\theta + 2\pi}{\left(\frac{1}{CA} - \frac{k^2}{4A^2}\right)^{\frac{1}{2}}}, \&c.,$$

$$\gamma_1 = \frac{Q}{A\left(\frac{1}{CA} - \frac{k^2}{4A^2}\right)^{\frac{1}{2}}} e^{-\frac{k}{2A}T_1}, \quad \gamma_i = \left(\frac{-1}{e^{\frac{k\pi}{2A\alpha'}}}\right)^i \gamma_1.$$

It is probable that many remarkable phænomena which have been observed in connexion with electrical discharges are due to the oscillatory character which we have thus found to be pos-

sessed when the condition

$$\frac{1}{CA} > \frac{k^2}{4A^2} \text{ or } C < \frac{4A}{k^2} \quad . \quad . \quad . \quad . \quad (15)$$

is fulfilled. Thus if the interval of time $\dfrac{\pi}{\left(\dfrac{1}{CA} - \dfrac{k^2}{4A^2}\right)^{\frac{1}{2}}}$, at which

the successive instants when the strength of the current is a maximum follow one another, be sufficiently great, and if the evolution of heat in any part of the circuit by the current during several of its alternations in direction be sufficiently intense to produce visible light, a succession of flashes diminishing in intensity and following one another rapidly at equal intervals will be seen. It appears to me not improbable that double, triple, and quadruple flashes of lightning which I have frequently seen on the continent of Europe, and sometimes, though not so frequently in this country, lasting generally long enough to allow an observer, after his attention is drawn by the first light of the flash, to turn his head round and see distinctly the course of the lightning in the sky, result from the discharge possessing this oscillatory character. A corresponding phænomenon might probably be produced artificially on a small scale by discharging a Leyden phial or other conductor across a very small space of air, and through a linear conductor of large electrodynamic capacity and small resistance. Should it be impossible on account of the too great rapidity of the successive flashes for the unaided eye to distinguish them, Wheatstone's method of a revolving mirror might be employed, and might show the spark as several points or short lines of light separated by dark intervals, instead of a single point of light, or of an unbroken line of light, as it would be if the discharge were instantaneous, or were continuous and of appreciable duration.

The experiments by Riess and others on the magnetization of fine steel needles by the discharge of electrified conductors, illustrate in a very remarkable manner the oscillatory character of the discharge in certain circumstances; not only when, as in the case with which we are at present occupied, the whole mechanical effect of the discharge is produced within a single linear conductor, but when induced currents in secondary conductors generate a portion of the final thermal equivalent.

The decomposition of water by electricity from an ordinary electrical machine, in which, as has been shown by Faraday, more than the electro-chemical equivalent of the whole electricity that passes appears in oxygen and hydrogen rising mixed from each pole, is probably due to electrical oscillations in the dis-

charger consequent on the successive sparks[*]. Thus, if the general law of electro-chemical decomposition be applicable to currents of such very short duration as that of each alternation in such an oscillatory discharge as may take place in these circumstances, there will be decomposed altogether as much water as is electro-chemically equivalent to the sum of the quantities of electricity that pass in all the successive currents in the two directions, while the quantities of oxygen and hydrogen which appear at the two electrodes will differ by the quantities arising from the decomposition of a quantity of water electro-chemically equivalent to only the quantity of electricity initially contained by the principal conductor. The formulæ investigated above will be applicable to this case if the end of the discharging train next the machine be placed in metallic communication with an insulated conductor, satisfying the conditions laid down with reference to the " principal conductor " at the commencement of this paper, and if this conductor be successively electrified by sparks from the machine. The whole quantity of water decomposed will therefore be the electro-chemical equivalent of the sum of the absolute values of the quantities of electricity flowing out of and into the principal conductor during the successive alternations of the current, that is, according to the preceding formulæ, the electro-chemical equivalent of the quantity,

$$Q\left(1 + 2\epsilon^{-\frac{k\pi}{2A\alpha'}} + 2\epsilon^{-\frac{2k\pi}{2A\alpha'}} + \&c.\right), \text{ or } Q\frac{1 + \epsilon^{-\frac{k\pi}{2A\alpha'}}}{1 - \epsilon^{-\frac{k\pi}{2A\alpha'}}},$$

of electricity. This quantity will be the greater the more nearly $\epsilon^{-\frac{k\pi}{2A\alpha'}}$ approaches to unity, that is the greater is $\frac{2A\alpha'}{k}$ or $\left(\frac{4A}{kC} - 1\right)^{\frac{1}{2}}$, or the greater is $\frac{4A}{kC}$. Hence the greater the electro-dynamic capacity of the discharger, the less its resistance, and the less

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[*] This explanation occurred to me about a year and a half ago, in consequence of the conclusions regarding the oscillatory nature of the discharge in certain circumstances drawn from the mathematical investigation. I afterwards found that it had been suggested, as a conjecture by Helmholtz, in his *Erhaltung der Kraft* (Berlin 1847), in the following terms :—

" [**] It is easy to explain this law if we assume that the discharge of a battery is not a simple motion of the electricity in one direction, but a backward and forward motion between the coatings, in oscillations which become continually smaller until the entire *vis viva* is destroyed by the sum of the resistances. The notion that the current of discharge consists of alternately opposed currents is favoured by the alternately opposed magnetic actions of the same ; and secondly, by the phænomena observed by Wollaston while attempting to decompose water by electric shocks, that both descriptions of gases are exhibited at both electrodes." [Quoted from the translation in Taylor's New Scientific Memoirs, Part II.]

the electro-statical capacity of the principal conductor, the greater will be the whole quantity of water decomposed. Probably the best arrangement in practice would be one in which merely a small ball or knob is substituted for a principal conductor fulfilling the conditions prescribed above ; but those conditions not being fulfilled, the circumstances would not be exactly expressed by the formulæ of the present communication. The resistance would be much diminished, and consequently the whole quantity of water decomposed much increased, by substituting large platinum electrodes for the mere points used by Wollaston ; but then the oxygen and hydrogen separated during the first direct current would adhere to the platinum plates, and would be in part neutralized by combination with the hydrogen and oxygen brought to the same plates respectively by the succeeding reverse current ; and so on through all the alternations of the discharge. In fact, if the electrodes be too large, all the equivalent quantities of the two gases brought successively to the same electrode will recombine, and at the end of the discharge there will be only oxygen at the one electrode and only hydrogen at the other, in quantities electro-chemically equivalent to the initial charge of the principal conductor. Hence we see the necessity of using very minute electrodes, and of making a considerable quantity of electricity pass in each discharge, so that each successive alternation of the current may actually liberate from the electrodes some of the gases which it draws from the water. Probably the most effective arrangement would be one in which a Leyden phial or other body of considerable capacity is put in connexion with the machine and discharged in sparks through a powerful discharger, not only of great electro-dynamic capacity and of as little resistance as possible except where the metallic communication is broken in the electrolytic vessel, but of great electro-statical capacity also, so that all, or as great a portion as possible, of the oscillating electricity may remain in it and not give rise to successive sparks across the space of air separating the discharger from the source of the electricity.

The initial effect of a uniform electromotive force in establishing a current in a linear conductor may be determined by giving C and Q infinite values in the preceding formulæ, and $\frac{Q}{C}$ a finite value V, which will amount to supposing the potential at one end of the discharger to be kept constantly at the value V, while the potential at the other end is kept at zero. The formulæ suitable to this case, which is obviously a case of non-oscillatory discharge, are (6) ; and from them we deduce

$$\gamma = \frac{V}{k} \left(1 - \epsilon^{-\frac{k}{A}t}\right). \quad . \quad . \quad . \quad . \quad (16),$$

which agrees with conclusions arrived at by Helmholtz and others.

This result shows how, when a linear conductor, initially in a state of electrical equilibrium, becomes subjected to a constant electromotive force V between its extremities, a current commences in it and rises gradually in strength towards the limit $\frac{V}{k}$. This limit cannot be perfectly reached in any finite time, although in reality only a very minute time elapses from the commencement in ordinary cases, until the current acquires so nearly the full strength $\frac{V}{k}$ that no further augmentation is perceptible*.

The equations (6), expressing generally a continuous discharge, assume the following forms when A is infinitely small,

$$\left.\begin{array}{l} \gamma = \dfrac{Q}{Ck}\,\epsilon^{-\frac{t}{kC}} \\[2ex] q = Q\epsilon^{-\frac{t}{kC}} \end{array}\right\} \quad \ldots \quad (17),$$

which show how, when anything like electrical inertia is insensible, the current commences instantly with its maximum strength, and then gradually sinks as the charge gradually and permanently leaves the principal conductor.

One of the results of the preceding investigation shows a very important application that may be made of Weber's experimental determination of the "duration" of a transient current, to enable us to determine the numerical relation between electro-statical and electro-magnetic units. For if σ denote the quantity of electricity in electro-statical measure which passes in the unit of time to constitute a current of unit strength in electro-magnetic measure, the strength of a current expressed above by γ will be $\frac{1}{\sigma}\gamma$, in terms of the electro-magnetic unit; and if K denote the resistance, in absolute electro-magnetic measure, of a linear conductor of which the resistance measured as above in terms of the electro-statical unit is k, we have

$$K\left(\frac{\gamma}{\sigma}\right)^2 = k\gamma^2,$$

which gives $$k = \frac{K}{\sigma^2} \quad \ldots \quad \ldots \quad (18).$$

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* See a paper by Helmholtz in Poggendorff's *Annalen*, 1852, which contains valuable researches, both theoretical and experimental, on this subject.

Hence the first of equations (11) gives

$$\sigma = \left(\frac{2\text{KC}}{\text{T}}\right)^{\frac{1}{4}} \quad . \quad . \quad . \quad . \quad . \quad (19).$$

Now Weber has not only determined T, in certain cases alluded to above, but has shown how K may be determined for any linear conductor. Again, the value of C for a Leyden phial is, according to Green and Faraday, expressed by the equation

$$\text{C} = \text{I} \frac{\text{S}}{4\pi\tau} \quad . \quad . \quad . \quad . \quad . \quad (20),$$

where S denotes the area of one side of the coated glass, τ the thickness of the glass, and I the specific inductive capacity of its substance*. Thus, either by using a Leyden phial or some other conductor of which the electro-statical capacity can be found, and by determining the "duration" of a discharge from it through a linear conductor, of which the resistance in absolute electro-magnetic measure has been determined, we have everything that is required for calculating the value of σ by means of equation (19). The determination of this quantity enables us to compare the electro-statical and electro-magnetic measures of electromotive force. For if V denote a constant difference of potentials kept up between the two extremities of a linear conductor, and if γ denote the strength of the uniform current that results, we have, according to the conclusions drawn above from (16),

$$\gamma = \frac{\text{V}}{k}.$$

But if F denote the electromotive force between the ends of the linear conductor in electro-magnetic measure, we have

$$\frac{1}{\sigma}\gamma = \frac{\text{F}}{\text{K}} = \frac{\text{F}}{\sigma^2 k},$$

and therefore

$$\text{F} = \sigma \text{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad (21).$$

Many different ways of determining the value of this important element, σ, besides that suggested above, might probably be put in practice. Perhaps the most accurate would be to take a multiple galvanic battery of constant and known electromotive force (consisting, for instance, of a hundred or more cells of Daniell's), and measure the force of attraction between two plane conduct-

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* The value of I for flint-glass is, according to Faraday (Experimental Researches, Series XI.), greater than 1·76, for shell-lac about 2, for sulphur rather more than 2·2. See a paper "On the Elementary Laws of Statical Electricity," § 8. (Cambridge and Dublin Mathematical Journal, vol. i. Nov. 1845.)

ors parallel to one another at a very small measured distance asunder, with only air between them. If X be the force of attraction thus measured, *a* the distance between the conductors, S the area of each or of that portion of each which is directly opposed to the other, and V the difference of the electrical potential kept up between them by the battery, we should have

$$V = a \left(\frac{8\pi X}{S} \right)^{\frac{1}{2}};$$

and therefore, if F be the electromotive force of the battery in electro-magnetic measure,

$$\sigma = \frac{F}{a \left(\frac{8\pi X}{S} \right)^{\frac{1}{2}}}.$$

LXVI. *On a proposed Test of the Necessity of Indirect Proof in Geometrical Demonstrations, with Remarks on Methods of Demonstration.* By James Adamson, *D.D.*

[Concluded from p. 338.]

AN earlier recognition of the true nature of axioms, and of the real use of definitions, would have restricted considerably the controversies which have arisen respecting parallelism. To assume a property as a criterion of parallelism, and another as a criterion of non-parallelism, except the one be proved to be a deduction from the other, would not only have been disapproved in Euclid, but have been avoided as contrary to reason. Considerable oversight as to the character of defining has also occurred in neglecting the principle, that the property assumed as the criterion of the thing defined must be single, or be a hypothesis alone, and not both a hypothesis and a conclusion.

The relation of parallels is such, that that which is true regarding two points, when a *given* line crosses them, or that which is true of two pairs of points, when there are *two* perpendiculars between them, determines necessarily the existence of the relation, and whatever other properties belong to it. Among the necessary consequences are the conclusions, that what is true of two points, or of two pairs of points, is true of all others ; but these properties must be made conclusions by demonstration. It is a remarkable characteristic of this relation, that sophisms intrude so unexpectedly into the minds of inquirers, and that the same failures are so often repeated. A sufficiently definite idea of the real source of the difficulty has not perhaps been entertained in these instances. However the properties under consideration may be treated, it is evident that somewhere in the series, either of the direct or of the converse statements,

it must happen that boundlessness of extent in two non-concurrent lines, combined with straightness in the individual lines, must occur as a hypothesis, from which, unaided by anything else, we must deduce positive conclusions as to equality of angles or of lines.   And it is as evident that whether we take as the defining criterion of parallelism, that *two* perpendiculars are equal, or that *two* alternate angles are equal, the difficulty equally awaits us, with regard to other perpendiculars and other alternate angles.

Assuming the hypothesis that two straight lines are equally inclined to a straight line crossing them, it is easy by a short argument to prove that if they met each other on either side of the crossing line, they would meet also on the other side of it, and that therefore they cannot meet, though produced on either side.   It would then be the natural mode of proceeding, and it has ever been the aim of geometricians, to take in hand for demonstration the converse statement, in which the hypothesis consists of the two facts of straightness and non-concurrence. The failure in this has mainly arisen from inattention to the nature of one of the facts of the hypothesis, and of its presence among the data, as determining the only mode of argument which the case admits of.   It has been hesitatingly suggested whether the doctrine of limits, as it is termed, can be admitted as an element in the proof.   Now the doctrine of limits means either that the existence of a limit to a set of varying objects exists, and may be demonstrated, or that there are opposite variations, real or imaginary, between which, following the law of continuity, a limit must exist.   In the first instance, that there is a limit is to be established, on a given hypothesis, as a fact.   The second affords undoubtedly the only mode by which, conversely, any conclusion can be deduced from the nature of a limit possessing the character of infinitude.   An unreal limit determined by unreal variations may be compared with a real one, so as to lead to a contradiction, when the limit so determined results from a false supposition.   We thence determine the *solitariness* of the real limit, or else that it *alone* can have the property which it is the object of our demonstration to attach to it.   It is thus that a curve is shown to be the solitary limit between interior straight lines enveloped by it, and exterior straight lines enveloping it, or that it is *greater* than the sum of the one set, and *less* than the sum of the other set; and its solitariness as the limit of any exterior set is demonstrated by the contradiction evolved from the assumption, that any set of exterior straight lines is the limit.   If however this solitariness be not established in such a case, the demonstration is a failure.

Now in regard to parallelism, it is to be observed that the

equality of inclination of two straight lines crossed by another, establishes the fact of their non-concurrence, so that their condition is the limiting one of all concurrent straight lines passing through the intersections of the crossing line. Instead therefore of the introduction of the doctrine of limits being a matter of doubt as to its propriety, it is the only logically legitimate mode in which the end can be attained, when we make the non-concurrence of the lines to be the hypothesis. The limiting case is in our hands as the only foundation of our reasoning. The argument has to predicate, "Because the straight lines never meet therefore,"—the hiatus never has been, and never can be supplied in any other than the mode now stated.

The late Sir John Lesslie introduced the doctrine of limits in this form,—that if there be a base and a perpendicular, and straight lines crossing the summit of the perpendicular and meeting the base, then, regarding the angles formed by these crossing lines on one side of the perpendicular, one group of them will form angles less than a right angle, and the other will form angles greater than a right angle; and since they diverge from the same point and approach one another, making, as they approach, angles which are more and more nearly equal, therefore the limit between the two groups, which does not meet the base on either side, must make angles which are equal, or are right angles. This seems the clearest mode of stating the argument, as rendering the notion of revolution unnecessary. But the argument fails in establishing the singleness of the limit of concurrence, for though the angles approach to right angles as the points of concurrence retire from the perpendicular, there is nothing *in the reasoning* to show that concurrence may not have ceased before the angles become right angles. It therefore leaves the point undetermined, whether there can, or cannot be more than one straight line through a point non-concurrent with another, and therefore fails to demonstrate that the straight line is necessarily perpendicular to both the non-concurrent straight lines, simply in virtue of their never meeting.

The defect however may be supplied as follows:—Let there be through the summit of the perpendicular a straight line which never meets the given base, then, if this be not also at right angles to the perpendicular, through the same point, *i. e.* the summit of the perpendicular, a straight line may make right angles with the perpendicular, and therefore never meet the given base. Hence there are two through the same point which never meet the given base, and since they form an angle, there must be an indefinite number having the same character. But through the same point there may be drawn innumerable straight lines which meet the given base, therefore there must be a limiting

one of this group, being the last of those which meet the given base, and forming, of all these lines, the furthest from the given perpendicular. Let such a straight line be drawn; then extend the base beyond it; and it is evident, that, from a point in the extension, another straight line may be drawn further from the perpendicular than that which is furthest. This is a contradiction, and therefore no other than the non-concurrent lines can be at right angles to the perpendicular, or it is perpendicular to both.

Legendre's mode of obviating this difficulty has been objected to, but not perhaps exactly at the point where it is objectionable. We may grant as legitimate, the conclusion that an angle of a triangle is, as to its magnitude, dependent on some function of the other two, or that $A = \phi(B, C)$; but though it be a truth, the certainty of which may mislead the mind in these inquiries, that when B and C are given A is determined, yet that truth does not necessarily result from this argument; for angles can be found which are determined when other two are given, but are not therefore absolutely determined to be equal. Even in the case before us, $180° - A$ and $90° - A$, or the supplement and complement of A, are expressible by the symbol ϕ (B, C), and so is any multiple or part of A. There is therefore nothing *in the nature of the argument,* whatever there may be in the nature of the facts, to exclude the position that when B and C are given there may be two triangles containing them, but having the other angles supplementary, or only definitely related, but not identical. In rectangular trihedral trigonometry (improperly called *spherical,* for arcs may have nothing to do with the matter), any one of three different angular quantities is expressible by $\phi(B, C)$, therefore from the fact that $A = \phi$ (B, C) and $A' = \phi$ (B, C), it is not logical to conclude $A = A'$.

All modes however of deriving anything from the properties of triangles is, in regard to this subject, inadmissible, from another consideration. If ever the vast advantage of *natural* arrangements in regard to scientific treatises is adequately conceived, then the subject of *angles* alone, independent of the magnitude or ratio of sides, will form the first or commencing section of elementary geometry. This will afford the subordinate sections of (*a*) angles at one point; (*b*) angles at two points, or parallelism; (*c*) angles at more than two points, or those of figures depending on the *number* of the sides. In proceeding from what is established regarding angles at one point to the subject of parallelism, logic *ought* not to fail, for there must be found in the nature of things reasoned about, sufficient foundations for all conclusions which are true regarding them. For this end we require a twofold preparation, consisting in a pro-

cess for the comparison of certain results, and a sound and suffi-
cient hypothesis expressed in a definition. The process alluded
to will be found in this, that if there be two equal straight lines,
and others at their extremities forming angles with them, which
are equal each to each, then we show by application and coin-
cidence, that if the straight lines attached to one of the given
equals meet each other, those attached to the other will also
meet. This is a consequence of *equality* in angles, which rela-
tion however we should take care to define. Our argument
affords the conclusion, that from a point in a straight line, or out
of it, there can be only one perpendicular to the straight line.

If, in proceeding onwards, we are to reason clearly, we must
avoid embarrassing ourselves with the *limit* as our hypothesis,
and also with those properties which require the limit to be in-
terposed as a hypothesis in our subsequent deductions. That
property of parallelism which satisfies the required conditions
may be thus stated. Parallel straight lines are such that every
straight line passing through a definite (*given* or *single*) point in
one of them, must, if prolonged, meet the other. If this defi-
nite point be in the one A, it is easy to show that every straight
line passing through any other point of A will meet the other B;
and from this truth, combined with the position that there can be
only one perpendicular to a straight line at or from any point,
the conclusion is obvious, that a straight line perpendicular to B
is also perpendicular to A; and then by the lemma above-men-
tioned, the converse position is deduced, that every straight line
which meets B must also meet A. Thus the defining test of
parallelism is extended to the form as a theorem—" that every
straight line which meets one must meet the other."

From this the conclusions readily emerge as theorems, " that
such straight lines never meet," and that " they are equally in-
clined to any straight line meeting them;" and as a corollary,
" that through any point a straight line may pass so as to be
parallel to another," according to the principle assumed in the
definition. This is a provision for treating the converses, among
which, in this mode of proceeding, the instance must occur in
which the *limit* of concurrence is the hypothesis. It is easily
deduced from what was stated above as to perpendiculars to both
of the parallels. Hence we demonstrate that straight lines
equally inclined to a given straight line are parallel, according
to the assumed definition. Hence the last converse statement
needed to complete the series is easily deduced, viz. if there be
two straight lines which, though produced ever so far, do not
meet, these lines are parallel; or are such, that every straight
line meeting the one must meet the other. This will follow
from the preceding argument, or more shortly, by assuming

that they are not parallel, and then applying the corollary above-mentioned, showing that through a point in one of them a straight line would pass so as to be parallel to the other, and therefore the two given straight lines would meet, which is a contradiction.

In the last two instances it might perhaps have been assumed, that if the given straight lines were not parallel, then through a point in either, a straight line might pass so as to be parallel to the other, in the sense of the assumed definition. This would have shortened the arguments, but is not so strictly logical. We are entitled to assume an example of anything defined, but are scarcely entitled to assume anything more than that one example, or some other exhibiting the assumed property under the same relation to other things.

The equality of the perpendiculars can be deduced from the same hypothesis, but this scarcely belongs to the same group of relations, inasmuch as it depends on and establishes the relations of straight lines as to length.

The following points will perhaps be found to be established by the considerations which have been offered :—

1. That definitions are the primary hypotheses on which all conclusions rest, and ought to be chosen so that these conclusions follow readily and clearly from them.

2. That in employing definitions or converses of theorems as *criteria,* the source of certainty is the reduction to a contradiction of some statement opposite in character to the criterion.

3. That all argument must necessarily, as to its nature, depend on the character of the hypothesis, and that logic fails only when this necessity is not attended to.

LXVII. *On Rubian and its Products of Decomposition*.*
By Edward Schunck, *F.R.S.*

Part II.†

Action of Alkalies and Alkaline Earths on Rubian.

IN the first part of this paper I have described, in general terms, the effect produced on rubian by alkalies. It is only the fixed alkalies that are capable of effecting the decomposition of rubian. Ammonia produces no perceptible change in a watery solution of this substance, except that of altering its colour from yellow to blood-red. This blood-red colour remains unchanged even after long-continued boiling, and the solution still contains rubian, for on supersaturating the ammonia with acid, the solution again becomes yellow, and not the slightest precipitate is produced. The fixed alkalies, on the other hand, act very

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† Part I. was given in the 3rd volume of this Journal.

differently. On adding caustic soda to a solution of rubian, the colour of the solution first changes to blood-red. On boiling the liquid, however, its colour soon changes again from blood-red to purple. This alteration in colour indicates the formation of alizarine. If the boiling be continued, there is deposited, as the liquid becomes more concentrated, a dark purple powder, which consists mainly of a compound of alizarine and soda, and separates in consequence of its insolubility in caustic lye. After the liquid has been boiled for some time, then, provided the quantity of soda employed be sufficiently large, the rubian is entirely decomposed. On now adding sulphuric acid in excess, a quantity of orange-coloured flocks, exactly like those produced by the action of acids on rubian, is precipitated, while the liquid becomes almost colourless. These flocks are separated by filtration and washed with cold water, until the sulphate of soda and sulphuric acid are entirely removed. They now consist mainly of four substances, viz. 1st, *Alizarine*; 2ndly, *Rubiretine*; 3rdly, *Verantine*; and 4thly, a substance which has not hitherto been observed, and to which I shall give the name of *Rubiadine*.

In order to separate these substances from one another, I adopt almost the same method as that employed for the separation of the products of decomposition with acids. The mixture is first treated with boiling alcohol, which dissolves the greatest part, leaving undissolved however a considerable quantity of a dark brown flocculent substance. This substance invariably accompanies the other products of the action. It is, however, as its properties and composition show, a secondary product of decomposition, the formation of which I shall explain presently. To the dark yellow alcoholic solution, after filtration from this flocculent substance, there is added acetate of alumina, which produces a dark red precipitate. This precipitate, which is a compound of alizarine and verantine with alumina, is decomposed with muriatic acid, and the alizarine and verantine are separated from one another by means of acetate of copper, and purified in the manner which I have before described, when treating of the products of decomposition with acids. The liquid filtered from this alumina compound is still yellow. On adding to it sulphuric acid and a large quantity of water, the substances dissolved in it are precipitated in the shape of yellow flocks, which after filtration and edulcoration are redissolved in boiling alcohol. On adding acetate of lead to this solution, there is produced a dark purplish-brown precipitate, which consists of rubiretine and verantine in combination with oxide of lead. The liquid still remains yellow, and is filtered from the precipitate. The latter, on being decomposed with boiling muriatic acid, gives a brown powder, which, after filtration and edulcoration, is treated

with cold alcohol.   This dissolves the rubiretine, leaving behind
the greatest part of the verantine.   The alcoholic liquid, after
filtration and evaporation, leaves a residue of rubiretine with its
usual appearance and properties.   Should it not melt easily when
thrown into boiling water, it must again be treated with a small
quantity of cold alcohol, when an additional portion of verantine
remains undissolved, and the alcohol on evaporation usually gives
pure rubiretine.   Should it, however, still contain verantine, the
process of treating with cold alcohol must be repeated, until it
is free from that substance.   The alcoholic liquid filtered from
the lead precipitate contains the substance which I call rubiadine.
In order to obtain it in a state of purity, water is added to the
solution.   This produces a yellow precipitate, which after filtra-
tion is treated with just sufficient boiling alcohol to dissolve
it.   To the solution while boiling, there is added hydrated oxide
of lead, which takes up the impurity, consisting chiefly of rubi-
retine, and renders the solution of a lighter yellow.   The liquid,
after being filtered boiling hot, deposits the rubiadine on cooling
in small yellow needles.   An additional quantity may be procured
by evaporating the mother-liquor, but this portion is not suffi-
ciently pure to assume a crystalline form, and has the appearance
of an amorphous yellow powder.   By exposing it to heat, however,
a crystalline sublimate of rubiadine may be obtained.

The acid liquid filtered from the mixture of the four preceding
substances still contains in solution another product of decom-
position.   After neutralizing the sulphuric acid with carbonate
of lead, and evaporating the filtered liquid to dryness, a residue
is left, which is treated with alcohol.   This leaves undissolved
the sulphate of soda, and after filtration and evaporation gives a
brown glutinous mass, which has all the properties of the sugar
produced by the action of acids on rubian.   It is this sugar,
which, by the continued action on it of the caustic alkali, gives
rise to the formation of the dark brown flocculent substance,
insoluble in alcohol, which is found mixed with the four pre-
ceding products of decomposition.   This fact is sufficiently proved
by the composition of the latter substance, which is identical with
that of the brown substance produced by the action of strong
acids on ordinary sugar.

The products formed by the action of caustic baryta on
rubian do not differ from those resulting from the action of
caustic soda.   On adding caustic baryta to a solution of rubian
and boiling for some time, the rubian is entirely decomposed;
the decomposition, as might be supposed, requiring a rather
longer time for its completion than in the case of caustic soda.
After the process is finished, the products of decomposition are
found for the most part in combination with baryta in the shape

of a purple powder, while the liquor still retains a red colour. The purple powder is placed on a filter and slightly washed with cold water. On passing carbonic acid through the filtered liquid, the latter loses its red colour, and a yellow flocculent substance is precipitated together with carbonate of baryta. The liquid, after being boiled in order to decompose the bicarbonate of baryta, and then filtered, is evaporated over sulphuric acid at the ordinary temperature, when it leaves a substance of a light brown colour, which resembles the sugar produced by the action of acids on rubian, but is not quite so deliquescent. This substance, as I shall presently show, is in fact a baryta compound of sugar. The mixture of yellow flocks and carbonate of baryta is now added to the purple powder before mentioned, and the whole is treated with muriatic acid in order to extract the baryta. The acid leaves undissolved a quantity of orange-coloured flocks, which consist, as before, of alizarine, rubiretine, verantine, and rubiadine, as well as of some of the dark brown flocculent substance, insoluble in boiling alcohol, and are separated from one another in the manner just described. If sulphuric acid be employed instead of carbonic acid for separating the baryta from the red liquid, and the excess of acid be neutralized with carbonate of baryta, the liquid on evaporation gives a substance exactly similar to that obtained in the other case, being also a compound of sugar and baryta, but differing from the latter in containing only half the quantity of baryta. The identity of the alizarine and rubiretine obtained by the action of alkalies, with those produced by the action of acids on rubian, is proved by the following analyses.

0·2255 grm. crystallized alizarine obtained by means of caustic soda, lost on being heated in the water-bath, 0·0405 water =17·96 per cent.

0·1820 grm. of the dry substance, burnt with chromate of lead*, gave 0·4610 carbonic acid and 0·0770 water.

These numbers correspond in 100 parts to—

Carbon	.	.	.	.	.	69·07
Hydrogen	.	.	.	.	.	4·70
Oxygen	.	.	.	.	.	26·23

I. 0·2610 grm. rubiretine, prepared by means of caustic soda, and dried at 100° C., gave 0·6565 carbonic acid and 0·1250 water.

II. 0·3630 grm. rubiretine, prepared by means of caustic baryta, gave 0·9130 carbonic acid and 0·1715 water.

* In order to avoid repetition, I may state that all the organic analyses given in this paper, in which the material used for combustion is not especially mentioned, were performed with chromate of lead.

These numbers correspond in 100 parts to—

	I.	II.
Carbon	68·60	68·59
Hydrogen . . .	5·32	5·24
Oxygen	26·08	26·17

I did not obtain a sufficient quantity of verantine in a pure state for the purpose of analysis, but the properties of the substance formed by the action of alkalies on rubian corresponded so exactly with those of the verantine produced by the action of acids, as to leave no doubt of their identity.

The compound of sugar and baryta, obtained by the action of baryta on rubian, is similar in appearance to the sugar itself. Its colour is light brown, and when dry it is brittle and transparent like dried gum or varnish. It exhibits no trace of crystalline form. It is more easily reduced to a state of dryness than the sugar itself, and deliquesces much less when exposed to a moist atmosphere than the latter. When its solution is mixed with acetate of lead, the whole of the sugar seems to be precipitated in combination with oxide of lead. On analysing a specimen of this substance prepared by precipitating the excess of baryta with carbonic acid, I obtained the following results:—

I. 0·6375 grm. heated for several hours in the water-bath, gave 0·5245 carbonic acid and 0·1945 water.

II. 0·7575 grm. gave 0·6320 carbonic acid and 0·2235 water.

0·4860 grm. gave 0·3480 sulphate of baryta, equivalent to 0·22838 baryta.

From these numbers may be deduced the following composition:—

	Eqs.		Calculated.	I.	II.
Carbon . . .	12	72	22·20	22·43	22·69
Hydrogen . .	11	11	3·39	3·39	3·26
Oxygen . . .	11	88	27·14		27·06
Baryta · . .	2	153·2	47·27		46·99
		324·2	100·00		100·00

If the baryta contained in the solution of sugar be precipitated with sulphuric acid, and the latter be again neutralized with carbonate of baryta, there is obtained, as I mentioned above, a compound of sugar and baryta containing less baryta than the preceding. Its analysis yielded the following results:—

I. 0·4330 grm. gave 0·4670 carbonic acid and 0·1825 water.

II. 0·3770 grm. heated for several hours longer at 100° C., gave 0·4150 carbonic acid and 0·1570 water.

0·3890 grm. gave 0·1890 sulphate of baryta, equivalent to 0·1240 baryta.

Hence may be deduced the following composition :—

	Eqs.		Calculated.	I.	II.
Carbon . . .	12	72	30·17	29·41	30·01
Hydrogen . .	10	10	4·19	4·68	4·62
Oxygen. . .	10	80	33·54		33·49
Baryta . . .	1	76·6	32·10		31·88
		238·6	100·00		100·00

This compound, it will be seen, is identical in composition with the baryta compound of cane-sugar. If the formulæ of the two compounds be compared together, it is evident that if both be correct, the one containing most baryta must be written thus,—$C^{12} H^{10} O^{10} + BaO + BaO\,HO$. In this case, however, it would be difficult to conceive how it is possible for the second atom of baryta to escape the action of the carbonic acid used in its preparation. It is therefore more probable that the true formula is $C^{12} H^{10} O^{10} + 2BaO$, which requires in 100 parts,—

Carbon	. . . . .	22·84
Hydrogen	. . . .	3·17
Oxygen	. . . . .	25·39
Baryta	. . . . .	48·60

In fact the amount of carbon and hydrogen found by experiment agrees better with these numbers than with those of the other formula.

These analyses lead to the conclusion, that the sugar formed by the decomposition of rubian contains 12, not 14 atoms of carbon. Nevertheless it is doubtful whether in these baryta compounds the sugar exists in the same state as it does when formed by the action of acids on rubian. In the latter state it seems incapable of entering into combination with bases such as baryta or oxide of lead. It is therefore probable that by the continued action on it of alkalies or alkaline earths it undergoes some change, possibly in consequence of the loss of the elements of water.

Among the products of the action of alkalies on rubian, I have mentioned a dark brown substance insoluble in boiling alcohol. This substance is soluble in alkalies with a brown colour, and is reprecipitated by acids. It resembles in all respects the black substances bearing the general name of humus, which are formed by the action of strong acids and alkalies on ordinary sugar. It was analysed with the following result :—

0·3215 grm. gave 0·8040 carbonic acid and 0·1260 water.

In 100 parts therefore it contains—

Carbon	. . . . .	68·20
Hydrogen	. . . . .	4·35
Oxygen	. . . . .	27·45

The ulmic acid of Mulder, obtained by the action of muriatic acid on cane-sugar, contains, according to that chemist ($C = 6\cdot11$),—

Carbon	.	.	.	.	.	68·95
Hydrogen	.	.	.	.	.	4·23
Oxygen	.	.	.	.	.	26·82

The formula given by Mulder for ulmic acid, viz. $C^{40} H^{14} O^{12}$, requires in 100 parts—

Carbon	.	.	.	.	.	68·57
Hydrogen	.	.	.	.	.	4·00
Oxygen	.	.	.	.	.	27·43

The identity in composition and properties of this acid, and the substance formed by the action of alkalies on rubian, leave no doubt of their being the same.

Rubiadine.—This substance, which has not hitherto been observed among the products derived from madder, bears a close resemblance in its appearance and many of its properties to rubianine, the place of which it in fact occupies in the series of substances produced by the action of alkalies on rubian. Besides its composition, however, there are several properties belonging to it so characteristic, that it cannot be confounded with rubianine or any of the substances previously described. When crystallized from alcohol, it is obtained in the shape of small yellow or orange-coloured needles. A very minute degree of impurity, however, seems to prevent its assuming a crystalline form, in which case it is obtained in small granular masses, or as a yellow amorphous powder. It may be purified by dissolving it in a small quantity of boiling alcohol, and adding to the boiling solution either hydrated oxide of lead, or protoxide of tin. On filtering boiling hot and allowing to cool, it crystallizes out. When heated on platinum foil, it melts and burns with flame. When cautiously heated between two watch-glasses, it may be almost entirely volatilized. On the lower glass a very slight carbonaceous residue is left, while the upper glass is covered with a quantity of partly yellow, partly orange-coloured micaceous scales, endowed with considerable lustre. These scales possess all the properties of rubiadine itself. Rubiadine is insoluble in water. It communicates hardly any colour to boiling water, and the filtered liquid deposits nothing on cooling. It is more soluble in alcohol than rubianine. It dissolves in concentrated sulphuric acid with a dark yellow colour, and is reprecipitated by water in yellow flocks. If the solution in sulphuric acid be boiled, the colour changes to a dark yellowish-brown, a little sulphurous acid is disengaged, and the addition of water now causes a yellowish-brown precipitate. On treating rubiadine

with boiling nitric acid, it dissolves, nitrous acid is disengaged, and the liquid on cooling deposits nothing, so that the substance seems to be decomposed by the acid. Towards alkalies rubiadine behaves in a similar manner to rubianine. Ammonia and carbonate of soda change its colour very little in the cold. It is only on boiling the alkaline liquids that it dissolves with a blood-red colour. It is precipitated from its alkaline solutions by acids in thick yellow flocks. The ammoniacal solution on exposure to the air loses its ammonia and deposits the substance in the shape of a yellow uncrystalline pellicle. On adding chloride of barium to the ammoniacal solution, no effect is produced at first, but after some time a slight dark red precipitate falls. Chloride of calcium, added to the ammoniacal solution, produces almost immediately a copious light red precipitate. Acetate of lead gives no precipitate in an alcoholic solution of rubiadine, and water throws down rubiadine in an uncombined state. On adding acetate of copper to the alcoholic solution, no effect ensues at first beyond a darkening of the solution, but after a few moments a dark brownish-red precipitate falls; the supernatant liquid remains yellow, but contains very little rubiadine in solution. When treated with a boiling solution of perchloride of iron, rubiadine does not dissolve. The liquid changes very slightly in colour, and gives after filtration only a very slight precipitate on the addition of muriatic acid. The residue left undissolved by the perchloride of iron is unchanged rubiadine. By its volatility, when exposed to heat, and by its insolubility in boiling water, rubiadine may be easily distinguished from rubianine, which cannot be strongly heated without decomposition, and is soluble in boiling water.

I only obtained sufficient rubiadine in a state of purity for one analysis, which gave the following results :—

0·2575 grm. gave 0·6725 carbonic acid and 0·1120 water.

These numbers correspond in 100 parts to—

Carbon	. . . . .	71·22
Hydrogen	. . . .	4·83
Oxygen	. . . . .	23·95

There are several formulæ with which this composition agrees, but only two which at the same time satisfactorily explain the formation of this substance, viz. $C^{32} H^{12} O^8$ and $C^{44} H^{15} O^{11}$. The great similarity in properties between rubiadine and rubianine makes it probable that both have a similar composition, and as it is almost certain from the experiments above detailed that the formula of the sugar resulting from the decomposition of rubian is $C^{12} H^{12} O^{12}$, it follows that the true formula of rubiadine must be one of the two just given.

These formulæ require respectively in 100 parts—

	$C^{32}H^{12}O^{8.}$	$C^{44}H^{15}O^{11}.$
Carbon	71·64	71·93
Hydrogen . . .	4·47	4·08
Oxygen · . . .	23·89	23·99

If the first formula be the correct one, then the formation of rubiadine is due to the splitting up of 1 equiv. of rubian, after the assimilation of 2 equivs. of water, into 1 equiv. of rubiadine and 2 equivs. of sugar, as will be seen by the following equation :—

$$2 \text{ eqs. of Sugar} = C^{24}H^{24}O^{24} \atop 1 \text{ eq. of Rubiadine} = C^{32}H^{12}O^{8} \Big\} = \Big\{ C^{56}H^{34}O^{30} = 1 \text{ eq. of Rubian.} \atop H^{2}O^{2} = 2 \text{ eqs. of Water.}$$

$$\overline{C^{56}H^{36}O^{32}} \qquad \overline{C^{56}H^{36}O^{32}}$$
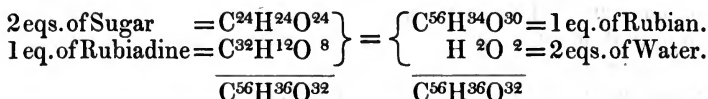
If the formula of rubianine be $C^{32}H^{19}O^{15}$, rubiadine will differ from it by containing the elements of 7 equivs. of water less. If the formula of rubiadine be $C^{44}H^{15}O^{11}$, then its formation would presuppose the splitting up of 1 equiv. of rubian into 1 equiv. of rubiadine, 1 equiv. of sugar and 7 equivs. of water, for

$$C^{44}H^{15}O^{11} + C^{12}H^{12}O^{12} + 7HO = C^{56}H^{34}O^{30}.$$
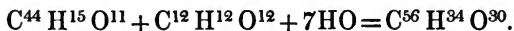
It appears therefore that the action of alkalies on rubian differs in its results from the action of acids only in one respect, that alkalies as well as acids give rise to the formation of five distinct products of decomposition, that four of these are in both cases perfectly identical, and that the fifth substance produced by the action of acids is replaced in the case of alkalies by another, which, though perfectly distinct, is so similar both in properties and composition, that it may be considered as in every respect its equivalent.

