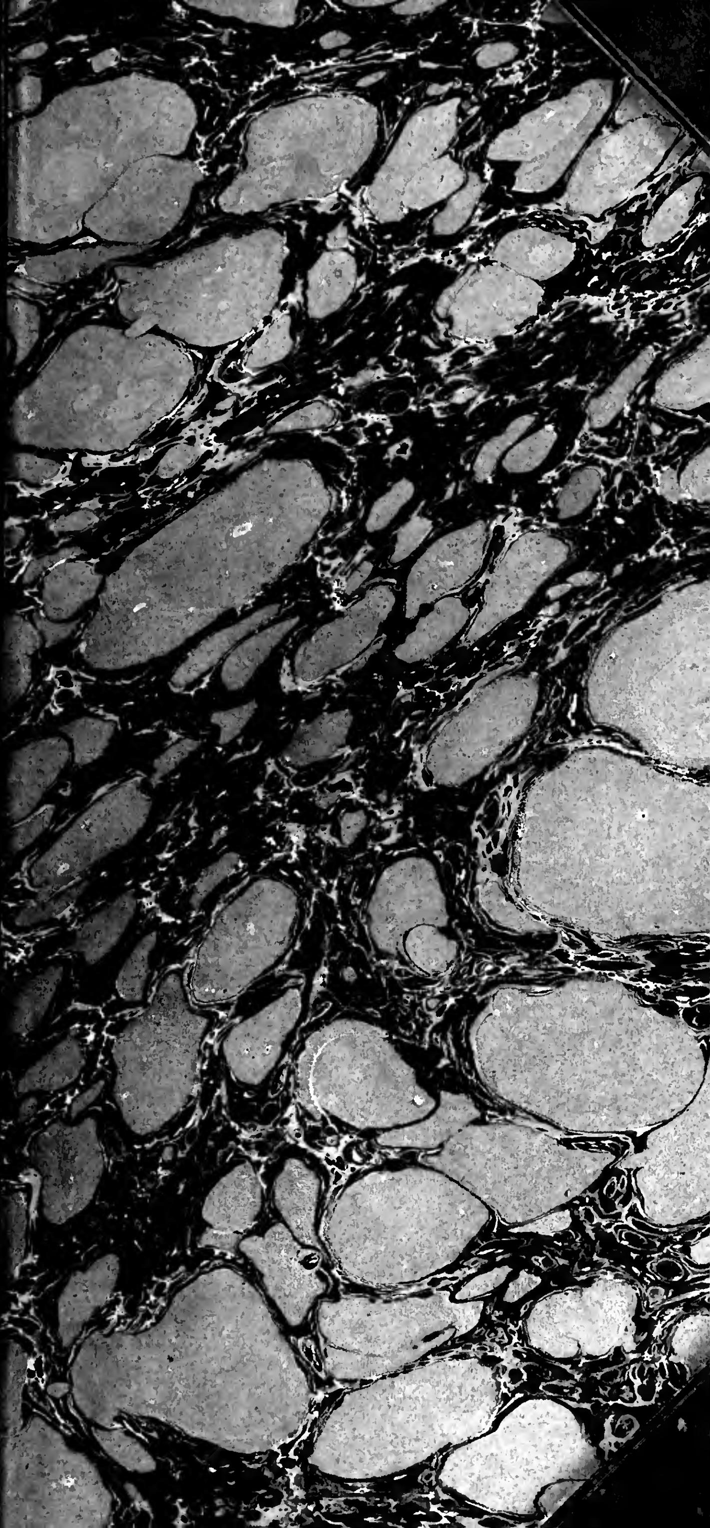


THE

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

CONDUCTED BY

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

"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. XXI.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE,
ANNALS OF PHILOSOPHY, AND JOURNAL OF SCIENCE.

JULY—DECEMBER, 1842.

LONDON:

RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;
SOLD BY LONGMAN, BROWN, GREEN, AND LONGMANS; CADELL; SIMPKIN,
MARSHALL AND CO.; S. HIGHLEY; WHITTAKER AND CO.; AND
SHERWOOD, GILBERT, AND PIPER, LONDON: — BY ADAM AND
CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH
AND SON, GLASGOW; HODGES AND SMITH, DUBLIN:
AND G. W. M. REYNOLDS, PARIS.

THE Conductors of the Philosophical Magazine have to acknowledge the editorial assistance rendered them by their friend Mr. EDWARD W. BRAYLEY, F.L.S., F.G.S., Assoc. Inst. C. E.; Member of the American Philosophical Society, and Corresponding Member of the Philosophical Society of Bâsle, &c. *Librarian to the London Institution.*

CONTENTS OF VOL. XXI.

NUMBER CXXXV.—JULY, 1842.

NUMBER CXXXVI.—AUGUST.

NUMBER CXXXVII.—SEPTEMBER.

NUMBER CXXXVIII.—OCTOBER.

NUMBER CXXXIX.—NOVEMBER.

NUMBER CXL.—DECEMBER.

NUMBER CXLI.—SUPPLEMENT TO VOL. XXI.

PLATE.

Errata.

Page 43, line 13 from the bottom, *for* $^{(t+\mathrm{T})^i}$ *read* $e^{(t+\mathrm{T})^i}$.

44, — 3, put sign ± before √.

55, — 7 from the bottom, *instead of* "M. Catalan," *read* "Professor MacCullagh."

176, line 13, *for* quina *read* cinchonia.

473, line 4 from the bottom, *for* Penim *read* Penine.

529, line 2 from the bottom, *for* node *read* note.

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1842.

I. *On the Influence of the Dew-point on Vegetables, considered especially with reference to their Temperature. By* D. P. Gardner, *M.D., Professor of the Physical Sciences, &c. in Hampden Sidney College, &c., Corresponding Member of the New York Lyceum of Natural History*[*].

THE object of this paper is to establish the mutual relation existing between the temperature of plants[†], their evaporation, and the amount of vapour existing in the atmosphere. The subject will be examined under four heads, which have been suggested by the results of the experiments instituted, and are therefore gradual developments of the proofs by which the connexion between the dew-point and temperature of plants is sought to be established.

1st. Certain vegetables are without any specific heat.

2ndly. The variations plus or minus the atmospheric temperature observable in plants are owing chiefly to the state of the dew-point, its elevation causing an increase of heat by checking evaporation, and its depression by favouring evaporation producing coldness; in other words, the rate of evaporation, and its effect in producing a decrease of temperature in plants, is directly as the greatness of the *drying power*, and inversely as its diminution.

3rdly. The sensible heat of plants is directly as the atmospheric temperature, and the chemical action going on in their cells; and inversely as the evaporation, radiation and conduc-

---

[*] Read before the Linnæan Society, November 16th, 1841, and now communicated at the request of the Author, by J. J. Bennett, Esq., Sec. L.S.

[†] On the subject of the heat of plants, see Meyen's Report for 1839, in the Annals and Magazine of Natural History, vol. viii. p. 27; also the original paper by Vrolyk and De Vriese, in the same work, vol. vii. p. 161 —Edit.

tion of the soil and surrounding air: to this we add, chemical action increases with atmospheric temperature, &c. &c., and consequently the amount of heat resulting therefrom.

4thly. A review of the foregoing doctrine, with some remarks on apparent anomalies.

§ 1. *That certain Vegetables are without any specific heat.*

A number of insulated measures of the temperature of flowers has hitherto been admitted into the books on vegetable physiology as the whole of our information on the subject of vegetable heat; and these measures have been received with distrust or altogether denied. M. de Lamarck observed an increase of temperature in the *spadix* of *Arum vulgare*, which M. Sennebier afterwards measured and found equal to 7° C. above the atmosphere. The German naturalist Schultz found a flower of *Calladium pinnatifidum* at 19° to 20° C. when the surrounding air was only 15° C. Messrs. Hubert and Bory measured the temperature of the *spadix* of *Arum cordifolium* in the Isle of France, and found it at sunrise 44° to 49° C.; the atmospheric temperature being only 19° C. M. de Saussure carried his experiments further, and with the differential thermometer ascertained an increase of $\frac{1}{2}$° C. in the male flowers of the melon and other Cucurbitaceæ.

Hypotheses have not been wanting to explain the reason why flowers should enjoy a more elevated temperature than the other parts of the plant. Mr. Murray imagined it was due to their colour. Brongniart ascribed it to the increased action of the molecules interested in the process of fecundation. Others have adopted the more plausible idea, that it depended upon increased chemical action, as the absorption of oxygen by the petals, &c. of the flower.

But Messrs. Treviranus, Goppert and Schübler, altogether deny that flowers give any indications of an increase of temperature. M. Aug. de Candolle ascribes this denial to the erroneous conclusions at which these botanists arrived from experimenting on imperfect plants; since his experience at Montpellier had led him to the same opinion as Saussure and others.

Placed in so embarrassing a situation, our only resource was to undertake a new series of experiments upon the subject; for although the mass of evidence appears to be in favour of the existence of a specific temperature in flowers, yet the measures given are too dissimilar to prove satisfactory, and the experiments appear to have been performed in too loose a manner to silence opposition. The mere introduction of a thermometer into a flower is a process undeserving any serious attention: the state of the atmosphere has been omitted, and in other respects the data are so imperfect, as to exclude the possibility of our repeating any of the experiments given under similar circumstances.

The instrument with which their measures have been made is altogether too bulky in such delicate researches; for although the bulb of a thermometer may be thrust into a pumpkin flower or tulip with *tolerable facility*, yet the contact of the circumambient air is not completely cut off by the shape of the flower; and if the fingers or any other contrivance be used as a means of closing the corolla upon the thermometer, the temperature of the new body complicates the result. Even when introduced with all care, a bulk of mercury or air of as many cubic lines as the flower has superficial measure, in either case an imperfect conductor, can only give a doubtful result. It is too large in most cases, and must be confined to experiments upon a few scattered flowers; nor can it in any instance be made use of to obtain a set of measures over the whole plant; most stems would be crushed in attempting to introduce it; and even if we succeeded so far, the measure obtained must be imperfect, from the injury inflicted upon the plant and the small amount of mercury or air in absolute contact.

These considerations have induced me to make use of a thermo-electric pair and the galvanometer as the most suitable thermoscope. The pair consists of a tinned iron and copper wire, each $\frac{1}{16}$th of an inch in diameter, soldered together at one extremity with tin for $\frac{1}{10}$th of an inch, and sharpened so as to enter with slight force any part of a plant; the wires used were about nine inches long, and were passed through a large bung, so that the fingers might not approach the junction, the cork serving as a non-conducting handle, and being sufficiently removed to hinder the possibility of producing a current of thermo-electricity by radiation from the hand. The galvanometer employed was the simple multiplier of Schweigger; the axis being suspended by a fibre of raw silk and bearing two needles perfectly *astatic*, and also at the lower end a parallelogram of tin-foil which was immersed in a vessel of water beneath the galvanometer; the object of this addition is to steady the vibrations of the needles, as shown by Dr. Draper (Phil. Journ.). The whole arrangement was covered by a glass bell-jar, having a graduated arc pasted on the inside at an appropriate height, which by moving the glass vessel can be brought to any place so as to arrange the *zero* point with great facility; the upper needle also bore a fine wire standing up at right angles from its extremity, which as the needle is deflected plays across the arc and tends to assist the admeasurement.

The thermo-electric pair and galvanometer can be made an extremely delicate differential thermometer; and from experiments already made by Drs. Forbes, Ritchie, Draper, &c., we are justified in stating that the degree of variation of the astatic needles is very uniform for equal increments of heat, in cases where the total amount of variation is as limited as in the following.

In obtaining the numbers of the tables, or the measures of temperature, the pointed extremity of the *pair* was thrust into the parts of the plant specified, care being taken to avoid contact by the fingers with either the plant or thermoscope; the numbers given are the mean of at least five measures made by forming and breaking the electric circle. The same *pair* and galvanometer were used throughout, and the value of a degree of the index equals two elevenths of a degree of Fahrenheit, or 1° F. = 5°·5 galvanometer. It is well to observe here that the whole of the junction of the thermo-electric pair must be introduced into the plant, otherwise the current of electricity does not circulate freely through the length of the wires, but passes round from the warm to the cold parts of the junction, forming a circle that does not include the galvanometer, and therefore producing no deflection of the needles.

The dew-point marked in the tables was taken immediately before and after each series of measures, and if any difference existed, the mean adopted.

The height of the thermometer is marked both at the time of the deposit of dew upon the exterior of a glass of iced water and its vanishing. The *drying power*, which is Dr. Dalton's expression for the difference between the dew-point and atmospheric temperature, is also marked in the tables; and it is well to remark, that that great philosopher has ascertained that the amount of evaporation is the same for all temperatures if the drying power be the same.

The experiments were performed in the shade, every disturbing cause, as currents of air, motion, &c. being avoided. The thermometer hanging at the side of the galvanometer, and the dew-point, &c., were all estimated at the same spot.

ARUM WALTERI (*foliis sagittatis*) was preferred for experiment; because it was in this genus Lamarck, Sennebier, &c. noticed the striking variations of temperature recorded in the commencement of this section; it moreover flourished in my immediate neighbourhood, and was of convenient size to establish a complete series of measures upon. The plants were dug from the marsh in which they grew, with several pounds of native soil around their bulbs, shortly after sun-rise, placed in a wooden box and carried at once to the place of destination about 200 yards distant; after having been left a sufficient time to allow the soil to radiate any excess of heat, or about two hours under any circumstances, the measures were commenced, and recorded at the time. Other examinations of the same group of plants took place however at different periods in the day, the plants being uninjured and vigorous.

It is necessary I should observe here, that all attempts made to examine plants *in situ* failed from various causes; the difference of temperature between parts exposed to the sun and those in the shade; the impossibility of managing the delicate thermoscope in the open air; the disturbing effects of currents, gusts of wind, &c.; nor does it appear to me at all necessary that such examinations should be made, even if the results could be depended upon. The measures derived from a vigorous plant removed under the foregoing circumstances are fully as trustworthy; and when the great deviations of the needles come to be considered, even the most sceptical will allow that the difference of situation would not have influenced the result beyond a few degrees; in which I may possibly be in error; but upon the general fact there cannot be any dispute.

So far the tables introduced may be regarded as exhibiting the measures made upon one species; but although it has not been considered necessary to tabulate the other results, yet a similar series of experiments were made on the undermentioned plants, as far as it was found practicable, but none offered the advantages possessed by *Arum*.

The examination of these plants gave the same general result, and they may therefore be dismissed, after simply stating that they corroborate in all respects the observations hereafter to be made on the subject of vegetable temperature, &c.

Symphytum officinale, *Pastinaca sativa*, *Cicuta maculata*, *Asclepias obtusifolia et syriaca*, *Arctium Lappa*, *Sagittaria sagittifolia*, *Rumex crispus*, *Lobelia cardinalis*, *Daucus Carota*, *Datura Stramonium*, *Delphinium consolidum*, *Cynoglossum officinale*, &c.

The botanist will recognise in this list, plants of sufficient bulk to allow of the introduction of the thermo-electric pair. They are also very frequently met, and were chosen partly from this cause, as well as from their proximity to the laboratory. The list could be elongated indefinitely if a smaller pair were used, but it is unnecessary to introduce other cases, as each observer can modify his apparatus as to the fineness of the elements according to his pleasure.

Lest the deviation of the needle of the galvanometer should be due to any other cause than a current produced by the temperature of the plants, several experiments were made to decide this point. The magnetic influence of the tinned iron, the action of vegetable acids, friction, radiation from the person or surrounding objects, were all examined, and it was found, that under the precautions adopted, all these disturbing causes were neutralized, so that *all the measures given are solely attributable to the presence of sensible heat in the plant.*

Where more than one measure is recorded, it was either made upon different parts of the same plant, or at different times upon different parts; in the latter case, the time which had elapsed between the measures is also recorded.

Table A.

June 8th, 1839. A vigorous group of *Arum Walteri* with well-developed spathæ, and several pounds of mud *in situ*.

Thermometer 66° Fahr. Dew-point 54°. Drying power 12°. Clear.

Parts of the plant examined.	Two hours after collection.	Three hours after collection.	
Ovarium	+14·8		All in degrees of galvanometer.
Male part of spadix	+14·8		
Fully developed leaf stem	0		
Stem (or rather collection of petioles) one inch below soil without adherent earth	− 7·15		Agreeing with the mercurial thermometer, or 3°·8 Fahr. below the atmospheric temperature.
Stem, six inches below soil, covered with adherent earth ...	−20·9	−20·9	
Temperature of soil	−20·9	......	

Table B.

June 11th. *Pastinaca sativa* in flower, with adherent soil. Thermometer 81° Fahr. Dew-point 66°. Drying power 15°. Clear.

Parts of the plant examined.	Galvanometer.		
Stem, near umbel with young seed	+ 8·	+8·	
Junction of flower stems	0	0	
Stem at 3 feet, 2½ feet and 1 foot above soil	0	0	
Stem six inches above ground...	− 1·4		Corresponding with a depression of a little more than 3·5° of Fahrenheit's thermometer.
Stem one inch above ground ...	−10·6		
Main root	−20		
Larger branches of root	−20		
Temperature of the soil	−20	......	

Table C.

June 12th. *Arum Walteri*, a fresh group, &c. Thermometer 86° Fahr. Dew-point 64°. Drying power 22°. Clear.

Parts of the plant examined.	Galvanometer.				
Male & female portions of spadix	0	0	0	0	
Scape one inch from spadix......	0	0	0	0	Agreeing with the thermometric temperature of 3°·6 Fahr. below the air.
Petioles	0	0	0	0	
Midribs of different leaves	+ 1	+1·2	+1·5	+1	
Collection of leaf-stems (stem)	0	0	0	0	
Stem three inches above soil ...	−1·4	−1·4			
Stem beneath soil	−20·5	−20·5			
Bulb	−20·5				
Soil	−20·5	...	...	...	

Table D.

June 7th. *Arum Walteri*, &c., three hours after collection. Thermometer 64° Fahr. Dew-point 51°. Drying power 13°. Fahr. Clear.

Parts of the plant examined.	Galvanometer.				
Spadix in vigorous action............ { male part...	+13	...	...	+ 8	
{ female part	+13·7	...	...	+11	
Petioles of various leaves	+ 8·8	+ 8·8	+ 7	+ 7	
Midribs of various leaves	+13	+12·5	+12		
Stem (collection of petioles two inches above soil ...... }	+ 2				Agreeing with the thermometric measure.
Stem one inch above soil	− 2·5				
Stem surrounded by soil	−14	− 14	−14	−14	
Temperature of soil	−14	...	...	...	

To these tables many others might be added, as they all tend to establish the same point. If we examine them solely to ascertain whether they afford any proof of the existence of a certain specific or vegetable heat, we are irresistibly led to acknowledge that the proof is against any such vital agent, and we deduce this,—.

1st. Because in the four tables the atmospheric temperatures quoted are 66°, 81°, 86° and 64° respectively, and yet the plant varies with each.

2d. We observe that the temperature of the soil is the same as that of the subterrene stem or root, and that the excess of temperature, if any such exist, is found in parts remote from the soil, and in which vital action is taking place. It is natural that the root should be of the same temperature as the earth, for along its vessels are passing the fluids derived from the soil; and the conducting power of the latter must tend to keep down the heat of the root, even when chemical action is taking place most actively in its structure.

We are therefore justified in asserting that *vegetables* (*so far as annuals and perennials*) *possess no specific heat similar to that belonging to mammals, &c., but that their temperature varies with the atmosphere within certain limits.*

§ 2. *That the variations plus or minus the atmospheric temperature are partly owing to the state of the dew-point, &c.* (*p.*1.)

It is well known that evaporation cannot take place from any surface unless the temperature and dew-point differ; for as a given bulk of air is only capable of retaining a certain amount of watery vapour in solution at a known temperature, it follows, that if the dew-point indicates that amount of saturation, all evaporation must cease so long as these conditions are maintained. It is also well known, that the heat produced by chemical and vital actions taking place in the highest animals is antagonized by evaporation from the skin and lungs, the tendency of which is to produce coldness. We have here therefore a source of heat and its opposite which likewise exists in plants, with this difference, that whilst the former power is considerably lessened, the latter is increased in consequence of the extensive surface from which evaporation takes place.

But the rapidity of evaporation is dependent upon several circumstances, as the amount of drying power, velocity of the wind, extent of surface, &c.; of these the first is the most important and easiest of examination. To show its influence, we introduce three other tables, selected as illustrating the influence of the amount of drying power most extensively.

Table E.

June 12th. *Arum Walteri*; soil extremely wet, and consequently adhering less firmly than in the previous cases. Thermometer 85°. Dew-point 60°. Drying power 25°. Clear.

Parts of the plant examined.	Galvanometer.				
Unopened spatha	−5	−5	−5	−5	
Spatha open and male portion	−5	−5	−3·6	−5	
spadix active female	−5	−5	−5	−5	
Male spadix giving off pollen	0	0	− ·2	0	
Unfolding leaf, stem	−2	0	− ·2		
... ... base of midribs	0	0			
Young expanded leaf-stem	−9				
... ... midrib	+2				
Expanded leaf, midrib	−7·5	−8·	−7·5		
Stem, or collection of petioles	−24	−24			
Stem at surface of ground	−27				
Stem three inches below soil	−30				With thermometer.
Temperature of the soil	−30	...	...	...	

In this table we are presented with an unusually high amount of drying power, the effect of which is to produce so rapid an evaporation, that the heat generated in the most active parts of the plant is neutralized. This group of plants, although very vigorous when examined, was drooping in six hours after from excessive evaporation.

Table F.

June 14th. *Arum Walteri*, with plenty of moist earth. Thermometer 86° Fahr. Dew-point 62°. Drying power 24°. Clouds rising.

Parts of the plant examined.	Galvanometer.		
Young spadix, male portion ...	—4		
... ... female...........	—4·5		
Expanded spatha male spadix	—5	—5·5	
female	—5	—5	
Young expanded leaf, stem......	—5	—6	
... ... midrib ...	—5	—5	
Fully expanded leaf, stem	—9	—9	
... ... midrib	—6	—6	
Main stem one inch above soil	—36		
... ... three inches below	—55		
Temperature of soil..............	—56	...	Agreeing with the thermometer.

Table G.

The same group as in Table E, again examined six hours after collection, about half an hour after the falling of rain. Plants very vigorous. Thermometer 75° Fahr. Dew-point 65°. Drying power 10°. Clearing.

Parts of the plant examined.	Galvanometer.			
Young spatha, male part.........	+8	+9		
... ... female............	+8	+7·8		
Expanded spatha, male	+8·5	+8·8	+10	
... ... female	+8	+9		
Young expanded leaf, midrib ...	+10			
... ... stem......	+ ·5	0		
Unfolded leaf, midrib	+12	+8		
Main stem, above ground.........	— 2			
... ... six inches below ...	—30			
Temperature of soil..............	—32	...	...	Agreeing with the thermometer.

In tables E, F and C of the previous section the drying power is extremely high, 22°, 24°, and 25° Fahr.; the effect according to hypothesis should be an exalted evaporation, and we find accordingly that all parts of the plant in these three tables exhibit a temperature below that of the atmosphere.

The tables G and A and D of section the first are of a different class; in these the drying power varies from 10° to 12° and 13°; being about half of the power in the above tables, and representing the air more saturated with watery vapour, and therefore less conducive to evaporation. In these tables we remark an uniform elevation of temperature in all the highly organized parts of the plant; notwithstanding the minus measures of the root from contact with a moist and therefore evaporating soil; a good illustration, *en passant*, of the non-conducting nature of living vegetable tissues.

Not to become diffuse, we perceive in these results,—

1st. An uniformity which recommends them to our reason.

2ndly. They are in conformity with the experience of mankind. The effects of moist air on vegetation is known to all, the rapid growth, the vigour of plants, or to speak more scientifically, the activity of the chemical and organic actions which maintain life are fully manifest. The result is an increment of temperature in exact proportion to the varying activity of each organ, whether in the respiration of the leaf or the generative functions of the parts appointed to the reproduction of the species.

The effects of a drought are no less apparent; the leaves hang down; there is an air of listlessness about plants very analogous to the effects of heat upon the human frame, and due to the undue evaporation.

How firm and succulent is the state of a leaf during moist weather; how exsiccated and flabby during a dry season! of this the tobacco planters in Virginia are so well aware, that they esteem moist foggy weather favourable when gathering their crop. It is somewhat curious that these remarks apply to the human family; the natives of moist countries, as the Netherlands, England, &c., being of fuller habit than those who live in arid regions; this similitude does not however extend so far as in plants, from the effects of the diseases prevalent in swampy countries. It gives me great pleasure here to recommend the paper of Mr. Hopkins in the London and Edinburgh Philosophical Magazine for February 1839, on Malaria, in which he examines the influence of the hygrometrical state of the air upon animal life.

At this stage of the investigation it is necessary to meet an objection already urged against the foregoing doctrine, that it levels the principle of life in vegetables to mere chemical action. We do not hold any such view. We simply claim that the sensible caloric generated by plants is the result of internal action; the amount of caloric is also more or less, according to the activity of the evaporation, the influence of high temperature radiation, and conduction of the soil. The organic molecule of plants is not a mere compound atom, for it is beyond the art of the chemist to create it synthetically.

But, further, to meet objections of this kind, and convince ourselves of the influence exercised by evaporation upon the temperature of vegetable substances, we resolved to have recourse to experimental proof of a direct nature. For this purpose an experiment made by Dr. Hales (Statical Essays, exp. 30) upwards of a century ago, was repeated with such modifications as to suit our purpose.

A green apple, about 1½ inch in diameter with a cluster of leaves, was plucked from the tree; and the stem introduced through a cork into a glass tube filled with water, to the lower end of which a smaller tube was cemented, the extremity passing downwards into a cistern of decoction of logwood; the apparatus being supported in the vertical position by a retort-stand, as represented in the sketch; and being found air-tight, the following experiments were made. The temperature of the apple was estimated at given intervals with the thermo-electric pair, at the same time the drying power and elevation of the coloured fluid in the smaller tube was examined, and the measures tabulated for the purpose of examining the connexion of these phænomena at a *coup d'œil.* A further experiment was then made by covering the apple and its leaves with a delicate caoutchouc bag, so as to arrest evaporation, and after a given interval examining the temperature of the fruit and elevation of the coloured fluid. These experiments were repeated many times, but it is unnecessary to adduce more than two series in this place.

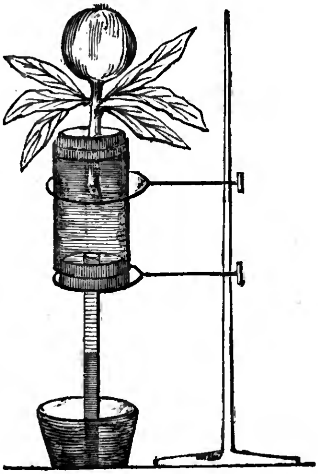


Table H.

June 14th. An apple with twelve leaves, examined immediately after collection at 1h 45′ p.m.

Examined at intervals of	Height risen in interval.	Galvanometer.	Temp. by Therm.	Dew-point.	Drying power.	State of the Atmosphere.
15′	$\frac{8}{10}$ inch.	0	84 F.	64	20	Cloudy.
18′	$\frac{9}{10}$	+ ·5	84	65	19	Very cloudy.
15′	$\frac{55}{100}$	+3·6	80	63	17	Thunder, &c.
15′	$\frac{5}{10}$	+5·5	76	63	13	Rain storm.

After a delay of 12′ the caoutchouc bag was used and tied tightly around the stem, and after 17′ the bag was pierced by the electric pair, the results being,—

Examined at intervals of	Height risen in interval.	Galvanometer.	Temp. by Therm.	Dew-point.	Drying power.	State of the Atmosphere.
17′	$\frac{6}{10}$	+13·75	80	67	13	Clear.

Beyond this period it is impossible to examine the gauge, for the included stem begins to give off gas into the water, and therefore partially arrests the ascent of the coloured fluid.

Table I.

June 15th. Experiment as before, time of collection 9ʰ 35′.

Examined at intervals of	Height risen in interval.	Galvanometer.	Temp. by Therm.	Dew-point.	Drying power.	State of the Atmosphere.
9ʰ 35′	0	—3·0	73	53	20	Fair.
20′	1·$\frac{10}{100}$ inch.	—2·5	72	55	17	Cloudy.
35′	1·$\frac{6}{10}$	—1·6	72	57	15	Cloudy.

The fruit and leaves were entirely covered with the caoutchouc at 10ʰ 40′, and pierced after 35′ delay.

35′	$\frac{7}{10}$	+15·0	74	59	15	Cloudy.

The coldness of the fruit in the three first measures of the table I. was due to the presence of a little external moisture, and the greater temperature of the room than the external air.

In both these tables the effect of arresting the evaporation is extremely apparent by an elevation of $8\frac{1}{4}$° and 16°·6 respectively; it is to be observed, however, that the drying power given in the two additional tables represent the external and not internal measure; the saturation within the caoutchouc-bag being probably greater. In the table H. there is another coincidence worthy of remark, the gauge marks a decreasing power of suction on the part of the apple as its temperature increases and the evaporation decreases, showing a compensation between the amount of perspiration of the leaves and fruit and the supply of fluid.

Without detaining the reader, it appears that the foregoing tables prove,—

1st. *That the temperature varies with the drying power.*

2ndly. *That the amount of evaporation and its effects in pro-*

ducing coldness is directly as the greatness of the drying power, and inversely as the approximation of the dew-point to the atmospheric temperature.

§ 3. *The sensible heat of plants is directly as the atmospheric temperature and the chemical action going on in their cells, and inversely as the radiation, evaporation and conduction together, &c.* (*p.* 1.)

We have introduced this postulate rather to give completeness to the subject than to enter into any lengthened examination. That it is true, can be readily shown by a few references to the foregoing tables; the proofs drawn may be conveniently ranged under three heads:—

1st. The temperature of plants varies nearly with the atmosphere, the greatest difference measured being about 5° Fahrenheit.

2ndly. The parts in which the greatest exhibitions of temperature above the air have been witnessed are the seat of active chemical and organic action, as the ovaries, male spadix, midrib of leaves, &c., the stem being seldom above or below the external temperature.

3rdly. Roots and subterrene stems are of the same temperature as the soil, and generally below the atmosphere, in consequence of evaporation taking place from the earth. This diminished temperature in the plant must depend partly upon conduction. That vegetables also lose heat by radiation, is shown by the copious deposit of dew seen upon their leaves after a clear chilly night.

§ 4. *A review of the subject, with some remarks on apparent anomalies.*

Since the preceding experiments were made there has been published in the *Journal de Chimie*, an article on vegetable heat by M. Dutrochet[*]. He inclosed a dead and living plant in an atmosphere saturated with moisture, and examined their temperature with Breschet's physiological pair. The result of his experiments brought him to the conclusion, that living plants possessed a temperature that exceeded the atmospheric temperature by one-third centigrade as a maximum. Van Beck has since repeated the experiments of M. Dutrochet and arrived at an opposite conclusion, viz. that the living plant betrayed two-thirds centigrade as a maximum below the dead plant.

Independently of the discordance in these measures, we cannot understand how a plant can be said to possess a spe-

---

[*] The author did not see the original paper, but an extract in the Edinburgh Philosophical Journal of Professor Jameson, 1840.

cific temperature that varies within one-third plus or minus the atmospheric temperature, which may be 90° Fahr. at noon, and 40° in the evening. The real cause of the elevation or depression measured, is to be found in the more or less perfect saturation of the atmosphere in which the experiments were conducted. There is, however, a great difference between the amount of heat measured by M. Dutrochet and myself; but whatever may be the cause of the discrepancy, the measures given in the tables are certainly free from error, since most of them were authenticated by the simultaneous examination of my friends at Hampden Sidney College.

We are much more concerned by the apparent anomalies exhibited by Nature. Why are not all plants destroyed by frost? Why do not tubers, bulbs, &c. perish during winter? For if there be no specific heat in these organized substances, their fluids should freeze and thereby produce disorganization. In reply to this we remark, that the fluids of vegetables congeal at temperatures below the freezing point of water in consequence of the presence of mucilage and acids, &c. Again, the degree of succulence of the plant and strength of the tissues, as well as their non-conducting nature, must not be lost sight of. It is remarkable that all northern evergreens have more or less coriaceous leaves. The vegetation of countries invaded by cold is hardier than that found in the tropics; in the former localities the majority of plants are annuals or perennials, or trees which cast their leaves; whilst in the south evergreen trees abound which are incapable of enduring exposure to one frost. Our trees are often found with their sap frozen without the texture being destroyed; and in the *Annales de Chem. et de Phys.*, tom. xv. p. 84, there is an account of a parcel of young trees which were kept in a frozen state for twenty-one months and yet finally vegetated when gradually thawed and planted out, showing conclusively that the woody fibre resisted the disruptive force of the expanding water when in the act of freezing. The non-conducting nature of the bark and wood is another powerful protection; we witnessed a poplar tree cut down in the depth of winter; on the northern side of the trunk the wood was quite dry and the sap probably frozen, whilst on the southern exposure the sap was fluid: this fact proves the necessity of paying every attention to the exposure of trees which are transplanted in the winter, especially evergreens.

Many roots, tubers, bulbs, &c. may be exposed with apparent impunity during winter, but if we examine the conditions necessary to secure them, it is found that they must be either covered with soil or are naturally of a dry and amylaceous nature. The protective power of a slight covering of soil or vegetable matter is extraordinary; some potatoes were covered with about two inches of earth and others left exposed on the surface of the ground at the same spot of the garden in November; a frost occurred at night, the thermometer sinking to 28° Fahr., and it was found that all the uncovered potatoes were frozen, their cellular tissue being broken up; whereas the buried specimens were entirely free from the action of the cold. The temperature of springs is worthy of notice as a proof of the non-conducting nature of the earth, whereby it is well calculated to preserve organic structures from the effects of frost.

These conjectures are advanced not as satisfactory arguments against the apparent objections detailed, but only as throwing out hints for future researches. These objections do not invalidate our measures, for they are demonstrable. The deductions may be in error, but we are content to offer the experiments as a contribution to the science of botany.

D. P. G.

II. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from vol. xx. p. 225.]

On the Oils of Fennel, Anise, and Star-anise (Illicium anisatum).

M. CAHOURS has examined the stearopten of these three oils, and has found them to be perfectly identical; the substance used for the experiments was generally made from the oil of anise, because from this oil it can be obtained in larger quantities than from either of the others. The solid oil can be very easily obtained pure by expression and crystallization in alcohol. It crystallizes in white shining leaves. Its specific gravity is nearly equal to that of water. It is pulverisable at 0°, melts at 18° C., and boils at 222°. On being converted into vapour it appears to suffer some change, so that the observed density of the vapour does not agree with that calculated from the formula. In a solid state it is not changed by exposure to the air, but if kept fluid for a length of time it is converted into a resin; chlorine and bromine act violently on it; alkalies have no action except when employed in the manner proposed by Dumas and Stass, in which case an acid product is obtained. Strong acids, as the sulphuric, phosphoric acids, &c., change it into an isomeric body. The atomic weight of the solid anise oil was determined by measuring the quantity of hydrochloric acid absorbed by it. The formula is $C^{20} H^{24} O^2$.

Bromide of anisal (*Bromanisal*).—Bromine acts violently on the solid oil, hydrobromic acid is evolved; on allowing the fluid mass to stand for some time it partly solidifies; small portions of æther extract an oil which contains bromine, and the solid substance may be purified by solution in boiling æther and pressing between bibulous paper. It is colourless, forms voluminous crystals, insoluble in water, somewhat soluble in alcohol, and more so in æther. It is decomposed at a temperature above 100°. Formula is $C^{20} H^{18} Br^{6} O^{2}$.

The action of chlorine is more complex; according to the length of time the chlorine has acted different products are formed, none of which crystallize, and whose purity therefore cannot be relied on. Once a substance was obtained with the formula $C^{20} H^{18} Cl^{6} O^{2}$. The next product is $C^{20} H^{15} Cl^{9} O^{2}$. Both bodies are decomposed by distillation.

Sulphuric and phosphoric acids and some anhydrous chlorides, as those of tin and antimony, convert the solid oil into a white crystalline substance, soluble in sulphuric acid with a red colour; it has exactly the same composition as the solid oil, viz. $C^{20} H^{24} O^{2}$; Cahours calls it Anisoïn.

By the action of nitric acid of 23–24° Beaumé a new crystallizable acid is obtained, which has been mentioned in one of our former reports.

Anisic acid.—The rough impure acid may be dissolved in ammonia, the salt recrystallized several times, and from the insoluble lead salt the pure acid may be obtained. The acid crystallizes in long needles, sparingly soluble in cold water, but much more so in boiling water; easily soluble in alcohol and æther.

It can be volatilized without decomposition, and forms soluble salts with the alkalies and earths. The lead and silver salts are soluble in hot water. The acid precipitates sesquioxide of iron, like benzoic and cinnamic acids. Formula is $C^{16} H^{14} O^{6}$. The æther may be prepared by passing hydrochloric acid into an alcoholic solution of anisic acid.

By heating anisic acid with an excess of baryta a fluid substance, *anisol*, is obtained similar to Mitscherlich's benzin, inasmuch as it seems to form analogous compounds; it differs, however, in so far that it contains oxygen, its formula being $C^{14} H^{14} O^{2}$.

Note.—[The confusion in chemical nomenclature seems nearly to have reached a climax. Berzelius has proposed some excellent rules for the terminations of names, but they have unfortunately been but little attended to. Mitscherlich's discovery of benzin paved the way to that of many similar substances. He called this substance, $C^{12} H^{12}$ *benzin.* Liebig calls it *benzol.* A similar substance obtained by Gerhardt and Cahours from cinnamic acid, $C^{16}H^{16}$, is called *cinnamen,* that from cuminic acid, *cumen.* Simoux and Marchand call *cinnamen* cinnamomin. Cahours calls the above substance anisol, it being prepared exactly like benzol (benzin).—H. C.]

Anisonitric acid is formed by boiling the anise oil with strong nitric acid until the oily substance first produced is redissolved. The acid solution is precipitated by water and the substance well washed, dissolved in ammonia and its salt crystallized several times, out of it the pure acid may be obtained. It is yellowish white, not very soluble even in warm water, and crystallizes out of its hot solution in small acicular crystals, tolerably soluble in alcohol. Forms insoluble salts with lead and silver. It cannot be sublimed unchanged. Formula for the free acid $C^{16}H^{12}N^{2}O^{10}$; one atom of water is driven out when it is combined with oxide of silver, the formula of that salt being $C^{16}H^{10}N^{2}O^{9}, Ag\,O$ (the crystallized æther of this acid has been noticed by Mitscherlich). By the action of fuming nitric acid on the solid anise oil a resinous substance is obtained, *nitranisid*; its probable formula is $C^{20}H^{20}N^{4}O^{10}$ (?). Treated with caustic alkalies it evolves ammonia and is converted into *melasinic acid.* Oil of bitter fennel (fenonilamer) appears to consist of two oils, one solid having the same composition as that of anise-oil, and a volatile one having the same constitution as the oil of lemon and turpentine.

If a stream of binoxide of nitrogen be passed into this latter oil, it becomes thick and opake, and alcohol of 0·80 causes a white silky precipitate which must be washed with alcohol. By a gentle heat this substance becomes yellow and is easily decomposed. Somewhat soluble in absolute alcohol, more so in æther, soluble in concentrated solution of alkali, and is precipitated again by acids. Formula $C^{15}H^{24}N^{4}O^{4}$.—(*Annales de Chem. et de Phys.*, Juillet 1841, p. 274.)

Action of Chromic Acid on several Volatile Oils.

Persoz has examined the products obtained by treating æthereal oils with a mixture of bichromate of potash, sulphuric acid and water. From the oils of anise, star-anise (anise étoilée) and fennel, acetic acid and an insoluble product consisting of two acids, were produced. These acids Persoz calls Umbellic and Badianic acids. The umbellic acid is little soluble in cold water, more in hot, soluble in alcohol, very little in æther; and can thus be separated from badianic acid; with concentrated nitric acid it forms an acid similar to the cinnamonitric. In its salts it resembles the benzoates. It melts at 175° or 180° C., boils at 275° to 280°. [This umbellic acid seems to differ from the anisic solely in being insoluble in æther; it would be worth while to examine this point further. Both acids are formed equally well out of all three oils.—H. C.]

Badianic acid is more soluble in water.

By the action of chromic acid on Roman carraway oil, Persoz obtained two acids, *cyminic* and *cumino-cyminic.* The former melts at 115°; it is tasteless, little soluble in cold water, easily in alcohol and æther. The latter is insoluble in all three liquids. It is not decomposed by boiling with strong sulphuric acid. From some other oils new acids have been obtained, but as both they and those above have not yet been fully described, it will be better to defer any further report upon them for the present. Oil of cinnamon gives acetic and benzoic acids, and according to Marchand a considerable quantity of hydruret of benzoyl. It must be remarked that in these reactions *acetic acid* is always formed.—(*Comptes Rendus*, tom. xiii. No. 8. p. 433.)

Action of Hydrate of Potassa on Hydrobenzamid.

Rochleder finds the formula for hydrobenzamid to be $C^{21} H^{18} N^{2}$; when fused with the hydrate it becomes yellow, at last black, and ammonia is evolved; the residual mass is washed with water. The washed powder is yellow, fusible at a gentle heat, decomposed at a higher temperature, partly soluble in alcohol and æther: it consists of three bodies; the first is found in small quantities at the commencement of the operation; it is a yellow oil soluble in alcohol, but has not been further examined: the second is soluble in alcohol, white and crystalline; the author calls it benzostilbin: the third, benzolon, is also white and crystalline, but insoluble in alcohol; it is formed during the latter part of the operation.

Benzostilbin when freed from oil is not very soluble in alcohol, soluble in æther, by means of which it can be obtained in large crystals. Melts at 244·5° C., and at a higher temperature sublimes, but not unchanged; soluble in concentrated sulphuric acid with blood-red colour. Not decomposed by boiling with caustic potassa. Formula $C^{31} H^{22} O^{2}$.

Benzolon is purified by solution in warm sulphuric acid and precipitation out of the red solution by alcohol. It is crystalline, insoluble in water and alcohol, melts at 248° C., sublimes almost unchanged. Decomposed by fuming nitric acid. Formula $C^{11} H^{8} O^{1}$, or benzon minus benzin (benzol.) —(*Ann. der Chem. und Pharm.*, vol. xli. p. 89.)

On the Salts of Uvic (Racemic) Acid.

Uvic acid is monobasic according to Fresenius, and this is its principal point of difference from tartaric acid. In the crystallized state it contains two atoms of water; only one can be driven out by heat, the other is basic. The neutral salts of the alkalies are easily soluble and crystalline, form acid salts, but the fixed alkalies do not form together double salts. The salts of the alkaline earths are difficultly soluble, form no double salts, but this is found to be the case with those salts of the magnesian series which contain halhydrate water.

Ammonia salt ...	$\overline{U}v + N^2 H^8 O$.	
Acid salt	$\overline{U}v . N^2 H^8 O + \overline{U}v . H^2 O$.	
Potassa salt......	$\overline{U}v\ KO + 2\ aq$.	Compounds similar to the Tartarus boraxalus and the Tart. bor. of the French may be obtained.
Acid salt	$\overline{U}v\ KO + \overline{U}v . H^2 O$.	
Soda salt	$\overline{U}v\ Na\ O$.	
Acid salt	$\overline{U}v\ Na\ O + \overline{U}v\ H^2 O + 2\ aq$.	
Uvate soda ...... + Uvate ammonia	$\overline{U}v\ Na O + \overline{U}v . N^2 H^8 O + 2\ aq$.	
Baryta salt	$\overline{U}v\ \dot{Ba}\ O + 2\frac{1}{2}\ aq$.	
Strontia salt ...	$\overline{U}v\ \dot{Sr}\ O + 4\ aq$.	
Lime salt.........	$\overline{U}v\ Ca\ O + 4\ aq$.	
Magnesia salt ...	$\overline{U}v\ \dot{Mg}\ O + 5\ aq$.	
Manganese salt	$\overline{U}v\ \dot{Mn}\ O + aq$.	
Nickel salt	$\overline{U}v\ Ni\ O + 5\ aq$.	
Copper salt	$\overline{U}v\ Cu\ O + 2\ aq$.	

&c. &c. &c.—(*Ann. der Chem. und Pharm.*, vol. xli.)

Nicotin.

Ortigosa gives the following statements with regard to nicotin. It is colourless, transparent, smells disagreeably of tobacco, distils perfectly at 100° C., and generally leaves a resin behind which is soluble in alcohol. With a small quantity of water it gives a clear solution, which is rendered opake by the addition of more water. Soluble in alcohol and æther, the solutions react alkaline.

Its neutral solution in hydrochloric acid gives a crystalline precipitate with bichloride of platinum. Formula $C^{10} H^{18} N^2 Cl^6 Pt$, consequently pure nicotin is $C^{10} H^{16} N^2$. Nicotin also combines with chloride of mercury; the compound is formed by mixing the solutions. The white crystalline precipitate is insoluble in water and æther, difficultly in alcohol, melts under 100° C., and becomes yellowish. Composition $C^{10} H^{16} N^2 + Hg\ Cl^2$.—(*Ann. Chem. u. Pharm.*, xli.)

On a new Acid of Sulphur.

Langlois some time since published a method for obtaining hyposulphurous acid. He prepared the potassa salt by digesting bisulphite of potassa with sulphur, and precipitating the alkali by means of hyperchloric acid. The acid thus separated is, however, not hyposulphurous but a new acid. A concentrated solution of the bisulphite is digested with flowers of sulphur, but without allowing the mixture to boil; sulphurous acid is evolved, sulphuric acid generated, and the solution acquires a yellow colour, which, however, soon vanishes. The crystals which separate on cooling are dissolved in a very small quantity of water and purified. The salt so obtained is not decomposed by hydrochloric acid, is not changed by exposure to the air, and when heated leaves neutral sulphate of potassa: 100 parts of the salt gave 23·76 sulphurous acid, 11·88 sulphur, and 64·36 sulphate of potassa. The constitution of the acid is therefore $3\,S + 5\,O$. Langlois calls it "acide hyposulfurique sulfuré" (Sulpho-hyposulphurous acid).

The new salt crystallizes in four-sided prisms with two terminal faces, tastes somewhat saline and bitter, is very soluble in water, insoluble in alcohol; the solution is decomposed by sulphuric and nitric acids when heat is employed, sulphur and sulphurous or nitrous acids are produced. Hydrochloric, chloric and iodic acid are without action. Hyperchloric acid isolates the new acid. It does not precipitate the salts of lime, strontia, baryta, magnesia, alumina, iron, zinc, nickel, cobalt, uranium, copper and lead; from the salts of dinoxide of mercury it precipitates the sulphuret, with salts of the oxide it gives a white precipitate of sulphate of the dinoxide; it precipitates silver salts yellowish-white, which soon changes to black.

The free acid possesses almost all the characters of hyposulphurous acid; it is rapidly decomposed by chloric and iodic acid.—(*Ann. der Chem. und Pharm.*, xl. p. 102–110.)

On some double Hyposulphites.

Lenz prepared these salts by means of the hyposulphite of soda, for the preparation of which Liebig proposes the following method. A solution of sulphurous acid or acid sulphite of soda is saturated with carbonate of soda, and a saturated solution of sulphur in caustic soda added until a tinge of colour shows that there is some sulphuret of sodium undecomposed. The filtered solution is evaporated, &c.

The hyposulphite of soda forms two double salts with oxide of silver. They may be obtained by means of either the chloride or the nitrate of silver; chloride of silver is added to a saturated solution of the hyposulphite until the solution begins to be opake, it is then filtered and precipitated by alcohol. The precipitate, which is the one double salt soluble in water, is thus obtained pure in shining scales, it is edulcorated with alcohol and dried *in vacuo*. The second salt is difficult to obtain pure by this method. In the second manner it may be obtained by adding to a solution of the hyposulphite perfectly neutral nitrate of silver until the precipitate becomes constant. The salt is at first flocculent, it afterwards becomes crystalline, and must be washed out with water.

First salt.—Has a sweet taste, is not changed by exposure to light and air, but becomes coloured at a temperature below 100° C. Its aqueous solution is decomposed by long boiling, sulphuret of silver is deposited; it is easily soluble in ammonia, not perfectly insoluble in alcohol. Hydrochloric acid affects it slowly; when boiled it produces a black precipitate, from which ammonia extracts chloride of silver. Its formula is $\dot{\mathrm{Ag}}\,\underline{\ddot{\mathrm{S}}} + 2\,\dot{\mathrm{Na}}\,\underline{\ddot{\mathrm{S}}} + 2$ aq.

The *second salt* is difficultly soluble in water, soluble in ammonia and excess of hyposulphite of soda (forming the above salt); it is a dirty white crystalline powder, which becomes black by boiling with water, and becomes gradually coloured in the air. Its formula is $\dot{\mathrm{Ag}}\,\underline{\ddot{\mathrm{S}}} + \dot{\mathrm{Na}}\,\underline{\ddot{\mathrm{S}}} +$ aq.

Plumbo-hyposulphite of Soda is best prepared by means of acetate of lead, like the first argento-hyposulphite. It becomes crystalline, is white, easily soluble in acetate of soda, little in water, difficultly in alcohol. Formula is $\dot{\mathrm{Pb}}\,\underline{\ddot{\mathrm{S}}} + 2\,\dot{\mathrm{Na}}\,\underline{\ddot{\mathrm{S}}}$; it is therefore anhydrous.

Cupro-hyposulphite of Soda.—On mixing a solution of the hyposulphite of soda with an excess of sulphate of copper, a yellow crystalline precipitate is formed; this must be quickly filtered and washed with very dilute acetic acid, and then dried *in vacuo*, for it easily becomes brown and is decomposed. It is not at all soluble in alcohol, difficultly in water, easily in hyposulphite of soda. Out of this solution alcohol precipitates a salt easily soluble in water. It dissolves in ammonia with a brownish yellow colour, which in the air changes to blue; it is consequently a salt of the dinoxide. It is decomposed immediately by concentrated sulphuric acid, and by dilute when boiled; sulphurous acid is evolved and sulphuret of copper precipitated, the solution contains oxide; when the decomposition is effected by means of hydrochloric acid the solution contains dinoxide. Formula $3\,\dot{\mathrm{Cu}}\,\underline{\ddot{\mathrm{S}}} + 2\,\dot{\mathrm{Na}}\,\underline{\ddot{\mathrm{S}}} + 5$ aq. By mixing solutions of the hyposulphite and of a neutral salt of sesquioxide of iron, a deep black red liquid is obtained, which speedily decolorizes and then contains protoxide.—(*Ann. der Chem. und Pharm.*, vol. xl. p. 94–101.)

III. *Further Remarks on* Fernel's *Measure of a Degree, in Reply to* Professor De Morgan's *Letter in the Number for May. By* THOMAS GALLOWAY, *A.M., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

SO long as the argument relative to the length of Fernel's degree of the meridian turned upon a standard of measure derived from the human body, or the length of a man's walking pace, I saw little reason for adding any thing to my former communication, being satisfied that a result expressed in terms of such a standard would bear any interpretation (at least within wider limits than would include the differences under discussion) that any one might choose to give it; but the new evidence which has been produced by Prof. De Morgan in your Number for May, changes entirely the state of the question.

The conclusion at which Mr. De Morgan has ultimately arrived is founded on two assumptions: first, that the diagram or figure in the *Monalosphærium* is, or originally was, a copy of a foot-length as laid down on a scale; and secondly, that Fernel used the same or an equivalent foot in measuring the diameter of the wheel of the vehicle in which he travelled from Amiens (or wherever his station was) to Paris. With respect to the second assumption there is no evidence whatever; and on looking at the copy of Fernel's work in the British Museum, I think there are strong reasons for doubting the accuracy of the first. Fernel does not say that his diagram was intended to define the *length* of the geometrical foot, or that it corresponded *in dimension* with any actual scale; on the contrary, *there is no allusion to it in the text at all*, and unless the title printed under it, "Figuratio pedis geometrici," be held to have reference to magnitude, there is nothing to lead us to infer that he had any other object in view than simply to represent by a diagram the divisions which should be cut on a measuring rod. The part of the work in which the passage occurs is a treatise on Mensuration; and in describing the measuring rod he remarks that it should be selected with great pains, "omni molimine" (referring probably to the accuracy of the division), and enriched with a diversity of measures, "mensurarum diversitate locupletata;" that it should be five feet in length, and marked with the divisions expressed in the following table, viz. four grains = a digit, four digits = a palm, four palms = a foot, five feet = a pace. The diagram shows all those divisions of the foot; but there seems to me no more reason for supposing that they were intended to be of their proper length, than there is for supposing that the drawings from the human body with which some of the old authors illustrated their measures, were intended to be of the natural size, which they manifestly are not. The figure is a line marked on the margin of the page, extending as high as the head-line at the top, and a little below the letter-press at the foot; and suggests the idea of its having been adapted by the printer to the length of the page, or made as long as possible for the purpose of showing the small divisions.

It should be kept in mind that the work in which the diagram is found is not that in which Fernel's operation for measuring the degree is described, nor does it contain any allusion to that operation, which was probably not executed when the work was printed. Neither is there in the *Cosmotheoria* any allusion to this figure.

Supposing, however, the fact to be as Mr. De Morgan assumes, how are we to reconcile the result with the reasoning in his previous papers respecting the geometrical pace and the Italian mile? In his first letter he stated the Italian mile to be 1628 English yards, or, according to Dr. Bernard, 1667 yards, the former statement giving Fernel's degree equal to about 63 statute miles, and the latter to 64½; and in his second letter he confirms this result by arguments which he considers to be decisive of the question. But when the actual measure, or what is assumed to be such, is produced, it turns out that the true length is only 53 miles and three quarters. The error was therefore between a fifth and a sixth of the whole quantity. Mr. De Morgan himself appears to feel this difficulty, and observes that the difference cannot be easily explained unless we adopt a surmise of Paucton, that the geometrical pace was 4½ Roman feet. What weight may be due to this surmise I cannot pretend to say, but the discrepancy seems to afford a pretty conclusive proof of the accuracy of the position maintained in my former letter, namely, that the Italian mile and geometrical pace were vague and indefinite terms, having no certain meaning unless defined with reference to some standard foot, and that therefore the use of them by Fernel afforded no presumption against the supposition of Picard, that his measure was given in Paris feet. If Paucton surmises that the geometrical pace was 4½ Roman feet, he also surmises that it was greater than 5 Roman feet (p. 179); but he likewise tells us that the idea of the geometrical pace has been lost for ages.

According to Mr. De Morgan's hypothesis Fernel's 68·096 Italian miles contained 3,404,800 English inches, and consequently a single mile was equivalent to 1389 English yards, which is 278 yards shorter than the Italian mile of Dr. Bernard, and 225 yards shorter than the old Roman mile, with which Mr. De Morgan states (I think on good grounds) that the Italian mile was commonly though vaguely supposed identical. The difference is so great, and the result so much at variance with all the other authorities which have been produced, and which concur in giving the Italian mile longer than the Roman mile, that if we admit the hypothesis we are driven to the improbable conclusion that Fernel, without intimation, laid down an arbitrary foot for himself, thereby rendering his statements unintelligible or deceptive.

There is another statement of Fernel's, which though of no value towards giving the exact length of his degree, may perhaps go for something when the question turns upon a difference of 16 miles in 70. He states that the northern extremity of his arc was reckoned by the country-people to be 25 leagues distant from Paris. Now it is not here material to inquire what the length of the league was. We know from the difference of latitudes that the distance in a straight line was somewhere about 70 English miles, and it cannot be supposed that the vulgar estimate was in error to the extent of anything like 16 miles. But as Fernel manifestly supposes his own determination was not at variance with the vulgar estimate, it is difficult to believe that he gave his result in terms of a scale by which the reputed distance must have been reduced nearly a fourth part. Amiens, from which it has generally been supposed he measured, is 75 miles from Paris by the road.

I may also add, that if the hypothesis be correct, Fernel's notions of a degree, before he attempted to measure it, must have been very extraordinary. In the same work in which the figure occurs (*Monalosphærium*, p. 15) there is a proposition explaining the method of measuring the terrestrial distances between places, in which he directs 60 Italian miles to be allowed for each degree of latitude, and one mile for each minute, and gives some examples of distances so computed. But according to the hypothesis his Italian mile of 5000 geometrical feet was only 1389 English yards, whence he must have supposed the degree to be less than 47½ English miles. This is surely without the limits of credibility.

It is proper, however, to remark, that Riccioli, in his *Geographia Reformata*, lib. ii. c. 2, mentions Fernel's diagram, and gives the ratio of its length to the ancient Roman foot, whence it may be inferred that he regarded it as intended for the representation of an absolute measure; but Riccioli allows no authority either to the figure or to the statements of

Fernel respecting the length of the geometrical pace. His words are, " Neque audiendus est Fernelius, qui lib. i. Cosmotheoriæ c. i. in Schol. ait passus 5 hominis mediocris staturæ efficere passus 6 geometricos, et parte 4 Praxis Geometricæ pedem geometricum exponit qui ad Romanum Vespasianicum est ut 1030 ad 1200."

On the grounds above stated,—the total absence of direct testimony that the line figured in the *Monalosphærium* is a copy of the foot used by Fernel, and the improbability of the consequences resulting from the supposition,—I think we must conclude either that the diagram was intended for nothing more than to illustrate the description of the measuring rod, or else that it was reduced by the printer; and that the question as to the true length of Fernel's degree remains as doubtful as ever. At the same time, considering the great uncertainty in which every thing connected with Fernel's operation is involved, and seeing that we have nothing better than conjectures to reason upon, I must own that it is with considerable diffidence I give my opinion in opposition to that of Professor De Morgan, who has evidently bestowed much attention on the subject.

I remain, Gentlemen, faithfully yours,

May 12, 1842. T. GALLOWAY.

IV. *Supplementary Notes on the true Position in the " Devonian System" of the Cornish Killas. By the Rev.* D. WILLIAMS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN one of the earliest communications I had the honour of submitting to the public through the medium of your valuable Journal, I pointed out both by text and section, that the Cornish killas was the last or newest formation in " the Devonian System." I entertained such entire confidence in the Chudleigh Sections, as much from their own evidences as from the crowd of contradictions and apparent anomalies elsewhere which they reconciled, that I felt no hesitation in committing myself absolutely to them, from the conviction that (as Nature could not deny herself) I should meet with nothing but additional confirmations in the large portion of country which I had not then surveyed. A recent excursion into Devon and Cornwall having furnished me with some important structural facts in addition to those I communicated in your Number for February last, I request permission to detail them to your readers, some of whom, in whose conversion I am more particularly interested, however silent, may not yet be satisfied.

Few circumstances in my short geological life have afforded me greater delight than having been enabled during that excursion to determine with precision, the true grade in the Coddon Hill series, No. 8, of the Ivy Bridge jasper rock, a rock which nearly all geologists who have been in South Devon can hardly have failed to observe, and in whose nether position as regards the killas country on the south of it, probably as many are agreed; for my own part I felt perfectly satisfied that it must be included in the lower culm measures, for the reasons mentioned page 132. No. 129 of your Journal; but I also felt that I might not be able to convey the same assurance to the minds of others. I apprehended that doubts might still be entertained that it was some peculiar altered rock, or that from its lower terms resting on the granite, that volcanic aggregate in its protrusion, or in its "*elevation-crater*" *movement*, might have brought up with it something almost as fundamental as itself. On following the bed of the East Okement river from Oakhampton, geographically up, but geologically down to the granite of Dartmoor, I passed in succession the following series, all dipping north at a mean of 45° to 50°.

No. 8. Coddon Hill grit series.	No. 9. Floriferous series.
Black slates.	
Olive grit beds, and fine foliated gray grit.	
Ivy Bridge jasper grit Trap.	Floriferous schist and grit.
Jasper grit Trap.	Smoky gray schist.
Jasper grit, passing into	
Granite.	

The jasper grit up the East Okement is as identical in mineral type and composition with the Ivy Bridge rock as it is possible for any two specimens from any continuous bed to be; it is a strikingly characterised mineral aggregate, in which I have nowhere observed a vestige of organic structure, but at each locality it is precisely the same striped, plated and layered compound, a coarse ribbon jasper, possessing the same variegated colours, the same mineral peculiarities, and the same aberrations from a mean normal type. The section up the East Okement presents us with nothing more than is shown above Ivy Bridge, viz. the same peculiarly marked rock resting upon granite; but let us ascend the West Okement up to the Posidonia lime-rock quarries at Meldon*, and we at once appreciate the force and value of the East Okement and Ivy Bridge sections. We there observe the following descending series dipping north at about 50°.

---

* Spelt *Elmdon* on the Ordnance sheet.

No. 8. Coddon Hill grit series.

- Black slates, rusty externally.
- Ivy Bridge, jasper grit.
- Trap.
- Coddon Hill grit.
- Posidonia limestone.
- Coddon Hill grit.
- Granite.

No. 9. Floriferous series.

- Olive-coloured grits and shale.
- Smoky gray schist.

The section after the margin of the river up to the Lime-rock quarries is so clear and unequivocal in its details, that nothing is left to inference or conjecture. From the far greater thickness of No. 8, on the south, and from other facts I have collected, I have now no doubt that this jasper grit is a new term introduced into the Coddon grit series on the south, which is altogether absent in the north of Devon*; while its position above the Coddon grit and its Posidonia limestones, each in a totally unaltered condition, assures us that it is not a metamorphic rock.

At Tavistock I observed that I was in error when I stated in your Journal, No. 129, p. 131, that on "the eastward road, after crossing the Tavy behind the Bedford Arms, the culm schists dipped into the killas hill," whereas they dip to the north there, or outwards from Whitchurch Down; they are however at the base of that hill; and as we ascend it from the first turnpike gate, we observe them to incline over easily to the south, and to be overlaid by a thick accumulation of a dull gray and pale green ash which passes insensibly into a delicate killas, in a flat position, or a gently and long undulating outline. I had marked this low southern dip of the carbonaceous schist on my field map, and from its small scale, I inadvertently read it off as the true dip along the south margin of the river, less than a furlong distant.

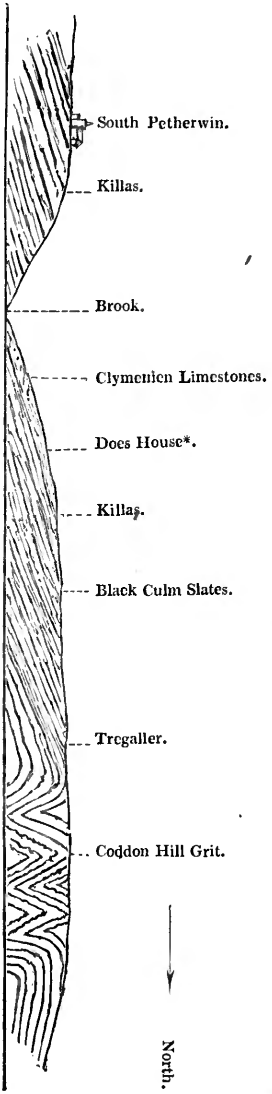
From hence I went to South Petherwin near Launceston, and on a more careful examination of the slates, I observed for the first time that at Does House the killas crosses the little brook and valley and ascends and flanks the carbonaceous slate ridge on the north, both dipping together to the south at about 15°, the floriferous or culmy beds as manifestly underlying the killas as any one rock on earth underlies another.

---

* In a prospectus of my intended publication on the Geology of West Somerset, Devon and Cornwall, which I circulated at Plymouth in August last, I mentioned this rock as occurring at Landkey Hill near Barnstaple; a subsequent examination of that hill in November last, satisfied me that I was in error in so stating.

The ascent is gradual and easy, without the least break or interval, and from the circumstance of the culm slate and killas cuttings being old, wayworn and dusty, and so symmetrically packed and disposed, I had previously concluded that the former were continued down to the brook, as they are a little north of Landlake by Bad Ash, about half a mile to the eastward; on accosting them however with the hammer foot by foot, their freshly fractured faces quickly undeceived me, and left me in no doubt that the order of superposition was evidenced here with as much simplicity, truth and precision, as it was at Boscastle, while it explained in the most satisfactory manner the imperfect and doubtful section at Landlake near at hand, which had been noticed by Mr. Phillips in his Palæozoic Fossils, pp. 195 and 196, and by myself in your Journal, No. 129, page 128.

Immediately adjoining "Does House" on the west, is a place marked "Tregaller" on the Ordnance Map; in a bye lane near this, and immediately under the letter *g*, I fell in with a quarry of the Coddon Hill grit which had been excavated for the roads: its beds or layers in the centre of the quarry have been compressed into the form of two pointed Gothic arches adjusted side by side, from which they depart by an easy inclination to the south on the south side, and by a low dip to the north on the north side. The quarry is on the summit of the ridge and apparently in its axis.



Section from South Petherwin to North of Tregaller.

---

* Does House is about a furlong east of the line of section.

I went from hence by Petherwin to the manganese mine at "Bolathan," and there I observed the pale green killas had been sunk through by a vertical shaft to a depth of twenty-five feet (as I was informed) down to the Coddon Hill grit and its lode of manganese. The killas here is unequivocally exposed on the surface, and is an uninterrupted continuation of that which near Petherwin abounds in Clymeniæ, Goniatites, Orthoceratites, Trilobites, Orthides, &c. &c.; and if the subordinate grit had been carted to the spot from Coddon Hill in North Devon, or from St. Stephen's Down near Launceston, it could not have offered closer points of comparison and agreement.

The lower killas, the lowest of the threefold division into which that great member of the Devonian group naturally resolves itself, overlies the floriferous or carbonaceous series: not a shadow of doubt or uncertainty is on my mind when I state it; the fact is proved by every variety and kind of recognized evidence by which the established order of superposition of rock formations has been determined elsewhere; and those several kinds of evidence cannot be disputed or rejected here without insecurity and peril to the foundations of the geological column, every stone of which has been hewed and squared and adjusted by some wise master builder. If in a perfect faith in, and uncompromising obedience to those laws which alone govern legitimate and secure induction, I have without pretension or design conveyed embarrassment or perplexity to the minds of some, or unkind or unworthy feelings to the minds of others, I am amply recompensed in the conscious indifference and singleness of purpose with which I have read off the great truths of the Creator, and in a dawn of hope that, ere long, He may enable me to sound a diapason note which may restore to harmony the apparently discordant elements.

I have the honour to remain, &c.

Bleadon, May 17, 1842. D. Williams.

V. *Note on Fluid Motion. By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., F.C.P.S., &c., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge*[*].

Through the able and interesting papers which Prof. Challis has recently published in the Philosophical Magazine[†], attention has been directed to the circumstances

---

[*] Communicated by the Author.

[†] S. 3, vol. xix. p. 229. vol. xx. p. 84, 281.

under which the equations of fluid motion can be solved. Whilst interest is awakened on the subject, it may not be deemed utterly unimportant to offer a few remarks on the general question, especially as any peculiarity in the mode of proceeding, however valueless in itself, may serve as a hint to guide or incite others to the most important investigations.

The question before us appears to me to be this—What new conditions must we introduce, or what transformations must we effect, in order that the four equations of fluid motion may be reduced to four other equations, each containing the differential coefficients of only one quantity? Before this question can be answered, at least before we can set about introducing any new conditions, it appears requisite to answer another question—Are there any *necessary* conditions? Of course the answer is in the affirmative. The equation of continuity is one. But it is not the only one; for unless the pressure and velocities are discontinuous quantities, the equations deduced by the application of D'Alembert's principle must be statical equations, depending on the time only in as far as the velocities depend on the time. Hence the relations which would exist amongst the differential coefficients of p, were the fluid at rest, must exist when it is in motion; that is,

$$\frac{d^2 p}{dx\,dy} = \frac{d^2 p}{dy\,dx}, \text{ \&c.}$$

These, then, are equations of condition; the bearing of which ought to be examined previous to the introduction of any new conditions. They will serve, in some cases, to show what new hypotheses are admissible, and, in all, to detect those which are not.

It is not my intention to enter fully into this subject in my present communication. I shall content myself with offering a few remarks on the results of the mode of proceeding which I have indicated, as applied to the motion of incompressible fluids acted on by gravity only.

By inclosing within brackets the complete differentials with respect to x, y, z and t, we obtain the following sets of equations:—

$$\left.\begin{aligned}\left(\frac{d\mathrm{M}}{dt}\right) &= \mathrm{M}\frac{du}{dx} + \mathrm{N}\frac{du}{dy} + \mathrm{P}\frac{du}{dz},\\ \left(\frac{d\mathrm{N}}{dt}\right) &= \mathrm{M}\frac{dv}{dx} + \mathrm{N}\frac{dv}{dy} + \mathrm{P}\frac{dv}{dz},\\ \left(\frac{d\mathrm{P}}{dt}\right) &= \mathrm{M}\frac{dw}{dx} + \mathrm{N}\frac{dw}{dy} + \mathrm{P}\frac{dw}{dz},\end{aligned}\right\} \quad \ldots \quad (1.)$$

or

$$\left.\begin{aligned}\left(\frac{d\,\mathrm{M}}{d\,t}\right) &= \mathrm{M}\frac{d\,u}{d\,x} + \mathrm{N}\frac{d\,v}{d\,x} + \mathrm{P}\frac{d\,w}{d\,x},\\ \left(\frac{d\,\mathrm{N}}{d\,t}\right) &= \mathrm{M}\frac{d\,u}{d\,y} + \mathrm{N}\frac{d\,v}{d\,y} + \mathrm{P}\frac{d\,w}{d\,y},\\ \left(\frac{d\,\mathrm{P}}{d\,t}\right) &= \mathrm{M}\frac{d\,u}{d\,z} + \mathrm{N}\frac{d\,v}{d\,z} + \mathrm{P}\frac{d\,w}{d\,z};\end{aligned}\right\} \quad \ldots \quad \text{(2.)}$$

where

$$\left.\begin{aligned}\mathrm{M} &= \frac{d\,v}{d\,z} - \frac{d\,w}{d\,y},\\ \mathrm{N} &= \frac{d\,w}{d\,x} - \frac{d\,u}{d\,z},\\ \mathrm{P} &= \frac{d\,u}{d\,y} - \frac{d\,v}{d\,x}.\end{aligned}\right\} \quad \ldots\ldots\ldots \quad \text{(3*.)}$$

1. One way of satisfying all the equations is by supposing $\mathrm{M} = 0$, $\mathrm{N} = 0$, $\mathrm{P} = 0$; in which case the equations (3.) indicate that $u\,d\,x + v\,d\,y + w\,d\,z$ is a complete differential.

2. Another way is to suppose M, N and P all absolutely constant; in which case the velocities u, v, w will be determined by the same equation, viz. by either of the equations (1.). Hence u, v, w all have the same *form*.

Also the equations (2.) give $\mathrm{M}\frac{d\,u}{d\,x} + \mathrm{N}\frac{d\,v}{d\,x} + \mathrm{P}\frac{d\,w}{d\,x} = 0$, &c. &c., or $\mathrm{M}\,u + \mathrm{N}\,v + \mathrm{P}\,w$ is a quantity whose partial differential coefficients, with respect to each of the coordinates, is zero. This quantity is therefore either zero, or a function of t only.

a. If it be zero, $u\,d\,x + v\,d\,y + w\,d\,z$ is integrable by a factor, for the equation $\mathrm{M}\,u + \mathrm{N}\,v + \mathrm{P}\,w = 0$ is the well-known equation of condition that this may be the case.

b. If $\mathrm{M}\,u + \mathrm{N}\,v + \mathrm{P}\,w = f(t)$, $u\,d\,x + v\,d\,y + w\,d\,z$ is *not* a complete differential after being multiplied by a factor.

The equations are nevertheless integrable in this case, and give as their result,

$$\begin{aligned}u &= \mathrm{F}(\mathrm{M}\,z - \mathrm{P}\,x,\ \mathrm{N}\,z - \mathrm{P}\,y,\ t),\\ v &= \phi(\mathrm{M}\,z - \mathrm{P}\,x,\ \mathrm{N}\,z - \mathrm{P}\,y,\ t),\\ w &= \psi(\mathrm{M}\,z - \mathrm{P}\,x,\ \mathrm{N}\,z - \mathrm{P}\,y,\ t);\end{aligned}$$

---

[*] See my Memoir on the Theory of Waves, Trans. Roy. Soc. Edin., vol. xv. p. 116.

the functions being subject to the condition $\mathrm{M\,F} + \mathrm{N}\,\phi + \mathrm{P}\,\psi = f(t)$.

3. If M, N, P are explicit functions of t only, our equations (1.) are reduced to

$$\frac{d\,\mathrm{M}}{d\,t} = \mathrm{M}\frac{d\,u}{d\,x} + \mathrm{N}\frac{d\,u}{d\,y} + \mathrm{P}\frac{d\,u}{d\,z},$$

$$\frac{d\,\mathrm{N}}{d\,t} = \mathrm{M}\frac{d\,v}{d\,x} + \mathrm{N}\frac{d\,v}{d\,y} + \mathrm{P}\frac{d\,v}{d\,z},$$

$$\frac{d\,\mathrm{P}}{d\,t} = \mathrm{M}\frac{d\,w}{d\,x} + \mathrm{N}\frac{d\,w}{d\,y} + \mathrm{P}\frac{d\,w}{d\,z}.$$

Hence

$$\mathrm{M}\frac{d^2 u}{d\,x^2} + \mathrm{N}\frac{d^2 u}{d\,x\,d\,y} + \mathrm{P}\frac{d^2 u}{d\,x\,d\,z} = 0,$$

$$\mathrm{M}\frac{d^2 u}{d\,x\,d\,y} + \mathrm{N}\frac{d^2 u}{d\,y^2} + \mathrm{P}\frac{d^2 u}{d\,y\,d\,z} = 0,$$

$$\mathrm{M}\frac{d^2 u}{d\,x\,d\,z} + \mathrm{N}\frac{d^2 u}{d\,y\,d\,z} + \mathrm{P}\frac{d^2 u}{d\,z^2} = 0,$$

from which equations we obtain, by eliminating M, N and P,

$$\frac{d^2 u}{d\,x^2}\,\frac{d^2 u}{d\,y^2}\,\frac{d^2 u}{d\,z^2} - \frac{d^2 u}{d\,x^2}\left(\frac{d^2 u}{d\,y\,d\,z}\right)^2 - \frac{d^2 u}{d\,y^2}\left(\frac{d^2 u}{d\,x\,d\,z}\right)^2$$
$$- \frac{d^2 u}{d\,z^2}\left(\frac{d^2 u}{d\,x\,d\,y}\right)^2 + 2\,\frac{d^2 u}{d\,x\,d\,y}\,\frac{d^2 u}{d\,x\,d\,z}\,\frac{d^2 u}{d\,y\,d\,z} = 0;$$

an equation of precisely the same form as that which occurs in the determination of the principal axes of a system, or of the diametral lines of a surface of the second order.

Similar equations are true in v and w. We conclude that the motion is such as to be symmetrical with respect to the coordinate planes.

Cor.—If x, y, z enter in such a way into the expressions for the velocities that $\frac{d\,u}{d\,y} = \frac{d\,u}{d\,z}$, &c., the equations are identically true.

4. If the motion be confined to two dimensions, the equations are reduced to

$$\frac{d\,u}{d\,x} + \frac{d\,v}{d\,y} = 0,$$

$$\frac{d\,u}{d\,y} - \frac{d\,v}{d\,x} = \mathrm{C},$$

where C is a quantity whose total differential with respect to t is zero.

a. If C be an absolute constant, the equations for determining u and v are

$$\frac{d^2 u}{d x^2} + \frac{d^2 u}{d y^2} = 0, \quad \frac{d^2 v}{d x^2} + \frac{d^2 v}{d y^2} = 0.$$

b. If C be not an absolute constant, the equation for u assumes the following complicated shape:—

$$\frac{d}{dy}\left\{\frac{\dfrac{d}{dy}\cdot\dfrac{\dfrac{ds}{dt}+u\dfrac{ds}{dx}-s\dfrac{du}{dx}}{\dfrac{du}{dy}}+\dfrac{ds}{dx}-\dfrac{\dfrac{du}{dx}\dfrac{ds}{dy}}{\dfrac{du}{dy}}}{\dfrac{d}{dy}\cdot\dfrac{\dfrac{ds}{dy}}{\dfrac{du}{dy}}}\right\}=\frac{du}{dx},$$

where $s = \frac{d^2 u}{d x^2} + \frac{d^2 u}{d y^2}$.

The equation for v is exactly similar to this.

It is unnecessary to add that this equation is too complicated to admit of integration in a general form.

We shall not prosecute these remarks further; we have offered them rather for the purpose of directing attention to the process than from a conviction of their novelty or importance.

VI. *Experiments in Magneto-Electricity, illustrative of a Passage in* Professor Faraday's *Researches. By Professor* DOVE*.

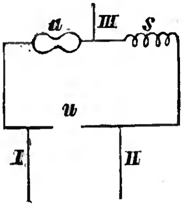
FARADAY says, § 1101, "As an electric current acts by induction with equal energy at the moment of its commencement as at the moment of its cessation, but in a contrary direction, the reference of the effects of a current when stopped to an inductive action would lead to the conclusion, that corresponding effects of an opposite nature must occur in a long wire, a helix or electro-magnet, every time that contact is made with the electro-motor. These effects will tend to establish a resistance for the first moment in the long conductor, producing a result equivalent to the reverse of a shock or spark. Now it is very difficult to devise means fit for the recognition of such negative results." This difficulty may,

---

* Communicated by H. Croft, Esq., Teacher of Chemistry, being an extract from a letter addressed to him by the Author.

however, be entirely overcome, so that perfectly corresponding experiments may be made with the extra-current both at its commencement and cessation.

In the following figure, let *a* represent the rotating anchor of a Saxton's machine, *s* a spiral connected with the wire of this anchor, for the production of the extra-current, and so arranged as to allow of iron being inserted into it, *u* the place where the wire opens by means of the spring when the anchor is in a perpendicular position. I, II, III, three conductors which can be close by any body, as a galvanometer, voltameter, &c. This arrangement allows of three kinds of junction, viz. I and II, I and III, II and III.



If we call the primary current *p*, and designate by A the *reversed* extra-current formed at its commencement, and by E the similar (in direction) extra-current produced at its cessation, we find as follows:—As long as the wire at *u* remains closed, the intensity of *p* increases during the rotation of the anchor from 0° to 90°, that is, *p* produces the current A, and we obtain $p-A$. If we close I and III by means of the body (which extra connexion does not experience the least physiological effects as long as *u* is closed), it receives the shock of the current $p-A$, inasmuch as E cannot be formed, because at the moment *u* is opened it passes out of the connexion; if we close II and III we obtain E; if a straight wire is substituted for *s* there is no action: by closing I and II we get $p-A+E$, as when *u* opens the extra-current is formed in *s*. The presence of iron in the spiral *s* produces the following effects:—

I and III) $\{p-A\}$. The shock with an empty spiral is much greater than when none is inserted, i. e. $p-A$ smaller than *p*; when iron is inserted it is much weaker, for A is increased while *p* remains unchanged.

II and III) $\{E\}$. The shock is strengthened by the insertion of iron.

I and II) $\{p-A+E\}$. The shock is much stronger than with I and III, for $p-A+E$ is greater than $p-A$; it remains almost unchanged when iron is inserted, because E increases almost the same as A.

The opening spark at *u* is considerably weaker when iron is put into the spiral, but recovers its intensity if II and III are metallically connected. The spark between II and III or I and III is increased in intensity by the presence of iron;

the extra-current in *s* acts namely as an increase of resistance to the passage of the current, and causes a greater part of it to flow off through I and III or II and III. A voltameter inserted at *u*, or between I and III or II and III, is, as regards the quantity of gas obtained, in exact proportion to the intensity of the spark. The galvanometer gives a similar direction for p, $p-A$, $p-A+E$ and E. All the results obtained apply as well for primary currents whose direction remains the same, and for those whose direction alternates.

As bundles of iron wires, when electro-magnetized by means of galvanic, thermic, or frictional electricity, surpass massive bars of the same metal in their physiological effects, and as this phænomenon may be explained by electric currents which are simultaneously excited in the iron during magnetization, it was interesting to examine how bundles of wires would be in comparison to bars when both were magnetized by bringing them in proximity to a steel magnet. This can be effected by means of a Saxton's machine with wooden anchor, in whose rolls of wire which are connected crosswise, a bundle of iron wires and an iron cylinder act against each other. The experiment shows, that for no action does the current excited by the bundle exceed that from the cylinder: moreover, two similar bundles of wire, one of which was contained in a perfect, the other in a *slit* cylinder of brass, were exactly equal in all their effects.

The rotating anchor of a Saxton's machine, when in the dark, and illuminated solely by the sparks it produces, appears to stand still and exactly in the same position, whether the anchor be turned slowly or as fast as possible. If there were any lapse of time between the interruption of the current and the appearance of the spark, the anchor would assume a different position, according to the rapidity of rotation. As this however is not the case, it follows that the time elapsing between the interruption of the current and the appearance of the spark is not measurable by this means.

H. W. Dove.

VII. *Note on the Composition of the Basic Sulphate of Mercury, or Turpeth Mineral.* *By* Robert Kane, *M.D., M.R.I.A.*

I AM induced to bring forward, in the present form, the results of some analyses of the basic sulphate of mercury, from the fact that its true composition does not appear to have become generally known; the best authorities, or at least those most in the hands of students, giving different and mostly incorrect views. Thus both in Christison's 'Dispensatory' and in Pereira's *Materia Medica*, this salt is stated to have the formula $SO_3 + 2HgO$, which in the former work is quoted on the authority of Barker and Gergèr, neither of whom was really the author of the analysis, which is a very old one by Braamcamp and Siquiera Oliva, as Dr. Pereira in his excellent work very properly states.

Another much more modern, and better analysis, by Dr. Phillips*, is quoted in Turner's Chemistry, and also by Dr. Christison; the formula deduced from it is $3 . SO_3 + 8HgO$. This analysis is very nearly correct, yet the slight error which it contains has the effect of giving to the formula a complexity which it does not properly possess.

In Berzelius's System, and in Gay-Lussac's *Chimie des Sels*, the formula given is $SO_3 + 3HgO$. This I have found to be the true composition of the salt, and it is adopted by Graham in his Elements; yet I have never been able to find in the Journals the analyses on which it is founded; hence I consider that the details of those by which I satisfied myself of its correctness may have some interest to chemists.

A. 6·503 grammes of perfectly dry and neutral sulphate of the red oxide of mercury were boiled for a long time with much water, and the yellow powder which formed was collected on a tared filter, and having been dried until it ceased to lose weight, was found to weigh 4·623 grammes, or 71·09 per cent.

The filtered liquor contained mercury. It was treated with sulphuretted hydrogen, and the sulphuret of mercury which fell was found, when perfectly dry, to weigh 0·706 or 10·85 per cent., corresponding to 10·11 of oxide of mercury.

The excess of sulphuretted hydrogen having been expelled by boiling, the sulphuric acid in the liquor was thrown down by nitrate of barytes. The sulphate of barytes weighed 3·607 or 55·5 per cent., containing 19·08 of sulphuric acid. Of this 3·71 had been united to 10·11 of oxide of mercury, forming 13·82 of sulphate of mercury which had not been decomposed by the water. Hence there had been decomposed 86·18 per cent. of the sulphate, yielding 15·37 of sulphuric acid and 71·09 of turpeth, indicating thus a slight excess of weight, owing probably to the turpeth not having been rendered absolutely dry. Hence 100 parts of the neutral salt, if perfectly decomposed, should give

17·67 of sulphuric acid,
82·49 of turpeth mineral.

100·16, showing a slight excess, as above noticed.

Now as 100 of neutral salt contains 73·17 of oxide of mer-

---

* Phil. Mag. Second Series, vol. x. p. 206.

cury and 26·83 of sulphuric acid, the quantity of acid left in the liquor is just two-thirds of the whole, as $\frac{2}{3}\cdot 26{\cdot}83 = 17{\cdot}98$. The turpeth contained therefore 73·23 of $Hg\,O$ and 9·16 of $S\,O_3$ in the 82·49 parts, or in 100 parts,

$$\left.\begin{array}{ll}\text{Sulphuric acid} & = 11{\cdot}10 \\ \text{Oxide of mercury} & = 88{\cdot}90\end{array}\right\} 100{\cdot}00.$$

The formula $S\,O_3 + 3\,Hg\,O$, requires

$$\begin{array}{rcrcr} S\,O_3 & = & 40{\cdot}1 & = & 10{\cdot}91 \\ 3\,Hg\,O & = & 328{\cdot}2 & = & 89{\cdot}09 \\ \hline & & 368{\cdot}3 & & 100{\cdot}00 \end{array}$$

B. 4·525 grammes of turpeth mineral prepared with boiling water were dissolved in dilute muriatic acid, and the liquor was precipitated by sulphuretted hydrogen. The sulphuret of mercury weighed 4·334, being 95·76 per cent., equivalent to 89·24 of oxide of mercury. The liquor, boiled to remove the excess of sulphuretted hydrogen, gave then with nitrate of barytes, 1·402 of sulphate of barytes, being 30·98 per cent., containing 10·65 of sulphuric acid. Hence the turpeth mineral consisted in 100 parts, of

89·24 oxide of mercury,
10·65 sulphuric acid,
·11 loss.

I need not enumerate more than these two results, although some others were obtained, all of which equally indicated exactly the relation of $S\,O_3 + 3\,Hg\,O$. Of course it will be at once seen that I take for the equivalent number of mercury on the hydrogen scale 101·4, and consider the red oxide as containing one equivalent of each element.

VIII. *On Pascal's Mystic Hexagram. By* T. S. Davies, *Esq., F.R.S., &c., Royal Military Academy, Woolwich.*

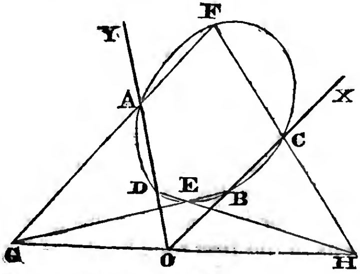
ONE of the two most general and prolific properties of the conic sections yet known, is that first given by Pascal in his *Essai pour les Coniques,* or rather one of the converses of that theorem, which we are told by Leibnitz he called the *mystic hexagram.* It was made by him the foundation of an entire system of conics, of which, however, all we know is the titles and general subjects of the books into which it was divided, as given by Leibnitz in his letter to Perrier in 1679. Mersennus speaks of Pascal having deduced from it four hundred corollaries; and Desargues (who says that in his time, 1642, it was called "the Pascal") tells us that it contains, either as cases or immediate consequences, the whole of the propositions in the first four books of Apollonius. The well-known properties of the quadrilateral inscribed and circumscribed to the conic section, known by modern geometers as "the theory of the pole and polar;" the description of the conic sections by revolving lines or the sides of revolving angles, first suggested by Newton, and followed out in detail by Maclaurin and Braikenridge, also flow at once from this theorem. In short, for generality and facility of employment there is only one other principle that can compete with it; which is that of the *anharmonic ratio* of M. Chasles, as developed in the notes to his *Aperçu Historique des Méthodes en Géométrie.*

The demonstration of this theorem was not, however, published by Pascal; nor, I think, has there ever been given a strictly geometrical demonstration in the manner of the ancients. For the circle the demonstration is very simple and elegant; of which four specimens may be seen in the Mathematical Repository, vol. iv. New Series, one of which by Mr. Ivory is inserted by Dr. Bland in his Geometrical Problems. The method of projection is employed to extend it to the other conic sections: but admitting the *theory of transversals,* the property admits of a very short and direct demonstration for the conic sections generally. The proposition itself in the general form was proposed in the Ladies' Diary for the present year, to be established without any direct or implied use of the circle; and in reply to that, the demonstration above alluded to has been given, and will appear in the next year's Diary.

Many attempts, with different degrees of success and elegance, have been made by the continental geometers to solve this by the method of coordinates. I believe, however, that except by Sir John Lubbock* and an imperfect sketch of my own† (which is here followed out and completed), no one of our countrymen has looked at the subject in this light. I am led, therefore, to think that the following investigation will be interesting to geometers; it being, I believe, very different from any process published by other writers.

THEOREM. *If the three pairs of opposite sides of a hexagon inscribed in a conic section be produced to meet, the three points of concourse will be in one straight line.*

Take the opposite sides A D, B C, uniting in O, as axes of coordinates; and denote the distances O A, O B, O C, O D by α, β, γ, δ, and the two remaining angular points F and E of the hexagon by $(x_1\, y_1)$ and $(x_2\, y_2)$.



---

* Phil. Mag. Third Series, vol. xiii. p. 83.

† Solutions to Hutton's Questions, p. 505.

The general form of the equation of the conic section referred to the axes O B, O D is

$$a y^2 + b x y + c x^2 - d y - e x + f = 0 \quad \ldots\ldots \quad (1.)$$

The equations of the other four sides of the hexagon will be

$$\begin{array}{lll} (\mathrm{A\,F}) \ldots\ldots & x_1 (y - \alpha) = x (y_1 - a) & \ldots\ldots\ldots \; (2.) \\ (\mathrm{B\,E}) \ldots\ldots & y (x_2 - \beta) = y_2 (x - \beta) & \ldots\ldots\ldots \; (3.) \\ (\mathrm{C\,F}) \ldots\ldots & y (x_1 - \gamma) = y_1 (x - \gamma) & \ldots\ldots\ldots \; (4.) \\ (\mathrm{D\,E}) \ldots\ldots & x_2 (y - \delta) = x (y_2 - \delta) & \ldots\ldots\ldots \; (5.) \end{array}$$

Let (X_1 Y_1) be the intersection G of the lines A F, B E denoted by equations (2.) and (3.); and (X_2 Y_2) be that H of C F, D E denoted by (4.) and (5.): then we readily find

$$\left.\begin{array}{l} X_1 = \dfrac{(\alpha x_2 + \beta y_2 - \alpha \beta) x_1}{x_1 y_2 - (x_2 - \beta)(y_1 - \alpha)} \\ X_2 = \dfrac{(\delta x_1 + \gamma y_1 - \gamma \delta) x_2}{x_2 y_1 - (x_1 - \gamma)(y_2 - \delta)} \\ Y_1 = \dfrac{(\alpha x_1 + \beta y_1 - \alpha \beta) y_2}{x_1 y_2 - (x_2 - \beta)(y_1 - \alpha)} \\ Y_2 = \dfrac{(\delta x_2 + \gamma y_2 - \gamma \delta) y_1}{x_2 y_1 - (x_1 - \gamma)(y_2 - \delta)} \end{array}\right\} \ldots\ldots\ldots \; (6.)$$

Write D_1 and D_2 for the denominators of X_1, Y_1 and X_2, Y_2; and find the values of X_2 Y_1 and X_1 Y_2, disregarding for the present the common denominator D_1 D_2. To effect this, actually multiply the values of X_2 Y_1 in (6.), and likewise those of X_1 Y_2: then these two expressions are respectively,

$$\left.\begin{array}{l} \dfrac{\{\beta\gamma y_1^2 + (\alpha\delta + \beta\gamma) x_1 y_1 + \alpha\delta x_1^2 - \beta\gamma(\alpha + \delta) y_1 - \alpha\delta(\beta + \gamma) x_1 + \alpha\beta\gamma\delta\} x_2 y_2}{D_1 D_2} = X_2 Y_1 \\ \dfrac{\{\beta\gamma y_2^2 + (\alpha\delta + \beta\gamma) x_2 y_2 + \alpha\delta x_2^2 - \beta\gamma(\alpha + \delta) y_2 - \alpha\delta(\beta + \gamma) x_2 + \alpha\beta\gamma\delta\} x_1 y_1}{D_1 D_2} = X_1 Y_2 \end{array}\right\} \; (7.)$$

Now since A, B, C, D are the points of intersection of the curve denoted by (1.) with the axes of coordinates, we get by putting x and y successively equal to zero, the values of α, β, γ, δ in terms of the coefficients of (1.), as follows:—

$$\alpha = \frac{d + \sqrt{d^2 - 4 a f}}{2 \alpha} \qquad \beta = \frac{e - \sqrt{e^2 - 4 c f}}{2 c}$$

$$\delta = \frac{d - \sqrt{d^2 - 4 a f}}{2 \alpha} \qquad \gamma = \frac{e + \sqrt{e^2 - 4 c f}}{2 c}.$$

Insert these values in the former of the equations marked (7.): then there results as the value of X_2 $Y_1 . D_1$ D_2,

$$\{a y_1^2 - \frac{ed - \sqrt{(d^2 - 4af)(e^2 - 4cf)}}{2f} x_1 y_1 + c x_1^2 - dy - e x_1 + f\} x_2 y_2.$$

But as $x_1 \, y_1$ is a point in the conic section denoted by (1.), we have

$$a y_1^2 + c x_1^2 - d y_1 - e x_1 + f = - b x_1 y_1,$$

which substituted in the preceding expression gives us

$$X_2 Y_1 = - \frac{2bf + ed - \sqrt{(d^2 - 4af)(e^2 - 4cf)}}{2f D_1 D_2} x_1 y_1 . x_2 y_2 .. (8.)$$

Again, since $X_1 \, Y_2$ is the same function of $x_2 \, y_2$ that $X_2 \, Y_1$ is of $x_1 \, y_1$, we shall in the same way obtain

$$X_1 Y_2 = - \frac{2bf + ed - \sqrt{(d^2 - 4af)(e^2 - 4cf)}}{2f D_1 D_2} x_2 y_2 . x_1 y_1 .. (9.)$$

It follows from (8.) and (9.) that we have the equation

$$X_2 Y_1 - X_1 Y_2 = 0$$

in virtue of the identity of the terms which compose them: and this is the familiar test of the line G H passing through O the origin of coordinates, and furnishing, therefore, a complete proof of the "Pascal."

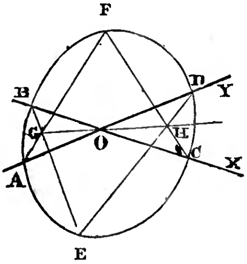
Scholium 1.—When the equation is of the above form (1.) with the exception of the last term negative,

$$a y^2 + b x y + c x^2 - d y - e x - f = 0,$$

the origin O will be *within* the curve; and the Pascal will then become an extension of a theorem of Pappus (prop. 139, book vii.) respecting a quadrilateral figure, to the conic sections generally.

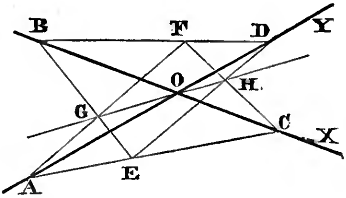
The proposition in reference to this case may be stated as follows:—

Let A D, B C be the diagonals of a quadrilateral inscribed in a conic section: from A C draw lines A F, C F to any point F in the arc B D, and from B D to any point E in the arc A C, meeting the former in G H; then the line G H will pass through O the intersection of A D, B C.



Scholium 2.—The theorem of Pappus, above referred to, applies to the case where the conic section, as the locus of E and F, is replaced by the *straight lines* A C, B D. To deduce this from the preceding investigation, it is only necessary to multiply together the two equations of the lines A C, B D, which gives an equation of the general form (1.), and to which the same process may be applied as that already employed: for the conclusion is deduced from (1.) being the equation of the locus (or loci) of E and F. A more independent and perhaps more elegant process, would be the following; the general principle, however, being the same as that before employed.

Let the absolute lengths of the lines O A, O B, O C, O D be α, β, γ, δ; then the several points concerned will be denoted as under.



(A)	$(0, -\alpha)$	(D)	$(0, \delta)$
(B)	$(-\beta, 0)$	(F)	$(x_1 y_1)$
(C)	$(\gamma, 0)$	(E)	$(x_2 y_2)$

and the several lines concerned will be expressed in the usual manner, thus:—

$$(BD) \;\ldots\; -\beta y + \delta x = -\beta\delta \quad \ldots\ldots \quad (1.)$$

$$(AC) \;\ldots\; \gamma y - \alpha x = -\alpha\gamma \quad \ldots\ldots \quad (2.)$$

$$(AF) \;\ldots\; x_1 (y + \alpha) = x (y_1 + \alpha) \quad \ldots\ldots \quad (3.)$$

$$(BE) \;\ldots\; y (x_2 + \beta) = y_2 (x + \beta) \quad \ldots\ldots \quad (4.)$$

$$(CF) \;\ldots\; y (x_1 - \gamma) = y_1 (x - \gamma) \quad \ldots\ldots \quad (5.)$$

$$(DE) \;\ldots\; x_2 (y - \delta) = x (y_2 - \delta) \quad \ldots\ldots \quad (6.)$$

Denoting as before G and H by $(X_1\, Y_1)$ and $(X_2\, Y_2)$, we get

$$\left.\begin{aligned}
X_1 &= -\frac{(\alpha x_2 + \beta y_2 + \alpha\beta)\, x_1}{x_1 y_2 - (x_2 + \beta)(y_1 + \alpha)} \\
X_2 &= +\frac{(\delta x_1 + \gamma y_1 - \gamma\delta)\, x_2}{x_2 y_1 - (x_1 - \gamma)(y_2 - \delta)} \\
Y_1 &= -\frac{(\alpha x_1 + \beta y_1 + \alpha\beta)\, y_2}{x_1 y_2 - (x_2 + \beta)(y_1 + \alpha)} \\
Y_2 &= +\frac{(\delta x_2 + \gamma y_2 - \gamma\delta)\, y_2}{x_2 y_1 - (x_1 - \gamma)(y_2 - \delta)}
\end{aligned}\right\} \ldots\ldots \quad (7.)$$

Also, since $(x_1\, y_1)$ is in (1.), and $(x_2\, y_2)$ in (2.), we have the equations,

$$\beta y_1 = \delta x_1 + \beta\delta \quad \ldots\ldots \quad (8.)$$

$$\gamma y_2 = \alpha x_2 - \alpha\gamma \quad \ldots\ldots \quad (9.)$$

In the values of X_1, X_2 substitute the values of y_1, y_2 from (8.) and (9.), and in those of Y_1, Y_2 those of x_1, x_2 from the same equations; and denote, as before, the denominators by D_1 and D_2 respectively: then

$$\left.\begin{array}{ll} X_1 = \dfrac{\alpha(\beta+\gamma)\,x_1\,x_2}{\gamma\,D_1} & Y_1 = \dfrac{\beta(\alpha+\delta)\,y_1\,y_2}{\delta\,D_1} \\ X_2 = \dfrac{\delta(\beta+\gamma)\,x_1\,x_2}{\beta\,D_2} & Y_2 = \dfrac{\gamma(\alpha+\delta)\,y_1\,y_2}{\alpha\,D_2} \end{array}\right\} \ldots \text{(10.)}$$

Substitute these in the expression $X_2\,Y_1 - X_1\,Y_2$, and we have

$$X_2\,Y_1 - X_1\,Y_2$$

$$= \frac{(\alpha+\delta)(\beta+\gamma)\,x_1 y_1 \cdot x_2 y_2}{D_1\,D_2} - \frac{(\beta+\gamma)(\alpha+\delta)\,x_2 y_2 \cdot x_1 y_2}{D_1\,D_2}$$

$$= 0,$$

which is again the ordinary criterion of G H passing through the origin, O.

Royal Military Academy,
May 4th, 1842.

IX. *New Process for Preparing Oxygen* *By* W. H. Balmain, Esq.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

OXYGEN being much in request for the oxyhydrogen blow-pipe, and indeed for purposes of illumination, it is important to have an expeditious and cheap process for preparing it. Moreover, in the present day, when practical chemistry is becoming so popular, it will, independently of all matter of expense, be no insignificant acquisition to the lecturer and juvenile experimentalist to have a ready method of preparing the principal supporter of combustion. It has occurred to me that it may be prepared from bichromate of potash by the action of sulphuric acid; and as the process has upon trial proved successful, I beg leave to suggest it to those whom it may concern through the medium of your Journal.

A mixture of three parts of bichromate of potash and four parts of common sulphuric acid contained in a capacious retort, will, on the application of a moderate heat, yield pure oxygen with a rapidity entirely at the command of the operator.

$$\overset{\dot{K}}{47\cdot5} + \overset{\dddot{C}hr_2}{104} = 151\cdot5 \text{ and } \overset{\dddot{S}_4}{160} + \overset{\dot{H}_4}{36} = 196 \text{ produce } \overset{\dot{K}}{47\cdot5} + \overset{\dddot{S}\,\text{and}\,Chr_2}{40+56} \overset{O_3+\dddot{S}_3}{+24+120}$$

$$= 287\cdot5 \text{ and } \overset{\dot{H}_4}{36} \text{ and } \overset{O}{24}.$$

This process is cheaper than that of heating chlorate of potash; for two parts of bichromate of potash will produce as much oxygen as one of chlorate of potash, while the latter is nearly three times the price of the former; and besides this, the residue of the first is valuable, and may be reconverted into bichromate of potash. It is likewise a more convenient process than any at present known, since it may be conducted at so low a temperature that an ordinary retort and lamp may be used for the production of a considerable quantity of oxygen.

Mechanics' Institution, W. H. Balmain.
Liverpool, May 10, 1842.

[*Note.*—I have tried this process and find that it answers very well, the gas being given off, I think, with greater readiness than when sulphuric acid and binoxide of manganese are employed. Occasions I have no doubt will occur in which this method may be advantageously substituted for others.—R. P.]

X. *On* Sir D. Brewster's *Deductions from the Hourly Observations at Leith in* 1824–25. *By* S. M. Drach, *Esq., F.R.A.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The deductions alluded to in the title of this article, as detailed in the Edinburgh Philosophical Transactions, vol. x., flow from any expression of the temperature in functions of the time. Let v = the temperature, t = the time; $-\mathrm{T}$ = a fixed instant; then to be real v = function of

$$\left\{\overline{t+\mathrm{T}|}^{i},\ {}^{(t+\mathrm{T})^{i}},\ \log(t+\mathrm{T}),\ \genfrac{}{}{0pt}{}{\sin}{\cos}\, i(t+\mathrm{T}),\ \text{constant}\right\},$$

which is developable into the series

$$v = \mathrm{A} + \mathrm{B}(t+\mathrm{T}) + \mathrm{C}(t+\mathrm{T})^2 + \mathrm{D}(t+\mathrm{T})^3 + \&c.$$

A, B, C, &c. are functions independent of the time, and comprehending the latitude, declination, radiation, &c.

When $t = -\mathrm{T}$, $v = \mathrm{A}$.

First. If A = the daily mean temperature, $t = -\mathrm{T} =$ time of morning mean, and $0 = \mathrm{B} + \mathrm{C}(t+\mathrm{T}) + \mathrm{D}(t+\mathrm{T})^2, + \&c.$ gives the other times of mean daily temperature.

There being only one (evening) mean, this series must be very convergent, and

$$t = -\mathrm{T} - \frac{\mathrm{B}}{\mathrm{C}}, \text{ or more correctly, } t = -\mathrm{T} - \frac{\mathrm{B}}{\mathrm{C} - \frac{\mathrm{B}}{\mathrm{C}} \cdot \mathrm{D}};$$

thus B D is very much less than C^2.

Secondly. For the maximum and minimum times:

$$\frac{dv}{dt} = 0 = B + 2C(t + T) + 3D(t + T)^2,$$

$$\therefore t = -T - \frac{C}{3D} + \sqrt{\frac{C^2 - 3BD}{9D^2}};$$

the first corresponds to a minimum, the second to a maximum; the former being nearer than the latter to the morning mean.

Thirdly. If A, T be the temperature and epoch, and t not great,

$$v = \left(\begin{matrix} A + BT + CT^2 \\ + DT^3 \end{matrix}\right) + \left(\begin{matrix} B + 2CT \\ + 3DT^2 \end{matrix}\right) t + (C^2 + 3DT)t^2 + D.t^3$$

is the equation for some time on each side of T; neglecting the small quantity Dt^3, it is that of a *parabola*, having v for an absciss and t for an ordinate.

Fourthly. Beginning at noon, $T = 0$, $v = A + Bt + Ct^2 + Dt^3 + \&c.$ Taking the mean of *homonymous* hours (the unit of t being one day), that is, taking the mean of $t + \frac{1}{4}$ and $t - \frac{1}{4}$, we obtain

$$v_{t+\frac{1}{4}} = A + B\left(t + \frac{1}{4}\right) + C\left(t^2 + \frac{t}{2} + \frac{1}{16}\right) + \&c.$$

$$= A + \frac{B}{4} + \frac{C}{16} + \left(B + \frac{C}{2}\right)t + Ct^2 + \&c.$$

$$v_{t-\frac{1}{4}} = A - \frac{B}{4} + \frac{C}{16} + \left(B - \frac{C}{2}\right)t + Ct^2 + \&c.$$

Whereof the mean $= A + \frac{C}{16} + Bt + Ct^2$ &c. For the mean of the twenty-four hours, we add $-t$ and $+t$, therefore

$$\text{General mean} = \frac{24A}{24} + \frac{2C}{24}\Sigma_{1^2}^{12^2}\frac{t^2}{24^2} + \frac{2E}{24}.\Sigma_{1^4}^{12^4}.\frac{t^4}{24^4}$$

$$= A + \frac{650C}{12 \times 576} + \frac{60810E}{12 \times 576^2} = A + \frac{C}{11} + \&c.$$

Now C, D, &c. being small, it is evident this nearly agrees with the homonymous mean, the chief error $Bt + C\left(\frac{1}{16} - \frac{1}{11} = \frac{5}{176}\right)$ indicating very nearly a progressively uniform error, so that by combining t and $-t$ this error $= \frac{5}{176}C = \frac{1}{35}C$ must very nearly vanish.

These extremely general theoretical results are amply confirmed by the above-mentioned observations.

London, December 8, 1841. S. M. D.

APPENDIX.

These Leith observations give the temperature at

P.M.		P.M.		A.M.		A.M.	
1 h^{r} =	51·149	7 h^{r} =	49·544	1 h^{r} =	46·134	7 h^{r} =	46·283
2	51·470	8	48·624	2	45·933	8	47·029
3	51·532	9	47·829	3	45·689	9	48·055
4	51·239	10	47·276	4	45·449	10	49·012
5	50·872	11	46·803	5	45·394	11	49·950
6	50·294	12	46·398	6	45·653	12	50·777

The sums of the homonymous hours are—

P.M. and A.M.		P.M. and A.M.		Sums.	Diff.
1 h^{r} =	97·283	7 h^{r} =	95·827	193·110	+ 1·456
2	97·403	8	95·653	193·056	+ 1·750
3	97·221	9	95·888	193·109	+ 1·333
4	96·688	10	96·288	192·976	+ 0·400
5	96·266	11	96·753	193·019	− 0·487
6	95·947	12	97·175	193·122	− 1·228

The near agreement in the third column shows the series expressing the daily temperature to be very nearly a periodic one, and of the form $h = \mathrm{H} + \mathrm{A} \sin t + a \cos t + \mathrm{B} \sin 2t + b \cos 2t + \mathrm{C} \sin 3t + c \cos 3t + \mathrm{E} \sin 4t + e \cos 4t$; h, H, &c. being thermometric degrees, and t the time.

Hence, as in my paper on the Plymouth barometric oscillations*, we can deduce the rule, that if the thermometer be observed only four times a day, at intervals of six hours, commencing at any time, the resulting average is all but equal to that deducible from twenty-four hourly observations. The greatest difference is *here* $48°{\cdot}266$ (mean) $- \frac{1}{4}\,(192°{\cdot}976) = 0°{\cdot}022$ = *one forty-fifth of a degree of Fahrenheit.*

The differences of the homonymous hours (P.M. − A.M.) are

1 h^{r} =	+5·015	4 h^{r} =	+5·795	7 h^{r} =	+3·161	10 h^{r} =	−1·736
2	+5·537	5	+5·478	8	+1·595	11	−3·147
3	+5·843	6	+4·641	9	−0·226	12	−4·379

Whence by a process exactly similar to the one in the paper above alluded to, there results

$$\text{temp. from noon} = h = 48°{\cdot}266 + 2°{\cdot}1437 \sin t + 2°{\cdot}1354 \cos t + 0{\cdot}295 \sin 2t + 0{\cdot}308 \cos 2t - 0{\cdot}1302$$

---

* Phil. Mag., June 1842 (Third Series, vol. xx. p. 477).

$$\sin 3t + 0{\cdot}00115 \cos 3t - 0{\cdot}00715 \sin 4t + 0{\cdot}00686 \cos 4t,$$

$$\left.\begin{array}{r}\text{temp. from}\\ \text{noon} = h\end{array}\right\} = 48^{\circ}{\cdot}266 + 3^{\circ}{\cdot}0257 \sin(t + 44^{\circ}\,53') + 0^{\circ}{\cdot}4265 \sin(2t + 46^{\circ}\,14') + 0^{\circ}{\cdot}1302 \sin(3t + 179^{\circ}\,30') + 0{\cdot}0099 \sin(4t + 136^{\circ}\,50').$$

The quantities c, E, and e are the only ones wherein the separate values in each combination disagree, but this is not very material, owing to the smallness of these quantities.

London, April 29, 1842. S. M. D.

XI. *On the Motion of Luminous Waves in an Elastic Medium, consisting of a system of detached particles, separated by finite intervals.* *By* S. Earnshaw, *M.A. of St. John's College, Cambridge.*

THE equations obtained at the close of my last communication on this subject (vol. xx. p. 373) involve six coefficients, A, B, C, D, E, F. From the peculiar manner in which they enter those equations it is known, that if the coordinate axes be turned through proper angles, their directions still remaining rectangular, the equations will assume the forms

$$d_t^2 \xi = -k_1^2 \xi, \quad d_t^2 \eta = -k_2^2 \eta, \quad d_t^2 \zeta = -k_3^2 \zeta.$$

These show that vibrations of m parallel to any one of the axes of dynamical symmetry cannot be affected by vibrations which are parallel to the other axes. Simple as these equations are, they have precisely the same degree of generality as the original ones, for the motion of the particle m. It might not happen that the axes of dynamical symmetry for every particle would be parallel to those for m, and that the same position of the coordinate axes would reduce the equations of motion for the other particles of the medium to the same form, and cause them to have the same coefficients as for m. A condition equivalent to mechanical homogeneity of the medium must be fulfilled that this may be the case. It is necessary therefore to appeal to experiment for license in this matter. By experimental means we learn that the positions of the axes of elasticity for waves of a given length are fixed, and that the velocity of transmission of such waves is uniform, and that both these properties are independent of the thickness of the medium: hence we may assume that $k_1\, k_2\, k_3$ have constant values through the whole interior of a medium, and that the equations in the simple forms above given are applicable to, and fully represent all the properties of, the transmission of waves of light through a luminiferous medium. It is necessary also to observe that the quantities $k_1\, k_2\, k_3$ are all possible, and finite; for were one of them otherwise, vibrations parallel to the corresponding axis of symmetry could in no case be transmitted; but as no media having this property have been yet found, we are permitted to assume that the law of molecular force and the mode of arrangement of the particles are such as to make $k_1\, k_2\, k_3$ possible in all cases. We are now at liberty, without affecting the generality of our investigations, to suppose that the axes of symmetry were the coordinate axes employed in my former paper; in which case $D = E = F = 0$, and the equations of motion are

$$d_t^2\, \xi = -2\,\Sigma\left(A_r \sin^2 \frac{rh}{2}\right) . \xi,$$

$$d_t^2\, \eta = -2\,\Sigma\left(B_r \sin^2 \frac{rh}{2}\right) . \eta,$$

$$d_t^2\, \zeta = -2\,\Sigma\left(C_r \sin^2 \frac{rh}{2}\right) . \zeta;$$

wherefore if $v\; v'\; v''$ be the velocities of transmission of vibrations which are parallel to the axes of symmetry, and if λ be the length of the wave, then

$$2\,\pi^2 . \left(\frac{v}{\lambda}\right)^2 = \Sigma\left(A_r \sin^2 \frac{rh}{2}\right),$$

$$2\,\pi^2 \left(\frac{v'}{\lambda}\right)^2 = \Sigma\left(B_r \sin^2 \frac{rh}{2}\right),$$

$$2\,\pi^2 \left(\frac{v''}{\lambda}\right)^2 = \Sigma\left(C_r \sin^2 \frac{rh}{2}\right).$$

The right-hand members of these equations involve λ implicitly, in a manner which depends upon the arrangement of the molecules of the æther and the law of molecular force; and thus a relation is established between the length of a wave and the velocity of its transmission; but unhappily the expressions are of such a nature as to imply that there is dispersion *in vacuo.* The case therefore stands thus: *dispersion in a refracting medium cannot be accounted for on the finite-interval theory unless there be also dispersion in vacuo.* Now as there is no dispersion *in vacuo,* I infer generally, that the finite-interval theory *cannot* account for dispersion.

Again, by referring to my former communication, it will be seen that the equations of motion do not depend upon the position of the front of the waves traversing the medium *. They show that a particle may vibrate in any direction, and that the vibrations have no necessary reference to the direction of transmission. And it is to be kept in mind that we have found our equations without the aid of any hypothesis respecting arrangement; and therefore it is impossible by means of arrangement to affect our results. And, again, we have assumed no particular law as the law of molecular action. I have elsewhere shown that there are laws under which the motion of the æthereal particles would not be a vibratory but a translatory motion: we have rejected these laws in assuming that k_1 k_2 k_3 are all possible: but of all the laws which would give vibratory motions and satisfy the known conditions of transmission we have rejected none: all possible cases are therefore included in our results. I consider it therefore as proved incontestably, that according to the finite-interval theory *there can be no connexion between the directions of the vibrations and the law of molecular force.* Hence, then, the transversality of vibrations never can be established on that theory, and is therefore opposed to it. Perhaps it is proper to remark here, that I have not taken account of the direct action of matter upon the æther; but as my results are independent of arrangement, it is obvious that the indirect effect of matter is included in them. Consequently the indirect effect of matter never can assist us in accounting either for the transversality of vibrations or for dispersion. If, therefore, these facts are to be accounted for, we must look to the *direct* action of matter on the æther.

These are some of the results which I proposed to lay before your readers in commencing these papers. They clear away a great deal of mist from the finite-interval theory, and point out the only direction in which we can look for success. Mr. O'Brien has proceeded in that direction, and has announced that in that quarter "the hypothesis of finite intervals cannot be correct;" if he succeed in establishing that position, and I doubt not he will, the finite-interval theory may be laid aside, and mathematicians will then be at liberty to pursue a more promising hypothesis. In the first of my papers I gave my reasons for thinking that those persons have fallen into error who suppose that the theory in question *has* accounted for

---

* For k_1 k_2 k_3 are absolutely constant for a given value of λ; and by transposing the coordinate axes back again from the axes of dynamical symmetry to their original positions, we shall of course obtain the equations exhibited in that communication: and by the nature of this process, the constants (*i. e.* A, B, C, D, E, F) will involve only k_1 k_2 k_3 and the angles of transposition: they are therefore independent of the position of the waves' front.

the experimental dispersion of light. The only reference to that communication which I have yet seen, is in the postscript of Professor Kelland's letter in your Journal of the present month, where, after admitting that all the values of *q* given in his memoir on Dispersion are erroneous, the Professor states that the error is of no importance, seeing that the formulæ are *of necessity* capable of fulfilling the conditions required of them. This must be admitted, I think, to be rather an unusual mode of disposing of a matter of such importance as the numerical verification of his theory. Am I to understand him to say, that his formulæ are *of necessity* capable of producing correct results even if the data employed be erroneous? May I not then ask, what is the nature of the connexion of these formulæ with theory? and in what degree is his theory supported and strengthened by coincidences obtained from such formulæ? I take it for granted that the results were considered as strengthening the theory in some way, else why have they been published both in Professor Kelland's memoir and in other places in connexion with theory? Now I showed, and Professor Kelland has now allowed, that fundamental errors were made in the application of the data; and the results thus obtained were announced as proofs of the soundness of the theory. I wish to ask, then, *how* the results could have any power at all in confirming the theory, if the formulæ were *of necessity* capable of producing correct results from correct or incorrect data indifferently?

I am aware that the position which I have taken in the present paper touching the transversality of vibrations is already by anticipation controverted in Professor Kelland's letter to Mr. O'Brien (p. 377), where we read, that "if the law" of molecular force "be that of the inverse square of the distance.........the vibrations are *transversal only*." I regret that the necessity of defending my own investigations from implied error prevents me from letting this statement pass without comment. I have turned to the part of the memoir to which the Professor has directed attention, and shall here state in as few words as possible the objections which seem to me to lie against the conclusion there come to; merely premising, that if I have misunderstood the nature of the reasoning, I am open to correction. My objections are

1st. I find it stated that "v and v'' are possible and equal, but v' impossible and of a different magnitude;" and thence it is inferred that "attractive forces give rise to transversal vibrations only." Now it appears to me that, admitting the former part of this to be true, there is some error in the inference. For since v v' v'' are the velocities of the *wave*, and not of the particles, the inference should have been, that there is *one direction in which waves cannot be transmitted*; or, in other words, that *the æther is opake in one direction.*

2nd. But I am unable to discover on what ground it is stated that v' is impossible. I see no reason why we may not say with equal truth that v' is possible, and v and v'' impossible; in which case the inference is, that *the æther is transparent in one direction only.*

3rd. After all, it appears to me that the implied impossibility of some one (or two, as the case may be) of the quantities $v\ v'\ v''$ has reference to a fact distinct from either of these inferences, viz. *the instability of the medium when the forces vary according to the Newtonian law.* If v' be impossible, as is asserted in the memoir referred to, it shows that the sines and cosines of all angles in which v' occurs ought to have been written in the form of exponentials, and that some equation has been integrated by sines or cosines which ought to have been integrated by exponentials. Hence it follows that a vibrating motion of the particles is impossible, and that the particles of the whole medium are in a state of either neuter equilibrium, or unstable. In either case it is unfit for the transmission of light, and results derived from it are, if at all, only accidentally applicable to the phænomena of nature.

Cambridge, May 3, 1842.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xx. p. 512.]

March 17, 1842. THE reading of a paper, entitled "Contributions to the Chemical History of the Compounds of Palladium and Platinum," by Robert Kane, M.D., M.R.I.A., communicated by Francis Baily, Esq., V.P.R.S., was resumed and concluded.

The author states it to be his object, in this and in some subsequent papers, to examine specially the composition and properties of the compounds of palladium, platinum, and gold; and to ascertain how far they agree, and in what they differ, as to the laws of combination to which these compounds are subjected. He commences with the investigation of the compounds of palladium, employing for that purpose a portion of that metal with which he was furnished by the Royal Society out of the quantity bequeathed to the Society by the late Dr. Wollaston. He describes the mode of obtaining the protoxide of palladium, and enters into the analysis of the hydrated oxide, the black suboxide, and the true basic carbonate of that metal; detailing their properties and the formulæ which express their mode of composition. The chlorides of palladium form the next subject of inquiry; and the author concludes from his experiments that the loss of chlorine which the protochloride undergoes, when kept for some time in a state of fusion at a red heat, is perfectly definite; and also that the loss represents one half of the chlorine which the salt contains. But in the double salts formed by the protochloride of palladium with the chlorides of the alkaline metals, he finds that the similarity of constitution usually occurring between the compounds of ammonium and potassium is violated. From his analysis of the oxychloride of palladium the author concludes that it is quite analogous to the ordinary oxychloride of copper. He then examines a variety of products derived from the action of a solution of caustic potash on solutions of ammonia-chlorides of potassium. Their properties he finds to indicate analogies between palladium and other metals, whose laws of combination are better known. The sulphate, the ammonia-sulphates, the nitrates, and the ammonia-nitrates of palladium, and lastly, the double oxalate of palladium and ammonium, are, in like manner, subjected to examination in a detailed series of experiments.

The second section of the paper relates to the compounds of platinum, and comprehends researches on the composition of the protochloride of platinum; on the action of ammonia on biniodide of platinum; and on the action of ammonia on the perchloride of platinum; in which the properties of these substances are detailed and the formulæ expressing their composition deduced.

There was also read, "Magnetic Observations made at Prague for September 1841." By C. Kreil. Communicated by S. Hunter Christie, Esq., M.A., Sec. R.S.

April 7.—The following papers were read, viz.—

Meteorological Observations, taken in conformity with the Report drawn up by the Committee of Physics, including Meteorology, for the guidance of the Antarctic Expedition, as also for the fixed Magnetic Observatories, transmitted to the Society by the Lords Commissioners of the Admiralty and the Master-General of the Ordnance, and communicated by the Council, were read; viz.—

1. "Meteorological Observations taken on board H.M. Ship Erebus, for August and September 1841." By Capt. James Clark Ross, R.N., F.R.S., Commander of the Expedition. (*Forms* 1 and 2.)

2. "Meteorological Observations taken by the Niger Expedition, for May, June and July 1841."

3. "Meteorological Observations taken at the Magnetic Observatory, Ross-Bank, Van Diemen's Land, for November and December 1840, and January, February and March 1841." (*Forms* 1 and 2.)

4. "Meteorological Observations taken at the Magnetic Observatory, Cape of Good Hope, for October and November 1841." By F. Eardley Wilmot, Esq., Lieut. in the Royal Artillery. (*Forms* 1 and 2.)

5. "Meteorological Observations taken at the Magnetic Observatory, Toronto, for January, February, March, April and May 1841." By C. W. Younghusband, Esq., Lieut. in the Royal Artillery. (*Forms* 1 and 2.)

6. "Of the ultimate distribution of the Air-passages, and of the modes of formation of the Air-cells of the Lungs." By William Addi-

son, Esq., F.L.S., Surgeon, Great Malvern. Communicated by R. B. Todd, M.D., F.R.S.

After reciting the various opinions which have prevailed among anatomists regarding the manner in which the bronchial tubes terminate, whether, as some suppose, by cells having free communication with one another, or, as others maintain, by distinct and separate cells having no such intercommunication, the author states that having been engaged in investigating, with the aid of the microscope, the seat and nature of pulmonary tubercles, he could never discover, in the course of his inquiry, any tubes ending in a *cul-de-sac*; but, on the contrary, always saw, in every section that he made, air-cells communicating with each other. He concludes from his experiments and observations, that the bronchial tubes, after dividing dichotomously into a multitude of minute branches, which pursue their course in the cellular interstices of the lobules, terminate, in their interior, in branched air-passages, and in air-cells which freely communicate with one another, and have a closed termination at the boundary of the lobule. The apertures by which these air-cells open into one another are termed by the author *lobular passages*: but he states that the air-cells have not an indiscriminate or general intercommunication throughout the interior of a lobule, and that no anastomoses occur between the interlobular ramifications of the bronchiæ themselves; each branch pursuing its own independent course to its termination in a closed extremity. Several drawings of the microscopical appearances of injected portions of the lungs accompany this paper.

April 14.—A paper was read, entitled, "Remarks on the probable natural causes of the Epidemic Influenza as experienced at Hull in the year 1833; with a delineation of the Curves of the maximum, the mean, and the minimum Temperatures in the shade, and the maximum Temperature in the sun's rays at Hull, during the years 1823 and 1833." By G. H. Fielding, M.D. Communicated by the Rev. Wm. Buckland, D.D., F.R.S.

The meteorological causes to which the author ascribes the sudden accession of the influenza at Hull, and its continuance from the 26th of April to the 28th of May 1833, are, first, the unusually cold weather during March, and also the cold and wet which prevailed during April in the same year: secondly, the sudden rise of temperature, amounting to 21° of Fahr., which occurred in a few hours on the 26th of April: and thirdly, the continuance, through May, of extreme vicissitudes of temperature between the day and the night; the burning heat of the days and the cold thick fogs, with easterly winds, commencing generally about sunset, and prevailing during the night.

A paper was also read, entitled, "Report of a remarkable appearance of the Aurora Borealis below the Clouds." By the Rev. James Farquharson, LL.D., F.R.S., Minister of Alford.

The phænomenon recorded in this paper occurred on the night of the 24th of February 1842, when a remarkable aurora borealis was seen by the author apparently situated between himself and lofty stratus clouds, which extended in long parallel belts with narrow intervals of clear sky in a direction from north-west to south-east. The author gives, in detail, the particulars of his observations*.

April 21.—The following papers were read:—

1. "On the Organic Tissues in the bony structure of the Corallidæ." By J. S. Bowerbank, Esq. Communicated by Thomas Bell, Esq., F.R.S., was in part read.

"Papers from the several Magnetic Observatories established in India, addressed to the Secretary of the Royal Society, by direction of the Honourable East India Company." Communicated by P. M. Roget, M.D., Sec. R.S.

1. From the Magnetic Observatory at Madras:—

Magnetic and Meteorological Observations for October, November and December 1841; as also for January 1842.

Term-day Observations for October and November, and Curves for August, September, October and November 1841.

Observations of the Direction and Force of the Wind, and the state of the Sky, during October and November 1841.

Extraordinary Magnetic Curves for September, October and December 1841.

2. From the Magnetic Observatory at Singapore:—

Magnetic Observations from March to October 1841, with Curves for the same period.

Anemometer Curves for March, April, May, June, July, August, September and October 1841.

Abstracts of the Weather for June, July, August and September 1841; as also the Determination of the Temperature at Singapore.

Tide Reports for April, May and June 1841.

3. From the Magnetic Observatory at Simla:—

Abstracts of Magnetic and Meteorological Observations for November and December 1841.

Magnetic Observations for February, May, October and December 1841, with Curves for the same period.

April 28.—A paper, entitled, "On the Organic Tissues in the bony structure of the Corallidæ." By J. S. Bowerbank, Esq., F.G.S., communicated by Thomas Bell, Esq. F.R.S., was resumed and concluded.

The author submitted small portions of nearly seventy species of bony corals to the action of diluted nitric acid, and thus obtained their animal tissue, freed from calcareous matter, and floating on the surface of the fluid in the form of a delicate flocculent mass. By the aid of the microscope, this mass was found to be pervaded by a complex reticulated vascular tissue, presenting numerous ramifications and anastomoses, with lateral branches terminating in closed extremities. There were also found, interspersed among these, another set of tubes, of larger diameter than the former, and provided, in many places, with valves; the branches from these larger vessels occasionally terminate in ovoid bodies, having the appearance of gemmules or incipient polypes. In other cases, masses of still larger size, of a more spherical shape, and of a

---

[* A notice of a former paper on the Aurora by Mr. Farquharson will be found in Phil. Mag., Second Series, vol. v. p. 304.—Edit.]

brown colour, were observed attached to the membrane, and connected with each other by a beautiful network of moniliform fibres. Numerous siliceous spicula, pointed at both extremities and exceedingly minute, were discovered in the membranous structure of several corals; and also other spicula of larger size, terminated at one extremity in a point, and at the other in a spherical head; a form bearing a striking resemblance to that of a common brass pin.

Besides these spicula, the author noticed in these membranous tissues a vast number of minute bodies, which he regards as identical with the nuclei of Mr. Robert Brown, or the cytoblasts of Schleiden.

A paper was also in part read, entitled, "Sixth Letter on Voltaic Combinations," addressed to Michael Faraday, Esq., D.C.L., F.R.S., &c. By John F. Daniell, Esq., For. Sec. R.S., Professor of Chemistry in King's College, London, &c.

May 5.—The reading of a paper, entitled, "Sixth Letter on Voltaic Combinations," addressed to Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry in the Royal Institution of Great Britain, &c., by John Frederic Daniell, Esq., Foreign Sec. R.S., Professor of Chemistry in King's College, London, was resumed and concluded.

The purport of this letter is to follow the consequences of the law of Ohm, and the expressions which result from it, relative to the electromotive force, and to the resistances in the course of a voltaic circuit; to apply this theory to the verification of the conclusions which the author had formerly deduced from his experiments; and to suggest additional experiments tending to remove some obscurities and ambiguities which existed in his former communications. In following out these principles, the author is led to offer various practical remarks on the different forms of voltaic batteries which have been proposed with a view either to the advancement of our theoretical knowledge of the science, or to the service of the arts. The author enters more particularly into an explanation of the principles on which the cylindric arrangement of the battery he has introduced is founded, which appear to him to have been greatly misunderstood. The formulæ and the calculations which form the body of this paper are not of a nature to admit of being reported in the present abstract*.

May 12.—"On the Rectification and Quadrature of the Spherical Ellipse." By James Booth, Esq., M.A., Principal of Bristol College. Communicated by John T. Graves, of the Inner Temple, Esq., M.A., F.R.S.

The author, at the commencement of this paper, adverts to a rather complex discussion of a portion of the subject of his inquiry by M. Catalan, published in the *Journal de Mathématiques*, edited by M. Liouville.

He then proceeds to establish two fundamental theorems, appli-

---

[* Abstracts of Prof. Daniell's preceding five letters on Voltaic Combinations have already been given in Phil. Mag., Third Series; see vol. xv. p. 312. Dr. Martin Barry's paper on Fibre, also read May 5, will be noticed in a future Number, together with Lieut.-Col. Yorke's on the Effect of the Wind on Barometers, read May 12th.—Edit.]

cable to,—1st, the quadrature, and 2nd, the rectification of the spherical ellipse.

1st. The quadrature of the spherical ellipse is reduced to the calculation of a complete elliptic function of the third order, whose parameter and modulus are quantities essentially related to the cone; its parameter being the square of the eccentricity of the ellipse, whose plane is at right angles to the axis of the cone, and its modulus being the sine of the semi-angle between the focals.

2nd. The rectification of the spherical ellipse is made to depend on a complete elliptic function of the third order, whose parameter is the same as in the preceding case, but whose modulus is the sine of the angle between the planes of the elliptic base and of one of the circular sections.

The author then proceeds to establish a remarkable relation between the area of a given spherical ellipse and the length of the spherical ellipse generated by the intersection of the supplemental cone with the same sphere.

He shows that if there are two concentric supplemental cones cut by the surface of a concentric sphere,—1st, the *sum* of their spherical bases, together with twice their lateral surfaces, is equal to the surface of the sphere; 2nd, the *difference* of their spherical bases is equal to twice the difference of their lateral surfaces.

Hence, also, he deduces a remarkable theorem, viz. the sum of the spherical bases of any cone whose principal angles are supplemental, cut by a sphere, together with twice the lateral surface of the cone comprised within the sphere, is equal to the surface of the sphere.