Action of Ferments on Rubian.—It has long been suspected by chemists that the colouring matter of madder owes its formation to some process of fermentation, but the exact nature of the process has hitherto remained unknown.   Mr. Higgin first pointed out the fact, that a formation of colouring matter takes place even during the short period occupied in the process of dyeing, and he attributes it to the action of some albuminous substance contained in madder on xanthine.   That some process of decomposition takes place on extracting madder with cold or tepid water and exposing the extract to a moderate temperature, is proved by the fact that the extract, if concentrated, becomes after some time thick and gelatinous; and that the process of decomposition takes effect chiefly on the rubian is apparent, since the extract, after it has become gelatinous, is found to have lost its bitter taste and the greater part of its yellow colour.

In order to prepare the peculiar substance contained in madder which has the property of effecting the decomposition of rubian, I proceed in the following manner. A quantity of madder having been placed on a piece of calico or fine canvas, there is poured on it for every pound of madder taken about four quarts of distilled water, which may be either cold or of a temperature of about 38° C. The latter temperature is preferable, as the water then percolates more rapidly. To the extract there is added, without any delay, about an equal volume of alcohol, which causes the separation of a quantity of dark reddish-brown flocks. These flocks are allowed to settle, and the supernatant yellowish-brown liquid is decanted. Fresh alcohol is added to the flocks, which are then placed on a paper filter, and washed on the filter with additional quantities of alcohol, until the percolating liquid, which is at first strongly coloured, becomes almost colourless. This process of washing, which often lasts a long time, may be assisted by removing the mass from the filter and shaking it up with moderately warm alcohol. The substance on the filter has now the appearance of a dark reddish-brown mass, which when rubbed between the fingers produces the same granular feeling as coagulated caseine, the sliminess which characterized it on its first precipitation with alcohol having disappeared. Now this substance possesses in an eminent degree the power of effecting the decomposition of rubian. If a quantity of it be added to a solution of rubian, and the mixture be left to stand at the ordinary temperature, a complete change is found to have taken place in the course of a few hours. The liquid is converted into a trembling jelly of a light brown colour, which is perfectly tasteless, insoluble in cold water, and so consistent, that if the solution of rubian was tolerably concentrated, the vessel may be reversed without its falling out. It is manifest from this experiment that it is this substance which constitutes the active fermenting principle of madder, and that for the purpose of understanding the nature of the process of fermentation peculiar to madder, it is necessary to examine the action of this substance on rubian. I shall therefore in the first place describe the products resulting from this action, after which I shall return to the consideration of its other properties.

On adding to a watery solution of rubian a quantity of the ferment, prepared in the manner just described, the latter loses its granular nature and diffuses itself in the solution without actually dissolving, forming a muddy, mucilaginous liquid. On allowing this mixture to stand in a moderately warm place, the rubian is gradually decomposed and converted into a brown jelly, similar in appearance and consistency to coagulated blood. Here and there interspersed in the mass yellow streaks and

spots, consisting of long hair-like crystals, are sometimes dis-
cernible.   If, after twenty-four hours, the jelly on being mixed
with cold water still yields to the latter any yellow colour, it is
a sign that the rubian is not entirely decomposed, and more
ferment must be added, the mixture being again allowed to stand
until the liquid has become tasteless and colourless.   During
this process none of the usual signs of fermentation are mani-
fested.   The liquid remains perfectly neutral, and no gas of any
kind is disengaged.   In order however to be perfectly sure of
the latter point, I took a solution of rubian in water, mixed it
with ferment, poured the mixture into a graduated glass tube,
and inverted the latter over mercury.   After standing in this
way for several days, not a particle of gas had collected at the
top of the tube, notwithstanding which the rubian was entirely
decomposed, and on examination the products of decomposition
were found to be precisely the same as those formed in the usual
manner.   This experiment proves not only that no gas is evolved
during the fermentation of rubian, but also that, as Higgin has
observed, the access of atmospheric air is not necessary during
the process.

The decomposition of the rubian being completed, a little water
is added to the mass, and the gelatinous substance left undissolved
by the water is placed on a filter and slightly washed with cold
water.   The percolating liquid is almost colourless.   The gela-
tinous mass on the filter now consists partly of the ferment
employed, and partly of substances formed by the decomposition
of the rubian.   These substances are six in number, of which three
are bodies previously described, and three are new.   They are,—
1st, *Alizarine*; 2ndly, *Verantine*; 3rdly, *Rubiretine*; 4thly, a sub-
stance closely resembling rubiacine, which I shall call *Rubiafine*;
5thly, a substance very similar to rubianine and rubiadine, to
which I shall give the name of *Rubiagine*; and, 6thly, a peculiar
fatty substance which I shall denominate *Rubiadipine*.   These
substances are separated in the following manner.   The mixture
is treated with boiling alcohol.   The alcohol, which assumes a
dark reddish-yellow colour, is filtered, and the substance on
the filter is treated again with boiling alcohol, until the latter
acquires thereby only a light yellow colour.   The substance left
on the filter consists of the ferment, which has undergone no
apparent change either in quantity or properties.   To the alco-
holic liquid there is now added acetate of alumina, which pro-
duces a dull yellowish-red precipitate.   This precipitate, which
consists of alizarine, verantine and rubiafine in combination with
alumina, is separated by filtration and decomposed with boiling
muriatic acid, which leaves undissolved a quantity of orange-
coloured flocks.   The liquid filtered from the alumina precipitate

has a dark brownish-red colour. By adding to it sulphuric acid and a large quantity of water, the substances contained in it are precipitated in the shape of a yellow powder. This powder yields nothing to boiling water, which proves the absence of rubianine among the products of decomposition. After being separated by filtration, and washed with water to remove the acid and aluminous salts, it is again dissolved in boiling alcohol, and to the boiling solution there is added an excess of sugar of lead, which causes a dark purple precipitate, the liquid becoming dark yellow. The latter is filtered boiling hot and then mixed with a large quantity of water, which produces a dull orange-coloured precipitate, consisting of rubiagine and rubiadipine in combination with oxide of lead. This precipitate is boiled with sulphuric acid, which turns it yellow; and after the excess of acid has been removed with water, it is treated with boiling alcohol, which leaves undissolved a quantity of sulphate of lead, and acquires a yellow colour. The alcohol on evaporation leaves a residue consisting of rubiagine and rubiadipine. This residue is treated with cold alcohol, in which the rubiadipine dissolves easily, and is obtained on again evaporating as a dark brown, soft, fatty mass. The rubiagine left undissolved is treated with a small quantity of warm alcohol, which removes an additional quantity of rubiadipine, together with a portion of the substance itself, and on redissolving it in boiling alcohol and evaporating spontaneously, it appears in the form of a lemon-yellow mass consisting of small crystalline grains. The dark purple precipitate produced by sugar of lead consists of oxide of lead in combination with rubiretine, alizarine, verantine and rubiafine. It is treated with boiling muriatic acid, which dissolves the oxide of lead. The yellow flocks left by the muriatic acid are, after washing with water, treated with cold alcohol which leaves a part undissolved, and on evaporation gives a residue consisting for the most part of rubiretine. By treating this residue again with cold alcohol, an additional quantity of substance is left undissolved, and the rubiretine is now obtained in a state of greater purity. Its appearance and properties are the same as when obtained by the action of acids or alkalies on rubian. The substance left undissolved by the cold alcohol is added to the orange-coloured flocks proceeding from the decomposition of the alumina precipitate, and the mixture is dissolved in boiling alcohol. To the alcoholic solution is added acetate of copper, which gives a dirty purple precipitate, consisting of verantine, rubiafine and a portion of the alizarine in combination with oxide of copper, while the liquid, which has a fine purple colour, contains the greatest part of the alizarine. The latter is obtained from this liquid by precipitating with muriatic acid and water, and purified by crystallization. The precipitate produced by acetate of copper is de-

composed with muriatic acid, which leaves undissolved a quantity of red flocks. These, after filtration and washing with water, are dissolved in boiling alcohol. Into the boiling solution is introduced a quantity of hydrated protoxide of tin, which thereby acquires a light brown colour, while the liquid becomes light yellow, and on being filtered boiling hot and allowed to cool, deposits the rubiafine in yellow shining plates and needles, which are purified by recrystallization from alcohol. The oxide of tin-lake left on the filter is treated with cold muriatic acid, which dissolves the greatest part of the oxide of tin, and leaves undissolved a dark reddish-brown powder. This is placed on a filter, washed first with muriatic acid, then with water, and then treated with boiling alcohol. The alcohol leaves a great part of it undissolved, and on being filtered boiling hot deposits on cooling a brown powder consisting of verantine. The liquid on evaporation gives a quantity of alizarine, mixed with verantine. The substance left undissolved by the boiling alcohol is dark brown. It is a compound of verantine and oxide of tin, from which I have not been able to extract the oxide of tin by acids or any other means. It is soluble in ammonia and carbonate of soda, and is precipitated unchanged by acids.

The same products as those just mentioned may be obtained from madder by extracting it with cold or tepid water, allowing the extract to stand until a coagulum has beeen formed in it, placing the coagulum on a calico strainer and then treating it with boiling alcohol. The alcohol leaves undissolved a quantity of ferment, and is found to contain the same substances as those originating from the direct action of the ferment on rubian.

The liquid filtered from the gelatinous mixture of substances insoluble in water, formed by the action of the ferment, still contains in solution a considerable quantity of sugar. On adding to it a small quantity of caustic baryta, a pinkish-white precipitate falls, consisting probably of pectic acid in combination with baryta. The liquid being filtered, the baryta is precipitated with sulphuric acid, the excess of the latter is removed with carbonate of lead, and a small quantity of lead which dissolves is thrown down with sulphuretted hydrogen. The liquid now leaves, on evaporation at the ordinary temperature over sulphuric acid, a brownish-yellow syrup, having the same appearance and properties as the sugar produced by the action of acids on rubian.

The following analyses prove the identity of the alizarine formed by the fermentation of rubian with that derived from other sources :—

I. 0·5210 grm. alizarine, produced by the action of the ferment on rubian, lost, on being heated in the water-bath, 0·0920 water = 17·65 per cent.

0·2500 grm. of the dry substance gave 0·6380 carbonic acid and 0·0960 water.

II. 0·4220 grm., obtained by allowing an extract of madder with tepid water to ferment, lost, on being heated, 0·0750 water = 17·77 per cent.

0·3225 grm. of the dry substance gave 0·8230 carbonic acid and 0·1170 water.

These numbers correspond in 100 parts to—

		I.	II.
Carbon		69·59	69·59
Hydrogen . . .		4·26	4·03
Oxygen		26·15	26·38

The quantity of rubiretine and verantine formed by the fermentation of rubian does not seem to be so large as it is, when acids or alkalies are employed for its decomposition, and as the process of separating and purifying the products of fermentation is, on account of the number of products formed, rather more complicated, the quantity of each substance obtained in a pure state is but small. The following analyses of these two substances were made with specimens obtained by the fermentation of an extract of madder; and even by preparing them in this manner I had some difficulty in procuring a sufficient quantity for examination, because the rubiretine formed by fermentation is always found mixed with rubiadipine, from which it is with difficulty freed; and the verantine is for the most part obtained in combination with oxide of tin, from which I have found it impossible to separate it.

0·3445 grm. rubiretine gave 0·8580 carbonic acid and 0·1695 water.

This gives in 100 parts—

Carbon	67·92
Hydrogen	5·46
Oxygen	26·62

0·3705 grm. verantine gave 0·9010 carbonic acid and 0·1420 water.

In 100 parts—

Carbon	66·32
Hydrogen	4·25
Oxygen	29·43

The great excess of carbon in this analysis indicates an admixture of alizarine. This proceeds from the circumstance that the method of separating verantine and alizarine, by means of acetate of copper, is not absolute. When these two substances are present together in solution, acetate of copper precipitates a portion of the alizarine together with the verantine, and the latter

can afterwards only be purified by repeated solution in boiling alcohol, and deposition from the boiling solution.

The compound of verantine and oxide of tin, obtained in the process employed for purifying the rubiafine, cannot, as I mentioned before, be separated into its constituents. It dissolves in ammonia and carbonate of soda with a dark-brown colour, and is reprecipitated unchanged by acids. Even if it be dissolved in caustic soda, and an excess of sulphuretted hydrogen be passed through the solution, the precipitate afterwards produced by acids yields nothing to boiling alcohol, which proves that the verantine has not thereby been set at liberty. By treating it with boiling alcohol until all the matter soluble in alcohol is removed, then dissolving it in carbonate of soda, filtering from a small quantity of oxide of tin, then reprecipitating with acid, it is obtained in dark brown flocks, which when dry cohere into black, brittle, shining masses. Its analysis now gave the following results :—

I. 0·5930 grm., dried at 100° C., gave 0·6500 carbonic acid and 0·1800 water.

0·3410 grm., heated in a crucible until all the organic matter was destroyed, gave 0·1580 grm. peroxide of tin, equivalent to 0·1411 protoxide of tin.

II. 0·8390 grm. gave 0·9240 carbonic acid and 0·2455 water.

It contains therefore in 100 parts—

	I.	II.
Carbon	29·89	30·03
Hydrogen . . .	3·37	3·25
Oxygen	25·36	
Protoxide of tin .	41·38	

The formula $C^{56}H^{36}O^{36} + 7SnO = 4C^{14}H^{5}O^{5} + 7SnO + 16HO$ requires in 100 parts—

Carbon	29·76
Hydrogen	3·18
Oxygen	25·52
Protoxide of tin . . .	41·54

The sugar obtained by the fermentation of rubian does not differ in its properties, as I mentioned before, from that derived from the action of acids on rubian. In composition too it does not materially differ from the latter. I succeeded, however, by exposing it for a considerable length of time to a temperature of 100° C., in depriving it of two atoms more of water, as will be seen by the following analysis :—

0·5540 grm. gave 0·8745 carbonic acid and 0·3055 water.

These numbers correspond with the formula $C^{12}H^{10}O^{10}$, as will be seen by the following calculation :—

	Eqs.		Calculated.	Found.
Carbon . . .	12	72	44·44	43·84
Hydrogen . .	10	10	6·17	6·23
Oxygen. . .	10	80	49·39	49·93
		162	100·00	100·00

On subjecting it, however, for some time longer to the same temperature, it became very brown, and its analysis now showed that it had absorbed oxygen.

0·5820 grm. now gave 0·8745 carbonic acid and 0·3000 water. In 100 parts it contained therefore—

Carbon . . . . . . 40·97
Hydrogen . . . . . 5·72
Oxygen . . . . . . 53·31

On redissolving the remainder in water, it gave a brown solution, but on adding to the solution hydrated oxide of lead, the brown portion was removed, while the solution became almost colourless, and on evaporation over sulphuric acid left a yellow syrup, the composition of which again corresponded with the formula $C^{12} H^{12} O^{12}$, as will be seen by the following analysis:—

0·3710 grm. gave 0·5535 carbonic acid and 0·2135 water. In 100 parts:—

Carbon . . . . . . 40·68
Hydrogen . . . . . 6·39
Oxygen . . . . . . 52·93

Rubiafine.—In my former papers on madder I have described a substance which I called rubiacine, and which I prepared partly from madder itself, and partly by the reduction of rubiacic acid with sulphuretted hydrogen. Now rubiafine cannot be distinguished by any of its properties from rubiacine. It crystallizes from its alcoholic solution in yellow glittering plates and needles, which are sometimes arranged in star-shaped or fan-shaped masses. By carefully heating it, it may be volatilized without leaving much residue, forming a yellow sublimate of small shining needles. It is but slightly soluble in boiling water. It is not decomposed by boiling nitric acid or by concentrated sulphuric acid, but merely dissolved by them. It dissolves in caustic alkalies with a reddish-purple, and in carbonated alkalies with a red colour. Its alcoholic solution gives with sugar of lead a fine crimson precipitate, with acetate of copper an orange-coloured precipitate. It dissolves in a solution of pernitrate of iron with a dark brownish-purple colour. The solution, after being boiled for some time, gives, on the addition of muriatic acid, a yellow precipitate, which is rubiacic acid. These properties, it will be seen, belong also to rubiacine. Nevertheless the

composition of rubiafine is different, and as I have succeeded in again preparing a substance of the same composition as the rubiacine formerly obtained, they must be considered as distinct bodies.

I am unable to give more than one analysis of rubiafine, which yielded the following numbers :—

0·1765 grm., dried at 100° C., gave 0·4485 carbonic acid and 0·0725 water.

The relation in which rubiafine stands to rubiacic acid proves that the former contains 32 atoms of carbon.   Its composition must then be as follows :—

		Eqs.		Calculated.	Found.
Carbon.	.	32	192	69·31	69·30
Hydrogen	.	13	13	4·69	4·56
Oxygen	.	9	72	26·00	26·14
			277	100·00	100·00

If this be the true composition of rubiafine, then the latter stands in the same relation to rubian as rubianine and rubiadine, from which, if the latter also contain 32 atoms of carbon, it only differs by the elements of water.   If to rubian be added 3 equivs. of water, there are given the elements of 1 equiv. of rubiafine, and 2 equivs. of sugar, for

$$\left. \begin{array}{l} 2\,\text{eqs. of Sugar} \quad = C^{24}H^{24}O^{24} \\ 1\,\text{eq. of Rubiafine} = C^{32}H^{13}O\ ^9 \end{array} \right\} = \left\{ \begin{array}{l} C^{56}H^{34}O^{30} = 1\,\text{eq. of Rubian.} \\ \quad H\ ^3O\ ^3 = 3\,\text{eqs. of Water.} \end{array} \right.$$

$$\overline{C^{56}H^{37}O^{33}} \qquad\qquad \overline{C^{56}H^{37}O^{33}}$$
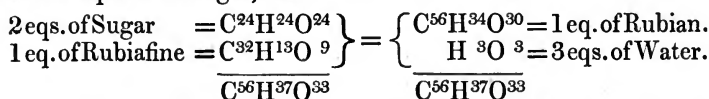
Rubiacine and Rubiacic Acid.—In my former papers on madder, I have given for rubiacic acid the formula $C^{31}H^8O^{16}$, and for rubiacine $C^{31}H^9O^{10}$.   In order to ascertain the correct formula, and also to determine the relation in which they stand to rubian and rubiafine, of which they must be derivatives, I prepared and submitted to analysis a fresh quantity of these substances.   For the purpose of preparing rubiacic acid, it is not necessary to take madder itself.   If any dyework is at hand in which madder-dyeing is carried on, the liquor in which the goods have been immersed, together with the madder, and which is generally allowed to run to waste, may be employed.   This liquid contains a considerable quantity of some substance, which by the action of pernitrate of iron may be converted into rubiacic acid. The mode of operation is as follows.   To the liquid, which is brown and muddy, there is added, after the separation of the woody particles of the madder, a quantity of muriatic acid, which gives a brown flocculent precipitate, while the liquid becomes clear and colourless.   This precipitate, after being collected on a calico strainer, is treated with pernitrate of iron until nothing more is dissolved.   The resulting dark reddish-brown liquid is

strained through calico, and acid is added to it, which produces a yellow precipitate. This is filtered, washed and dissolved in boiling carbonate of potash. The solution on cooling deposits crystals of rubiacate of potash, which are placed on a filter and washed with cold water. From a solution of the potash salt, the acid may be precipitated by muriatic or any other strong acid. Some rubiacic acid prepared in this manner was analysed with the following result :—

I. 0·3160 grm. gave 0·7090 carbonic acid and 0·1010 water. In 100 parts it contained, therefore,—

Carbon	61·19
Hydrogen	3·55
Oxygen	35·26

In my former experiments I obtained as a mean of three determinations the following numbers :—

Carbon	57·28
Hydrogen	2·47
Oxygen	40·25

I was at first unable to explain the great discrepancy here seen, as the new preparation both of the acid and the potash salt had the same appearance as usual. Before proceeding to explain how this discrepancy arose, I may state that the analysis just given corresponds with the formula $C^{64} H^{20} O^{27}$, which requires in 100 parts—

Carbon	61·93
Hydrogen	3·22
Oxygen	34·85

With the same specimen of potash salt as that employed for the preparation of the acid, I made a quantity of silver salt by dissolving the potash salt in water, and adding nitrate of silver. A brilliant cinnabar-red precipitate fell, which was filtered, washed and dried. It was not decomposed either by light or by the heat of boiling water.

0·5800 grm. of this salt, dried in the water-bath, gave 0·9800 carbonic acid and 0·1150 water.

0·3310 grm., treated in a crucible with muriatic acid, and heated until all the organic matter was destroyed, gave 0·1120 chloride of silver.

These numbers lead to the following composition :—

	Eqs.		Calculated.	Found.
Carbon . . .	64	384	46·04	46·08
Hydrogen . .	18	18	2·15	2·20
Oxygen . .	25	200	24·00	24·37
Oxide of silver	2	232	27·81	27·35
		834	100·00	100·00

I now took a fresh quantity of waste dye liquor, and divided it into two parts. To the first part I added acid. The precipitate produced by the acid was treated with boiling alcohol, which dissolved a small part with a deep yellow colour, and after being filtered boiling hot and allowed to cool, deposited a quantity of orange-coloured powder. This powder I dissolved again in boiling alcohol, and to the boiling solution I added hydrated protoxide of tin, and filtered boiling hot. The liquid, which had become light yellow, deposited on cooling a quantity of light yellow needles. These needles had the appearance and properties of rubiafine. Their composition, however, proved them to be a distinct substance.

0·1515 grm. gave 0·3730 carbonic acid and 0·0540 water.

In 100 parts, therefore, it consisted of—

Carbon	67·14
Hydrogen	3·96
Oxygen	28·90

To the other part of the liquor I also added acid, and the precipitate, after straining the liquor through calico, and washing with water, I treated with a boiling solution of pernitrate of iron, which dissolved a portion with a dark purplish-brown colour. Muriatic acid produced in the filtered liquid a yellow precipitate, which after filtering and washing with water, I treated with boiling alcohol. The alcohol being filtered boiling hot, deposited on cooling a yellow powder, which on being redissolved in fresh boiling alcohol, yielded a quantity of yellow shining plates and needles. These corresponded in all their properties with rubiafine and rubiacine. By treatment with pernitrate of iron they were convertible into rubiacic acid.

0·2355 grm. gave 0·5790 carbonic acid and 0·0860 water.

In 100 parts it contained, therefore,—

Carbon	67·05
Hydrogen	4·05
Oxygen	28·90

This analysis proves that the substance contained in the liquor existed in the same state before and after the treatment with pernitrate of iron, and that the latter, in the first instance at least, merely dissolved without changing it. A comparison of the composition of this substance with that of the body which I formerly called rubiacine, proves that they are identical. A specimen of rubiacine obtained on a former occasion from rubiacate of potash by means of sulphuretted hydrogen, I found to contain in 100 parts—

Carbon	67·01
Hydrogen	3·28
Oxygen	29·71

The most probable formula for this substance is $C^{32} H^{11} O^{10}$, which requires in 100 parts—

$$
\begin{array}{llr}
\text{Carbon} & . . . . . . & 67\cdot84 \\
\text{Hydrogen} & . . . . . & 3\cdot88 \\
\text{Oxygen} & . . . . . . & 28\cdot28
\end{array}
$$

The remainder of the substance I dissolved in boiling alcohol, and added acetate of lead. This gave a dark red precipitate, which after filtering, washing with alcohol, and drying, was found to have the following composition :—

0·5470 grm., dried at 100° C., gave 0·6630 carbonic acid and 0·0890 water.

0·4060 grm. gave 0·2800 sulphate of lead, containing 0·20602 oxide of lead.

These numbers correspond with the formula $3C^{32} H^{11} O^{10} + 8PbO$, as the following calculation shows :—

	Eqs.		Calculated.	Found.
Carbon . .	96	576	33·05	33·05
Hydrogen . .	33	33	1·89	1·80
Oxygen . .	30	240	13·78	14·41
Oxide of lead .	8	893·6	51·28	50·74
		1742·6	100·00	100·00

The precipitate produced by muriatic acid in the nitrate of iron solution, was not entirely soluble in boiling alcohol. That part left undissolved by the latter I dissolved again in nitrate of iron; I kept the solution boiling for some time, and then precipitated again with muriatic acid. The precipitate, after filtering and washing, I treated with a boiling solution of carbonate of potash, in which it dissolved. On cooling, a considerable quantity of rubiacate of potash crystallized out with its usual appearance. Some of this salt was decomposed with nitric acid, and the rubiacic acid so obtained was analysed.

II. 0·5250 grm. gave 1·1595 carbonic acid and 0·1465 water. In 100 parts—

$$
\begin{array}{llr}
\text{Carbon} & . . . . . . & 60\cdot23 \\
\text{Hydrogen} & . . . . . & 3\cdot10 \\
\text{Oxygen} & . . . . . . & 36\cdot67
\end{array}
$$

These numbers do not differ very widely from those found in the first analysis. On recrystallizing, however, the remainder of the salt from water, and analysing some acid obtained from the recrystallized salt, I obtained the following numbers :—

III. 0·4300 grm. gave 0·9135 carbonic acid and 0·1100 water.

In 100 parts—

Carbon	57·93
Hydrogen	2·84
Oxygen	39·23

With another portion of the recrystallized potash salt I prepared some silver salt, as before.   0·6260 grm. of the silver salt gave 1·0340 carbonic acid and 0·1070 water, corresponding in 100 parts to—

Carbon	45·04
Hydrogen	1·89

The mother-liquor of the salt from which No. III. was made I precipitated with muriatic acid.   Part of the precipitate I dissolved again in carbonate of potash, and the acid from the crystallized potash salt showed the following composition :—

IV. 0·4010 grm. gave 0·8690 carbonic acid and 0·1160 water.
In 100 parts—

Carbon	59·10
Hydrogen	3·21
Oxygen	37·69

The remainder of the precipitate I now dissolved again in pernitrate of iron, and after keeping the solution boiling for several hours I precipitated again with muriatic acid, filtered, washed, dissolved the precipitate in carbonate of potash, filtered and precipitated again with muriatic acid.   The precipitated rubiacic acid now gave on analysis the following results :—

V. 0·3925 grm. gave 0·8240 carbonic acid and 0·1030 water.

These numbers correspond with the formula $C^{32} H^9 O^{17}$, as will be seen by the following calculation :—

		Eqs.	Calculated.	Found.
Carbon	. . 32	192	56·97	57·25
Hydrogen	. . 9	9	2·67	2·91
Oxygen	. . 17	136	40·36	39·84
		337	100·00	100·00

This composition differs, as will be seen, very little from that found in my former experiments.

Some fresh rubiacic acid made in the same manner as that of the last analysis, was dissolved in carbonate of potash, the solution was evaporated to crystallization, the crystallized potash salt was again dissolved in boiling water, and nitrate of silver was added to the solution.   The precipitate was now no longer red, but of a dull orange colour.

0·1700 grm. of this precipitate gave 0·2720 carbonic acid and 0·0370 water=43·63 per cent. C and 2·40 H; the formula $C^{32} H^8 O^{16} + AgO$ requires 43·24 C and 1·80 H.

If the formula of the potash salt be similar to that of the silver salt, viz. $C^{32} H^8 O^{16} + KO$, it must contain in 100 parts—

Carbon . . . . . . 51·17
Hydrogen . . . . . 2·13
Oxygen . . . . . . 34·12
Potash . . . . . . 12·58

In my former experiments I obtained as an average of three determinations—

Carbon . . . . . . 51·37
Hydrogen . . . . . 2·41
Oxygen . . . . . . 33·18
Potash . . . . . . 13·04

It appears therefore that the four first analyses given above were made with impure acid. The analysis No. I. corresponded, as I have shown, with the formula $C^{64} H^{20} O^{27}$. Now if from this formula be deducted that of the pure acid $C^{32} H^9 O^{17}$, the difference will be $C^{32} H^{11} O^{10}$, which is the formula given above for rubiacine. It is therefore almost certain that it was an admixture of the latter substance with the acid which raised the amount of carbon and hydrogen in the four first analyses. Whether this impure acid is to be considered as a chemical compound of acid and rubiacine, or whether it contains them in a state of mechanical mixture, is a point not easily determined. That the acid of the analysis No. I. contained both substances in atomic proportions may be accidental; and the ease with which rubiacate of potash made from the impure acid yields, by mere recrystallization, a salt containing an almost pure acid, tends to prove that the two substances are merely mechanically mingled. Nevertheless it is difficult to detect the presence of rubiacine in the impure acid, which behaves towards almost all reagents in the same manner as the pure acid. The acid of the analysis No. IV., for instance, could not be distinguished from pure acid by its appearance. When treated with boiling alcohol the latter acquired a yellow colour, but on being filtered boiling hot, no rubiacine crystallized out, as would probably have been the case had the latter only been mixed with the acid. Nevertheless, on heating it cautiously between two watch-glasses, a considerable quantity of yellow shining crystals, doubtless of rubiacine, were formed on the upper glass, while the rubiacic acid of analysis No. V. gave, on being heated in the same manner, only a trace of yellow sublimate and an abundant carbonaceous residue. The potash salt of the impure acid has a more granular and less silky appearance than the salt made from pure acid, and is also of a darker red. When heated it does not detonate so strongly as the pure salt. Its solutions give the same

reactions as the pure salt with all reagents except nitrate of silver, which, as I mentioned above, gives with the pure salt an orange-coloured, with the impure salt a bright cinnabar-red precipitate.

In order to ascertain whether rubiacic acid is reconvertible not only into rubiacine but also into rubiafine, I took some rubiacate of potash of the same preparation as that employed for the rubiacic acid No. I., dissolved it in boiling water, added caustic soda, and passed sulphuretted hydrogen through the solution for several hours; I then precipitated with chloride of barium, filtered, washed the precipitate, decomposed it with muriatic acid, and crystallized the residue twice from alcohol.   The crystals had the appearance of rubiacine or rubiafine, and possessed considerable lustre.   On analysis I obtained the following numbers :—

0·1685 grm. gave 0·4340 carbonic acid and 0·0705 water.

In 100 parts it contained therefore—

$$\begin{array}{lr} \text{Carbon} & 70\cdot24 \\ \text{Hydrogen} & 4\cdot64 \\ \text{Oxygen} & 25\cdot12 \end{array}$$

Though this is not exactly the composition of rubiafine as given above, still it proves that the hydrogen and oxygen are contained in it in the same proportion as in that substance; for if the formula $C^{32} H^{12\frac{1}{2}} O^{8\frac{1}{2}}$, which differs from that of rubiafine by containing $\frac{1}{2}HO$ less, be calculated for 100 parts, it gives—

$$\begin{array}{lr} \text{Carbon} & 70\cdot45 \\ \text{Hydrogen} & 4\cdot58 \\ \text{Oxygen} & 24\cdot97 \end{array}$$

Though I have not, from want of material, been able to trace the steps of the process with the requisite accuracy, I think I am justified in inferring from these experiments, that by the oxidizing agency of persalts of iron rubiafine is changed first into rubiacine and then into rubiacic acid, and that the latter is reconverted by the action of reducing agents, such as sulphuretted hydrogen, first into rubiacine and then into rubiafine. The presence of rubiacine, however, in the liquor which has been used for dyeing with madder, seems to prove that its direct formation from rubian is possible.

Rubiagine.—This substance belongs to the same group of bodies which includes rubianine, rubiadine and rubiafine, and bears a strong resemblance to these substances in properties and composition.   It scarcely ever appears in well-defined crystals. When its alcoholic solution is evaporated spontaneously, it is obtained in the shape of small lemon-yellow spherical grains, which, when crushed and examined under a lens, are found to

consist of small crystalline needles grouped round a centre. Occasionally it has an orange tinge, but this is probably due to some impurity. When heated on platinum foil, it melts to a brownish-red liquid and then burns with flame, leaving a large quantity of carbonaceous residue which burns away with difficulty. When heated in a tube, it gives a small quantity of crystalline sublimate mixed with oily drops. When slowly heated between two watch-glasses, it melts to a brownish-red mass, but gives no sublimate. It is quite insoluble in boiling water, to which it hardly communicates a tinge of colour. It is more easily soluble in boiling alcohol than rubianine or even rubiadine, and does not crystallize out on the solution cooling, but is left, on evaporation of the alcohol, in crystalline masses as just described. It is soluble in concentrated sulphuric acid with a dark reddish-brown colour ; the solution, on being heated, disengages sulphurous acid and becomes black. Boiling nitric acid dissolves it with a disengagement of nitrous acid to a yellow liquid, while some oily drops rise to the surface. On the solution cooling, a quantity of light yellow crystals, possessed of much lustre, are deposited. Whether these crystals are a product of decomposition, or whether they are the substance itself in a state of purity, the impurities having been destroyed by the nitric acid, I am unable to state. The latter is the more probable view. Rubiagine is soluble in boiling acetic acid with a yellow colour, and crystallizes out again, on the solution cooling, in small needles. Ammonia turns it red, and on boiling dissolves it with some difficulty, forming a blood-red solution, which on evaporation loses its ammonia and leaves the substance behind in the shape of small yellow crystals. It dissolves more easily in caustic soda, with the same colour. It is precipitated from its alkaline solution by acids in lemon-yellow flocks. The ammoniacal solution gives very slight precipitates with the chlorides of barium and calcium, the solution remaining red with chloride of barium, and becoming crimson with chloride of calcium. It is soluble in baryta and lime-water with a blood-red colour, and is reprecipitated by a current of carbonic acid. The alcoholic solution gives, on the addition of acetate of lead, at first no precipitate, but the colour of the solution becomes dark yellow, and after some time, provided the solution be not too dilute, an orange-coloured granular precipitate falls, which is the lead compound of rubiagine. If no deposit is formed, then the addition of water causes an orange-coloured flocculent precipitate, which, after being separated by filtration and washed with water in order to remove the acetate of lead, is found to be very little soluble in boiling alcohol, but is easily soluble in a boiling alcoholic solution of acetate of lead with a dark yellow or orange

colour. Acetate of copper changes the colour of the alcoholic solution from light yellow to brownish-yellow, and after some time an orange-coloured precipitate is formed. When rubiagine is treated with a boiling solution of perchloride of iron, the solution acquires a darker colour, but does not assume the deep brownish-purple characteristic of solutions of rubiafine and rubiacine in that menstruum. The liquid being filtered boiling hot, deposits on cooling a quantity of yellow shining scales, but the addition of muriatic acid produces no further precipitate. These scales dissolve easily in boiling alcohol, and the solution on cooling and standing deposits a number of small yellow grains and nodules consisting of crystalline needles, which are apparently nothing but rubiagine itself, for they are not capable of sublimation, and their alcoholic solution is not precipitated by acetate of lead. The greatest part of the rubiagine is left undissolved by the perchloride of iron in the shape of a yellowish-brown powder, which does not dissolve on treating it with an additional quantity of the iron salt. Boiling muriatic acid changes the colour of this powder to yellow, and it has then all the properties of rubiagine. Rubiagine is therefore not changed into rubiacic acid by the action of persalts of iron. Notwithstanding the great resemblance which rubiagine bears to the other bodies belonging to the same series, its reactions prove it to be a distinct substance. It is distinguished from rubianine, by its insolubility in water ; from rubiadine, for which it might most easily be mistaken, by its being incapable of sublimation ; and from rubiafine, by its not being convertible into rubiacic acid. Its behaviour towards acetate of lead, which is different from that of all the other three substances, also serves to characterize it.

The analysis of rubiagine gave the following results :—

0·3800 grm., prepared directly from madder, gave 0·9490 carbonic acid and 0·1760 water.

In 100 parts it contained therefore—

Carbon	.	.	.	.	.	68·10
Hydrogen	.	.	.	.	.	5·14
Oxygen	.	.	.	.	.	26·76

There are two formulæ with which this analysis corresponds, and both of which explain the formation of the substance equally well, viz. $C^{32} H^{14} O^{10}$ and $C^{44} H^{17} O^{13}$. These formulæ require respectively in 100 parts—

				$C^{32} H^{14} O^{10}$.	$C^{44} H^{17} O^{13}$.
Carbon	.	.	.	67·13	68·57
Hydrogen	.	.	.	4·89	4·41
Oxygen	.	.	.	27·98	27·02

I am unwilling to draw any inference from the greater or less correspondence of either of these calculations with the experimental result, because I am not convinced of the absolute purity of the specimen employed for analysis.

The lead compound of rubiagine was prepared by adding a small quantity of an alcoholic solution of sugar of lead to a concentrated alcoholic solution of the substance, taking care not to employ an excess of the precipitant. The orange-coloured precipitate was collected on a filter, washed with alcohol, dried and submitted to analysis.

0·4610 grm., dried in the water-bath, gave 0·5290 carbonic acid and 0·1110 water.

0·2010 grm. gave 0·1460 sulphate of lead, containing 0·10742 oxide of lead.

In 100 parts it contained therefore—

Carbon	31·29
Hydrogen	2·67
Oxygen	12·60
Oxide of lead	53·44

These numbers do not decide between the two formulæ, since they correspond equally well with $C^{32}H^{14}O^{10} + 3PbO$ and $C^{44}H^{17}O^{13} + 4PbO$, as the following calculation shows :—

	Eqs.			Eqs.		
Carbon . .	32	192	30·91	44	264	31·73
Hydrogen .	14	14	2·25	17	17	2·04
Oxygen . .	10	80	12·89	13	104	12·53
Oxide of lead	3	335·1	53·95	4	446·8	53·70
		621·1	100·00		831·8	100·00

If $C^{32}H^{14}O^{10}$ be the true formula of rubiagine, then it is formed by rubian taking up 4 equivs. of water and splitting up into 1 equiv. of rubiagine and 2 equivs. of sugar, as seen by the following equation :—

$$\left. \begin{array}{l} 2\text{ eqs. of Sugar} = C^{24}H^{24}O^{24} \\ 1\text{ eq. of Rubiagine} = C^{32}H^{14}O^{10} \end{array} \right\} = \left\{ \begin{array}{l} C^{56}H^{34}O^{30} = 1\text{ eq. of Rubian.} \\ H\ ^{4}O\ ^{4} = 4\text{ eqs. of Water.} \end{array} \right.$$

$$\overline{C^{56}H^{38}O^{34}} \qquad \overline{C^{56}H^{38}O^{34}}$$
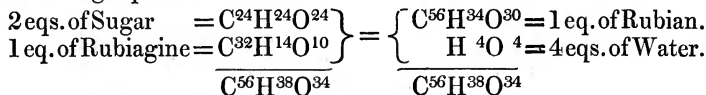
If, on the other hand, $C^{44}H^{17}O^{13}$ be the correct formula, then 1 equiv. of rubian loses 5 equivs. of water, and splits up into 1 equiv. of rubiagine and 1 equiv. of sugar, for

$$C^{44}H^{17}O^{13} + C^{12}H^{12}O^{12} + 5HO = C^{56}H^{34}O^{30}.$$
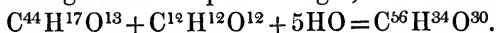
Rubiadipine.—This substance is a characteristic product of the fermentation of rubian. I have never subjected rubian to the action of ferment, under ordinary circumstances, without being able to detect it among the bodies formed. In its appearance and general properties it resembles rubiretine. It differs from

the latter in always remaining soft and viscid, and never becoming hard and brittle, however long it may be heated. It is similar in appearance to a semifluid fat tinged with colouring matter. Its colour is yellowish-brown. When heated on platinum foil it melts to a brown liquid and then burns with a bright flame, leaving a carbonaceous residue. When heated in a tube it evolves acrid fumes, similar to those produced by fat when exposed to destructive distillation. It is not much affected by boiling nitric acid, but concentrated sulphuric acid chars it when heated. When thrown into boiling water rubiadipine melts, forming oily drops, which rise to the surface. It is soluble in caustic alkalies with a blood-red colour, but the solutions do not froth when boiled like solutions of soap. The ammoniacal solution gives only a slight precipitate with chloride of barium. On adding to the alcoholic solution a small quantity of acetate of lead, a pale reddish-brown precipitate is formed, which is the lead compound. This precipitate is insoluble in boiling alcohol, but dissolves entirely when an excess of acetate of lead is added to the boiling liquid, forming a dark brownish-red solution. From this solution it is again precipitated by water, and after filtering and washing is found to be again insoluble in boiling alcohol. In its behaviour to sugar of lead it therefore resembles rubiagine. The alcoholic solution gives no precipitate on the addition of acetate of copper. The substance itself cannot be obtained in a state fit for analysis, I therefore confined myself to the examination of the lead compound formed in the manner just described. The quantity of the substance obtained was, nevertheless, so small that I had only sufficient for one analysis at my disposal.