The author then, alluding to some researches of Professor MacCullagh and of the Rev. Charles Graves, Fellow of Trinity College, Dublin, proceeds to give a simple elementary proof of a well-known formula of rectification, and thence deduces some remarkable properties of the tangent at that point of the ellipse, which is termed by him the point of *rational section.*

Assuming the properties of the plane ellipse, he proceeds to show that a similar formula of rectification holds for any curve generated by the intersection of a spherical surface with a concentric cone of any order. He goes on to develope a series of properties of the spherical ellipse, bearing a striking analogy, as indeed might have been expected, to those of the plane curve. Thus he establishes a point of *rational section* as in the plane ellipse, shows that the tangent arc is at this point a *minimum*, and developes some other curious analogies. It is a simple consequence of his formula that the spherical elliptic quadrant may be divided into two arcs whose difference shall be represented by an arc of a great circle. This theorem, previously obtained by M. Catalan, is analogous to that of Fagnani, which shows that the difference of two plane elliptic arcs may be represented by a straight line.

The author concludes by reducing the quadrature of the surface of a cone of the second degree, bounded by a plane perpendicular to the axis, to the determination of a complete elliptic function of the second order.

ROYAL ASTRONOMICAL SOCIETY.

(Continued from vol. xix. p. 584.)

Nov. 12, 1841.—The following communications were read:—

I. On the Longitude of Dr. Lee's Observatory at Hartwell.

The longitude of this observatory was assumed from various authorities to be $3^{m}\,20^{s}{\cdot}6$ west from the Royal Observatory at Greenwich, by the late Mr. Epps, for some time after his arrival at Hartwell. These authorities appear to have been as follows:

	m	s
Capt. Smyth, by means of two trips with a chronometer from Bedford Observatory	3	20·2
By the moon's culminations as computed by Mr. Riddle		19·9
Mr. Epps, by chronometers		21·7
Ditto		20·7
Mean longitude	3	20·6

The mean of these determinations was naturally supposed by Mr. Epps to be very near the truth. In October 1838 this mean result was found, however, to differ considerably from the difference of meridians as determined by twelve chronometers, taken by Mr. Dent from the Royal Observatory (which was $3^{m}\,24^{s}{\cdot}46$). It was evident, therefore, that there was either an error of nearly four seconds of time in the longitude of Hartwell, as previously assumed, or in the observations made there on this occasion to determine the error of the clock with which the chronometers were compared. A careful recomputation of the observations, as recorded in the Hartwell transit books, was therefore made, and the result (as far as the reductions were concerned) was found to be correct.

A reference was then had to Aylesbury church spire, the position of which had been determined by the Trigonometrical Survey. This was done by means of an estimated distance of the spire from the Hartwell Observatory, taken from a county survey, and the observed azimuth of the former from the observatory. This gave a result ($3^{m}\,23^{s}{\cdot}07$) differing $2^{s}{\cdot}5$ of time from Mr. Epps's former determination, and $1^{s}{\cdot}5$ from that obtained from Messrs. Arnold and Dent's chronometers, and was therefore far from being satisfactory.

In the following January another series of results was obtained by means of ten chronometers, which were taken by Mr. Dent as before, from the Royal Observatory to Hartwell, on the 6th of that month, and the comparisons made with the transit clock at the latter place on the same day. The chronometers were brought back to the Royal Observatory on the 9th following. The difference of meridians by these observations was $3^{m}\,24^{s}{\cdot}06$.

Other results were also obtained by means of chronometers taken from the Royal Hospital Schools at Greenwich to Hartwell Observatory; and, in reference to these results, as well as to those before obtained, Mr. Epps observes, in a letter to Mr. Fisher, "The results agreeing so well with the former, I think we may conclude that $3^{m}\,24^{s}{\cdot}2$ (as you have already noticed) is extremely near the truth. This may be called the mean result of thirty chronometrical determinations. I may remark to you, that my observations for time are made with as much attention as possible to the state of the transit instrument; viz. that it works with no apparent error in collimation, nor level error, but correcting as occasion may require for azimuthal deviation. With the exception, therefore, of minute differences in the right ascensions of the stars by which the clock-errors were determined, and some trifling optical defects, I conclude that nothing of importance can be urged against the mean of all the results. Indeed, all the observations respecting the chronometrical comparisons are plain and straightforward matters of fact in conjunction with the transit observations, as recorded in the observation books."

The error in the former assumed longitude being now fully confirmed by so many chronometrical results, it was resolved to connect in a more accurate manner than before the position of Aylesbury spire with that of the observatory at Hartwell by actual measurement and triangulation; since it was possible that an error might have occurred so as to have caused the discrepancy observed between the chronometrical longitude and that obtained by the Trigonometrical Survey. This was done in April 1840, and the result was nearly identical with that previously deduced by means of the county survey.

As there is a considerable error in the longitude of this spire as given in the third edition of the Requisite Tables, Mr. Yolland, of the Ordnance Map Office, very kindly undertook the recomputation of its geographical position from the original data of the Trigonometrical Survey, and found it to be as follows:—

	°	′	″	
Latitude	51	49	1·0	North.
Longitude	0	48	50·15	West.
In time		3^{m}	$15^{\mathrm{s}}{\cdot}34$	

From this corrected position of the spire, we have the following for the position of the observatory at Hartwell:—

Latitude............	51° 48′ 14″·8	North.
Longitude	$3^{\mathrm{m}}\ 22^{\mathrm{s}}{\cdot}57$	West.

Final results for difference of meridians:—

	m s
By the chronometrical determinations	3 24·26
By Aylesbury spire, as determined by the Trigonometrical Survey	3 22·57
Difference............	1·69

II. Observations of the Beginning and Termination of the Solar Eclipse of July 18, 1841, at Aberdeen. By Charles Crombie, Esq. Communicated by George Innes, Esq.

The eclipse was observed in the garden attached to Mr. Crombie's residence, which is a short distance from the Marischal College. The instrument used was a 2½ feet achromatic telescope, with a power of about thirty-six; and the times were taken with a pocket chronometer, whose rate was determined by two comparisons with a clock belonging to Mr. Innes, and the error by several altitudes of the sun.

The Aberdeen mean solar times of the beginning and ending of the eclipse, resulting from the observations, are—

	h	m	s
For the beginning	2	17	48·7
And for the ending..........	2	58	10·2

III. Observation of the Lunar Occultation of Venus on September 11, 1841, at Mr. Bishop's Observatory, in the Regent's Park.

The occultation of Venus by the moon was observed here, but not under favourable circumstances. The morning was clear, but the wind easterly. The equatoreal telescope was charged with a power of 105. Venus was badly defined in general, the air being in a very disturbed state. The enlightened edge of the moon completely hid the planet at about $18^h\ 31^m\ 21^s$, Greenwich mean astronomical time. The time was not accurately noted, the observer's attention being principally directed to the phænomena of the occultation. No projection on the moon's limb, nor any distortion of the form of Venus, was perceivable. The edge of the moon was well seen, and sharply defined on the planet's disc.

The commencement of the reappearance at the unenlightened edge was not well caught, the planet becoming visible at some distance from the centre of the field. This being instantly rectified, the dark edge was well seen on the planet, which did not appear in the least distorted. The reappearance was complete at about $19^h\ 41^m\ 54^s$, Greenwich mean time, and was observed with the power 105. The air had become very smoky, and vision was extremely bad.

IV. Notice of the Occultation of Venus on the morning of the 12th of September, 1841. Observed at Malta by Capt. Basil Hall, R.N. Communicated by Capt. Beaufort, R.N.

"The beginning of this interesting occultation was observed at Valetta within a second of time, I think I may venture to say. An unlucky cloud prevented my observing the planet's reappearance. Telescope magnifying sixty times.

" The following are the times by chronometer:—	h	m	s
First contact of the north limb of Venus with the south limb of the moon (civil reckoning)......	6	45	54
Instant when the centre of Venus appeared cut by the enlightened limb of the moon, as nearly as I could judge..............................	6	46	26
Contact of the eastern, or enlightened, limb of Venus with the eastern, or enlightened, limb of the moon	6	46	36·0
Chronometer slow of Malta mean time..........	1	6	33·2
Mean time at Malta of the disappearance of the eastern limb of Venus behind the east limb of the moon	7	53	9·2
Difference of longitude........................		58	1·8
Mean time at Greenwich of the disappearance of the eastern limb of Venus behind the moon....	6	55	7·4

"The time was ascertained by equal altitudes of the sun, and, I think, may be considered correct to about a second. The difference of longitude is taken from the Table No. 8. in Lieut. Raper's recently published work, in which you will observe that the observatory (which is no longer an observatory) on the palace is placed in .... 14° 30′ 42″ = $58^m\ 2^s{\cdot}8$. But
my house lies west of the palace .... 1·0
Consequently the difference of longitude is ...... 58 1·8

"The latitude of my house is the same as that of the observatory, viz. 35° 53′ 54″, as given by Lieut. Raper; but I have not yet had an opportunity of verifying this point.

"On the voyage to Malta from England, and since my arrival here, I have had ample means of examining the work above alluded to; and I feel it right to say,—and I hope you will communicate my testimony (such as it is worth) to the Astronomical Society, in favour of the book of my highly valued friend, their secretary,—I have gone over almost every part of the Practice of Navigation, and some of the parts a great many times, and I can say without qualification, that I am acquainted with no work so well adapted for the use of sailors, none so luminous and precise in its style, nor so simple in its use. The tables, too, are well arranged and of very ready application, in consequence not only of the distinctness of the precepts, but the good selection of illustrative examples. It is much to be desired that Lieut. Raper should publish his second volume, for such works contribute greatly to the improvement of practical navigation, not merely by the information they furnish, but by raising the standard of accuracy, and teaching that even by moderate, but *well-directed*, exertions, any ship may be navigated with far more certainty and speed than by the ordinary and loose methods still, unfortunately, too much in use afloat."

V. Observations of Bremicker's Comet made with the Equatoreal Instrument of the Observatory of Padua. By M. Santini.

As soon as the notice of this discovery was received, the comet was immediately sought for at the Observatory of Padua; but clouds and the light of the moon prevented it from being seen till the evening of the 22nd of November: it was extremely faint, and presented itself under the appearance of a light mass of vapour faintly illuminated, without sensible trace of a nucleus. It was observed till the evening of the 27th of November; after which time other occupations hindered M. Santini from making further observations of it till the 1st of December. After this time the clouds and the light of the moon caused him to give up the hope of seeing it again.

Day, 1840.	Mean Time at Padua.	Apparent R.A. of the Comet.	Apparent Declin. of the Comet.	Comparison-Stars from Piazzi's Catalogue.
	h m s	h m s	° ′ ″	
Nov. 23.	9 3 6·5	21 40 12·78	+55 54 37·1	Piazzi xxi. 385.
24.	7 26 8·1	21 47 24·58	55 25 1·8	Ditto xxi. 373 & 385.
	7 51 5·4	21 47 31·92	55 24 31·8	
25.	7 1 33·4	21 54 57·55	54 51 45·1	Ditto xxi. 54 & 92.
	7 32 4·9	21 55 4·13	54 50 47·1	
	8 5 56·2	21 55 6·19	54 49 4·1	
26.	7 13 13·7	22 2 35·76	54 15 23·4	Ditto.
	7 36 40·5	22 2 41·47	54 15 9·5	
	7 58 32·0	22 2 43·06	54 14 6·5	
27.	7 33 31·0	22 10 12·29	53 36 56·0	Ditto xxii. 92 & 137.
	7 56 38·6	22 10 16·95	53 35 40·8	
	8 8 34·7	22 10 26·32	+53 36 23·8	

M. Santini has computed elements of the parabolic orbit of the comet, based on the observation made at Berlin on October 28, communicated to astronomers by M. Schumacher; on that made at Vienna on November 12; and on the mean of the above positions of November 24.

The following are the elements derived:—

Perihelion passage, November, 15·25525*, Berlin mean time.

Long. of the perihelion..	23° 42′·5	from the true equinox.
Long. of the node	248 47·7	... ...
Inclination	58 5·05	... ...

Motion Direct.

Log. perihelion dist. = 0·16984

perihelion dist. = 1·4786

VI. Introduction to a Catalogue of 1677 Stars included between the Equator and 10° of North Declination, observed at the Royal Observatory of Padua. By M. Santini. Communicated by Sir J. F. W. Herschel, Bart.

The observations of the stars in this catalogue were made with a meridian circle constructed by Starke, a description of which is to be found in the fifth volume of the Transactions of the Academy of Padua. The object of M. Santini has been so to arrange his new catalogue that, at every eight or ten minutes of right ascension, there should be found in each parallel of declination a well-determined star, with the view of facilitating the comparisons of planets and comets with neighbouring stars, by means of micrometrical measurements.

The brightest stars that could be found were chosen for this purpose, very few being admitted which are below the eighth magnitude. They were observed for convenience of reduction in contiguous groups, in such a manner that the corrections necessary for reducing them to the mean equinox of 1840 might be applied to the mean of the apparent positions observed, for the mean instant of the series; and the greater number of the stars were observed three times in both elements. It is the author's intention to proceed immediately with similar observations of stars in the zone extending from the equator to 20° of south declination; and he invites astronomers to participate in his labours by observing some other zones.

The observed right ascensions of Bessel's fundamental stars were compared with their right ascensions given in the Berlin Ephemeris, for obtaining the clock-correction; and the azimuthal deviation of the instrument was obtained by the superior and inferior transits of Polaris.

The polar point of the circle was obtained by observed zenith distances of Polaris and the same fundamental stars, using Carlini's Refraction Tables, and the apparent declinations of the Berlin Ephemeris. The agreement of the individual results both for clock errors and for polar point was in general highly satisfactory. To obtain the mean places for 1840, small special tables were used similar to those

---

* In the manuscript the time of the perihelion passage is also written 320^{d}·24525.

employed for Bessel's zones, the values of the constants, *f, g, h, i,* G, H, of the Berlin Ephemeris being adopted; and in the annual variations no allowance has been made for proper motions of any of the stars.

LONDON ELECTRICAL SOCIETY.

Feb. 15, 1842. The papers read were,—1st, "On the Electrical relation between Plants and Vapours." By Mr. Pine. The author, still pursuing the same path as that traced out in his former communications, makes copious extracts, from various quarters, both of natural and experimental facts, in support of his views of the relation between the subtle fluid—electricity, and the functions of vegetable life. His opinions and reasonings are worthy of examination.

2nd, "Further Observations on Electrotype Manipulation—Depositing on Plumbago—Electro-lace." By Charles V. Walker, Esq., Hon. Sec. The difficulty attendant on the reduction of copper upon the parts of plumbagoed surfaces most remote from the connecting wire, is obviated by a very simple process. One or more fine leading wires are twisted round the main wire, and made to abut upon any part of the surface where the reduction has not occurred. The value of this apparently trivial piece of information can be appreciated by experimentalists alone. The material, to which the term "electro-lace" has been given (and of which specimens were before the Society), is obtained by depositing copper upon net or lace, previously prepared with wax and black-lead. It was first fabricated by Mr. Phillips of Cornwall, in lieu of the copper gauze required in the construction of Prof. Grove's modification of Smee's battery. But it will be readily seen that such fabrics as gauze and lace, when covered with copper, and plated or gilded, may be introduced, in a multitude of ways, into the construction of ornamental work, where at present embossed and perforated cards are employed.

3rd. "Nitrate of Soda compared with other Salts employed for Constant Batteries." By Geo. Mackrell, Esq., Mem. Elec. Soc. Cells were excited with solutions of sulphate of copper, bichromate of potash, nitrate of potash, and *nitrate of soda*. The palm of superiority, for constancy of action, is awarded to the latter: in addition to this, when employed for electrotype purposes, it throws down more copper in proportion to the zinc consumed, than either of the other three: the zinc plates (no slight advantage) are *clean* when removed from the battery.

Mr. Weekes's Register for January was next read. At the suggestion of several scientific correspondents, with a view to promote the objects of coincident observation, Mr. Weekes begins the Register of 1842 by giving the readings of the barometer and thermometer at 9 A.M. instead of 2 P.M.

March 15.—The papers read were,—1st, "Details of an experiment, in which certain insects, known as the *Acarus Crossii*, appeared, incident to the long-continued operation of a voltaic current upon Silicate of Potash within a close atmosphere over mercury." By W. H. Weekes, Esq.

After alluding to the original experiment of Mr. Crosse, and to the objections made that the insects might have sprung from ova in the atmosphere, Mr. Weekes states that he had resolved to provide against such contingencies. This he effected by placing the solution, which was prepared with the utmost caution, beneath a bell-glass, which has not been disturbed from Dec. 3rd, 1840. Late in October 1841 the first insect was detected; on Nov. 27th several were seen: since then they are constantly to be seen, sometimes solitary, at other times in pairs, and occasionally three or four together. The operation was conducted in the dark, light being only admitted at those times when the progress was under examination. The voltaic current was from a short series of Daniell's battery. These creatures appear to love darkness; for on the admission of a ray of light they hasten away and seek hiding-places in the recesses of the apparatus. Simultaneously with this another arrangement was made, in which the current from a water battery was made to pass through a solution contained in a bell-glass of oxygen. Insects appeared in this on the 20th Feb. 1842, and eight or ten fine vigorous *Acari* were visible. This is but a brief summary from a very long and carefully written communication. The author assumes nothing; he does not venture to theorise, but gives a plain and explicit account of his experiments and of their results. The operation is still going on, as there is every reason to expect a further development of insect life. More completely to preclude objections, he is preparing another apparatus in which nothing but glass, metal, and mercury (distilled from its sulphuret) will enter.

2nd. "Note on Electro-tint, and on etching Daguerreotype Plates." By W. G. Lettsom, Esq., M.E.S.

This note was illustrated by specimens of tints produced by Prof. Von Kobell of Munich, and Dr. Berres of Vienna. The former has improved upon his original process of electro-tint by a method of retouching the plates and then reobtaining others.

3rd. Extracts of a letter from John Samo, Esq., of Surinam, M.E.S., containing "Information respecting the *Gymnotus Electricus.*"

Among the specimens possessed by Mr. Samo were two in one tub, whose relative lengths were 30 and 15½ in. The smaller was missed, and it was found that the other had swallowed it. He soon however cast it up, and in the space of a few hours died. On post-mortem examination it was found that the stomach was considerably ruptured. Mr. Samo mentions that the report that a certain drug is an antidote to the shock of the Gymnotus is without foundation.

4th. The Secretary then communicated to the Society the death of the London Gymnotus, which has from time to time furnished such interesting results to Prof. Faraday, Dr. Schœnbein, Mr. Gassiot, and others.

5th. "On Voltaic Apparatus." By James P. Joule, Esq., M.E.S. The author details the results of a series of experiments upon local action, and upon the relative intensities of several voltaic arrangements under different circumstances.

6th. Mr. Weekes's Electro-Meteorological Register for February 1842 was then submitted to the Society *.

May 17.—A note from Mr. Weekes was read, stating that, when he commenced those experiments, during which insects had been developed, he made similar arrangements, and placed them in various parts of his house, without allowing the voltaic current to pass through them; and in no case, by the strictest examination, could he detect any appearance of the insect.

A paper "On Lightning Conductors, and on the Lightning-Flash which struck Brixton Church," by Charles V. Walker, Esq., Hon. Sec., was next read. Having examined the steeple of this church, which was struck by lightning on Sunday, April 24th, the author of the paper saw in the damage done so good an illustration of the opinions delivered by Dr. Faraday a few days previously at the Royal Institution, that he was induced to survey more carefully the path, and report it to the Society. We cannot, without drawings, enter into detail on the subject, but will condense the general conclusions which result from the investigation. The steeple was surmounted by a copper cross, which formed the *first* good conductor: the *second* was *twenty* feet from this, and in passing along the interval the masonry about the cross was shivered to pieces, and the cross itself was forced out of its place: the *third* conducting series was *twelve* feet from the second: here a second explosion occurred, and the base of a column three feet in diameter was shattered and the column rent. How strange it is that such occurrences as these are not better guarded against! If the "lateral discharge" is not well understood, the "disruptive" is. The "lateral discharge" occurred in the belfry; and Mr. Walker showed how it was connected with that property of electricity which induces it to take the *widest* as well as the *shortest* road. He explained that, when the fluid is passing along a most *ample* conductor, some of it will enter vicinal conductors, developing light and heat. The main object of the communication was to trace the connexion between the experiments of the Royal Institution and the phænomena illustrated by nature on a large scale. He then explained the method of conveying the fluid *safely* and *tranquilly* into these vicinal conductors, by forming metallic communications between them and the lightning rod; otherwise a lightning rod may become a most dangerous enemy instead of a trustworthy protector.

Extracts of Notes from the Rev. Mr. Lockey, Mr. Clarke and Mr. Mayo were read, containing valuable additions to our present knowledge on Electrotype Manipulation. Mr. Lockey introduces black-lead in his composition moulds, and Mr. Mayo *flake-white*. The moulds with the latter were exhibited, and were superior to any we have seen. A copper medal, with a silver surface for the design, by Mr. Clarke, was exhibited. Mr. Weekes's Register was then read.

June 21st.—"A Notice on Native Malleable Copper," by John A. Phillips, Esq., of St. Austell, was read, in which the author states that copper in this form, as well as arborescent and moss copper, is produced by an action in principle the same as that artificially em-

---

* The proceedings for April will be noticed in a future Number.

ployed in the electrotype process. Several mineralogical specimens were submitted to the Society. A long and highly interesting paper was then read, "On the Transfer of Mineral Substances, through various Fluids, by Electric Agency," by Andrew Crosse, Esq., Mem. Elec. Soc. The first experiment related in this paper was as follows:—Mr. Crosse kneaded some pipeclay into the consistency of putty, and imbedded in it a piece of limestone and a shell; this was in a basin: he then made a mixture of powdered sand and sulphate of iron which he placed above the pipeclay, and having filled the vessel with water he allowed the whole to stand for many months. This arrangement was made in imitation of a natural arrangement of like character which had fallen under his notice, and in which the shells and carbonate of lime had become coated with sulphate of lime. In hopes of attaining the same result artificially, this experiment was instituted; and to the great satisfaction of the author when he examined the results, the shell and the limestone had lost in weight, and around each were crystals of *sulphate of lime*. It is Mr. Crosse's strong conviction, that though many mineral productions may result from the direct action of electric currents, yet far the largest portion proceed from operations analogous to this,—from the direct electrical affinity or attraction between particles of matter coming into contact by this slow and constant action. The only point in which this experiment differed from nature is, that the vessel in which the operation was carried on was not *porous*. On this point Mr. Crosse stated a fact which will not be forgotten by electrotypists, that voltaic deposits are more abundant when the vessel employed is porous, so that the sulphate of copper can slowly filter through. A series of experiments, some completed, others in progress, were then described, in one of which the mould of a sovereign was produced in solid marble, by an action not dissimilar in principle to that just described; and in a modification of the arrangement *a rod of glass*, connected with the *positive* end of the battery, was *gilded*. The author does not doubt the possibility of forming any minerals, even the precious gems, by electric agency. He thinks the pearl to be nothing more than alternate layers of animal and mineral substances, electrically concreted. In one of the experiments a magnificent group of fine *Acari* were developed: the production of these insects is still an object of attention to Mr. Crosse, and he anticipates ere long communicating with the Society on the subject.

Mr. Weekes's Register was then read: and the Chairman stated that Mr. Walker's second paper on Lightning Conductors would be read at the next meeting.

ROYAL IRISH ACADEMY.

[Continued from vol. xx. p. 600.]

May 10, 1841.—A Note on some new Properties of Surfaces of the second Order, by John H. Jellett, Esq., F.T.C.D., was read.

I. Let the points on the focal conic, at which the tangent is parallel to the trace of the tangent plane, be considered analogous to foci.

II. Let the axis of the surface, perpendicular to the plane of the conic, be considered analogous to the conjugate axis; then, since the square of the distance from focus to centre, in a conic, is equal to the difference between the squares of the transverse and conjugate semi-axis, we may consider, as analogous to the transverse semi-axis, the line drawn to the extremity of the perpendicular axis from the point analogous to the focus.

III. Since the square of the semiconjugate diameter is equal to the sum of squares of semiaxes minus the square of central radius vector, let the same be supposed true of the line analogous; i. e. if A be the line analogous to the transverse, and B to the conjugate semi-axis, let

$$B' = \sqrt{A^2 + B^2 - A'^2}.$$

Assuming these definitions, we shall have the following theorems analogous to those *in plano*.

1. The sum or difference (according as the focal conic is perpendicular to a real or imaginary axis) of the distances from the points analogous to the foci, to the corresponding point on the surface, is equal to 2 A.

2. The rectangle under them $= B'^2$.

3. The sine of the angle, made by either with the tangent plane, is $\frac{B}{B'}$.

4. The rectangle under the perpendiculars from these points on tangent plane $= B^2$.

5. The sine of the angle between the central radius vector and tangent plane $= \frac{A\,B}{A'\,B'}$ (A′ being the central radius vector).

6. The portion of the normal intercepted between the surface and the plane of the focal conic is $\frac{B}{A} \cdot B'$.

7. If a plane be drawn perpendicular to the line joining points analogous to the foci, and at a distance from the centre equal to $\frac{A^2}{C}$ (C being the distance of one of the focal points from the centre), the distance of a point in the surface from the corresponding focus will be to its distance from this plane : : C : A.

8. Hence, given a focal conic and the perpendicular axis, we can find points and tangent planes *ad libitum*, by the following construction:—Take in the focal conic two diametrically opposite points; with one as centre, and twice the distance from it to the extremity of the perpendicular axis as radius, describe a sphere. Through the other point draw a plane, normal to the focal conic; it will cut the sphere in a certain circle. Connect any point in this circle with the two points on the focal conic, and at the middle point of the line connecting it with the second point draw to it a perpendicular plane. This is a tangent plane to the surface, and the point where it cuts the first connecting line is a point on the surface.

Another mode of generating the surface is easily derivable from (7.).

TABLES Nos. I.

Showing the Chemical and Physical Properties of the Atomic

	1.	2.	3.	4.	5.	6.	7.
	No. of Experiment.	Chemical Constitution. − +	Composition by weight per cent. − +	Atomic weight. H = 1	Specific gravity.	Colour.	Fracture.
Table I.—Copper and Zinc.	1	Cu +	100·00 + 0	31·6	8·667	Tile red	E.
	2	10 Cu + Zn	90·72 + 9·28	348·3	8·605	Reddish yel. 1	C.C.
	3	9 Cu + Zn	89·80 + 10·20	316·7	8·607	Reddish yel. 2	F.C.
	4	8 Cu + Zn	88·60 + 11·40	285·1	8·633	Reddish yel. 3	F.C.
	5	7 Cu + Zn	87·30 + 12·70	253·4	8·587	Reddish yel. 4	F.C.
	6	6 Cu + Zn	85·40 + 14·60	221·9	8·591	Yellowish red, 3	F.F.
	7	5 Cu + Zn	83·02 + 16·98	190·3	8·415	Yellowish red, 2	F.C.
	8	4 Cu + Zn	79·65 + 20·35	158·7	8·448	Yellowish red, 1	F.C.
	9	3 Cu + Zn	74·58 + 25·42	127·1	8·397	Pale yellow	F.C.
	10	2 Cu + Zn	66·18 + 33·82	95·5	8·299	Full yellow, 1	F.C.
	11	Cu + Zn	49·47 + 50·53	63·9	8·230	Full yellow, 2	C.C.
	12	Cu + 2 Zn	32·85 + 67·15	96·2	8·283	Deep yellow	C.C.
	13	8 Cu + 17 Zn	31·52 + 68·48	801·9	7·721	Silver white, 1	C.
	14	8 Cu + 18 Zn	30·30 + 69·70	834·2	7·836	Silver white, 2	V.C.
	15	8 Cu + 19 Zn	29·17 + 70·83	866·5	8·019	Silver grey, 3	C.
	16	8 Cu + 20 Zn	28·12 + 71·88	898·8	7·603	Ash grey, 3	V.
	17	8 Cu + 21 Zn	27·10 + 72·90	931·1	8·058	Silver grey, 2	C.
	18	8 Cu + 22 Zn	26·24 + 73·76	963·4	7·882	Silver grey, 1	C.
	19	8 Cu + 23 Zn	25·39 + 74·61	995·7	7·443	Ash grey, 4	F.C.
	20	Cu + 3 Zn	24·50 + 75·50	128·5	7·449	Ash grey, 1	F.C.
	21	Cu + 4 Zn	19·65 + 80·35	160·8	7·371	Ash grey, 2	F.C.
	22	Cu + 5 Zn	16·36 + 83·64	193·1	6·605	Very dark grey	F.C.
	23	+ Zn	0 + 100·00	32·3	6·895	Bluish grey	T.C.
Table II.—Copper and Tin.	1	Cu + Sn	100·00 + 0	31·6	8·667	Tile red	E.
	2	10 Cu + Sn	84·29 + 15·71	374·9	8·561	Reddish yel. 1	F.C.
	3	9 Cu + Sn	82·81 + 17·19	343·3	8·462	Reddish yel. 2	F.C.
	4	8 Cu + Sn	81·10 + 18·90	311·7	8·459	Yellowish red, 2	F.C.
	5	7 Cu + Sn	78·97 + 21·03	280·1	8·728	Yellowish red, 1	V.C.
	6	6 Cu + Sn	76·29 + 23·71	248·5	8·750	Bluish red, 1	V.
	7	5 Cu + Sn	72·80 + 27·20	216·9	8·575	Bluish red, 2	C.
	8	4 Cu + Sn	68·21 + 31·79	185·3	8·400	Ash grey	C.
	9	3 Cu + Sn	61·69 + 38·31	153·7	8·539	Dark grey	T.C.
	10	2 Cu + Sn	51·75 + 48·25	122·1	8·416	Greyish white, 1	V.C.
	11	Cu + Sn	34·92 + 65·08	90·5	8·056	Whiter still, 2	T.C.
	12	Cu + 2 Sn	21·15 + 78·85	149·4	7·387	Whiter still, 3	C.C.
	13	Cu + 3 Sn	15·17 + 84·83	208·3	7·447	Whiter still, 4	C.C.
	14	Cu + 4 Sn	11·82 + 88·18	267·2	7·472	Whiter still, 5	C.C.
	15	Cu + 5 Sn	9·68 + 90·32	326·1	7·442	Whiter still, 6	E.
	16	+ Sn	0 + 100·00	58·9	7·291	White, 7	F.

Abbreviations used in Column 7th to denote character of fracture :—F.C. Fine Crystalline, C.C. Coarse Crystalline, T.C. Tabular Crystalline, F.F. Fine Fibrous, C. Conchoidal, V.C. Vitreo-Conchoidal, V. Vitreous, E. Earthy.

The maxima of ductility, malleability, hardness, and fusibility, are = 1.

The numbers in Column 6th denote intensity of shade of the same colour.

The atomic weights are those of the hydrogen scale.

The specific gravities were determined by the method indicated in Report "On Action of Air and Water on Iron," Trans. Brit. Assoc. vol. vii. p. 283.

The ultimate cohesion was determined on prisms of 0·25 of an inch square, without having been hammered or compressed after being cast. The weights

AND II.

Alloys of Copper and Zinc, and of Copper and Tin.

8.	9.	10.	11.	12.	13.	14.
Ultimate Cohesion, per square inch. Tons.	Order of ductility.	Order of malleability, at 60° Fahrenheit.	Order of hardness, &c.	Order of fusibility.	Characteristic properties, in Working, &c.	Relation to cast iron, in presence of a solvent, i. e. sea-water.
24·6	8	1	22	15	Well known.	All these alloys increase the corrosion of cast iron in sea-water, when in their presence.
12·1	6	13	21	14	Similar, &c. Several of these are malleable at high temperatures.	
11·5	4	11	20	13		
12·8	2	10	19	12		
13·2	9	9	18	11		
14·1	5	8	17	10		
13·7	11	2	16	9	Bath metal.	
14·7	7	3	15	8	Dutch brass.	
13·1	10	4	14	7	Rolled sheet brass.	
12·5	3	6	13	6	British brass.	
9·2	12	5	12	6	German brass.	
19·3	1	7	10	6	... ... brass, watchmakers'.	
2·1	0	22	5	5	Very brittle, Too hard to file or turn, lustre nearly equal to speculum metal.	All these alloys decrease the corrosion of cast iron in sea-water, when in their presence.
2·2	0	23	6	5	Very brittle,	
0·7	0	21	7	5	Very brittle,	
3·2	0	19	3	5	Brittle,	
0·9	0	18	9	5	Brittle,	
0·8	0	20	8	5	Very brittle,	
5·9	0	15	1	5	Barely malleable.	
3·1	0	16	2	4	Brittle.	
1·9	0	14	4	3	White button-metal.	
1·8	0	17	11	2	Brittle.	
15·2	13	12	23	1	Brittle, well known.	
24·6	1	2	10	16	Well known.	Every alloy of Cu + Sn increases the corrosive action of sea-water on cast iron, in their presence; the maximum increase is due to tin.
16·1	2	6	8	15	Gun-metal, &c.	
15·2	3	7	5	14	Gun-metal, &c.	
17·7	4	10	4	13	Gun-metal and bronze.	
13·6	5	11	3	12	Hard mill brasses, &c.	
9·7	0	12	2	11	Brittle, All these alloys found occasionally in bells, with mixtures of Zn and Pb.	
4·9	0	13	1	10	Brittle,	
0·7	0	14	6	9	Crumbles,	
0·5	0	16	7	8	Crumbles,	
1·7	0	15	9	7	Brittle,	
1·4	0	9	11	6	Small bells, brittle.	
3·9	0	8	12	5	... ... brittle.	
3·1	0	5	13	4	Speculum metal of authors.	
3·1	8	4	14	3	... ... files, tough.	
2·5	6	3	15	2	... ... files, soft and tough.	
2·7	7	1	16	1	Well known.	

given are those which each prism just sustained for a few seconds before disruption.

The copper used in these alloys was granulated, and of the finest "tough pitch;" the zinc was Mossleman's, from Belgium; and the tin "grain tin," from Cornwall. They were alloyed in a peculiar apparatus, to avoid loss by oxidation, and the resulting alloy verified by analysis.

No simple binary alloy of Cu + Zn or of Cu + Sn works as pleasantly in turning, planing, or filing, as if combined with a very small proportion of a third fusible metal, generally Cu + Zn + Pb; or Cu + Sn + Zn, as is known to workers in metals.

May 24.—Mr. Robert Mallet read a paper "On the Physical Properties and Electro-Chemical and other Relations of the Alloys of Copper with Tin and Zinc."

These experiments are collateral to the researches on the action of air and water on iron, upon which the author has been engaged at the desire of the British Association. In the progress of these inquiries, it became necessary to determine the action of solvents on iron in presence of various definite alloys of copper and tin and of copper and zinc. Hence it was requisite to form many such alloys in rigidly assigned proportions as to their constituents, a matter known to experimenters to be one of difficulty, especially in the case of so oxidable and volatile a metal as zinc. The difficulties were overcome by a peculiar arrangement of apparatus, permitting the metals to be fused and combined in close vessels. The results were verified by assay. Having these alloys which belong to the classes of brass or gun-metal, of which most of our instruments of precision are made, and their constitution being atomic and certain, it seemed useful to determine some of their properties for practical purposes. The results are given in the two tables prefixed, pp. 66, 67.

The author has also determined the numerical conditions governing the rate of solution, or amount of loss sustained in a given time by equal surfaces of iron in solvent menstrua, when in presence of all these alloys, and of the alloys themselves. Tables of these were presented: the results do not seem to coincide with the law of volta equivalents, which is explained by showing galvanometrically that the $\varepsilon -$ and $\varepsilon +$ metals of the alloy are often not acted on equally by a solvent; thus, that an alloy of $Zn_y + Cu_x$ may assume a copper surface after a certain time of reaction. This circumstance, the author has shown, suggests a method of determining the molecular arrangement of an alloy; and, in general, whether any alloy be a chemical compound or a mixture.

The author also enters into several details as to peculiar, and, in some cases, singular reaction of these and other alloys upon solutions of the salts of their own metals: thus, certain alloys of lead and zinc decompose solutions of lead as rapidly as pure zinc; while others, containing much zinc, act as lead towards the salts of lead.

In the case of three metals, A, B, C, whereof A is $\varepsilon +$, and C is $\varepsilon -$ to B, the author investigates the question as to what will be the electro-chemical relation of the atomic alloys of $A_x + C_y$ towards B, in solvent menstrua; and in the class of alloys of copper and zinc, has determined *the alloy of no action*, with reference to iron; and has also found alloys which protect iron in solvents electro-chemically as fully as pure zinc, and yet are not themselves acted on by the solvent.

He enters into the subject of the specific gravities of the alloys of $Zn + Cu$ and $Sn + Cu$ minutely, and shows reason to doubt the accuracy of the published specific gravities of most alloys of these and some other classes.

XIII. *Notices respecting New Books.*

A Cycle of Eighteen Years in the Seasons of Britain; deduced from Meteorological Observations made at Ackworth in the West Riding of Yorkshire, from 1824 *to* 1841; *compared with others before made for a like period (ending with* 1823) *in the vicinity of London. By* LUKE HOWARD, Esq., F.R.S. With Five Plates. London, Leeds, and Pontefract, 1842, pp. 22, 8vo.

WE are happy to find from the little work now before us, that this veteran meteorologist is still prosecuting his labours with his pristine ardour; and we congratulate him on the result, now, as he truly states, ascertained beyond controversy, that a periodical revolution takes place, bringing alternate warmth and coldness through successive trains of seasons in our variable climate. We do this with the greater satisfaction, because we think that between the torrents of shameless empiricism on the subject of *predicting the changes of the weather*, on the one hand, and the profound and extensive systems of meteorological observations on the other, which have been brought forward and pursued within the last few years, Mr. Howard's researches, more humble perhaps than the latter,—yet admirably adapted for mathematical investigation,—and which ought to have been a sufficient antidote to the former, have been in great measure forgotten, and when not forgotten still not duly regarded.

In his account of the climate of London, first printed 1818–1820, and reproduced with many additions and improvements in 1833, Mr. Howard gave a view of the series of changes embraced by the cycle which it is his present object to illustrate, on the basis which his observations then seemed to present, of *alternate periods of seven and ten years*, the former *ascending*, the latter *descending* in the scale of heat. He then admitted, from appearances, the probability of spaces between these successive periods not agreeing with this rule, and answering to the "intercalations" of an imperfect calendar. Having since pursued the subject further, he finds "these spaces or interposed years to be necessary parts of the scheme at large, which now resolves itself into a *cycle of eighteen years*, in which our seasons appear to pass through their extreme changes in respect of warmth and cold, of wet and dryness."

In the Proceedings of the Royal Society for March 11 and April 29, 1841 (or Phil. Mag., Third Series, vol. xviii. p. 552–559), are given abstracts of papers containing the author's views on this subject, as regards the seasons near London, then read before the Society; and one of those papers, relating to the periodical variations of the barometer from year to year in this neighbourhood, has since been published in the Philosophical Transactions*.

---

* In the paper last mentioned, "On a Cycle of Eighteen Years in the Mean Annual Height of the Barometer in the Climate of London, and on a constant Variation of the Barometrical mean according to the Moon's Declination," the author showed that the barometrical mean in this climate is depressed (on an average of years) by the moon's position in south

Mr. Howard's present object is to bring in confirmation of the views enunciated in his former work and in the papers already alluded to, "*the fact of a new period, observed in a new locality,* and that differing so considerably in latitude from the former, as to justify the inference that the periods are not confined to any part of our island, but will be found, variously modified, in all."

Referring to the papers above mentioned, Mr. Howard continues, "For a variety of facts relating to atmospheric periodicity, stated in a more elaborate way, I shall here briefly analyse the results of the Ackworth Register, and apply them to my object; saying little about the *barometer*, however, because the present observations on this instrument, however constantly made from day to day, have not the comprehensive character of those insisted on in my former papers; *which were taken from the face of a registering clock.* The Tables annexed to this paper, then, comprise the *results of a daily meteorological Register,* kept at my instance, and with instruments furnished by myself, *at the Friends' Public School in Ackworth.* I have observations, not so continuous, made at my own residence there; by collation with which in many parts I have satisfied myself that I can depend on *these,* for the purpose to which they are here applied, of deducing the differences of seasons from previous and subsequent ones of like denomination, *by comparison with each other.*"

We cannot follow the author through the particulars which constitute his memoir, nor describe in detail the plates, all consisting of curves or flexuous lines traced on rectangular scales, exhibiting the range of the temperature, depth of rain, &c. for the eighteen years composing the cycle. The following extract, explanatory of one of them, will serve to indicate their nature :—

"The *dotted* curve, or flexuous line in fig. 1. shows the variation from year to year of the mean temperature, or *average heat* of the year, the *mean*

---

declination; and that there is also manifested in the lunar influence a gradation of effects which operates through a cycle of eighteen years.

Mr. Howard's researches on this subject had been commenced prior to the first publication of his Climate of London, at the suggestion of Silvanus Bevan, Jun., and had been further discussed in the second edition of that work, published in 1833: their results, as given in the paper now referred to, recalled the attention of Sir John W. Lubbock to a paper by himself in the Companion to the British Almanack for 1839, in which he had inserted certain results obtained with a view of ascertaining the influence of the moon on the barometer and on the dew-point, some of which appeared to indicate that the moon's position in declination influences the barometer. Investigating the subject in a manner altogether different from that adopted by Mr. Howard, but capable of more rigorous application, his results, as stated in a paper of which an abstract is given in the Proceedings of the Royal Society for March 25, 1841 (or Phil. Mag., Third Series, vol. xviii. p. 555), seem to indicate an elevation of nearly one-tenth of an inch for 17 degrees of declination.

We have noticed Sir John W. Lubbock's discussion of the subject simply in relation to the history of this point in meteorological science; it has no direct or particular bearing on the contents of the work before us.

of the climate (or average of all the observations of these eighteen years) being 48°·126*. The nine years from 1824 to 1832 average 48°·879; the nine years from 1833 to 1841 give 47°·374. The difference of 1·405 is about equal to the difference in warmth between Ackworth, N. lat. 53° 38′ 57″, and London. I therefore call the former nine the *warm*, and the latter nine the *cold* years of the cycle. The curve shows palpably the bulk of the years of high temperature on the *right*, and of those of low temperature on the *left* of the dividing line, but with two striking exceptions. There is a very cold year, 1829, among the *warm*, and a very warm year, 1834, among the *cold*; and these considerably reduce the difference between the two averages: the comparison or contrast holds best, therefore, *among the years in detail.*

"The *full* flexuous line in fig. 1. shows the variation from year to year of the total rain collected by the gauge in each. It is not here as with the temperatures; the amount of *rain* is balanced, or nearly so, in each nine years. Thus out of 472·93 inches fallen in the whole cycle, 238·60 inches appear to have fallen on the *warm*, and 234·33 inches on the *cold* side, making the annual averages respectively 26·51 and 26·04 inches nearly; which is about *an inch more* on the whole *per annum* than is found to fall near London—the level being at the ground, in both. If we now look through the *curve* (I beg pardon of mathematicians for applying the term to such a line), we shall probably be first struck with an extreme of dryness (1826) followed by an extreme of wetness (1828) on the *warm* side; then, with a gradation from very wet (again following very dry) in 1830 to very dry in 1835; and this again mounting by steps to extreme wet again in 1839. In fact, ten years, from 1830 to 1839, show a gradual decrease, and again an increase of rain, protracted through the half-cycle, while eight years, from 1840 to 1829 (passing thus back to make the cycle), show repeated and more extended oscillations performed in shorter times; yet with results so nearly the same, that the first set of years here specified show an average rain of 26·36 inches, while the second set average 26·16 inches. Again, on comparing rain with temperature, we find 1826 in the extreme at once of *warmth* and *dryness*, and 1839 in those of *wet* and *coldness*; but 1828 (in the extreme of wetness) is equal in *heat* to the dry 1826; and 1829 is both *dry* and very *cold*. The quantity of rain therefore is not regulated by the *temperature* of the year: we may get it with *heat*, brought by winds highly vaporized from the tropic; or with *cold*, from the condensation effected by the approach of northern air to our own atmosphere, previously charged with vapour to the full; and the dryness of 1829, with so much of *cold*, may have been the result of the great deposition of rain in the previous season. The only rule then that prevails throughout seems to be *compensation*; a wet year against a dry one, &c., and so of whole runs of seasons; and we must examine the *winds* for the cause."

The author next proceeds, from the review of the rain and temperature of *whole years*, as above, to an analysis of the distribution of these through the several months of the year, with the view of ascertaining the "difference, *under equal quantities of rain*, of the warm from the cold side of the cycle, as regards the most important of its effects, *the fruitful or unfruitful character of our seasons.*"

A full flexuous line in fig. 2., presents the monthly rain, in its total amounts under each month, for the *nine years* 1824 to 1832,

---

* The numbers are here stated as given in the *Corrigenda* at the end of the work.

or warm period; a dotted curve, the same for the nine years 1833 to 1841, or cold period of the cycle.

Fig. 3. gives the rain under each month, *for the whole eighteen years,* represented by a full line; in connexion with the average temperature of each month for the like series of years, in a *curve* corresponding as nearly as those in the author's Climate of London, with the *curve of the sun's declination,* which is placed in a fine dotted line, in connexion with both.

In figures 4, 5, 6, and 7 are given the rain and temperature *of the four seasons of the year,* through the cycle here treated.

The results in figures from which these lines and curves are laid down, are all given in tables, either introduced into or annexed to the work.

The mean height of the barometer for the warm period of the cycle, taken at Ackworth School, is 29·851 inches: that for the cold period 29·807: the mean of the entire cycle is 29·829; the warm side having the higher average of pressure by 0·44 inch.

The mean temperature we have already noticed.

The rain of the warm years amounts to 238·60 inches; of the cold to 234·33; rain of the whole cycle 472·93, or per annum 26·27 inches; the warm side averaging 26·51 inches, the cold 26·037; but the author has "found cause, on examining into past periods, to conclude that the small excess of rain here found on the warm side is not a constant result; but that the *cold* may sometimes be the wetter." The main point affecting our harvests appears to be the different distribution of the rain *within the year* in each period, which he next proceeds to examine.

After concluding the discussion of the observations, Mr. Howard remarks,—

> "It is proper I should caution my reader against *expecting too much* from the information here presented to him. Should he look for the *same mean temperature* and the *same amount of rain* in each returning year of the coming cycle, as are found recorded of a corresponding one in the past, he will probably meet with frequent disappointments; and this more especially in a locality somewhat different. We are yet far from being able to predict seasons in meteorology with the like certainty of date as the astronomer does the coming phænomena of the heavens; and it is even possible that, from the very nature of the causes concerned, we may never arrive at this. The judicious observer, finding certain facts fully ascertained and clearly noted for him, *will know how to make use of these for himself;* and by watching their occurrence in detail, making notes as he proceeds, will endeavour to feel his own way towards the future, independently of empirical and fallacious *predictions.* This is the kind of service which I expect my present labour to render to the country; besides gratifying a reasonable curiosity as to the past. We do not expect to become skilful in other arts without a due share of study and practice; but we seem to forget this self-evident truth when we take up that of foretelling the weather. The facts here detailed cannot fail to be useful to such as will be at the trouble to examine and compare them, though the inferences they may draw from them should differ. And admitting only that in the course of years here treated, we experience in succession the various degrees of warmth and coldness, of rain and dryness, incident to our climate, it must needs help

the farmer, the market-gardener, the planter or nurseryman, the grazier, the sheep-master, to have before him such an approximation to the times and order of their occurrence.

* * * * * * *

"There is a class of persons, however, to whom the paper may be immediately acceptable, and possibly also useful in regulating their future plans. The poor invalid may be soothed, and those of delicate constitutions encouraged, by the immediate prospect of a nine years' run of seasons having, with little exception, *the higher temperature of our climate*. It may be the means of inducing these to make trial at least of one or two of these, before they resort to other skies more favoured by natural position, but extending over countries far less desirable as residences to a truly British mind. And medical gentlemen, when they have read and considered what is here laid before them, may find arguments in it to strengthen such a conclusion."

The work terminates with tables, showing the mean height of the barometer, mean temperature, and depth of rain in each month, at Ackworth, Yorkshire, through the cycle of eighteen years from 1824 to 1841.

XIV. *Intelligence and Miscellaneous Articles.*

ON THE RED MOLYBDATE OF LEAD. BY M. G. ROSE.

It is well known that the molybdate of lead from Retzbanya in the Bannat is distinguished by its red colour from the other varieties, and particularly from that which is found at Bleyberg in Carinthia; the crystallization of these two varieties is, however, the same. Prof. Johnston lately made some researches to discover chromic acid in the red crystals; he submitted them to examination by the blow-pipe and reported that they were entirely chromate of lead; and he concluded from this that chromate of lead is a dimorphous body.

I brought with me from Siberia some red crystals of molybdate or lead, perfectly similar to those of Retzbanya, and I was able, notwithstanding their extreme smallness, on account of the great brightness of their faces, to measure the angles with the reflective goniometer: their form is the haupto-octohedron of molybdate of lead, slightly truncated on the superior edges. The inclination of the faces, which by their intersection form their superior edges, is 99° 38′, and that of the lateral faces is 131° 55′. I also submitted these small crystals to the action of the blow-pipe, and obtained results different from those stated by Prof. Johnston. I was hence induced to examine the crystals of Retzbanya, and I found that they behaved with every test like the molybdate of lead of Bleyberg (Berzelius, *Traité du Chalumena*, 3e edition, § 252). There is only one exception to this general statement: when fused with an excess of borax in the exterior flame the red crystals yield a glass which becomes opake on cooling, and has a slightly greenish colour, whilst the glass obtained from the Bleyberg crystals is of a very pure white.

The crystals of Retzbanya are easily decomposed in a mixture of hydrochloric acid and alcohol; a crystalline precipitate of chloride of lead is formed, and the solution, which is greenish and transparent, yields by evaporation a blue mass of oxide of molybdenum, similar to that obtained by the same means from the yellow lead of Bleyberg. When tried by the blow-pipe no difference is found between the two oxides.

It results from the preceding researches, that far from being composed entirely of chromic acid and oxide of lead, as Professor Johnston has stated, the red variety of yellow lead consists principally of molybdate of lead; it may however contain a little chromic acid. The presence of this acid is readily explained by the analogous composition of chromic and molybdic acids.—*Annales des Mines*, tome xvii.

[*Note.*—In our last Number the measure of the angles of leucophan was stated as given in the *Journal für praktische Chemie*; the reader will perceive that there must be some error in the statement, but which we have not the means of correcting. With respect to andesine, also, we followed the same authority in mentioning it to have been found "in twin crystals very similar to albite," yet it is stated to be a leucite: this is not very intelligible when we recollect that albite has a doubly oblique prism, and leucite a cube, as their primary forms.—Ed.]

METHOD OF DISTINGUISHING BETWEEN WEAK SOLUTIONS OF NITRATES AND CHLORATES. BY M. VOGEL, JUN.

When a few drops of tincture of litmus are added to a solution of nitrate of potash so as to render it blue, and afterwards concentrated sulphuric acid, the tincture is merely reddened by the sulphuric acid, and by the nitric acid set free, but it is not at all decolorated. A solution of chlorate of potash, on the contrary, which has been rendered blue by tincture of litmus, is entirely decolorated by the addition of concentrated sulphuric acid, a result by which the chlorate is effectually distinguished from the nitrate.

This effect is produced with the chlorate when one part is dissolved in sixty-four parts of water, but it ceases with eighty parts of water; but a solution of indigo is decolorated when water contains only one-500th of its weight of chlorate of potash.

This method of distinguishing the chlorates from the nitrates, both in very dilute solutions, has besides the advantage of giving certain results, in decolorating the tincture of litmus, even when the chlorates are accompanied with chlorides and other salts.

Tincture of litmus is not decolorated by a very weak solution of nitrate of potash on the addition of sulphuric acid, even when some hundredths of common salt or of other chlorides are present: it is decolorated only when the nitrate of potash is dissolved in a concentrated solution of common salt.—*Journ. de Pharm. et de Chim.*, Mai 1842.

ON THE EXISTENCE OF SULPHUR IN PLANTS.

M. Vogel, Sen., remarks, that it has been proved by the late M. Planche and other chemists, that many plants contain sulphur. Water-cresses, *Lepidium sativum*, L., are among those which especially contain much sulphur.

As soils distant from volcanos do not contain perceptible traces of sulphur, it appears to M. Vogel not impossible that plants, which are much disposed to assimilate sulphur, may have the property of deriving it from the decomposition of the sulphuric acid of sulphates. M. Vogel, however, found that seeds placed in a soil perfectly free from sulphur and sulphates, yielded plants which contained a notable quantity of sulphur.

The soil employed for this experiment consisted of coarsely powdered white glass; it was first strongly heated, but not fused, in a crucible, and being afterwards washed with boiling water, not the slightest trace of any sulphate could be detected.

Seeds of water-cresses kept in a moist state were placed in this, and when the plants were several inches in height, they were removed with their roots; after having washed the plants, the white fibrous roots were cut off, and these as well as the plants were dried, and on heating them in a retort, it was found that both of them yielded considerably more sulphur than the seeds contained; the expressed juice of the young plants cultivated in the powdered glass also contained soluble sulphates. The seeds of water-cresses, sown in coarsely powdered quartz, flint-glass, and very fine silica obtained from silicated hydrofluoric acid, yielded similar results with respect to sulphur and sulphates, though the plants did not flourish so well in the last as in the two former substances.

To obtain approximative results as to the quantity of sulphur in the water-cress seeds and the plants which they yielded, the following experiments were made:—The seed [100 grains ?] was heated to redness in a retort, and the gases disengaged were received into a solution of potash; acetate of lead was added to the alkaline liquor as long as precipitation occurred. The precipitate was of a brownish colour, and consisted of hydrate, carbonate and sulphuret of lead; the two former were dissolved by dilute nitric acid, and the sulphuret of lead remained, which after washing and drying weighed 0·95 gr., which indicated 0·129 gr. of sulphur. According to this experiment, 100 grs. of the seed contained 0·129 gr. of sulphur.

The young plants obtained from the growth of 100 grains of the seed were similarly treated; their weight was 2040 grs.; they yielded by the above-described processes 15·1 grs. of sulphuret of lead, equivalent to 2·03 grs. of sulphur: consequently the dried plants contained nearly fifteen times as much sulphur as the 100 grs. of seed which produced them.

Another experiment was made by projecting into a red-hot platina crucible small successive portions of a mixture of powdered cress-leaves with nitrate and carbonate of potash. The residue, heated in the crucible and treated with nitric acid, gave a considerable precipitate with chloride of barium, but, on account of the sulphate of potash which the plant contains, the quantity of sulphur cannot be accurately determined by this process, although in general it is preferable to that above described; 100 grs. of the leaves yielded in this way 4·6 grs. of sulphate of barytes, equivalent to 0·624 gr. of sulphur; but the quantity of sulphate of potash is to be deducted from this.

As the growth of the young plants of water-cresses took place in a soil devoid of sulphur and sulphates, and in a room which contained no sulphurous vapours, the origin of the sulphur, M. Vogel remarks, is to him a perfect enigma, and at present he confesses that he is unable to give a satisfactory explanation of it.—*Journ. de Pharm. et de Chim.*, Mai 1842.

ACTION OF SALTS ON LIVING PLANTS.

From the various experiments which M. Vogel, Sen. has made on the action of salts on living plants, he has arrived at the following conclusions:—

1st. That plants with their roots when immersed into a solution of sulphate of copper, totally absorb the salt, convert it into protosulphate, and die quickly.

2nd. That acetate of copper produces the same effects, the salt absorbed becoming proto-acetate of copper.

3rd. That plants absorb sulphate of magnesia, nitrate of potash, and iodide of potassium, and die more or less quickly.

4th. That the sulphates of zinc and manganese are absorbed by plants without suffering decomposition, and the plants die.

5th. That plants absorb nitrate of cobalt and nickel, without being able to absorb the whole of them from solution; but they die, and the same effect is produced by emetic tartar.

6th. That the oxalate and tartrate of oxide of chromium and potash are slowly absorbed by plants, and the bichromate of potash much more quickly. The *Datura Stramonium* and *Galega officinalis* absorb the salt of chromium with the greatest rapidity; they become of a yellow colour and die.

7th. That plants absorb nitrate of silver; but they decompose it, and the oxide of silver is reduced to the metallic state.

8th. That plants absorb also, and totally, the protonitrate of mercury from solution, but the salt is decomposed.

9th. That corrosive sublimate is absorbed by plants; some of them decompose it into calomel, and others absorb it without decomposition.

10th. That plants slowly absorb acetate of lead; and it is decomposed by some plants and not by others.

11th. That plants which contain much carbonate of lime, such as the *Chara vulgaris* and the *Stratiotes aloïdes*, do not absorb a salt of copper from solution: the same also occurs with the *Cereus variabilis*.—*Ibid.*

ON CHLORITE AND REPIDOLITE. BY M. KOBELL.

Chlorite is characterized by the proportion of water which it contains, and by its property of being completely decomposed by sulphuric acid. M. Kobell made a comparative analysis of four varieties from Schwarzenstein in Zillerthal, from Achmatof in Siberia, from Grenier in Zillerthal, and from Rourès near Salzbourg. They were treated with sulphuric acid in a platina crucible; the excess of acid was expelled and the residue treated with hydrochloric acid; the iron and alumina were precipitated with carbonate of barytes, and the alkalies were sought for but not found.

The results were—

	Schwarzenstein.	Achmatof.
Silica	32·68	31·14
Alumina	14·57	17·14
Magnesia	33·11	34·40
Protoxide of iron	5·97	03·85
——— manganese	0·28	0·53
Water	12·10	12·20
Gangue	1·02	0·85
	99·73	100·11

	Zillerthal.	Rourès.
Silica	27·32	26·96
Alumina	20·89	18·47
Magnesia	24·69	14·69
Protoxide of iron	15·23	26·87
——— manganese	0·47	0·62
Water	12·00	10·45
Gangue.		1·24
	100·60	99·30

The composition of the first two chlorites differs essentially from that of the two latter; and M. Kobell considers that the first two form a new species, to which he gives the name of répidolite (*pierre en éventail*).

The repidolite of Schwarzenstein is of an emerald-green colour by transmitted light, and crystallizes in hexagonal tables with triangular aggregated laminæ; it is accompanied with amianthus.

The repidolite of Achmatof is of an emerald-green colour parallel to one of its axes, and of an asparagus-green perpendicular to it; its crystallization is hexagonal, and it is associated with garnet.

The chlorite of Zillerthal is penetrated with crystals of magnetic iron, and becomes black before the blow-pipe; that of Rourès also becomes black when heated by the blow-pipe, and is more fusible than that of Zillerthal.—*Annales des Mines*, tome xvii.

ANALYSIS OF THE TACHYLYTE OF VOGELSGEBIRGE.

BY M. KLETT.

Ten years ago the late Professor Humdeshagen gave the analysis of a mineral under the name of tachylyte of Vogelsgebirge, which was perfectly similar to that of Sasëbühl; of this there is no analysis, and we have that only of the mineral from Vogelsgebirge; this mineral has a specific gravity of 2·7144; before the blow-pipe it fuses into an opake glass free from bubbles; fragments of considerable size fuse into a globule on charcoal; with microscomic salt it fuses into a transparent pearly bead, which becomes opake on cooling. On heating this glass in the reducing flame, the reaction indicating titanic acid (a red colour) is not perceptible; most frequently it is of a pale violet colour, like titanic acid with borax. Tachylyte in powder fuses into a greenish bead, which is without bubbles, and is transparent even after cooling; strong hydrochloric acid acts upon it, even when cold, and separates gelatinous silica; and in analysing it the action of this acid was continued till the titanic acid was dissolved by avoiding the degree of heat which would have rendered it insoluble.

The results of the analysis were—

Silica	50·220
Titanic acid..........	1·415
Alumina	17·839
Lime	8·247
Soda	5·185
Potash..............	3·866
Magnesia............	3·374
Protoxide of iron......	10·266
———— manganese	0·397
Ammoniacal water	0·497
	101·306

The tachylyte does not contain titanic acid in the state of titaniate of iron, for this is not acted upon by cold hydrochloric acid; the calcined mineral is also acted upon by this acid, but the silica is of a brown colour.—*Annales des Mines*, tome xvii.

ANALYSIS OF NATIVE ALUMINATES.

M. H. Rose states that native aluminates which are decomposed with so much difficulty and so imperfectly by the alkaline carbonates, and even by hydrate of potash, which also resist the action of hydrofluoric acid, and in the analysis of which Abich has so successfully employed carbonate of barytes, are completely and readily decomposed by fusion with bisulphate of potash.

He first employed it in the analysis of the chlorospinelle of Flatonsk: this mineral was reduced to fine powder in a steel mortar without having been previously bruised in an agate, flint or chalcedony mortar, was heated with bisulphate in a platina crucible over a spirit-lamp with a double current of air, until the powder was completely dissolved.

The fused mass dissolved entirely in water, and the constituent principles of the solution may be determined by the well-known methods. The alumina, when the quantity is not too small, ought to be redissolved in hydrochloric acid, and precipitated by carbonate of ammonia, to avoid an excess of it in the result. The use of the bisulphate of potash especially requires this precaution, because the salts of the fixed alkalies are separable with so great difficulty from precipitated alumina by washing.

M. Rose did not find any silica in the chlorospinelle, although this mineral occurs in schistose talc, and consequently in a silicate. A series of experiments proved that silica is entirely wanting in native aluminates, such as the corundum of China and Bengal, Oriental sapphire, the spinel of Ceylon and Norway, the gahnite of Ekeberg; and that the silica found by other chemists comes from the agate mortar in which the mineral is pulverized.

Though the bisulphate of potash is very advantageously employed in the analysis of aluminates, it is not applicable to that of those silicates which are not decomposable by acids. Felspar is only partially decomposable by this salt: it is therefore evident that silica is a much stronger acid than alumina when it acts the part of an acid; for if the bisulphate of potash so readily effects the decomposition of aluminates, it is entirely because alumina always acts as a base with sulphuric acid.—*Ann. der Chem. und Pharm.*, and *Journ. de Pharm. et de Chim.*, Mai 1842.

SOCIÉTÉ GÉOLOGIQUE DE FRANCE.

We are able to inform our readers, that the great Annual Meeting of the French Geologists will take place this year on Sept. 4th, at Aix (dept. Bouches du Rhône), and we have no doubt will be attended by a vast number of foreigners, attracted both by the beauty and geological interest of the neighbourhood.

METEOROLOGICAL OBSERVATIONS FOR MAY 1842.

Chiswick.—May 1, 2. Clear and very dry. 3, 4. Very fine. 5. Cloudy: heavy rain. 6. Fine: showery. 7. Rain: stormy showers. 8. Cloudy: stormy. 10, 11. Very fine. 12. Drizzly. 13—15. Slight haze in the mornings: very fine: clear at night. 16, 17. Very fine: clear. 18, 19. Overcast. 20. Densely clouded. 21. Cloudy and fine. 22. Cloudy and fine: slight rain. 23. Cloudy. 24. Rain. 25. Rain: overcast. 26. Rain: cloudy: clear at night. 27. Cloudy and fine. 28. Very fine. 29, 30. Clear and very fine. 31. Very fine: cloudy.

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

Sandwick Manse, Orkney.—May 1. Clear: fog. 2. Cloudy: clear. 3. Clear: cloudy. 4. Cloudy: damp. 5. Cloudy: rain. 6. Bright: cloudy. 7. Cloudy: thunder. 8. Showery. 9. Cloudy. 10. Rain: clear. 11, 12. Cloudy. 13—15. Clear. 16. Clear: fog. 17. Fog cloudy. 18. Cloudy. 19. Cloudy: drizzle. 20. Cloudy: shower. 21. Bright: shower. 22. Clear. 23. Clear: fog. 24. Clear: cloudy. 25. Cloudy: damp. 26. Bright: cloudy. 27. Bright: shower. 28. Bright: cloudy. 29. Cloudy: showery. 30. Bright: cloudy. 31. Bright.

Applegarth Manse, Dumfries-shire.—May 1, 2. Dry and withering. 3. Cloudy. 4. Fine. 5. Cloudy, with rain. 6. Showery. 7. Wet day. 8. Showers A.M.: cleared. 9. Fair, but cool. 10. Fair, but threatening. 11. Showery. 12—17. Fair and fine. 18. Fair and fine, but cloudy. 19. Fine rain P.M. 20. Rain and hail. 21. Fair and fine. 22. Showery. 23. Showery: growing weather. 24. Showery. 25. Fair and fine. 26. One shower: fine P.M. 27. Fair and fine. 28. Fair till noon; then rain. 29, 30. Showers. 31. Slight showers.

Sun shone out 29 days. Rain fell 12 days. Thunder 2 days. Hail 1 day.

Wind North-east 1 day. East 3 days. East-south-east 1 day. South-east 5 days. South-south-east 4½ days. South 5 days. South-west 4½ days. West-south-west 4 days. West 1½ day. North-west 1½ day.

Calm 7 days. Moderate 14 days. Brisk 2 days. Strong breeze 6 days. Boisterous 2 days.

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

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
1842. May.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R. S. 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	30·002	30·007	29·933	29·53	30·05	30·09	30·23	30·24	59·3	69·6	47·6	73	44	57	66	39	50	43	n.	e.	e.	ne.	se.	......	......	......	......	......	50
☾ 2.	30·084	30·022	29·962	29·59	30·10	30·00	30·16	30·04	57·3	67·7	47·8	66	30	57·5	66½	37	48	48½	ne.	ne.	e.	ese.	se.	......	......	......	......	......	50
3.	29·986	29·967	29·854	29·47	29·84	29·80	29·92	29·84	50·2	62·5	42·0	71	46	46·5	64	37	51	48½	wnw.	nw.	n.	se.	se.	......	·02	......	......	......	43
4.	29·956	29·907	29·892	29·33	29·78	29·75	29·81	29·81	53·7	66·4	49·3	65	33	55	62	45	50	47	w.	n.	nw.	wsw.	w.	·069	......	......	......	......	49
5.	29·904	29·886	29·530	29·24	29·59	29·21	29·45	29·20	56·8	62·0	47·6	63	46	58	56	45½	50	48½	s.	s.	w.	wsw.	s.	......	·16	......	......	......	50
6.	29·438	29·423	29·376	28·77	29·02	29·00	29·02	28·91	55·3	61·8	50·6	60	47	55	55	46	52	49	s.	w.	sw.	wsw.	se.	·122	·12	·22	......	·15	52
7.	29·340	29·322	29·197	28·73	28·87	28·77	28·81	28·79	51·7	62·3	49·0	63	46	51	51½	45	55	48	s.	sw.	sw.	se&sw	sw.	·213	·22	·05	0·90	......	51
8.	29·416	29·706	29·382	28·80	29·06	29·47	29·00	29·30	54·2	62·0	48·4	63	41	55	55½	42½	45½	45	w var.	w.	w.	w&nw	nw.	·241	·20	·18	......	·14	50
9.	29·948	30·084	29·892	29·40	29·80	29·97	29·79	29·97	51·3	61·6	46·4	60	32	50	55	37	45	44½	w.	n.	w.	nw.	nw.	·130	·10	......	......	·06	45
● 10.	30·218	30·172	30·068	29·69	30·02	29·90	30·04	29·98	52·0	60·5	41·8	64	35	53	60	34	51	47	w.	nw.	calm	s.	w.	·061	......	......	......	·20	45
11.	30·006	29·966	29·842	29·44	29·75	29·78	29·91	29·91	57·3	61·0	46·2	65	44	57	59½	46	50	47	e.	s.	se.	sse.	se.	......	·26	......	......	......	47
12.	29·948	29·984	29·920	29·44	29·86	29·90	30·00	30·02	46·3	64·8	46·0	51	37	48·5	58½	43	50½	47	nw.	n.	n.	s.	se.	·130	·02	·09	......	......	47
13.	30·082	30·078	30·033	29·45	29·92	30·01	30·07	30·04	54·7	57·0	47·6	70	36	53	60	43½	51½	48½	s.	nw.	calm	sw.	se.	·225	......	·16	......	......	48
14.	30·200	30·230	30·138	29·67	30·08	30·14	30·16	30·24	53·2	64·2	48·4	72	38	51	65½	50	55	50½	w.	w.	calm	s.	se.	·025	......	......	......	......	48
15.	30·408	30·387	30·341	29·55	30·29	30·34	30·37	30·50	55·7	64·4	48·0	67	36	58·5	68	43	58	50	e.	ne.	calm	sse.	w.	......	......	......	0·18	......	54
16.	30·436	30·390	30·307	29·87	30·38	30·31	30·53	30·50	57·8	64·3	48·7	68	42	58	72	45	55	50	n.	ne.	calm	s.	calm	......	......	......	......	......	54
☽ 17.	30·314	30·278	30·139	29·79	30·29	30·17	30·43	30·31	52·8	66·5	45·7	68	43	50·5	68½	49	50	48	n.	ne.	calm	e.	n.	......	......	......	......	......	51
18.	30·072	30·046	29·911	29·52	30·02	29·84	30·13	30·03	50·7	60·3	47·8	69	40	52	67½	41½	49	48½	n.	ne.	calm	sse.	se.	·022	......	......	......	......	50
19.	29·834	29·807	29·711	29·32	29·70	29·58	29·93	29·80	53·8	57·0	47·6	65	36	51·5	57	48	50	48	se.	sw.	calm	e.	e.	......	·04	......	......	......	48
20.	29·706	29·658	29·646	29·05	29·45	29·37	29·68	29·55	57·0	70·3	47·0	63	49	46·5	60	46	49	49	s var.	sw.	sw.	se.	e.	......	......	·15	......	·04	50
21.	29·730	29·724	29·676	29·20	29·47	29·50	29·56	29·70	56·7	62·3	51·3	61	41	55	55½	43	51	49	se.	s.	sw.	se.	se.	......	......	·04	......	·06	49
22.	29·798	29·748	29·674	29·29	29·60	29·60	29·75	29·87	57·8	69·0	49·8	65	42	59	60	40	50½	48	se.	s.	calm	se.	se.	·022	·01	·08	0·50	·17	51
23.	29·854	29·857	29·785	29·29	29·62	29·70	29·89	29·94	57·2	75·3	51·4	64	43	57·5	61½	48	54	50	sse.	s.	w.	se&sw	e.	......	·01	......	......	......	50
○ 24.	29·882	29·838	29·790	29·33	29·79	29·76	30·04	30·05	53·3	64·6	52·7	65	40	54	59	48	52	49	se.	sw.	calm	e.	e.	......	·20	·12	......	......	51
25.	29·904	29·854	29·845	29·27	29·64	29·70	29·88	29·87	55·7	60·0	47·6	63	48	51	59½	47	55	49½	s.	sw.	sw.	sse.	e.	·261	·23	·82	......	......	51
26.	29·806	29·823	29·767	29·22	29·70	29·69	29·90	29·86	56·2	61·6	51·2	66	47	55	62	43½	54½	50	s.	sw.	calm	sse.	se.	·119	·01	·40	......	......	52
27.	29·980	29·929	29·911	29·34	29·71	29·78	29·73	29·80	61·3	63·8	52·0	68	51	61	61½	47	53½	50	s.	s.	calm	wsw.	se.	·102	·13	·06	......	......	57
28.	30·050	30·066	29·988	29·38	29·81	29·88	29·75	29·77	57·5	68·4	54·0	70	41	56·5	60½	45½	52½	53	nw.	w.	calm	sw.	se.	·066	......	·11	......	·06	54
29.	30·184	30·124	29·991	29·48	29·86	29·72	29·73	29·60	61·8	78·3	51·2	71	46	62	60½	50	61	54	s.	s.	calm	s.	s.	·126	......	......	1·00	......	55
30.	30·010	30·050	29·955	29·23	29·70	29·88	29·58	29·75	62·3	69·6	53·7	73	46	63	58	48	55½	50½	ssw.	w.	s.	w.	sw.	......	......	......	......	·10	53
31.	30·192	30·155	30·118	29·45	29·96	30·03	29·87	30·00	60·7	73·0	53·7	73	41	61	60	44½	53½	51	s.	nw.	w.	sw.	s.	......	......	......	......	......	56
Mean.	29·958	29·952	29·857	30·01	29·44	29·44	29·842	29·844	55·5	64·9	48·8	65·97	41·51	54·8	62·4	44·1	51·88	48·69						Sum. 1·934	1·73	2·48	2·58	0·98	Mean. 50

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

[THIRD SERIES.]

AUGUST 1842.

XV. *On the Scientific Labours of* Jeremias Benjamin Richter. *Addressed to the Imperial Academy of Sciences of St. Petersburg, at the public sitting of Dec.* 29, 1840, *by* M. Hess, *Member of the Academy**.

Gentlemen,

THERE is perhaps no one here present who does not reckon amongst the fairest enjoyments of thought those moments which from time to time he is able to devote to the remembrance of men of genius who have bequeathed to us important truths. And when I proceed to show that the veil which obscures the memory of one of these has yet to be torn away,—that the labours of twenty years employed in rendering a truth evident to the eyes of the most incredulous, are not yet appreciated,—you will then, I cannot doubt, grant me a moment of the attention which your kindness would not have refused to a cause less disinterested.

In the exact sciences, as in all other cases, nature does not allow us to proceed *per saltum*; it is necessary that every thing should be unfolded gradually. It is the most simple phænomenon which first takes its place in the domain of intelligence; the most complicated—the most difficult, is that which comes the last. Thus when at the beginning of the eighteenth century, astronomy, thanks to the numerous labours summed up by the mind of one great man, thanks to the simplicity of his principle, assumed the rank of a science almost perfect, about that time did chemistry, with difficulty, attempt to assume a scientific form. You will perhaps suppose that this is to be attributed to the men who were engaged in it; but you will soon abandon this idea when I tell you that Newton, who discovered the law of gravitation, also applied himself to chemistry, that he decomposed the subtile matter of light, whilst

---

* From the *Recueil des Actes de la Séance Publique*, Dec. 29, 1840.

not only the air, but water and even earth still resisted the efforts of three generations.

However, George Stahl, a resident at Berlin, established his theory of phlogiston which so long preserved its dominion in the mind. Air was at last decomposed, and water also. Lavoisier next analysed the phænomenon of combustion; and from this period the new ideas became diffused; the impulse was given, earth itself was analysed, and the number of combinations was increased in a wonderful manner, without the existence as yet of any known law to reduce this labyrinth to order. Many persons still remember the manner in which analyses were recorded; everything was reduced to hundredths, and thence resulted a confusion the shackles of which must have been felt, in order to appreciate the system of notation now used, at its just worth. It was Jeremias Benjamin Richter, assessor at the office of mines at Berlin, who first gave order to this chaos. You therefore would expect that the highest esteem would invest his works, that his name was revered. No; Richter was not appreciated, he was almost forgotten whilst alive. He died at Berlin the 4th of May, 1807. The same year a celebrated author tells us, that being employed in drawing up a treatise on chemistry, amongst other works but *little read* he ran through those of Richter. He was struck with the mass of light which he found there; but by a fatal chance he attributed to Wenzel, whose works he must have read at the same time, the most beautiful result obtained by Richter, that which was to serve for a foundation to the whole edifice. In order to explain how it was that Richter had been forgotten, the author to whom we allude says that his results were not exact, which must have weakened the impression the perusal of his works must have made, and so much the more as Richter almost always took the *carbonate of alumina* as the point of departure, a combination which we know does not exist. Let us not be surprised, then, that the most celebrated French authors repeat, on the authority of a great name, the same errors concerning works which they have not read; we see, for example, the author of the *Leçons sur la Philosophie Chimique* explain things in the same way, and reduce the merit of Richter almost to nothing. "Can you believe," says he, "that in establishing his doctrines he nearly always takes the carbonate of alumina as the point of departure?" In short, Richter is there reproached with having *too much obscured the questions upon which Wenzel had begun to throw light* *.

[* Our own countryman Dr. Wollaston, it would appear, justly appreciated the labours of Richter: see the paper explaining his "*Synoptic Scale of Chemical Equivalents*" in the Philosophical Transactions for 1814, p. 3, 4. —Edit.]

If in general, gentlemen, it is a duty to render justice to merit, in the present case it is at the same time a right; for J. B. Richter, almost unknown by the rest of Europe, was elected a correspondent of this Academy on the 14th of May 1800. Let us examine his title to our esteem. It is the best homage we can render to his memory.

Richter published in 1792 and 1793, a work in three volumes under the title of *Anfangsgründe der Stöchiometrie, oder Messkunst chemischer Elemente*, in which he sets forth his ideas in the form of a systematic treatise. But this form, you know, is little suited for spreading new ideas. How can a reader be expected to gather five hundred known ideas in order to discover one that is original! Has not each professor his treatise, and would it not be a punishment to have to study nearly the greater part of it? This mode of publication does not promise success to any but authors who have already acquired great celebrity, and with whose works we are obliged to become acquainted. So Richter, beginning by a work in three volumes, was not read. Seeing that the great truth which he had in view was not appreciated, that he was exposed to unjust criticisms, whilst his work was not read, he resolved to publish his researches separately, which he did under this title, *Ueber die neueren Gegenstände der Chemie*, in eleven small volumes of from 100 to 250 pages each. They appeared from 1793 until 1802. "I should (says Richter in 1799) certainly not have followed up these two first volumes (Stücke) by seven others, if too severe a criticism of the antiphlogistic school did not endeavour to put under the bann of sound reason all those who think differently from it, and if to this was not added the annoying circumstance that my *Stöchiometrie*, although endowed with a sound constitution, is nevertheless consigned to the shelf of the shop-keeper."

In the introduction to the first part, Richter tells us he hopes that the part of chemistry which treats of affinities and quantities will soon become a part of applied mathematics. Here then is the preconceived idea, the point whence Richter set out;—weigh even the form of his expressions, and you divine nearly all his life. "Some experiments which I have just made, having the same aim in view (says Richter, vol. i. § 121), make me think that if we could employ suitable expedients, we should find that the neutrality of pure elements, setting out from one amongst them which is taken as unit, increases in a positive progression." We see the idea was truly philosophical; it was necessary to develop it and to become assured whether such a relation existed or not. It was a source of serious errors to him, and drew upon him too severe judgements. He devoted a part of his works to fathom this question, and remained persuaded that the equivalent of all bases belongs to an arithmetical progression, whilst the numbers, which express the equivalents of the acids, form geometrical progressions, the ratio of which is different according to the different groups of acids.

Now it is well established that facts do not support this notion of Richter's: we shall therefore pass over this part of his works, and I shall return to them but once, in order to show how it was that his experiments were sometimes so far from the truth as not to undeceive him. But if we go back to the time when he lived, we shall feel that the question raised was vast, and that if his undertaking was not crowned with success, he at least deserves that these words should be applied to him:

> "Quem si non tenuit, magnis tamen excidit ausis."

Amongst the numerous subjects which Richter treats of in the first volume, I shall only quote the method which he points out for extracting platinum from the ore of that metal; for it is still used. He recommends precipitating the solution of that metal by sulphate of potash, to wash and dry the precipitate and to decompose it by the carbonate of potash, so as to divest it afterwards of all the salts by washing it with water. The metal then remains brilliant as silver. The explanation of the processes follows, which gives him an opportunity of making some very important remarks. When we shall have found, says he, numerical expressions for affinity, then these seeming anomalies will disappear. Upon this occasion he explains the difference between simple affinity and double affinity, and observes that it is nowhere proved that we can really isolate a simple body, for, he says, each time that we disengage an alkali or a metallic acid, if it be only carbonic acid, heat must then be substituted for the acid; lime is an example of this. So it is with the acid from which we take a base, it is combined with, or even neutralized by heat. In the case of a simple affinity, we suppose but two elements, whilst this shows you that there are at least three, for every time that neutrality takes place, heat is substituted for the third element. This is even the case when a metal is dissolved by an acid and neutralizes it, for then it is the acid that furnishes the heat, which becomes united with the other elements. Richter therefore knew that bodies were pervaded by heat; he urges the necessity of taking these phænomena into consideration, but he does not yet take a perfectly just view of them; he believes that heat is just added to the elements, when we know, on the contrary, that it has just been disengaged.

The third volume (1793) is wholly devoted to a critical examination of Lavoisier's antiphlogistic system. Up to that time Richter had only known it by very insufficient extracts. But in 1792 appeared a German translation of Lavoisier's treatise on chemistry, by Girtanner. Richter obtained and read it, and was convinced of the truths of the new system. Yet indulgent towards others and a stranger to the spirit of party, he excuses those who refuse to admit it. "For," he says, "in the ancient system, metals and sulphurs were considered as compound bodies, earths and acids as simple bodies; in the new system it is just the contrary: now imagine a man whom you would persuade that all he sees he sees reversed, and then condemn him for his incredulity. But, nevertheless, an error does not become a truth should it even count myriads of ancestors."

Do not suppose however that Richter, upon embracing the new system openly, abandons himself to it without criticism. No. No one to my knowledge has better perceived what there was good in the fundamental principle of the phlogistic system. We must not expect that a system which served, for nearly a century, as a starting-point for the numerous investigations of chemists, that a system which could rally round it all facts, should be entirely illusive. "All the facts on which the partisans of the antiphlogistic system rest," says Richter, "are not only insufficient for the refutation of the reality of phlogiston; but on the contrary, they do but rectify our ideas with regard to it and render its existence more evident; for example, when we assert that phosphoric acid is composed of phosphorus and oxygen, this conclusion has no foundation, since in reality no other conclusion can be drawn from the experiment, except that this acid is composed of the radical of phosphorus and of oxygen. Not any induction can be drawn respecting the nature of this radical itself, for it is only known as combined with oxygen or with phlogiston (*Brennstoff*); which, however, does not prevent us from indicating the relative quantity of the elements, since, for us, the weight of phlogiston, like that of heat, is an infinitely small quantity." Such was the capacity of Richter's mind, that in the midst of the lively contention of two parties who do not agree, he quietly examines the question, seizes the literally palpable truths of the new school, and yet does not abandon the more abstract, more hidden but not less real truths of the old system. Perhaps Richter had a model, but then this model was Lavoisier, and no other. But it is certain that at the present time, this manner of viewing the subject is banished from all works which treat of this science, and that it is after a lapse of forty years that considerations of another order, supported by decisive experiments, seem to enable us to appreciate his ideas properly.

Before Richter, and in his time also, it was supposed that the affinity of a substance was in the direct ratio of the quantity necessary to saturate another body. Richter compares the quantities of tartaric and of acetic acid necessary to saturate the same quantity of lime. He finds that more tartaric acid is necessary, and concludes that its affinity is greater, and that consequently this acid should displace acetic acid. He makes trial of this, and really it is tartaric acid which seizes the lime and displaces the acetic acid. Few examples are found more suitable than this for characterizing the difficulties which are met with every day in chemistry, for here is a well-observed fact, a conclusion drawn; an hypothesis follows, then comes the experiment which confirms it. You believe your principle well established? By no means. Another fact comes and overturns it. Subsequently Richter again takes up the question, and this time he clearly proves, that *affinity is not exerted in the ratio of the masses which combine.*—Vol. x. p. 187–195.

It is in the fourth volume (viertes Stück, 1795) that Richter establishes truths which will always be reckoned amongst the most important acquisitions in the region of the exact sciences. He begins by researches on the capacity of saturation of hydrofluoric acid; for this he uses several bases, and does not neglect alumina. He tells us (p. 10) that he took 650 grains of very pure carbonate of alumina, which he saturated with hydrofluoric acid. Here then is what he is accused of, for carbonate of alumina does not exist! The parenthesis, then, where he says that 1000 parts of this alumina contained 542 of pure alumina, has not been read. Nor have his calculations been followed, for he everywhere takes into account alumina at the rate of 542 parts for 1000. All of you, gentlemen, who add experience to a general knowledge of chemistry, will know that it is very difficult, I may say almost impossible, to obtain pure alumina; if we precipitate it from its solutions by the carbonate of ammonia, it always retains a little of this salt, and water besides. It is only by calcination that we can obtain it really pure; but then it becomes difficult to dissolve in acids. This, doubtless, is the reason why Richter used non-calcined alumina, and determined by a separate experiment the real amount of that earth which it contained.

After having found the quantity of different bases by which 1000 parts of hydrofluoric acid were saturated, a verification is required. For this purpose he decomposes fluoride of calcium by sulphuric acid, and infers the quantity of lime to be found in the hydrofluate from the quantity contained in the sulphate. He thus finds by analysis, that 1000 parts of hydrofluoric acid require 1882 of lime for their saturation; by synthesis he finds 1865 parts. After that, he finds that the same quantity of acid was saturated by 3797 parts of potash, and continues in these terms: " It has been shown (he speaks of his *Stöchiometrie*) that the quantities, whether of alkali, or of alkaline earth, which served to saturate the same quantity of one of the three *volatile** mineral acids, were in *constant relation* with each other." Richter then examines whether the results which he has just obtained support this proof: he had before found that 1000 parts of muriatic acid require 1107 parts of lime for perfect saturation, and 2239 of potash. For hydrofluoric acid he had obtained 1882 parts and 3797. But 1107 : 2239 = 1882 : 3807, which differs very little from the result of the experiment.

A happy and important discovery is not all; the consequences of it must be felt; the promptitude of intelligence must go beyond the tardiness of experience, for it is only in this future that we can be armed against all the shackles of the present. Now this is the manner in which Richter announces and extends the consequences of his experiments (vol. iv. p. 67, year 1795). When two determining (*determinants*) elements (i. e. two acids,) each taken at the rate of 1000 parts, are saturated by the substances a, b, c, d and α, β, γ, δ, so that each time a and α, b and β, &c. are always the same substance; in this case the (substances) quantities a, b, c are among themselves absolutely in the same relation as α, β, γ.

This theorem of Richter's is a true touchstone for all experiments which relate to neutrality; for if the results do not agree with this principle, they must be rejected without hesitation. But, he adds, although according to the announcement of the principle we may use relations known and determined by experiment, in order to find others by calculation, it will always be useful to verify these last by the fact, for we gain by it, after having recognised certain relations, the means of verifying the numbers themselves from which we had set out, and thus to correct the little inaccuracies by which they might be affected.

Richter then points out the work to be done; but in order to feel all the importance there is in its being done with the greatest precision, it will suffice to tell you that he forms a *plexus of number*, which covers the entire domain of chemical researches whatever they may be, and that it is precisely from not having

---

* By these he understood the sulphuric, nitric and muriatic acids.

performed analysis with skill enough, that Richter remained all his life uncertain on several points.

Here is certainly one of the most striking proofs of the progress we owe to him. He makes analyses, and deduces a general principle from them, and from that time these same analyses are no longer sufficient for the increasing wants of the science. To set out from hence the task imposed by Richter becomes gigantic. New methods are necessary. We owe them to M. Berzelius; it is he who executed this work with a precision very rarely equalled, and which not only has not been surpassed, but never will be by these methods.

Richter, after having established this principle, continues to apply himself to the subject; he determines the capacity of saturation of acetic acid, by lime, by magnesia, by barytes, and finds that in order to saturate 1000 parts of this acid, supposed anhydrous, $\dot{Ca}$ 523, $\dot{Mg}$ 405·6, $\dot{Ba}$ 1465 are necessary, which gives for the composition of these salts,

	According to Richter.		According to Berzelius.	
For 100 of $\dot{Ca}\bar{A}$ ..	$\dot{Ca}$ 34·34	$\bar{A}$ 65·66	$\dot{Ca}$ 35·63	$\bar{A}$ 64·37
$\dot{Mg}\bar{A}$ ..	$\dot{Mg}$ 28·8	71·2	$\dot{Mg}$ 28·66	71·34
$\dot{Ba}\bar{A}$ ..	$\dot{Ba}$ 59·4	40·6	$\dot{Ba}$ 59·8	40·2

Let us observe that there is no question of alumina; it is, says he, because he is not able to find with precision the point of saturation for this base. You therefore see a real difficulty which stops him, this combination being one of those which he is more certain of determining by calculation than by experiment.

These researches lead Richter to the conclusion that acetic acid follows the same law as the acids before considered. He then shows that the same law is also applicable to the citric, oxalic, tartaric, formic, and several other acids. It is essential to observe, that in order thus to prove by experiment the generality of the principle which he had established, an entire series of analyses was necessary for each acid, and it will be easy to judge of the ardour and time he must have expended on these labours. But in these same works he applies his principle; as for example, he often meets with difficulties in finding the point of saturation for carbonic acid, he sets out then from a combination which he thinks well known. There again he avoids alumina as not adapted to his object, and he selects carbonate of lime. His choice could not then fall better. He finds that 1000 parts of carbonic acid are saturated by 1373 parts of lime, which gives for 100 parts $\dot{Ca}$ 57·86 and

$\dot{C}$ 42·14; according to Berzelius, $\dot{C}a$ 56·29 + $\ddot{C}$ 43·71. Not only does Richter not choose the carbonate of alumina, but he examines the question to discover why the carbonate of this base treated by an acid disengages less carbonic acid than another base. You see then the ambiguity that there is in this substance by no means escapes him, and he continually returns to it as an enigma. Richter, armed with so powerful a principle as that which he had discovered, could not limit the application of it to his own labours; he also applies it to those of others, and rectifies or confirms them; for he was, so to say, endowed with a sense more than his contemporaries.

Berthollet had found, as Lavoisier says in his treatise on chemistry, that 69 parts of sulphur absorb 31 parts of oxygen to become transformed into sulphuric acid. Richter repeats the experiment and comes to a very different result. He oxidates sulphur by nitric acid; then converts it into sulphate of lime and obtains 947 parts of this latter for 222 of sulphur, which makes 856 parts for 201 of sulphur, whilst we admit at present 857·1. He then greatly approaches the truth, but to deduce the composition of sulphuric acid, that of the sulphate must be known exactly. This not being sufficiently well known to him, he finds that 201 parts of sulphur absorb 227 instead of 300 of oxygen to be converted into sulphuric acid (vol. v. p. 124), which compared to Berthollet's result, is still a very beautiful approximation, since this latter had only found 90 parts instead of 300. Then he is reproached with Bergmann's researches on the sulphates of potash and barytes. They are not just, he says, for if we suppose the salts compound, as Bergmann points out, and if one of them is mixed with a neutral salt, by which it may be mutually decomposed, there will be an excess of acid or of base, which cannot happen; every one knows that the solutions remain neutral (vol. vii. p. 94 and 95): therefore his analyses are false.

Klaproth had discovered strontian*; he describes and analyses several of its salts, without attention to Richter's principles. The latter applies them and finds that the analyses of Klaproth agree with the principle, and consequently that they are exact.

It is this very important discovery which has been attributed to Wenzel. This question therefore demands an attentive examination; for, take this title from Richter, and you make him fall back into the category of ordinary philosophers.

---

* [Strontian was first discovered by Dr. Hope; though its discovery about the same time, or shortly after, by Klaproth, appears to have been an independent one.—Edit.]

It is no longer a summit; it is no longer to him that the chemist owes the compass without which he could not navigate. Well, not only does Richter in his *Stöchiometrie*, vol. iii. p. 285, use this principle in order to verify the results of his contemporaries, but even those of Wenzel are submitted to this test. This, it may be objected, is not a proof, for he may not have quoted the author from whom he has borrowed the idea. But I have read and re-read Wenzel, and not a word, not a trace of this idea is to be found in his work. It was possible that an edition reprinted in 1800 might be inexact; I referred to that of 1782, and with the same result. Here however is an unexceptionable proof that the principle in question really belongs to Richter and not to Wenzel. Open Wenzel's work, and you will find at the end a chapter which is entitled "Applications of the laws of affinity to particular cases" (*Anwendung der Lehre von der Verwandtschaft der Körper auf besondere Fälle*). This is the manner in which Wenzel expresses himself: "In chemistry, as in every other natural science, the essential aim is to compare recognised facts in their mutual relation, in order to deduce other truths which are not perceived at first view. In the experiments above quoted, we came to a knowledge of the phænomena which took place, by the fact of the union of two substances. We saw in what order, under what condition, and in what proportions they are combined. The greater part however of these experiments, considered singly, are not of great importance, whilst we only limit science to that. But they acquire importance as soon as we apply them properly, for their merit essentially depends upon a happy application."

Let us follow Wenzel in his applications, and let us choose for this purpose § 7. There he proposes as a question to find the simplest and most advantageous manner of obtaining crystallized verdigris. Here is what he proposes:—the sulphate of copper and the acetate of lead are both soluble in water; if these two solutions are mixed, the sulphuric acid by virtue of its affinity for the oxide of lead will seize upon this and form an insoluble substance, which may be utilized in the arts on account of its whiteness. The liquid will contain some acetate of copper which we separate from the precipitate. Depending upon his analyses, Wenzel calculates the quantity of oxide of lead contained in a given quantity of acetate of lead. He then calculates the quantity of sulphate of copper necessary to precipitate all the oxide of lead. That done, he examines the question, to learn whether the acetic acid which the oxide of lead has just left is sufficient to dissolve all the oxide of copper which has just been left by the sulphuric acid, and always starting from his analyses, he comes to the conclusion that the acetic acid set at liberty is not sufficient to dissolve all the oxide of copper, and that for the quantity of copper employed, which is 124 parts, there will be found of it $9\frac{1}{3}$ parts mixed with the sulphate of lead as an oxide. In this case Richter, starting from his principle, would necessarily say, these analyses are false! as he did in many cases. What does Wenzel? he, on the contrary, concludes that after having separated the solution from the precipitate, this last must be treated with a little sulphuric acid to remove the oxide of copper. Here then is a very evident proof that Wenzel did not even suspect a similar relation to that which was discovered by Richter. Richter not merely discovers this principle, but he comprehends it in its totality; he follows it in all its consequences, and nothing can show us more fully the depths of his convictions with respect to this, than some words which are to be found in the preface to the 10th volume. "The theorems of stœchiometry," says he, "contain a *necessity*; they may be constructed and have the value of *à priori* principles."

These principles conduct him to new generalities. He finds that when a metal is precipitated from its solution by another metal, the quantities of oxygen necessary to preserve equal quantities of the two metals in solution, are to each other in the inverse ratio of the masses of the two metals. Further on, he concludes, since when several metals are precipitated from solution by one another, the solution always remains neutral, it is sufficient to know the difference of weight between one of these metals and its oxide, to deduce from it the quantity of oxygen which all the others contain in the state of oxide. For this it is sufficient to take a constant quantity of the same acid, for then all the metals that may be dissolved in this acid will contain the same *quantity of oxygen*, which will then only have to be deducted from the weight of the oxide, in order to obtain that of the metal.

Richter takes sulphuric acid for a starting-point, and prepares a table of the composition of the metallic sulphates; in this table the quantity of oxygen of the metal being necessarily constant, he designates it by the letter U. This is what we now designate by the letter O. Richter was then very near establishing a system of equivalents, just like that which is at present used; for that object it was sufficient to refer all the numbers to this constant quantity U. But this simple idea had not struck him, for in another column he gives the composition of the muriates, taking 1000 parts of muriatic acid as a starting-point; in another column, indeed, he gives the composition of the nitrates, taking for starting-point 1000 parts of nitric acid. His numbers therefore varied continually, which must have kept many relations concealed from his sight.

Nevertheless these tables constructed by Richter have another peculiarity which merits our attention. The names of metals are not found in them in writing, but the signs then used are substituted for them, as ☌ manganese, ♂ iron, ♁ zinc, ☽ silver. But here signs fail him, for several metals had just been newly discovered; these Richter expresses by the two initial letters of the name, for example, χϱ for chrome, Ti for titanium, Te for tellurium. Here then is the first idea of the notation so happily completed by M. Berzelius.

We see Richter continually occupied with the phænomenon of neutrality. What then is the neutrality of a solution? This is a thorny question, and one to which, even at the present time, many authors answer only in an obscure and evasive manner. Neutrality, says Richter, is absolute or relative: it is absolute when the solution does not exert any reaction on test papers; it is relative when the neutral salt nevertheless exerts an acid or alkaline action. But in this case, he says, however decided may be the reaction exerted by a metallic solution (for example the nitrate of silver), you recognize, nevertheless, that it is neutral, because the least addition of an alkali causes a precipitate which will not dissolve again without adding an acid.

Although Richter had recognised the fact that different metals required the same quantity of oxygen in order to be dissolved in the same quantity of acid; notwithstanding, he says, when metals become charged with oxygen without the intervention of an acid, that by no means prevents them from taking very different quantities. Richter, as we see, was not ignorant that there were different degrees of oxidation, and he employed himself in determining several of them. As, however, the works of Richter which relate to the oxides of metals are far from being very exact, let us examine an example in order to discover to what the inaccuracies met with in his determinations are to be attributed.

He knew, for example, that arsenic formed two combinations with oxygen, arsenious acid and arsenic acid. He determines by a direct experiment the quantity of oxygen which arsenious acid takes to become converted into arsenic acid, and finds that 100 parts of acid absorb 17·2 of oxygen, which is not far distant from the real number, 16·17. He afterwards seeks to determine the quantity of oxygen which metallic arsenic absorbs to become converted into arsenious acid, and he finds for 100 parts of metal 15·1 parts of oxygen, deviating greatly from the true number, which is 31·9. Having a false idea of the composition of arsenious acid, he necessarily deduces a false composition for arsenic acid. Now this is the way he obtains a number so far from the truth: he converts a given weight of regulus of arsenic into arsenic acid, and then into arseniate of lead. But instead of drawing a conclusion from the weight of this latter, he first tries to determine the quantity of arsenic acid which the precipitate should contain, and for that purpose sets out from the arseniate of magnesia, which must necessarily compromise all the results; for in order to determine the composition of that salt, he saturates a solution of arsenic acid by the carbonate of magnesia, a salt whose composition is not always constant. Then he determines the quantity of arsenic acid from a table of density previously constructed. In this then consists Richter's greatest fault, I will even say the only one which he has committed, but from which several others originate: he did not yet quite appreciate the difference which exists between a direct and an indirect method. This is the true source of all his errors. To make amends for this, each time that he makes a direct experiment, he approaches very nearly to the truth; for example, if he wished to know of how much oxygen and cobalt the oxide of this metal is formed, he determines this quantity in a direct manner, and finds for 100 parts of cobalt 26·5 of oxygen, which does not widely deviate from 27, which is the real number. But Richter distrusts himself. He tells us (vol. ix. 1798, preamble) that he cannot easily manipulate; that he was never able to finish an analysis without losing something at the end of all the operations; and that he never dared to undertake an investigation if there was any question of stœchiometrical determinations, with so small a quantity as 100 grains, but that he needed 500. This is perhaps the reason why Richter attached great value to the tables of density, whether for acids or for salts. A considerable part of his time too was employed in making them. At the end of each acid he gives a table indicating the acid contents in a solution at different degrees of density. He does the same for the salts which are most used. Richter was also much employed at different times in constructing areometers and alcoholometers; we still use many instruments which bear his name.

It is not only when Richter treats of general questions that he merits all our attention; he often captivates it by questions which are quite special. A few examples will suffice in order to appreciate him. We have seen that he confirms the researches of Klaproth on the composition of the salts of strontian, but, he says, my conclusions are not just unless the salt which I have used was pure. He had prepared this salt by dissolving the natural carbonate; the object in question then was to know if it did not contain lime or barytes. He finds that a solution of strontian is not troubled by adding a solution of ferrocyanate of potash, whilst the least portion of lime or of barytes may be discovered by this means. In its turn lime is distinguished from barytes by the solubility of its sulphate. This work has been quite forgotten, and in our time a chemist in high esteem at Berlin again takes up the question, and supported by more recent works gives absolutely the same solution of the problem which his countryman Richter had given so long before (Pogg., *Ann.* vol. xliv. p. 445*).

Richter finds that it is difficult to prepare very concentrated nitric acid because of the great quantity decomposed by heat. Now this inconvenience is remedied by using a quantity of sulphuric acid double that necessary for decomposing the nitre. Richter proposes another means which merits our attention; he adds to the nitre one-third of its weight of peroxide of manganese, and the quantity of sulphuric acid necessary for decomposing the two substances. He finds that the disengagement of oxygen which accompanies the distillation of nitric acid prevents the formation of nitrous acid.

It was already known in Richter's time that salts while passing from the state of solution to that of crystals, gave out heat. The same phænomenon takes place when water becomes ice; it was therefore thought fit to indicate the analogy of the two phænomena by saying ice of crystallization, instead of water of crystallization, the term which had been used till then. Richter puts the question, whether water which is found combined in crystals exists in them in the state of ice or not. This is the manner in which he succeeds in solving this interesting question. He dissolved 1440 parts of crystallized sulphate of soda ($\dot{\mathrm{N}}\mathrm{a}\,\dddot{\mathrm{S}}+10\,\dot{\mathrm{H}}$); the temperature of which was 15°·55 C. in 3405 parts of water, the temperature of which was 76°·67 C. The solution obtained indicated a temperature of 48°·96. Supposing that the capacity of the elements for heat remains the same, Richter finds that

$$\frac{1440 \,.\, 15{\cdot}55 + 3405 \,.\, 76{\cdot}67}{4845} = 58{\cdot}4$$

should be the temperature of the liquid. There is therefore a lowering of temperature of 9°·44. He admits that the specific heat of the liquid was 0·75, and that consequently the

---

[* A translation of the paper (by H. Rose) here referred to will be found in Phil. Mag. S. 3. vol. xiv. p. 78.—Edit.]

depression of temperature observed is equivalent to that which would have been produced by the fusion of 457·4 parts of ice at 0°. But as he finds that the 1440 parts of salt employed contain, not 457 parts of solidified water, but 803 parts, he concludes thence that this water had not lost as much heat as the water should necessarily have lost in order to freeze, and that consequently it is not correct to say ice of crystallization.

Notwithstanding the depth of his views, Richter was not the less exposed to critical attacks which were often unjust. His replies were always not only moderate, but in general as calm as if he had discussed an uncontested subject. When M * * * makes me such a reproach, says he, I bear it without thinking myself injured; I merely believe that irony does not suit the end which criticism ought to have in view and which should be to convince. Besides, every one cannot follow an author step by step in order to judge with knowledge of the subject, for it is not sufficient, for this purpose, to turn over the leaves of a work. Several times in his prefaces Richter complains of not being read with attention. Thus to give an idea of the manner in which his views were treated, I will mention another critic (M. Fries) who thought, for example, that it was impossible to explain why the elements followed a fixed law in their relations of neutrality. To that Richter replies, that nature would be very poor if she were limited only to what was intelligible for him and for his criticism.

Another critic asked him with more reason to give a summary of his doctrine which might be comprehended by every one. Richter's fault was that he did not express himself clearly; if circumstances had caused him to undergo the severe discipline of the French language, if Richter, like Lavoisier, had drawn his logic from the school of Condillac, the truths which he published would have spread with more facility, and he would have produced the same results with less labour.

In the sciences, gentlemen, labour is divided into two very distinct categories; some from their novelty and the generality of their results open a new field to investigation, and spread great truths which astonish the generation which sees them originate. These works, gentlemen, make an epoch in the history of the development of intelligence, and man is hardly ever ungrateful for this benefit. Others, sometimes as difficult as the preceding ones, are but a tribute of our love for science,—a right to the esteem of our contemporaries. They pursue and extend paths already opened. They cause us to be esteemed while we live; a certain deference surrounds us: but let us not deceive ourselves; it is but the homage which politeness imposes by the fact of our presence, for after us, a generation which passes over our grave is sufficient to cause these titles not to be remembered; the facts are quoted, the authors are forgotten.

The works of Richter, as we have seen, belong to these two distinct classes, and if it is true that a few words should suffice to sum up the entire life of a celebrated man, that of Richter is altogether summed up in these words (taken from the Wisdom of Solomon, xi. 22) which he placed as an epigraph at the head of all of his works which treat of chemical proportions:

> "God made all things, in measure, and number, and weight."

XVI. *On the Extension* of* Budan's *Criterion for the Imaginary Roots, and a new Method of effecting the Separation of the nearly equal Roots of a numerical Equation. By* JAMES R. CHRISTIE, *Esq.*†

BUDAN has shown that his criterion of the presence of imaginary roots only fails when, in the pair of roots $\alpha \pm \beta \sqrt{-1}$, α is a positive proper fraction and β is less than ·5, on account of the effect of his reciprocal transformation being that of converting these roots to the new form

$$\frac{\alpha + \beta \sqrt{-1}}{\alpha^2 + \beta^2} \text{ or } \alpha_1 \pm \beta_1 \sqrt{-1},$$

wherein α_1 must, in the failing case, be less than unity.

In the reduced reciprocal equation these roots become

$$\alpha_1 - 1 \pm \beta_1 \sqrt{-1};$$

and they may, as before, be shown to be imaginary unless β_1 be less than ·5.

If we suppose α to be *not greater* than β, then $\frac{1}{2\beta}$ will be the least value of the fraction β_1; but β is less than ·5, consequently this value of β_1 must exceed unity. It appears therefore that, in the case of α *not greater* than β, the condition upon which the failure of the criterion depends, ceases to exist in the roots as they appear in the first reduced reciprocal equation. The same will hold true if α does not exceed $\beta \sqrt{3}$, since the least value this condition allows for β_1 is ·5.

Let us now see in what manner α and β enter into the second reciprocal equation.

---

* It is proper to mention that, in 1840, I pointed out the practical application of this method, in an example which was casually brought under my notice, to my friend and colleague Mr. Davies, who considered the then crude remark as of sufficient importance to be inserted, with the example, in his last edition of Hutton's "Course of Mathematics."—J. R. C.

† Communicated by the Author.

Supposing that the variations (which correspond to those of the original equation whose indications of roots, real or imaginary, we are attempting to discover by aid of the criterion) disappear from the equation in $\frac{1}{x} - (p+1)$, the roots in the immediately preceding equation will be of the form

$$\frac{\alpha}{\alpha^2+\beta^2} - p \pm \frac{\beta\sqrt{-1}}{\alpha^2+\beta^2},$$

or

$$\frac{\alpha - p(\alpha^2+\beta^2) \pm \beta\sqrt{-1}}{\alpha^2+\beta^2},$$

and in the second reciprocal equation

$$\frac{(\alpha^2+\beta^2).\{\alpha - p(\alpha^2+\beta^2) \mp \beta\sqrt{-1}\}}{\{\alpha - p(\alpha^2+\beta^2)\}^2+\beta^2},$$

which finally reduces to

$$\frac{\alpha - p(\alpha^2+\beta^2)}{(1-p\alpha)^2+p^2\beta^2} \pm \frac{\beta\sqrt{-1}}{(1-p\alpha)^2+p\beta^2}.$$

Now p evidently represents the integer next less than $\frac{\alpha}{\alpha^2+\beta^2}$, to which, if we assume α *greater than* ·5, the superior limit is 2; consequently, in this case, $p = 1$, and the above expression becomes

$$\frac{\alpha - (\alpha^2+\beta^2) \pm \beta\sqrt{-1}}{(1-\alpha)^2+\beta^2},$$

or

$$\alpha_{11} \pm \beta_{11}\sqrt{-1},$$

making

$$\alpha_{11} = \frac{\alpha-(\alpha^2+\beta^2)}{(1-\alpha)^2+\beta^2} \text{ and } \beta_{11} = \frac{\beta}{(1-\alpha)^2+\beta^2}.$$

It is easily seen that β_{11} decreases with the value of α, and the lower limit of its value will therefore, in this case, be

$$\frac{\beta}{\cdot 25+\beta^2},$$

which decreases with the decrease of β; solving therefore the equation

$$\frac{\beta}{\cdot 25+\beta^2} = \cdot 5,$$

we obtain $\beta = 1 \pm \cdot 866$; and since β must be less than ·5, we have $\beta = \cdot 134$ as the value which β cannot exceed if β_{11} is less than ·5.

Should β_{11} be less than ·5 (α_1 and α_{11} being *greater* than ·5) two more reciprocal transformations will give $\beta_{\text{IV}}\sqrt{-1}$ as the imaginary part of the corresponding pair of roots, β_{IV} depending in value on β_{II} as β_{II} does upon β; we get therefore, from the equation $\beta_{\text{II}} = {\cdot}134$, the value

$$\beta = {\cdot}033$$

as that which exceeds all values of β which can make β_{IV} less than ·5.

It appears therefore that, in the case of α greater than ·5, a small *odd* number of reciprocal transformations can hardly fail to detect the imaginary roots, supposing α_n always greater than ·5.

Taking now the case α *not greater* than ·5, we shall obtain the least value which β_1 can in this case hold, by making in it $\alpha = {\cdot}5$; it becomes then

$$\frac{\beta}{{\cdot}25 + \beta^2},$$

which is precisely the same as the inferior limit to the value of β_{II} in the preceding case: it follows therefore from what has been there shown, that if α be not greater than ·5 the second reciprocal equation must detect the imaginary roots, unless β be less than ·134.

On a similar hypothesis the third reciprocal equation cannot fail unless β_1 be less than ·134, which involves the condition β less than ·033: and so on.

In thus developing the changes which this limit of β successively undergoes, it has been assumed that α_n, the real part of the imaginary roots in the nth reciprocal equation, retains the character assigned to it in each particular case; but it is manifest that if it does not retain its character, the change will only have the effect of altering the hypothesis from $\alpha > {\cdot}5$ to $\alpha < {\cdot}5$, or *vice versâ*.

Independently of the additional value which these considerations give to the criterion of Budan, there is yet another most difficult case which the same operations tend to elucidate, viz. that in which two or more roots are *nearly* equal to one another. In fact, let a and b be two roots very nearly equal, both of them positive and *less than unity*, a condition always attainable; in the first reciprocal equation these roots will appear under the form $\frac{1}{a}$ and $\frac{1}{b}$, and their difference becomes $\frac{a-b}{a\,b}$, *greater* than before, since $a\,b$ is a proper fraction. Now to whatever extent the roots of this equation are diminished, their difference is unaltered; if therefore this difference should still be less than unity, another reciprocal transformation will again increase it; so that each transformation must of necessity bring us nearer to the point at which the roots corresponding to a and b are separable by means of unital reductions. When this point is arrived at, we are at once enabled to assign the true values of the roots by means of a continued fraction, similarly to the method employed by Lagrange, as the following example will show.

The given equation is

$$x^5+7\,x^4-144\,x^3+611\,x^2-928\,x+362 = 0,$$

from which we get successively,

$$x_1{}^5+12x_1{}^4-106\,x_1{}^3+231\,x_1{}^2-105\,x_1-91 = 0 \;\; (x_1 = x-1)$$
$$x_{\text{II}}{}^5+17\,x_{\text{II}}{}^4-48\,x_{\text{II}}{}^3-5\,x_{\text{II}}{}^2+92\,x_{\text{II}}-58 = 0 \;\; (x_{\text{II}} = x-2)$$
$$x_{\text{III}}{}^5+22x_{\text{III}}{}^4+30\,x_{\text{III}}{}^3-37x_{\text{III}}{}^2+11\,x_{\text{III}}-1=0 \;\; (x_{\text{III}}=x-3)$$

at the next transformation we shall evidently lose three variations; taking therefore the reciprocal equation and reducing, we have

$$y_1{}^5-6\,y_1{}^4+3y_1{}^3+25\,y_1{}^2-10y_1-26=0\left(y_1=y-1, y=\frac{1}{x_{\text{III}}}\right).$$

Since this equation retains the three variations, there is every probability, by Budan's criterion, that the indicated roots are all real. Proceeding with the reductions, and retaining the same notation, we obtain

$$y_{\text{II}}{}^5-y_{\text{II}}{}^4-11\,y_{\text{II}}{}^3+8\,y_{\text{II}}{}^2+30\,y_{\text{II}}-13 = 0\text{...3 variations.}$$
$$y_{\text{III}}{}^5+4\,y_{\text{III}}{}^4-5\,y_{\text{III}}{}^3-21\,y_{\text{III}}{}^2+14\,y_{\text{III}}+14 = 0\text{...2 var.}$$

so that there is a root of the equation in y between 2 and 3. In continuation, we have

$$y_{\text{IV}}{}^5+9\,y_{\text{IV}}{}^4+21\,y_{\text{IV}}{}^3-2\,y_{\text{IV}}{}^2-22\,y_{\text{IV}}+7 = 0\text{...2 variations,}$$

and in the equation in y_{V} there will be no variations. Again, therefore, we take the reciprocal equation in $z = \dfrac{1}{y_{\text{IV}}}$ and continue the reductions:

$$7z_1{}^5+13\,z_1{}^4-20\,z_1{}^3-47\,z_1{}^2-8\,z_1+14 = 0\text{...2 variations;}$$

from which it still appears that these roots are not imaginary. Proceeding as before, we get

$$7\,z_{\text{II}}{}^5+48\,z_{\text{II}}{}^4+102\,z_{\text{II}}{}^3+41\,z_{\text{II}}{}^2-75\,z_{\text{II}}-41 = 0\text{ ...1 var.}$$

and the equation in z_{III} will contain only permanences; one root therefore of the equation in z is between 1 and 2, and the other between 2 and 3.

In order to determine the actual approximate values of these roots in the original equation, we have, by making $y = 2{\cdot}5$, $z = 1{\cdot}5$, and $z = 2{\cdot}5$, the three continued fractions,

$$x_{\text{III}} = \cfrac{1}{2+\cfrac{1}{2}} \qquad x_{\text{III}} = \cfrac{1}{4+\cfrac{1}{1+\cfrac{1}{2}}} \qquad x_{\text{III}} = \cfrac{1}{4+\cfrac{1}{2+\cfrac{1}{2}}}:$$

whence $x = 3{\cdot}4$, $x = 3{\cdot}214$, $x = 3{\cdot}227$.

As a still more difficult example of the separation of roots very nearly equal to one another, let us take the equation

$$v^5 - 82\,v^4 + 2404\,v^3 - 26394\,v^2 + 6132\,v - 360 = 0,$$

and it will be found that the following transformations will be obtained, viz.

$$360\,w_{\text{VIII}}^5 + 8268\,w_{\text{VIII}}^4 + 60570\,w_{\text{VIII}}^3 + 119564\,w_{\text{VIII}}^2$$
$$-156270\,w_{\text{VIII}} + 40335 = 0 \ \ldots\ w = \frac{1}{v},$$
$$40335\,x_{\text{II}}^5 + 247080\,x_{\text{II}}^4 + 482804\,x_{\text{II}}^3 + 254274\,x_{\text{II}}^2$$
$$-88524\,x_{\text{II}} + 6088 = 0 \ \ldots\ldots\ldots\ x = \frac{1}{w_{\text{VIII}}},$$
$$6088\,y_{\text{VII}}^5 + 124556\,y_{\text{VII}}^4 + 758712\,y_{\text{VII}}^3 + 678342\,y_{\text{VII}}^2$$
$$-3983874\,y_{\text{VII}} + 2418165 = 0 \ldots y = \frac{1}{x_{\text{II}}},$$
$$2418165\,z_1^5 + 8106951\,z_1^4 + 8924496\,z_1^3 + 3072144\,z_1^2$$
$$-167665\,z_1 + 1989 = 0 \ \ldots\ldots\ldots\ z = \frac{1}{y_{\text{VII}}},$$
$$1989\,t^5 - 167665\,t^4 + 3072144\,t^3 + 8924496\,t^2 + 8106951\,t$$
$$+2418165 = 0 \ \ldots\ldots\ldots\ldots\ldots\ldots\ t = \frac{1}{z_1}.$$

Now it will be found that one root of this equation is between 30 and 31, the other between 50 and 51, so that we have the continued fractions,

$$v = \cfrac{1}{8+\cfrac{1}{2+\cfrac{1}{7+\cfrac{1}{1+\cfrac{1}{30+\cfrac{1}{2}}}}}} \qquad v = \cfrac{1}{8+\cfrac{1}{2+\cfrac{1}{7+\cfrac{1}{1+\cfrac{1}{50+\cfrac{1}{2}}}}}}$$

which give the values

$$v = {\cdot}118057 \text{ and } v = {\cdot}11805649:$$

the actual approximate roots to twelve places are

$${\cdot}118056983866 \text{ and } {\cdot}118056440257.$$

The number of reciprocal transformations necessary to effect the separations of the roots, will of course primarily depend upon the number of figures in them which are identical; but there are certain points in the scale between 0 and 1, from which, if the roots differ, in however small a degree, the one in excess and the other in defect, the *first* reciprocal equation suffices to show their inequality. These points are, evidently, the reciprocals of the integers above unity, that is

$$\cdot 5, \quad \cdot 33, \quad \cdot 25, \quad \cdot 2, \quad \cdot 167, \quad \cdot 143, \&c.$$

It would at first sight appear that the least favourable case for the separation of the roots is that in which they differ least from unity, since this gives its maximum value to the denominator of the fraction $\frac{a-b}{ab}$; but it must be remarked that this hypothesis involves the consequence that the corresponding pair of roots in the reduced reciprocal equation will have their *smallest* value, and therefore be in the most favourable state for separation.

In order then to obtain some general insight into the extent of separation effected by each reciprocal transformation, we may take, as that of slowest divergence, the case in which the roots occupy a point midway between two of the above numbers, and are as near to ·5 as this condition will allow. Assuming, therefore, the roots to be rather greater than ·41, we have $\frac{1}{ab} = 6$; and taking this number as the factor by means of which may be determined the divergence of two roots having, originally, their difference less than unity, it follows that the number of figures in its *n*th power will, not inaptly, represent the smallest number of figures which can be identical in the roots which become separable in the *n*th reciprocal equation.

From the preceding investigation we obtain a correct idea of the extreme rarity of the cases in which the impossibility, or the near equality of two or more doubtful roots can fail to be made manifest by means of this simple method of reciprocals; and the improbability of the occurrence of these cases affords the strongest evidence of the general utility of the method.

Royal Military Academy, June 13, 1842.

XVII. *On the Analytical Condition of the Rectilinear Motion of Fluids. By the Rev.* J. Challis, *M.A., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

THE mathematical reasoning which I gave in the April Number of this Journal (S. 3. vol. xx. p. 281) respecting a new equation in hydrodynamics, led me by indirect considerations to the conclusion, that when $u\,dx + v\,dy + w\,dz$ is a

[*] Communicated by the Author.

complete differential of a function of three independent variables, the motion of the fluid is rectilinear. This theorem, when once established, becomes so essential a part of analytical hydrodynamics, and so materially affects much that has been written in this department of science, that I make no apology for adding a *direct* proof of it.

Let x, y, z be the coordinates of any point of the fluid at a given time, and $x + dx$, $y + dy$, $z + dz$ the coordinates at the same time of another point distant from the former by the indefinitely small line ds. Let ds make angles α, β, γ with the axes of rectangular coordinates, and let the direction of the velocity V at the point $x\,y\,z$ make angles α', β', γ' with the same axes, and an angle θ with the line ds. Then, the components of V in the directions of the axes being u, v, w, we have

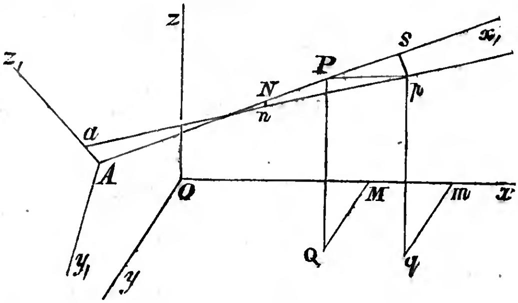
$$u dx + v dy + w dz = \mathrm{V}\, ds \,.\left(\frac{u}{\mathrm{V}}\cdot\frac{dx}{ds} + \frac{v}{\mathrm{V}}\cdot\frac{dy}{ds} + \frac{w}{\mathrm{V}}\cdot\frac{dz}{ds}\right),$$
$$= \mathrm{V} ds.(\cos\alpha\cos\alpha' + \cos\beta\cos\beta' + \cos\gamma\cos\gamma'),$$
$$= \mathrm{V}\, ds \cos\theta.$$

If, therefore, dr be the projection of ds on the line of motion, it follows that

$$u\,dx + v\,dy + w\,dz = \mathrm{V}\,dr.$$

This equality is true whether the left-hand side be an exact differential or not. Supposing it to be an exact differential, we might perhaps at once assert (since V dr must be exactly integrable) that V is a function of x, y, z, which varies at a given instant by change of position from point to point of the line of motion, but not by change of position in any direction transverse to this; in other words, that V is invariable in passing from point to point of the surface of displacement of which $u\,dx + v\,dy + w\,dz = 0$ is the differential equation. But that nothing may appear to be taken for granted in a question of so much importance, I proceed to prove that V must be a function of this kind, in order that the three equations $\frac{du}{dy} = \frac{dv}{dx}$, $\frac{du}{dz} = \frac{dw}{dx}$, $\frac{dv}{dz} = \frac{dw}{dy}$, may be satisfied.

When the condition of the continuity of the fluid is maintained, the most general supposition that can be made respecting the directions of motion in an indefinitely small element of the fluid, is that they are normals to a surface of continued curvature, and consequently intersect at right angles each of two *focal lines* situated in the planes of greatest and least curvature. In the annexed diagram let Ox, Oy, O z be the axes of rectangular coordinates, and let the coordinates O M, M Q, Q P of the point P be x, y, z, and the



coordinates O m, $m\,q$, $q\,p$ of p be $x + dx$, y, z; so that the indefinitely small line P p is parallel to the axis O x. Draw the straight lines P N A, $p\,n\,a$, in the directions of the motion at the points P, p, at a given instant. Since these points are supposed to be indefinitely near each other, they may be considered to belong to the same indefinitely small element of the fluid, and consequently, by what has just been said, the lines P N A, $p\,n\,a$, are ultimately normals to the same curve surface, and pass through two focal lines such as N n and A a. Take A, the intersection of P N A with the focal line A a, for a new origin of rectangular coordinates $x_{\prime}$, $y_{\prime}$, $z_{\prime}$; and let the axis A $z_{\prime}$ coincide in direction with A a, the axis A $x_{\prime}$ with A N P, and the axis A $y_{\prime}$ be parallel to N n. Draw $p\,s$ perpendicularly on A $x_{\prime}$. Let A N $= l$, N P $= r$, and P $s = r_{\prime}$. Also let the velocity at P be V, and that at the same time at p be V + $\mathrm{V}_{\prime}$.

The component of the velocity at P in the direction of z being w, let the component of the velocity at p in the same direction be $w + dw$. Then,

$$w = \mathrm{V} \cos \angle \mathrm{A\,P\,Q},$$

and
$$w + dw = (\mathrm{V} + \mathrm{V}_{\prime}) \cos \angle a\,p\,q,$$
$$= \mathrm{V} \cos \angle a\,p\,q + \mathrm{V}_{\prime} \cos \angle \mathrm{A\,P\,Q},$$

terms of the second order being neglected. Hence

$$dw = \mathrm{V}\,(\cos \angle a\,p\,q - \cos \angle \mathrm{A\,P\,Q}) + \mathrm{V}_{\prime} \cos \angle \mathrm{A\,P\,Q}.$$

Also,
$$dx = \mathrm{P}\,s \sec \angle p\,\mathrm{P}\,s = r_{\prime} \sec \angle p\,\mathrm{P}\,s,$$

therefore

$$\frac{dw}{dx} = \frac{\mathrm{V}\,(\cos \angle a\,p\,q - \cos \angle \mathrm{A\,P\,Q}) + \mathrm{V}_{\prime} \cos \angle \mathrm{A\,P\,Q}}{r_{\prime} \sec \angle p\,\mathrm{P}\,s}.$$

The limiting value of the right-hand side of this equation is now to be found.

Let the equations of the three lines $a\,n\,p$, $\mathrm{P}\,p$, $p\,q$, referred to the axes $\mathrm{A}\,x_{\prime}$, $\mathrm{A}\,y_{\prime}$, $\mathrm{A}\,z_{\prime}$, be respectively

$$\left.\begin{array}{l} x_{\prime} = m\,z_{\prime} + n \\ y_{\prime} = p\,z_{\prime} + q \end{array}\right\} (1.), \quad \left.\begin{array}{l} x_{\prime} = a\,z_{\prime} + \alpha \\ y_{\prime} = b\,z_{\prime} + \beta \end{array}\right\} (2.), \quad \left.\begin{array}{l} x_{\prime} = a'\,z_{\prime} + \alpha' \\ y_{\prime} = b'\,z_{\prime} + \beta' \end{array}\right\} (3.)$$

Then by known formulæ,

$$\cos \angle p\,\mathrm{P}\,s = \frac{a}{\sqrt{1 + a^2 + b^2}}, \quad \cos \angle \mathrm{APQ} = \frac{a'}{\sqrt{1 + a'^2 + b'^2}},$$

and $$\cos \angle a\,p\,q = \frac{1 + m\,a' + p\,b'}{\sqrt{1 + a'^2 + b'^2}\,.\,\sqrt{1 + m^2 + p^2}}.$$

The values of m and p may be found as follows:—Let $\mathrm{A}\,a = h$, and $\mathrm{N}\,n = k$. Then because the line $a\,n\,p$ passes through the points a and n, whose coordinates are 0, 0, h, and l, k, 0, respectively, the equations (1.) become

$$\left.\begin{array}{l} x_{\prime} = \dfrac{l}{p}\,(h - z_{\prime}) \\[2ex] y_{\prime} = \dfrac{k}{p}\,(h - z_{\prime}) \end{array}\right\} \; \ldots\ldots\ldots \; (4.)$$

And because the line $\mathrm{P}\,p$ passes through the point P, whose coordinates are $r + l$, 0, 0, the equations (2.) become

$$\left.\begin{array}{l} x_{\prime} = a\,z_{\prime} + r + l \\ y_{\prime} = b\,z_{\prime} \end{array}\right\} \; \ldots\ldots\ldots \; (5.)$$

Now the coordinate $\mathrm{A}\,s$ of the point p is $l + r + r_{\prime}$. Hence it follows from equations (5.) that the other coordinates of p are $y_{\prime} = \frac{b\,r_{\prime}}{a}$, and $z_{\prime} = \frac{r_{\prime}}{a}$. These values must satisfy equations (4.), because the line $a\,n\,p$ passes through the point p. By substituting them in those equations, it will be found that

$$-\frac{l}{p} = a\left(\frac{r}{r_{\prime}} + 1\right), \text{ which is the required value of } m;$$

$$\text{and } -\frac{k}{p} = \frac{b\,(r + r_{\prime})}{l + r + r_{\prime}}, \text{ which is the value of } p.$$

By substituting these values of m and p in the foregoing expression for $\cos \angle a\,p\,q$, expanding and neglecting powers of $r_{\prime}$ above the first, and bearing in mind that $1 + a\,a' + b\,b' = 0$, it will appear that

$$\cos \angle a\,p\,q = \frac{a'}{\sqrt{1 + a'^2 + b'^2}}\,.\left\{1 - \frac{(a\,a'\,r - l)\,r_{\prime}}{a\,a'\,r\,(l + r)}\right\}.$$

Hence, $\cos \angle apq - \cos \angle \mathrm{APQ} = \dfrac{(a a' r - l) r_{,}}{a r (l + r) \sqrt{1 + a'^2 + b'^2}}$.

We have, therefore, by obvious substitutions,

$$\frac{dw}{dx} = \frac{\dfrac{\mathrm{V}(a a' r - l)}{r(r + l)} + \dfrac{\mathrm{V}_{,} a a'}{r_{,}}}{\sqrt{1 + a^2 + b^2} \,.\, \sqrt{1 + a'^2 + b'^2}}.$$

So if V_2, r_2 be the increments of V and r, the coordinate z only being supposed to vary, by exactly the same reasoning we shall obtain,

$$\frac{du}{dz} = \frac{\dfrac{\mathrm{V}(a a' r - l)}{r(r + l)} + \dfrac{\mathrm{V}_2 a' a}{r_2}}{\sqrt{1 + a'^2 + b'^2} \,.\, \sqrt{1 + a^2 + b^2}}.$$

If, therefore, $\dfrac{du}{dz} = \dfrac{dw}{dx}$, we must have $\dfrac{\mathrm{V}_2}{r_2} = \dfrac{\mathrm{V}_{,}}{r_{,}}$.

Hence, V_3 and r_3 being corresponding increments of V and r when y only varies, we may conclude that

$$\frac{\mathrm{V}_1}{r_1} = \frac{\mathrm{V}_2}{r_2} = \frac{\mathrm{V}_3}{r_3},$$

if $\dfrac{du}{dy} = \dfrac{dv}{dx}$, $\dfrac{du}{dz} = \dfrac{dw}{dx}$, and $\dfrac{dv}{dz} = \dfrac{dw}{dy}$;

that is, if $u\,dx + v\,dy + w\,dz$ be an exact differential. Assuming now that $r_1 = r_2 = r_3$, we shall also have $\mathrm{V}_1 = \mathrm{V}_2 = \mathrm{V}_3$. Hence the increments of velocity in the directions of the axes of coordinates are the same, when the projections of the increments of the coordinates on the line of motion are the same. As the directions of the axes of coordinates may be arbitrarily assumed, the general inference from this result is, that when $u\,dx + v\,dy + w\,dz$ is an exact differential, the increment of velocity from one point to another at a given time depends only on the change of position in the direction of the motion; which it was required to prove.

Supposing now that $u\,dx + v\,dy + w\,dz = d\phi$, that p is the pressure and ϱ the density at the point $x\,y\,z$, and that X, Y, Z are the impressed accelerative forces at that point in the directions of the axes of coordinates, we have the known general equation,

$$\int \frac{dp}{\varrho} = \int (\mathrm{X}\,dx + \mathrm{Y}\,dy + \mathrm{Z}\,dz) - \frac{d\phi}{dt} - \frac{\mathrm{V}^2}{2} + f(t),$$

which, being differentiated with respect to space, gives

$$\frac{dp}{\varrho} = \mathrm{X}\,dx + \mathrm{Y}\,dy + \mathrm{Z}\,dz - d\,.\,\frac{d\phi}{dt} - \mathrm{V}\,d\mathrm{V}.$$

Now, if the coordinates be supposed to vary from one point to another of a surface of displacement, from what is proved above, $d\mathrm{V} = 0$. Also $d.\frac{d\phi}{dt} = \frac{d.(d\phi)}{dt} = \frac{d.}{dt}(u\,dx + v\,dy + w\,dz)$ $= 0$, because for a surface of displacement $u\,dx + v\,dy + w\,dz$ $= 0$. Hence, dividing by ds the increment of space,

$$\frac{dp}{\varrho\,ds} - \left(\mathrm{X}.\frac{dx}{ds} + \mathrm{Y}.\frac{dy}{ds} + \mathrm{Z}.\frac{dz}{ds}\right) = 0.$$

The left-hand side of this equation is the *effective* accelerative force in any direction perpendicular to that of the motion. As this force vanishes, the motion must be *rectilinear.*

It follows from this reasoning that *the sole and the necessary condition of the rectilinear motion of a fluid is, that* $u\,dx + v\,dy + w\,dz$ *be an exact differential of a function of three independent variables.*

It has been argued by Lagrange in the *Mécanique Analytique,* that $u\,dx + v\,dy + w\,dz$ is an exact differential when the motion begins from rest, and again, when the motion is so small that the squares and higher powers of u, v, and w may be neglected. These propositions are inserted in the edition of Poisson's *Traité de Mécanique* of 1811, but are omitted in that of 1833. In the Memoirs of the Academy of Paris (tome x. 1831), Poisson considers a problem in which that condition is not fulfilled, though the motion is small. Against the former of the above propositions it may be urged that when $u = 0$, $v = 0$, $w = 0$, it cannot be asserted of $u\,dx + v\,dy + w\,dz$ either that it is integrable or that it is not integrable; and against the latter, that the integrability of the quantity in question is in no respect dependent upon the magnitudes of u, v and w. For example, $\mathrm{V}.\frac{x-a}{z-c}\,dx + \mathrm{V}.\frac{y-b}{z-c}\,dy$ $+ \mathrm{V}.\frac{z-c}{z-c}\,dz$, is as far from being integrable when V is a very small quantity, as when V is large. On this account, the cases of fluid motion in which $u\,dx + v\,dy + w\,dz$ is an exact differential must be determined by considerations independent of the *magnitude* of the motion, as I have done in this communication.

To prevent misapprehension on this subject I may also remark, that it is possible to assume at pleasure values of u, v and w, which will satisfy the equation of continuity and make $u\,dx + v\,dy + w\,dz$ integrable, and at the same time give a *curvilinear* motion. For example, if $u = mx$, $v = -my$ and $w = 0$, and the fluid be incompressible, each particle moves in a hyperbola. But in such cases the arbitrary quantities introduced by integration cannot be satisfied, unless the motion be in confined spaces or narrow canals, such that the coordinates in passing from one point of the fluid to another do not vary *independently* of each other. These instances are not, therefore, exceptions to the general rule.

Cambridge Observatory, June 15, 1842.

XVIII. *Contributions to the Minute Anatomy of Animals.* By GEORGE GULLIVER, *F.R.S., &c. &c.*—No. II.*

On the Nuclei of the Blood-Corpuscles of the Vertebrata.

BY subjecting the blood of adult mammals to the slow action of a very minute quantity of dilute acetic acid, Dr. Martin Barry states that he has observed nuclei in the corpuscles, which he has depicted in his recent and elaborate researches on the blood (Phil. Trans., 1841, part 2). Yet it seems fair to conclude that there is an essential difference between the blood-corpuscles of mammals and those of the lower vertebrata, since the very same treatment which never fails to show the nuclei in the latter will not exhibit them in the former. This, as I have elsewhere stated (Appendix to Gerber's Anatomy, pp. 13 and 30), does not prove that the corpuscles of mammals include no central matter, although it induced me to believe that these corpuscles have no nucleus like that contained in the corpuscles of the lower vertebrate animals.

When the corpuscles of the oviparous vertebrata are mixed with water, or with dilute or strong acetic acid, the nuclei are instantly exposed in the clearest manner, appearing thick, oval or spherical, and much smaller than their envelopes. Several other vegetable acids, and sulphurous acid, may be used with the same effect; and the nuclei may also be readily shown by gently moistening with the breath some dry blood, which may be again quickly dried so as to preserve the nuclei on the slip of glass for future demonstration. But when the blood-corpuscles of Man and of other mammals, not excepting the oval discs of the Camelidæ (Med.-Chir. Trans., vol. xxiii., and Lancet, vol. ii. p. 101, 1840–41) are treated by any of the means just specified, and precisely under the same circumstances, no similar nuclei will be observed, unless in very young embryos; for the corpuscles of these inclose a temporary and obvious nucleus, which is probably the true *analogue* of the persistent nucleus of the corpuscles of the oviparous vertebrata.

In the Philosophical Magazine for February 1840, (S. 3.

---

* Communicated by the Author. No. I. will be found in p. 480 of the preceding volume.

vol. xvi.) p. 106–107, I have noticed that the blood-discs of mammalia become smaller after the removal of their colouring matter by repeated additions of water. Thus some human corpuscles having an average diameter of $\frac{1}{3429}$th of an inch, measured only $\frac{1}{4800}$th after the whole of their colouring matter had been separated in this manner, when they appeared flat and pellucid, very faint, and obviously differing in size and general characters from the particles usually described as the nuclei of the blood-corpuscles. No nuclei can be discerned in these washed corpuscles, either by the aid of acids, of corrosive sublimate, or of iodine.

The first part of the preceding observation agrees in some essential points with the results obtained by Sir E. Home (Phil. Trans., 1818, pl. viii. figs. 1, 2, and 3), Schultz (Lancet, 1838–39, vol. ii. p. 713), and Donné (Mandl, Anat. Micros., liv. i. p. 8–9).

If the colouring matter be in like manner washed completely from the blood-corpuscles of the lower vertebrata, both the nuclei and envelopes will remain, the latter becoming quickly circular, and the former also after a few hours. Subsequently the envelopes are scarcely visible, and the colourless matter of the corpuscles, which subsides in the water, appears to be composed chiefly of the nuclei, although with the aid of iodine many of the envelopes may be seen; and these are more or less reduced in size after a few days, especially in warm weather. Corrosive sublimate affects them very feebly, although it instantly increases the opacity of the washed corpuscles of mammalia. When the former corpuscles have been kept some days in water, the envelopes become very irregular, and hardly perceptible by any means; the size of the nuclei is diminished, and they at length break up into extremely minute molecules.

Dilute muriatic acid renders the nucleus clearly visible in the blood-corpuscles of the oviparous vertebrata. If the corpuscles of a mammal be treated with the same acid, many of them appear shrunk and puckered, notched at the edges, and granulated; some present a distinct central spot, irregular at the margin, like a granular nucleus; others remain smooth at the circumference, often misshapen, and generally with a dark or brilliant central part, according to the focal distance in which they are placed.

The two following figures will illustrate the foregoing observations. The blood-corpuscles of man, and of an adult bird, with some fibrine from the blood of the latter, are represented as magnified about 820 diameters.

Fig. 1. Outlines of blood-corpuscles of Man. In the lower part of the figure, at A, corpuscles in pure blood from a prick of the finger: some of them, lying flat, exhibit the central

spot, which others are without; several are seen on their edges collected into a pile; of the two standing separately on their edges, one appears concavo-concave, and the other concavo-convex. B. The corpuscles after thirty hours' washing in cool weather, the water having been changed until the whole of the colouring matter was completely removed. These membranous bases of the discs are extremely faint; but, as shown at C, they may be rendered very distinct by corrosive sublimate. D. Appearance of fresh corpuscles quickly after treating them with dilute muriatic acid: six of them extend horizontally across the figure.

Fig. 1.

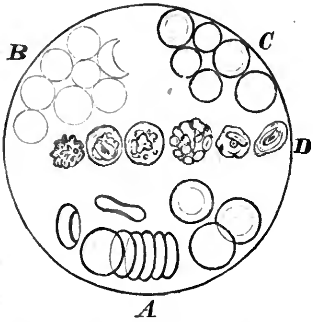


Fig. 2.

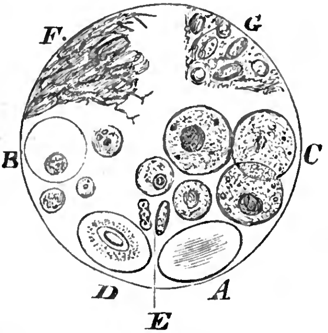


Fig. 2. Blood-corpuscles and fibrine of a Goose. At A is a fresh unchanged corpuscle. B. Corpuscles after having been washed precisely in the same way as those of the man, but in colder weather; four nuclei are seen, one of which appears to contain minuter granules or nucleoli, and another has a faint envelope. C. The washed corpuscles treated with iodine; some minute molecules adhere to the envelopes, and the nuclei seem to contain nucleoli; the two smaller corpuscles had remained three or four days in the water, at which time many of the envelopes were destroyed, others made irregular in size and shape, and the nuclei reduced to very minute molecules. D. A fresh corpuscle treated with dilute muriatic acid. E. Two oval nuclei obtained by dilute acetic acid from fresh corpuscles, for comparison with the nuclei which appear globular after having been kept in water, as seen at B and C. F. Fibrine obtained from fresh blood by washing it in a linen bag. G. The same fibrine, in which a multitude of oval particles, like the nuclei of the blood-discs, are shown by acetic acid.

On the Structure of Fibrine.

In the English version of Gerber's Anatomy, I have depicted organic germs, or objects resembling nucleated nuclei, in clots of fibrine. Those drawings were made from clots which were either pale and opake, or as transparent and colourless as the serum of the blood. I have lately examined the red portions often found towards the edges of such clots, and observed in these coloured parts a multitude of objects like the organic germs above mentioned, but tinged with the colouring matter of the blood. These ruddy bodies appeared to be merely blood-discs entangled in the fibrinous clot and altered in their characters; and hence the pale germs formerly delineated may likewise have been blood-discs still more changed, especially as the corpuscles of the blood are regarded as cells by Schwann, and cell-nuclei by Valentin, while Dr. Barry, as the result of his interesting observations, asks how many tissues are there which the blood-corpuscles may not form?

The corpuscles, of a yellowish or ruddy hue when highly magnified, were contained abundantly in the coloured fibrine: they were rather more irregular in shape than the free corpuscles of the same blood, and differed especially from the latter in exhibiting nuclei when washed either with dilute or strong acetic acid, and even occasionally without the aid of any reagent. The nuclei often appeared as if flattened and with a central point, and sometimes like mere granules; they were commonly grouped together in the centre of the corpuscle, frequently separated, and sometimes scattered about its circumference.

The following figure was made from a minute red part, magnified 800 diameters, of a large, white and very firm clot of fibrine from the heart of a woman, aged 20, who died of puerperal peritonitis and acute pleurisy.

Fig. 3.

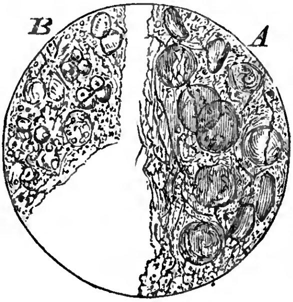


Fig. 3. A. A portion of the coloured fibrine without any addition. The corpuscles are contained in a mesh of most delicate fibrils, such as I have formerly described in clots of fibrine

(Gerber's Anatomy, p. 31); some of the corpuscles, just like misshapen blood-discs, are seen on their edges; others appear mottled, and one exhibits three nuclei. Many minute circular molecules are seen in the fibrine; they were generally from $\frac{1}{30,000}$th to $\frac{1}{15,000}$th of an inch in diameter, but their appearance has not been at all clearly preserved in the engraving. B. The same washed with dilute acetic acid; the nuclei of the corpuscles and the minute molecules are distinctly exhibited. Several of the latter are attached to a corpuscle made very faint by the acid.

In fibrine obtained by washing from the blood of the oviparous vertebrata, there is also frequently an appearance of minute fibrils, as shown at F, in fig. 2; but this fibrine is chiefly characterized by its containing numerous particles similar to and probably identical with the nuclei of the blood corpuscles: these particles may often be seen in the fibrine without the addition of any reagent, and acetic acid renders them very plain, as at G in fig. 2.

XIX. *An Account of some Experiments with the Torsion-rod, for Determining the Mean Density of the Earth. By* Francis Baily, *Esq., F.R.S., Vice-President of the Royal Astronomical Society*[*].

THE author commences his account with a short preliminary history of the subject, and a reference to the previous labours of Maskelyne and Cavendish. He considers the experiments of Maskelyne, on the attraction of the Schehallien mountain, by no means decisive of the question; and with respect to those of Cavendish, by means of the torsion-rod, he is of opinion that Cavendish's object in drawing up his memoir was more for the purpose of exhibiting a *specimen* of what he considered to be an excellent method of determining this important inquiry, than of deducing a result, at that time, that should lay claim to the full confidence of the scientific world. For, Cavendish himself (who made only 23 experiments), in allusion to this very point, expresses a doubt on the subject, and hints at some further experiments which he had in view, for clearing up some of the irregularities which he had met with. But, as no further account of any subsequent experiments is on record, and as no trace of any new light on this subject can be found amongst Cavendish's papers, the propriety and advantage of repeating the experiments, under

---

[*] From the Monthly Notices of the Royal Astronomical Society, having been read May 13 and June 10, 1842. An abstract of Mr. Baily's preliminary paper was given in Phil. Mag. Third Series, vol. xii. p. 233: a notice of M. Menabrea's paper on Cavendish's Experiments will be found in vol. xix. p. 62. A translation of Laplace's memoir on the mean density of the Earth, in which Cavendish's results are examined, was communicated by Dr. Hutton to the First Series, vol. lvi. p. 321.—Edit.

new circumstances, and with all the improvements of modern artists, had consequently been frequently discussed amongst scientific persons: and in the year 1835 the Council of this Society appointed a Committee for the express purpose of considering the subject. No effective steps, however, were taken even by this body for carrying the measure into execution till the autumn of the year 1837, when Mr. Airy, the Astronomer Royal (one of the Vice-Presidents of this Society), applied for, and obtained from his late Majesty's Government, a grant of 500*l.* to defray the expenses of this object.

Mr. Baily having offered to undertake the laborious task of making the proposed experiments, and of computing the results, the whole arrangement of the plan, and the entire execution of the work, was placed at his disposal and under his control.

It is somewhat singular, that, whilst this plan was in agitation in this country, a similar course of experiments had been actually undertaken and accomplished by M. Reich, Professor of Natural Philosophy in the Academy of Mines, at Freyberg in Saxony; an account of which was read before the German Scientific Association, which met at Prague in September 1837; and an abstract of the results was printed in the Monthly Notices of this Society, for December following*. Though the experiments are, on the whole, in good accordance with the general result obtained by Cavendish, yet they do not interfere with the plan that this Society had in contemplation; which was not merely to repeat the original experiments of Cavendish in a somewhat similar manner, but also to extend the investigation by varying the magnitude and substance of the attracted balls—by trying the effect of different modes of suspension—by adopting considerable difference of temperature—and by other variations that might be suggested during the progress of the inquiry. Reich made use of *one* mass only, and that much inferior in weight to the *two* adopted by Cavendish. The weight of Reich's large ball was little more than 99 pounds avoirdupois; whilst the two spheres, used by Cavendish, weighed nearly 700 pounds. Reich's experiments also were (like Cavendish's) too few in number; 57 only having been made, from which fourteen results have been deduced; the mean of which makes the density of the earth equal to 5·44, almost identical with that of Cavendish.

As a great portion of the apparatus, which had been ordered, was at this time actually completed, and the remainder of it in considerable progress, Mr. Baily resolved to proceed in the

---

[* This abstract appears in Mr. Baily's preliminary paper, already referred to.—Edit.]

inquiry, notwithstanding this apparent confirmation of Cavendish's results. Various places were suggested, by different persons, as the most suitable and fit for performing experiments of this kind: but, after inspecting several situations that were proposed, and considering all the circumstances of the case, Mr. Baily at length decided to carry them on at his own house, which he considers to be not only the most convenient that he could have selected, but which he has since found to be as suitable and fit as any that could have been specially erected for the express purpose. This house stands detached from any other building, in a large garden, some distance from the street, and consists of one story only.

The author then proceeds to give a description of the room in which the experiments were made, and likewise of the apparatus that was constructed for this special purpose. Although the apparatus was in a general view similar to that of Cavendish, yet in some respects it was essentially different. The great balls (or *masses*, as they are called) were suspended from the ceiling by Cavendish and Reich: but Mr. Baily supported them, from the floor, on a plank which turned on a pivot, and suspended the small balls from the ceiling; thus reversing the mode of operations. This method of moving the masses he considers to be a great improvement: for he says, "Nothing can exceed the ease, the steadiness, and the facility with which these large bodies are moved: and during the many thousands of times that they have been turned backwards and forwards, I have never observed the least deviation from the most perfect accuracy. At the final close of all the experiments, the pivot turns as steadily, as freely, and as accurately as at the commencement of the operations." The small balls were also, by Cavendish and Reich, suspended by a fine wire from the ends of the torsion-rod; whereas Mr. Baily screwed them to the ends of the torsion-rod, of which they thus formed an integral and solid portion. The motion of the torsion-rod was observed by means of a reflected image of the scale, from a small mirror attached to the centre of the torsion-rod, in the manner proposed by Gauss in magnetical experiments*, and adopted by Reich. Some other alterations were likewise made in the construction and arrangement of the apparatus, to which it is unnecessary to allude more minutely on the present occasion.

Mr. Baily made use occasionally of several small balls, of different sizes, and formed of different substances, with a view

---

[* See Phil. Mag. Third Series, vol. ii. p. 296: also Taylor's Scientific Memoirs, vol. ii. p. 31,—EDIT.]

of ascertaining whether the results would be affected by such a variation: these were platina, lead, zinc, glass, ivory, and *hollow* brass, varying from 1½ inch to 2½ inches in diameter. The mode of suspension was also diversified, with a similar view: iron, copper, brass, and silk were successively used, not only single, but also double, similar to the bifilar mode suggested by Gauss* for certain magnetical experiments. The mean weight of *each* of the great balls (or masses) was 2,663,282 grains, or about 380½ pounds avoirdupois, as determined by the accurate weights and scales of the Bank of England. And the weight of each of the small balls varied from 1950 to 23,742 grains. The length of the suspension-line was 60 inches; and the length of the torsion-rod (between the centres of the two balls affixed thereto) was nearly 80 inches. The torsion-rod was made of fine deal, of an uniform shape throughout its whole length, and weighed only about 2300 grains. Another torsion-rod was afterwards made, for some special experiments, the weight of which was nearly ten times as great: it consisted of a solid brass rod, and was occasionally used without any balls attached to the ends.

The torsion-rod and the suspension-lines were screened by a mahogany box, constructed exactly similar in form to that used by Cavendish, but supported from the ceiling in a very firm manner, and unconnected with the floor or any other part of the surrounding apparatus. Every precaution was taken to secure the torsion-rod from the influence of any sudden or partial change of temperature; and also to insure the stability and firmness of the support to which it was attached. The author's remarks on this subject are worthy of notice: for he says, "In order to satisfy myself on this point, at the time of the original construction of the apparatus, I made various attempts to create a sensible disturbance in the motion of the torsion-rod, by causing the doors to be frequently and violently slammed—by jumping heavily on the floor of the room—and also *above* the ceiling—and in other different ways, having a similar tendency; but in no instance could I observe the least effect upon the lateral motion of the rod. I have also frequently tried the same experiment, when different visitors were present, since the apparatus has been completed; and have moreover many times not only accidentally, but also designedly, made a regular series of experiments for determining the density of the earth, during the most violent storms that I have ever witnessed, when the wind has been so boisterous, and blowing in such gusts, that the house has been

---

[* See Taylor's Scientific Memoirs, vol. ii. p. 252.—Edit.]

shaken to its centre. But in no instance have I ever seen the least disturbance in the lateral motion of the torsion-rod, nor any difference produced in the results of the experiments. I have thought it proper to make these remarks and thus to place them on record, because some persons at first hazarded an opinion that the place which I had selected might not be quite adapted for experiments of so delicate a nature. But a moment's consideration will convince a person conversant with the subject, that no *dancing* motion of the suspension-line (even if it did exist) would tend to produce an irregular *lateral* or *angular* motion in the torsion-rod; and this is the only anomalous motion we need guard against.

"There is also another remarkable circumstance connected with this subject, which I think it requisite likewise here to place on record. When the torsion-rod has been in a state of repose, I have frequently shaken the torsion-box, by rapidly moving the ends backward and forward from side to side fifty or sixty times, and even more: but I could never discover that this disturbance of the box caused the least motion in the torsion-rod, which still retained its stationary position. This experiment has been witnessed at various times by several scientific persons. Yet, notwithstanding this torpid state of the torsion-rod, if the slightest change of temperature be applied near the *side* of the torsion-box, or if either *side* near the balls be sprinkled with a little spirit of wine, the torsion-rod is immediately put in motion and the resting-point undergoes a rapid change."

Notwithstanding these favourable circumstances the author at first met with certain irregularities and discordances, which he found it difficult to remove; and which appear to have been experienced also by Cavendish and Reich,—caused, as it is presumed, by variations in the temperature of the room in which the experiments were carried on. Cavendish chose an out-house in his garden at Clapham Common; and, having constructed his apparatus *within* the building, he moved the masses by means of ropes passing through a hole in the wall, and observed the torsion-rod, by means of a telescope fixed in an ante-room on the *outside*. The general temperature of the interior was therefore probably uniform during the time that he was occupied in any one set of experiments: but it is scarcely to be expected that a building of this kind, and in such a situation, would preserve the same uniform temperature for twenty-four successive hours; especially at the season which he selected for his operations. Reich pursued a similar plan, but under circumstances apparently more favourable; for he selected a dark cellar, where the temperature was not so likely to be disturbed: and, having closed up the door, he adopted Cavendish's plan of observing the motions of the torsion-rod, on the outside. But, even in a situation like this, we must not expect a constant uniformity of temperature for a long period. Neither of these authors, however, has given any information on this subject; both of them, however, met with anomalies for which they could not satisfactorily account: and, although Cavendish suspected the cause of some of those anomalies, yet he does not appear to have applied any remedy for the evil, in any of his subsequent experiments.

Mr. Baily remarks, that his first experiments were tolerably regular, although the results were generally greater than those obtained either by Cavendish or Reich; but that he soon observed discrepancies which convinced him that some disturbing force was in operation, which he had not yet contemplated, and which he could not discover. One of the most striking evidences of such anomaly was the remarkable circumstance, that the arc of vibration, during one and the same experiment, would seldom decrease in the regular manner which it ought to pursue, if the torsion-rod were guided by an uniform influence; and moreover, that in fact it would frequently *in*crease, contrary to all the known laws of bodies so circumstanced. Notwithstanding these interruptions, he not only considered it proper to continue the experiments, for some time, in the usual manner, in the hope that he might thereby eventually throw some light on the probable cause of the anomalies, and perhaps be enabled to apply a correction for the effect of their influence; but also was induced to institute several new courses of experiments, as circumstances and suggestions occurred, for the express purpose of elucidating the subject. The theories of electricity, magnetism, temperature, and currents of air—the influence of different modes of suspension by single and double wires and by double silk lines—the trial of balls composed of different substances and magnitudes—were successively and frequently appealed to, and various experiments made to discover their probable effect on the results. The mode of conducting the experiments was also varied in different ways, with a view of eliciting information on the point in question. Some of them were carried on like those of Cavendish, and others like those of Reich (for the methods of these two experimentalists were very different from each other), whilst many more were conducted on a plan essentially different from either of them. Heated balls and powerful lamps were occasionally applied near the torsion-box, with a view to raise an artificial temperature, and thus create a powerful influence; and, on the other hand, masses of ice have been employed for a similar purpose. The manner likewise of putting the masses in motion was frequently diversified, under the hope of being enabled thereby to obtain a clue to the object of research. But the author has considered it needless to proceed with a detail of these fruitless operations, which were carried on, without much interruption, for upwards of eighteen months, and amounted in number to nearly 1300 experiments. Many of these were of a mere speculative nature, with a view to discover the cause of the anomalies here alluded to; but a thousand of them, at least, were more especially made for the purpose of determining the density of the earth, and were eventually reduced. But the results, although in many cases very consistent amongst themselves, were upon the whole so discordant and unsatisfactory, that no confidence could be placed on the general result, as a correct value of the true object of inquiry. And, as he had pre-determined not to select merely those experiments which might appear to be the most favourable specimens, or supporting any particular theory, and to keep out of view and reject the rest, he consequently abandoned the *whole*.

During these investigations the author was frequently visited by several scientific persons who took a lively interest in the pursuit in which he was engaged, and who kindly offered him their opinion and advice on several occasions. But he remarks, that he was principally indebted to Professor Forbes of Edinburgh, for the most satisfactory removal of the principal anomalies that he had met with. This gentleman's intimate acquaintance with the theory of heat, and its various operations, effects, and influence, led him to agree with Cavendish in opinion, that one source, at least, of the anomalies might arise from the *radiation of heat* from the masses, when they were brought up to the sides of the torsion-box: and that this might even still operate notwithstanding the interposition of the sides of the box, and the precautions already taken. As a remedy for this influence he suggested the propriety of having the masses *gilt*, and also of procuring a *gilt case*, as a cover to the torsion-box, for the purpose of preventing the effect of radiation, from whatever source it might arise. Acting upon this advice, Mr. Baily not only caused a gilt case to be made in the manner here proposed, but also caused the torsion-box itself to be previously covered, all over, with thick flannel. These and other alterations and improvements having been completed, the author resolved to commence a *new series* of experiments, that were likely to be thus made under more favourable auspices, for the correct determination of the mean density of the earth: and it appears that the results soon convinced him that the proper mode had been taken for the removal of the principal source of discordance. For although, in some cases, slight discrepancies may still appear to exist, as might be expected in any inquiry that involves so delicate a system of operations, yet where the discordances are of greater magnitude they seem to be confined to one class of experiments, and to depend principally on the nature and construction of the material of which the suspension-line or torsion-rod is composed, and do not materially affect the general result of the whole. In fact, Mr. Baily states that he has since met with very few experiments, made in the regular mode of proceeding, that are objectionable, or that need be rejected. Every experiment therefore that has been made, under this new arrangement of the apparatus (whether good, bad, or indifferent), has been recorded and preserved; and they are all given without any reserve whatever; it being left to the reader himself to reject or retain, at his pleasure, such as he may think fit.

After these introductory remarks, the author proceeds to the several modes of carrying on the regular system of operations which he had undertaken. With respect to the torsion-rod, he states that it is never at absolute rest, but is constantly in a state of vibration on its centre; and consequently when the end of it is viewed at a distance with the telescope, it appears to oscillate on each side of a mean point, called the *resting-point*. For, even when it is apparently in a state of complete repose, minute vibrations are always perceptible with the telescope; and the times of performing such infinitesimal arcs correspond, in most cases, very nearly with the mean time of vibration that takes place when the torsion-rod is in full action. Mr. Baily however observes, that this resting-point is by no means permanent or stationary, and seldom remains in the same position for any length of time, even when the torsion-rod is not influenced by the approach of the masses. The extent and direction of its disturbance, as well as its rate of motion when so disturbed, are very variable, and seem to depend on causes which have not been sufficiently accounted for, but which may in some measure arise either from slight changes of temperature, or some latent alteration in the component parts of the suspension-line. These vibratory motions of the resting-point (which must be carefully distinguished from the regular vibratory changes in the position of the torsion-rod itself, caused by the near approach of the masses) do not materially affect the mean results in a series of experiments; more especially if their march be regular. It is only when any sudden and considerable transition takes place, that a sensible and material error is likely to occur: but this seldom happens if due precaution has been taken to screen the torsion-box effectually. Yet the author is still of opinion that discordances sometimes arise which cannot wholly be attributed to change of temperature, but to some other occult influence with which we are at present unacquainted. The regular march of the resting-point of the torsion-rod is one of the most important objects of attention; since any considerable deviation therefrom is the source of great discordance, and therefore requires to be watched with care.

The torsion force comes next under consideration. Mr. Baily justly remarks that the torsion force of a wire is that elastic power in the body, by means of which it is enabled to return to its original position, after being drawn aside by any external impulse. It varies with the substance, magnitude, and length of the wire; but it is generally considered to be constant for the same wire, whatever be the weight suspended thereto. This, however, must be taken within certain limits, since the time of vibration (which is one of the elements for determining the force of torsion) will frequently differ very considerably without any apparent or sensible alteration in the component parts of the apparatus. For the author states that we frequently have in the *same hour* very considerable variations in the time of vibration, which evidently show that the force of torsion has undergone some sensible change. But this alteration in the torsion force does not appear to affect the results of the experiments, since we find that, when the time increases, the deviation is also increased in due proportion. The magnitude, therefore, of the force of torsion is not a necessary object of inquiry in these investigations.

The only two objects requiring close attention, for the purpose of obtaining results from any of the experiments, are the determination of the *mean resting-point* of the torsion-rod, and the *time* of its vibration. Now, it fortunately happens that these two objects can, in all cases, be observed with the greatest ease and accuracy, however anomalous they may be; and they are never accompanied with any doubt or difficulty. There is however another subject that is required also to be accurately ascertained in every experiment; namely, the exact distance of the centre of the masses from the centre of the balls. This has been effected by means of plumb-lines, which abut against the masses, and the distances between which are measured, at every experiment, by means of a microscopical apparatus, carefully adjusted.

From the results of the several experiments that the author has made, it would appear that *single wires*, of different diameters, give slight differences in the results. But, he states that the most discordant results occur where the *double* suspension-lines are formed of silk; and he apprehends that these anomalies have arisen from the circumstance that *all* the fibres, of which the skein is composed, are not *equally* stretched by the different balls as they are successively attached to the torsion-rod; and that they are thus severally operated on by different forces, which consequently produces a discordancy in the results. These discordances, however, appear to be generally confined within certain limits.

The author then gives a detailed account of the various experiments that he has made, under the improved form of apparatus, which amount in the whole to 2153; and which were pursued and conducted in different ways, for the purpose of throwing some light on the slight discrepancies that, in spite of his care and caution, would occasionally intrude themselves. It would be impossible in an abstract like this to give a minute detail of the several modes that were adopted in carrying on these operations, and which must therefore be left unexplained till the work itself is published. But the following short synoptical view will enable the reader to form an estimate of the general results obtained from the different balls, according to the manner in which they have been successively suspended. The seven different balls employed are arranged, in the first column, in the order of their weight; and the number of experiments made therewith, together with the mean resulting density therefrom, is classed in the three collateral columns, according as the suspension was formed of double silk lines, double metal wire, or single copper wire. The three detached series, at the bottom of the table, containing 149 experiments, will be presently explained.

Balls.	Double silk.		Double wire.		Single wire.	
	No.	Density.	No.	Density.	No.	Density.
2½-inch lead	148	5·60	130	5·62	57	5·58
2-inch lead.........	218	5·65	145	5·66	162	5·59
1½-inch platina ...	89	5·66	...	...	86	5·56
2½-inch brass	46	5·72	...	...	92	5·60
2-inch zinc	162	5·73	20	5·68	40	5·61
glass	158	5·78	170	5·71	...	...
ivory	99	5·82	162	5·70	20	5·79
2½-inch lead, with brass rod......			44	5·62		
2-inch lead, with brass rod			49	5·68		
Brass rod, alone			56	5.97		

It cannot be supposed, amongst such a number of experiments, prosecuted in such a variety of ways and with such different materials, that the several mean results, obtained from the individual classifications, can be of equal weight. In fact, the author himself has, in his investigations of the subject, clearly shown that some of them are entitled to more confidence than others; and moreover that, in a few instances, there may be a fair cause for rejection. On these points however there is no room for explanation in this place: and it may be sufficient here to state, that, assuming every experiment to be of equal weight, the mean result of the whole 2004 experiments is 5·67. Nor is there much probability that the result of this immense number of experiments will be materially altered, even if those few experiments, which may appear to be affected with some source of error or discordance, should be wholly omitted.

The author remarks that it cannot escape observation that the general mean result, obtained from these experiments, is much greater (equal to $\frac{1}{23}$th part) than that deduced either by Cavendish or Reich, who both agreed in the very same quantity, namely, 5·44: but he does not assign any probable cause for this discordance. It is evident, however, from the detail which he has given of his own experiments, that perceptible differences not only arose according to the mode in which the torsion-rod was suspended, but also depended on the materials of which the suspension-lines were formed: but it is somewhat singular that none of the mean results, in any of these classifications, are so low as that obtained by the two experimentalists above mentioned.

In these remarks, no notice has yet been taken of the remaining 149 experiments that have been made with the brass torsion-rod; a class of experiments that were undertaken for the express purpose of ascertaining the effect of such a measure on the general result. This torsion-rod was nearly of the same weight as the two 2-inch lead balls, and about half the weight of the two $2\frac{1}{2}$-inch lead balls. The experiments were made not only with each of these balls successively attached to the rod, but also with the rod alone, without anything attached thereto. The results show that the attraction of the masses on the rod should be diminished about $\frac{1}{20}$th part, in order to render these three several results consistent with each other, and also accordant with the same balls and the same mode of suspension, attached to the lighter wooden torsion-rods.

XX. *Note on* Mr. Earnshaw's *Paper in Phil. Mag. for April* 1842. *By* Professor POWELL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I DID not happen to see your Number for April till a few days ago, or I should long before this have addressed to you the very brief remarks which I now feel called upon to offer in consequence of certain observations in a paper inserted in the Number referred to, (S. 3. vol. xx. p. 304) " On the Theory of the Dispersion of Light," by Mr. Earnshaw.

I am truly glad to see that a mathematician of such eminence has felt interested in the subject, and has given his attention to what I have published upon it: there is nothing I more desire than fair discussion: no one can have read my treatise on the Dispersion, I trust, without perceiving that I am no prejudiced undulationist, and that so far from asserting that that theory has explained the dispersion, I on the contrary expressly point out the *extent* to which it does apply, and the precise degree and nature of its *failure*. So far then Mr. Earnshaw and myself are quite agreed.

But in the mode in which he sets about the more particular proof of this assertion, there are I confess several particulars which strike me as being, to say the least, extraordinary oversights on the part of so able a mathematician, who seems to have read my treatise, though I can only imagine, too cursorily to perceive wherein it differs from certain *earlier* researches, on a reference to which his whole objections seem founded.

More precisely: Mr. Earnshaw points out certain imperfections in a formula which he assumes as that I have adopted for the dispersion; he contends that this formula is theoretically defective, and also that it is discordant with the results of observation; and enormously so in the case of the more highly dispersive media.

Now all this is *precisely what I have stated* in my work on Dispersion, where (in section vi.) he and your readers will find the nature of the formula fully discussed; the formula on which he has commented being avowedly but *an approximate* one, which applies *nearly* for low dispersive substances, and which I so applied in my *earliest* researches, *but which I long since discarded for a more accurate one.* This simple circumstance then renders all his elaborate criticisms superfluous. My published volume contains my latest view of the whole subject, and *supersedes all my previous researches*; while it investigates the entire series of experimental results by one uniform and exact method derived from a formula *similar* indeed to that referred to by Mr. Earnshaw, but in which the *very imperfections pointed out by him are expressly corrected.*

As to the discrepancies between observation and theory in the higher cases of dispersion, I do not consider them as nearly so serious as Mr. Earnshaw appears to do; and this mainly from the experience I have had in ascertaining the experimental numbers, and the degree of accuracy to which they can be relied on,—for which I would refer to my Report presented to the British Association on refractive indices.

Thus much however is clear: the formula even in the extreme cases agrees as well as I think can be expected with observation, *provided one of the constants receive a certain empirical change in its value, constant for each medium.*

It will therefore be the next step for theory to investigate whether such a change can be justified; but all this I have stated at large in my work, at the conclusion.

Mr. Earnshaw enters also upon the question of the *logic* of the case, and the *sufficiency* of what is merely an *interpolation*; *three* indices being assumed. This point again I had, I thought, fully discussed (p. 84 *et seq.*); at all events, the formula, in whatever manner calculation be applied to it, is surely a direct deduction *from theory.* In particular, the very simple form in which I have used it, is that deduced by Sir W. R. Hamilton by a highly elegant analysis *directly* from the principles of M. Cauchy, and to that pre-eminently gifted mathematician it appeared a sufficient basis for calculation, as was evinced by his own use of it, to which I have referred, Art. 261.

Upon the whole, I will merely add an expression of my satisfaction that the subject has been taken up by Mr. Earnshaw, and my hope that in his hands some formula will eventually be elicited which may be found applicable to the results of observation to such an extent as to clear up the discrepancies which hang over the existing investigations; in which I am well satisfied to have made a first approximation, if it lead to more accurate results from the researches which I may thus have excited more able analysts to undertake.

I am, Gentlemen,

Your most obedient Servant,

Oxford, July 8, 1842. BADEN POWELL.

XXI. *Reply to some Objections against the Theory of Molecular Action according to* Newton's *Law. By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., F.C.P.S., &c., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge**.

WHEN I wrote my reply to an anonymous correspondent in the Phil. Mag. (S. 3. vol. xx. January 1842, p. 8), I did not contemplate extending my remarks beyond the limits of the objections before me. But finding, as well from the private communications of my friends, as from what has appeared in your Journal, that silence is construed into an admission of the indefensibility of the Newtonian law as applied to molecular actions, I am induced most reluctantly to enter on the defence of the hypothesis. The following remarks are the substance of a paper which I read before the Philosophical Society of Cambridge in 1840, but which, from my extreme dislike to controversy, I never printed. Nor should I have now done so, but for the expressed opinion of two of the first mathematicians in Europe, whom I am proud to number amongst my friends, both of whom have united in urging me either to remove the difficulties which attend the theory, or to point out in what way they may be regarded as not subversive of its truth. It shall be my endeavour in what follows to argue with perfect candour, not *against* the objections so much as *for* the theory. I hope nothing I shall say will induce any one to imagine that I undervalue the importance, or the ingenuity of the objections themselves, or that I lightly esteem the memoirs in which they are embodied. Let it be understood that I do not attempt to overthrow the arguments of my opponents to any extent further than as they, if admitted, would subvert a theory in which I am deeply interested, and which, indeed, I partly originated†.

Before I enter on my subject I wish to state expressly what is the hypothesis itself which I am about to defend. It is this: *That bodies consist of molecules, simple or aggregated in groups, surrounded by particles of a fluid which pervades all space. Both the former and the latter molecules are endued with attractive or repulsive forces towards each other, and each system likewise attracts or repels the particles of the other. The law of force in all cases is that of the inverse square of the distance.*

---

* Communicated by the Author.

† M. Mossotti's paper was printed at Turin in 1836; mine was read in February of the same year. [M. Mossotti's paper was scarcely known in this country, until its contents, especially as bearing upon the theory of electricity, were announced by Mr. Faraday at the Royal Institution, on the 20th of January 1837 (see Phil. Mag. S. 3. vol. x. p. 84, 317): a translation of the entire paper appeared in Taylor's Scientific Memoirs, (vol. i. p. 448) on the 1st of February.—Edit.]

In what way the alternative of attraction or repulsion is defined, I do not profess accurately to specify. I prefer, for the present, to consider matters of detail as open for future investigation. That I may be *allowed* to do so it is necessary that I should premise the grounds on which I consider them as not yet satisfactorily established. Whether the molecules of *matter* attract or repel each other is perfectly indifferent; I believe either hypothesis will do very well. Neither does it signify whether the particles of matter attract or repel those of the other fluid (called æther), provided it be allowed that the latter can come in contact with and rest against the former. But whether the particles of æther attract or repel each other is a question of more importance, and one which, when decided, will probably settle the other two. The *primâ facie* probability is that they act by repulsion. It is argued in favour of this supposition, that were it not so, the slightest displacement which should bring two particles near each other would of necessity cause them to run together. That this argument is fallacious will appear presently, when we shall show that they would not *instantaneously* tend either to unite or to separate. Another argument is that when they had once come in contact they could never again be separated. This argument applies with equal force against *any* hypothesis of attractive particles. At the same time I do not think the arguments *in favour* of the hypothesis of attraction to be by any means conclusive.

The popular grounds on which I rested this hypothesis (Trans. Camb. Phil. Soc. vol. vi. p. 178) can, of course, only be held as an illustration. That they are quite insufficient to build anything upon, is obvious enough; but it is most completely shown by Mr. Earnshaw in his memoir on the Nature of Molecular Forces, to which I am about to direct attention presently. (See Art. 8.) Nor is the argument deduced from an approximate estimation of the value of the function which expresses the time of vibration of a particle, at all conclusive. It will be found in my memoir (Trans. Camb. Phil. Soc. vol. vi. p. 183 and 241). It rests on the assumptions, first, that the principal effect is due to the particles in the immediate neighbourhood of that whose motion we are investigating; secondly, that the effect of the action of any particle is independent of its position relative to the direction of transmission. The former assumption is doubtless admissible to a certain extent; the latter, I believe, not at all. The attractive nature of the particles is still further supported by an argument which I do not now regard as satisfactory. It is this:—We have good reason to suppose that the vibrations of the air are normal, in the production of sound; we are certain that the particles of air act repulsively on each other: our analysis shows, that if repulsive forces produce normal vibrations, attractive forces must act to produce the transverse ones which constitute light. There are, however, two things connected with the mutual action of the particles of air, which are here left out of the account; the one arises from the repulsion of their surrounding æther, the other from its pressure against them. I do not think, therefore, that anything has been offered in favour of the hypothesis of attractive forces, so strong as to induce us to reject the contrary. I would be understood rather as waiting for more evidence previous to pledging myself to the adoption of either. The arguments, then, to which I am about to reply are arguments against the *law of force*.

Those which I have met with are the following:—

1. That a particle placed in a medium constituted of discrete molecules which exert actions varying according to the law of the inverse square of the distance *will not vibrate*.
2. That the equilibrium of such a medium will not be stable.
3. That the principal action on a vibrating particle will be due to the remoter parts of the system; and,
4. That the velocity of transmission will *not* depend on the length of the wave.

1. The first argument is brought forward by Mr. Earnshaw in a memoir "On the Nature of Molecular Forces," printed in the Transactions of the Cambridge Philosophical Society, vol. vii. p. 97. The memoir is one of great interest, and the analytical equations are very valuable, but I cannot admit the correctness of the interpretation which the author has assigned to them, in deducing "that the molecular forces which regulate the vibrations of the æther do not vary according to Newton's law of universal gravitation."

The following is an outline of the argument. V is taken for the sum of each particle divided by its distance from the one which is under discussion. The coordinates of any particle m are x, y, z, whilst those of the particle attracted are f, g, h: then, as Laplace and others have shown, the forces are $\frac{dV}{df}$, &c., and the relation existing is $\frac{d^2V}{df^2}+\frac{d^2V}{dg^2}+\frac{d^2V}{dh^2}$ $= 0$.

Now if $V = C$, $V' = C'$ be two values of V for different positions of the same particle, it is shown that $2(C-C')$ $=\frac{d^2V}{df^2}\delta f^2+\frac{d^2V}{dg^2}\delta g^2+\frac{d^2V}{dh^2}\delta h^2$ is the equation to a surface, in any point of which, if the particle be placed, it will commence to move in the direction of a normal. But on account of the existing relation, the three quantities $\frac{d^2 V}{df^2}$, $\frac{d^2 V}{dg^2}$, $\frac{d^2 V}{dh^2}$, cannot all have the same sign. The surface is consequently an hyperboloid, and thus "there are in general only three directions in which a particle can be displaced, so that the force called into play may act in the direction of the displacement." It appears then that "the constitution of a medium, composed of detached attractive particles, can never be such that the force of restitution called into play by a disturbance in *any* direction shall act in the line of displacement. Hence those media which are distinguished as *uncrystallized* cannot consist of detached particles which either attract or repel each other, with forces varying inversely as the square of the distance; because it is assumed as a characteristic of such media, that the forces of restitution act always in the direction of displacement." (Art. 10.)

To this argument there are two objections:—

a. That the *excepted* case embodies the real state of things;

b. That even were it otherwise, nothing is established against the molecular theory.

a. It is evident that the case in which $\frac{d^2 V}{df^2} = 0$, $\frac{d^2 V}{dg^2} = 0$, $\frac{d^2 V}{dh^2} = 0$ is excepted; indeed the author expressly points out this circumstance in Art. 8. We proceed to show that this is the very case to be considered, in a medium of symmetry. But this phrase will perhaps itself raise an objection to our arguments. We hope to be excused then if we make a short digression hereupon. A medium of perfect symmetry, it has been argued, "has never been shown to exist in nature, nor is it proved even that it *can* exist." We reply that, most assuredly, a medium of perfect symmetry amongst detached particles *cannot* exist in nature. It is quite inconceivable. Those who have adopted it, have done so "for the sake of simplifying their equations." (Earnshaw, Phil. Mag., S. 3. vol. xx. May 1842, p. 370). Nor have they regarded themselves as proceeding without reasons as valid and as well founded as those on which any one process in mathematical physics is based. If it be true from experiment that it is perfectly indifferent in what direction light passes through certain media, then is it *of necessity* equally true that the sensible forces are altogether uninfluenced by direction. And moreover if it is quite the same thing whether motion takes place from right to left or from left to right, it is inconceivable that forces which depend on the excess of the action due to the right-hand direction above that due to the left can produce any sensible effect. Let me repeat that it is not *geometrical* symmetry which we assumed; a cubical arrangement which we sometimes speak of by way of illustration is *not* an arrangement of geometric symmetry. But what we do assume is a medium of *mechanical* symmetry; an arrangement of such a nature that all forces are independent of direction either throughout or on either side of a particle. Perhaps the word *isotrope*, which M. Cauchy uses, or isodynamical, might express the condition better than the word symmetrical, but further than the employment of a term which is incorrect, and of illustrations which are unsatisfactory, nothing can be urged against the introduction of the hypothesis of perfect symmetry.

To return to our argument. The value of $\frac{d^2 V}{d f^2}$ is

$$\Sigma\, m\, \frac{2\,(x-f)^2-(y-g)^2-(z-h)^2}{r^5}.$$

Now in a medium of symmetry

$$\Sigma\, m \frac{(x-f)^2}{r^5} = \Sigma\, m \frac{(y-g)^2}{r^5} = \Sigma\, m \frac{(z-h)^2}{r^5}.$$

Hence $\frac{d^2 V}{d f^2} = 0$. Similarly $\frac{d^2 V}{d g^2} = 0$, $\frac{d^2 V}{d h^2} = 0$.

Nor is it otherwise with an isotrope or isodynamical medium, whatever be its constitution. In such a medium the value of the square of the velocity of transmission of a vibration depends on that of the function

$$\Sigma\, m \left(\frac{1}{r^3} - \frac{3(x-f)^2}{r^5}\right) \sin^2 \frac{\pi\,(y-g)}{\lambda},$$

$$\text{or of} \quad \Sigma\, m \left(\frac{1}{r^3} - \frac{3\,(z-h)^2}{r^5}\right) \sin^2 \frac{\pi\,(y-g)}{\lambda};$$

for the velocity is independent of the direction of vibration.

The equality of these two expressions gives us

$$\Sigma\, m\, \frac{(x-f)^2}{r^5} \sin^2 \frac{\pi\,(y-g)}{\lambda} = \Sigma\, m \frac{(z-h)^2}{r^5} \sin^2 \frac{\pi\,(y-g)}{\lambda}.$$

Now this equality is true whatever be the position of the vibrating particle; that is, it is perfectly independent of $y-g$. Consequently the portions which depend on each particular value of $y-g$ must be separately equal to one another. This gives us $\Sigma\, m\, \frac{(x-f)^2}{r^5} = \Sigma\, m\, \frac{(z-h)^2}{r^5}$. In exactly the same way does it appear that

$$\Sigma\, m \frac{(x-f)^2}{r^5} = \Sigma\, m\, \frac{(y-g)^2}{r^5}. \quad \text{Hence } \frac{d^2 V}{d f^2} = 0, \text{ \&c.}$$

We have taken it for granted that by "a position of equilibrium" is meant the place originally occupied by a particle in its state of rest. The arguments adduced by Mr. Earnshaw evidently require that this should be the case.

Having shown that $\frac{d^2 V}{d f^2}$, $\frac{d^2 V}{d g^2}$, and $\frac{d^2 V}{d h^2}$ are all zero, it follows that any argument based on the express assumption of the contrary is invalid.

But now it may be urged that we have only removed the objection from one point to another. For, admitting it to be established that $\frac{d^2 V}{d f^2}$, $\frac{d^2 V}{d g^2}$ and $\frac{d^2 V}{d h^2}$ are zero, in the case in question, the argument against the possibility of vibration remains in full force. For "the displacements of particles placed in such positions as those here considered would not bring into action any forces of restitution, on which account the particles would not vibrate." (Earnshaw, art. 8.) This is the argument. I fear I do not rightly see the connexion between it, and the inference which follows: "it is evident therefore that the phænomena of light and sound are not due to the motions of particles placed in such positions." If I am wrong in conjecturing the inference, I hope to be set right; but so far as I am able to make out, it is as follows: a particle is moved, its motion calls no force into play to draw it back, therefore it will remain in its new position, and will not vibrate. Now we reply, that before it can be inferred that the particle will not vibrate, it is necessary to show, not only that it receives no instantaneous action owing to its change of position, but that it likewise exerts none on the surrounding particles. But the latter requirement is assuredly not fulfilled. The particles in advance of that which has been moved are *more* acted on than they were before. Motion will therefore inevitably ensue. This argument then falls to the ground. We have thus shown that the objections are based on a state of things different from that which the hypothesis requires; and that nothing which has been said on the contrary supposition is available against the theory.

b. But were it otherwise, were we to admit the correctness of all the reasonings referred to—should we thereby be subject to the inference which has been drawn, "that a force, whether attractive or repulsive, varying according to Newton's law, *cannot possibly actuate* the particles of a *vibrating* medium?" (Earnshaw, Int.) By no means. The inference rests on the *assumption* that a particle of the æther, when disturbed, must be acted upon by forces in the line of displacement. Now this assumption is never made by writers on the molecular hypothesis, nor do I know that it is requisite; at least, before we can admit any argument based on it, we require to be shown that it is actually or virtually made in the application of the hypothesis against which the objection is raised. We are not aware that any one has attempted to show how vibrations are *generated*: the question is how they are *propagated*. Now in order to the propagation of a vibration it is assuredly requisite that the force put in play by a relative series of displacements, should, on each particle, act in the line of the displacement. But this force is not a *statical* force; it is due to the actions of the *displaced* particles, and dependent *altogether* on their displacement; in a medium of symmetry, and on the Newtonian law. (See my Memoir, Trans. Camb. Phil. Soc. vii. p. 244.) The whole *line of argument*, therefore, is inadmissible. No objection based on the want of fulfilment of the conditions of vibration can be valued, unless it distinctly recognises all those conditions.

P.S. Since writing the above, Professor Braschmann of Moscow has favoured me with a sight of his "Theory of Equilibrium," which contains M. Mossotti's views. It is written in Russ, but as the author promises me a copy of the work with manuscript translations of some of the more important passages, I hope in a future communication to profit by it.

XXII. *On some peculiar Changes in the Internal Structure of Iron, independent of, and subsequent to, the several Processes of its Manufacture.* By CHARLES HOOD, *Esq., F.R.A.S., &c*[*].

THE important purposes to which iron is applied have always rendered it a subject of peculiar interest; and at no period has its importance been so general and extensive as at the present time, when its application is almost daily extending, and there is scarcely anything connected with the arts, to which, either directly or indirectly, it does not in some degree contribute. My object in the present paper is to point out some peculiarities in the habitudes of iron, which appear almost wholly to have escaped the attention of scientific men; and which, although in some degree known to practical mechanics, have been generally considered by them as isolated facts, and not regarded as the results of a general and important law. The circumstances, however, well deserve the serious attention of scientific men, on account of the very important consequences to which they lead.

---

[*] Communicated by the Author: having been read before the Institution of Civil Engineers, June 21, 1842.

The two great distinctions which exist in malleable wrought iron, are known by the names of "red short" and "cold short" qualities. The former of these comprises the tough fibrous iron, which generally possesses considerable strength when cold; the latter shows a bright crystallized fracture, and is very brittle when cold, but works ductile while hot. These distinctions are perfectly well known to all those who are conversant with the qualities of iron: but it is not generally known that there are several ways by which the tough red shot iron becomes rapidly converted into the crystallized, and by this change its strength is diminished to a very great extent.

The importance which attaches to this subject at the present time will not, I think, be denied. The recent accident on the Paris and Versailles Railway, by which such a lamentable sacrifice of human life has occurred, arose from the breaking of the axle of a locomotive engine, and which axle presented at the fractured parts the appearance of the large crystals which always indicate cold short and brittle iron. I believe there is no doubt, however, that this axle, although presenting such decided evidence of being at the time of this accident of the brittle cold short quality, was at no distant period tough and fibrous in the highest degree; and as the French Government have deemed the matter of sufficient importance to be inquired into by a special commission, I trust that some remarks on the subject will be interesting to the members of the Institution of Civil Engineers. I propose, therefore, to show how these extraordinary and most important changes occur, and shall point out some at least of the modes by which we can demonstrate the truth of this assertion by actual experiment.

The principal causes which produce this change, are percussion, heat, and magnetism: and it is doubtful whether either of these means *per se* will produce this effect; and there appear strong reasons for supposing that generally they are all in some degree concerned in the production of the observed results.

The most common exemplification of the effect of heat in crystallizing fibrous iron, is by breaking a wrought-iron furnace bar, which, whatever quality it was of in the first instance, will in a short time invariably be converted into crystallized iron: and by heating and rapidly cooling, by quenching with water a few times, any piece of wrought iron, the same effect may be far more speedily produced.

In these cases we have at least two of the above causes in operation,—heat and magnetism. In every instance of heating iron to a very high temperature, it undergoes a change in its electric or magnetic condition; for at very high temperatures iron entirely loses its magnetic powers, which return as it gradually cools to a lower temperature. In the case of quenching the heated iron with water, we have a still more decisive assistance from the electric and magnetic forces; for Sir Humphry Davy long since pointed out* that all cases of vaporization produced negative electricity in the bodies in contact with the vapour; a fact which has lately excited a good deal of attention, in consequence of the discovery of large quantities of negative electricity in effluent steam.

These results, however, are practically of but little consequence; but the effects of percussion are at once various, extensive, and of high importance. We shall trace these effects under several different circumstances.

In the manufacture of some descriptions of hammered iron, the bar is first rolled into shape, and then one half the length of the bar is heated in a furnace and immediately taken to the tilt-hammer and hammered; and the other end of the bar is then heated and hammered in the same manner. In order to avoid any unevenness in the bar, or any difference in its colour, where the two distinct operations have terminated, the workman frequently gives the bar a few blows with the hammer on that part which he first operated upon. That part of the bar has, however, by this time become comparatively cold; and if this cooling process has proceeded too far when it receives this additional hammering, that part of the bar *immediately* becomes crystallized, and so extremely brittle that it will break to pieces by merely throwing it on the ground, though all the rest of the bar will exhibit the best and toughest quality imaginable. This change, therefore, has been produced by percussion (as the primary agent), when the bar is at a lower temperature than a welding heat.

We here see the effects of percussion in a very instructive form. And it must be observed that it is not the excess of hammering which produces the effect, but the absence of a sufficient degree of heat at the time the hammering takes place; and the evil may probably be all produced by four or five blows of the hammer, if the bar happens to be of a small size. In this case we witness the combined effects of percussion, heat, and magnetism. When the bar is hammered at the proper temperature no such crystallization takes place, because the bar is insensible to magnetism. But as soon as the bar becomes of that lower degree of temperature at which it can be affected by magnetism, the effect of the blows it re-

---

* Davy's Chemical Philosophy, p. 138.

ceives is to produce magnetic induction, and that magnetic induction and consequent polarity of its particles, when assisted by further vibrations from additional percussion, produces a crystallized texture. For it is perfectly well known that in soft iron magnetism can be almost instantaneously produced by percussion; and it is probable that the higher the temperature of the bar at the time it receives the magnetism, the more likely will it be to allow of that re-arrangement of its molecules which would constitute the crystallization of the iron.

It is not difficult to produce the same effects by repeated blows from a hand-hammer on small bars of iron; but it appears to depend upon something peculiar in the blow, which to produce the effect must occasion a complete vibration among the particles in the neighbourhood of the part which is struck. And it is remarkable that the effects of the blows in all cases seem to be confined within certain limited distances of the spot which receives the strokes. Mr. Charles Manby has mentioned to me a circumstance which fully bears out this statement. In the machine used for blowing air at the Beaufort Iron Works, the piston-rod of the blowing cylinder, for a considerable time, had a very disagreeable jar in its motion, the cause of which could not be discovered. At last the piston-rod broke off quite short, and close to the piston; and it was then discovered that the key had not properly fastened the piston and the rod together. The rod at the fracture presented a very crystallized texture; and as it was known to have been made from the very best iron, it excited considerable surprise. The rod was then cut at a short distance from the fracture, and it was found to be tough and fibrous in a very high degree; showing what I have already pointed out, that the effects of percussion generally extend only a very short distance. In fact, we might naturally expect, that as the effect of vibration diminishes in proportion to the distance from the stroke which produces it, so the crystallization, if produced by this means, would also diminish in the same proportion. The effect of magnetism alone may also be estimated from this circumstance. The rod would of course be magnetic throughout its whole length; this being a necessary consequence of its position, independent of other circumstances; but the necessary force of vibration among its particles only extended for a short distance, and to that extent only did the crystallization proceed. The effect of magnetism in assisting the crystallization, I think it unnecessary to dwell upon, as the extensive use of galvanic currents in modern times has fully proved their power in crystallizing some of the most refractory substances; but by themselves they are unable to produce these effects on iron, or at least the operation must be extremely slow.

Another circumstance which occurred under Mr. Manby's observation, confirms generally the preceding opinions. A small bar of good tough iron was suspended and struck continuously with small hand-hammers, to keep up a constant vibration. The bar, after the experiment had been continued for some considerable time, became so extremely brittle, that it entirely fell to pieces under the light blows of the hand-hammers, presenting throughout its structure a highly crystallized appearance.

The fracture of the axles of road vehicles of all kinds is another instance of the same kind. I have at different times examined many broken axles of common road vehicles, and I never met with one which did not present a crystallized fracture, while it is almost certain that this could not have been the original character of the iron, as they have frequently been used for years with much heavier loads, and at last have broken without any apparent cause, with lighter burdens and less strain than they have formerly borne. The effects, however, on the axles of road vehicles are generally extremely slow, arising, I apprehend, from the fact that, although they receive a great amount of vibration, they possess a very small amount of magnetism, and are not subject to a high temperature. The degree of magnetism they receive must be extremely small, from their position and their constant change with regard to the magnetic meridian, the absence of rotation, and their insulation by the wood spokes of the wheels. Whether the effects are equally slow with iron wheels used on common roads, may perhaps admit of some question.

With railway axles, however, the case is very different. In every instance of a fractured railway axle, the iron has presented the same crystallized appearance; but this effect, I think, we shall find is likely to be produced far more rapidly than we might at first expect, as these axles are subject to other influences, which, if the theory here stated be correct, must greatly diminish the time required to produce the change in some other cases. Unlike other axles, those used on railways rotate with the wheels, and consequently must become during their rotation highly magnetic. Messrs. Barlow and Christie were the first to demonstrate the magnetism by rotation produced in iron, which was afterwards extended by Messrs. Herschel and Babbage to other metals generally, in verifying some experiments by M. Arago. It cannot, I think, be doubted, that all railway axles become from this cause highly magnetic during the time they are in motion, though they may not retain the magnetism permanently. But in the axles of locomotive engines we have yet another cause which may tend to increase the effect. The vaporization of water and the effluence of steam have already been stated to produce large quantities of negative electricity in the bodies in contact with the vapour; and Dr. Ure has shown* that negative electricity, in all ordinary cases of crystallization, instantly determines the crystalline arrangement. This of course must affect a body of iron in a different degree to that of ordinary cases of crystallization; but still we see that the effects of these various causes all tend in one direction, producing a more rapid change in the internal structure of the iron of the axle of a locomotive engine, than occurs in almost any other case.

Dr. Wollaston first pointed out that the forms in which native iron is disposed to break, are those of the regular octohedron and tetrahedron, or rhomboid, consisting of these forms combined. The tough and fibrous character of wrought iron is entirely produced by art; and we see in these changes that have been described, an effort at returning to the natural and primal form; the crystalline structure, in fact, being the natural state of a large number of the metals; and Sir Humphry Davy has shown that all those which are fusible by ordinary means assume the form of regular crystals by slow cooling.

The general conclusion to which these remarks lead us, appears, I think, to leave no doubt that there is a constant tendency in wrought-iron, under certain circumstances, to return to the crystallized state; but that this crystallization is not necessarily dependent upon time for its development, but is determined solely by other circumstances, of which the principal is undoubtedly vibration. Heat, within certain limits, though greatly assisting the rapidity of the change, is certainly not essential to it; but magnetism, induced either by percussion or otherwise, is an essential accompaniment of the phænomena attending the change.

At a recent sitting of the Academy of Sciences at Paris, M. Bosquillon made some remarks relative to the causes of the breaking of the axle on the Versailles Railroad; and he appears to consider that this crystallization was the joint effect of time and vibration, or rather, that this change only occurs after a certain period of time. From what has here been said, it will be apparent that a fixed duration of time is

---

* Journal of Science, vol. v. p. 106.

not an essential element in the operation; that the change, under certain circumstances, may take place instantaneously; and that an axle may become crystallized in an extremely short period of time, provided that vibrations of sufficient force and magnitude be communicated to it. This circumstance would point out the necessity for preventing as much as possible all jar and percussion on railway axles. No doubt one of the great faults of both engines and carriages of every description—but particularly the latter—is their possessing far too much rigidity; thus increasing the force of every blow produced by the numerous causes incidental to railway transit; by causing the whole weight of the entire body in motion to act by its momentum in consequence of the perfect rigidity of the several parts and the manner of their connection with each other, instead of such a degree of elasticity as would render the different parts nearly independent of one another, in the case of sudden jerks or blows; and which rigidity must produce very great mischief, both to the road and to the machinery moving upon it. The looseness of the axles in their brasses must also be another cause which would greatly increase this evil.

Although I have more particularly alluded to the change in the internal structure of iron with reference to the effects on railway axles, it need scarcely be observed that the same remarks would apply to a vast number of other cases, where iron, from being more or less exposed to similar causes of action, must be similarly acted upon. The case of railway axles appears to be of peculiar and pressing importance, well deserving the most serious consideration of scientific men, and particularly deserving the attention of those connected with railways, or otherwise engaged in the manufacture of railway machinery, who have the means of testing the accuracy of the theory here proposed. For if the views I have stated be found to harmonize with the deductions of science, and to coincide with the results of experience, they may have a very important effect upon public safety. It may be observed, on the other hand, however, that at the present time all railway axles are made infinitely stronger than would be necessary for resisting any force they would have to sustain in producing fracture, provided the iron were of the best quality; and to this circumstance may perhaps be attributed the comparative freedom from serious accidents by broken axles. The necessity for resisting flexure and the effects of torsion, are reasons why railway axles never can be made of such dimensions only as would resist simple fracture; but it would be very desirable to possess some accurate experiments on the strength of wrought iron in different stages of its crystallization, as there can be no doubt that very great differences exist in this respect, and it is probable that in most cases, when the crystallization has once commenced, the continuance of the same causes which first produced it goes on continually increasing it, and thereby further reduces the cohesive strength of the iron.

Earl Street, May 31, 1842.

[Several samples of broken railway axles accompanied this paper, and were exhibited at the Meeting. In some of them the same axle was broken in different places, and showed that where the greatest amount of percussion had been received, the crystallization of the iron was far more extensive than in those parts where the percussion had been less.]

XXIII. *Notice of a remarkable Magnetic Disturbance which occurred on the 2nd and 4th of July*, 1842. *By the Rev.* HUMPHREY LLOYD, *D.D., F.R.S., V.P.R.I.A., Professor of Natural Philosophy in the University of Dublin.*

To Richard Taylor, Esq.

DEAR SIR,

A VERY remarkable magnetic disturbance (the *most remarkable* I ever witnessed) occurred in the beginning of the present month. A brief sketch of some of the principal features of the phænomenon, as they were observed at the Dublin Magnetical Observatory, may probably interest some of your readers.

On the 2nd of July, at 6 a.m. (Göttingen mean time), the attention of one of the assistant observers (Mr. O'Neill) was arrested by the extraordinary deviation of all the magnets from their mean positions, accompanied by a large vibration; and he immediately commenced a series of observations at short intervals. The *disturbance* of the declination (by which I mean the deviation of the freely suspended horizontal magnet *from the mean place corresponding to that hour*) then amounted to 149·2 divisions of the scale of the instrument, or 1° 47′·3 of arc, —the north end of the magnet deviating *towards the west*, or the declination *increased*. The magnet of the bifilar magnetometer was driven beyond the limits of the scale of its collimator; and the diminution of the horizontal intensity *exceeded* the $\frac{1}{30}$th of the whole force. Both magnets were returning rapidly towards their mean positions at the moment of the first observation; so that the epoch of the greatest change was *before* 6 a.m., and its amount *exceeded* that observed. The observations taken at the regular hours immediately preceding (2 and 4 a.m. Göttingen mean time) gave no warning of the approaching change.

From 6 a.m., for nearly an hour, both magnets returned rapidly, and almost uninterruptedly, towards their mean positions, the declination diminishing, and the horizontal intensity increasing. The latter element reached its maximum at 6^h 56^m; the declination continued to decrease until 7^h 12^m. After this, no very marked change occurred for some time, and the extra observations were discontinued at 8^h 36^m.

At 10 a.m. the declinometer indicated an increase of declination amounting to 18·6 minutes; and the extra observations were resumed, and continued for an hour. By this time (11 a.m.) both instruments had attained nearly their mean positions, from which the observations taken at the regular magnetic hours next following (noon, 2 p.m. 4 p.m.) showed no variation.

The extra observations were resumed at 5^h 36^m p.m., the bifilar magnetometer then indicating an *increase* of the horizontal intensity, amounting to ·0062 of the whole. The observations were continued for more than an hour, but without the occurrence of any very marked change.

The regular observation at 10 p.m. showed a considerable decrease of declination, accompanied by a decrease of horizontal intensity; and at 11 p.m. the extra observations were resumed, and continued, with both instruments simultaneously, until Sunday morning. In this interval another very remarkable change took place. The declination, after some irregular oscillations, began to increase rapidly, and reached its maximum at 11^h 48^m, the deviation from its mean value being then 28·1 minutes. It then returned with a very rapid movement, and in eight minutes the magnet traversed 83 divisions of the scale, or 1° of arc; after which it made some smaller oscillations of the same rapid kind. The change of the horizontal intensity which occurred at the same time was still more remarkable. This element increased from 11^h 8^m to 11^h 20^m; it then rapidly diminished for 12 minutes more; in another 6 minutes it reached a second maximum (at 11^h 38^m); and finally the magnet was driven impetuously beyond the limits of the scale in the opposite direction, the intensity reaching its minimum at 11^h 50^m, and the disturbance exceeding the $\frac{1}{30}$th of the whole intensity. The returning oscillation occupied 12 minutes more; and at 12^h 2^m the magnet returned to its extreme position on the opposite side, the fluctuation in this time exceeding 111 divisions of the scale. The disturbance during these two hours was characterized by the absence of all vibratory movement, notwithstanding the magnitude of the changes.

There seemed to be a faint auroral light in the N.W. horizon, but without streamers.

When the regular observations were recommenced, on Monday the 4th instant, the disturbing forces were found to be still in activity. At 2 and 4 a.m. the instruments showed a very considerable *decrease* of declination, accompanied by a great decrease of horizontal intensity. At 6 a.m. the declination *exceeded* the mean of the hour by a still greater amount; and the horizontal intensity had also increased, though still below its mean value. All the magnets were then vibrating through very large arcs. The series of observations at short intervals was then begun, and continued (almost without interruption) for ten hours.

At 6^h 24^m the declination reached its maximum, the deviation then amounting to 43·2 minutes. The horizontal intensity also attained its maximum very nearly at the same moment. The two elements then began to diminish rapidly and simultaneously; and between 7 and 8 a.m. there was a double minimum of both, separated by an intervening maximum, that of the horizontal intensity taking place a few minutes earlier than the other element.

At 9 a.m. the disturbance was extremely rapid. The magnets were hurried to and fro with a violent movement; and these changes of mean position were accompanied by a large vibration, amounting in some instances (notwithstanding the copper rings) to 20 divisions of the scale.

This combination of movements rendered it difficult to seize the moment of greatest deviation, or to determine its precise amount. The declination attained a minimum at 9 a.m., which was followed by a marked maximum at 9^h 22^m, the range of the oscillation being 29·4 minutes. There was a corresponding change of the intensity, but somewhat later in time,—the minimum occurring at 9^h 14^m, and the maximum at 9^h 50^m; and the range amounting to ·0147.

The changes of declination which occurred afterwards did not present any remarkable features; but the horizontal intensity, which was previously less than in its mean state, after reaching a minimum at 1^h 44^m, suddenly increased to an amount exceeding its mean value, and reached a maximum at 2^h 5^m p.m. The period of this maximum was characterized by a sudden increase of the arc of vibration, as if by impulse. The intensity continued above its mean value (though with some considerable oscillations) during the remainder of the time of observation. The disturbance ceased about 5 p.m.

The induction inclinometer was observed, in conjunction with the other two instruments; but the observations are unreduced, and I am therefore unprepared as yet to offer any remark respecting the changes of inclination or total intensity. It is manifest, however, even from this imperfect sketch, that this disturbance presents many features of prominent interest:

1. In the great magnitude, and marked and abrupt character of the principal changes. In both these respects the changes at 6 a.m. and 12 p.m. on the 2nd instant, afford perhaps the most interesting points of comparison of any that the system of simultaneous observation has yet furnished; and much light may be expected to be thrown on the phænomena by a comparison of the results which may certainly be expected to arrive from the colonial observatories, as well as of those which have been probably obtained at Port Louis, in the moveable observations of the Antarctic expedition.

2. In the striking confirmation which it affords to the conclusion of Prof. Kreil, viz. that all the greater changes are accompanied by a diminution in the horizontal component of the intensity. The whole of the day following the disturbance (July 5) was also characterized by a diminished intensity, which is also in accordance with the inductions of Prof. Kreil; but the increase of this element *towards the close* of the disturbance (in the afternoon of the 4th) is in opposition to one of his conclusions.

3. In the two classes of changes exhibited; in one of which (as on the evening of the 2nd) the disturbances from the mean position, although great and rapid, were accomplished without any sensible vibration of the magnets; while in the other (as on the morning of the 4th) the vibration exceeded any ever witnessed in this observatory, since the application of the copper rings.

4. In the occurrence of great magnetic changes without any marked auroral phænomena. The sky was clear on the night of the 2nd, during a very remarkable part of the disturbance, and a light was seen in the N.W.,—but of a very uncertain nature, and without any of the distinguishing characters of the aurora. I may observe, however, that throughout the whole of the 3rd, and the greater part of the 4th, the sky was covered during the day with a peculiar milky whiteness, apparently belonging to something distinct from and above the clouds; and that this disappeared suddenly, and the blue sky became visible, about 5 p.m. on the 4th, when the disturbance was at an end. I could not help regarding this appearance as connected with aurora.

Believe me, dear Sir, faithfully yours,

H. Lloyd.

Trinity College, Dublin,
July 19, 1842.

XXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xx. p. 594.]

Nov. 3, 1841.—A MEMOIR entitled "Supplement to a 'Synopsis of the English Series of Stratified Rocks inferior to the Old Red Sandstone,' with Additional Remarks on the Relations of the Carboniferous Series and Old Red Sandstone of the British Isles," by the Rev. Adam Sedgwick, F.G.S., Woodwardian Professor in the University of Cambridge, was begun.

Nov. 17.—Professor Sedgwick's paper, commenced at the preceding meeting, was concluded.

The author states that his former synopsis* is now modified; 1st, by the new classification of the stratified rocks of Devon and Cornwall (*Devonian system*); 2ndly, by a larger knowledge of fossils derived from some of the groups described; 3rdly, by new observations made during the past summer in the south of Ireland, the south-western parts of Scotland, and in the north of England.

NEW RED SANDSTONE.—1. *England.*—It is shown, by sections derived from Warwickshire, that the upper part of the new red sandstone is sometimes unconformable to the lower part, which represents the magnesian limestone and lowest division of the new red sandstone group. It is also shown that the coal-measures pass into the overlying new red sandstone series through the intervention of bands of red marl alternating with two bands of freshwater limestone, the whole beds of passage being loaded with common coal-plants. The author then discusses the sections near Whitehaven. They show no passage from the lower new red sandstone (*rotheliegende*) to the coal-measures; but they show that the flora of the coal-field existed apparently in full perfection during the period of the lower new red sandstone: of this flora he has obtained many new specimens. He states that the additional facts lend support to the suggestion thrown out by Mr. Murchison and himself respecting the age of the coal-field on the flanks of the Hartz.

2. *Scotland.*—The new red sandstone of Dumfries-shire is continuous with that of the plains of Carlisle, and is seen overlying the coal-measures from the valley of the Esk, near Canobie, to the neighbourhood of Dumfries. Near the latter place it is in mineral structure the same with the red sandstone of Corncockle-moor, and, at both places, the red flags contain impressions of footsteps. The author therefore asserts that the red sandstone near Loch Maben (visited by Mr. Murchison and himself in 1827) was rightly placed in the new red group. The lower divisions of the new red sandstone series do not appear to range into this part of Scotland.

To the north of the Galloway chain (the great southern greywacke chain of Scotland), the new red séries almost dies away, and is seen in very few parts of Scotland. The author found no traces of it between Girvan and the mouth of the Clyde. Coupling this fact

---

* Proceedings, vol. ii. p. 675. [or Phil. Mag. S. 3. vol. xiii. p. 299.]

with the great development of red sandstones in many parts of the true carboniferous series of Scotland, he concludes that the highest stratified beds of Arran do not represent the new red sandstone, but (more probably) a portion of the carboniferous group. To the upper conglomerates of Arran there is however no counterpart in England; and the exact place of the red beds which overlie them is still left in some doubt; but these upper conglomerates may perhaps be compared with some great trappean conglomerates which are subordinate to the Scotch coal-fields.

CARBONIFEROUS SERIES.—The author briefly notices the changes in this series during its range from the northern counties of England into the basin of the Tweed, where a coal-field occurs developed after the Scotch type, and far below the great coal-field of Newcastle. He then discusses shortly the carboniferous deposits of Scotland, which are divided as follows, in descending order:—

1. The rich coal deposits with numerous beds of coal; in their subordinate beds of shale, ironstone, fire-clay, and fossils, presenting the closest analogies to the great English coal-fields. Their exact place in a general scale cannot however be determined, as they offer no passages, like those above noticed, into any higher formation.

2. A great group with many thin bands of carboniferous limestone, alternating with sandstone and shale; and generally with well-defined thick beds of limestone at the top of the group, so as to form the base of the most productive coal-fields. This group also contains beds of coal, but generally of inferior quality. The alternating sandstones are not unusually of a red colour.

3. Beds of red sandstone, shale, &c.—They undergo many modifications of structure and colour, and are in some places of great thickness. In some of their higher portions they contain coal-plants, and even thin bands of coal; but they pass downwards by gradations the most insensible, and blend themselves with the old red sandstone. Examples of such passages are found on the north side of St. Abb's Head, on the north shores of the Solway Firth, and on the coast of Ayrshire.

The Dumfries-shire carboniferous groups are developed after the Scotch type above described; which is the more remarkable, as the groups on the south side of the Firth conform to the English type. Near Whitehaven there is no passage from the carboniferous limestone to the old red sandstone; and the *thickest* beds of limestone are at the *bottom*, and not (as in Scotland) at the *top* of the calcareous series.

The author then notices the geological map of Scotland, and states that Dr. M'Culloch has not merely introduced much confusion by giving the mountain limestone series and the old red sandstone a common colour; but that he has committed a great error in principle, by confounding, along a considerable part of the country bordering on the north shores of the Solway Firth, the new with the old red sandstone.

OLD RED SANDSTONE.—The author, after briefly noticing the extraordinary irregularity in the development of this formation in the

British Isles, compares the old red conglomerates of Cumberland with those on both sides of the Galloway chain. In these localities they often form unconnected masses resting on the edges of the greywacke; but in Galloway they are not only more largely developed than in the north of England, but show, as above stated, many passages into the overlying carboniferous groups.

Ireland.—He then briefly notices the sections which, in the south of Ireland, connect the old red sandstone with the overlying carboniferous deposits, and form a good passage from one formation to the other. The sequence is complete, and there is nothing to mark any interruption of the deposits. He adopts Mr. Griffith's classification, as most agreeable to the physical character of the groups and to their suites of fossils.

In the south of Ireland the lower carboniferous shales (of Mr. Griffith) pass into the state of roofing-slates with a transverse cleavage, resembling the black slates at the base of the culm measures of Devonshire. The great coal-field in the west of the island overlies the mountain limestone; but it puts on the form of the culm measures of Devon, and was formerly considered as a great transition group. These facts appear to remove a difficulty in classification which was presented by the mineral structure of the Devon culm series.

The author, by way of conclusion, affirms that the Scotch and Irish sections enable us to show that no new formations can be interpolated between the old red sandstone and carboniferous series, inasmuch as the sequence is complete. In like manner, the sections in the Silurian country show that no member is wanting between the old red sandstone and the Ludlow rock. Hence he concludes that, from the lower divisions of the new red sandstone down to the Llandeilo flagstone, there is one continuous unbroken sequence in which no term is wanting. Hence also the argument for the true place of the Devonian system is complete. For any formation, with fossils intermediate between the carboniferous and Silurian systems, must have an intermediate position,—must therefore be on the parallel of *some part of* the old red sandstone, which fills that whole intermediate position. But allowing the above sequence to be complete, there may still be great difficulties in fixing the lines of demarcation by which it is to be finally subdivided. For example, the lower carboniferous limestone, and the carboniferous slates of Ireland, appear to overlap and descend below the base line of the carboniferous series of England: and the same remark appears to be applicable to the lowest beds of the carboniferous series of Scotland. And there are similar difficulties in determining the best base line for the old red sandstone, as appears from subsequent details.

Sections of North Wales, &c.—The author next discusses two sections illustrating the structure of North Wales. One is drawn from the Menai Straits, in a direction about E.S.E., so as to cross the Berwyn chain and end in the carboniferous series near Oswestry. The other is drawn from the Berwyn chain to the carboniferous limestone range on the north side of Denbighshire. The greater portion of the first section crosses the older beds (the *Cambrian system*) which strike towards the N.E. The other section intersects the upper series (*Silurian system*) which strike towards the N.W., passing (in some places unconformably) round the beds of the older system. From a consideration of the whole evidence the rocks are grouped in *the ascending order*, as follows:—

1. Chlorite slate, quartz rock, and mica slate of Anglesea and Caernarvonshire. These are placed at the base of the section, and form a distinct class; and nothing is discovered in this part of the section which is perfectly analogous with the Skiddaw slate, or first Cumbrian group, to be after described.

2. The old slate series of Caernarvonshire and Merionethshire, alternating indefinitely with bands of porphyry and felspar rock: the group is of enormous but unknown thickness, and is bent into great undulations, the anticlinal and synclinal lines of which are parallel to the strike of the chain. Through wide tracts of country it is without fossils; but at Moel Hebog, Snowdon, and Glider Fawr, encrinites, corals, and a few species of bivalves have been discovered in it. It ends with the calcareous beds which range from Bala to the neighbourhood of Dinas Mowddy. This is called the *Lower Cambrian group*.

3. The next group (the *Upper Cambrian group*) commences with the fossiliferous beds of Bala, includes all the higher portion of the Berwyns, and all the slate rocks of South Wales which are below the Silurian system. Its slate beds are less crystalline, and its general structure is more mechanical, than the preceding group, and it contains incomparably more fossils, which (though there are many extensive portions of the group without fossils) are disseminated through the more calcareous beds in great abundance. Many of the fossils are identical in species with those of the lowest divisions of the *Silurian system*, nor have any true *positive* zoological characters of the group been well ascertained.

In many parts of South Wales it is separated from the *Silurian system* by great faults and derangements of the strata, marked by a broad band of rotten non-fossiliferous schist. At the north end of the Berwyn chain it appears to pass by insensible gradations into the lower division of the Upper system (the *Caradoc sandstone*).

4. The last natural group (the *Silurian system*). For all details respecting this system the author refers to the abstracts of Mr. Murchison's papers, and to his published works.

The author then describes a series of sections:—

(1.) East of the Berwyns, in which the Caradoc sandstone is finely developed; containing the *Llandeilo flagstone* and other characteristic calcareous and shelly bands.

(2.) The sections north of the Berwyns, connecting Montgomeryshire with Denbighshire. The ascending series derived from these sections is described as follows:—

(1.) A series of beds several thousand feet in thickness, and at the north end of the Berwyns apparently forming a passage between the Upper Cambrian and lowest portion of the Silurian system.

(2.) Bands of calcareous slate with numerous organic remains of the "*Caradoc sandstone,*" surmounted by roofing slate.

(3.) Series of flagstones, more or less calcareous, with many Orthoceratites and two species of Cardiola, &c.; overlaid by, and associated with, irregular masses of roofing slate with a transverse cleavage.

(4.) Flagstones and rotten slates, many parts in an imperfect state of induration, and the whole surmounted by the carboniferous limestone.—Of the preceding section the lower part of No. 3 is identical with the series of Long Mountain in the Silurian sections of Mr. Murchison; but No. 4 is mineralogically unlike anything he has described, although it has been found by Mr. Bowman to contain, in its highest portion, some of the fossils of the *Upper Ludlow rock*. It appears from these details that the Silurian system, although its subdivisions are obscure from the absence of the Wenlock and Ludlow limestones, is very fully developed in North Wales.

An examination of the few Snowdonian fossils of the author gives the following results:—

(1.) Impressions of corals (Turbinolopsis ?) (Cwm Idwal and Moel Hebog).

(2.) Stems of Encrinites (Cwm Idwal).

(3.) *Orthis pecten, O. Actonia, O. flabellulum, O. canalis* (Snowdon and Moel Hebog).

He has many fossils from different parts of the Berwyn chain; and he believes them (as stated in a former abstract) to be nearly all known Silurian species, but they have not yet been carefully examined. He possesses also a good series of fossils from the eastern side of the Berwyns, and from portions of the more northern sections; but as the whole series is *unequivocally Silurian* (extending from the Llandeilo flagstone to the Upper Ludlow rocks), he has not thought it at present necessary to trouble the Society with any enumeration of species.

From a review of these facts he concludes, that in the great section of North Wales there is no *positive* zoological distinction in the successive descending groups, however vast in thickness or distinct in mineral structure. It is not by the addition of new species, but by the gradual disappearance of the species in the higher groups, that the successive groups are zoologically characterized. Below the Caradoc sandstone there seems to have been very few new types of creation, as far at least as we have learnt from any *positive* facts in the country here described. This conclusion is nearly in accordance with a statement made by the author in a former paper, viz.

"The difficulty of classification by organic remains increases as we descend, and is at length insurmountable; for in the lowest stratified groups, independently of metamorphic structure, all traces of fossils gradually vanish; and the great range of certain species through numerous successive groups, and the very irregular distribution of fossils even in some of the more fossiliferous divisions, add greatly to the difficulties of establishing true definite groups even within the limits of our island. The difficulties are indefinitely increased in comparing the formations of remote continents. But these circumstances are compensated by the magnificent scale of development of the successive groups, and their wide geographical distribution. Taken together, they have a great unity of character; and even in remote continents they seem to form a common base, from which we may hope to compute the whole series of secondary and tertiary deposits that surmount them."

Cumbrian groups, exhibited, in *ascending order*, in a section from Keswick through Kendal to Kirkby Lonsdale:—

1. The group of Skiddaw Forest, &c., the lower part of which rests on the granite, and passes into a system of crystalline strata resembling the rocks of the first class in North Wales; the upper part abounds in a fine dark glossy clay slate, interrupted here and there by beds of more mechanical structure. The whole is of great thickness, almost without calcareous matter, and without any trace of organic remains, and forms the mineral axis of the Cumbrian mountains.

2. A group essentially composed of quartzose and chloritic roofing slates alternating with mechanical beds of coarser structure, and also with innumerable igneous rocks (compact felspar, felspar porphyry, brecciated porphyries, &c. &c.) which partake of all the accidents of the slates. It is of enormous thickness, and rises into the highest mountains of the country; and though chiefly developed on the south side of the preceding group (No. 1), it also appears extensively on the north side of the lower group, which thus forms a mineral axis—a fact not yet noticed in any of the published geological maps. Though abounding in calcareous matter, it has no organic remains. This group is bounded by calcareous slates, which extend from the south end of Cumberland to the neighbourhood of Shap Wells, and have been described by the author in a former paper. (See Transactions of Geological Society.)

3. The next group extends from the calcareous slates (above noticed) to the carboniferous rocks, &c. which surround and cut off the older series*. The highest part of the ascending section is shown on a line which descends to the Lune near Kirkby Lonsdale. The other sections are much less perfect. The whole group is separated, provisionally, into two divisions.

The *Lower division* commences with the calcareous slates above

---

* In a geological map lately presented by the author (which professes only to be a copy of a map made by himself nearly twenty years since), he represents all the beds above the calcareous slates of one colour. He does this, because he is unable to fix the demarcations of the several divisions of the whole group. As he considered the whole to represent the Silurian system he wished to represent the surface by three colours; but he found it impossible, even approximately, to represent their boundaries. And even with a simpler system of two divisions, he is unable, at present, to define correctly their line of demarcation; nearly all the middle portions of the sections being devoid of fossils.

mentioned*. The beds over the calcareous bands are composed of slates and flagstones, hard bands occasionally passing into thick, hard, arenaceous beds of greywacke, &c. It is supposed to end a little to the north of Kendal; but its upper limit is not defined, and there are no distinct calcareous bands to assist in connecting it with, or separating it from, the *upper division*. The fossils derived from the lower portion of this division are Lower Silurian. Among the fossils in the possession of the author, which have as yet been very imperfectly examined, Mr. Lonsdale has found among the corals Catenipora, Porites, Favosites, Ptilodictya, all of known Lower Silurian species, and one or two new species.

Among the shells are three species of Leptæna and five species of Orthis, all of described Caradoc sandstone species; in addition to which there are one or two new species of Orthis. With the above are also found *Atrypa affinis* and *A. aspera*; also *Terebratula bipartita*. With the above occur many specimens of *Tentaculites annulatus*; also several Trilobites, among which are *Asaphus Powisii, Isotelus Barriensis*, and a new Paradoxite, &c.

All the above fossils are found in the calcareous slates.

The *Upper division* is composed of arenaceous flagstone, with imperfect slaty bands, and with beds of hard greywacke. It is generally of a grey, bluish-grey, or greenish-grey colour, rarely of a reddish colour. It has some calcareous portions, but no beds of limestone fit for use; and, near Kirkby Lonsdale, ends with red fossiliferous and flaggy beds containing concretionary limestone, which are overlaid unconformably by the marls and conglomerates of the old red sandstone. The fossils of the above group (which is of great thickness, though partially repeated by undulations) are of one type. Several species are new, *e. g.* two or more species of Pterinæa, &c.: but the great majority of specimens, whether from the hills south of Kendal, or from Kirkby Moor, are Upper Silurian; or in the beds Mr. Murchison places at the base of the old red sandstone (tilestone).

The following list is made out by Mr. Sowerby from what the author considers a very imperfect collection:—

Terebratula nucula.
Orthis lunata.
Leptæna lata. Very abundant.
Spirifera interlineata.
Cypricardia cymbiformis.
Avicula rectangularis.
——— retroflexa.
Cucullæa antiqua.
Bellerophon trilobatus.

Trochus helicites.
Turbo Williamsii.
Natica.
Turritella obsoleta. ⎫
——— gregaria. ⎬ Very abundant.
——— conica. ⎭
Orthoceras trochleare.
Calymene Blumenbachii.

From the above lists we obtain this definite information, that the

---

* When a former abstract was published, the author placed these beds on the parallel of the Bala limestone, over which the slates of the Berwyns and all the Devonian slates were provisionally arranged; but since the removal of the Devonian system to a place *superior* to the Silurian, the sections present no real ambiguity. The calcareous slates above described are true Lower Silurian, and not a part of any sub-Silurian group that is represented by the older rocks of South Wales.

lower division is Lower Silurian, and that the upper division ends at the very top of the Silurian system, and includes beds which have been classed with the old red sandstone—an arrangement which is natural in South Wales, but is not sanctioned by the Westmoreland sections.

The want of good mineral or fossil groups to distinguish the middle portion of the section, makes the real difficulty of representing the divisions on a map.

The author then briefly noticed two other sections; one from the Shap granite, through the fossiliferous slates, &c., to Howgill Fells. These, in their range southwards through Middleton Fells, &c., are placed in the upper division, though not in the highest part of it, which is described above. They contain very few fossils, but those which have been found are of the Upper Silurian system.

Lastly, the author briefly mentioned the phænomena of another ascending transverse section from the western end of the calcareous slates, as follows:—

(1.) Calcareous slates (Caradoc) of Millum in Cumberland.
(2.) Quartzose flagstone, coarse pyritous shale and slate, &c.
(3.) Roofing slates of Kirkby Jreleth.
(4.) Second band of calcareous slates, also with Lower Silurian fossils.
(5.) Upper series of flags and roofing-slate extending to the neighbourhood of Ulverston; and in turn overlaid by coarser beds, which, however, in a section continued to Morecambe Bay, did not show any of the upper fossil bands.

Ireland and South of Scotland.—The author then shortly notices some sections in the counties of Waterford and Kerry (to which he was conducted by Mr. Griffith). They exhibit a fine sequence of true Lower Silurian rocks, but do not show their relations (at least in any section seen by the author) to the older non-fossiliferous slates of the south of Ireland. Hence, though excellent examples of a group of upper fossiliferous slates, they do not offer any help as to the number and order of the natural groups into which the great infra-carboniferous series may be conveniently divided. He then points out that the grouping of the older strata in the south of Ireland, now given by Mr. Griffith, is not only sanctioned by the sections, but gets rid of a great supposed anomaly,—viz. the re-appearance of the carboniferous fossils at different levels on a general descending section of the older rocks of Ireland.

The author then briefly notices the fossils in the true Silurian rocks in the north of Ireland, in progress of publication by Captain Portlock. They form an admirable series, but the sections do not appear to connect the group of rocks containing them with the older formations, so as to lend much help in their subdivisions or grouping.

Mourne mountains, Galloway chain, &c.—After a few details respecting the mineral structure, strike, altered rocks, granite veins, &c., of Downshire, the author proceeds to notice the Galloway chain (which extends from the Mull of Galloway to St. Abb's Head). Its prevailing strike, like that of the Mourne mountains, is about N.E. by E.; and this is sometimes persistent, even in the neighbourhood of protruded masses of granite. It is generally made up of beds of a hard arenaceous greywacke, sometimes of a very coarse structure, sometimes finer, and occasionally passing into a good roofing slate, —generally it is without fossils; but the *Graptolites foliaceus* (first noticed by Mr. Carrick Moore) occurs, though rarely, among the finer slates. In these respects the chain is analogous to that in Pembrokeshire, where the same fossil occurs in the slates below the Lower Silurian rocks of Mr. Murchison.

He then notices a ridge of rocks visited by Mr. Carrick Moore and himself, which breaks out from under the carboniferous basin of Girvan-water in Ayrshire. It contains many fossils, among which Mr. Sowerby finds three or four new species of Orthis, Tentaculites, Atrypa, and one or two species of Terebratula. Near it, and probably forming a part of it, is a small mass of limestone, with many corals and some Trilobites, the latter unfortunately lost by the author. Mr. Lonsdale states that the corals are difficult and obscure, but there is a true *Favosites fibrosa*, probably also a *Favosites spongites*; and there are, among the specimens, several small hemispherical corals which may be young *Stromatopora concentrica*. From this evidence he would be inclined to refer the limestone to an Upper Silurian or Devonian group. From the number of Orthidia, Mr. Sowerby would refer the fossiliferous slates to the Lower Silurian; but the whole mass, including slates and limestone, is of small extent, and seems to form but one group, which may be considered as Silurian.

To show the position of these beds, the author gives a transverse section from the Solway Firth over the Galloway chain to the fossil group above mentioned. The groups on the section appear in the following order, beginning at the south end:—1. Old red sandstone. 2. Greywacke of the Galloway chain. 3. Granite. 4. Greywacke of the Galloway chain on the north side of the axis. 5. Unconformable masses of old red sandstone. 6. Coal-basin of Girvan-water. 7. Fossiliferous slates and limestone rising from under the coal series.

Conclusion.—It appears, from the preceding synopsis, that there is a continuous and apparently uninterrupted sequence of deposits from the lower beds of the new red sandstone formation to the lowest known strata of England; that beds of masses of limestone appear here and there in the descending series; and (with the exception of the mountain limestone) that they are neither so continuous nor so fixed in their place as to offer any good bases for the general classification of the groups; that the divisions into which the descending series may be separated often pass into one another, so as to make their demarcations doubtful or arbitrary; and that, in the lower divisions, organic remains gradually disappear. The great divisions of the descending series hitherto ascertained are as follows:—

1. *Carboniferous.*—Passing in some places at its upper limits into the lower new red sandstone.

2. *Old red sandstone.*—Passing in its upper limits (Scotland and Ireland) into the first division, and including the slate rocks, &c., of Devon and a part of Cornwall.

3. *Silurian.*—Passing in its upper groups into the old red sandstone.

All the country described by Mr. Murchison as superior to the Llandeilo flags, separated into three groups—upper, middle, and lower. East of Berwyn chain, lower group. North of the Berwyn chain (Denbighshire), upper, middle, and lower groups; but with a new mineral type, and without any upper bands of limestone. Westmoreland: upper group largely developed, and including fossils of the *tilestone*; middle group without limestone bands or fossils; lower group with many characteristic fossils. Horton and Ingleton, middle and upper groups. Ireland (Waterford and Kerry), lower group. Scotland (Ayrshire), Silurian group, but not defined.

4. *Sub-Silurian, or Upper Cambrian.*—The old rocks of South Wales below the preceding division; containing Graptolites, but no well-defined calcareous band, and very few fossils. A part of the Berwyn chain based on the Bala limestone. The upper part of the roofing slates, &c., of Cumberland, immediately under the Caradoc limestone (of Coniston, &c.). Slates of Charnwood Forest? Slates of the Mourne mountains, of the Galloway chain, &c.

5. *Lower Cambrian.*—The great slate group of North Wales below the Bala limestone. The old roofing slates of Cumberland.

6. *Lower Cumbrian, or Skiddaw slate.*—Slates of Skiddaw Forest, lower part metamorphic. Provisionally arranged in this place, the chlorite slates, &c., of Anglesea and Caernarvonshire.

AMERICAN PHILOSOPHICAL SOCIETY.

January 21, 1842.—Dr. Hare made an oral communication respecting a new æthereal liquid which he had succeeded in obtaining.

He mentioned that he had procured, by means of hyponitrite of soda, diluted sulphuric acid and pyroxylic spirit, an æthereal liquid, in which methyl ($C_2 H_3$) might be inferred to perform the same part as æthyl ($C_4 H_5$) in hyponitrous æther. In fact, by substituting pyroxylic spirit for alcohol, this new æther was elaborated by the process for hyponitrous æther, of which he had published an account in the Society's Transactions, vol. vii. part 2.

The compound which was the subject of his communication had a great resemblance to alcoholic hyponitrous æther, similarly evolved, in colour, smell and taste, although there was still a difference sufficient to prevent the one from being mistaken for the other.

Pyroxylic spirit appeared to have a greater disposition than alcohol to combine with the æther generated from it, probably in consequence of its having less affinity for water. The boiling point appeared to be nearly the same in both of the æthers; and in both, in consequence of the escape of an æthereal gas, an effervescence, resembling that of ebullition, was observed to take place at a lower temperature than that at which the boiling point became stationary. The æthereal gas, of which Dr. Hare had given an account in his communication respecting hyponitrous æther, seemed to have escaped the attention of European chemists; and, even after it had been noticed by him, seemed to be overlooked by Liebig, Kane, and others, in their subsequent publications.

Dr. Hare attached the more importance to his success in producing the æther which was the subject of his communication, since, agreeably to Liebig, no such compound exists, and it is to be inferred that efforts to produce it had hitherto failed. It was presumed that this would excite no surprise, when the difference was considered between the consequences of the reaction of nitric acid with pyroxylic spirit and with alcohol.

The liquid last mentioned is now viewed as a hydrated oxide of æthyl, while pyroxylic spirit is viewed as a hydrated oxide of methyl. When alcohol is presented to nitric acid, a reciprocal decomposition ensues. The acid loses two atoms of oxygen, which, by taking two atoms of hydrogen from a portion of the alcohol, transforms it into aldehyd; while the hyponitrous acid, resulting inevitably from the partial deoxidizement of the nitric acid, unites with the base of the remaining part of the alcohol. But when pyroxylic spirit is presented to nitric acid, this acid, without decomposition, combines with methyl the base of this hydrate; so that, as no hyponitrous acid can be evolved, no hyponitrite can be produced. Thus, in the case of the one, there can be no æthereal hyponitrite; in that of the other, no æthereal nitrate.

Dr. Hare regretted that Liebig should not have been informed of the improved process for hyponitrous æther, to which he had referred in commencing his communication. Instead of recommending a resort to that process, it was advised that the fumes, resulting from the reaction of nitric acid with fecula, should be passed into alcohol, and the resulting vapour condensed by means of a tube surrounded by a freezing mixture.

This process Dr. Hare had repeated, and found the product very inferior in quantity and purity to that resulting from the employment of a hyponitrite. In this process, nascent hyponitrous acid, as liberated from a base, is brought into contact with the hydrated oxide. In the process recommended by Liebig, evidently this contact could not take place; since it was well known that hyponitrous acid could not be obtained by subjecting fecula and nitric acid to distillation, and condensing the aëriform products*.

March 4th.—Dr. Goddard presented specimens of Daguerreotype on a surface of gilded silver, and stated that the surface of iodide of gold was more susceptible to the Daguerreotype action of light than that of the iodide of silver, that the surface of the plate might be polished without injury before the action of the iodine, and that the lights came out better than on the silver surface.

April 1.—Dr. Hare related some experiments, showing that the vapour of nascent steam, generated by the hydro-oxygen flame, was not productive of electricity.

He observed that, before his late voyage to Europe, he had made some experiments in order to ascertain whether any electricity was

---

* The process alluded to is as follows:—Seven parts of acid, eight parts of alcohol, fourteen parts of water, and fourteen of hyponitrite being prepared, add seven parts of water to the salt and seven to the acid, and allow the mixture to cool. The saline solution and alcohol are introduced into a tubulated retort, of which the recurved and tapering beak enters a tube, which occupies the axis, and descends through the neck of an inverted bell-

given out by the flame of the hydro-oxygen blowpipe, or by the elements of water during their conversion into steam.

The unexpected electrical results, previously ascertained respecting high steam*, naturally gave importance to this inquiry, the result of which he had no previous opportunity of communicating to the Society.

Even the flame produced by means of a very powerful hydro-oxygen blowpipe was not found to be productive of electrical indication, when allowed to act upon a metallic mass supported upon the canopy of an extremely delicate electroscope. As it was suggested that, the flame being a conductor, the electricity evolved might retrocede by it to the metallic pipe, the experiment was modified in the following way:—

The mixture of one part of oxygen and two of hydrogen being, as in the first instance, condensed within a mercury bottle, was made, by means of a valve cock and safety tube, to communicate, through a glass tube, with a jet pipe of platinum, a foot in length and in bore.

The apparatus being thus arranged, and the cock so adjusted as to allow the gaseous mixture to escape through the jet pipe with sufficient celerity, a flame of hydrogen was applied to the outside of this pipe about the middle. By these means, the temperature being raised so as to cause the elements of water to combine, the flame was removed, the heat being sufficiently kept up by the internal combustion. Thus that which entered at one end of the tube as gas, came out at the other as steam. Under these circumstances, a single-leaf electrometer, more susceptible than a condensing electrometer, was not indicative of any electrical excitement, either in the insulated jet tube, or in any body on which the steam was allowed to condense.

Dr. J. K. Mitchell having expressed a wish to see these experiments, they were repeated, with his assistance, with the same results.

Dr. Hare also mentioned that he had observed an æthereal liquid to subside on the addition of pure pyroxylic spirit to an aqueous solution of hypochlorous acid, obtained by passing chlorine into water in contact with bioxide of mercury.

Having separated the æther thus produced, he found it to have an agreeable and peculiar fragrance. Like oil of wine, it could not be distilled without decomposition. There was an effervescence at the temperature of 140° F.; but the boiling point rose beyond that of a

---

glass, so as to terminate within a tall phial. Both the tube and phial must be surrounded by ice and water. The diluted acid is then added gradually. A water-bath, blood-warm, is sufficient to cause all the æther to come over.

Agreeably to another plan, the materials, previously refrigerated by ice, are introduced into a bottle, also similarly refrigerated. Under these circumstances the æther soon forms a superstratum which may be separated by decantation.

This last-mentioned process does not answer so well for the hyponitrite of methyl, on account of the pyroxylic spirit being prone to rise with the æther; yet the spirit may be separated from the æther by anhydrous chloride of calcium.

* [See Phil. Mag. Third Series, vol. xvii. p. 370, and various subsequent papers in that volume, and in vols. xviii. xix. xx.—Edit.]

boiling water-bath. When a naked flame was applied, the æther, previously colourless, acquired a yellowish wine colour, and, by the crackling evolution of vapour, indicated decomposition.

When the liquid hypochlorous acid was subjected to the process of distillation, before the addition of the spirit, an æther resulted which floated on the solution, and which appeared to differ from that obtained as first mentioned.

Dr. Hare made these observations, and those previously communicated respecting the hyponitrite of methyl, by the aid of a small quantity of pure pyroxylic spirit, supplied to him by his friend Dr. Ure, and regretted that both ill-health and the exhaustion of his stock of spirit had prevented him from making further observations and experiments, tending to decide whether the æthers obtained, as he had described, were either or both hypochlorites, or whether mercury entered into the composition of the heavier æther. This there was some reason for believing; since, when boiled to dryness at a high temperature, a reddish residuum was apparent, which being redissolved, and a small strip of copper immersed in the resulting solution, a minute deposition, apparently metallic, was observable.

XXV. *Intelligence and Miscellaneous Articles.*

FOURTH MEETING OF THE ITALIAN CONGRESS OF MEN OF SCIENCE.

A CIRCULAR has arrived in England announcing that the scientific men of Italy will meet this year at Padua on the 15th of September, under the presidency of Signors Nicolo da Rio and Giovanni Santini, both of the University of Padua. The warmest invitations are given to such scientific persons of all nations as may be disposed to attend the meeting.

ON THE EARTHQUAKE FELT IN PARTS OF CORNWALL, ON FEBRUARY 17, 1842 *.

At the last annual meeting of the Royal Institution of Cornwall, a communication was read from Mr. William Henwood, recording three shocks of earthquakes, which had been felt at different periods in the county. In addition to these, the following are mentioned in a paper, by Mr. D. Milne, 'On the Shocks of Earthquakes felt in Great Britain.'

1757, July 15.—The shock of an earthquake was felt at Falmouth, at seven p.m., attended with great noise. It came from the south-west, and was heard in the mines of Cornwall at a depth of seventy fathoms. The shock extended as far east as Liskeard, and as far north as Camelford. "Several small risings as big as mole-hills were observed in the morning before the shock happened, on the sands of the beach, having a black speck in the middle of the top, as if something had issued from it. From one of the risings be-

---

* From the Report of the Polytechnic Society of Cornwall for 1841. The particulars were collected by Mr. Robert Hunt, Secretary.

tween the hollows there issued a strong gush of water, about as thick as a man's wrist. For a week before the shock the weather had been warm and sultry. In one of the mines the earth was felt to move with a prodigious swift and apparently horizontal tremor." —Gent.'s Mag., v. xxix. 146; and Transactions R.S.S.*

1759, Feb. 24.—The shock of an earthquake was felt at Liskeard. A bright aurora borealis seen in the evening.

From the statements of several persons residing at Budock and at Stithians, it appears some disturbance was felt in 1836.

As this paper is designed to record as correctly as possible all the circumstances connected with the phænomenon of the 17th of February, 1842, I shall without hesitation state, in the first place, the manner in which it was felt at my own residence, in Berkeley Vale, Falmouth.

About twenty minutes before nine a.m., I heard a peculiar rumbling sound, more like the moaning of the wind than thunder, which was immediately followed by a shaking of the doors and windows of the house, the whole effect lasting about half a minute.

In the environs of the town of Falmouth, the noise particularly attracted attention, and although but few speak of any tremor, yet all describe it either as resembling the fall of a heavy body, or like a distant explosion. Many persons were fully persuaded a steam vessel had blown up in the harbour.

At Penryn the disturbance was more decidedly felt than at Falmouth, and most persons speak of the doors of their houses shaking, and some of the earthenware rattling on the shelves: many left their houses in alarm. It has been stated that the tide rose and fell again suddenly; such does not, however, appear to have been the case: an individual, who observed the tide-mark at the bridge at the time, says that no variation was produced.

At Enys, one mile from Penryn, the shock is described by J. S. Enys, Esq., "as a noise twice quickly repeated, like a heavy weight falling and rebounding:" this gentleman also speaks distinctly of the shaking of articles in the rooms.

At Ponsanooth and down the valley to Perranwharf, the shock is described by all persons as considerable, and the first impression was that the powder mills in the neighbourhood had exploded. Along this line, still extending to the north, the disturbance appears to have been equally felt. The inhabitants of the villages of Comfort and Lanner, under Carn Marth, about the junction of the granite and killas or clay-slate, left their houses, thinking that some serious explosion had occurred at the neighbouring mine; and on the southern side of the granite hill, Carn Marth, the people felt a great tremor. An intelligent person, captain of Poldory mine, describes it thus:—"I imagined some of the empty railroad waggons had been let go at the top of the incline, and were rapidly rushing past the door of my house: my neighbour, a widow woman, ran out shrieking that the side of her house was coming in." In Poldory, the

---

* We presume this is intended to refer to the Transactions of the Royal Society of Edinburgh, in which Mr. Milne's paper appeared.

western part of the United Mines, the shock was felt by the men working 130 fathoms below the surface; but it does not appear to have been noticed at all in the eastern part of these or the Consolidated Mines. At Tresavean mine the shock was felt at all depths.

The people dwelling to the north of Carn Marth do not appear to have been conscious of anything uncommon; the noise was heard at Tuckingmill and Pool, but was attributed to the discharge of a cannon at a great distance, so faint and indistinct was it. In the south parts of the parish of Camborne the noise was also heard, but no tremor felt.

In the parish of Stithians the shock was decidedly felt, and several persons in the village, who were taking breakfast at the time, speak of their tables having been shaken, and the cups and saucers having clattered. It appears to have been felt with equal intensity in the parishes of Mabe and Constantine, perhaps more powerfully in the latter than in any other part.

An intelligent correspondent, who has kindly been at some pains to procure authentic accounts, thus writes:—"On the morning of the 17th a shaking of the earth was felt in this village, accompanied by a sound resembling distant thunder. At one house, where some men were working, they left their work and ran out to know what it was that gave the shock. In another a book fell from the bookshelf to the floor. Adjoining the village, where there were some persons in bed at the time from sickness, the beds were felt to shake; a door was even seen to fly open from the shock. At Wheal Vyvyan mine some men working about twenty or thirty fathoms under ground also felt it very distinctly; and one man, who was leaning against a rock at the time, still more so. My wife also felt it, and it appeared to her as if the roof of the house was falling in."

At Helstone the disturbance was considerable. Mr. Moyle of that town thus describes it:—"While at breakfast, about half-past eight, I started suddenly from my chair, with the impression that a heavy truck had run suddenly down the stone steps of a passage forming a back entrance to my premises." At Nansloe, half a mile south of Helstone, the servants say the earthenware evidently clattered; and the same was experienced at Trevarno, two miles northwest of the town. Captain Richards, of Wheal Vor mine, situate to the west of Helstone, writes as follows:—"The shock of the earthquake on the 17th was very distinctly heard and felt at this mine, 175 fathoms under the surface; also at the 80 fathom level under the surface. It was also felt at Penhale mine 50 fathoms under the surface, and by several persons within a mile of Wheal Vor mine. It was very distinctly heard and felt near Godolphin, and in and about Great Work mine; also at Wheal Penrose mine near Porthleaven."

It does not appear to have been very evident at Porthleaven.

From West Wheal Virgin, in the parish of St. Hilary, I have the following communication from Captain Henry Francis:—"A little before nine on the morning of the 17th, some of our men at work in the 100 fathom level, in the south lode, felt a shock, and as it were a rush of air, *so much so that one of the candles was put out by* *it,* accompanied by a noise which made them think that one of our shafts had crushed in, or *runned* together; but on examining with Capt. Crose, who was in the mine, we could find nothing at all amiss, or any cause for the shock."

This appears to be the most westerly part at which the tremor was felt, and although the noise was heard away to the south, towards the Lizard, it is clear it was much diminished in force.

On referring to a geological map of the county, it will be found that the greatest effects were produced near the edge of the granite mass, which extends from the north-east to the south-west, from Carn Marth to the south of Penryn. Although it was felt at Falmouth, Helstone, and other places which are on the clay-slate, yet all my inquiries go to show that it diminished rapidly in force, as the distance from the granite increased*.

A gentleman of Helstone says, "I felt it very sensibly, and my house shook, but I experienced an effect on the sight which I always find attends electricity;" from which he appears inclined to deem the disturbance as atmospheric. Had that been the case, it would not have been felt in the mines; but it is not improbable that a manifestation of electricity may have attended this disturbance of the earth.

ON THE BLUE COLOUR OF ULTRAMARINE. BY M. ELSNER.

According to all analyses hitherto published, ultramarine is composed principally of soda, alumina, silica and sulphur, as shown by the following statements:—

Lapis Lazuli.

	(Clement Desormes.)		(Varrentrapp.)
Soda	23·2		9·09
Alumina	34·8		31·67
Silica	35·8		45·50
Sulphur	3·1		0·95
Carbonate of lime	3·1	Lime	3·52
		Iron	0·86
		Chlorine	0·42
		Sulphuric acid	5·89
		Water	0·12

Artificial Ultramarine of Paris.

(C. G. Gmelin.)	
Soda (mixed with potash)	12·063
Lime	1·546
Alumina	22·000
Silica	47·306
Sulphuric acid	4·679
Sulphur	0·188
Resinous substance, sulphur and loss.	12·218

Artificial Ultramarine manufacture of Meissen.

(Varrentrapp.)	
Soda	21·47
Potash	1·75
Lime	0·02
Alumina	23·30
Silica	45·00
Sulphuric acid	3·83
Sulphur	1·683
Iron	1·063

---

* Mr. Hunt here adds some remarks on the condition of the atmosphere, and the heights of the barometer and thermometer at the period of the earthquake.

It appears that the analyses of Varrentrapp only, give iron as present in these substances, and which is essential to the production of the blue colour of ultramarine: lapis lazuli is well known to contain iron pyrites.

M. Elsner has analysed the blue and green varieties of ultramarine from Nuremberg, and he found them to be composed as follows:—

	Blue Ultramarine.	*Green Ultramarine.*
Silica	40·0	39·9
Alumina	29·5	30·0
Soda	23·0	25·5
Sulphuric acid	3·4	·4
Sulphur	4·0	4·6
Peroxide of iron	1·0	·9
	100·9	101·3

These contained traces of chlorine, potash, lime and magnesia. These analyses show that there is much more sulphur present than is required for the production of a simple sulphuret of iron; this excess of sulphur can be combined only with the sodium; and it results also from the analysis, as is also shown by synthetical researches, that sulphuret of sodium is not less necessary than sulphuret of iron to the production of ultramarine.—*Journal de Pharm. et de Chim.*, Avril 1842.

PREPARATION OF OXICHLORIC ACID. BY M. AD. NATIVELLE.

Oxichloric acid, which is so useful as a reagent, M. Nativelle remarks, is seldom to be found in laboratories; and he supposes this to be owing to the small quantity of it which is obtained by employing the proportion of sulphuric acid usually recommended in chemical works: he gives the following process as separating the whole of the acid from the oxichlorate of potash:—

Put into a glass retort 500 parts of oxichlorate of potash reduced to powder, deprived as much as possible of chlorate; add 1000 parts of sulphuric acid of specific gravity 1·845, and 100 parts of distilled water; this small quantity of water is not indispensable, for it will be shown that, by omitting it, oxichloric acid is obtained in the crystalline state. An adopter with a long tube is to be passed into a tabulated retort, surrounded with cold water; the apparatus must not be luted with any organic substance, for the oxichloric acid gas coming into contact with it while hot decomposes it and produces slight detonations; when proper vessels are employed lute need not be employed, but, when required, filaments of amianthus answer the purpose. The oxichlorate is to be carefully heated; it readily dissolves, and the fire must be regulated so as to prevent the oxichloric acid from carrying over with it too much sulphuric acid. The best method of regulating the operation is to keep below the boiling point; but little sulphuric acid goes over, for oxichloric acid volatilizes at 284°, which is much lower than the temperature at which sulphuric acid distils. The operation is complete when the residue in the retort is transparent and colourless, or when the product drops very slowly and the temperature of the retort is nearly sufficient to volatilize sulphuric acid; the weight of the product depends upon the quantity of sulphuric acid carried over; for by a carefully conducted operation the ingredients mentioned give about 300 parts of crude acid of the density of about 1·455; when the operation has been too quickly conducted the density and weight of the product is greater.

In order to separate the sulphuric acid and the small quantity of chlorine which the product contains, it is to be shaken with a slight excess of a saturated solution of sulphate of silver, and the chloride of silver formed is separated by filtration; the acid is then to be put into a capacious capsule, and artificial carbonate of barytes added till all the sulphuric acid is precipitated, and even till a little oxichlorate of barytes is formed. The liquor now contains only oxichloric acid, with a little oxichlorate of barytes and of silver, and is to be distilled, in the apparatus already described, with the addition of ice, separating the first product, which is only water, and ascertaining that the acid is coming over by test paper. The distillation is to be carried on to dryness, but taking care not to decompose the oxichlorates of barytes and silver, for then the rectified oxichloric acid might contain traces of chlorine. The oxichloric acid thus obtained is perfectly pure, colourless and transparent; its specific gravity is between 1·717 and 1·800, and it is oleaginous like sulphuric acid; 500 parts of oxichlorate of potash yielded 150 parts of this concentrated acid.—*Journal de Pharmacie et de Chimie*, June 1842.

ON THE ACTION OF WATER ON LEAD. BY PROF. CHRISTISON.

In a second paper on this subject, just published in the Transactions of the Royal Society of Edinburgh (vol. xv. part 2. p. 271), Dr. Christison states the following as the results of his entire investigation:—

"From the facts now detailed, together with the results of my former inquiries, the following conclusions may be drawn as to the employment of lead pipes for conducting water.

"1. Lead pipes ought not to be used for the purpose, at least where the distance is considerable, without a careful chemical examination of the water to be transmitted.

"2. The risk of a dangerous impregnation of lead is greatest in the instance of the purest waters.

"3. Water which tarnishes polished lead when left at rest upon it in a glass vessel for a few hours, cannot be safely transmitted through lead pipes without certain precautions*.

"4. Water which contains less than about an 8000th of salts in solution, cannot be safely conducted in lead pipes, without certain precautions.

"5. Even this proportion will prove insufficient to prevent corrosion, unless a considerable part of the saline matter consist of carbonates and sulphates, especially the former.

"6. So large a proportion as a 4000th, probably even a consider-

---

* "Conversely, it is probable, though not yet proved, that, if polished lead remain untarnished or nearly so for twenty-four hours in a glass of water, the water may be safely conducted through lead pipes."

ably larger proportion, will be insufficient, if the salts in solution be in a great measure muriates.

"7. It is, I conceive, right to add, that in all cases, even though the composition of the water seems to bring it within the conditions of safety now stated, an attentive examination should be made of the water after it has been running for a few days through the pipes. For it is not improbable that other circumstances, besides those hitherto ascertained, may regulate the preventive influence of the neutral salts.

"8. When the water is judged of a kind which is likely to attack lead pipes, or when it actually flows through them impregnated with lead, a remedy may be found either in leaving the pipes full of the water and at rest for three or four months, or by substituting for the water a weak solution of phosphate of soda in the proportion of about a 25,000th part."

APOTHECARIES' HALL.

On Thursday, June 23rd, Mr. ROBERT WARINGTON, Secretary to the Chemical Society, and formerly assistant to the late Dr. Edward Turner, Professor of Chemistry in University College, London, was elected Chemical Operator in this establishment, in consequence of the recent lamented decease of Mr. Henry Hennell, F.R.S.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1842.

Chiswick.—June 1—3. Very fine. 4—7. Hot and dry. 8—11. Fine: hot and dry: clear at night. 12, 13. Clear and hot, thermometer as high as 90° in shade. 14. Hot and dry. 15. Fine, with clouds. 16. Overcast. 17. Overcast and fine. 18. Heavy showers. 19. Very heavy rain. 20. Cloudy and fine. 21. Slight rain. 22, 23. Very fine. 24. Slight rain. 25. Overcast: cloudy and windy: boisterous, with rain at night. 26. Fine: cloudy: clear, with dry air at night. 27, 28. Clear and fine. 29. Hot and dry. 30. Slight rain: overcast: very heavy rain at night. The mean temperature of the month was 2°·65 above the average.

Boston.—June 1. Cloudy. 2, 3. Fine. 4. Fine: thermometer 76° two o'clock P.M. 5. Fine: rain with thunder and lightning P.M. 6—8. Fine. 9. Cloudy. 10—12. Fine. 13. Cloudy. 14. Fine: thermometer 77° eleven o'clock A.M. 15. Fine. 16, 17. Cloudy. 18. Rain. 19. Cloudy: rain P.M. 20. Fine: rain P.M. 21. Cloudy: rain with thunder and lightning P.M. 22, 23. Fine: rain P.M. 24. Fine. 25. Windy: rain P.M. 26, 27. Windy. 28. Cloudy. 29. Fine. 30. Cloudy. N.B. The warmest June since June 1826.

Sandwick Manse, Orkney.—June 1. Clear: shower. 2. Cloudy: clear. 3. Cloudy. 4. Cloudy: rain. 5. Clear. 6—8. Clear: fog. 9. Clear: fine. 10. Cloudy: fine. 11, 12. Clear: fine. 13. Clear: damp. 14. Damp. 15. Clear: rain. 16. Clear: shower. 17. Sleet: showers. 18. Clear. 19. Clear: fine. 20. Cloudy. 21. Cloudy: damp. 22. Rain: clear. 23. Cloudy: thunder. 24. Clear: cloudy. 25. Clear: shower. 26. Damp: clear. 27. Showers: sleet. 28. Cloudy: rain. 29. Cloudy: showers. 30. Showers: cloudy.

Applegarth Manse, Dumfries-shire.—June 1. Showery. 2, 3. Fair and fine. 4. Fine: shower P.M. 5. Warm and showery. 6—8. Fair and fine. 9—11. Fair and fine: droughty. 12. Fair and fine. 13. Fair and fine: thunder. 14. Fair, but threatening change. 15. Fair till P.M.: a few drops. 16. Fair, but cloudy. 17. Some drops of rain. 18. Fair and fine. 19. Shower early A.M. 20. Showers and thunder. 21. Showers: warm. 22. Shower early A.M. 23. Heavy rain P.M. 24. Frequent showers. 25. Heavy rain. 26. Showers. 27. Showers: mackerel sky. 28. Rain all day. 29. Showers A.M.: cleared up. 30. Fine, but cloudy.

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

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R. S. 9 a.m.
1842. June.		Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.											
☾ 1.	30·284	30·211	30·125	29·47	29·98	29·85	29·94	29·89	64·5	81·5	53·2	76	56	68	58	48	52	49	s.	sw.	w.	s &sw.	sw.	·019	……	……	……	……	54
2.	30·250	30·265	30·169	29·47	30·08	30·18	30·07	30·25	64·5	72·3	60·0	73	41	63	63½	47	53	49	nw.	nw.	w.	wnw.	w.	……	……	……	……	·12	60
3.	30·368	30·296	30·160	29·74	30·18	30·10	30·14	30·07	63·3	76·5	53·0	76	42	63·5	66	38	60	59	e.	nw.	calm	sw.	s.	……	……	……	……	……	55
4.	30·168	30·112	29·965	29·54	30·00	29·90	30·01	30·00	66·3	77·5	54·0	82	48	68	69	44½	55	50	nw.	w.	calm	sw.	n.	……	……	……	……	……	54
5.	30·018	29·953	29·809	29·36	29·88	29·85	29·99	30·03	66·4	76·0	59·4	80	48	68	66	48½	58	52	s.	e.	calm	sw.	se.	……	……	……	……	·70	58
6.	30·002	30·038	29·915	29·40	29·93	30·03	30·06	30·17	68·5	77·0	57·4	82	41	66	70	45	53½	51½	n.	e.	calm	ne&e.	se.	……	……	·13	……	……	61
7.	30·264	30·217	30·163	29·63	30·11	30·20	30·26	30·40	66·7	76·0	56·4	78	51	68	74½	50	62	52	e.	ne.	calm	sse.	calm	……	……	……	0·25	……	55
● 8.	30·324	30·244	30·204	29·75	30·27	30·30	30·45	30·45	66·8	72·7	55·3	81	47	66	75½	54	54	52	nw.	ne.	ne.	ese.	e.	……	……	……	……	……	63
9.	30·314	30·218	30·103	29·75	30·27	30·24	30·45	30·43	63·4	78·3	54·0	76	49	56·5	75	51	55	56	n.	ne.	e.	se&ne	s.	……	……	……	……	……	60
10.	30·182	30·100	30·064	29·65	30·20	30·17	30·39	30·37	66·3	74·2	52·7	81	51	66	72	48	58	55	ne.	ne.	e.	ne.	e.	……	……	……	……	……	61
11.	30·254	30·180	30·133	29·74	30·20	30·22	30·35	30·40	65·2	77·4	53·8	89	54	62	75	48	58	54	n.	ne.	e.	ne.	calm	……	……	……	……	……	62
12.	30·362	30·263	30·244	29·77	30·28	30·29	30·46	30·42	68·3	82·2	56·6	90	51	68·5	78½	52½	56½	59	n.	se.	e.	ne.	ne.	……	……	……	……	……	66
13.	30·358	30·293	30·153	29·77	30·23	30·10	30·29	30·18	64·8	82·4	58·2	90	55	59	78½	49	62	53	n.	ne.	calm	se.	w.	……	……	……	……	……	61
14.	30·168	30·094	30·012	29·50	30·05	30·00	30·10	30·07	72·3	79·7	62·6	88	53	73	75	50½	54	50	e.	sw.	calm	wnw.	w.	……	……	……	……	……	63
☽ 15.	30·110	30·048	29·989	29·47	30·02	29·91	30·06	29·95	66·3	82·4	59·0	80	48	59	70	52½	51	50½	nw.	ne.	calm	nw.	nw.	·033	……	……	0·00	……	63
16.	30·062	29·995	29·989	29·44	29·90	29·97	30·03	30·14	66·3	79·2	57·2	76	48	63	68	52½	48	45	nw.	nw.	w.	nw.	nw.	……	……	……	……	·11	58
17.	30·132	30·084	30·058	29·58	30·08	30·08	30·15	30·16	64·0	72·7	56·7	73	51	57	62	48½	46½	45	s.	e.	calm	e.	n.	……	……	……	……	·07	58
18.	30·086	30·041	29·888	29·59	29·96	29·79	29·93	29·92	59·5	68·8	54·4	71	49	53	64	46	50	42	e.	s.	calm	sw.	nw.	……	·12	……	……	·04	56
19.	29·806	29·769	29·688	29·25	29·68	29·65	29·85	29·85	60·3	66·8	54·6	71	49	62	62	45	50	43	s.	w.	calm	nw.	ne.	·069	·34	……	……	……	58
20.	29·788	29·734	29·665	29·23	29·62	29·55	29·76	29·73	66·2	69·7	56·2	72	52	65·5	66	50	53½	53	se.	s.	calm	ese.	se.	·263	·01	·51	……	……	60
21.	29·696	29·670	29·640	29·14	29·48	29·44	29·67	29·60	65·8	76·3	59·3	73	51	60·5	67	54½	54½	54	sse.	sw.	calm	sw.	se.	·077	·01	·06	……	……	58
○ 22.	29·824	29·767	29·725	29·19	29·57	29·60	29·60	29·69	65·7	83·6	56·8	75	48	65	63½	49½	53	53	w.	w.	w.	ssw.	nw.	·027	·20	·22	0·60	·25	59
23.	29·896	29·823	29·767	29·25	29·63	29·38	29·67	29·50	62·5	78·7	53·0	73	57	63	64	42	52½	52½	w.	w.	calm	s.	se.	·041	·01	·03	……	·12	58
24.	29·732	29·679	29·420	29·05	29·37	29·34	29·30	29·22	62·3	71·6	61·0	72	54	67·5	62	53	59	55½	s.	sw.	w.	s.	s.	·033	……	·03	……	·16	61
25.	29·864	29·806	29·585	29·20	29·40	29·15	29·28	29·12	64·7	71·3	57·3	66	54	62	59	52½	59	53	s.	sw.	w.	sw.	sw.	·033	·09	……	……	……	58
26.	29·748	29·915	29·674	29·05	29·35	29·68	29·23	29·56	62·0	74·0	57·0	70	49	63	60½	49	53½	53½	nwvar	sw.	w.	wsw.	nw.	·019	……	·07	……	·05	52
27.	30·228	30·213	30·143	29·54	29·98	30·10	29·82	30·03	63·3	78·3	53·3	73	44	60	63	51½	54	53	nw.	w.	w.	nw.	nw.	·016	……	……	……	·10	57
28.	30·344	30·274	30·128	29·68	30·00	29·82	30·03	29·72	65·3	83·6	53·0	80	51	64·5	59	47½	58½	52½	w.	sw.	w.	sw.	s.	……	……	……	……	·05	55
29.	30·154	30·078	29·989	29·45	29·80	29·87	29·68	29·76	66·5	79·8	57·8	82	52	66·5	61	53	55	53	w.	w.	w.	wnw.	sw.	……	……	……	1·37	·51	61
☾ 30.	30·006	29·933	29·791	29·40	29·88	29·80	29·85	29·90	64·2	77·3	57·0	71	50	66	63½	47½	54	55	n.	ne.	w.	w.	w.	……	·80	……	……	·06	60
Mean.	30·093	30·043	29·945	29·46	29·910	29·886	29·962	29·966	65·1	76·6	58·3	73·33	49·80	63·7	67·0	48·9	54·76	51·85						Sum. 6·30	1·58	1·05	2·22	2·34	Mean. 59

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

[THIRD SERIES.]

SEPTEMBER 1842.

XXVI. *Chemical Examination of the Fruit of* Menispermum Cocculus (*Semina Cocculi Indici*). *By* William Francis, *Ph. D.*[*]

NOTWITHSTANDING the numerous investigations to which the grains of this plant have been submitted, much doubt still remains respecting the constitution, and even with regard to the existence of some of the many interesting bodies said to occur in them.

Boullay[†], to whom we are indebted for the first examination, found in them a fatty oil, stearine, yellow extractive colouring matter, picrotoxine,—to which he ascribed the properties of an alkaloid,—menispermic acid, vegetable fibre, albumen, and several of the inorganic salts usually contained in plants. They were subsequently investigated by Casaseca[‡], principally with regard to the menispermic acid; he showed that no such acid existed in them, a fact which has been confirmed by all later researches. The same chemist, in conjunction with Lecanu[§], made the fatty bodies which occur in this fruit the subject of a distinct treatise, which I shall hereafter have occasion to notice more fully. Oppermann[||] and quite recently Regnault[¶] have published analyses of picrotoxine.

The most complete memoir on these grains is one published by Peltier and Couerbe[**]. They describe in it two new alkaloids, menispermine and paramenispermine, which are said to occur in the shells, and a new acid, hypopicrotoxinic acid, and they ascribe to picrotoxine acid properties. The manner in

---

[*] Communicated by the Author.

[†] *Bulletin de Pharmacie,* vol. iv.

[‡] Ibid. xii$^{\text{ième}}$ *Année, Fév.* 1826, p. 99.

[§] Ibid. *Janv.* 1826, p. 55.

[||] *Mag. für Pharmacie,* xxxv. p. 233.

[¶] *Ann. de Chimie et de Phys.*, lxviii. p. 157.

[**] *Ann. der Pharm.* B. x. p. 181.

which they arrived at their conclusions has not merely appeared satisfactory to few chemists, but seems rather to have created increased doubt with respect to the true constituents of these grains.

Under these circumstances I was induced to subject this fruit to an entirely fresh analysis: the results of my investigations I shall from time to time communicate, as soon as they are so far advanced as to be fit for publication; in the present memoir I shall treat of the fatty substances which occur so abundantly in the Cocculus grains.

I. *The Fatty Substances—Stearophanic Acid.*

The only paper with which I am acquainted in which these substances are made the subject of investigation, is the one above-mentioned by Casaseca and Lecanu. By treating the coarsely pounded grains with boiling water they obtained as extract a considerable quantity of a fatty matter, impregnated with a green colouring substance which reddened litmus paper: treated with strong boiling alcohol a green solution was obtained possessed of acid properties, and from which on cooling were deposited flocky masses of a neutral fat of a white colour. On evaporating this alcoholic solution they obtained a fat substance, which pressed between bibulous paper afforded a nearly colourless adherent nacreous mass, easily soluble in boiling alcohol, but sparingly in cold, and which melted at 59°. From these properties the authors regarded it as margaric acid.

The mass which had been imbibed by the blotting-paper was extracted with alcohol, which on evaporation left behind an oily substance of a deep green colour, which Casaseca and Lecanu considered to be oleic acid. They conclude therefore from their examination, that margaric and oleic acids occur in an uncombined state in the grains of Cocculus, and moreover a neutral fat, probably analogous to stearine.

From the following experiments, however, it will be seen that this acid, which it is true occurs in large quantity in a free state in these grains, is not margaric acid, but a new acid nearly related to the latter in its constitution, but widely differing from it in its properties; and further, that this acid likewise occurs combined with the oxide of glyceryle, and thus constitutes the neutral fat of Casaseca and Lecanu. For this acid I propose the name of *stearophanic acid*, (from στέαρ and φαίνομαι), on account of its beautiful lustre in the crystallized state, and for the neutral fat that of *stearophanine*.

When the coarsely pounded grains are digested with boiling alcohol (that commonly used for spirit-lamps is sufficiently strong) and the extract concentrated by distilling off the alcohol, on cooling, a cake floating on the surface is obtained, which consists of a deep green-coloured smeary fat matter. If the kernels after having been removed from the outer shells are submitted to a similar treatment, the same fat mass is obtained, only with this difference, that it is no longer green but of a yellowish colour, proving that the green colouring matter is only contained in the outer shells. The fat mass was separated from the remainder of the extract and boiled several times with distilled water, to remove all traces of picrotoxine and other soluble substances. It possessed an acid reaction, and was easily saponified by boiling with a dilute solution of caustic potash. The soap, which was separated by common salt, is hard, of a green colour, and affords after decomposition by an acid a green mass which solidifies on cooling. It is very easily soluble in weak boiling alcohol; on cooling, a portion, which however is still of a yellow colour, crystallizes, the solution remaining green. The solid acid thus prepared, although recrystallized ten or twelve times, could not be obtained white; it always preserved its yellow tint, which was especially evident on its being melted. The filtered alcoholic liquor afforded on evaporation a dark green acid oily liquid, which could not be obtained free either from colouring matter or from solid acid.