0·2020 grm., dried at 100° C., gave 0·3770 carbonic acid and 0·1260 water.

0·1150 grm. gave 0·0490 sulphate of lead, containing 0·03605 oxide of lead.

These numbers lead to the formula $C^{30} H^{24} O^5 + PbO$, as the following calculation shows :—

	Eqs.		Calculated.	Found.
Carbon . . .	30	180	50·60	50·89
Hydrogen . .	24	24	6·74	6·93
Oxygen . . .	5	40	11·26	10·83
Oxide of lead .	1	111·7	31·40	31·35
		355·7	100·00	100·00

If this formula represents the true composition of rubiadipine, I confess I am unable to explain its formation from rubian. The great excess of hydrogen contained in it, shows that some substance must be formed simultaneously containing a large proportion of oxygen, but which has hitherto escaped detection.

[To be continued.]

LXVIII. *Mechanical Theory of Heat.—Specific Heat of Air.*
By W. J. Macquorn Rankine, *C.E., F.R.S.E. &c.**

THE results of the recent experimental researches of M. Regnault on the specific heat of air at constant pressure (*Comptes Rendus,* April 18, 1853) agree so closely with those deduced by me theoretically, about three years since, from the principle of the convertibility of heat with mechanical power, that a tabular comparison of them may prove interesting, especially as they have an important bearing on the theory of heat in general. The annexed table contains also the mean result of Mr. Joule's experiments, as given in the Philosophical Transactions for 1852, page 74.

The theoretical calculation may be found in the Transactions of the Royal Society of Edinburgh, vol. xx. p. 192. The ratio of the specific heat of air at constant pressure to that at constant volume, as determined from the velocity of sound, is there taken as approximately equal to $1\cdot4$; but as the true value of this ratio probably lies between $1\cdot4$ and $1\cdot41$, I have added the results of a computation in which the latter value is used.

The following are the numerical data and formulæ employed :—
Height of the modulus of elasticity of air at $0°$ Centigrade,

$$h = 26214 \text{ feet.}$$

Temperature of melting ice, as measured in Centigrade degrees from the absolute zero of a perfect gas-thermometer,

$$\tau_0 = 274\cdot6.$$

Mechanical value of the specific heat of liquid water (Joule's equivalent),

$$K_W = 1389\cdot6 \text{ feet of fall per Centigrade degree.}$$

Ratio of the specific heat of air at constant pressure, to that at constant volume,

$$1 + N = 1\cdot4 \text{ in the original calculation,}$$

$$1\cdot41 \text{ in the second calculation,}$$

being the limits between which the true value is probably contained.

Mechanical values of the specific heat of air :—
at constant volume,

$$K_V = \frac{h}{\tau_0 N} \; ;$$

at constant pressure,

$$K_P = \frac{h}{\tau_0}\left(\frac{1}{N} + 1\right).$$

* Communicated by the Author.

[These formulæ are approximative only; but their deviation from accuracy is inappreciably small in practice.]

The following table shows the results:—

Authority.	K_V	K_P	$\dfrac{K_V}{K_W}$.	$\dfrac{K_P}{K_W}$.
	In feet per deg. Centig.			
Theory, $1+N=1\cdot40$............	238·6	334·0	0·1717	0·2404
Theory, $1+N=1\cdot41$............	232·8	328·2	0·1675	0·2361
Mr. Joule's experiments	....	....	....	0·23
M. Regnault's experiments:—				
between $-30°$ and $+10°$ C.	....	....	....	0·2379
... $10°$ and $100°$	....	....	....	0·2379
.. $0°$ and $225°$	....	....	....	0·2376

In the paper already referred to, I have shown that the probable range of error in the theoretical determination of the specific heats of air is about $\frac{1}{40}$th or $\frac{1}{50}$th of their amount; an uncertainty which originates chiefly in the determination of the velocity of sound. The agreement, therefore, of the theoretical with the experimental results in the foregoing table is as close as the uncertainty of the data will permit.

M. Regnault's experiments also prove a fact, of vital importance both in the theory of heat and in the laws of its practical application to the production of motive power, viz. that the specific heat of air does not sensibly vary between $-30°$ and $+225°$ Centigrade; and therefore that equal numbers of degrees on the scale of the air-thermometer represent equal quantities of heat. This fact, which had already been rendered probable by the experiments of Messrs. Joule and Thomson on the thermic phænomena of air rushing through small apertures, is favourable to the adoption, as a means of deducing the laws of heat from mechanical principles, of the hypothesis that the elasticity of heat is due to the centrifugal force of molecular vortices, by deduction from which hypothesis the fact had been anticipated.

Its most important consequence, as regards the application of heat to produce motive power, is the following formula for the maximum proportion of the total heat expended which can be converted into mechanical work by any thermo-dynamic machine, receiving heat at the maximum temperature τ_1, and giving it out at the minimum temperature τ_2, a formula deduced from the hypothesis in question in the Transactions of the Royal Society of Edinburgh, vol. xx. pp. 207, 438.

Let H_1 be the mechanical equivalent of the total heat received by the machine at the temperature τ_1;

H_2 that of the heat given out at the temperature τ_2;

$E=H_1-H_2$ the maximum mechanical effect of the machine;

κ the temperature corresponding to absolute privation of heat;

then

$$\frac{E}{H_1} = \frac{\tau_1 - \tau_2}{\tau_1 - \kappa}.$$

It follows from this, that that which is called by Prof. William Thomson *Carnot's Function,* has the following value :—

$$\mu = \frac{K_W}{\tau - \kappa}.$$

From the specific heat of steam of saturation, as determined by M. Regnault, viz. 0·475, it is to be anticipated, as various authors have inferred from other circumstances, that the relations between its pressure, density, and temperature for high pressures, will be found to deviate widely from those of a perfect gas. But it would be premature to discuss this subject further until the details of M. Regnault's experiments are published.

Glasgow, April 25, 1853.

LXIX. *On the Iris seen in Water.* By J. J. WALKER[*].

IN those parts of optical and meteorological treatises usually devoted to the phænomena of rainbows, and to the rational explanation and mathematical demonstrations of their theory, a singular omission occurs ; an omission common to all such works from Newton's time down to the present day, as far as the writer's examination of them, for the purpose of satisfying himself on this head, has led him. The omission alluded to is the absence of all mention of the iris seen on the horizontal surface of an extent of sufficiently calm water, lying between the spectator and a falling shower of rain, and opposite the sun. Under favourable circumstances, in such a case, an inverted arch is seen on the water, which is popularly called a "reflexion of the rainbow," and to cursory observation bears a considerable resemblance to what such a "reflexion"—supposing it for a moment possible—would be. A very slight acquaintance, however, with the Cartesian or Newtonian theory of the rainbow suggests at once the correct explanation of the phænomenon ; that *it is a secondary iris, formed by pencils of parallel rays, which, emerging from rain-drops, after undergoing one reflexion within the drops, are reflected a second time at the horizontal surface of the water, and so reach the eye.* It is proposd briefly to investigate the true figure of this *"primary horizontal iris,"* as it may for the sake of distinction be called, and its position on the surface of the water with reference to the spectator's eye.

Let ρ be the angle which the emergent parallel pencil of red

rays from any drop (*d*) makes with the line drawn in the direction of the sun's centre (*s*) ; let this pencil, after undergoing reflexion at the point (*g*) at which it meets the surface of the water (which may be supposed to coincide with a tangent plane to the surface of the terrestrial spheroid, at the foot of a vertical, *ec*, let fall from the spectator's eye, *e*) enter the spectator's eye : the locus of *g* on the horizontal plane will evidently be a line of red-tinted spots, forming part of the exterior band of the horizontal iris.

Let the vertical *ec* be produced beneath the horizontal plane to *e'*, so that *ce'* = *ec* ; then *dg*, if produced, will intersect the line *ece'* at the fixed point *e'* : supposing, therefore, a line *se'f* drawn through *e'* in the direction of the sun's centre, the line *dg*, as it varies for successive drops, will generate a right cone about *e'f* as axis, of which *e'* will be the vertex. The locus of *g* will therefore be the hyperbola in which this cone is intersected by the horizontal plane. The centre and axes of the hyperbola may perhaps be thus most easily found in terms of, *h* = *ec* = height of spectator's eye above the horizontal plane, ρ the deviation for red rays as above described, and α the altitude of the sun's centre. Let a vertical plane through the sun's centre and through the eye intersect the cone in the side *e'*AV, and the horizontal plane in the line *c*A ; A will plainly be the vertex of the hyperbola, and *c*A its transverse axis ; its centre (*o*) in this line will be found by drawing from *e'* in the vertical plane a line *e'o*, making with the production of *se'* an angle (*i*) such that

$$\tan i \,.\, \tan \alpha = \tan^2 \rho.$$

The angle between the asymptotes is equal to the angle between the two sides in which the cone is cut by a plane through the vertex parallel to the horizontal plane ; and, calling this angle 2φ, it is easy to see that

$$\cos \phi \,.\, \cos \alpha = \cos \rho.$$

Hence, for the species and position of the hyperbola, we have—
ratio of semiaxes

$$= \tan \phi = \frac{\{\cos (\rho + \alpha) \cos (\rho - \alpha)\}^{\frac{1}{2}}}{\cos \rho}; \quad . \quad . \quad . \quad (1)$$

distance of the centre from the foot of vertical let fall on the horizontal plane from the spectator's eye

$$= co = h \cot (i - \alpha) = \frac{h \sec^2 \rho}{\cot \alpha \,.\, \tan^2 \rho - 1}; \quad . \quad . \quad (2)$$

distance of vertex of hyperbola from the same point

$$= c\mathrm{A} = h \cot(\rho - \alpha). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Since, for the same value of ρ, the rain-drops forming the cor-

responding line of the primary rainbow lie on a similar cone similarly placed, and whose vertex is at the eye, the vertical distance between *corresponding* drops for the rainbow and the horizontal iris is double the elevation of the spectator's eye above the surface of the water. Also, since the cones of differently coloured pencils are not similar, those of *violet* rays in the case of the former bow may, the rain-drops being sufficiently distant, intersect those of *red* rays forming the horizontal iris in curves of double curvature; and which will therefore be the loci of drops transmitting one pencil of parallel pencils directly to the eye to form a portion of the primary rainbow, and a *differently-tinted* pencil, which, after reflexion at the surface of the water, shall also reach the eye and contribute to form the horizontal iris.

Similarly, on account of the sun's apparent diameter, the height of the observer's eye from the surface of the water not being great, and the rain-drops sufficiently distant, the same drop may transmit pencils of parallel rays of the *same tint,* one directly, the other by horizontal reflexion; that forming part of the rainbow corresponding to the more elevated points of the sun's disc, the other forming part of the horizontal bow corresponding to the points having a less altitude.

Suppose the most distant drops of a falling shower to lie in a vertical plane perpendicular to that through the sun's centre and eye of the spectator, situated at a distance δ from the spectator's eye; then if, h being given, the sun's altitude be such that

$$h \cot (\rho - \alpha) > \delta,$$

no horizontal bow will be visible, though the primary rainbow be so; and, the sun's altitude being given, the same absence of the horizontal bow may occur from the increase of the observer's elevation above the surface of the water.

It is evident that the similarly tinted parts of the two bows will not coincide at the horizontal surface of the water if h bear a sufficiently great ratio to δ; and that at a considerable elevation of the spectator's eye, the ends of the horizontal bow may appear to lie wholly within the ends at which " . . . . *bibit* ingens Arcus . . . . "; thus, even to the most unscientific observer, dispelling the illusion of its being the " reflexion of the rainbow " in the water.

The arrangement of the colours is, of course, the same as in the case of the primary rainbow. If the sun be just on the horizon, the angle between the asymptotes will be equal to 2ρ; and the bow, therefore, nearly an equilateral hyperbola.

In the degree of intensity of its colours the primary horizontal bow would, *cæteris paribus,* coincide with the secondary rainbow. I have found, by such inquiries as I have been able to make

from those who have seen it under the most favourable circumstances, that its brightness much exceeded that of the secondary, and more nearly equalled that of the primary rainbow.   Under very favourable circumstances, therefore, a secondary horizontal iris might be seen more frequently than a tertiary rainbow; and cases might be conceived, as not beyond the possibility of occurrence, in which the secondary horizontal iris should be visible, while from the causes above alluded to, the primary could not be seen.   I have never myself seen this secondary bow, nor have I met with any one who had.

One illustration of the popular error prevalent with respect to the 'horizontal iris' may be alluded to.   It is not uncommon in some of those sketches of scenery under the effect of shower and sunshine, which our artists love so well, to observe a very palpable attempt at painting in a calm sheet of water, occupying the foreground or middle distance, a reflexion of part of an arch of the rainbow; the attempt being made evident by the exercise of those artifices by which painters throw images formed by reflexion in water, *beneath* its surface.

Kingstown near Dublin,
    April 25, 1853.

P.S. It may seem almost affectation to observe, in reference to a paper so unpretending as the above, that it was *not* suggested by Mr. Pratt's paper which appeared in the Philosophical Magazine for February; having, in fact, been written previously to the writer having seen that article.   The two papers obviously have no further connexion than the circumstance of both referring to phænomena occasionally accompanying rainbows.

LXX. *Description of a New Pseudoscope.*
By WALTER HARDIE, *Edinburgh.*

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

PERHAPS the following description of a new pseudoscope may interest those of your readers who have studied Professor Wheatstone's and Sir David Brewster's papers upon binocular vision.   It consists of a simple combination of plane reflectors, and may be constructed of pieces of thin looking-glass.   In the accompanying diagrams these are represented in section by the thick black lines; the thin lines, representing the visual rays as reflected by the mirrors, will also serve to indicate the position of the reflecting surfaces.   The peculiar properties of this instrument are,—

1st. It exhibits the pseudoscopic image either reversed, or in its true aspect, each of these appearances being instantaneously changeable into the other at pleasure. This change is effected by the alternate removal and replacement of the mirrors *a, b* (Plate VI. fig. 1). When these are removed, the visual rays of the two eyes are transposed; and being twice reflected, the image of course is not reversed. When they are replaced, the rays are not transposed; but being now thrice reflected, the image is reversed. In both cases the pseudoscopic phænomena are exhibited, but not equally well in cases where the mode in which the object is illuminated is unfavourable, namely, where a side-light occasions corresponding shading. In objects thus illuminated, the conversions of relief are more easily seen with the reversing arrangement than with the other. This seems to arise from the reversed position of the shading corresponding in some degree with the converse form suggested by the instrument, while the unaltered position of it in the other case so strongly contradicts the suggested idea that the illusion is destroyed. A *strong* side-light, however, except in the case of simple rounded forms, such as the sphere, cylinder, &c., generally overpowers the effect even of the reversing arrangement.

2nd. By another arrangement of the mirrors, *a, b* being removed, and *c* and *d* turned upon hinges into the position shown in fig. 2, a number of new and very strange phænomena are displayed. The most striking among these may be generally described as an apparent *exaggeration of the depth of space.* It is exactly the reverse of the conversions produced by the original pseudoscope. Every prominence seems heightened, every depression deepened, and the space which separates any two objects placed one behind the other appears increased. As examples of these effects I may instance the following:—A round ball seems as if drawn out into an egg-shaped body with the end pointing towards the observer. A piece of money presented with its face towards the observer appears very considerably thicker than it really is; but when turned round so as to be viewed edgeways, it resumes its natural thickness, and at the same time stretches itself out into an oval medal with its length in the direction of vision. A common wine-glass looked into perpendicularly seems a very deep ale-glass, and an ordinary tumbler viewed in the same manner assumes the appearance of one of those tall glasses used for effervescing drinks. The open hand with the fingers slightly separated appears, if viewed sideways, of a most unnatural breadth, the intervals between the fingers being correspondingly magnified. The nose and projecting features of a person's face stand out with additional prominence; yet, strange to say,

this effect is not so ludicrous as might at first be expected, being unaccompanied with anything like *unequal* or local distortion of the features.

Of course these effects of alteration of distance are necessarily accompanied (as will be shown when the principle of the instrument is explained) with corresponding variations of apparent magnitude, or *lateral* extension in the field of view.   This is best exemplified by a square card, which, when viewed perpendicularly at a certain distance, appears of its true form and dimensions; but when turned upon its diameter without changing its mean distance, the side which approaches the observer *diminishes,* while that which recedes *enlarges*; and these changes combining with the more rapid elongation of the other two sides, convert the square into a long trapezium seen rather more obliquely than the card really is, and with its smallest side next the observer.   Thus it appears that when an object approaches, its breadth and height are diminished; when it recedes, these are increased.   Beyond and within certain distances, however, these changes of size as well as the exaggeration of depth cease, and the object is seen *double*; because binocular combination is rendered impossible in the one case by the visual rays from the same point entering the eyes *convergently,* and in the other by their *divergence increasing* beyond what the eyes can accommodate themselves to by squinting.

Another effect which is produced by both forms of this pseudoscope is, that the size and distance of the binocular image never both at the same time correspond with those of the object. If the sizes appear the same, the distance of the image seems greater; if the distances agree, the size of the image is less. This effect, however, so far at least as the increase of distance is concerned, is independent of the binocular principle of the instrument, and may be seen with one eye alone; it is merely incidental to the construction, and arises from the reflected visual ray being necessarily longer than the straight line drawn between the eye and the object.

The above binocular effects are all the results of an *increase of the difference between the right and left retinal images*.   That this is an adequate cause for them, and how it operates, will be readily understood by those already familiar with the phænomena exhibited by the stereoscope, and their explanation.   The same effect of *increased* depth is seen with that instrument when the binocular combination of the two drawings is produced *under a diminished angle of the optic axes*.   In both cases it results from a *similar alteration* of the *natural relation between the axial angle and the difference of the retinal images*.   The mode in which this

new pseudoscope increases the difference of the retinal images is illustrated by fig. 2, in which also an attempt is made to indicate the nature and mode of production of the binocular effects. In this drawing R and L are intended to represent the right and left eyes; O and *o* two real objects at different distances; I*r* and *ir* the reflected images of these objects seen by the right eye *alone*; I*l* and *il* the images seen by the left eye *alone*; and B and *b* the binocular images of the two objects, resulting from the combinations of the reflected images when viewed by both eyes at once. All these reflected and binocular images are represented as near as may be in their true places, positions, and forms relative to the objects O and *o*, and the rest of the drawing. The thin dotted lines are introduced to indicate more clearly the directions of the various visual rays after their last reflexion, so that the connexion of each reflected image with its proper object may be more distinctly traced. Now in order to understand this illustration, the following facts must be borne in mind, namely, that the relative position as regards right and left of any two visual rays entering one eye corresponds with the observed relative positions of the objects from which they proceed; and also that the *angle* which these rays make with one another at their intersection in the centre of the eye agrees with, and may be adopted as a certain index of the real distance from one another of the images of their objects in the picture upon the retina. Applying these facts, then, to the diagram, it will there be seen, that, supposing the instrument removed, the right eye would see the more distant object *o* situated to the *right* of the nearer one O, while the *left* eye sees it on the *left* of O ; the angles OR*o* and OL*o* being the measures of the distances which separate the images of these two objects in each retinal picture. This shows that the *natural* difference between the two retinal pictures is, that the images of the two objects in the one occupy a relative position the reverse of what they occupy in the other ; and that the sum of the angles OR*o* and OL*o* consequently represents the amount of that difference. In the same way it may be shown, with respect to the reflected images seen by either eye alone, when the instrument is interposed, that the sum of the angles I*r*-R-*ir* and I*l*-L-*il* must represent the amount of the difference between them ; but these angles respectively include the other two ; their sum must therefore be greater, and consequently so must the difference it represents be greater than that represented by them.

I have now only to mention that either form of this pseudoscope makes an excellent stereoscope. With drawings of convertible forms, the first described instrument (fig. 1) is capable

of exhibiting an instantaneous changing of relief into depression, or the reverse, simply by the rapid withdrawal or insertion of the mirrors *a, b*.

<div align="center">

I am, Gentlemen,

Your obedient Servant,

WALTER HARDIE.

</div>

6 Pitt Street, Edinburgh,
    March 7, 1853.

The mirrors *e* and *f* have a movement on hinges at *h*, by which the instrument can be adjusted for various distances.

LXXI. *On a remarkable Modification of* Sturm's *Theorem.* By J. J. SYLVESTER, *F.R.S.*[*]

LET me be allowed to use the term *improper* continued fraction to denote a fraction differing from an ordinary continued fraction, in the sole circumstance of the numerators being all negative units instead of positive units, as thus:

$$\cfrac{1}{q_1 - \cfrac{1}{q_2 - \cfrac{1}{q_3 - \&c.}}}$$

The successive convergents of such a fraction as that written above will be

$$\frac{1}{q_1}, \quad \frac{q_2}{q_2 q_1 - 1}, \quad \frac{q_3 q_2 - 1}{q_3 q_2 q_1 - q_3 - q_1}, \&c.$$

If we call these respectively

$$\frac{N_1}{D_1}, \quad \frac{N_2}{D_2}, \quad \frac{N_3}{D_3}, \&c.,$$

we have the general scale of formation

$$N_\iota = q_\iota . N_{\iota-1} - N_{\iota-2}$$

$$D_\iota = q_\iota . D_{\iota-1} - D_{\iota-2}.$$

Moreover, we shall have universally

$$N_\iota . D_{\iota-1} - N_{\iota-1} . D_\iota \text{ equal to } +1,$$

instead of alternating between $+1$ and -1, as is the case in continued fractions of the ordinary kind.

Again, let me be allowed to use the term *" signaletic "* series to denote a series of disconnected terms, designed to exhibit a certain succession of algebraical signs $+$ and $-$, and to speak of

two series being *signaletically* equivalent when the *number* of continuations of signs and of variations of signs between the several terms and those that are immediately contiguous to them is the same for the two series; a condition which evidently may be satisfied without the *order* of such changes and continuations being identical. I am now able to enunciate the following remarkable *theorem of signaletic equivalence* between two distinct series of terms, each generated from the same improper continued fraction. But first I must beg to introduce yet another new term in addition to those already employed, viz. *reverse convergents,* to denote the convergents generated from a given continued fraction by reading the quotients in a reverse order, or if we like so to say, the convergents corresponding to the given continued fraction reversed.

The two forms

$$\cfrac{1}{q_1 - \cfrac{1}{q_2 - \cfrac{1}{q_3 - \cfrac{\vdots}{\cfrac{1}{q_n}}}}} \qquad \text{and} \qquad \cfrac{1}{q_n - \cfrac{1}{q_{n-1} - \cfrac{1}{q_{n-2} - \cfrac{\vdots}{\cfrac{1}{q_1}}}}},$$

are obviously reciprocal; and if the two last convergents of either one of them be respectively

$$\frac{N_{n-1}}{D_{n-1}}, \quad \frac{N_n}{D_n},$$

$\dfrac{D_{n-1}}{D_n}$ will serve to generate the other. For the clearer and more simple enunciation of the theorem about to be given, it will be better to take as our first convergent $\dfrac{0}{1}$, so that 1 will be treated as the denominator of the first convergent in every case; and calling D_0 such denominator, we shall always understand that $D_0 = 1$. Let now $D_0, D_1, D_2, \ldots D_n$ be the $(n+1)$ denominators of any improper continued fraction of n quotients, and $\mathbb{C}_0, \mathbb{C}_1, \mathbb{C}_2, \ldots \mathbb{C}_n$ the corresponding denominator series for the same fraction reversed; then, I say, that these *two series* are *signaletically equivalent*.

I do not here propose to demonstrate this proposition, to which I was led unconsciously by researches connected with the theory of elimination, which afford a complete and general but somewhat indirect and circuitous proof. Doubtless some simple and direct proof cannot fail ere long to be discovered*. For

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* See Postscript.

the present I shall content myself with showing *à posteriori* the truth of the theorem for a particular case.   Let $n = 3$.   The two series which are to be proved to be signaletically equivalent may be written

$$1, \quad A, \quad BA - 1, \quad CBA - C - A$$
$$1, \quad C, \quad BC - 1, \quad ABC - A - C.$$

Call these respectively S and (S).   In S we may substitute in the third term, in place of $BA - 1$, CA without affecting the signaletic value of the series; for if the second and fourth terms have different signs, the third term may be taken anything whatever, since the sequence of the second, third, and fourth terms will give one continuation and one change, whatever the middle one may be.   Suppose, then, that the second and fourth terms have the same sign, and let

$$CBA - C - A = m^2 . A,$$
$$\therefore C(BA - 1) = (m^2 + 1)A,$$
$$\therefore (BA - 1) . AC = (m^2 + 1)A^2.$$

Hence $(BA - 1)$ and AC will have the same sign; hence S is signaletically equivalent to S', where S' denotes the series

$$1, \quad A, \quad CA, \quad CBA - C - A.$$

Now, again, if CA is negative, we may put instead of A anything whatever, and therefore, if we please, C, without affecting signaletically the value of S'.   But if CA is positive, A and C will have the same sign, and therefore on this supposition also C may be substituted for A.   Hence always S' is signaletically equivalent to S'', where S'' denotes

$$1, \quad C, \quad CA, \quad CBA - C - A.$$

Again, if C and $CBA - C - A$ have different signs, the value of the intermediate term is immaterial; but if C and $CBA - C - A$ have the same sign, let

$$CBA - C - A = m^2 C;$$

then $$A(CB + 1) = (1 + m^2)C,$$

and $$A^2(CB - 1) = (1 + m^2)AC;$$

and consequently $CB - 1$ and AC have the same sign.   In every case, therefore, S'' is signaletically equivalent to

$$1, \quad C, \quad CB - 1, \quad ACB - A - C;$$

i. e. S is signaletically equivalent to S', and therefore to S'', and therefore to (S), as was to be proved.

The application of the foregoing theory to Sturm's process for finding the number of real roots of an equation is apparent; for a very little consideration will serve to show, that if we

expand $\frac{f'(x)}{fx}$, [$f(x)$ being of the nth degree in (x),] algebraically under the form of a continued fraction

$$\cfrac{1}{Q - \cfrac{1}{Q_2 - \cfrac{1}{Q_3 - \\ \vdots \\ -\cfrac{1}{Q_n}}}},$$

where Q_1, Q_2, Q_3, ... Q_n may be supposed linear functions of x (although, in fact, this restriction, as will be hereafter noticed, is unnecessary), the denominators of the reverse convergents

$$\frac{0}{1}, \quad \frac{1}{Q_n}, \quad \frac{Q_{n-1}}{Q_n \cdot Q_{n-1} - 1}, \quad \cdots \quad \frac{Q_{n-1} \cdot Q_{n-2} \cdots Q_1 - \&c.}{Q_n \cdot Q_{n-1} \cdots Q_1 - \&c.}$$

will be signaletically equivalent with the Sturmian series of functions for determining the number of real roots of fx within given limits; in fact,

$$1, \quad Q_n, \quad Q_n \cdot Q_{n-1} - 1, \ldots Q_n \cdot Q_{n-1} \ldots Q_1 - \&c.$$

will be the Sturmian functions themselves, divided out by the negative of the last or constant residue which arises in the application of the process of continued division, according to Sturm's rule; and as we have shown that the series of the denominators to the convergents of any continued fraction, and the series of the denominators to the convergents of the same fraction reversed, are signaletically equivalent, we have this surprisingly new, interesting, and suggestive mode of stating Sturm's theorem, viz. the denominators to the convergents of the continued fraction which represents $\frac{f'x}{fx}$ constitute a Rhizoristic series for $f(x)$, *i. e.* a signaletic series which serves to determine the number of roots of fx comprised within any prescribed limits. Moreover, in applying this theorem it is by no means necessary that, in the continued fraction which represents $\frac{f'x}{fx}$, all or any of the quotients should be taken linear functions of x. A very little consideration of the principles upon which the demonstration of Sturm's theorem is founded will serve to show that the convergent denominators to any continued fraction whatever which represents $\frac{f'x}{fx}$, whether the quotients be linear or non-linear,

integral or fractional, or mixed functions of x, and whatever the number of quotients (which, it may be observed, cannot be less than, but may be made to any extent greater than the exponent of the degree of $f(x)$), will equally well furnish a Rhizoristic series for fixing the position of the roots, provided only that the last *divisor* in the process of expanding $\dfrac{f'x}{fx}$ under the form of an improper continued fraction be a constant quantity or any function of x incapable of changing its sign.

Let us, however, for the present confine our attention to the ordinary Sturmian form, where all the quotients are linear functions of (x). Let these quotients be respectively

$$a_1x + b_1; \quad a_2x + b_2; \quad a_3x + b_3; \quad \ldots a_nx + b_n.$$

In order to determine the total number of real and imaginary roots of $f(x)$, we must count the loss of continuations of sign in the Rhizoristic series in passing from $x = +\infty$ to $x = -\infty$. When x is infinitely great, it is clear that, whether positive or negative, the parts b_1, b_2, ... b_n may be neglected, and only the highest powers of x need be attended to in writing down the signaletic series corresponding to these two values of x. Accordingly for $x = \pm\infty$ the signaletic series becomes

$$1, \quad a_1x_1, \quad a_1a_2x^2, \ldots a_1 . a_2 \ldots a_n . x^n;$$

and consequently the number of pairs of imaginary roots of $f(x)$ is the number of changes of sign in the series

$$1, a_1, a_1 . a_2, \ldots a_1 . a_2, \ldots . a_n,$$

i. e. is the number of negative quantities in the series

$$a_1, a_2, a_3, \ldots a_n.$$

Hence we have the curious and hitherto strangely overlooked theorem, that in applying Sturm's process of successive division to fx and $f'x$, the number of negative coefficients of x in the successive quotients gives the number of pairs of real roots of fx; as a corollary, we learn the somewhat curious fact that never more than half of these coefficients can be negative; and in general it would appear that the better practical method of applying Sturm's theorem would be not to deal with the Residues, which have hitherto been the sole things considered, but rather with the linear quotients which have been treated as merely incidental to the formation of the Residues.

To find the value of the Rhizoristic series corresponding to a given value of x, the better method would accordingly seem to be to commence with finding the arithmetical values of the (n) quotients

$$a_1 . x + b_1; \quad a_2x + b_2; \quad \ldots a_nx + b_n.$$

We thus obtain (n) numbers $\mu_1, \mu_2, \ldots \mu_n$, and have only to form a progression according to the well-known law

$$1, N_1, N_2, \ldots N_n,$$

where $N_1 = \mu$, and in general $N_i = \mu_i \cdot N_{i-1} - N_{i-2}$.

The number of arithmetical operations required by this method (after the division part of the process which is common to the two methods has been performed) will be $2n$ multiplications and $2n$ additions or subtractions; whereas if we deal with the residues directly, the number of multiplications will be

$$n + (n-1) + \ldots + 1, \ i. \ e. \frac{n \cdot (n+1)}{2}$$

(besides having to raise x to the nth power), and the same number of additions. The practical advantage, however, of this method over the old method is not quite so great as it may at first sight appear, in consequence of the quantities operated with on applying it being larger numbers than those which have to be used in the old method.

If we were to employ, instead of the direct series,

$$1, \quad N_1, \quad N_2, \quad N_1 - 1, \ \&c.,$$

the signaletically equivalent reverse series

$$1_1, \quad N_n, \quad N_{n-1} \cdot N_n^* - 1, \ \&c.,$$

the arithmetical difficulty would be much increased in consequence of the quotients becoming rapidly more complex as the division proceeds. It were much to be desired that some person practically conversant with the application of Sturm's method, such as that excellent and experienced mathematician, my esteemed friend Professor J. R. Young, would perpend and give his opinion upon the relative practical advantages of the two methods of substitution; the one that where the residues are employed, the other that where the quotients are employed.

I am bound to state, that but for a valuable hint furnished to me by my friend, that most profound mathematician, M. Hermite, who discovered a theorem virtually involving the transformation of Sturm's theorem here presented, but founded upon entirely different and less general considerations, and in the origin of which hint, as arising out of my own previous speculations upon which I was in correspondence with M. Hermite, I may perhaps myself claim a share, this theory would probably not have come to light. It is of course not confined to Sturm's theorem, which deals only with the special case of two functions, whereof one is the first derivative of the other.

There is a larger theory, to which M. Sturm's is a corollary, which contemplates the relations of the roots of any two func-

tions whatever. This is what I term the *theory of interpositions*, upon which I do not propose here to enter, but which will be fully developed in a memoir nearly completed, and which I shortly propose to present to the Royal Society, wherein will be found combined and flowing into one current various streams of thought bearing upon this subject which had previously existed disunited, and appearing to follow each a separate course.

7 New Square, Lincoln's Inn,
    May 13, 1853.

Remark.

I am not aware that anyone has observed what the effect would be of omitting to change the signs of the successive residues in the application of Sturm's method, *i. e.* of employing a proper in lieu of an improper continued fraction to express $\frac{f'x}{fx}$.

Although easily made out, it is well worthy of being remarked. Suppose

$$\frac{\phi}{f} = \cfrac{1}{Q_1 - \cfrac{1}{Q_2 - \cfrac{1}{Q_3 - \cfrac{1}{\vdots - \cfrac{1}{Q_n}}}}}.$$

and in general (P being any letter) use $\overline{P}$ to denote $-P$. Now we may write

$$f = Q_1\phi - \rho_1$$
$$\phi_1 = Q_2\rho_1 - \rho_2$$
$$\rho_1 = Q_3\rho_2 - \rho_3$$
$$\rho_2 = Q_4\rho_3 - \rho_4$$
$$\rho_3 = Q_5\rho_4 - \rho_5$$
$$\rho_4 = Q_6\rho_5 - \rho_6.$$
$$\&c. = \&c.$$

This gives

$$f = Q_1\phi + \overline{\rho_1}$$
$$\phi = \overline{Q}_2\overline{\rho}_1 + \rho_2$$
$$\overline{\rho}_1 = Q_3\overline{\rho}_2 + \rho_3$$
$$\overline{\rho}_2 = \overline{Q}_4\rho_3 + \rho_4$$
$$\rho_3 = Q_5\rho_4 + \overline{\rho}_5$$
$$\&c. = \&c.$$

The law evidently being that the quotients change their sign alternately, *i. e.* in the 2nd, 4th, 6th, &c. places, and remain

unaltered in the 1st, 3rd, 5th, &c. places; whereas the residues or excesses change their signs in the 1st and 2nd, 5th and 6th, 9th and 10th, &c., and remain unaltered in the 3rd and 4th, 7th and 8th, 11th and 12th, &c. places. The effect is, that if, in applying Sturm's method, we omit to change the signs of the remainders, and take as our signaletic series

$$fx, f'x, R_1, R_2, R_3, \ldots R_{n-1},$$

R_1, R_2, R_3, &c. being the successive unaltered residues, the signaletic index corresponding to any value of x instead of being the number of continuations in the above series, will become the number of continuations in going from a term in an odd place to a term in an even place *plus* the number of variations in going from a term in an odd place to a term in an even place.

If we adopt the quotient method, the rule will be simply to change the sign of the alternate quotients (beginning with the second) in forming the signaletic series.

As an artist delights in recalling the particular time and atmospheric effects under which he has composed a favourite sketch, so I hope to be excused putting upon record that it was in listening to one of the magnificent choruses in the 'Israel in Egypt,' that unsought and unsolicited, like a ray of light, silently stole into my mind the idea (simple, but previously unperceived) of the equivalence of the Sturmian residues to the denominator series formed by the reverse convergents. This idea was just what was wanting,—the key-note to the due and perfect evolution of the theory.

Postscript.

Immediately after leaving the foregoing matter in the hands of the printer, a most simple and complete proof has occurred to me of the theorem left undemonstrated in the text.

Suppose that we have any series of terms $u_1, u_2, u_3, \ldots u_n$, where

$$u_1 = A_1 \quad u_2 = A_1 A_2 - 1 \quad u_3 = A_1 A_2 A_3 - A_1 - A_3 \ \&c.,$$

and in general

$$u_\iota = A_\iota \cdot u_{\iota-1} - u_{\iota-2},$$

then $u_1, u_2, u_3, \ldots u_n$ will be the successive principal coaxal determinants of a symmetrical matrix. Thus suppose $n = 5$; if we write down the matrix

$$
\begin{array}{ccccc}
A_1 & 1 & 0 & 0 & 0 \\
1 & A_2 & 1 & 0 & 0 \\
0 & 1 & A_3 & 1 & 0 \\
0 & 0 & 1 & A_4 & 1 \\
0 & 0 & 0 & 1 & A_5
\end{array}
$$

(the mode of formation of which is self-apparent), these successive coaxal determinants will be

$$1, \; \begin{vmatrix} A_1 \end{vmatrix} \; \begin{vmatrix} A_1 & 1 \\ 1 & A_2 \end{vmatrix} \; \begin{vmatrix} A_1 & 1 & 0 \\ 1 & A_2 & 1 \\ 0 & 1 & A_3 \end{vmatrix} \; \begin{vmatrix} A_1 & 1 & 0 & 0 \\ 1 & A_2 & 1 & 0 \\ 0 & 1 & A_3 & 1 \\ 0 & 0 & 1 & A_4 \end{vmatrix} \; \begin{vmatrix} A_1 & 1 & 0 & 0 & 0 \\ 1 & A_2 & 1 & 0 & 0 \\ 0 & 1 & A_3 & 1 & 0 \\ 0 & 0 & 1 & A_4 & 1 \\ 0 & 0 & 0 & 1 & A_5 \end{vmatrix}$$

i. e.

$$1, \; A_1, \; A_1A_2-1, \; A_1A_2A_3-A_1-A_3, \; A_1A_2A_3A_4-A_1A_2-A_1A_4$$
$$-A_3A_4+1, \; A_1A_2A_3A_4A_5-A_1A_2A_5-A_1A_4A_5-A_3A_4A_5-A_1A_2A_3$$
$$+A_5+A_3+A_1.$$

It is proper to introduce the *unit* because it is, in fact, the value of a determinant of zero places, as I have observed elsewhere. Now I have demonstrated directly in this very Magazine (August 1852), under cover of the *umbral* notation, that the *signaletic* value of a regularly ascending series of principal coaxal determinants formed from any symmetrical matrix is unaffected by any such transposition whatever of the lines and columns of the matrix as does not destroy the symmetry about the principal axis. Hence, then, beginning from the lower extremity of the axis A_5, and reading off the ascending series of coaxal minors from that point, we obtain the reverse series,

$$1, \; A_5, \; A_5A_4-1, \; A_5A_4A_3-A_5-A_3, \; A_5A_4A_3A_2-A_5A_4$$
$$-A_5A_2-A_3A_2+1, \; A_5A_4A_3A_2A_1-A_5A_4A_1-A_5A_2A_3-A_3A_2A_1$$
$$-A_5A_4A_3+A_1+A_3+A_5.$$

Hence we see that the denominators to the convergents of

$$\cfrac{1}{A_1}-\cfrac{1}{A_2}-\cfrac{1}{A_3}-\cfrac{1}{A_4}-\cfrac{1}{A_5},$$

beginning with 1, form a series signaletically equivalent to that similarly formed from the fraction

$$\cfrac{1}{A_5}-\cfrac{1}{A_4}-\cfrac{1}{A_3}-\cfrac{1}{A_2}-\cfrac{1}{A_1};$$

and the reasoning is of course general, which establishes the theorem in question.