If after all the picrotoxine and colouring matter have been removed by treatment three or four times with boiling alcohol, the grains be now acted upon by æther employing the gentle heat of a sand-bath, and the filtered æthereal solution be placed in the cold, a shining white fat crystallizes slowly out of it in arborescent aggregations. It was obtained perfectly pure by dissolving it once or twice in absolute boiling alcohol, which takes up very little of it, and from which it separates on cooling in grains and flocks; it has then a dull white colour and a constant melting-point.

Stearophanic Acid.—The pure fat thus prepared was saponified by a solution of caustic potash until it formed a perfectly clear jelly, and then treated by salt, the solid soap dissolved in much water, and decomposed by hydrochloric acid. It collects on the surface as a colourless oil, which soon solidifies into a white crystalline mass. It was now boiled with distilled water till all the hydrochloric acid was removed, and dissolved in weak warm alcohol and filtered warm. On cooling, the acid separates in small needles, which having been dried by exposure to the air, or by pressing between bibulous paper, possess a strong lustre of mother-of-pearl. Its melting point is constant 68° C.; on cooling it crystallizes in stellate groups strongly resembling some kinds of Wavellite, and has a shining white colour. It may easily be reduced to a fine powder; it is very soluble in warm weak alcohol, from which nearly the whole quantity separates on cooling; the solution has strong acid properties.

The acid obtained in the above manner is the hydrate; the anhydrous acid has, according to the analyses of several of its compounds, the following composition:—

			Calculated for 100.
35 atoms	Carbon	2654·89	78·57
68 ...	Hydrogen	424·30	12·55
3 ...	Oxygen	300·00	8·88
		3379·19	100·00

The composition of the hydrated acid in the state in which it is separated from the salts, and likewise occurs free in the kernels, was determined in the following ultimate analyses:—

I. 0·275 grm. of the hydrated acid gave 0·757 carbonic acid, and 0·312 water.

II. 0·294 grm. of the hydrated acid gave 0·8054 carbonic acid, and 0·337 water.

III. 0·224 grm. of the hydrated acid gave 0·613 carbonic acid, and 0·252 water.

IV. 0·331 grm. of the hydrated acid gave 0·913 carbonic acid, and 0·357 water.

V. 0·242 grm. of the hydrated acid gave 0·667 carbonic acid, and 0·272 water.

In No. I. the combustion was effected by chromate of lead, in the remainder oxide of copper was employed. No. V. is an analysis of the hydrated acid as it occurs uncombined in the grains; it was still coloured somewhat yellow, but was quite crystalline, and had the same melting-point as the perfectly white hydrate.

The above numbers afford in 100 parts, —

	I.	II.	III.	IV.	V.
Carbon	75·71	75·32	75·24	75·84	75·79
Hydrogen...	12·60	12·73	12·50	11·98	12·49
Oxygen......	11·69	11·95	12·26	12·18	11·72
	100·00	100·00	100·00	100·00	100·00

These closely approach the formula $C^{35} H^{70} O^{4}$.

			In 100 parts.
35 atoms	Carbon.........	2654·89	76·04
70 ...	Hydrogen ...	436·78	12·51
4 ...	Oxygen	400·00	11·75
		3491·67	100·00

The acid therefore contains in the state of hydrate 1 atom of water, which is replaced in the salts by one equivalent of base.

Stearophanate of Soda.—This salt was prepared by digesting the pure acid with an excess of carbonate of soda. On exposing it to a gentle heat the carbonic acid is expelled with violent ebullition, and a perfectly clear solution formed, which was evaporated to dryness in the water-bath. The finely powdered mixture was then digested with absolute alcohol, which leaves the excess of carbonate of soda undissolved: a perfectly clear solution is obtained, which however soon solidifies into a gelatinous mass, which, transferred to a filter and dried by exposure to the air or between folds of bibulous paper, leaves behind a crystalline tissue consisting of long prisms, with a strong nacreous lustre.

This compound, when treated with a small quantity of water, forms a stiff jelly; it is decomposed on the addition of much water into an acid crystalline salt, which settles slowly, and imparts to the liquid an opake appearance.

Several stearophanates may be prepared from this salt by double decomposition.

Stearophanate of Silver.—This compound was prepared by decomposing a weak alcoholic solution of the preceding salt by a perfectly neutral solution of the nitrate of silver. The precipitate is very bulky, but it soon settles. The white colour which it at first possesses is only of momentary duration; it acquires a slight tint of brown. Well washed and dried, it can be exposed to light without undergoing apparently any further decomposition. It dissolves easily in a solution of caustic ammonia.

I. 1·134 grm. of the salt, well dried at 100°, left after ignition 0·317 metallic silver, corresponding to 0·3404 oxide of silver.

II. 0·379 grain of the salt gave 0·105 silver, corresponding to 0·1127 of the oxide.

This gives in 100 parts,—

	Calculated.		Obtained.	
			I.	II.
1 atom Oxide of silver. .	1451·61	30·05	30·01	29·73
1 ... of Stearophanic acid	3379·19	69·95	69·99	70·27
	4830·80	100·00	100·00	100·00

On burning with oxide of copper,—

I. 0·543 grm. of the silver salt gave 1·0695 carbonic acid, and 0·433 water.

II. 0·4925 grm. of the salt gave 0·9763 carbonic acid, and 0·393 water.

III. 0·259 grm. of the salt gave 0·205 water, which reduced to 100 parts, gives

	According to theory.	I.	II.	III.
35 at. Carbon.	54·94	54·15	54·51	
68 ... Hydrogen	8·78	8·83	8·86	8·79
3 ... Oxygen	6·23	7·15	6·76	
1 ... Oxide of silver. .	30·05	29·87	29·87	
	100·00	100·00	100·00	

Stearophanate of the Oxide of Æthyle.—This salt is a solid, brownish-white, semi-transparent mass. It was formed by passing a stream of dried muriatic gas for several hours into a warm saturated alcoholic solution of the acid. After some time the æther collects upon the surface as a colourless oily fluid, which solidifies on cooling. A portion still remaining in solution is obtained on the addition of water. To free it from acid it was boiled several times with a dilute solution of the carbonate of soda, and afterwards with water. It melts at 32° C., is very fragile, void of smell in the cold, but on being warmed acquires a slight fruity odour. It melts easily on the tongue, imparting to it a sensation of cold, and has a buttery taste: it is very volatile, but is partially decomposed on distillation. It is decomposed by potash into the stearophanate of potash and alcohol.

The composition was ascertained in the following ultimate analyses:—

I. 0·381 grm. of the æther gave 1·0668 carbonic acid, and 0·436 water.

II. 0·247 grm. gave 0·6925 carbonic acid, and 0·286 water.

No. I. analysis was made with oxide of copper. For No. II. I am indebted to the kindness of Dr. Lawrence Smith; it was made with oxide of copper and chlorate of potash.

	I.	II.
Carbon . .	77·01	77·09
Hydrogen.	12·69	12·85
Oxygen . .	10·30	10·06

which agree with the formula $C^4 H^{10} O + C^{35} H^{68} O^3$.

39 Carbon . .	2958·40	77·11
78 Hydrogen.	491·70	12·51
4 Oxygen . .	400·00	10·38
	3850·10	

Stearophanine.—The mode of preparation has been described above. When the fat is extracted by pressing the grains between hot plates, or by means of boiling water, it is always contaminated by the free acids, colouring matter, &c., from which it separates with great difficulty. If, on the contrary, the grains are first digested several times with moderately strong alcohol, all the substances which would otherwise be taken up by the æther are removed, and the fat alone remains. In warm æther it is very easily soluble, from which it crystallizes on cooling in dendritic aggregations. It does not crystallize from alcohol, in which it is but sparingly soluble, but separates as a white powder. When perfectly pure it melts at from 35° to 36° C., does not crystallize on cooling, but shrinks together, forming a wave-like rough surface; it cannot be reduced to a powder, and strongly resembles wax. It does not saponify easily on being boiled with dilute solution of potash, but immediately when melted with potash and a small quantity of water. It then affords, when boiled with water, a clear solution, from which acids separate the stearophanic acid.

When subjected to dry distillation it afforded acroleine, a solid fat acid body, and a liquid product, but no sebacic acid; it therefore contains glycerine, but is free from oleine.

Before burning it with oxide of copper it was kept for some time in the water-bath, to free it from adhering traces of alcohol. The following numbers were obtained:—

I. 0·329 grm. of stearophanine gave 0·919 carbonic acid, and 0·361 water.

II. 0·231 grm. gave 0·645 carbonic acid, and 0·257 water.

III. 0·213 grm. gave 0·236 water.

In 100 we have

	I.	II.	III.
Carbon . .	76·81	76·69	
Hydrogen.	12·19	12·36	12·30
Oxygen . .	11·10	10·95	

which agrees with the formula $C^{38} H^{72} O^4$.

38 atoms	Carbon. . .	2882·45	77·24
72 ...	Hydrogen . .	449·25	12·04
4 ...	Oxygen. . .	400·00	10·72

In the present case the same formula must be admitted for the constitution of glycerine as was proposed by Mr. Stenhouse in his memoir on Palmitine*, and which has likewise been adopted by M. Marsson for that occurring in combination with Laurostearic acid in the bay berries†. According to the above analyses, stearophanine consists of

1 atom of Stearophanic acid = $C^{35} H^{68} O^3$
1 ... Glycerine. . . . . = $\underline{C^3 H^4 O}$
1 ... Stearophanine . . = $C^{38} H^{72} O^4$

However similar the constitution of stearophanic acid may appear to that of margaric acid, there cannot be the least doubt

---

[*] Philosophical Magazine, S. 3. vol. xviii. p. 190.

[†] *Annalen der Chem. und Pharm.*, vol. xli. p. 329; see also the present Number, pp. 237, 238.

as to their distinctness: margaric acid melts at 60° C., the margarate of the oxide of æthyle at 22°; stearophanic acid, on the other hand, has its melting-point at 68° C., and its compound with æther at 32°. But this is more effectually proved by the splendid crystallization of the acid and of its soda salt. When compared with the very numerous preparations of fats and their salts in the collections of the Giessen laboratory, they surpassed all in lustre and beauty, and by the well-defined form of the crystals of the soda salt.

As above stated, the acid occurs in a free state in the grains, but only in small quantity, the greater part consists of the oily acid; it probably varies according to the time the grains have been preserved, as is the case with palmitic acid. On the whole the fatty substances may probably constitute 15 per cent. of the grains, of which about a third would consist of the neutral fat. Wittstock obtained 11·2 per cent. of oily matter by pressing the grains between hot plates.

A portion of the oily mass was subjected to dry distillation, and the products boiled with water, from which on cooling a large quantity of sebacic acid was deposited in beautiful needles with a nacrous lustre, and at the same time another fat acid separated on the surface, which was probably margaric acid. This experiment proved the oily fat and oily acid which occur together with that above described in the fruit of *Menispermum Cocculus*, to be oleine and oleic acid, since, according to Redtenbacher, these alone afford sebacic acid on dry distillation.

The colouring matter which is peculiar to the shells could not be obtained in a state fit for analysis.

XXVII. *Contributions to the Minute Anatomy of Animals.* By GEORGE GULLIVER, *F.R.S., &c. &c.*—No. III*.

On the Pus-like Globules of the Blood.

IN the Philosophical Magazine for September 1838, (S. 3, vol. xiii., p. 193) I have described the frequent occurrence of globules of pus in the blood of persons affected with various severe inflammatory and suppurative diseases, and have since shown how the pale globules of the blood of healthy mammalia and birds differ from the lymph-globules of the same animals (Gerber's Anatomy, p. 83 and 84; Appendix to the same, p. 19; and Philosophical Magazine for June, 1842). In the present communication the globules first mentioned will be compared with the pale globules now so well known as belonging to healthy blood.

---

* Communicated by the Author. No. II. will be found in our last Number, p. 107.

In some of my earlier observations these two varieties of globules were doubtless confounded; and their similarity is often so close, that it may well be questioned whether there is any essential difference between them in many cases, although it is difficult to avoid attributing to the effects of disease the unusual abundance of pus-like globules in the blood of patients labouring under numerous inflammatory distempers.

But it often happens that the pale globules appearing in diseased blood are manifestly different from those found in the blood during health. The former are generally rather larger, more irregular in size and form, and not uncommonly more opake than the latter. The globules occurring in disease too often appear to be tinged, especially when examined by lamp-light, of a red colour, like the blood-corpuscles described by Dr. Barry as in progress of change into pus-globules.

Case 1.—A mare, aged 19, was lame of the hind-leg, which in three days became prodigiously swoln; there were many purulent deposits beneath the integuments, and she had much fever. Some blood, from the facial vein, was now examined, and found to contain an unusual number of pus-like globules, (fig. 1. A.) the average diameter of which was about $\frac{1}{2666}$th of an English inch. They occurred for the most part singly, and occasionally in clumps. When treated with dilute acetic acid the globules exhibited nuclei, generally central but sometimes attached to the circumference; and the smaller particles or molecules (the disc-like objects of Dr. Barry), of which the nuclei were composed were either closely connected together or separated by minutely granular matter (fig. 1. B.). On the fifth day, when the disease had increased, some blood from a cutaneous vein of the affected limb contained about half as many pus-like globules as red discs; the former were most commonly in clusters, and darker-coloured than they were two days before.

The pale globules in the blood of a healthy mare, examined at the same time for comparison, were by no means so numerous; they were more regular in size and shape, almost all between $\frac{1}{3500}$th and $\frac{1}{2900}$th of an inch in diameter; when subjected to the action of dilute acetic acid they presented a nucleus, the molecules of which were closely aggregated together; the globules appeared rather paler than those of the diseased blood, and were rarely to be seen in clumps (fig. 1. C.).

Case 2.—A gelding, aged 8, had the disease termed by veterinarians laminitis, that is to say, inflammation of the vascular laminæ of the corion beneath the crust of the hoof. The disease was violent; relief was attempted by abstracting blood from the brachial vein, which became inflamed, and the animal soon afterwards died. In the blood there was a vast number of pale globules resembling pus, (fig. 2, A.) besides others of a reddish colour. The latter corpuscles (fig. 2, B.) appeared to be composed of very delicate pale envelopes including from one to four blood-discs, rarely five or six, some of which were altered in shape, while others presented nearly their usual size and contour. They were not spherical, as some of them appear to be in the figure. The envelopes, which seemed at first like shadows, were distinct enough in different lights, even after the addition of water and dilute acetic acid, and were rendered very obvious by the action of tincture of iodine.

Fig. 1.

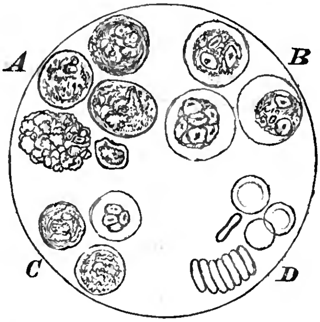


Fig. 2.

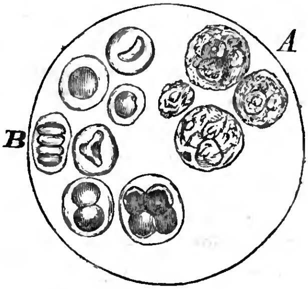


Fig. 1. Globules mentioned in case 1. A, pus-like globules of blood from the facial vein; two of them are round, another is rather oval, a fourth is made up of aggregated granules, and the remaining one is much smaller and more shapeless than the others. B, the same globules treated with dilute acetic acid. C, pale globules from the blood of a healthy mare, in one of which the nucleus is shown by dilute acetic acid. D, blood-discs or unchanged red particles from the same animal for comparison.

Fig. 2. Corpuscles described in case 2. A, pus-like globules of blood from the digital vein, as they appeared without addition. B, reddish corpuscles, of which seven are here depicted, from the same blood; four of them contain what appear to be single blood-discs, three of which are variously misshapen; of the three other corpuscles one includes two discs seen on their flat surfaces and touching merely at the margins, another has four slightly overlapping at the edges, and the remaining one incloses a pile of similar discs seen on their edges and with their flat surfaces together. Compare these discs with the unchanged red particles at D in fig. 1.

All the objects in both figures are magnified exactly to the same degree, namely, about 800 diameters. Compare the nuclei at B and C in fig. 1, with those of the lymph and chyle globules, which I have depicted in Gerber's Anatomy.

Structure of Fibrine.

In the section on this subject in the last Number of the Philosophical Magazine, p. 109–111, it should have been stated that in many fibrinous exudations or false membranes, resulting from inflammation, the structure is the same as that of fibrine, coagulated either after removal from the body or within the circulating channels simply from death. In false membranes the fibrils are often very distinct: they form a delicate net-work, which incloses exudation corpuscles, much resembling the organic germs before described in pale clots of fibrine formed without inflammatory action. As these fibrils in both instances appear to be formed in the act of coagulation, it would require some modification of or departure from the theory of M. Schwann to explain their origin.

Tubercle.

It has long been a question whether tubercular matter in the lungs be situated in the cellular (filamentous) tissue outside the air-cells, or at the surface of the mucous membrane within these cells. It may be merely mentioned that I have clearly detected tubercular deposit in the latter situation; and that Dr. Willis, in his forthcoming English version of Prof. Wagner's Physiology, will give an engraving of tubercular matter within the air-cells. This of course will not decide what is always the case in tubercular consumption; but it seems to be a fact of interest in regard to the precise seat of tubercle of the lungs.

XXVIII. *On the Preparation of Quina and Cinchonia.*

By M. F. C. Calvert, *Préparateur du Cours de Chimie appliquée au Jardin des Plantes à Paris.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Allow me, through the medium of your widely-circulated Journal, to make public a new chemical fact discovered by me relating to the extraction of quina and cinchonia from cinchona bark, by the knowledge of which, I believe, the process usually followed by the manufacturers of these alkaloids may be considerably improved.

In order to obviate some of the difficulties which have hitherto been experienced in extracting the alkaline bases of cinchonia, it appeared to me desirable to discover a process by which, from a certain quantity of cinchona bark, all the quina and cinchonia contained in it might be extracted. In the French manufactories, and probably also in the English, the same quantity of these bases has never been extracted with any regularity from equal weights of cinchona even of similar quality: this irregularity will, I think, admit of easy explanation from the fact which I have ascertained, that quina is very soluble in lime water and in the solution of chloride of calcium; hence, when lime is employed to precipitate those bases from their solution in the hydrochloric acid, which is used to extract them from cinchona bark, a part of the quina is re-dissolved, especially should the lime be added in excess even in the smallest quantity. It is true that the re-solution of the quina depends in great measure on an excess of lime being added; but at the same time it must be admitted, that even should the greatest care be taken by the manufacturer to guard against adding an excess of lime, it would be impossible wholly to prevent the solution of some of the quina, as chloride of calcium will inevitably be formed, and consequently a part of the quina will be dissolved in it.

Considering that such must be the unavoidable result of the process usually followed, and reflecting on the serious, if not insurmountable obstacle which the re-solution offers to the economical manufacture of those important articles, I was led to inquire by experiments whether some other and less objectionable means could be discovered of precipitating those substances.

I first experimented with solutions of caustic ammonia and potash, and soon found that the use of these alkalies was liable to the same objection as that of the chloride of calcium and hydrate of lime, viz. of dissolving a portion of the quina when added in excess. But the result was found to be very different when a solution of caustic soda was employed, as this alkali, even when added in excess, dissolves neither quina nor cinchonia. Of this insolubility I satisfied myself by the following experiment.

I precipitated a mixed solution of the sulphates of quina and cinchonia by caustic soda, and afterwards filtered it; the filtered liquor was next divided in two equal parts, one of these treated for the purpose of ascertaining whether any quina had been re-dissolved by the soda; with this object in view, I saturated the excess of alkali with hydrochloric acid, and then poured chlorine into the neutral solution and afterwards ammonia. It is well known that if there had been a trace of quina or any one of its salts in the solution, a green colour would have been produced[*]; but in my experiment not the slightest colour was observed.

---

[*] Vide *Journal Hebdomadaire de Pharmacie* (vol. xxii. p. 37). Published by M. Adrien of Lyons.

I repeated this experiment several times both with the sulphuric and hydrochloric acids, and the result being always similar, I concluded that all the quina had been precipitated and none re-dissolved.

To the other portion of the filtered and alkaline liquor, after having saturated it with hydrochloric acid, I applied chloride of lime, which is a very sensible test of the presence of cinchonia (as will be made to appear presently), and having obtained no precipitate, I felt satisfied from this experiment that no particle either of quina or cinchonia had been re-dissolved: I therefore concluded, from the result of these experiments, that the process of extracting those two vegeto-alkalies by lime is imperfect, and I propose, instead of employing hydrate of lime for the purpose of precipitating the alkaline bases of cinchonia, that caustic soda should be used, because, by employing it, all the cinchonia, and especially the quina, which may be contained in the acid liquors, will certainly be precipitated;—an object of great importance to those who are engaged in this branch of manufacture.

I afterwards endeavoured to discover a method by which the quantity of cinchonia contained in sulphate of quina might be easily ascertained, as the adulteration of the latter by the addition of the former is a fraud frequently practised in commerce, and one which is with difficulty detected by the chemical means usually applied. For this purpose it has been considered necessary to have recourse to a complicated analysis, especially should it be wished to ascertain the exact extent of the adulteration. As in many works on chemistry it is directed to treat the solution of those salts with an alkali, by which their bases are precipitated, to wash the precipitate, and then treat it with æther, which dissolves the quina and not the cinchonia, I must here take the liberty of remarking, that should such an analysis be undertaken, it will be necessary to guard against using ammonia or potash, as a small excess of these alkalies will re-dissolve a part of the quina; but, on the contrary, by employing soda this source of error is avoided, no quina being re-dissolved.

It is true that the end proposed can be compassed by following the directions indicated in many works on chemistry, and using the precautions recommended in the concluding part of the last paragraph. But it appeared to me that it would be advantageous to employ tests by which the fraud in question could be more easily discovered, and I succeeded by the application of the six following reagents, and especially the chloride of lime.

I saturated two portions of cold water, one with very pure sulphate of quina, and the other with very pure sulphate of cinchonia; I found that 10 grammes of water contained 0·033 of sulphate of quina, and that the same quantity of water contained 0·165 of the sulphate of cinchonia, or five times the proportion of sulphate of quina; therefore, in order to act on the same quantities of each salt dissolved in the same quantity of water, I took 10 grammes of the solution of sulphate of quina, or 0·033, and only 2 grammes of the saturated solution of sulphate of cinchonia, and to this latter solution I added 8 grammes of water, and thus in both cases I acted on 0·033 of solid salt in 10 grammes of water.

1st. The solution of the sulphate of quina gave a precipitate with chloride of lime, which was immediately re-dissolved by the addition of an excess of the reagent.

The solution of the sulphate of cinchonia, on the contrary, gave a precipitate which was not re-dissolved on the addition of even a large excess of the reagent.

I afterwards mixed the solutions of the sulphates of quina and cinchonia in equal quantities, and poured into the mixture chloride of lime; a precipitate was formed, of which one half was re-dissolved on the reagent being added in excess; the precipitate which was re-dissolved was quina: hence it appears, that sulphate of quina, mixed with an equal quantity of sulphate of cinchonia, could be separated from it, and the quantity of cinchonia ascertained.

I next experimented on a mixture containing two parts of sulphate of quina and one of sulphate of cinchonia, and a similar result was obtained; that is, a precipitate was produced on the addition of chloride of lime, a portion of which, equal to the proportion of sulphate of quina, was again re-dissolved on the reagent being added in excess.

When the small quantity of sulphate of cinchonia upon which I experimented is considered, it will be perceived how easily, by means of these reagents, any adulteration of the sulphate of quina by sulphate of cinchonia may be detected, and the smallest quantity of the substance discovered, as quina has no influence on the result of the experiment, provided the liquor is sufficiently diluted to guard against the precipitation of sulphate of lime.

In applying the tests the greatest care was taken to prevent the precipitation of the sulphate of lime; and the best proof that can be adduced of this source of error having been avoided was, that if the precipitate had been sulphate of lime, it would not have disappeared in the experiment made with the pure solution of sulphate of quina, and remained in that of the sulphate of cinchonia.

The following experiment will demonstrate in a manner perhaps still more evident, the non-formation of the sulphate of lime, and will besides show how very sensible a test the chloride of lime is to detect the presence of cinchonia.

I took 2 grammes of the solution of sulphate of cinchonia, containing 0·033, and diluted it with 48 grammes of water, and had therefore 33 parts of this substance diffused in 50,000 parts of water; to this solution I added a little chloride of lime, and obtained a precipitate of cinchonia, whereby the sensibility of the chloride of lime, as a test of cinchonia, as well as the non-precipitation of sulphate of lime, was demonstrated; for the sulphate of lime formed in the experiment must have dissolved in the very dilute solution of cinchonia which was employed, and consequently the precipitate which appeared could not be attributed to it.

The experiment was tried with even double the quantity of water, that is, with 33 parts of sulphate of cinchonia to 100,000 parts of water; but in this case the precipitate was scarcely perceptible.

2nd. The chloride of calcium does not precipitate a sulphate of quina, but it produces a precipitate with a sulphate of cinchonia.

3rd. The sulphate of quina gives a precipitate with lime-water, but it disappears by an excess of the reagent being added; while, on the other hand, the sulphate of cinchonia gives a precipitate which remains even on the addition of an excess of the reagent.

4th. The sulphate of quina gives a precipitate with ammonia, which disappears on the addition of it in excess; whereas, in the case of the sulphate of cinchonia, a precipitate is produced which does not disappear on adding a large excess of ammonia.

5th. The carbonate of ammonia acts in precisely the same way as ammonia.

6th. With potash, a precipitate is produced with sulphate of quina, but it re-dissolves almost entirely when the potash is added in excess; while with a sulphate of cinchonia it yields a curdy-white precipitate, which is insoluble in an excess of the reagent.

7th. Soda precipitates the bases of both these salts, and the precipitate does not re-dissolve on the addition of an excess; there is, however, this difference between the precipitate from these two salts; that from the sulphate of quina is pulverulent, while that from the sulphate of cinchonia is curdy-white.

By means of the first six tests, it will always be easy to distinguish between quina and cinchonia, and, judging from the results of my experiments, a mixture of those two salts can be detected; but chloride of lime in particular is the most sensible test of the presence of cinchonia, and it therefore is the reagent which should be employed, when this base is mixed in small proportion with quina or any of its salts. The results obtained by the use of the seven above-mentioned tests fully confirm all that has been said in speaking of the extraction of these alkaloids, and of their quantitative analysis when mixed together.

The sulphate of quina treated with the chloride of platinum gives a white pulverulent precipitate. The sulphate of quina, treated with the same reagent, gives a curdy-white precipitate.

The sulphate of quina, treated by the red ferro-cyanide of potassium, gives a precipitate which disappears in an excess of the reagent; the liquor assumes a greenish-brown colour, and ammonia does not change it nor produce any precipitate.

The sulphate of cinchonia, submitted to the same reagent, gives a precipitate less deeply coloured than the preceding; it is equally soluble in an excess, but ammonia re-produces the precipitate and destroys the colour in great part.

I have likewise performed experiments with the following substances, namely, bichloride of mercury, chloride of nickel and cobalt, the iodide of potassium, and solution of iodine, but they offer no distinctive characters.

I will only repeat in conclusion, first, that in the preparation of quina and cinchonia, lime should be replaced by soda: carbonate of potash or soda may be employed, but they have the inconvenience of dissolving part of the cinchonia; secondly, that in case of a quantitative analysis being undertaken, the same alkali alone should be employed to precipitate those bodies; and thirdly, that in case of sulphate of quina being supposed to be adulterated with the sulphate of cinchonia, and that it may be wished to ascertain the extent of adulteration, the tests upon which reliance can be placed, are, first, chloride of lime; secondly, chloride of calcium; thirdly, lime-water; and fourthly, ammonia and carbonate of ammonia.

XXIX. *On a Theorem in Analytic Geometry.* *By* James Booth, *Esq., LL.D., M.R.I.A.*[*]

IT has been justly remarked by an author who has himself largely contributed to the advance of mathematical science,

---

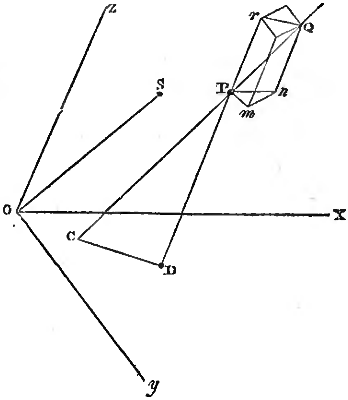
[*] Communicated by the Author.

"qu'on sert peut-être plus encore la science en simplifiant, de la sorte, des théories déjà connues, qu'en l'enrichissant de théories nouvelles, et c'est là un sujet auquel on ne saurait s'appliquer avec trop de soin."—*Annales de Mathématiques*, tom. xix. p. 338.

Extending this remark to the simplification of the methods of establishing theorems already known, and remarkable for their difficulty, I am induced to give an exceedingly simple demonstration of a theorem, which may be found at p. 342 of Dupin's *Développements de Géométrie*, where the accomplished author bestows more than four quarto pages of analytical calculation of extreme complexity on this theorem, and yet leaves its solution incomplete.

The following is the theorem to which I allude:—

Three points assumed on a right line are always retained in three fixed planes, any fourth point P *in this right line will describe an ellipsoid, whose centre is the common intersection of the three fixed planes.*



Let O x, O y, O z, be the intersections of the three fixed planes, O x, O y, O z being the axes of coordinates, and C P the moving right line in any position, meeting the plane of O $x\,y$ in the assumed point C; let the distances of P to the points in the planes of $x\,y$, $y\,z$, $z\,x$ be c, a, b; and let the angles between the axes of x and y, $y\,z$, and $z\,x$ be ν, λ, μ; through P let three right lines be drawn P m, P n, P r, parallel to the lines O x, O y, O z; in the line P C assume the point Q, so that P Q $= \delta$, and complete the parallelopiped of which P Q is the diagonal; let the sides of this parallelopiped parallel to the axes O x, O y, O z be α, β, γ, then we shall have by a well-known theorem, given in most elementary works on the subject*, which expresses the relation between the diagonal sides and contained angles of a parallelopiped.

$$\alpha^2 + \beta^2 + \gamma^2 + 2\,\beta\,\gamma\cos\lambda + 2\,\alpha\,\gamma\cos\mu + 2\,\alpha\,\beta\cos\nu = \delta^2;$$

or dividing by δ^2,

$$\frac{\alpha^2}{\delta^2} + \frac{\beta^2}{\delta^2} + \frac{\gamma^2}{\delta^2} + 2\frac{\beta\,\gamma}{\delta^2}\cos\lambda + 2\frac{\alpha\,\gamma}{\delta^2}\cos\mu + 2\frac{\alpha\,\beta}{\delta^2}\cos\nu = 1 \quad (1.)$$

---

* See Legendre's Geometry, p. 249 (Brewster's Edition).

Now the triangles P C D, P Q r are similar, hence

$$\mathrm{PD}:\mathrm{PC}::\mathrm{P}r:\mathrm{PQ}, \text{ or } \mathrm{Z}:\mathrm{C}::\gamma:\delta, \text{ hence}$$

$$\frac{z}{c}=\frac{\gamma}{\delta}; \text{ in like manner } \frac{x}{a}=\frac{\alpha}{\delta}, \ \frac{y}{b}=\frac{\beta}{\delta};$$

making these substitutions in (1.), we find

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}+2\frac{y\,z}{b\,c}\cos\lambda+2\frac{x\,z}{a\,c}\cos\mu+2\frac{x\,y}{a\,b}\cos\nu=1 \quad (2.)$$

The equation of an ellipsoid whose centre is at the origin when the coordinate planes are rectangular, the equation becomes simply

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=1.$$

It follows immediately from (2.) that the coordinate planes can never be conjugate planes of the surface, except when rectangular, as in no other case do the rectangles vanish.

To find the coordinates of the point where the tangent plane is parallel to one of the coordinate planes, that of $x\,y$. Suppose V = 0, being the equation of the surface, the general equation of the tangent plane is

$$\frac{d\,\mathrm{V}}{d\,x}(x-x')+\frac{d\,\mathrm{V}}{d\,y}(y-y')+\frac{d\,\mathrm{V}}{d\,z}(z-z')=0;$$

and when the tangent plane is parallel to that of $x\,y$,

$$\frac{d\,\mathrm{V}}{d\,x}=0, \ \frac{d\,\mathrm{V}}{d\,y}=0.$$

Now

$$\frac{d\,\mathrm{V}}{d\,x}=\frac{x}{a}+\frac{y}{b}\cos\nu+\frac{z}{c}\cos\mu=0,$$

$$\frac{d\,\mathrm{V}}{d\,y}=\frac{y}{b}+\frac{x}{a}\cos\nu+\frac{z}{c}\cos\mu=0;$$

from these equations, finding the values of x and y in terms of z, and substituting in (2.), there results

$$z^2\left\{\frac{1-\cos^2\lambda-\cos^2\mu-\cos^2\nu+2\cos\lambda\cos\mu\cos\nu}{\sin^2\nu}\right\}=c^2;$$

the expression within the brackets is the square of the sine of the angle which the axis of z makes with the plane of $x\,y$, calling this angle ϕ, $z=\dfrac{c}{\sin\phi}$; now this value of z is evidently a conjugate diameter to the plane of $x\,y$, since the tangent plane is parallel to the plane of $x\,y$; hence whenever the generating line is perpendicular to one of the coordinate planes, the line drawn from the centre to the point where this line intersects the surface is a conjugate diameter to this plane, a result which might be obtained from geometrical considerations.

We may, as a simple consequence from the preceding demonstration, obtain a theorem in spherical trigonometry apparently new.

Let a, b, c be the sides of a spherical triangle, and P'', P', P *the arcs of three great circles drawn from the vertices* A, B, C *of the spherical triangle, through a point* S *assumed on the surface of the sphere to the opposite sides,* p'' p' p *the segments of those arcs between the point* S *and the sides* a, b, c, we shall have

$$\left[\frac{\sin p}{\sin P} + \frac{\sin p'}{\sin P'} + \frac{\sin p''}{\sin P''}\right]^2 = 1$$

$$+ 2\,\frac{\sin p \sin p' \sin p''}{\sin P \sin P' \sin P''}\left[\frac{\sin P}{\sin p}(1-\cos c) + \frac{\sin P'}{\sin p'}(1-\cos b) + \frac{\sin P''}{\sin p''}(1-\cos a)\right]$$

To show this, through the point O let a right line be drawn parallel to P Q, meeting the surface of the sphere in S, and let the sides of the spherical triangle, opposite the angles λ, μ, ν, be a, b, c; then in the triangle P C D : P D : P C :: sin P C D : sin P D C : sin p : sin P.

Since P D is parallel to O Z, and P C parallel to O S, hence $\frac{z}{c} = \frac{\sin p}{\sin P}$.

Similarly, $\frac{x}{a} = \frac{\sin p''}{\sin P''}$, $\frac{y}{b} = \frac{\sin p'}{\sin P'}$; making these substitutions in (2.), after some obvious simplifications we find

$$\left[\frac{\sin p}{\sin P} + \frac{\sin p'}{\sin P'} + \frac{\sin p''}{\sin P''}\right]^2 = 1$$

$$+ 2\,\frac{\sin p \sin p' \sin p''}{\sin P \sin P' \sin P''}\left[\frac{\sin P}{\sin p}(1-\cos c) + \frac{\sin P'}{\sin p'}(1-\cos b) + \frac{\sin P''}{\sin p''}(1-\cos a)\right]$$

When the triangle becomes plane, the sines are changed into the corresponding arcs, and cos a, cos b, cos c are each equal to unity, and we thus derive the known theorem in plane geometry,

$$\frac{p}{P} + \frac{p'}{P'} + \frac{p''}{P''} = 1.$$

XXX. *Notes on the Effects produced by the Ancient Glaciers of Caernarvonshire, and on the Boulders transported by Floating Ice.* *By* Charles Darwin, *Esq., M.A., F.R.S. and F.G.S.*

GUIDED and taught by the abstract of Dr. Buckland's memoir "On Diluvio-Glacial Phænomena in Snowdonia and the adjacent parts of North Wales*," I visited several of the localities there noticed, and having familiarized myself with some of the appearances described, I have been enabled to make a few additional observations.

Dr. Buckland has stated that a mile east of Lake Ogwyn there occurs a series of mounds, covered with hundreds of large blocks of stone, which approach nearer to the condition of an undisturbed moraine, than any other mounds of detritus noticed by him in North Wales. By ascending these mounds it is indeed easy to imagine that they formed the north-western lateral moraine of a glacier, descending in a north-east line from the Great Glyder mountain. But at the southern end of Lake Idwell the phænomena of moraines are presented, though on a much smaller scale, with perfect distinctness. On entering the wild amphitheatre in which Lake Idwell lies, some small conical, irregular little mounds, which might easily escape attention, may be seen at the further end. The best preserved mounds lie on the west side of the great black perpendicular face of rock, forming the southern boundary of the lake. They have been intersected in many places by streams, and they are seen to consist of earth and detritus, with great blocks of rock on their summits. They at first appear quite irregularly grouped, but to a person ascending any one of those furthest from the precipice, they are at once seen to fall into three (with traces of a fourth) narrow straight linear ridges. The ridge nearest the precipice runs someway up the mountain, but the outer one is longer and more perfect, and forms a trough with the mountain-side, from 10 to 15 feet deep. On the eastern and opposite side of the head of the lake, corresponding but less developed mounds of detritus may be seen running a little way up the mountain. It is, I think, impossible for any one who has read the descriptions of the moraines bordering the existing glaciers in the Alps, to stand on these mounds and for an instant to doubt that they are ancient moraines; nor is it possible to conceive any other cause which could have abruptly thrown up these long narrow steep mounds of unstratified detritus against the mountain-sides. The three

---

* Read before the Geological Society, December 15th, 1841, and the Abstract is published in the Athenæum, 1842, p. 42. [An Abstract of Dr. Buckland's paper, from the Proceedings of the Society, will appear in an early number of the Philosophical Magazine.—Edit.]

or four linear ridges evidently mark the principal stages in the retreat of the glacier; the outer one is the longest, and diverges most from the great wall of rock at the south end of the lake. The inner lines distinctly define the boundary of the glacier during the last stage of its existence. At this period a small and distinct glacier descended from a narrow but lofty gorge on the north-western end of the lake; and here remnants of a terminal moraine may be traced in the little mounds, forming a broken semicircle round a rushy plain, scarcely more than a hundred yards in diameter. The rocks are smoothed, mammillated and scored, all round the lake, and at some little depth beneath the surface of the water, as I could both see and feel. Similar marks occur at great heights on all sides, far above the limits of the moraines just described, and were produced at the time when the ice poured in a vast stream over the rocky barrier bounding the northern end of the amphitheatre of Lake Idwell.

I may here mention, that about eighty yards west of the spot where the river escapes from the lake, through a low mound of detritus, probably once a terminal moraine, there is an example of a boulder broken, as described by Charpentier and Agassiz, into pieces, from falling through a crevice in the ice. The boulder now consists of four great tabular masses, two of which rest on their edges, and two have partly fallen over against a neighbouring boulder. From the distance, though small in itself, at which the four pieces are separated from each other, they must have been pitched into their present position with great force; and as the two upright thin tabular pieces are placed transversely to the gentle slope on which they stand, it is scarcely possible to conceive that they could have been rolled down from the mountain behind them; one is led, therefore, to conclude that they were dropped nearly vertically from a height into their present places.

The rocky and steep barrier over which the ice from the amphitheatre of Lake Idwell flowed into the valley of Nant-Francon, presents from its summit to its very foot (between 400 and 500 feet) the most striking examples of boss or dome-formed rocks; so much so, that they might have served as models for some of the plates in Agassiz's work on Glaciers. When two of the bosses stand near and are separated only by a little gorge, their steep rounded sides are generally distinctly scored with lines, slightly dipping towards the great valley in front. The summit of the bosses is comparatively seldom scored; but on one close to the bridge over the river Ogwyn, I remarked some singular zigzag scores. At this spot the cleavage of the slate is highly inclined, and owing apparently to the different degrees of hardness of the laminæ, smooth and gentle furrows have been produced by the grinding of the ice, transversely to the scores, and to the probable course of the glacier. Here, as well as in some few other places, I noticed an appearance which made it vividly clear that these bosses had been formed by some process quite different from ordinary aqueous or atmospheric erosion; it is the abrupt projection from the smooth surface of a boss of a piece of rock a few yards square, and one or two feet in height, with its surface smoothed and scored like the boss on which it stands, but with its sides jagged: if a statuary were to cut a small figure out of a larger one, the abrupt projecting portions, before he quite completed his work, might be compared to these masses of rock: how it comes that the glacier, in grinding down a boss to a smaller size, should ever leave a small portion apparently untouched, I do not understand.

On the summit of some of the bosses on this barrier there are perched boulders: but this phænomenon is seen far more strikingly close to Capel-Curig, where almost every dome of rock south of the Inn is surmounted by one or more large angular masses of foreign rock. The contrast between the rude form of these blocks, and the smooth mammillated domes on which they rest, struck me as one of the most remarkable effects produced by the passage of the glaciers. On the sides of the mountains above Capel-Curig, I observed some boulders left sticking on very narrow shelves of rocks, and other boulders of vast size scattered in groups. The largest boulder I noticed there was about 26 feet in length by 12 in breadth, and buried to an unknown thickness.

Proceeding down the great straight valley of Nant-Francon, which must formerly have conveyed the united glaciers from Lakes Idwell and Ogwyn, we continue to meet with boss-formed rocks till below the village of Bethesda. From this point towards Bangor these boss-formed rocks become rare; at least it is certain that a large number of hummocks of rock with rugged surfaces project, whereas higher up in this valley, and in all the great central valleys of Snowdonia, such unground hummocks are not to be met with. At Bethesda, unstratified masses of whitish earth, from ten to forty feet in thickness, full of boulders mostly rounded, but some angular, from one to four feet square, are first met with. This deposit is interesting from the boulders being deeply scored, like the rocks *in situ* over which a glacier has passed. The scores are sometimes irregular and crooked, but generally quite parallel, as I distinctly saw over the entire side of one large block. Some of the blocks were scored only on one side, others on two sides, but from the difficulty of turning over the larger ones, I do not know which case is most common. I saw one large block on which the scores on the opposite sides were all parallel; and another irregularly conical one, four feet in length, of which three-fourths of the circumference was marked with parallel striæ, converging towards the apex. In the smaller *elongated* blocks, from six to twelve inches in diameter, I observed that the striæ were generally, if not always, parallel to their longer axes, which shows that when subjected to the abrading force, they arranged themselves in lines of least resistance. Out of three large blocks which remained imbedded in a perpendicular cliff, the vertical sides of two were scored in horizontal lines, and of the third in an oblique direction. These several facts, especially the parallel striæ on the upper and lower surfaces, show that the boulders were not scored on the spot where they are now imbedded, as seems to have been the case with the boulders described by Mr Maclaren* in the till near Edinburgh. The contrast is very striking in the state of the surface of these boulders, and those which lie scattered high up on the sides of the adjoining hills and of the great central valleys, or are perched on the worn bosses of naked rock; such boulders, as I particularly noticed, present no signs of scores or striæ, as might have been anticipated, if, as is supposed, they were transported on the surface of the glaciers. In the quarries which I examined, namely, below Bethesda, and at some little height on the eastern side of the village, the till rested on slate-rocks, not worn into bosses. I found, however, a rather smooth pap of greenstone marked with a few deep scores. The till forms, at the height probably of 600 feet above the sea, a little plain, sloping seaward; and between Bethesda and Bangor, there are other gently inclined surfaces composed of till and stratified gravel. Considering these facts, together with the proofs of recent elevation of this coast, hereafter to be mentioned, I cannot doubt that this till was accumulated in a sloping sheet beneath the waters of the sea. In composition it resembles some of the beds of till in Tierra del Fuego, which have undoubtedly had this origin. I presume the scored, rounded, and striated boulders were pushed, in the form of a terminal moraine, into the sea, by the great glacier which descended Nant-Francon.

Mr. Trimmer† reports, on the *authority of some workmen*,

---

* Geology of Fife and the Lothians, p. 212.

† Proceedings of the Geological Society, vol. i. p. 332, or Phil. Mag. S. 2. vol. x. p. 143. Mr. Trimmer was one of the earliest observers of the scores and other marks on the rocks of North Wales. He has also remarked that "some of the larger blocks amid the gravel have deep scratches upon their surface." Mr. Trimmer himself found broken sea-shells in the diluvium at Beaumaris.

that sea-shells have been found on Moel Faban, two miles N.E. of Bethesda. I ascended this and some neighbouring hills, but could find no trace of any deposit likely to include shells. This hill stands isolated, out of the course of the glaciers from the central valleys; it exceeds 1000 feet in height; its surface is jagged, and presents not the smallest appearance of the passage of glaciers: but high up on its flanks (and perhaps on its very summit) there are large, angular and rounded boulders of foreign rocks.

Along the sea-coast between Bangor and Caernarvon, and on the Caernarvonshire plain, I did not notice any boss-formed hillocks of rock. The whole country is in most places concealed by beds of till and stratified gravel, with scattered boulders on the surface: some of these boulders were scored. From the account given by Mr. Trimmer* of his remarkable discovery of broken fragments of Buccinum, Venus, Natica, and Turbo, beneath twenty feet of sand and gravel, on Moel Tryfan (S.E. of Caernarvon), I ascended this hill. Its height is 1192 feet† above the sea; it is strewed with boulders of foreign rock, most of them apparently from the neighbouring mountains; but near the summit I found the rounded chalk-flints‡ and small pieces of white granite alluded to by Dr. Buckland. Its form is conical, and it stands isolated: wherever the bare rock protrudes its surface is jagged, and shows no signs of being in any part worn into bosses. The contrast between the superficial part of the bare rock on this hill and on Moel Faban, with that of the rocks within the great central valleys of Caernarvonshire, is very remarkable; it is a contrast of precisely the same kind as may be observed in these same valleys by ascending on either side above the reach of the ancient glaciers. A little way down the hill, a bed two or three feet in thickness, of broken fragments of slate mixed with a few imperfectly rounded pebbles and boulders of many kinds of rock, is seen in several places to rest on the slate, the upper surface of which, to the depth of several feet, has been disintegrated, shattered and contorted in a very curious manner. The laminated fragments, however, sometimes partially retain their original position.

I did not succeed in finding any fragments of shells, but near the summit of the hill on the eastern or inland side, I found beds, at least twenty feet in thickness, of irregularly stratified gravel and boulders, with distinct and quite defined layers of coarse yellow sand, and others of a fine argillaceous

---

* Proceedings of the Geological Society, vol. i. p. 332. [Phil. Mag. *loc. cit.*]

† Murchison's Silurian System, p. 528.

‡ I may mention, that at Little Madely, in Staffordshire, I have found chalk-flints in the gravel-beds, associated with existing species of sea-shells.

nature and reddish colour. These beds closely resemble those of Shropshire and Staffordshire, in which are found (as I have myself observed in very many places) fragments of sea-shells, and which every one, I believe, since the publication of Mr. Murchison's chapters on the drift of these counties, admits are of submarine origin. It may therefore be concluded that the layers of coarse and argillaceous sand, and of gravel, with far-transported pebbles and boulders, do not owe their origin to an inundation, but were deposited when the summit of Moel Tryfan stood submerged beneath the surface of the sea. As there are no marks of the passage of glaciers over this mountain (which indeed from its position could hardly have happened), we must suppose that the boulders were transported on floating ice; and this accords with the remote origin of some of the pebbles, and with the presence of the sea-shells. Within the central valleys of Snowdonia, the boulders appear to belong entirely to the rocks of the country. May we not conjecture that the icebergs, grating over the surface, and being lifted up and down by the tides, shattered and pounded the soft slate-rocks, in the same manner as they appear to have contorted the sedimentary beds of the east coast of England (as shown by Mr. Lyell)*, and of Tierra del Fuego? Although I was unable to find any beds on Moel Faban likely to preserve sea-shells, yet, considering the absence of the marks of the passage of glaciers over it, I cannot doubt that the boulders on its surface were transported on floating ice.

The drifting to and fro, and grounding of numerous icebergs during long periods near successive uprising coast-lines, the bottom being thus often stirred up and fragments of rock dropped on it, will account for the sloping plain of unstratified till, occasionally associated with beds of sand and gravel, which fringes to the west and north the great Caernarvonshire mountains.

In a paper read before the Geological Society †, I have remarked that blocks of rock are transported by floating ice under different conditions; 1st, by the freezing of the sea, in countries where the climate does not favour the low descent of glaciers; 2nd, by the formation of icebergs by the descent of glaciers into the sea, from mountains not very lofty, in latitudes (for instance in that of Geneva, or of the mouth of the Loire, in the southern hemisphere) where the surface of the

---

* "On the Boulder Formation of Eastern Norfolk;" Phil. Mag., S. 3, vol. xvi. May 1840, p. 351.

† May 5th. 1841, "On the distribution of the Erratic Boulders, and on the contemporaneous unstratified deposits of South America." [Phil. Mag. S. 3, vol. xix. p. 536.]

sea never freezes; and 3rd, by these two agencies united. I have further remarked that the condition and kind of the stones transported, would generally be influenced by the manner of production of the floating ice. In accordance with these views, I may remark that it does not seem probable from the low level of the Chalk-formation in Great Britain, that rounded chalk-flints could often have fallen on the surface of glaciers, even in the coldest times. I infer therefore that such pebbles were probably inclosed by the freezing of the water on the ancient sea-coasts. We have, however, the clearest proofs of the existence of glaciers in this country; and it appears, that when the land stood at a lower level, some of the glaciers, as in Nant-Francon, reached the sea, where icebergs charged with fragments would occasionally be formed. By this means we may suppose that the great *angular* blocks of Welch rocks, scattered over the central counties of England, were transported*. I looked carefully in the valleys near Capel-Curig and in Nant-Francon for beds of pebbles, or other marks of marine erosion, but could not discover any: when, however, Moel Tryfan and Faban stood beneath the level of the sea, inland creeks of salt-water must have stretched far up or quite through these valleys, and where they were deep, the glaciers (as at present in Spitzbergen†) would have extended, floating on the surface of the water, ready to become detached in large portions. From the presence of boss-formed rocks low down in the valley of Nant-Francon, and on the shores of the Lakes

---

* On the summit of Ashley Heath in Staffordshire, there is an angular block of syenitic greenstone, four feet and a half by four feet square, and two feet in thickness. This point is 803 feet above the level of the sea. From this fact, together with those relating to Moel Tryfan and Faban, we must, I think, conclude that the whole of this part of England was, at the period of the floating ice, deeply submerged. From the reasons given in my paper (Phil. Trans., 1839 [Phil. Mag. S. 3, vol. xiv. p. 363.]), I do not doubt that at this same period the central parts of Scotland stood at least 1300 feet beneath the present level, and that its emergence has since been very slow. The boulder on Ashley Heath probably has been exposed to atmospheric disintegration for a longer period than any other in this part of England. I was therefore interested in comparing the state of its lower surface, which was buried two feet deep in compact ferruginous sand (containing only quartz pebbles from the subjacent new red sandstone), with the upper part. I could not, however, perceive the smallest difference in the preservation of the sharp outlines of its sides. I had a hole dug under another large boulder of dark green felspathic slaty rock, lying at a lower level; it was separated by 18 inches of sand, (containing two pebbles of granite, and some angular and rounded masses of new red sandstone) from the surface of the new red sandstone. One of the rounded balls of this latter stone had been split into two, and deeply scored, evidently by the stranding of the boulder.

† Dr. Martens on the Glaciers of Spitzbergen, New Edinb. Phil. Journ. 1841, (vol. xxx.) p. 288.

of Llanberis (310 feet above the sea), it is evident that glaciers filled the valleys after the land had risen to nearly its present height; and these glaciers must have swept the valleys clean of all the rubbish left by the sea. As far as my very limited observations serve, I suspect that boss or dome-formed rocks will serve as one of the best criterions between the effects produced by the passage of glaciers and of icebergs*.

Dr. Buckland has described in detail the marks of the passage of glaciers along nearly the whole course of the great central Welch valleys; I observed that these marks were evident at the height of some hundred feet on the mountain-sides, above the water-sheds, where the streams flowing into the sea at Conway, Bangor, Caernarvon, and Tremadoc, divide: hence it appears that a person starting from any one of these four places (or from some way up the valley where the glacier ended), might formerly, without getting off the ice, have come out at either of the other three places, or low down in the valleys in which they stand. The mountains at this period must have formed islands, separated from each other by rivers of ice, and surrounded by the sea. The thickness of the ice in several of the valleys has been great. In the vale of Llanberis I ascended a very steep mountain, E.N.E. of the upper end of the upper lake, which slightly projects where the valley bends a little. For the lower 1000 feet (estimated, I think, correctly) the marks left by the glacier are very distinct, especially near the upper limit, where there are boulders perched on bosses of rock, and where the scores on the nearly vertical faces of rock are, I think, more distinct than any others which I saw. These scores are generally slightly inclined, but at various angles, seaward, as the surface of the glacier must formerly have been. But on one particular face of rock, inclined at an angle of somewhere about fifty degrees, continuous, well-marked and nearly parallel lines sloped upwards (in a contrary sense to the surface of the glacier) at an angle of 18° with the horizon. This face of rock did not lie parallel to the sides of the main valley, but formed one side of the sloping end of the mountain, over and round which the ice appears to have swept with prodigious force, expanding laterally after being closely confined by the shoulder above

---

* In the Appendix to my Journal of Researches (1839), I endeavoured to show that many of the appearances attributed to debacles, and to the movements of glaciers on solid land, would in all probability be produced by the action of stranded icebergs. I have stated (p. 619), on the authority of Dr. Richardson, that the rocky beds of the rivers in North America which convey ice, are smoothed and polished; and that (p. 620) the icebergs on the Arctic shore drive before them every pebble, and leave the submarine ledges of rock absolutely bare.

mentioned. At this point, where the glacier has swept to the westward, and has expanded, its surface seems in a short space to have declined much: for on a hill lying about a quarter of a mile N.W. of the shoulder, and forming a lower part of the same range (it stands S.S.E. of the Victoria Inn, and has a reddish summit), the marks of the passage of the glacier are at a considerably lower level. At the very summit, however, of this hill, several large blocks of rock have been moved from their places, as if the ice had occasionally passed over the summit, but not for periods long enough to have worn it smooth.

I cannot imagine a more instructive and interesting lesson for any one who wishes (as I did) to learn the effects produced by the passage of glaciers, than to ascend a mountain like one of those south of the upper lake of Llanberis, constituted of the same kind of rock and similarly stratified, from top to bottom. The lower portions consist entirely of convex domes or bosses of naked rock, generally smoothed, but with their steep faces often deeply scored in nearly horizontal lines, and with their summits occasionally crowned by perched boulders of foreign rock. The upper portions, on the other hand, are less naked, and the jagged ends of the slaty rocks project through the turf in irregular hummocks; no smooth bosses, no scored surfaces, no boulders are to be seen, and this change is effected by an ascent of only a few yards! So great is the contrast, that any one viewing these mountains from a distance, would in many cases naturally conclude that their bases and their summits were composed of quite different formations.

XXXI. *Application to particular instances of the general Formula for eliminating the Weights of Mixed Bases.* By JOSIAH REES, JUN., *F.G.S., of Her Majesty's Ordnance Geological Survey**.

THE general formula for eliminating the weights of any two bases, where the whole weight of any particular acid with which they are combined has been previously ascertained, is not easily available to those who are unaccustomed to mathematical inquiry.

If, however, we apply the general rule to particular instances, we are enabled to obtain a very simple place for each, by the application of which the weight of the bases may be ascertained.

I have thought it would not be altogether useless to draw up a few such rules for the use of chemists.

---

* Communicated by the Author.

The following combinations have been chosen as the most likely to come under the notice of the practical chemist:—

Potash and soda combined with sulphuric acid; sodium and magnesium with chlorine; sodium and calcium with chlorine; lime and magnesia with carbonic acid.

The equivalents adopted by Brande have been used in the calculation.

Carbonic acid . . .	22	Magnesium.	12
Chlorine	36	Potash	48
Sulphuric acid . . .	40	Soda	32
Calcium	20	Potassium	40
Lime	28	Sodium	24

1. When potash and soda exist in combination with sulphuric acid, the weight of mixed sulphates being known, and also the weight of acid with which they are combined, to ascertain the weight of each base present.

Rule.—Multiply the whole weight of material experimented on by 15; from the product subtract 27 times the weight of the acid in combination, and divide the remainder by 5, the quotient will be the weight of potash: b being the weight of material experimented on, and a the known weight of acid, the rule stands thus:—

$$\frac{15\,b - 27\,a}{5} = \text{the weight of potash.}$$

The whole weight of acid and the weight of potash being ascertained, the weight of soda is of course at once known by subtracting the weight of acid and potash from that of the whole material experimented on.

2. When magnesium and sodium exist in combination with chlorine, the whole weight of the chlorine in combination being known, to ascertain the weight of each base.

Rule.—Multiply the whole weight of material experimented on by 6, from the product subtract 8 times the weight of the chlorine, and divide the remainder by 3, the quotient will be the weight of the sodium:—

$$\frac{6\,b - 8\,a}{3} = \text{weight of the sodium.}$$

3. When sodium and calcium exist in combination with chlorine, the weight of chlorine being known, to ascertain that of each base.

Rule.—Multiply the whole weight of material experimented on by 18; from the product subtract 28 times the weight of chlorine, and divide the remainder by 3; the quotient will be the weight of the sodium:—

$$\frac{18\,b - 28\,a}{3} = \text{weight of sodium.}$$

4. When lime and magnesia exist in combination with carbonic acid, the whole weight of the acid in combination being previously known, to ascertain the weight of each base.

Rule.—Multiply the whole weight of material experimented on by 77; from the product subtract 147 times the weight of acid, divide the remainder by 22, and the quotient will be the weight of lime:—

$$\frac{77\,b - 147\,a}{22} = \text{weight of the lime.}$$

Crickhowel, July 2, 1842.

XXXII. *On the Employment of Polar Coordinates in expressing the Equation of the Straight Line, and its application to the proof of a property of the Parabola.* By T. S. Davies, Esq., F.R.S., F.S.A., &c., Royal Military Academy, Woolwich*.

ABOUT ten years ago I gave in a note to my paper on Spherical Coordinates (in the Trans. Roy. Soc. Edinb., vol. xii.) the general equation of a straight line in reference to polar coordinates. The idea, which is very simple, was suggested by the method which I had employed in the discussion of spherical loci; the equation of the line in plano corresponding to that of the great circle on the surface of the sphere: and it was made apparent that the treatment of the straight line by such means was quite as simple and elementary in all its details as that by means of rectilinear coordinates.

Beyond the occasional employment of the expression $\frac{d\,r_1}{r_1\,d\,\theta_1}$ to express the angle of the tangent and radius vector, or the relation between the perpendicular on the tangent and the corresponding radius vector, the method of polar coordinates has been generally disregarded by mathematicians in treating of the tangents and normals to curve lines: and I do not recollect a single instance where the general polar form of the equation of a line subject to its adequate number of defining conditions has even been noticed, much less used, by any author, prior to the appearance of my paper. However, that it is a very efficient method of investigating the properties of rectilineal figures, any reader may readily convince himself by a few experiments upon such theorems as express those properties; and I wish here to illustrate its utility in reference to tangencies by the investigation of a theorem which has ex-

---

* Communicated by the Author.

cited some interest amongst the readers of the Philosophical Magazine, and which treated purely by rectangular coordinates, involves expressions of considerable complexity.

THEOREM. *If three tangents to a parabola mutually intersect, the circle described about the triangle formed by them will always pass through the focus of the parabola*[*].

The polar equation to the tangent at the point $r_1\theta_1$ of any curve is

$$r\left\{\cos(\theta-\theta_1)-\sin(\theta-\theta_1)\frac{d\,r_1}{r_1\,d\,\theta_1}\right\}=r_1.$$

Edinb. Trans., vol. xii. p. 408.

And the equation of the parabola, referred to its focus as pole and diameter as origin of polar angles, is, at the point $r_1\,\theta_1$,

$$r_1\,(1+\cos\theta_1)=2\,a.$$

From (2.) we get

$$\frac{d\,r_1}{d\ \ _1}=\frac{\sin\theta_1}{1+\cos\theta_1},$$

which, inserted in the general equation, gives at once

$$r\,\{\cos(\theta-\theta_1)+\cos(\theta-\theta_1)\,\cos\theta_1-\sin(\theta-\theta_1)\sin\theta_1\}$$
$$=r_1\,(1+\cos\theta_1),$$

or finally, $r\cos(\theta-\tfrac{1}{2}\theta_1)\cos\tfrac{1}{2}\theta_1=a.$ . . . . . (1.)
Similarly, $r\cos(\theta-\tfrac{1}{2}\theta_2)\cos\tfrac{1}{2}\theta_2=a,$ . . . . . (2.)
and $r\cos(\theta-\tfrac{1}{2}\theta_3)\cos\tfrac{1}{2}\theta_3=a,$ . . . . . (3.)

which represent the three tangents at $r_1\,\theta_1$, r_2, θ_2, $r_3\ \theta_3$; and from which the proof of the theorem is deducible as follows:

Denote by $R_1\ \Theta_1$ the coordinates of the intersection the tangents represented (2, 3), by $R_2\,\Theta_2$ that of (3, 1), and by $R_3\ \Theta_3$ that of (1, 2). Then we get immediately

$$\begin{array}{l|l|l}
\Theta_1=\frac{1}{2}(\theta_2+\theta_3) & R_1=a\sec\frac{1}{2}\theta_2\sec\frac{1}{2}\theta_3 & \Theta_1-\Theta_2=\frac{1}{2}\ (\theta_2-\theta_1) \\
\Theta_2=\frac{1}{2}(\theta_3+\theta_1) & R_2=a\sec\frac{1}{2}\theta_3\sec\frac{1}{2}\theta_1 & \Theta_2-\Theta_3=\frac{1}{2}\ (\theta_3-\theta_2) \\
\Theta_3=\frac{1}{2}(\theta_1+\theta_2) & R_3=a\sec\frac{1}{2}\theta_1\sec\frac{1}{2}\theta_2 & \Theta_3-\Theta_1=\frac{1}{2}\ (\theta_1-\theta_3)
\end{array}$$

Hence,

---

[*] Wallace, in the Mathem. Repos., vol. ii. p. 54, Old Series, and in his Conic Sections, p. 167; Timmermanns, in Quetelet's *Correspondance Math. et Phys.*, tom. ii. p. 75; Strong and Avery, Gill's Math. Misc. New York, No. 6; Jones in the Gentleman's Diary, 1831; Poncelet, *Traité des propriétés projectives*, section iv. *Annales des Mathématiques*, tom. viii.; Phil. Mag., S. 3, vol. ix. p. 100; x. pp. 32, 35; xi. p. 302; and Young's Conic Sections, p. 189.

I would not be understood to contest the simplicity of the geometrical methods of proving this theorem; but merely take this theorem as an illustration of the occasional advantage of the polar over the rectangular equation of the tangent to a curve.

$$R_1 \sin(\Theta_2 - \Theta_3) = a\,(\tan \tfrac{1}{2}\,\theta_3 - \tan \tfrac{1}{2}\,\theta_2)$$
$$R_2 \sin(\Theta_3 - \Theta_1) = a\,(\tan \tfrac{1}{2}\,\theta_1 - \tan \tfrac{1}{2}\,\theta_3)$$
$$R_3 \sin(\Theta_1 - \Theta_2) = a\,(\tan \tfrac{1}{2}\,\theta_2 - \tan \tfrac{1}{2}\,\theta_1).$$

By addition of these, we have

$$R_1 \sin(\Theta_2 - \Theta_3) + R_2 \sin(\Theta_3 - \Theta_1) + R_3 \sin(\Theta_1 - \Theta_2) = 0,$$

which is the criterion of the circle through $R_1\,\Theta_1$, $R_2\,\Theta_2$, $R_3\,\Theta_3$ passing through the polar origin, or, in this case, the focus of the parabola.

It may not be irrelevant to remark, that the geometrical property expressed by the values of Θ_1, Θ_2, Θ_3 in terms of θ_1, θ_2, θ_3 is the familiar one found in all works on the conic sections; as in Hutton's Course, for instance, at vol. ii. pp. 111, 135, 147 of the 11th edition, and nearly in the same places in the edition now printing.

Lines drawn to the focus of a conic section from the intersection of two tangents, bisects the angle formed by the radii vectores drawn to the points of contact.

The property in reference to the other conic sections is deducible in the same way, as will be obvious on forming the equations of the tangent in each of them, and which are put down here for the ellipse and hyperbola:—

$$r\,\{\cos(\theta - \theta_1)\theta + e \cos\} = a\,(1 - e^2),$$

and

$$r\,\{\cos(\theta - \theta_1)\theta - e \cos\} = a\,(e^2 - 1).$$

Many other properties may be obtained by this method with great simplicity and elegance; but the method being once pointed out, the details are too elementary to require further notice in this place.

Royal Military Academy,
July 5, 1842.

XXXIII. *On the Change of Colour in the Biniodide of Mercury.* By Robert Warington, *Esq., Secretary to the Chemical Society**.

It is well known that when a solution of the iodide of potassium is added to a solution of the bichloride or pernitrate of mercury, a yellow precipitate, passing rapidly to a scarlet, is formed; this is the biniodide of mercury. It is soluble in an excess of either of the agents employed for its production, and if this act of solution be assisted by heat, the biniodide may be obtained, as the solution cools in fine scarlet crystals, having the form of the octohedron with the square base, or its modifications.

---

* Communicated by the Chemical Society, having been read Feb. 1, 1842. Some of the facts related in this paper had been previously observed by Mr. Talbot, and described by him in Phil. Mag. Third Series, vol. ix. p. 2.—Edit.

If this precipitated biniodide, in the dry state, be subjected to the action of heat, it becomes of a bright pale yellow colour, fuses into a deep amber-coloured fluid, and gives off a vapour which condenses in the form of rhombic plates of the same bright yellow; these crystals, by any mechanical disturbance, arising from the unequal contraction of their molecules in cooling, from varying thickness in different parts of the same crystal, or from partial disintegration, return again to the original scarlet colour of the precipitate, the change commencing, in the latter case, from the point ruptured, and spreading over the whole of the crystalline mass; they may however be frequently preserved in the yellow state for a great length of time, if sublimed slowly and not exposed to the contact of other substances, which is readily effected by conducting the sublimation in closed vessels, and allowing the crystals to remain in them undisturbed.

The resumption of the scarlet colour has been attributed to an alteration in the molecular arrangement of the crystals, and it was with the view of clearly ascertaining this point that the following microscopic investigations were undertaken.

When a quantity of the precipitated biniodide is sublimed, the resulting crystals are very complicated in their structure, consisting of a number of rhombic plates, of varying size, superposed, sometimes overlapping each other and causing considerable variableness in their thickness, but generally leaving the extreme angle and the two lateral edges clear and well-defined; the annexed sketch, taken by the camera lucida from the field of view of the microscope, will give a better idea of their character. The length of these crystals was about ·015 of an inch in length. On cooling, the first change that is observed is usually a scarlet marking, commencing at the extreme angle and extending gradually inwards, always retaining a perfectly well-defined line in its progress; when this change has reached as far as the line *a b*, fig. 1, the scarlet line will suddenly shoot along one of the lateral edges, as shown at *c d*, and instantly the whole mass is converted, in a most rapid and confused manner, which the eye in vain endeavours to follow, to the scarlet colour, the crystal being frequently, if detached, twisted and contorted during the transition.

In order to obtain these crystals in a more defined and clearly developed form, a small glass cell was constructed of two slips of window-glass, leaving a space of about the thickness of cartridge paper between the upper and under plates, in which the sublimations could be readily conducted, and the whole of the subsequent changes at once submitted to the microscope;

by this means beautifully well-defined and perfect crystals were obtained, having the form of right rhombic prisms, as in the accompanying outlines, fig. 2, *a* and *b*. The following interesting phænomena were then observed: a defined scarlet line of varying breadth would shoot across the crystal, as at 1. *c*, *d*, *e*, *f*, fig. 2, and then gradually spread throughout the whole of its structure, keeping a straight and well-defined line in its onward progress, until the whole had undergone the change of colour. Nos. 2, 3, 4, 5 in *e*, and No. 2 in *f*, are the stages which the transition had reached at intervals of observation; in many cases, after the crystal has undergone this metamorphosis, two angles can be distinctly seen, as at *e*, fig. 1, and at times two edges are visible, as at *c* 6 and *d* 6, fig. 2. This observation must of course depend entirely on the position of the crystal to the eye of the observer.

Fig. 1.

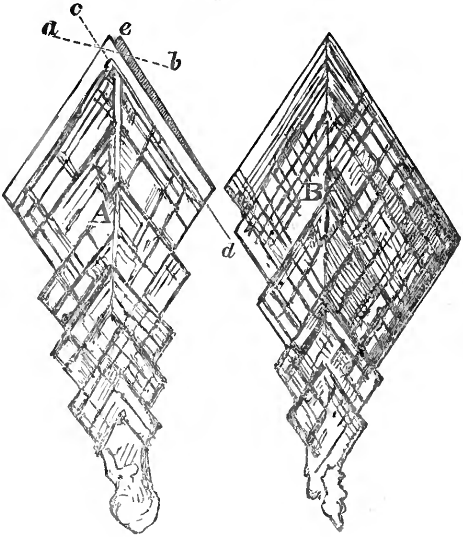
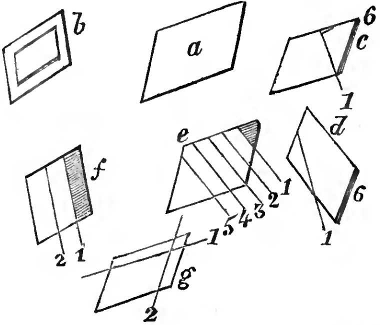


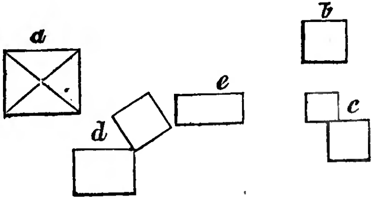
Fig. 2.



These phænomena prove, I consider, in the most perfect manner, that the change in the colour of this compound arises from the plates of the crystal having been separated from each other, by the means alluded to, in the direction of their cleavages; and in further confirmation of this view, the laminæ so separated may, by the sudden application of heat, be again fused together, and the yellow colour reproduced without materially altering the dimensions of the crystal, a slight rounding of the edges from partial sublimation being the only other concomitant.

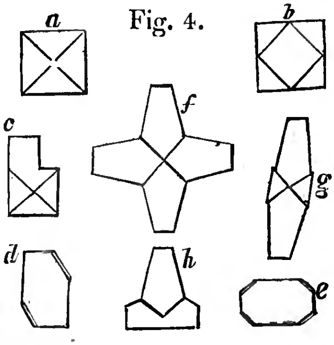
When the temperature is raised slowly and the sublimation conducted with great care, a very large proportion of red crystals, having a totally different form, are obtained, the octahedron with the square base, as shown fig. 3, *a*, *b*, *c*, *d*, *e*. If, however, the heat is quickly raised, the whole mass of the sublimed crystals are yellow and of the rhombic form. It is evident from these facts, that the biniodide of mercury has two vapours which are given off at different temperatures, and also that it is dimorphous, which facts have been substantiated by some experiments of M. Frankenheim, who has carefully examined this part of the subject.

Fig. 3.



From the circumstance that the first effect which occurs in the process for preparing this iodide by precipitation is the production of a yellow powder which passes rapidly through the orange colour to a scarlet, I was induced to submit this phænomenon also to the test of microscopic examination, and with this valuable instrument of research, results were exhibited which could not have been anticipated. As I expected, the precipitate was in small crystalline grains, and the first step of the investigation was to effect its formation in the field of view of the microscope, so as to observe, directly as they occurred, the transitions of colour which have been alluded to, and this was effected by the following means:—A slip of common window-glass, about three inches long by one and a half wide, and having a very narrow slip attached on one of its edges, so as to act as a ledge, was taken, and a drop of the salt of mercury employed placed on it; this was then covered with a small piece of extremely thin glass, about one inch long by half an inch wide, and the whole carefully adjusted to focus in the field of the instrument; the iodide of potassium was then introduced by capillary attraction between the glasses. The instant the solutions came in contact, a myriad of pale-yellow crystals, having the same rhombic form as those obtained by sublimation, formed in a curved line across the field of view and extended slowly downwards; by the strong transmitted light these minute crystals appeared colourless; but when viewed by reflected light, the pale yellow colour was readily apparent. After a short interval a very extraordinary change commenced; the crystals, which had been perfectly sharp and well-defined, became ragged at their edges, as though some dissolving action were going on, gradually decreased in size, and at last disappeared altogether; but as this act of solution progressed, numbers of red crystals made their appearance, forming across the field and following at a regular distance the yellow crystals as they disappeared, and occupying their place. These red crystals, which appear to be formed by the disintegration through the medium of solution, if I may be allowed the expression, from those first produced, had the form of the octohedron with the square base, exactly similar to those procured by careful sublimation at a low heat, only modified in the most beautiful manner. Some few of these are sketched in the forms, *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, fig. 4. When either the salt of mercury or the iodide of potassium, employed in the production of the biniodide of mercury, was in excess, another curious act of disintegration took place; the red crystals in fig. 4 were slowly dissolved, a property mentioned in the first part of this paper, the first act of solution commencing apparently by the disjunction of the crystals *a*, *b*, *c*, *f*, *g*, *h*, at the lines of marking, these lines being at first bright red, and gradually deepening in colour when the act of solution commenced, and at last perfect separation taking place, so that the light could be seen between the compartments. At times the field would become dry from evaporation, and some of the yellow rhombic crystals which had not been dissolved, prior to the formation of the octohedra with the square base, were observed with scarlet lines on them similar to the first act of transition in the sublimed crystals, as shown at *g* 1 and 2 in fig. 2.

Fig. 4.



By polarized light the appearances now described were beautiful beyond all description, the yellow crystals presenting the most superb and brilliant colours, varying in hue with the varied thickness of the crystalline plate, and in the dark field having the appearance of the most splendid gems the imagination can conceive: the red crystals do not appear to be affected by polarized light, so far as the display of colour is concerned.

The magnifying powers used in these investigations were, for the experiments on the sublimed crystals, 200 times linear measurement or diameters; in the precipitated compound, 620 diameters.

XXXIV. *On a new Oxalate of Chromium and Potash. By* HENRY CROFT, *Esq.*[*]

IT is well known that in 1830 Wilton Turner accidentally discovered a salt composed of oxalate of the oxide of chromium and oxalate of potash. Its curious optical properties have been examined by Brewster[†]. Gregory also discovered the same salt independently, and proposed a much better method for obtaining it than that used by Turner, which consisted in adding oxalic acid to a solution of bichromate of potash until effervescence ceased; the solution became deep green or black, and on evaporation yielded beautiful crystals of the black salt. Gregory supposed it to consist of 3 equivalents of oxalic acid, 2 of potash, 1 of oxide of chromium, and 6 of water. Its true composition, $3\,(KO, C_2\,O_3) + Cr_2\,O_3$, $3\,C_2\,O_3 + 6\,HO$ has been shown by Graham and Mitscherlich, who have also prepared a number of salts similarly constituted.

On attempting to prepare the black salt by Turner's method I could never completely succeed, but obtained in its stead, when a very concentrated hot solution of the bichromate was employed, a red granular precipitate, which proved to be a new salt, and forms the subject of the present notice.

Perhaps the best method of preparing it is that above described, viz. to employ as concentrated a solution of the bichromate as possible, in which case the salt crystallizes out on cooling. The precipitated salt must be redissolved in a small quantity of water and allowed to crystallize. It is however one of the most difficult salts to crystallize that is known: in nine cases out of ten it separates in the form of a somewhat granular bluish gray powder, and it appears to be only under particular circumstances that it will crystallize well, which, however, I was not able to discover. It does not seem to

---

[*] Communicated by the Chemical Society, having been read February 15, 1842.

[† See Phil. Mag. Third Series, vol. vii. p. 436. Some of the optical and crystallographical properties of this salt have also been described by Mr. Talbot, in Phil. Mag. Third Series, vol. x. p. 218, and vol. xiv. p. 21.—EDIT.]

crystallize any better by spontaneous evaporation than out of a very concentrated solution; it seems however to form more regularly in warm air, as in summer. The best crystals are generally formed on the surface of the solutions: they are very minute, in the form of triangular plates; when the crystals form a mass at the bottom of the liquid the plates are thicker, but their form is indistinguishable. The salt is of a deep red colour by reflected as well as by transmitted light; the solution is green, or even black (when concentrated) by reflected and red by transmitted light. The solution when at a boiling temperature remains red, as is seen best by candle-light: the same is the case with the solution of the black salt, which shows that the purple oxide of chromium contained in these salts is not converted by a boiling heat into its green modification; the purple oxide must, however, as is well known, be first brought into combination with the oxalic acid, for the black salt can never be obtained by dissolving *green* oxide of chromium in binoxalate of potash.

A solution of caustic potash added to a solution of the red salt turns it bright green, but causes no precipitate until boiled, when the greater part of the oxide of chromium is thrown down. Carbonates of the alkalies partly change the colour in the same manner, but do not precipitate the oxide so readily. Ammonia causes no precipitate, nor does chloride of calcium, owing to the formation of Dingler's oxalate of chromium and lime; when ammonia is added a green precipitate containing oxide of chromium is formed.

This salt contains a large quantity of water of crystallization, which can only be driven out by a strong heat, as is also the case with the black salt (Graham). It loses about 15–16 per cent. at 100° cent., and 19 per cent. at 200° cent. The last portions of water can only be driven out at 300° cent. Near this point the salt begins to be decomposed, and consequently the determination of the water is rendered somewhat difficult.

per cent.
0·9986 gramme of salt lost 0·2638 water = 26·42
0·7481 ... ... 0·1965 ... = 26·27
0·8971 ... ... 0·2532 ... = 28·22

The determinations of the oxide of chromium and the potash were performed in the following manner. The salt was heated red-hot: in this operation great care must be taken, for the salt possesses the curious property of decomposing with considerable violence (without explosion) into a green powder, which unless the heat is applied very gradually, is forced out of the crucible, and the analysis is thus lost. When the temperature is raised gradually the crystals retain their form, but become of a bright dark green colour: as soon as the decomposition of the oxalates commences they fall into a light green powder, which when stronger heated becomes brown. In closed vessels carbonate of potash is formed; in open ones, when the heat is continued for a length of time, chromate is produced. This chromate must be extracted by water, reduced, and the oxide of chromium precipitated by ammonia: in this operation, however, it is better to evaporate the ammoniacal solution to dryness, as the ammonia always dissolves a small quantity of the oxide. This method is preferable to that usually employed (Heinrich Rose's Analytical Chemistry): the ammoniacal and potash salts must be dissolved out, evaporated, the ammonia driven off, and the potash determined either as chloride or by means of platinum.

The oxalic acid may be determined by boiling the salt with sulphuric acid, as proposed by Prof. Graham.

The salt being excessively difficult to crystallize, it seldom happens that a perfectly homogeneous substance can be obtained for analysis: the method of analysis is moreover somewhat complicated, and consequently the analyses do not agree so perfectly as could be desired.

	I.	II.	III.	IV.	V.	VI.
Cr_2O_3	21·80	21·83	23·11	22·05	21·10	24·11
KO	13·18	13·11	12·22	12·92		
C_2O_3	37·00	36·98				40·89

The water as obtained by other experiments, is

HO 26·42 26·27 28·22

The most plausible formula is $KO, C_2O_3 + Cr_2O_3, 3\,C_2O_3 + 12\,HO$.

C_2O_3	4	1811·50	38·098
Cr_2O_3	1	1003·63	21·107
KO	1	589·92	12·405
HO	12	1349·75	28·390
		4754·80	100·000

This differs from the black salt in containing one atom of basic oxalate instead of three. It may be said to be related to the black salt in the same way as metaphosphates are to phosphates. It is evident, therefore, that if we add two atoms of oxalate of potash to one atom of the red salt, we ought to obtain the black salt, which is indeed the case.

2·37 grammes of red salt were mixed with 1·15 gr. of oxalate of potash (these are the atomic proportions), the solution boiled and evaporated, they yielded 3·119 grs. of the black salt in good crystals, and perfectly pure: according to theory it ought to have given 3·070. The weight of the black salt must be equal to that of the red salt, *plus* two atoms of anhydrous oxalate of potash, *minus* six atoms of water. The agreement of the experiment with the calculation speaks for the correctness of the above formula, in which one might, perhaps, otherwise not place so much confidence.

The constitution of this salt led me to consider the theory of its formation, and also that of the black salt, more particularly as in employing the known formulæ for making the black salt I always obtained it mixed with other bodies.

In forming the red salt from bichromate of potassa, 7 atoms of oxalic acid are required. $K\,O, 2\,Cr\,O_3$ and $7\,C_2\,O_3 = K\,O, C_2\,O_3 + Cr_2\,O_3, 3\,C_2\,O_3$ and $3\,C_2\,O_3 + 3\,O$, or $6\,C\,O_2$. On mixing the two substances in this proportion I obtained perfectly pure red salt. It is evident that seven atoms of oxalic acid, either free or in combination with potash, must be used in making the black salt. None of the numbers in the formulæ given for preparing the black salt agree with this.

Dr. Gregory gives 190 parts bichromate of potash, 157·5 parts crystallized oxalic acid, and 517 parts binoxalate of potash; that is, one atom of the bichromate, two atoms oxalic acid, and three of binoxalate of potash; on trying these numbers I obtained a mixture of black salt with oxalate and chromate of potash.

Prof. Graham proposes one part of bichromate, two of binoxalate, and two of crystallized oxalic acid. In these proportions a large quantity of chromate of potassa remains undecomposed, which requires, if 19 grains bichromate, 23 grains binoxalate, and 16 grains crystallized oxalic acid be taken, exactly 36 grains of crystallized oxalic acid to effect its perfect decomposition, and making the whole quantity of oxalic acid 52 grains.

According to the formula which I would propose, there are required

19	grains	bichromate of potash
23	...	oxalate of potash
55	...	crystallized oxalic acid.

If the salts be taken in these proportions, nothing but black salt is obtained; it is however better to evaporate the whole to dryness and then re-dissolve.

I have not been able to obtain an intermediate salt, namely, $2\,K\,O, C_2\,O_3 + Cr_2\,O_3, 3\,C_2\,O_3$. This, if it exists, ought to be produced from two atoms chromate of potash, and eight atoms oxalic acid: I obtained, however, oxalate of potash and red salt.

A similar salt may probably exist with oxide of iron, but it does not crystallize. On dissolving sesquioxide of iron in quadroxalate of potash a solution is obtained, which dries to a brown gummy mass without traces of crystallization.

XXXV. *Some additional Observations on the Red Oxalate of Chromium and Potash.* *By* ROBERT WARINGTON, *Esq., Secretary to the Chemical Society**.

HAVING in the year 1832 obtained this salt by the same method as that described by Mr. Croft, namely, in the endeavour to prepare the dark blue oxalate of chromium and potash by the process originally given by its discoverer Dr. Wilton Turner, and having in my possession some crystals of a much larger size than those usually obtained, I was induced to avail myself of the kind offer of Professor Miller of Cambridge, "to determine the form of any crystalline products that the members of the Society might obtain in their researches," and have great pleasure in laying before the Society the following letter and measurements:—

"St. John's College, Cambridge, April 25, 1842.

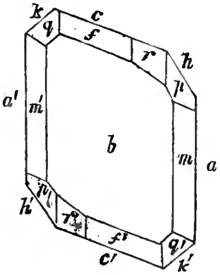
"DEAR SIR.—The crystals of the oxalate of chromium and potash are represented in the accompanying figure. The numbers expressing the angles between normals to the faces must be considered as rough approximations only, for although I measured all the measurable crystals you sent me, the variations of the angles between corresponding faces showed that the crystals were by no means so perfect as could be wished.

"The angles given are however abundantly accurate for the purpose of identifying the substance. One of the crystals was a twin, the face (*a*) being the twin face or the face with respect to which the two individuals were symmetrically situated.

"*Oxalate of Chromium and Potash. System Oblique prismatic.*

"*Angles between normals to the faces of the crystal.*

a c	70°	45′	*c p*	50°	40′
a h	33	2	*c m*	77	32
c h	37	43	*a r*	61	0
b p	53	13	*a f*	78	30
c k	59	16	*a′q*	63	50
a p	47	49	*b f*	37	40
a m	49	5			



"The faces *a p r f q* are all in one zone; *h p b* are in one zone; *k q b* are in one zone; *a h c k* are in one zone. The other zones are sufficiently well indicated by the parallelisms of the edges.

"The symbols of the faces are,—*a* (100), *b* (010), *c* (001), *h* (101), *p* (111), *q* (111), *f* (011), *m* (110), *k* (101), *r* (112).

"I remain yours faithfully,
"W. H. MILLER."

---

* Communicated by the Chemical Society, having been read May 17, 1842.

These crystals, submitted to measurement by Professor Miller, were obtained by slow spontaneous evaporation: the difficulty of procuring this salt in crystals of any size has been fully pointed out by Mr. Croft.

I have only one observation which does not coincide with Mr. Croft's statements, but which, however, confirms in a great measure the results of his analysis; I allude to the statement that these double salts of chromium cannot be formed by the direct combination of their ingredients. The process which I have followed has been to digest the hydrated oxide of chromium in a mixed solution of oxalic acid and oxalate of potash in the proportions indicated by analysis, and when it ceases to dissolve the oxide, to decant the clear solution and allow it to crystallize. By the same means the analogous salts of soda and ammonia have been obtained, but not in crystals sufficiently large for measurement, as also other double salts of chromium. To prepare the hydrated oxide of chromium, the best and most œconomical process that I have found, is to take 150 grs. of the bichromate of potash and 200 grs. of liquid sulphuric acid, oil of vitriol, these proportions being nearly in the ratio of their atomic weights, so that the chrome alum, sulphate of the green oxide of chromium and potash, may be formed; the deoxidation of the chromic acid is easily effected by the addition of a little sugar and boiling the solution. When the deoxidation is complete, the green oxide may be precipitated by ammonia or by a carbonated alkali, and only requires to be well washed to remove all trace of alkali or saline matter.

XXXVI. *Reply to some Objections against the Theory of Molecular Action according to* Newton's *Law.* *By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., F.C.P.S., &c., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge.*

[Continued from p. 130.]

2. THE next objection to the molecular hypothesis of particles acting on each other, with forces varying inversely as the square of the distance, is that the equilibrium of such a system would not be stable. This objection is stated by Mr. Earnshaw in his memoir, Art. 15. The argument is as follows. The force due to a displacement parallel to either principal axis depends on the second differential coefficient of V, with respect to the coordinate along that axis. Now the sum of the second differential coefficients for the three coordinates is zero. Hence one of them must be *positive*, and the corresponding force put in play acts to draw the particle *from* its system of rest. Of course this reasoning depends on the as-

sumption that $\frac{d^2V}{dx^2}$, &c. are not zero. In the contrary case, as Mr. Earnshaw had previously pointed out (Art. 8), "the displacements of particles would not bring into action any forces of restitution." Another part of the objection relates to the boundaries of the medium, or rather of space. "If the particles of æther exert a repulsive action on each other, they will naturally endeavour to disperse themselves throughout all space, and form a medium coextensive with the boundaries of the universe. Here, then, a formidable difficulty presents itself to our notice. If the medium be of finite dimensions it must be inclosed in an envelope capable of restraining the expansive energy of the whole mass of particles. The more extensive the medium, the greater must be the strength of the envelope. Is it probable that the constitution of the universe is such as to require that the whole should be enclosed in a huge vessel of inconceivable strength?" (Art. 20.) The author then goes on to remove the difficulty by assuming a law of force, partly attractive, partly repulsive.

In replying to these objections we will reverse their order.

a. The difficulty thrown out relative to the equilibrium of the remote parts of space is one which has often presented itself, but from a consideration of which philosophers have, in general, cautiously abstained. The Newtonian system of the universe is beset with difficulties of a similar nature, which, although by no means satisfactorily removed, are never regarded as subversive of the hypothesis. We must, I conceive, be content with a theory capable of explaining phænomena which come within the limits of our own observation, without requiring that it should penetrate to the boundaries of the universe, if, in truth, such boundaries exist. I shall consider myself, therefore, at liberty to pass over this objection, with merely requesting that, should it be pressed, I may be informed how it is got over in the Newtonian system. I shall merely add that the molecular hypothesis does not assume that all the particles act with attractive, or all with repulsive forces.

b. We proceed to examine the circumstances under which the equilibrium may be neuter. It appears to me that this is really the state of things in nature, and accordingly, when replying to Mr. Earnshaw before the Philosophical Society of Cambridge, a little more than two years ago, I argued in support. I then expressed my belief that, in a medium of symmetry, *no force whatever* is put in play on a particle by its displacement alone. Subsequent investigation has confirmed me in my conjecture. So far as I had proceeded in the investigation I found that V appeared to be constant, so that *all* the differential coefficients $\frac{dV}{df}$, $\frac{d^2V}{df^2}$, $\frac{d^3V}{df^3}$, &c. are zero; and since the force put in play on a particle by a displacement δ depends on the expansions of $\frac{dV}{df}$, &c., and therefore of V in terms of δ, it is evident that the force is zero. The equilibrium is consequently what is technically called *neuter*.

The following investigation is copied from the paper above referred to. The complete demonstration of the proposition that $\frac{d^nV}{df^n}$ is equal to zero, involves some little analysis; and as it leads to a number of most important results, as, for instance, that $\Sigma\, m\,(x-f)^{2n} f(r) = \frac{1}{2n+1}\, \Sigma\, m\, r^{2n} f(r)$, I will reserve it to my next communication.

When $V = \Sigma\, m \frac{1}{\sqrt{(x-f)^2 + (y-g)^2 + (z-h)^2}}$

let $V' = \Sigma\, m \frac{1}{\sqrt{(x-f-\alpha)^2 + (y-g-\beta)^2 + (z-h-\gamma)^2}}$,

α, β, γ being the increments of f, g, and h.

Now if we put $\alpha(x-f) + \beta(y-g) + \gamma(z-h) = \varepsilon$, $\alpha^2 + \beta^2 + \gamma^2 = \delta^2$, and expand V′, there results

$$V' = V + \Sigma\, m\left(\frac{1}{2}\,\frac{2\varepsilon - \delta^2}{r^3} + \frac{1\,.\,3}{2\,.\,4}\,\frac{(2\varepsilon - \delta^2)^2}{r^5} + \&c.\right)$$

$$= V + \Sigma\, m\left\{\frac{-\delta^2}{2r^3} + \frac{1\,.\,3}{2\,.\,4}\,\frac{(4\varepsilon^2 - 4\varepsilon\delta^2 + \delta^4)}{r^5} + \&c.\right\}$$

$$= V + \Sigma\, m\left\{-\frac{\delta^2}{2r^3} + \frac{3}{2}\,\frac{\alpha^2(x-f)^2 + \beta^2(y-g)^2 + \gamma^2(z-h)^2}{r^5} + \frac{3}{8}\,\frac{\delta^4}{r^5} + \&c.\right\}$$

$$= V + \Sigma\, m\left\{-\frac{\delta^2}{2r^3} + \frac{\alpha^2 + \beta^2 + \gamma^2}{2r^3} + \frac{3}{8}\,\frac{\delta^4}{r^5} + \&c.\right\}.$$

We have obtained our reductions by introducing the results of symmetry. Thus the coefficient of δ^2 is zero. By proceeding a step further, we get

$$V' = V + \Sigma\, m\left\{\frac{3}{8r^5}\,\delta^4 + \frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\left(-\frac{12\varepsilon^2\delta^2}{r^7}\right) + \frac{1\,.\,3\,.\,5\,.\,7}{2\,.\,4\,.\,6\,.\,8}\,\frac{16\varepsilon^4}{r^9} + \&c.\right\}$$

$$= \mathrm{V} + \Sigma m \left\{ \frac{3\,\delta^4}{8\,r^5} - \frac{15}{12} \frac{(\alpha^2 + \beta^2 + \gamma^2)}{r^5} \delta^2 \right.$$

$$\left. + \frac{105}{24} \frac{\alpha^4 (x - f)^2 + \beta^4 (y - g)^4 + \gamma^4 (z - h)^4 + 6\,\alpha^2 \beta^2 (x - f)^2 (y - g)^2 + \&c.}{r^7} \right\}$$

$$= \mathrm{V} + \Sigma\, m \left\{ - \frac{7\,\delta^4}{8\,r^5} + \frac{105}{24} \frac{(x - f)^4}{r^9} (\alpha^4 + \beta^4 + \gamma^4) \right.$$

$$\left. + \frac{105}{24} \frac{6\,(x - f)^2 (y - g)^2 (\alpha^2 \beta^2 + \alpha^2 \gamma^2 + \beta^2 \gamma^2)}{r^9} + \&c. \right\}.$$

Now the hypothesis of symmetry, from which we have reduced the results by making

$$\Sigma \frac{m\,(x - f)^2}{r^9} = \frac{1}{3}\ \Sigma\, m\, \frac{r^2}{r^9}\ \&c.$$

imposes further the condition that V′ — V is a function of δ only, independent of α, β and γ. Consequently,

$$\Sigma\, m\, \frac{105}{24} \left(\frac{(x - f)^4 (\alpha^4 + \beta^4 + \gamma^4) + 6\,(x - f)^2 (y - g)^2 (\alpha^2 \beta^2 + \alpha^2 \gamma^2 + \beta^2 \gamma^2)}{r^9} \right)$$

$$= \Sigma\, m\, \frac{105}{24} \frac{(x - f)^4}{r^9} \delta^4$$

Hence we obtain the equation

$$\Sigma \frac{m\,(x - f)^4}{r^9} = 3\,\Sigma \frac{m\,(x - f)^2 (y - g)^2}{r^9} \ldots (\mathrm{A}).$$

This equation is of considerable importance. The method by which we have obtained it appears to be totally different from the ordinary methods, such as that employed by Cauchy, *Exercises*, 3. 201.

By substitution

$$\mathrm{V}'\ \mathrm{V} = \Sigma\, m \left\{ - \frac{7}{8\,r^5} + \frac{35}{8} \frac{(x - f)^4}{r^9} \right\} \delta^4 + \&c.$$

The coefficient of δ^4 depends on the value of

$$\Sigma \frac{m}{r^5} \left(- 1 + 5 \frac{(x - f)^4}{r^4} \right).$$

But

$$r^4 = (x - f)^4 + (y - g)^4 + (z - h)^4 + 2\,(x - f)^2 (y - g)^2$$
$$+ 2\,(x - f)^2 (z - h)^2 + 2\,(y - g)^2 (z - h)^2$$

$$\therefore 3\,\Sigma \frac{m\,(x - f)^4}{r^9} = \Sigma \frac{m}{r^5} - 6\,\Sigma \frac{m\,(x - f)^2 (y - g)^2}{r^9}$$

$$= \Sigma \frac{m}{r^5} - 2\,\Sigma \frac{m\,(x - f)^4}{r^9} \text{ (by A.)},$$

$$\text{or } \Sigma \frac{m}{r^5} = 5\,\Sigma \frac{m\,(x - f)^4}{r^9} \ldots (\mathrm{B.})$$

Hence the coefficient of δ^4 is zero.

So far, then, as we have proceeded, we have obtained, as our result, that V is constant. We have thus strengthened the argument, if any exists, based on the neutrality of the equilibrium. But what is the argument? Mr. Earnshaw says (Art. 8), "the displacements of particles placed in such positions as those here considered would not bring into action any forces of restoration; on which account the particles would not vibrate." Mr. O'Brien says, too, "I have shown ...... that if such be the case the whole universe is in a state of neuter equilibrium." [Phil. Mag. June, p. 487.] The only shadow of an argument contained in these quotations exists in the words "on which account the particles would not vibrate." What would they do then? and why? It really is hard that I should be obliged to make the objections and answer them too. I hope Mr. Earnshaw will point out, in a future communication, whereon he supposes the inference to hang. So far as is stated nothing more appears than this: a particle is moved, no instantaneous force is put in play by the motion; therefore the particle cannot vibrate. Now to this argument we reply,—1st, that the statement embodies a proposition which is very difficult of proof: for although the particle receives no instantaneous force, it certainly communicates one to the adjacent molecules. On those in advance it acts more powerfully, on those behind less so, than when in its position of rest. Motion will therefore ensue. Whether the particles will vibrate or not we do not affirm; the *onus* of proving that they will not, rests with those who make the assertion. But 2nd, suppose it could be proved that the particles will not vibrate, what follows? I repeat that we do not attempt to explain how vibrations are generated. It is not to be conceived that the motion of a single particle should produce a system of *transverse vibrations*; and he who rejects every hypothesis which will not admit such to be the case, excludes virtually (if I mistake not) the possibility of the existence of such vibrations. All that can be made to follow from the above inference, therefore, appears to be, that the motion of a single particle cannot put in play a system of vibrations. This is a very different thing indeed from what is supposed to be made out by it, viz. "that the constitution of such a medium is incapable of *transmitting* light, a phænomenon due to vibration." When it shall have been shown to be incapable of *transmitting* vibrations, it will be time to reject it; but nothing of the kind has as yet been attempted.

c. From what has preceded, it will be evident that we conceive the constitution of media to be such that the equilibrium is of the kind technically called *neuter*; yet as we are desirous of saying a few words relative to the argument actually insisted on by Mr. Earnshaw, we propose to examine briefly the contrary case.

Let us suppose the medium unsymmetrical; and let us further conceive (which by no means necessarily follows) that $\frac{d^2V}{df^2}$, $\frac{d^2V}{dg^2}$ and $\frac{d^2V}{dh^2}$ are not zero. Then, as Mr. Earnshaw has proved (Art. 12), there is at least one direction in which, if a particle be moved, the immediate tendency is to cause it to recede further from its position of rest. The consequence will be, either that the other particles by their motion tend to stop it, or that its motion continues. We have no hesitation in affirming that the former is the case. If all the particles commence to move in the same direction, the principle of the conservation of the motion of the centre of gravity will be violated. If, on the contrary, some move in one direction, some in the opposite, there must be vibration unless it can be shown that the particles pass each other. In the latter case there would be perpetual interchange of place amongst the particles. This is certainly very unlikely: but even now admitting the worst we can conceive, the possibility of such a system is not disproved. As it stands at present, I am disposed to think that the objections, based on a want of stability, have rather strengthened than undermined the hypothesis of the inverse square of the distance. The fact, that in a medium of symmetry the equilibrium is neuter, is a very strong one in favour of the theory. But for this it might have required some violent effort to move a particle at all: as it is, a very slight force will cause motion, so that the medium possesses the character of molecular non-resistance. We do not doubt, however, that there are some difficulties attending this as well as every other theory. To any which may be brought forward I will do my best to reply. I trust that a desire for truth, rather than a love of controversy, will appear in all that shall be said on either side.

Since the above remarks were written Mr. Earnshaw has resumed his objections, in a paper which appears in the Philosophical Magazine for July. Although all the arguments which appear in that paper have not reference, either to the want of fulfilment of the requisites for vibration, or to the instability of the medium, yet to avoid confusion I propose to reply to them in this place. The consideration of the other two objections placed at the head of this paper will probably demand a more detailed mathematical investigation than could possibly appear within my present limits, on which account I desire to reserve it to a separate communication.

[To be continued.]

XXXVII. *On the Connexion between the Phænomena of the Absorption of Light, and the Colours of thin Plates.* *By Sir* David Brewster, *K.H., LL.D., F.R.S.*[*]

SINCE the phænomena of the absorption of light by coloured media began to be studied with attention, various philosophers have regarded them as inexplicable by the theory of the colours of thin plates, and have consequently regarded Sir Isaac Newton's theory of the colours of natural bodies as either defective in generality, or altogether unfounded. Mr. Delaval[†] was the first person who brought an extensive series of experiments to bear upon this subject. Dr. Thomas Young[‡] considered it "impossible to suppose the production of natural colours perfectly identical with those of thin plates," unless the refractive density of the particles of colouring bodies was at least twenty or thirty times as great as that of glass or water, which he considered as "difficult to believe with respect to any of their arrangements constituting the diversities of material substances." Sir John Herschel has expressed a still more decided opinion upon this subject. He regards, "the speculations of Newton on the colours of natural bodies" as only "a premature generalization," and "limited to a comparatively narrow range; while the phænomena of absorption, to which he considers the great majority of natural colours as referable, have always appeared to him to constitute a branch of photology *sui generis*[§]."

The general opinion advanced by these three philosophers I have long entertained[||]; and with the view of supporting them I have analysed a great variety of colours which are exhibited by the juices of plants. In a paper "On the Colours of Natural Bodies[¶]," I have shown that the *green* colour of plants, the most prevalent of all the colours of natural bodies, in place of being a *green of the third order*, as Newton and his commentators assert, is a colour of no order whatever, and having in its composition no relation at all to the colours of thin plates.

---

[*] From the Philosophical Transactions, 1837, p. 245.

[†] Manchester Memoirs, vol. ii. p. 131.

[‡] Elements of Nat. Phil. vol. i. p. 469, 481; and vol. ii. p. 638.

[§] Philosophical Magazine, Dec. 1833, S. 3, vol. iii. p. 401. See also his Treatise on Light, Encyc. Metrop., p. 580, 581.

[||] Life of Newton, chap. vii.

[¶] Edinb. Trans., vol. xii. [Also Phil. Mag., Third Series, vol. viii. p. 468.]

In arriving at these conclusions, however, and drawing a distinct line between the phænomena of absorption and those of thin plates, two classes of facts are compared under very different circumstances. In the one case philosophers have studied *in cumulo* the result of the successive actions of an infinite number of the colorific particles upon the intromitted light, whereas in the other case they have observed only the colour of a single particle, whose thickness is equal to that of the films of air, water, glass and mica submitted to experiment. The impracticability of combining a number of such films, and studying their united action upon light, was doubtless the reason which prevented natural philosophers from bringing the two series of facts under the same conditions. Sir Isaac Newton, indeed, had spoken so confidently of the result of such a combination, as to discourage any attempts to effect it; and it is a singular fact that his successors have never called in question his bold though ingenious assumption. "If a thinned or plated body," says he, "which being of an even thickness, appears all over of an uniform colour, shall be slit into threads or broken into fragments of the same thickness with the plate, I see no reason why every thread or fragment should not keep its colour, and by consequence why a heap of those threads or fragments should not constitute a mass or powder of the same colour which the plate exhibited before it was broken. And the parts of all natural bodies being like so many fragments of a plate, must on the same grounds exhibit the same colours."

This remarkable opinion I have often been desirous to submit to the test of direct experiment, in the conviction that the result would be different from what is here stated; but I have been baffled in every attempt to make such an experiment; and had not accidental circumstances placed in my hands two substances in which thin plates were combined nearly in the very manner which I wished, and which I believe had never before been submitted to examination, the problem might have remained long without a solution.

The first of these substances to which my attention was called, is the remarkable nacreous body which Mr. Horner has described in the last volume of the Transactions, and whose singular optical properties I have explained in a letter which accompanies his paper. This substance consists of laminæ of considerable transparency, separated by extremely thin films, which exhibit in the most brilliant manner the colours of thin plates.

In order to compare the effect produced by a number of such films with that of a single film, we must either analyse the light reflected and transmitted by a single film by means of a fine prism placed in front of a telescope, or examine the prismatic spectrum produced by such an apparatus when it is reflected or transmitted by the film in question. When we thus examine the reflected tints of the three first orders of colours, we find them to consist of that part of the spectrum which gives the predominating colour of the tint mixed with the rays on each side of it. The reflected *green* of the *third* order, for example, consists of the green part of the spectrum, bounded on one side with some blue, and on the other side with some yellow rays, all the rest of the spectrum being wanting, having passed, as it were, into the transmitted beam. In analysing, therefore, the transmitted beam, its spectrum is found to consist only of the violet and blue, and the orange and red spaces, a dark band corresponding to the reflected spectrum separating it into two parts. In the higher orders of colours the reflected spectrum consists of two or more portions separated by perfectly dark bands, while the transmitted light exhibits analogous bands, which are much less dark in consequence of the tint being diluted with a portion of white light. The coloured bands of the reflected spectrum occupy the same place among the fixed lines of the spectrum as the dark bands of the transmitted one; and if the two spectra were superposed they would form a perfect spectrum, whose rays when united would form white light. Hence the reflected and the transmitted tints are complementary to each other.

When this analysis is made with a highly magnified spectrum, the numerous lines of which are distinctly seen, it forms one of the most splendid experiments in optics. The spectrum is crossed throughout its whole extent with alternate dark and coloured bands, increasing in number and diminishing in magnitude with the thickness of the plate by which the tint is produced.

If we use a thin film of mica, of such a thickness as polarizes the *white* of the first order, the transmitted spectrum will be crossed by upwards of three hundred dark and three hundred luminous bands, thirty-four of each being included between the lines C and D of Fraunhofer, a space less than one tenth of the whole spectrum.

When we use *polarized light*, and interpose a *doubly refracting plate*, and subsequently analyse the transmitted beam, the spectrum is crossed with an analogous series of bands, which are still more splendid and more perfect than those given by a singly refracting film. The bands in the complementary spectra are equally and perfectly dark; and when the tints are pure as in calcareous spar, the colours are nearly identical with those of thin plates. Through the natural faces of a rhomb of calcareous spar about one sixth of an inch thick, I observed in the space C D above mentioned hundreds of the most minute lines almost as sharp and black as those in the solar spectrum.

In the phænomena of periodical colours which we have now described, there are three peculiarities which demand our attention. 1. The dark lines change their place by inclining the plate which produces them. 2. Two or more lines never coalesce into one, and one line of the series is never seen without all the rest being equally visible. 3. The colours of the luminous bands in the complementary spectra are the same as those of the original spectrum when the thin plate is perfectly colourless. In the case of polarized tints this similarity is not general.

In order to obtain a correct idea of the phænomena of absorption, I shall describe those which are exhibited by a *solid*, a *fluid*, and a *gaseous* body,—by the common *smalt blue glass*, by the *green sap* of vegetables, and by *nitrous acid gas*.

Dr. Young has described the smalt blue glass as dividing the spectrum "into *seven* distinct portions." I have given in the Edinburgh Transactions* rude coloured drawings of the effect it produces on the spectrum, and Sir John Herschel† has represented its action in a different manner. Excepting in the single circumstance of the spectrum being divided into bands, there appears no analogy whatever between this phænomenon and those of thin plates. The bands diminish in number as the thickness of the plate increases, and their colour suffers no other change by inclining the plate but that which arises from the small increase of thickness which the ray traverses. There is one remarkable point of difference between the two classes of phænomena which requires to be specially attended to. The *colours of some of the luminous bands are not the same as those of the spectrum*, and therefore the glass has removed certain colours while it has left others of exactly the same refrangibility. The *green*, for example, is changed into *yellow* by the removal of blue rays, and in certain glasses a band, *almost white*, is produced. The colours thus removed are said to be absorbed; and by an extensive series of experiments with such absorbing substances I have been able to insulate white light in the spectrum, which no prism can decompose, and to establish the existence of three equal and superposed spectra of red, yellow and blue light.

Analogous phænomena are exhibited in an alcoholic solu-

---

* Vol. ix. p. 439. pl. xxvii.

† Ibid. p. 449. pl. xxviii.

tion of the colouring matter of the green leaves of vegetables. The spectrum which it forms consists of *six* luminous bands, separated by *five* dark ones*, and the phænomena have the same character as those of the blue glass.

When the spectrum is viewed through nitrous acid gas the phænomena are still more remarkable. While the gas exerts a general absorbent action over the violet extremity of the spectrum, it attacks it when in a diluted state in definite lines as sharp and distinct as those in the solar spectrum; and what is still more important, it acts upon the same parts of light as the cause which produces the fixed lines in the sun's spectrum. In other respects the character of its action is similar to that of the blue glass and the green sap of plants.

In thus comparing the phænomena of absorption with those of thin plates, we find no connecting link but that of giving a divided or a mutilated spectrum; and even this common fact has not the same character in both. In coloured media the bands of light and darkness have no fixed relation, as in periodical colours; and the light removed from the dark portions, as well as the tints from some of the coloured spaces, have wholly disappeared, in place of being found in the reflected beam.

I have already mentioned, that by the aid of two substances I have been able to study this subject under a new aspect, and that the nacreous substance described by Mr. Horner was the one which first exhibited to me the connexion between absorption and periodical action.

This substance when it contains no thin plates acts generally in absorbing the violet and blue end of the spectrum; but when it includes within it, or has on its surface thin films which act like thin plates, it exercises an additional action upon the spectrum. In some cases when the thickness of the plate is small, it produces bands perfectly identical with those of thin plates, but in other cases the bands are exactly similar to those of coloured media. In one specimen I obtained a dark and distinct band in the orange space at D, with another faint band in the red. These bands were parallel to the fixed line D at a vertical incidence, but by inclining the plate the bands moved towards the green space, and became inclined to the line D. In a recent specimen I obtained the darkest band in the green space, with other lesser bands of unequal size and breadth in the other spaces, all of which moved along the spectrum, while new ones advanced from the red ex-

---

* A full account of this experiment, and a coloured drawing of the divided spectrum, will be found in the Edinburgh Transactions, vol. xii.

tremity during the inclination of the plate. In a third specimen the phænomena were still more varied, and what was a new feature in the results, the *colour* of the tints was changed exactly as in the phænomena of absorption. It is very obvious that these results are not produced by the same action which causes the orange colour of the substance, for this action could not vary by the inclination excepting in producing a greater absorption of the more refrangible rays; but in order to place this beyond a doubt, I detached a film which had none of the colours of thin plates, and which, as I expected, produced none of the bands above described. In these experiments the nacreous plate was placed in Canada balsam to remove the imperfect smoothness of its surface, but the phænomena were essentially the same with plates surrounded by air. I now divided the first of the plates above mentioned into two, and having viewed the spectrum through both, I found the principal black band considerably widened, as happens with absorbent media.

When the light reflected from the nacreous plates is examined in a similar manner, the division of the spectrum into bands is extremely brilliant and beautiful, and the phænomena the same; but owing to the light having entered the substance to different depths before it was reflected, the spectrum is by no means complementary to the one seen by transmission.

Satisfactory as these experiments are, I was still desirous of obtaining similar results with perfectly transparent plates; but after failing in every attempt to combine them, I thought of trying the iridescent films of decomposed glass*. This idea succeeded beyond my most sanguine expectations. I obtained combinations of films which gave me by transmitted light the most rich and splendid colours, surpassing anything that I had previously seen either among the colours of nature or of art. I obtained the deepest and richest blues shading off into the palest, and the finest reds and yellows, with all those intermediate and mixed tints which are seen only in the vegetable kingdom. The reflected tints had quite a different character. They possessed all the brilliancy of metallic reflexion, like the colours in the Diamond Beetle and other insects, and the tints varying within a considerable range were disposed in straight lines and bands, as if the film had formed part of a regularly organized body†.

---

* For a very fine collection of these films I have been indebted to the kindness of Mrs. Buckland, the Marquis of Northampton, and Mr. Children.

† The surface of these films is beautifully mammillated, the parts that are curves on one side being concave on the other.

The reflected tints of course vary with the obliquity of the incident light; and at great incidences the transmitted ones, however splendid and varied, all become pale yellow. When these combinations of glass films are immersed in a balsam or an oil, their colours, whether transmitted or reflected, all disappear, excepting a pale yellow light like that which is transmitted at great incidences. These facts prove, beyond a doubt, that the transmitted colours, though wholly unlike to those of thin plates, are yet produced by the same cause, and are residuary, and generally complementary to the hue of the reflected tints.

The analysis of these colours by the prism affords a series of most beautiful and instructive phænomena, and it is only by coloured drawings that any adequate idea of them can be conveyed. All the phænomena of coloured media, with bands of various breadths and various intensities of illumination, are exhibited in great perfection, so as to identify completely *in this feature* the two classes of facts. But what is still more striking, the colours of the bands are changed, and we thus find that the characteristic phænomenon of absorption is produced by the action of thin plates. To such a degree indeed is the change of tint carried, that I have insulated a white band in the orange part of the spectrum.

Notwithstanding this identification of absorption and periodical action in their primary features, there are two points of difference which separate widely the two classes of phænomena: the first of these is, that the bands and tints of absorbing media are not changed by obliquity; and the second, that the reflected tints are not visible in such media. Sir Isaac Newton endeavoured to remove the first of these difficulties by supposing that the particles of bodies on which their colours depended have an enormous refractive power; and M. Biot * has endeavoured to meet it more effectually by introducing two new suppositions; viz. that the particles are capable of transmitting light *only through their centre of gravity*, and that the lateral transmissions may be prevented or turned aside by the inflecting forces which act at a distance on the luminous molecules which approach them.

These explanations of the uniformity of the tints at all incidences have been rendered necessary, not perhaps by the real difficulties of the case, but in consequence of Sir Isaac Newton and his followers taking it for granted that the colours of natural bodies were pure tints of a particular order. Hence it becomes a necessary assumption in the theory that

---

* Traité de Physique, tom. iv. p. 126.

the particles had sizes corresponding to these pure tints, and that the light which composed them should not pass through different thicknesses of these particles. As I have demonstrated, however, in a paper already referred to, that the tint which Newton reckoned one of the third order, has no connexion whatever with that or with any other order, and that all other tints of absorbent media are in the same predicament, we are not only free from the difficulty which embarrassed Newton; but it is actually necessary to have recourse to particles of an ordinary refractive power, and having such forms and occupying such positions as will permit lateral transmissions and thus produce compound tints, such as we actually observe in natural bodies, and as we have shown to be produced by thin plates.

Now if we suppose the colouring particles to be spherical, or to have the form of plates or cubes, or other solids disseminated through the fluid or solid bodies which they colour, the tints would be permanent and compound as we find them in nature.

The second point of difference to which I have referred, namely, the absolute disappearance of the reflected tints in several coloured solids, fluids, and gases, is one of great magnitude. Newton has evaded this difficulty in his theory; but from the manner in which he gets rid of the intromitted light in black bodies, it is obvious that he would ascribe the disappearance of the reflected tints to their being "variously reflected to and fro until they happened to be stifled and lost."

As I shall have occasion to discuss this subject experimentally in a paper on the permanent colours of natural bodies, I shall only state at present that I have succeeded by particular methods in rendering reflected tints visible in many coloured fluids and glasses, but I cannot consider them as equivalent to the reflections of thin plates.

I have endeavoured to corroborate the views contained in the preceding pages by a series of collateral experiments on the periodical colours of polarized light. When we divide the spectrum into bands by doubly refracting plates, the phænomena are beautiful beyond all description. If we dissect or subdivide the luminous bands in the spectrum, as seen by one analysing prism, by means of successive plates and prisms, the result is very remarkable; and if the doubly refracting plates are inclined to each other or to the incident beam, the black bands will also be inclined to each other, and the luminous spaces have the form of a triangle either complete or truncated at its apex. By using plates of the same or of various substances *, and placing their axes in different azimuths to the plane of primitive polarization, we obtain extremely singular spectra, in which the bands approximate to those of absorbing media.

But there is another result of this class of experiments to which I would especially call the attention of philosophers. The colours of the bands thus produced have no resemblance to those of the original spectrum, so that the spectrum has actually been analysed by dissection. This effect is so decided, that even by a single subdivision of a banded spectrum I have succeeded in insulating a band nearly *white*, and of course incapable of being decomposed by the prism.

Hence we deduce from the phænomena of thin plates, and polarized tints, the existence of a new property of light, in virtue of which the reflecting force selects, as it were, out of differently coloured rays of the same refrangibility rays of a particular colour, allowing the others to pass into the transmitted beam; or to use the language of the undulatory theory, the colour produced by the interference of homogeneous pencils reflected from the first and second surfaces of thin plates, is different from the colour produced by the interference of the transmitted light with that which has suffered two internal reflexions within the plate. If, for example, we use the *greenish yellow* light of the spectrum between the lines D and E, the system of reflected rings will be more *yellow* than the transmitted rings towards E, and more *green* than the same rings towards D; a result, which, in so far as the transmitted tints are concerned, is seen in the colours of smalt blue glass.

Here then we have a principle not provided for in either of the theories of light to which the phænomena of absorption,

---

* I have constructed apparatuses of this kind made out of composite crystals of calcareous spar, including one and more thin plates of its own substance. The beautiful and apparently capricious tints which such crystals exhibit when properly cut into prisms, or when prisms are applied to their surface, are nothing more than the luminous bands of the spectrum subdivided by one or more dissections. I have now before me such a crystal, in which a prism cemented externally brings out the spectrum, which would otherwise have suffered total internal reflexion. A virtual prism forming part of the rhomb polarizes the incident light, an included hemitrope plate affords the polarized tints, and a second virtual prism analyses the light which the plate transmits. In some parts of the rhomb there are plates of different thickness, by which the luminous bands are beautifully subdivided. In this manner, by the slight aid of an applied prism, we are furnished with a complicated optical apparatus. Such a combination, which it is easy to make artificially by inclosing thin doubly refracting plates between prisms of calcareous spar, affords an ocular explanation of those beautiful forms of the system of polarized rings which are produced in composite crystals of calcareous spar. These subdivided bands, indeed, are portions of that system seen obliquely by prismatic refraction.

produced by nacrite, by decomposed films of glass and by polarizing plates, are distinctly referable. Here also we have the probable cause of certain remarkable phænomena of dichroism in doubly refracting bodies, in which rays of the same refrangibility, but of different colours, pass into the ordinary and extraordinary pencils.

Allerly, May 5th, 1837.

XXXVIII. *On the Theory of the Dispersion of Light*; *in reply to* Prof. Powell's *Note.* *By* S. Earnshaw, *M.A.*, *Cambridge*[*].

THE object which I had in view in writing the letter printed in your Magazine for April, was to show that the "opprobrium of all theories,—the dispersion of light,"—has not yet been removed from the undulatory theory. I endeavoured to accomplish this object by showing two things;—1st, that a certain formula, derived directly from theory, which was said to have supplied "both the laws and the explanation of the phænomena of dispersion," is insufficient for that purpose; and 2ndly, that the method of calculation employed in compiling the tables given in Professor Powell's book is a method of interpolation only, and therefore from its very nature *incapable* of verifying a physical theory of dispersion. It is not necessary to repeat the arguments by which I endeavoured to establish these two points. In answer to the former, the Professor distinctly states that he has "long since discarded" the formula animadverted upon; and therefore I suppose that, as far as that formula is concerned, I may consider the object of my letter accomplished. In answer to the remaining parts of my letter, the Professor, if I rightly understand his note, puts forward three arguments:—

1st. That Sir W. R. Hamilton has taken the trouble of simplifying the mode of calculation, a circumstance which implies his approval of the general principle.

2ndly. That that "pre-eminently gifted mathematician M. Cauchy" has considered his own investigations a sufficient basis for calculation; and,

3rdly, That the method of calculation used in computing the tables "is surely, at all events, a direct deduction from theory."

Now I will not accuse Professor Powell of bringing forward the first two of these with the *intention* of carrying the disputed point by the force of great names; but if such had been his intention, they are, as it seems to me, better suited for

---

[*] Communicated by the Author.

that mode of argument than for fair philosophical discussion. I am willing to pay my humble tribute to the merits of the two eminent philosophers quoted; but the matter in dispute between Professor Powell and myself lying entirely within the limits of my own reading and understanding, it is not likely that I shall be convinced by any other than a fair appeal to philosophical argument.

With respect to the Professor's third argument, it appears to me to assume too much. It ought to have been shown that theory has done more for the series (upon which the calculations are founded) than merely to indicate that it must proceed according to inverse powers of λ; for if it has not done more than this, it has in effect done nothing. But even granting that there is something meritorious in the form of the suggested series, I would beg the Professor's attention to two of my objections which still remain in force;—1st, that the mode of applying it to calculation disconnects it from theory, by rendering the method one of ordinary interpolation; and 2ndly, that the results obtained do not coincide sufficiently with experiment to warrant us in concluding from them that the *form* of the series furnished by theory *is the correct one*.

Before I conclude it is necessary to advert to two other matters: the Professor seems to consider that I have used him unfairly in not distinguishing between "certain earlier researches" and those contained in his "published volume." If the Professor will turn again to my letter (p. 309) he will there read that the errors of which I had been speaking, are charged only upon "the *first* applications of the method." I trust therefore he will be satisfied that I am not guilty of the unfairness of which he complains, and have not committed those "remarkable oversights" of which he (somewhat unfairly I think) accuses me. And with respect to his having discarded his earliest researches,—"the simple circumstance which renders *all* my elaborate criticisms superfluous,"—I do not regard it as being by any means so fatal to my letter as the Professor seems to think it is: for if he will do me the favour to refer to my letter again he will find that the first part only was directed against the "superseded researches," the second part he will find summed up in these words: "the methods of computation employed in compiling the tables contained in *the book* referred to are wholly unconnected with a physical theory of dispersion, and therefore were they even coincident with experiment add nothing to the strength of M. Cauchy's theory; and were they even more discordant than they are with experiment, tend in no degree to overturn it." But supposing that my criticisms upon "the published volume" could be set aside by the Professor's abandonment of his earliest researches, I think in having produced a distinct public declaration of this fact it has done service to science, and therefore to that extent my desire has been accomplished, for it cannot be said that there is in the published volume any statement to the effect that those researches were to be considered as superseded by the book; so far otherwise indeed, that we are told in the introduction that it is sent forth "partly as a *résumé* of previous researches which have from time to time appeared, and partly as supplying what was *wanting to complete* them," and more than once the early researches are referred to in terms of approval. It is clear therefore that without a distinct declaration, such as my letter has drawn forth, neither I nor any other person would have been justified in treating as discarded the researches in which the author has stated it to be his opinion that "the refractive indices are related to the lengths of waves, *as nearly as possible* according to the formula deduced from M. Cauchy's theory."

August 11, 1842.

XXXIX. *Proof of Professor* Wallace's *Property of the Parabola. By* Henry Albert Goodwin, *Esq.*[*]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

If the accompanying proof of Professor Wallace's property of the parabola appears to you to have any advantage over former solutions in symmetry and conciseness, it is much at your service. My object in offering it is to exemplify the great use of the simple equation to the tangent, which I have used, and because the method employed brings out the result in a most direct manner.

I am, Gentlemen, yours obediently,

Corpus Christi College,
Cambridge.

Henry Albert Goodwin.

Let $\alpha_1 \alpha_2 \alpha_3$ be the tangents of the $\angle$s which the three tangents make with the axis of x. The equations to these tangents are

$$y = \alpha_1 x + \frac{m}{\alpha_1} \text{ (1.)} \quad y = \alpha_2 x + \frac{m}{\alpha_2} \text{ (2.)} \quad y = \alpha_3 x + \frac{m}{\alpha_3} \text{ (3.)}$$

(1.) and (2.) intersect, $\therefore$ if $x_1 y_1$ be the coordinates of point of intersection $\quad x_1 = \dfrac{m}{\alpha_1 \alpha_2} \quad y_1 = m\dfrac{\alpha_1 + \alpha_2}{\alpha_1 \alpha_2}$;

---

[*] On the subject of this paper, see p. 191 of the present Number.—Edit.

(2.) and (3.) intersect, ∴ if $x_2 y_2$ be the coordinates of point of intersection, $x_2 = \frac{m}{\alpha_2 \alpha_3}$ $y_2 = m \frac{\alpha_2 + \alpha_3}{\alpha_2 \alpha_3}$.

Hence the equation to a line through the first point of intersection and the focus will be

$$y = \frac{\alpha_1 + \alpha_2}{1 - \alpha_1 \alpha_2} (x - m) = k_1 (x - m) \text{ suppose} \qquad (4.)$$

and the corresponding equation to the line through the second point of intersection and the focus will be

$$y = \frac{\alpha_3 + \alpha_2}{1 - \alpha_3 \alpha_2} = k_2 (x - m) \text{ suppose} \; \ldots\ldots \qquad (5.)$$

and if ϕ be the ∠ contained by these lines, $\tan \phi = \frac{k_1 - k_2}{1 + k_1 k_2}$, which by reduction from (4.) and (5.) manifestly becomes

$$\tan \phi = \frac{\alpha_1 - \alpha_3}{1 + \alpha_1 \alpha_3} \quad \frac{(1 + \alpha^2_2)}{(1 + \alpha_2^2)}$$

$$= \frac{\alpha_1 - \alpha_3}{1 + \alpha_1 \alpha_3}.$$

Hence ϕ is clearly supplementary to the angle between tangents (1.) and (3.), and the circle described about the △ formed by the tangents (1.) (2.) (3.) will of course pass through the focus.

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

(Continued from p. 55.)

May 5, 1842.* (*Continued.*) A PAPER was also read, entitled, "On Fibre:" additional observations. By Martin Barry, M.D., F.R.S., Lond. and Ed.†

On examining coagulating blood, the author finds that it contains discs of two different kinds; the one comparatively pale; the other, very red. It is in the latter discs that a filament is formed; and it is these discs which enter into the formation of the clot; the former, or the pale discs, being merely entangled in the clot, or else remaining in the serum. He thinks that the filament escaped the notice of former observers, from their having directed their attention almost exclusively to the undeveloped discs which remained in the serum,

---

* For abstracts of the other papers read on May 5th and 12th, see p. 54.—EDIT.

† We are requested by Dr. Barry to add the following as a correction of the fifth paragraph in the above abstract.

That the corpuscles of the blood are reproduced by means of parent-cells, and by division of their nuclei, he had recorded, not as conjectures, but as *observed facts.* (See Phil. Trans., 1841, p. 204 and 244, pl. xviii.)

Dr. Barry's previous observations on Fibre will be found in our last volume, p. 321, 344.—EDIT.

and thus conceived that the blood-discs are of subordinate importance, and are not concerned in the evolution of fibrin.

To render the filament distinctly visible, Dr. Barry adds a chemical reagent capable of removing a portion of the red colouring matter, without altogether dissolving the filament. He employs for this purpose chiefly a solution of one part of nitrate of silver in 120 parts of distilled water; and sometimes also the chromic acid. He admits that the use of these reagents would, on account of their destructive tendency when concentrated, be objectionable as proofs of the absence of any visible structure; but as the point to be proved is that a certain specific structure does exist, he contends that the same appearance would not equally result from the chemical actions of reagents so different as are those of chrome and the salts of mercury and of silver. After the appearance of the filament, thus brought to light, has become familiar to the eye, it may be discerned in the blood-discs, when coagulation has commenced, without any addition whatever. Those blood-discs of the newt, which contain filaments, often assume the form of flask-like vesicles, the membranes of which exhibit folds, converging towards the neck, where, on careful examination, a minute body may be seen protruding. This body is the extremity of the filament in question, its protrusion being occasionally such as admit of its remarkable structure being recognised.

The author proceeds to describe various appearances which he has observed in the coagulum of the blood, and which strongly resemble those met with in the tissues of the body, and are obviously referable to a similar process of formation. He bears testimony to the accuracy of the delineations of coagulated blood given by Mr. Gulliver. One of the most remarkable phænomena discovered by the author in the coagulation of the blood is the evolution of red colouring matter; a change corresponding to that which he had previously observed to take place in the formation of the various structures of the body out of the corpuscles of the blood. He considers the production of filaments as constituting the essential circumstance in coagulation.

He conjectures that the notched or granulated fibres noticed in the blood by Professor Mayer, may have been of the same kind as the flat, grooved, and compound filaments described by himself; but he thinks that, in that case, Mayer's explanation of their mode of origin must be erroneous; for they may be seen to be produced by a portion of the blood not mentioned by him, namely, the corpuscles.

Mr. Addison's discovery of globules in the uppermost stratum of inflammatory blood, and of their influence in the formation of the buffy coat, is confirmed by Dr. Barry, who remarks that these globules are altered red blood-discs. That the blood corpuscles are reproduced by means of parent-cells, as suggested by Mr. Owen and by the author, is confirmed by the observations of Dr. Remak; but the author had long ago indicated a division of the nucleus as being more particularly the mode of reproduction, not only of those corpuscles, but of cells in general. With this conjecture the observations of Remak on the blood-corpuscles of the fœtal chick fully accord. Whether the author's further speculation, namely, that the parent-cells are altered red blood-discs, is correct, still remains to be seen.

The phænomenon of the "breaking off short," or notching of the fasciculus of a voluntary muscle in a transverse cleavage of the fibre, is regarded by Dr. Barry as a natural consequence of the interlacing of the larger spirals, which he has described in a former paper; the fracture, in proceeding directly across the fasciculus, taking the direction in which there is least resistance.

The position of the filament in the blood-corpuscle is represented as bearing a striking resemblance to that of the young in the ovum of certain intestinal worms, the filaments of which are reproduced by spontaneous division. The author subjoins the following quære, "Is the blood-corpuscle to be regarded as an ovum?"

May 12.—The following papers were read, viz.—

"Barometrical Observations, showing the effect of the Direction of the Wind on the Difference between distant Barometers." By Lieut.-Colonel Philip Yorke, S. F. Guards. Communicated by Lieut.-Colonel Sabine, R.A., F.R.S., &c.

The author institutes a comparison between the barometric heights as observed at the Apartments of the Royal Society, and at his house in Herefordshire, in the neighbourhood of Ross, with a view to ascertain the influence of prevailing winds on the atmospheric pressure. The barometers thus compared together were of the same construction, and by the same maker; and the times of observation, namely nine o'clock A.M. and three o'clock P.M., were the same at both places, the distance between which is 110 miles in longitude, and about 20 in latitude. The degree of accordance in the march of the two barometers is exhibited by that of curves traced on three sheets accompanying the paper. The results are given in eight tables. The author agrees with Schubler in ascribing the currents prevailing in the atmosphere to the variable relations of heating and cooling which obtains between the Atlantic Ocean and the continent of Europe at different seasons; the facts ascertained by the series of observations here presented being in accordance with that hypothesis. If the northerly and westerly winds in England be partly the effect of the expansion of the air on the continent, then the barometer which is nearest to the continent, or in this instance that at London, ought to be relatively more depressed than the one more distant; or if the southerly and easterly winds be regarded as proceeding to the ocean, then, for a similar reason, the barometer nearest to the ocean ought to be relatively depressed; and that both these effects are produced, is shown by the tables. This view of the subject also, the author remarks, is corroborated by Raymond's observations, detailed in his memoir on the determination of the height of Clermont Ferrand, from which it appears that with the north winds, the southern barometer was most depressed; while the reverse occurred with the southerly winds.