It seems only proper and natural that I should not leave unstated here the signaletic properties of the series of *numerators* to the convergents to $\dfrac{f'x}{fx}$ expanded under the form of a continued fraction.

Let the number of changes of sign in the denominator series for any given value (a) of x be called $D(a)$, and for the numerator series $N(a)$. Then $N(a) - N(b)$ may be equal to, or at most can only differ by a positive or negative unit from $D(a) - D(b)$. The relation between these differences depends on the nature of the interval between the greater of the two limits (a) and (b), and the root of $f(x)$ next less that limit, and of the interval between the less of the two limits (a) and (b), and the root of fx next greater than such limit. If a root of $f'(x)$ is contained in each such interval,

$$N(a) - N(b) = D(a) - D(b) + 1;$$

if a root of $f'(x)$ is contained within one interval, but no root within the other,

$$N(a) - N(b) = D(a) - D(b);$$

if no root of $f'(x)$ is contained within either interval,

$$N(a) - N(b) = D(a) - D(b) - 1.$$

I may conclude with noticing that the determinative form of exhibiting the successive convergents to an improper continued fraction affords an instantaneous demonstration of the equation which connects any two consecutive such convergents as

$$\frac{N_{\iota-1}}{D_{\iota-1}} \text{ and } \frac{N_\iota}{D_\iota}, \text{ viz. } N_\iota . D_{\iota-1} - N_{\iota-1} . D_\iota = 1.$$

For if we construct the matrix, which for greater simplicity I limit to five lines and columns,

$$\left| \begin{array}{ccccc} A & 1 & 0 & 0 & 0 \\ 1 & B & 1 & 0 & 0 \\ 0 & 1 & C & 1 & 0 \\ 0 & 0 & 1 & D & 1 \\ 0 & 0 & 0 & 1 & E \end{array} \right| \quad . \quad . \quad . \quad . \quad \text{(M)}$$

and represent umbrally as

$$\begin{array}{ccccc} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5; \end{array}$$

and if, by way of example, we take the fourth and fifth convergents, these will be in the umbral notation represented by

$$\frac{\begin{array}{ccc} a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 \end{array}}{\begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \end{array}} \text{ and } \frac{\begin{array}{cccc} a_2 & a_3 & a_4 & a_5 \\ b_2 & b_3 & b_4 & b_5 \end{array}}{\begin{array}{ccccc} a_1 & a_2 & a_5 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \end{array}}$$

respectively.   Hence

$$N_5D_4-N_4D_5 = \begin{array}{cccc} a_2 & a_3 & a_4 & a_5 \\ b_2 & b_3 & b_4 & b_5 \end{array} \times \begin{array}{cccc} a_2 & a_3 & a_4 & a_1 \\ b_2 & b_3 & b_4 & b_1 \end{array}$$

$$- \begin{array}{ccc} a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 \end{array} \times \begin{array}{ccccc} a_2 & a_3 & a_4 & a_5 & a_1 \\ b_2 & b_3 & b_4 & b_5 & b_1 \end{array}$$

$$= \begin{array}{cccc} a_2 & a_3 & a_4 & a_5 \\ b_2 & b_3 & b_4 & b_5 \end{array} \times \overline{ \begin{array}{cccc} a_2 & a_3 & a_4 & a_1 \\ b_2 & b_3 & b_4 & b_1 \end{array} - \begin{array}{cccc} a_2 & a_3 & a_4 & a_1 \\ b_2 & b_3 & b_4 & b_1 \end{array} }$$

$$= \begin{array}{cccc} a_2 & a_3 & a_4 & a_5 \\ b_2 & b_3 & b_4 & b_1 \end{array} \times \begin{array}{cccc} a_2 & a_3 & a_4 & a_1 \\ b_2 & b_3 & b_4 & b_5 \end{array}$$

$$= \begin{array}{cccc} a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 \end{array} \times \begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 & b_5 \end{array}$$

i. e.

$$= \begin{array}{cccc} 1 & B & 1 & 0 \\ 0 & 1 & C & 1 \\ 0 & 0 & 1 & D \\ 0 & 0 & 0 & 1 \end{array} \times \begin{array}{cccc} 1 & 0 & 0 & 0 \\ B & 1 & 0 & 0 \\ 1 & C & 1 & 0 \\ 0 & 1 & D & 1 \end{array}$$

$$= 1 \times 1 = 1,$$

as was to be proved.   And the demonstration is evidently general in its nature.   We may treat a proper continued fraction in precisely the same manner, substituting throughout $\sqrt{-1}$ in place of 1 in the generating matrix, and we shall thus, by the same process as has been applied to improper continued fractions, obtain

$$N_{\iota+1}.D_\iota - N_\iota.D_{\iota+1} = (\sqrt{-1})^\iota \times (\sqrt{-1})^\iota$$
$$= (-1)^\iota.$$

I believe that the introduction of the method of determinants into the algorithm of continued fractions cannot fail to have an important bearing upon the future treatment and development of the theory of Numbers*.—J. J. S.

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* If in the above matrix (M) we write throughout $\sqrt{-1}$ in place of 1, we have a representation of the numerators and denominators of the convergents to a proper continued fraction, and such representation gives an immediate and visible proof of the simple and elegant rule (not stated in the

LXXII. *On Chiviatite, a new Mineral from Peru.* By Professor RAMMELSBERG[*].

MR. BROOKE sent to me a considerable time since a mineral which he looked upon as a new species, and which is said to have been met with at Chiviato in Peru. In many respects it resembles bismuthine, is of a lead-gray colour, and possesses a marked metallic lustre. The foliated crystalline mass is cleavable in three directions situated in the same zone, but most readily in the direction of a plane, with respect to which, according to Professor Miller's approximative measurements, the

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ordinary treatises on the subject, nor so well known as it deserves to be) for forming any such numerators or denominators by means of the principal terms in each; the rule, I mean, according to which the ith denominator may be formed from $q_1 . q_2 . q_3 . q_4 \ldots q_i$ ($q_1, q_2, \ldots q_i$ being the successive quotients), and the ith numerator from $q_2 . q_3 \ldots q_i$, by leaving out from the above products respectively any pair or any number of pairs of consecutive quotients as $q_\rho . q_{\rho+1}$. For instance, from $q_1 . q_2 . q_3 . q_4 . q_5$, by leaving out $q_1 . q_2$, $q_2 . q_3$, $q_3 . q_4$, and $q_4 . q_5$, we obtain

$$q_3 . q_4 . q_5 + q_1 . q_4 . q_5 + q_1 q_2 . q_5 + q_1 . q_2 . q_3 ;$$

and by leaving out $q_1 . q_2 \times q_3 . q_4$, $q_1 . q_2 \times q_4 . q_5$, $q_2 . q_3 \times q_4 . q_5$, we obtain $q_5 + q_3 + q_1$; so that the total denominator becomes

$$q_1 . q_2 . q_3 . q_4 . q_5 + q_3 . q_4 . q_5 + q_1 . q_4 . q_5 + q_1 . q_2 . q_5 + q_1 . q_2 . q_3 + q_1 + q_3 + q_5 ;$$

and in like manner the numerator of the same convergent is

$$q_2 . q_3 . q_4 . q_5 \left\{ 1 + \frac{1}{q_2 . q_3} + \frac{1}{q_3 . q_4} + \frac{1}{q_4 . q_5} + \frac{1}{q_2 . q_3 . q_4 . q_5}, \right.$$

i . e.
$$q_2 . q_3 . q_4 . q_5 + q_2 . q_5 + q_2 . q_5 + q_2 . q_3 + 1.$$

The most cursory inspection of the form of the generating matrix will show at once the reason of this rule. It may furthermore be observed, that every progression of terms constructed in conformity with the equation

$$u_n = a_n . u_{n-1} - b_n . u_{n-2} + c_n . u_{n-3} \pm \&c.,$$

may be represented as an ascending series of principal coaxal determinants to a common matrix. Thus if each term in such progression is to be made a linear function of the three preceding terms, it will be representable by means of the matrix

$$\begin{matrix} A & B & C'' & 0 & 0 \\ 1 & A' & B'' & C''' & 0 \\ 0 & 1 & A'' & B''' & C'''' \\ 0 & 0 & 1 & A''' & B'''' \\ 0 & 0 & 0 & 1 & A'''' \end{matrix}$$

indefinitely continued, which gives the terms

$$1, \; A, \; AA' - B, \; AA'A'' - BA'' - AB'' + C'', \&c.$$

second is inclined at an angle of 153°, and the third at an angle
of 133°. The specimen transmitted to me is intermixed with
pyrite and baryte.

I found the specific gravity to be $=6\cdot920$. Its behaviour
before the blowpipe and in the moist way resembles that of patri-
nite. Decomposed by means of chlorine, it gave the following
result:—

Sulphur	. . . . .	18·00
Bismuth	. . . . .	60·95
Lead	. . . . .	·16·73
Copper	. . . . .	2·42
Iron	. . . . .	1·02
Silver	. . . . .	trace
Insoluble	. . . .	0·59
		99·71

Calculating for the separate metals what are the requisite
amounts of sulphur, we obtain—

$$13\cdot80 = 74\cdot75 \ Bi^2S^3$$
$$\left.\begin{array}{c} 2\cdot60 \\ 0\cdot61 \end{array}\right\} 3\cdot21 \begin{array}{l} = 19\cdot33 \ PbS \\ = \ 3\cdot03 \ Cu^2S \end{array}$$
$$1\cdot21 = \ 2\cdot23 \ Fe\,S^2$$
$$\overline{18\cdot22}$$

After deducting the inconsiderable amount of the intermixed
pyrite, the quantity of sulphur contained in the PbS (Cu²S) and
the Bi^2S^3 affords the ratio of $1:4\frac{1}{2}$, so that the formula for this
new compound is—

$$2\left\{\begin{array}{c} PbS \\ Cu^2S \end{array}\right\} 3Bi^2S^3.$$
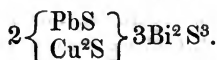
The mineral, the name for which is derived from its locality,
therefore approaches patrinite, which is $3\left\{\begin{array}{c} PbS \\ Cu^2S \end{array}\right\} Bi^2S^3$. Ko-
bellite contains 3PbS, Bi^2S^3, and Klaproth's cupreous bismuth
from Wittichen is perhaps $2Cu^2S$, Bi^2S^3.

LXXIII. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. iii. p. 317.]

Feb. 16,   PROFESSOR MILLER made a communication on the
1852.   Artificial Formation of Crystallized Minerals.

March 1.—Mr. Hopkins, F.R.S. &c., gave a Lecture on the In-
fluence of Internal Heat, Stellar Radiation, and Configuration of
Land and Sea in producing Changes of the Earth's Superficial Tem-
perature.

March 15.—Professor Miller made a communication on different improvements in the Reflective Goniometer; and a description of a New Reflective Goniometer.

Professor Stokes concluded a paper on the Composition and Resolution of Streams of Polarized Light from different Sources (see the abstract under the date Feb. 16, 1852, Phil. Mag. vol. iii. p. 316).

He also made a communication on Haidinger's Brushes.

Also on the Optical Properties of a New Salt of Quinine. The salt alluded to is that which had recently been discovered by Dr. Herapath (Phil. Mag. vol. iii. p. 161). The substance of this communication formed the subject of a notice of the properties of the salt which the author read at the Meeting of the British Association at Belfast, which will be found in the Report of the Transactions of the Sections.

April 26.—The Rev. Mr. Kingsley gave an account of the application of Photography to the Microscope.

The earliest attempts in photography were directed both by Sir H. Davy and Mr. Fox Talbot to the fixing upon prepared paper the images of objects by the solar microscope, and the latter gentleman succeeded completely, as far as his instrument allowed, in obtaining pictures of minute structures. Shortly after the publication of Mr. Talbot's process, various attempts were made to apply the oxyhydrogen microscope to the same purpose, as that instrument had superseded the solar. The result however was, that it was abandoned on account of the great time that was found to be necessary for impressing an image; and after a great variety of trials by Prof. Owen, Dr. Carpenter, Dr. Leeson and others, the use of the instrument for this purpose was given up.

The discovery of the collodion process, so much more sensitive than that of Mr. Talbot, led the author to think, as soon as he became acquainted with it, that we were in possession of the means of impressing microscopic objects by means of artificial light without any great trouble. A friend of his had an oxyhydrogen microscope of the common form, and on making a trial with it, he found that by using a very sensitive kind of collodion, he could obtain images by about a minute's exposure. On examining the instrument, however, he saw that its form must be completely changed, in fact, that an entirely new kind of instrument was required to obtain the best effect. The two points to be regarded as the peculiar principles of this microscope are, 1st, that none of the radiant light be lost, or as little as possible; 2ndly, that the magnifying power be obtained by such means as would not place the screen for receiving the image beyond such a distance from the object, that the motions of the instrument could be governed at the same time that the image was closely inspected. The first of these objects is secured by giving a very large angular aperture to the system of lenses used for collecting the light, and by using another set of lenses for condensing it again on the object, and so arranging their focal length in proportion to the focal length of the object-glass, as to cover the plate

to be acted upon, and that space only: the second, by using a sort of eye-piece for enlarging the image formed by the object-glass.

The lenses divide themselves into four groups, as represented in the figures, in which the light is supposed to proceed from the left hand to the right. The first set for collecting the light is composed of three large lenses, a meniscus, plano-convex and double convex, being a combination of three lenses similar in effect to Herschel's doublet; the second set for condensing the light on the object is a
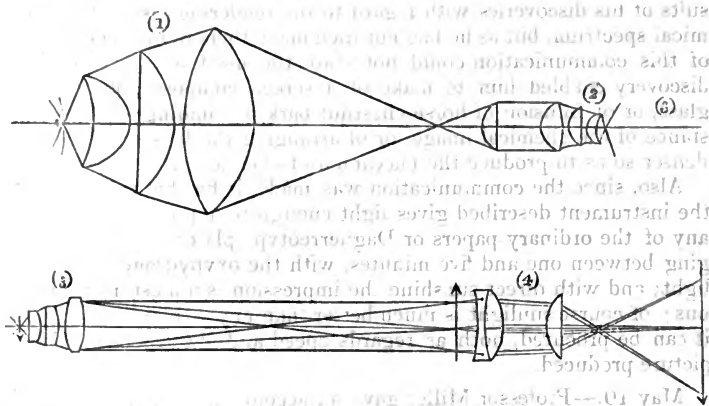
similar set of lenses, but of much shorter focal length, and turned the other way; between these two sets is a plano-convex or plano-concave lens placed at its focal length from the convergence of the rays from the first group, so as to make the rays pass to the condensers in a state of parallelism, and so do away with the necessity of changing the distance between the collectors and condensers for each adjustment of the latter: the third group forms the object-glass, which must be so corrected as to have the rays of the spectrum between the fixed lines G and H as much as possible brought to a point, as these rays are those that produce the maximum action on the silver salts used in photography; this will require the red rays to be left untouched, just in the same way as Fraunhofer left those of the blue end of the spectrum dispersed in correcting an object-glass for light. The fourth group is the common eye-piece left under-corrected. A rather better form for this is a Ramsden's eye-piece with the first lens partially achromatized, by making it a compound lens with the radius of curvature of the common surface nearly double that of the surface that would render it achromatic. This form of eye-piece gives a better correction of the oblique pencils than the common negative.

The time of exposure to obtain an intense negative six inches diameter, on a collodion plate prepared as below, is about a minute; a positive is obtained in a fraction of a second.

The collodion is formed by dissolving gun-cotton in sulphuric æther, and adding to it a small portion of iodide of silver dissolved

in iodide of potassium, and also a very small portion of bromide of iron, or of iodide or bromide of arsenic. The image is developed by protonitrate of iron, or by a solution of pyrogallic acid in acetic acid and water, and fixed by a solution of hyposulphite of soda.

By taking out the two first lenses of the collectors, the instrument is adapted for using sunlight.

Note.—At the time that this communication was made to the Society, Prof. Stokes had kindly made known to the author the results of his discoveries with regard to the rendering visible the chemical spectrum, but as he had not then made them public, the author of this communication could not state the use that Prof. Stokes's discovery enabled him to make of a screen composed of uranium glass, or of infusion of horse-chestnut bark, for finding the focal distance of the chemical image, or of arranging the lenses of the condenser so as to produce the maximum of chemical action.

Also, since the communication was made, it has been found that the instrument described gives light enough to impress an image on any of the ordinary papers or Daguerreotype plates in periods ranging between one and five minutes, with the oxyhydrogen and lime light; and with direct sunshine the impression is almost instantaneous; of course sunlight is much better than any artificial light when it can be procured, both as regards speed and the clearness of the picture produced.

May 10.—Professor Miller gave an account of a new method of adjusting the Knife-edges of a Balance.

Also of a method of determining the height of clouds by night.

May 24.—Professor Stokes gave a Lecture on the Internal and Epipolic Dispersion of Light.

Nov. 8.—Mr. Adams, F.R.S. &c., gave an account of some Trigonometrical Operations to ascertain the difference of geographical position between the Observatory of St. John's College and the Cambridge Observatory.

The observations, especially those of eclipses and occultations, which were made during many years by the late Mr. Catton at the Observatory of St. John's College, and which have recently been reduced under the superintendence of the Astronomer Royal, render it a matter of some importance to determine the exact geographical position of that Observatory. The simplest and most accurate means of doing this appeared to be, to connect it trigonometrically with the Cambridge Observatory. For this purpose, a base was measured along the ridge of the roof of King's College Chapel, by means of two deal rods terminated by brass studs, the exact lengths of which were determined by comparison with a standard belonging to Professor Miller. The extremities of the base were then connected by a triangle, with a station on the roof of the Observatory at St. John's, from which, as well as from the two former points, a signal post on the roof of the Cambridge Observatory could be seen. The angles at the extremities of the base, combined with the corresponding ones at the station at St. John's, furnished two determinations of the

distance of the Cambridge Observatory, which served to check one another. The meridian line of the transit instrument at St. John's passes through King's College Chapel, so that by observing the point at which it intersected the base, the azimuths of the sides of the triangles could be immediately found.

The result thus obtained is, that the transit instrument of the Cambridge Observatory is 2313 feet to the north, and 4770 feet to the west of that at St. John's College. Hence it follows that the difference of latitude is $22''\cdot8$, and the difference of longitude $5''\cdot10$; and the latitude of the Cambridge Observatory being $52°\ 12'\ 51''\cdot8$, and its longitude $23''\cdot54$ east of Greenwich, we have finally for the geographical coordinates of the Observatory of St. John's College,

Latitude.. $52°\ 12'\ 29''\cdot0$
Longitude $\quad 0°\ 0'\ 28''\cdot64$ E. of Greenwich.

These operations, of course, furnish incidentally, a very exact determination of the orientation of King's College Chapel. The line of the ridge of the roof points $6°\ 20'\cdot3$ to the north of east.

Nov. 22.—Professor Challis made a communication on the recent return of Biela's Double Comet.

Dec. 6.—Professor Stokes gave an account of M. de Sénarmont's Researches relating to the Doubly-refracting Properties of Isomorphous Substances.

Feb. 7, 1853.—An addition was read to a paper by Professor De Morgan on the Symbols of Logic, the Theory of Syllogism, &c.

A paper was read by Mr. Denison on some Recent Improvements in Clock Escapements.

The object of this paper was to explain the construction of a new *remontoire* or *gravity escapement* invented by the author, which has now been in action for some time on the pendulum of the great clock for the houses of parliament, and is in course of application to others, both turret clocks and astronomical.

But by way of introduction to this, which may be called the *three-legged gravity* escapement (from the form of the scape-wheel), Mr. Denison gave a description of another, which would similarly be called the *three-legged dead* escapement, and had been previously invented by him for the purpose of giving the impulse to the pendulum with far less friction than usual. He found that it required only $\frac{1}{4}$th of the force which a common dead escapement had required to make the pendulum swing the same arc. And therefore, as compared with a gravity escapement in which there is no sensible friction on the pendulum, there must be still more than $\frac{3}{4}$ths of the force in a common dead escapement wasted, in first producing friction on the pendulum, and then overcoming it by an increased impulse. The time of the pendulum would be much more disturbed than it is by the inevitable variations of this large amount of friction, as well as that of the clock train, but for a fortunate tendency of the different errors, which are caused by these variations of force and friction, to correct each other.

But the amount of this self-correction is uncertain, and sometimes one set of errors preponderates and sometimes the other; and so a dead escapement clock sometimes gains and sometimes loses simultaneously with either an increase or a decrease of the arc of vibration. And, consequently, none of the contrivances for isochronizing a pendulum for different arcs can secure isochronism of the clock; and no further material improvement in clocks can be expected, but from the solution of what has long been known as the great problem of clock-making, viz. the invention of a simple escapement which will give a constant impulse to the pendulum without any sensible friction.

Mr. Denison showed that his new gravity escapement satisfies all the requisite conditions, mechanical, mathematical, and œconomical. Its principal features are, that the scape-wheel has only three pins, not far from the centre, which lift the pallets or gravity-arms, and three long teeth which are locked by stops on the arms. The velocity of the scape-wheel, which usually produces *tripping*, if the force of the train is increased beyond what is just enough to lift the arms, is moderated by a fan-fly set on the axis of the scape-wheel. The arms are necessarily longer in this than in any other gravity escapement, and this also gives a greater depth of locking within a given angle, and therefore a still further security against tripping. And if an arm is by accident lifted a little too high, the tooth does not escape, and the arm falls down again to its proper height until the pendulum carries it off, the pressure of the long teeth on the stops not being enough to hold it up. For these reasons also there is no difficulty in satisfying the mathematical condition investigated by Mr. Denison in a paper read before the Society in 1848, viz. that γ (the angle at which the pendulum leaves one arm and takes up the other) should $= \dfrac{a}{\sqrt{2}}$, or at any rate not be less than $\dfrac{a}{3}$ (a being the extreme arc of vibration). The escapement requires no oil in the parts affecting the pendulum; and it contains no delicate work, and is very easy to make; and as a highly finished train will be no longer necessary, astronomical clocks may be made on this plan much cheaper, as well as better, than heretofore.

In turret clocks an escapement of this kind supersedes the necessity for a remontoire in the train to equalize the force on the scape-wheel, and also of long and heavy pendulums, which are expensive when compensated, and are sometimes difficult to fix. It will also allow cast-iron wheels to be used throughout the clock (which Mr. Dent has now used for several years in connexion with Mr. Denison's spring remontoire for the train), as the friction of the train can no longer affect the pendulum.

Feb. 21.—Professor Challis gave a Lecture on Halos, Parhelia, and Paraselenæ.

March 7.—Professor Stokes gave an account of some further researches relating to the Change of Refrangibility of Light.

April 11.—The Rev. Mr. Pritchard, F.R.S., gave an account of the Processes requisite to render Quicksilver tremorless for Astronomical Observation.

The great improvements recently introduced, and especially by the present Astronomer Royal, in the construction and methods of using astronomical instruments, require a far more extended use of reflexion from mercury than heretofore. Unfortunately, however, both the convenience and the accuracy of these methods have been greatly limited and impaired by the tremors to which mercury is liable. Many attempts have been made both in France and in Germany to remove or obviate these tremors, but hitherto by no means with perfect success. The Rev. C. Pritchard, of Clapham near London, has proposed a method which appears fully adequate to the requirements of astronomy. It consists in the adoption of a silver-plated or amalgamated copper vessel of a peculiar form, admitting the use of a very thin stratum of mercury without the necessity of an inconvenient amount of shallowness in the vessel itself. Mercury, however, placed in an amalgamated vessel after a short time becomes covered with a singular film of amalgam, which impairs the reflecting power of the surface, and if at all agitated, soon entirely destroys it. And this is the case even when the vessel is made of amalgamated platina. The most important, and by far the most difficult part of Mr. Pritchard's experiments, consisted in the invention of a method by which these films can be easily and practically removed. The details, many of which are curious and interesting, would here occupy too much space, but they are fully explained in a memoir recently read to the Royal Astronomical Society of London; and it may be added, that the process has been adopted at the Royal Observatory at Greenwich, and is now in progress of trial at the Observatories of Paris and Cambridge.

ROYAL SOCIETY.

[Continued from p. 381.]

March 3, 1853.—A paper was read, entitled " On the Meteorology of the English Lake District (Sixth paper, for 1852)." By John F. Miller, Esq., F.R.S. &c.

This paper contains records of the meteorology of the Lake district, similar to those of former years which have been communicated by the author. These are given in tables :—Table I. is a Synopsis of the fall of Rain in the Lake district of Cumberland and Westmoreland in the year 1852. Table II. Wet days (the number in each month and the whole year at each station). Table III. The quantity of Rain received by the mountain gauges in the year 1852 (for each month). Table IV. The quantity of Rain for the summer months (May to October). Table V. For the winter months (November to April). Table VI. Temperature (max. min. approximate mean) at Seathwaite, Borrowdale, 368 feet above the sea-level (for each month and for the year). Table VII. Temperature at Whitehaven. Table

VIII. Minimum temperature of each month, on Sca Fell Pike and Gabel, and at Sprinkling Tarn, from July 1851 to December 1852 inclusive. Table IX. Monthly hygrometrical observations taken at the mountain stations adjacent to the Vale of Wastdale, in the year 1852. Table X. Deductions relative to the humidity of the atmosphere at the mountain stations in 1852. Tables XI., XII., XIII., XIV. contain hygrometrical observations made at various stations in April and July 1848, and in December 1850 and July 1851.

In the remarks which follow the tables, the author states that the past year is distinguished by several marked peculiarities, of which the most prominent are—the very large amount of rain and its very unequal distribution over the different seasons; the enormous and unprecedented fall in the first two and last two months; and the protracted drought of ten weeks in the spring, the longest, though not the most severe which has occurred in the northern counties within the memory of the existing generation. The year is further remarkable for its high temperature; the large amount of surface evaporation; the great heat of July and August; the great quantity of free electricity, as manifested by the unusual number and almost tropical severity of the thunder-storms; the small number of frosty nights, and the entire absence of snow; and, lastly, for the violent gales of wind which prevailed during the last week of December, particularly the hurricane on the morning of Christmas-day. After the discussion of these irregularities, tables are given showing the excess or deficiency per cent. of the principal mountain gauges over or under the quantity of rain received by the adjacent valleys, both in the summer and winter months, in each year since the instruments were erected in 1846. The remainder of the paper is occupied with details referring to the temperature and the hygrometrical observations at the mountain stations.

March 10.—The following letter, addressed to Michael Faraday, Esq., and by him communicated to the Society, was read:—

Dunse (N. Britain), March 1, 1853.

Dear Sir,—In the report in the Athenæum of your lecture at the Royal Institution on the 21st of January, I observe that you refer to the highly interesting observations of Schwabe, Sabine, Wolf, Gautier, &c., from which it would appear that a connection exists between the solar spots and the variations of the terrestrial magnetic forces. Since a connection has been demonstrated to exist between the latter and auroral phenomena, I was induced to look over my notes relating to the auroræ observed at this place, with a view to ascertain whether these also exhibited maxima and minima, and if so, whether the periods of such agreed with those of the solar spots and of the magnetic variations. The subjoined table shows the distribution of the auroræ seen here in the years 1838 to 1847 inclusive:—

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sum.
1838	5	3	4	3	...	...	...	2	4	1	2	3	27
1839	9	1	2	4	1	...	...	...	11	7	2	1	38
1840	5	5	2	4	...	...	...	3	7	6	6	5	43
1841	6	3	4	4	2	...	...	3	3	3	7	7	42
1842	2	2	...	...	...	...	1	...	3	...	1	...	9
1843	2	1	1	1	...	...	...	2	...	3	...	...	10
1844	1	...	2	...	...	...	...	...	...	1	4	2	13
1845	1	2	...	1	...	...	...	1	2	1	1	1	10
1846	...	1	...	...	...	...	1	2	7	4	1	...	16
1847	2	2	3	1	...	...	...	1	5	6	6	4	30
	33	20	18	18	3	...	2	14	43	34	30	23	238

These figures speak for themselves. I may remark that the returns for 1842 are incomplete, as I was absent from home during March and April of that year. In 1848 I was also absent for some months, but from the number of auroræ which I have noted during that year, I am satisfied that a maximum then occurred, both as regards the number and the intensity of auroral displays. This present winter has been very barren in auroral phenomena.

Of crimson auroræ I find I have noted two in 1837, one in 1839, one in 1846, three in 1847, and no less than six in 1848.

A discussion of the auroræ seen in North America and the North of Europe during a series of years would be interesting with reference to the points in question.

Apologizing for troubling you,

I am, dear Sir,

With the greatest respect, yours faithfully,

Wm. Stevenson.

The following communication was likewise read:—" On the Reproduction of the Toad and Frog without the intermediate stage of Tadpole." By Edward Joseph Lowe, Esq., F.G.S., F.R.A.S. Communicated by J. Lee, LL.D., F.R.S. &c.

The following brief remarks on the Toad (*Bufo vulgaris*) and the Frog (*Rana temporaria*) may perhaps be received with some degree of interest, as they are, I believe, contrary to the generally received notion of the procreation of these reptiles. Ray, and most naturalists, at least, consider toads and frogs as oviparous animals, yet it is apparent that they are viviparous as well, or if they do not bring forth their young alive, have the power of reproduction in a different manner to the ova and subsequent tadpole.

Mr. J. Higginbottom of Nottingham, who has paid great attention to this subject, has clearly proved the development of the tadpole to the perfect toad in situations wholly deprived of light, as I have through his kindness several times witnessed. My present remarks are intended to show that *occasionally* frogs and toads are reproduced in localities where it would be impossible for the intermediate stage of tadpole to have any existence.

First. *Toads deposit spawn in cellars and young toads are after-wards observed.*

Last summer several masses of spawn were procured from my cellar, having been found deposited amongst decaying potatoes, &c., and subsequently young toads were noticed. The cellar is free from water, and at a considerable distance from any brook.

Secondly. *Young toads are observed about hot-beds.*

In the kitchen-garden at Highfield House (which is entirely walled round) young toads have been noticed about the cucumber- and melon-beds. The gardeners have been in the habit of bringing toads to these beds to destroy the insects; these have continued amongst the warm damp straw all summer. It is after these beds have remained three or four months that the young ones have been noticed. Toads would have to travel nearly half a mile to reach this garden from the brook or lake, and also to mount a steep hill, besides taking the opportunity of coming through the door. Toads so small are not seen in any other part of the gardens.

Thirdly. *Young toads and frogs observed in abundance at the summit of another hill, whilst quite small.*

During the past summer, especially in the month of July, very many young toads and frogs were seen amongst the strawberry plants, apparently from a week to a month old. These might possibly have travelled from the brook a few hundred yards distant; yet it is strange, that with the exception of these beds, no young toads could be found elsewhere in the garden. A number of full-grown toads are mostly to be seen about these beds.

Fourthly. *Young frogs dug out of the ground in the month of January.*

In digging in the garden amongst the strawberry-beds (near where so many toads were observed last summer) in the middle of January in the present year, a nest of about a score young frogs were upturned. These were apparently three or four weeks old. This ground had been previously dug in the month of August and many strawberry plants buried; it was amongst a mass of these plants in a state of partial decomposition that these young ones were observed.

Fifthly. *Young frogs are bred in cellars where there is no water for tadpoles.*

In mentioning this subject to Mr. Joseph Sidebotham of Manchester (an active botanist), he informed me that young frogs, and in fact frogs of all sizes, were to be seen in his cellar amongst decaying dahlia tubers. The smallest of them were only about half the ordinary size of the young frog when newly developed from the tadpole. He further stated that there was no water in the cellar, and no means of young frogs entering, except by first coming into the kitchen, a mode of entry, if not impossible, highly improbable. Mr. Sidebotham never found any spawn.

It seems probable from the above, that frogs are occasionally born alive in situations where no water can be found for the spawn to be deposited in, and that toads are either reproduced in the same

manner, or from the egg directly. The latter mode seems most likely, owing to spawn having been found previously to the young toads.

Mr. Higginbottom tells me, the same remark on the birth of the Triton, without the stage of tadpole, has been mentioned to him.

These are the facts; should the subject be deemed worthy of further investigation, I shall be glad to continue observations upon these reptiles during the present year, or to make any experiments that may be deemed advisable.

LXXIV. *Intelligence and Miscellaneous Articles.*

REMARKS ON ELECTRIC LIGHT. BY A. MASSON.

THE researches[*] which have already been made known by the author lead to the following results.

A barometric vacuum offers a resistance to the passage of electric currents which increases with its length; this resistance is less in the partial vacuum produced by an air-pump.

A ponderable medium is necessary for the production of the electric light.

The author, together with M. Breguet[†], has likewise succeeded in verifying most completely the identity of the luminous phænomena produced by induced currents and those which are caused by the discharge of a condenser. The former have the advantage of being continuous, the latter possess greater tension. Some recent experiments which they have made serve to establish the accuracy of these propositions.

The impossibility of making the current of a powerful inductive apparatus traverse a Torricellian vacuum three decimetres in length[‡], can only be attributed to the fact that the electrical tension is insufficient, since the discharge of a very small condenser illuminates the vacuum, and the authors have obtained currents in the vacuum of a barometer.

The following experiment made with improved instruments constructed by Ruhmkorff confirms this assumption. The exhausted tube having been placed between the two poles of an induced current, the phænomena described in their memoir upon electrical spectres were produced; the entire vacuum was filled with a pale white phosphorescent light.

The tube presented the same appearance when perfectly isolated on being connected at one end with a single pole of the induced current. In this case the current is interrupted, and the discharge from the opposite extremity of the tube must necessarily take place into the air. This curious fact is very analogous to those mentioned in the author's memoir on induction.

[*] Études de Photométrie Électrique, *Ann. de Chimie et de Phys.* sér. 3. vols. xiv. xxx. xxxi.

[†] Mémoire sur l'Induction, *Ann. de Ch. et de Phys.* sér. 3. vol. iv.

[‡] *Ann. de Ch. et de Phys.* sér. 3. vol. xxxi.

The rapid succession of discharges of an induced current, which, since the publication of Masson's researches on the physiological effects of electricity*, has been so successfully applied in the treatment of paralysis, the continuity of the effects obtained by means of induced currents, render it a very desirable object to increase the tension of these currents, and the results already obtained in this direction by M. Ruhmkorff, justify the opinion that the electrical machines now in use will be superseded by an apparatus of induction.

In order to increase the power of such an apparatus, the authors have attempted to make the induced current of two coils traverse the same wire; the primary currents were kept apart and interrupted at the same instant.

When both the induced currents were in the same direction, the luminous phænomena in the vacuum indicated that the action of the combined currents was greater than that of a single current, but the increase of the action was not such as had been anticipated.

When the two currents traversed the same wire in opposite directions, they did not exercise any mutual influence on each other, and the results observed are not unimportant. The two balls forming the poles in the vacuum were surrounded, as well as their supports, by the violet-blue atmosphere, previously described by the author and M. Breguet†. The two poles were of the same substance, and between them was a reddish flame which disappeared when the balls were brought nearer together; at a distance of 4 or 5 centimetres the space between the poles is perfectly dark, although they were surrounded by their luminous atmospheres. The luminous phænomena appeared to be more intense than in the former experiment.—*Comptes Rendus,* February 1853.

ON THE PERMEABILITY OF METALS TO MERCURY.
BY J. NICKLÈS.

In an interesting memoir on the permeability of metals to mercury, by Prof. Horsford, this author establishes the fact of the permeability of tin, lead, gold, silver, zinc and cadmium, and contests that of copper and brass.

The negative results which this chemist has obtained with copper and brass have evidently proceeded from the process which he has followed; for some time since, while engaged in my researches on magnetization, I observed positive facts to the contrary. I was using at this time a Bunsen's battery with zinc exterior. The connecting pieces of copper were riveted to the zinc, and on amalgamating the latter metal, it often happened that the mercury spread itself over the copper in contact with the zinc; and after a certain time the metal of the connecting pieces became brittle, and had on

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* Théorie de Phénomènes Electrodynamiques, *Ann. de Ch. et de Phys.* sér. 2. vol. lxvi.

† *Mémoire sur l'Induction.*

breaking a white colour. It was obviously no longer copper, but an amalgam of this metal.

This fact had too important a bearing on the amalgamation of zinc, which occupied me at that time, to be allowed to pass without examination. The experiments which I made on this subject led to the general conclusion, that the metals which absorb mercury are permeable by it, and communicate this property to the alloys which contain a certain proportion of permeable metal. The metals on which I have operated are zinc, iron, cadmium, tin, copper, lead, antimony, silver, gold and platinum. The manner of experimenting was suggested by the fact which had led to the researches. With a stylet I traced a line or furrow on the plate under experiment, and placed a little mercury in this furrow. In order to hasten the amalgamation, I introduced before the mercury a drop of bichloride of mercury acidified with hydrochloric acid. By this means the amalgamation takes place instantly, and the surface is fitted to retain at once the quantity of mercury necessary for the effect I wished to produce.

A plate of zinc, a millimetre thick, is immediately attacked, and at the end of a minute it is cut in two in the direction of the furrow. A thicker plate requires more time and a deeper scratch on its surface; with a thickness of 6 millims. the plate took ten minutes and a little effort to break it; but in all cases the fracture was neat and in the direction of the scratch or furrow.

After zinc comes cadmium and tin, then lead, silver, gold, and finally copper. All of these metals become amalgamated; the mercury infiltrates after a time more or less long, and renders them brittle. No permeability has been detected in iron, antimony and platinum, and these metals form no amalgam. Daniell found a bar of platinum free from mercury, that had been for six years in contact with it, and to this time no amalgam of iron has been reported.

There exists, it is true, an amalgam of antimony; but, according to Wallerius, when this amalgam, of a pasty consistence, is triturated in the air, or when agitated with water, it loses its antimony in the form of a black powder*.

As to the action of mercury on metals, this fact at least is established,—the resistance of some to amalgamation and the easy amalgamation of others. We have also shown that the permeability takes the same direction, and we may conclude therefore that the alloys will participate in these qualities in the order of their composition. In fact, a plate of bronze 5 millims. thick may be cut in two by mercury after ten minutes of contact, and a plate $\frac{1}{2}$ a millim. thick yields instantaneously. On amalgamating a strip of brass, it may after a few minutes be reduced to fragments between the thumb and finger. A plate of 4 millims. required ten minutes to cut it in two. An alloy, formed of equal parts of antimony and tin, did not appear to be impressed by the mercury. Chaudet's alloy (3 or 4 per

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* It should be borne in mind, in connexion with this subject, that mercury also is reduced to a black powder by agitation in the air.

cent. of antimony and 96 to 97 of tin), very elastic in its pure state, amalgamates instantly, and is easily cut off.

It is here seen, that if the permeability of brass and copper for mercury is not established by the process, based on *capillarity*, followed by Prof. Horsford, we may still make this property evident by an inverse course, that is by inducing *infiltration*.

As these experiments were made only incidentally, and were not intended to be published, I have not attempted to give them precision. Nevertheless the facts which have been mentioned above are well established, for I have been accustomed to use this process for dividing plates of zinc or sheets of brass in my experiments on electro-magnetism.—Silliman's *Journal*, January 1853.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1853.

Chiswick.—April 1. Rather boisterous : fine. 2. Cloudy : fine : hazy. 3. Overcast : showery. 4. Cloudy : very fine : rain. 5. Densely overcast : rain : clear. 6. Overcast throughout. 7. Overcast : very fine : clear. 8. Fine : heavy thunderstorm : clear at night frosty. 9. Clear : fine, but cold : overcast. 10. Overcast. 11. Overcast : fine : overcast. 12. Clear : fine : overcast. 13. Cloudy and cold : rain : clear : sharp frost at night. 14. Cloudy. 15, 16. Overcast throughout. 17. Fine : overcast. 18. Very fine. 19. Cloudy : rain. 20. Low white clouds and cold. 21. Rain. 22. Constant rain. 23. Fine. 24. Heavy rain. 25. Snow early A.M., boisterous, with rain. 26. Fine. 27. Hoar-frost : very fine. 28. Light haze : fine. 29. Cold rain. 30. Overcast : very fine.

  Mean temperature of the month .... 44°·93
  Mean temperature of April 1852 .... 44 ·81
  Mean temperature of April for the last twenty-seven years . 47 ·21
  Average amount of rain in April .... 1·65 inch.