May 26.—A paper was in part read, entitled, "On the Transparency of the Atmosphere, and the Law of Extinction of the Solar Rays in passing through it." By James D. Forbes, Esq., F.R.S., Sec. R.S. Edinb., Professor of Natural Philosophy in the University of Edinburgh.

June 2.—The reading of Prof. Forbes's paper was resumed and concluded.

This paper is divided into seven sections. In the first, the qualities of heat and light are considered in as far as they modify the comparability and absolute nature of our measures of the influence of the solar rays. All instruments, whether called *Thermometers*, *Photometers*, or *Actinometers*, measure but the peculiar effect to which their construction renders them sensible, but are incompetent to give absolute measures of either heat or light.

The second section treats of the history of the problem of the law and measure of extinction of the solar rays in passing through the atmosphere of the earth in clear weather. The labours of Bouguer, Lambert, De Saussure, Leslie, Herschel, Kämtz and Pouillet are successively passed under review, and their instrumental methods considered.

In the third section, a mathematical problem of considerable difficulty and interest is investigated; principally after the manner of Laplace. It consists in the determination of the length of the path and the mass of air which a ray of light must traverse in passing through the earth's atmosphere at every different angle of obliquity. The author determines the numerical value of these quantities for all angles of incidence from 0° to 90°.

The fourth section contains an account of the observations made by the author in conjunction with Professor Kämtz in 1832. These were conducted in 1832 at the top and bottom of the Faulhorn, a mountain of the canton of Berne in Switzerland. The lower station was Brientz, and the intercepted stratum of air had 6800 English feet of thickness, corresponding in its weight to about one-fourth of the entire atmosphere. Frequent observations were simultaneously made with the actinometer and other meteorological instruments at both stations, and the loss of solar heat in passing through the intervening mass of air was thus directly determined.

In the fifth section, the observations made from sunrise to sunset, on one peculiarly favourable day (the 25th September, 1832), are carefully analysed; and from the absorption at various obliquities, the law of extinction in the atmosphere, within the limits of observation, is attempted to be deduced.

The sixth and seventh sections include the results of similar, but less perfect observations in 1832 and in 1841.

From the facts and reasonings of this paper, the author deduces, on the whole, the following conclusions:—

1. The absorption of the solar rays by the strata of air to which we have immediate access is considerable in amount for even moderate thicknesses.

2. The diurnal curve of solar intensity has, even in its most nor-

mal state, several inflections; and its character depends materially on the elevation of the point of observation.

3. The approximations to the value of extra-atmospheric radiation, on the hypothesis of a geometrical diminution of intensity, are inaccurate.

4. The tendency to absorption through increasing thicknesses of air is a diminishing one; and in point of fact, the absorption almost certainly reaches a limit beyond which no further loss will take place by an increased thickness of similar atmospheric ingredients. The residual heat, tested by the absorption into a blue liquor, may amount to between half and a third of that which reaches the surface of the earth after a vertical transmission through a clear atmosphere.

5. The law of absorption in a clear and dry atmosphere, equivalent to between one and four thicknesses of the mass of air traversed vertically, may be represented, within those limits, by an intensity diminishing in a geometrical progression, having for its limit the value already mentioned. Hence the amount of vertical transmission has always, hitherto, been greatly overrated; or the value of extra-atmospheric solar radiation greatly underrated.

6. The value of extra-atmospheric solar radiation, on the hypothesis of the above law being generally true, is 73° of the actinometer marked B 2. The limiting value of the solar radiation, after passing through an *indefinite* atmospheric thickness, is 15° 2′.

7. The absorption, in passing through a vertical atmosphere of 760 millimeters of mercury, is such as to reduce the incident heat from 1 to 0·534.

8. The physical cause of this law of absorption appears to be the non-homogeneity of the incident rays of heat, which, parting with their more absorbable elements, become continually more persistent in their character; as Lambert and others have shown to take place, when plates of glass are interposed between a source of heat and a thermometer.

9. Treating the observations on Bouguer's hypothesis of a uniform rate of extinction to the intensity of the incident rays, the author obtains for the value of the vertically transmitted shares of solar heat in the entire atmosphere,—

By the *relative* intensities at Brientz and the Faulhorn... 0·6842

By the observations at the Faulhorn alone,—

First method .... 0·6848

Second method .... 0·7544

By the observations at Brientz alone,—

First method .... 0·7602

Second method .... 0·7827

June 9.—A paper was read, entitled, "On the Specific Inductive Capacities of certain Electric Substances." By William Snow Harris, Esq., F.R.S.

The author, pursuing the experimental inquiry suggested by the theory of Mr. Faraday relative to the differences in specific inductive capacity exhibited by different dialectric substances, instituted a series of experiments for determining with precision their comparative powers of insulation, and of sustaining by induction charges of electricity. The substances to be examined were cast into the form of circular plates and furnished on both their surfaces with circular coatings of tinfoil of a diameter equal to one-half that of the plate, and the electric intensities were measured by electrometers of the same construction as those which he used in his former experiments, and which he has described in his paper* already published in the Philosophical Transactions for 1839. The results are stated in tables; from the last of which it appears that the inductive capacities of the dialectric bodies tried, that of air being expressed by unity, are proportional to the following numbers:—

Substances.	Relative capacities.
Air	1
Rosin	1·77
Pitch	1·8
Bees' wax	1·86
Glass	1·9
Brimstone	1·93
Shell-lac	1·95

The author, in conclusion, offers some observations on the experimental processes employed in his investigation; and points out several circumstances which require to be attended to in order to ensure success.

June 16.—The following papers were read, viz.—

1. "On the Action of the Rays of the Solar Spectrum on Vegetable Colours." By Sir John F. William Herschel, Bart., K.H., F.R.S.

The author, having prosecuted the inquiry, the first steps of which he communicated in a paper read to the Royal Society in February 1840†, relating to the effects of the solar spectrum on the colouring matter of the *Viola tricolor*, and on the resin of guaiacum, relates, in the present paper, the results of an extensive series of similar experiments, both on those substances, and also on a great number of vegetable colours, derived from the petals of flowers, and the leaves of various plants. In the case of the destruction of colour of the preparations of guaiacum, which takes place by the action of heat, as well as by the less refrangible rays of light, he ascertained that although the non-luminous thermic rays produce an effect, in as far as they communicate heat, they are yet incapable of effecting that peculiar chemical change which other rays, much less copiously endowed with heating power, produce in the same experiment. He also found that the discoloration produced by the less refrangible rays is much accelerated by the application of artificial terrestrial heat, whether communicated by conduction or by radiation; while, on the other hand, it is scarcely or not at all promoted by the purely thermic rays beyond the spectrum, acting under precisely similar circumstances, and in an equal degree of condensation. The author proceeds to describe the photographic effects produced on papers

---

[* Noticed in Phil. Mag., Third Series, vol. xv. p. 320.—Edit.]

[† An abstract of the paper here referred to will be found in Phil. Mag., Third Series, vol. xvi. p. 331.—Edit.]

coloured by various vegetable juices, and afterwards washed with various solutions. The action of solar light he found to be exceedingly various, both as regards its total intensity and the distribution of the active rays over the spectrum. He observed, however, that the following peculiarities obtain almost universally in the species of action exerted on vegetable colours.

First, the action is *positive*; that is to say, light destroys colour, either totally, or leaving a residual tint, on which it has no further, or a very much slower action; thus effecting a sort of chromatic analysis, in which two distinct elements of colour are separated, by destroying the one and leaving the other outstanding. The older the paper, or the tincture with which it is stained, the greater is the amount of this residual tint.

Secondly, the action of the spectrum is confined, or nearly so, to the region of it occupied by the luminous rays, as contra-distinguished both from the so-called chemical rays beyond the violet, (which act with chief energy on argentine compounds, but are here for the most part ineffective,) on the one hand, and on the other, from the thermic rays beyond the red, which appear to be totally ineffective. Indeed, the author has not hitherto met with any instance of the extension of this description of photographic action on vegetable colours beyond, or even quite up to the extreme red.

Besides these, the author also observed that the rays which are effective in destroying a given tint, are, in a great many cases, those whose union produces a colour complementary to the tint destroyed, or at least one belonging to that class of colours to which such complementary tint may be referred. Yellows tending towards orange, for example, are destroyed with more energy by the blue rays; blues by the red, orange and yellow rays; purples and pinks by yellow and green rays. These phænomena may be regarded as separating the luminous rays by a broadly defined line of chemical distinction from the non-luminous; but whether they act *as such*, or in virtue of some peculiar chemical quality of the heat which accompanies them *as heat*, is a point which the author considers his experiments on guaiacum as leaving rather equivocal. In the latter alternative, he observes, chemists must henceforward recognize, in heat from different sources, differences not simply of intensity, but also of quality; that is to say, not merely as regards the strictly chemical changes it is capable of effecting in ingredients subjected to its influence.

One of the most remarkable results of this inquiry has been the discovery of a process, circumstantially described by the author, by which paper washed over with a solution of ammonio-citrate of iron, dried, and then washed over with a solution of ferro-sesquicyanuret of potassium, is rendered capable of receiving with great rapidity a positive photographic image; and another in which a picture negatively impressed on a paper washed with the former of these solutions, but which originally is faint and sometimes scarcely perceptible, is immediately called forth on being washed over with a neutral solution of gold. The picture does not at once acquire its full intensity, but darkens with great rapidity up to a certain point, when the resulting photograph attains a sharpness and perfection of detail which nothing can surpass. To this process the author applies the name of *Chrysotype**, to recall to mind its analogy with the Calotype process of Mr. Talbot, to which in its general effect it affords so close a parallel†.

2. "Experimental Researches on the Elliptic Polarization of Light." By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

This paper contains an experimental investigation of the phænomena of elliptic polarization resulting from the reflexion of polarized light from metallic surfaces, and the theory on which they are explicable; the analytical results being given in a tabular form, and applied to the cases of the experiments themselves.

3. "On the Influence of the Moon on the Atmospheric Pressure, as deduced from the Observations of the Barometer made at the Magnetic Observatory at St. Helena." By Lieutenant J. H. Lefroy, R.A., late Director of that Observatory. Communicated by Lieut.-Col. Sabine, R.A., F.R.S.

In order to determine the dependence of the barometric pressure on lunar influence, the author arranges all the two-hourly observations in each lunar month with relation to the time of the moon's passing the meridian; entering in one column the observation of each day nearest to the meridian passage, whether before or after; and entering in separate columns those corresponding to two hours, four hours, six hours, &c., before and also after that observation. The monthly means at every two hours from the meridian passage are then taken; and again, the means at the same intervals, for each three months from September 1840 to December 1841. From the results thus obtained the author states that it appears that the moon's passage over both the inferior and superior meridian produces a slight increase of pressure; a maximum in the curve occurring at both (that of the latter being slightly the greater), while the minima correspond to the moon's rising or setting.

It appears also, that the rise of the tides will not account for the whole amount of the increase of pressure, even admitting that it has a tendency to produce an effect of that nature. The times of maxima do not correspond; and there appears to be no atmospheric establishment. The pressure is greater about the period of new moon than at full moon; and greater in the third and fourth than in the first and second quarters; a result which agrees with that given by Mr. Howard for the climate of London. The observations of both years agree in making the pressure greater under the Perigee than under the Apogee. Mr. Howard had found that the mean pressure in Great Britain, which is in the opposite hemisphere from St. Helena, is greater under the Apogee than under the Perigee.

4. "Notices of the Aurora Australis from the 1st to the 31st of

---

* *Note by the Author.*—A solution of silver produces a like effect, and with greater intensity, but much more slowly. Consequently the name Chrysotype would seem less appropriate than Siderotype.—J. F. W. H.

[† Mr. Talbot's account of his Calotype process appeared in Phil. Mag., Third Series, vol. xix. p. 88, 164.—Edit.]

March 1841, made on board H.M.S. Erebus; extracted from the log-book." By Captain James Clark Ross, R.N., F.R.S.

5. "An Appendix to a paper on the Nervous Ganglia of the Uterus, with a further Account of the Nervous Structures of that Organ." By Robert Lee, M.D., F.R.S.

After premising a short history of the opinions of Galen, Dr. William Hunter, Mr. John Hunter, Professor Tiedemann, Professor Lobstein, and Professor Osiander, relative to the existence, course, and enlargement of the nerves of the uterus, the author adverts to his own researches on this subject, which commenced with his discovery, in April 1838, of the trunk of a large nerve accompanying the uterine vein, and of the great nervous plexus with which it was continuous. Of this discovery he gave an account to the Royal Society in a paper read on the 12th of December of the same year. In a subsequent paper, he described some large nervous ganglia* situated at the neck of the uterus; and in the present appendix he describes other nervous structures of still greater size which presented themselves to him, on a still more complete dissection which he made of a gravid uterus at the full period of gestation. It appears from the results of these dissections that the human uterus possesses a great and extensive system of nerves, which enlarge during pregnancy, along with the coats, blood-vessels, and absorbents of that organ, and which after parturition resume their original condition. It is chiefly through the influence conveyed by these nerves that the uterus is rendered capable of performing its various functions, and by which sympathies are established between it and other parts of the system.

6. "Magnetic-term Observations of the Declination, Inclination and Total Intensity, made at the Magnetic Observatory at Prague, for February, March, and April 1842." By C. Kreil, Director of the Prague Observatory. Communicated by S. Hunter Christie, Esq., M.A., Sec. R.S.

7. "Magnetic and Meteorological Observations for February 1842, taken at the Magnetic Observatory at Madras." Presented by the Honourable Court of Directors of the East India Company. Communicated by the Council of the Royal Society.

8. "Magnetic and Meteorological Observations from May 1841 to March 1842, made at the Observatory established by the Rajah of Travancore, at Trevandrum, transmitted to the Royal Society by command of His Highness the Rajah." By John Caldecott, Esq., F.R.S., Director of the Observatory at Trevandrum.

ROYAL IRISH ACADEMY.

[Continued from p. 68.]

May 24, 1841.—Professor MacCullagh read a supplement to his paper "On the dynamical Theory of Crystalline Reflexion and Refraction."

In his former paper on that subject (see Proceedings, 9th Dec. 1839,

---

* See Phil. Mag., Third Series, vol. xvi., p. 590; and vol. xix. p. 487.—Edit.]

Phil. Mag. S. 3, vol. xvi. p. 229) the author had given the general principles for solving all questions relative to the propagation of light in a given medium, or its reflexion and refraction at the separating surface of two media; but he had applied them only to the common case of waves, which suffer no diminution of intensity in their progress, and in which the vibration may be represented by the sine or cosine of an arc multiplied by a constant quantity. Some months after that paper was read, it occurred to him that he might obtain new and important results by substituting in his differential equations of motion a more general expression for the integral, that is (as usual in such problems), by making the displacements proportional to the sine or cosine of an arc, multiplied by a negative exponential, of which the exponent should be a linear function of the coordinates. Such vibrations would become very rapidly insensible, and would therefore be fitted to represent the disturbance which, in the case of *total reflexion*, takes place immediately behind the reflecting surface; and the laws of this disturbance being thus discovered, the laws of polarization in the totally reflected light would also become known, by means of the general formulæ which the author had established for all cases of reflexion at the common surface of two media.

The present supplement is the fruit of these considerations. It contains the complete theory of the new kind of vibrations, not only in ordinary media, but in doubly refracting crystals; and also the complete discussion of the laws of total reflexion at the first or second surface of a crystal, including, as a particular case, the well-known empirical formulæ of Fresnel for total reflexion at the surface of an ordinary medium.

The existence of vibrations represented by an expression containing a negative exponential as a factor, had been recognized by other writers, and was indeed sufficiently indicated by the phænomenon of total reflexion; but it was impossible to obtain the laws of such vibrations, so long as the general equations for the propagation of light were unknown.

The method of deducing these equations was given in the abstract of the author's former paper (see Proceedings, as above); but as they were not there stated, it may be well to transcribe them here. If then we put

$$X = \frac{d\eta}{dz} - \frac{d\zeta}{dy}, \quad Y = \frac{d\zeta}{dx} - \frac{d\xi}{dz}, \quad Z = \frac{d\xi}{dy} - \frac{d\eta}{dx} \quad . \quad . \quad . \quad (1.)$$

and suppose the axes of coordinates to be the principal axes of the crystal, the equations in question may be thus written:—

$$\left.\begin{aligned} \frac{d^2\xi}{dt^2} &= c^2\frac{dZ}{dy} - b^2\frac{dY}{dz}, \\ \frac{d^2\eta}{dt^2} &= a^2\frac{dX}{dz} - c^2\frac{dZ}{dx}, \\ \frac{d^2\zeta}{dt^2} &= b^2\frac{dY}{dx} - a^2\frac{dX}{dy}; \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

and if we further put

$$\xi = \frac{d\eta_1}{dz} - \frac{d\zeta_1}{dy}, \quad \eta = \frac{d\zeta_1}{dx} - \frac{d\xi_1}{dz}, \quad \zeta = \frac{d\xi_1}{dy} - \frac{d\eta_1}{dx}, \quad . \quad (3.)$$

they will take the following simple form:—

$$\frac{d^2\xi_1}{dt^2} = -a^2 X, \quad \frac{d^2\eta_1}{dt^2} = -b^2 Y, \quad \frac{d^2\zeta_1}{dt^2} = -c^2 Z, . \quad . \quad (4.)$$

in which it is remarkable that the auxiliary quantities ξ_1, η_1, ζ_1 are exactly, for an ordinary medium, the components of the displacement in the theory of Fresnel. In a doubly refracting crystal, the resultant of ξ_1, η_1, ζ_1 is perpendicular to the *ray*, and comprised in a plane passing through the ray and the wave normal. Its amplitude, or greatest magnitude, is proportional to the amplitude of the vibration itself, multiplied by the velocity of the ray.

The conditions to be fulfilled at the separating surface of two media were given in the abstract already referred to. From these it follows, that the resultant of the quantities ξ_1, η_1, ζ_1, projected on that surface, is the same in both media; but the part perpendicular to the surface is not the same; whereas the quantities ξ, η, ζ are identical in both. These assertions, analytically expressed, would give five equations, though four are sufficient; but it can be shown that any one of the equations is implied in the other four, not only in the case of common, but of total reflexion; which is a very remarkable circumstance, and a very strong confirmation of the theory.

The laws of double refraction, discovered by Fresnel, but not legitimately deduced from a consistent hypothesis, either by himself or any intermediate writer, may be very easily obtained, as the author has already shown, from equations (2.), by assuming

$$\xi = p \cos\alpha \sin\phi, \quad \eta = p \cos\beta \sin\phi, \quad \zeta = p \cos\gamma \sin\phi, \quad . \quad (5.)$$

where
$$\phi = \frac{2\pi}{\lambda}(lx + my + nz - st);$$

but the new laws, which are the object of the present supplement, are to be obtained from the same equations by making

$$\left.\begin{aligned} \xi &= \varepsilon\,(p \cos\alpha \sin\phi + q \cos\alpha' \cos\phi) \\ \eta &= \varepsilon\,(p \cos\beta \sin\phi + q \cos\beta' \cos\phi) \\ \zeta &= \varepsilon\,(p \cos\gamma \sin\phi + q \cos\gamma' \cos\phi) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

where ϕ has the same signification as before, and

$$\varepsilon = e^{-\frac{2\pi r}{\lambda}(fx + gy + hz)}$$

the vibrations being now elliptical, whereas in the former case they were rectilinear. In these elliptic vibrations the motion depends not only on the distance of the vibrating particle from the plane whose equation is

$$lx + my + nz = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

but also on its distance from the plane expressed by the equation

$$fx + gy + hz = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

and if the constants in the equation of each plane denote the cosines of the angles which it makes with the coordinate planes, we shall have λ for the length of the wave, and s for the velocity of propagation; while the rapidity with which the motion is extinguished, in receding from the second plane, will depend upon the constant r. The constants p and q may be any two conjugate semidiameters of the ellipse in which the vibration is performed; the former making, with the axes of coordinates, the angles α, β, γ, the latter the angles α', β', γ'.

As vibrations of this kind cannot exist in any medium, unless they are maintained by total reflexion at its surface, we shall suppose, in order to contemplate their laws in their utmost generality, that a crystal is in contact with a fluid of greater refractive power than itself, and that a ray is incident at their common surface, at such an angle as to produce total reflexion. The question then is, the angle of incidence being given, to determine the laws of the disturbance within the crystal.

The author finds that the refraction is still *double*, and that two distinct and separable systems of vibration are transmitted into the crystal. He shows that the surface of the crystal itself (the origin of coordinates being upon it at the point of incidence) must coincide with the plane expressed by equation (8.), a circumstance which determines the three constants f, g, h. The plane expressed by (7.) is parallel to the plane of the refracted wave; and a normal, drawn to it through the origin, lies in the plane of incidence, making with a perpendicular to the face of the crystal an angle ω which may be called the angle of refraction, so that if i be the angle of incidence, we have

$$\sin \omega = s \sin i,$$

the velocity of propagation in the fluid being regarded as unity.

To each refracted wave, or system of vibration, corresponds a particular system of values for r, s, ω. These the author shows how to determine by means of the *index-surface* (the reciprocal of Fresnel's wave-surface) which he has employed on other occasions (Transactions of the Academy, vol. xvii. and xviii.), and the rule which he gives for this purpose affords a remarkable example of the use of the imaginary roots of equations, without the theory of which, indeed, it would have been difficult to prove, in the present instance, that there are two, and only two, refracted waves. Taking a new system of coordinates x', y', z', of which z' is perpendicular to the surface of the crystal, and y' to the plane of incidence, while x' lies in the intersection of these two planes, put $y' = 0$ in the equation of the index-surface referred to those coordinates, the origin being at its centre; we shall then have an equation of the fourth degree between x' and z', which will be the equation of the section made in the index-surface by the plane of incidence. In this equation put $x' = \sin i$, and then solve it for z'. When i exceeds a certain angle i', the four values of z' will be imaginary, and if they be denoted by

$$u \pm v \sqrt{-1}, \qquad u' \pm v' \sqrt{-1},$$

each pair will correspond to a refracted system, and we shall have, for the first,

$$\tan \omega = \frac{\sin i}{u}, \qquad s = \frac{\sin \omega}{\sin i}, \qquad r = s v; \quad . \quad . \quad . \quad . \quad (9.)$$

and for the second,

$$\tan \omega' = \frac{\sin i}{u'}, \qquad s' = \frac{\sin \omega'}{\sin i}, \qquad r' = s' v' \quad . \quad . \quad . \quad . \quad (10.)$$

When i lies between i' and a certain smaller angle i'', two of the roots will be real, and two imaginary. The real roots correspond to waves which follow the law of Fresnel; the imaginary roots give a single wave, following the other laws just mentioned.

Lastly, when i is less than i'', all the roots are real, the refraction is entirely regulated by Fresnel's law, and the reflexion by the laws already discovered and published by the author.

If the crystal be uniaxal, and all the values of z' imaginary, the ordinary wave normal will coincide with the axis of x'; whilst the extraordinary wave normal and the axis of z' will be conjugate diameters of the ellipse in which the index-surface is cut by the plane of incidence.

When $a = b = c$, the crystal becomes an ordinary medium; there is then only single refraction, and the refracted wave is always perpendicular to the axis of x'.

With regard to the ellipse in which the vibrations are performed, it may be worth while to observe, that if it be projected perpendicularly on the plane of incidence, the projected diameters which are parallel to the surface of the crystal and to the wave plane will, in all cases, be conjugate to each other, and their respective lengths will be in the proportion of r to unity. The vibrations, it is obvious, are not performed in the plane of the wave, though they take place without changing the density of the æther.

The new laws here announced are, properly speaking, laws of double refraction, and are necessary to complete our knowledge of that subject. Between them and the laws of Fresnel a curious analogy exists, founded on the change of real into imaginary constants.

The laws of the total reflexion, which accompanies the new kind of refraction, need not to be dwelt upon in this abstract, as nothing is now more easy than to form the equations which contain them. In fact, the difficulties which formerly surrounded the problem of reflexion, even in the simplest cases, have completely disappeared, since the author made known the conditions which must be fulfilled at the separating surface of two media.

In what precedes, it has been supposed that the reflexion and refraction take place at the *first* surface of the crystal, because this is the more difficult and complicated of the two cases into which the question resolves itself. But it will usually happen in practice that a ray which has entered the crystal will suffer total reflexion at the *second* surface, while the new kind of vibration is propagated into the air without. The refracted wave will then be always perpendicular to the axis of x'; the two reflected rays, within the crystal, will be plane-polarized, according to the common law, but they will each undergo a change of phase; and the *vis viva* of the two rays together will be equal to that of the incident ray, the *vis viva* being measured by the square of the amplitude multiplied by the proportional mass.

In conclusion, the author states a mathematical hypothesis, by which both the laws of dispersion, and those of the elliptic polarization of rock crystal, may be connected with the laws already developed.

XLI. *Intelligence and Miscellaneous Articles.*

ON CURCUMINE. BY M. VOGEL, JUN.

To obtain the colouring principle of turmeric root, the author treated it, reduced to powder, repeatedly with boiling water, till it nearly ceased to be coloured by it. The dried residue, thus deprived by water of its mucilaginous, gummy, and a part of its extractive matter, was repeatedly boiled in portions of alcohol of specific gravity 0·8; this dissolved the greater part of the colouring matter, but it is not possible to extract it totally, for the turmeric powder always remains coloured; the alcoholic solution is to be filtered when cold, and is of a deep brownish-red colour. A portion of the alcohol is to be separated by distillation, and the residue is to be evaporated to dryness in a porcelain capsule. A brown viscid mass remains, which retains some brown extractive matter and traces of chloride of calcium, which is one of the salts that the root contains. To separate these two substances, M. Pelletier's plan was adopted; this consists in treating the residue with boiling æther, which becomes of a brownish-yellow colour. The extractive matter, which resists the action of the æther, is of a black colour, and attracts moisture from the air on account of the chloride of calcium which it contains. The decanted æther ought to be slowly evaporated, and after cooling, brownish-red fragments remain, which readily fuse, and may be poured into stone moulds or on glass plates. In this state the curcumine, when heated to redness on platina foil, does not leave the smallest residue of inorganic substances.

Attempts were made to volatilize the oil which the odour of the curcumine evinced that it still retained, by repeatedly fusing it; but as this method did not succeed perfectly, another was tried, which led to a more satisfactory result.

The residue obtained by evaporating the æthereal solution was dissolved in alcohol, and on the addition of an alcoholic solution of acetate of lead, a red precipitate was immediately formed; the salt of lead was added as long as precipitation occurred. When this precipitate is washed and dried, a reddish-yellow powder remains, which consists of the yellow colouring matter and oxide of lead, the proportion of the latter varying from 43·67 to 56·33 per cent. To separate the lead, the powder is to be diffused in water and treated with hydrosulphuric acid gas; when the action of this is complete, the powder, which has become of a deep brown colour, is to be washed and dried and treated with boiling æther, which dissolves the curcumine and leaves the sulphuret of lead.

By evaporating the æther slowly, the curcumine is deposited in thin laminæ, which are transparent and inodorous; when reduced to a fine powder, curcumine is of a beautiful yellow colour, which is more intense as the powder is finer; in small laminæ it is of cinnamon colour, but when held up to the light it is of a deep red colour.

By the process above described, about half an ounce of curcumine was obtained from a pound of the root; attempts were made, but in vain, to sublime and crystallize it. At 104° Fahr. it fuses, and even at common temperatures the fine powder agglutinates; it burns with a bright flame accompanied with much soot; by exposure to the sun's rays it soon loses its intense colour, and becomes gradually of a yellowish-white; as curcumine is insoluble in water, but very soluble in alcohol and in æther, it appears to resemble the resins.

M. Chevreul had already stated that curcumine is composed of oxygen, carbon and hydrogen, and M. Vogel proved that it contained no azote, by fusing it in a tube with six times its weight of hydrate of potash, no trace of ammonia being obtained.

The mean of four combustions of curcumine, prepared as above described, yielded

Carbon	69·501
Hydrogen ..	7·460
Oxygen	23·039—100·

Journal de Pharm. et de Chim., Juillet 1842.

ON THE ACTION OF ACIDS ON CURCUMINE. BY M. VOGEL, JUN.

Dilute acids do not dissolve curcumine, but the concentrated do. When concentrated sulphuric acid is poured upon powdered curcumine it is dissolved, and a crimson solution is obtained; the red colour immediately disappears on the addition of water, and greenish-yellow flocculi are deposited, which appear to be pure curcumine; and hydrochloric and phosphoric acids act in a similar manner, but concentrated acetic acid dissolves it without effecting any change in its colour.

The action of nitric acid differs from the above. One part of curcumine was mixed, in a porcelain capsule, with two parts of concentrated nitric acid, previously diluted with an equal volume of water; at common temperatures no change appeared to take place, but when heated in a sand-bath rapid action occurred, the liquid rose in bubbles, so that it was requisite to remove the vessel from the fire till the violence of the action ceased; after this the mixture was gently heated till it ceased to evolve any gas; by this action the curcumine is separated into a resinous mass, which is deposited in yellow fragments, and a yellow substance, soluble in water. The resinous substance, when repeatedly washed with hot water, and afterwards dried, may be easily reduced to a fine powder, which is yellow, and differs much from curcumine on account of its peculiar odour and elementary composition. The yellow substance, soluble in water, crystallizes from a concentrated solution in transparent needles; the quantity formed is however so small, and it deliquesces so readily in the air, that its chemical constitution has not been hitherto sufficiently examined.

The above-related experiments on the action of acids on curcumine readily explain how turmeric paper becomes of a brown colour by the action of concentrated acids, as well as by that of alkalies. The concentrated acids dissolve the curcumine and form a brown solution with it.—*Ibid.*

[There is, however, this difference between the action of concentrated acids and that of alkaline solutions upon turmeric paper: water immediately removes the colour occasioned by the former, but not that produced by the latter.—Edit.]

ACTION OF ALKALINE SUBSTANCES ON CURCUMINE.

Curcumine forms compounds with the alkalies, which are very soluble in water. When powdered curcumine is treated with caustic potash, a brown mass results which is very soluble in water. The curcumine is completely precipitated from this alkaline solution by diluted acids. Dilute sulphuric acid occasioned a precipitate in the alkaline solution, which, when sufficiently washed, had the properties of pure curcumine.

According to the observations of M. Kartner, it is not the alkalies and alkaline earths only which change the yellow colour of curcumine to brown, but the salts of lead, uranium, boracic acid and borates occasion the same change in a greater or less degree.

The shades of brown produced on turmeric paper by the alkalies and alkaline earths do not materially differ from each other; they depend on the concentration of the alkaline solutions employed. All weak acids restore the original yellow colour of turmeric paper browned by the alkalies: this happens simply because the acid combines with the alkali, and thus decomposes the brown compound of the alkali and curcumine. Turmeric paper, browned by a salt of lead, has its colour very readily restored by dilute acids; but when altered by the salts of uranium the colour is almost black, and the yellow colour is not restored until the paper has been immersed in tolerably concentrated acid for nearly a quarter of an hour.

A solution of boracic acid in alcohol alters turmeric paper to an intense orange colour, which is not removed by the action of any other acid; but when touched with ammonia, it assumes for a short time a fine blue colour, which soon disappears by the volatilization of the ammonia. This blue tint is also more or less shown by immersing paper browned by boracic acid in solutions of alkaline substances.

A solution of borax renders turmeric paper blackish-gray; the neutral borates of potash or ammonia impart to it a less intense gray colour.—*Ibid.*

INSOLUBLE SALTS OF THE ALKALINE EARTHS DISSOLVED BY HYDROCHLORATE OF AMMONIA AND CHLORIDE OF SODIUM.

M. H. Wackenroder states that sulphate of barytes is quite insoluble, but that the sulphates of lime and strontia are soluble in solution of chloride of sodium; the latter, though slowly, yet completely, and it is entirely precipitable from solution by dilute sulphuric acid. Sulphate of lime dissolves very readily in solution of chloride of sodium, and cannot be precipitated by dilute sulphuric acid.—*Ibid.*

PRODUCTION OF FORMIC ACID IN OIL OF TURPENTINE.

The acid reaction of the oil of turpentine of commerce is derived from formic acid, the presence of which is readily detected in the water employed in its rectification.

According to M. Weppen, the formation of this acid can be explained only by the oxidation of the oil by contact with the air. The action may be very simple:

1 atom of oil of turpentine .... $= 5\,C\;8\,H + 10\,O =$

2 .... formic acid .... $= 4\,C\;4\,H + 6\,O$
1 .... carbonic acid ...... $= 1\,C\;\;2\,O$
2 .... water .... $= 4\,H + 2\,O$

It appeared to M. Weppen a subject of interest to inquire if these changes really occurred, or whether other products were not also formed during oxidation.

As oil of turpentine oxidizes slowly by exposure to the air, he endeavoured to effect it by distillation with chromate of lead and dilute sulphuric acid. Soon after ebullition had commenced, the chromate of lead was reduced, and acidulous water distilled with the oil of turpentine, in which the presence of formic acid was discoverable; there was evolved, at the same time, carbonic acid sufficient to render lime-water very turbid. A question however arises, whether this carbonic acid is really derived from the oxidation of the oil of turpentine, or is a secondary product of the formic acid.—*Ibid.*

PRECIPITATION OF CERTAIN SALTS BY EXCESS OF ACIDS.

BY M. WACKENRODER.

It is an important circumstance in analysis, that certain salts, especially sulphates and oxalates, are precipitated by an excess of acid, if they are dissolved in other acids, and especially in nitric or hydrochloric acid. If, for example, protosulphate of mercury be dissolved in diluted nitric acid, this salt may be almost perfectly separated by the addition of dilute sulphuric acid. Nitric acid, though with difficulty, dissolves sulphate of lead completely; but if dilute sulphuric acid be added to the solution, the sulphate of lead is precipitated.

If a great excess of nitric acid or hydrochloric acid holding lead in solution have not the excess got rid of either by saturation or evaporation, a small quantity of oxide of lead may escape conversion into sulphuret by hydrosulphuric acid; and this circumstance may lead to considerable errors.

If sulphuret of ammonium be added to a dilute solution of lead, sulphuret of lead is formed, which completely and readily redissolves in moderately strong nitric acid and in hydrochloric acid: a current of hydrosulphuric acid gas may be passed for a long time in these solutions, especially in that of hydrochloric acid, without any effect; but when the solution is diluted with water black sulphuret of lead is precipitated, and after the addition of a sufficient quantity of water the precipitation is complete.

If oxalic acid be added to a solution of chloride of strontium acidulated with a sufficient quantity of hydrochloric acid, it does not become turbid; but this effect is produced by the addition of a small portion of lime.—*Ibid.*

SOLUBILITY OF SALTS IN PERNITRATE OF MERCURY.

M. Wackenroder finds that the chloride, bromide, iodide, cyanide, and sulpho-cyanide of silver are soluble in pernitrate of mercury, and that the ferrocyanide, sulphuret, and seleniuret of silver are insoluble in the mercurial salt. These solutions are of a peculiar and uncommon nature. For example, neither nitric acid nor nitrate of silver precipitates anything from the solution of cyanide of silver in pernitrate of mercury; but a sufficient quantity of hydrocyanic or hydrochloric acid, or metallic chlorides, precipitate from it cyanide or chloride of silver. On the contrary, hydrochloric acid, chloride of sodium or hydrochlorate of ammonia, readily precipitate chloride of silver from this solution; an excess of nitrate of silver also precipitates this salt completely, which nitric acid does not precipitate. The chloride, bromide and iodide of mercury also dissolve readily in pernitrate of mercury. Chloride of mercury can be separated from these solutions by a great excess only of chloride of sodium.—*Ibid.*

ON LAUROSTEARINE. BY M. MARSSON.

M. Bonastre found bay-berries to contain volatile oil, resin, gum, a fluid fatty matter and a solid fatty matter, which last he called stearine, and a peculiar crystallizable substance which he named *laurine*. As the characters assigned to this last substance resemble those of the stearoptens, its true nature appears to remain unascertained. By the recommendation of M. Liebig, the investigation was undertaken by M. Marsson, who discovered a fatty substance differing from those previously known, and which he has distinguished by the name of *laurostearine*. It was obtained by treating bay-berries reduced to powder, three or four times with boiling alcohol, filtering it as quickly as possible, washing the substance deposited by cooling with cold alcohol, purifying it at first by fusion in a salt-water bath, and filtering while hot, in order to separate an uncrystallizable resinous matter, and afterwards by repeated crystallizations from alcohol.

The properties of laurostearine are, that when purified by alcohol it is in the form of small white brilliant silky light needles, which are frequently grouped in the form of stars. It is very difficultly soluble in cold alcohol, but readily soluble in strong boiling alcohol, and is deposited almost entirely in crystals as the solution cools. It is very soluble in æther, and by spontaneous evaporation crystallizes, as it does from the alcoholic solution. It fuses at about 112° Fahr., and on cooling becomes a mass resembling stearine, presenting no traces of a crystalline texture, and is brittle and friable. Solution of potash saponifies it pretty readily, and forms a perfectly bright soapy solution: the soap separated by chloride of sodium is hard, and yields by decomposition with acids a fatty acid, the *laurostearic acid.* By dry distillation it yields acroleine, and a solid fatty body, crystallizable from æther. It is formed of

1 atom laurostearic acid $= C^{24} H^{46} O^3$
1 atom glycerine...... $= C^3 H^4 O$
1 atom laurostearine .. $= C^{27} H^{50} O^4$

Ibid.[*]

ON LAUROSTEARIC ACID. BY M. MARSSON.

This acid is obtained in the usual mode, by the addition of tartaric acid to a hot solution. Soda-soap prepared with pure laurostearine has the appearance of a colourless oil, which on cooling becomes a solid crystalline transparent mass; it is very soluble in strong alcohol, and still more so in æther, but it does not separate from either of these solvents in the form of crystals. Its fusing-point is lower than that of the laurostearine itself, being about 107° Fahr.

The alcoholic solution has a strong acid reaction. The acid separated in the mode above described is a hydrate; its formula is $= C^{24} H^{48} O^4$, and that of the anhydrous acid, combined with bases in salts, is $= C^{24} H^{46} O^3$. Laurostearic acid, therefore, contains, in the state of hydrate, an atom of water, which in salts is replaced by an equivalent of base.

Bay-berries contain, besides, a considerable quantity of fluid green fatty matter and resin, but the last-mentioned does not possess any peculiar acid properties.—*Ibid.*

ON THE PRESENCE OF ANTIMONY IN ARSENIOUS ACID.

M. A. Wiggers attempted some time since to preserve transparent fragments of arsenious acid under hydrochloric acid. He did not succeed; the arsenious acid became gradually cloudy and opake, but the examination of the hydrochloric acid proved that it contained a considerable quantity of oxide of antimony, $Sb^2 O^3$. Several cases may occur in which it is advantageous to be aware of this admixture, and in this point of view the statement of the facts is not unimportant. A large portion of oxide of antimony sublimes with arsenious acid; the hydrochloric acid completely

---

[*] See p. 167 of the present Number.

dissolves this impure arsenious acid, and yields a solution from which water throws down a white precipitate, sulphuretted hydrogen an orange one of sulphuret of antimony, and then a yellow one of sulphuret of arsenic. Nitric acid, when heated, dissolves the mixture, leaving a residue of oxide of antimony containing arsenic acid, which is readily dissolved by hydrochloric acid and by tartaric acid; and it forms solutions with these acids, which possess all the reactions of oxide of antimony. M. Wiggers found oxide of antimony only in the vitreous arsenious acid from Andreasberg in the Hartz.—*Ibid.*

DISCOVERY OF A NEW METAL.

"In Part Seventh of my Journal, which you will receive next week, you will find a notice of the discovery of a new metal; it has been named *Didym*; it always accompanies Lanthanium, from which unfortunately it has not yet been separated. All the researches on Lanthanium, as well as those on Cerium, are erroneous."—*Extract of a letter from Prof. Poggendorff to W. Francis.*

METEOROLOGICAL OBSERVATIONS FOR JULY 1842.

Chiswick.—July 1. Heavy rain: fine. 2, 3. Very fine. 4. Densely overcast. 5. Dry and windy: showery: clear and fine. 6. Very fine. 7. Overcast: rain. 8. Cloudy: heavy rain at night. 9—11. Fine. 12—14. Cloudy and fine. 15. Fine: dry haze. 16. Dry and clear. 17. Slight haze. 18. Sultry. 19. Slight rain. 20. Fine: showery. 21. Densely overcast. 22, 23. Very fine. 24. Cloudless and hot. 25, 26. Very fine. 27. Slight rain in the morning: lightly overcast and fine. 28. Thunder-storm early in the morning, most violent between five and six A.M.: sultry: cloudy and fine. 29. Densely clouded: clear at night. 30. Cloudy: fine. 31. Cloudy and fine: clear at night.

Boston.—July 1. Rain: rain early A.M. 2. Fine: stormy, with rain, thunder and lightning P.M. 3. Fine: rain P.M. 4. Cloudy. 5. Stormy. 6. Windy. 7. Fine. 8. Fine: rain P.M. 9—12. Fine. 13. Cloudy: three o'clock thermometer 76°. 14—16. Fine. 17. Cloudy. 18. Fine. 19. Cloudy. 20, 21. Cloudy: rain early A.M. 22. Cloudy: rain P.M. 23. Cloudy. 24. Fine: twelve o'clock thermometer 78°. 25. Cloudy. 26. Fine. 27. Fine: rain P.M. 28. Fine. 29. Cloudy: rain early A.M. 30. Windy. 31. Cloudy.

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

Applegarth Manse, Dumfries-shire.—July 1. Showers. 2. Wet nearly all day. 3, 4. Showery. 5. Rain and wind. 6. Fair and fine. 7—11. Heavy showers. 12. Fair and fine. 13. Showery. 14. Fair and fine. 15. Very fine. 16. Very fine: thunder. 17. Very fine, but cloudy. 18. Showers. 19—21. Fair and fine. 22—24. Very fine. 25. Very fine: sultry. 26. Very fine: cloudy. 27. Cool and cloudy. 28. Cool but fine. 29. Cloudy and threatening. 30, 31. Very fine.

Sun shone out 30 days. Rain fell 12 days. Thunder 1.

Wind North-north-east 1 day. North-east 2 days. East 4 days. South-east 1 day. South-south-east 1 day. South 4 days. South-west 1 day. West-south-west 2 days. West 9 days. West-north-west 1 day. North-west 3 days. North-north-west 2 days.

Calm 13 days. Moderate 8 days. Brisk 6 days. Strong breeze 3 days. Boisterous 1 day.

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

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
1842.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R. S. 9 a.m.
July.		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	29·814	29·849	29·737	29·19	29·72	29·62	29·84	29·74	51·3	70·0	52·3	70	50	53	65½	47	56	52	w.	w.	n.	wsw.	e.	·111	·05	1·38	......	......	57
2.	29·914	29·848	29·830	29·25	29·50	29·70	29·74	29·83	63·0	76·6	52·2	69	46	60	60	47½	54	51	w.	w.	w.	w.	ne.	·316	......	·07	......	·11	55
3.	30·034	29·961	29·826	29·32	29·78	29·70	29·88	29·86	62·8	77·6	52·0	75	55	59	60	46½	54½	50	w.	w.	w.	w.	ne.	·213	·01	·36	......	......	57
4.	29·806	29·770	29·626	29·14	29·46	29·23	29·70	29·46	64·7	71·4	58·8	76	59	68	60	49	54	48	s.	sw.	w.	wsw.	e.	·100	......	·06	......	......	60
5.	29·746	29·823	29·652	29·00	29·15	29·49	29·08	29·20	66·7	75·7	61·4	70	46	65	58½	54	60½	54	ssevar	w.	w.	wnw.	se.	·055	·11	......	......	·24	59
6.	30·136	30·161	30·103	29·46	29·80	29·97	29·67	29·96	62·3	79·4	52·4	72	40	60	61	46½	50	50	w.	w.	w.	w.	nw.	·033	......	......	......	·25	52
7.	30·150	30·009	29·826	29·57	29·84	29·57	29·95	29·80	63·2	72·7	52·3	62	50	62	58	41	54½	52	sse.	s.	calm	w.	e.	·022	·09	......	......	......	55
● 8.	29·856	29·804	29·611	29·25	29·53	29·40	29·61	29·60	61·8	66·6	54·0	66	48	61·5	59	45	53	52	s.	sw.	calm	w.	e.	·041	·43	......	1·75	·36	56
9.	29·748	29·712	29·686	29·11	29·49	29·38	29·59	29·47	62·8	73·3	54·3	72	49	61	60	43	57	53½	w.	w.	calm	s.	se.	·102	·03	·21	......	·18	58
10.	29·970	29·894	29·864	29·30	29·50	29·65	29·41	29·59	62·2	77·8	53·2	73	50	62	60	50	58	53	s.	sw.	w.	w.	s.	·116	......	......	......	......	57
11.	29·724	29·869	29·611	29·12	29·29	29·39	29·60	29·50	67·2	82·7	56·3	78	53	65	62	50	58	53	s.	sw.	sw.	se.	se.	·072	·02	......	......	......	61
12.	29·922	30·026	29·832	29·25	29·68	29·80	29·71	29·85	65·0	75·2	58·0	75	49	63	65	49	56	55	wnw.	sw.	calm	s.	w.	·038	......	......	......	·09	59
13.	30·212	30·232	30·143	29·50	29·71	29·95	29·68	29·77	66·5	76·2	57·3	73	48	64	65	54	57	56	sw var	sw.	w.	sw.	s.	......	......	......	......	·03	60
☽ 14.	30·432	30·337	30·300	29·71	30·10	30·20	29·89	30·03	63·2	76·3	55·0	76	45	65	60½	50	56	53½	w.	w.	w.	w.	sw.	......	......	......	......	·13	60
15.	30·468	30·388	30·252	29·82	30·24	30·20	30·23	30·30	64·3	76·3	56·0	74	46	64	68	45	54½	54½	n.	ne.	w.	s.	w.	......	......	......	1·70	·13	54
16.	30·250	30·175	30·003	29·65	30·10	29·96	30·22	30·13	62·8	73·2	54·6	75	55	63·5	72½	49	62	55	e.	e.	e.	e.	s.	......	......	......	......	·06	59
17.	29·978	29·891	29·789	29·48	29·87	29·80	30·08	30·06	64·0	74·6	57·3	75	52	62	69	50½	61½	53	e.	e.	c.	e.	se.	......	......	......	......	......	63
18.	29·886	29·993	29·822	29·28	29·80	29·80	30·04	30·11	67·2	75·6	59·7	81	57	69	62	53	59½	55	nw.	sw.	calm	e.	e.	·036	·02	......	......	......	62
19.	29·888	29·842	29·754	29·26	29·83	29·78	30·12	30·11	64·3	77·8	62·0	73	52	69	64	54½	58	54	e.	se.	calm	c.	ne.	......	·01	......	......	......	61
20.	29·790	29·735	29·690	29·13	29·75	29·76	30·04	30·01	64·7	71·0	57·3	73	50	60	60	50	56	51	s.	s.	calm	nne.	e.	·033	·12	·92	......	......	60
21.	29·772	29·864	29·718	29·17	29·77	29·82	29·96	30·04	60·8	72·4	56·8	65	47	58·5	70	49½	54	51	w.	n.	calm	ne.	n.	·038	......	·25	......	......	61
○ 22.	30·046	30·128	29·695	29·46	29·96	30·05	30·12	30·20	59·8	72·2	52·6	66	46	55	69	43	53	51	nw.	n.	w.	s.	nw.	·025	·02	......	0·05	......	54
23.	30·262	30·197	30·153	29·72	30·10	30·09	30·19	30·13	58·4	64·4	55·0	70	42	56	73½	43	53	54	nnw.	n.	calm	sse.	w.	......	......	·04	......	......	52
24.	30·180	30·118	29·927	29·58	30·01	29·92	30·07	30·06	64·3	79·7	53·7	84	49	66	73½	43	56	51½	nnw.	ne.	calm	nnw.	nw.	......	......	......	......	......	58
25.	29·966	29·902	29·812	29·43	29·90	29·91	30·02	30·03	64·7	76·7	57·6	77	43	63	68	51½	58	54	e.	e.	calm	nw.	w.	......	......	......	......	......	63
26.	30·046	30·066	29·967	29·48	29·90	30·05	30·07	30·15	61·7	72·3	52·4	73	48	65	69½	52	56	54½	n.	e.	calm	nw.	nw.	......	·02	......	......	......	58
27.	30·212	30·156	30·106	29·60	30·08	30·05	30·10	30·10	57·4	71·4	56·8	75	55	67	68	52	56½	55	e.	se.	calm	nw.	nw.	·050	·56	......	......	......	58
28.	30·112	30·055	29·896	29·51	29·98	29·88	29·89	29·93	61·3	70·3	56·3	75	55	65	64	48	56½	51	w.	nw.	calm	nnw.	nw.	·091	·03	·22	......	·23	60
29.	29·910	29·987	29·851	29·33	29·98	29·98	30·02	30·06	53·0	71·6	55·2	65	45	60	66½	47	52	50	n.	n.	ne.	ne.	n.	·205	......	·06	......	......	53
☾ 30.	30·082	30·064	30·018	29·52	30·00	29·98	30·06	30·11	57·3	64·4	50·4	65	52	56	65½	53½	54½	54½	nw.	n.	n.	w.	nw.	·150	......	......	0·05	·07	52
31.	30·186	30·224	30·127	29·65	30·10	30·12	30·12	30·23	57·8	64·5	55·7	68	47	59	69	55	57	54	nw.	n.	calm	w.	w.	......	......	......	......	......	53
Mean.	30·016	30·002	29·878	29·39	29·771	29·812	29·893	[illegible]	[illegible]	[illegible]	[illegible]	72·29	49·32	62·1	64·7	48·7	56·01	52·77						Sum. 1·847	1·52	3·57	3·55	1·88	Mean. 58

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

[THIRD SERIES.]

OCTOBER 1842.

XLII. *Contributions to the Minute Anatomy of Animals. By* GEORGE GULLIVER, *F.R.S., &c. &c.*—No. IV.*

On the Structure of Fibrinous Exudations or False Membranes.

AS mentioned in the last Number of the Philosophical Magazine, p. 171, in false membranes, resulting from inflammation, the structure is frequently identical with that of fibrine which has coagulated within or out of the body simply from rest. In friable exudations, as I have noticed in Gerber's Anatomy, p. 29–30, fig. 234, the corpuscles approach pretty nearly in number and appearance to those of pus, except that the former are commonly more loose in texture than the latter. In these exudations too the fibrils are now and then not visible, though they may often be seen clearly enough, and the minute molecules are generally very abundant, yet occasionally scanty, and sometimes altogether absent, or at least not recognizable.

The figures in Gerber's Anatomy (244–251) are tolerably good representations of the fibrils and corpuscles which may be commonly seen in clots of fibrine. The fault of some of those drawings is that the fibrils are depicted too forcibly, and without that softness which they present when viewed in a clear transmitted light. Indeed, these fibrils often form a network so extremely delicate that it must be a matter of some difficulty to get it struck off satisfactorily, even if the drawings are made with accuracy; and the same remark is applicable to the more straight and parallel arrangement which these fibrils often assume.

The structure of false membranes will now be illustrated

---

* Communicated by the Author, August 26, 1842. No. III. will be found in our last Number, p. 168.

by examples. All the figures are magnified about 800 diameters.

Case 1.—A soldier, aged 22, 72nd Regiment, was admitted into hospital with pulmonary consumption, on the 25th of January, and died February 4th, 1842. Thirty-six hours previous to death he had pneuma-thorax, the air having escaped through an opening leading from a superficial vomica to the cavity of the pleura. The lungs contained several vomicæ filled with what is commonly called softened tubercle, and lined with the very common kind of friable and whitish false membrane. The surface of the pulmonary pleura at a distance from the opening was covered with a rather thin and tough false membrane, and on the pleura nearer to the opening was a more soft and friable exudation.

Fig. 1.

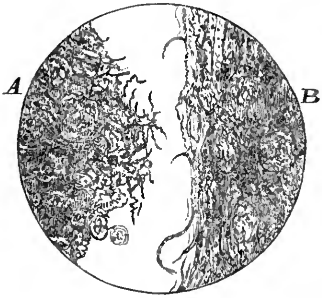


Fig. 2.

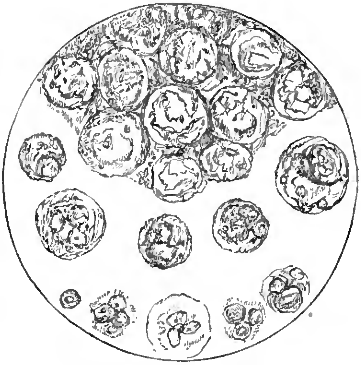


Fig. 1. The structure of the toughish false membrane just mentioned, made up of fibrils similar to those in clots of fibrine either coagulated within or out of the body. At A a portion of the free surface is shown, and at B a portion of the attached or pulmonary surface. Several very minute molecules pervade the false membrane; at B there is an obscure appearance of corpuscles among the fibrils, and with the aid of acetic acid these corpuscles were clearly exposed.

Fig. 2. The softer exudation from the same pleura. In the upper part of the figure the corpuscles are held together by an amorphous clot; just below several of them are floating free in the serum, and at the bottom of the figure their nuclei are clearly exposed by acetic acid. There were no minute molecules either free or in the clot, though some of them were observed in and on a few of the corpuscles. Compare this with the friable exudation, fig. 5, in which themolecules were remarkably abundant.

Fig. 3.

Fig. 4.

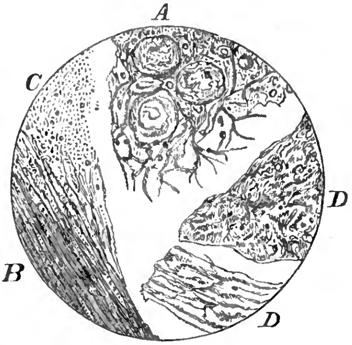


Fig. 3. Structure of the friable false membrane lining a vomica of the same lung. In the upper part of the figure the corpuscles are connected by a clot which is pervaded by granular matter. Lower down are several free corpuscles, a few of which are perhaps altered epithelial cells, together with smaller objects, some of which may be free nuclei or nucleoli. At the bottom of the figure the effect of acetic acid on the corpuscles is shown; it did not produce any ropiness or precipitate in the matter. The pulpy matter contained in the same vomica was composed of corpuscles like those in the figure, but with a larger proportion of granular matter.

Case 2.—A man, aged 41, had an old dropsy of the belly, of which he died five days after tapping. The intestines were connected together by coagulated lymph, which in some places extended in the form of a thin whitish and semitransparent membrane from one convolution of the large intestine to another, being in parts very thin and pellucid, and thicker, more opake, and white at intervals.

Fig. 4. Structure of the false membrane last mentioned. A, corpuscles and very minute molecules in a network of delicate fibrils at the edge of a fragment of the exudation. B and C, from a transparent pellicle-like part; at B the fibrils present a parallel arrangement, and some of them appear granulated; but they are commonly smooth, semitransparent, and apparently cylindrical. C, some isolated molecules of extreme delicacy and minuteness; they were rather fainter than here shown. D D, from thicker parts of the exudation in which no distinct structure is apparent. All the objects represented in this figure were occasionally seen in different parts of the same fragment of the false membrane, and sometimes even in one field of vision. Compare the fibrils and corpuscles with those which I have formerly depicted in a

false membrane (Gerber's Anat., fig. 272), and the parallel arrangement of the fibrils with the same appearance in fibrine obtained from blood out of the body (*l. c.* fig. 246).

Case 3.—A child 18 days old died of inflammation of the peritoneum, on the surface of which was some friable coagulated lymph.

Fig. 5.

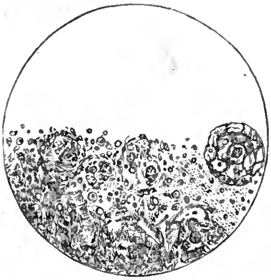


Fig. 6.

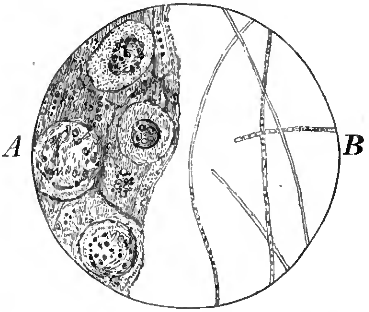


Fig. 5. The exudation just mentioned. There are some corpuscles, and an abundance of minute molecules. One of the corpuscles appears to be made up of objects like the primitive discs of Dr. Barry. Compare this friable exudation with that, fig. 2, in which the molecules were absent.

Structure of Fibrinous Exudations in Birds.

It would be interesting to examine the organic germs in the fibrine of animals with blood-discs differing widely from those of man; and, as remarked by Dr. Carpenter in his valuable work on Human Physiology, p. 471, observations of this kind should be multiplied, in order to test the accuracy of Dr. Barry's views respecting the origin of the tissues and of pus-globules from the blood-discs.

The fibrine obtained by washing from the blood of birds contains a multitude of particles, which are figured in the Philosophical Magazine for August 1842, like the nuclei of the blood-discs. I had recently an opportunity of examining some large amber-coloured and nearly transparent clots of fibrine from the peritoneum of a silver pheasant.

Fig. 6. A, corpuscles in the exudation from the bird just mentioned. The connecting fibrine is very minutely granulated, and the granules are so arranged in some parts as to present a very faint appearance of fibrils; but some of these seemed to be quite smooth, and they are somewhat too distinctly represented in the engraving. B, filaments about $\frac{1}{30000}$th of an inch in diameter, which as they are always

most abundant when putrefaction is just commenced, may be infusory productions. They occur in fibrine from the healthy blood of man and other animals, as well as in fibrinous exudations resulting from inflammation. In my first observations, some of the filaments seemed as if jointed, but this appearance was not seen afterwards. Mr. Dalrymple, who examined them at my request, remarked that they appeared like fine tubes containing round particles, and that the filaments were similar in form to some of the *Vibrionia* of Ehrenberg. But we could never see any motion in the filaments.

As I propose on a future occasion to give a short historical notice of the observations of authors on the structure of fibrine, I shall merely allude here to this branch of the subject. The fibrinous products of inflammation have commonly been described as exudations from the blood. Thus J. Hunter, who mentions the toughness and elasticity of coagulating lymph, as well as its fibrous and laminated appearance, says that the swelling in inflammation is owing to the extravasation of this lymph and some serum. Blumenbach's views are of the same kind. Dr. Hodgkin speaks of the products of inflammation of the serous membranes as effusions; and Dr. Alison of "inflammatory effusions, especially that of pus." Dr. Davy particularly describes the viscidity of coagulated lymph as it passes from the fluid to the solid state, in explanation of the formation of the fibres and bands of the common adhesions of the lungs; this property of fibrine I think has not been noticed by any other author, though it is important, and may most easily be demonstrated. M. Magendie* has given an admirable account, from microscopic observation, of the cellular, laminated, and filamentous structure of fibrine, which he says is to be found again in the coagulum that obliterates blood-vessels, as well as in the formation of adhesions and false membranes; and Dr. Addison, in an interesting paper lately published (Prov. Med. and Surg. Journal, August 20, 1842), concludes from his observations, that "all abnormal products are effusions and not secretions."

Mr. Gerber (Gen. Anat., figs. 16–18) has delineated what he terms the first, second, and complete stages of *fibrillation* in the progress of organization in the fibrine composing coagulable lymph; but he does not say how much his drawings are magnified, though in some of them a very low power must have been employed. Others are sufficiently enlarged to show the cells from which he says the fibres are formed; and this is precisely the point in which my observations are at issue

---

* See Mr. Ancell's Lectures on the Blood, *Lancet* 1839–40, vol. i. p. 459; and those of M. Magendie in the same journal, 1838–39, vol. i. p. 255.

with the views now generally entertained concerning the origin of fibres.

"All the organic tissues," says Dr. Schwann, "however different they may be, have one common principle of development as their basis, viz. the formation of cells; that is to say, nature never unites molecules immediately into a fibre, a tube, and so forth, but she always in the first instance forms a round cell, or changes, when it is requisite, the cells into various primary tissues as they present themselves in the adult state." (Wagner's Physiology by Willis, p. 222.)

How is the origin of the fibrils which I have depicted in so many varieties of fibrine to be reconciled with this doctrine? And what is the proof that these fibrils may not be the primordial fibres of animal textures? I could never see any satisfactory evidence that the fibrils of fibrine are changed cells; and indeed in many cases the fibrils are formed so quickly after coagulation, that their production, according to the views of the eminent physiologist just quoted, would hardly seem possible. Nor have I been able to see that these fibrils arise from the interior of the blood-discs, like certain fibres delineated in the last interesting researches of Dr. Barry.

I have to express my acknowledgements to Dr. Dumbreck, Surgeon 72nd Regiment, and to Dr. Boyd, Resident Physician to the Marylebone Infirmary, for their kindness in affording me opportunities of examining the cases just mentioned; and to Mr. Siddall for calling my attention to the state of the blood in the cases noticed in the last Number of the Philosophical Magazine.

XLIII. *Description of Greenovite. By* M. Dufrenoy*.

M. DUFRENOY states that this mineral, so called in honour of G. B. Greenough, Esq., is a titaniate of manganese, and except crichtonite, which is a titaniate of iron, is the only one hitherto described. It was discovered by M. Bertrand-de-Lom in the manganese deposit of Saint Marcel, in Piedmont; it occurs in small rose-coloured veins which run irregularly in the mass, and is accompanied by quartz, epidote and manganesian garnets. It was supposed originally

Fig. 1.

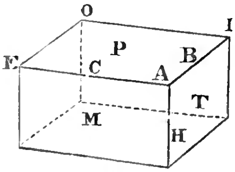
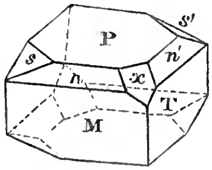


Fig. 2.



---

* From the *Annales des Mines*, vol. xvii.

to be silicate of manganese, and is placed as such in several collections in Paris.

Greenovite occurs in crystals and in small amorphous crystalline masses; it is of a deep rose colour, and its specific gravity is 3·44. Its hardness is greater than that of fluor spar or phosphate of lime, but it does not scratch glass; the crystals are splendent, especially the faces M and T; the terminal faces are often dull and tarnished.

The primary form of the crystal is represented by fig. 1, but other faces have been observed, as shown in fig. 2.

The measured and partly calculated angles are as follows:—

P on M = 87° 10′
P ... T = 85 50
M ... T = 110 35
x ... M = 119 20
x ... T = 118 10
x ... P = 140 6 56″
s ... M = 107 50
s on T = 83° 56′
s ... P = 153 25
s' ... T = 106 30
s' ... x = 146 20
n ... T = 110 13
n ... P = 155 37
n' ... P = 112 ?

This mineral is not acted upon by acids, and is not fusible *per se* by the blow-pipe; microcosmic salt denotes the presence of titanium, and with soda it shows manganese.

To analyse this mineral, M. Cacarrié fused it with five times its weight of bisulphate of potash; the residue when cold dissolved slowly in water, but almost entirely; the very small quantity which remained undissolved contained traces of silica, evidently derived from quartz mixed with the greenovite; the rest was titanic acid. The solution was treated with hydrosulphuric acid, and then supersaturated with ammonia to separate the lime. The residue, composed of titanic acid and sulphuret of manganese, was digested in sulphurous acid, which dissolved the sulphuret. The titanic acid unacted upon was collected, and there was also obtained by ebullition a trace of it from the solution of manganese; an accident prevented the quantity of lime from being determined, but it could not have amounted to one per cent. The proportions of the other constituents were ascertained by M. Cacarrié to be

Titanic acid...........	74·5
Oxide of manganese...	24·8
Lime.....................	·
	99·3

[The crystal of this substance appears, from the author's statement, to be a doubly oblique prism, but from the symmetrical nature of the faces, and the near approximation of the angles, it may possibly turn out to be an oblique rhombic prism. We have not however seen this mineral.—Edit. Phil. Mag.]

XLIV. *New Definition of the Voltaic Circuit, with Formulæ for ascertaining its Power under different circumstances. By* Alfred Smee, *F.R.S.*[*]

Theory of the Voltaic Circuit.

In conducting my experiments on the reduction of alloys, certain phænomena and peculiarities were noticed that have so important a bearing on the theory, or rather the rationale of the voltaic current, that it becomes my duty at once to draw up the curtain and expose the conclusions to which they lead, as a knowledge of them will give to the operator great advantages, and enable him, by rightly understanding the force with which he is working, to conduct his various processes to the best possible advantage.

In these experiments I noticed that in various mixed solutions the quantity of voltaic force passing was not at all dependent on the nature of the negative element, but upon the ease with which the hydrogen was removed from it. Thus in a solution of sulphate of zinc very slightly acidulated the hydrogen could not be evolved from smooth copper, but would rather reduce the sulphate of zinc when connected with a small battery. The substitution of smooth platinum in no way added to the power, but the employment of platinized platinum caused an abundant evolution of gas, even to the removal of the zinc already reduced on the smooth platinum. Any metal having but little affinity for hydrogen caused a similar result; thus, iron caused gas to be evolved and increased the force passing, when smooth platinum would not have the effect, and even zinc itself caused a little gas to be evolved, because the adhesion of the gas to it is slighter than the adhesion to smooth platinum.

In the same way I observed that nitric acid allowed far more electricity to pass than sulphate of copper; and that again, than dilute sulphuric acid, simply from the facility with which hydrogen reduces these substances being greater than the facility of its evolution. I moreover noticed in other cases that the hydrogen would rather be evolved than reduce a metallic salt,—as sulphate of zinc;—and in every case that the facility of its removal affected the amount of power passing, quite independently of the nature of the negative plate.

Now these facts appeared to me a positive proof of there being no such thing as a negative plate contributing to the

---

[*] Reprinted, with additions and corrections by the author, from a pamphlet extracted, for private circulation, from his "Elements of Electro-metallurgy."

production of power, and that this latter is of no value, further than as a means for the removal of the second element of the intervening compound fluid. On the other hand, the multitude of experiments by Faraday all show that the chemical action between one element of a compound fluid and some conducting body appears to be the source of the power, or rather that the power is always directly proportionate to this chemical action. Putting these two series of facts together, an idea presented itself to my mind explanatory of the nature of the voltaic force, for if the force from the experiments of Faraday is proved to depend on chemical action, and the negative pole from my own experiments is proved to be useless, except as affording the means for the removal of the second element of the compound fluid, then it follows as a natural consequence, that if the chemical affinity of any substance for one element of a compound fluid is greater than the resistance offered to the evolution of the second, force is produced. Now it immediately occurred to me that some metals might be made to reduce from a solution of one of their own salts, metal of the same description, by placing the metal partly in a solution for one element of which it has great affinity and in which it is easily dissolved, and partly in a solution of one of its salts. This was actually found to take place in various cases, by following the facts that were made out respecting the ease with which hydrogen reduces various salts.

Zinc reduces zinc by taking a piece of the metal and doubling it, one half is then to be amalgamated and placed in dilute muriatic acid, and the unamalgamated into a strong solution of chloride of zinc, made as neutral as possible, when the affinity of the zinc for the oxygen and the quick removal of the oxide by muriatic acid is sufficiently great to cause zinc to be reduced at the other end of the same piece of metal. The use of platinum, palladium, silver, copper, or any other metal appears not to increase the action in the least, which experiment shows most powerfully the utter fallacy of the contact theory, or in other words, that the voltaic force is in any degree dependent on the opposition of one substance to another. In this experiment, according to the advocates of this now untenable doctrine, the force should have set from the amalgamated zinc to the mercury, the two metals, according to those electricians, having from simply looking at each other the property of evolving power,—but we find that the chemical affinity determined the course of the current.

Copper may by very simple means be made to reduce copper with truly great rapidity; for if a test tube be half filled with sulphate of copper, and then muriatic acid be poured gently at the top, so that the two fluids do not mix to any great extent, and a copper wire be then placed throughout the whole length of the tube, it will speedily show signs of action. The copper in the acid will rapidly dissolve, whilst copper will be as freely deposited at the lower part of the vessel. Now copper will undergo no action alone, either in muriatic acid or sulphate of copper. This experiment may be varied by the use of different acids or even some salts at the upper part of the vessel, for although muriatic acid shows this experiment most strongly, dilute sulphuric acid or muriate of ammonia will produce the same result.

Silver reduces silver by placing one end of a silver wire in a porous tube containing nitrate of silver, the other in dilute sulphuric acid, though the metal placed in either separately is not affected.

Lead reduces lead by immersing one end of a piece of lead in a solution of the tris-nitrate of lead, the other in dilute nitric acid.

Tin reduces tin by placing one portion of a piece of metal in muriate of tin, the other in muriatic acid.

Gold even reduces gold by immersing one end of a gold wire in the chloride, the other in dilute muriatic acid, the two solutions being separated as in all the former cases by a porous diaphragm.

There is a beautiful experiment detailed by Mr. Grove, which is analogous to those last described, though he attributed the results to a different cause*. His experiment is to place two pieces of gold wire in muriatic and nitric acid, separated by a porous diaphragm, when no action will take place on either, but on being connected, that in muriatic acid will rapidly be dissolved, and the nitric acid will at the same time be decomposed by the hydrogen transferred to the other part of the wire.

From the various experiments which I have examined, added to the extensive researches of Faraday on the chemical portion of the voltaic pile, the voltaic phænomena may be defined to be certain effects produced by the chemical action of a body on one element of a compound, and manifested between this point of action and the evolution of the second element. The voltaic phænomena might in other words also be defined to be peculiar properties evinced between the chemical action of a body on one element of a compound, and the evolution of the second element, the point of abstraction and subsequent combination of the first element being called the positive pole; the point of evolution or removal of the second element of the compound body, the negative pole.

[* See Phil. Mag., Third Series, vol. xiv. p. 388.—Edit.]

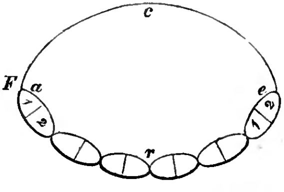
Hence it might be called circular chemical action, because the phænomenon always evinces itself as a circle.

These definitions suit equally every possible case, and there is but one point included in those definitions which is uncertain, though as they now stand, whichever way that doubtful case be taken, they equally apply. The difficulty, and the only one, that I know concerning the production of the voltaic force, is an uncertainty whether the force is produced by the analysis of the compound body, or the synthesis of the newly-formed salt. This is a point concerning which, perhaps, we shall ever be ignorant, yet analogy would rather lead us to suppose that the combination rather than the analysis is the source of the voltaic force. These definitions show why we cannot obtain the force from the union of two elements; indeed, we can never hope to obtain voltaic power from ordinary combustion; for though the energy of the combination of oxygen with carbon is immense, there is no second element, and therefore no intermediate point at which the effects can be manifested. For the same reason no force can be obtained from the union of liquid sulphur or bromine with metals.

The intensity of chemical action being always proportionate to the voltaic power, and being the only source of power in the pile, it follows that (I) the intensity or the power the voltaic fluid possesses of overcoming obstacles is equal to (F), the affinity which regulates the chemical action. But as we find that this power is lessened under different circumstances, $I = F - O$; O standing for the whole of the obstacles afforded to its passage.

Let us take at once a circle and examine its properties. We find that the intensity of the action (I) is equal to the affinity (F) of the body used to separate one element of the compound fluid (in the galvanic battery this is produced by the zinc and oxygen) lessened by the mechanical resistances afforded by the removal of the newly-formed compound (*a*) by the obstruction offered to the passage of the force by the compound solution (*r*), by the imperfection of the conducting power of the solid parts of the circuit (*c*), and lastly, by the obstacle which is afforded to the removal of the second element of the compound fluid (*e*); thus we have algebraically $I = F - \overline{a+c+r+e}$. This circle is supposed to consist of but a single atom of fluid, exposed at one time to the action of the body combining with one of its elements,

Fig. 1.



and all the resistances are supposed to be constant. In some cases we might be desirous of ascertaining the values of the other parts of the circle; thus if we desired to find the affinity (F) $F = I + a + c + r + e$,—the conducting power of the connecting part of the arrangement (*c*) $c = F - \overline{I + a + r + e}$. The removal of the newly found compound (*a*) $a = F - \overline{I + c + r + e}$, the resistance offered by the compound fluid (*r*) $r = F - \overline{I + a + c + e}$, the resistance to the removal of the second element of the compound $e = F - \overline{I + a + c + r}$.

Sometimes this circle is exceedingly small, the (*r*) consisting of but one atom of the compound, and (*c*) but of a single atom of the body combining with one element. This might be properly called an atomic circle, a good specimen of which has heretofore been called local action.

We must now consider the different parts of the circle in detail; and now a question naturally arises whether the intervening compound may consist of any number of elements, or whether it is essential that the compound should be made up of only two elements. From a consideration of the voluminous experiments of our great authority Faraday, it would appear probable that the second hypothesis is correct, although it is just possible that if the body consists of more than two elements, that the impediment to the evolution of the other elements (*e*) or the resistance of the fluid part (*r*) become so enormously increased as to stop any (F) or series of (F) that we have ever applied. Another question also arises, as to whether compound must necessarily be a fluid which requires the same consideration as the first question.

(F) the chemical affinity of a body for one element of a compound is immensely strong where zinc is employed, the attraction of that metal for oxygen being most powerful; but if we substitute iron, tin, lead, copper, or gold, for the zinc, the attraction being feeble, the value of (F) would be reduced in various proportions, in some cases almost to zero.

(*a*) the removal of the newly-formed compound affords but little resistance when the new salt is soluble in the fluid and a sufficiency is supplied for that purpose. In batteries generally the removal of sulphate of zinc affords but little obstacle, being quickly dissolved by water; (*a*) in some cases is the removal of the first element of the compound by evolution, thus in the voltameter oxygen is evolved. In these cases (*a*) is very large, and offers great obstacles to the passage of the current. The removal of the first element is sometimes accomplished by decomposition; thus oxygen may be removed by hydriodic acid, by the decomposition of which body (*a*) is diminished and the current of one battery will pass through it. The observations made with regard to the reduction of alloys in the case of *e* apply equally to (*a*), for the first element will always be removed in the manner which affords least resistance.

(*r*) varies very much from the extent of the interposed fluid, and its conducting power being very different in each case. It varies much in different batteries. Sometimes *r* is a very complex quantity, as when two or more solutions of different conducting power are used between the combination of one element of a compound and the evolution of the second. In Daniell's battery, for instance, it is made up of three parts, not only the resistance offered by dilute sulphuric acid and solution of sulphate of copper, but also a resistance offered by the interposed diaphragm. It might be made up of a far greater number of parts, for different parts may be of different temperatures, which alone (if the temperature interferes with the conducting power) would cause *r* to be complex. (*r*) becomes enormously increased when the force is compelled to travel round a corner.

(*c*) the resistance of the connecting part of the arrangement is generally in batteries very slight, because we select metals which conduct pretty freely; (*c*) may be very complex by being made of a variety of conducting substances; thus, if the connexions are made of wires of different kinds of metal, a different resistance is offered by each. (*c*) in every battery, is generally made up of three parts, the conducting power of the positive and negative plates, and the intervening connecting wires.

(*e*) the resistance to the removal of the second element*, is generally very great, affording a considerable obstacle in all cases, but the differences in this respect are very remarkable. Ordinarily (*e*) is a simple quantity, but becomes complex when the hydrogen is removed in a variety of ways at the same moment. It becomes a curious question to ascertain whether (*e*) might ever be made a plus quantity. If the force proceeds from analyis, then the use of any body having great affinity for the second element might cause the current to be increased. If from synthesis, and this is most probable, if not absolutely certain, (*e*) can never be a plus quantity, but always a minus. In the removal of the second element by decomposition of another compound body, it is by no means uncommon for a voltaic circuit to be formed. In Grove's

---

* The term second here may require explanation, for it is only used in contradistinction to the term first, which is applied to that element which by combination forms F. Either element of a compound may be first or second, according as it may happen to assist in the propagation of the force.

battery the hydrogen acts upon nitric acid, forming water, and setting deutoxide of nitrogen, &c. free; but in this case the intermediate part between the combination of the first element and the removal of the second, is only the atom of hydrogen; it therefore follows that this action must be regarded as nothing but a series of little local batteries, or atomic circles, having nothing to do with the great battery which we make available for our purposes.

It is absolutely essential, according to our definition of the voltaic force, that to be enabled to apply this principle for any purpose, however small a quantity of the force may be required, that either (*c*) or (*r*) should possess a capability of being so far prolonged as to enable us, with the imperfect powers that nature has furnished us, to handle or deal with these intervening portions of the circuit.

In the principal batteries now in use, their relative powers and attributes may be fully understood by considering each of the above properties in their construction.

	F.	*a.*	*c.*	*r.*	*e.*
Grove…………	large	small	small	medium	little.
Daniell ………	large	small	small	most	much.
Smee …………	large	small	small	small	much.
Smooth platinum	large	small	small	small	enormous.

Thus the four batteries may be considered equal in the properties of the F, *a*, *c*, the differences being only in (*r*) and (*e*). In Grove's the (*e*) is so small as not only to compensate a slight increase in the (*r*) over mine, as usually constructed, but to give a great advantage to his form of battery. In Daniell's the (*e*) is perhaps rather smaller than in mine, but that is more than counterbalanced by (*r*) being larger in Daniell's than in mine. The effect of these properties are, that F in Grove's is diminished but little, F in mine more, in Daniell's more still; and in the smooth platinum battery by far the most. Thus is explained the decomposition of dilute sulphuric acid between platinum plates, by one cell of Grove's battery, and the same result not being obtained by the others. This equation is not only valuable for batteries, but applies to every single case where any substance acts upon a compound fluid in such a way as first to decompose it, then to combine with one of its elements, and set free in some way the other. Thus, if potassium be cast into dilute muriatic acid, (F) is immensely large, potassium having a violent affinity for oxygen; (*a*) is exceedingly small, potash being readily soluble in water; (*r*) is almost nothing, only one atom of fluid being traversed by the force; (*c*) is practically nothing from the same cause; (e) is very small. The result of such a state of things necessarily causes a vast intensity of action, and an explosion is the result.

Good specimens of contrasts in the magnitude in the several parts of the circuit are to be seen in the relative power of (F), as obtained by zinc and silver; in the relative resistance of (a) in the solubility of sulphate of lead and sulphate of zinc; in the resistance of (r) in the conducting power of pure water and muriatic acid; of the resistance of (c) in a leaden wire a hundred miles long, and a short silver one; in the resistance of (e) in the evolution of hydrogen from smooth platinum, and its removal by nitric acid.

The relative degrees of action evinced by zinc, tin, iron, and lead upon sulphate of copper are easily explained; (F) differs from being larger, (a) in being smaller when zinc is employed, whilst (c), (r), (e) in each case remain nearly the same; (a) indeed is so large when lead is employed as soon to put a stop to the action.

How intelligible is the want of action of dilute sulphuric acid on amalgamated zinc, if examined by our equation for (c)! the adhesion of the second element, hydrogen, being increased enormously, counterbalances (F), the affinity of zinc for the first element, or oxygen, and no action takes place. Amalgamated zinc is rapidly dissolved if placed in a solution of salts of copper or silver, for (e) in that case is depressed, the hydrogen rapidly reducing the copper. Nitric acid in the same way does not respect the amalgamation of the zinc, for (e) in that case is also diminished by the removal of hydrogen from the decomposition of the acid. As the adhesion of hydrogen to plumbago is very great, it occurred to me that the simple application of black-lead to zinc would, by preventing the evolution of hydrogen, increase (e), and therefore stop the local action; but although the experiment fully succeeded, the plumbago so quickly came off, that I have not at present made any practical application of the experiment.

The above cases, with all their analogies, are not the only ones to which the equation applies, for it will account perfectly for the action of bodies on each other.

In cases of single elective affinity, as the action of sulphuric acid on nitrate of barytes, a compound is decomposed, one element enters into another combination, the other is set free; a voltaic circuit is therefore produced, the parts of which are thus made:

(c) Sulphuric acid ⎫ (F)
(r) ⎧ Barytes ⎭ (a)
⎩ Nitric acid (e)

In cases of double elective affinity, as the action of sulphate of ammonia on nitrate of barytes, a similar circuit is formed thus:—

$$\begin{matrix}(\mathrm{F})\\(a)\end{matrix}\left\{\begin{array}{ll} \multicolumn{2}{c}{(c)} \\ \hline \text{Sulphuric acid} & \text{Ammonia} \\ \text{Barytes} & \text{Nitric acid} \\ \hline \multicolumn{2}{c}{(r)} \end{array}\right\}(e)^{*}$$

In both these cases, however, we have not the means of increasing the (*r*) and (*c*) to a tangible size (at least I have never been able to do it), and at present these actions have been restricted to the formation of atomic circles.

There are some cases where we can extend the intermediate parts (*c*) and (*r*), and then our definition of the voltaic force with the formula arising from it enables us to form most extraordinary voltaic circles, which indeed we never could have formed before, unless we happened to light upon them by chance: thus proto-sulphate of iron, placed on one side of a diaphragm, and nitrate of silver on the other, will give a current when connected with a platinum wire, and a beautiful deposit of silver will be reduced on the platinum wire, on the nitrate of silver side of the circuit.

In the same manner circuits may be formed of proto-sulphate of iron and chloride of gold—of proto-nitrate of mercury and chloride of gold—of oxalic acid and chloride of gold, &c. In all of which cases the metal is freely reduced on that part of the platinum wire inserted in the metallic salt. The reason why a galvanic circuit is formed in these cases is sufficiently obvious; water is the electrolyte or compound decomposed, proto-sulphate of iron is the substance combining with one element, and the metallic salt affords a means for the removal of the second element or hydrogen, and as we have the power of extending the compound (*r*) and connecting parts (*c*), not only an atomic circuit, but a working battery may be made. At the diaphragm or the point of juncture of the two liquids, indeed, an atomic or local battery is formed independently of the general or working battery. A second local battery is formed at the point of decomposition of the metallic salt by the hydrogen. The following are the parts of the circuit in the above cases.

$$\begin{matrix}(\mathrm{F})\\ \dagger(a)\end{matrix}\left\{\begin{array}{ll} \multicolumn{2}{c}{(c)} \\ \hline \text{Proto-sulphate of iron} & \text{Platinum wire} \\ \text{Oxygen} & \text{Hydrogen } (e)\dagger \\ \hline \multicolumn{2}{c}{(r)} \end{array}\right.$$

---

* The (*e*) in this case does not form a secondary voltaic circle, but is the union of two primitive elements.

† (*a*) is the removal of the per-sulphate of iron by solution; (*e*) is the removal of the hydrogen by the decomposition of the metallic salt.

It would be extremely interesting to find every case of decomposition of a compound fluid obedient to the equation, and indeed there is every appearance of that being the fact.

The impossibility of giving a negative tendency to a metal when hydrogen is removed from its surface is also perfectly accounted for by our equation; for hydrogen, as has been already shown, protects the metal; so when a facility is offered for its removal, not only is the direct protection removed, but by diminishing the value of (e), (F) the natural affinity of the metal for one element of the fluid, having but little resistance opposed to it, begins to act, and the metal is therefore dissolved.

The superior action of a rough metal in contrast with a smooth one, is explainable on the equation most satisfactorily, for in the first case the affinity (F) is but feebly opposed by the resistance to the evolution of the hydrogen (e), whilst in the latter case (F) is so strongly opposed by (e) that no action can take place. Zinc shavings, which always have one side bright and the other rough, show this phænomenon clearly. Polished zinc or iron also show this effect in a striking manner.

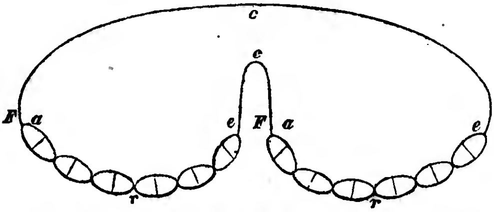
Hitherto we have considered (F, a, c, r, e) in every case to be constant, but in many instances they are subjected to continual variation. I do not, indeed, happen to recollect an instance of (F) varying to any amount, but (a) varies frequently; in the gradual saturation of a fluid it progressively increases, so much so, as at last to equal (F). This accounts for zinc ceasing to be dissolved on the saturation of the fluid by sulphate of zinc, although still intensely acid. (c) generally remains constant. (r) is very unsteady, for as in all voltaic arrangements the fluid is always undergoing change, it is therefore sure to be altered in its conducting power. (e) is subject to great variations from alteration of the liquid and other causes.

In every case of a single battery we have seen that the intensity is equal to chemical affinity, minus the resistances to that affinity. In a compound battery the expression is equally simple, for the intensity is equal to the sum of the affinities, minus the sum of the resistances. In a series of batteries all of the same nature, $I' = \overline{F-\overline{a+c+r+e}} \times n$. Sometimes ($n$) is very complex. For example, if a compound battery be made up of a Grove's, a Daniell's, and my own, the values of (I) must be considered separately, and their sum taken.

The diagram exhibits well the arrangement and properties of the compound battery.

A good example of the effect of (n) is seen in the water battery, where (I) is exceedingly small from the resistances of (a) and (r) being large, but becomes amplified to such a

Fig. 2.



degree by (n) as to possess prodigious force; indeed as it possesses a capability of being amplified infinitely by an infinite series completely insulated, a battery might be constructed powerful enough for the force to pass from one electrode, placed in the Thames at London Bridge, and the other in some river in Australia, though the resistances of (r) and (c) in this case, from their extreme length, would be very great. In every water battery, as (a) instead of being constant gradually increases, the power gradually declines at length to nothing. The curious and wonderfully-multiplying powers of (n), whereby the intensity can be increased, precludes our saying that the galvanic power is unable to effect any particular object; for, after all, it might turn out that (n) was not magnified sufficiently to attain that end.

If we desire to find the number of batteries in any arrangement, it could be easily ascertained by the following equation:

$$n = \frac{I'}{F - \overline{a+c+r+e}}.$$

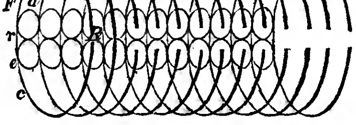
When we are turning our power to some application it is very convenient to consider the purpose for which it is applied as a resistance, and call it R. If we have a series of them alike it would be $R \times m$, m standing for the number composing the series. If, however, the series is not alike, it would be $R + R' + R''$. The intensity of the current after having passed this resistance would be also equal to the sum of the intensities, minus the sum of the resistances, $I'' = F - \overline{a+c+r+e} \times n - Rm$. The R is frequently very complex, as in the reduction of metals in a decomposition trough, where it is made up of as many parts as a voltaic battery.

Having amply discussed the power of the force to overcome obstacles, we are led to determine the time in which any given number of equivalents of voltaic power can be obtained. Hitherto we have considered the circuit to be made up of a single atom of the body combining with one element of the compound, and if the affinity exceeds but ever such a trifle its obstacles, then in time any amount of work would be performed provided the current remained constant. A current can easily be conceived so feeble as to take millions of years to reduce a pound of copper. If the entire circuit of single atoms be increased at every part, in fact if the mathematical voltaic circle be increased to the size of a tunnel, then (W), the amount of work performed in a given time, would be equal to the intensity of the battery, minus the resistance of our working apparatus, multiplied by the number of parts of the tunnel (A) thus: $W = \overline{I' - R}. \times A$.

This equation, however, gives us the total amount of chemical actions in the whole series of batteries and decomposition troughs, or, in other words, the sum of the actions evinced in each; we generally, however, are desirous of estimating the amount done in one particular cell, in which case we divide our equation by the number of cells and troughs (n) thus: $W' = \frac{\overline{I' - R} \times A}{n}$.

Sometimes this equation is rendered extremely complex by an increase of the circuit at one side but not at another; in fact, the tunnel is cut away on one side, and this is a case that is perpetually occurring in practice. In this case it is not impossible but that the force is only derived from those parts of the circuit which are complete: in that case the equation would be $W'' = \frac{\overline{I' - R} \times \overline{A - p}}{n}$, p standing for the incomplete parts. In this view of the question we are supported by the analogy of water running through a pipe of given dimensions from a cistern; for however large this cistern be, provided there be no more pressure, the water running through the pipe would be the same. So far as the voltaic fluid is concerned I feel certain, from numerous observations, that beyond a certain point the increase of a battery does not cause a greater amount of electricity to pass through a given resistance; and, perhaps, in those cases, where the enlargement of a battery increases the voltaic force, the battery in the former instance was deficient in size in relation to the size of the resisting part R, the tunnel, in fact, having been defective originally in that part. It is possible that the expression for this condition might be altered; for R, the resistance to the single

Fig. 3.



voltaic circle, might possibly vary in some new manner, for which further experiments are wanted. In that case it would be $W'' = \frac{I'A - \mathfrak{R}}{n}$; the old English $\mathfrak{R}$ standing for the new resistance afforded to the whole current. The tunnel might be cut away at any other part besides (R), thus it might be deficient at (F), (a), (c), (r), or (e); but the student will readily perceive the expressions for these cases.

The algebraic formula for (W) is replete with interest, for it accurately defines the value of (W) in determining the so-called power of any battery. The power of a battery is the intensity multiplied by the quantity, in other words, $I' \times A$; but our equations show that W is not equal to $I\,A$ but to $I\,A - R\,A$, and from that we deduce that $I\,A = W + R\,A$; therefore it is impossible by Faraday's voltameter to ascertain the value of $I'\,A$ at once, and it is necessary first to find the value of A. The immense mass of experiments in which the voltameter has been assumed to be equal to $I'\,A$, must now be discarded as inaccurate, and no deductions drawn from them; but all future experimenters, by attending to these equations, may make the results obtained by the voltameter absolutely correct.

The symbol (A) I have before noticed stands for the value of atoms of the compound fluid exposed to the action of a body removing one of its elements. It sometimes becomes an inconstant quantity, as in the cases where non-conducting substances incase the poles of the battery. A is tolerably constant in Daniell's and in Grove's battery, most so in the former. It is less constant in mine, and very inconstant in most smooth metal batteries. A is analogous to what experimenters formerly very properly called the quantity of a battery. The contact theorists indeed would fain make us believe that there is no such thing as either quantity or intensity, but they have erroneously multiplied intensity (I') with quantity (A), and called it electromotive power, and then denied the existence of the several parts of that power, which is nothing but the $I'\,A$ of my equation with the (c) and (r) abstracted from the (I'), and considered separately. This is the point on which Ohm and his followers have perplexed all English philosophers; still, notwithstanding this mystification, to Ohm is due the hearty thanks of every electrician for showing that voltaic force is diminished by resistances, and doubtless his doctrine of resistance is a most important and lasting discovery. Ohm's equation, in the complex manner in which he has given it, presents fewest difficulties when applied to solid connecting resistances; thus those who used batteries with connected wires could appreciate it in many cases, but those who were engaged in experiments either in the construction of the voltaic battery itself or in complex resistances, found it perfectly inapplicable. By the equation for (W) we find that $A = \frac{W}{I' - R}$ which I have already mentioned must be determined before we can find the value of I′ A, or what is properly called the power of any battery.

Sometimes W is very small, as in De Luc's columns, where the total amount of chemical action, although (*n*) is frequently 500 to 1000, is so small, that experimenters have even denied its existence; but when we consider that these very persons assert, that as soon as chemical action does become decidedly manifest, the action ceases, how strongly do they favour our views! for, according to our equation, we expect (*a*) to be gradually increased till all action would be stopped. W, indeed, according to our equation, might be so small, as not to be cognizable to our senses for weeks, months, years, or centuries; and yet (I) multiplied by a very large (*n*) would show enormous intensity or power of overcoming resistances.

The present modifications of the theory of galvanism are perfectly consonant with every practical direction given in the preceding pages, and the only difference in the theory will be found in the uncertainty expressed upon the contact and chemical action theories. Indeed, in page 54 of my work, as already cited, the result is almost given in words though not in letters. By removing the slight difficulties which appeared to envelope the latter theory, by showing the necessity for a negative pole to cause power is unfounded, the beautiful doctrine of Faraday is placed on the surest foundation, and the extraordinary and dogmatical paradox of a power without a cause is proved to be a fanciful chimæra.

With regard to the connexion of the voltaic power with that of electricity produced from other sources, perhaps it might be expected I should say a few words. In the voltaic battery (I) is small, but may be increased to any size by (*n*), and as we have the power of increasing (A) also unlimitedly, we can perform any amount of work per second, indeed we might throw down hundreds of tons of copper per second, if we were disposed to make our circuit large enough. In frictional electricity (I) is enormous, but (A) is depressed to its utmost limit, so that not having a perfect command over (A) to increase it indefinitely, we cannot at present obtain what work we please in a given time. In animal electricity (I) is great, (A) is moderately large. In thermo-electricity (I) is depressed, perhaps increasingly, so that although (A) and (*n*) may be multiplied indefinitely, yet, practically, we should never be able thoroughly to overcome the smallness of (I). In that mighty operation of Nature which has just occurred, where the noise accompanying the discharge of the electricity over the metropolis was so awful as to alarm not only delicate females, but the stoutest hearts of men, and even the heretofore unterrified nervous system of infants—in that terrific storm, when every living creature trembled, and Nature seemed almost alarmed at her own operations, how vast was (I)! how large (A)! Could I therefore but have imprisoned that collection of force which in discharging itself committed such devastation on houses, churches, and trees, and, having encased it, been able to have let it loose as it might have been required; then indeed would all batteries have been henceforth discarded as playthings for children—philosophical toys to be admired, still despised, for (I A) being unlimitedly great, we could obtain what work we pleased in any given time, at no expense.

The estimate of the parts of (I) in other cases where force is produced, *i. e.* an electricity not proved to be derived from chemical action, I do not deem it my business now to consider, but great difficulties would attend its accurate investigation, as it is almost impossible to magnify the size of the circle in these cases, in such a way as to make the action in each part cognizable by our senses. It is however quite evident, that as in the voltaic and thermo circuits (I) may be magnified to any extent by (*n*), that the power of (I) in every case might be brought to the same standard in the power overcoming the resistances R', R'', R''', &c.

The obstacles to the completion of the voltaic circuit (O), are made up as we have seen of several parts, *a*, *e*, *r*, *c*, but, although they differ in kind, still as they have similar resisting properties, a perfect table might be made, referring them to one given standard, showing the separate value of each. The principle on which it should be constructed, is the law of the completion of the voltaic current, detailed when treating of the reduction of alloys; and as soon as we have this table accurately and numerically drawn up, the principles of the passage of the voltaic circuit, which formerly puzzled the most enlightened experimenters, will be rendered certain, and the difficulties will be also reduced to the facility and certainty of common arithmetic. Having obtained perfect tables of (O) and its several parts, we can readily obtain the relative value of (I), derived from various sources, by finding out what extent of (O) neutralizes each individual (I), and the value of (I), or the force of any battery, will be determined with equal facility. Complete tables of (O) and (I) now become the greatest *desiderata*, not only to electro-metallurgists, but to all who use the voltaic battery.

I now bid adieu to my theory of galvanism and my formulæ and to those who have neither time nor inclination to dive into these mysteries, I would say,—remember, in all operations that the sum of the resistances does not exceed the sum of the intensities; and that in increasing the circuit, every part is equally enlarged:—to those who have devoted themselves to these properties—remember they will be useless if not brought into active operation; thus, if any difficulty occurs in your voltaic circuit, refer it at once to its proper head, and the operator may be sure that a continual practice and habit of using these formulæ will enable him to conduct his proceedings with a certainty never obtainable by blind experiment.

In concluding these formulæ, I herewith leave theory and rationale altogether, for having completed the principles, as far as I am capable, of everything relating to electro-metallurgy, I shall enter at once into the applications of the science for the direct purposes of the arts; and although everything that will be contained in the subsequent parts of this work has already been comprised in the parts already finished, yet there are many little practical difficulties to be surmounted—many little circumstances to be pointed out which the operator is likely to overlook or forget in conducting his operations, and these are the circumstances to which the concluding pages will more especially be devoted. Henceforth the work will be entirely practical, as heretofore it has been exclusively theoretical. There is a reproach attached to the very word, theory; the sense in which it is employed means rather rationale than theory, for whilst it has been my constant endeavour to shun theories without facts, I have tried and tried hard to generalize all extensive series of facts, and to give the rationale of every circumstance which is likely to occur to the operator.

XLV. *Reply to some Objections against the Theory of Molecular Action according to* Newton's *Law. By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., F.C.P.S., &c., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge.*

[Continued from p. 208.]

MR. EARNSHAW'S first argument is, "*Dispersion in a refracting medium cannot be accounted for on the finite-interval theory, unless there be also dispersion in vacuo.* Now as there is no dispersion *in vacuo*, I infer generally, that the finite-interval theory *cannot* account for dispersion" (pres. vol. p. 47).

The difficulty which is here brought forward is the same that has so often been started; it in fact goes to the foundation of the *finite-interval theory*. If that theory be supposed to consist in the hypothesis, that the vibrations of the particles of æther within a medium are unaffected by the presence of the particles of matter in any shape, I shall not undertake to be its advocate. I will simply refer to M. Cauchy's Memoir (Prague), p. 188. But it is evident that Mr. Earnshaw admits into that theory the *indirect* action of the particles of matter; for he says, "I have not taken account of the direct action of matter upon the æther; but as my results are independent of arrangement, it is obvious that the indirect effect of matter is included in them. Consequently the indirect effect of matter never can assist us in accounting either for the transversality of vibrations or for dispersion" (p. 48). I am obliged to ask Mr. Earnshaw what he conceives to be the direct effect of matter. The phrase was, I think, originated by myself, and was meant to express the attractions or repulsions of the quiescent particles of matter on those of æther. If this be the sense in which Mr. Earnshaw uses the phrase, then I must understand from the above quotation that he has not estimated the direct action of the particles of matter, simply because he has assumed that those particles vibrate, or rather perhaps, because he has assumed that they vibrate respectively in precisely the same manner as the particles of æther would do if they filled the same place. If this be the case, indeed, whatever Mr. Earnshaw assumes, the expressions for the velocity of transmission *must contain* a term due to the action of the particles of matter. Let us even take the extreme case of supposing that these particles are at rest, and that their attractions or repulsions produce no effect: still is there an indirect effect due to them, which although not easily calculated, is clearly of the utmost importance. I allude to the effect due to the *want of action* of particles of æther in the portions of space occupied by the material particles. Neither this, nor the pressure of the particles of matter on the adjacent particles of æther tending to stop their motion, does Mr. Earnshaw say one word about; and yet he asserts "that the indirect effect of matter is included in his equations." How is it included? If it be replied, that the equations in p. 47 are supposed to contain terms dependent on the particles of matter, then is it evident that Mr. Earnshaw's argument is an antithesis to his premises; the latter being the expressions for the velocity of transmission *in vacuo* and in a refracting medium are different in form, the former, *therefore* the velocities themselves must have the same form. Now as I am not willing to accuse Mr. Earnshaw of any such reasoning, I am anxious to imagine on what his argument *is* based. I can only conceive it to be the assumption that the equation

$$2\pi^2\left(\frac{v}{\lambda}\right)^2=\Sigma\left(A_r\sin^2\frac{rh}{2}\right)$$

can in no case render v dependent on λ. That Mr. Earnshaw admits it does not *in vacuo*, is evident from the fact that he believes the equations he has deduced to be correct in that case. He says, Phil. Mag., May, p. 373, "these, then, are the equations of transmission of common light through any transparent medium whatever." If I am right in my conjecture, then, I reply that Mr. Earnshaw is not at liberty to base so sweeping an argument as he brings forward on any *assumption* whatever, much less on one so little likely to be correct. I repeat, that I am unwilling to suppose that Mr. Earnshaw has made use of any false reasoning, but I am convinced that any one who shall peruse his paper will agree with me in affirming, that with so few words devoted to explaining the influence of the particles of matter it is utterly impossible for any one to know what Mr. Earnshaw does mean. I am the more anxious to express this fully, that I may not be accused of misinterpreting the argument, and I trust it will have the effect of eliciting a more full and satisfactory statement.

On the next remark of Mr. Earnshaw I shall not dwell. It has reference to the promised proof by Mr. O'Brien, that "the hypothesis of finite intervals cannot be correct," and to the adoption of the hypothesis of the *direct* action of the particles of matter. I shall only observe, that so far as I can see, the application of this hypothesis is insufficient, unless it be admitted that the particles of "matter are compound, consisting of many different atoms," all of which vibrate along with the particles of æther. If you allow the same assumptions to the finite-interval theory, it will account for the same facts by a formula very much of the same kind. It is by this means that I accounted for dispersion in my 'Theory of Heat,' p. 152. The equations of motion of two sets of vibrating particles were first obtained by me in the Transactions of the Cambridge Philosophical Society, p. 237 *et seq.*

The next matter to which I will direct attention has more pointed reference to myself. Mr. Earnshaw, in a paper printed in the Philosophical Magazine for April, points out the process which I had adopted in my first Memoir on Dispersion, and adds, "the remaining four lines are used as a test of the truth of the *undulatory theory*‡" (p. 308). Where, and by whom, he does not state. For my own part, I disclaim any such unphilosophical opinion. What I hold is this: "that a theory which has succeeded so well in accounting for a great variety of intricate and delicate phænomena" (Earnshaw, p. 304), is *strengthened* by the removal of any obstacle, and consequently by bringing under it the explanation of the phænomenon of dispersion. But has the phænomenon been explained? I answer, most assuredly. It is done as satisfactorily as almost any one phænomenon in nature is explained. Its doubtful nature, the "uncertainty‡" which I mentioned in my 'Theory of Heat' as attached to it, is referable, not to the *kind* of explanation, but to its detail. Nay, even Mr. Earnshaw himself appears to look for a complete explanation to the very quarter at which he aims his objections. Unless Mr. Earnshaw adopts the hypothesis that the particles of matter are *at rest*, there is *no difference whatever* between the *hypothesis* of Mr. O'Brien, which he designates as a "more promising one," and my own. Are my *equations* then incorrect, and why? I see them open at p. 248 of vol. vi. of the Transactions of the Camb. Phil. Soc., they are certainly not of *exactly* the same form as Mr. O'Brien's; but his are only approximations. I do not say that even then they are identical, the difference probably will be removed by supposing B and B equal in the latter. So far as I am concerned with the numerical verification of the formulæ for dispersion (which occupies between five and six pages in my Memoir), I may state that it is essential to show that our results are *in the form* which the phænomena require they should be: and having premised this, I will gladly answer the questions which Mr. Earnshaw puts me in p. 49.

"Am I to understand him to say, that his formulæ are *of necessity* capable of producing correct results even if the data employed be erroneous?" Yes: but the *data* are not erroneous.

"May I then ask, what is the nature of the connexion of these formulæ with theory? and in what degree is his theory supported and strengthened by coincidences obtained from such formulæ?" The numerical verifications were used, as is stated at the place, as a test of the *general accuracy* of the deductions. Let me quote my own words. "Results more nearly agreeing might doubtless be obtained by proceeding to one place further in the expansion of sin $\frac{\pi \delta \rho}{\lambda}$, but the above will suffice to establish the general accuracy of the formula" (p. 174). "If, however, it were requisite to determine *accurately* the values of p, q, ... of course the plan to be adopted would be that of introducing *seven* constants, and determining their values from the seven given equations" (pp. 172–3). "I wish to ask, then, *how* the results could have any power at all in confirming the theory, if the formulæ were *of necessity* capable of producing correct results from correct or incorrect data indifferently?"

In answering this question, I must premise that I fear I do not rightly understand what Mr. Earnshaw means by "from correct or incorrect data indifferently." Perhaps I shall make the matter more clear by putting an hypothetical case. The formula being *general*, admitting as many arbitrary constants as you please, is sufficient to satisfy any numerical results continuous and not inconsistent with each other. This I presume will be allowed. Suppose, then, the results had been exactly the converse of what they are: suppose μ to have increased with λ. The formula, then, could probably never have been made *apparently* applicable; and, although sufficient, would assuredly have been held as not at all probably true. By reversing the process, and showing that a formula not only satisfies the requisite demand, but does so in the most simple manner, we certainly add weight to its authority, and strengthen the process on which it is founded.

I proceed now to the consideration of the other objections which Mr. Earnshaw has adduced, for the most part to my own results, in the same paper. They all originate in one and the same error which Mr. Earnshaw has fallen into in deducing his equations at p. 47. I dare say Mr. Earnshaw has himself discovered the oversight ere now, and, but that he has wielded the erroneous results to which it led him in dealing blows most at my conclusions, I should have left it to himself to supply the correction: but as Mr. Earnshaw has set his conclusions in opposition to the truth of my deductions, and those, too, of the most important kind, I cannot delegate the power of replying to his own convictions. The error I allude to is this. Mr. Earnshaw says, "We are now at liberty, without affecting the generality of our investigations, to suppose that the axes of symmetry were the coordinate axes employed in my former paper; in which case $D = E = F = 0$," &c. (p. 47). Now it is not at all true that *because the axes are axes of symmetry* therefore $D = E = F = 0$. The method which Mr. Earnshaw has employed in his former paper (Phil. Mag. May, p. 373) to obtain his equations, is more similar to that which M. Cauchy uses to obtain the same equations in his recent publications, than to his original method. In his *Nouveaux Exercises*, p. 4, for instance, he makes

$$\Sigma\, m' \, \frac{y\, z}{r} \, \frac{d f r}{d \gamma} \, (\eta - \eta') = \mathrm{F} \, \eta$$

as Mr. Earnshaw does, without giving explicitly the value of F.

But in his *Mémoire sur la Dispersion de la Lumière* (Prague), he gives the value of F as

$$\Sigma \left\{ \frac{2\, m f r}{r} \cos\beta \cos\gamma \sin^2. \frac{k r \cos\delta}{2} \right\} \text{ (p. 9.)}$$

Had Mr. Earnshaw seen this last value, he would hardly have conceived that it could be made zero by the symmetry of the axes: he would have been convinced that the relative values of β, γ, and δ, *i. e.* the relative directions of transmission and of the axes, alone could effect that object. The fact is, that if any one of the axes of coordinates coincide with that of transmission, the three quantities do vanish; in other cases they do not. Mr. Earnshaw's oversight consists, then, in assigning to an axis of symmetry a property which belongs only *to the axis of transmission.* It is remarkable that Mr. Earnshaw did not inquire into the cause of difference between his equations and mine, for in form they are identical. [See Phil. Mag., May, 1837, p. 388, and various other places.] I say it is remarkable, for Mr. Earnshaw perceived that the cause of difference lay in the dependence or want of dependence of the equations of motion on the direction of transmission. All the argument he offers in support of his view is contained in the following words:—"Again, by referring to my former communication, it will be seen that the equations of motion do not depend upon the position of the front of the waves traversing the medium" (p. 47). And this is in reality all the *reasoning* on which he founds his remarks subversive of so many of my conclusions. One word will serve to answer it. Mr. Earnshaw's *former communication* did not contain the equations of motion on which his arguments are founded. These are to be found only in the latter communication, and in a form which *does* depend on the position of the front of the wave. Having then shown that Mr. Earnshaw's argument is founded in a mistake, I will adopt his language (p. 48), modified to suit my own purpose:—

I consider it therefore as proved incontestably, that according to the finite-interval theory there IS *a connexion between the directions of the vibrations and the law of molecular force. Hence, then,* I HAVE *established the transversality of vibrations on that theory on a basis which defies opposition.*

Having thus shown that an error lies at the foundation of all Mr. Earnshaw's objections, it might be deemed unnecessary to refute them in detail: yet since they are so plainly and prominently brought forward as opposed to my conclusions, I owe it to myself briefly to do so. They are,—

1. "The vibrations have no necessary reference to the di-

rection of transmission." This is *assumed* by Mr. Earnshaw when he omits D, E, and F, and hence all his objections.

2. "There can be no connexion between the directions of the vibrations and the law of molecular force." It has been proved by me in the Transactions of the Cambridge Philosophical Society, vol. vi. p. 180, and Philosophical Magazine, May, 1837, p. 341, that if the law of force in a medium of symmetry be that of the inverse square of the distance, the vibrations must be altogether transversal or altogether normal. I call on Mr. Earnshaw to point out an error in my reasoning.

3. But Mr. Earnshaw has attempted to impugn, not indeed my reasoning, but my inference. He says (p. 49, *last line*), "since v v' v'' are the velocities of the *wave*, and not of the particles, the inference should have been, *that there is one direction in which waves cannot be transmitted*; or, in other words, *that the æther is opake in one direction*." Mr. Earnshaw ought, I repeat, to have attempted to show that there is some error in the *argument*, for he must know that such an inference as he draws tends to throw discredit (if legitimate) upon any reasoning from which it is made to follow. The *hypothesis* is that the æther is equally affected in all directions, the *conclusion*, that it is opake in one.

The inference, however, *cannot* follow from *my* equations, for Mr. Earnshaw will see, if he turns to my Memoir, that v' is the velocity of a *normal vibration* which is assumed to exist. Since then (I argue) the normal vibration has not a possible velocity of transmission, it does not exist. In fact, if there be a normal motion at all it must be a transmissory one, due to exponential in place of circular functions. On this last fact I have based my Theory of Heat (Preface, p. 8, and Memoirs, &c., *passim*). Since Mr. Earnshaw quotes Mr. O'Brien, I will refer him to the same quarter to be set right, for his conclusions are equally controverted by Mr. Earnshaw's objections.

4. "But I am unable to discover on what ground it is stated that v' is impossible," &c. Had Mr. Earnshaw read through the page he refers to he would have found the reason: all that he suggests is there plainly discussed, the inference *that the æther is opake in one direction only excepted.*

5. Mr. Earnshaw concludes with a suggestion that the inference ought rather to have reference to the *instability of the medium according to the Newtonian law.* How he connects the impossibility of transmission of an assumed vibration with instability it is easy to see, and that it arises from the assumption of the want of dependence of the equations of motion on the direction of transmission. But I shall not dwell on this

subject here. It has already been amply dealt with at the commencement of my reply: I will only add, when it is concluded from the hypothesis of a cubical arrangement of the particles, acting by forces which vary according to the Newtonian law, that the direction of one side of the cube is stable and of one unstable (Earnshaw on the Nature of Molecular Forces, Art. 15), ought we not to ask, Is it the hypothesis, or the reasoning based on it which is erroneous? Must it not of *necessity* be the latter?

We have now done with the objections to the statical possibility of the law. It remains to reply to the two objections to its dynamical applicability. It is fit that a matter so important as the rejection of a law which explains so many phænomena (see Gauss, in the last No. of the Scientific Memoirs), which has so strong an *à priori* probability, and which is the proved law of material action, should rest on none but the most unexceptionable evidence. Whatever may become of the question ultimately, I trust that by rigidly examining that evidence which has been afforded and showing its inadequacy, I shall be considered as actuated by no captious or unphilosophical spirit. My next communication will be a reply to M. Cauchy, whose arguments being based on a refined analysis, can scarcely be answered without the use of similar means.

[To be continued.]

XLVI. *Observations on the Formation of Compounds of Boron and Silicon with Nitrogen and certain Metals.* *By* W. H. Balmain, *Esq., Lecturer on Chemistry in the Mechanics' Institution, Liverpool*[*].

CONSIDERING the strong affinity existing between hydrogen and nitrogen, and between carbon and nitrogen, together with the circumstances under which they will combine, and their chemical relations to boron and silicon, I was led to imagine that the two latter elements must have a very strong affinity for nitrogen, and concluded that they might be caused to combine with it by double decomposition; and, bearing in mind the strong affinities of ammonia and cyanogen, it appeared probable that the compounds, if obtained, would play an important part as chemical agents; and I had hopes that some of the bodies at present supposed to be elementary might prove to be compounds of nitrogen with these or other elements. Some experiments instituted to establish these points have been in a measure successful, but as they form only a small part of the great number which will at once

---

[*] Communicated by Dr. Kane.

suggest themselves to the mind of the chemist, and as my time is of necessity devoted to other objects and my means very limited, I beg leave to lay the few facts which I have been able to ascertain before the working chemists of the day through the medium of the Philosophical Magazine.

Silica and boracic acid undergo no change when heated in ammoniacal gas by means of the oxyhydrogen flame nearly to the point at which platina melts, but when heated to that temperature with cyanide of potassium instead of ammonia, apparent action ensues. Boracic acid and cyanide of potassium, in the proportion of two atoms of the former to three of the latter, were placed in a covered porcelain crucible, that inclosed in a larger Hessian crucible, and the space between being filled with small pieces of charcoal, the whole was heated to whiteness in a wind furnace. The result was a white porous substance, which was found not only at the bottom of the crucible, but also lining the sides and the top, having been carried there by spurious sublimation. The relative quantities given above were used in order that the carbon of the cyanide might be exactly in the right proportion for taking all the oxygen from the boracic acid and forming carbonic oxide only, and when by accident an excess of boracic acid or cyanide was employed it appeared to remain as an impurity in the white solid; but these points were not closely examined, because the white solid, which was homogeneous and evidently a distinct and stable compound, was a more interesting object of study. The following is the best process for preparing it:—Take seven parts of finely powdered anhydrous boracic acid and twenty parts of cyanide of potassium free from water, and as far as possible from cyanide of potass and iron; and having lined a Hessian crucible with a paste of powdered charcoal and gum, and heated it until all water has passed away, place the mixture in the crucible, cover it by inverting and luting a smaller crucible over it, and heat it to whiteness for an hour: it is advisable to use a crucible as a cover, that there may be sufficient room for spurious sublimation, and the vent-hole should be bored in the bottom of this crucible and not in the luting at the side; and further, to avoid the penetration of oxygen to the materials, it is well to line the upper crucible in like manner with the lower. The result found in both crucibles, when washed and dried, will be the white solid in a state of purity. It is a light porous solid which readily falls to powder, and when compressed, presents that peculiar surface which is observable in some of the precipitated cyanurets, and in a slight degree in chloride of silver, and in some iodides, &c.; it is infusible, insoluble, even when heated, in water, in solution of potass, hydrochloric acid, sulphuric acid (strong and diluted), nitric acid, and solution of chlorine; it is not altered upon exposure to air, nor does it affect the most delicate turmeric paper when left upon it in a moist state. Passing over for the present the remarkable stability of this compound, these characters are important as proving the absence of boracic acid and cyanide of potassium (with some results it was found necessary to wash away the excess of cyanide of potassium; but this does not interfere with the nature of the white solid, and was not necessary when the boracic acid and cyanide of potassium were quite pure and free from water, and their proportions very carefully adjusted). Heated with hydrate of potass or soda it yields ammonia abundantly; in the deoxidizing flame of the blowpipe it is not altered, nor does it communicate any colour to the flame, but in the oxidizing flame it gives a strong green colour, and gradually fuses, yielding a perfect bead, which is transparent, hot and cold, and when placed with a drop of water upon test papers, turned tumeric brown, and red litmus blue. When the outside flame impinges upon a large surface of the substance in powder, as when a glass tube soiled with it is held at the extreme point of the flame, it presents a beautiful green phosphorescence, owing no doubt to the gradual formation of boracic acid at the surface, and if it be removed to the inner flame, the centre will incandesce, while the outer edges, where it meets with the oxygen of the air, will still yield the elegant green. When thrown upon fused chlorate of potass it deflagrates with a soft green light, and it will also deflagrate with nitrate of potass. It is not altered by being gently heated with potassium or sodium, nor when heated before the blowpipe on charcoal, with lead, zinc, &c. Chlorine has no action upon it at a low red heat, and iodine, sulphur and corrosive sublimate may be sublimed from it without decomposing it. It is not decomposed by hydrogen at a red heat, but below that temperature is decomposed with the evolution of ammonia by the vapour of water, or by any substance which will yield water, as hydrate of potass, hydrate of lime, common clay, hydrated phosphoric acid, and the rhombic phosphate. It is not decomposed by hydrochloric acid at a low red heat, and I think it is not altered by hydrofluoric acid, for a small portion of it was mixed with a large quantity of fluorspar, with more than sufficient sulphuric acid to make it all into hydrofluoric acid, and heated as long as fumes passed off, when, after the sulphate of lime had been washed away with dilute nitric acid, it still yielded ammonia with hydrate of lime.

From some of these facts it appears that the compound contains boron, nitrogen, and potassium, and I suppose that the nitrogen and boron are united, and that the compound so formed is combined by a very strong affinity with potassium.

My inability to obtain a better balance than such as I could construct myself of wood and paper, or suitable apparatus for an analysis, prevents me from speaking at all positively as to the proportion of the elements; but some analyses and decompositions seem to point out the proportion $K_3 N_3 B_2$ as the correct one, from which it would appear that during its preparation there is no loss either of potassium or of nitrogen; nothing passing off but carbonic oxide:

$$(2\,B\,O_3 \text{ and } 3\,(N\,C_2 + K) = N_3\,B_2\,K_3 \text{ and } 6\,C\,O).$$

This theory very nearly agrees with several estimations of the quantity of ammonia and boracic acid found when the compound is decomposed by the hydrates of lime and potass, and is corroborated by there being no gas but ammonia disengaged, and no boron deposited during the decomposition:

$$(N_3\,B_2\,K_3 \text{ and } 9\,H\,O = 2\,B\,O_3 + 3\,K\,O \text{ and } 3\,N\,H_3).$$

However, it may be that there are only two atoms of potassium, since the compound can only be obtained at such a temperature as would volatilize potassium; from which it would appear that potassium was set free during its formation; and moreover, during the decomposition by hydrate of potass or lime, a new compound is formed which may possibly contain the original compound with oxygen, being somewhat analogous to cyanate of potass, in which case the oxygen, set free from the hydrogen which has to form ammonia, might be theoretically disposed of without the supposition that there are three atoms of potassium ($N_3\,B_2\,K_2$ and $9\,H\,O = 2\,B\,O_3 + 2\,KO$ and $3\,N\,H_3$ and O, which would go to undecomposed substance); but at the same time, this new compound may contain oxygen and have derived it, not from the decomposed water, but from the air in the vessel. It is formed when the "boronitruret of potassium" is fused with potass, and an excess of acid added to the solution of the result; at first it appears as a milkiness in the liquid, but by continued ebullition, collects into a distinct precipitate, which when dry is a remarkably coherent thready solid.

When heated before the blowpipe it gives a strong green flame without melting; it yields ammonia abundantly with hydrate of lime and carbonate of potass (a mixture which I used instead of hydrate of potass), and in other respects behaves like the "boronitruret of potassium," excepting that it yields no phosphorescence, and when slowly oxidized forms a very fusible bead, which during its oxidation throws out small vesicles owing to the escape of gas. The substance operated upon was obtained chiefly from an incomplete analysis of the "boronitruret of potassium," by heating it with hydrate of lime; the result being diffused through water, a stream of carbonic acid passed through it, and the whole boiled, borate of potass was in solution and carbonate of lime precipitated, which, being acted upon by muriatic acid, yielded an imperfectly transparent liquid, and from this the thready substance was deposited on long-continued ebullition.

All attempts to decompose the "boronitruret of potassium," so as to isolate the theoretical "boride of nitrogen," have hitherto been unsuccessful; each experiment adding its testimony to the remarkable stability of the compound. It cannot be done by means of oxidizing agents, for both the potassium and the boron take oxygen at the same time, and either boracic acid and potass are formed, or else the thready substance alluded to above, as appeared to be the case when peroxide of manganese with boracic or sulphuric acid was used as the oxidizing agent, since, after diluting and acting upon the residue with a solution of oxalic and sulphuric acids to remove boracic acid and any remaining peroxide of manganese, a white solid was left which had the same appearance and, before the blowpipe, the same characters as that substance.

Finding that cyanogen passed over a mixture of boracic acid and charcoal heated to redness gave me no result, I endeavoured, as a last resource, to obtain compounds of "boride of nitrogen" with the common metals by heating their cyanides with boracic acid, fully expecting that these cyanides would decompose at too low a temperature for the deoxidation of the boron to take place, and I was agreeably surprised when upon trial it appeared that the cyanide of copper heated with boracic acid gave a result, which, after being washed, yielded ammonia when heated with a mixture of hydrate of lime and carbonate of potass; and cyanide of lead, a result which not only yielded ammonia, but produced a phosphorescence before the blowpipe which differed from that of the "boronitruret of potassium" only in its colour, which was more yellow and less green.

Both of these results were so impure, owing to the presence of oxides in the cyanides, that their characters could not be taken as those of the compounds of the metals with "boride of nitrogen," and they were only valuable as proving the possibility of making those compounds by such a process. The copper result gave a very fine green flame before the blowpipe, but would not phosphoresce; and after the metallic copper had been removed by nitric acid a substance remained which appeared more like the "thready compound" supposed to contain oxygen, than the "boronitruret of potassium." Cyanide of mercury heated with boracic acid gave cyanogen abundantly, which burned with a tinge of green in its flame; and at the same time a small quantity of white crystalline solid sublimed, which may prove to be a compound of mercury with the "boride of nitrogen," and being such, if it could be obtained in larger quantity, might probably be a means of isolating the much-wished for "boride of nitrogen." It was soluble in water, giving it a bitter taste; and the solution gave no precipitate with a salt of iron, but an abundant white with protochloride of tin: with iodide of potassium none, with acetate of lead none, with nitrate of silver a slight precipitate, which was insoluble in excess of acid. It was likewise soluble in alcohol, but the solution did not burn with a green flame. Boiled with a solution of carbonate of potass it yielded ammonia, and it communicated a green colour to flame, passing off rapidly in vapour, and giving a greenish blue colour to the flame in its immediate neighbourhood.

A mixture of one part of anhydrous boracic acid with two and a half parts of cyanide of zinc, heated to whiteness in a lined crucible (covered and well luted), yielded a white solid similar in appearance to that obtained by heating a mixture of boracic acid and cyanide of potassium. It gave ammonia abundantly when heated with a mixture of hydrate of lime and carbonate of potass, and was insoluble (with and without heat) in water, sulphuric acid, hydrochloric acid, nitric acid, solution of chlorine, solution of potass and ammonia. It is not decomposed by chlorine at a *full red heat*, nor by corrosive sublimate, nor by potassium or sodium. Before the blowpipe it is infusible, but in the oxidizing flame communicates a green colour, and when at the outer edge emits a very brilliant bluish phosphorescence, which appearance it also produces when simply dropped into the flame of a spirit-lamp. Thrown on fused chlorate of potass it deflagrates with a faint blue light. These characters are exactly such as we should expect to find in a compound of zinc with "boride of nitrogen" analogous to the compound of potassium. It appeared to be in a state of purity, and is more readily obtained than the potassium compound, since the preparation of a pure cyanide of zinc is accomplished with greater facility than that of cyanide of potassium. Besides its interest in being distinctly a second compound of the kind, and the remarkable beauty of its phosphorescence before the blowpipe, it is of importance as affording a means of preparing the analogous compound of other metals by heating it with their chlorides. Heated to whiteness in a lined crucible in the proportion of one atom of itself (taking its composition to be $Zn_2\ N_3\ B_2$) to two atoms of the chloride, it yielded, with chloride of lead, a white solid which gave ammonia abundantly when heated with a mixture of hydrate of lime and carbonate of potass, and phosphoresced with a yellowish green light at the point of the blowpipe flame; water boiled with it afterwards gave no precipitate with nitrate of silver, and when it was heated before the blowpipe with soda upon charcoal, it gave a distinct button of lead and only a minute trace of zinc; with chloride of copper, a result similar to that obtained by heating together cyanide of copper and boracic acid.

With chloride of silver, a result which resembled the lead compound, and phosphoresced brilliantly with a yellowish-green light. It was not decomposed by hydrochloric acid, nor by chlorine at a low red heat, nor by corrosive sublimate, and indeed appeared under all circumstances as stable as the rest, remaining unaltered even when heated in a tube with sodium and potassium. With the chlorides of sodium, barium, strontium, calcium and manganese, results which appeared to be "boronitrurets" of those metals; but in these cases the experiments were made with small quantities, solely with a hope of finding a soluble compound; and as not one of them would yield ammonia when boiled in water with hydrate of lime and carbonate of potass, and as water after ebullition in contact with them gave no precipitate with solutions of the oxides of lead, silver, copper, iron, &c., I concluded that I had not been successful in my search.

Six parts of silica heated to whiteness with thirteen parts of cyanide of potassium gave a brittle porous vitreous solid, which, after being well washed, yielded ammonia abundantly when heated with hydrate of lime and carbonate of potass. Heated with fused potass it yielded ammonia abundantly. After ebullition with sulphuric acid it still yielded ammonia when heated with hydrate of lime and carbonate of potass. In the deoxidizing flame it fused tranquilly, and in the oxidizing with escape of gas. With carbonate of soda it gave a red bead in the deoxidizing flame, the colour of which disappeared in the oxidizing flame, and could not be recovered. After being heated with nitrate of ammonia and well washed, it yielded ammonia with hydrate of lime and carbonate of potass, more abundantly than before. From this it appears that a compound of silicon and nitrogen with potassium analogous to the boron compound had been formed, and that it is nearly as stable as that substance; but as I had no means of separating the compound from impurity, nothing further can be said at present.

From the above results, and from a few doubtful experiments which have not been mentioned, I conclude that compounds of nitrogen with boron and silicon had been formed, and that their chemical relations are similar to those of cyanogen; and I have no doubt that analogous compounds of aluminium, glucinium, &c. may also be formed; moreover, I have hopes that the fundamental principles of the science of chemistry may be further elucidated by some of these compounds proving to be, if not some of our "elements," at least of a nature closely analogous. We are not to suppose that the affinity of nitrogen for the other elements is weak because it will not unite with them directly as by a process of combustion, especially as the compounds of nitrogen at present known are not formed directly, and in many the affinity has proved stronger than was at first supposed. This compound of boron and nitrogen resists all agents but oxygen, and analogous compounds with bases not so easily oxidized might appear to us elementary, and a glance over the relative constitution of our earth and atmosphere may in some measure justify us in expecting to find nitrogen abundantly in the mineral kingdom; and this point decided positively, may throw much light upon the connexion between organic and inorganic chemistry. My opinion is founded upon a careful review of many well-known facts, and is not solely dependent upon these recent experiments for its support, but, on the contrary, *they* have been instituted to discover evidence, and I hope that while my labours are still continued others will be induced to join in the same pursuit.

William H. Balmain.

XLVII. *On the Optical Constants of Tourmaline, Dioptase and Anatase.* By W. H. Miller, *M.A., F.R.S., Professor of Mineralogy in the University of Cambridge**.

THE values of the optical constants of Tourmaline were deduced from observations made with a prism cut out of a colourless crystal in the possession of Mr. Brooke, which, though not sufficiently perfect to show the dark lines in the spectrum, exhibits the bright line in the flame of alcohol very distinctly. For this light the index of refraction of the ordinary ray out of air into the crystal is 1·6366; in an extraordinary ray perpendicular to the axis of the rhombohedron the velocity of light in air divided by its velocity within the crystal is 1·6193. A slice of the same crystal bounded by planes perpendicular to the axis, 0·68 inch thick, being placed in a po-

---

* Communicated by the Author.

larizing apparatus, the diameter in air of the darkest part of the first ring is about 7° 30′. When this mineral is coloured, as is usually the case, the optical constant belonging to the extraordinary ray cannot be determined, on account of the absorption of the light polarized in the plane of the axis.

In Dioptase, according to observations made with a very perfect and transparent crystal, for which I am indebted to Mr. Heuland, for the brightest part of the solar spectrum the index of refraction of the ordinary ray is 1·667; in an extraordinary ray perpendicular to the axis the velocity of light in air divided by its velocity within the crystal is 1·723.

In Anatase, for the brightest part of the solar spectrum, the index of refraction of the ordinary ray is 2·554; in an extraordinary ray perpendicular to the axis, the velocity of light in air divided by its velocity within the crystal is 2·493.

St. John's College, Sept. 9, 1842. W. H. Miller.

XLVIII. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 21.]

On Cerium and some of its Salts, and on Didymium.

An examination respecting the true atomic weight of cerium has been made by M. A. Beringer in the laboratory of Professor Wöhler; new experiments on this subject were necessary on account of the discovery of lanthanium. It will however be useless to insert this treatise in these reports, inasmuch as a notice has appeared in Poggendorff's *Annals*, vol. lvi. p. 503, from which we learn that Mosander has discovered a third metal mixed with cerium and lanthanium, which he calls Didymium. It is scarcely possible to separate the oxide of this metal; Mosander, although he has known this body one year and a half, has as yet been unable to isolate it in a pure form. Oxide of didymium causes the brown colour of the so-called oxide of cerium, and also the rose or amethyst tinge of some salts of yttria. The perfectly pure oxides of lanthanium and cerium are probably quite colourless. In the usual mode of preparing oxide of lanthanium by means of dilute nitric acid, the whole of it is never extracted, but part remains with the oxide of cerium. Mosander is engaged with the examination of the three bodies, and from him we may expect a full description. Beringer has examined metallic cerium (impure), the double sulphates, and some other salts. —(*Annalen der Chemie und Pharmacie*, vol. xlii. p. 134.)

On the Atomic Weight of Chlorine, Zinc, &c.

Laurent has made some experiments on the atomic weight of chlorine; the assumption of Berzelius's number agrees completely with the analyses, while considerable differences are visible if the atom be considered as a multiple of that of hydrogen. Marignac determines the atomic weight by passing hydrochloric acid gas over heated oxide of copper; he finds 225·013, or thirty-six times that of hydrogen. From this he reckons the atomic weight of silver 1374·0, and of potassium 498·5. Jacquelain finds the atomic weight of zinc to be 414.—(*Comptes Rendus*, Mar. 1842, p. 456; *Ibid.* Avril 1842, p. 570; *Ibid.* Mai 1842, p. 636.)

On the Hyposulphites.

Rammelsberg has published an examination of this class of salts: the deliquescent potash salt has the formula $3\ \dot{K}\underline{\ddot{S}}+\dot{H}$. The soda salt contains 5 atoms of water, that with ammonia has the same composition as the potash salt. The baryta salt contains 1 atom of water, that with strontia 5 atoms, with lime and magnesia 6 atoms. A deliquescent double salt of magnesia and potassa has the formula $\dot{K}\ \underline{\ddot{S}} + \dot{M}g\ \underline{\ddot{S}} + 6$ aq. Hyposulphite of manganese decomposes on evaporation into sulphur and sulphate, the zinc salt the same; a compound of the zinc salt may be obtained by adding ammonia in excess to a solution of the hyposulphite and precipitating the salt by alcohol; it is $\dot{Z}n\ \underline{\ddot{S}}+N\,H^3$. The nickel and cobalt salts have the same constitution as the magnesia compound; the nickel salt combines with ammonia, and gives $(\dot{N}i\ \underline{\ddot{S}}+6\ \dot{H})+2\ N\,H^3$.

Hyposulphite of lead dissolves in solutions of alkaline and earthy hyposulphites, and forms double salts which are easily decomposed. Their solutions must not be heated, for then sulphuret of lead is formed; they are partly decomposed by water. The potassa salt is $\dot{P}b\,\underline{\ddot{S}}+2\ \dot{K}\ \underline{\ddot{S}}+2$ aq, the ammonia salt $\dot{P}b\,\underline{\ddot{S}} + 2\ N\,H^4\,O\ \underline{\ddot{S}}+3$ aq. The soda salt has been described by Lenz (*vide* the last Report). Salts may also be formed with baryta and strontia; the lime salt is $\dot{P}b\ \underline{\ddot{S}}+2\dot{C}a\,\underline{\ddot{S}}+4$ aq. Hyposulphite of oxide of mercury cannot be obtained; but by digesting the oxide with solutions of hyposulphites double salts are formed; the ammonia and potassa salts crystallize, the former is $\dot{H}g\underline{\ddot{S}}+4N\,H^4O\underline{\ddot{S}}+2$aq; the formula of the potassa salt is rather uncertain; the soda salt does not crystallize, nor do the compounds of the earthy hyposulphites.

A solution of the potassa double salt added to sulphate of copper causes a brownish-red precipitate, which has the same formula as the potassa salt, viz. $3\,\dot{Hg}\,\underline{\ddot{S}} + 5\,\underline{\dot{Cu}}\,\underline{\ddot{S}}$.

On adding hyposulphite of potassa to sulphate of copper a yellow precipitate is produced, the formula of which is $\dot{K}\,\underline{\ddot{S}} + \underline{\dot{Cu}}\,\underline{\ddot{S}} + 2\,aq$; it dissolves in excess of alkaline hyposulphite and alcohol precipitates from this solution another crystallizable salt, $3\,\dot{K}\,\underline{\ddot{S}} + \underline{\dot{Cu}}\,\underline{\ddot{S}} + 3\,aq$.

A soda salt similar to the first of these has been described by Lenz, it dissolves in excess of $\dot{Na}\,\underline{\ddot{S}}$, and gives $3\,\dot{Na}\,\underline{\ddot{S}} + \underline{\dot{Cu}}\,\underline{\dot{S}} + 2\,aq$.

Rammelsberg has also examined the products of the destructive distillation of the hyposulphites; he finds that sulphurets, sulphates, and sometimes sulphites are formed.—(Poggendorff's *Annalen*, vol. xlvi. p. 295.)

On the Sulphocyanurets.

Meitzendorff has made an extensive series of experiments on these salts, under the direction of Rammelsberg. The acid was obtained by distilling the potassium salt with tartaric acid. We will here only mention the chief peculiarities of the salts, and refer the reader for fuller information to the long paper itself. The ammonium and sodium salts are anhydrous, $Na, Cy\,S^2$ and $N\,H^4, Cy\,S^2$. The barium, strontium, magnesium and calcium salts are crystallized and deliquescent, $Ba, Cy\,S^2 + 2\,aq$, $Sr, Cy\,S^2 + 3\,aq$, $Ca, Cy\,S^2 + 3\,aq$, and $Mg, Cy\,S^2 + 4\,aq$. The solution of the aluminum salt is decomposed by evaporation into an insoluble basic and a soluble neutral salt. The crystallized manganese salt contains 3 atoms of water; the zinc salt is anhydrous, it combines with ammonia, forming a salt which crystallizes in beautiful crystals, it is $Zn, Cy\,S^2 + N\,H^3$. Cobalt salt does not crystallize, it is $2\,Co, Cy\,S^2 + \dot{H}$; it forms two compounds with ammonia. The nickel salt has the same composition, and the anhydrous salt forms with two atoms of ammonia a crystallizable salt. The crystallized cadmium salt is anhydrous, combines with one atom of ammonia. There are two salts of bismuth, $Bi, Cy\,S^2$ and $Bi, Cy\,S^2 + 4\,\dot{Bi} + 2\,aq$. The *di*sulphocyanuret of copper is anhydrous, but retains a little moisture with great obstinacy. The sulphocyanuret may be obtained by using very concentrated solutions of the sulphate of copper and the sulphocyanuret of potassium; it is precipitated as a black powder, and is anhydrous. Its decomposition with water, which has been studied by Claus, is very curious; it changes in water into the white disulphocyanuret; it appears that at the same time hydrosulphocyanic, hydrocyanic and sulphuric acids are formed (the iron salt appears to be similar in properties). The sulphocyanuret of copper forms a crystallizable salt with one atom of ammonia.—(Poggendorff's *Annalen*, vol. xlvi. p. 63.)

On the Sulphates of Alumina and of Chromium.

In the 45th volume of Poggendorff's Annals, page 99, Hertwig published a paper on the proportions in which aluminosulphate of potassa (alum) can combine with water; he found that when large crystals of common alum are allowed to lie for some time in concentrated sulphuric acid they are not dissolved but dispersed through the acid, forming a gelatinous mass; water throws down a crystalline powder, which on being pressed between bibulous paper and recrystallized from a hot solution, gives a salt in the form of regular octohedrons, which contains only 14 atoms of water, whereas the common alum contains 24. In the same volume, page 331, there is a paper by Heintz who has not been able by these means to procure anything but common alum ($\underline{\dddot{\mathrm{Al}}}\,\dddot{\mathrm{S}}^3, \dot{\mathrm{K}}\,\dddot{\mathrm{S}} + 24\ \mathrm{aq}$). By the united action of heat and sulphuric acid Hertwig obtained another compound, $\underline{\dddot{\mathrm{Al}}}\,\dddot{\mathrm{S}}^3 + \dot{\mathrm{K}}\dddot{\mathrm{S}} + 3\ \mathrm{aq}$, which is a very insoluble salt, and becomes still more so when strongly heated; it must therefore be an isomeric modification of anhydrous alum. Common alum when kept for a length of time at a temperature of 100° C. loses 10 atoms of water, but this salt differs considerably in its properties from that with 14 atoms of water mentioned above. By a heat of 120° to 160° a compound of 5 atoms is obtained, at 200° with 1 atom. By somewhat similar means to those employed by Hertwig, Heintz has obtained two salts with oxide of iron, $\underline{\dddot{\mathrm{Fe}}}\,\dddot{\mathrm{S}}^3, \dot{\mathrm{K}}\,\dddot{\mathrm{S}} + 3\ \mathrm{aq}$ and $\underline{\dddot{\mathrm{Fe}}}\,\dddot{\mathrm{S}}^3, \dot{\mathrm{K}}\,\dddot{\mathrm{S}} + 2\ \mathrm{aq}$. The true colour of the iron alum appears to be violet, when mixed with common alum it is quite white. In vol. lvi. of the same Journal, p. 95, Hertwig has described some modifications of the chrome alum. If a very concentrated solution of the green double sulphate be evaporated with concentrated sulphuric acid as long as water is driven off, a green anhydrous chromosulphate of potassa is precipitated, which is insoluble in boiling or cold water, hydrochloric, sulphuric and nitric acids; it is not altered by ammonia, but it is decomposed by boiling with caustic potassa; formula, $\underline{\mathrm{Cr}}\,\dddot{\mathrm{S}}^3, \dot{\mathrm{K}}\,\dddot{\mathrm{S}}$. It is easily decomposed by heat. It is evident therefore that the chrome alum can exist in three different isomeric modifications. When chrome alum is heated at 200° C. as long as water is driven off, a "difficultly soluble" green modification is formed; it contains 2 atoms of water; it is insoluble in cold water, and also in sulphuric and hydrochloric acids, but it is dissolved by continued boiling with water; decomposition is also effected by boiling ammonia. When heated to 300°–400° it passes into the "insoluble" modification, its dark green colour changes to light green, and it has lost all its water. This anhydrous salt differs from that obtained with sulphuric acid, inasmuch as by long boiling with water, sulphate of potassa is dissolved and insoluble sulphate of chromium remains behind.

In vol. xliii. of the same Journal, p. 513, Schrötter has described some sulphates of chromium. $\underline{\dddot{\mathrm{Cr}}}\,\dddot{\mathrm{S}}^2$ is obtained by adding as much hydrated oxide of chromium to sulphuric acid as it can take up when kept boiling for a long time; it is not crystallizable, and forms a green mass on evaporation; on the addition of water a light green powder separates, which is $\underline{\dddot{\mathrm{Cr}}}^3\,\dddot{\mathrm{S}}^2 + 12$ aq. If a solution of the first salt be heated with excess of sulphuric acid the green colour disappears and a peach red precipitate is formed, which is not soluble in water, and is not decomposed by acids or ammonia, but easily by caustic potassa or soda. A solution of this salt may be obtained by dissolving eight parts of oxide in nine parts of English sulphuric acid; alcohol does not precipitate the fresh solution; if it be allowed to stand several weeks it forms a greenish blue crystalline mass, which dissolved in water forms a dark blue (by transmitted light ruby red) solution. Out of this alcohol precipitates a light violet-coloured crystalline salt, $\underline{\dddot{\mathrm{Cr}}}\,\dddot{\mathrm{S}}^3 + 15$ aq, which is easily soluble in water, becomes green when heated to 100°, and loses 10 atoms of water. For preparing the chrome alum, Schrötter proposes to pass sulphurous acid into a solution of one atom of bichromate of potassa and one atom of sulphuric acid, as long as it is absorbed, the mixture being kept cool. He has also prepared the ammonia and soda chrome alum; they both contain 24 atoms of water. [The ammonia compound was prepared by Mr. Warington several years ago (*vide* Turner's Chemistry);

it has also been examined by Mitscherlich; *vide* Lehrbuch, vol. ii. part 2.]

Hydrated oxide of chromium dried at 100° contains six atoms of water. Schrötter also states that the green modification of chrome alum when in solution passes gradually into the blue one. [This statement I can fully confirm from my own old observations. This change of the green into the blue oxide accounts for Warington's preparation of the double oxalates of chromium and potassa by means of green oxide of chromium*, although from the mode of preparation it is evident that the blue oxide is the base in the black and red oxalates.—H. C.]

On some Chromates.

Kopp has examined several of these salts, principally with a view to determining their specific gravities and atomic volumes. The chromates of zinc and copper may be obtained by dissolving the oxides or carbonates in dilute chromic acid (prepared by Fritzsche's method), or by digesting chromate of baryta with the sulphates [several salts of the magnesian class were prepared some years since in this manner by Mr. Playfair, but no account of them has been published]. The salts of copper and zinc have the same form and composition as the sulphates of those oxides; the soda salt is similar to the sulphate, it deliquesces. By evaporating its solution at 30° C. anhydrous chromate may be obtained. The chromates of ammonia and magnesia are precisely similar to the corresponding sulphates.—(*Annalen der Chemie, &c.*, vol. xlii. p. 97.)

Bensch has published a notice on some basic chromates obtained by pouring a solution of chromate of potassa into boiling neutral metallic solutions. These precipitates must be washed with hot water, or else they retain some potassa; by boiling they appear to be decomposed. None of them have been properly examined as yet; the manganese salt is black, its formula is $\dot{M}n^2 \dddot{C}r + 2\,aq$. When heated red-hot the water and some oxygen are driven off.—(Poggendorff's *Annalen*, vol. l. p. 97.)

[The same salt appears to have been formed by Mr. Warington (Reports of the Chem. Soc., part 3), who has obtained the same formula. Salts of *protoxide* of manganese are white or pinkish; the salts of chromic acid are seldom very darkcoloured, and it appears rather anomalous that this basic salt should be black. It might be $Cr\,O^2 + Mn^2\,O^3 + 2\,aq$; when treated with hydrochloric acid the sesquioxide of manganese would cause evolution of chlorine, and a brown chloride of chromium might be produced, which by the addition of alco-

---

* See p. 201 of the present volume.—Edit.

hol would be reduced to the green chloride; this agrees with Warington's experiments.—H. C.]

On Glucinium and its Compounds.

Awdejew has made a series of experiments on the salts of glucina, under the direction of H. Rose. Great care was used in the preparation and analysis of the chloride of glucinium. It was found to contain 87·54 per cent. of chlorine, whereas it has been supposed to contain only 66·70. When dissolved in water it forms hydrochlorate of glucina; on evaporating the solution a crystalline mass is obtained which has the composition $GCl+4$ aq. [Awdejew supposes the oxide to contain one atom of oxygen.] The atomic weight of the oxide was determined from the analysis of the neutral sulphate, which is obtained by dissolving the carbonate in excess of sulphuric acid and separating by alcohol; the salt is precipitated and may be dissolved and recrystallized; its formula is $\dot{G}\dddot{S}+4$ aq. The atomic weight of glucina is, according to these analyses, 158·084, and that of the metal 58·084. A double sulphate of glucina and potassa may be obtained by gently evaporating a mixture of equal atoms of the two sulphates; it is decomposed by boiling, slowly soluble in cold water. Its formula is $\dot{K}\dddot{S}+\dot{G}\dddot{S}+2$ aq. It might be $3\dot{K}\dddot{S}+\underline{\dddot{G}}\dddot{S}^3$, but $\dot{K}\dddot{S}+\underline{\dddot{G}}\dddot{S}^3$ cannot be formed. The double fluoride of glucinium and potassium was also analysed; it is $KF+GF$; it is anhydrous, difficultly soluble. There are three basic sulphates which have been described and analysed by Berzelius (*Lehrbuch*, iv). These formulæ, according to the new equivalent, are $\dot{G}^3\dddot{S}$, $\dot{G}^2\dddot{S}$ and $\dot{G}^6\dddot{S}+3$ aq. At the end of his treatise Awdejew considers how the formulæ of minerals containing glucina are affected by this change in the atomic weight. Chrysoberyll becomes $\dot{G}\underline{\dddot{Al}}$; phenakite $\dot{G}^3\dddot{Si}$; beryll $\dot{G}^3\dddot{Si}+\underline{\dddot{Al}}\dddot{Si}$; euklas $2\dot{G}^3\dddot{Si}+\underline{\dddot{Al}}^2\dddot{Si}$; leucophane $\dot{G}^3\dddot{Si}+\dot{Ca}^3\dddot{Si}^2+NaF$ —(Pogg. *Ann.*, vol. lvi. p. 101.)

In vol. l. of the same Journal Count Schaffgotsch published some experiments on glucina. He analysed the hydrate, and gave as its formula $\underline{\dddot{G}}+8$ aq; according to the new atomic weight $\dot{G}^3+4$ aq would agree best with the analysis; the oxide is dissolved by concentrated caustic potassa, and is not precipitated by boiling, unless the solution be diluted, when the whole is thrown down. By boiling the solution of glucina in carbonate of ammonia a granular salt is precipitated, for which Schaffgotsch proposes the formula $2\,\dddot{G}\,\ddot{C}^3\,\dot{H}^6 + 3\,\underline{\dddot{G}}\,\dot{H}^6$; this complex proportion becomes somewhat more simple if we take the new equivalent, when we find it to be $\dot{G}\,\ddot{C}, \dot{H} + 4\,\dot{G}\,\dot{H}$.

In the same volume is also a paper by Ch. Gmelin on some properties of glucina.

Action of Water on certain Sulphurets and haloid Salts.

H. Rose has published a most interesting paper on this subject; most of the experiments were made with sulphuret of barium, which was prepared by strongly heating a mixture of charcoal and sulphate of baryta. The black mass was treated in a closed bottle with a quantity of water far insufficient to dissolve all the sulphuret; after standing twenty-four hours it was decanted and a fresh portion added, and this repeated nine times; each portion was kept separate. The first and second solutions contained hydrosulphuret of barium (H S, Ba S), which was proved by the evolution of sulphuretted hydrogen, on adding to them a concentrated neutral solution of sulphate of manganese; the sulphur was oxidized by treating the salt with hydrochloric acid and passing the sulphuretted hydrogen into strong nitro-hydrochloric acid; the sulphur was thus perfectly oxidized. A stream of air was passed through the solution to carry over all the hydrosulphuric acid, and then chlorine passed into it to oxidize any sulphur. The oxidized fluids mixed together, the sulphate of baryta separated; in the filtered liquor a large precipitate was produced by chloride of barium. The third solution gave only a slight smell of H S, with sulphate of manganese, but a copious evolution with hydrochloric acid. Chloride of barium (as above) produced only a slight precipitate; it contained therefore sulphuret with a small portion of hydrosulphuret. The fourth gave no trace of S H with sulphate of manganese, abundance with hydrochloric acid; no precipitate was produced by chloride of barium, but a slight one by sulphuric acid; it contained therefore sulphuret and baryta. The fifth contained less sulphuret and more baryta, and the others only a trace of sulphuret. When large quantities of sulphuret of barium are boiled with water the same products are obtained; sometimes hydrate of baryta crystallizes, sometimes sulphuret, and sometimes compounds of both; the hydrosulphuret is the most soluble product. The composition of one compound, which formed good crystals, was $\dot{Ba}\ \dot{H}^{10} + 3\ Ba\ S, \dot{H}^{6}$. Another gave the formula $4\ \dot{Ba}\ \dot{H}^{10} + 3\ Ba\ S, \dot{H}^{6}$; a third appeared to be $\dot{Ba}\ \dot{H}^{10} + Ba\ S, \dot{H}^{10}$; but it is possible that the last two were only mixtures.

Sulphuret of barium crystallizes with six atoms of water; water acts upon this salt in the same manner as upon the reduced sulphate of baryta. The sulphuret can hardly be obtained free from hydrate of baryta. The solid hydrosulphuret of barium was not analysed, as it cannot be obtained free from supersulphurets, sulphuret and hydrate of baryta.

It appears therefore that sulphuret of barium is decomposed by water and forms hydrosulphuric acid and baryta; the affinity which the $H\ S$ has for the sulphuret causes the separation of baryta, which crystallizes, sometimes as hydrate, and at other times in combination with the sulphuret.

Sulphuret of strontium, as formed from sulphate and charcoal, is decomposed in the same manner as that of barium; the more difficult solubility of the hydrate of strontia causes it to be separated from the other salts with great ease. H. Rose could obtain neither sulphuret of strontium nor its compound with strontia; the sulphuret is decomposed by boiling into the earth and the hydrosulphuret. Hydrate of strontia contains 10 atoms of water, which agrees with the statements of Phillips and Noad*; the baryta compound also contains 10 atoms.

Sulphuret of calcium was prepared by heating the sulphate with charcoal; the mass when heated with water furnishes solely hydrosulphuret and hydrate of lime; the principal cause of this appears to lie in the insolubility of the hydrate. On boiling the solution of the hydrosulphuret in a retort, hydrosulphuric acid is evolved and lime precipitated; on further evaporation the solution assumes a yellow colour; a white powder, sulphite of lime, is often precipitated, formed from the hyposulphite produced by the boiling. In the concentrated solution long golden yellow crystals are formed, they are very small in quantity although large in volume. The crystals evolve no hydrosulphuric acid when treated with sulphate of manganese, but only with acids, sulphur being separated; treated with a large quantity of water they leave behind a quantity of lime. When heated they give off water and sulphur; the residue treated with acid gives sulphur and hydrosulphuric acid. The formula of this compound is $Ca\ S^{5} + 5\ Ca\ O + 20$ aq.—(Pogg. *Ann.*, vol. lv. pp. 415–437.)

---

* See Phil. Mag., Third Series, vol. xi. p. 301.—Ed.

In a second paper H. Rose takes into consideration the long-disputed point, whether the haloid salts and sulphurets decompose water when dissolved. Judging from analogy and the example of the sulphuret of barium, one would suppose that sulphuret of potassium would be decomposed with water into hydrosulphuret and caustic potassa; *the solution of this sulphuret turns red litmus paper blue*; and by its solution in water heat is evolved, and we do not know that sulphuret of potassium combines with water of crystallization. Rose concludes that the higher sulphurets are not decomposed by water. The compounds of fluorine are so similar to those of sulphur that we might almost be justified in placing fluorine in a class with sulphur and not with chlorine; it is *possible* that on dissolving fluoride of potassium in water, potassa and hydrofluoride are formed. Rose could not obtain them separate, but, as is well known, the solution reacts alkaline and also attacks glass. The fluoride of ammonium gives ammonia and hydrofluoride.

Chlorides of potassium, sodium and ammonium produce a considerable degree of cold when dissolved in water, and hence we may conclude, that on the solution of these salts *water is not decomposed.* Chloride of calcium evolves heat when dissolved, and Thenard and Gay-Lussac adduced this to prove the decomposition of water, but it is simply a combination of the salt with water of crystallization. Chloride of sodium produces less cold than chloride of ammonium, but we know that under certain circumstances the former can combine with four atoms of water. The same is the case with several oxysalts.

Rose has found that chlorides of antimony and bismuth evolve heat when dissolved, and supposes that they decompose water. As a general approximate rule we may say, that all compounds of bromine, chlorine, iodine, cyanogen and sulphocyanogen with metals which are *equivalent* to the basic oxides, dissolve in water without decomposition, while those that represent the acid oxides decompose water. Fluoride of potassium evolves heat, but it combines with water; the hydrated salt may be obtained by gentle evaporation, or by adding alcohol to a solution of the fluoride; it contains four atoms of water.—(Pogg. *Ann.*, vol. lv. pp. 534, 557.)

It is impossible to give any but an imperfect report of this most excellent paper in these notices without exceeding our limits; we must most earnestly recommend the perusal of the original to all chemists.

XLIX. *On the Occurrence of Shells and Corals in a Conglomerate Bed, adherent to the face of the Trap Rocks of the Malvern Hills, and full of rounded and angular fragments of those rocks.* *By* JOHN PHILLIPS, *Esq., F.R.S., &c.*

THE researches of Sir H. T. De la Beche during the autumn of 1841 into the nature, antiquity and organic contents of the trappæan ash-beds of North Pembroke, coupled with other parallel inquiries, have excited in the minds of those persons who are attached to the Ordnance Geological Survey a lively interest in the study of the relations between trap rocks and the strata amongst which they appear. A very common result of this study in South Wales is a conviction of the rarity of irruptive trap and the frequency of interstratified (and, in ordinary language, contemporaneous) beds of plutonic rocks and felspatho-hornblendic sediments, which are not always clearly distinguished from the fused rocks. On these points in the same or neighbouring districts, Professor Sedgwick and Mr. Murchison deliver nearly the same judgment.

The great obligations which geology owes to Mr. Leonard Horner and to Mr. Murchison for their descriptions of the fused and sedimentary rocks in this chain, and of the grand movements in the crust of the earth, of which it is a noble monument, are universally admitted, but demand a glad acknowledgement from one who, following in their steps and profiting by their experience, desires to join to theirs the additional information which he may be so fortunate as to gather.

After finishing the colouring of a great part of the Ordnance map of this district, I turned to examine with care and interest the great problem which the Malvern hills present, viz. the determination of the *circumstances under which the plutonic rocks were elevated.* For this purpose the appearance of the fused and sedimentary rocks in every part of the Malvern chain and the surrounding country has been considered, separately and in combination; and the general result is, that the elevation of these hills is a part of that grand series of associated movements, which the Director and other members of the Geological Survey have been tracing between St. Bride's Bay and the Severn, between the Teivy and the Bristol Channel.

Viewed in this association, the geological epoch when the great movement of the Malvern rocks occurred, becomes determinable, and has in fact been determined by the eminent geologists already named. No one can witness the many anticlinal and synclinal curvatures which on the western flank of the Malverns affect equally the Silurian and old red formations, and then survey the comparatively horizontal and unmoved strata of new red marls and sandstones, which on the east and south touch indiscriminately the sienites, Caradoc sandstones, Wenlock limestone, and old red sandstone, without being satisfied that the great upward movement of the Malvern rocks happened in the interval between the old and the new red sandstones.

But in what state were these plutonic masses raised? as fused and liquid matter, or solidified rock? To determine this question, the observed *positions* of the strata which adjoin the trap range are important, but their *condition* and *contents* are still more essential. My first expectation, on looking generally at the narrow continuous range of the Malverns, was, that here might be found an example of a gigantic sinuous mass, emitted in a liquid state along a portion of that great irregular fracture which is the western boundary of the new red sandstone, from the Severn to the Dee. The complicated nature of the trap, its innumerable vein-like segregations, its included gneissic beds, gave an additional interest to the examination of the appearances at and near the junction of the trap with the exterior stratified masses.

In aid of this inquiry I fortunately discovered two remarkable localities where Silurian strata of determinate age were in contact with the trap masses; one exposed in the deep cutting at the Wych, the other on the depressed summit of drainage between the Hereford beacon and Swinyard hill. Besides these are several examples of the sedimentary aggregates of the lower Silurian strata in juxtaposition or actual contact with the trap rocks of the high Malvern ridge; with a detached series of low insulated ridges and bosses of trap on the western side near the southern extremity of the chain; and with some low mounds described by Mr. Murchison at the northern extremity.

The appearances connected with the low points at the northern end, and with a part of the ridge near the southern extremity, have been considered to indicate metamorphism in stratified rocks by heat[*]; and the phænomena associated with the detached bosses and hillocks on the western side of the chain, may be believed to indicate irruption of trap amongst the *lowest of the Silurian strata*; but generally along the chain itself, and especially in all the northern parts of it, there appears no evidence that the adjacent exterior strata have been invaded by liquid irruptive rock.

---

[*] Murchison's Silurian System, p. 417 *et seq.*

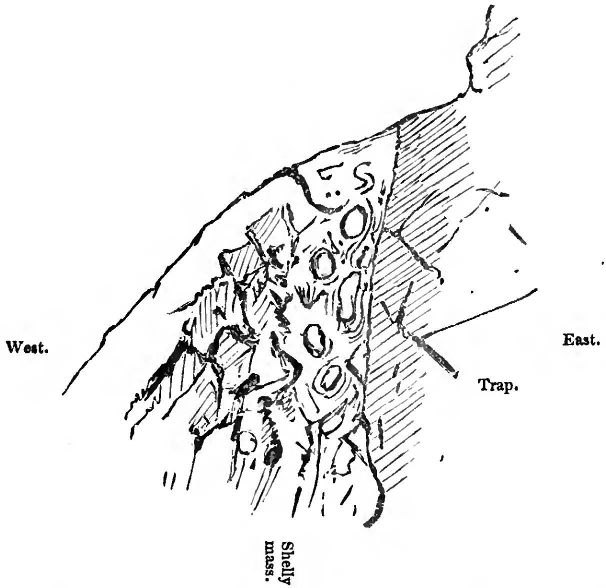
In the deep cutting at the Wych, sandstones and shales of the Caradoc formation are placed in a singular manner between masses of trap, but are entirely unchanged in aspect, and retain the usual organic remains. On the summit ridge near Swinyard hill, the upper beds of the Caradoc series, with the usual limestone bands and shales of that part of the Silurian strata, rest against solid felspathic trap on the south side and cover it as with a saddle. The corals and shells here gathered were in their usual state, and the strata appear unaltered.

Contrasting these cases with others in the midst of the Malvern hills, where stratified rocks are irregularly mixed with the fused rocks, and have the character of gneiss, and with others on the western flanks where dykes and bosses of trap appear amongst peculiar sandstones and black shales, it appeared *probable* that some parts at least of the Malvern ridge were of higher antiquity than any of the exterior strata; that amongst the lowest of these strata, local and limited irruptions of *a different sort of trap* had occurred; but that the greater part of the Silurian strata visible in the northern parts of the hills had been subject to no peculiar heat emanating from the Malvern ridge.

In this condition of the argument Mr. Murchison and Count Keyserling passed through Malvern and inspected the section of the Wych, as well as the north end of the Malverns, and Professor Sedgwick accompanied me on a leisurely survey of this and other points further south. On the day (August 1) while I was enjoying the advantage of his experience in examining the facts thus briefly adverted to, a discovery was made which threw a new and concentrated light on the phænomena we were discussing.

My sister, knowing the interest I felt in tracing out the history of the stratification visible in these trap hills, sought diligently for organic remains in the midst of and on the western flanks of the sienitic masses of the North hill and Sugar-loaf hill. In this most unpromising search she was entirely successful, and collected from the midst of heaps of fallen stones, which seemed to be all trap, several masses richly charged with organic remains, and full of felspar, quartz, and hornblende, in grains and large lumps. On careful examination, it was seen that those lumps were fragments, generally rolled to pebbles, and distributed with reference to one another and to the shells, just as quartz pebbles and chips are in a common conglomerate. It was, in fact, certainly and evidently a conglomerate full of Silurian shells, and pebbles and fragments of the sienitic, felspatho-quartzose and other rock-masses of the Malvern hills.

The next thing to determine was the position of this conglomerate in relation to the ridge of sienitic rocks amongst the detritus of which its fragments lay. This was difficult.



The abundance of detritus on all the slopes is so great as to conceal for the most part the junction of the stratified and unstratified rocks. The loose shelly pieces we found abundantly for fully one-third of a mile along the mountain side, and at length the conglomerate rock itself was plainly seen adhering to the extreme western nearly vertical face of the trap mass, west of the Worcestershire beacon, in a situation contiguous to a large excavation of the lower Caradoc sandstone.

These facts ascertained, I waited for the arrival of Sir H. T. De la Beche at Malvern, to have the shelly bed thoroughly explored, and its contact with the trap rocks carefully traced. We found the surface of the trap nearly vertical, but undulating and irregular, and its strike nearly north and south; the rock is here hornblendic, dark green or purplish in colour, and, as usual in all these hills, it is within short distances mixed and variegated with more felspathic portions, felspatho-quartzose veins, &c. Closely adhering to it was usually a softish laminated clay; bedded in the clay, or touching the trap rock, were multitudes of rolled pebbles and angular chips and fragments of stone, accumulated in an irregular bed above a foot or only a few inches in thickness against the trap. In the intervals of these pebbles were partial admixtures of argillaceous shale, abundance of shells, and smaller chips and fragments of stone, more or less stained brown, in the same manner as commonly happens in shelly cavities in other conglomerates and sandstones far removed from the trap. Exterior to this very pebbly mass, the shells were equally numerous, but the rock fragments amongst which they lay were generally angular, appearing just as if they had fallen from a cliff upon a pebbly beach, and received into their interstices abundance of shells and sand drifted by the water.

The degree of *firmness* of the shelly masses thus examined *in situ*, is less on an average than that of the loose pieces on the hill slopes which were first observed; these latter being the hardest portions which best withstood destroying agencies.

The shells, corals and encrinites, are commonly represented by casts and moulds, but a few specimens have occurred of Turbinolopsis, with the calcareous substance entirely preserved.

The pebbles and fragments of stone mixed with the shells are of the same nature as the rocks immediately adjacent and composing the neighbouring hills; that is to say, characteristic compounds and segregations of hornblende, felspar, quartz, and mica, in great variety. The whole mass is stained by ferruginous admixtures, and at a small distance looks like some of the dark trap of the hills with which it is in contact. What may be its degree of induration at a considerable depth is unknown, the situation allowing only of an exploration to the depth of a few feet.

The just inference from the occurrence of the shelly conglomerate thus briefly described, appears to be that the sienitic and other associated rocks of the northern portion of the Malvern hills were accumulated and indurated previous to the aggregation of the lower portions of the Caradoc sandstone series; and that they were, with the whole Silurian series, raised in a solid state.

In harmony with this conclusion, is the abundance of fragments and disintegrated grains of the Malvern rocks in other conglomerates (not shelly) of the Caradoc series, about the north end of the chain, examined by Sir H. T. De la Beche and myself. Even in Ankerdine hill, eight miles north of Malvern, fragments of the sienitic rocks were observed in the Caradoc sandstone by Capt. James, R.E., and myself; and the conglomerate of May hill yielded similar results to Sir H. T. De la Beche.

Observations of this nature, combined with accurate surveys of the great lines of subterranean movement, may hereafter enlarge the limited view now presented of a part of the Malvern hills, into a general contemplation of the agency of heat during the Palæozoic periods in the great physical region between the vale of the Severn and the coasts of Wales. But to state such a speculation without the data which have been collected for its illustration, would be useless or injurious, and the constitution of even the Malvern chain itself is sufficiently varied in its different parts, to induce a long pause before the apparently proved high antiquity of the northern sienites should be implicitly extended even to the southern portion of the same chain.

Malvern, Sept. 19, 1842.

L. *On the Dispersion of the Optic Axes, and of the Axes of Elasticity, in Biaxal Crystals. By* JAMES MACCULLAGH, *LL.D., M.R.I.A., Fellow of Trinity College, and Professor of Mathematics in the University of Dublin*[*].

IN the last Number of the Philosophical Magazine (p. 228), there appeared an extract from the Proceedings of the Royal Irish Academy, containing a notice of a memoir which I had the honour of reading to that body on the 24th of May, 1841; and in the concluding paragraph of the notice a brief allusion is made to a "mathematical hypothesis" by which I had connected the laws of dispersion and those of the elliptic polarization of rock-crystal with the other laws that were there announced. My present object is to indicate the development of that hypothesis, with reference more particularly to the subject of dispersion in crystals, and to communicate a very simple result which I have lately had occasion to obtain from it. The result is remarkable as embracing and explaining a class of intricate phænomena which hitherto have not been connected with any theory, or rather have stood in opposition to all theories; I mean the phænomena of the dispersion of the optic axes, and of the axes of elasticity (as they are called) in biaxal crystals.

The name of axes of elasticity was given by Fresnel to three rectangular directions, which, according to his theory, exist in every crystallized medium, and which are distinguished by the property, that if a particle of the medium be slightly displaced in the direction of any one of them, the elastic force thereby called into play will act precisely in the line of the displace-

---

[*] Communicated by the Author.

ment. These directions coincide with the axes of the ellipsoid by which he constructs his wave-surface; and the position of the axes being thus fixed, it is only their *lengths* that can be supposed to vary for the differently coloured rays. Such is the view taken by Fresnel with regard to crystalline dispersion, and it is obviously the only view that his theory admits. Succeeding theorists, in their numerous attempts to deduce Fresnel's beautiful laws from dynamical principles, have always been obliged to assume that the medium is symmetrically arranged with respect to three rectangular planes; and as, in this hypothesis, the axes of elasticity, or of optical symmetry, necessarily coincide with those of symmetrical arrangement, their directions are fixed, as before, independently of colour.

From these principles it follows that the optic axes for different colours all lie in the same plane, namely, the plane of the greatest and least axes of the ellipsoid, and that they are equally inclined to each of the latter axes, so that the angle made by any pair, to whatever colour they belong, is always bisected by the same right line. This was accordingly, for a long time, believed to be the case; and the earlier experiments of Sir J. Herschel (Phil. Trans. 1820) which are appealed to by Fresnel, as well as the observations of Sir David Brewster, seemed to establish it as a general law. But it was afterwards discovered by Sir J. Herschel, that, in *borax*, the optic axes for different colours lie in different planes inclined at very sensible angles to each other; and the same discovery was made about the same time (1832) by M. Nörrenberg. The latter observer further ascertained, that even when the optic axes all lie in the same plane, there are cases, as in sulphate of lime, wherein their angles are not bisected by the same right line. These facts, and others of a like nature that have been since observed, show the falsehood of the supposition that the lines called the axes of elasticity have always the same directions whatever be the colour of the light; they are inconsistent with all received notions, and contradict every theory that has been hitherto proposed. No person, as far as I am aware, has even attempted to explain them.

But in the theory which I have constructed to represent the laws of the action of crystallized bodies upon light, and which has already brought so much within its grasp, the phænomena in question do not offer any difficulty whatever; on the contrary, they are of a kind that would naturally be looked for, antecedently to experiment. For in this theory, I make no hypothesis as to the constitution of the æther, or the arrangement of its molecules; nor any hypothesis, like that of Fresnel, respecting the mechanical signification of the axes of elasticity. The existence of three rectangular axes possessing peculiar properties is not a principle, but a result, of theory; their directions are determined by conditions perfectly analogous to those which determine the principal axes of an ellipsoid from its general equation; and these directions are functions of certain quantities which are constant when differentials of the second and subsequent orders are neglected, but which vary when these are taken into account. The differentials of higher orders introduce terms depending on the wave-length; and thus the *directions*, as well as the *lengths*, of the principal lines depend on the colour of the light, or, to speak more accurately, on the length of the wave.

All this will be easily understood if we recur to the first principles of the theory. According to these, everything depends on the form assigned to the function V in the general dynamical equation

$$\iiint dx\,dy\,dz\left(\frac{d^2\xi}{dt^2}\delta\xi+\frac{d^2\eta}{dt^2}\delta\eta+\frac{d^2\zeta}{dt^2}\delta\zeta\right)=\iiint dx\,dy\,dz\,\delta V,$$

from which the motion of the æther is deduced. In my first memoir on the subject (read to the Academy on the 9th of December, 1839), I showed that when differentials of the first order only are preserved, the function V—which may perhaps with propriety be called the *potential*, since the motion of the system is potentially, or virtually, included in it—is a function of the second degree, composed of the three quantities X, Y, Z, which are connected with the displacements ξ, η, ζ by the following relations:—

$$X=\frac{d\eta}{dz}-\frac{d\zeta}{dy},\qquad Y=\frac{d\zeta}{dx}-\frac{d\xi}{dz},\qquad Z=\frac{d\xi}{dy}-\frac{d\eta}{dx}.$$

To show this, I make use simply of the consideration that the motion must be such as to satisfy the condition

$$\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}=0,$$

which seems to be characteristic of the vibrations of light. But the same condition allows us to suppose that the potential contains not only the quantities X, Y, Z, but their differential coefficients of any order with respect to the coordinates. This supposition, however, is too general, and requires to be limited by other considerations. Now the most natural restriction which can be imposed consists in the assumption that the quantities of all orders are formed on the same type, those of any order being derived from the preceding in the same way that the quantities X, Y, Z are derived from ξ, η, ζ; there are particular reasons also which go to strengthen this hypothesis, and have led me to adopt it. Putting therefore

$$X_1 = \frac{dY}{dz} - \frac{dZ}{dy}, \quad Y_1 = \frac{dZ}{dx} - \frac{dX}{dz}, \quad Z_1 = \frac{dX}{dy} - \frac{dY}{dx},$$

$$X_2 = \frac{dY_1}{dz} - \frac{dZ_1}{dy}, \quad Y_2 = \frac{dZ_1}{dx} - \frac{dX_1}{dz}, \quad Z_2 = \frac{dX_1}{dy} - \frac{dY_1}{dx},$$

and so on, I suppose the potential to be a function of the second degree, composed of all the quantities X, Y, Z, X_1, Y_1, Z_1, X_2, Y_2, Z_2, &c.; and this is the "mathematical hypothesis" alluded to in the beginning of this article. The hypothesis occurred to me more than three years ago (June 1839), but I did not venture to communicate it to the Academy until the date of my second memoir (May 1841); and even then I had not studied it with the attention which I now conceive it merits. It was only very lately, in fact, in some conversations which I had with M. Babinet during a short visit to Paris, that my attention was strongly drawn to the subject of dispersion in crystals, particularly the dispersion of the axes of elasticity. My thoughts then naturally reverted to the hypothesis which I have mentioned, and since my return I have found that it affords a complete explanation of all the phænomena *.

I have also found that it gives the general law, extended to biaxal crystals, of that elliptic and circular polarization which has hitherto been detected only in quartz and in certain fluids; while for the case of rectilinear polarization it gives a law (very possibly a true one) more general than that of Fresnel, but quite as elegant, and differing very slightly from it. The hypothesis, therefore, is still too general for our present purpose. To make it include only those crystals to which the law of Fresnel is rigorously applicable, the alternate derivatives X_1, Y_1, Z_1, X_3, Y_3, Z_3, &c. must be supposed to vanish in the function which represents the potential. Then, the axes of coordinates having any fixed directions within the crystal, the axes of elasticity will be the principal axes of an ellipsoid represented by an equation of the form

$$A x^2 + B y^2 + C z^2 + 2 D y z + 2 E x z + 2 F x y = 1,$$

---

* I am indebted, for my information on the subject, to a short article, drawn up by MM. Quetelet and Babinet, in the Bulletin of the Royal Academy of Brussels, vol. ii. p. 150; as also to Poggendorff's Annals, vol. xxvi. p. 309; vol. xxxv. p. 81.

in which *each* of the six coefficients, the first, for example, expresses a series of the form

$$A_0 + \frac{A_1}{\lambda^2} + \frac{A_2}{\lambda^4} + \frac{A_3}{\lambda^6} + \&c.,$$

where λ denotes the wave-length, and all the other quantities are constant. The ellipsoid itself is the reciprocal of that ellipsoid by which the wave-surface is constructed, and its semiaxes are the three principal indices of refraction. As λ is supposed to vary, not only the length but the direction of the principal axes vary, and thus we have a different wave-surface for every different wave-length *within* the crystal.

The optic axes are perpendicular to the circular sections of the above ellipsoid, and describe, in general, two fragments of a cone, the equation of which may be found by supposing λ to be variable in the equation of the ellipsoid. But only very particular cases have been hitherto observed, and I shall not stop to discuss them.

Trinity College, Dublin,
September 1842.

J. MacCullagh.

LI. *Remarks on a paper by* Professor Challis, "*On the analytical Condition of the Rectilinear Motion of Fluids.*" *By* G. G. Stokes, *B.A., Fellow of Pembroke College, Cambridge*[*].

In the August Number of this Magazine (p. 101), Professor Challis has written an article, of which the object is to prove that, in all cases of fluid motion in which $u\,dx + v\,dy + w\,dz$ is an exact differential, the motion is rectilinear. The importance of this question may apologize for these remarks, since, if the reasoning in that article be correct, it will affect the validity of much that has been written on the subject. It appears to me however that Professor Challis has made an assumption which is not allowable, and consequently the conclusion founded on it is not allowable either. In what follows, I shall call the path of a particle of fluid in space *a line of motion*, and a line traced at a given instant from point to point in the direction of the motion *a line of direction*.

As the basis of his reasoning Professor Challis assumes, that in every case where the continuity of the fluid is maintained, the most general supposition that can be made respecting the directions of motion in each indefinitely small element of the fluid is, that they are normals to a surface of continuous curvature, and as such pass ultimately through

---

[*] Communicated by the Author.

two focal lines; that is to say, that the above is true neglecting quantities of the order $\mathrm{P}\,p^2$, P and p being any two points in the element; that this is the meaning is shown by the fact that the whole investigation depends on quantities of the order $\mathrm{P}\,p$. Now, not only in the case where $u\,dx + v\,dy + w\,dz$ is an exact differential, but also in the case where it is integrable by a factor, there exists a surface of displacement passing through P, and the above statement will be true for an element of this surface. But it will not generally be true for an element of three dimensions; for, let p be taken in the line of direction passing through P; then, if ϖ be the radius of absolute curvature of this line at the point P, and $\mathrm{P}\,p = \delta s$, the angle between the tangents at P and p will be ultimately $\frac{\delta s}{\varpi}$. Neglecting quantities of the order δs^2, a line PT' drawn through P parallel to the tangent at P may be taken instead of the tangent at p. Now, even if we suppose the line $\mathrm{P\,T}'$ to pass through the focal line which is at a distance r from p, the least distance between it and the other focal line, which is at a distance r' from p, will be ultimately $r'\frac{\delta s}{\varpi}$. Hence, it cannot ultimately pass through both focal lines, unless ϖ be at every point infinite, i. e. unless all the lines of motion be right lines, which is evidently a very limited case. Consequently, it is only in this case that it is proved that surfaces of displacement are surfaces of equal velocity.

There is another part of Professor Challis's reasoning with which I cannot agree. It is that $d\left(\frac{d\phi}{dt}\right)$ or $\frac{d^2\phi}{dt\,dx}\,dx + \frac{d^2\phi}{dt\,dy}\,dy + \frac{d^2\phi}{dt\,dz}\,dz = 0$, in passing from one point to another of a surface of displacement. For, $d\left(\frac{d\phi}{dt}\right) = 0$ is the differential equation to a family of surfaces whose general equation is $\frac{d\phi}{dt} = \mathrm{C}$, which family of surfaces is in general quite different from that whose equation is $\phi = \mathrm{C}'$. Now the proof requires that the variations dx, dy, dz should be taken along that surface of the second family which passes through the point (x, y, z), whereas the variations for which $d\left(\frac{d\phi}{dt}\right) = 0$, must be taken along that surface of the first family which passes through the same point. If $\phi = \psi(t)\,(x^2 - y^2) + \chi(t)\,xy$, for instance, these two surfaces will be different.

In any possible case of fluid motion, the motion, which would result by supposing the whole mass of fluid so in motion to be besides moving forward in space with a uniform velocity, would also be possible. But if the components u, v, w of the velocity in the first case be such that $u\,dx+v\,dy+w\,dz$ is an exact differential, it will be easily seen that the components u', v', w' of the velocity in the second case will also be such that $u'\,dx+v'\,dy+w'\,dz$ will be an exact differential. But if the lines of motion in the first case be right lines, they will not be so in the second, unless the velocity at each point of the same line be the same. If, for instance, $u=\frac{a\,x}{x^2+y^2}$, $v=\frac{a\,y}{x^2+y^2}$, $w=0$, and if we now suppose the whole mass of fluid to be moving besides with a uniform velocity parallel to x, the lines of motion and of direction will both be right lines in the first case, but neither of them will be right lines in the second.

Professor Challis objects to the case of motion to which he alludes, where $u=a\,x$, $v=-a\,y$, $w=0$, by saying that the arbitrary quantities introduced in the process of integration cannot be satisfied, unless the fluid be in confined spaces or narrow canals; that is in *indefinitely* narrow canals, as his reasoning which follows shows to be the meaning. It will appear however from the following reasoning that the canals need not be narrow.

Conceive a mass of incompressible fluid to be at rest, bounded by material parallel planes, and by cylindrical surfaces whose bases are part of a branch of a rectangular hyperbola, its asymptotes (which I shall take for the axes of x and y), and two lines perpendicular to them. Of the two planes whose bases are the two latter lines, conceive one, whose equation is $y=y_1$, to be made to move with a velocity $-f(t)\,y_1$ parallel to y, and the other, whose equation is $x=x_1$, with a velocity $f(t)\,x_1$ parallel to x, and conceive the planes to contract or expand, so as always to reach from the hyperbola to an asymptote. Then the motion is determined by the equations of motion, the equation of continuity, and the condition that the particles in contact with a surface, whether fixed or moveable, neither penetrate into, nor separate from it. Since the motion is determinate, and these are the only conditions to be satisfied, any values of u, v, w and p which satisfy them, will be the true values. Such values will be found to be

$$u=f(t)\,x,\quad v=-f(t)\,y,\quad w=0,\quad \frac{p}{\varrho}=\psi(t)-\tfrac{1}{2}(u^2+v^2).$$

The function f is arbitrary, and may be discontinuous. It is supposed to be nothing at first. If it suddenly acquires a finite value, the motion will begin with impact. It will be easily seen that the equations of impulsive motion, and the conditions with respect to the surfaces, will be satisfied by the above values of u and v, and the value of the impulsive pressure $C - \frac{\varrho}{2}(u^2+v^2)$.

LII. *On Conchyliometry. By the Rev.* H. Moseley, *M.A., F.R.S., Professor of Natural Philosophy and Astronomy in King's College, London.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a paper printed in the Transactions of the Royal Society (1838, part ii.*) I have investigated certain properties of the spiral curves traced upon the surfaces of shells (concho-spirals†) common to them and to the well-known logarithmic spiral.

The results deduced from my admeasurements have since been confirmed by those of Professor Naumann of Freiberg (Poggendorff's Journal, 1840), who has developed, by an independent investigation, several new properties of these curves, and determined with his accustomed accuracy, in respect to an extensive series of Conchylia, the particular value of the constant angle according to which each traces its concho-spiral.

With a view to a further development of the geometrical properties of shells, I have in my paper, above referred to, investigated certain formulæ representing the equation to the concho-spiral, the volume of a conchoidal solid, the position of its centre of gravity, and the area of a conchoidal surface. In the inclosed paper I have continued these researches in respect to concho-spirals and conchoidal surfaces, and in some particulars corrected them.

King's College, London,
July 20, 1842.

Yours, &c.,
Henry Moseley.

I. *The Polar Equation to a Concho-spiral.*

Let a logarithmic spiral, whose polar equation is $R = R_0 \varepsilon^{\theta \cot A}$, be conceived to be *wrapped* upon a cone the angle at

---

[*] An abstract of Prof. Moseley's paper here referred to was given in Phil. Mag. S. 3. vol. xiii. p. 464.]

† I have adopted the nomenclature of Prof. Naumann.

whose apex is 2ι, the pole of the spiral coinciding with the apex of the cone. The circular arc Θ, whose radius is unity when developed, will when wrapped upon the cone, become a circular arc θ, whose radius is $\sin\iota$,

$$\therefore \Theta = \theta \sin\iota,$$

whence it follows that R representing the distance of any point in the spiral from the apex of the cone, and θ the angle included between two planes, intersecting in the axis of the cone, one passing through that point of the spiral, and the other through the point where $R = R_0$, we have

$$R = R_0\, \varepsilon^{\theta \sin\iota \cot A}.$$

Let R_1 R_2 R_3, &c. be distances from the apex of the cone of points of the spiral in the same straight line passing through the apex,

$$\therefore R_1 = R_0\, \varepsilon^{\theta \sin\iota \cot A}$$
$$R_2 = R_0\, \varepsilon^{(\theta + 2\pi) \sin\iota \cot A}$$
$$R_3 = R_0\, \varepsilon^{(\theta + 4\pi) \sin\iota \cot A}$$
$$(R_2 - R_1) = R_0\, (\varepsilon^{2\pi \sin\iota \cot A} - 1)\, \varepsilon^{\theta \sin\iota \cot A}$$
$$(R_3 - R_2) = R_0\, (\varepsilon^{2\pi \sin\iota \cot A} - 1)\, \varepsilon^{\theta \sin\iota \cot A} \,.\, \varepsilon^{2\pi \sin\iota \cot A}$$
$$\therefore Q = \frac{R_3 - R_2}{R_2 - R_1} = \varepsilon^{2\pi \sin\iota \cot A},$$

Q representing the quotient of any two consecutive distances between the whorls measured on the same straight line passing through the apex.

On the supposition made therefore, viz. that a plane logarithmic spiral is wrapped upon a cone, its pole coinciding with the apex of the cone, it follows that the distances of the successive whorls of the spiral measured on the same straight line passing through the apex of the cone, are in geometrical progression; and conversely. Now in shells they are found, by admeasurement, thus to be in geometrical progression. The spirals described on shells, and called concho-spirals, are therefore such as would result from winding plane logarithmic spirals on cones.

To determine in respect to any shell the constant angle A which the tangent to its concho-spiral when developed makes with its radius vector, let it be observed that

$$\log_\varepsilon Q = 2\pi \sin\iota \cot A$$

$$\therefore \tan A = \frac{2\pi \sin\iota}{\log_\varepsilon Q}, \quad \ldots\ldots\ldots\ldots \quad (1.)$$

where A is the angle required.

Now the quotient Q is the same for all the spirals described on the surface of the same shell; if then we represent

$$\frac{\log_\varepsilon Q}{2\pi} \text{ by } c,$$

we have $\sin\iota \cot A = \dfrac{\log_\varepsilon Q}{2\pi} = c$,

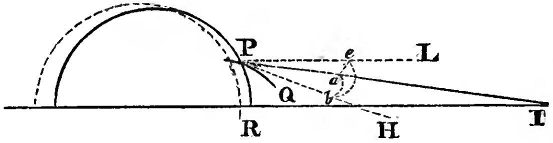
and $$R = R_0\, \varepsilon^{c\,\theta} \quad \ldots\ldots\ldots\ldots \quad (2.)$$

which is the general equation to a concho-spiral.

Since each of the concho-spirals on any shell must have its origin in a corresponding point of the generating curve of that shell when in its *initial* position, and since the *initial* dimensions of the generating curve of every such shell are exceedingly (perhaps infinitely) small, it follows that all such spirals have their origins very nearly (perhaps accurately) in the same point, and therefore that the conical surfaces on which they are severally described have their *apices* in the same point*; the value of R_0 being the distance from the common apex to that particular point of the generating curve, at which the spiral intersects it, in that position in which θ is assumed to be zero.

II. *To determine the inclination α of the tangent at any point of a concho-spiral to a line drawn from that point parallel to the axis of the shell.*

Let P Q represent any portion of a concho-spiral, P H a tangent to it at P, P L a line drawn from P parallel to the axis I R of the shell, I the apex of the cone on which the concho-spiral is described. Join I P, then is I P H a constant angle represented by A, and H P L (represented by α) is the angle required.



Describe a sphere with radius unity from the centre P, and let *a e*, *a b*, *b e* be the intersection of the planes I P L, I P H, L P H with its surface. The spherical angle *b a e* is a right

---

* It is a law common to all surfaces of revolution whose generating curves varying their dimensions remain always geometrically similar, that the spiral lines described by given points in these curves lie all on the surfaces of cones having a common apex.

angle, since the plane I P H is a tangent to the cone, and I P L passes through its axis,

$$\therefore \cos b\,e = \cos a\,e \,.\, \cos a\,b.$$

$$\text{Now } b\,e = \text{L P H} = \alpha,\ a\,e = \text{L P I} = \text{R I P} = \iota$$

$$a\,b = \text{I P H} = \text{A},$$

$$\therefore \cos\alpha = \cos\iota \,.\, \cos \text{A} \quad \ldots\ldots\ldots \quad (3.)$$

$$\therefore\ 1 + \tan^2\alpha = \sec^2\alpha = \frac{1}{\cos^2\iota\cos^2\text{A}}$$

$$\therefore \tan^2\alpha = \frac{1-\cos^2\iota\cos^2\text{A}}{\cos^2\iota\cos^2\text{A}} = \frac{\sin^2\text{A}+\sin^2\iota\cos^2\text{A}}{\cos^2\iota\cos^2\text{A}}$$

$$= \frac{\tan^2\text{A}}{\cos^2\iota} + \tan^2\iota = \frac{\tan^2\iota}{\cot^2\text{A}\sin^2\iota} + \tan^2\iota = \left\{\frac{1}{\cot^2\text{A}\sin^2\iota} + 1\right\}\tan^2\iota.$$

$$\text{Now}\quad \cot\text{A}\sin\iota = \frac{\log_\varepsilon \text{Q}}{2\pi}\ \text{(equation 1.)},$$

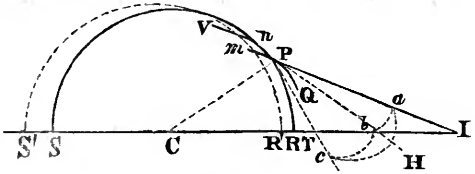
$$\therefore \tan\alpha = \left\{1 + \left(\frac{2\pi}{\log_\varepsilon \text{Q}}\right)^2\right\}^{\frac{1}{2}} \tan\iota. \quad \ldots\ldots\ldots \quad (4.)$$

Similarly, it may be shown that

$$\sin\alpha = \left\{1 + \left(\frac{\log_\varepsilon \text{Q}}{2\pi}\right)^2\right\}^{\frac{1}{2}} \sin\text{A} \quad \ldots\ldots \quad (5.)$$

III. *The Area of a Conchoidal Surface.*

Let R P S represent any position of the generating curve, and Q P *m* a portion of one of the spiral lines generated by any point P in it. Let I represent the apex of the cone on whose surface the spiral P Q is described. Join P I and draw P H a tangent to the spiral, and P T a tangent to the generating curve in P. Imagine a sphere described with radius unity from the centre P, and let *a b*, *b c*, *a c* represent the intersections of the planes H P I, H P T, and I P T with its surface. Now the plane H P I, being a tangent to the cone at P, is perpendicular to the plane R I P which passes through its axis I R S; the spherical angle *b a c* is therefore a right angle.



Moreover, the angle I P Q made by a tangent to the spiral with the line I P drawn from the summit of the cone is, in the case of shells, a constant angle, represented by A, and the angle P I R, being half the angle at the apex of the cone, is also a constant angle in respect to that spiral, represented by ι. The angle P T R made by a tangent to the generating curve at P with the axis of the cone is constant for the different positions of the same point P on the generating curve, *as the curve varies its position* by the variation of θ, but variable for different points on the generating curve, in any given position of that curve; let it be represented by ϕ. ϕ is then a function of the coordinates of the point P in any given position of the curve, and is wholly independent of the angle θ which determines the *position* of the generating curve.

Now in the right-angled spherical triangle $a\,b\,c$,

$$\cos b\,c = \cos a\,b \,.\, \cos a\,c,$$

or
$$\cos \mathrm{H\,P\,T} = \cos \mathrm{H\,P\,I} \,.\, \cos \mathrm{I\,P\,T},$$

but
$$\mathrm{I\,P\,T} = \mathrm{P\,T\,R} - \mathrm{R\,I\,P} = \phi - \iota,$$

$$\cos \mathrm{H\,P\,T} = \cos \mathrm{A} \,.\, \cos(\phi - \iota)$$

$$\therefore \sin \mathrm{H\,P\,T} = \sqrt{1 - \cos^2 \mathrm{A} \,.\, \cos^2(\phi - \iota)}.$$

Let R′ V S′ be a position of the generating curve exceedingly near to the former, and V n a portion of another spiral line on the surface of the shell exceedingly near to the spiral Q P m. Then may the elementary surface P V be considered (in the limit) an oblique parallelogram, whose sides are straight lines, and whose area is therefore represented by

$$\overline{\mathrm{P}\,m} \,.\, \overline{\mathrm{P}\,n} \,.\, \sin m\,\mathrm{P}\,n.$$

Let $\overline{\mathrm{P}\,n}$ be represented by $\Delta\, s$, s representing the arc R P of the generating curve; and P m by Δ S, S representing the length of the spiral measured from the point where $\theta = 0$; and let it be observed that $\sin m\,\mathrm{P}\,n = \sin \mathrm{H\,P\,T}$

$$= \sqrt{1 - \cos^2 \mathrm{A} \cos^2(\phi - \iota)}$$

$$\therefore \text{area P V} = \sqrt{1 - \cos^2 \mathrm{A} \cos^2(\phi - \iota)} \,.\, \Delta\, s \,.\, \Delta\, \mathrm{S}.$$

Now the whole surface is made up of elements similar to P V, therefore passing to the limit and integrating,

$$\text{whole surface} = \iint \sqrt{1 - \cos^2 \mathrm{A} \cos^2(\phi - \iota)} \,.\, ds \,.\, d\mathrm{S}$$

$$= \iint \sqrt{1 - \cos^2 \mathrm{A} \cos^2(\phi - \iota)} \,.\, \frac{d\,\mathrm{S}}{d\,\theta}\, d\theta\, d\,s, \quad \ldots\ldots\ldots \quad (6.)$$

which is a general expression for the area of a surface of revolution, whose generating curve, varying its dimensions, remains always similar to itself.

In the case of shells, if the surface of the cone on which the spiral P Q is described be developed, this spiral will become a plane logarithmic spiral, whose polar equation has been shown to be

$$R = R_0\,\varepsilon^{\Theta \cot A} \text{ where } \Theta = \theta \sin\iota$$

$$\therefore \frac{dS}{d\Theta} = \sqrt{R^2 + \left(\frac{dR}{d\Theta}\right)^2} = R_0 \operatorname{cosec} A \,.\, \varepsilon^{\Theta \cot A}$$

$$\text{Now } \sin\iota \cot A = \frac{\log_\varepsilon Q}{2\pi} = c$$

$$\therefore \frac{dS}{d\theta} = \frac{dS}{d\Theta} \cdot \frac{d\Theta}{d\theta} = R_0 \sin\iota \operatorname{cosec} A\, \varepsilon^{c\theta}$$

$$\therefore \text{con.surf.} = \iint R_0 \sin\iota \operatorname{cosec} A \{1 - \cos^2 A \cos^2(\phi - \iota)\}^{\frac{1}{2}c\theta} \varepsilon\, d\theta\, ds.$$

Now $\sin\iota \operatorname{cosec} A\{1 - \cos^2 A \cos^2(\phi - \iota)\}^{\frac{1}{2}} = \{\sin^2\iota \operatorname{cosec}^2 A$ $- \cot^2 A \sin^2\iota \cos^2(\phi - \iota)\}^{\frac{1}{2}} = \{\sin^2\iota + \sin^2\iota \cot^2 A$ $- \cot^2 A \sin^2\iota \cos^2(\phi - \iota)\}^{\frac{1}{2}} = \{\sin^2\iota + \sin^2\iota \cot^2 A \sin^2(\phi - \iota)\}^{\frac{1}{2}}$ $= \{\sin^2\iota + c^2 \sin^2(\phi - \iota)\}^{\frac{1}{2}}$

$$\therefore \text{con. surf.} = \iint R_0 \{\sin^2\iota + c^2 \sin^2(\phi - \iota)\}^{\frac{1}{2}} \varepsilon^{c\theta}\, d\theta\, ds.$$

Let s_0 be taken to represent the value of s when $\theta = 0$,

$$\therefore s = s_0 \,.\, \varepsilon^{c\theta};$$

differentiating this value of s in respect to a *given position* of the generating curve $\frac{ds}{ds_0} = \varepsilon^{c\theta}$

$$\therefore \text{conch. surf.} = \iint R_0 \left\{\sin^2\iota + c^2 \sin^2(\phi - \iota)\right\}^{\frac{1}{2}} \varepsilon^{c\theta}\, d\theta \frac{ds}{ds_0}\, ds_0$$

$$= \iint R_0 \left\{\sin^2\iota + c^2 \sin^2(\phi - \iota)\right\}^{\frac{1}{2}} \varepsilon^{2c\theta}\, d\theta \,.\, ds_0.$$

Or integrating in respect to θ and observing that R_0, ι, ϕ do not involve θ,

$$\text{conch. surface} = \frac{1}{2c}\left(\varepsilon^{2c\theta} - 1\right) \int R_0 \left\{\sin^2\iota + c^2 \sin^2(\phi - \iota)\right\}^{\frac{1}{2}} ds_0 \quad \ldots\ldots\ldots \quad (7.)$$

where the integral $\int R_0 \left\{\sin^2\iota + c^2 \sin^2(\varphi - \iota)\right\}^{\frac{1}{2}} ds_0$ represents a constant determined by the geometrical form of the generating curve, and its dimensions when $\theta = 0$.

[The *general form* of the expression agrees with that given in equation 15, p. 368 of a paper on the geometrical properties of turbinated and discoid Shells in the Phil. Trans., part ii. 1838.]

LIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 150.]

Nov. 17, 1841. A LETTER addressed to Dr. Fitton, by Mr. Lyell, and dated Boston the 15th of October, 1841, was read.

Mr. Lyell's attention, between the period of his arrival in the United States and the date of his letter, had been principally devoted to the grand succession of Silurian, Devonian, and Carboniferous strata in the state of New York and on the borders of Pennsylvania, having been accompanied during a portion of his tour by the States' Geologist, Mr. J. Hall; but he had also visited, in company with that gentleman, the Falls of Niagara and the adjacent district, and he states, that he purposes to communicate a paper on the phænomena of the recession, drawn from new arguments, founded on the position of a fluviatile deposit below the Cataract. He expresses his intention of also communicating a notice of five localities of Mastodon bones which he had visited, digging up some remains himself, and collecting the accompanying shells, which he says, seem to have been neglected. He had likewise examined, accompanied by Prof. Silliman and his son, the new red, with intrusive trap, in Connecticut; and, assisted by Mr. Conrad, he had collected fossils in every member of the cretaceous system in New Jersey*. The principal object, however, of the present communication is, to point out the extension to the United States of Mr. Logan's generalizations on the beds of fire-clay containing Stigmaria, formerly laid before the Society in a paper on the coal-field of South Wales†. Mr. Lyell had met Mr. Logan at New York, previously to that gentleman's visit to the anthracite coal-field of Pennsylvania, and he adverts to the delight which Mr. Logan must have felt in witnessing the occurrence of beds of Stigmaria fire-clay to an extent far exceeding what could have been expected. On the confines of the states of New York and Pennsylvania, Mr. Lyell found remains of Holoptychius and other fishes in the old red sandstone, and at the bottom of the overlying coal series a thick quartzose conglomerate; and he says that the coal-measures, with their imbedded plants, bear an exact analogy to British coal-measures, both in detail and as a whole. In investigating the coal district of Blossberg, Mr. Lyell had for a guide Dr. Saynisch, president of the mines. The first point which they examined presented three seams of bituminous coal resting on fire-clay containing Stigmariæ, with the leaves attached to the stems, and extending in all directions through the clay; and they observed, in a gallery lighted on purpose, that the stems seen *in situ* were very nearly all parallel to the planes of stra-

---

* Mr. Lyell mentions incidentally having observed between Easton and Trenton, on the Delaware, and in 40° of north latitude, that all the trees were barked on one side, at the height of twenty-two feet above the present level of the river, owing to a freshet and stoppage by ice in the spring of 1841. The stuccoed parts of the houses were also strangely scraped; and in one place the canal, the towing-path of which is twenty-two feet above the river, was so filled with gravel that carriages did not cross by the bridges.

[† See Phil. Mag., S. 3., vol. xviii. p. 217; vol. xx. p. 430.]

tification, only one being in an oblique position. Every stratum underlying a coal-seam examined by Mr. Lyell, presented the same phænomena, except one, and in that case the bed was so sandy that it could not be considered as a fire-clay. The thickness of these Stigmaria deposits varied from one foot to six feet. The roof of the Blossberg coal-seams consists usually of bituminous slates, but occasionally of very micaceous grit, and it contains great varieties of ferns, as well as other plants, agreeing, generically at least, with those common in the British coal-measures.

Mr. Lyell next examined the anthracitic coal-district at Pottsville, on the Schuylkill, in the southern part of the Alleghanies. This district had been examined and described, as well as modelled, by Mr. R. C. Taylor, and the model had been inspected by Mr. Lyell previously to his visit. The whole of Pennsylvania has been mapped by Prof. H. D. Rogers, by direction of the State Legislature. Mr. Lyell refers to this survey, and he states that, by consulting Prof. Rogers's map, it will be found that the Alleghanies, or more properly the Appalachians, which, viewed geologically, are 120 miles broad, consist of twelve or more great parallel ridges, or anticlinal and synclinal flexures, having a general north-north-east and south-south-west strike, but in Pennsylvania a nearly east and west strike prevails. The strata are most tilted on the southern border of the chain, where their position is often inverted, and the folds become less and less towards the central ridges and troughs, which again increase in breadth the more northward their position, till at last the beds are almost horizontal. The oldest formations also are chiefly exposed in the most southern or disturbed regions, where syenite and other plutonic rocks are intruded into the lower part of the Silurian series. It has long been observed, that the anthracitic coal is confined to the southern or Atlantic side of this assemblage of small parallel chains, and that the bituminous occurs in the more inland or less disturbed region; the conclusion, therefore, Mr. Lyell states, seems inevitable, that the change in the condition of the coal was a concomitant of the folding and upheaval of the rocks. The conversion, moreover, is most complete where the beds have been most disturbed; and there are tracts in Pennsylvania and Virginia, near the centre of the chain, where the coal is in a semi-bituminous state. Chemical analysis, likewise, has shown that a gradation from the most bituminous to the most anthracitic coal may be found in crossing the chain from north to south*. The associated shales, &c., of the disturbed regions exhibit no alterations.

It has also been supposed that the anthracite belonged to the transition, and the bituminous coal to the secondary period; but this belief, Mr. Lyell says, has been gradually abandoned, as the knowledge of the geological position and the fossil plants of the coal-districts have become better known. Both the anthracitic and the bituminous coal overlie the old red sandstone, and contain the same ferns, Sigillariæ, Stigmariæ, Asterophyllites, &c.; and they are as abundant and perfect in the anthracite as in the bituminous coal.

---

* See papers by Prof. H. D. Rogers, Dr. Silliman, &c.

At the first point where Mr. Lyell, accompanied by Prof. Rogers, examined the Pottsville coal-measures, the strata are nearly vertical, being cut off by a great fault from the less inclined beds which form the northern prolongation of the measures. They present thirteen beds of anthracite, the lowest of which alternate with the uppermost strata of the coarse underlying conglomerate. The southern wall of an excavation from which the coal had been removed, and which wall occupied the place of the underclay, presented impressions of the stems and leaves of Stigmaria; and on the more solid and slaty beds of the opposite wall, or original roof, there were leaves of Pecopteris, reed-like impressions, and Calamites. In the slightly inclined northern continuation of the coal-measures, Mr. Lyell observed in the Peachmount vein, three miles north-east of Pottsville, a bed of anthracite eight feet thick, overlaid by the usual roof of grey grit, and underlaid by blue clay or shale with Stigmariæ. Impressions of ferns were likewise noticed in the coal itself. Only one instance was met with in the Pottsville coal-district, by Mr. Lyell and Prof. Rogers, of a Stigmaria, placed at right angles to the plane of stratification.

The Pottsville, or southern anthracitic coal-field of Pennsylvania was illustrated by a section resulting from the former labours of Prof. Rogers, under whose guidance Mr. Lyell examined the country. The following remarks may explain the general structure of the country; the names applied to the formations are not, however, those previously employed by the American geologists, but those suggested by Mr. Lyell, in conformity with the conclusions at which he arrived after his tour in New York, and a comparison of the strata of that state with their British equivalents. The contrast between the relative importance of most of the Silurian and Devonian groups in Pennsylvania and in New York, Mr. Lyell states, is very great, arising from a larger portion of sandstones and grits in the Pennsylvanian rocks. The section extends from north of Pottsville to the country ranging immediately south of Orwigsburg. To the south of the vertical coal-measures and the subjacent conglomerate there are displayed successively—1st, a vast series, composed of red shales 3000 feet thick, of grey sandstone 2400 feet thick, and of red sandstone 6000 feet thick, the whole being considered by Mr. Lyell as portions of the old red sandstone; and 2nd, of olive-coloured shale containing Devonian fossils. The dip of the strata is either nearly vertical or inverted. Still further south, and a short distance north of Orwigsburg, the olive-coloured shales are succeeded by very highly inclined or inverted beds of upper Silurian rocks flanking a protruded band of lower Silurian strata; and lastly, on the southern confines of the section is a trough of the Devonian olive-coloured shales resting on the upper Silurian strata.

Beautiful exhibitions of the underclay with its associated plant, and of the overlying roof with its distinct remains, were observed by Mr. Lyell and Prof. Rogers at Tamaqua, in the southern coal-field. The thinning out of the grits and conglomerates of the west causes the beds of anthracite to be brought more nearly together in this district; and Mr. Lyell says, the decrease in the thickness of the intervening strata prepares the observer for the union of several of the seams still farther east, and for the enormous thickness of the anthracite at various places near the village of Mauch Chunk, or Bear Mount, particularly at the well-known Lehigh-Summit Mines. At this point a mass of anthracite forty feet thick, deducting three intercalated fire-clays and a fine thin vein of impure coal, is quarried in open day, a covering of forty feet of sandstone being entirely removed. In the south mine, where there is a sharp anticlinal fold in the coal, the Stigmaria-clay, four feet thick, was well seen, with nearly forty feet of coal above it and four below. In the Great mine Mr. Lyell observed the following section :—

Top, yellow quartzose grit.	
Coal, two or three inches of the uppermost part of the bed being in the state of dust, as if they had been crushed or rubbed by the yellow quartzose grit......	5 feet.
Blue fire-clay with Stigmariæ......................	15 inches.
Coal, including two or three seams of an impure slaty nature	25 feet.
Blue fire-clay with Stigmariæ......................	2 feet.
Coal, with an intervening layer of hard, bituminous slate	8 feet.

The anthracite, as in other parts of these coal-measures, often exhibits a texture exactly like that of charcoal; and frequently impressions of striated leaves, exactly resembling, as pointed out by Prof. Rogers, those of liliaceous plants, particularly the iris.

Mr. Lyell, accompanied by Prof. Rogers, afterwards examined the Room Run mines, on the Nesquahoning, where he saw a splendid exhibition of Stigmariæ in a bottom clay, one stem, about three inches in diameter, being no less than thirty-five feet in length. In the roof of slaty sandstone were impressions of Pecopteris, Glossopteris, and other ferns.

At Beaver Meadow, or the middle coal-field, a bed of anthracite is overlaid as well as underlaid by Stigmaria blue clay; the upper fire-clay, however, soon thins out, and is replaced by sandstone. No coal rested upon it, but Mr. Lyell observes that the carpeting of coal may not be always large enough to cover the flooring of fire-clay, or some change of circumstances or denudation may have interfered with the usual mode of deposition. Upon the whole, Mr. Lyell says, the accumulation of mud and Stigmariæ was, in Pennsylvania as in South Wales, the invariable forerunner of the circumstances attending the production of the coal-seams. The two extreme points at which he observed the Stigmaria-clay, Blossberg and Pottsville, are about 120 miles apart in a straight line, and the analogy of all the phænomena at those places, and still more on both sides of the Atlantic, is, he says, truly astonishing. In conclusion, Mr. Lyell states, that he had just received a letter from Mr. Logan, announcing the existence of the bottom clay, with Stigmariæ, in Nova Scotia; and that Mr. Logan had visited Mauch Chunk.

LONDON ELECTRICAL SOCIETY.

[Continued from p. 64.]

July 19*.—The Society assembled for the first time in its new apartments in Cavendish Square. The following papers were read:—

1. "On the Solution of Gold in Muriatic Acid by Voltaic Agency." By H. Prater, Esq., Memb.

2. "On the Action of Lightning Conductors." By Mr. Charles V. Walker, Hon. Sec.

Having introduced the subject by referring to his observations on the lightning-flash at Brixton Church (vide Phil. Mag. for July, p. 63.), the author states that a series of recent experiments have rather tended to confirm than change his opinion upon the phænomena termed often "lateral discharge;" and that his present object is to direct the attention of the Society to certain facts, which have not been so prominently regarded as their nature demands; and here especial allusion is made to the Leyden discharge. That this discharge is often employed in illustrating the action of lightning is manifest to all who have paid any attention to the matter, and that a large portion of the experiments, which have given rise to so much difference of opinion, are the effects of Leyden discharges, is likewise well known.

Mr. Walker commences by endeavouring to show the difference between such discharge and a flash of lightning: he states, that supposing a cloud to resemble one coating of a jar, the air to correspond with the glass, and the earth with the other coating, the discharge of that cloud is *directly* between the coatings, viz. through the insulator; and he then shows that a Leyden discharge only resembles this, when it is of force sufficient to perforate the glass. He explains that the regular discharge is operated upon by two forces acting counter to each other; the one *directly* between the two coatings in direction a, the other between the discharging balls in a direction b; and that the length of shock, or as it is termed striking distance, is the difference between these forces: when $a-b$ represent the resultant, the glass is perforated; when $b-a$ is the equivalent, the regular discharge occurs. That this explanation is not imaginary, is shown by comparing the striking distance of the Leyden discharge with that from the prime conductor. With the Polytechnic battery, containing 70 feet of coated glass, the distance is about one inch, while from the large conductor of the machine sparks upwards of two feet long will appear. He then calls into requisition the ocular illustration of difference; when one spark is direct and compact, the other is long and zigzag; and this leads him to point out the resemblance between lightning flashes and sparks from the conductor; not merely in their visible and accidental characters, but in their passing just as lightning does, *directly* from a charged body towards the earth in the direction of least resistance. Having shown

---

* The papers read before the Society in April, omitted to be noticed in our last, will be found in the Proceedings, Part V.

his reasons for excluding Leyden jars from this inquiry, and glanced at the importance of establishing such a position, he proceeds to throw sparks from the machine into wires arranged to represent lightning rods, and makes his observations upon the effects produced by these wires. Some of them pass perpendicularly between the conductor and the earth, others are led off horizontally: *all* give rises to the said "lateral spark." The next point was to show that these wires did resemble lightning-rods; and for this purpose an arrangement was made, as closely resembling nature as possible: a brass rod, terminating in a ball, was erected beneath a similar ball proceeding from the prime conductor of the machine, and sparks were passed between the two: beside the rod was held a smaller and shorter one, also terminating in a ball; the larger rod was screwed into a brass disc, the smaller rested on the floor; each was separately connected with a good discharging train. All things being in order, sparks were thrown from the prime conductor, and "lateral sparks" passed in abundance between the rods: and if this represented a lightning rod, it appeared lawful to infer that in every other arrangement when sparks were obtained, they proceeded from the wires being a representation of a lightning rod. Without entering into the various experiments, all tending to develope the same truth, we come to show the explanation this last affords of the action of an elevated rod between two metallic discs.

It is well known that such a rod will not give off sparks to vicinal bodies; but Mr. Walker is of opinion that this want of the lateral discharge is due to the fact that the vicinal body rests on the lower disc, and is thus a direct metallic connexion with the main rod; in proof of which he shows, that the sparks in his experiment just noticed, ceases the instant the end of the lower rod touches the disc; and thus too are confirmed the principles described in his former paper, by which the safety of lightning rods is ensured by establishing such contact.

3. "On a new form of Battery, particularly adapted to Blasting Rocks," &c. By Martyn Roberts, Esq., F.R.S. Ed., Memb.

This battery consists of alternate and parallel plates of iron and zinc, and is excited by sulphuric acid 1 +, water 30: the plates are supported in a frame, by which they can readily be immersed in the trough of liquid (which may be of wood luted with white lead), and be removed at the termination of the experiment. The peculiar features of this battery in contradistinction to others, are the modes of connecting the plates. If we consider the figures 1, 2, 3, 4, &c. to represent the zinc plates, and the letters *a, b, c, d,* &c. the iron, *a* and *b* must be first connected; then 1 and *c*, 2 and *d*, 3 and *e*, and so on, by which means both sides of each plate are brought into requisition, and no counter currents reduce the action. Mr. Roberts recommends a series of twenty for blasting, and says that they may be comprised within a space of eight inches.

4. Electro-Meteorological Register for June, by W. H. Weekes, Esq., Memb.

Aug. 16.—A Letter from Walter Hawkins, Esq., F.S.A., F.Z.S., Memb. Elect. Soc., was read, in which allusion was made to the recent serious accidents occasioned by lightning, and which suggested the propriety of the Society's taking the matter into consideration, and publishing some general directions as to the best methods of protecting churches and other elevated buildings.

A paper from a member, Mr. Mackrell, was then read, detailing the plan by which he had succeeded in obtaining ferric acid by electrolysis.

A paper by Henry Letheby, Esq., A.L.S., was read, detailing the particulars of the dissection of a *Gymnotus Electricus*, and containing reasons for believing that the electric energy originates in the brain and spinal cord. In reference to the anatomy of the fish, the author shows that the electrical organs are not super-additions of a peculiar structure, but are the result of an increased development of the aponeurotic termuscular septa, which become so arranged as to form long tubes, running diagonally from within outwards, so that the juxtaposition of these tubes produces laminæ which run longitudinal to the animal. The number of tubes in the entire organs is estimated at upwards of half a million. The organ is supplied largely by the spinal nerves; the peculiar nerve of Hunter, called by Mr. Letheby the posterior or dorsal branch of the fifth, is distributed entirely to the muscles. The author then alludes to the well-known researches of Williamson, Humboldt, Faraday, Walsh, Todd, Davy, Matteucci and others, which have proved the analogy between the effects produced by electrical fishes, and those developed by our artificial combinations. He then goes on to trace the connexion between these two divisions of the subject, and directs attention to two important facts:—1st, that the organs are made up of aponeurotic septa containing an albuminous gelatinous fluid; and 2ndly, that these are furnished with a supply of nerves far exceeding the wants of the parts for the purposes of life. Bearing in mind this latter fact, and then alluding to the voluntary nature of the shock, to its annihilation when the nerves are severed, to its increase when the nerves are irritated, he concludes that the electric force originates in the brain and spinal cord, and is concentrated or made tense in the organ itself. He then gives a series of deductions to show that electricity and vital energy are in a manner identical.

This paper was illustrated by an elaborate series of drawings, and also by anatomical preparations of the organ and the supplying nerves.

Mr. Weekes's Electro-Meteorological Report for July was then read, from which we gather that while the metropolis has been so seriously visited by lightning, the neighbourhood of Sandwich has been comparatively tranquil.

September 20. — The papers read this evening were, — 1st, "Additional Notes on the Production of *Acari*, &c. in close Atmospheres, incident to the operation of Voltaic Currents." By W. H. Weekes, Esq., M.E.S.

Mr. Weekes finds, from continued observation, that these insects, whatever be their origin, are multiplied by the ordinary means of generation: he has observed the devolopment and departure of successive families, and perceives that the defunct are devoured by their survivors. On the 20th of July, 1842, he terminated the experiment with the sulphate battery, and was so unfortunate as not to secure a single specimen of the insect. With respect to the spongy aggregations around the positive electrode, he has found they are not, as he anticipated, pure silicon, but apparently an inferior oxide of that element. He quotes Dr. Brown's opinion, that "it may throw light on the doubtful question of the atomic weight of silicon."

2nd. "Observations by W. Snow Harris, Esq., F.R.S., on a paper by Charles V. Walker, Esq., Hon. Sec. L.E.S., entitled 'On the Action of Lightning Conductors.'"

The author of this paper does not agree with Mr. Walker in fearing danger from the passage of a spark from the lightning-rod to a vicinal conducting body; and he thinks, contrary to Mr. Walker, that the discharge of a Leyden jar does resemble a flash of lightning. He says, that "the lightning-rod, so far from sending out sparks to neighbouring bodies, directs the passing charge from them altogether." He states, that "when a great variety of circuits are open to a passing discharge of electricity or lightning, the charge will be likely to divide on them all;" and that this is by no means a new fact: this he alludes to as the division of charge. He adds, that it will not go off to semi-insulated bodies; and this he appears to consider "lateral discharge." He then proceeds to analyse Mr. Walker's experiments, which had induced the latter gentleman to doubt the analogy between Leyden and lightning discharges, and allows the *distinction* between the two cases, but not the *difference.* He conceives that the difference in the direction of the discharge does not operate against its special character. With respect to the difference in the length of spark, he considers this as "altogether an affair of intensity, and of the form and disposition of the charged conductors;" and proceeds to show varied phænomena, in connection with varied form and arrangement. He does not place so much reliance as Mr. Walker upon experiments from the prime conductor, but allows certain general points in which it does resemble a charged cloud. He then examines the experiments which were made with the prime conductor of the Polytechnic Institution, and shows in what respects he is unwilling to receive them. He concludes with expressing a conviction that there is no danger of lightning leaving a conductor to enter vicinal bodies; and hence considers that Mr. Walker's suggestions relative to connecting these bodies with the main rod, are not needed.

Mr. Weekes's Electro-Meteorological Register for August was then laid before the Society.

CHEMICAL SOCIETY.

[Continued from vol. xx. p. 344.]

Dec. 21, 1841.—The following communications were read:—

"On the Agency of Caloric in permanently modifying the state of Aggregation of the Molecules of Bodies," by Warren De la Rue, Esq.

The subject of this short notice is the practical application of the action which takes place in masses, composed of palpable particles, when raised to a temperature insufficient even for their partial fusion.

In illustration of the particular action alluded to, may be quoted the following familiar facts:—Precipitated gold, when heated to a low red heat, contracts in volume, becomes more coherent and yellow in colour; clay contracts in volume when heated, and generally in proportion to the intensity of the heat; the carbonaceous deposit in the inside of gas retorts, by the continued action of heat, acquires sufficient hardness to scratch glass; ordinary coke and charcoal become harder the longer the action of heat is continued on them; these and many other analogous facts are examples of a new molecular arrangement being produced in various substances, by subjecting them to an increase of temperature, not however sufficient for their fusion.

To cause the foregoing changes a red heat is employed; we shall however presently see that a temperature but little above that of boiling water is quite sufficient to materially alter the cohesion of some substances.

It may be as well here to premise, that the particles should be brought as closely as possible together; to effect this, if the substance be in powder, it must be made into a paste with water to displace the air, and the paste so prepared submitted to a pressure of four tons or upwards on the square inch; air being so exceedingly compressible it cannot be got rid of without the use of some liquid. The manner of pressing need not here be entered on, the operation being purely mechanical.

White lead precipitated by carbonic acid gas from a hot solution of the sub-nitrate always falls as an exceedingly light deposit; if it be pressed as before described, and the pressed cake dried at the ordinary temperature of the atmosphere, it coheres but imperfectly, but on being subjected to a heat of between 200° and 300° Fahrenheit, it becomes exceedingly hard and compact; and if the cake be ground up with water and redried, it will be found far more dense and opake than the original precipitate, showing the change to be permanent.

The following fact was communicated to me by Messrs. Nasmyth and Co. of Patricroft:—Common chalk cannot readily be sawn into thin slips, as it crumbles under the operation; if however it be baked at the temperature before named it becomes far more tenacious, and may be then cut into any form we choose, still being sufficiently soft for drawing or writing, to which purposes it is far more applicable than before baking.

Almost all precipitates dry much more crisp at high than at low temperatures, the agency of heat facilitating the attraction of such particles as may happen to be in contact.

In conclusion, I may remark that it appears by no means improbable that the long-continued action of temperatures, but slightly elevated above the ordinary temperature of the atmosphere, may have been, and still may be, the cause of the formation of hard rocks from materials originally but slightly coherent.

"Notice of the Decomposition of Oxalic Methylic Æther (Oxalate of Oxide of Methyl) by Alcohol," by Henry Croft, Esq.

While in Berlin I was led to examine the action of potassa on oxalate of methyl, by a statement of Weidmann and Schweitzer in their first treatise on Wood-spirit; namely, that the compounds of the oxide of methyl with acids are decomposed by alkalies, not into their constituent acid and wood-spirit, as Dumas and Peligot have stated, but into the acid and a peculiar oil which they called *methol.* From this Löwig drew some conclusions unfavourable to the accuracy of Dumas and Peligot's research. This statement of Weidmann and Schweitzer I found to be incorrect, as they themselves also allowed in their second paper. Oxalate of methyl is best prepared by distilling a mixture of 1 part wood-spirit, 1 part anhydrous oxalic acid ($HO + C_2O_3$), and from $\frac{1}{4}$th to $\frac{1}{8}$th of sulphuric acid. The first portion which passes over may be returned, and afterwards another part of wood-spirit added, or even two. The æther obtained must not be allowed to stand in solution for any length of time, for it easily decomposes. The above proportions I have found to be the best; the method with oxalic acid alone is troublesome, on account of the great volatility of wood-spirit, and the length of time required for forming any considerable quantity of the æther. If, on the other hand, so much as an equal weight of sulphuric acid is taken, the mixture becomes brown or black, and towards the end of the operation sulphurous acid, methol, and other products are formed. By passing hydrochloric acid gas into a solution of oxalic acid in wood-spirit no æther could be obtained; it is possible, however, that the result of further experiments may be more favourable, only one experiment being made, owing to the very small quantity of wood-spirit in my possession.

It is well known that Mitscherlich formed the oxalovinate of potassa by adding to an alcoholic solution of oxalic æther just so much of an alcoholic solution of potassa as was sufficient to saturate half the oxalic acid contained in the æther. As no acid oxalate of methyl is known, I therefore attempted to form it in the same manner, but owing to the excessively small quantity of spirit which I possessed, and which is not to be obtained in northern Germany, I was obliged to dissolve both the oxalic methylic æther and the potassa in alcohol, it appearing very unlikely that the alcohol could have any disturbing influence, as it is only the æther which ought to be decomposed. On adding the solution of potassa until the mixture became slightly alkaline, a white salt in pearly scales was obtained; this was washed with alcohol and dried. The filtered solution gave more of it on evaporation.

In analysing this substance it was useless to attempt to determine the carbon and hydrogen, owing to the admitted insecureness of the analyses of potash salts, and I had not enough material to prepare either the lead or baryta salt. The oxalic acid and the potassa were therefore alone determined: it contained,—1st, 30·81, and 2nd, 30·76 per cent. of potassa, and 46·58 of oxalic acid. This agrees very well with the formula for oxalomethylate of potassa, plus one atom of water; but no water could be driven out by a heat of 150° C., and I at length found that the salt was only oxalovinate of potash, with the composition of which the analyses agree very well:—

		1.	2.
Oxalic acid . . .	46·12	46·58	
Potassa	30·04	30·76	30·81.

The salts agreed, moreover, completely in their properties. On repeating the experiment with wood-spirit instead of alcohol I did not obtain an insoluble salt, but on evaporation one which is probably the true oxalomethylate of potash, and which I am now about examining.

Such a decomposition as the above is, I believe, of very rare occurrence; I am not aware of any other instance of it being known, although the possibility of some such kind of decomposition has not escaped the acuteness of Berzelius. (*Lehrbuch,* viii. 703.) We may perhaps suppose that oxalomethylate of potash is first formed, but that the attraction of oxalic acid for æther, and of oxalic æther for oxalate of potash is so strong as to cause the decomposition of hydrate of æther into its elements, when the alcoholic æther will combine with the oxalic acid, and the oxide of methyl, whose place it takes, combines with water to form wood-spirit. That some kind of what is called predisposing affinity is here in play, is evident from the fact that oxalate of methyl may be boiled with alcohol for hours without any such change taking place.

It may be stated, in conclusion, that the process last described is a very good and œconomical method of obtaining the oxalovinate of potassa in a very beautiful form.

"On the Radical of the Cacodyl Series of Compounds," by Professor Bunsen of Marburg. (In Phil. Mag. S. 3. vol. xx. p. 382.)

Jan. 4, 1842.—The following communications were read:—

"On some of the Substances contained in the lichens employed for the preparation of Archil and Cudbear," by Edward Schunck, Esq. (This paper will be found in Phil. Mag. S. 3. vol. xx. p. 495.)

"On a re-arrangement of the Molecules of a Body after solidification," by Robert Warington, Esq. (Inserted in Phil. Mag. S. 3. vol. xx. p. 537.)

Jan. 18.—Colonel Yorke exhibited a specimen of a silver ore from Mexico, containing bromide of silver, from his collection, in confirmation of the late discovery, by M. Berthier, of the existence of bromine in silver ores.

The following communications were read:—

"On the Conversion of Benzoic Acid into Hippuric Acid, in the Animal Economy," by Mr. Alfred Baring Garrod, of University College. (In Phil. Mag. S. 3. vol. xx. p. 501.)

'On the Constitution of the Sulphates, as illustrated by late Thermometrical Researches," by Thomas Graham, Esq., F.R.S. (In Phil. Mag. S. 3. vol. xx. p. 539.)

February 1.—The following communication was read:—

"On the Change of Colour in the Biniodide of Mercury," by

Robert Warington, Esq., Sec. Chem. Soc. This paper will be found at p. 192 of the present volume.

February 15.—The following communications were read:—

"On a new Oxalate of Chromium and Potash," by Henry Croft, Esq. For this paper also see pres. vol. p. 197.

"Some Observations on Brewing," by Septimus Piesse, Esq.

The author's attention was directed to the subject by the following inquiry:—"Is it possible to obtain a greater quantity of *extract* from malt by any other process than that usually followed? Is any thing left in the grains which ought to be in the wort?"

Now from an examination of several samples of the malt taken when supposed to be completely exhausted, and from the circumstance of the grains affording such a large quantity of nourishment to cattle, I was led to suspect that it was possible to increase the weight of extract; in fact, the grains were found to contain a notable quantity of starch.

The non-conversion of this starch into sugar does not depend, in the cases I have witnessed, upon the use of improper temperatures, but arises from a deficiency of *diastase* (the principle which effects the change of starch into sugar). In the ordinary process of brewing, a certain quantity of water and malt are mixed together of a proper temperature. After standing for a time, this water, or as it is then termed, wort, is drained from the malt, and a second portion of water is run on to form the second wort. There can be no doubt but the principal portion of the starch is converted during the first mashing, but it never is all. Now it must be remembered that as diastase is soluble, it is taken up by the first wort, and when that is run off, the diastase passes away also. The improvement consists simply in adding diastase to the second wort, to convert the remaining starch into sugar. This is done by the addition of a portion of malt (which contains diastase) previous to mashing a second time. In a brewing of 30 quarters, I should take 29 quarters for the first mash, and add the remaining quarter to the second. There is such an increase as to warrant me in advising its adoption by all brewers and distillers.

Another improvement in brewing is recommended by the author, to prevent the absorption of oxygen by the wort, and thus in a great measure prevent acidity.

The wort, as it flows from the tun, passes into the underback, according to the usual practice, where it is exposed to the air; and that for some time, because the wort must run slowly in order to come bright. The improvement consists in having a float in the back, that is, a surface of wood the size of the bottom of the back, upon which it rests when empty. As the wort runs into the back the float rises with it, and falls again when it is pumped up to the copper, thus effectually keeping it out of the contact of air previous to boiling, when the danger ceases. When this precaution has not been taken, I have invariably found the wort to indicate more or less acid, which may be looked upon as likely to lead to sour beer.

March 1.—The following communications were read:—

"On the Preparation of Cyanide of Potassium, and its applications," by Professor Liebig of Giessen. (Inserted in vol. xx. p. 265.)

"On the Specific Heat and Conducting Power of Building Materials," by John Hutchinson, Esq.

The following is the substance of Mr. Hutchinson's paper :—The author, after mentioning the state of our knowledge respecting the conducting powers for heat of different substances, proceeds to point out an important source of error in all such investigations hitherto made arising from the neglect of correction for differences of specific heat among the bodies examined; the effects observed being evidently *mixed effects*, arising from both causes. This being the case, before any correct investigation of the relative conducting powers of building materials referred to could be advantageously undertaken, it became indispensable to acquire a previous knowledge of their relative capacities for heat, in order that correction for differences of this kind might be made. This inquiry, therefore, naturally preceding that of the proper subject of the paper, first attracted the author's attention.

The building materials selected for experiment were the following: —Oak, beech and fir-woods; common, facing and fire-brick; Asphalte composition, hair and lime mortar, lath and plaster, Roman cement, plaster and sand, plaster of Paris, Keene's cement; slate, Yorkshire flag-stone, Lunelle marble, Napoleon marble, Portland and Bath-stone; and lastly, three specimens of the stones now used in building the Houses of Parliament.

The plan of experimenting chosen was that known as the "method of mixture," this appearing by all evidence on the subject to be the most unobjectionable. The process followed differed but little from that described by Regnault in his recent researches. A suitable quantity of material in fragments being accurately weighed out and placed in a little wire basket with the bulb of a delicate thermometer in the midst, the whole was exposed in an inclosure heated by steam until the thermometer ceased to rise, when the basket was withdrawn and plunged with suitable precautions into a vessel of water at a temperature a little below that of the atmosphere. After the lapse of a very short interval the temperature of the water was carefully observed, and its rise gave the means of calculating the specific heat of the substance.

The author remarks on the necessity of equalizing as much as possible the times of heating of the different substances, having observed a great difference in the results given by the same body when slowly and when quickly raised to the high temperature required for the experiment, and attributes this difference to an alteration in the state of the currents or waves of heat travelling inwards towards the centre of the solid.

A number of minute precautions, indispensable to a correct result, were also pointed out and exemplified. The results of the investigation were given in a tabular form, and the principle of the calculation described.

With the knowledge thus obtained the author proceeded with his inquiries respecting the conducting powers of the substances under examination.

The plan usually adopted in this kind of research, namely, observing by the aid of thermometers the time occupied by the passage of a certain amount of heat lengthways through the substance of a prism, one end of which was exposed to a high and constant temperature, having failed on trial with these bodies, in consequence of their feeble conducting powers, the following method was had recourse to with perfect success:—The various substances examined were cut with the greatest care into cubes of 2·8 inches in the side, and a hole drilled in the centre of one of the faces half way through, large enough to receive the bulb of an exceedingly sensitive thermometer, together with a little mercury to improve the contact with the substance of the cube. The temperature of the mass being exactly observed, it was next plunged, all but its upper surface, into a large bath of mercury heated by steam, whose temperature remained constant at 211°, and the time of rise of the thermometer for every successive 10° accurately noted until the maximum was reached, thus affording a comparison of the relative conducting powers, or perhaps more properly, resistance to the passage of heat towards the centre of the mass.

In the course of these experiments a very extraordinary circumstance was observed: although the greatest care was taken to equalize the temperature of the cubes by suffering them to remain at least twenty-four hours before experimenting in an uniform temperature, yet they never exactly acquired that of the room, or even agreed among themselves in this respect; an observation which led the author to the suspicion that the generally received doctrine of an equal distribution of sensible heat among bodies in contact and not influenced by external sources of disturbance, might not prove strictly true, but that, on the contrary, each of a number of different substances, exposed under similar circumstances to the influence of a medium of uniform temperature, acquires a *proper temperature of its own*. The same thing was observed with higher degrees of heat; a mass of slate, for example, plunged beneath the surface of uniformly heated mercury and maintained there long after the thermometer in the slate had reached its maximum, always exhibited a temperature decidedly below that of the surrounding metal*.

A third series of experiments were made with a view of ascertaining the relative rates of cooling in air of the various materials examined, from a higher temperature to that of the atmosphere. The arrangement consisted of the cubes before described, covered externally with thin paper for the sake of uniformity of surface, the same delicate thermometer being inserted in the hole in the centre, together with a little mercury for the sake of contact. The cubes were each in turn heated in the steam-chest used for the specific heat experiments, until the included thermometer rose to 200°; they were then removed, suspended in the air, and the time of fall of temperature for every 10 degrees carefully noted.

[* On this subject a paper by Mr. Parnell was subsequently read, an abstract of which will appear in a future Number.—Edit.]

The precautions required to be taken to avoid errors of different kinds were fully described, and drawings of the apparatus used exhibited, together with a most elaborate and complete set of tables embodying the whole of the results.

LIV. *Intelligence and Miscellaneous Articles.*

BICHLORIDE OF HYDROGEN.

THIS compound, which contains one proportion of chlorine more than exists in hydrochloric acid, may be obtained, according to M. Millon, by slowly and gradually projecting binoxide of lead into concentrated hydrochloric acid, surrounded by a cooling mixture of ice and salt. In the reaction which occurs under these circumstances, the liquor produced assumes a deep yellow colour, without any sensible disengagement of chlorine, and an abundance of protochloride [of lead] is formed.

The bichloride of hydrogen, which gives the liquor its colour and properties, has not yet been separated from the medium in which it is dissolved. This compound possesses but little stability, for, at common temperatures, it continues to evolve chlorine during several days. Mercury decomposes it by absorbing part of the chlorine, and thus causing the reproduction of hydrochloric acid. Its composition would appear to be 1 equivalent of hydrogen + 2 equivalents of chlorine $= \mathrm{H\,Ch^2}$.

This bichloride would be formed by the reaction of 3 equivalents of hydrochloric acid, or 1 equivalent of binoxide of lead, as shown by the annexed equation:

$$3\,\mathrm{H\,Ch}, + \mathrm{Pb\,O^2} = \mathrm{Pb\,Ch} + 2\,\mathrm{H\,O}, + \mathrm{H\,Ch^2}.$$

Journal de Chim. Médicale, Juillet 1842.

ON THE ACTION OF CHLORIDES UPON PROTOCHLORIDE OF MERCURY. BY M. MIALHE.

M. Mialhe remarks that Capelle, in 1763, first observed the danger arising from a mixture of calomel and sal-ammoniac; Proust afterwards proved the conversion of calomel into corrosive sublimate by the action of the alkaline chlorides. After mentioning other authors, M. Mialhe refers to a note of his own contained in the *Journal de Pharmacie* for February 1840, in which he details experiments proving,—1st, that calomel acted upon by the alkaline chlorides always yields more or less corrosive sublimate; 2ndly, that it is to this partial conversion calomel owes its medicinal powers; and he afterwards mentions different authors who have confirmed his opinions.

M. Mialhe then relates various experiments which he has since performed to determine the proportion of corrosive sublimate resulting, under certain conditions, from this action.

Experiment I.—1000* parts of distilled water, 60 of common salt, 60 of sal-ammoniac, and 60 of calomel (*à la vapeur*) which had been

---

* We have reduced the French weights of the original to parts.

perfectly washed, were mixed and allowed to react for twenty-four hours, the temperature varying from 68° to 77° Fahr.; there was produced 0·6 of a part of corrosive sublimate.

Similar experiments were made with calomel prepared by precipitation, with precisely similar results.

Experiment II.—1000 parts of the assay liquor* had 60 parts of calomel (*à la vapeur*) digested in it for 24 hours, at a temperature varying from 104° to 128° Fahr.; 1·5 part of corrosive sublimate was produced.

The preceding experiments repeated with precipitated calomel (*précipité blanc*), yielded a mean of 1·7 part of corrosive sublimate. This chemical result confirms the opinion of therapeutists, who have always considered the calomel obtained by precipitation as sensibly more active than that prepared in the dry way.

The following question was then examined: Is the quantity of sublimate produced proportional to that of the calomel employed, or to that of the alkaline chloride?

1*st Experiment.*—Assay liquor 1000 parts, calomel 10 parts; after reacting for twenty-four hours, between 104° and 112° Fahr., 1·4 part of sublimate was produced.

2*nd Experiment.*—Assay liquor 1000 parts, calomel 20 parts; sublimate produced 1·5 part.

3*rd Experiment.*—Assay liquor 1000 parts, calomel 40 parts; sublimate formed 1·5 part.

4*th Experiment.*—Assay liquor 1000 parts, calomel 60 parts, sublimate produced 1·5 part.

The preceding experiments repeated with calomel obtained by precipitation, gave

1*st Experiment.*—Assay liquor 1000 parts, calomel 10 parts; sublimate produced 1·4 part.

2*nd Experiment.*—Assay liquor 1000 parts, calomel 20 parts; sublimate produced 1·4 part.

3*rd Experiment.*—Assay liquor 1000 parts, calomel 40 parts; sublimate produced 1·5 part.

4*th Experiment.*—Assay liquor 1000 parts, calomel 60 parts; sublimate produced 1·7 part.

All these experiments show that the quantity of sublimate produced is not at all proportional to that of the calomel employed.

The experiments about to be related, prove beyond all doubt, that the quantity of sublimate formed is always in proportion to that of the alkaline chloride.

1*st Experiment.*—After twenty-four hours' contact, between the temperature of 104° and 122° Fahr., distilled water 1000 parts, calomel 20 parts, common salt and sal-ammoniac each 60 parts; sublimate produced 1·6 part.

2*nd Experiment.*—Calomel 240 parts, common salt and sal-ammoniac each 10 parts; sublimate produced 0·5 part.

---

* To avoid repetition, the author calls the solution of alkaline, just described, the *assay liquor*.

These two experiments, repeated with precipitated calomel, gave the following results:—

1st Experiment.—Sublimate 1·8 part.

2nd Experiment.—Sublimate 0·6 part.

Experiments were then made to decide the question, whether the degree of dilution of the alkaline chlorides put into contact with the calomel, had any remarkable influence on the quantity of sublimate produced: this was found to be the case, as indeed theory would indicate.

It was also proved, by direct experiment, that the presence of neutral organic bodies does not hinder the conversion of calomel into sublimate; on the contrary, dextrine favours the change; sugar and albumen probably do not modify it; and lastly, lard and gum-arabic very evidently retard it.

M. Mialhe remarks, that in his first experiments on the conversion of calomel into sublimate, he supposed it to take place by the conversion of 1 equivalent of calomel into 1 equivalent of mercury and 1 of sublimate; he now finds that the presence or absence of atmospheric air modifies the results.

1st Experiment.—Without the presence of air. Water 2000 parts, common salt and sal-ammoniac of each 120 parts, precipitated calomel 60 parts, digested for twenty-four hours in a stopped bottle; sublimate produced 0·3 part.

2nd Experiment.—The same substances allowed to react with the presence of the air, yield 1·1 part of sublimate.

It appears, then, that calomel and the alkaline chlorides, when air is present, produce three times as much sublimate as when they react without it. The explanation of this appears to be, the fact, as stated by M. Guibourt, that calomel absorbs a certain quantity of oxygen at common temperatures; at a higher temperature the absorption is greater; and in the case now mentioned, the absorption is accelerated by the presence of the alkaline chlorides. It is not therefore surprising that the proportion of sublimate should be greater when the air is present, since for every equivalent of oxygen absorbed, an equivalent of sublimate is produced; and moreover each equivalent of binoxide of mercury formed, gives by double decomposition with the alkaline chloride, 1 equivalent of sublimate and 1 of alkaline oxide.

To check these researches, the following experiments were made:

1st Experiment.—Water 1000 parts, calomel and hydrochloric acid each 60 parts, digested twenty-four hours at temperatures between 104° and 122° Fahr.; sublimate produced, without the contact of air, 0·4 part.

2nd Experiment.—The same substances, reacting with air present, gave 1·4 part of sublimate.

It may be concluded from the foregoing, that about two-thirds of the sublimate produced are formed by the influence of oxygen, and that one-third only is derived from the mere and simple conversion of calomel into metallic mercury and calomel.

M. Mialhe finds also, that calomel may partly be converted into sublimate, &c. by the influence of boiling distilled water deprived of air. 1000 parts of boiling distilled water and 60 parts of calomel were kept at 212° for an hour; after cooling, the water was found to contain 0·1 part of sublimate.

This experiment repeated with precipitated calomel gave 0·1 part of sublimate.

When calomel then is boiled in distilled water, sublimate is unquestionably produced without the contact of air, but the quantity produced is infinitely smaller than when oxygen is present; but in this case it is oxichloride of mercury which is formed, and not mere bichloride of mercury.—*Annales de Chimie et de Physique*, Juin 1842.

ON CINCHOVATINA—A NEW VEGETABLE ALKALI.

M. Manzini obtained this alkali from the *Cinchona ovata*, which has always been admitted not to possess any febrifuge power. The process employed in preparing this substance was exactly similar to that used for obtaining quina.

Its properties are, it crystallizes in prisms, which are longer than those of cinchonia; they are white, inodorous, bitter, but this is long in being developed, on account of the slight solubility of this substance. Alcohol dissolves it very well, especially when hot, but æther is not so perfect a solvent, and in water it is almost insoluble. Dilute acids dissolve it readily and form salts, which usually crystallize readily, are very soluble even in weak alcohol, but more so when hot than cold; these salts are decomposed by the alkalies and their carbonates, which precipitate cinchovatina; they are also decomposed by tannin, iodide of potassium, bichloride of mercury, chloride of platina, chloride of gold and other metallic chlorides. Ammonia also precipitates the salts of cinchovatina, setting the base at liberty, but only a part of it is separated in an insoluble state, especially if the excess of ammonia is considerable; a portion of the base remains dissolved by the ammonia, and is deposited in slender crystals by the evaporation of the alkali; even that portion of the cinchovatina which is precipitated, and which is perfectly amorphous, eventually becomes a crystalline mass of a splendid pearly whiteness; it requires some days for the production of this change. The alcoholic solution of cinchovatina is very bitter; it restores the blue colour of reddened litmus, and renders syrup of violets green. When subjected to a heat gradually increasing to 366° Fahr., cinchovatina suffers no loss of weight, nor any change of appearance; when heated in a tube to 370° Fahr. it melts into a brownish liquid without volatilizing; on cooling it solidifies into a mass of a resinous appearance, of the colour of colophony, with its surface covered with cracks; in this state its weight is the same as before fusion, and if it be melted again, its fusing point is found not to have changed. Cinchovatina, therefore, cannot be ranged with those bodies, which, as observed by Wöhler in his memoir on lithofellic acid, possess the remarkable property, of having two different fusing points, according as they are amorphous or crystallized.

Cinchovatina which has been fused and cooled is as soluble as before in boiling alcohol, and is deposited in crystals on cooling. At about 374° Fahr. it decomposes, and then yields extremely fetid empyreumatic products, and leaves a very bulky charcoal. These experiments show that crystallized cinchovatina is perfectly anhydrous.

By analysis it yielded very nearly,

			(Foreign equivalents.)
Carbon	69·80	or	C^{46} = 3450·00
Hydrogen	6·83	..	H^{54} = 337·50
Oxygen............	16.21	..	O^{8} = 800·00
Azote	7·16	..	Az^{4} = 353·08
	100·	Equivalent	= 3941·50

Journ. de Pharm. et de Chim., Août 1842.

PREPARATION OF PURE POTASH AND SODA.

M. Schubert observes that the mode of preparing caustic barytes from sulphuret of barium, by means of oxide of copper, admits of its being used for the ready obtaining of potash and soda chemically pure. Crystals of neutral sulphate of potash or sulphate of soda are to be dissolved in a concentrated solution of caustic barytes, until chloride of barium shows an excess of these salts in a small quantity of the filtered liquor, then barytes water and a solution of the sulphates are to be alternately used till neither produces any precipitation, and proving that there is neither barytes nor sulphate in excess. It is, however, better to have an excess of barytes than of sulphuric acid, because the former precipitates during evaporation in the state of carbonate; but then the evaporated alkali must be redissolved, filtered, and again evaporated, and these operations necessarily introduce a considerable quantity of carbonic acid into the product.—*Journal de Pharm. et de Chimie*, Août 1842.

DETECTION OF IODINE IN BROMIDES.

The presence of the alkaline iodides in the bromides which are prepared with the bromine obtained from the mother waters of soda, is less rare than is supposed. This fact depends, as chemists well know, on the difficulty found in separating from bromine, which is liquid at common temperatures, the small proportions of iodine, which exist in it in the state of bromide. Various specimens of the bromide of potassium of commerce, which have been offered to M. Lassaigne, constantly contained a very small quantity of iodide, and it is by the very sensible reaction of starch that he has been able to detect it.

On adding to the solution of bromide of potassium to be examined, a few drops of a weak solution of chlorine, the liquid soon becomes of a yellow colour; if there then be immersed in it white paper starched, or covered with a mixture of starch and water, and afterwards dried, it becomes of a violet or of a light indigo blue colour. This colour depends on the iodine set free by the first portions of chlorine added to the impure bromide.

When sufficient chlorine has been added to decompose the whole of the alkaline bromide, the paper immersed is not immediately coloured, for then the iodine exists in the liquor in the state of bromide, and no longer acts upon the starch; but this remarkable circumstance occurs, that the paper being withdrawn from the liquor and exposed to the air, the moistened part assumes a reddish tint in about two minutes, then becomes violet, and afterwards blue; the same reaction occurs, but in a longer time, when the starched paper is left to macerate in the liquor.

This effect, unquestionably owing to the decomposition of the bromide of iodine by the organic matter of the paper, or perhaps even of the starch itself, admits of detecting minute quantities of iodine in the alkaline bromides.—*Journal de Chim. Médicale*, Septembre 1842.

PREPARATION OF FERROCYANIC ACID. BY M. POSSELT.

This acid, now sometimes also termed hydroferrocyanic acid, was discovered by Porret, and called by him ferrochyazic acid. According to M.Posselt the following is an improved process for obtaining it:—agitate with æther a concentrated aqueous solution of ferrocyanic acid as obtained by the decomposition of ferrocyanide of lead by means of sulphuric or hydrosulphuric acid, the acid separates immediately and may be obtained by filtration; this remarkable separation of the acid from the water which holds it in solution, requires but little æther. If the solution is moderately concentrated, the whole forms a thick mass by agitation, and after some time the ferrocyanic acid suspended in the æther, separates from the water saturated with æther, and swims on the surface. The water is to be removed by a pipette; the thick mass is to be put on a filter and washed repeatedly with a mixture of alcohol and æther, containing a considerable portion of the latter; it is then to be passed between folds of absorbent paper to remove the moisture, and afterwards to be perfectly dried over sulphuric acid in the air-pump.

In order to avoid preparing ferrocyanide of lead and the aqueous solution of ferrocyanic acid, a concentrated solution of ferrocyanide of potassium may be prepared in boiled water, and it is to be allowed to cool, entirely excluded from the air; it is then to be mixed with an excess of hydrochloric acid, also deprived of air, and this mixture is to be shaken with æther in the manner described. The acid separates in the same manner, and is to be dissolved in alcohol, to which a little sulphuric acid is to be added to combine with the potash which it may still contain; the liquor is to be filtered if it is not clear, and this alcoholic solution is to be agitated with æther; this again separates the acid, which is to be dried as before described.

This substance possesses all the properties of an acid, and presents a complete analogy with other hydracids. It has a very sour taste, an acid reaction, decomposing the carbonates with effervescence; it also decomposes with the greatest facility the acetates, tartrates and even the oxalates. It does not when cold dissolve binoxide of mercury; but if it be heated the acid is decomposed into hydrocyanic acid, which forms a cyanide with the mercury of the binoxide, and into cyanide of iron, which is additionally oxidized at the expense of a part of the binoxide of mercury, and metallic mercury separates.

The ferrocyanic acid prepared by the process described is in the state of a white powder, frequently with a slight blue or yellow tint. When it is perfectly dry it may be long exposed to the air without alteration, when moist the decomposition takes place more rapidly; the acid becomes gradually blue, and is slowly and totally converted into Prussian blue.

It may be long exposed in a covered platina crucible to a temperature of 212°, and excluded from the air, without losing weight or suffering any sensible change; eventually, however, it is decomposed under these circumstances.

When it is more strongly heated, hydrocyanic acid is disengaged and cyanide of iron remains, which is oxidized. If it be heated in a current of carbonic acid gas, and the temperature be not raised above 212°, hydrocyanic acid is evolved and white cyanide of iron is left, and this decomposes also at a temperature somewhat above 212°. It is, as is well known, very soluble in water, and the solution submitted to ebullition in contact with air becomes blue; but without the presence of air it deposits, on the contrary, white cyanide of iron.

Ferrocyanic acid is even more soluble in alcohol than in water. It forms a syrupy, transparent solution, which decomposes either by long exposure to the air or ebullition. This solution under the air-pump, yields mammillated hard crystals of a yellow colour.

The acid obtained as described is anhydrous, not losing, as already mentioned, any weight at a temperature of 212°. Two analyses gave the following results as the composition of this acid:—

	I.	II.	Calculated.
Cyanogen	72·71	73·33	73·09
Hydrogen	1·99	2·27	1·84
Iron............	25·22	25·08	25·06
	99·92	100·68	99·99

Journ. de Pharm. et de Chimie, Août 1842.

PREPARATION OF FERRIDCYANIDE OF POTASSIUM.

M. Posselt remarks that it is well known with what facility an excess of chlorine, when passed through a solution of ferrocyanide of potassium, decomposes the ferridcyanide of potassium as it is formed, and the difficulty which exists in completely separating the green substance which is then produced, because it readily passes through the filter. It is only by repeated crystallizations that the crystals are completely freed from it, and these operations are always attended with loss.

The following process is stated by M. Posselt to give pure and very fine crystals at once:—Pass chlorine gas through a very dilute solution of ferrocyanide of potassium, and evaporate it when the oxidation is complete, and add to the boiling liquor, when it is near its crystallizing point, a few drops of solution of potash; the green substance is then decomposed, and flocks of peroxide of iron separate. It is very easy to observe the moment at which the object is attained, and care must be taken not to add too much potash, because an excess of it would convert the ferridcyanide of potassium into ferrocyanide. The solution is to be filtered hot to separate the peroxide of iron; it possesses a deep purplish red colour, is to be cooled very slowly, and then fine crystals of the salt are obtained. —*Ibid.*

OBITUARY.

We record with much regret the decease of our highly distinguished correspondent Mr. Ivory, Fellow of the Royal Society, and Member of the Institute of France, who died at Hampstead on the 21st of September, aged 77:—Also the death of our much respected and venerable friend Mr. Peter Ewart (an occasional contributor to our Journal), occasioned by an accident in the proving of a chain cable, to which he was attending in the discharge of his duties at Woolwich.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1842.

Chiswick.—August 1. Overcast: very fine. 2. Sultry. 3. Sultry: distant thunder. 4. Sultry: high temperature maintained day and night. 5. Cloudy and fine. 6. Cloudy: rain. 7—9. Clear, hot and dry. 10. Sultry: excessively hot and dry: heavy thunder-storm at night, with rain in torrents. 11. Cloudy: clear and fine. 12. Clear and fine throughout. 13. Overcast: clear and fine. 14. Sultry. 15. Cloudless and hot. 16. Hot and dry. 17. Dry easterly haze: very hot. 18. Excessively hot and sultry: lightning in the evening. 19, 20. Cloudy: fine. 21. Very fine. 22. Hot and dry, with easterly wind: lightning. 23. Cloudless, hot and dry. 24. Hot and dry: lightning, distant thunder, with wind and rain at night. 25. Overcast: heavy thunder-showers in the evening. 26. Hazy: sultry. 27. Cloudy and fine. 28. Rain: cloudy and fine. 29. Heavy thunder-showers early A.M.: violent thunder-storm commenced four P.M., with very heavy rain: clear at night. 30. Hazy. 31. Clear and fine.—Mean temperature of the month 4° above the average.

Boston.—Aug. 1—3. Cloudy. 4. Fine. 5. Cloudy. 6. Rain. 7—9. Fine. 10. Fine: rain, with thunder and lightning P.M.: thermometer 85° three o'clock. 11. Fine. 12. Cloudy. 13. Cloudy: thermometer 79° two o'clock P.M. 14. Cloudy: thermometer 80° two o'clock P.M. 15. Fine: thermometer 80° eleven o'clock A.M. 16. Foggy. 17. Cloudy. 18. Fine: thermometer 83° two o'clock P.M. 19. Cloudy. 20. Fine. 21, 22. Cloudy. 23. Fine: thermometer 82° two o'clock P.M. 24. Cloudy: rain with thunder and lightning at night 25—28. Cloudy. 29. Cloudy: rain A.M. 30. Fine: rain P.M. 31. Fine.

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

Applegarth Manse, Dumfries-shire.—Aug. 1—3. Very fine. 4. Showers. 5. Showery. 6. Fine. 7. Slight showers. 8. Rain P.M. 9. Showers. 10. Heavy rain and thunder. 11. Fair and bracing. 12. Cloudy and drizzly. 13. Fair and fine. 14—16. Very fine. 17, 18. Very fine: very hot. 19. Showers. 20. Heavy showers. 21, 22. Fair and bracing. 23. Fine: one shower: thunder. 24. Wet A.M.: cleared up. 25—27. Fair and fine. 28. Fair and fine, but hazy. 29—31. Slight showers.

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

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
1842. Aug.		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mn.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	30·340	30·277	30·218	29·77	30·15	30·14	30·23	30·10	58·0	67·0	55·3	72	43	61	71½	50	62½	56	n.	ne.	calm	sw.	w.	......	......	......	......	......	53
2.	30·230	30·183	29·979	29·68	30·02	29·90	29·93	29·92	60·0	69·0	55·0	79	55	61	70	50	65	58	sse.	e.	calm	ssw.	s.	·186	......	......	......	......	54
3.	30·004	29·950	29·889	29·46	29·85	29·90	30·01	29·92	61·2	71·5	58·5	83	52	62	72	54	58	55	n.	ne.	calm	ssw.	calm	·027	......	......	......	......	62
4.	29·916	29·902	29·828	29·26	29·68	29·68	29·40	29·54	70·0	78·5	61·0	86	62	68	64	58	57	54	n.	w.	calm	sw.	s.	......	......	......	......	·40	60
5.	29·958	29·930	29·904	29·25	29·60	29·65	29·35	29·38	69·4	81·5	64·5	79	59	67	65	50½	57	55	s.	sw.	w.	se.	w.	......	......	......	......	·16	63
● 6.	29·968	29·937	29·844	29·28	29·75	29·76	29·66	29·72	68·2	75·5	63·2	72	53	61	65	48	57½	52	s.	w.	calm	w.	s.	......	·15	......	......	·04	64
7.	30·010	29·963	29·859	29·38	29·70	29·72	29·64	29·61	65·0	70·3	60·0	78	48	64·5	63½	52	59½	54	se var.	sw.	calm	w.	se.	·172	......	·19	0·10	......	56
8.	30·110	30·029	30·025	29·41	29·80	29·75	29·85	29·76	68·0	74·5	58·0	82	51	64	66	53	57	55	s.	s.	calm	sw.	sw.	......	......	......	......	......	60
9.	30·092	30·029	29·814	29·39	29·78	29·77	29·68	29·70	69·8	80·0	60·2	86	55	67	67	58½	63	57	ese.	s.	w.	sw.	nw.	......	......	......	......	·24	59
10.	29·794	29·772	29·621	29·11	29·68	29·52	29·74	29·67	80·0	82·5	66·0	93	60	76	71½	60	65	57	se.	se.	s.	sw.	se.	......	1·06	......	......	......	63
11.	29·936	30·131	29·832	29·07	29·69	29·87	29·49	29·73	65·0	87·0	62·0	71	47	62	64	57	55	53	e var.	w.	w.	w&sw	w.	·250	......	·64	......	·99	57
12.	30·350	30·257	30·200	29·60	29·92	30·02	29·86	30·00	66·4	76·0	59·5	78	55	61	63	56	57	55	sse.	sw.	w.	s.	sw.	·138	......	......	......	·43	58
☽ 13.	30·468	30·399	30·379	29·70	30·15	30·20	29·98	30·13	67·5	76·0	61·2	73	56	70	67	59	64	58	s.	sw.	calm	sw.	w.	·172	......	......	......	......	64
14.	30·472	30·359	30·214	29·71	30·28	30·27	30·19	30·23	72·0	75·2	65·2	85	48	72	69½	58	63	54	w.	ne.	calm	sw.	w.	·083	......	......	......	·29	65
15.	30·288	30·204	30·153	29·55	30·19	30·12	30·23	30·25	70·7	80·2	61·0	92	52	73	65	56	56	50	ese.	e.	calm	sw.	e.	......	......	......	0·80	·16	65
16.	30·218	30·162	30·130	29·55	30·20	30·15	30·31	30·27	72·7	84·0	63·4	89	57	64·5	69	48	58	52	ene.	e.	e.	e.	sw.	......	......	......	......	·39	63
17.	30·186	30·111	30·092	29·54	30·03	29·90	30·10	29·95	67·0	82·5	61·5	80	58	65	79	50	58	56	e.	e.	calm	s.	se.	......	......	......	......	......	62
18.	30·012	29·939	29·830	29·35	29·87	29·82	29·96	30·00	73·4	74·5	63·0	92	62	71	78½	51	58	57	ese.	e.	s.	w.	w.	......	·01	......	......	......	68
19.	29·996	29·901	29·879	29·16	29·71	29·67	29·85	29·66	70·4	86·0	67·2	73	61	73	72	62½	59	60	wsw.	sw.	calm	s.	se.	......	·01	......	......	......	65
20.	30·058	30·038	29·981	29·27	29·70	29·72	29·71	29·66	66·8	75·5	63·0	73	57	66·5	65	56	57	56	wnw.	w.	w.	s.	sw.	......	......	......	......	......	60
○ 21.	30·058	29·981	29·913	29·38	29·81	29·91	29·77	30·02	69·8	72·2	62·0	77	55	61	64	48	57	58	s.	sw.	calm	wnw.	sw.	......	......	......	......	......	60
22.	30·012	29·929	29·913	29·38	29·99	29·95	30·05	29·98	70·0	74·5	60·5	83	58	68	69	45	57	55	nne.	e.	calm	var.	sw.	......	......	......	0·13	·07	59
23.	29·966	29·888	29·869	29·30	29·84	29·68	29·75	29·65	72·0	79·0	65·0	85	46	68	72	47	51	57	s.	s.	e.	var.	s.	......	......	......	......	·42	64
24.	29·900	29·840	29·725	29·22	29·67	29·82	29·68	29·87	62·8	80·0	59·5	70	55	63	64	51½	57	54	sw.	ne.	calm	w.	sw.	......	·22	......	......	·80	56
25.	29·760	29·725	29·702	29·25	29·87	29·88	30·03	30·07	61·5	74·5	57·5	71	57	61	65	39	55	54	ne.	n.	ne.	ne.	calm	·166	·28	·03	......	......	61
26.	29·890	29·916	29·818	29·27	29·90	29·93	30·11	30·15	68·0	70·5	61·0	80	56	65	65	52½	57	54	ene.	e.	calm	n.	calm	·369	......	......	......	......	63
27.	30·008	29·910	29·933	29·37	29·97	29·97	30·15	30·17	63·0	74·0	60·0	74	57	63	70	52½	58	53	n.	ne.	calm	nne.	w.	......	·20	......	......	......	61
28.	30·046	29·977	29·976	29·42	30·00	29·99	30·21	30·16	62·0	71·5	61·0	72	56	65	68½	56½	59	55	n.	ne.	calm	ene.	e.	·127	·14	......	......	......	61
☾ 29.	30·028	29·959	29·956	29·46	29·97	29·90	30·00	29·90	63·0	70·5	60·0	74	55	64	70½	53½	58½	57	nne.	ne.	calm	sw.	se.	·283	·52	......	......	......	63
30.	30·076	30·011	30·003	29·40	29·94	29·98	30·00	30·14	63·0	72·0	60·0	70	46	61	63½	53½	56	56	nw.	w.	calm	nw.	n.	1·827	......	·16	0·32	·30	61
31.	30·202	30·151	30·137	29·57	30·05	29·97	29·96	29·88	56·5	69·0	50·0	64	50	55	60	38	55	50	n.	nw.	n.	w.	w.	......	·22	·03	......	......	48
Mean.	30·079	30·025	29·957	29·40	29·896	29·888	29·899	29·909	67·0	76·0	60·8	78·58	54·22	65·6	67½	52½	58·38	55·09						Sum. 3·800	2·81	1·05	1·35	4·69	Mean. 60

THE

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

[THIRD SERIES.]

NOVEMBER 1842.

LV. *Letter addressed by* M. Edmond Becquerel *to the Editors of the* Annales de Chimie et de Physique, *in Reply to* Mr. Daniell's *Letter to* Mr. R. Phillips *on the Constant Voltaic Battery, inserted in the Phil. Mag. for April* 1842*.

IN the *Annales de Chimie et de Physique* for December 1841, I published a Notice on constant voltaic batteries, in which I stated the facts relating to the subject just as they result from experiments performed by various natural philosophers who have been occupied with this subject.

Mr. Daniell, thinking that I had not done him justice, has thought it necessary to reply to several of my assertions in the Philosophical Magazine for April 1842. It was far from my intention to have wished to say anything which might be displeasing to him, and to have sought to misrepresent facts, with a view to attribute to my father a discovery which did not belong to him; in this respect Mr. Daniell is strangely mistaken as to my intentions, and without this motive, I should not have replied to him, having nothing to change, with respect to the main point, in the facts which I mentioned in my notice.

In every physical question three things are to be considered; the idea, the principle, and the applications. Now, it is proved by undoubted facts, that from 1829, and even several years before, my father had invented and constructed constant voltaic batteries, which, in truth, had not the power of action and the advantages possessed by the constant voltaic batteries of Mr. Daniell, who made them known in 1836. The apparatus invented by my father at once received the denomina-

---

* From the *Ann. de Chim. et de Phys.* for August 1842 (Third Series, vol. v. p. 412), published towards the end of September.

tion of constant voltaic batteries (*appareils à courant constant*), and, as they still perfectly fulfil the purpose he had intended, it is impossible to contend with him for the idea, the principle, or the application within certain limits.

The details into which I shall enter will leave no doubt about the priority of invention, at least I hope so. For more than fifteen years the electro-chemical reactions, by means of which my father was enabled to obtain crystallized mineral substances, were produced by the aid of small apparatus composed of tubes in the form of the letter U, closed at their curvature by a partition of moist clay designed to separate the two liquids placed in the two branches of each tube, one of which contained a solution of sulphate, nitrate or chloride of copper in contact with a plate of copper, and the other contained a solution of sea-salt, into which a plate of zinc or of another metal was immersed. Such is the arrangement of the simple apparatus which is scientifically known by the name of *pile à cloison**.

The form of this apparatus is of little importance, since it may be infinitely varied: for example, instead of a tube in the form of a U, we may take any kind of vessel, separated into two compartments by a diaphragm of bladder, baked earth, plaster, or linen, &c. But all these various modifications enter into the principle of the U-tube.

After the year 1829, and before Mr. Daniell's publication, my father made several communications relating to the same subject; in fact, we find in the *Compte Rendu des Séances de l'Académie des Sciences* for 1835, the description of an apparatus giving a current which was sensibly constant for two entire days.

According to this, therefore, Mr. Daniell cannot pretend to the discovery of the general principle on which the construction of constant voltaic batteries rests, but he may justly claim the good arrangement which he has given to his pile, and, amongst others, the advantage of always having a saturated solution of sulphate of copper, and of obtaining in a small compass effects far more energetic than those for which my father had occasion in the beginning, for the production of crystallized substances analogous to those formed by nature, a discovery for which he received the Copley Medal from the Royal Society of London, and which Mr. Daniell himself received some time after for the constant voltaic battery.

Mr. Daniell, notwithstanding facts so evident, declares in his

---

* An English translation of the description of this apparatus, and of M. Becquerel's Researches on Crystallization produced by Voltaic Action, was published in Taylor's Scientific Memoirs, Part 3. Jan. 1837.—Ed.

answer that he was not guided by the works of his predecessors in the construction of his battery, and that the principles upon which it rests are different from those which my father had long since admitted. He states, for example, that the rapid diminution, as well as the definitive cessation of the current in ordinary batteries, are due to the deposition of zinc on the negative plates of each couple. We agree perfectly upon this point; the annihilating action produced by the presence of the zinc comes under that designated by the term *polarization of the electrodes*. In my Notice, indeed, I mention, p. 438, in the eighth and following lines, that "each negative plate (of copper or of platinum) retains on its surface alkaline elements, such as hydrogen arising from the decomposition of water, and bases arising from the decomposition of saline matters dissolved in water." This phrase does not exclude any of the bases; the zinc therefore arising from the decomposition of the salt of zinc must equally be deposited on the negative plate. This deposition being effected, the action of the liquid on the zinc necessarily gives birth to a counter-current which more and more destroys the action of the first; in order to have an apparatus of continued force, it was necessary to prevent the zinc and the alkalies from being deposited on the negative electrodes.

Mr. Daniell afterwards says that the passage of the electric current across diaphragms of bladder is well known to experimentalists; he quotes Dr. Ritchie as having made use of them. To which I reply, that the use of diaphragms in physics is very ancient, since one of the Bernoullis had already separated two different liquids by a membrane, in an experiment in which he wished to produce an effect of endosmose. Porrett also adopted the same expedient in order to show that in separating, by means of a membrane, a mass of water into two parts, into each of which a plate of platinum was plunged communicating with one of the poles of a battery pile, the water passed from the positive into the negative compartment. I might still quote other examples; but the use of membranes, of diaphragms permitting the current to pass in order to obtain a couple giving a constant current, was brought into use by my father nine or ten years before Mr. Daniell was occupied with this question, and particularly in the experiments communicated to the *Académie des Sciences* on the 23rd of February, 1829*.