Boston.—April 1. Fine : rain A.M. 2. Fine : rain P.M. 3. Cloudy : rain P.M. 4. Cloudy : rain A.M. and P.M. 5. Cloudy. 6. Cloudy : rain P.M. 7. Fine. 8. Cloudy : rain and hail A.M. and P.M. 9. Fine. 10, 11. Cloudy. 12. Fine. 13. Cloudy : rain A.M. 14—18. Cloudy. 19. Cloudy : rain P.M. 20. Cloudy : rain A.M. 21. Cloudy. 22. Cloudy : rain A.M. 23. Fine. 24. Fine : hail A.M. 25. Rain : rain A.M. and P.M. 26. Fine. 27. Fine : rain A.M. 28. Fine. 29. Cloudy : rain P.M. 30. Cloudy.

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

  Mean temperature of April for twenty-six previous years ... 43°·44
  Mean temperature of April 1852 .... 47 ·64
  Mean temperature of this month .... 44 ·49
  Average quantity of rain in April for seven previous years . 1·73 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. 1853. April.	Barometer. Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Orkney, Sandwick. 9½ a.m.	Orkney, Sandwick. 8½ p.m.	Thermometer. Chiswick Max.	Chiswick Min.	Boston 8½ a.m.	Orkney, Sandwick. 9½ a.m.	Orkney, Sandwick. 8½ p.m.	Wind. Chiswick 1 p.m.	Boston.	Orkney, Sandwick.	Rain. Chiswick.	Boston.	Orkney, Sandwick.
1.	29·479	29·354	28·94	29·02	28·88	57	36	48	43	43	sw.	s.	e.	·07	·07	·04
2.	29·782	29·644	29·24	29·05	29·25	56	36	45	45½	43	w.	sw.	calm			·20
3.	29·705	29·510	29·32	29·35	29·42	54	42	48	45	45	s.	s.	se.	·12	·12	·24
4.	29·760	29·662	29·30	29·47	29·23	62	50	49	48	46	sw.	wnw.	s.	·14	·16	
5.	29·976	29·775	29·40	29·25	29·50	61	43	54	47	46	w.	w.	sw.	·02	·10	·36
6.	29·948	29·828	29·46	29·37	29·31	59	41	47·5	52	43½	w.	wsw.	wsw.	·02		·18
7.	29·806	29·726	29·28	29·32	29·57	57	32	50	45½	41	sw.	sw.	nw.		·02	·21
8. ●	30·119	29·809	29·46	29·87	30·14	53	28	46	41	36½	n.	w.	w.			·10
9.	30·304	30·175	29·94	30·10	29·72	50	31	39	43	45	n.	nnw.	w.	·06	·07	·01
10.	30·128	30·039	29·67	29·88	29·96	55	29	51	44½	43½	nw.	nnw.	sw.	·04		·12
11.	30·161	30·082	29·76	29·80	29·94	60	37	50	46	41	w.	nnw.	wnw.			·40
12.	30·015	29·887	29·66	29·94	30·11	56	32	46·5	44	40	nw.	nnw.	n.			·06
13.	30·057	29·892	29·64	30·13	30·04	48	22	40·5	43	42	n.	nne.	nw.	·04	·06	
14.	30·145	29·999	29·70	30·01	30·05	57	31	44	45	44½	n.	n.	wnw.			·04
15.	30·106	30·105	29·79	30·01	29·95	51	42	45	49	45	nw.	nw.	w.			
D 16.	30·098	30·088	29·75	29·86	29·75	55	39	49·5	47½	45	nw.	nw.	w.			
17.	30·068	29·997	29·64	29·80	29·80	58	45	53	48	48	w.	n.	s.			
18.	30·070	30·000	29·70	29·81	29·76	65	38	51	48	51	n.	w.	ssw.			·20
19.	29·896	29·760	29·53	29·60	29·66	56	37	58·5	52	41	sw.	ssw.	nw.	·07		·05
20.	29·775	29·716	29·40	29·72	29·71	55	38	46	47	40	nw.	nnw.	calm	·25		·38
21.	29·518	29·440	29·24	29·64	29·61	55	42	51	46	41	se.	sse.	calm	·28	·06	
22.	29·486	29·315	29·16	29·58	29·62	51	36	45·5	47½	43	w.	e.	calm	·24		
○ 23.	29·879	29·765	29·46	29·56	29·54	54	32	46	44	43	sw.	nnw.	wnw.	·09	·04	·06
24.	29·858	29·570	29·45	29·53	29·56	47	31	45·5	44½	41	n.	w.	wsw.	·60		
25.	29·526	29·266	29·00	29·67	29·82	41	31	40	43	37½	n.	ne.	e.	·16	·17	·43
26.	29·805	29·668	29·33	29·84	29·82	53	25	43	46	39	nw.	n.	n.		·24	
27.	29·872	29·836	29·56	29·71	29·71	60	28	42	47	42½	sw.	n.	calm		·06	·12
28.	29·821	29·718	29·57	29·79	29·84	56	40	44	45	43	e.	ese.	s.	·38		
29.	29·634	29·586	29·43	29·83	29·81	56	37	49·5	48	41	e.	ne.	ese.			·10
☾ 30.	29·808	29·665	29·36	29·72	29·76	66	31	52	52	44	sw.	w.	se.		·33	
Mean.	29·888	29·762	29·47	29·674	29·694	55·46	35·40	47·3	46·23	42·76				2·58	1·50	3·30

LXXV. *Researches upon the Specific Heat of Elastic Fluids**.
By M. V. REGNAULT.

FOR more than twelve years I have been occupied in col-
lecting the data necessary for the solution of the following
problem :—

What is theoretically the motive power which may be ob-
tained from a given quantity of heat by applying it to the deve-
lopment and dilatation of various elastic fluids under the various
conditions which are practically realizable ?

The complete solution of this problem should furnish not
merely the true theory of the steam-engines used at the present
day, but likewise that of machines in which the vapour of water
may be replaced by another vapour, or even by a permanently
elastic fluid whose elasticity is augmented by heat.

At the time when I entered upon these researches, the ques-
tion appeared to me more simple than it does at present. Adopt-
ing the views then recognised in science, it was easy to define
precisely the various data upon which it rested ; and I pictured
to myself the methods by the aid of which I hoped to be able
successively to discover the laws and determine the numerical
data. But, as is usually the case in sciences of observation, in
proportion as I advanced in my studies, the subject continually
expanded ; the questions which in the first instance appeared to
be very simple, now present a considerable degree of complexity ;
and perhaps I should not have had the courage to enter upon
the investigation of the subject, if I had from the first perceived
all the difficulties with which it is encompassed.

Until very recently it was admitted that the quantities of heat
disengaged or absorbed by any one elastic fluid were always equal
when that fluid passed from a particular initial state to an iden-
tical final state, whatever might be the direction in which the
transition took place ; in short, it was considered that these

* From the *Comptes Rendus*, April 18th, 1853.

quantities of heat were dependent only on the initial and final conditions of temperature and of pressure, and that they were independent of the intermediate circumstances through which the fluid had passed.   In 1824, Carnot published, under the title of *Réflexions sur la Puissance Motrice du Feu,* a work which did not in the first instance attract much attention, and in which he assumed as a principle that the motive power produced in a steam-engine is due to the passage of heat from its source to the colder condenser by which it is finally collected.   M. Clapeyron has developed the hypothesis of Carnot mathematically, and he has shown that the quantities of heat gained or lost by any one gas does not depend solely upon its initial state and its final state, but likewise upon the intermediate states through which it passes.

Within the last few years, the mechanical theory of heat has met with more favourable consideration, and at the present moment occupies the attention of a great number of mathematicians.   But an important modification has been made in the principle of Carnot; it has been assumed that heat may be converted into mechanical work, and reciprocally that mechanical work may be converted into heat.   According to Carnot's theory, the quantity of heat possessed by the elastic fluid at its entrance into the steam-engine is found undiminished in the elastic fluid which is discharged from it, or in the condenser; the mechanical work being produced solely by the passage of heat from the boiler to the condenser in traversing the engine.   According to the new theory, the whole of this quantity of heat does not remain in the state of heat; a portion of it disappears during the passage through the engine, and the motive work produced is in all cases proportional to the quantity of heat lost.   Thus in a steam-engine, with or without condensation, working or not working expansively, the mechanical work is proportional to the difference between the quantity of heat which the vapour possesses at its entrance into the engine, and that which it possesses at its discharge, or at the moment when it is condensed.   According to this theory, to obtain from the same quantity of heat the maximum mechanical effect, an arrangement should be adopted in which that loss of heat should be as great as possible, that is to say, the elastic force which the vapour retains at the moment when it enters the condenser should be as small as possible. But in every case referring to the steam-engine, the quantity of heat rendered available for the mechanical effect would only be a very small fraction of that which it is necessary to communicate to the boiler.   In a high-pressure engine without a condenser, into which the vapour enters under a pressure of five atmospheres and escapes under the ordinary atmospheric pressure, the quan-

tity of heat possessed by the vapour at its entrance is, according to my experiments, about 653 units; that which it retains at its escape amounts to 637. According to the theory of which I am speaking, the quantity of heat rendered available for mechanical work would be $653 - 637 = 16$ units, that is to say, only $\frac{1}{40}$th of the quantity of heat communicated to the boiler. In a condensing engine, receiving vapour of maximum density under a pressure of five atmospheres, and in which the condenser constantly presents an elastic force of 55 millims. of mercury, the quantity of heat of the vapour entering would be 653 units, and that of the vapour at the moment of condensation, that is to say, when it is lost for the mechanical action, is 619 units. The heat rendered available would be 34 units, or little more than $\frac{1}{20}$th of the heat communicated to the boiler.

A much larger fraction would be obtained for the heat rendered available as mechanical work, either by overheating the vapour before it entered the engine, or by lowering as much as possible the temperature of the condenser. But this latter method is difficult to realize in practice; it would require, moreover, a considerable increase in the quantity of cold water by which the condensation is effected, which would waste the motive power, and the water used to feed the boiler would not be very warm. The same object might be more easily attained by subjecting the vapour to a lower pressure, and then condensing this vapour by injecting a very volatile liquid, such as chloroform or æther. The heat possessed by the water-vapour at the moment of this condensation, of which a very small quantity only could be converted into mechanical work, passes into the volatile liquid, which it converts into high-pressure vapour. By making this vapour pass into a second engine, where it expands until its elastic force is equal to that to which the injected water might practically bring it in the condenser, one portion of the heat is converted into motive work; and the theory based upon the numerical data of my experiments shows that this quantity is much greater than that which should be obtained by means of a more considerable pressure of the water-vapour in the first engine. In this manner a perfect explanation may be given of the œconomic result obtained by employing two connected engines, the one worked by water-vapour, the other by vapour of æther or chloroform, and with which experiments have been made for some time.

In air-machines, where the motive force is produced by the expansion of the air in the machine by heat, or by the increase in its elastic force, the motive work produced at each stroke of the piston should always be proportional to the difference between the quantities of heat possessed by the air on entering and

escaping from the engine, that is to say, in short, equal to the loss of heat by the air in traversing the engine. But as, according to Ericson's system, the heat which the air possesses at the time of its escape is deposited in the substances from which the fresh air in entering abstracts it in order to convey it back again into the engine, it is evident that, theoretically speaking, in these latter engines all the heat is rendered available for the mechanical work; while in the best constructed engines worked by water-vapour the heat rendered available for mechanical work does not amount to $\frac{1}{20}$th of that generated. It must, however, be remembered that I do not here take into consideration the exterior losses, such as mechanical or industrial obstacles, which may present themselves in practice.

MM. Joule, Thomson, and Rankine in England, MM. Mayer and Clausius in Germany, setting out from different points of view, have developed mathematically the mechanical theory of heat, and they have endeavoured to deduce from it the laws of all phænomena relating to all elastic fluids. I have, for my own part, long put forward in my lectures analogous views, to which I have been led by my experimental researches upon elastic fluids. In the course of those researches, I have encountered indeed at every step anomalies which appeared to me inexplicable in accordance with the theories formerly recognised. For the sake of illustration I will quote some of the most simple instances :—

First Example.—1. A mass of gas under a pressure of 10 atmospheres is contained in a space which is suddenly doubled; the pressure falls to 5 atmospheres.

2. Two reservoirs of equal capacity are placed in a calorimeter; the one is filled with gas under a pressure of 10 atmospheres, the second is perfectly empty. When a communication is suddenly made between the two reservoirs, the gas expands to double its volume, and the pressure is reduced to 5 atmospheres.

In these two experiments, then, the initial and final conditions of the gas are the same; but this identity of conditions is accompanied by calorific results, which are very different; for while in the former experiment there is a considerable reduction of temperature, in the second the calorimeter does not indicate the slightest alteration of temperature.

Second Example.—1. A mass of gas M traverses under the ordinary atmospheric pressure a winding tube in which it is heated to 212° F., and then a calorimeter whose initial temperature is 32° F. The temperature of the calorimeter is raised t degrees.

2. An equal mass of the gas traverses, under a pressure of 10 atmospheres, the winding tube, in which it is heated to

212° F.; then the calorimeter at 32° F. under the same pressure. The temperature of the calorimeter is raised t' degrees, and experiment shows that t' differs very little from t.

3. An equal mass of the gas traverses, under a pressure of 10 atmospheres, the winding tube, in which it is heated to 212° F.; but on reaching the orifice of the calorimeter at 32° F., the gas expands, and the pressure falls below that of the atmosphere in such a manner that it escapes from the calorimeter with the same temperature and pressure as the surrounding atmosphere. The temperature of the calorimeter is raised t'' degrees.

According to the theories formerly recognised, the quantity of heat disengaged by the gas in the experiment No. 3 should be equal to that in No. 2, minus the quantity of heat absorbed by the gas during the enormous expansion it has undergone, seeing that its volume has increased tenfold; but, on the contrary, experiment shows that t'' has a value greater than t' and t.

It would be possible to bring forward more examples, but this would involve an anticipation of what follows. I shall reserve the discussion of this subject until the publication in a complete form of my experiments upon the compression and expansion of gases.

In any case, the examples already given will suffice to show that great circumspection should be exercised in drawing conclusions from experiments in which elastic fluids are in a state of motion, undergoing variations of elasticity, and effecting mechanical work frequently difficult to appreciate; for the calorific effects produced depend in a great measure upon the order and mode in which these variations take place.

Unfortunately, although it is easy to propound in a vague manner a physical theory, it is very difficult to state it with precision in such a manner as not only to include all the facts already known, but likewise to deduce from it those which have hitherto escaped observation. In this respect the theory of luminous undulations, as established by Fresnel, is an isolated fact in physical science. The equation of problems relating to heat, regarded under a mechanical point of view, leads, like all analogous problems, to an equation of partial differences of the second order, between several variable quantities which are unknown functions of each other. These functions represent the true elementary physical laws, which it is necessary to know in order to arrive at a complete solution of the problem. The integration of the equation introduces arbitrary functions, the nature of which should be sought for by comparing the results given by the equation with those obtained by direct experiment, and with the laws deduced from those experiments. In researches upon heat, unfortunately, direct experimentation is rarely applicable to

simple phænomena; generally speaking, it has to do with complex questions, which depend upon many of those laws at one and the same time, and very frequently it is difficult to assign to each its proper influence. The experimenter should therefore endeavour to modify the circumstances in which he operates in such a manner as to vary as much as possible in the several experiments the influence of each of the elementary phænomena and the law which expresses them. He will thus obtain equations of condition, which may be of very great assistance towards discovering the general theory, for that, whatever it may be, should always be fulfilled.

It is from this point of view that I have prosecuted my researches; and I have always striven to define, in the most precise manner, the conditions under which I have operated, in order that whatever theory may finally prevail, some advantage may be derived from my experiments.

In 1847, I published the first part of my researches; they constitute the twenty-first volume of the *Mémoires de l'Académie.* Since that time I have not ceased to carry them on; but the necessary experiments were so numerous, the numerical calculations so long and laborious, that it would have been impossible for me to execute them if left to my individual efforts. I have been very considerably aided by M. Izarn, who had already taken part in the first part of my labours, and by a young mining engineer, M. Descos, whom the Minister of Public Works has given me as an assistant, in order to hasten the completion of the investigation.

The subjects to which my recent experiments relate are the following :—

1. The relations which exist between the temperatures and tension of a great number of vapours in a state of maximum density, from the most feeble tension to that of 12 atmospheres.

2. The tension of the same vapours at their maximum density.

3. The tension of vapour of maximum density produced by mixed liquids.

4. The latent heat of these vapours under different pressures, from the most feeble to that of 8 or 10 atmospheres.

5. The latent heat of vaporization of the same substances in gases.

6. The specific heat of permanent gases and vapours under different pressures.

7. The quantities of heat absorbed or disengaged by the compression and the expansion of gases, either when that expansion is effected in a space the capacity of which is augmented, or when it takes place during the passage through a capillary opening in a thin partition or through a long capillary tube.

8. The quantity of heat absorbed by a gas when it produces during its expansion motive work which is completely expended in the interior of the calorimeter, or of which the greater part is rendered available externally.

9. Lastly, the densities of vapour of maximum density under different pressures.

The experiments which refer to these different questions are, with the exception of the latter, already almost completed. But as it will still require some considerable time to arrange them in their proper order and discuss them with suitable care, I purpose to bring before the Academy in succession the general results, preparatory to publishing them in a complete form.

I shall, in the present instance, limit myself to the consideration of my researches upon the capacity for heat of elastic fluids.

Capacity for Heat of Elastic Fluids.

Two definitions of the specific heat of elastic fluids may be given. According to the former, the specific heat is that quantity of heat which must be communicated to a gas in order to raise its temperature one degree, under conditions which admit of its free expansion, so that it may preserve a constant tension ; according to the second, it is the quantity of heat necessary to elevate its temperature one degree under such conditions that its volume remains the same while its tension increases.

The former of these capacities has been termed the specific heat of gases under constant pressure ; the second has been termed the specific heat with constant volume. The former definition alone coincides with that which has been adopted for the capacity for heat of solids and liquids ; it is therefore the only one which has hitherto been found to admit of a direct experimental determination.

During the last century a great number of physicists have been engaged with investigations relating to the specific heat of elastic fluids. Crawford, Lavoisier and Laplace, Dalton, Clément and Désormes, Delaroche and Bérard, Haycraft, Gay-Lussac, Dulong, De la Rive and Marcet, have successively published researches upon this subject. Most of these physicists have endeavoured to demonstrate by experiment certain laws, to which they had been led by *à priori* reasoning on the constitution of elastic fluids. They applied themselves less to determine the numerical values of the capacity of different gases for heat in relation to that of liquid water, generally adopted as unity, than to seek for simple relations, which they supposed ought to exist between them. The conclusions at which they arrived are in most instances very erroneous.

The memoir of Delaroche and Bérard, which was rewarded

with a prize by the Academy in 1813, is at the present time the most complete one upon this subject, and that in which the results differ least from the truth. This superiority is owing not only to the extreme care with which these skilful experimenters carried out their investigation, but likewise to the direct method which they adopted; while the greater number of other physicists have had recourse to indirect methods, in which the element sought frequently exercised but a very slight influence.

The general results at which Delaroche and Bérard arrived are the following :—

1. All gases do not possess the same specific heat in regard either to volume or weight. The specific heat of equal weights or equal volumes of the several gases has the following values for these two modes of comparison :—

Specific heat of	Equal volumes.	Equal weights.	Spec. grav.
Air	1·0000	1·0000	1·0000
Hydrogen	0·9033	12·3401	0·0732
Carbonic acid	1·2583	0·8280	1·5196
Oxygen	0·9765	0·8848	1·1036
Nitrogen	1·0000	1·0318	0·9691
Protoxide of nitrogen .	1·3503	0·8878	1·5209
Olefiant gas	1·5530	1·5763	0·9885
Carbonic oxide . . .	1·0340	1·0805	0·9569

2. The capacity for heat of the same gases in relation to water is expressed by the following numbers :—

Specific heat of water	1·0000
...	atmospheric air . . .	0·2669
...	hydrogen	3·2936
...	carbonic acid	0·2210
...	oxygen	0·2361
...	nitrogen	0·2754
...	protoxide of nitrogen .	0·2369
...	olefiant gas	0·4207
...	carbonic oxide . . .	0·2884
...	aqueous vapour . . .	0·8470

3. The specific heat of atmospheric air with constant volume increases with its density, but in less rapid progression. The relation of the pressures being $\frac{1}{1\cdot3583}$, that of the specific heats is $\frac{1}{1\cdot2396}$.

4. Delaroche and Bérard assumed, for theoretical reasons founded upon the direct experiments of Gay-Lussac, that the specific heat of a gas increased rapidly with the temperature.

Such are the most accurate views which we possess at the

present day upon the specific heat of gases, and which are gene-
rally recognised by physicists. The limits within which I am
compelled to restrict myself in this abstract render it impossible
to enter upon any discussion of the methods which have been
adopted by my predecessors, or any description of those which
I have followed myself. It will be sufficient to remark, that I
have encountered in this class of investigations very great diffi-
culties, not only in regard to the actual experiments, but like-
wise in a theoretical point of view. The remarks which have
been made at the commencement of this abstract will render this
easily conceivable. Thus, although my first experiments were
made five years ago, and announced at that time in my memoirs
upon the specific heat of solid and liquid substances, it is only
now, after having employed the most different methods, and
having caused the elements of correction to destroy each other,
that I can with confidence lay the results of my investigation
before the Academy.

According to my experiments, the specific heat of air in rela-
tion to that of water is—

Between − 30° and +	10°	0·2377
Between + 10° and	100°	0·2379
Between +100° and	225°	0·2376

Thus, contrary to the experiments of Gay-Lussac, the specific
heat of air does not vary sensibly at different temperatures. Ex-
periments made with several other permanent gases have led to
a similar result.

In experimenting with atmospheric air under a pressure
varying from 1 to 10 atmospheres, I have not found that there
is any sensible difference between the quantities of heat disen-
gaged by a given mass of air in cooling a certain number of
degrees. Thus, contrary to the experiments of Delaroche and
Bérard, who have shown that there is a very considerable dif-
ference for pressures varying only as much as from 1 atmosphere
to 1·3 atmosphere, the specific heat of a given mass of gas would
be independent of its density. Experiments made with a num-
ber of other gases have led to analogous results. Nevertheless
I state this law with some degree of reserve; for I am as yet
unable to decide whether the capacity for heat is absolutely con-
stant under different pressures, or whether it is subject to a
slight correction arising from the state of motion of the gas.

The specific heat, 0·237, of air in relation to water is con-
siderably less than the number 0·2669, adopted by Delaroche
and Bérard; it is moreover the result of a hundred determina-
tions made under various conditions.

The other elastic fluids, for which I have determined the spe-
cific heat, are—

Gases.	Specific heat for weights.	Specific heat for volumes.	Densities.
Oxygen	0·2182	0·2412	1·1056
Nitrogen	0·2440	0·2370	0·9713
Hydrogen	3·4046	0·2356	0·0692
Chlorine	0·1214	0·2962	2·4400
Bromine	0·05518	0·2992	5·39

On looking through this table, it will at once be seen that the specific heat of oxygen differs but very little from that of an equal volume of either nitrogen or hydrogen ; thus one would be led to infer that the specific heat of simple gases is the same for equal volumes under the same pressure. But for chlorine and bromine numbers have been obtained which are almost equal to each other, but much greater than those which have been obtained for other simple gases :—

Compound gases.	Specific heat for equal weights.	Specific heat for equal volumes.	Densities.
Protoxide of nitrogen . . .	0·2238	0·3413	1·5250
Binoxide of nitrogen . . .	0·2315	0·2406	1·0390
Carbonic oxide	0·2479	0·2399	0·9674
Carbonic acid	0·2164	0·3308	1·5290
Sulphuret of carbon . . .	0·1575	0·4146	2·6325
Sulphurous acid	0·1553	0·3489	2·2470
Hydrochloric acid	0·1845	0·2302	1·2474
Hydrosulphuric acid . . .	0·2423	0·2886	1·1912
Ammonia	0·5080	0·2994	0·5894
Protocarburet of hydrogen (marsh gas)	0·5929	0·3277	0·5527
Bicarburet of hydrogen . .	0·3694	0·3572	0·9672
Water vapour	0·4750	0·2950	0·6210
Alcohol vapour	0·4513	0·7171	1·5890
Æther vapour	0·4810	1·2296	2·5563
Chloride of æthyle vapour . .	0·2737	0·6117	2·2350
Bromide of æthyle vapour . .	0·1816	0·6777	3·7316
Sulphuret of æthyle vapour .	0·4005	1·2568	3·1380
Cyanide of æthyle vapour . .	0·4255	0·8293	1·9021
Chloroform vapour	0·1568	0·8310	5·30
Dutch liquid vapour . . .	0·2293	0·7911	3·45
Acetic æther vapour . . .	0·4008	1·2184	3·0400
Acetone vapour	0·4125	0·8341	2·0220
Benzine vapour	0·3754	1·0114	2·6943
Turpentine vapour	0·5061	2·3776	4·6978
Chloride of phosphorus vapour	0·1346	0·6386	4·7445
Chloride of arsenic vapour .	0·1122	0·7013	6·2510
Chloride of silicium vapour .	0·1329	0·7788	5·86
Chloride of tin vapour . . .	0·0939	0·8639	9·2
Chloride of titanium vapour .	0·1263	0·8634	6·8360

These are all the volatile substances of which I was able to prepare considerable quantities in a state of purity.

The specific heat which I have obtained for the vapour of water in a great number of experiments is 0·475 ; this is barely one-half of that found by Delaroche and Bérard. It is remarkable that the specific heat of the vapour of water should be nearly equal to that of solid water, and only one-half that of liquid water.

It now remains for me to discuss the values which I have found for the specific heat of compound elastic fluids, in relation to the specific heat of the simple gases of which they are composed, and to the amount of condensation which the latter have experienced ; to compare the specific heat connected respectively with the solid, liquid, and gaseous states in several substances which are capable of assuming all these different states. I shall, however, reserve this discussion for a later communication, in which I shall give the latent heat of vaporization of the same substances.

LXXVI. *Mechanical Theory of Heat.—Velocity of Sound in Gases. By* W. J. MACQUORN RANKINE, *C.E., F.R.S.E. &c.*[*]

SINCE the date of my last communication on this subject, I have had an opportunity of reading M. Regnault's table of the specific heats of a great number of gases under constant pressure, published in the *Comptes Rendus* for the 18th of April.

The results contained in this table, taken in conjunction with other data obtained by the aid of the mechanical theory of heat, furnish the means of calculating *à priori* the velocity of sound in various gases.

The velocities thus calculated being compared with the velocities as determined directly by observation, afford a very satisfactory experimental test of the accuracy of Laplace's theory of the propagation of sound in gases, and of the fundamental principles of the mechanical theory of heat.

Let V represent the volume of unity of weight of a fluid under the pressure P, at a given fixed temperature (such as that of melting ice) ; $\dfrac{d \cdot P}{d \cdot \frac{1}{V}}$ the rate of variation of pressure with density for a sudden change of volume, and $\dfrac{dP}{d \cdot \frac{1}{V}}$ the rate of variation at constant temperature ; K_V the specific heat of the fluid at constant volume, and K_P at constant pressure ; u the velocity of sound in the fluid at the given temperature ; then it has been

[*] Communicated by the Author.

shown by Laplace and Poisson that the following formula is true for perfect gases :

$$u = \sqrt{\left(g \cdot \frac{d \cdot P}{d\frac{1}{V}} \right)} = \sqrt{\left(g \cdot \frac{dP}{d\frac{1}{V}} \cdot \frac{K_P}{K_V} \right)}. \quad . \quad . \quad (1)$$

And I have shown further (Trans. Roy. Soc. Edinb. vol. xx. p. 440), that the same formula is applicable, not only to perfect gases, but to all fluids whatsoever.

In making use of this formula to calculate theoretically the velocity of sound in gases, it is generally sufficiently correct for practice to make

$$\frac{dP}{d \cdot \frac{1}{V}} = PV = h,$$

which represents the height of an imaginary column of uniform density equal to that of the gas, whose weight or unity of base is equal to the pressure of the gas.

To determine the ratio of the specific heats

$$\frac{K_P}{K_V},$$

we have in the first place the mechanical equivalent of K_P, which is found by multiplying the specific heat of unity of weight of the gas under constant pressure as compared with liquid water, by the mechanical equivalent of the specific heat of liquid water.

In the second place, the *difference* between the mechanical equivalents of the specific heats at constant pressure and at constant volume (being the heat which is converted into expansive power while the gas is heated one degree under constant pressure), is to be calculated *à priori* by the following formula, deduced from the mechanical theory of heat,

$$K_P - K_V = \phi(\tau) \cdot \frac{\left(\frac{dP}{d\tau} \right)^2}{-\frac{dP}{dV}}; \quad . \quad . \quad . \quad (2)$$

in which τ is the absolute temperature, measured from the absolute zero of a perfect gas thermometer, $274°{\cdot}6$ below zero on the Centigrade scale.

According to the views of M. Clausius and Professor Thomson, $\phi(\tau)$ is an unknown function, to be determined by experiment; according to the supposition of M. Mayer, it is simply the absolute temperature itself; according to the hypothesis adopted in my own researches, and which I shall here continue to follow, it is of the form

$$\phi(\tau) = \tau - \kappa; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

κ being the absolute temperature on the perfect gas-thermometer

corresponding to total privation of heat, whose value is very small, and according to the latest determination, is about $2°\cdot1$ Centigrade.

Of the five gases which I have selected as having their properties sufficiently well-ascertained to enable them to be used for the purposes of the comparison in question, four, viz. atmospheric air, oxygen, hydrogen, and carbonic oxide, approach sufficiently near to the condition of a perfect gas to enable the following approximations to be employed,

$$\frac{d\mathrm{P}}{d\tau} = \frac{\mathrm{P}}{\tau - \lambda}\,; \quad -\frac{d\mathrm{P}}{d\mathrm{V}} = \frac{\mathrm{P}}{\mathrm{V}}\,;$$

and consequently

$$\mathrm{K_P} - \mathrm{K_V} = \frac{\tau - \kappa}{(\tau - \lambda)^2} \cdot \mathrm{PV}, \quad \ldots \ldots \quad (4)$$

where λ is a small constant, being about $1°\cdot6$ Centigrade; but for carbonic acid the deviation from the perfectly gaseous condition is sufficient to render it necessary to use a more complicated method of calculation, taking into account the cohesion of its particles.

The data and results of these computations are given in the annexed table; from which it appears, that, in the case of atmospheric air and carbonic oxide, the agreement between the calculated and observed velocities of sound is sensibly perfect; that for oxygen and hydrogen, the differences are so small as to be nearly inappreciable; and that for carbonic acid, the discrepancy of about one-fortieth part is not greater than might have been anticipated, from the number of additional causes of error which affect the experimental data for the less perfect gases.

The ratio of the two specific heats of air agrees to a minute fraction with that which had been previously deduced from the velocity of sound.

Glasgow, May 6, 1853.

Names of gases.	Experimental data.			Theoretical data.			Velocity of sound at 0° Centigrade.		Observers.
	h (Regnault).	$\dfrac{\mathrm{K_P}}{\mathrm{K_W}}$. (Regnault).	$\mathrm{K_P}$.	$\mathrm{K_P} - \mathrm{K_V}$.	$\mathrm{K_V}$.	$\dfrac{\mathrm{K_P}}{\mathrm{K_V}}$.	u by theory.	u by observation.	
	Metres.		Metres of fall.	Metres of fall.	Metres of fall.		Metres per second.	Metres per second.	
Air	7990	0·2379	100·76	29·27	71·49	1·4094	332·35	332·37 / 332·25	{ Bravais and Martins. / { Moll and Van Beek.
Oxygen	7226·8	0·2182	92·41	26·47	65·94	1·4014	315·19	317·7	Dulong.
Hydrogen ...	115462	3·4046	1442	423	1019	1·4150	1265·9	1269·5	Dulong.
Carbonic oxide	8259·25	0·2479	105·0	30·25	74·75	1·4047	337·34	337·40	Dulong.
Carbonic acid	5225·6	0·2164	91·65	19·56	72·09	1·2714	255·28	261·6	Dulong.

Notation.

h, volume of unity of weight of the gas, under a pressure of unity of weight on the square metre, at $0°$ Centigrade.

K_W, mechanical equivalent of the specific heat of unity of weight of liquid water $=1389·6$ feet, or $423·54$ metres of fall per Centigrade degree, as determined by Joule.

K_P, mechanical equivalent of the specific heat of unity of weight of the gas under constant pressure.

K_V, mechanical equivalent of the specific heat of unity of weight of the gas at constant volume.

g, accelerating force of gravity $=9·80896$ metres per second (at the observatory of Paris).

u, velocity of sound in the gas at $0°$ Centigrade

$$= \sqrt{gh \cdot \frac{K_P}{K_V}}.$$

LXXVII. *On Copper Smelting.* By JAMES NAPIER, *F.C.S.*[*]

[Concluded from p. 354.]

THE metal being brought to the state of *blister* copper, the next operation is that of refining—bringing it into a state fit for the market.   The refining-furnace is similar in form to a roasting-furnace, but a little smaller, and the bottom is made to incline a little to the front door, where a small well is made. The copper being ladled from the furnace, this well allows of the ladles being dipped into the metal when the quantity in the furnace is small, so that the last portions of the metal can be taken out.

About six tons of copper from the roasting-furnace are put into the refining furnace, the doors and air-holes of which are closed, and the heat raised until the metal is in fusion, when the air-holes are opened.   A short roasting is generally required, which is done in the manner we have described, and the scoria which collects is carefully skimmed off.   The separation of impurities is facilitated by occasionally stirring the metal with a rake.   Some refiners throw pieces of green wood upon the surface, under the impression that it assists the escape of sulphur. The roasting is continued until a ladleful of the metal taken out sets with contraction.   If the metal be very coarse, it will set with a surface having a frothy appearance; if finer, it sets with expansion, first round the edge, then swelling towards the centre, forming a little mound or cone, and occasionally boils over and

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throws up jets of metal, forming a miniature volcano. These phænomena are caused no doubt by the presence of gases in the copper. Several metals when in fusion absorb oxygen; but the fact that copper, when it sets in this way, does not refine well by the operation of poling, which would soon take away oxygen, is presumptive that the gas which escapes is not oxygen, and renders probable the prevailing idea that it is caused by the presence of sulphur or sulphurous acid.

When the setting of the metal in the ladle is favourable, the charge is ready for the operation of poling. A quantity of charcoal or anthracite coal is first thrown upon the metal to prevent oxidation by the air, and then the end of a large pole of green wood, generally of birch or oak, is inserted into the melted copper, and kept pressed down to the bottom of the metal, which occasions violent ebullition, the metal spurting and boiling with loud noise. The action which takes place during this operation will be apparent to every one slightly versed in chemistry—the reduction of any oxide or suboxide. But the reason why such an operation as poling is required, is owing to the peculiar nature of copper in relation to its oxides. If, for example, a quantity of melted lead be exposed to the air, it oxidates, but the oxide formed floats upon the surface of the metal, and may be reduced by putting wood or other carbonaceous matters on the surface; but when copper is so oxidated, the oxide formed is dissolved in the metal as completely as a salt is dissolved in water, which renders the copper brittle. To put pieces of wood or charcoal upon the surface would not remove the oxygen; hence the necessity of poling in order to bring the carbonaceous matters into contact with the dissolved oxide.

The copper before poling has a granular fracture and a red-brown colour; as the poling proceeds, these appearances change. The refiner takes from time to time small samples, called *assays*, which he hammers and breaks for examination. When it reaches the proper *pitch*, the assay bends without breaking; a small cut, however, is made with a chisel on the centre of the assay, so that in bending it opens up and shows the fracture, which when tough, or in pitch, should have a fibrous silky lustre and light colour. When this pitch is attained, the pole is withdrawn and a larger sample taken out, termed a *trial*, and cast in a small mould. It is then beat by sledge hammers into a plate; if the copper be good, this plate should not crack on the edge, and the surface will be smooth and not scaly; if it crack, the copper is not in a good condition, and it would not be safe to take out if it is intended to be used for hammered work. But frequently copper that will not hammer will stand rolling into sheets without much loss. Previous to taking out the trials a quantity of

lead is generally added, from 10 lbs. to 30 lbs., to the charge; this softens the copper and renders it more tough.   Sometimes, when the trial cracks upon the edge, a little additional lead will cure it.

The addition of lead is supposed to effect the complete deoxidizement of any remaining oxide of copper, and it is imagined that all the lead comes off as scoria, or is volatilized.   In our analyses of copper, and they have been very numerous, the presence of lead was always detected wherever it had been added in the refining operation, and often in quantity equivalent to what had been added.   Smelters finding in their practice that copper made from the ordinary quality of ores will not roll without lead being added, are of opinion that its presence is essential for toughening the copper; but it is only so when impurities are present.   We have often seen copper refined without lead; and when the metal was pure and free from antimony, was superior to those containing lead.   Copper made from a mixture of Cornish ores purified from antimony, tin, &c. in the early stages of smelting, and refined without lead, has given the following results:—A cake of 14 inches by 10 inches has been rolled at one heat to 17 feet in length, with a wire edge; and a cake from the same copper was hammered into a bowl 2 feet deep and 19 inches diameter without cracking.   The necessity of adding lead we consider to be owing to the presence of antimony in the copper, the smallest trace of which without lead makes it brittle; while we have seen copper with half a per cent. made to roll by the addition of lead.

When the copper is brought to the proper pitch by the refining operation, it is ladled out into moulds.   A ladle holds about 30 lbs. of fluid metal, three of which form a cake, the ordinary size of which is 14 × 10 inches.   Twenty moulds are set round the floor in front of the furnace, three ladlefuls are poured together into each mould, going over the whole, by which time the cake first poured is set, and the mould is ready to receive another charge.   Each mould holds five such charges or cakes. During the lading out the refiner takes an assay at short intervals, as the metal is liable to get out of pitch, or become *dry*, as underpoled copper is termed, in which case poling has to be resumed.   So much depends upon refining, that the best copper by a defect in this operation will be rendered unmarketable.

It occasionally happens that poling is carried on too long; in which case the metal assumes a light colour and a crystalline structure, loses its toughness and becomes hard; when this occurs, the charcoal is removed from the surface of the metal to expose it to the air for some time and allow it to imbibe oxygen, after which it is again poled as described.   Both overpoled and

underpoled copper crack in rolling; the former cracks along the surface as well as at the edges, the latter only on the edges, the surface remaining smooth.

In making what is termed best select copper, the refining is performed in the manner described, but no lead is added.   This quality of copper is used for the manufacture of fine alloys, such as Muntz's yellow metal; copper a little overpoled is generally preferred for this purpose.   Overpoled metal expands in setting, giving the ingots a roundness on the top.   Select metal is cast into ingots about 8 inches long and 3 thick.   The moulds are fitted upon a frame placed over a pit or tank of water, so that whenever the metal becomes solid, the mould which moves upon a pivot is turned over and the copper ingot drops into the water, which takes off all oxide from the surface.   Each of these ingots weighs about 15 lbs.   The ingot-moulds are sometimes made of iron, but these soon give way, crack upon the surface, and disfigure the ingot; they also retain the heat so much as to give the copper moulded in them a spongy appearance.   Moulds made of copper suit best, and are most generally used.

We have said that the operation of poling is to effect the reduction of the oxide and suboxide of copper dissolved in the metal, but what constitutes overpoling is not so easily defined. Mr. Vivian, in his paper on copper smelting, published in 1822, gives it as his opinion that carbon is taken up by the copper, forming a carburet, which remains dissolved in the metal as the oxides do.   In our inquiry we have never been able to detect the slightest trace of carbon in overpoled copper, even in samples where that operation had been purposely continued, until the metal was, in the language of the workmen, rotten, being so brittle.   The following are a few observations upon this interesting subject:—

1st. In refining, the purer the copper is, the more difficult it is to keep in the proper pitch.

2nd. Overpoling causes the copper to expand when setting.

3rd. Overpoled copper effervesces, which is no doubt produced by the escape of gas, and which causes a shower of minute globules of solid metal coated with suboxide.

These favour the idea that, whatever be the substance present, it exists in the copper in a gaseous state, and is not permanently combined with the metal.   The following are the appearances which copper in different states of poling have under the microscope, with their specific gravities:—

1st. Copper ready for poling.   Red-brown colour, vitrified, sandy fracture.   Spec. grav. 8·491.