As to the publication of Dr. Ritchie in the Philosophical Transactions, it is of the month of May 1829, and consequently some months later. I therefore look upon Mr. Daniell's

---

* *Ann. de Phys. et de Chimie*, t. xli.

pile, although very convenient, as based upon the same principles as the apparatus which my father has used for a long time.

Further on Mr. Daniell adds: "Even in the use of the diaphragm, which might at first sight appear similar, there is a *direct* opposition, for my object is to keep the two electrolytes which I employ perfectly separate, so that no portion of one may penetrate to the other, except in the process of electrolysis."

I confess that I know not how Mr. Daniell can separate two liquids by a membrane moistened by them and which they can penetrate, without that passage from one to the other taking place which is otherwise called endosmose and exosmose. It is impossible to realize this condition; the only means of retarding for as long a time as possible the mixture of the two liquids, is by substituting for the membrane a thick diaphragm of clay, as did my father; the intensity of the current is then diminished, but constant effects are obtained which may continue for months, for years.

Still further on Mr. Daniell says: "and I repeat, that in my constant battery nothing depends upon the contact and action of the two liquids upon each other."

I do not understand this assertion; for every one knows that two different liquids acting upon each other by an intermedial membrane, disengage electricity enough to produce a current; and if Mr. Daniell wishes to convince himself of it, he has only to take away, in one of his couples, the plate of zinc and that of copper, and to substitute two plates of platina for them; he will have a current owing to the reaction of the two liquids upon each other, less intense indeed than that obtained with a couple in which an oxidable metal is included.

Mr. Daniell also says, that "the amount of force obtained by my father's apparatus is insignificant with regard to its application to the arts."

I will reply *yes* and *no* to him; yes, if there is a question of apparatus like those of Mr. Daniell, designed to obtain currents which are to be transmitted into liquids placed in separate vessels; no, if the currents are to react chemically on the liquids making a part of the apparatus themselves.

In short, the apparatus constructed by my father, six years ago, for the treatment of ores of silver of lead and of copper, are based on the same principles which I have before explained, and are of much more considerable dimensions than those of Mr. Daniell, since each couple requires 1000 litres of liquid to act, and six similar couples have been united, so that 6000 litres have acted at the same time, and the energy of action has been still greater than that produced with the apparatus of Mr. Daniell, since all the silver and the lead contained in the ores, that is to say about one kilogramme of silver and 100 kilogrammes of lead, were extracted in the space of a few hours.

I now leave it to the judgement of the reader which is in the right, Mr. Daniell or myself; and it will then be seen whether *filial piety blinded me*, or whether I have not rather been actuated by the love of truth.

Paris, July 7, 1842.

LVI. *Remarks on a Letter of* Professor Daniell *contained in the Philosophical Magazine for April. By* W. R. Grove, *Esq., M.A., F.R.S., Professor of Experimental Philosophy in the London Institution.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ALLOW me to request your insertion of a few remarks on a letter of Professor Daniell published in your Magazine for April. Absence from London and occupations of other than a scientific nature prevented my noticing it at the time; my attention has been recalled to the matter by its republication in the *Annales de Chimie*.

A few words at the conclusion of this letter refer to me: after stating that M. Becquerel has inadvertently described my experiments as anterior to Mr. Daniell's, this gentleman goes on to say, "Professor Grove has never spoken of his battery but as the further application of principles which I had previously deduced."

It is perhaps of little moment to the public what principles led me to the construction of the battery in question, but it may be of some moment to me, as should I, by silence, be held to assent to certain principles, I may be accused of contradiction and inconsistency if in any future paper I should state my adherence to others. M. Becquerel, again, in the 5th volume of his *Traité de l'Electricité*, describes my battery as "Pile voltaïque construite d'après les principes exposés dans les chapitres 1er, &c.:" these chapters contain the papers of M. Becquerel in respect of which he claims priority to Mr. Daniell. It is obvious, that as M. Becquerel and Mr. Daniell differ in their notions as to the principles of the constant battery, I could not derive my battery from both, and I have looked over my papers on this subject to see whether I have expressly referred it to principles enounced by either of these philosophers; I cannot see that I have. I have on many occasions mentioned their experiments before my own in the history of the voltaic pile, both as acknowledging their priority and as not wishing to claim what was not my due; probably it is this which has led to a misconception on the part of Mr. Daniell, but I have distinctly stated the idea which immediately led to the construction of my battery in the paper which describes it (Phil. Mag., May 1839). After detailing an experiment with two strips of gold-leaf in nitric and hydrochloric acids separated by a porous diaphragm, and showing that upon contact of the two strips the gold in the hydrochloric acid was dissolved, and that a voltaic current was established, I say, " It now occurred to me, that as gold, platina and two acids gave so powerful an electric current," *à fortiori* " the same arrangement, with the substitution of zinc for gold, must form a combination more energetic than any yet known :" this was the simple deduction which led to my subsequent experiments. I have in most cases been content to publish experiments with no more of theory than was requisite to connect them; it is a general and I think a just complaint that there are already too many speculations on this subject; but in a letter published in the Philosophical Magazine for Feb. 1839, p. 129, previous to the discovery of my battery, I gave my own notions of the principles of voltaic batteries, notions which in some respects agree with those of Mr. Daniell, but which also suggest some new views of voltaic action. There is one experiment there detailed in which copper is reduced by copper, which had much influence on my subsequent experiments, but which is not explicable by any principles laid down by Mr. Daniell; at the conclusion of this paper I say, " if these principles be correct, very superior combinations may be discovered:" how this prediction has been fulfilled the public is the best judge.

Far be it from me to disclaim any assistance from the experiments of Mr. Daniell or of M. Becquerel; I shall ever retain a grateful recollection of the assistance rendered to my first efforts in science by the latter gentleman. I cannot at this distance of time well describe what effect their experiments had upon my mind. In the progress of science it is difficult to define the frequently unperceived effect of prior discoveries upon subsequent experimentalists, but I cannot for many reasons acquiesce in the assertion of Mr. Daniell above quoted.

Mr. Daniell was for a long time attached to the theory of the deposition of metals in the voltaic circuit being the result of a secondary action of the nascent hydrogen, a theory generally adopted until combated by Hisinger and Berzelius; thus in his papers, Phil. Trans., 1836, p. 117 *et seq.*, he proceeds to explain his constant battery as dependent upon the removal of that hydrogen by causing it to deoxidate copper: in a subsequent publication (Phil. Trans., 1839) he abandons this view, and considers the deposition of the copper as "a primary result of electrolytic action." This would altogether alter the theory of his battery and of mine. I do not think it is a matter of great consequence which theory be adopted; each has many peculiar difficulties, each tends to many similar conclusions, and either may lead to equally successful experimental results. Theory is valuable as a means not as an end, and that theory of the voltaic battery is in my opinion the best which best collates the observed phænomena and which leads to the discovery of the best voltaic combinations. But although I would hesitate, without more conclusive experiments, in ascribing this superiority to either of these theories, there is another principle of the voltaic battery enounced by Mr. Daniell, as to which, so far from agreeing with him, I must take leave (with every respect for his scientific attainments) to differ *toto cœlo*: it is as to the relative extent of surface to be given to the metals of voltaic combinations. Mr. Daniell has in the Phil. Trans. for 1836, p. 128, and in several subsequent papers, stated that the best theoretical form for a voltaic combination is when the generating metal is arranged with regard to the conducting one as the centre of a sphere to its periphery, and recommends a rod within a cylinder as the nearest practical approximation to such an arrangement; following the authority of Mr. Daniell, I first constructed my batteries of this form, but very soon abandoned it (see Phil. Mag. for Oct. 1839, p. 288); and I am now convinced, by three years' experience and by repeated experiments, corroborated by the experiments of others, that this is by no means the best form of arrangement, as regards œconomy either of space, time, or material. I believe the old arrangement of equal surfaces to be sufficient for most practical purposes; but the relative size may be considerably modified according to the nature of the electrolytes, the conducting power of the metals, and other circumstances. I cannot enter more fully on this point without writing a paper especially on this subject.

P.S. Since the above was written I have received a paper of Mr. Daniell's just printed, Phil. Trans. 1842, part ii., for which I have to thank the author: it contains a series of experiments on my battery, and with a voltameter of my contrivance. In this paper I see Mr. Daniel alters many of his opinions upon the relative size of the plates in voltaic combinations.

LVII. *On the Iodide of Mercury.* *By* H. F. Talbot, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOUR Number for last September contains a paper by Mr. Warington "On the Change of Colour in the Biniodide of Mercury." Permit me to observe, that the facts contained in the first part of that paper were long ago discovered and published by myself, in your Journal (S. 3. vol. ix. p. 2*).

As I do not wish to be deprived of the discovery of one of the most curious phænomena in optics, I beg leave to draw Mr. Warington's attention to that paper, and briefly to recapitulate its contents.

In that memoir I have shown,—

1. That when iodide of mercury is sublimed between two plates of glass nearly in contact with each other, it cools in the form of thin rhombic plates of a pale yellow colour.

2. These often retain their colour when cold, if left undisturbed.

3. But if such a crystal is disturbed, as for example, by touching it with a needle at any point of its surface, it instantly turns scarlet at the point touched, and the scarlet colour is rapidly propagated over the whole crystal. I showed this experiment to Sir David Brewster in the year 1836, and I have no doubt he remembers it well, as he expressed great admiration of the beauty of the phænomenon. The crystal was touched with the needle while under examination with a powerful microscope.

4. The crystal moves and is spontaneously agitated during the time it is changing colour.

5. During the progress of this change, the scarlet portion remains bounded by straight lines, very well defined, and parallel to the edges of the rhombic crystal.

6. I thence drew the conclusion, that the change of colour was caused by the displacement of the rows of molecules or laminæ of the crystal. This I think will be admitted to be the true explanation; and it was one which had not been previously suggested. I added, that I thought this phænomenon "*the most evident proof we yet possessed of the dependency of colour upon internal molecular arrangement.*"

7. I also remarked that these little rhombic crystals were very fine objects for the polarizing microscope. The expressions of Mr. Warington, that the crystals "*in the dark field had the appearance of the most splendid gems,*" have recalled

---

[* On inserting Mr. Warington's paper we referred to Mr. Talbot's previous experiments, as stated by him in Phil. Mag.—Edit.]

to my memory the very similar words which I used when I first announced the invention of the polarizing microscope in your Journal (vol. v. p. 324), viz.

"The field of view appears scattered with the most brilliant assemblage of highly coloured gems, affording one of the most pleasing sights that can be imagined. The darkness of the ground on which they display themselves greatly enhances the effect."

With regard to the above points, then, I consider that they were sufficiently established by me in 1836.

The second part of Mr. Warington's paper, however, contains a fact both new and important; I mean the solution of the yellow crystals in the liquid and the formation of the red ones, of a different form, in their places. But this observation is most strictly analogous to the phænomenon which I discovered in the *iodide of lead*, and published in your Journal (vol. ix. p. 405), viz. the sudden change of a crystal of that salt from the form of a white needle to that of a row of thin yellow regular hexagons lying in a straight line. Such a metamorphosis was previously unexampled; Mr. Warington has now furnished us with a second example (also the iodide of a metal): I have myself observed something similar in the iodide of tin; and I recommend the whole subject of the crystalline form of the metallic iodides to the renewed and careful consideration of chemists.

I am glad of the opportunity afforded me by Mr. Warington's paper of again calling attention to these very curious facts, which appear to me to open a path that promises to lead far into those arcana of Nature, the mysteries of molecular action.

I remain, Gentlemen, yours, &c.,

London, Oct. 1, 1842. H. F. TALBOT.

LVIII. *On the Progress of Embryology in the Year* 1840*.

"SOME interesting discoveries rendered the past year a highly productive one for embryology. Two main problems which engaged the various physiologists here occupy the foreground, namely, the earliest development of the Mammalia, and the metamorphoses of the germinal membrane in its transformation into the embryo******. So long as the metamorphoses of the germinal vesicle following fecundation could be considered only hypothetically, it was assumed that the Purkinjean [germinal] vesicle either burst and poured out its contents, or became flattened; and now contributed to the forma-

---

* From Professor Valentin's Report in the *Repertorium für Anatomie und Physiologie*, Jahrgang 1841.

tion of the germinal membrane in one of these two ways. Both theories had been put forth before the discovery of the germinal spot. But when the existence of the latter became known, the discoverer of the same said that probably the *macula germinativa* represented the first foundation of the germinal membrane. This conjecture obtained more probability from the obvious fact, that the number, size and distribution of the germinal spots alternated according to the different stages. Research, however, first in the Mammalia, and then in Reptiles and Fishes, showed that in consequence of fecundation the interior of the germinal vesicle presents new cells, or that (as was seen in the Rabbit) within the germinal vesicle new cells are really built up upon the foundation of the germinal spots." (Introductory Remarks, p. 13.)

First stages in the development of the fecundated ovum, especially that of the Mammalia. As was already remarked in the introduction, the most important publications of the past year concerning embryology are concentrated in the subjects of this chapter. We will therefore, before presenting some extracts of the details, state the most important results. With few exceptions, to be mentioned, all the observations have reference to the Mammalia, and indeed to the Rabbit.

1. *At the period of the rut certain changes have already taken place in the ovarium, the* [*Graafian*] *follicles, and the structures appertaining thereto.* Through an increased congestion of the ovary single follicles become more strongly developed. The germinal spot, which gives the impulse to the formation of the new cells, probably undergoes changes of this kind. From the observations of Négrier, above mentioned (p. 248), it may be conjectured that in the human female also the period of menstruation is attended by similar phænomena.

2. *Fecundation itself apparently comes to pass in the following manner: a portion of the semen that has been brought to the surface of the ovarium probably passes into the ovum, and gives the stimulus to the formation of cells within the germinal vesicle*******.

3. *The number of ova prepared for fecundation by the rut, does not correspond with the number of the subsequently fecundated ova, but generally exceeds the same.* This fact, already known, has been confirmed by the latest researches on the Rabbit.

4. *It often happens that more ova pass out of the ovary than are fecundated, or at least than become developed.* Herein accord the observations of Barry with those of Pappenheim. The former found in the tubes and uterus unfecundated or

aborted ova. In like manner, parts of the [Graafian] follicle which usually remain in the ovary, for example, portions of Barry's *ovisac*, may be found in the oviducts.

5. *Neither the place to which the ova in the tubes and uterus have advanced, nor the size of the same, nor the time that has elapsed since they left the ovary, affords an exact criterion for the degree of their internal development.* This position furnishes only a confirmation of what was already known******.

6. *The germinal vesicle does not disappear nor burst through fecundation, but fills with cells, the formation of which proceeds from the germinal spot: and this takes place by no means in a peculiar manner, but according to a normal mode which manifests itself elsewhere.* These circumstances, which really extend our knowledge, have been made known by the laborious researches of Barry. The general process is as follows;—It is known that in the interior of the germinal spot there exists a central body, which often becomes surrounded by concentric traces. This body now enlarges and fills with a pellucid fluid. That part of the germinal spot which is directed towards the interior of the germinal vesicle passes into cells, arranged like pill-boxes one within the other, yet so that the pellucid central vesicle remains near to the periphery [of the ovum]. Within the cells thus arisen there are formed new cells. This cell-formation proceeds in layers from the centre towards the periphery. The outer strata of cells are thus pushed further out, and the most external disappear while new inner strata form, so that the middle ones advance to the outer part. In this manner the germinal vesicle becomes filled with masses of cells, while its membrane disappears. But in the situation of what was originally the centre of the germinal spot there are formed two cells, distinguished by their larger size: and out of these two larger cells new cells arise, as before through the formation of cells in cells,—4, 8, 16, and so on,—the number doubling every time. These two cells of the central part of the germinal spot, with their succeeding cells, form the foundation of the germ. In it, the germ, again, there is to be seen a cell distinguished by its larger size. The nucleus of this latter cell generates, through further development, the foundation of the embryo. It may hence be conceived, that the seminal fluid taken up by imbibition, arrives at what was originally the central part of the germinal spot; first gives a stimulus to the cell-formation in the peripheral part of the germinal spot, and to the consequences of the same; then, through the formation of cells, becomes itself the germ; and that, subsequently, within the germ the nucleus of a principal cell gives the stimulus to the formation of the embryo. Fe-

cundation thus consists in the imbibed seminal fluid stimulating the germinal spot to the cell-formation, according to the type of cells in cells. But many more cells are formed than remain; the outer layers being constantly absorbed.

7. *The furrows known to be presented by the yelk arise from the formation of cells* (see *Repertorium*, v. 306). Their presence in Fishes was etablished by Rusconi, in Mammals by Barry. In Birds they may either entirely fail, or, as is more probable, be limited to the germinal membrane and not extended to the yelk.

8. *The rotation of the yelk or of the embryo in the ovum, previously observed in invertebrated animals and in Batrachian Reptiles, is also found to take place in Fishes and Mammalia.* Rusconi perceived this rotation thirty hours after fecundation in ova of the Pike; so that it is thus met with where there is a circumscribed germinal membrane. In the Rabbit it was seen by Barry, although he remained in doubt as to the nature of the rotating body which was determined by Bischoff. The latter described also vibrating cilia on the superficial cells. It now remains a point of especial interest, to extend the observation to classes which otherwise do not exhibit ciliary motion, for instance the Crustacea.

9. *Of the other structures of the* [*Graafian*] *follicle which pass out* [*of the ovary*] *along with the ovum, the tunica granulosa and retinacula* (*discus proligerus*) *undergo liquefaction*; *while within the zona there arise concentric formations of membranes and fluid or semifluid rings.* According to Barry, this formation amounts to from four to five membranes. The attenuation of the *zona* above mentioned soon disappears. The chorion is not formed out of the *zona*, but out of cells, which arise in the tube and are laid down around the metamorphosed structures.

[Professor Valentin then proceeds to give details of the observations of Dr. Barry, the principal of which are the foregoing nine. These details will be found in the Philosophical Transactions for 1839 and 1840. Abstracts of them have been already furnished by this Journal.]

LIX. *On the Theory of Molecular Action according to* Newton's *Law: in reply to* Professor Kelland. *By* S. EARNSHAW, *M.A., Cambridge*[*].

HAVING been long of opinion that the molecular forces which regulate the vibratory motions of particles cannot vary according to Newton's law of universal gravitation, it

---

[*] Communicated by the Author.

was with great pleasure that I read in Professor Kelland's letter that the attention of the greatest mathematicians in Europe is now alive to the necessity and importance of having "the difficulties which attend the theory" removed: and I rejoice that Professor Kelland has undertaken the task of thoroughly reviewing the grounds of my opinion. In my memoir on the subject printed in the Cambridge Philosophical Transactions, I have shown, apparently to Professor Kelland's satisfaction, that when $\frac{d^2 V}{d f^2}, \frac{d^2 V}{d g^2}, \frac{d V}{d h^2}$ are not zero, the medium is incapable of transmitting light, and have dismissed at once as foreign to the subject the case where these quantities are zero, which case the Professor argues "embodies the real state of things." The grounds on which I dismissed this case in so summary a manner were these:—

1st. The acknowledged experimental fact of the superposition of waves of light requires that the forces called into play by a displacement should depend only (or at any rate *chiefly*) on the *first* power of the displacement.

2ndly. The received explanations of refraction through crystals and of other phænomena, assume that the force of restitution depends only on the *first* power of the displacement; and,

3rdly. If $\frac{d^2 V}{d f^2}, \frac{d^2 V}{d g^2}, \frac{d^2 V}{d h^2}$ be zero, the first powers of the displacement disappear; and therefore this case is inconsistent with the known results of experiment and the requirements of received and established theory. Yet Professor Kelland thinks that the real state of things is embodied in the excepted case, and founds his belief on arguments drawn from analytical expressions in his memoir. It appears to me, then, that the shortest way of bringing the controversy to an end, will be to show that the Professor's own investigations, under proper mangement, lead us to the same results as were given in my memoir. At pages 162, 163 of the Professor's paper on Dispersion, we are told that on the hypothesis which he has adopted each of the quantities

$$2\,\Sigma\left\{\phi r + \frac{F(r)}{r}\,\delta x^2\right\}\sin^2\frac{k\,\delta\varrho}{2}$$

$$2\,\Sigma\left\{\phi r + \frac{F(r)}{r}\,\delta y^2\right\}\sin^2\frac{k\,\delta\varrho}{2}$$

$$2\,\Sigma\left\{\phi r + \frac{F(r)}{r}\,\delta z^2\right\}\sin^2\frac{k\,\delta\varrho}{2}$$

is equal to n^2. It follows therefore that n^2 is equal to one third of their sum, i. e.

$$n^2 = \frac{2}{3}\Sigma\left\{3\,\phi\,r + r\,\mathrm{F}\,r\right\}\sin^2\frac{k\,\delta\,\varrho}{2}.$$

The quantity n is the coefficient of t in the expression of a displacement, $\alpha = a\cos(n\,t - k\,\varrho)$.

Now on the Newtonian law $\phi\,r = \frac{1}{r^3}$ and $\mathrm{F}\,r = -\frac{3}{r^4}$, and therefore $n^2 = 0$; which being substituted, the Professor's equations of motion assume the following forms:—

$$\frac{d^2\alpha}{d\,t^2} = 0,\quad \frac{d^2\beta}{d\,t^2} = 0,\quad \frac{d^2\gamma}{d\,t^2} = 0,$$

which indicate that on the Newtonian hypothesis no forces are called into play by the vibratory displacements of the particles. Now the Professor, having treated the quantity n^2 as finite in all his investigations on this subject, will see that all arguments based on them against what I have written fall to the ground, and that my arguments remain in full force.

Cambridge, August 19, 1842.

LX. *Some Additional Remarks upon a Communication of* Professor Kelland, *published in the Philosophical Magazine for May last. By the Rev.* M. O'Brien, *late Fellow of Caius College, Cambridge*[*].

IN the Philosophical Magazine for June 1842, I asserted that certain fundamental equations in Professor Kelland's memoir on Dispersion (in the Cambridge Phil. Trans., vol. vi.) were erroneous. A friend has suggested to me that I ought to have proved *more distinctly* the existence of those errors. This I will now do in the following manner:—

(1.) *With respect to the equations of motion in page* 159, *vol.* vi. *Camb. Trans.* Professor Kelland has overlooked the terms arising from the part of the equation in page 158, which is multiplied by $\delta\,\beta$ and $\delta\,\gamma$: for instance, there is a term in the expansion of $\delta\,\beta$ $\left(\text{viz. } \frac{d^2\beta}{d\,x\,d\,y}\,\delta\,x\,\delta\,y\right)$ which gives rise to the following term in the expression for $\frac{d^2\alpha}{d\,t^2}$, viz.

$$\Sigma\,\frac{\mathrm{F}\,(r)}{r}\,\delta\,x^2\,\delta\,y^2\,\frac{d^2\beta}{d\,x\,d\,y},$$

which term does not appear *as it might* in Professor Kelland's equation.

And there is another similar term omitted, viz.

$$\Sigma\,\frac{\mathrm{F}\,(r)}{r}\,\delta\,x^2\,\delta\,z^2\,\frac{d^2 r}{d\,x\,d\,z}.$$

---

[*] Communicated by the Author.

(2.) *With respect to the equations at the foot of page* 162, we have

$$\delta\varrho^2 = e^2\delta x^2 + f^2\delta y^2 + g^2\delta z^2$$
$$+ 2(ef\delta x\,\delta y + fg\,\delta y\,\delta z + eg\,\delta x\,\delta z).$$

Hence expanding $\sin^2\frac{k\,\delta\rho}{2}$, and omitting the parts multiplied by k^4 k^6, &c., we have

$$\Sigma\left\{\frac{\mathrm{F}(r)}{r}\delta x\,\delta y\sin^2\frac{k\,\delta\rho}{2}\right\} = \Sigma\left\{\frac{\mathrm{F}(r)}{r}\delta x\,\delta y\frac{k^2\delta\rho^2}{4}\right\}$$
$$= \Sigma\left\{\frac{\mathrm{F}(r)}{r}\frac{k^2}{4}\cdot 2\,ef\delta x^2\,\delta y^2\right\},$$

omitting all terms in which an odd power of either δx, δy or δz occurs.

Hence the term which Professor Kelland makes out to be zero, equals

$$ef\frac{k^2}{2}\,\Sigma\left\{\frac{\mathrm{F}(r)}{r}\delta x^2\,\delta y^2\right\} + \text{higher powers of } k^2,$$

which is clearly not zero.

The error by means of which Professor Kelland shows that this term is zero, is quite apparent in the middle of page 162. He reasons upon $\delta\rho$ just as if it was r, i.e. the distance of the particle whose coordinates are $(x+\delta x)$ $(y+\delta y)$ $(z+\delta z)$ from that whose coordinates are $x\,y\,z$; whereas $\delta\rho$ is quite a different thing, namely, *the perpendicular* let fall from the point $x\,y\,z$ *on the wave surface* passing through the point $(x+\delta x)$ $(y+\delta y)$ $(z+\delta z)$; which perpendicular is altered in length when we put $-\delta x$ for δx, leaving δy and δz unaltered; and this is fatal to Professor Kelland's reasoning.

8 Park Terrace, Cambridge,
June 7, 1842.

P.S. Oct. 7, 1842.—Professor Kelland evidently does not suppose the axis of y to coincide with the direction of transmission: for suppose that it does, then $\delta\rho = \delta y$, and therefore equating the two expressions which Professor Kelland assumes to be equal to n^2 at the foot of page 162, we have,

$$\Sigma\frac{\mathrm{F}(r)}{r}\delta x^2\sin^2\frac{k\,\delta y}{2} = \Sigma\frac{\mathrm{F}(r)}{r}\delta y^2\sin^2\frac{k\,\delta y}{2};$$

or, retaining only the first power of k^2,

$$\Sigma\frac{\mathrm{F}(r)}{r}\delta x^2\,\delta y^2 = \Sigma\frac{\mathrm{F}(r)}{r}\delta y^4.$$

Now it is well known that one of these expressions is three times the other. Hence Professor Kelland does not suppose the axis of y to coincide with the direction of transmission. The same may be said of the axes of x and z.

LXI. Professor Kelland's *Vindication of himself against the Charges of the Rev.* M. O'Brien.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Paris, June 5, 1842.

I HAVE read, with extreme astonishment, the attack made on me by Mr. O'Brien in your Magazine for this month. My first impression was that it did not become me to reply to it in any shape, but, on reflection, it has appeared probable that many persons may read it who are not intimately acquainted with the subject, on whom the effect of silence would be equivalent to the admission of the justice of the statements made. I shall therefore enter into a brief explanation, with a view to direct your readers to the *facts*, not to carry on a controversy. But in commencing I am naturally induced to ask, wherein have I offended Mr. O'Brien? For it must be noted by every one that Mr. O'Brien's object is not to "reply" to my remarks, but to remove an impression which he thinks I have endeavoured to create, "that he has done nothing in his paper which had not been already done by myself in my memoirs." Now I should be exceedingly sorry to create a false impression; and I am sure no person who reads my remarks will accuse me of having done so intentionally. Still had Mr. O'Brien candidly stated that such was the impression *on his mind*, either in your Journal or personally when I saw him in Cambridge, I would have addressed myself diligently to remove it. But Mr. O'Brien's procedure leaves no room for any other course than to reply publicly to his charges, and leave it to the world to judge between us.

First, then, have I attributed to myself the "notation" employed, "the equations of M. Cauchy," the conclusion "that transverse and normal vibrations are in general propagated with different velocities?" *Never*; I am not chargeable with such dishonesty. They are all, as far as I know, due to Cauchy. Nor is there *one* of the conclusions of M. Cauchy which I have, even by accident, called my own. I will not waste words about this. M. Cauchy himself assures me that I have spoken *with perfect justice and propriety.*

Secondly, have I attributed to myself the conclusion "that homogeneous light must in general suffer dispersion in passing through a prism, and dispersion of a discontinuous nature, and that this accounts for the dark lines in the spectrum?" *I never heard of the conclusion.*

Thirdly, have I endeavoured to attribute to myself the conclusion "that the results obtained on the hypothesis of perfect symmetry, are also true when the symmetry is disturbed by the action of the particles of matter?" *I did not know even what has to be done on this subject.* I spoke of it thus: "It is true I did *not* succeed in proving that the conditions resulting from such an arrangement are the same as those which depend on the supposition of perfect symmetry. Mr. O'Brien proposes to do this, and if he succeeds, it will, I am sure, be an important step in our theoretical investigations." All that I did and do know on this subject is, that M. Cauchy has arrived at the same conclusion; but, if my memory serves me right, under certain limitations.

Lastly, the only portion of Mr. O'Brien's paper which I can be said even remotely to have attributed to myself, is that which is contained in the following sentences of Mr. O'Brien's reply. "I have assumed that the particles of æther are acted upon by those of matter; and I have employed the equations of M. Cauchy, viz.

$$\frac{d^2 \alpha}{d t^2} = \Sigma m, \text{\&c.}$$

adapting them to the case of a set of æthereal particles acted on by material."

"He certainly endeavours, in his 'Theory of Heat,' to account for dispersion independently of the hypothesis of finite intervals" of the particles of matter.

To the former of these Mr. O'Brien adds the observation, "So far as this I lay no claim to originality, nor has Professor Kelland any right to do so either." Now as regards the *laying claim* to the process, your readers will be so kind as to refer to my paper and judge for themselves whether or not I have spoken modestly; and as regards the *right*, I certainly did believe, and do so still, that it was due to me. At any rate it was incumbent on him who made the charge to have furnished the proof.

I think I have said enough to clear myself from any imputation of dishonesty. I may add, that my remarks were written in *defence* of a theory to which I had contributed. The express object of my writing was to prepare for further discussion on the *possibility* of the hypothesis of finite intervals. I was therefore constrained to show what I believed to be the state of the theory, and how Mr. O'Brien's hypotheses, &c. differ from it. In doing so, I never combated one of Mr. O'Brien's conclusions, I never disparaged his hypothesis or his mode of accounting for dispersion. Had I erred in any one point, it was Mr. O'Brien's duty to have set me right. He has not done so, unless I am to understand that certain assertions about what I have said or denied suffice to effect it. Let us take an instance or two, and I will add all that can be added to such assertion by way of answer. Mr. O'Brien says, "I have arrived at a result never obtained before, namely, that dispersion must arise from the *direct* action of the particles of matter upon those of æther. This result is denied by Professor Kelland." I answer, *I never* denied it. Of course I object to the word *must.* The facts are simply these. With regard to the action of the particles of matter on those of æther, M. Cauchy and Mr. O'Brien adopt one hypothesis, whilst I had adopted another. On theirs, the velocity of transmission depends on the *direct* action of the particles of matter; on mine, it does not. That either or both may be wrong is perfectly possible; in groping after truth, we cannot reason directly from data up to laws, but must work our way back from *assumed* laws to the experimental data. Hence the value of researches such as those before us. They may ultimately lead to truth even at the time when least approaching to it. I ought to point out that M. Cauchy regards his equation, *Nouveaux Exercices*, vol. i. p. 98, as embodying the explanation of dispersion by the *direct* action of the particles of matter on those of æther. I am not aware, however, that he has ever stated so in print, nor do I wish to rob Mr. O'Brien of the credit of the explanation.

But this leads me to another of Mr. O'Brien's assertions. "I would ask Professor Kelland, is it possible that he thinks this formula capable of accounting for dispersion independently of the hypothesis of finite intervals? Is it not very evident, except that hypothesis be true, that $k \Delta x$ is extremely small, &c.? Why then has Professor Kelland produced this expression *as equivalent* to mine?" My best answer to this will be to direct your readers to turn back (which I trust they will not omit to do) and see what I *have* said. They will find the following sentence (and I trust they will in all cases read the context):—" But that effect depends on their mutual distances, and thus *finite intervals*, not indeed of the particles of æther, but of those of matter, necessarily play a conspicuous part:" and again, "the real difference between the received theory and that before us is this; that the former rejects the direct attraction of the particles of matter as producing no effect on the time of vibration of a particle of æther," &c. &c.* I was perhaps hardly justified in using the word *received*, but against this there can be no present complaint.

Again, Mr. O'Brien says, "I have given a simple proof of

---

* See Phil. Mag., S. 3. No. 132, (vol. xx.) p. 377.

what was only *asserted* by Mr. Green, viz. that transverse and normal vibrations are in general propagated with different velocities. I have learned since that M. Cauchy had previously arrived at the same result. Professor Kelland distinctly denies the correctness of this result in the Royal Edinburgh Transactions, vol. xiv. p. 396." I have not the passage to turn to, but your readers have, and I fearlessly assert, *I never denied the correctness of M. Cauchy's result.* I *could* not have done so. The assertion, or rather *hypothesis*, of Mr. Green is, if my memory does not deceive me, a very different affair. But instead of denying Mr. Green's vibration (which he *calls* normal, but which is really not so), I have adopted, applied, and *acknowledged* it over and over again.

I do not intend to touch on every point in Mr. O'Brien's reply. I do not conceive that an acrimonious personal contest can ever benefit the cause of science. I shall therefore rest satisfied with clearing myself. Had Mr. O'Brien contented himself with saying, "I assert that the equations at the foot of p. 162 of the Transactions of the Cambridge Philosophical Society are *essentially erroneous*," &c., "they prove that Mr. Kelland's equations in the Cambridge Philosophical Transactions, vol. vi. p. 159, are *essentially erroneous*," I would have excused the harshness of the term "essentially erroneous" in italics, and have given the following explanation. It is perfectly possible that these two equations may be written down unaccompanied by the restriction that one of the axes of coordinates is the direction of transmission; nay more, it is perfectly possible that I have stated that ϱ is not necessarily measured along an axis. The fact is this: all these equations were *deduced* on the hypothesis that the axis of y is the axis of transmission.

When the paper was copied for the press (by my friend Mr. Bird) certain interpolations were introduced, which, as I never saw the proof sheets, remain on the pages. This explanation must not be understood as the admission of my having fallen into error further than it states.

I assert, first, that when it is remembered that one of the axes is that of transmission, *all* my equations in that memoir are correct; and secondly, that I never deduced *one* result from an equation which is not correct, so far as that memoir is concerned.

Mr. O'Brien's argument that because M. Cauchy's equation is of one form and mine of another, *one* must be incorrect, is only good when the hypotheses are identical. That they are not stated to be otherwise must be a fault of mine. But I have *never* employed the equations in this form, to the best of my knowledge, so that the erroneousness of an equation affects nothing but the equation itself.

Mr. O'Brien's argument against the equations I *have* used, viz. that

$$\Sigma \left\{ \frac{F\,r}{r} \delta x\, \delta y \sin^2 \frac{k\, \delta \varrho}{2} \right\}$$

is not zero, is good only on the hypothesis that ϱ is not measured along one of the axes. Had Mr. O'Brien read my papers he would have seen that I have *twenty times over* at least, given this expression a value which is not zero. But when the direction of transmission in an *isotrope* medium is under one of the axes, the expression *is* zero. And these are the only circumstances in which I have used it as such; your readers will find my fundamental equations deduced in your Magazine for May, 1837. I think they will see my views correctly stated there, and trust they will do me the justice to examine them before they give credence to the following assertions of Mr. O'Brien:—

"And here I must enter a decided protest against *all* Professor Kelland's reasoning on the subject of transverse and normal vibrations." "Now this error in the *fundamental equations* vitiates all his results, so far as they relate to the nature of the vibrations and the velocity of propagation," &c. "This error runs through *all* Professor Kelland's papers and his 'Theory of Heat,' so far as I have read them," &c.

What could have dictated such expressions so utterly ungrounded, I leave to the world to judge.

In conclusion, Gentlemen, allow me to thank you for your kindness in receiving my former communications, and to request that you will publish this in your forthcoming Number.

I have the honour to be your obliged Servant,

P. KELLAND.

LXII. *On certain Spectral Appearances, and on the discovery of Latent Light.* *By* J. W. DRAPER, *M.D., Professor of Chemistry in the University of New York.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IF there be a thing in which I have a disinclination to engage, it is controversy of a personal kind with scientific fellow-labourers. But, as you well know, it ordinarily happens that there is no other gain to philosophers beyond the *mere credit* of their discoveries, they may be forgiven for reluctantly endeavouring to secure this their only reward.

I have recently returned from a long journey, undertaken

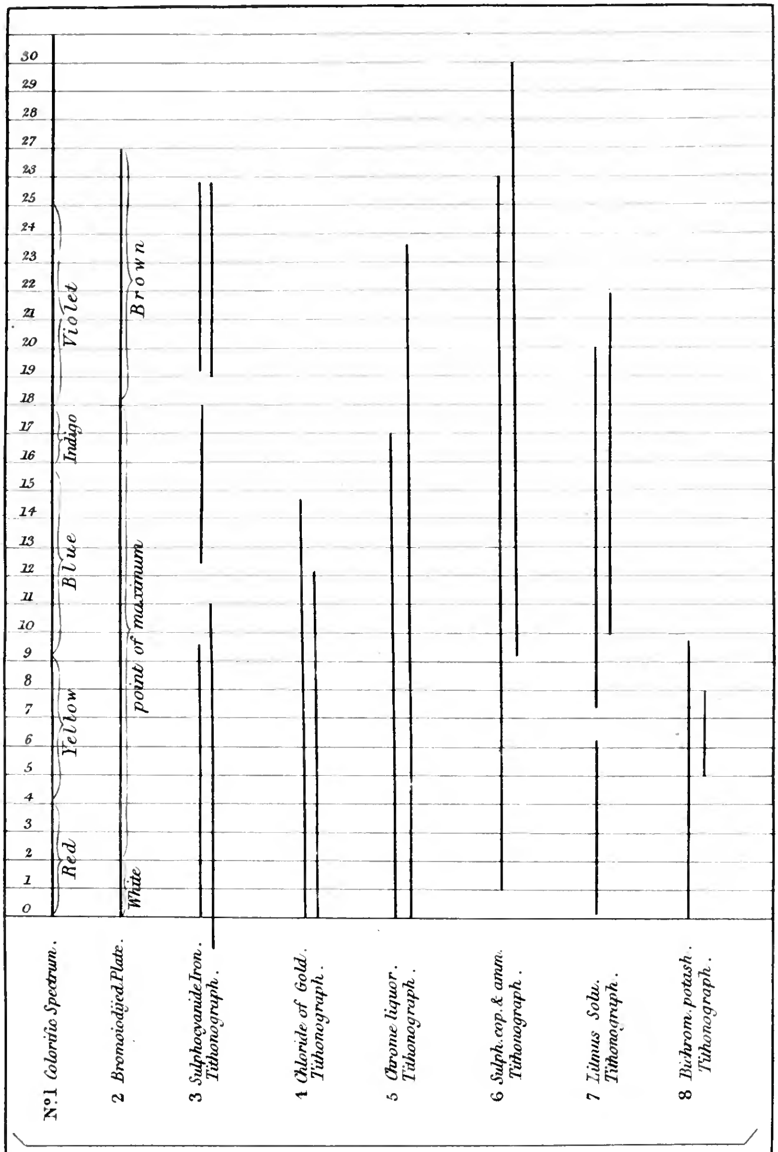


Fig. 3

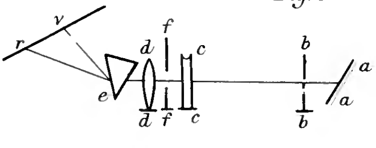


Fig. 1.

Fig. 2.

Linear Solar Spectra with their corresponding Tithonographs shewing the Physical independence of Tithonicity and Light.

for the purpose of making trials on the sunlight in lower latitudes, and am grieved to see in the reports that have reached this country of the Proceedings of the British Association, certain announcements, received from Professor Bessel*, of phantoms which can be produced on surfaces by mercury vapour, by the breath, and other means,—*as though the thing were new.* Years ago, if you look in your own Journal (February 1840, p. 84); Sept. 1840, p. 218; Sept. 1841, pp. 198, 199; you will find that I had published facts of the kind; spectral appearances, that could be revived on metals, glass, and other bodies, by the breath, by vapour of camphor, by mercury vapour, &c. The very purpose for which I described them was the striking resemblance of some of them to Daguerreotype images. I have repeatedly shown, that by placing a coin or any other object on iodized silver, *in the dark*, the vapour of mercury will bring out a representation of it. And in one of the papers just quoted, the condition under which camera images can be reproduced on a silver plate, even after the plate has been rubbed with rottenstone, is described.

I have further seen (Literary Gazette, July 23, 1842, Paris letter) that the fact that light becomes latent in bodies, after the manner of heat, was announced in France as a new and important discovery of Professor Möser of Königsburg. In your own Journal, more than a year ago, you printed a long paper written by me on this very topic (September 1841, pp. 196, 204, 205, 206), not merely announcing the fact, but giving rude estimates of the amounts: more exact numerical determinations I have *now* nearly ready for the press.

But I will trouble you no further with these private matters, simply hoping that your numerous readers, who feel an interest in such things, will turn for themselves to the pages I have quoted.

The accompanying photographic impression of the solar spectrum, which I will thank you to give to Sir John Herschel, was obtained in the south of Virginia:—probably you can make nothing like it in England, the sunlight here in New York wholly fails to give any such result. It proves, that under a brilliant sun, there is a class of rays commencing precisely at the termination of the blue, and extending beyond the extreme red, which totally and perfectly arrest the action of the light of the sky. This impression was obtained when the thermometer was 96° Fahr. in the shade, and the negative rays seem almost as effective in protecting, as the blue rays are in decomposing iodide of silver.

---

* We give among the miscellaneous articles of the present Number, page 409, a report from the Athenæum of what passed at the meeting at Manchester upon this subject.—Ed.

The most remarkable part of the phænomenon is, that the same class of rays makes its appearance again beyond the extreme lavender ray. Sir J. Herschel has already stated, in the case of bromide of silver, that these negative rays exist low down in the spectrum. This specimen, however, proves that they exist at both ends, and do not at all depend on the refrangibility. It was obtained with yellow iodide of silver, Daguerre's preparation, the time of exposure to the sun fifteen minutes.

In this impression, six different kinds of action may be distinctly traced by the different effects produced on the mercurial amalgam. These, commencing with the most refrangible rays, may be enumerated as follows:—1st, protecting rays; 2nd, rays that whiten; 3rd, rays that blacken; 4th, rays that whiten intensely; 5th, rays that whiten very feebly; 6th, protecting rays.

It is obvious we could obtain negative photographs by the Daguerreotype process by absorbing all the rays coming from natural objects, except the red, orange, yellow, and green, allowing at the same time diffused daylight to act on the plate.

This constitutes a great improvement in the art of photography, because it permits its application in a negative way to landscapes. In the original French plan the most luminous rays are those that have least effect, whilst the sombre blue and violet rays produce all the action. Pictures, produced in that way, never can imitate the order of light and shadow in a coloured landscape.

If it should prove that the sunlight in tropical regions differs intrinsically from ours, it would be a very interesting physical fact. There are strong reasons to believe it is so. The Chevalier Fredrichstal, who travelled in Central America for the Prussian government, found very long exposures in the camera needful to procure impressions of the ruined monuments of the deserted cities existing there. This was not due to any defect in his lens; it was a French achromatic, and I tried it in this city with him before his departure. The proofs which he obtained, and which he did me the favour to show me on his return, had a very remarkable aspect. More recently, in the same country, other competent travellers have experienced like difficulties, and as I am informed failed to get any impressions whatever. Are these difficulties due to the antagonizing action of the negative rays upon the positive?

Yours truly,

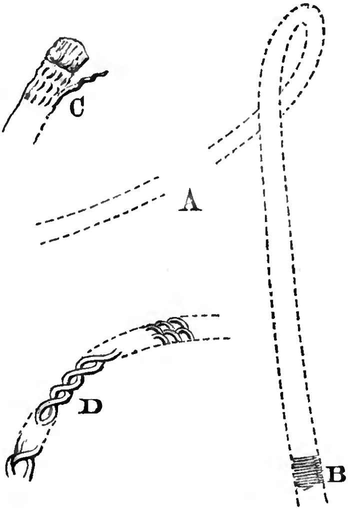
J. W. Draper.

Uuiversity, New York,
Sept. 26, 1842.

LXIII. *Note regarding the Structure of Muscle.*

By Martin Barry, *M.D., F.R.SS. L. and E.*[*]

IN Part I. of the Philosophical Transactions for the present year (p. 99) I mentioned having often seen a muscular *fibril* becoming a *fasciculus*; and gave delineations of fibrils undergoing this change. I happen to have just made a preparation in which the transition is remarkably well seen, and have sketched it (chiefly in outline) in the accompanying figure. At A, the young fasciculus still exhibits the double spiral; while at B, it is so far advanced as to present the usual transverse striæ. Here the striæ are too minute for examination. The preparation is from muscle of a fish. In the fasciculus C (from the Turtle), the transverse striæ are obviously produced by the windings of spiral threads. D represents an enlarged fibril or young fasciculus, varying in its appearance at different parts from being twisted on itself. The preparations themselves are in a state in which they may be viewed by my friends.



It is a striking fact, that the conversion of the fibril into the fasciculus is more frequently met with in the *ever-acting heart*, than in any other part that I have examined. The heart of the Turtle is that which I usually employ and recommend. The muscle may be preserved in very dilute spirit, a drop of which is preferable to water as a medium for the examination. It will be found advantageous to freeze the muscle, as then it is possible, by means of a razor, drawn in the direction of the fibres, to slice off an exceedingly thin lamina, which being thawed, a narrow strip of it should be detached and teased out with needles.

Fibrils are reproduced and multiplied by means of nuclei, which in certain states present the appearance of rows of bead-like particles. These,—the mere elements of spirals,—seem to be what some observers have supposed to represent the structure of the *formed* fibril.

---

[*] Communicated by the Author.

LXIV. *On the Preparation of Artificial Yeast.* *By* George Fownes, *Ph.D.*[*]

It often becomes a matter of great practical importance to have it in our power to excite the vinous fermentation under circumstances in which ordinary yeast cannot be obtained. In making bread, for example, although the use of yeast may be avoided by employing what is called "leaven," or dough which has already become sour and partly putrefied by spontaneous change—a practice which has been followed from the most remote antiquity, and is still occasionally in use—the bread so made is always to be distinguished by a peculiar sour and nauseous taste and smell, and can never bear comparison with that fermented by yeast.

The object of the present notice is to point out a method by which yeast of the most unexceptionable quality can be artificially produced at will. I am aware that some substitute for ordinary ferment in brewing has long been known to certain persons, who go about the country and impart their secret to those who are willing to purchase it: of the nature of this preparation I am ignorant, and a reference to systematic chemical works will suffice to show, that whatever it be it has never been made public.

On turning to Berzelius, it will be found stated[†], that although the reproduction, as it were, of yeast, the conversion of a small into a large quantity, is a very easy thing, yet to produce that substance from the beginning is very difficult. He describes a process for this purpose on the authority of Dr. Henry, and which consists in taking a strong infusion of malt, saturating it with carbonic acid, and then exposing it for some days to the proper fermenting temperature, when a small quantity of yeast is gradually formed and deposited, which may, by various contrivances, be made to give origin to a larger. I shall have occasion to notice presently the behaviour of a malt infusion when left to itself at a temp. of 70° or 80° F. for some time, and to show that the addition of carbonic acid is wholly unnecessary.

The principle of induced chemical action, which Liebig has assumed to explain a great number of those extraordinary phænomena to which Berzelius gave the term "Catalysis[‡]," and which principle has been so fully confirmed, and even, perhaps, extended by the late valuable researches of MM.

---

[*] Communicated by the Chemical Society, having been read March 15, 1842.

[†] Lehrbuch, vol. viii. 89. foot note, third edition.

[[‡] See Phil. Mag., S. 3. vol. x. p. 490.—Edit.]

Boutron and Frémy on the formation of lactic acid, serves to solve this difficulty, as it will doubtless many others of far greater magnitude and importance. It has been shown that "the kind of chemical change going on in the decomposing azotized body or ferment, determines the kind of decomposition which shall occur in the neutral ternary substance, subject to its influence;" that diastase, for example, according to its peculiar condition, whether fresh from the germinated grain, slightly putrefied, or in a still more advanced state of that change, possesses the singular power, in the first case, of changing starch into dextrin, and ultimately into grape sugar; in the second, of causing the conversion of sugar into lactic acid; and in the third and last, of exciting the vinous fermentation.

Now if common wheaten flour be mixed with water to a thick paste, and exposed, slightly covered, to spontaneous change in a moderately warm place, it will be observed to run through a series of changes which seem very closely to resemble those described by MM. Boutron and Frémy in the case of diastase.

About the third day of such exposure it begins to emit a little gas, and to exhale an exceedingly disagreeable sour odour, much like that of stale milk; after the lapse of some time this smell disappears, or changes in character, the gas evolved is greatly increased, and is accompanied by a very distinct and somewhat agreeable vinous odour: this will happen about the sixth or seventh day, and the substance is then in a state to excite the alcoholic fermentation.

A quantity of brewers' wort is next to be prepared in the usual manner, by boiling with hops; and when cooled to 90° or 100°, the decomposed dough before described, after being thoroughly mixed with a little tepid water, is added to it, and the temperature kept up by placing the vessel in a warm situation. After the lapse of a few hours active fermentation commences; abundance of carbonic acid, having its usual agreeable pungent smell, is disengaged, and when the action is complete and the liquid clear, a large quantity of excellent yeast is found at the bottom, well adapted to all purposes to which that substance is applied.

In one experiment the following materials were used:—a small handful of ordinary wheat flour was made into thick paste with cold water, covered with paper, and left seven days on the mantel-shelf of a room where a fire was kept all day, being occasionally stirred: at the end of that period three quarts of malt were mashed with about two gallons of water, the infusion boiled with the proper quantity of hops, and when sufficiently cooled, the ferment added. The results of the experiment were, a quantity of beer, not very strong, it is true, but quite free from any unpleasant taste, and at least a pint of thick barm, which proved perfectly good for making bread.

It appears to me that this simple plan would enable distant residents in the country, and settlers in the colonies, to enjoy the luxury of good bread when a little malt could be got—a very easy home manufacture from grain of any kind: the hops might probably be omitted when the yeast alone was the object.

A moderately strong infusion of malt which has not been boiled, suffered to stand in a warm place for some days, speedily becomes sour and turbid, and begins to evolve gas; this change rapidly progresses, carbonic acid is given out plentifully, and a deposit of thick insoluble whitish matter formed, which readily excites fermentation in a dilute solution of sugar; the supernatant liquid contains alcohol, acetic acid, and, I believe, lactic acid.

When wort which has been boiled and hopped is set aside to decompose spontaneously, the change it undergoes appears to depend very much upon its strength. When weak, three or four days elapse before anything is noticed; a scum then collects upon the surface, and a brown flocculent substance is thrown down, which is incapable of exciting fermentation in a solution of sugar, while the liquid gives off a flat, offensive smell. If the infusion experimented on be stronger, then the change is different: the liquid becomes turbid from the separation of a yellowish adhesive substance, a good deal of gas is very slowly emitted, alcohol is formed, and the deposit at the bottom of the vessel proves a pretty active ferment to sugar. The acidity of the liquid is but trifling, and its smell is somewhat disagreeable. These differences in the behaviour of boiled wort may also depend upon the quantity of hops added and the length of time during which the ebullition had been continued.

The effect produced in a spontaneously fermentable liquid by vegetable acids, or acid salts, such as cream of tartar, is a curious subject of inquiry. From an experiment made upon some wort, it appeared not improbable that the result of such addition showed an interference in the formation of lactic acid. We know that when the juice of grapes, or currants and gooseberries, is exposed to the air, the vinous fermentation is set up apparently at once; whereas in an unboiled infusion of malt, which is destitute of these substances, lactic acid seems to be first formed, although ultimately the two fermentations go on together.

I stated, when speaking of the spontaneous decomposition of wheaten dough, that an acid state preceded that in which it became an alcoholic ferment; and if in this condition it be mixed with a dilute solution of common sugar, and the whole kept warm for several days, it furnishes a sour liquid which is rich in lactic acid, and from which white crystallized lactate of zinc is easily prepared. There is a tendency in the liquid to run into the alcoholic fermentation, and to produce vinegar by a subsequent change, but still the quantity of lactic acid so formed is very considerable.

Common wheat-gluten then in its mode of decomposition strikingly resembles diastase; like that substance it runs in succession through two different dynamic conditions; it is successively a lactic acid and an alcohol ferment; is it too much to expect that it might by proper means be detected in a third condition, namely, as a "sugar ferment," like diastase itself in the state in which it exists in malt? Is it not possible that diastase, as a definite proximate principle, has no more existence than yeast; that its powers are purely dynamic, and that it is, in short, nothing more than the gluten of the seed in one of its earliest stages of decomposition? This is an interesting inquiry, but its prosecution will be somewhat difficult from the rapidity with which these changes succeed each other; it must be remembered that no one has yet succeeded in getting diastase in a state fit for analysis.

LXV. *On some Salts of Cadmium.* By HENRY CROFT, *Esq.**

CHLORIDE of cadmium is exceedingly soluble in water and cannot be obtained in good crystals. If it be treated with a solution of ammonia, it is not at first dissolved; but on heating, the white powder which is at first formed, disappears, and on cooling a granular crystalline powder falls out of the solution. It is a compound of the chloride with ammonia. By heating, it loses 16·63 per cent of ammonia; according to the formula $\mathrm{Cd\,Cl} + \mathrm{H^3\,N}$ it would lose 15·12; the excess obtained is owing to a portion of the chloride being decomposed when sal-ammonia is evolved. The proof of this is that the heated salt is not perfectly soluble in water.

If dry ammonia be passed over pulverised anhydrous chloride of cadmium, the powder increases greatly in bulk under evolution of heat. At first there is but little action, and the stream of ammonia must be passed over the salt for some time before violent absorption takes place. 1·276 gr. absorbed

---

* Communicated by the Chemical Society, having been read May 17, 1842.

0·6835 gr. of ammonia, or 100 parts absorbed 53·56; according to the formula $Cd\,Cl + 3\,N\,H^3$ it would be 56·47: the difference probably arises from the great increase in bulk which the salt undergoes, and which may prevent the ammonia reaching every particle.

This compound loses ammonia when exposed to the air; when it has ceased to smell of ammonia, it is converted into the first-mentioned compound, viz. that containing one atom of ammonia.

Bromide of cadmium crystallizes in long prisms somewhat similar to nitre; it loses its water of crystallization when exposed to a dry atmosphere: 2·422 grs. lost, when heated to 100°, 0·5075 gr. of water; that is, 20·95 per cent; according to the formula $Cd\,Br + 4\,aq$ it should be 21·17: it fuses easily and crystallizes on cooling. Bromide of cadmium dissolves in hot caustic ammonia, and gives on cooling a granular crystalline powder; by slow cooling the salt is deposited in the form of regular octohedrons. It contains 11·69 per cent. of ammonia, or 1 atom, and is therefore analogous to the chloride.

The anhydrous bromide absorbs a large quantity of ammonia, like the chloride, but the quantity varies between two and three atoms*.

All these compounds are decomposed by water, and oxide of cadmium is separated.

The chloride, bromide and iodide of cadmium form very beautiful double salts with the alkaline chlorides, bromides and iodides.

They may be prepared by dissolving the respective salts in atomic proportions.

Cadmio-chloride of potassium.—From the concentrated solution the salts crystallize in silky needles which contain water. If these crystals be allowed to stand in the solution they gradually disappear, and large crystals are formed in their stead; they have the form of regular rhombohedrons; they contain no water. Their formula is $Cd\,Cl + KCl$; the acicular salt contains one atom of water. 100 parts of water at 60° F. dissolve 33·45.

Cadmio-bromide of potassium is precisely similar to the double chloride: it is, however, much more soluble in water. Formula $Cd\,Br + KBr$. The acicular salt contains water.

Cadmio-iodide, &c., does not crystallize like the bromide

---

* In the last number of the Reports of the Academy of Berlin, I find that Rammelsberg has prepared and analysed the crystallized bromide and its compounds with ammonia. That prepared in the dry way contains, as he says, two atoms of ammonia.

and chloride; the anhydrous salt is $CdI + KI$. It is very soluble in water.

Cadmio-chloride of sodium does not crystallize in a regular form, but in verrucose crystals. The formula is $CdCl + NaCl + 3\,aq$. 100 parts of water at 60 dissolve—71·32.

Cadmio-chloride of ammonium crystallizes like the potassium salt in two forms; the large crystals are anhydrous.

All these salts are somewhat soluble in alcohol and wood-spirit, but not so much so as the simple chloride, iodide and bromide.

The analyses of these, as well as some other salts of cadmium, will be published in a second paper.

LXVI. *On the Salt Steppe south of Orenburg, and on a remarkable Freezing Cavern.* *By* Roderick Impey Murchison, *Esq., Pres. G.S.*[*]

THIS salt steppe is distinguished from many of those which are interposed between the Ouralsk and the Volga or are situated on the Siberian side of the Ural Mountains, by consisting not of an uniform flat resembling the bed of a dried-up sea, but of wide undulations and distantly separated low ridges; nevertheless it is, Mr. Murchison states, a true steppe, being devoid of trees and little irrigated by streams. The surface consists of gypseous marls and sands, considered by the author to be of the age of the zechstein[†], and it is pierced in the neighbourhood of the imperial establishment of Illetzkaya Zatchita by small pyramids of rock-salt. These protruding masses attracted the attention of the Kirghiss long before the country was colonized by the Russians, but it is only during a short period that the great subjacent bed has been extensively worked. The principal quarries, exposed to open day, are situated immediately south of the establishment, and have a length of 300 paces, with a breadth of 200 and a depth of 40 feet. The mass of salt thus exposed, is of great purity, the only extraneous ingredient being gypsum, distantly distributed in minute filaments. At first sight the salt seems to be horizontally stratified, but this apparent structure, Mr. Murchison states, is owing to the mineral being extracted in large parallelopipedal blocks twelve feet long, three feet deep and three wide. On the side where the quarry was first worked, the cuttings presented, in consequence of the action of the weather, a vertical face as smooth as glass, but at its base there was a black cavern formed by the water which accumulates at certain periods of the year, and from its roof were saline stalactites.

---

[*] From the Proceedings of the Geological Society, vol. ii. part 2; having been read March 9, 1842.

[†] His extensive surveys of Russia have convinced Mr. Murchison that rock-salt and salt springs occur in all the lower sedimentary rocks of that empire, from great depths below the Devonian or old red sandstone system to the zechstein and the overlying marls and sandstones.

The entire range of this bed of salt is not known, but the mass has been ascertained to extend two versts in one direction, and Mr. Murchison is of opinion that it constitutes the subsoil of a very large area; its entire thickness also does not appear to have been determined, but it is stated to exceed 100 feet. The upper surface of the deposit is very irregular, penetrating, in some places, as already mentioned, the overlying sands and marls.

In consequence of the salt occurring at so small a depth every pool supplied with springs from below is affected by it*; and one of them used by the inhabitants as a bath is so highly charged with saline contents that there is a difficulty in keeping the body submerged, and the skin on leaving the pool is encrusted with salt. This brine swarms with animalcules.

Mr. Murchison then describes the freezing cavern and the phænomena exhibited by it. The cave is situated at the southern base of a hillock of gypsum at the eastern end of the village connected with the imperial establishment; and it is one of a series of apparently, for the greater part, natural hollows, used by the peasantry for cellars or stores. The cave in question is, however, the only one which possesses the singular property of being partially filled with ice in summer and of being destitute of it in winter. "Standing on the heated ground and under a broiling sun, I shall never forget," says the author, "my astonishment when the woman to whom the cavern belonged unlocked a frail door and a volume of air so piercingly keen struck the legs and feet that we were glad to rush into a cold bath in front of us to equalize the effect." Three or four feet within the door and on a level with the village street, beer and quash were half frozen. A little further the narrow chasm opened into a vault fifteen feet high, ten paces long, and from seven to eight wide, which seemed to send off irregular fissures into the body of the hillock. The whole of the roof and sides were hung with solid undripping icicles, and the floor was covered with hard snow, ice, or frozen earth. During the winter all these phænomena disappear, and when the external air is very cold and all the country is frozen up, the temperature of the cave is such that the Russians state they could sleep in it without their sheep-skins.

In order to lay before the Society an explanation of these curious opposite conditions of the cave, the author communicated with Sir John Herschel and received the documents which follow this abstract. With respect to the observations in Sir J. Herschel's letter, Mr. Murchison says, he does not conceive that the ice caverns at Teneriffe, in Auvergne and elsewhere are analogous cases with that at Illetzkaya Zatchita, the frozen materials in the last not arising from the preservation of the snow or ice of the preceding winter, but from the

---

* The abundance of these brine-springs in various parts of Russia must lead, the author says, to the abandonment of Pallas's hypothesis, that the saline pools and lakes are the residue of former Caspians; though he admits that some of the vast low steppes of the South formed the bottom of a former condition of the existing Caspian.

peculiar condition of the cavern during the hottest summer months. He states also that he particularly urged the authorities at Orenburg as well as the directors of the Salines to keep accurate registers of the temperature throughout the year, and to ascertain precisely the changes which the cave undergoes between the extremes of summer and winter. There is, he observes, a very marked difference between the climate of the steppes south of Orenburg and that of Ekaterinburg, not merely due to the difference of six degrees of latitude, but arising also from the altitude of the position of Ekaterinburg and the shortness of its varying summers as well as from the long droughty summers of the steppes, which are removed from all mountain chains, and possess comparatively no great altitude above the sea. In the southern region, he conceives, a substratum of frozen matter cannot exist, there being a most extraordinary difference between the climate of Yakatsk (lat. 62½° N. long. 131° E.) and that of Orenburg (lat. 51° 46′ N.), the winter of the former lasting eight or nine months, with the thermometer during long periods constantly 30° and sometimes 40° of Reaumur below zero*.

Respecting the explanation that the difference of temperature in the cave is due to the propagation through the gypsum hillock of the heat or cold of the preceding summer or winter season, Mr. Murchison conceives that the fissures which ramify from the cave into the hill, present difficulties to such a solution. When he was on the spot, the existence of these fissures led him to speculate upon the possibility of the phænomena being due to currents of air passing over subterranean floors of moistened rock-salt, and on the effects which would be produced when such currents came in contact with a stream of dry heated air.

LXVII. *Extracts from a letter addressed by* Sir J. HERSCHEL, *Bart., F.G.S., to* Mr. MURCHISON, *explanatory of the Phænomena of the Freezing Cave of Illetzkaya Zatchita*†.

"THAT the cold in ice-caves (several of which are alluded to in a part of this letter not published) does NOT *arise from evaporation*, is, I think, too obvious to need insisting on. It is equally impossible that it can arise from condensation of vapour, which produces heat, not cold. When the cold (by contrast with the external air,

---

* Mr. Murchison ascertained during his journey in the North of Russia in 1840, that much remains to be done relative to the circumstances of the recorded frozen substratum at Yakatsk; and he states the following as points requiring attention. 1st. With the exception of about sixty feet of alluvial soil, the whole shaft to a depth of 350 feet was sunk through solid strata of limestone two to six feet thick, and shale with a little coal; 2ndly, That none of the sinkings took place in summer although renewed for several years, on account of the foul air generated in the shaft; 3rdly, That when Admiral Wrangel descended the shaft during the summer, and the surface was burnt up, he found the thermometer to stand at 6° Reaum. below zero.

† From the Proceedings of the Geological Society, vol. ii. part 2; having been read March 9, 1842.

i. e. the difference of temperature) is greatest, the reverse process is going on. Caves in moderately free communication with the air are dry and (to the feelings) warm in winter, wet or damp and cold in summer. And from the general course of this law I do not consider even your Orenburg caves exempt, since however apparently *arid* the external air at 120° Fahr. ! may be, the moisture in it may yet be in excess and tending to deposition, when the same air is cooled down to many degrees beneath the freezing point.

"The data wanting in the case of your Orenburg cave are *the mean temperature of every month in the year of the air*, and of thermometers buried say a foot deep, on two or three points of the surface of the hill, which if I understand you right is of gypsum and of small elevation. I do not remember the winter temperature of Orenburg, but for Catherinenburg (only 5° north of Orenburg), the temperatures are given in Kuppfer's reports of the returns from the Russian magnetic observatories. If anything similar obtains at Orenburg I see no difficulty in explaining your phænomenon. Rejecting diurnal fluctuations and confining ourselves to a single summer wave of heat propagated downwards alternately with a single winter wave of cold, every point at the interior of an insulated hill rising above the level plain will be invaded by these waves in succession (converging towards the centre in the form of shells similar to the external surface), at times which will deviate further from mid-winter and mid-summer the deeper the point is in the interior, so that at *certain depths* in the interior, the cold-wave will arrive at mid-summer and the heat-wave in mid-winter. A cave (if not very wide-mouthed and very *airy*) penetrating to such a point will have its temperature determined by that of the solid rock which forms its walls, and will of course be so alternately heated and cooled. As the south side of the hill is *sunned* and the north *not*, the summer wave will be more intense on that side and the winter less so; and thus though the *form* of the wave will still generally correspond with that of the hill, their *intensity* will vary at different points of each wave-surface. The analogy of *waves* is not strictly that of the progress of heat in solids, but nearly enough so for my present purpose.

"The mean temperature for the three winter months, December, January, February, and the three summer months, June, July, August, for the years 1836, 7, 8, and the mean of the year, are for Catherinenburg as follows:—

	Winter.	Summer.	Annual Mean.
1836.	− 10°·93 R.	+ 11°·90 R.	+ 1°·22 R.
1837.	− 12°·90	+ 12°·93	+ 0°·30
1838.	− 12°·37	+ 12°·37	+ 0°·60
Mean.	− 12°·07 R.	+ 12°·40 R.	+ 0·70 R.
	+ 4°·83 Fahr.	+ 59°·9 Fahr.	+ 33°·57 Fahr.

"The means of the intermediate months are almost exactly that of the whole year, and the temperature during the three winter as well as the three summer months most remarkably uniform.

"This is precisely that distribution of temperature over time which ought under such circumstances to give rise to well-defined and intense waves of heat and cold; and I have little doubt therefore that this is the true explanation of your phænomenon.

"I should observe, that in the recorded observations of the Catherinenburg observatory, the temperatures are observed two-hourly, from eight A.M. to ten P.M., and not at night. The mean monthly temperatures are thence concluded by a formula which I am not very well satisfied with; but the error, if any, so introduced must be far too trifling to affect this argument. The works whence the above data are obtained are, '*Observations Météorologiques et Magnétiques faites dans l'intérieur de l'Empire de Russie,*' and '*Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie,*' works which we owe to the munificence of the Russian government, and which it is satisfactory to find thus early affording proofs of utility to science in explaining what certainly might be regarded as a somewhat puzzling phænomenon, as it is one highly worthy of being further studied and being made the subject of exact thermometric researches on the spot, and wherever else anything similar occurs."

Sir John Herschel then states, that since he began this letter he had examined some old documents and found the paper which accompanied his letter. "The date of this manuscript," he adds, "as nearly as I can collect it from collateral circumstances, must have been somewhere about the year 1829, or rather before than after.

"I remain, &c.,

"J. F. W. HERSCHEL.

"P.S. Thermometric observations in the Steppes, of the mean monthly temperature of the soil at different depths from one to 100 feet (at Forbes's intervals), would be most interesting. At Catherinenburg the mean temperature of the air being 33°·6 Fahr., no *permanently frozen soil* would probably be reached, but a very little more to the northward that phænomenon must occur.

"The 'thinning out' of the frozen stratum would be most interesting to trace, but in thinning out by decrease of latitude it might possibly at the same time 'dip' beyond reach, all above it being occupied by soil subject to the law of periodic frost and thaw, and giving room under favourable circumstances to ice-caverns, pits, or galleries. What determines the distinct definition of the *hot* and cold alternating layers is the exceedingly peculiar form of the curve of the monthly temperatures as given in the tables above referred to."

LXVIII. *On some Phænomena observed on Glaciers, and on the internal temperature of large Masses of Ice or Snow, with some Remarks on the natural Ice-caves which occur below the limit of perpetual Snow.* *By* Sir JOHN HERSCHEL, *Bart., F.G.S., &c.*[*]

IN a visit to the glacier of Chamouni in the summer of 1821, I was struck with the very remarkable positions of several large blocks of granite resting on the glacier in various parts. They were perched on stools of ice of less diameter than the blocks themselves, which overhang their supports on all sides, as a mushroom does its stalk. The position of these large masses was rendered the more striking when contrasted with that of small fragments of stone, equally (to appearance) exposed to all the local heating and cooling influences, but which were uniformly found to have sunk into the ice, and that the deeper, (within certain limits) the less their size. On consideration, the cause became apparent, and, as it affords a very pretty illustration of the laws of the propagation of heat through bad conductors, and the steps by which an average temperature is attained in large masses from a varying source, I will here state it as it occurred to me at the time.

With regard to the sinking of small masses into the ice when heated by the sun, it is the natural effect of the greater power of absorbing heat which stone possesses beyond ice. Whenever the sun shines, the stone will detain more of its heat than an equal surface of ice would do; and as it gives this out to the ice below *nearly as fast as it receives it*, a greater depth of ice is melted in a given time beneath the stone than in the parts around. On the other hand, at night, ice radiates *terrestrial* heat nearly or quite as copiously as stone, and thus they are on a par in frigorific power.

The elevation of great masses above the general level, which at first sight would appear to contradict this explanation, is however equally a consequence of the laws of the propagation of heat. To conceive this, let us imagine a very large block of stone, at the commencement of the summer, to lie on a level surface of ice, in a situation exposed to the direct rays of the sun, where the *mean temperature of day* and *night* is (even in summer) but little above the freezing-point, but where, however, no fresh snow falls during the whole summer. In the day time then, while receiving the sun's rays, the upper surface of the stone will be strongly heated, and a *wave of heat* will be propagated slowly downwards through the stone towards the ice, diminishing in intensity rapidly, however, as it travels, since each superior stratum only divides its excess of temperature with that below. Long before this can reach the ice, however, night comes on. The surface cools below the mean or even below the actual temperature of the air by radiation, and a *wave of cold* is propagated (or, which comes to the same thing, *heat is abstracted* from stratum to stratum) by the same laws. This fol-

---

[*] From the Proceedings of the Geological Society, vol. ii. part 2; having been read March 9, 1842.

lows close on the wave of heat below and travels with equal velocity. In consequence, the heated stratum parts with its heat, now, both upwards and downwards, and thus the intensity of the wave of heat diminishes with much greater rapidity as it proceeds downwards. It is manifest, that were the thickness of the stone infinite, the wave of heat being *always* followed *close* up by the wave of cold, and a perpetual tendency to an equilibrium of temperature going on between them, they would ultimately reduce each other to their mean quantity and (not to take the extreme case of infinity) at some very moderate depth, the fluctuations above and below the mean temperature of the air, as the successive nocturnal and diurnal waves pass through a particle of the stone there situated, will be rendered very trifling, and may for our present purpose be regarded as evanescent. Beyond this depth, whatever mass of stone may exist, may be regarded as a slow conducting mass, interposed between a surface of ice constantly maintained at 32°, and a surface of stone constantly maintained at the mean temperature of the air, which by hypothesis is very little above it. Through this then the heat will percolate uniformly but feebly, and the ice below will be very slowly melted, and the more so in proportion to the thickness of the interposed stratum. Let us now consider what happens to the ice on the parts undefended by the stone. In the day time these experience the direct radiation of the sun, and therefore melt and run off in water. At night, it is true, the remaining surface cools by radiation; but this cold is propagated downwards, and on the return of day the *superficial lamina* is necessarily put in equilibrium with the air and melted by the sun, and however cold the interior of the mass may be, the surface will still be kept all day in a state of fusion. Thus the degradation of the general surface of the ice will be in proportion to the direct intensity of the sun's rays and the time they shine, while that of the surface beneath the stone will only be in proportion to the excess of the mean temperature of day and night above 32°, diminished by the effect of the thickness of the stone. This of course will produce a difference of level, and a *relative* elevation of the stone sunk as really observed. One curious and, at at first sight, paradoxical consequence seems to follow from this reasoning, viz. that the ice of a glacier, or other great accumulation of the kind, may, at some depth beneath the surface, have a permanent temperature very much below freezing, though in a situation whose mean *annual* temperature is sensibly above that point. In fact (continuing to use the metaphorical expression already employed), there is no reason why *waves of cold*, of any intensity below 32°, may not be propagated downwards into the interior of the ice; but waves of heat above that point, of course, never can. Thus, the cold of winter and the frost produced by radiation in the clear nights of summer, will enter the mass and lower its internal temperature, while the heat of the summer air and that imparted by solar radiation will mainly be employed in melting the surface, and will run off with the water produced.

I am not aware of any observations on the internal temperature of glaciers—they are of course difficult from their usual rifty state; but the point may not be unworthy the attention of the scientific traveller. May not this be the cause of those natural formations of ice which have been observed in caverns, in Teneriffe, and on some elevated points of the Jura chain, below the level of perpetual snow? It is obviously no matter whether the interior mass in the above reasoning be ice or rock. It is enough, that its surface, during the whole or great part of the year, should be covered with ice to bring down the mean annual temperature of its interior materially below the temperature due to its elevation, and which it *would* have were it not so covered. Conceive now a mountain whose summit is in this predicament, viz. constantly maintained at a *mean* temperature below that due to its elevation. This intense cold will not break off at the level of the line of perpetual snow, which is determined by the mean temperature of the atmosphere due to elevation, but will be propagated downwards in the interior of its mass. Hence, if at a short distance below the line of perpetual snow, where the mean diurnal temperature of the exposed part, taken at a few feet or a few yards deep in the soil or rock, is a little above freezing, we drive an adit, or take advantage of a natural fissure to obtain the internal temperature at a much greater depth from the surface; we ought to find it below 32°, and ice ought constantly to form in such cavities.

But even when the summit of a hill is not covered with ice, and when therefore this particular principle does not apply, it is easy to see, on the same general grounds, that something of the same kind may obtain. It is obvious, that whenever a change of temperature on the surface of a solid takes place, *a wave* of heat or cold, as the case may be, will be propagated through its substance; and if the changes be regularly periodic, the waves will be also. Moreover it is clear that the longer the periods of the external fluctuations are supposed, the greater will be the interval of the waves, so as to make the time taken for the propagated heat to run over them precisely equal to the period of fluctuation. Now the rapidity with which successive waves of heat and cold destroy each other, is inversely as the intervals, and thus the fluctuations of temperature depending on long periods of external change will be propagated to greater depths than those arising from shorter periods, nearly in the ratio of the lengths of the periods. Thus the depths at which the annual fluctuations of temperature cease to be sensible, will be between 300 and 400 times greater than those at which the diurnal ones are neutralized. Now it may happen, from the slowness of propagation through so considerable a depth, that the winter wave of cold (consisting of many diurnal waves of alternate, greater and less intensity) may not travel down to the adit or cavern till the hottest period of the next summer, or of many summers; in short, that if at any given time the interior of the mountain were *sounded* by thermometers down its whole axis, these instruments would exhibit alternate deviations + and − from the mean temperature of the air.

LXIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 309.]

Dec. 1, 1841. A paper was first read, entitled, "Report of the Destruction by Earthquake of the Town of Praya de Victoria, on the 15th of June, 1841." By Mr. Consul Hunt; communicated by direction of the Right Hon. the Foreign Secretary of State.

The town of Praya stood at the east end of the island of Terceira, and contained 562 houses; near it were the villages of Lageas (523 houses), Villa Nova (206 houses), Agoalva (244 houses), Fontinha (203 houses), and Fonte do Bastardo (144 houses), the total population being about 9000 souls. The town of Praya had been on a former occasion (1614) totally destroyed by an earthquake, and Angra, the capital of the island, situated twelve English miles distant, was considerably injured, the shocks being severely felt in the island of St. Michael. Although menaced during many earthquakes, Praya had escaped injury from that time till the 12th of June 1841, when, at 4 P.M., a violent shock was felt, and with diminished force to the westward. At twenty-five minutes past five, a second, more powerful shock was experienced, and throughout the 13th of June, tremblings were felt at short intervals. At 4 A.M. on the 14th a perfectly perceptible undulation destroyed all those buildings which had been previously weakened, but during the remainder of that day the island was visited by only occasional slight shocks. On the 15th, at 3 A.M., violent tremblings and horizontal undulations of the ground commenced, and continued, with intervals of ten minutes, and a duration of about 10 seconds, until 30 minutes past 3 o'clock, when a strong, vibrating and distinctly visible rocking motion was communicated to the surface, and threw down the undestroyed portion of Praya, several churches and houses of the adjacent villages, and considerably injured the remainder, as well as many elevated public buildings in other parts of the island. The ground then remained comparatively at rest until 40 minutes past 2 A.M. on the 16th, when a violent earthquake did further damage; but from that period no additional injury was sustained, though the island did not resume a permanently quiescent state till the 26th of June. The number of houses thrown down is estimated to be 800, but several others must be rebuilt, and of the remainder the greater number require extensive repairs.

During the whole of these earthquakes the motion was greatest at Praya, diminishing in force to the westward, and every convulsion was preceded by a loud subterranean or submarine noise to the eastward of Terceira, which so exactly varied in intensity with the force of the succeeding shocks, that the noise became not only the harbinger but the measure of the severity of the earthquake. A rent an English mile in length was formed in the ground, extending from the shore to the westward.

The less severe shocks were not felt beyond Terceira: others were experienced, of apparently equal force, at St. George's, about fifty miles to the south-west, and at Graciosa, about the same distance to the north-west of Praya; but only the earthquake which destroyed that town was felt, though not powerfully, at the capitals of Pico, sixty-eight miles south-west, and of St. Michael's, the same distance to the south-east. At Fayal, eighty-five miles west by south, and at the eastern end of St. Michael's, 105 miles south-east by east, no motion was perceived, as far as Mr. Consul Hunt had been able to ascertain. If the shocks felt about 30 minutes past 3 o'clock on the morning of the 15th of June, in the several islands, be divided into four degrees of intensity, each interval, the author says, will be found to contain a distance of about seventeen miles, the eastern end of Terceira being on the first degree, or seventeen miles from the centre of eruption; the western end thirty-four miles; Graciosa and St. George's fifty-one, and the capitals of Pico and St. Michael's sixty-eight miles. The latter places, equally distant from the centre of eruption, experienced shocks of equal degrees of diminished force.

Mr. Consul Hunt then alludes to Buffon's notice of submarine explosions between St. Michael's and Terceira, attended by earthquakes in those islands, and the appearance of newly formed islets; also to the throwing up of Sabrina, near St. Michael's, in 1811*, the effects of which were powerfully felt in that island, but not in Terceira, fifty miles distant; and, on account of these phænomena, he, in conclusion, advises mariners to keep a sharp look-out for shoal water on approaching Terceira from the eastward.

A paper, entitled "Some Geological Remarks made in a Journey from Delhi, through the Himalaya Mountains, to the frontier of Little Thibet, during 1837," by the Rev. Robert Everest, F.G.S., was then read.

The author's route, after quitting Delhi, lay through Seharunpore, the Keeree pass in the Sevalik hills, and Mussoori to the Jumna, thence nearly north-west to the valley of the Paber, as far as Rooroo, where it quitted the course of that river and crossed the mountain range to Rampore. It then ascended the valley of the Sutluj to the Leo River, and terminated near the Khealkhur Fort, on the frontier of Little Thibet. The country consists of alluvial deposits, the tertiary strata of the Sevaliks, a vast sandstone deposit, an extensive clay-slate formation containing limestone and sandstone, various metamorphic rocks, greenstone and granite.

Delhi is situated on the most northern promontory of an extensive sandstone formation, which stretches many miles in a south-west and south-east direction, following the course of the Jumna, and resembles, in mineral characters, the transition quartzose sandstones of Europe. It alternates, though rarely, with layers of soft talc slate, and a few miles to the southward of Delhi with clay slate. To the south-west, a little beyond Goongony, and in other localities, sienitic

---

[* An original letter on the elevation of Sabrina appeared in Phil. Mag., S. 1. vol. xxxviii. p. 229, and a reprint, from the Philosophical Transactions, of Capt. Tillard's narrative respecting it, in vol. xxxix. p. 451.—Edit.]

rocks are connected with it. No fossil remains have been discovered in the formation. At Delhi the strata are highly inclined towards the east-south-east.

From Delhi to beyond Seharunpore, a distance of more than 100 miles, the surface of the country consists of a fine sandy soil, and contains nodules of kunkur, similar to alluvial granitic or primary detritus brought down by the Jumna. Beyond Seharunpore the tertiary beds of the Sevalik range commence; but Mr. Everest alludes to their mammalian remains only for the purpose of remarking, that no portions of the wild elephant, which now abounds in that district, have been found in the tertiary strata; and he quotes, as an analogous case, the absence of the bones of the Asiatic elephant in the mammalian deposits of the Irawaddi. From these facts he infers that the present species did not co-exist with the *Elephas primigenius*, the mastodon, or the associated mammifers.

The chain of the Himalayas, which rises like a black wall on the opposite of the valley of the Dhoon, or that which separates it from the Sevalik hills, consists, where crossed by the author (about 77° 55′ E. long.), of strata highly inclined to the north-east, and composed of dark blue or variegated clay slate, sometimes sufficiently hard to be used for roofing slates, but generally soft, of compact, dark blue and black carbonaceous limestone, and of highly consolidated quartzy sandstone resembling that near Delhi. No organic remains have been noticed in these beds. Dykes of greenstone containing diallage were observed by the author.

From Mussoori* (lat. 30° 25′, long. 77° 55′ E.), Mr. Everest descended to the Jumna, over beds similar to those just described, and of slate containing angular fragments. In the bed of the river the strata are very much disturbed. Beyond the Jumna the rocks consist of purplish clay slate, often passing into quartz slate and talc slate. The general dip is to the north-east, but the angle of inclination is stated to vary from nearly horizontal to vertical. Beyond the village of Luchwarree, not far from the Jumna, occur blocks of greywacke similar to those observed in the descent to that river. Thence to the heights of Deobun, the most lofty point between the Jumna and the Tonse (lat. about 30° 47′, long. about 77° 48′ E.), the strata present little variety, but the last 2000 feet of ascent consist of rugged, black and grayish blue limestone, similar to that at Mussoori. The descent towards the Tonse exhibits slates similar to those previously described, dipping between north and east. They are occasionally intersected by greenstone containing pistacite, and passing in some places into hornblende slate and serpentine. At the village of Kundah, before reaching the Tonse, limestone reappears, highly inclined to the north-east, and extends to the bridge. The bed of the Tonse, and of its tributary the Paber, are filled with boulders of gneiss, and they occur at heights of 200 feet above those rivers. The slate rocks, in ascending the river-valleys, change in

---

* The degrees of latitude and longitude given in this abstract must be considered only as approximations.

their composition from that previously exhibited; containing, first, frequently nodules and layers of quartz, and, though rarely, of felspar, and afterwards passing into well-defined gneiss; and still further, as at Raeenghur and Rooroo, different varieties of gneiss alternate with talc-slate, quartzose slate and mica slate. This progressive change, from the party-coloured earthy slates of Mussoori to crystalline schists, on approaching the higher ranges of mountains covered with perpetual snow, perfectly accords, Mr. Everest states, with what he had previously observed in two journeys to the sources of the Ganges and the Jumna. The dip of the beds in the valleys of the Tonse and Paber is to the north-east.

At Rooroo Mr. Everest quitted the course of the Paber and crossed the mountain range to the valley of the Sutluj. The highest point which he attained on this ridge was only 8000 feet above the level of the sea, and it was then, the middle of April, nearly free from snow. From the view which this pass afforded, the author ascertained that the country shelves or declines from the north-east to the south-west, the mountains between the north and east rising far above the limits of forests and being white with snow, while among those to the westward or southward few peaks appeared above the range of forests, and little snow was seen. The rocks composing this mountain range consist near Rooroo of mica slate, with a very slight dip to the east and south-east, but the inclination of the beds in ascending towards the pass becomes considerable, but in the same direction. North of Kersole (lat. 31° 25′, long. 77° 33′ E.) gneiss appears dipping south and south-east, and approaching occasionally granite in character. This rock ranges half way to the Sutluj, where black, compact limestone, and black, glimmering, soft slate are exposed. Near the junction of the Nuggur with the Sutluj, strata of crystalline, white quartz slate dip to the south, and are traversed by a mass of greenstone, which first rises vertically through the strata, then passes horizontally between them, and finally bursts upwards and projects above the surface. Where the position of the greenstone conforms to the bedding of the slate, the laminæ of mica and hornblende assume a similar arrangement, and where the greenstone intersects the slate, those minerals have a position vertical to it. A gradual passage from greenstone into the quartz slate was likewise noticed by the author. About two miles below Rampore (lat. 31° 34′, long. 77° 30′), in the valley of the Sutluj, quartz slate alternates with chlorite slate and talc slate, the dip being to the west and south-west at a considerable angle. Above Rampore the rocks first consist of alternations of white quartz slate and clay slate, the strata being much disturbed; and afterwards of talc slate associated with greenstone or hornblende rock, dipping north-east. Before reaching Seran, gneiss containing kyanite appears, and extends with occasionally intervening masses of granite to Nasher (lat. 31° 47′, long. 77° 46′ E.). On the opposite side of the river at that place are precipices of slate traversed by white veins; but at the bridge, a large-grained white granite with tourmalines appears, and extends, in connexion with mica slate and gneiss intersected by granite veins, seven days' journey to Akbah (lat. 31° 56′, long. 78° 8′ E.). At this village granite also occurs, but separated from that rock by a narrow ravine is a low promontory of clay slate and dark flinty slate dipping to the north. Beyond Akbah the Sutluj bends to the north, and on both sides of the river the outline of the rocks is considerably softened in consequence of their being evidently composed of perishable clay slate similar to that at Mussoori; but in the more distant ranges, granite, mica slate and gneiss may be detected by the rugged outline and the great height of the rocks. This clay slate, Mr. Everest says, is not of later origin than the granite and crystalline schists, because it is penetrated by veins of granite which may be traced to the great masses of that formation. The dip of the slate on one side of the river is west, and on the opposite apparently east. Beyond Lipi, a few miles from Akbah, are precipices of clay slate, talc slate, and dark flinty slate interstratified with greenstone. After quitting Khanum the country becomes still more desolate, and the strata consist, first of earthy slate, in some places carbonaceous, in others brecciated, then of greyish green highly consolidated greenstone, and afterwards of masses of blackish and brownish grey compact limestone. The valley of the Namkulling, a small tributary of the Sutluj, presents a fine section of these strata, the upper part being composed of the limestone and the lower of the slate. The dip from Khanum is between west and south-west. From Seenum (lat. 32° 5′, long. 78° 16′ E.) Mr. Everest proceeded across the Hungung pass, 14,837 feet above the sea. The ground being covered with snow, little of the structure of the country was visible, but projecting strata of reddish brown compact limestone appeared on the crest of the hill. The view northward presented bare rocks as far as the eye could reach, but from the softness of the outlines, Mr. Everest infers, that the strata belong to secondary or tertiary deposits. Rugged ridges of primary rocks resembling dykes cross this dreary expanse. Beyond Hango (lat. 32° 12′, long. 38° 18′ E.) beds of reddish and greenish grey compact limestone alternate with earthy and carbonaceous shale, the dip being to the north-west, and blocks of greyish quartzose sandstone are scattered over the surface. These appearances extend to the heights above Leo, where the earthy shales are traversed by veins and layers of granite, and at the point of contact are changed into mica slate. In the descent to the village, nearly 2000 feet, the granite veins gradually increase in number, predominating in the lower parts; and the associated rocks become more and more crystalline, so that near the river nothing is visible but mica slate, gneiss, quartz slate and granular limestone, the strata dipping to the south-west. Beyond Leo (lat. 32° 18′, long. 78° 17′ E.) the road ascends over granite and dark mica slate, containing kyanite and pistacite; but the earthy strata are stated to occur at higher levels. On the opposite side of the river is a section several thousand feet in vertical dimensions intersected by a net-work of granite veins and crossed by black stains derived from the carbonaceous layers. On opening on the hollow in which the village of Change is situated earthy strata again appear. This point was the boundary of Mr.

Everest's journey, and he was prevented from examining the locality which produces the Ammonites and other fossils obtained by Dr. Gerard; but he believes, from the information supplied by the natives, that they are met with abundantly beyond the frontier, imbedded in black compact limestone and earthy carbonaceous shale. Mr. Everest further states, that since his journey Captain Hutton has discovered them within the frontier.

In the course of the memoir the author mentions having seen at Seenum the skin of a "leopard" recently killed near the village, though large quantities of snow were then (May) lying upon the ground, and that he has frequently observed in February and March their tracks on the snow as high as the limit of the forests. He also states that he has observed monkeys at the height of full 8000 feet above the sea in the same months when the ground was covered deep with snow, feeding in great numbers on the seeds of the fir cones.

A paper was afterwards read containing a "Description of the Remains of Six Species of Marine Turtles (*Chelones*) from the London Clay of Sheppey and Harwich." By Richard Owen, Esq., F.R.S., F.G.S., Hunterian Professor in the Royal College of Surgeons.

The author commences by quoting the generalizations given in the latest works which treat of Fossil Chelonians, and examines the evidence on which those from the Eocene clay of Sheppey had been referred exclusively to the freshwater genus *Emys* by Cuvier and others, and he points out the circumstances which invalidate the conclusions that had been deduced from it. He then proceeds to describe the fossils and to show the characters by which he has established the existence of five species of marine turtles from the London Clay at Sheppey, and a sixth species from the same formation near Harwich.

1. *Chelone breviceps.*—The first species, found at Sheppey, is called by the author *Chelone breviceps*, and its unequivocal marine nature was recognised by a nearly perfect cranium, wanting only the occipital spine, and presenting a strong and uninterrupted roof, extended from the parietal spine on each side over the temporal openings; the roof being formed chiefly by a great development of the posterior frontals. Further evidence of its marine origin exists in the large size and lateral aspect of the orbits, their posterior boundary extending beyond the anterior margin of the parietals; also in the absence of the deep emargination which separates the superior maxillary from the tympanic bone in freshwater tortoises, especially the *Emys expansa*.

In general form the skull resembles that of the *Chelone Mydas*, but it is relatively broader, the anterior frontals are less sloping, and the anterior part of the head is more vertically truncate: the median frontals also enter into the formation of the orbits in rather a larger proportion than in *C. Mydas*. In *Chelone imbricata* they are wholly excluded from the orbits.

The trefoil shape of the occipital tubercle is well-marked; the laterally expanded spinous plate of the parietal bones is united by a straight suture to the post-frontals along three-fourths of its extent, and for the remaining fourth with the temporal or zygomatic element.

These proportions are reversed in the *Emys expansa,* in which the similarly expanded plate of the parietals is chiefly united laterally with the temporal bones. In other freshwater tortoises the parietal plate in question does not exist.

The same evidence of the affinity of the Sheppey Chelonite in question to the marine turtles is afforded by the base of the skull:—the basi-occipital is deeply excavated; the processes of the pterygoids which extend to the tympanic pedicles are hollowed out lengthwise; the palatal processes of the superior maxillary and palatine bones are continued backwards to the extent which characterizes the existing Cheloniæ; and the posterior or internal opening of the nasal passages is, in a proportional degree, carried further back in the mouth. The lower opening of the zygomatic spaces is wider in the Sheppey Chelonite than in the *Emys expansa.*

The external surface of the cranial bones in the fossil is broken by small irregular ridges, depressions, and vascular foramina, which give it a rough shagreen-like character.

The lower jaw, which is preserved in the present fossil, likewise exhibits two characters of the marine turtles; the dentary piece, *e. g.*, forms a larger proportion of the lower jaw than in land or freshwater tortoises. The under part of the symphysis, which is not larger than in *Chelone Mÿdas,* is slightly excavated in the fossil.

In the rich collection of Sheppey fossils belonging to Mr. Bowerbank, there is a beautiful Chelonite, including the carapace, plastron, and the cranium, which is bent down upon the forepart of the plastron; and which, though mutilated, displays sufficient characters to establish its specific identity with the skull of the *Chelone breviceps* just described. The outer surface of the carapace and plastron has the same finely rugous character as that of the cranium, in which we may perhaps perceive a slight indication of the affinity with the genus Trionyx.

The carapace is long, narrow, ovate, widest in front, and tapering towards a point posteriorly; it is not regularly convex, but slopes away, like the roof of a house, from the median line, resembling in this respect, and its general depression, the carapace of the turtle. There are preserved eleven of the vertebral plates, the two last alone being wanting. The eight pairs of expanded ribs are also present, with sufficient of the narrower tooth-like extremities of the six anterior pairs to determine the marine character of the fossil, which is indicated by its general form. Other minute characters are detailed; and a comparison with the Chelonite from the tertiary beds near Brussels, figured by Cuvier, is instituted.

The sternum of the *Chelone breviceps,* although more ossified than in existing Cheloniæ, yet presents all the essential characters of that genus. There is a central vacuity left between the hyosternals and hyposternals; but these bones differ from those of the young Emys in the long pointed processes which radiate from the two anterior angles of the hyosternals, and the two posterior angles of the hyposternals.

The xiphisternals have the slender elongated form and oblique union by reciprocal gomphosis with the hyposternals, which is characteristic of the genus Chelone.

The posterior extremity of the right episternal presents the equally characteristic slender pointed form.

With these proofs of the sternum of the present fossil being modified according to the peculiar type of the marine Chelones, there is evidence, however, that it differs from the known existing species in the more extensive ossification of the component pieces: thus, the pointed rays of bone extend from a greater proportion of the margins of the hyo- and hyposternals, and the intervening margins do not present the straight line at right angles to the radiated processes.

In the *Chelone Mydas*, for example, one half of the external margin of the hyo- and hyposternals, where they are contiguous, are straight, and intervene between the radiated processes, which are developed from the remaining halves; while in the *Chelone breviceps* about a sixth part only of the corresponding external margins are similarly free, and there form the bottom, not of an angular, but a semicircular interspace.

The radiated processes from the inner margins of the hyo- and hyposternals are characterized in the *Chelone breviceps* by similar modifications, but their origin is rather less extensive; they terminate in eight or nine rays, shorter and with intervening angles more equal than in existing Chelones. The xiphisternal piece receives in a notch the outermost ray or spine of the inner radiated process of the hyposternal, as in the Chelones, and is not joined by a transverse suture, as in the Emydes, whether young or old.

The characters thus afforded by the cranium, carapace, plastron, and some of the bones of the extremity, prove the present Sheppey fossil to belong to a true sea-turtle; and at the same time most clearly establish its distinction from the known existing species of Chelone; from the shortness of the skull, especially of the facial part as compared with its breadth, the author proposes to name this extinct species *Chelone breviceps*.

2. *Chelone longiceps*.—The second species of Sheppey turtle, called *Chelone longiceps*, is founded upon the characters of the cranium, carapace, and plastron. The cranium differs more from those of existing species, by its regular tapering into a prolonged pointed muzzle, than does that of the *Chelone breviceps* by its short and truncated jaws.

The surface of the cranial bones is smoother; and their other modifications prove the marine character of the fossil as strongly as in the *Chelone breviceps*.

The orbits are large, the temporal fossæ are covered principally by the posterior frontals, and the exterior osseous shield completely overhangs the tympanic and ex-occipital bones. The compressed spine of the occiput is the only part that projects further backwards.

The palatal and nasal regions of the skull afford further evidence of the affinities of the present Sheppey Chelonite to the Turtles.

The bony palate presents in an exaggerated degree its great extent from the intermaxillary bones to the posterior nasal aperture, and it is not perforated, as in the Trionyxes, by an anterior palatal foramen.

The extent of the bony palate is relatively greater than in the *Chelone Mydas*; the trenchant alveolar ridge is less developed than in the *Chel. Mydas*; the groove for the reception of that of the lower jaw is shallower than in the existing Cheloniæ, or the extinct *Chel. breviceps*, arising from the absence of the internal alveolar ridge.

The present species is distinguished by the narrowness of the sphenoid at the base of the skull, and by the form and groove of the pterygoid bones, from the existing Cheloniæ, and *à fortiori* from the Trionyxes; to which, however, it approaches in the elongated and pointed form of the muzzle, and the trenchant character of the alveolar margin of the jaws.

The general characters of the carapace are next given, and a specimen from Mr. Bowerbank's collection is more particularly described.

This carapace, as compared with that of the *C. breviceps* in the same collection, presents the following differences: it is much broader and flatter; the vertebral plates are relatively broader; the lateral angle, from which the intercostal suture is continued, is much nearer the anterior margin of the plate; the *C. longiceps* in this respect resembling the existing species: the expanded portions of the ribs are relatively longer; they are slightly concave transversely to their axis on their upper surface, while in *C. breviceps* they are flat. The external surface of the whole carapace is smoother, and although as depressed as in most turtles, it is more regularly convex, and sloping away by two nearly plane surfaces from the median longitudinal ridge of the carapace.

Among the minor differences of the two Sheppey fossils the author states, that the first vertebral plate of *C. longiceps* is more convex at its middle part, and sends backwards a short process to join the second vertebral plate, in which it resembles the *C. Mydas*. The second plate is six-sided, the two posterior lateral short sides being attached to the second pair of ribs, in which the present species differs from both *C. Mydas* and *C. breviceps*. The third vertebral plate is quadrangular instead of the second, as in *C. breviceps* and *C. Mydas*. The impressions of the epidermal scutes are deeper, and the lines which bound the sides of the vertebral scutes meet at a more open angle than in the *C. breviceps*, in which the vertebral scutes have the more regular hexagonal form of those of the *C. Mydas*.

The plastron is more remarkable than that of the *C. breviceps* for the extent of its ossification, the central cartilaginous space being reduced to an elliptical fissure. The four large middle pieces, called hyosternals and hyposternals, have their transverse extent relatively much greater, as compared with their antero-posterior extent, than in *C. breviceps*. The median margins of the hyosternals are developed in short toothed processes along their anterior two-thirds; and the median margins of the hyposternals have the same structure along their posterior halves.

The xiphisternals are relatively broader than in *C. breviceps* or in any of the existing species, and are united together by the whole of their median margins. The entosternal piece is flat on its under surface.

Each half of the plastron is more regularly convex than in *C. Mydas*. The breadth of the sternum along the median suture, uniting the hyosternals and hyposternals, is five inches; and the breadth at the junction of the xiphisternals with the hyposternals is two inches.

The posterior part of the cranium is preserved in this fossil, withdrawn beneath the anterior part of the carapace; the fracture shows the osseous shield covering the temporal fossæ; and the pterygoids remain, exhibiting the wide and deep groove which runs along their under part.

It has been most satisfactory, the author says, to find that the two distinct species of the genus Chelone, first determined by the skulls only, should thus have been established by the subsequent observation of their bony cuirasses; and that the specific differences manifested by the cuirasses should be proved by good evidence to be characteristic of the two species founded on the skulls.

Thus the portion of the skull preserved with the carapace first described, served to identify that fossil with the more perfect skull of the *Chelone breviceps*, by which the species was first indicated. And, again, the portion of the carapace adhering to the perfect skull of the *Chelone longiceps* equally served to connect with it the nearly complete osseous buckler, which otherwise, from the very small fragment of the skull remaining attached to it, could only have been assigned conjecturally to the *Chel. longiceps*; an approximation which would have been the more hazardous, since the *Chel. breviceps* and *Chel. longiceps* are not the only turtles which swarm those ancient seas that received the enormous argillaceous deposits of which the isle of Sheppey forms a part.

3. *Chelone latiscutata*.—A considerable portion of the bony cuirass of a young turtle from Sheppey, three inches in length, including the 2nd to the 7th vertebral plates, with the expanded parts of the first six pairs of ribs, and the hyosternal and hyposternal elements of the carapace, most resembles that of the *Chelone coniceps* in the form of the carapace, and especially in the great transverse extent of the above-named parts of the sternum; it differs, however, from the *Chel. longiceps* and from all the other known Chelonites in the great relative breadth of the vertebral scutes, which are nearly twice as broad as they are long.

The central vacuity of the plastron is subcircular, and, as might be expected, from the apparent nonage of the specimen, is wider than in the *Chel. longiceps*; but the toothed processes given off from the inner margin of both hyo- and hyposternals are small, subequal, regular in their direction, and thus resemble those of the *Chel. longiceps*.

The length of the expanded part of the third rib is one inch seven lines; its antero-posterior diameter or breadth, six lines; in the form of the vertebral extremities of the ribs and of the vertebral plates to which they are articulated, the present fossil resembles the *Chel. longiceps.*

The author knows of no recent example, however, of the Chelone that offers such varieties in the form of its epidermal scutes as would warrant the present Chelonite being considered a variety merely of the *Chel. longiceps*; and he therefore indicates the distinct species which it seems to represent, by its main distinctive character, under the name of *Chelone latiscutata.*

4. *Chelone convexa.*—The fourth species of Chelone, indicated by a nearly complete cuirass, from Sheppey, holds a somewhat intermediate position between the *C. breviceps* and *C. longiceps*; the carapace being narrower and more convex than that of *C. coniceps*; broader, and with a concavity arising from a more regular curvature than in *C. breviceps.* The expanded parts of the ribs have an intermediate length with those of the two Chelones with which this specimen is compared, and therefore is a difference independent of age.

The distinction of *C. convexa* is still more strikingly established in the plastron, which in its defective ossification more nearly resembles that of the existing species of Chelone. All the bones, especially the xiphisternals, are more convex on their outer surface than in other turtles, recent or fossil. The internal rays of the hyosternals are divided into two groups; the lower consisting of two short and strong teeth projecting inwards, while the rest extend forwards along the inner side of the episternals. The same character may be observed in the corresponding processes of the hyposternals, but the external process is relatively much narrower than in *C. breviceps.* The following differences are stated to distinguish the sternum of *C. convexa* from that of *C. Mydas.* The median margin of the hyosternals forms a gentle curve, not an angle: that of the hyposternals is likewise curved, but with a slight notch. The longitudinal ridge on the external surface, and near the median margin of the hyo- and hyposternals, is less marked in the Sheppey fossil; especially in the hyposternals, which are characterized by a smooth concavity in their middle.

The suture between the hyo- and hyposternals is nearer to the external transverse radiated process of the hyposternals. The median vacuity of the sternal apparatus is elliptical in the *Chel. convexa*, but square in the *Chel. Mydas.*

The characteristic lanceolate form of the episternal bone in the genus Chelone is well seen in the present fossil.

The true marine character of the present Sheppey Chelonite is likewise satisfactorily shown in the small relative size of the entire femur which is preserved on the left side, attached by the matrix to the left xiphisternal. It presents the usual form, a slight sigmoid flexure, characteristic of the Chelones; it measures one inch in length. In an Emys of the same size, the femur, besides its greater bend, is $1\frac{1}{2}$ inch in length.

5. *Chelone subcristata.*—The fifth species of Chelone from Sheppey, distinguishable by the characters of its carapace, approaches more nearly to the *Chelone Mydas* in the form of the vertebral scutes,

which are narrow in proportion to their length, than in any of the previously described species; but is more conspicuously distinct by the form of the 6th and 8th vertebral plates, which support a short, sharp, longitudinal crest. The middle and posterior part of the first vertebral plate is raised into a convexity, as in the *Chel. longiceps*, but not into a crest.

The keeled structure of the sixth and eighth plates is more marked than in the fourth and sixth plates of *Chelone Mydas*, which are raised into a longitudinal ridge.

The characters of the carapace are then minutely described.

Sufficient of the sternum is exposed in the present fossil to show, by its narrow elongated xiphisternals, and the wide and deep notch in the outer margin of the conjoined hyo- and hyposternals, that it belongs to the marine Chelones.

The xiphisternals are articulated to the hyposternals by the usual notch or gomphosis; they are straighter and more approximated than in the *Chel. Mydas*; the external emargination of the plastron differs from that of the *Chel. Mydas* in being semicircular instead of angular, the *Chel. subcristata* approaching, in this respect, to the *Chel. breviceps*.

The shortest antero-posterior diameter of the conjoined hyo- and hyposternals is two inches seven lines. The length of the xiphisternal two inches six lines. The breadth of both, across their middle part, one inch three lines.

The name proposed for this species indicates its chief distinguishing character, viz. the median interrupted carina of the carapace, which may be presumed to have been more conspicuous in the horny plates of the living animal than in the supporting bones of the fossilized carapace.

6. *Chelone planimentum.*—This species is founded on an almost entire specimen of skull and carapace of the same individual, in the museum of Prof. Sedgwick; on a skull and carapace belonging to different individuals, in the museum of Prof. Bell; and on a carapace in the British Museum; all of which specimens are from the London clay at Harwich.

The skull resembles, in the pointed form of the muzzle, the *Chel. longiceps* of Sheppey, but differs in the greater convexity and breadth of the cranium, and the great declivity of its anterior contour.

The great expansion of the osseous roof of the temporal fossæ, and the share contributed to that roof by the post-frontals, distinguish the present, equally with the foregoing Chelonites, from the freshwater genera Emys and Trionyx. In the oblique position of the orbits, and the diminished breadth of the interorbital space, the present Chelonite, however, approaches nearer to Trionyx and Emys than the previously described species.

Its most marked and characteristic difference from all existing or extinct Chelones is shown by the greater antero-posterior extent and flatness of the under part of the symphysis of the lower jaw, whence the specific name here given to the species.

Since at present there is no means of identifying the well-marked species of which the skull is here described with the Chelonite figured in the frontispiece to Woodward's 'Synoptical Table of British Organic Remains,' and alluded to without additional description or characters as the '*Chelonia Harvicensis*' in the additions to Mr. Gray's 'Synopsis Reptilium,' p. 78, 1831; and since it is highly probable that the extensive deposit of Eocene clay along the coast of Essex, like that at the mouth of the Thames, may contain the relics of more than one species of our ancient British turtles, the author prefers indicating the species here described by a name having reference to its peculiarly distinguishing character, to arbitrarily associating the skull with any carapace to which the vague name of *Harvicensis* has been applied.

Besides the specimen of Chelonite from Harwich, in the museum of Norwich, figured by Woodward, there is a mutilated carapace of a young Chelone from the same locality in the British Museum. This specimen exhibits the inner side of the carapace, with the heads and part of the expanded bodies of four pairs of ribs. It is not sufficiently entire to yield good specific characters, but it demonstrates unequivocally its title to rank with the marine turtles. It is figured in Mr. Kœnig's 'Icones Sectiles,' pl. xvi. fig. 192, under the name of *Testudo plana*.

The carapace of a larger specimen of Chelone, from the coast of Harwich, was purchased, by the British Museum, of Mr. Charlesworth, by whom a lithograph of the inner surface of this Chelonite, of the natural size, has been privately distributed, without description.

The carapace in the museum of Prof. Sedgwick, forming part of the same individual (*Chelone planimentum*) as the skull above described, exhibits many points of anatomical structure more clearly than the last-mentioned Chelonite in the British Museum; it also displays the characteristic coracoid bone of the right side in its natural relative position. The resemblance of this carapace in general form to that of the *Chelone caretta* is pretty close; it differs from that and other known existing turtles, and likewise from most of the fossil species, in the thickness and prominence of the true costal portions of the expanded vertebral ribs, which stand out from the under surface of the plate through their entire length, and present a somewhat angular obtuse ridge towards the cavity of the abdomen.

In the large proportional size of the head, the *Chelone planimentum* corresponds with the existing turtles; and that the extinct species here described attained larger dimensions than those given above, is proved by a fossil skull from the Harwich clay, in the collection of Prof. Bell, which exhibits well the character of the broad and flattened symphysis.

A carapace of a smaller individual of *Chelone planimentum* from the Harwich coast, with the character of the inwardly projecting ribs strongly marked, is likewise preserved in the choice collection of the same excellent naturalist. One of the hyosternal bones enclosed in the same nodule of clay testifies to the partial ossification of the plastron in this species.

In the summary of the foregoing details the author observes, that they lead to conclusions of much greater interest than the previous opinions respecting the Chelonites of the London basin could have originated. Whilst these were supposed to have belonged to a fresh-water genus, the difference between the present fauna and that of the Eocene period, in reference to the Chelonian order, was not very great; since the *Emys* or *Cistudo Europæa* still abounds on the Continent, and lives long in our own island in suitable localities: but the case assumes a very different aspect when we come to the conviction, that the majority of the Sheppey Chelonites belong to the true marine genus Chelone; and that the number of species of the Eocene extinct turtles already obtained from so limited a space as the isle of Sheppey exceeds that of the species of existing Chelone.

Notwithstanding the assiduous search of naturalists, and the attractions to the commercial voyager which the shell and the flesh of the turtles offer, all the tropical seas of the world have hitherto yielded no more than five well-defined species of Chelone, and of these only two, as the *C. Mydas* and *C. caretta*, are known to frequent the same locality.

It is obvious, therefore, that the ancient ocean of the Eocene epoch was less sparingly inhabited by turtles; and that these presented a greater variety of specific modifications than are known in the seas of the warmer latitudes of the present day.

The indications which the Sheppey turtles afford of the warmer climate of the latitude in which they lived, as compared with that which prevails there in the present day, accord with those which all the organic remains of the same depositary have hitherto yielded in reference to this interesting point.

That abundance of food must have been produced under such influences cannot, Mr. Owen states, be doubted; and he infers, that to some of the extinct species—which, like the *C. coniceps* and *C. platygnathus*, exhibit either a form of head well adapted for penetrating the soil, or with modifications that indicate an affinity to the Trionyxes—was assigned the task of checking the undue increase of the extinct crocodiles of the same epoch and locality, by devouring their eggs or their young, becoming probably, in return, themselves an occasional prey to the older individuals of the same carnivorous saurian.

CHEMICAL SOCIETY.

[Continued from p. 320.]

March 15, 1842.—The following communications were read:—

Second Part of Mr. Hutchinson's Paper. (See p. 318.)

"On the Preparation of artificial Yeast," by George Fownes, Ph.D. This paper appears in the present Number, p. 352.

March 30.—Anniversary Meeting, Thomas Graham, Esq., F.R.S., Professor of Chemistry in University College, London, President, in the Chair.

The Report of the Council on the state and prospects of the Society was read, and the following gentlemen were elected as Officers and Council for the ensuing year:—

President.—Thos. Graham, Esq. *Vice-Presidents.*—William Thos. Brande, Esq.; John Thos. Cooper, Esq.; Michael Faraday, Esq., D.C.L.; Richard Phillips, Esq. *Treasurer.*—Arthur Aikin, Esq. *Secretaries.*—Robert Warington and George Fownes; *Foreign Secretary.*—E. F. Teschemacher. *Council.*—Dr. Thos. Clark; Dr. Chas. Daubeny; John Fred. Daniell, Esq.; Thos. Everitt, Esq.; W. R. Grove, Esq.; James F. W. Johnston, Esq.; Percival N. Johnson, Esq.; George Lowe, Esq.; William H. Miller, Esq.; Robert Porrett, Esq.; Dr. G. O. Rees; Lieut.-Colonel Philip Yorke.

The laws of the Society, as drawn up by the Council, were submitted to the meeting, and having been read and discussed, were confirmed, with amendments, and ordered to be printed for the use of the members.

April 5.—The following communications were read:—

Extract from a letter from Wm. H. Miller, Esq., Professor of Mineralogy in the University of Cambridge.

"I regret that my engagements in Cambridge have prevented my being present at the meeting of the Chemical Society, especially as I was desirous of offering my services in determining the form of any crystalline products that may present themselves to chemists who are engaged in original researches. Also, in return, I might make bold to ask some members of the Society to supply me with certain objects of crystallographic and optical research from their laboratories."

"On the Analysis of the Chalk of the Brighton Cliffs," by Dr. Edw. G. Schweitzer.

My attention was directed to the soil of this neighbourhood, for the purpose of ascertaining if the chalk contains any ingredient peculiarly favourable to the growth of Gramineæ, in consequence of the well-known fact, that the herbage of the South Downs, along the coast of Sussex, affords a superior food for cattle, producing meat of excellent quality, for which these Downs are justly celebrated. The result of my analysis substantiates the presence of phosphate of lime, an ingredient valuable for the nutrition of plants. The chalk is composed of the following substances in 100 parts:—

Carbonate of lime	98·57	
...... of magnesia	0·38	
Phosphate of lime	0·11	
Protoxide of iron	0·08	
...... of manganese....	0·06	
Alumina.................	0·16	
Silica	0·64	
	——	100·00

To ascertain the quantity of phosphoric acid, I followed Dr. Schulze's method (*Journal für prakt. Chemie,* xxi. S. 387–389), which he recommends for the analytical investigation of soils. Finding it useful and correct, I subjoin an extract from his treatise. The process is based upon the fact, that phosphate of lime and phosphate of magnesia are soluble in acetic acid, while the phosphate of peroxide of iron and phosphate of alumina are not so.

This being the case, the soil or mineral is to be treated with hydrochloric acid, and the iron which the solution contains per-oxidised, the phosphate of protoxide of iron being soluble in acetic acid. Should the muriatic solution contain more phosphoric acid than oxide of iron or alumina, (which seldom is the case, as the latter are usually predominant,) peroxide of iron or alumina is to be added, the solution must also be freed from every trace of silica. The earthy muriates are precipitated with ammonia, after which acetic acid is added, and the whole gently digested. The precipitate will dissolve again, with the exception of the phosphates of peroxide of iron and alumina. When both these ingredients enter into the precipitate, caustic potassa will give the means of ascertaining their respective quantities.

The solubility of the phosphate of protoxide of iron, and the insolubility of the phosphate of peroxide of iron in acetic acid, when freshly precipitated, give an excellent method to separate quantitatively these two degrees of oxidation. The manipulation is obvious.

The discovery by Professor Ehrenberg, that the Brighton chalk consists of microscopic shells, is a decided proof of its animal origin, to which may now be added an additional one, viz. the presence of phosphate of lime, which is a usual, although secondary ingredient of the shells of Crustaceæ.

"On the Action of Chromate of Potash on the Protosulphate of Manganese," by Robert Warington, Esq. See Chem. Soc.

In the course of some experiments on the formation of double salts of chromic acid with various bases depending on the tendency which might arise, from the resulting affinities, to the formation of certain crystallized combinations, the subject of the present brief communication came under my notice.

On adding a solution of the yellow chromate of potash to one of the protosulphate of manganese, no turbidity or precipitate takes place, but the mixed fluids become of a deep orange red colour, and after a short period the surface is covered with a dark brown crust or film, and the whole of the containing vessel is coated with the same substance; at times when the solutions are dilute, this deposit assumes a crystalline appearance. If this compound is prepared under the microscope, in the manner described in a former paper, the first effect is the appearance of numerous minute spherical granules of a fine crimson brown colour, which gradually increase in size until about from six to seven 250ths or ·025 of an inch in diameter; a number of delicate crystallized spiculæ are then observed to start out in radii from their sides; and when the solutions employed for its production are diluted, fine stellated groups of prismatic crystals are obtained. When this substance, which has a dark chocolate hue, is examined by a strong transmitted light, it has a rich crimson brown colour: it possesses the following properties:—it is soluble in diluted nitric or sulphuric acids, without residue, yielding an orange-coloured solution; when acted upon by hydrochloric acid chlorine is evolved, and a brown fluid results, which by the addition of a few drops of alcohol or other deoxidizing agent, becomes of a fine emerald green. The following analysis was made of it:—8·2 grains, previously dried at a temperature of boiling water, were submitted to a long-continued red heat in a small green glass tube, to which a chloride of calcium tube was attached; it lost 1·0 grain, which corresponded with the weight gained by the absorption tube; 8·2 grains dissolved in dilute nitric acid, and precipitated while boiling by caustic potash, gave, after the necessary treatment, 4·5 grains of the red oxide of manganese; the solution was then acidified by sulphuric acid, and evaporated to dryness to expel the nitric acid, redissolved, deoxidized by alcohol and the oxide of chromium thrown down by ammonia, again evaporated to dryness, to avoid the possibility of any of the oxide being in solution, and the oxide of chromium, well washed, gave 2·3 grains. We have therefore

4·5 grains red oxide manganese.. = 4·188 protoxide
2·3 ... protoxide chromium .. = 3·000 chromic acid
1·0 ... water .... 1·000 water
—— 8·188

By calculation this should be .. 4·141 protoxide
3·014 chromic acid
1·043 water

Or, 1 atom chromic acid + 2 atoms protoxide of manganese + 2 atoms water. Represented by $Cr\,O_3 + 2\,Mn\,O\,. + 2\,H\,.\,O$

April 19.—The following communications were read:—

"On the Equilibrium of the Temperature of Bodies in contact," by E. A. Parnell, Esq.

In reference to observations recently made by Mr. Hutchinson on the difficulty of raising the temperature of any substance to the degree of the medium by which the heat is applied*, Mr. Parnell observes, "From what I know of the mode in which Mr. Hutchinson operated, it is probable that a loss of heat occurred by radiation from the substance operated on; by radiation, first to the cover of the bath, and from this to external objects. On adopting precautions to avoid this source of error, I found that in a steam-bath the temperatures attained by substances, were

1. Olive oil...... $\frac{1}{3}$ degree below the temperature of the steam.
2. Water .... $\frac{2}{3}$ ... ...

And in a water-bath,—

3. Water .... $\frac{1}{3}$ degree below the temperature of the water.
4. Vapour of æther 1 ... ...
5. Air .... 1 ... ...

In the two first experiments, the apparatus used was a large flask, closed with a cork, having several perforations, through one of which was admitted a wide tube containing the liquid operated on, the tube not dipping so far as the surface of the water in the flask, which was kept boiling.

In the remaining three a copper water-bath was employed, the

[* An abstract of Mr. Hutchinson's paper will be found at p. 318.]

water, vapour or air being contained in a glass globe of about fifteen cubic inches capacity, having a narrow neck, through which the thermometer was admitted. The globe was supported in the bath by a wire-cage in the same manner as is done in the operation of taking the density of vapours.

It would hence appear from the proximity of the temperature of the substance heated and the bath, that if the experiments were continued a sufficient length of time, and every chance of error avoided, the substance might be heated to an equal degree, and the law of equilibrium of temperature maintain its universality.

I could never, however, raise the temperature of æther vapour nearer than one degree below the temperature of the bath, and to effect this required at least half an hour. I would therefore recommend, in taking the density of vapours, that the temperature of the globe be considered as one degree less than that of the bath, in making the calculations. Notwithstanding, with this correction the weight of the vapour can scarcely be effected to a greater extent than ·04 grain.

"On the Preparation of Hippuric Acid," by Geo. Fownes, Esq.*

Being very desirous of possessing a specimen of a very interesting substance, hippuric acid, namely, and failing to obtain it in any quantity from the horse-urine collected in London stables, I was induced to make trial of that of cows, and speedily found it to be a substance highly advantageous for the purpose.

Perfectly fresh cow-urine presents the aspect of a transparent amber-coloured liquid of peculiar but not disagreeable odour, and quite neutral to test-paper. When this is evaporated down in a water-bath to about one-tenth, and mixed with hydrochloric acid, a very large quantity of a brown crystalline substance separates, which is hippuric acid. It is very easy in this way to operate upon whole gallons of the liquid, and thus procure many ounces of hippuric acid.

To purify this substance, I find the following method very advantageous. The brown rough acid is dissolved in boiling water, of which, by the way, it requires a much larger quantity than from the descriptions given would be imagined, and through the solution a stream of chlorine gas is transmitted, until the odour of that gas becomes perceptible in the liquid, and its brown colour passes into a sort of deep amber-yellow. The hot solution is then filtered through cloth, and upon cooling, the acid, still very impure, crystallizes out. The acid is next dissolved in a dilute hot solution of carbonate of soda, taking care to have a little excess of the alkali, digested for a few minutes with a little animal charcoal, filtered, and lastly, the solution strongly acidified by hydrochloric acid, which removes the base and sets free the hippuric acid. Should that substance not be by such treatment rendered perfectly white, it

---

[* A paper on the conversion of benzoic into hippuric acid, by Mr. Garrod, read before the Chemical Society, January 18, will be found in Phil. Mag., S. 3. vol. xx. p. 501.]

may be again dissolved in hot water, a little chlorine passed, the solution supersaturated with carbonate of soda, digested with animal charcoal, and once more decomposed by an acid.

It is to be observed, that hippuric acid only crystallizes in a distinct and characteristic manner when pure, or at least when in a condition approaching that state; under other circumstances it usually separates either as short radiated needles, or as a granular crystalline powder. The latter happens when soluble salt is present.

If the urine, instead of being quite fresh, is at all ammoniacal, then during the evaporation a very large quantity of ammonia is disengaged, accompanied by slow effervescence, and the liquid affords, as Liebig has already pointed out, benzoic acid only, without a trace of hippuric.

The great density of the urine of the cow is a remarkable circumstance; one sample, affording much hippuric acid, gave the sp. gr. of 1·0325, which is considerably higher than that of human urine. This density is chiefly due to a most prodigious quantity of urea, which is easily extracted from the brown liquid remaining after the separation of the hippuric acid, by the aid of a hot strong solution of oxalic acid, which throws down the slightly soluble oxalate. This can be decomposed by chalk, and the urea extracted without having recourse to alcohol. Besides these two substances, hippuric acid, or rather hippurate of an alkali, and urea, cow-urine contains a little uric acid, phosphates and other salts in tolerable abundance.

The constant occurrence of so much urea in the urine of all animals, both granivorous and flesh-eating, tends greatly to strengthen the opinion, that it is by this channel almost alone that the removal of those portions of the azotized constituents of the body, which have been worn out, as it were, or in the act of undergoing decay, is effected. It is well known that such substances, by ordinary putrefaction, furnish carbonate of ammonia; but in the body this process seems to have been modified in such a manner, that in place of that substance, urea or carbamide is generated, which is destitute of the irritating power upon the organs which a corresponding quantity of the ammoniacal salt would possess.

It has been suggested that hippuric acid is not a direct product of the animal system, but is formed by the union of benzoic acid or its elements with those of lactate of urea, the benzoic acid being present in the food, and the recent experiments of Mr. Garrod certainly countenance the opinion. But these attempts to detect benzoic acid in the food of these animals were in the hands of Liebig quite unsuccessful, and it seems unlikely that it would be found at any rate in considerable quantity in such substances as grains and mangel-wurzel, which, with the addition of a little hay, constituted the food of the cows from which such an abundant supply of hippuric acid was obtained.

There is only one other point which requires notice, and that is the nature of the change which hippuric acid so readily undergoes by putrefaction. It is astonishing that a substance which so powerfully resists the action of chlorine, should be so easily affected by simple contact with putrefying matter.

A glance at the composition of hippuric acid will show that this change is altogether different from that which urea suffers under similar circumstances, the assimilation, namely, of the elements of water by which it becomes carbonate of ammonia. Hippuric acid, on the contrary, seems to pass into benzoic by an absorption of oxygen from the air, carbonic acid and ammonia being at the same time produced.

$$\begin{array}{rl} \text{Hippuric acid} \ldots\ldots & C_{18}\ H_8\ N\ O_5 \\ \text{Subtract—Benzoic acid} \ldots\ldots & C_{14}\ H_5\ \ \ \ O_3 \\ \hline & C_4\ H_3\ N\ O_2 \end{array}$$

which by addition of 6 eq. of oxygen from the air, would furnish 1 eq. ammonia and 4 eq. carbonic acid.

May 3.—The following communication was made:—

"On a curious Formation of Prussian Blue," by Robert Porrett, Esq.

Mr. Porrett was led to attend to this subject by an observation accidentally made while walking in the garden of a friend. He found that a great number of the pebbles in the gravel walk were tinged of a fine bright blue colour; and on remarking the appearance to the owner, and inquiring as to the cause, though it had never before attracted notice, he ascertained that before the fresh gravel had been laid down, the walks had been strewed with some refuse lime from the gas-works, for the purpose of destroying the worms, and over which the red gravel of the neighbourhood of London had been placed only a few weeks before the appearances described were observed.

The blue colour was entirely confined to the upper surface of the pebbles which was exposed to the atmospheric air, and was found to be Prussian blue. The pebbles affected were siliceous, having a white exterior coating. Mr. Porrett considers this production of Prussian blue to have arisen from some of the gas-lime having been dropped accidentally on the surface of the new gravel, and that the peroxide of iron there found had been deoxidized by some of the sulphur compounds contained in the gas-lime, giving rise to the formation of a combination of iron with cyanogen, also present in the lime, and calcium, and that this compound had been decomposed by the action of the carbonic acid of the atmosphere, or by the siliceous matter of the stone, and thus causing the formation of the Prussian blue*.

May 17.—The following communications were read:—

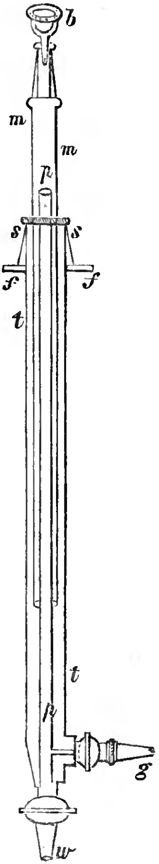
Extract from a letter from Professor Clark.

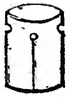
"The burner is to be fixed into a table by screwing thereto the cir-

---

[* On a subject allied to that of Mr. Porrett's paper, see Phil. Mag., S. 3. vol. x. p. 329, and also the notice referred to, p. 333.]

cular projection *ff*. There are two stop-cocks. The horizontal one *g* is for admitting the supply of gas, which passes up the fixed tube *pp* into the sliding tube *m m*. Between the outer fixed tube *t t* and the inner fixed tube *p p*, water is contained to serve as a lute to confine the gas. The sliding tube is kept at whatever height it may be placed, by means of a spring inserted in a stuffing-box formed by the screw *s s* above *ff*. The spring is represented apart, *r*. It is formed out of a short bit of another metallic tube of such bore as only to permit the tube *m m* to slide through it easily. Four holes in the circle of the wider tube *r* are bored at equal distances, and a vertical slit is cut by a saw from each hole through to the bottom of the tube. After being thus cut, the cut parts are squeezed together by the hand, and the tube *r* being put over the tube *m* and confined in the stuffing-box at *s*, forms a convenient spring for keeping the sliding tube *m* at whatever height it may be placed. The stop-cock *w* is to let out any water that may by accident get into the tube *pp*. The tube *m m* should not be less than half an inch in diameter. The burner *b*, which is copied after one in Professor Graham's laboratory, University College, burns after the manner of a rose-burner, but it is in the form of a ring, instead of being solid. It may be called a ring-burner. It permits a much more free access of air, especially when the flame is placed very close to a vessel. This burner also supplies gas very advantageously for mixture with air in a cylinder, at the top of which the mixture burns over wire gauze. The sliding tube relieves the operator from all cumbrous supports to his burner, or from the necessity of having moveable supports to the vessels to be heated. A ring supported by three legs, the whole made of tinned iron, affords a cheap, stable and convenient support to vessels, although of considerable weight."





"On some Salts of Cadmium," by Henry Croft, Esq. This paper is inserted in the present Number of the Philosophical Magazine, p. 355.

"An Examination of two specimens of South Sea Guano, imported for agricultural use," by George Fownes, Esq.

No 1.—Presented the aspect of a pale-brown soft powder, with a few lumps, having in their inside whitish specks; its odour was exceedingly offensive.

Treated with hot water and filtered, it gave a yellow, feebly alkaline solution, not rendered turbid to any extent by the addition of acid, which contained much ammoniacal salt, some sulphate and chloride, a very large quantity of oxalate, and both potash and soda, the latter most abundant.

The undissolved substance appeared to be a mixture of uric acid, earthy phosphates, and brown organic matter.

Fifty grains of guano by incineration in a platinum vessel left 16·9 grs. fine greyish white-ash. This ash, treated with hot water, and the whole placed on a filter, left a quantity of insoluble matter, weighing, after being well washed, dried and ignited, 14·6 grs.: this was almost entirely soluble in warm dilute hydrochloric acid, precipitated by the addition of ammonia, and evidently consisted of phosphates of lime and magnesia.

The aqueous solution was slightly alkaline, contained much chloride, some sulphate, a very notable quantity of soluble phosphate, some potash, and a good deal of soda.

Hence the following approximate result:—

Oxalate of ammonia with trace of carbonate, undecomposed uric acid, brown organic matter and water	33·1	66·2
Earthy phosphates, with very little sandy matter	14·6	29·2
Alkaline phosphate and chloride with little sulphate	2·3	4·6
	50·	100·0

No. 2.—Darker in colour, and having but little smell. Examined as in preceding case; it contained no uric acid.

Fifty grains gave—

Oxalate of ammonia, with little carbonate, organic matter and water	22·3	44·6
Earthy phosphates, with little gritty matter..	20·6	41·2
Alkaline sulphates, chlorides and phosphates, (both potash and soda, the latter most abundant)	7·1	14·2
	50·	100·

The last specimen is evidently older and in a more advanced state of decomposition than the other; its odour is far less powerful and offensive; it contains little or no uric acid, but a larger proportion of inorganic substances*.

It is difficult to imagine a manure better fitted for almost universal use than this "guano;" it contains in a highly concentrated form everything that plants require for their sustenance, with the exception perhaps of potash, which however is often abundantly supplied by a soil poor in other respects.

The presence of a large quantity of oxalate of ammonia is a curious fact, and was early noticed; there can be no doubt that this substance owes its existence in some way or other to the uric acid contained in the excrement of the sea-birds, to the decomposition of which the guano-deposits are due. We can easily imagine that in this mass of putrefying substance, kept in a moistened state by the dews of night, a decomposition of a peculiar kind may be set up in the uric acid, and its gradual conversion into new products, among

---

[* On the composition of guano, see also Phil. Mag., S. 3. vol. xix. p. 49.]

which may easily be oxalate of ammonia, effected perhaps somewhat after the following fashion:—

$$\left.\begin{array}{ll}\text{Uric acid} \ldots\ldots\ldots & C_5\,H_2\,N_2\,O_3\\ \text{4 eq. water} \ldots\ldots & H_4\,O_4\\ \text{1 eq. oxyg. from air} & O\end{array}\right\}=\left\{\begin{array}{ll}\text{2 eq. oxal. acid} & C_4\,O_6\\ \text{2 .. ammonia} & H_6\,N_2\\ \text{1 .. carb. acid} & C\,O_2\end{array}\right.$$

$$\overline{C_5\,H_6\,N_2\,O_8} \qquad\qquad \overline{C_5\,H_6\,N_2\,O_8}$$

This view, it must be remembered, is merely hypothetical, yet is borne out by the facts.