2nd. After a little poling.   Colour redder than last, sandy fracture, not so vitreous-looking.   Spec. grav. 8·526.

3rd. Further poling. Colour lighter, with more lustre, fracture hard, not sandy, but vitreous. Spec. grav. 8·481.

4th. Longer poling. Colour brighter, with more of the metallic lustre, less vitreous than former, and having minute cavities. Spec. grav. 8·397.

5th. Near to tough pitch. Colour still brighter, fracture full of minute cavities, as if the metal had set during escape of gaseous matters, slightly vitreous. Spec. grav. 8·582.

6th. Tough pitch. The appearance of this was singular; the cavities in the last specimen seemed now collapsed, giving a fracture resembling a honey-comb, strongly compressed, the colour of silvery brightness and a soft metallic appearance. Spec. grav. 8·654.

7th. A little overpoled. The collapsed cells extended something like No. 5; some of the cavities oval, as if a gas had escaped; fracture, a reedy hard appearance. Spec. grav. 8·594.

8th. Further overpoled. Fracture crystalline and vitreous, full of cavities. Spec. grav. 8·518.

9th. Much overpoled. Fracture like a crystalline network, very hard and vitreous. Spec. grav. 8·298.

These appearances seem to indicate the changes going on within the metal during poling, and the specific gravity of each corresponds with these appearances. In Nos. 4 and 8, when the cavities are very numerous, the specific gravities are lowest.

From these experiments and observations, we think that the first reaction in poling is the reduction of the oxide of copper, and the complete reduction of the oxide constitutes the *tough pitch*. Overpoling we think to be the imbibition of carbonic oxide or carbonic acid, and not the formation of a carburet of copper. That metals imbibe gas when in a melted state is well known; some of them, as silver, give it out when setting. That overpoling is due to gaseous matter contained in the metal, we think is evident by its effervescence, and by its becoming tough if kept melted until this effervescence ceases.

It is worthy of remark, that green wood is much more effective in poling than dry wood or charcoal; probably the hydrogen in the moisture and juices is more effective in reducing the oxides.

When copper is pure and of good quality, the ingot when struck by a hammer or thrown upon another ingot gives a dead solid sound, not sonorous; while copper with the slightest quantity of alloy when so treated gives a sharp clink. By this character the founder often judges of the quality of the metal. We have seen copper received by a purchaser with great suspicion because of its sound, which gave by analysis—

Copper	.	.	.	.	99·80
Iron	.	.	.	.	·05
Antimony	.	.	.		·04
					99·89

While copper of the following composition, but without sound, was received with every confidence :—

Copper	.	.	.	.	98·90
Iron	.	.	.	.	·07
Oxygen and loss		.			1·03
					100·00

That the poling has no influence on this phænomenon has been proved by trials made at all stages of that operation.

Pure copper dissolves much more freely in strong nitric acid than impure copper, and gives an immediate clear dark blue solution. Most alloys impart to the solution a greenish tint; a small portion of silver, quite an olive-green; indeed different alloys furnish characteristic tints. These remarks, however, only refer to undiluted solutions; dilution takes away the peculiarity of hue; and if antimony or bismuth be present, the dilute solution is slightly turbid, requiring several hours to clear.

The purifying of copper from alloy during the operation of refining has formed the subject of many experiments. In general some substance has been added to the copper in fusion to dispel or scorify the impure metal, such as would combine with the alloy and form with it a volatile compound, as chlorides to form a chloride of antimony; or ground glass to form a silicate of antimony, which would be removed as slag; but none of these experiments gave any hopes of successfully and profitably removing the alloys. One great difficulty to overcome in these experiments is, that the matters added float on the melted metal, and consequently can only affect the surface. The oxide of one metal may scorify or oxidize another metal when present in copper, but this would only be a change of alloy, which in some cases may be done to advantage; thus the addition of a little oxide of lead will greatly aid in purifying copper from antimony and bismuth.

That silica has an attraction for antimony, and will extract it from copper, we have had sufficient evidence in its being accumulated in the sand-bottoms of the refining furnace. A refining-furnace used for copper containing small quantities of antimony will soon become so deteriorated that metal of good quality cannot be made in it. A refining-furnace bottom which had been used for several months for ordinary cake copper, not containing

at any time more than 0·13 per cent. of antimony, gave by analysis, about two inches from the surface,—

Copper	.	.	.	.	50·30
Silica	.	.	.	.	42·43
Sulphur	.	.	.	.	2·11
Iron	.	.	.	.	1·68
Tin	.	.	.	.	·16
Antimony		.	.	.	1·92
					98·60

The relation of the antimony to the copper in this analysis is 3·8 per cent., showing the accumulation of that metal, caused no doubt by the strong attraction which silica has for the oxide of antimony; and suggests the probability of some of the antimony existing in the copper, not as an alloy, but in the form of a compound, probably antimoniate of copper, which the silica of the bottom decomposes. All the impurities found deteriorating copper exist in the ore; and we repeat what has been previously stated, that if these metals be not removed before the copper reaches the state of metal, their entire separation is as yet commercially impracticable.

The effect of minute quantities of metal in copper has not yet been fully investigated, and a considerable difference of opinion exists on this question. A few years ago a quantity of copper was returned to the makers as unfit for the purposes for which it was sold, being brittle. This on being analysed gave a trace of nickel, which was considered the deteriorating impurity. Experiments made by Bunsen on this subject showed that small quantities of nickel do not affect the rolling property of copper.

Arsenic in copper is considered very deleterious both by smelters and copper-workers. Dr. Percy has shown that the presence of that metal is not hurtful.

Silver has been found in copper complained of, and consequently considered to be the deteriorating agent. We have found copper highly prized by the manufacturers for its quality, containing from 20 to 25 oz. of silver per ton of copper. We have no doubt but all these statements are true; an alloy in one proportion may deteriorate, and in another improve certain working qualities of copper. A minute quantity of lead does not seem to hurt copper, yet it requires but a small proportion of that metal to make copper brittle. Iron makes copper hard, yet we have seen it with 0·3 per cent. of that metal highly prized both for rolling and hammering. These cases are cited to show how much has yet to be done in this inquiry.

There is another circumstance connected with the working of copper closely related to the presence of minute quantities of

alloy, that is, the *pickling*. Copper is rolled when at a red heat, and in this operation it becomes covered with a black scale of oxide, which adheres firmly to the surface. This is removed by moistening the surface with urine, then making the sheet red-hot and plunging it immediately into water, when the oxide scales off; this operation is termed pickling. Some qualities of copper part with this scale with great difficulty; large portions adhere, and can only be imperfectly removed by rubbing, which affects the value of the sheet of metal. We have seen copper of the purest quality pickle so bad as to affect its sale. It is very probable that a little alloy, if not altogether essential, greatly assists this operation. Lead and tin prevent; arsenic, antimony, and silver assist pickling, circumstances which suggest the probability of the cause being electrical; as those metals which are positive to copper prevent pickling, and those that are negative to it in the electrical scale assist.

We have in these papers given a brief detail of the processes at present in use for manufacturing copper from its ores, and traced, so far as our experience enabled us, the various chemical changes which take place. A great variety of improvements have been proposed and patented for copper smelting. A few of these are worth a trial, and bid fair to be of use; but the greater portion of them exhibits a want of practical knowledge of the processes adopted, and consequently the requirements of the trade. The merits or otherwise of these processes it is not our present intention to review; but we hope that these papers will supply a desideratum, and enable those who are not in the trade to appreciate the difficulties experienced by those who are engaged in it.

LXXVIII. *On a Method of Transforming Equations by means of Symmetric Functions.* By G. B. Jerrard.

[Continued from p. 363.]

8. IN the solution of *Problem* I., when $\lambda = 3$, it is implied that the six equations

$$\dot{q}_1 = \dot{\mathfrak{D}}_1,$$
$$\dot{q}_2 = \dot{\mathfrak{D}}_2,$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot$$
$$\dot{q}_6 = \dot{\mathfrak{D}}_6$$

will obtain at once in virtue of the condition

$$\dot{q} = \dot{\mathfrak{D}};$$

so that the first members of the equations

$$\dot{q}^6 + \frac{C_5}{C_6}\dot{q}^5 + \frac{C_4}{C_6}\dot{q}^4 + \ldots + \frac{C_0}{C_6} = 0,$$

$$\dot{\mathfrak{D}}^6 - \frac{2B_1}{\alpha}\dot{\mathfrak{D}}^5 + \frac{2^2 B_2}{\alpha^2}\dot{\mathfrak{D}}^4 - \ldots + \frac{2^6 B_6}{\alpha^6} = 0$$

will be identical.   But the question respecting the possibility of these two equations having a common factor of a lower degree than the sixth ought here to be considered.

We may, it is true, very readily perceive that there cannot generally exist a common factor of either the first or the second degree with rational coefficients, since no cubic radical could under such circumstances be involved in the solution.   And when $m=4$ (for every other case may be excluded as being already solved by the first method in which $\lambda=4$)*, it is manifest that $\dot{q}$ and $\overset{..}{\mathfrak{H}}$, which are each of them expressible as rational functions of the roots of the original equation in x, will necessarily lead from the equality of the typical forms to identical equations of the sixth degree; unless, indeed, there can exist, for equations of the fourth degree as well as for those of higher degrees, a function analogous to $\mathbf{D}(V_{F(\mathbf{a}v)}, V_{G(\mathbf{a}v)}, V_{H(\mathbf{a}v)})$†.

We see, then, what strong grounds there are for inferring that the equations

$$\dot{q}^6 + \frac{C_5}{C_6}\dot{q}^5 + \ldots = 0,$$

$$\overset{..}{\mathfrak{H}}{}^6 - \frac{2B_1}{\alpha}\overset{..}{\mathfrak{H}}{}^5 + \ldots = 0$$

will be identical when $m=4$.

But the subject is very instructive, and I hope to return to it at some future time.

Long Stratton, Norfolk,
    May 14, 1853.

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* By the mode of solution in which $\lambda=4$, we can, while $m > 4$, always effect the proposed transformation by means of an equation of a lower degree in x than the given one; but when $m=4$, the equations

$$x^4 + A_3 x + A_4 = 0,$$
$$Tx^4 + Sx^3 + Rx^2 + Qx + P - y = 0$$

rise to the same power of x, and one of them becomes a multiple of the other, as I have shown in my Mathematical Researches.   In order, therefore, to include this case, λ was taken equal to 3.

The method in which $\lambda=3$ may, however, be made to extend to high values of m.   But the equation of the sixth degree in $\dot{q}$ and that in $\overset{..}{\mathfrak{H}}$ will no longer be identical, except for particular values of $A_3, A_4, . . A_m$.   This is obvious.

† See my " Notes on the Resolution of Equations of the Fifth Degree " in the Philosophical Magazine for February 1852.

LXXIX. *On Rubian and its Products of Decomposition.*
By Edward Schunck, *F.R.S.*

[Concluded from p. 436.]

THE experiments above detailed prove that the decomposition which rubian undergoes by fermentation, does not differ in its general nature from that which results from the action of acids or alkalies. The decomposing effect of the ferment extends like that of these agents to three portions of rubian. The first portion of rubian loses water and gives rise to the formation of alizarine. The second loses water and produces rubiretine and verantine in equivalent proportions. The third takes up water and gives sugar and rubiafine, or it takes up 1 equiv. more of water, and yields sugar and rubiagine. The rubianine produced by acids, and the rubiadine formed by alkalies, give place, when ferment is the decomposing agent, to rubiafine and rubiagine. The rubiadipine, in consequence of the uncertainty of its nature and origin, and the minute quantity in which it is formed, I leave out of consideration. The relative proportion in which the various products of decomposition are formed, seems to be about the same in the case of ferment as when acids or alkalies are employed. Alizarine is formed in the smallest quantity; of rubiretine and verantine a little more is produced, but the sugar and the rubiafine and rubiagine exceed all the others in amount.

It appears further from these experiments, that the ordinary process of dyeing with madder is, in fact, mainly a process of fermentation, that it consists essentially of a re-arrangement of the elements of rubian induced by the action of ferment, whereby a formation of colouring matter takes place, and that the precautions necessary to be taken in regard to the regulation of the temperature in madder dyeing have reference principally to the progress of the fermentation. The extraordinary rapidity with which this process of fermentation is completed, is the only circumstance which distinguishes it from other processes of a similar nature.

A very interesting question now arises, suggested by the analogy which must be presumed to exist between this and other processes of fermentation, viz. By what means is the action of the ferment on rubian either destroyed, retarded or promoted; and do not any means exist of so modifying the action as to lead to the formation of particular substances in preference to others? With the view of throwing some light on this question, I instituted a series of experiments, which I shall now proceed to describe.

Having taken a solution of rubian and mixed it with a quantity of ferment sufficiently large to effect its decomposition under

ordinary circumstances, I raised the temperature of the mixture
to the boiling-point and kept it boiling for a short time.   After
the mixture had stood for some days exposed to a moderate tem-
perature, I found that the rubian had not undergone the least
change.   The liquid remained yellow and bitter, and after fil-
tration and evaporation left a residue of rubian with its usual
appearance and properties.

I took in the next place a quantity of ferment, dried it slowly
at a moderate temperature, reduced it to a fine powder and mixed
it with a solution of rubian.   After the mixture had stood for
some days, the rubian was entirely decomposed.   The filtered
liquid left on evaporation a quantity of sugar, and the residual
mass on the filter, which was interspersed with numerous small
yellow crystals, was treated with boiling alcohol, which, after
being filtered boiling hot and allowed to cool, deposited a large
quantity of rubiafine in crystals.   The alcoholic liquid, on being
examined in the usual manner, was found to contain besides
rubiafine, alizarine, rubiagine, rubiretine and verantine, the two
latter being rather less in amount than usual.

After drying the ferment at a moderate temperature, then
heating it for some hours in a water-bath, before adding it to a
solution of rubian, no apparent change took place for some time,
but gradually the rubian began to disappear, and after two
months was entirely decomposed.   The filtered liquid left on
evaporation a quantity of sugar, and the residue on the filter,
on being treated with alcohol, yielded a solution which contained
rubiafine, and a pretty large quantity of rubiretine and veran-
tine, but no alizarine.

To a solution of rubian I now added a sufficient quantity of
ferment to effect its decomposition when used alone, and then a
small quantity of sulphuric acid.   The mixture remained appa-
rently unchanged for some time.   The liquid was filtered, the
acid was neutralized with carbonate of lead, the liquid was then
again filtered, and left on evaporation a yellowish-red, bitter,
glutinous substance, which was apparently a mixture of rubian
and sugar.   The mass left on the filter was treated with boiling
alcohol, which was then found to contain a trace of alizarine, a
little rubiagine, and a large quantity of rubiretine and verantine.

The same experiment was performed, substituting for the sul-
phuric acid a minute quantity of carbonate of soda.   This had
the effect of completely dissolving the ferment, forming a solution
to which the rubian imparted a red colour.   After standing for
some time, the rubian was entirely decomposed, for on neutral-
izing the soda with sulphuric acid, a yellow precipitate fell, while
the liquid became colourless.   The latter being filtered, the acid
was neutralized with carbonate of lead, and the liquid being

again filtered left on evaporation a quantity of sugar mixed with sulphate of soda. The mixture of ferment with the products of decomposition yielded to boiling alcohol a large amount of rubiafine, rather more than the average quantity of alizarine, and a moderate quantity of rubiretine and verantine, but no rubiagine. I repeated this experiment, using caustic soda instead of carbonate of soda; but having employed rather too large a quantity of alkali, I found that the action of the ferment was very much retarded, so much so that after standing for some days there was still a quantity of undecomposed rubian present; and among the products of decomposition formed there was little or no alizarine, rubiafine or rubiagine, but a large quantity of rubiretine and verantine. The addition of lime water produced a similar effect to that of carbonate of soda, though of not quite so marked a character.

Having mixed a solution of rubian with ferment in the usual manner, I added a small quantity of sugar of lead. The ferment, which had been previously diffused through the liquid, was immediately precipitated by the lead salt in brown flocks, leaving a clear yellow solution floating above it. After standing however for some days the colour of the solution became gradually paler, and at length almost disappeared. After filtration and evaporation it left a small quantity of sugar. The mass left on the filter was purple, and contained oxide of lead. It was treated with cold sulphuric acid, which turned it red. The acid liquid after filtration was neutralized with carbonate of lead, and again filtered, when it appeared colourless, and produced no precipitate with basic acetate of lead, a proof that it contained no rubian. The mass which had been treated with cold sulphuric acid, was then treated with boiling alcohol, which on being filtered and allowed to cool deposited some crystallized rubiafine, and was found to contain a trace of alizarine and a large quantity of rubiretine and verantine. It appears therefore that even sugar of lead, which produces an insoluble compound with the ferment, is incapable of arresting the action of the latter on rubian.

The same experiment was made, using corrosive sublimate instead of sugar of lead. The action of the ferment was in this case considerably retarded, for the yellow colour of the liquid did not entirely disappear, even after several days. The filtered liquid left on evaporation some corrosive sublimate with a little rubian, but no sugar. The mass on the filter was treated with boiling alcohol, which was then found to contain no alizarine, rubiafine or rubiagine, but a considerable quantity of rubiretine and verantine.

Arsenious acid had a very similar effect to sugar of lead and corrosive sublimate, that is to say, it retarded the action of the

ferment, prevented the formation of alizarine, and promoted that of rubiretine and verantine.

The addition of a large quantity of alcohol to a mixture of rubian and ferment had likewise the effect of retarding, though not of altogether preventing the fermentation. A great part of the rubian remained undecomposed, while the remainder had given rise to the formation of a small quantity of alizarine and a large quantity of rubiretine and verantine.

A solution of rubian having been mixed with ferment, I poured the mixture into a bottle, and then added a quantity of oil of turpentine, after which the whole was well shaken up and left to stand. The decomposition of the rubian was much retarded, and took several days for its completion. The oil of turpentine floating on the surface was removed. The liquid underneath was colourless. After filtration and evaporation it left a quantity of sugar. The remaining products of decomposition were rubiafine, a trace of alizarine, and a large quantity of rubiretine and verantine, but no rubiagine.

From these experiments, which seem to me sufficiently decisive to require no further confirmation, I draw the following inferences:—

1. There exist no means, short of the complete destruction of the ferment, capable of arresting its action on rubian, except exposing it while in a moist state to the temperature of boiling water. Even when exposed to that temperature, after having been previously dried, its fermenting power is not entirely lost, but merely weakened.

2. By the addition of various substances, usually classed as antiseptic, such as sulphuric acid, arsenious acid, sugar of lead, corrosive sublimate, alcohol and oil of turpentine, during the process of fermentation the action of the ferment is not destroyed; it is merely retarded and modified.

3. The more the action of the ferment on rubian is retarded, the more rubiretine and verantine, and the less alizarine are formed, so much so that in some cases the alizarine disappears entirely from among the products of decomposition, which then consist almost solely of rubiretine and verantine. The formation of rubiafine and rubiagine is promoted when the action of the ferment is moderately retarded, but diminishes again, or entirely ceases when the retardation is very great. Of the two, the rubiagine is the first to disappear when any retardation takes place.

4. By the addition of small quantities of alkalies during the process of fermentation, the action is, as regards its duration, if not promoted, at all events not retarded; and as regards the relative quantities of the various substances produced, the amount of alizarine is thereby decidedly increased, while that of the rubiretine and verantine is diminished.

These experiments also confirm the view derived from analytical results, that there is a mutual relation and interdependence on the one hand between rubiretine and verantine, and on the other between rubiafine and rubiagine and the sugar, whereas alizarine occupies an independent position; for rubiretine is never found among the products of decomposition by fermentation without an accompaniment of verantine, though both may be present to the exclusion of all other products; and the formation of sugar always indicates that of rubiafine or rubiagine, whereas all these substances may be present without a trace of alizarine being at the same time produced.

The deleterious effect resulting from the presence of acids in the dye-bath during the process of madder dyeing hereby finds an additional explanation. Not only do acids act injuriously by seizing hold of the mordants with which the colouring matter ought to combine, not only do they allow the constituents of the root which are injurious in the process to have full scope by depriving them of the alkalies or alkaline earths with which they would otherwise unite, but they also retard the peculiar process by which the alizarine is formed, and even lead to the formation of deleterious substances at the expense of the colouring matter itself.

In order to place in a more striking light the influence exerted respectively by acids and alkalies during the process of the fermentation of rubian, I will here give the results of an experiment to ascertain quantitatively the amount of the various products of decomposition formed on the addition of either during the process. For this purpose I took 5·59 grms. rubian, dissolved it in water, added to the solution a quantity of ferment, and mixed both together very well. I then divided the mixture into two equal parts, and added to one half a small quantity of carbonate of soda, to the other a little sulphuric acid. Both were allowed to ferment for a length of time, and the portion insoluble in water was treated in both cases with boiling alcohol. The alcohol deposited on being filtered boiling hot a quantity of rubiafine, which was collected on a filter and weighed. To the alcoholic liquid I added acetate of alumina. The precipitate thereby occasioned was separated by filtration, washed with alcohol and decomposed with boiling muriatic acid. The red flocks thus obtained were collected on a filter, washed, dried and weighed. They consisted of alizarine, verantine and rubiafine. After being weighed, they were treated with boiling dilute nitric acid, which destroyed the alizarine, leaving the two other substances behind in the shape of a yellow powder, which was filtered, washed, dried and weighed. By deducting its weight from the total weight of the three, the weight of the alizarine was ascertained. The liquid filtered from the alumina precipitate deposited on the

addition of sulphuric acid and water a yellow powder, which was collected on a filter, washed, dried and weighed. It was then treated with a little cold alcohol. This left undissolved a quantity of rubiagine, which was again collected on a filter, dried and weighed. The alcohol left on evaporation a dark brown mass, the fatty nature of which showed that it contained rubiadipine in addition to rubiretine. Now I obtained from that half of the solution to which carbonate of soda had been added, the following quantities of these various substances :—0·323 grm. alizarine, 0·220 grm. rubiafine, 0·167 grm. of the mixture of rubiafine and verantine, 0·250 grm. rubiagine, and 0·170 grm. of the mixture of rubiretine and rubiadipine. From the other half, to which sulphuric acid had been added, I obtained the following quantities of the same bodies :—0·2030 grm. alizarine, 0·340 grm. rubiafine, 0·157 grm. of the mixture of rubiafine and verantine, 0·310 grm. rubiagine, and 0·230 grm. of the mixture of rubiretine and rubiadipine. In both cases a quantity of rubian remained undecomposed, but the quantity actually decomposed was capable of being approximately determined from the quantities of products of decomposition formed. Assuming the formula of rubiafine to be $C^{32} H^{13} O^9$, that of rubiagine $C^{32} H^{14} O^{10}$, assuming further that the mixture of rubiafine and verantine consisted entirely of the latter substance, by which no great error is committed (since the quantities of rubiafine and verantine which one atom of rubian is capable of yielding are to one another as 277 to 258), and leaving out of consideration the trifling quantity of rubiadipine formed (the rubiretine being given by the amount of verantine), then the quantities of the various substances obtained correspond respectively to the following quantities of rubian :—

	Rubian.		Rubian.
Alizarine .	0·323 corresp. to 0·407	0·203 corresp. to 0·255	
Rubiafine .	0·220 corresp. to 0·484	0·340 corresp. to 0·748	
Rubiagine	0·250 corresp. to 0·533	0·310 corresp. to 0·661	
Verantine .	0·167 corresp. to 0·394	0·157 corresp. to 0·371	
	1·818		2·135

Hence it appears that though the amount of rubian which actually underwent decomposition was less when carbonate of soda was added than when sulphuric acid was employed in addition to the ferment, still the amount of the alizarine produced was greater in the former than the latter case, while the amount of almost all the other substances was proportionally less. In using carbonate of soda, the quantity of alizarine formed amounted to 17·7 per cent., in using sulphuric acid it amounted to 9·5 per cent. of the rubian decomposed. The quantity which should be formed, if the rubian were entirely converted into alizarine, is 79·3 per cent.

In the first part of this paper I have pointed out the possibility which exists in theory of converting rubian entirely into alizarine, to the exclusion of every other product of decomposition; and though I cannot say that I have approached much nearer to its practical realization, still if, as the experiments just described prove, the fermentative process whereby alizarine is formed is of so plastic a nature as to be capable of yielding under different circumstances, sometimes more, sometimes less, occasionally none, of the latter substance, then the prospect of our being able so to modify it as to lead to the formation of a much larger quantity of alizarine from a given quantity of rubian than is at present possible, becomes less distant.

The experiments which I shall now describe were made in order to ascertain the effect produced on rubian by other fermentative substances besides that contained in madder itself.

I first took a solution of rubian and mixed it well with a solution of albumen made from white of egg. The mixture was allowed to stand in a moderately warm place for two months, but though the albumen had entered into a state of putrefaction, the rubian was still for the most part undecomposed, as was evident from the taste and colour of the liquid. At the bottom of the vessel there was found a small quantity of a dark reddish-brown deposit, which was collected on a filter. On treating this deposit with boiling alcohol, the latter acquired a red colour, and left on evaporation a small quantity of a yellow bitter substance, soluble in water, which only differed from rubian in being somewhat viscid. The remainder of the dark brown deposit I treated with a mixture of muriatic acid and alcohol, when it immediately became almost white, while the substance which had coloured it dissolved in the alcohol with a yellow colour. The alcohol was filtered from the insoluble part, which was probably merely modified albumen, and evaporated to dryness, when it left a reddish-yellow, uncrystalline residue. This residue was impure alizarine, for it gave on being heated in a tube a crystalline sublimate, it dissolved in caustic alkali with a violet colour, and its alcoholic solution became purple on the addition of acetate of copper. In the reddish-brown deposit it was doubtless contained in combination with some base, perhaps lime. The greatest part of the rubian remained in solution mixed with the albumen. The solution was slowly evaporated to dryness, when it left a red transparent substance like gum or varnish, which was treated with boiling alcohol. The alcohol acquired a reddish-yellow colour, and left on evaporation a yellowish-red, deliquescent, bitter residue, consisting apparently of rubian mixed with some nitrogenous substance. It was entirely soluble in water with a reddish colour. The solution gave with acetate of lead a

copious brown precipitate, the supernatant liquid being almost colourless, but with acids and alkalies it gave the usual reactions of a solution of rubian. The greater part of the gum-like substance was of course insoluble in alcohol. On being treated with a mixture of sulphuric acid and alcohol in the cold, an additional quantity of yellow liquid was obtained, which after filtration, neutralization with carbonate of lead and evaporation, left a reddish, deliquescent residue similar to the last. It appears, therefore, that the action of albumen on rubian is but slight.

In order to ascertain what effect caseine has on rubian, I mixed a solution of the latter with a quantity of milk, and allowed the mixture to stand in a warm place for several days, until the smell indicated that the caseine had entered into a state of putrefaction. The liquid, which was still yellow and bitter, was filtered from the caseine, which had separated in yellow flocculent masses, and basic acetate of lead was added, which produced a red precipitate. This precipitate, after filtering and washing, was decomposed with cold sulphuric acid; the excess of sulphuric acid was removed with carbonate of lead; and after passing sulphuretted hydrogen through the filtered liquid to precipitate a little lead contained in the solution, it was again filtered and evaporated, when it left a quantity of rubian possessing the usual properties of that substance, with the exception of its being somewhat viscid. The caseine on the filter after being washed with water was treated with boiling alcohol, which deprived it of its yellow colour, and rendered it white. The filtered liquid, which was yellow, gave no precipitate either with acetate of alumina or sugar of lead, proving the absence of alizarine and the usual products of decomposition of rubian. On evaporation it left a residue of rubian mixed with fat, the whole, with the exception of the fat, being soluble in water.

A similar negative result was obtained with gelatine and yeast. A solution of rubian being mixed with a solution of glue, the mixture after standing for several weeks began to emit a putrid smell, and acquired a reddish tinge, but remained bitter, and deposited nothing insoluble. On adding yeast to a solution of rubian, and allowing to stand until putrefaction commenced, no apparent change took place in the solution, which retained its yellow colour and bitter taste.

The action of emulsine on rubian was very different. The emulsine employed was prepared in the usual manner, by treating sweet almonds, after being pounded and pressed between paper to deprive them of their oil, with water, separating the milky liquid from the residue by straining, allowing it to stand until he fat had collected on the surface, and then precipitating the emulsine from the clear liquid underneath, by means of alcohol.

Some emulsine prepared in this manner was placed in a solution of rubian, and well mixed up with it. The mixture was allowed to stand in a warm place for some days. By degrees the liquid became almost colourless, while the emulsine acquired a yellow colour. It was filtered and evaporated, when it left a quantity of emulsine. The yellow mass on the filter was treated with boiling alcohol, which became yellow, while the residue lost almost the whole of its colour. The alcoholic liquid was filtered and spontaneously evaporated, when it left a yellowish-red mass, among which crystals of alizarine were discernible. This mass was treated with cold water, in which a part dissolved with a yellow colour. The liquid after filtration and evaporation left a quantity of unchanged rubian. The part insoluble in water was dissolved again in alcohol, and to the solution was added acetate of alumina, which produced a red precipitate. This precipitate, after filtration and washing with alcohol, was decomposed with muriatic acid, and the yellow flocks left by the acid were dissolved in alcohol, on the spontaneous evaporation of which there was left a mass of pure alizarine in well-defined yellow crystals. The liquid filtered from the alumina precipitate was evaporated to dryness, and the residue left was treated with water and muriatic acid, when a brown mass was obtained, which after filtering and washing was treated with cold alcohol. The alcohol left undissolved a brown powder, consisting probably of verantine, and after filtration and evaporation left a brown resinous substance, easily fusible when thrown into boiling water, and consisting doubtless of rubiretine. The quantity of alizarine formed seemed to be much larger in proportion to the rubian employed, and to the quantity of other substances formed, than in the case of the ferment of madder. Nevertheless, I must state, that on repeating the experiment I was unable to attain the same result, the alizarine being formed in much smaller proportion and giving place to other products of decomposition.

Lastly, I resolved to examine what effect, if any, would be produced on rubian by some fermentative substance derived like that of madder from a vegetable root. For this purpose I chose the albuminous substance described by Braconnot as existing in the root of the *Helianthus tuberosus**. I selected this substance, because it has, like the ferment of madder, the property of causing a solution of sugar to undergo an acetous fermentation. It was prepared in the following manner. The tubers were cut into slices, then pounded in a mortar with a little water until the lumps had disappeared. The mass was then placed on a piece of calico, and the liquor expressed with the hands. The muddy liquid which ran through was then mixed with a con-

* *Ann. de Chem. et de Phys.* xxv. 358.

siderable quantity of alcohol.   This produced a gray precipitate, which was collected on a filter and well washed with cold alcohol. Some of this precipitate was then added to a solution of rubian, and the mixture was allowed to stand in a moderately warm place until it began to disengage a putrid smell.   The liquid was then filtered and evaporated to dryness.   The residue was treated with boiling alcohol, which dissolved a part with a yellow colour, leaving undissolved a quantity of brown flocks, consisting probably of some of the ferment.   The alcohol on evaporation left a residue, consisting of rubian with its usual appearance and properties, and apparently free from sugar.   The substance left on the filter had acquired a brownish-yellow colour, which was not removable by washing with water.   On being treated with boiling alcohol, it lost its yellow colour, which was now transferred to the alcohol.   The alcohol after filtration and evaporation left a dark brown substance in the shape of a pellicle, which yielded nothing to boiling water.   On being treated with cold alcohol, a brown powder resembling verantine was left undissolved, while the filtered liquid gave on evaporation a residue of a resinous nature, which melted when thrown into boiling water and became brittle again when cold, and consisted doubtless of rubiretine.   The effect produced by this substance on rubian, therefore, though it does not equal in energy that of the madder ferment itself, or even of emulsine, exceeds that of albumen or caseine.   As regards the substances produced by it, its action resembles that of madder ferment when retarded by the addition of antiseptic substances, and it confirms the law which I have deduced from previous experiments, viz. that the more slowly rubian is decomposed, the more rubiretine and verantine are produced.

It appears from these experiments, that none of the common and well-known fermentative substances, with the exception of emulsine, are capable of effecting in any considerable degree the decomposition of rubian, and that none of them, with that single exception, can be employed as a substitute for the ferment contained in madder itself, which produces an effect on rubian altogether *sui generis*.   This circumstance alone, apart from all other considerations, would entitle the ferment of madder to be considered as an entirely distinct and peculiar substance, on which it will therefore be necessary to bestow a distinct name.   For this purpose I venture to suggest the name of *Erythrozym* (from ἐρυθρὸς *red*, and ζύμη *ferment*) as most appropriately indicating its chief characteristic, and I shall now proceed to give a short account of its properties and composition.

When prepared in the manner above described by precipitation with alcohol, erythrozym is obtained as a chocolate-coloured

granular mass. When dried it coheres into hard lumps, which are almost black, and are with difficulty reduced to powder. When the dry substance is heated on platinum foil it emits a smell somewhat resembling that of burning peat and burning horn, and then burns without much flame, leaving a considerable quantity of residue, which, on being further heated, is soon converted into a grayish-white ash, consisting almost entirely of carbonate of lime. If erythrozym be well mixed while in a moist state with water, a reddish-brown muddy liquid is formed, having all the appearance of a solution. It is, however, no solution; the erythrozym is merely suspended in the liquid, for on filtering through paper a clear liquid passes through, while a mucilaginous substance remains on the filter. The latter, on being mixed with a solution of rubian, exerts the usual decomposing effect on that substance, while the liquid, when tried in the same way, is found to be entirely without effect. Hence it follows that erythrozym, after having once been precipitated from its watery solution, even by alcohol, cannot again be dissolved in water. The liquid obtained by treating erythrozym with water and filtering, contains a small quantity of a substance, which, from its reactions, I conclude to be pectic acid, or some body nearly allied to it. In fact, the method of preparing erythrozym implies that all substances contained in the watery extract of madder, insoluble in alcohol, must be found mixed with it; but since the erythrozym itself by precipitation with alcohol becomes insoluble in water, these substances may afterwards be easily removed by treating with water. If the watery liquid in which the erythrozym is contained in a state of suspension be boiled, a sort of coagulation takes place, and the erythrozym separates in the shape of dirty red flocks, while the liquid retains a reddish colour. The same effect is produced by adding alcohol or salts, such as common salt or sal-ammoniac, the substance separating in dark reddish-brown flocks, with a clear yellowish liquid floating above them. That erythrozym is not an uncombined substance, but a compound of an organic substance with lime, is proved by its behaviour towards acids. If it be treated with any acid, even acetic acid, its colour changes from reddish-brown to yellowish-brown, and the filtered liquid is found to contain a considerable quantity of lime. The yellowish-brown flocks left on the filter, after all the excess of acid has been removed, do not again form with water a mucilaginous liquid like the original substance; and even an addition of lime water, though it restores the original chocolate colour, does not reproduce that peculiar condition of suspensibility in water characteristic of it in its original state. The brown substance into which erythrozym is changed by the action of acids is soluble in caustic alkalies, forming pale purple

turbid liquids.   The ammoniacal solution gives reddish-brown flocculent precipitates with most earthy and metallic salts.   When the solution in caustic soda is boiled, a disengagement of ammonia takes place.   If erythrozym be treated with boiling nitric acid, it is dissolved and decomposed with a disengagement of nitrous acid.   A small quantity of a white flocculent substance remains behind.   Concentrated sulphuric acid chars it on heating.

If erythrozym be mixed with water, and the mixture be allowed to stand for a length of time in a warm place, signs of a more active process of fermentation begin to show themselves, especially in summer weather ; bubbles of gas are given off, and a peculiar smell is emitted, which, though disagreeable, cannot exactly be called putrid.   During this process, which is evidently one of putrefaction in the stricter sense, the erythrozym loses its sliminess, and is converted into a red flocculent mass, which may easily be separated by filtration from the liquid.   The latter is clear, colourless and quite neutral.   After erythrozym has passed through this second stage of decomposition, its power of decomposing rubian is found to have lost much of its intensity.   It is during the first period of its decomposition, when no apparent change is taking place, that this power is most energetically exerted.   During the second, or more strictly putrefactive stage, it acquires, however, the property of decomposing sugar.   If erythrozym be mixed with a solution of cane-sugar, and the mixture be allowed to stand for a considerable time until gas begins to be disengaged, the solution acquires by degrees a decided acid reaction.   What the acid is which is thereby formed, I have not yet ascertained.

The erythrozym which I submitted to analysis, was prepared by precipitating it from an extract of madder with tepid water, by means of alcohol, collecting it on a filter, then treating it repeatedly with boiling alcohol until all matter soluble therein was removed, and then washing it on a filter with cold water until the percolating liquid no longer gave a precipitate with sugar of lead, after which it was rapidly dried in the water-bath.   It follows from the analyses which I have made, that the amount of lime which it contains is tolerably constant, and that it must consequently be considered as a definite compound of an organic substance with lime.

I. 0·6220 grm., dried at 100° C. and burnt with oxide of copper and chlorate of potash, gave 0·8670 carbonic acid and 0·2365 water.

0·7770 grm. gave 0·4035 chloride of platinum and ammonium.

0·9630 grm. gave on being incinerated 0·2350 carbonate of lime.

II. 0·4885 grm. of another preparation gave 0·6865 carbonic acid and 0·2060 water.

0·1540 grm. gave on being incinerated 0·0370 carbonate of lime, containing 0·02076 lime = 13·48 per cent. After being treated with sulphuric acid and again heated, it weighed 0·0500 grm., which, estimated as sulphate of lime, is equivalent to 0·02062 lime = 13·39 per cent.

From these numbers may be deduced the formula $C^{56}H^{34}N^2O^{40}$ + 4CaO, as the following calculation shows:—

	Eqs.		Calculated.	I.	II.
Carbon . .	56	336	40·48	40·93	41·20
Hydrogen .	34	34	4·09	4·22	4·68
Nitrogen .	2	28	3·37	3·26	
Oxygen .	40	320	38·57	37·90	
Lime . .	4	112	13·49	13·69	13·48
		830	100·00	100·00	

If this be the true composition of erythrozym, it stands in a very interesting relation to that of rubian. If to 1 equiv. of rubian be added the elements of 2 equivs. of nitric acid, the sum will represent the composition of the organic substance contained in the erythrozym in combination with lime, $C^{56}H^{34}O^{30}$ + 2NO⁵ = $C^{56}H^{34}N^2O^{40}$.

I shall now give the results of an analysis of erythrozym which had been employed for the decomposition of a quantity of rubian, and then treated successively with cold water and boiling alcohol until all the products of decomposition were entirely removed.

0·5330 grm., dried at 100° C. and burnt with oxide of copper and chlorate of potash, gave 0·8325 carbonic acid and 0·2220 water.

0·2780 grm. gave 0·1835 chloride of platinum and ammonium.

0·0645 grm. gave on being incinerated 0·0130 carbonate of lime, containing 0·00729 lime.

These numbers lead to the following composition:—

	Eqs.		Calculated.	Found.
Carbon . . .	52	312	44·82	44·99
Hydrogen . .	32	32	4·59	4·62
Nitrogen . .	2	28	4·02	4·14
Oxygen . . .	30	240	34·51	34·95
Lime . . .	3	84	12·06	11·30
		696	100·00	100·00

If this composition be compared with that of the substance in its undecomposed state, the exact nature of the change which it undergoes during the process of fermentation will be apparent. It appears that the change consisted in this case in the loss of

4 atoms of carbonic acid, 2 atoms of water and 1 of lime, for

$$C^{52} H^{32} N^2 O^{30} + 3CaO + 4CO^2 + 2HO + CaO$$
$$= C^{56} H^{34} N^2 O^{40} + 4CaO.$$
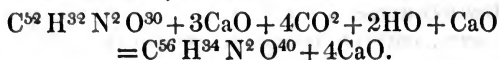
It appears also that no loss had been sustained in the nitrogen. The atom of lime lost was probably abstracted by the carbonic acid disengaged at the same time.

In order to obtain, if possible, the organic substance, which is contained in the erythrozym in combination with lime, and to which the name of erythrozym ought perhaps to be restricted, in a free state, I prepared an extract of madder with tepid water, and added to it tartaric acid. A brown precipitate was produced, which was collected on a paper filter, washed with water in order to remove the excess of acid, then treated with boiling alcohol, until everything soluble in that menstruum was removed, and lastly, washed with cold water, in order to separate any pectic acid which might be present; after which it was dried. Its analysis showed, however, that the acid had removed only half of the lime, and that the substance itself must, during the process of purification, have undergone decomposition.

0·5695 grm., burnt with oxide of copper and chlorate of potash, gave 0·9585 carbonic acid and 0·2600 water.

0·5940 grm. gave on being incinerated 0·0900 carbonate of lime, containing 0·0505 lime $= 8·50$ per cent.

0·2835 grm. gave on being incinerated, and then treated with sulphuric acid, 0·0560 sulphate of lime, containing 0·02310 lime $= 8·14$ per cent. The average per-centage of lime was therefore 8·32.

The nitrogen was not determined; but presuming no loss to have taken place in that constituent, then these numbers correspond to the following composition :—

			Eqs.		Calculated.	Found.
Carbon	.	.	52	312	48·00	47·68
Hydrogen	.	.	30	30	4·61	5·07
Nitrogen	.	.	2	28	4·30	
Oxygen	.	.	28	224	34·48	
Lime	.	.	2	56	8·61	8·32
				650	100·00	

It appears, therefore, that the decomposition of the substance had advanced in this case still further even than in the last, as it contained 2 equivs. of water less.

The last specimen I examined was one which was prepared in the usual manner by precipitation with alcohol, then mixed with water and allowed to decompose until bubbles of gas began to be disengaged, and the smell peculiar to this substance when in a

state of putrefaction began to be emitted, then treated with boiling alcohol until nothing more was dissolved, and dried.

0·4630 grm., obtained in this manner and burnt with oxide of copper and chlorate of potash, gave 0·7260 carbonic acid and 0·1760 water.

0·5270 grm. gave 0·2705 chloride of platinum and ammonium.

0·2990 grm. gave on being incinerated 0·0720 carbonate of lime, containing 0·04040 lime = 13·51 per cent. After treatment with sulphuric acid, this yielded 0·0980 sulphate of lime, containing 0·04042 lime = 13·52 per cent.

In 100 parts it therefore consisted of—

$$
\begin{array}{ll}
\text{Carbon} & 45\cdot65 \\
\text{Hydrogen} & 4\cdot22 \\
\text{Nitrogen} & 3\cdot22 \\
\text{Oxygen} & 33\cdot40 \\
\text{Lime} & 13\cdot51
\end{array}
$$

The only formula with which this composition agrees, and which is at the same time in accordance with the preceding formulæ, is $C^{52}H^{28\frac{1}{2}}N^{1\frac{1}{2}}O^{28} + 3CaO$, which requires in 100 parts—

$$
\begin{array}{ll}
\text{Carbon} & 46\cdot60 \\
\text{Hydrogen} & 4\cdot25 \\
\text{Nitrogen} & 3\cdot13 \\
\text{Oxygen} & 33\cdot48 \\
\text{Lime} & 12\cdot54
\end{array}
$$

From this formula it may be inferred, that during the further progress of decomposition erythrozym loses ammonia in addition to carbonic acid and water, for

$$C^{52}H^{28\frac{1}{2}}N^{1\frac{1}{2}}O^{28} + 4CO^2 + 4HO + \tfrac{1}{2}NH^3 = C^{56}H^{34}N^2O^{40}.$$
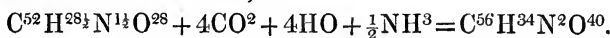
I shall conclude this part of my paper with a few remarks on the *vexata quæstio* of the existence or non-existence of more than one colouring matter in the madder root. Robiquet, the discoverer of alizarine, attributed the tinctorial power of madder partly to that substance and partly to another, on which he bestowed the name of purpurine. Gaultier de Claubry and Persoz were of opinion that there are two colouring matters in madder. Runge has described three substances obtained by him from madder, all of which he considers as colouring matters playing a part in the process of madder dyeing. Schiel, Debus and Wolff and Strecker all assume the existence of two distinct colouring matters. Notwithstanding, however, the array of authority in favour of this view, I have never been able to convince myself that the entire tinctorial power of madder was not due to aliza-

rine alone, and that consequently all substances derived from madder, if endowed with any such power, owe it to their containing alizarine; and after having isolated and examined the primitive substance, from which all the bodies in madder endowed with a red or yellow colour, or capable of producing these colours, are derived, and after having exposed this substance to the influence of all the reagents with which madder itself under ordinary circumstances is brought into contact, I find myself entirely confirmed in my opinion. I should indeed think it unnecessary to add anything confirmatory of the conclusions which must suggest themselves at once from the perusal of the preceding pages, but as Messrs. Wolff and Strecker have made it one of the special objects of their investigation to prove the existence of more than one colouring matter in madder, I think it may not be out of place here to add a few remarks to show how, in my opinion, these chemists, as well as their predecessors, have been misled, and also to give an account of some experiments still further confirmatory of the opinion which I have always held.

The second colouring matter, which, according to the chemists just mentioned, exists in madder in addition to alizarine, and which has received at various times the names of purpurine, madder-purple, and oxylizaric acid, possesses, according to those observers, the property of being easily soluble with a red colour in boiling alum liquor. Wolff and Strecker assert that this substance is peculiarly a product of fermentation, and in order to prepare it, they mix madder with water and yeast, allow the mixture to stand in a warm place until the effervescence produced by fermentation has ceased, and the liquid has acquired a strong acid reaction and contains alcohol, after which they strain the liquid through a cloth, wash the mass on the cloth with water, and then treat it with boiling alum liquor. From the bright red solution a substance separates on cooling in red flocks, which, as well as the orange-coloured flocks produced by adding sulphuric acid to the liquid, consist, according to them, entirely of purpurine without any trace of alizarine. They purify it by crystallization from alcohol. Now I have shown above that the fermentation of madder, which is in fact synonymous with the fermentation of rubian, is due to the action of a peculiar substance, which I have called erythrozym, on rubian; that the action of this substance is very rapid; that it is not accompanied by any disengagement of gas; that it is terminated long before any effervescence or any acid reaction of the liquid begins to appear; that the products of the action do not differ essentially from those due to the action of acids and alkalies; that the formation of alizarine in about the same proportion as when acids or alka-

lies are employed is one of the results of the process, and that yeast exerts no decomposing power on rubian whatever. Hence it necessarily follows, that if the purpurine of Wolff and Strecker be not found as such among the products of the fermentation of rubian, it must consist of a mixture of two or more of those products.

Now I have mentioned in the first part of this paper, that though alizarine and verantine are both perfectly insoluble in boiling alum liquor when acted on separately, yet that when a mixture of both is employed, the mixture is found to be soluble in alum liquor with the colour characteristic of purpurine. Hence I concluded that purpurine is in fact a mixture of those two substances, a view with which all that is mentioned regarding purpurine completely coincides. I may mention incidentally, that in making this experiment it is necessary to treat the verantine with a little dilute nitric acid, in order to destroy the alizarine which usually accompanies it, and then to remove the acid by washing with water before employing it, and that it generally succeeds best when a large excess of alizarine is used.

To this synthetical proof of the opinion here advocated, I will now add a few analytical ones. I will show, in the first place, that purpurine prepared in the manner mentioned by Wolff and Strecker is a substance of very variable composition, but that the variations in its composition may be easily explained by supposing it to consist of alizarine and verantine in different proportions; and secondly, that by treatment with nitric acid purpurine yields unchanged verantine and an acid, which is identical with that formed by the action of nitric acid on alizarine.

In the course of my investigation I obtained at the termination of the process for separating the products of the fermentation of rubian, an alcoholic liquid, from which the verantine had been deposited, but still containing a substance, which, from its solubility in alum liquor, would by most chemists be called purpurine. This substance was precipitated from the solution with water. Its colour was brownish-yellow. It was treated with boiling alum liquor, to which it communicated a bright red colour. The liquid was filtered boiling hot, and deposited on cooling a quantity of red flocks. The residue was treated with fresh quantities of alum liquor, until on cooling very few flocks separated. A great proportion of the substance employed remained undissolved. The flocks deposited from the alum liquor were collected on a filter, and washed with water in order to remove all the alum. After drying they formed a dark reddish-brown powder, which was almost entirely soluble in alcohol. The alcoholic solution left on evaporation a bright red mass, in

which no trace of anything crystalline was discernible. Its analysis gave the following results :—

0·4820 grm. gave 1·1240 carbonic acid and 0·1800 water.

0·8060 grm. left on being incinerated 0·0310 alumina=3·84 per cent.

After making the proper correction for the alumina, the quantity of which, in relation to that of the other constituents, seems to be indefinite, these numbers correspond in 100 parts to—

$$\begin{aligned}
&\text{Carbon} &&. &&. &&. &&. &&. &&66\cdot13 \\
&\text{Hydrogen} &&. &&. &&. &&. &&. &&4\cdot31 \\
&\text{Oxygen} &&. &&. &&. &&. &&. &&29\cdot56
\end{aligned}$$

This composition does not differ very widely from that given for oxylizaric acid by Debus.

Now the formula $C^{56} H^{20} O^{19} = C^{14} H^5 O^4 + 3C^{14} H^5 O^5$ requires in 100 parts—

$$\begin{aligned}
&\text{Carbon} &&. &&. &&. &&. &&. &&66\cdot14 \\
&\text{Hydrogen} &&. &&. &&. &&. &&. &&3\cdot93 \\
&\text{Oxygen} &&. &&. &&. &&. &&. &&29\cdot93
\end{aligned}$$

A quantity of material similar to the last, obtained on a different occasion, was treated in the same way with boiling alum liquor, and the deposit formed on the liquor cooling was submitted to analysis. It had a much lighter colour than the preceding, and when dissolved in alcohol, the latter left on evaporation crystals, apparently of alizarine, mingled with red crystalline masses resembling impure alizarine.

0·4000 grm. gave 0·9870 carbonic acid and 0·1390 water.

0·3840 grm. left on being incinerated 0·0060 alumina=1·56 per cent.

After making the necessary correction for the alumina, these numbers correspond in 100 parts to—

$$\begin{aligned}
&\text{Carbon} &&. &&. &&. &&. &&. &&68\cdot36 \\
&\text{Hydrogen} &&. &&. &&. &&. &&3\cdot92 \\
&\text{Oxygen} &&. &&. &&. &&. &&. &&27\cdot72
\end{aligned}$$

The formula $C^{56} H^{20} O^{17} = 3C^{14} H^5 O^4 + C^{14} H^5 O^5$ requires in 100 parts—

$$\begin{aligned}
&\text{Carbon} &&. &&. &&. &&. &&. &&68\cdot29 \\
&\text{Hydrogen} &&. &&. &&. &&. &&4\cdot06 \\
&\text{Oxygen} &&. &&. &&. &&. &&. &&27\cdot65
\end{aligned}$$

These two specimens therefore of a substance prepared in the same way, both of which would, according to the definition of Wolff and Strecker, pass for purpurine, possessed a composition, which in the one case corresponded to a mixture of 1 equiv. of alizarine and 3 equivs. of verantine, in the other case to a mixture of 3 equivs. of alizarine and 1 equiv. of verantine. A still

more manifest proof of the fact of purpurine not being a substance of uniform composition, is derived from an examination of the liquid from which these specimens were deposited. To the bright red liquid from which the last was deposited, I added after filtration muriatic acid and boiled. A yellow precipitate was produced, which was collected on a filter and washed. A small quantity of it being dissolved in alcohol, the alcohol left on evaporation crystals of apparently pure alizarine. Its analysis also showed that it consisted of alizarine almost in a state of purity.

0·2390 grm. gave 0·6040 carbonic acid and 0·0980 water.

In 100 parts it contained therefore—

		Alizarine.
Carbon	68·92	69·42
Hydrogen . . .	4·55	4·13
Oxygen	26·53	26·45

I now prepared some so-called purpurine from garancine. The garancine was treated with boiling alum liquor, and the liquor was strained boiling hot through calico. On cooling there was formed a copious deposit, which was redissolved in fresh alum liquor. The deposit formed this time, which was very trifling, was separated, and the substance contained in the liquid was precipitated with muriatic acid. The precipitate was yellow. Its analysis proved it to be almost pure verantine.

0·3685 grm. gave 0·8760 carbonic acid and 0·1400 water.

In 100 parts it contained therefore—

		Verantine.
Carbon	64·83	65·11
Hydrogen . . .	4·22	3·87
Oxygen	30·95	31·02

It appears therefore that the substance called purpurine cannot even be called a compound of alizarine and verantine, for it consists sometimes of one alone, sometimes of the other, sometimes of a variable mixture of both.

I now treated some purpurine, made in the same manner as the last, with boiling dilute nitric acid. Nitrous acid was disengaged, the bulk of the substance diminished very much, and its colour became lighter. After the action was completed, I allowed the liquid to cool, added water, collected the yellow flocks on a filter, and washed them with water to remove all the acid. After drying their colour was brownish-yellow, similar to that of pure verantine.

0·3820 grm. gave 0·9070 carbonic acid and 0·1240 water.

In 100 parts—

Carbon	64·72
Hydrogen	3·60
Oxygen	31·68

Its composition therefore, with the exception of a slight deficiency in the amount of hydrogen, was that of verantine.

The acid liquid filtered from the substance was evaporated almost to dryness, when it yielded a quantity of yellow crystals. These were washed with cold water, and then redissolved in a little boiling water. The boiling solution was decolorized with animal charcoal, and after being filtered boiling hot deposited on cooling a quantity of colourless crystals, having the appearance, and as their analysis showed, the composition of Laurent's naphthalic acid.

0·5230 grm. gave 1·1020 carbonic acid and 0·1770 water.
In 100 parts—

						Naphthalic acid.
Carbon	.	.	.	.	57·46	57·83
Hydrogen	.	.	.	.	3·76	3·61
Oxygen	.	.	.	.	38·78	38·56

Wolff and Strecker mention, that purpurine yields this acid when subjected to the action of nitric acid, but the verantine which is found in an undecomposed state, after the action of the nitric acid has ceased, seems to have eluded their observation.

From these experiments I infer that purpurine, madder-purple, and the various similar bodies derived from madder, owe their property as colouring matters to an admixture of alizarine, and that they simply consist of the latter substance in a state of impurity.

LXXX. *Mathematics and Mathematicians. The Journals of the late* Reuben Burrow. *By* T. T. WILKINSON, *Esq., F.R.A.S., and Corresponding Member of the Manchester Literary and Philosophical Society.*

No. II.

SINCE the preceding article of this series was written, the indefatigable Astronomer Royal has communicated a paper to the Royal Society which sets at rest the occurrence of an eclipse when Xerxes was setting out from Sardes on his expedition to Greece (Phil. Mag. March 1853, pp. 216–218).

" The author thinks it most likely that the phænomenon really was the total eclipse of the moon which occurred on the morning of March 14, B.C. 479 ; " but Mr. Burrow appears to have been led to the conclusion that an eclipse of the sun must have taken place about the same period, as is evident from a memorandum which I have since met with, as follows, in an obscure corner of some disfigured leaves at the end of the Journal.

" In the year 478 before Christ.

"Costard has treated this eclipse particularly in a dissertation.

"Apparent conjunction at Smyrna, 35 minutes past 12 at noon; $11\frac{1}{2}$ digits eclipsed."

The disfigured pages to which I allude are mostly occupied by rough sketches of the diagrams belonging to the geometrical papers inserted in the first number of Carnan's Diary. One of these has reference to the properties of Halley's Diagram, which has since been found so prolific of relations in the hands of Davies, Weddle and Elliott, under the title of *Horæ Geometricæ*; another belongs to the "very useful Lemma" that "the difference of any two sides of a plane triangle is less than the third side" (Diary, 1776, p. 34);—of the rest several relate to the papers on "Geometrical Sums and Differences," "Limits of Geometrical Quantities;" and one or two appear to have been sketched when considering the "very difficult problems on the maxima and minima," subsequently proposed as prize questions in his Diary. A "List of Oxford and Cambridge terms," and a series of notes on the "Eclipses in 1776," which occur among these rude sketches, with a memorandum to "*take the rest from the Ephemeris*," afford sufficient evidence that the Lady's and Gentleman's Diary was in active preparation, and suggest the reflection that at all events Mr. Burrow knew "*how to make an Almanack*," whatever might be the defects of Hutton and Maskelyne.

"September 10, 1775. Messrs. Wales, Bayly and Todd called upon me at night, and Wales proposed a question which I told him how to solve directly by means of a hyperbola. He seemed to think that I had not used Maskelyne well, but said little about it. I mentioned that I had done a good deal about Euclid's Porisms, and we had some talk about the method of the Ancients. I told him that Dr. Horsley's book *De Inclinationibus* was much longer than it need to be;—I also showed him my paper about Perfect Numbers, and he offered me a question, but desired me to put it under Thomas Barker's name, so I refused it. We afterwards went out and called upon Keech, but he was not at home. I told Bayly that I should put a method of placing a sector in the plane of the meridian into my Almanack, and he said that Maskelyne had mentioned some such method to the Royal Society when the paper about the business was read when I was not there. Bayly said that Maskelyne got great praise for speaking so favourably of me in his paper. I asked him why? He said that Maskelyne stated that I did all the surveying [at Schiehallien] and found out all the methods for doing it. I asked whether he also mentioned what I had done about the observations, and he said that Maskelyne did not. I

told them that he ought to have stated that I had put up all the instruments and drawn the meridian line, and also put the instrument in order when he was at a loss how to do it.   Bayly disputed this, so I told him he was at liberty to think as he pleased, but that was a fact which I supposed Mr. Maskelyne would not deny.   Wales mentioned the Sieve of Eratosthenes, which led to a deal of conversation.

" *Mem.*   To call on Carnan on Friday noon."

Mr. William Bayly was a respectable astronomer and mathematician, who filled the office of assistant to the Astronomer Royal, Dr. Maskelyne, for several years.   In 1769 he was sent out by the Royal Society to the North Cape, to observe the transit of Venus; he subsequently accompanied Captain Cook in his second and third voyages, and on the decease of Mr. George Witchell, was appointed as his successor in the mastership of the Royal Naval Academy at Portsmouth.   It does not appear that Mr. Burrow ever published anything relating to Euclid's Porisms, but the following note found in his copy of Dr. Simson's *Locis Planis,* now in my possession, will show that at the time it was written he had advanced no further in the subject than to be able to transform the enunciation of a Locus into a Local Problem, under the idea that by this process he had obtained the enunciation of a Porism.

" On Porisms.   If in the enunciations of the Propositions, which are now in the form of Theorems, the hypothesis was to remain as it is, and instead of saying ' the *Locus* will be such a line, &c.;' if it was required to determine what the Locus would be:—then all this book (Simson's *Apollonius de Locis Planis*) would be exactly in the form of Porisms, except the latter part of each proposition, which shows the composition, or how to determine the Locus.   For example, if Prop. VIII. had been in this form :—' if from a given point two right lines be drawn *in directum,* containing a given space, and one of them touch a right line given in position; required to shew in what line the other point will always fall:'—this would be in the form of a Porism. In most of the problems in *Apollonius de Locis Planis,* in Theorems, altered as above, instead of right line and circle, &c., substitute Conic Section, &c., and see what would be the result of *such an inquiry.*" (Mech. Mag. No. 1437.)

His opinions respecting the operose character of Dr. Horsley's restoration of *Apollonii Pergæi Inclinationes Oxonii,* 1770, are frequently enforced in his Diaries for 1777–1779, and received an elegant and practical illustration in a Restitution of the Geometrical Treatise of Apollonius Pergæus on Inclinations, which he published and dedicated to the Right Honourable Lord Viscount Townsend in the latter year.   The papers on Perfect

Numbers and on placing a Zenith Sector in the plane of the Meridian, occur as Articles II. and III. in the first number of the Diary, but I have not been able to find that Mr. Wales ever had any connexion with this periodical, either anonymously or otherwise. Mr. Burrow's opposition to Dr. Maskelyne does not appear to have rested on good grounds, and there is little doubt that many of his supposed injuries were merely imaginary. All who are acquainted with the writings and labours of this Astronomer Royal will not place much credit in such depreciations of scientific character as are exhibited in this extract; whilst the fact that the mutual friends of both parties disapproved of Mr. Burrow's views and conduct, affords strong presumptive evidence that Dr. Maskelyne's proceedings are not represented under their real character. In a subsequent extract we shall find that Mr. Burrow had an advertisement in the St. James's Chronicle relating to these differences, but I have not been able to ascertain its nature.

" September 25. I came to Greenwich to call on Collins, but he was not at home. Coming through Lee Churchyard, I copied the following epitaph from the tombstone of Dr. Halley:—

<div align="center">

" ' Sub hoc marmore

Placide requiescit cum uxore carissima

Edmundus Halleius, LL.D.

Astronomorum sui sæculi facile princeps.

Ut vero scias Lector,

Qualis quantusque vir ille fuit,

Scripta ejus multifaria Lege,

Quibus omnes fere artes et scientias

Illustravit, ornavit, amplificavit,

Æquum est igitur,

Ut quem cives sui vivum

Tantopere coluere

Memoriam ejus posteritas

Grata veneretur.

</div>

Natuus $\Big\}$ est A.C. $\Big\{$ MDCLVI.
Mortuus $\quad$ $\quad$ MDCCXLII.

<div align="center">

Hoc saxum optimis parentibus

Sacrarunt duæ filiæ pientissimæ

Anno C. MDCCXLII.'

</div>

Here is also interred Margaret Halley, the eldest daughter of the above Dr. Halley. She died on the 3rd of October, 1743, in the 55th year of her age. Also Mrs. Catharine Price, youngest daughter of the above Dr. Halley, who died November 10th, 1765, aged 77 years, and Mr. Henry Price her husband."

" *Mem.* To call on Lord Cavendish next Tuesday at 10 o'clock.

" October 16.   I lent Mr. Clarke £1 2s. 6d.

" October 18. Lent Mr. Clarke two shillings.

" October 28. I let Mr. Clarke have half-a-guinea.

" Since the above was written I lost my ink-bottle, and the tailor made my coat without a side-pocket, which is the reason why I neglected to keep my Diary as I used to do.   I put up for the place of Mathematical Master of Christ's Hospital and lost it.

" December 26. I called on Clarke and went to New Square, where I found a note for me to wait on Mr. Humfrey, junior, in the Tower.   Waited on him, and am to go tomorrow to give him a Lesson in Geometry.

" December 28. Did nothing or worse; was with Rogers in Chancery Lane.

" January 6, 1776. Was at the Nest, and Isaac Dalby was there.

" January 12. Called and supped with Mr. Robertson at the Royal Society.   In conversation Mr. Robertson said that Mr. William Jones, who was Secretary to the Royal Society, lived in Beaufort's Buildings.   He was a little shortfaced Welshman, and used to treat his mathematical friends with a great deal of roughness and freedom.   He rated Mr. Thomas Simpson in such a manner about his paper against De Moivre, that Simpson said he would never go to see him more, but he did see him again however.   Gardiner, the Logarithm fellow, and Dodson he used to treat very harshly.   Mr. John Robertson was his great favourite : the first of their acquaintance was caused by Mr. Robertson's book of Mensuration.   Mr. Jones asked Dr. Bevis if he knew the author, when the Doctor replied that he was a young man of his acquaintance.   ' Tell the young man,' said Jones, ' I should like to see him.'   On Mr. Robertson calling, Mr. Jones told him there was an error in his book, and gave him some papers to copy where it was corrected.   After some few visits, Jones began to take the liberty of talking in his harsh manner to Mr. Robertson, and he took it once or twice, but finding that it increased, he resolved to take it no longer, and began to retort.   Jones seemed surprised to find that Mr. Robertson had more spirit than others had exhibited, but never talked to him in that strain again, and ever afterwards treated him with the greatest kindness.   It was Mr. Jones's failing to be always speaking of the faults of his mathematical acquaintances :—one was conceited, another was obstinate; this was ignorant, and that had some fault or other.   His remarks, however, were generally shrewd and satirical.   He left a whole Course of Mathematics nearly ready for the press, and Mr. Robertson was to have had the care of printing them, but when Mrs. Jones found that it could not

be done without incurring considerable expense she dropped the intention of publishing, although her husband (Mr. Jones) died worth some two or three thousand pounds.   Mr. Jones left his library, which was considered the most curious and valuable of any at that time, to Lord Macclesfield, who was very anxious to publish a catalogue of it, which Jones had made out after a very curious method.   This manuscript catalogue was shown to Dr. Bliss, who was afterwards Astronomer Royal, and he pretended that the Latin was very erroneous in the declensions and cases, and that it would be an endless piece of business to write the manuscript over again corrected.   This was occasioned by a pique that Bliss had against Jones, for the latter refusing him a copy of the Abstract of the Conic Sections, which was afterwards printed by Mr. Robertson in the Philosophical Transactions.   When Bliss applied for the copy, Mr. Jones made excuses and pretended he had not got them by him, but when he next saw Mr. Robertson he told him that he had refused Dr. Bliss a copy for the following reasons :—' In the first place, if I gave them to him he does not understand them, and in the next it is the way of the parsons to make everything they can get their own; besides, he would probably give them out at the University, and then some of them would publish the properties as their own, and I do not know but that I may some time or other wish to publish them myself.'   His Course of Mathematics, and other papers which he had intended to publish, fell into Lord Macclesfield's hands, who about this time had an electioneering affair against Sir Francis Dashwood, in Oxfordshire, which cost him more than a hundred thousand pounds, and almost ruined him.   After this he kept no accounts, and also married improperly; his family were in confusion, and when he died Lady Macclesfield ordered all his papers to be burned except such as related to money matters.   Mr. Jones's papers were never heard of more, and some think they were burned among the rest, but Horsfall of the Temple, who was one of those employed, says that there were no such papers among those that were destroyed. Others say that a number of papers were sent down to Sherborne Castle in the late Lord's lifetime, and that the present Lord has them yet in his possession, although he may not know it.

" January 16. My advertisement about Maskelyne was in the St. James's Chronicle.

" Major Watson sent for me and told me he had spoken to Lord Townsend about making an appointment in the Tower for a master to teach mathematics to the cadets in the Drawing Room, and that he had recommended me to fill the place.

" January 21. I called on Mr. Robertson and dined with him.

He told me that in the latter part of Sir Isaac Newton's life, when foreigners used to ask him questions about mathematical subjects, he used to inform them that he had no time to apply himself to such subjects, but if they would apply to Dr. Brook Taylor, he would resolve them anything they might desire."

From several successive entries in this portion of the Journal, it would appear that Mr. Burrow was pretty fully occupied with private pupils, amongst whom Mr. Humfrey, " Mr. Chapman, the schoolmaster at Redriff," Mr. Grove and his two sons, are most frequently mentioned. His associates at this time comprised most of the ablest members of the Spitalfields Mathematical Society; the name of Mr. Dalby, afterwards Professor Dalby of the Royal Military College, Sandhurst, occurs on several occasions as having been in his company and on terms of great intimacy. Mr. Dalby was in fact one of the principal writers in the early portion of Burrow's Diary; contributing occasionally in his real name, and frequently under the fictitious signature " Caput Mortuum," to both the mathematical and poetical departments. The assistance rendered by Mr. Burrow to " poor Clarke" appears worthy of notice, since it confirms Mr. Swale's remark, that " his *heart* was good although his habits had not been formed by the hand of a master : "—most probably Mr. Clarke was entirely dependent upon such assistance during the latter part of his stay in London. The failure at Christ's Hospital is very laconically noticed, but his temper seems to have been somewhat more seriously disturbed by the oversight of the unaccommodating tailor than by the decision of the Hospital examiners. The terms in which his opinions of the workman's default are expressed, rendered it necessary to suppress a portion of the Journal at this point. Mr. Samuel Rogers was a frequent contributor to the early numbers of the Diary, and at one time I was led to conclude that he was identical with the noted poet and banker of Lombard Street; subsequent examination, however, in the absence of positive proof, has led me to doubt the accuracy of this conjecture. The application to Lord Townsend eventually proved successful, since the Diary for 1777 contains an announcement that correspondents must address their communications to " Mr. Reuben Burrow, Mathematical Master of the Drawing Room in the Tower ; " his salary, however, was not on a very liberal scale, being only 100*l.* a year ; nor did the Board of Ordnance think proper to grant him any remuneration for the extra duties they imposed upon him in making surveys of the Essex and Sussex coasts, during the time he held the appointment, for on the " 27th August, 1782," he complains to the Board that " he has been allowed nothing ; when at the same time that wretched compiler of other men's productions, the

Mathematical Master at Woolwich (*Dr. Hutton*), is paid with profusion for his extra services; though he has more than double the salary that I had, and his scholars never made half the improvement that those in the Drawing-room did in the same interval."

Mr. William Jones, F.R.S. was the father of the late celebrated Sir William Jones, and this extract adds many curious particulars respecting him to the sketch of his life given by Mr. Robertson in Hutton's Mathematical Dictionary. No doubt Mrs. Jones acted upon the best advice when she relinquished the project of publishing her late husband's Course of Mathematics; she had now the prospect of having an infant son to protect, educate, and prepare for the actual struggles of life, and under such circumstances the considerable outlay which must have been incurred in case of publication, could not be justified by the uncertain sale of a Course of Mathematics, although her husband had died in better circumstances than usually falls to the lot of mathematicians. The pamphlet referred to as having been written by Simpson against De Moivre, was most probably the Appendix to the Doctrine of Annuities, published by the former in 1743. In the treatise on this subject, Mr. Simpson had spoken favourably of De Moivre's work, but these well-meant compliments were misunderstood by the latter, and led him to insert some very harsh expressions respecting Simpson in the Preface to a new edition of his Annuities on Lives. These improper inferences gave rise to Simpson's Appendix, which contained a dignified answer to De Moivre's " personal and malignant representations," and so far convinced the latter of the erroneousness of his conclusions, that he omitted the obnoxious passages in the next edition of 1750. Most probably Mr. Jones felt anxious for the welfare of the Royal Society, and did not desire that such unseemly differences should exist between two of its most distinguished ornaments. Mr. Gardiner is well known as the editor of the best edition of Sherwin's Logarithmic Tables, and Dr. Bevis was one of the ablest astronomers of the last century. He erected an Observatory at Newington Green at his own expense, and published Dr. Halley's Astronomical Tables in 1749 with considerable additions. He assisted in the composition of several works on science; was a correspondent to the Mathematical Magazine, conducted by his friends Messrs. Moss and Witchell, and contributed twenty-seven papers to the Philosophical Transactions. The account of Mr. Jones's Catalogue and the disposal of his library are without doubt in the main correct, although the motives attributed to Dr. Bliss seem scarcely sufficient to account for his opposition to its publication. Whether the MSS. were burnt or sent down to Sherborne

Castle does not appear to have been as yet ascertained, nor could it answer any useful purpose to attempt the necessary inquiry: the Course would probably have lost much of its value even in Burrow's time, and would certainly possess no interest now, except as the production of a mathematician who occupied a high position in the world of science a century ago. With regard to the troubles in the Macclesfield family, it may be observed that Mr. Jones exerted himself very successfully in favour of reconciliation, and so far arranged matters to the satisfaction of all parties, that it became a common saying, as Mr. Robertson remarks, " that Macclesfield was the making of Jones, and Jones the making of Macclesfield." (Hutton's Dictionary, vol. i. p. 692). Dr. Brook Taylor " was a profound and elegant mathematician of the old school of Newton, Jones, Cotes, &c., and one of the chief writers in the disputes with the Bernoullis " and other continental writers. His Theorem will perpetuate his name so long as the Differential Calculus exists; nor will those who know anything of the contents of his *Methodus Incrementorum,* his Principles of Perspective, &c., consider that his merits have been at all overstated in the extracts from Burrow and Hutton.

Burnley, Lancashire,
March 18, 1853.

[To be continued.]

LXXXI. *The Singularities of Physical Science, their Classification, and their Uses.* By Robert Carmichael, *A.M., Fellow of Trinity College, Dublin**.

IN the scientific study of nature, the attention is perpetually awakened by the discovery of various singularities in her generally regular and even course.

In one point of view it would be unmeaning to institute any comparison between such singularities and those which occur in pure mathematics, since they are logically identical, the operations of nature conforming to mathematical laws. In another point of view, however, there is an extreme contrast. The one class is the result of our own arbitrary conceptions embodied in definitions; the other is founded upon general laws of nature, whether discovered or undiscovered: a knowledge of the one serves to no practical purpose, unless we are to accept that of decoration, as those singular points of an ellipsoid, the umbilics, were proposed to be employed by the celebrated geometer Monge; the great benefits which a knowledge of the other has conferred upon mankind are appreciated by all who feel the slightest interest in the researches of modern science.

* Communicated by the Author.

If we examine these *physical* singularities with a view to their classification, we shall find that some of them are the rigid consequences of general laws with which we are acquainted. For example, the existence of magnetic poles, that is, of points at which the horizontal magnetic force vanishes, results from the general character of the Potential surface. Again, the existence of a position of minimum deviation is the necessary consequence of the laws of prismatic refraction.

There are, however, other physical singularities which are to us, in the present state of our knowledge, absolutely anomalous. We are unable to account, philosophically speaking, for their existence; and as they appear to depend in general upon the molecular constitution of bodies, there seems little probability that the demands of science will ever be completely satisfied. As a familiar instance, in cooling down from the point of ebullition, water gradually contracts until it reaches the temperature 4° Cent., when it commences to expand, and continues to do so until it solidifies in the shape of ice. The existence of a *point of maximum density* is thus indicated for *a liquid*. A more remarkable example of this class of physical singularities is furnished by the expansion of Rose's fusible metal. This substance, an alloy of bismuth, lead and tin, "contracts on cooling until it solidifies at 75°; after this it still continues to contract until it attains to 55°, from which point to 35°, on the contrary, it expands, and after that contracts again, and continues to do so to the lowest temperatures. It is to be remarked, that the volume at the melting-point, 75°, is the same as at the point of relative maximum, 35°. It is probable that this anomaly is connected with some change which takes place in the arrangement of the molecules of the metal consequent on the change from the liquid to the solid state, and that it is analogous to the similar phænomenon in the case of water; with this difference, that in the latter substance the anomaly begins to exhibit itself before the change (solidification) takes place, but in the fusible metal not until afterwards." (Dixon's Treatise on Heat, part 1, p. 41.)

The expansion of this body may, in fact, be represented by the curve
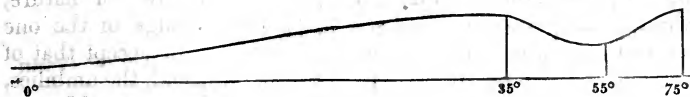
where the abscissæ represent the temperatures, and the ordinates the corresponding volumes; and we are now furnished with an instance of *a solid*, exhibiting not only a point of maximum *density*, but also a point of maximum dilatation. I shall discuss

this illustration at some length, as it is suggestive of some points most valuable in a theoretical point of view.

At first sight it would seem as if a knowledge of the conditions which have been just stated would be sufficient to determine the law of expansion, the method employed being applicable to all similar cases.

Thus δ_t being the coefficient of expansion, supposing generally that

$$\delta_t = f(t),$$

it is evident that the form of f must be discontinuous; the first break occurring for $t = 75°$, and a second at the value of t corresponding to the point of vaporization.

Moreover for $t = 55°$, we should have

$$f'(55) = 0, \quad f''(55) > 0 ;$$

for $t = 35°$,

$$f'(35) = 0, \quad f''(35) < 0;$$

and since the volume at $75°$ is equal to the volume at $35°$,

$$f(75) = f(35).$$

These conditions, however, are manifestly insufficient to determine the form of f, or the law of expansion of the metal; indeed it is evident that an infinite number of curves might be described so as to satisfy those conditions, but that one curve alone can give the law required.

This remark will serve to explain a difficulty alluded to by M. Pierre* with regard to the expansion of liquids. He has shown, that, although the formula

$$\delta_t = at + bt^2 + ct^3$$

in general represents their expansion with considerable accuracy, in some cases we must adopt different coefficients for the upper and lower parts of the thermometric scale. Thus in the case of amylic alcohol, or fusel oil, the coefficients for temperatures from $-15°$ to $+80°$ Cent. differ from those suited to the range from $80°$ to $131°$. M. Pierre states also that a formula of this kind fails to represent the expansion of water between the limits $-13°·14$ and $100°$, and that he has not succeeded in discovering any simple expression which will do so.

From what has been said it is obvious that *à priori* this should be so, unless the formula express *the exact* law of expansion, *which is by hypothesis unknown.* The partial coincidence of the deductions from the formula with the results of experiment is easily intelligible from the circumstance, that any two curves possessing tolerable regularity and passing through the same

* *Annales de Chimie et de Physique.*, vol. xix. p. 210, 3rd series.

three points, which are necessary to determine the three coefficients *a, b, c,* will nearly coincide between those points.

We may also now anticipate what the *ultimate form* of coefficients of expansion must be, if ever the knowledge of molecular physics shall be so far advanced, that this form may be investigated *à priori* from the laws of the elementary molecules, without the necessity of having recourse to experiment except for verification.

It is well known that most solids expand uniformly by increase of temperature until they reach the point of *liquefaction.* At this point an abrupt change takes place, and the expansion of the body commences to follow a different law. This new law holds until the body reaches the temperature corresponding to the point of *vaporization,* when a second abrupt change takes place, and the expansion obeys a third law differing in character from both the preceding.

In the present state of our knowledge we are obliged to employ three distinct coefficients of expansion, expressed by empirical formulæ, for these three several stages. It would seem, however, that in general coefficients of expansion *should be represented by discontinuous functions,* with breaks corresponding to the points of liquefaction and vaporization respectively, and the temperatures corresponding to the points of maximum and minimum condensation, if there be such, being given by the roots of the respective derived functions. Any discrimination of substances into solids, liquids and gases, may be regarded, so far as the previous remark is concerned, as unnecessary, since modern skill has shown that they may be all reduced to the class *solid.*

Of the two classes to which we have proposed to reduce all physical singularities, the one comprehending those deduced as consequences of known laws, and the other those not yet so deduced, the former has subserved the most important practical purposes. Thus, in the science of optics, we avail ourselves of our knowledge of the position of minimum prismatic deviation in order to determine the indices of refraction and the dispersive powers of various substances, accurate values of which are indispensable to the perfection of all optical instruments and many of the useful arts. The other instance by which this first class was illustrated was borrowed from the science of terrestrial magnetism, and it is unnecessary to dwell upon the advantages which have resulted from the reaction of the determination of the magnetic poles upon a great branch of physical science.

But while such valuable ends have been thus attained, how comes it that the other class of these physical singularities has as yet contributed little or nothing for the practical good of

mankind ? Thus, in the case of the point of maximum density in water, man has traced the wise design, the benevolent provision of the Almighty, by which our seas, lakes, and rivers are available for human purposes, and not converted into solid frozen masses. He has wondered, but as yet his wonder has resulted in no discovery for the benefit of his species.

In France, indeed, philosophers have adopted this point as a natural unit of density. It has thus supplied us with a particular standard; but how little is this in comparison with the advantages which ought to result from a due application of this surprising phænomenon ! Again we repeat our inquiry, How comes it that this second class of physical singularities has contributed little or nothing for the practical good of mankind ?

The answer plainly is, because we are unacquainted with the general laws of which they are the consequences. How is it that in optics we can employ so extensively and so usefully the position of minimum deviation ? Evidently because we are acquainted with the general laws upon which its existence depends.

In order, then, to be enabled to employ with advantage the singularities of nature, we must acquaint ourselves with the general laws of which they are the consequences. The principal of those which have not been yet so employed appear to depend upon the molecular constitution of bodies. Another reason is thus perhaps added to the many which combine to render molecular physics one of the most interesting subjects of the present day. Experimental research has done much, the difficulties which encompassed its mathematical treatment are fast disappearing, and its claims upon consideration seem fairly appreciated.

LXXXII. *Considerations on the Theory of Chemical Changes, and on Equivalent Volumes. By* T. S. HUNT, *of the Geological Commission of Canada*.*

IN the proposed inquiry we commence by distinguishing between the phænomena which belong to the domain of physics, and those which make up the chemical history of matter. We conceive of matter as influenced by two forces, one of which produces condensation, attraction, and unity, and the other, expansion, repulsion, and plurality. Weight, as the result of attraction, is a universal property of matter. Besides this, we have its various conditions of consistence, shape, and volume, with the relation of the latter to weight, constituting specific

gravity, and the relations of heat, light, electricity and magnetism. A description of these qualities and relations constitutes the physical history of matter, and the group of characters which serve to distinguish one species from another, may be designated the *apparent form* of a species, as distinguished from its *essential form.*

The forces above mentioned modify physically the specific characters of matter, but they have besides important relations to those higher processes, which give rise to new species by a complete change in the specific phænomena of bodies. In the capacity of this complete change, consists the *chemical activity* of matter.

It is necessary to distinguish between the production of new species differing in physical characters, and that reproduction which belongs to organic existences. The distinction arises from that individuation which marks the results of organic life, and is eminently characteristic of its higher forms. The individuality not only of the organism, but of its several parts, is more evident as we ascend the scale of organic life, while inorganic bodies have a specific existence, but no individuality; division does not destroy them. Solidity or crystallization is a commencement of individuation, and crystals like the tissues of plants and animals, must be destroyed before they can become the subjects of chemical change; "*corpora non agunt nisi sint soluta.*"

That mode of generation which produces individuals like the parent, can present no analogy to the phænomena under consideration; metagenesis or alternate generation*, and metamorphosis, are perhaps, however, to a certain extent prefigured in the chemical changes of bodies. Their metagenesis is effected in two ways, by condensation and union on the one hand, and by expansion and division on the other. In the first case, two or more bodies unite, and merge their specific characters in those of a new species. In the second case, this process is reversed, and a body breaks up into two or more new species. Metamorphosis is in the same manner of two kinds; in metamorphosis by condensation, only one species is concerned; and in that by expansion the result is homogeneous, and without specific difference.

The chemical history of bodies is a record of these changes; it is in fact their genealogy. The processes of union and division embrace by far the greater number of chemical changes, in

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* The term *metagenesis* I find used by Prof. Owen to designate that multiform or serial generation observed in many of the lower orders of animal life. This, however, is not strictly a metagenesis, and the term, if not rightly understood, may lead to dangerous errors; as applied to chemical transformations, however, it appears unobjectionable.

which metamorphosis sustains a less important part. By union we rise to indefinitely higher species; but in division a limit is met with, in the production of species which seem incapable of further division, and these, being regarded as primary or original species, are called *chemical elements.* These two processes continually alternate with each other, and a species produced by the first may yield by division species unlike its parents. From this succession results *double decomposition* or *equivalent substitution,* which always involves a union followed by division, although under the ordinary conditions the process cannot be arrested at the intermediate stage. the production of molecules, but are putting together of molecules

The prevalence of certain modes of division in related species, has given rise to the different hypotheses of copulates and radicals, which have been made the ground of systems of classification; but these hypotheses are based on the notion of dualism, which has no other foundation than the observed order of generation, and can have no place in the theory of the science. A body may divide into two or more new species, yet it is evident that these did not pre-exist in it, from the fact that a different division may yield other species whose pre-existence is incompatible with the last; nor can the pre-existence of any species but those which we have called primary, be admitted as possible. Apart from these considerations, it is to be remarked that our science has to do only with phænomena, and no hypothesis as to the noumenon or substance of a species under examination, based upon its phænomena, or those of its derived species, can ever be a subject of science, for it transcends all sensible knowledge. supersalts of ammonium to HCl. and to volatile

For these reasons, it is conceived that the notion of pre-existing elements or groups of elements, should find no place in the theory of chemistry. Of the relation which subsists between the higher species, and those derived from them, we can only assert the possibility, and under proper conditions, the certainty of producing the one from the other. Ultimate chemical analyses, and the formulas deduced from them, serve to show what changes are possible in any body, or to what new species it may give rise by its changes.

Chemical union is interpenetration, as Kant has taught, and not juxtaposition, as conceived by the atomistic chemists. When bodies unite, their bulks, like their specific characters, are lost in that of the new species. Gases and vapours unite in the proportion of one volume of each, or in some other simple ratio, and the resulting species in the gaseous state occupies one volume, so that the specific gravity of the new species is the sum of those of its factors. The converse of this is true in division, and the united volumes of the resulting species are some simple multiple

of that of the parent; in metamorphosis a similar ratio is always observed.

Apart from the apparent exceptions about to be noticed, the weights of equal volumes of gases and vapours are their *equivalent weights,* and the doctrine of chemical equivalents is that of the equivalency of volumes. According to the atomic hypothesis, these weights represent the relative weights of the atoms, and as equal volumes contain the same number of atoms, these must have similar volumes, so that we come at last to the equivalency of volumes. As chemical combination is not a putting together of molecules, but an interpenetration of masses, the application of the atomic hypothesis to explain the law of definite proportions becomes wholly unnecessary. Chemical species are homogeneous; "*tota in minimis existit natura.*" Solution is chemical union, as is indicated by the attendant condensation; mechanical mixtures are not accompanied by any change of volume.

As two volumes of water-vapour yield one volume of oxygen and two of hydrogen, this was assumed to be the equivalent of water and of hydrogen, while oxygen was represented by one volume, whose weight was 8, that of the volume of hydrogen being ·5, so that the weight of the equivalent of water was 9. But two volumes of hydrogen unite without condensation, with two of chlorine, and the resulting four volumes of hydrochloric gas are found to be equivalent to four volumes of chlorine, hydrogen or water-vapour. Hence four volumes are to be taken for the equivalent of water, and it becomes H^2O^2, with an equivalent of 18, corresponding to HCl, and to volatile species generally, whose equivalents are represented by four volumes of vapour; from these, the equivalents of non-volatile species are determined by comparison.

Hydrogen, chlorine, and some other primary species offer apparent exemptions to the general law of condensation and equivalency of volumes. When four volumes of chlorine unite with four of olefiant gas, or naphthaline, the product is condensed into four volumes; but if the chlorine unite with the same volume of hydrogen gas, there is no condensation, and eight volumes or two equivalents of hydrochloric gas are produced. This, however, is explained when we find that four volumes of the chloro-hydrocarbon MH, Cl^2, may break up into four of a new species MCl, and four of HCl, a change which with the chloride of etherene is effected by the aid of hydrate of potash, and with the chloride of naphthaline takes place spontaneously at an elevated temperature. In the production of hydrochloric gas from chlorine and hydrogen, union takes place followed by immediate expansion without specific difference or metamor-

phosis, while in its production with the hydrocarbons we observe the intermediate stage.

If an equivalent of four volumes of hydrochloric gas were to undergo a change like the chloride of naphthaline, and yield four volumes of chlorine and four of hydrogen, these species would appear with one-half their observed densities; hence we conclude that they are actually condensed to one-half their theoretical volumes, so that four volumes of hydrogen gas represent not H, but H^2. In the same way, if we conceive the quantity of oxygen produced from four volumes of water-vapour to represent two equivalents, it should equal eight volumes instead of two, so that it is condensed to one-fourth, precisely as the vapour of sulphur is condensed to one-twelfth of its theoretical volume. As there are no bodies which are known to yield for four volumes a less quantity than two volumes of oxygen, this may be taken to represent its equivalent, and the condensation of the theoretical volume is like that of hydrogen and chlorine, one-half. Water with an equivalent of four volumes is then H^2O, and its weight $2+16=18$; the same formula is deduced by those chemists who take two volumes for the equivalent, and dividing the weight of hydrogen, write water H^2O, with an equivalent weight of 9. The condensation of these elements is that mode of metamorphosis which constitutes polymerism, and evidently offers no exception to the law of equivalent volumes.

The law of Laurent, " that the number of atoms of hydrogen, or of hydrogen, chlorine, nitrogen, metals, &c., in any formula, corresponding to four volumes of vapour, is always a sum divisible by two," clearly follows from the principles already laid down, and from the fact that nitrogen and the metals are subject to the same conditions as hydrogen and chlorine; the *atoms* have the value which has been assigned to H and to Cl in the formulas given above. The same rule of divisibility, as Laurent has already shown, necessarily holds in regard to the number of atoms of carbon, as well as to the oxygen and sulphur, if we take for their equivalent weights the numbers 6, 8 and 16 respectively [*].

It is to be remarked that while the coefficients of H, Cl or N, in formulas where these are associated, may be odd numbers, those of O, S, and C are always even; this seems a conclusive reason for doubling the equivalents of the latter, or dividing those of hydrogen, chlorine, the metals, &c., according as four or two volumes are taken for the equivalent.

I have elsewhere pointed out that carbon and oxygen sustain

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[*] See Laurent's Récherches sur les combinaisons azotées, *Ann. de Chimie et de Physique*, Nov. 1846, and the American Journal of Science for Sept. 1848, p. 174.

such relations that $C^2 H^2$ may be compared with $O^2 H^2$ and with $O^2 M^2$, and by the substitution of nitrogen for hydrogen, with $C^2 HN$, prussic acid, and $O^2 N^2$ nitrous oxide (the so-called compounds of nitrous oxide with bases are probably $O^2 MN$, corresponding to the cyanides, $C^2 MN$); while the peroxide of hydrogen, $O^4 H^2$, corresponds to $O^4 N^2$, nitric oxide, and to $C^4 N^2$, cyanogen. This relation has important bearings on the history of the cyanic series, and the nitric derivatives of the hydrocarbons[*].

The formulas of such related species as Gerhardt has designated chemical homologues, differ from each other by $nC^2 H^2$; if now the relation between C and O be what we have supposed, it may be expected that mineral species will exhibit the same relations as those of the carbon series, and the principle of homology be greatly extended in its application. Such is really the case, and the history of mineral species affords many instances of isomorphous silicates, whose formulas differ by $nO^2 M^2$, as the tourmalines, and the silicates of alumina and magnesia, while the latter, with many zeolites, exhibit a similar difference of $nO^2 H^2$. The relation is in fact that which exists between neutral and surbasic or hydrated salts.

Laurent has asserted that salts of the same base, with homologous acids of the type $(C^2 H^2) nO^4$, may be isomorphous when they differ by $O^2 H^2$, and has pointed out besides, several instances of what he has called hemi-morphism in species thus related, as well as in others differing by nCl^2. The observations of Pasteur and Nicklès have greatly extended the application of these cases, which assume a new importance in connexion with the views here brought forward, and demand further study[†].

But to return: we have seen that in gases and vapours the specific gravity of a species enables us to fix its equivalent, which is often a multiple by some whole number of that calculated from the results of ultimate analysis. As the equivalents of non-volatile species are generally assumed to be those quantities which sustain the simplest ratio to certain volatile ones, the real equivalent weight corresponding to four volumes of vapour, and consequently the theoretical vapour-density of such species, is liable to a degree of the same uncertainty as those deduced from ultimate analysis. Having, however, determined the true equivalent of a

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[*] See page 502 of my Introduction to Organic Chemistry, appended to Silliman's " First Principles of Chemistry," Philad. 1852 ; and the Amer. Journal for January, 1853, p. 151.

[†] See Laurent, *Comptes Rendus de l'Acad.*, t. xxvi. p. 353, and p. 257 of L. and Gerhardt's *Comptes Rendus des Travaux de Chimie* for 1848 ; also Pasteur, ibid., p. 165 ; and Nicklès, *Comptes Rendus des Travaux* for 1849, p. 347.

species from the density of its vapour, the inquiry arises whether a definite and constant relation may not be discovered between its vapour-density and the specific gravity of a species in the solid state. Such a relation being established, and the value of the condensation in passing from a gaseous to a solid state being known, the equivalents of solids, like those of vapours, might be determined from their specific gravities.

A connexion between equivalent weight and density is evident in some allied and isomorphous species. H. Kopp, in dividing the assumed equivalent weights of such bodies by their specific gravities, obtained quantities which were found to be equal for some of these related species. These numbers evidently represent the volumes of equivalents, and in accordance with the atomic hypothesis, are said to denote the *atomic volumes.* The inquiry of Kopp has been pursued by many investigators, among whom are Schrœder, Filhol, Playfair and Joule, and more recently, Dana. Their results show that the volumes thus calculated for related species of similar crystallization are generally identical, or sustain to each other some simple ratio ; while Mr. Dana, who has compared isomorphous species of unlike chemical constitution, finds that the calculated volumes are often to each other as the number of equivalents of elements in the formulas representing the species; thus leading to the conclusion that the real equivalent weight is either a mean of that of all the elements, or some multiple of it. The reason of this appears in the fact that the formulas of those species in which this relation is apparent, generally differ in the proportions of $Al^2 O^3$, SiO^3, MgO, CaO, &c., and the quantities obtained in dividing the equivalent weights of these by the number of elements are nearly equal. If we divide by the number of elements the equivalents calculated from the formulas of those species, it will be seen that the mean equivalents vary with the specific gravity.

These investigations have been principally confined to native and artificial mineral species, and the equivalents have been calculated from the formulas of Berzelius and Rammelsberg, which express the simplest ratios deducible from analysis. While in conformity with the dualistic notions, a mineral like calcite or magnesite was regarded as a compound of one equivalent of carbonic acid and one of lime or magnesia, dolomite was said to be composed of one equivalent of each of these carbonates, or of two to three, as the case might be, while its density was the mean of those of its constituents ; thus implying that this union, unlike that observed in gases, is juxtaposition, and not interpenetration. This system of formulas has introduced such difficulties into the study of the relations before us, that we find Mr. Dana led to the conclusion that "the elemental molecules are not com-

bined together or united with one another in a compound, but that under their mutual influence each is changed alike, and becomes a mean result of the molecular forces in action *."

The solution of these difficulties is very simple, and will have been inferred from the plan of our inquiry. It is found in the principle that *all species crystallizing in the same shape, have the same equivalent volume*; so that their equivalent weights, as in the case of vapours, are directly as their densities, and the equivalents of mineral species are as much more elevated than those of the carbon series, as their specific gravities are higher. The rhombohedral carbonates must be represented as salts having from twelve to eighteen equivalents of base, replaceable so as to give rise to a great number of species, and the variations in the volume of different carbonates, as observed by Kopp, indicate the existence of several homologous genera, which are isomorphous.

The researches of Playfair and Joule have led them to the conclusion that in some hydrated salts which crystallize with twenty and twenty-four equivalents of water, as the carbonate, the triphosphates and triarseniates of soda, the calculated volume coincides with that obtained by multiplying the volume of ice ($9 \cdot 8$ for HO with an equivalent weight of 9) by the number of equivalents of water. This result is thus explained; water in these salts is in the same state of condensation as in ice, and 24 HO thus condensed would occupy the volume of $24 \times 9 \cdot 8 = 235$, which is identical with that of the rhombic phosphate, as $20 \times 9 \cdot 8 = 198$ is with that of the carbonate of soda, $C^2 Na^2 O^6$, 20 HO. Alum, crystallizing with 24 HO, has a volume which is greater than that of phosphate of soda, and according to Playfair and Joule, equals that of the water in the state of ice, with the addition of the bases, the acid being excluded †. In reality, the equivalent volume of alum is to that of the rhombic phosphate as $270 : 235$, and 24 HO crystallizing in the monometric system, would have the same volume as alum with a specific gravity of about $\cdot 8$, giving for HO $11 \cdot 25$ instead of $9 \cdot 8$.

What are called the atomic volumes of crystallized species are the comparative volumes of their crystals. In the rhombohedral system, the length of the vertical axis being constant, the volume varies with the length of the lateral axes, or in other words, increases as the rhombohedron becomes obtuse, and diminishes as it becomes acute, the cube being the limit between the two. So in the dimetric and trimetric systems, the length of the vertical axis being unity, the volume diminishes as the base of the prism,

* American Journal of Science [2], vol. ix. p. 245.

† Chem. Soc. Quar. Journal, i. p. 139, cited in Liebig and Kopp's An. Rep., 1847–48, vol. i. p. 30.

the specific gravity increasing. Monoclinic and triclinic crystals may be calculated as if derivatives of the trimetric system, with which they will be found to correspond in volume.

It is now necessary to determine what equivalent corresponds to a given specific gravity in any crystalline solid, or in other words, what is the value of the condensation which takes place in the change from the gaseous to the solid state; and here a degree of uncertainty is met with, because the equivalent of a crystallized species may often be a multiple of that deduced from those chemical changes which only commence with the destruction of its crystalline individuality. The simplest formula deducible for alum is $KO\,SO^3$, $Al^2\,O^3\,3SO^3$, $24HO$, or $S^4\,Kal^3\,O^{16}$, $12H^2\,O^2$, and hydrogen being unity, its equivalent is at least $474\cdot6$, which with a specific gravity of $1\cdot75$, gives a volume of about 270. Again, grape-sugar is not less than $C^{24}\,H^{24}\,O^{24}$, if we regard its combination with common salt as corresponding to one equivalent of each, and the ferrocyanides in the same way are represented by C^{12}, &c.; there are reasons for believing that the equivalents of these species in the crystalline state correspond to some multiple of the above formulas, a question to be decided by an examination of the crystallization and specific gravity of species whose equivalents are admitted to be higher.

Favre and Silbermann, from their researches upon the heat evolved in fusion and solution, have been led to conclude, first, that crystallized salts are polymeric of these same salts in solution, *i. e.* are represented by formulas which are multiples of those deduced from analysis; secondly, that double salts and acid salts do not exist in solution, being produced only during crystallization; and thirdly, that water in crystallizing changes from HO to nHO, n being some whole number[*]. These conclusions are seen to be in accordance with those deduced from a consideration of the relations of density and equivalent volume; a polymerism is evident in such salts as sulphate of potash and cyanide of potassium, when their specific gravities are compared with those of alum and the ferrocyanide.

In the liquid state, the relation between specific gravity and equivalent is not so apparent in solid species. The condensation often varies greatly, even in allied and homologous species, but still exhibits a relation of volumes. The alcohols $C^2\,H^4\,O^2$, $C^4\,H^6\,O^2$, $C^{10}\,H^{12}\,O^2$, and $C^{16}\,H^{18}\,O^2$, have very nearly the same specific gravity, so that the condensation is inversely as their vapour-equivalents. The densities of wine-alcohol, acetic acid and aldehyd in the liquid state, vary as their equivalents, so that

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[*] *Comptes Rendus*, xxii. 823–1140, and xxiii. 199–411; cited in Liebig and Kopp's Annual Report for 1847–48, vol. i. p. 40.

the calculated volumes are 57·5, 55·5 and 55. Formic and valeric acid show a similar relation in density to their respective alcohols, their calculated volumes being to these as 37·3 : 39, and 108 : 106·7. If to these we add butyric acid, which gives a volume of 90, and the density of whose alcohol has not yet been determined, the liquid volumes for the four acids $C^2 H^2 O^4$, $C^4 H^4 O^4$, $C^8 H^8 O^4$ and $C^{10} H^{10} O^4$, are 37·3, 55·5, 90, and 108. These numbers approximate to multiples of the liquid volume of water $H^2 O^2$, which is 18; or taking this as unity, are very nearly as 2, 3, 5 and 6. The interval between 3 and 5 corresponds to propionic acid $C^6 H^6 O^4$, of whose specific gravity I find no recorded observation. The density of many of these liquids is not accurately known, and the results of different experimenters are not precisely accordant. The specific gravity at their boiling-points should probably be chosen for the purpose of comparison, and these approximations lead us to expect that future observations will establish a simple relation between the densities of liquids and their vapours.

In a succeeding paper, it is proposed to apply the principles explained in the present essay, in an examination of the equivalents of a number of minerals and other crystallized species.

LXXXIII. *Notices respecting New Books.*

An Elementary Treatise on the Lunar Theory, with a brief sketch of the History of the Problem up to the time of Newton. *By* Hugh Godfray, *B.A., of St. John's College, Cambridge.* Cambridge, Macmillan and Co.: London, G. Bell, &c.

THE author of this treatise was third wrangler a few years ago, and is well-known already as one of the most successful of the private tutors at Cambridge. The subject of his book is one which, as our mathematical readers are aware, is perhaps the most interesting and important of all the branches of mathematical physics; and as such has engaged the attention of nearly every great mathematician for centuries. By slow degrees the subject has attained a state approaching as near to perfection as possible, both as regards our knowledge of the lunar motions, and their theoretical explanation. But the *mode of presenting the subject to the student* has not kept pace with the progress of the science itself, nor indeed was this to be expected. Within the last twenty or thirty years, however, several attempts have been made to render the subject more easy of access to the student, especially in that university where it holds so conspicuous and prominent a place in the course of study and examination. The names of Woodhouse, Airy and Pratt, will at once occur to our academical readers as writers on the subject whose works have had a very great and deserved influence and estimation. Some of these however have only treated the *lunar* theory as a por-

tion of more extensive subjects, and have consequently presented it in a shape not so well adapted as it might be to the beginner. Of course no one can attempt even to commence such a study without a previous familiarity with all the other branches of mathematics, such as dynamics, differential calculus, &c., necessary to its comprehension, and a knowledge of these is of course assumed by all the writers on the lunar theory in a greater or less degree. But supposing this preliminary knowledge in the reader, there yet remain numerous difficulties peculiar to the lunar theory itself, and it is in treating of these that the author of the work before us has shown his aptitude and abilities as a *teacher*. To those who are not already familiar with the science, it would be impossible to state what are the peculiar merits of the work; to those who *are*, it will be sufficient to say, that there are *improvements* introduced here and there throughout the whole, and more especially in those parts where the *beginner* is most likely to make mistakes. In particular we would direct their attention to the sixth chapter on the "Physical Interpretation" of the various terms in the equations, and to the last chapter on "The History of the Problem before Newton," in both of which the author shows himself greatly superior to very many of the Cambridge mathematical book-makers (especially those of his own college) who slur over the *principles* and real foundations and difficulties of a subject, in order to hasten to the mere mechanical part, requiring only algebraical dexterity. In short, Mr. Godfray's 'Lunar Theory' is the work of a man who thoroughly understands the subject himself, and possesses more than the usual ability to convey his knowledge to others. As an elementary treatise and introduction to the subject, we think it may justly claim to supersede all former ones, and we have little doubt that as such it will soon be adopted as the text-book at Cambridge and elsewhere.

LXXXIV. *Intelligence and Miscellaneous Articles.*

ON A METHOD OF DISTINGUISHING THE VOLATILE OILS OF THE SERIES $C^5 H^4$. BY C. GREVILLE WILLIAMS.

To the Editors of the Philosophical Magazine and Journal.

Laboratory, Oxford Court,
April 25, 1853.

GENTLEMEN,

THE fact has long been notorious, that we possess no strictly chemical means of distinguishing the volatile oils of the series $C^5 H^4$ from each other. The taste, smell, and the action of polarized light have been hitherto the only distinctive character. Any facts tending to throw a light, however small, on this branch of the science, cannot, I think, be otherwise than acceptable.

Schönbein tells us that certain of the oils, when ozonized, have the power of converting the black sulphuret into the white sulphate of lead; but from some experiments which I had occasion to make some time back, I felt convinced that several of the oils possessed

this property in their normal state, or at least in the condition in which they are found in commerce; others appear destitute of this property, and it is upon these differences that I propose a means of distinguishing them.

My method of experimenting is to take slips of lead test-paper and hold them over a bottle of hydrosulphuret of ammonia until it acquires a uniform brown tint; I then let a drop of the oil to be tried fall upon the paper so prepared, and hold it over the plate of the furnace to quicken the process and evaporate the excess of oil. In this manner the following results were obtained:—

Oil of turpentine	. .	colour discharged.
mint	. . .	do.
lavender	. . .	do.
amber	. . .	do.
lemons	. . .	no change.
anise	. . .	do.
cassia	. . .	do.

Turpentine and oil of lemons offer, perhaps, the most marked contrast; the one instantly destroys the colour, the other produces a scarcely perceptible effect, and that only after frequent repetitions of the process.

We have thus in our power to detect with ease any adulteration of the latter oil with the former.

One or two per cent. may be detected thus, and with four per cent. the effect is almost as strongly marked as with the pure oil of turpentine.

When the quantity of the last-mentioned oil is very small in the sample to be examined, it is as well to repeat the addition and the subsequent heating two or three times.

I enclose a specimen of the effect of oil of lemon adulterated with two and four per cent. of turpentine; also a slip showing the action of the oils in the pure state, and am,

Gentlemen,
Your very obedient Servant,
C. Greville Williams.

NOTE ON INDUCTIVE ELECTRICAL MACHINES, AND ON A READY MEANS OF INCREASING THEIR EFFECT. BY M. FIZEAU.

The electrical machines which have been constructed of late years on the inductive principle are now well known; the constancy and regularity of their effects, as well as the facility of their employment, present marked advantages, which render these new machines preferable in some cases to those of the old construction.

Having undertaken some new experiments on the rapidity of the propagation of electricity, especially with the view of comparing in this respect electricity of tension with galvanic electricity, I found the employment of this apparatus very suitable for the purpose, but

nevertheless that it would be useful to give greater power to the instrument, and especially to be able to increase the tension of the electricity furnished by it.

A very sensible increase of effect is obtained by employing a stronger pile to set the apparatus in action, and the electricity developed at the two poles of the machine acquires thus a very marked increase of tension. But this increase is accompanied by an inconvenience which deprives the instrument of its principal advantage, which consists in the regularity and the duration of its effects. One of the essential parts of the machine is M. de la Rive's vibratory contact-breaker. When the instrument is in action, very brilliant sparks are produced between the surfaces of the breaker, and although these surfaces may be formed of platinum, they are soon fused and destroyed when the current is rendered more intense; the vibrations becoming less constant in consequence, the production of electricity soon loses its regularity.

The same inconvenience would no doubt be produced by giving the machine larger dimensions than those adopted by M. Ruhmkorff, for the force of the sparks which are produced at the point of vibration is due principally to the current induced in the conducting wire itself; and if the dimensions of the wires and the number of turns of the spire be increased, this current would of course become more intense and the sparks stronger.

But an attentive study of the peculiarities of the apparatus soon led to the discovery of an entirely different and very simple means of increasing the energy of the effects produced. Many experiments, which it would take too long to describe, tend to show that the current of induction which is produced in the inducting wire itself at the moment of the rupture of the circuit, exercises a considerable influence on the production of electricity in the induced wire which terminates the two poles of the machine. When this current is produced freely and takes a great development, the poles give but little electricity; but when, on the contrary, this current meets with obstacles, and only attains a slight development, the poles give much electricity, and the power of the machine becomes greater. Several arrangements served to prove this fact; I may mention the employment of metals more fusible than platinum on the surfaces of the breaker, and the union of the vibrating parts by fine wires of different lengths. This principle being admitted, it follows, that in order to increase the power of the machine, it is sufficient to oppose the development of the current which is produced in the inductor wire at the moment of the rupture of the circuit, and it is easy to see that this result must be obtained by acting upon the tension possessed by this current and rendering it weaker. In fact, when the machine is in action, the great light of the sparks which appear at the point of rupture, indicates that the current in question acquires a great development, and this is the case because the electricity possesses sufficient tension to pass with facility the space which separates the vibrating pieces: if the tension became weaker, the space to be passed presenting a constant resistance, the passage

would not take place with the same facility, the sparks would be less brilliant, and the current would acquire a smaller development.

A very efficacious mode of diminishing the tension is to have recourse to the well-known properties of the Leyden jar, and other apparatus founded on the same principle. A condenser is formed of two leaves of tin in juxtaposition, but separated and insulated from one another by a layer of varnish, and each of the leaves put into communication with each extremity of the inductive wire ; the points of attachment must be on both sides of the point of interruption when the sparks are produced. Then the two electricities, before arriving at the point of interruption, spread over the two surfaces of the tin, where they lose their tension to a great extent, in consequence of the mutual influence exercised across the isolating layer of varnish.

When the condenser presents a sufficient surface (5 or 6 square decimetres), the light is seen immediately to become weakened at the point of interruption, whilst the machine acquires a remarkable increase of energy ; the poles then give stronger sparks and at a greater distance than before. The condenser may be conveniently placed in a horizontal position, a little above the electro-magnet, and sustained by four glass supports. With this addition, which is very easily made, not only does the machine give more electricity, but it also continues longer in regular action, because the surfaces of the breaker are no longer exposed to the action of the very intense sparks which change them so rapidly.

An arrangement invented by M. Sinsteden, in which the principle of condensation has been employed to obtain stronger discharges with inductive machines, has only an apparent analogy with the method which I here indicate ; the principle and the effects of the two methods being, in reality, very different. In fact, it is the electricity developed in the second wire, the inductive wire, which is modified by M. Sinsteden, so as to cause more brilliant sparks ; but these stronger discharges are not accompanied by an augmentation of tension, which is, on the contrary, weakened. The employment of this method does not in any way injure the efficiency of that proposed by me, and they may both be employed together, where such a course is found advisable.

To furnish an idea of the increase of effect which I have obtained in my experiments, I will give the following observation. A galvanometer being placed in the circuit, the electricity produced by the machine was passed into rarefied air, when the beautiful phænomena of light recently studied by M. Quet were produced. When the machine acted under ordinary conditions, the needle of the galvanometer indicated a deviation of 8°. When the condenser was employed, the light produced acquired greater splendour, and the deviation of the needle reached 15°, the intensity of the current being consequently nearly doubled.—*Comptes Rendus*, March 7, 1853, p. 418.

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Fig. 4

Fig. 2

Fig. 1

Fig. 6

Fig. 5

Fig. 3.

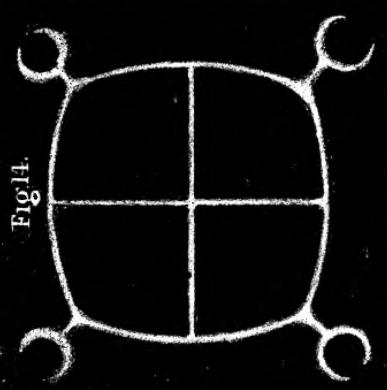A   B   C   D
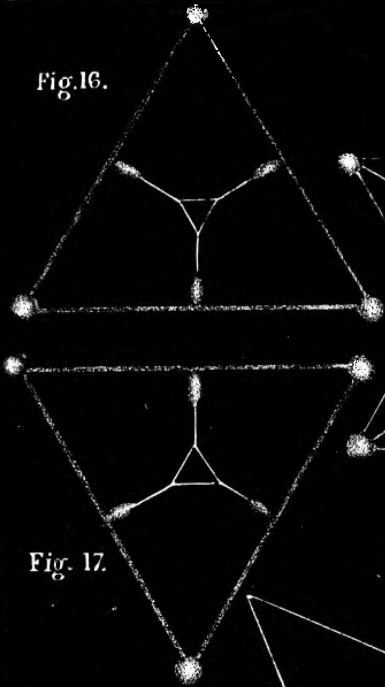
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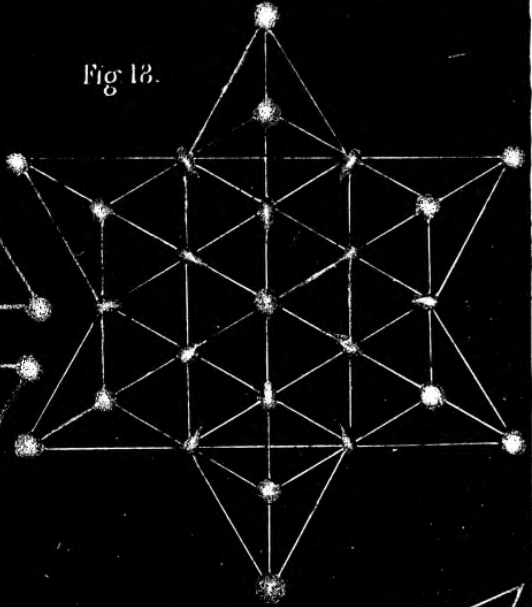
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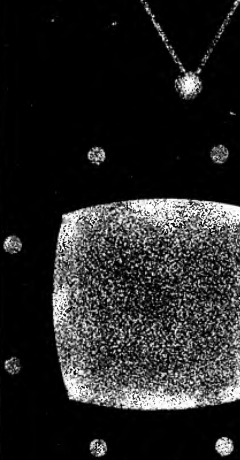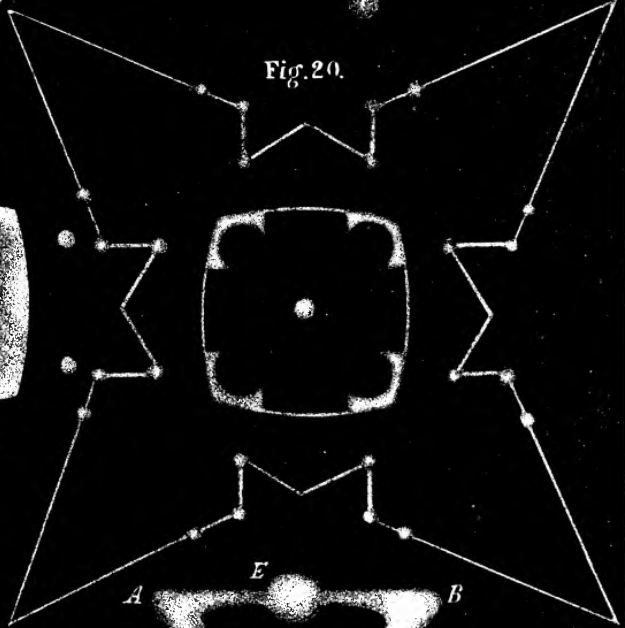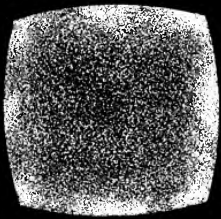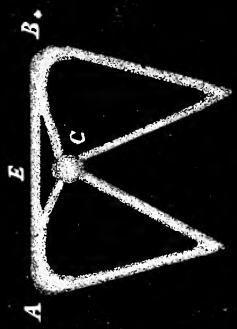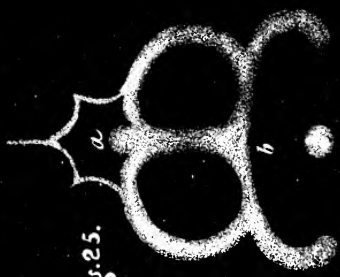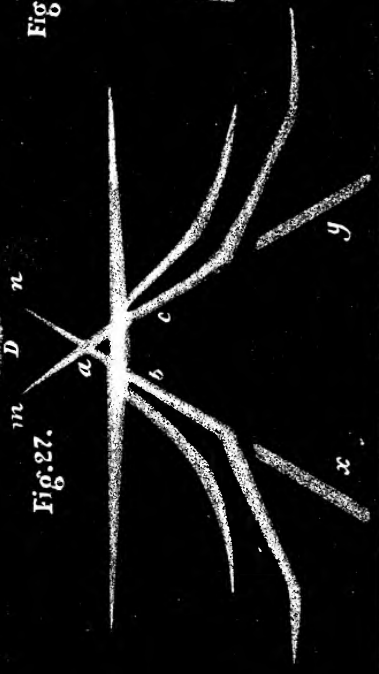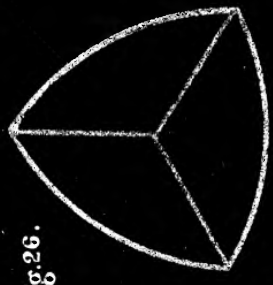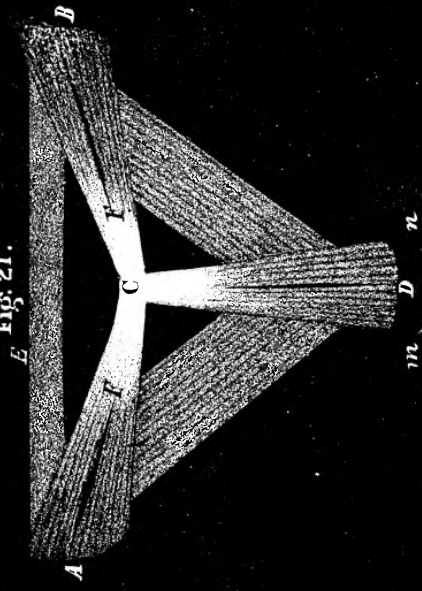
Fig. 20.

Fig. 19.

Fig. 22.
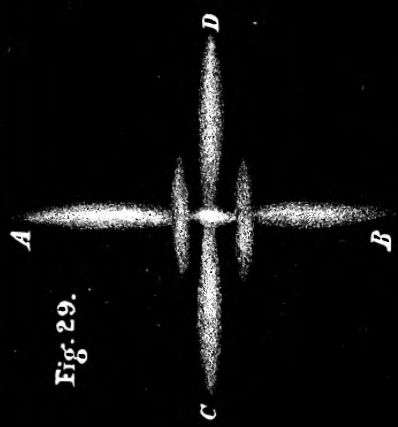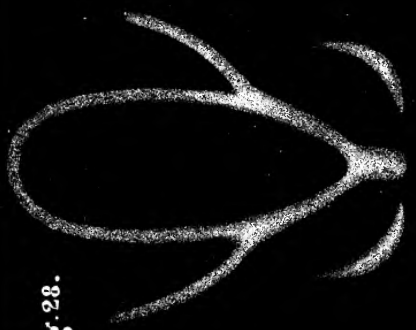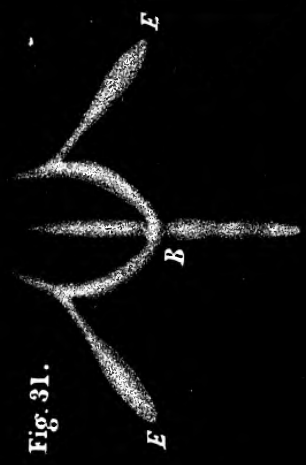
Fig. 24.

Fig. 26.

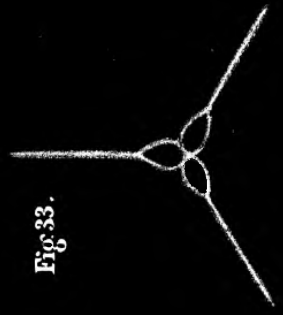Fig. 21.

Fig. 27.

Fig. 23.

Fig. 25.
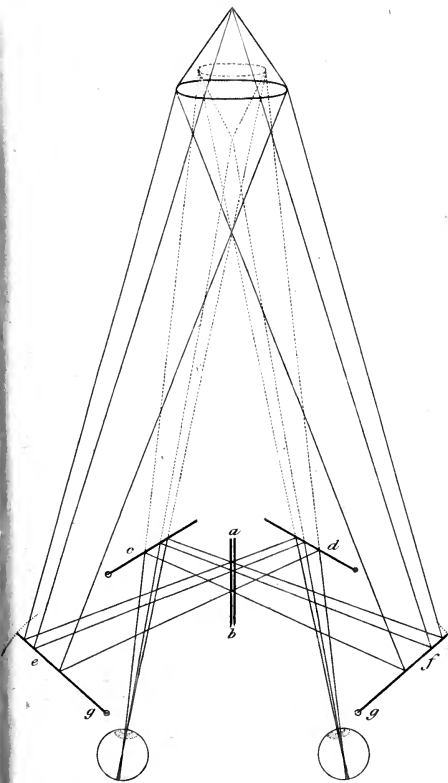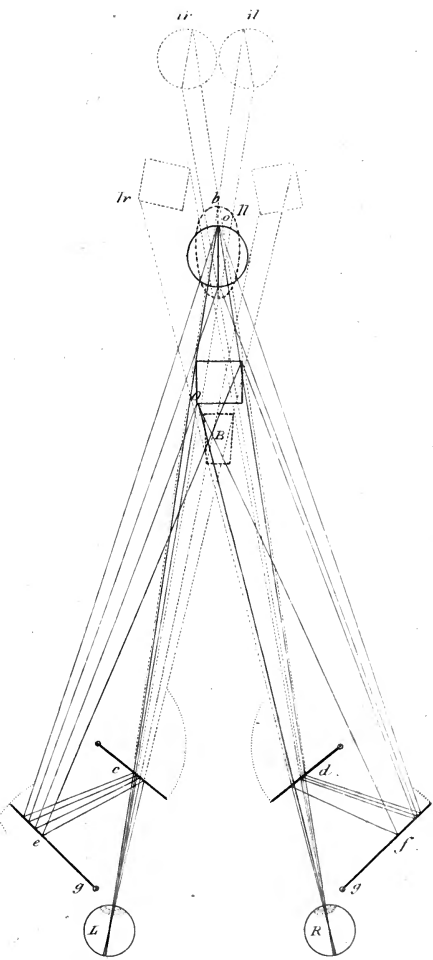
Fig. 29.

Fig. 28.

Fig. 31.

Fig. 30.

Fig. 32.

Fig. 33.

Fig. 1.

Fig. 2.