The only case in which oxalic acid is known to arise from uric acid, is in the artificial formation of allantoin discovered by Liebig, and in which uric acid, water and peroxide of lead being boiled together, give rise to oxalate of the protoxide of lead, allantoin and urea; it is in short an oxidizing action, so far resembling the one imagined, but more complex.

$$\left.\begin{array}{ll}\text{Uric acid (doubled)} & C_{10}\,H_4\,N_4\,O_6\\ \text{3 eq. water} \ldots\ldots & H_3\,O_3\\ \text{2.. ox. from perox.} & O_2\end{array}\right\}=\left\{\begin{array}{ll}\text{Allantoin....} & C_4\,H_3\,N_2\,O_3\\ \text{Urea} \ldots\ldots\ldots & C_2\,H_4\,N_2\,O_2\\ \text{2 eq. oxal. acid} & C_4\,O_6\end{array}\right.$$

$$\overline{C_{10}\,H_7\,N_4\,O_{11}} \qquad\qquad \overline{C_{10}\,H_7\,N_4\,O_{11}}$$

It is very unlikely that this peculiar mode of decomposition should occur under the circumstances in which the guano is produced; urea certainly would not resist destruction a week, and no doubt the allantoin would share the same fate.

It was thought worth while nevertheless to examine one of the specimens (No. 1) carefully for these two bodies, a portion of the substance being acted upon by hot water, and the filtered solution cautiously evaporated to a small bulk, whereupon crystals were abundantly formed on cooling. These being dissolved in hot water, decolorized with animal charcoal, and the solution once more concentrated, a second crop was got, but slightly coloured. These however turned out on examination to be nothing but oxalate of ammonia. The search for urea was equally unsuccessful.

There is a curious relationship between the three bodies, oxalate of ammonia, oxamide and allantoin, the only difference in composition being the diminishing proportion of the elements of water.

Anhydrous oxalate of ammonia (doubled) ..	$C_4\,H_6\,N_2\,O_6$
Oxamide (doubled)	$C_4\,H_4\,N_2\,O_4$
Allantoin	$C_4\,H_3\,N_2\,O_3$

"On the production of Artificial Uranite," by W. J. Cock, Esq.

The subject of the present communication was observed during the preparation of the oxide of uranium from its mineral, Pitchblende; it was obtained as follows:—

The mineral was pulverized and well calcined; it was then digested with diluted nitric acid, which dissolved the greater part of the soluble contents. (From this solution none of the precipitate was obtained.)

The undissolved residuum was washed and dried, and again calcined. It was digested in nitric acid rather stronger than before, and gave a solution of a darker green than the first. This solution was left several weeks in open vessels, and upon its being drawn off, a quantity of the green precipitate was found adhering to the bottom and sides of the vessels.

The composition, which is very variable, of the mineral Pitchblende, as given by Berthier in his *Traité des Essais par la voie sèche* from two analyses, is in the 100 parts,—

Protoxide of uranium	51·6	60·0
Carbonate of magnesia	3·3	
Peroxide of iron	7·2	2·5
Alumina (clay)	17·2	9·0
Sulphuret of iron and copper	1·2	5·5
Arsenical pyrites (iron)	5·8	9·2
Sulphuret of lead	6·0	3·5
Sulphuret of zinc		1·4
Carbonate of lime	2·2	2·2
Water and bitumen	4·2	5·2
	98·7	98·5

No mention is here made of the phosphoric acid which enters into the composition of the artificial uranite. The composition of the native uranite, as also of the double phosphate of uranium and copper (chalkolite), are thus given by Berzelius :—

	Uranite.	Chalkolite.
Oxide of uranium	59·37	60·25
Lime	5·65	
Oxide of copper		8·44
Barytes	1·51	
Magnesia and oxide of manganese	·19	
Phosphoric acid	14·63	15·56
Water	14·90	15·05
Gangue	2·85	·70
Fluoric acid and oxide of tin	trace	
	99·10	100·

It appears that these two minerals are found mixed together in all proportions, and from the artificial compound which forms the subject of the present notice, containing both oxide of copper and lime, that it is also a mixture of these salts.

The following analysis of the "Artificial Uranite," made under the superintendence of Mr. Parnell, was read as an appendix to the above :—

Phosphate of uranium	49·
Oxide of copper	19·5
Lime	1·8
Water	21·5
Phosphoric acid in combination with oxide of copper and lime (loss)	8·2
	100·00

The process of analysis was the following :—

(1.) Having previously ascertained by a qualitative analysis that the sole constituents of the substance are phosphoric acid, peroxide of uranium, oxide of copper, lime and water, a known weight was dissolved in hydrochloric acid, and copper was precipitated as sulphuret by transmitting sulphuretted hydrogen gas through the solution. The precipitated sulphuret, when filtered and washed, was digested in nitric acid, and from the solution thus obtained, oxide of copper was precipitated by potash, washed, ignited and weighed.

(2.) The solution, separated by filtration from the sulphuret of copper, was next evaporated to dryness and mixed with a little concentrated sulphuric acid to convert phosphate of lime into sulphate, the mixture was diluted with alcohol, in which sulphate of lime is quite insoluble, and filtered. The sulphate of lime was washed with alcohol, dried, ignited and weighed.

(3.) The filtered alcoholic solution, containing phosphate of uranium dissolved in the excess of sulphuric acid, was evaporated to dryness, the residue digested in nitric acid, and phosphate of uranium precipitated from the acid solution by ammonia. This, when washed and dried, was gently ignited and weighed.

(4.) The water contained in the substance was determined by observing what loss in weight it sustained when calcined at a dull red heat; and

(5.) The remaining ingredient, the phosphoric acid in combination with oxide of copper and lime, was considered as the deficiency on the weight of the original substance.

"Some additional Observations on the Red Oxalate of Chromium and Potash," by Robert Warington, Esq. This paper has been inserted in the present volume, p. 201.

ROYAL IRISH ACADEMY.

[Continued from p. 233.]

May 24, 1841 (Continued).—The following Note "On the Force of aqueous Vapour within the Range of atmospheric Temperature," was read by James Apjohn, M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons.

Having had it in contemplation some time since to investigate by means of an indirect, but I believe a very accurate process, the caloric of elasticity of the vapours of several liquids, I found myself stopped on the threshold of the inquiry by a want of knowledge of the tension of such vapours at different temperatures; for, with the exception of the vapours of water, alcohol, æther, and oil of turpentine, the tension of no others had been made the subject of experiment; and even in the case of the fluids just named, the results recorded in the books appeared to me very far from being of such a nature as to preclude the necessity of further research.

The method which I intended to employ, in order to arrive at the latent heats of vapours, not requiring a knowledge of their tensions beyond the range of atmospheric temperature, it occurred to me, that the necessary data for the solution of the preliminary problem might be obtained with facility, and, at the same time, with much precision, in the following manner:—

Let a known volume of dry air be charged with moisture at any given temperature, and let the expansion produced by the moisture be accurately noted. The pressure being also measured by an accurate barometer, we have the means of calculating the force of the vapour which has produced the expansion. For if v be the volume of the dry air, and v' that of same air when charged with moisture, f the force of the vapour, and p the existing atmospheric pressure, we shall have

$$v' = v \times \frac{p}{p - f},$$

from which we deduce

$$f = \left(\frac{v' - v}{v'}\right) \times p.$$

It was not my original intention to make any experiments upon the force of *aqueous* vapour, believing the table which I have hitherto employed, and which was calculated by the author of the article "Hygrometry," in Brewster's Encyclopædia, from the experiments of Dalton, to have been sufficiently exact. But the correctness of this table having been indirectly called in question by so high an authority as M. Kupffer, who has come to the conclusion, that the table of the force of aqueous vapour, given by a German meteorologist of the name of Kämtz, is alone to be relied upon, I resolved to commence with the vapour of water, in the hope that I might be able, by the results of direct experiment, to corroborate a conclusion previously drawn by Professor Lloyd, from a discussion of some hygrometrical observations of mine, viz. that for temperatures within the atmospheric range, the table of Kämtz is less accurate than that of Dalton, the values given in the former being all too low.

The apparatus I have employed in my experiments is composed of a glass ball prolonged on the one side into a short tube, furnished with a cap and stop-cock, and, on the other, into a long tube of somewhat smaller diameter, divided into 100 equal parts, each being ·042 of a cubic inch, or the ·001 of the total capacity of ball and tubes down as far as the division marked 1000.

The first step consisted in filling this vessel with dry air, which was done in the following manner: into the extremity of the graduated tubular portion, a cork pierced by a small tube, open at both ends, was inserted, and this tube was then connected with the orifice of a table air-pump usually occupied by a syphon gauge. The stop-cock was now connected with one end of a long tube, packed with fragments of fused caustic potash, while the other end of this tube was attached by means of a slip of caoutchouc to a second tube passing through an air-tight cork fixed in one of the mouths of the bottle, at present used for the inhalation of chlorine. This bottle being charged with oil of vitriol, and the orifice of the plate of the pump being closed, the pump was worked, and a current of air was thus drawn through the glass vessel for about fifteen minutes, which in passing through the oil of vitriol, and over the fused potash, was deprived of all hygrometric moisture. The included air being now absolutely dry, the stop-cock was closed, and the small tube connecting the air vessel with the pump having been drawn out in the middle, and sealed hermetically by means of a spirit lamp, the air apparatus was separated from the potash tube, and transferred to a tall jar containing mercury, after which the sealed end of the small glass tube was broken beneath the surface of the quicksilver. The apparatus, however, being now completely filled, it became necessary to remove some of the air, and this was done by opening the stop-cock very gradually, care being taken that during this manipulation the external mercury should be higher than its level within the tubular portion. The entire was then placed in a small room, the temperature of which was found not to vary more than one degree Fahrenheit during the twenty-four hours, the stop-cock having been first attached to one extremity of a string, which was carried over a fixed pulley placed in the ceiling, and whose other end carried a counterpoise by which the air vessel was kept in a vertical position, and the observer was enabled readily to bring the mercury within and without to the same level, before he registered the volume of the included air.

On the next day, after the apparatus was mounted, and the four following ones, the volume of the dry air, its temperature, and the existing pressure were accurately noted. This pressure, which was measured by a portable barometer of Newman's, having undergone a variety of corrections, for the capacity of the cistern compared to that of the tube, for the excess of the temperature of the quicksilver over 32°, for capillarity, and for a constant error by which I found my barometer affected, when compared with the standard instrument in the Observatory of Trinity College, I reduced by calculation in each instance the observed volume of air to what it would be at 32°, and under a pressure of 30, using for the expansion of air the corrected coefficient $\frac{1}{493}$, which has resulted from the experiments of Rudberg, and thus obtained the following numbers, which, it will be observed, differ very little from each other :—

1	911·11
2	911·85
3	910·21
4	913·30
5	911·72

911·64, therefore, the mean of the five observations, may be assumed as the true volume of the included dry air, at 32°, and under a pressure of 30.

The volume of the dry air being determined, the next step was to charge it with moisture. In order to accomplish this, the air vessel was lifted by means of the string, so as that the mercury within should be about an inch higher than the external mercury, and distilled water was then poured into the upper cavity of the stop-cock, so as completely to fill it. The stop-cock was now cautiously turned, so as to admit the entrance of the moisture *guttatim*; and more water being occasionally poured on, this manipulation was repeated until the mercury within came to be covered by a film of water of about one-tenth of an inch in thickness. The stop-cock was now closed, and the apparatus being lowered, the whole was left to itself until the following day, when the first of a series of observations, continued for twenty successive days, was made, each comprehending the volume of the moist air, the pressure, and the temperature both of the air and of the mercury in the barometer. To deduce from these by the formula $f = \frac{v' - v}{v'} \times p$, the force of vapour, it was necessary, in the first instance, to apply to p all the corrections already explained, and in addition to raise 911·64, the volume of the dry air, to what it would be at the temperature and pressure of the moist air, as noted in each observation. But, as this involved tedious arithmetical computations, and as the thermometer during the performance of the twenty experiments varied only about 15°, I came to the resolution, being at the time upon the eve of leaving town for a couple of months, to postpone the calculations until I should be possessed of *data* applicable to the solution of the problem I had undertaken, throughout a more extended range of temperature.

Accordingly, in November last, I resumed the subject with the very same apparatus, which had been left *statu quo* in the interval, and succeeded in completing a series of forty-five additional observations, extending nearly as low as 32°, and which I had every reason to expect would lead to satisfactory results. Upon, however, submitting the whole to calculation, I have been led to the mortifying conviction, that in consequence either of the absorption of the oxygen by the mercury and brass-work, or some accident which befel the apparatus during my absence from town, the entire of the latter series of observations is of no value, as they lead to results for the force of aqueous vapour, which are certainly greatly below the truth. Upon the present occasion, therefore, I can direct attention only to the observations made in July and August last. These are contained in the following table, and, as has been already stated, they amount to twenty in number, the highest temperature having been 65°, and the lowest 49°·6. The numbers in the last column represent the bulks which the 911·64 volumes of dry air would have, if reduced to the temperature t, and the corrected pressure p.

TABLE I.

v'.	t.	p observed.	Temperature of barometer.	p corrected.	911·64 reduced to t and p corrected.
1001	60·4	29·450	59·9	29·430	982·82
1001·5	59·8	29·364	60·1	29·338	984·77
997	60	29·548	60	29·524	978·94
984	59·1	29·822	59·5	29·807	967·97
977	58·4	29·980	58·6	29·971	961·38
984	58·4	29·780	58·9	29·767	967·97
991	59	29·624	59·4	29·607	974·33

TABLE (*continued*).

v'.	t.	p observed.	Temperature of barometer.	p corrected.	911·64 reduced to t and p corrected.
983·5	59·4	29·862	59·8	29·847	967·23
979·5	60·2	30·100	60·6	30·086	962·69
977·5	61·2	30·132	61·3	30·165	960·35
983	61·6	30·05	62·2	30·037	965·18
973·3	62·2	30·230	62·4	30·212	960·69
978·4	61·6	30·214	62·2	30·197	960·06
983·5	63·1	30·156	63·6	30·131	964·93
987·5	64·3	30·130	64·7	30·104	968·01
991	64·1	30·032	64·6	30·005	970·83
994·5	64·8	29·989	65	29·961	973·55
994·5	65	29·972	66	29·940	974·61
989	65·2	30·152	66·5	30·120	969·12
1000	64·8	29·834	65	29·306	978·62

From the first, last, and second last columns of the preceding table, the force of aqueous vapour has been calculated in the manner already explained. The values thus obtained are exhibited in the second column of Table II. Column 1 contains the temperatures; column 3 the tensions, as deduced from Dalton's experiments; and column 4 the same as given by Kämtz.

TABLE II.

1.	2.	3. Dalton.	4. Kämtz.
60°·4	·5345	·5302	·5125
59·2	·4908	·5197	·5023
60·	·5348	·5232	·5061
59·1	·4855	·5077	·4893
58·4	·4917	·4960	·4768
58·4	·4849	·4960	·4768
59·	·4980	·5060	·4875
59·4	·4937	·5128	·4949
60·2	·5169	·5265	·5093
61·2	·5292	·5444	·5261
61·6	·5445	·5517	·5343
62·2	·5412	·5628	·5458
61·6	·5660	·5517	·5343
63·1	·5689	·5798	·5615
64·3	·5941	·6033	·5860
64·1	·6107	·5993	·5824
64·8	·6311	·6133	·5949
65·	·5988	·6173	·5985
65·2	·6054	·6214	·6029
64·8	·6372	·6133	·5949

When the corresponding numbers in the three columns are compared, it will be at once observed, that the values of *f*, investigated by the method just explained, are somewhat less than those extracted from the table I have been hitherto in the habit of using; but that they are considerably greater than the values of Kämtz, the differences being generally better than twice as great in the latter instance as in the former. This will be more manifest by taking a mean of the different results in column 2, and comparing it with the force of vapour corresponding to the same temperature as given in the two other tables. Now, the mean of the temperatures is 61°·63, the quotient got by dividing their sum by twenty. But the corresponding mean value of *f*, in column 2, must be differently calculated, seeing that the temperature and the corresponding tensions of the vapour augment at a very different rate. For temperatures, in fact, in arithmetic progression, the corresponding tensions are in geometric progression, and, although this is well known to be but an approximate law, it may be considered as rigorously true for the limited range of temperature within which my experiments have been made. To calculate, therefore, the mean force of vapour, as deducible from the numbers in column 2, and which must correspond to the temperature 61°·63, it is only necessary to add together the logarithms of the numbers in this column, and divide their sum by twenty, and the quotient will be the logarithm of the mean. When this process is gone through, the mean logarithm is found to be ·73699, and the corresponding number ·54575. The following, therefore, are the tensions of aqueous vapour at 61°·63, as deduced from my experiments, and as extracted from the tables of Dalton and Kämtz.

	My experiments.	Dalton.	Kämtz.
61°·63 ……	·5457 …….	·5523 ……	·5349

Difference between Dalton's number and mine ….. = + ·0066
Difference between Dalton's number and that of Kämtz = + ·0174.

It thus appears, that the result at which I have arrived is somewhat less than the Daltonian number, but considerably greater than that given by Kämtz; and that, therefore, my experiments, as far as they have been discussed, give at least a *prima facie* countenance to the opinion, that the values of the elastic force of aqueous vapour, as given by the latter philosopher, are, at and about 61°·63, below the truth.

Before, however, this conclusion can be considered as fully established, and before we can judge correctly of the amount of the errors by which his table is affected, it will be necessary to inquire whether the thermometer I have employed be a true one. This essential inquiry I have been enabled to institute by my friend Professor Lloyd, who has put into my possession, for the purpose, a thermometer given him by Professor John Phillips, together with a table of differences between it and the standard thermometer belonging to the Royal Society. Upon a comparison of the two instruments, I find, that at and about 60°, the thermometer I have employed stands ·6 of a degree higher than that lent me by Professor Lloyd, while the latter stands ·3 of a degree higher than the standard in possession of the Royal Society; so that the indications of my instrument are at 60°·9-10ths of a degree higher than the truth. If such be the case, ·5457, instead of being the force of vapour at 61°·63, is the force at 61·63 — 0·9 = 60°·73; and to compare the result of my experiments with the tables of Dalton and Kämtz, it is only necessary to extract from these the values of the force of vapour corresponding to the temperature 60°·73.

	My experiments.	Dalton.	Kämtz.
60°·73	·5457	·5361	·5157

Difference between Dalton's number and mine. . . . . — ·0096.
Difference between Dalton's number and that of Kämtz . + ·0184.

The consideration, therefore, of the error of my thermometer, and the allowance made for it, only strengthens the conclusion already arrived at; and I do not now feel any difficulty in giving it as my deliberate opinion, that the table of the force of vapour given by Kämtz is, within the atmospheric range of temperature, erroneous, his values being all too low.

June 14, 1841.—The Rev. H. Lloyd, V.P. read a "Note on the mode of observing the vibrating Magnet, so as to eliminate the Effect of the Vibration."

The following modification of one of the methods proposed by Gauss for the attainment of this end, appears to combine the greatest number of advantages; namely, *to take three readings, at the times*

$$t - T, \quad t, \quad t + T;$$

t being the epoch for which the position of the magnet is desired, and T *its time for vibration**. In order to show that this method is adequate, it is necessary to deduce the equation of motion of a vibrating magnet in a retarding medium.

Let X denote the horizontal part of the earth's magnetic force; q the quantity of free magnetism in the unit of volume of the suspended magnet, at the distance r from the centre of rotation; and θ the deviation of the magnet from its mean position. The moment of the force exerted by the earth on the element of the mass, dm, is

$$X\, q\, r\, dm \sin\theta;$$

and the sum of the moments of the forces exerted upon the entire magnet is

$$X \mu \sin\theta;$$

where μ denotes the value of the integral $\int q\, r\, dm$, taken between the limits $r = \pm l$, $2l$ being the length of the magnet.

Again, the velocity being small, the resistance may be assumed to be proportional to the velocity. Accordingly, if ω denote the angular velocity, the retarding force due to resistance, upon any element of the surface, ds, at the distance r from the centre of motion, is

$$-K\, ds\, r\, \omega;$$

and the entire moment of this force upon the whole magnet is

$$-K\omega \int r^2\, ds = -K\omega \int \frac{r^2\, dm}{H};$$

---

* In practice, it is sufficient to take the nearest whole number of seconds for the value of T.

where $H = \frac{dm}{ds}$. The ratio H is constant for all bodies of prismatic form; and for these, therefore, the moment of resistance is

$$-\frac{MK}{H}\omega;$$

M denoting the moment of inertia $\int r^2\,dm$.

The differential equation of motion is, therefore,

$$\frac{d\omega}{dt} = \frac{X\mu}{M}\sin\theta - \frac{K}{H}\omega.$$

But $\omega = -\frac{d\theta}{dt}$; and, θ being small, we may substitute θ for $\sin\theta$. The equation thus becomes

$$\frac{d^2\theta}{dt} + \frac{K}{H}\frac{d\theta}{dt} + \frac{X\mu}{M}\theta = 0.$$

Making, for abridgement, $\frac{K}{H} = 2A$, $\frac{X\mu}{M} = B^2$, the integral is

$$\theta = (c\cos\sqrt{B^2 - A^2}\,.\,t + c'\sin\sqrt{B^2 - A^2}\,.\,t)\,e^{-At}.$$

But, A being small, we have approximately

$$e^{-At} = 1 - At;$$

and, if T denote the time of vibration,

$$\sqrt{B^2 - A^2}\,.\,T = \pi.$$

Hence the preceding equation may be put under the form

$$\theta = (1 - At)\left(c\cos\pi\frac{t}{T} + c'\sin\pi\frac{t}{T}\right).$$

Now, let θ_1 and θ' denote the values of θ, when t becomes $t - T$ and $t + T$. It will be seen at once, on substitution, that

$$\theta_1 + 2\theta + \theta' = 0.$$

Hence by combining the three readings according to the preceding formula, the deviation of the magnet from its mean position, arising from the vibratory movement, is completely eliminated; and it will readily appear that the same result may be attained by any greater number of readings, taken and combined according to the same law.

Now, let the value of θ contain an *additional* term, $+ pt$, proportional to the time: or, in other words, let us suppose that there is a *progressive* change of the declination, which may be regarded as *uniform* during the whole interval of observation. It is then manifest that $\theta_1 + 2\theta + \theta' = 4pt$; and accordingly that the quantity

$$\tfrac{1}{4}(\theta_1 + 2\theta + \theta')$$

will give the mean place of the magnet corresponding to the epoch t.

The supposition of a uniform change can, however, be regarded as an approximation to the truth, only when the interval of time between the first and last reading is very small, in comparison with the interval between the successive maxima and minima, in the fluctuations of the irregular movement. Hence, we may conclude, that it is important, in the first place, to employ three readings in preference to any greater number; and, secondly, that it is desirable that the time of vibration of the magnet itself should be as small as possible, consistently with the accuracy of its indications in other respects.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 61.]

January 14, 1842.—I. Observations of Halley's Comet, made at the Observatory of Geneva in the years 1835 and 1836. By M. Müller, under the direction of M. Gautier, Director of the Observatory. Communicated by Sir J. F. W. Herschel, Bart.

These observations were made on fifty-two nights, beginning with August 31, 1835, and ending with May 7, 1836; of which thirty-one were before the perihelion passage of the comet, and twenty-one after the passage. The instrument used is an equatoreal of Gambey, whose telescope has an object-glass of four French inches diameter, and of forty-two French inches focal length. The declination circle and the hour circle of the instrument are each thirty inches in diameter; the former being divided to every three minutes of a degree, and by means of its verniers giving arcs of 3″; and the latter being divided in time, and by means of its verniers giving the fifth part of seconds of time. The times were taken with a clock by Lepaute, which was every evening compared with the transit clock. The index corrections, obtained chiefly by observations of stars found in the Astronomical Society's Catalogue, and whose observed places were compared with the places taken from that Catalogue, and from Pond's Catalogue of 1112 stars, were very consistent throughout the whole series of observations, and show that the firmness of the instrument, as well as its state of adjustment, were highly satisfactory. Absolute observations of both elements were obtained in every instance by reading off both circles; this method being preferred by M. Gautier to differential observations with a micrometer. A reticular micrometer, made of fine plates of metal, was used, the faintness of the comet scarcely ever admitting of any illumination of the field.

In the reduction of the observations, the mean refractions were computed for all the observations of the comet and the comparison-stars; and the instrumental right ascensions and north polar distances are given, cleared of the effects of them. The index corrections obtained from all the observations of stars are also given. It is, however, left to those who may be desirous of using the observations of the comet to apply them, and also the effects of parallax, to the observed places.

The height of the observatory above the level of the sea (above 400 metres) caused the comet to be visible at this observatory longer than at most other places in Europe; and the author hopes that the circumstance may render the latter part of the series especially valuable, the southern position of the comet and the unfavourable state of the weather causing the observations of it to be in general very scarce, after its perihelion passage.

II. Note on the Masses of Venus and Mercury. By R. W. Rothman, Esq. The following is the conclusion of this note, the whole of which is given in the Society's Monthly Notices for January.

On the whole, it is very remarkable that the planetary masses given in the *Mécanique Céleste* (vol. iii. p. 61), satisfy the secular motions affecting the orbit of Venus much better than the masses of later astronomers. It appears that in later times the mass of Mercury has been too much increased, and that of Venus too much diminished. What has been previously remarked concerning the masses of Venus and Mercury is confirmed by the motion of the node of Mercury. If this motion be calculated by theory with the masses of the *Mécanique Céleste,* the result agrees almost exactly with the motion determined from observation by Lindenau.—See his *Tabulæ Mercurii,* p. 9.

III. Observations of the Immersion of p^2 Leonis behind the Dark Limb of the Moon. By R. Snow, Esq.

The observed Ashurst sidereal time of the immersion was 15^h 37^m $23^s{\cdot}9$. The observation was made with a power of 75 on the telescope of the five-feet equatoreal, under very favourable circumstances.

IV. Extract of a letter from Professor Encke to Mr. Airy, dated 20th December, 1841. Translated from the German. Communicated by G. B. Airy, Esq. This communication will be found, entire, in the preceding volume, p. 137.

V. Comparisons of the Planet Venus in Right Ascension and N. P. D. with the Star A. S. C. 423, made with the Equatoreal Instrument of the Observatory at Ashurst, on April 9, 1841. By R. Snow, Esq.

The equatoreal instrument employed for these observations is of Fraunhofer's construction, and furnished with clockwork; the object-glass is of five feet focal length, and of four inches aperture. It is supported on a very firm pier, and retains its position very well.

The observations were made with a position micrometer, adjusted for transit and declination observations. They consist of thirty transits of the star and of the first limb of Venus over the meridian wire, and of nine micrometrical measures of the differences of N. P. D. of the star and the south limb of the planet: the corrected sidereal times of the observations are given.

The value of a revolution of the micrometer-screw had been determined by 400 transits of stars near the equator. Measures of the semidiameter of Venus were made at the same time, by which it was found that the measured value exceeded the tabular value given in the Nautical Almanac by $3''{\cdot}1$.

The circumstances of the observations were favourable.

VI. Reduction of Mr. Snow's Observations of Venus and the Star A. S. C. 423, with some remarks upon the employment of equatoreals in Planetary Observations. By the Rev. Richard Sheepshanks.

Mr. Snow's observations admitted of being so grouped as to furnish four sets of comparisons in right ascension and five sets in declination. The effects of parallax and refraction were computed by the formulæ used at Greenwich (Greenwich Observations, 1836, pages lxiv. and lxv.). The right ascension of the star was taken from Lord Wrottesley's Catalogue, the declination from the Astronomical Society's Catalogue, and the semidiameter of Venus from Mr. Snow's Observations; and thus the right ascension and declination of the planet were obtained for the Ashurst sidereal times of observation and compared with the places interpolated from the Nautical Almanac for the same times. The resulting corrections to be applied to the right ascensions and declinations of the Nautical Almanac are as follow :—

	Right Ascension.	Declination.
	s	''
	−1·10 from 15 obs.	+3·1 from 1 obs.
	−1·32 ... 5 ...	+5·4 ... 1 ...
	−1·25 ... 5 ...	+5·3 ... 4 ...
	−1·27 ... 5 ...	+5·2 ... 2 ...
		+3·6 ... 1 ...
Mean	−1·19 ... 30 ...	+4·9 ... 9 .—

The mean epoch is about 8^{h} 30^{m} Greenwich mean solar time.

The author remarks generally with respect to the treatment of such observations, that they may be boldly grouped without sensible error, so as to make one reduction serve for a considerable number of observations; and that to ensure the greatest facility for grouping, the observations of one element (if both cannot be made simultaneously) should be repeated several times as rapidly as possible alternately with similar sets of observations of the other element.

With respect to the value of such observations, the results above given will show that an equatoreal, when thus used, is no mean rival to meridian instruments. The star can be subsequently determined with any required degree of accuracy, and the observations can be made with as great freedom from constant error with an equatoreal as in the meridian. In this latter respect, indeed, the power of repetition gives to the equatoreal a great superiority, and may be made to counterbalance the disadvantages arising from want of steadiness. The last-named quality can, however, in most instances, be obtained in as great a degree as is requisite. The hour-circle being firmly clamped, if the instrument be well balanced, sudden changes can arise only from careless handling.

The supposed uncertainty and instability of the adjustments are probably the principal obstacle to the free use of equatoreals in England; but the author considers that most equatoreals can be adjusted very nearly, and that when ordinary care has been taken, the position remains sufficiently permanent; and it is certain that when rationally used, the effect of any unavoidable derangement is so nearly annihilated as to be quite insensible. The *difficulty* of performing the adjustments of an equatoreal is very trifling, if it be methodically undertaken, and the residual errors much smaller than would at first sight seem possible. With well-turned collars and pivots an error of half a minute, arising from flexure or other causes, must be looked upon as an impossible quantity, in which case the differential effects upon objects in the zodiac might be disregarded. With respect to methods of observing, the author recommends that the telescope be moved in declination like a transit, in order that the star and planet may pass over the same part of the wire. In this case reliance is placed only on the adjustment of the cross-axis; but when the declination is not changed, it is presumed that the position of the wire is correct; and this can be ascertained with only a moderate degree of certainty. In equatoreals which can be reversed in every position, the observations should be made, one group in one position, and the second in the position reversed. The best wiring for such observations, the author considers to consist of three, five, or seven immovable wires, at equal distances, and parallel to the meridian, transit-wires, in fact, and seven equidistant wires at right angles to these, at 5′ interval, the plate which carries the latter wires being moved by a micrometer-screw. The advantages of this system are a saving of time in screwing the micrometer, less wear of the screw, and less dependence on it for large intervals.

Thus far it has been shown, that an equatoreal instrument may be made to rival meridian instruments, by the bestowal of a little more time and trouble; there are, however, many cases where the equatoreal is more convenient, and many where it can, and the others cannot be used.

A planet which comes to the meridian at a late or inconvenient hour of the night may be observed several hours earlier with the equatoreal. In so variable a climate as ours, it is not too much to say, that the number of good planetary observations might be thus very much increased; and if an equatoreal were steadily directed to this object in the southern hemisphere, to meet the case where the planet has considerable south declination, we should soon have the materials by which the present sufficiency of theory might be satisfactorily tested. The superior planets cannot always be observed in full daylight with large meridian instruments, yet equatoreals of even a small size might be made to determine their places with great accuracy after sunset. Again, large equatoreals, which are now tolerably abundant, might take charge of the minor planets. Micrometrical observations only have been taken notice of in the preceding remarks, the divided circles of the instrument being considered only as finders, and for performing the adjustment, though in some instruments they are large and good enough to be used in differential observations. Still the proper use of the equatoreal is the ascertaining of small differences by means of the micrometer and time.

In conducting the observations, the author recommends that there should be made each night two or three transits of the star of comparison, and of two other stars, one above and one below it a few degrees, the instrument being clamped in right ascension, by which means it would be made evident whether the derangement of the adjustment had any sensible effect upon the place of the planet. It is the want of observations to *accuse* derangement which makes the stars observed as moon-culminators less satisfactory than if they were more widely spread in declination.

With respect to observations of the moon, the author mentions one set, originally suggested by Struve, but never carried into effect. There are three observations which might be made when a bright star is occulted by or reappears from under the moon's bright limb:—

1. The time of disappearance or reappearance of the star.

2. Micrometrical measures of distance between the star and the moon's bright limb, the clock-work and the wire micrometer with the slipping piece being used.

[This is the common observation of distance, and might be usefully applied to the case of a near approach.]

3. Differences of right ascension between the moon and star, the hour-circle being clamped as in ordinary transit observations.

If the place of the moon be computed from these three observations, we *ought* to arrive at the same result; and if we do not, the difference between the first and second result arises from the moon's irradiation, and will give a measure of it; also a difference between the second and third results would show some error in the mode of taking the transit of the moon's limb, which is at present rather a doubtful point in practical astronomy. If by certain corrections, constant either to the observer or the telescope, these results can be made to agree in *each* case, and *always* the longitude might be determined in a shorter period, though with more calculation than at present, and a greater certainty be obtained from transits of the moon's limb.

In conclusion, the author hopes that the attention of persons who possess good equatoreals may be directed to the planets whenever those bodies are favourably situated with respect to an observable star. The adjustment is really nothing, and if pairs of stars above and below be observed, any error arising from mal-adjustment can be ascertained and allowed for. The artist will take care, if warned, that the cross-axis shall be at right angles to the polar axis, and the reductions, in ordinary cases, are very trifling, especially if by judicious grouping one reduction is made to serve for several observations.

INSTITUTION OF CIVIL ENGINEERS.

May 3, 1842.—"Description of the Tunnels, situated between Bristol and Bath, on the Great Western Railway, with the methods adopted for executing the works." By Charles Nixon, Assoc. Inst. C.E.

The works described in this paper comprised a large quantity of heavy earth-work in tunnels, &c.; they were commenced in the spring of the year 1836, and terminated in the year 1840. The whole of the tunnels are 30 feet in height from the line of rails, and 30 feet in width; they are curved to a radius of about 120 chains; the gradient of that part of the line is four feet per mile. The strata through which they were driven consisted generally of hard gray sandstone and shale, with the gray and dun shiver, &c.; in a few places only, the new red sandstone and red marl were traversed. Every precaution was taken for securing the roofs, by lining them with masonry where the nature of the strata demanded it, and in some places invert arches were turned beneath.

Driftways were driven before the tunnels were commenced, and shafts were sunk to enable the work to proceed at several points simultaneously. The modes of conducting the works by these means are fully described, with all the difficulties that were encountered. The construction of the centres is given, with the manner of lining the arches with masonry, which is stated to be what was termed "coursed rubble;" but was of a very superior description, and in every respect similar to ashlar-work.

The author offers some remarks with regard to the expense of working tunnels by means of centre driftways. He states this plan to be costly, and in many instances without corresponding advantages, on account of the difficulty of keeping the road clear for the waggons. He recommends that when driftways are used they should be on the lower side of the dip of the strata, as the excavation would be facilitated, and the road would be kept clearer. In long tunnels he has found the cheapest and most expeditious mode of working to be by excavating the centre part from shafts, and both the ends (together if possible) from the extremities after the open cuttings are made. The drawing accompanying the paper gave a longitudinal section of all the tunnels, and showed to an enlarged scale several transverse sections of them, where the variations of the strata rendered either partial or entire lining necessary.

In answer to questions from Mr. Vignoles and other members, Mr. Nixon explained that the extra number of shafts had been required in order to enable the works to be completed within a given time: there had not been any accidents during his superintendence, but subsequently one of the shafts had collapsed. The cost of driving the driftways, the dimensions of which were 7 feet wide by 8 feet high, was ten guineas per yard lineal. He then described more fully his proposed plan of cutting the driftways on the lower side, instead of the centre of the tunnel, and stated the advantages chiefly to consist of a saving in labour and gunpowder, as a small charge sufficed to lift a considerable mass of rock when acting from the dip: the road was also less liable to be closed by the materials falling into it when the enlarged excavation proceeded from one side instead of upon both sides.

Dr. Buckland, after returning thanks for his election as an honorary member of the Institution, expressed his gratification at the prospect of a more intimate union between engineering and geology, which could not fail to be mutually beneficial, and cited examples of this useful co-operation in the cases of railway sections, and models that had recently been furnished by engineers to the Museum of Œconomic Geology.

He then proceeded to remark upon the geological features of the South-Western Coal-Field near Bristol and Bath, which had been described by Mr. Conybeare and himself, in the Transactions of the Geological Society of London (1824).

Some of the tunnels near Bristol are driven in the Pennant grit of the coal formation, where it is thrown up at a considerable angle, and composed of strata yielding slabs and blocks of hard sandstone used extensively for pavement.

In traversing such inclined and dislocated strata, the engineer's attention should, he conceived, be especially directed to the original joints that intersect the beds nearly at right angles to their planes of stratification, and also to the fractures produced during the movements they have undergone. These natural divisions and partings render such inclined stratified rocks unworthy of confidence in the roof of any large tunnel, and liable to have masses suddenly detached.

Inclined strata of a similar sandstone are perforated by many tunnels on the railway near Liège, in nearly all of which the roofs are supported by brick arches.

It has been found impossible to make the tunnels through lias and red marl without continuous arches of masonry.

In any of the tunnels which have been carried through strata of the great oolite, the parts left unsupported by masonry would, in his opinion, be peculiarly liable to danger, because even the most compact beds of oolite are intersected at irregular intervals byloose joints at right angles to the planes of the strata, and occasionally by open cracks: and it is to be feared that the vibration caused by the railway carriages would tend eventually to loosen and detach these masses of stone.

He apprehended still greater danger would exist in tunnels cut through the loosely joined strata of chalk, unless they are lined throughout with strong masonry; and even that, in a recent case, had been burst through by the weight of the incumbent loose chalk coming suddenly upon the arch.

In open cuttings through chalk, where the numerous interstices and the absence of alternating clay-beds prevent any accumulation of water, there is little chance of such frequent landslips as occur where beds of stone, gravel, or sand rest on beds of clay; but until the side walls of chalk are reduced to a slope at which grass will grow, they will be subject to continual crumblings and the falling down of small fragments, severed by the continual expansion and contraction of the chalk, under the destructive force of atmospheric agents, and chiefly of frost.

In open cuttings, where the inclination of the strata is towards the line of rails, the slope should be made at a greater angle than if the strata incline from the rails; if this be done, fewer landslips will occur from accumulations of water between the strata thus inclined towards the rails; and such slips may be further guarded against by minute and careful observation of the nature of the individual strata, and a scientific application of subterranean drains at the contact of each permeable stratum with a subjacent bed of clay.

Tunnels can be safely formed without masonry in unstratified rocks of hard granite, porphyry, trap, &c., and in compact slate rocks; also in masses of tufa, such as cover Herculaneum, and are pierced by the grotto of Pausilippo near Naples; but, in his opinion, wide tunnels driven in stratified rock could not be considered secure unless they were supported by arches.

Mr. Sopwith confirmed the remarks on the importance to the civil engineer of a knowledge of the geological character of the strata through which tunnels or open cuttings were to be made: the cost was materially affected, as well as the stability of the works. The angle of inclination and the lines of cleavage should be carefully studied: on one side of a cutting the slope might be left steep, and all would be firm and dry; whilst on the other, if the same slope was adopted, all would appear disintegrated and wet, and a series of accidents would be the necessary consequence. He could not sufficiently urge the importance of a more intimate connexion between the geologist and the engineer.

LONDON ELECTRICAL SOCIETY.

[Continued from p. 313.]

Oct. 18, 1842.—The Chairman announced that Walter Hawkins, Esq., M.E.S., F.Z.S., &c., had presented the Society with a third specimen of the *Gymnotus Electricus*; but which, like the two former, has not survived the voyage. It is now undergoing dissection, the result of which will be laid before the Society. Mr. Hawkins intends persevering until he succeeds in his desire to present a living specimen to the Society.

A letter to the Secretary from Mr. Phillips, M.E.S., was read, containing "the particulars of a fatal accident by lightning at St. Blazey." Some children had taken refuge from a storm in a toll-house, near which was an elevated crane, and also a comparatively lofty house. The electric matter discharged itself, not on either of these (apparently) better objects, but burst upon the low hut, and in its passage to the earth killed two of the children, and hurt others. From the drawing which accompanied this letter, it appears that the toll-house was immediately at the edge of a rivulet. The lightning divided itself in its passage down the house, first entering by the soot of the chimney. The letter also contains an account of the damage done to a ship at Par by the same storm. The top-mast was shattered to pieces; a large piece was knocked out of the *lower* part of the mainmast; the rupture occurred exactly at the termination of a chain hanging from the cross-trees, the said chain having protected the upper portion of the same mast. Several men were knocked down. The crew spoke of a suffocating smell of sulphur.

A translation, by Mr. Walker, Hon. Sec., of M. Becquerel's first observation "On the Electro-Chemical Properties of Simple Bodies, and on their application to the Arts," was then read. The author speaks of electro-chemistry as being "a bond between physics and chemistry." He says, that formerly our experiments were carried on by large, but now by small series of Volta's pairs, and thus are our operations easier of practice. He intends treating on all simple bodies, beginning with the metals, and of those with gold. He alludes to certain principles established in former papers, and purposes showing the application of electro-chemistry to the arts, as in assaying, gilding, &c. He dwells in his introduction upon the chemical theory, and adduces two important facts in confirmation of its truth;—1st, that there is no chemical action without a considerable disengagement of electricity; 2nd, that a Volta's pile, charged with a liquid not acting chemically on either of the two elements of which each body is composed, does not become charged, that is, produces neither current nor electricity of tension. If one of the two elements is attacked, even very feebly, by the liquid, the effects of current and those of tension immediately follow. As the chemical action increases, so do the electrical effects. He offers an observation, due to his son Edward, which he considers of much weight in favour of this theory. "When one substance acts on another, under the influence of light, electrical effects are produced, as in all chemical reactions, which effects are manifested so long as this influence remains. If it ceases to exist, there is no longer any sign of electricity, and nevertheless the contact of the newly-formed substances with the metallic plates, still exists, and nothing is changed in the circuit." He then introduces gold, its extraction from the ore, and the modes of assay, illustrated by several experiments of his own upon the ores of the Oural and the Altai, in order to examine the nature and extent of the stamping and washing best fitted to produce least waste. He then adverts to amalgamation, &c., and proceeds to the further execution of his task, at which point the present translation ceases, the remaining portion being reserved for a future meeting.

An abstract of observations on the degree of identity between electrical and chemical affinity, by Mr. Prater, M.E.S., was read.

Mr. Weekes's Electro-Meteorological Journal for September was laid before the Society.

LXX. *Notices respecting New Books.*

The Difficulties of Elementary Geometry, especially those which concern the Straight Line, the Plane, and the Theory of Parallels. By Francis William Newman, Tutor at Manchester College. Longmans.

THE *philosophy* of our mathematical processes is far from being a favorite subject of investigation in this country; though amongst the continental geometers it is cultivated with singular predilection. There are, however, two aspects under which this class of inquiries may be viewed; or more properly, two distinct branches of the inquiry, which seem to require faculties of a considerably dissimilar kind. The first class is that in which the logical character of the several methods is examined, in connexion with the phænomena of the human mind. The character of our *first principles*, and the logic of the early theorems of each branch of pure mathematics, are proposed by this class of philosophers as the immediate subjects of their investigation. The other class, and that the more influential and learned one, proposes to discover the influence of *methods of research* upon the progress of discovery, to classify our knowledge according to its bearing upon this one point, and to generalize, as far as possible, the isolated and incompletely connected propositions which are already known.

Of this latter class M. Chasles is a splendid example; and of the former, Mr. Newman is a very respectable and (which renders it of more value) a very useful one.

The "off-handed" manner in which the fundamental principles of geometry are generally dismissed by systematic writers on the subject, is essential to the general style and objects with which such works are composed, namely, the most brief development of the greatest possible number of geometrical truths in a given space. Still, we think that the general purposes of mental culture would be better studied in making geometry merely one of the illustrations of the phænomena of mind: and in this Mr. Newman has evidently entertained the same views that we do, and as was so forcibly urged by that distinguished master of the philosophy of the human mind, Dugald Stewart; though perhaps we differ from each of them, as they do from each other, on certain points brought under discussion.

In a notice like the present, it would be impossible to give any idea of the details of the work. We would moreover remark, that to the discussion of the fundamental principles of the geometry of the school of Euclid our approbation and recommendation is mainly confined. When the author travels beyond these boundaries he is evidently "not at home," as his acquaintance with the higher branches of modern geometry is evidently very limited, and his criticisms, therefore, of little value. We can, however, with this reservation, and without pledging ourselves to the entire adoption of the author's views and reasonings, most cordially recommend the perusal of the book to the speculative geometer, and urge its careful study upon those who are engaged in teaching the elements of the science for the purpose of cultivating the faculties, rather than of "creating mathematicians by profession."

Logarithmic and Trigonometric Tables, &c. London: Simpkin and Marshall, 1836.

Six years ago a private gentleman residing in the country caused to be printed an edition of Hassler's Logarithmic Tables. By various causes the advertisement of this book was delayed, so that up to the present time it has remained altogether unknown, even to those who take pleasure in collecting and comparing tables. On these facts coming to the knowledge of the writer of this paragraph, he recommended that, considering the length of time which had elapsed, the work should not be brought into notice without some re-examination. In consequence of this recommendation, a well-practised computer in the Nautical Almanac Office was employed to read three thousand of the logarithms of numbers and eight degrees of the trigonometrical portion (all chosen at hazard), and compare them with tables of undoubted accuracy. The consequence was, the detection of only three errors, one in the numbers, two in the sines, &c.; of these three there was only one which an expert user of the tables could not have detected at sight. This being considered, and also the number of errors which were detected in Hassler's book during the printing, it is certain that the work before us must be very correct; as correct, indeed, as any table is likely to be unless it have been first stereotyped and then re-examined, and much more so than most others of the same size.

The work is an imitation of Hassler's, and has the same small octavo form. All the logarithms are to seven decimals. The logarithms of numbers are as usual: in the trigonometrical portion the first and last five degrees are to every ten seconds, all the rest to every *half* minute, with differences for ten seconds annexed. In the first two degrees is added a factor for facilitating the determination of the logarithmic sine or tangent of the fractional part of a second. The type is clear and the paper good. We can decidedly recommend the work, and have we think shown reasons for our confidence.

LXXI. *Intelligence and Miscellaneous Articles.*

On the Law of Double Refraction. By JAMES MACCULLAGH, *Fellow of Trinity College, and Professor of Mathematics in the University of Dublin* *.

HAVING mentioned, in an article† which I sent a few days ago for insertion in the Philosophical Magazine, that I had been led, in following out an hypothesis, to a law of double refraction more general than that of Fresnel, I think it may be well to state very briefly the nature of that law, and to point out the difference between it and the law of Fresnel, especially as I have since observed that the difference is one of a very extraordinary kind, and one which, if it has a real existence (a question which experiment only can decide), may serve to account for phænomena that have seemed hitherto inexplicable.

I have said, in the article referred to, that when the potential V, which is a function of the second degree, is supposed to contain only the squares and products of the derivatives X, Y, Z, X_2, Y_2, Z_2, X_4, &c., we get the law of Fresnel, as well as the law of crystalline dispersion; but if we make the more general, and apparently the more natural supposition, that it

---

* Communicated by the Author.

† On the Dispersion of the Optic Axes, and of the Axes of Elasticity, in Biaxal Crystals. [Inserted in the last Number, p. 293.]

contains also the squares and products of the alternate derivatives X_1, Y_1, Z_1, X_3, Y_3, Z_3, &c., then we get, of course, a different law. Now I find that there will still be two optic axes for each colour, and that the two directions of vibration in a given wave-plane will have the same relation to them as before; while the difference of the squares of the two velocities of propagation will continue proportional to the product of the sines of the angles which the wave normal makes with the optic axes; but the *sum* of the squares of these velocities will be increased or diminished by a quantity proportional to the square of a perpendicular let fall from the centre on the tangent plane of a certain very small ellipsoid, this tangent plane being supposed parallel to the wave. Such is the general result for biaxal crystals; but its bearing will be best perceived by taking the case of a uniaxal crystal, wherein the law of Fresnel reduces itself to that of Huyghens.

In this case the wave-surface will, instead of the sphere and spheroid of Huyghens, consist of two ellipsoids touching each other at the extremities of a common diameter, which coincides with the axis of the crystal; one ellipsoid differing slightly from a sphere, the other slightly from a spheroid. Neither of the rays will be refracted according to the ordinary law, nor will the wave-surface be symmetrical round the axis. As the law of refraction is unsymmetrical, that of reflexion will be so likewise, and thus we may perhaps obtain an explanation of the extraordinary phænomena observed by Sir David Brewster in reflexion at the common surface of oil of cassia and Iceland spar.

It will no doubt appear strange to call in question the accuracy of the Huyghenian law, which is generally considered to be established beyond dispute by the experiments of Wollaston and Malus. But the fact is that no exact experiments have ever been made on the refraction of the ordinary ray. Neither of those philosophers seems to have entertained any suspicion that the ordinary law might be inapplicable to it; they both took for granted that it followed the law of Snellius. But their results seem to be quite consistent with the supposition that the ordinary index, for rays passing in different directions through Iceland spar, may vary in the third place of decimals, perhaps even in the second. The experiments of Rudberg throw no light upon the question, for it happens, oddly enough, that though he had two prisms in every other case, he used only one of Iceland spar; he could not therefore compare the velocities of rays passing in different directions. On comparing his numbers, however, with those of Wollaston and Malus, there is, as Sir David Brewster has observed (Phil. Mag., S. 3. vol. i. p. 8), a "surprising discrepancy," so great indeed as to be quite "alarming." After remarking the difficulty of finding any explanation of it, Sir David concludes that it must arise from the different refractive powers possessed by different specimens. But though this cause must operate in some degree, we cannot tell to what extent it is effective, and the discrepancy may notwithstanding be occasioned, in a great measure, by a deviation from the Huyghenian law. The whole question must therefore be reopened, and the ordinary indices for the fixed lines of the spectrum must be determined by means of different prisms cut out of the *same piece* of Iceland spar.

Whatever the result may be, whether it shall confirm the law of Huyghens, or show that another must be substituted for it—it will at least be useful for science, by removing the uncertainty in which the subject is at present involved.

Trinity College, Dublin, Sept. 24, 1842.

ATOMIC WEIGHTS OF ELEMENTS.

MM. Marchand and Erdmann are at present engaged in a series of researches which seem to prove that Prout's idea that all atomic weights are multiples of that of hydrogen, is correct. They have as yet examined only the following bodies:—

Oxygen..	= 100·	 1	Calcium ..	= 250	 20
Hydrogen	= 12·5	 8	Chlorine ..	= 450	 36
Carbon ..	= 75·	 6	Silver	= 1250	 100
Nitrogen	= 175·	 14	Lead	= 1300	 104

Extract from a letter from Berlin addressed to W. Francis.

ON A VERY CURIOUS FACT CONNECTED WITH PHOTOGRAPHY, DISCOVERED BY M. MŒSER OF KŒNIGSBERG, COMMUNICATED BY PROF. BESSEL TO SIR D. BREWSTER *.

Sir D. Brewster said, he was requested to communicate an account of some remarkable facts connected with the theory of photography. A new process of producing photographic impressions had been discovered by Dr. Mœser of Kœnigsberg; and an account of the discovery had been brought to this country by Prof. Bessel, who received it from the discoverer himself. The subject was most important, and it would have been a great misfortune if the Physical Section had separated without being made acquainted with it. The following were the general facts connected with it:—A black plate of horn, or agate, is placed below a polished surface of silver, at the distance of one-twentieth of an inch, and remains there for ten minutes. The surface of

---

* From the Report of the proceedings of the British Association, Manchester, June 29, 1842.—Athenæum, No. 770. See Dr. Draper's letter on the subject at p. 348 of the present Number.

the silver receives an impression of the figure, writing, or crest, which may be cut upon the agate, or horn. The figures, &c., do not appear on the silver at the expiration of the ten minutes, but are rendered visible by exposing the silver plate to vapour, either of amber, water, mercury, or any other fluid. He (Sir D. Brewster) had heard Prof. Bessel say, that the vapours of different fluids were analogous to the different coloured rays of the spectrum; that the different fluids had different effects, corresponding to those of the spectrum; and that they could, in consequence of such correspondence, produce a red, blue, or violet colour. The image of the *camera obscura* might be projected on any surface,—glass, silver, or the smooth leather cover of a book,—without any previous preparation; and the effects would be the same as those produced on a silver plate covered with iodine.

This paper gave rise to an animated conversation, in the course of which M. Bessel said that he had seen some of the pictures taken by this process, which were nearly, but not quite, as good as those obtained by Mr. Talbot's process.—Sir D. Brewster said, this was the germ of one of the most extraordinary discoveries of modern days; by it there seemed to be some thermal effect which became fixed in the black substance; and not only so, but M. Bessel informed him, that different lights seemed to affect different vapours variously, so that there seemed to be something like a power of rendering light latent; a circumstance which, if it turned out so, would open up very new and curious conceptions of the physical nature of light: on the emission theory, it would be easy to account for this; on the undulatory theory, he could not conceive how it could be possible.—Prof. MacCullagh said, he believed Newton had somewhere thrown out a suggestion, that luminous particles, as they entered into bodies, might be caught and retained, within certain bounds, by continual attractions.—Sir D. Brewster said, that the experiments which he had performed with nitrous gas seemed to strengthen some such view as this, for, at certain temperatures, we had here an instance of a gaseous body as impervious to light as a piece of iron.—Sir J. Herschel thought it a pity to encumber this new and extensive field of discovery now laid open to them by any speculations connected with the theory either of undulations or emissions. He had found that paper could be so prepared, as that the impressions of some colours might become permanent upon it, while others were not; and thus it became possible to impress on it coloured figures by the action of light. He exhibited to the Section a piece of paper so prepared, which, at present, had no form or picture impressed on it, but which was so prepared, that, by holding it in a strong light, a red picture would become developed upon it. He wished much he could prevail on Sir W. Hamilton to explain to the Section a metaphysical conception, which he had disclosed to him, and which seemed to him, though darkly he owned, to shadow forth a possible explanation of many difficulties.—Sir W. Hamilton said, that, appealed to by Sir J. Herschel in this manner, he could not avoid placing before the Section the theory alluded to, however imperfect and obscure. He then explained it; but we regret our inability to express it adequately. It appeared to depend on the conception of points, absolutely fixed in space, and endowed with certain properties and powers of transmission, according to determined laws.—Prof. MacCullagh had indulged in speculations allied to, and, as he conceived, involving this very conception of Sir W. Hamilton, and had even followed out some of its consequences, by reducing it to a mathematical form—the conception was of double points, or poles, transmitting powers—but he had abandoned it as mere speculation.—Sir D. Brewster thought these speculations tended to repress experimental research, and to turn men's minds from what was solid to what was fanciful.—Sir J. Herschel considered that there could be no true philosophy without a certain degree of boldness in guessing; and such guessing, or hypothesis, was always necessary in the early stages of philosophy, before a theory has become an established certainty; and these bold guesses, in their proper places, he conceived, should be encouraged, and not repressed. Sir W. Hamilton's conception, he thought, perfectly clear in its metaphysics, and should not be thrown overboard merely because it was mataphysical.

USE OF IRON WIRE FOR SECONDARY ELECTRO-MAGNETIC COILS.

Mr. J. E. Ashby, B.A., of University College, London, informs us that fine iron wire covered with cotton may be substituted for copper in secondary coils, with an increase rather than diminution of effect, at less than 1-6th of the price, and with a great saving of space. Half a pound of this wire costs 1*s*. 3*d*. and measures nearly 1400 feet.

With secondary coils so constructed, he has been able, he states, to make the magnetic spark pass through nearly 1-100th of an inch between two wires, as in Mr. Crosse's experiment; and by means of a battery of about four square inches negative plate and a length of only 1100 feet in the secondary, to excite a current in the primary coil. Mr. Gassiot, Mr. Ashby observes, used for the same purpose 2100 feet of copper wire and twenty large cells of Mr. Daniell's battery.

NON-CONVERSION OF CALOMEL INTO SUBLIMATE BY THE ALKALINE CHLORIDES.

We have in our last Number adduced the numerous experiments of M. Mialhe on the conversion of calomel into corrosive sublimate. The following notice, denying such change, signed Lepage, is from the *Journal de Chimie Médicale* for September.

M. J. Righini d'Ollegio, in a notice relative to the action of the vapour of water on calomel (*Journal de Chimie Médicale*, Avril 1842), gives the result of an experiment which he performed in order to ascertain if, as had been lately announced, calomel is converted into corrosive sublimate, by the influence of the alkaline chlorides, at the temperature of the human body. M. Lepage states that the result announced by the Italian chemist entirely corroborates his own numerous observations on the same subject; and the following he states to be the results of his experiments:—

1. Calomel which is perfectly free from sublimate, digested with its own weight of hydrochlorate of ammonia, or any other alkaline chloride, in distilled water at a temperature of 100° to 104° Fahr., during 24, 36, or even 48 hours, underwent no change of colour. The filtered liquor did not, by means of any reagent, appear to contain a trace of a mercurial salt.

Some pigeons which were made to drink of this same liquor for several successive days suffered no inconvenience: the calomel lost no sensible weight.

2. The same mixture exposed to a temperature of 122° to 140° Fahr., yielded a liquor which acted precisely in the same way with reagents and on the animal œconomy as the foregoing.

3. By continued boiling, however, and under the influence of a great excess of chloride, the conversion took place, but only partially.—*Journal de Chimie Médicale*, Septembre 1842.

METHOD OF DISTINGUISHING ZINC FROM MANGANESE IN SOLUTIONS CONTAINING AMMONIACAL SALTS. BY M. OTTO.

If solutions of chloride of zinc and chloride of manganese, containing much hydrochlorate of ammonia, be rendered alkaline by solution of ammonia, the addition of the smallest quantity of solution of hydrosulphuric acid precipitates white hydrated sulphuret of zinc, whilst no effect is produced by it in the solution of manganese, more being required to obtain a precipitate of the sulphuret of the latter metal. If acetic acid be then added to the solutions, the sulphuret of manganese dissolves very readily, whilst that of zinc remains undissolved. M. Otto advises the use of hydrosulphuric acid and not hydrosulphate of ammonia, because the latter, always containing persulphuret, may occasion mistakes, since acetic acid separates sulphur from it. If, for example, it be required to determine whether iron filings contain brass, they are to be dissolved in aqua regia, the peroxide of iron is to be precipitated by ammonia, the liquor is then to be acidulated, the copper precipitated by hydrosulphuric acid, and ammonia is then to be added to the filtered liquor, which usually still contains a sufficient quantity of hydrosulphuric acid. If a white precipitate be formed which does not dissolve in acetic acid, it shows that zinc is present. M. Wackenroder has especially recommended the solubility of sulphuret of manganese in acetic acid, to separate manganese from other metals.—*Journal de Pharm. et de Chem.*, Sept. 1842.

ON MM. VARRENTRAPP AND WILL'S METHOD OF DETERMINING AZOTE IN ORGANIC ANALYSES. BY M. REIZET.

M. Reizet has submitted to examination the new process recommended by MM. Varrentrapp and Will, for determining the azote in organic substances. This process is based on the general law of the decomposition of animal substances, by the hydrated fixed alkalies, into water, carbonic acid and ammonia, if they contain azote. It results from the experiments of M. Reizet, that this process is not entirely free from all chances of error. In the first place the mixture of soda and lime retains atmospheric air confined in a peculiar state of condensation; this air cannot be expelled either by a current of gas, nor under the influence of a vacuum. During combustion, the azote of this air gives rise to ammonia, which is added to that coming from the substance submitted to analysis. Faraday has observed that non-azotized organic substances, even charcoal and the metals which decompose water, yield ammonia when calcined with potash in contact with air.

Another chance of error in the process of MM. Varrentrapp and Will results from the circumstance, that the alcohol in which the perchloride of platina is dissolved, reduces this salt to the state of insoluble protochloride; this operation takes place very slowly, it is true, but it is so considerable that the protochloride formed, mixing with an ammoniacal salt of platina, adds to its weight, and consequently sensibly increases the proportion of azote. It is not explained how MM. Varrentrapp and Will always obtained less azote than indicated by theory in the substances which they analysed, since the causes of error in their process tend to give an excess, unless during the operation azote is disengaged either in a free state, or in some other form than of ammonia, or that this gas is not entirely condensed.—*Ibid.*

NEW DOUBLE SALT OF SODA AND PROTOXIDE OF PLATINA.

MM. Litton and Schnedermann, endeavouring to discover an easy and certain method of preparing the double cyanides of platina, passed a current of sulphurous acid gas to perfect saturation through a solution of chloride of platina, and afterwards saturated the liquor with carbonate of soda. They thus obtained a very bulky precipitate, which was nearly colourless, and this, after perfectly washing it, they submitted to an attentive examination; and they found it to be a double salt of soda and protoxide of platina. When dry, this salt is a white powder. It is very slightly soluble in water, and insoluble in alcohol. The usual reagents do not at all indicate the presence of platina in the aqueous solution. If hydrosulphuric acid be passed into it, or if it be mixed with hydrosulphate of ammonia, it does not change even after a long time has elapsed, or by increase of temperature; but if there be added at the same time an acid which decomposes the salt, the liquor becomes slowly coloured at common temperatures, and when heated it soon becomes reddish-brown; and afterwards sulphuret of platina separates. The alkalies do not decompose this salt; when heated with potash or soda, it undergoes no sensible change. Treated in a dry state with a solution of hydrosulphate of ammonia, or of sulphuret of potassium, it suffers no change at common temperatures, but by ebullition it becomes gradually coloured, is eventually completely dissolved; and from this solution sulphuret of platina is precipitated by acids.

Even diluted acids readily dissolve this salt, decomposing it and evolving sulphurous acid. The solution in hydrochloric acid yields crystals of chloride of sodium by evaporation, and by the addition of ammonia a green crystalline precipitate of ammonio-chloride of platina. The solution in sulphuric acid yields, after the requisite evaporation, crystals of sulphate of soda, and assumes the deep colour well known to be owing to the protosulphate of platina. At a certain degree of concentration, metallic platina separates, a property which is well known to belong also to the protosulphate of platina prepared by direct combination.

The solution in nitric acid when evaparated by heat has a deep reddish-brown colour; if to this hydrochlorate of ammonia be added no precipitate is formed, but if the solution be evaporated with the hydrochlorate of ammonia almost to dryness, and water be added to the residue, there remains a great quantity of ammonio-chloride of platina, which does not dissolve. It appears that the reddish-brown colour is owing to the formation of sulphate of platina, a salt, which, as observed by Mr. E. Davy, is not decomposed by hydrochlorate of ammonia, unless they be evaporated together to dryness.

The double salt in question dissolves readily in an aqueous solution of cyanide of potassium, and by evaporating the solution, acicular crystals of cyanide of potassium and platina separate. If this salt be exposed to a temperature of 356° to 392° Fahr., it loses its water completely; and when heated to 464° Fahr. it undergoes no further alteration; but if the temperature be raised still higher, it begins to suffer slight decomposition, its colour becoming deeper. It requires, however, a continued red heat for its complete decomposition, and there then remains a mixture of sulphate and sulphite of soda with metallic platina. The formula of this anhydrous salt is $3\,NaO,\ SO^2 + PtO,\ SO^2$, and that of the hydrated salt $2\,(3\,NaO,\ SO^2 + PtO^2) + 3\,H^2O$*.—*Ibid.*

COMPOSITION OF CONIA.

According to M. V. Ortigosa, conia when completely anhydrous consists of

32 equiv. of	Hydrogen....	199·67	12·55
16 ...	Carbon......	1213·60	76·31
2 ...	Azote......	177·04	11·14
	Equivalents......	1590·31	100·

Pure conia distils without any residue, but if it contains water, a resinous matter is left; its boiling-point is 413° Fahr.

Conia is a powerful base; like ammonia it gives a precipitate with the proto-salts of tin and of mercury, and with the persalts of iron it appears even to expel ammonia from its compounds. It reduces the salts of silver, gives with sulphate of copper a precipitate slightly soluble in water, and very soluble in alcohol and æther.

The precipitate obtained by mixing a solution of bichloride of mer-

---

* M. Liebig had previously obtained a double sulphite of ammonia and protoxide of platina composed according to the formula $2\,SO^3,\ PtO,\ N^2H^6$.—*Chimie Organique de Liebig.* Paris, 1840, p. 102.

cury with conia is insoluble in water, alcohol or æther; the compound is white, pulverulent, and decomposes at 212° Fahr., becoming yellow. If to an aqueous solution of conia one of sulphate of alumina be added, crystals are gradually formed, which with the microscope are easily seen to be octohedrons. These crystals, when they have been carefully washed, blacken if heated on platina foil.—*Ibid.*

MR. LUKE HOWARD'S CYCLE OF EIGHTEEN YEARS IN THE SEASONS OF BRITAIN.

The readers of the Philosophical Magazine will doubtless learn with pleasure that the cycle shows well this year *to the end of September*, viz.—

1842. Nine months rain	17·35 inch.
1824. The same	18·68 inch.
So that we are	1·33 inch.

(only) in arrear for rain.

1842. Average temperature of nine months	50·86°
1824. The same	49·95
So that we appear to have of heat in advance	0·91°

The Villa, Ackworth, Sept. 7, 1842. LUKE HOWARD.

METEOROLOGICAL OBSERVATIONS FOR SEPTEMBER 1842.

Chiswick.—September 1. Constant rain: temperature increasing towards night. 2. Overcast: sultry. 3. Overcast: clear. 4. Cloudy and fine. 5. Foggy: very fine. 6. Very fine: clear. 7. Slight fog: fine. 7—10. P.M. violent thunder-storm, much sheet- and sometimes forked lightning: heavy rain, with some hail: clear at night. 8. Boisterous, with heavy rain. 9. Rain: cloudy. 10. Showery. 11—15. Very fine. 16. Foggy: fine. 17. Cloudy: rain. 18. Fine, with slight haze: rain. 19. Cloudy: showers. 20. Showery. 21. Cloudy and fine: clear. 22. Foggy: cloudy and fine: slight rain. 23. Overcast: heavy rain. 24. Rain: overcast. 25. Slight showers: stormy, with rain at night. 26. Heavy clouds and showers: clear. 27. Overcast: stormy and wet. 28. Fine. 29. Clear: boisterous, with rain. 30. Clear and fine: slight rain. Mean temperature of the month 0·47° above the average.

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

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

Applegarth Manse, Dumfries-shire.—Sept. 1. Very wet morning. 2. Fair but cloudy. 3. Rain P.M. 4. Fine and fair. 5. Thick: rain P.M. 6. Fair but cloudy. 7. Fair and fine. 8. Heavy rain early A.M. 14. Cloudy and moist. 15, 16. Fair but cloudy. 17. Rain A.M. 18. Fair and fine: lightning. 19. Fair and fine: thunder. 20. Fair and fine. 21. Fair and fine: thunder. 22. Fair and fine till P.M.: rain. 23. Rain early A.M. 24. Rain. 25—28. Fair and cool. 29. Fair and cool: a few drops. 30. Fair and cool.

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

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point
1842. Sept.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	30·034	30·003	29·902	29·44	29·68	29·78	29·54	29·62	58·5	62·0	55·0	66	58	55	65	49½	54	56	s.	sw.	nw.	w.	s.	·166	·46	·12	......	·43	57
2.	30·170	30·175	30·075	29·50	29·90	29·99	29·70	29·88	68·7	70·3	58·0	81	58	68	66	60	57	54	nnw.	w.	w.	w.	sw.	·311	......	......	......	·10	64
3.	30·286	30·203	30·153	29·57	30·00	30·02	29·98	30·15	62·5	76·0	60·4	74	53	68	63½	58	54½	49	nw.	w.	w.	w.	e.	......	......	......	......	·23	63
● 4.	30·274	30·197	30·193	29·60	30·15	30·14	30·16	30·20	61·3	70·6	57·3	74	49	63	66½	42	50	50	sw.	nw.	calm	s.	w.	......	·61	......	......	......	.62
5.	30·268	30·196	30·036	29·63	30·05	29·85	30·13	29·95	63·7	70·2	56·4	74	47	61	60½	41	53½	51½	se.	s.	calm	sw.	se.	......	......	......	......	·16	61
6.	30·008	30·025	29·942	29·35	29·80	29·83	29·81	29·91	61·0	71·3	57·6	73	46	63	63	48	55	53½	sse.	w.	calm	w.	w.	......	......	......	......	·36	60
7.	29·928	29·886	29·427	29·37	29·78	29·50	29·85	29·68	62·0	70·8	54·8	73	58	60	64	41	58½	55	e.	e.	calm	s.	w.	......	·55	......	0·87	·04	59
8.	29·466	29·616	29·396	28·78	29·32	29·38	29·52	29·40	63·4	70·4	60·3	62	51	61	63	53	55	54	s.	sw.	w.	ene.	se.	·383	·64	·40	......	......	62
9.	29·574	29·527	29·127	28·98	29·24	......	29·38	29·24	61·7	66·6	54·6	66	53	57·5	64½	49½	56	55	s.	sw.	sw.	....	w.	·594	·06	·06	......	·28	59
10.	29·458	29·577	29·385	28·85	......	......	29·40	29·61	60·7	67·4	56·3	66	51	60·5	...	48	58	51	sw var	w.	w.	....	ne.	·022	·07	·25	......	·25	59
☽ 11.	29·720	29·683	29·655	29·13	......	......	29·76	29·78	59·9	65·0	52·7	67	53	58	...	...	55	53	w.	w.	nw.	....	....	·052	......	·12	......	......	57
12.	29·844	29·930	29·788	29·28	......	......	29·85	29·99	60·3	65·3	56·2	68	45	57	...	...	56	52	nw.	n.	w.	....	w.	......	......	·04	......	......	57
13.	30·144	30·126	30·071	29·60	......	......	29·97	30·10	59·0	66·4	52·4	68	51	58	...	...	56	58	n.	ne.	n.	....	s.	......	......	......	......	·14	58
14.	30·234	30·167	30·149	29·67	......	30·10	30·14	30·20	62·0	66·6	55·0	72	53	62	64½	...	61	58½	n.	ne.	calm	wnw.	sw.	......	......	......	......	......	59
15.	30·216	30·144	30·101	29·66	30·07	29·97	30·10	29·93	61·5	70·0	57·2	74	45	62	65	52	60	57½	n.	e.	calm	sw.	se.	......	......	......	0·37	......	60
16.	30·150	30·087	29·918	29·55	29·92	29·80	29·91	29·84	58·3	69·3	54·8	72	48	61	65	57	60	57	e.	e.	calm	sw.	se.	......	......	......	......	......	59
17.	29·852	29·787	29·720	29·26	29·51	29·58	29·51	29·56	65·7	68·3	56·2	74	51	65	63½	55	58	53	e.	se.	s.	sw.	se.	......	·29	......	......	......	60
18.	29·810	29·757	29·611	29·22	29·63	29·64	29·69	29·82	56·5	71·0	54·4	62	48	58	63½	44	54	48½	n.	se.	calm	s.	sw.	·261	·10	·22	......	......	59
○ 19.	29·644	29·583	29·543	29·07	29·46	29·40	29·69	29·49	61·0	65·4	52·4	68	39	56	63	46	53½	50	s.	s.	calm	se.	ne.	·205	·08	·18	......	......	59
20.	29·570	29·522	29·478	29·04	29·35	29·40	29·66	29·63	58·7	65·4	50·6	63	40	55	63	47	52½	50½	sw.	sw.	calm	ese.	ne.	·033	·02	......	......	·56	55
21.	29·534	29·464	29·458	29·05	29·43	29·48	29·58	29·66	57·7	64·3	49·7	65	38	52	59	44½	51	48½	ssw.	sw.	calm	ne.	n.	·063	·02	......	......	·31	54
22.	29·528	29·494	29·460	29·06	29·49	29·42	29·65	29·68	49·8	62·2	44·4	60	42	48	59	42½	50	54	w.	w.	calm	sw.	n.	......	·01	......	0·20	·06	52
23.	29·534	29·494	29·346	28·98	29·40	29·47	29·73	29·70	52·3	59·7	46·7	56	47	50	62½	47½	53	51	s.	nw.	nw.	ne.	ne.	......	·36	·65	......	·13	51
24.	29·400	29·424	29·341	28·93	29·45	29·52	29·88	29·86	52·4	58·4	48·6	59	52	57	58	47	50	48	e.	ne.	e.	ne.	n.	·630	·06	1·15	......	......	53
25.	29·580	29·612	29·536	29·12	29·67	29·80	29·96	30·11	55·7	57·3	46·6	62	51	54	58	48	49	47½	n.	ne.	calm	ne.	n.	·111	·26	·02	......	......	54
26.	29·866	29·906	29·795	29·35	29·99	30·10	30·23	30·27	57·0	62·7	52·6	62	49	59	57	49	49	51	n.	e.	calm	ne.	n.	·338	·08	......	......	·03	55
☾ 27.	29·931	29·929	29·859	29·45	30·10	30·12	30·30	30·38	52·2	62·0	50·5	58	49	56·5	57	43½	52½	48	nne.	ne.	e.	ne.	n.	·088	·27	......	......	......	50
28.	30·110	30·059	30·047	29·56	30·24	30·29	30·46	30·55	52·7	54·8	50·0	58	48	54	53½	45	48	45	n.	ne.	e.	n.	ne.	·288	......	·03	......	......	50
29.	30·080	30·093	30·026	29·66	30·33	30·35	30·58	30·56	53·5	58·7	49·6	58	45	52	57	42	51	43	ne var	ne.	e.	ene.	e.	......	·04	·22	......	......	49
30.	30·208	30·150	30·138	29·72	30·32	30·27	30·47	30·40	52·3	59·3	47·5	57	44	53	57	41	53	50	ne.	ne.	calm	ne.	w.	......	·01	·26	0·13	·05	47
Mean.	29·881	29·861	29·765	29·31	29·771	29·812	29·887	29·905	58·8	65·6	53·5	66·40	48·73	58·1	61·6	47·7	54·13	51·76						Sum. 3·545	3·99	3·72	1·57	3·13	Mean. 57

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

[THIRD SERIES.]

DECEMBER 1842.

LXXII. *On a Gaseous Voltaic Battery. By* W. R. GROVE, *Esq., M.A., F.R.S., Professor of Experimental Philosophy in the London Institution.*

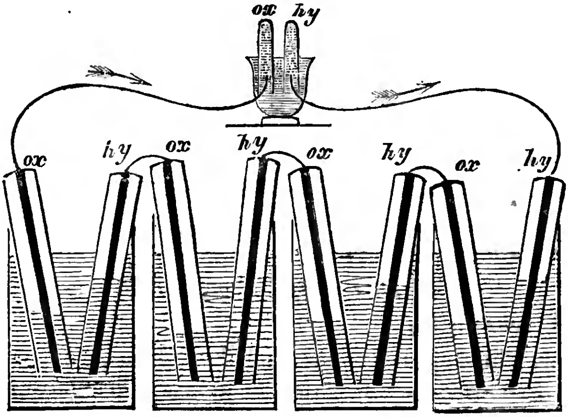
To R. Phillips, Esq., F.R.S.

MY DEAR SIR,

IN the Philosophical Magazine for February 1839 I have given an account of an experiment in which a galvanometer was permanently deflected when connected with two strips of platina covered by tubes containing oxygen and hydrogen. At the conclusion of my notice, I say, "I hope, by repeating this experiment in series, to effect decomposition of water by means of its composition." The next paper of mine published in the same year contains an account of a battery to which the public has since attached my name, and which led me into a different field of research.

In reading over my papers lately for a purpose alluded to in my letter of last month, I was struck with the above sentence. My impression was, that I had expressed a hope not very likely to be realized; but after a few days' consideration I saw my way more clearly, and determined to try the experiment.

As the chemical or catalytic action in the experiment detailed in that paper, could only be supposed to take place, with ordinary platina foil, at the line or water-mark where the liquid, gas and platina met, the chief difficulty was to obtain anything like a notable surface of action. To effect this my first thought was to surround the platina foil with spongy platina precipitated in the usual way by muriate of ammonia. This was suggested to me by the known action of spongy platina on mixed gas, which would by its capillary attraction expose a considerable surface of metal and liquid to the action of the gases. I still think this would be the best mode of effecting the object; but as it was very troublesome in manipulation, I determined to try the platina platinized by voltaic deposition from the chloride, as proposed for a different purpose by Mr. Smee. I therefore caused a series of fifty pairs to be constructed, the form and arrangement of which is given in the annexed figure, where *ox* denotes a tube filled with oxygen; *hy* one filled with hydrogen, and the dark line in the axis of the tube platinized platina foil, which in the battery I constructed was about one-fourth of an inch wide. It is obvious that, by allowing the platina to touch the liquid, the latter would spread over its surface by capillary action and expose an extended superficies to the gaseous atmosphere. The battery was charged with dilute sulphuric acid, sp. gr. 1·2, and the following effects were produced:—



1st. A shock was given which could be felt by five persons joining hands, and which when taken by a single person was painful.

2nd. The needle of a galvanometer was whirled round and stood at about 60°; with one person interposed in the circuit it stood at 40°, and was slightly deflected when two were interposed.

3rd. A brilliant spark visible in broad daylight was given between charcoal points.

4th. Iodide of potassium, hydrochloric acid, and water acidulated with sulphuric acid were severally decomposed; the gas from the decomposed water was eliminated in sufficient quantity to be collected and detonated. The gases were evolved in the direction denoted in the figure, *i. e.* as the chemical theory and experience would indicate, the hydrogen travelling in one direction throughout the circuit, and the oxygen in the reverse. It was found that 26 pairs were the smallest number which would decompose water, but that four pairs would decompose iodide of potassium.

5th. A gold leaf electroscope was notably affected.

6th. The battery was charged with distilled water; the electroscope was affected, and iodide of potassium decomposed.

7th. Although the phænomena were too marked to render it in the least probable that accidental circumstances could have produced the current, still counter experiments were carefully gone through; thus the gases were repeatedly changed, oxygen being placed in the tubes which had contained hydrogen, and *vice versâ.* The effects were equally powerful, and the direction of the current was reversed.

8th. All the tubes were charged with atmospheric air; no effect was produced.

9th. The battery was charged with carbonic acid and nitrogen in the alternate tubes; not the slightest effect observable.

10th. It was charged with oxygen and nitrogen; not any effect.

11th. With hydrogen and nitrogen, slight effects. The difference between this and the last experiment at first struck me as extraordinary, but upon consideration was easily explicable. The liquid being exposed to the air would necessarily absorb some oxygen, and this with hydrogen would give rise to a current. This was proved by the liquid rising in the hydrogen tubes, but not in those containing nitrogen; and, as a further proof, one set of tubes was charged with hydrogen, and the alternate set with acidulated water without gas; a slight current was perceptible: with oxygen and the liquid in alternate tubes there were no effects produced.

12th. As the oxygen and hydrogen were procured in the first instance by electrolysis, and as Dr. Schœnbein in his careful experiments on polarized electrodes supposed the peculiar substance which he has named Ozone to be a principal agent, I caused the tubes to be charged with oxygen evolved from chlorate of potash and oxide of manganese, and hydrogen from zinc and sulphuric acid; the effects were the same.

The tubes were not all of equal size, nor were they graduated; the exact proportional diminution of gas in each tube could not be ascertained with perfect accuracy; both gases did diminish, and the hydrogen so much more rapidly than the oxygen, that my assistant, who was unacquainted with the rationale of the battery, observed that the hydrogen was absorbed twice as fast as the oxygen. Mr. Gassiot is now preparing a graduated battery of this sort, by which the point will be accurately determined; supposing the gases at the electrodes and at the plates exposed to uniform facilities of solution, the quantity evolved should be equal to that absorbed.

Several curious points are suggested by this novel battery.

α. How is its action explicable on the contact theory? I am by no means wedded to any theory, and have constantly endeavoured to look with the eye of a contact theorist upon the facts of voltaic electricity, but I cannot see them in that light; if there be any truth in the contact theory, I either misunderstand it, or my mind is unconsciously biassed. Where is the contact in this experiment, if not everywhere? Is it at the points of junction of the liquid, gas, and platina? If so it is there that the chemical action takes place; and as contact is always necessary for chemical action, all chemistry may be referred to contact, or upon the theory of an universal plenum, all natural phænomena may be referred to it. Contact may be necessary, but how can it stand in the relation of a cause, or of a force?

β. Its phænomena present to my mind a resolution of catalysis into voltaic force, in other words, the action of this battery bears the same relation to the phænomena of catalysis as that of the ordinary batteries does to those of ordinary chemistry. Whether these effects could be produced by other inoxidable metals (such as gold or silver) is an experiment worth trying. The more we examine chemical and voltaic actions, the more closely do we assimilate them. For some mysterious reason three elements seem necessary for very many if not for all chemical actions.

γ. This battery is peculiar in having the current generated by gases, and by synthesis of an equal but opposite kind at both anode and cathode; it is therefore, theoretically, more perfect than any other form, as the batteries at present known, act by one affinity at the anode, and have to overcome another at the cathode.

δ. This battery establishes that gases in combining and acquiring a liquid form evolve sufficient force to decompose a similar liquid and cause it to acquire a gaseous form. This is to my mind the most interesting effect of the battery; it exhibits such a beautiful instance of the correlation of natural forces.

Many other notions crowd upon my mind, but I have occupied sufficient space and must leave them for the present, hoping that other experimenters will think the subject worth pursuing. I remain, my dear Sir, yours very sincerely,

London Institution, Oct. 29, 1842. W. R. GROVE.

LXXIII. *On the Constant Voltaic Battery. By* J. F. Daniell, *Esq., For. Sec. R.S., Prof. Chem. in King's College, London; in a Letter addressed to R. Phillips, Esq., F.R.S., &c.*

My dear Sir,

It appears from Professor Grove's letter, published in the last Number of the Philosophical Magazine, that I was under a *misconception* in supposing that he had derived his battery from principles announced by me; and that my memory was treacherous in suggesting that I had heard him, at a very crowded meeting of the members of the London Institution, admit (with a compliment which was impressive, but doubtless much greater than the occasion required) that it was in following up my train of reasoning that he was led to the construction of the instrument whose wonderful powers he was then about to illustrate. But waving this point of recollection, the error is certainly excusable, inasmuch as the nitric acid battery exactly resembles the constant battery in every particular except the substitution of platinum and nitric acid for copper and sulphate of copper; and an experimentalist might, very obviously, have been led to the change by following up the principle of diminishing contrary electromotive powers and resistances to a current originating with the zinc. Professor Grove, however, states (although he "cannot at this distance of time well describe what effect my experiments had upon his mind") that he cannot acquiesce in the assertion that he was so guided; but that the idea which *immediately* led to the construction of his battery is distinctly stated in the Phil. Mag. for 1839. The experiment referred to, with two strips of gold leaf in nitric and hydrochloric acids, separated by a porous diaphragm, showing that upon contact of the two strips the gold in the hydrochloric acid was dissolved, is certainly a most beautiful one; but the origin of the force must be admitted to be at the junction of the two acids; which, when a path for its circulation is opened, react upon one another, and transfer by their polarization chlorine to one electrode, and hydrogen to the other; the former being taken up by the gold, and the latter by the nitric acid. What this has to do with the nitric acid battery, in which the two acids in contact are the nitric and sulphuric, I really cannot perceive. The origin of the force in this case has always appeared to me to be the action of the zinc upon the dilute sulphuric acid, but Professor Grove may possibly consider it to be still the contact of the two acids. He has, however, stated that he was so led to the construction of his battery, and I can have nothing more to say upon the subject.

It is singular enough that M. E. Becquerel's claim for his father's priority in the discovery of the principles upon which my battery is constructed appears from his reply (also published in the last Number of the Phil. Mag.) to be founded principally upon a similar supposed generation of force at the contact of the two liquids.

If this be its true origin, I at once allow that there is some foundation for the reclamation; but at the same time I must repeat that such an idea never occurred to me; as will be evident to those who will take the trouble to consult my consecutive papers in the Philosophical Transactions: and I must in that case be content with the somewhat mortifying reflection that I was led to a right result by wrong principles.

The matter is, however, now fairly before the scientific community, and having corrected M. Becquerel's inadvertent remark about the priority of Professor Grove's experiments, I will promise you to take up no more of your valuable space with the subject. I remain, dear Sir, very truly yours,

King's College, Nov. 2, 1842. J. F. Daniell.

To R. Phillips, Esq., &c. &c.

LXXIV. *On certain Arguments adduced in the last Number of the Philosophical Magazine. By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., F.C.P.S., &c., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge.*

To Richard Taylor, Esq.

My dear Sir,

The Philosophical Magazine has this moment reached me, by which I am sorry to see that a misprint, or rather a mis-transcription of my paper in the 6th volume of the Cambridge Transactions has led both Mr. Earnshaw and Mr. O'Brien astray. I ought to take the blame of this on myself, and do so; your readers will find my acknowledgement of it at p. 347 of the last Number of your Journal. The three quantities which Mr. Earnshaw copies in p. 341 are *not* equal. I supposed the axis of y to be that along which transmission takes place, and ought to have made the first and last expression equal to n^2, and the middle one to $n_1{}^2$; and so in my own copy it is, but I presume the correction was made with a pen. The equality of these two expressions has been employed by Mr. O'Brien to prove that I do not suppose the axis of y to coincide with the direction of transmission; and if, in applying the equations I had used these quantities as equal, the argument would have been a strong one. But on turning to Camb. Trans., vol. vi. p. 180, it will be seen that I have proved them to be *unequal.* I am truly sorry that this misprint, or mis-transcription, or whatever it may be, has caused so much trouble. It was very natural that it should mislead Mr. Earnshaw, and produce the argument at p. 342 of Nov. Phil. Mag.; but I should have hardly imagined it possible to have deceived Mr. O'Brien, who appears to have perceived (see his P.S. p. 343) that I supposed the axis of y to be in the direction of transmission.

For having given these gentlemen the trouble of arguing the incorrectness of equations which are undoubtedly erroneous (if u is not n_1 in the last line of p. 162), I hope they will accept my apology.

I am, dear Sir, with great respect,
Your obliged Servant,

Edinburgh, Nov. 2, 1842. P. KELLAND.

LXXV. *On the Analytical Condition of Rectilinear Fluid Motion, in Reply to* Mr. Stokes's *Remarks. By the Rev.* J. CHALLIS, *M.A., Plumian Professor of Astronomy in the University of Cambridge**.

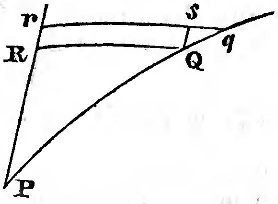
MR. STOKES has brought forward four arguments against a new theorem in hydrodynamics which I have advanced, viz. that fluid motion is rectilinear whenever $udx+vdy+wdz$ is an exact differential. The observations I am about to make in reply will follow the order of the arguments.

1. In the first argument (p. 297) it is contended that my demonstration in the August Number of this Journal is deficient in generality, because it takes no account of the curvature of the lines of motion. I admit the validity of this objection. The geometrical reasoning I have there given proves only that $u\,dx + v\,dy + w\,dz$ is an exact differential when the motion is rectilinear, *if* the surfaces of displacement are surfaces of equal velocity. I have not proved, as Mr. Stokes asserts, that for the case of rectilinear motion the surfaces of displacement *are* surfaces of equal velocity. This is not necessarily the case unless $u\,dx + v\,dy + w\,dz$ be an exact differential.

The following demonstration derived from the equation $u\,dx + v\,dy + w\,dz = V\,dr$, is more to the purpose. In this equation V is the velocity at a point whose coordinates are x, y, z at a given time; u, v, w are the components of V in the directions of the axes of coordinates; and dr is the increment of space in the direction of the motion through the point $x\,y\,z$. The proof of the equation is sufficiently well known.

---

* Communicated by the Author.



Let $u\,dx + v\,dy + w\,dz$ be an exact differential. Then, and not otherwise, it is possible to integrate this quantity, and consequently its equivalent $V\,dr$, from any one point of the fluid to any other. P and Q (in the figure) being any two points in the fluid, let P R be the line of direction of motion through P at a given time, and let Q R represent the surface of displacement through Q at the same time. The integral of $u\,dx + v\,dy + w\,dz$, and therefore that of $V\,dr$, may be taken indifferently along the line P Q, or along P R and R Q. But the integral of $V\,dr$ along R Q is nothing, because by hypothesis this line is on a surface of displacement. Therefore the integral of $V\,dr$ from P to R is *identical* with the integral from P to Q. Hence if S be the integral, the differential coefficient $\frac{dS}{dr}$, which is the velocity at R, is also the velocity at Q. This reasoning applies wherever the point Q is situated on the surface of displacement. Hence this surface is a surface of equal velocity. Draw another surface of displacement indefinitely near the former. Then if $S + \delta S$ be the integral of $V\,dr$ from P to r, the same will be the integral from P to q; consequently, Qs being drawn through Q in the direction of the motion at that point, we have ultimately, $\delta S = \frac{dS}{dr} \times$ the line Qs, and $\delta S = \frac{dS}{dr} \times$ the line Rr. Hence Qs, which is ultimately the interval between the surfaces of displacement at Q, is equal to Rr the interval between them at R. It follows that the surfaces are at all points equidistant, and therefore parallel. A normal to one is therefore accurately a normal to the other, and the lines of direction of motion are consequently rectilinear.

The above reasoning proves that whenever $u\,dx + v\,dy + w\,dz$ is an exact differential the motion is rectilinear. This is the important part of the theorem I have announced, and it is all that there is any occasion to contend for. In my preceding communication I said incorrectly that the exactness of that differential is a *necessary* condition of rectilinear motion. Nothing that I have advanced disproves the possibility of there being rectilinear motion when $u\,dx + v\,dy + w\,dz$ is not an exact differential.

2. If u, v, w be functions of the time, and $u\,dx + v\,dy$

$+ w\,dz = 0$, then by a common step in analytical reasoning, $\frac{du}{dt}dx + \frac{dv}{dt}dy + \frac{dw}{dt}dz = 0$, provided dx, dy, dz do not vary with the time. Hence as it is proved above that dx, dy, dz do not vary with the time in the equation $u\,dx + v\,dy + w\,dz = 0$, when the left-hand side is an exact differential $(d\,\phi)$, it appears that $d\,\phi = 0$, and $d\,.\,\frac{d\,\phi}{dt} = 0$, are differential equations of the same curve surface. The following is an instance. Let the velocity V be directed to or from a fixed centre whose coordinates are α, β, γ, and be the same at the same distance (r) from the centre at a given time. Then because

$$udx+vdy+wdz, \text{ or } V.\left(\frac{x-\alpha}{r}dx+\frac{y-\beta}{r}dy+\frac{w-\gamma}{r}dz\right)=0,$$

it follows that

$$\frac{du}{dt}dx+\frac{dv}{dt}dy+\frac{dw}{dt}dz, \text{ or}$$

$$\frac{dV}{dt}\cdot\left(\frac{x-\alpha}{r}dx+\frac{y-\beta}{r}dy+\frac{w-\gamma}{r}dz\right)=0,$$

and these are differential equations of the same curve surface.

3. In answer to the third argument it is sufficient to say, that any proposition proved respecting *fluid* motion, that is, motion by which the parts of the fluid alter their relative positions, cannot be affected by motion which is common to all the parts. There is no dependence of the one kind of motion on the other. The equation of continuity and the equation derived from D'Alembert's principle are identically satisfied by the latter kind of motion, which must be considered to be eliminated before any use is made of those equations for determining fluid motion.

4. The solution here given of a hydrodynamical problem is inadmissible on this ground. If a direct solution of the problem had been attempted, it would have been found necessary to inquire whether $u\,dx + v\,dy + w\,dz$ were an exact differential for that instance; and no mode of solution could evade the consideration of this question, unless the fluid were supposed to be confined between two cylindrical surfaces indefinitely near each other, and having hyperbolic bases. As in Mr. Stokes's solution that question is not considered, I conclude that it only applies to the limited case.

There is another point connected with this subject, and of no little consequence in the mathematical theory of fluid motion, which I am desirous of adverting to. In my former communication I inferred from the writings of Poisson that he did not accede to a proposition which occurs in the *Mécanique Analytique*, viz. that $u\,dx + v\,dy + w\,dz$ is an exact differential whenever the motion is small. But I am not aware that any general reason has been given for concluding that this proposition is untrue. By putting ϱ for the density of the fluid, and P for k. Nap. log. ϱ, and neglecting powers of u, v, and w above the first, we have the known equations,

$$\frac{dP}{dx}+\frac{du}{dt}=0,\quad \frac{dP}{dy}+\frac{dv}{dt}=0,\quad \frac{dP}{dz}+\frac{dw}{dt}=0;$$

the impressed forces for shortness' sake being omitted. Hence approximately,

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

and it might be argued from these equations that $u\,dx+v\,dy+w\,dz$ is an exact differential for small motions, whether they are rectilinear or not. But the answer is, that the condition of integrability requires that those equations should be *identically* true, which they cannot be said to be, because powers of u, v, w above the first have been omitted.

The same answer applies in another instance. If fluid issues at a constant rate from an orifice in a vessel of indefinitely large dimensions, it may be shown that the conditions of integrability of $u\,dx + v\,dy + w\,dz$ are satisfied if the motion at parts infinitely distant from the orifice be neglected. Those equations are, therefore, numerically satisfied; but as a state of motion differs from a state of rest however large the vessel may be, it follows that they are not identically satisfied, and it cannot therefore be concluded that $u\,dx + v\,dy + w\,dz$ is in this instance an exact differential.

Cambridge Observatory, Oct. 22, 1842.

LXXVI. *Experiments on the coloured Films formed by Iodine, Bromine, and Chlorine upon various Metals. By* AUGUSTUS WALLER, *M.D.*[*]

IN a paper presented by me to the Academy of Sciences of Paris, an extract from which may be seen in the *Comptes Rendus* for October 5, 1840, I first demonstrated the error committed in ascribing to the iodide of silver alone the power of fixing the vapours of mercury, after it had been exposed

[*] Communicated by the Author.

to the action of light. Instead of this property being exclusively confined to a film of iodide of silver, as obtained in the process of M. Daguerre, I found that it existed in many other substances when presented to the action of light in the state of thin films, viz. by the bromide and chloride of silver; by the oxide, bromide, iodide and chloride of copper and some others; all these however possessing less sensibility than the iodide of silver of Daguerre, and therefore less available for the reproduction of the images of the camera than the compound originally discovered by that gentleman. The iodide of Daguerre was found already too little sensitive to the influence of light in this climate, especially when applied to the reproduction of the image of animate objects, so that those films discovered by me seemed still less suitable to be employed for that purpose; this objection has, however, been completely removed by recent improvements, more particularly those of M. Claudet, who effected this principally by combining the original discovery of Daguerre with those mentioned above as having been subsequently made by myself. Pursuing the first stage of Daguerre's process, he obtained the film of iodide of silver, and, added to this another film of bromide, either in a simple state,—as practised in my experiments published more than six months before,—or after two of these substances had been combined together, as the chloride of iodine and the bromide of iodine, which he was the first to employ.

These coloured films, however, merit attention independently of the purposes to which they may be applied in photography: the beauty of some of the phænomena themselves is peculiarly attractive; the numerous changes of colour they undergo, either by a variation in the thickness of the film, or by the action of light, assign them a place among the most curious facts of science, and the extreme facility with which they are obtained adds to the interest they excite.

Impressed with these ideas, I was induced to pursue a train of investigation on this subject; among the results of which, one of the most interesting was a new method of making coloured rings, like those generally known under the name of "Newton's coloured rings," on many of the metals, by the same chemical process as that employed for forming the films of uniform thickness in photography. In order to procure these coloured rings, and at the same time to show the identity of the origin of the colours with those of the ordinary transparent films, that is, as residing simply in the thickness of the lamina and not dependent on the ordinary cause of colour, we have but to place a piece of iodine on a well-polished surface of silver or copper, and in a short time we find around the iodine a series of coloured zones of the various tints of the spectrum, and approaching in a greater or less degree to the form of a circle, according as they have been more or less disturbed in their formation by currents of the surrounding air. In order that they may be perfectly regular, as large as possible, and with tints undisturbed by the action of light, it is necessary to place a piece of iodine in the centre of a well-polished plate, as before described; this is then to be shaded by an opake screen superimposed a few lines from the surface to cause the vapours which would otherwise ascend and partially escape, to expand over its silver surface. Coloured rings may be formed in the same manner by bromine and chlorine and the various combinations of these bodies with each other, except that for those that are gaseous or liquid it is requisite to pay a little attention to the manner of disengaging them on the surface of the metal, either by passing them through a glass tube, or by some other contrivance easy to execute. These rings correspond to those formed by reflected light in Newton's experiments, with this difference, however, that in the coloured films of the soap bubble, and in those formed by the glass lenses, the thinnest film is in the centre; whilst in these rings, obtained by chemical action, it exists at the circumference, as is the case with the coloured rings of Nobili. In watching the formation of these phænomena, at first are seen two or three very small circles, which appear almost as soon as the iodine and the metal are placed in contact with each other; as the experiment continues, the circumferences of these circles become gradually greater; whilst the external colours extend themselves over a greater space, those of the centre grow fainter; red and green now only remain visible, and these at last, when the film has attained a certain thickness, in their turn also give place to a dull coating of brown. The formation of these rings evidently depends on the vaporization of the iodine from the solid nucleus. The variety in colour and extent of these zones is caused by the difference between the strength of the vapour at the centre and the circumference of the iodic atmosphere whilst expanding over so large a surface. In the metal thus combining with the vapour, we have to consider,—1, the force of the vapour at different distances from the centre; 2, the obstacle which a film of iodine, once formed, opposes to any further action between the iodine and the metal.

This experiment may be varied in different ways: two pieces of iodine of about the same size, placed at a small distance from each other on a silver plate, form separate coloured circles, until these come in contact at their circumferences, when the two systems will slowly coalesce and produce one common outline of the form of an ellipsis.

As the colours formed on various metals by the above-mentioned agents are very similar to one another, it may be sufficient to examine in particular those produced on silver by iodine.

The external film of the iodide of silver rings, which corresponds to the central black spot in those of Newton, is completely invisible, it being impossible to perceive any difference between the parts so covered, and those where the metal is intact; but by exposing half the plate to the influence of light, whilst the other part remains covered, the silver is then found darkened far beyond the limits of the external gold-coloured zone, where previously the surface was perfectly clear. The dark film thus rendered apparent is now liable to be rubbed off by the slightest friction, whereas before it was very adherent to the subjacent surface. The first zone is of a pale gold colour, which assumes a deeper tint as the thickness of the film increases: the second zone is blue, the third white; after these appear the different colours of the spectrum in regular succession, as in the films studied by Newton and others, viz. yellow, orange, red, blue, green, yellow, &c. The presence of the golden-coloured zone in the place mentioned is worthy of remark, as in the tables of Newton of the colours presented by films of various thicknesses, the blue is stated as immediately following the black. The same gold film is the first which appears on most metals when their surface is attacked in this manner. Chlorine and bromine on silver; oxygen on steel; chlorine and bromine on titanium, bismuth, &c., commence their colours in the same way. Copper, however, is in one respect an exception, this metal first becoming of a dark red, which increases to a ruddy brown and then changes into blue; this deviation is fully accounted for by the colour of the copper itself; with this single particularity, this metal undergoes the same alterations as the others.

The action of light on the different colours of the iodide of silver is very interesting: the most correct way of studying this is to protect one half of a system of coloured rings by an opake screen, while the other half is exposed for a short time to the influence of the solar rays. The golden zone undergoes the greatest change; at first it grows darker, then red, and at length is converted into a beautiful green. The blue film, which comes next in thickness, suffers considerable alteration in its tint, assuming a much deeper and more brilliant shade; the rest of the colours appear to be similarly affected by the action of light, though to a very slight degree, acquiring a trifling accession in their brilliancy. It has already been remarked that light destroys the adherence of the external invisible film: the same thing obtains with the second or gold-coloured film, which turns green, *but only to a certain depth* of the film, as may be proved by slightly rubbing the part thus altered; the green colour is then seen to disappear, and beneath the pulverulent portion thus removed is found the gold colour, having almost the same appearance as before the plate had been exposed. As this experiment may be repeated several times with the same results, it shows to how inconceivably small a depth the light has acted to produce this effect. To ascertain what would take place on augmenting the thickness of the portion turned green, and the adherence of which was destroyed, a piece of iodine was placed on the plate so that its vapour, by expanding, might arrive upon the green, at the same time the whole being kept from the light; the result was that the additional film combined with the one already existing, producing a blue, being the colour which would have resulted by the combination of the unaltered yellow films. I have found no chemical substance possessing the power of arresting, or in any way influencing these changes of colour; strong acids, provided they do not attack the silver,—for then, of course, the experiment would be destroyed,—and alkalies in concentrated solution, allow the action of light to go on as usual. The hyposulphite of soda, and ammonia in solution have no longer the power of dissolving the green film as they had before the action of light.

When the plate is left still longer exposed, after the changes above stated have taken place, the colours become more faint, and within the zone of green a white cloudy film is caused by the light, which, as it increases, veils the spectral colours beneath.

The knowledge we at present possess in chemistry of the affinities with which different bodies are endowed for combining with each other is but very imperfect, and the causes which complicate most chemical phænomena are so numerous, that it is scarcely possible to compare any two chemical actions to each other. Most of the facts upon which chemical science is founded, are acquired either by bringing the two bodies destined to act on each other into contact by dissolving them in a liquid, or by subjecting them to a temperature more or less elevated.

In the first of these methods, we are so far from being able to calculate the force of the chemical powers called into play, that Berthollet was induced to deny the existence of chemical power in the various phænomena of solution and precipitation of saline substances, and according to him what is called insolubility in a body is merely the result of its strength of cohesion, an entirely physical property.

When the intervention of caloric is required, the effects are still more complicated, as they vary according to the intensity of the heat employed, and the time its action is exerted; besides, the chemical action when it does take place is frequently so instantaneous that it is impossible in our present state of science to imagine any means by which it might be measured. In the combination of the three bodies, iodine, bromine and chlorine, with the metals, however, most of these objections cease to exist, or may be easily avoided. As their vapours combine with the metallic surfaces at the ordinary temperature, they are all of them in the same circumstances in that respect; and if the temperature should be required more elevated, the gasiform state of these substances, iodine not excepted, enables us to submit the metals to be experimented upon all at the same time to the same influence. If, therefore, it were possible to reduce the metallic substances into fine powders the particles of which were of the same dimensions, by acting upon them with either of these vapours, an idea might be formed of the affinities which produce their binary compounds by the increased weight acquired by the powders in this process; but the difference which exists in the physical properties of the various metals would preclude the possibility of any near approach to accuracy in this mode of proceeding; but by acting on the polished metallic surfaces, as in the preceding experiments, all the advantages offered by the process with the powders are included, whilst several of the difficulties are removed. As the film of the compound augments, it undergoes the various changes of colour which take place in all transparent films, thus affording a means of ascertaining the absolute thickness obtained in different circumstances, when it would be difficult to detect the slightest difference in weight by means of the most delicate balance. The depth of this coating may be ascertained when either the index of refraction of the compound itself is known, or if the angle of polarized light is given by means of the law discovered by Sir David Brewster, between the tangent of the angle of polarization, and the index of refraction. The most convenient way which occurred to me of performing these experiments, was the employment of a bell-glass within which some iodine is fixed at the top; this apparatus being placed over the metal to be acted on, the experiment may be watched in all its progress, and the action can be retarded or accelerated at pleasure by varying the interval of the iodine from the metal, or by interposing at some distance from its surface a disc of paper so as to cause the vapours of iodine to pass through it. Bromine may be made use of likewise by pouring a few drops of it over some carded cotton, and using it in a similar manner with the iodine. In respect to chlorine, it is most convenient to disengage it slowly by dropping a little sulphuric acid upon some chlorinated lime.

In illustration of the objects of this mode of experimenting, I will adduce some of the results it has given me with various metals. Some of the experiments below were performed before I had the idea of watching the progress of the combination through a transparent medium; they are therefore less exact than they might otherwise have been: but I have preferred stating them as I had inserted them in my note-book before I had conceived any idea as to their probable utility in the elucidation of chemical affinity, and when I intended them for other purposes, which I shall hereafter explain.

Iodine with Silver and Copper.

1st change.	Silver	pale gold.
...	Copper	assumes a darker red.
...	Silver	blue.
2nd do.	Copper	blue.
...	Silver	white.
3rd do.	Copper	white.
...	Silver	yellow.
4th do.	Copper	yellow more extended than on the silver.
...	Silver	orange.
5th do.	Copper	red.
...	Silver	blue, bluish-red.
...	Copper	red, with a tinge of green on some parts.
...	Silver	greenish blue.
...	Copper	red, tinged with green.
...	Silver	green.
...	Copper	orange.
...	Silver	yellowish green.
...	Copper	orange tending to red.
...	Silver	yellowish green.
...	Copper	orange-red.
...	Silver	red.
...	Copper	dull green.
...	Silver	red.
...	Copper	green.
...	Silver	deep green.
...	Copper	dull red.

Bromine with Silver and Copper.

5th change.	Copper . . .	sensibly darkened.
...	Silver . . .	unchanged.
...	Copper . . .	deep red.
...	Silver . . .	unchanged.
...	Copper . . .	red, blue.
...	Silver . . .	pale gold.
...	Copper . . .	white, orange of the 2nd order.
...	Silver . . .	yellow.
...	Copper . . .	green of the 1st order, red 3rd order.
...	Silver . . .	blue.

Chlorine with Silver and Copper.

The affinity of chlorine with silver is much inferior to that which it possesses for copper.

Iodine with Titanium.

Iodine at the common temperature has no action upon this metal.

Bromine with Titanium.

Bromine, when the surface of this substance is perfectly dry, has no more action upon it than iodine; but if it have a slight coating of moisture, as is formed by merely condensing on it the vapour of the breath, the coloured films are formed without difficulty by the vapours of bromine. Their appearance is the same as those of the iodide of silver, viz. gold, deep gold, blue, white, yellow, orange, red, &c.

Chlorine with Titanium and Copper.

Titanium has a stronger affinity than it has for either of the preceding vapours. The combination takes place when the metallic surface is either dry or moist.

Copper . . .	much reddened.
Titanium . . .	not affected.
Copper . . .	passed through several of the spectral orders of red and green until it arrived at almost its last changes of colours.

Titanium under the same action received a dull film, which viewed obliquely showed red, green, yellow.

Silver, exposed to the same influence as the two former, had yellow in the centre and blue more externally.

Iodine with Bismuth and Silver.

Silver . . .	pale gold.
Bismuth . . .	some parts yellow, others not attacked.
Silver . . .	blue, white, yellow, orange.
Bismuth . . .	blue, yellow, orange.

In the action of iodine on bismuth, the influence of the physical condition of metallic surface is very manifest. The crystalline texture of this metal may be perceived, and the difference of its hardness admits, to a certain point, of being measured by the difference of the colour of the films that are formed on various points; while most parts are yellow, there exist others of an angular outline which remain still unattacked; the same difference is remarked in the other stages of the combination.

Iodine with Mercury.

It is impossible to estimate the affinity between mèrcury and iodine by means of the coloured films, because, on combining, these two substances merely cause a dirty white appearance on the surface of the latter. Their combining affinity appears to be considerable, for when exposed together with silver the action produced with both was red at the edges, little altered in colour; on the rest of its surface a dull white film, in the midst of which were seen several dark spots, where the metal was apparently unaltered.

Bromine with Mercury and Copper.

1st. Mercury . . . gold colour.
Copper . . . slightly darkened.
2nd. Mercury . . . blue.
Copper . . . dark red.
3rd. Mercury . . . green on some parts.
Copper . . . white.

After this the copper underwent its usual changes of colour on prolonging the action of the vapour of bromine, but the colour of the mercury suffered no further change.

Chlorine with Mercury and Copper.

Mercury . . . a slight film.
Copper . . . no alteration of colour.
Mercury . . . deep gold colour.
Copper . . . deep red on some parts, blue on others.
Mercury . . . red tinged with blue.
Copper . . . blue, white.
Mercury . . . blue.
Copper . . . same as before.

With respect to the bromide and chloride of mercury, it is necessary to view them obliquely in order to perceive all the changes of colour they undergo; for if looked at perpendicularly, there is seen on both a dull uneven film of white which reflects none of the above colours; consequently, to avoid any error, the copper must be inspected under the same angle.

Bromine with Bismuth and Silver.

Silver	. . . pale gold.
Bismuth	. . . not apparently changed.
Silver	. . . deep gold, blue.
Bismuth	. . . yellow, blue.
Silver	. . . blue, yellow.
Bismuth	. . . dull colourless film.

Chlorine with Bismuth and Silver.

Bismuth is slowly attacked by chlorine gas, much in the same way as with iodine and bromine in vapour.

Bromine with Lead.

At the common temperature neither bromine nor chlorine forms coloured films upon this metal, which it is very difficult besides to bring to any high state of polish on account of its softness. But when lead is heated, as over the flame of a spirit-lamp, the vapours of bromine then form very fine coloured films, which are in succession gold, deep blue, &c.

Iodine with Iron.

These two may be made to form coloured films when combined rapidly together, but generally a dull coating without any spectral colour is obtained, on account of the deliquescence of that salt.

Until we know the index of refraction of the different films enumerated, it would be impossible to give a correct table of the combining powers in the experiments that have been detailed; nor is the table of the relative thickness of transparent plates as it has been transmitted to us by Newton, sufficient in the present instance, if any great degree of precision be required. Besides these objections, it is necessary before leaving this subject to pass in review several others inseparable from the mode of performing the experiments themselves. The principal circumstances complicating these experiments and liable to vary in different observations, are,—

First, the hardness of the metal acted upon; 2ndly, the obstacle opposed to the continuation of chemical action by the inert film formed upon the metal; 3rdly, the force of the vapours that attack the metal. The influence of the texture of the metallic surface on chemical action is most evident when bismuth is the metal employed. Here the chemical action may be seen to commence on small isolated portions of the surface, which have already assumed a deep gold colour, before other parts are in the least changed, from the natural appearance of the metal. To determine how far this might influence the formation of the iodide of silver, a silver coin was exposed to iodine with a piece of pure silver; as the former was so much the harder of the two, it was naturally supposed that the chemical action would be slower in exerting itself on it than on the latter. This, however, was not the case, as may be seen by the following statement of the result of the experiment:—

Silver coin	pale gold colour.
Pure silver	pale gold.
Silver coin	deep gold.
Pure silver	deep gold.
Silver coin	light blue.
Pure silver	light blue.
Silver coin	yellow.
Pure silver	blue, white, yellow not visible.
Silver coin	yellow, red at edges.
Pure silver	yellow, no red edges.
Silver coin	red, blue at edges.
Pure silver	yellow, no red apparent.

The intensity of the resistance offered by the different films of iodide of silver to a continuation of the chemical combination, may be determined by noting the moment at which the various spectral tints make their appearance.

′	″	Colour of the film of iodide of silver.
0	50	beginning to darken.
2	0	pale gold.
4	40	deep gold.
6	40	orange blue.
7	30	blue.
9	30	light blue.
11	30	commencement of yellow.
18	30	orange red.
20	15	blue.
22	55	deep blue.
24	40	green.
28	0	yellowish green.
30	25	ruddy brown.
40	10	green.
46	30	green.
50	10	red.
53	15	green.

By comparing the thickness of the colours with the space of time required for their production, it will be found, however imperfect the table given by Newton may be when applied to this subject, that towards the end of the experiment above given, the chemical combination is retarded by the presence of the inert film, and that to obtain the same thickness of film as at the commencement, about double the time is required.

The third cause of error may be avoided by operating with vapours of about the same force. In those described, the average time employed in passing to the maximum was generally about half an hour; if that were not taken into consideration, different results might be obtained.

In regard to chlorine, there exists another cause of complication, the affinity which it possesses for water; for when disengaged in the ordinary manner, chlorine carries with it a certain quantity of water which may very much alter the results of the experiment.

No. 1, St. Mary Abbot's Terrace, Kensington.

[To be continued.]

LXXVII. *Reply to* Professor Kelland's *Defence of the* Newtonian *Law of Molecular Action. By* S. Earnshaw, *M.A. Cambridge* *.

PROFESSOR Kelland's defence of the extension of Newton's law of force to molecular action consists of a critique upon my memoir "On the Nature of Molecular Forces;" and of a reply to my letter which appeared in your Magazine for July (pres. vol. p. 46). I shall therefore for the sake of precision divide what I have to say in answer to his remarks into two corresponding heads.

1. With respect to the critique on my memoir, it is evident that it has been written by the Professor under the notion that my investigations have supposed each particle of the medium, except the one for which the forces are calculated, to be in their respective equilibrium positions. I gather this from the repeated charge he brings against me of drawing *dynamical* inferences from a *statical* investigation. Will the Professor point out what step, in that part of my paper which is written against Newton's law, requires that the particles of the medium should be in their equilibrium positions? With the exception of the last article, where it is expressly stated that the particles are *in equilibrium*, my paper is an investigation of the properties of a *vibrating* medium, *i. e.* a medium *in a state of vibration*. At any instant during the motion of the medium I fix upon a particle and investigate the properties of the forces which urge it at *that* moment; the other particles meanwhile are supposed to be in the positions which as particles in a state of vibration they had at the instant fixed upon. [Let

---

* Communicated by the Author.

the Professor point out one link of my argument against Newton's law which violates this supposition.] I find as a result that there is always one direction in which the particle is urged *from* its position of rest; and therefore, as the motion of the particle in that direction could not be vibratory, Newton's law cannot be the law of molecular force in the luminiferous æther. This explanation, I trust, will enable the Professor to see that he has written his review of my memoir under the influence of a complete misconception of its nature, to which is due the origin of his complaints that some of my reasonings are unintelligible to him, and that the whole line of my argument is inadmissible (August, p. 130), to which charges it is obviously not necessary for me to make any further reply. There is, however, one argument, which though it belongs to this head, I cannot allow to pass without more particular notice, because upon reading it I could not but consider it as a strong indication of the Professor's having allowed other motives than "a desire for truth" (Sept. p. 207) to influence him in bringing it forward. It stands in the Magazine for this month (p. 270) in these words: "I will only add, when it is concluded from the hypothesis of a cubical arrangement of the particles, acting by forces which vary according to the Newtonian law, that the direction of one side of the cube is stable and of one unstable, ought we not to ask, Is it the hypothesis, or the reasoning based on it which is erroneous? Must it not of *necessity* be the latter?" Now one would think from the manner in which this argument is brought forward that the matter animadverted upon by the Professor forms a part of my reasoning. Your readers therefore will be surprised to be informed that it stands in my memoir as a purely casual observation, upon which not a step nor even a word of my reasoning against Newton's law depends. Why then did the Professor bring it forward and draw from it the sweeping inference that my reasoning is erroneous? Unfortunately for the Professor, in this instance he reaps no advantage by stepping out of the line of legitimate argument, as his objection is founded on the misconception that I have supposed the particles to be in equilibrium.

2. In commencing his reply to my letter printed in your Magazine of July, the Professor calls upon me to state "what I conceive to be the direct effect of matter." I conceive it to be that effect which arises from the supposition that matter and æther act upon each other by attraction or repulsion (*en passant*, I do not see why I am called upon for this definition, as I have nowhere employed the direct action of matter). By the indirect action of matter I mean that effect which results

when the density and arrangement of the æther are changed by the introduction of particles of matter which exclude some particles of æther from certain portions of space, and thereby affect the equilibrium positions of the remaining particles of æther. The Professor next endeavours to guess the reason why I did not draw from my equations any inferences respecting the direct action of matter. The answer is simple; a most important step required that the vibrating medium should consist of homogeneous particles. The step I allude to is that where (S. 3. vol. xx. May, p. 372) I have "assumed the law of displacement at the time t to be $\xi_r = a \sin (r\,h + \mathrm{T})$," which assumption is not true when the particles of matter vibrate, because then a could not be invariable through the medium. I think no blame attaches to me for causing this perplexity to the Professor, as I have expressly added, "it will be understood that what follows applies only to media in which this law of disturbance can be transmitted," which I understand to be a formal renunciation of all connexion with the direct action of matter. The Professor, therefore, in referring me to Mr. O'Brien (October, p. 269) to be set right in my notions, seems to have fallen into the mistake of supposing that what Mr. O'Brien has written on the *direct* effect of matter (March, note at p. 208) can refute what I have written on the *indirect* effect of matter.

I do not think I fully understand on what ground the Professor affirms (October, p. 264) that I have not taken account of "*the want of action* of particles of æther in the portion of space occupied by the material particles" (October, p. 264); for, as I have taken into account all those particles of æther which do exist, and none else that I am aware of, I suppose I *must* have omitted those which do not exist. Perhaps the Professor will point out what step of my investigation implies the existence of the absent particles.

I am next accused (p. 264) of not saying a word about "the pressure of the particles of matter on the adjacent particles of æther tending to stop their motion." In reply it seems sufficient to state, that the particles of matter are supposed to be so few in number in comparison with the particles of æther in a refracting medium, that though a wave may in some degree be broken up in its passage through the medium by material impediments, the sensible properties of its general front will remain almost, if not entirely unaffected; wherefore in an argument based on the broad features of refraction, any allusion to this consideration were a useless refinement, a needless entering upon difficulties, and an unnecessary interruption of my investigations; which reasons will, I trust, prove satisfactory to the Professor for its having been passed over in silence.

The quotation which the Professor gives at the bottom of p. 265 from my letter (April) I can assure him was not intended to have any reference to his writings. The Professor must also have mistaken my views when he states (p. 266) that I "appear to look for a complete explanation of dispersion to the very quarter at which I aim my objections," for I look to the direct action of matter, against which I have not as yet brought forward any objection.

In the middle of p. 266 the Professor begins his reply to my remarks on his defence of his numerical calculations. It appears to me that he is hereupon somewhat inconsistent with himself. For (May, p. 378) his words are, "my calculations *are affected with an error*, in that *I have neglected to shorten* λ;" but here he writes, "the data are not erroneous." These two statements seem hardly reconcilable. Also, if "the calculations *are* affected with an *error*," I do not comprehend how they can "strengthen theory." What he states (p. 267) about his "formula admitting as many arbitrary constants as you please," amounts to a confession that he employed the common principles of interpolations, instead of theory, which is all I have contended for in this part of the subject.

The latter part of the Professor's letter is employed in controverting my remarks on his proof of the transversality of vibrations. The values of $v\ v'\ v''$ which the Professor makes use of in establishing this principle are derived from the equations of motion, which in my last letter I have proved to be non-existent. That letter is therefore a sufficient answer to this part of the Professor's reply. I cannot, however, dismiss the subject without remarking, that the non-existence of normal vibrations is not proved when it has been shown that (v) the velocity of their transmission is imaginary. It must be shown that v is zero, or very much greater or very much less than the velocity of transmission of the transversal vibrations. For, if it turn out that v is imaginary, the proper inference is, as I have before stated, that the equations of motion have been incorrectly integrated, and the whole investigation needs to be revised. As the remarks which I have made in my last letter respecting the evanescence of the quantity n, and, with it, of the equations of motion extend to all that the Professor has written in his Memoirs on Light, and in his Theory of Heat, as far as they are respectively dependent on Newton's law of molecular action, it is needless to enter further upon the inferences from them which the Professor in various parts of his letters has placed in opposition to my results.

It now only remains to reply to the accusation (p. 267) that I have fallen into an error in turning the equations of motion into that form, from which I drew all my inferences. I can assure the Professor that I did not lay my investigations before the public, without having first carefully revised them, compared them with what other persons have written on the same subject, and satisfied myself as to the cause of difference where any existed. The Professor may therefore for the future take it for granted that I have seen and examined the equations in M. Cauchy's *Mémoire sur la Dispersion de la Lumière*, to which he refers me for correction. I fear it will give to my letter an air of great sameness if I again accuse the Professor of misunderstanding what he has undertaken to criticise. I shall not, however, make the charge without bringing forward the proof of it. The Professor tells me that the coefficient of a certain term of my equations differs in appearance from the corresponding coefficient in M. Cauchy's equations; and his inference is, therefore these coefficients are not equal, and therefore mine are erroneous. Now I ask, how does the Professor know that these coefficients are not equal? I admit that they appear *to the eye* to be different, but the symbol Σ in M. Cauchy's differs entirely from the same symbol in mine. M. Cauchy's coefficients have been brought into the state referred to by reductions suggested by *theoretical* considerations; but my coefficients were brought into the state in which I leave them by reductions effected upon *experimental* grounds. If M. Cauchy's differ in *value* from mine they disagree with experiment, and are therefore to be rejected, as will be made manifest by the following process, which applies equally to M. Cauchy's equations and mine own. But I will first state the matter in another way. In my investigations (March, p. 372), A represents the value of $\Sigma\ \{m'\ d_x{}^2\ \mathrm{F}\ (\mathrm{R})\}$, the summation represented by Σ extending to all particles in the rth wave surface, and in all other surfaces the particles of which are in the same state of displacement as in the rth. Also A represents the value of

$$2\ \Sigma\left(\mathrm{A}_r \sin^2 \frac{r\,h}{2}\right),$$ Σ now denoting summation for all the values of r in one wave's length. The limiting value of r in performing the operation Σ is therefore the number of particles in a wave's length, which number in any conceivable geometrical arrangement of the particles depends upon the

position of the wave's front. Hence A_r and $\sin^2 \frac{r h}{2}$ depend upon the direction of transmission; but does A, *i. e.* $2\Sigma\left(A_r \sin^2 \frac{r h}{2}\right)$, also depend upon the direction of transmission? This question, and a similar one for each of the other coefficients, M. Cauchy has not answered, but I have answered it for myself in the negative on *experimental grounds*, as follows. My equations of motion (and they are M. Cauchy's also) are,

$$\begin{aligned} d_t^2 \xi &= -A\xi - F\eta - E\zeta \\ d_t^2 \eta &= -F\xi - B\eta - D\zeta \\ d_t^2 \zeta &= -E\xi - D\eta - C\zeta. \end{aligned}$$

The question is, are the coefficients dependent on the position of the wave's front? Multiply these equations respectively by $\cos\alpha$, $\cos\beta$, $\cos\gamma$, and add the results, at the same time assuming $k^2 = A + F\frac{\cos\beta}{\cos\alpha} + E\frac{\cos\gamma}{\cos\alpha} = B + D\frac{\cos\gamma}{\cos\beta} + F\frac{\cos\alpha}{\cos\beta} = C + E\frac{\cos\alpha}{\cos\gamma} + D\frac{\cos\beta}{\cos\gamma}$; from which eliminating $\cos\alpha$, $\cos\beta$, $\cos\gamma$, we find the following cubic in k^2,

$$(k^2-A)(k^2-B)(k^2-C)-D^2(k^2-A)-E^2(k^2-B)$$
$$-F^2(k^2-C) = 2DEF.$$

Having from this found three roots k_1^2, k_2^2, k_3^2, we can then find three corresponding sets of values of $\cos\alpha$, $\cos\beta$, $\cos\gamma$; and our equations of motion by this process of mere algebra take the following simple forms,

$$d_t^2 \xi' = -k_1^2 \xi', \quad d_t^2 \eta' = -k_2^2 \eta', \quad d_t^2 \zeta' = -k_3^2 \zeta';$$

where

$$\begin{aligned} \xi' &= \xi \cos\alpha_1 + \eta \cos\beta_2 + \zeta \cos\gamma_1 \\ \eta' &= \xi \cos\alpha_2 + \eta \cos\beta_1 + \zeta \cos\gamma_2 \\ \zeta' &= \xi \cos\alpha_3 + \eta \cos\beta_3 + \zeta \cos\gamma_3, \end{aligned}$$

that is, ξ' η' ζ' are the displacements of the particle m estimated parallel to a new set of rectangular axes. The forms of the new equations of motion show that these axes are axes of dynamical symmetry,—those in fact which are better known as the axes of elasticity. Now from experiment we know that for waves of a given length k_1^2, k_2^2, k_3^2 are constant quantities, *i. e.* independent of the position of the waves' front (by the above process I have only changed the axes of coordinates, the waves' front remains unaltered in position). And not to occupy room unnecessarily, I now refer the Professor to the note (July, p. 48) to my letter for the remainder of the proof that "A, B, C, D, E, F are independent of the position of the wave's front." By this process it is established beyond the possibility of a doubt, that when the operation represented by Σ is performed in the expression which Professor Kelland quotes (p. 268) from M. Cauchy, the result ought to be independent of the position of the wave's front; and so it is proved either that my equations and M. Cauchy's are identical, or that M. Cauchy's are at variance with experiment. The *methods* by which we have obtained our equations are perfectly dissimilar, but I believe *the equations themselves are identical.* In deducing his M. Cauchy has adhered closely to theoretical considerations; but in deducing mine, I have proceeded to a certain point by the guidance of theory, and then beginning from a more advanced point, where the results of experiment were known, have worked backwards to meet theory. It is therefore easily seen that my results being a mixture of theory and experiment would not present the same appearance *to the eye* as the results of M. Cauchy, which are obtained from theory alone. They must, however, be identical *in fact*, or else theory is discordant with experiment. What therefore Professor Kelland has written (p. 268) about "the axis of transmission" is grounded on a misconception, from which also has sprung his idea that "the *form* of my equations" (p. 46), from which my inferences have been drawn against the Newtonian law, &c., "*does* depend on the position of the front of the wave."

I believe I have now replied to every objection of importance which Professor Kelland has brought forward; I cannot however conclude this letter without remarking, that it is obvious that a discussion like the one in which we are now engaged never can be brought to a satisfactory conclusion unless both parties write with perfect candour and a single eye to the discovery of the truth. All arguments which do not really bear upon the Newtonian law must be avoided; and those which do bear upon it, if after due scrutiny they be found to be true, unhesitatingly admitted with all their consequences. I would therefore, with a view of shortening our labours, respectfully request the Professor not to take so wide a field, but to confine himself to the prominent and really important points of the argument; because if objections of this character cannot be answered, it is clearly quite unnecessary for him to descend with M. Cauchy into the mystical and doubtful subtilties of "refined analysis." May I then respectfully request the Professor to answer in the spirit here recommended the four following queries, which seem to me better calculated than any others to bring our discussion to a speedy termination?—

1. Does Professor Kelland admit that I have satisfactorily proved that the quantity n used in his memoir on dispersion is equal to zero?

2. Does he admit that the evanescence of that quantity destroys his equations of motion?

3. Does he admit that the evanescence of his equations of motion destroys his proof of the transversality of vibrations?

4. Does he admit that the disappearance of his equations of motion in a medium of perfect symmetry *whenever Newton's law is introduced*, is a sufficient proof that that cannot be the law of molecular action?

If he does admit these points our discussion is at an end; but if he does not, I shall with great willingness answer any objections against *these* which he may think it necessary to bring forward. The introduction of collateral questions (such as, "whether the force acts by attraction or repulsion," "whether a cubical arrangement is or is not one of geometric symmetry," "whether the æther has boundaries," "how vibrations are generated," "whether it is probable that a violent effort would be requisite to move a particle of æther out of its position of equilibrium," and others of a similarly discursive nature which the Professor has mooted in his letters) tends unnecessarily to distract attention from the main question; they may therefore safely be allowed by both parties to stand over as unimportant till all objections which are of the first magnitude have been refuted or allowed.

Cambridge, Oct. 7, 1842.

LXXVIII. *On a Theorem in Analytical Geometry.*

By the Rev. James Booth, *LL.D., M.R.I.A.*

[Continued from p. 179.]

HAVING shown that if three fixed points assumed on a right line are always retained in three fixed planes, any fourth point P will describe an ellipsoid, whose centre is the common intersection of the three planes, we proceed to establish the following remarkable property, that the volume of this ellipsoid is independent of the angles between the coordinate axes; a singular result, to which an analogous property may be found in the ellipse.

Resuming the equation found at page 178,

$$V = \frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} + \frac{2\cos\lambda}{b\,c}y\,z + \frac{2\cos\mu}{a\,c}x\,z + \frac{2\cos\nu}{a\,b}x\,y - 1 = 0.$$

When the equation of the ellipsoid is in this form, having all its terms positive, the point P is supposed to be external to the three fixed points; on the contrary, when P is between any two of the points, the corresponding pair of rectangles become negative.

To determine the volume of this surface, let $U = 0$, be the equation of a sphere, whose radius is r, referred to the same oblique axes of coordinates, having its centre at the origin, and touching the ellipsoid at one of its vertices; then if a tangent plane to the ellipsoid be drawn at this point, it will also touch the sphere, and we shall consequently have, the equation of the sphere being

$$U = x^2+y^2+z^2+2yz\cos\lambda+2xz\cos\mu+2xy\cos\nu-r^2=0 \quad \ldots\ldots \quad (3.)$$

$$\frac{dV}{dx}=\frac{dU}{dx},\quad \frac{dV}{dy}=\frac{dU}{dy},\quad \frac{dV}{dz}=\frac{dU}{dz}, \quad . \ . \ . \quad (4.)$$

as the coefficients of the variables in the equations of the coincident tangent planes are identical; hence

$$\left.\begin{aligned} \frac{x}{a^2}+\frac{y}{ab}\cos\nu+\frac{z}{ac}\cos\mu &= \frac{x+y\cos\nu+z\cos\mu}{r^2} \\ \frac{y}{b^2}+\frac{z}{bc}\cos\lambda+\frac{x}{ab}\cos\nu &= \frac{y+z\cos\lambda+x\cos\nu}{r^2} \\ \frac{z}{c^2}+\frac{x}{ac}\cos\mu+\frac{y}{bc}\cos\lambda &= \frac{z+x\cos\mu+y\cos\lambda}{r^2} \end{aligned}\right\} \quad . \quad (5.)$$

or putting $t=\frac{x}{z}$, $u=\frac{y}{z}$, there results

$$\left.\begin{aligned} \frac{t}{a^2}+\frac{t\cos\mu}{ab}+\frac{\cos\mu}{ac} &= \frac{t+u\cos\nu+\cos\mu}{r^2} \\ \frac{u}{b^2}+\frac{t\cos\nu}{ab}+\frac{\cos\lambda}{bc} &= \frac{u+t\cos\nu+\cos\lambda}{r^2} \\ \frac{1}{c^2}+\frac{t\cos\mu}{bc}+\frac{u\cos\lambda}{bc} &= \frac{1+t\cos\mu+u\cos\lambda}{r^2} \end{aligned}\right\} \quad . \ . \quad (6.)$$

From these equations, eliminating t and u, we find the cubic equation, putting

$$1-\cos^2\lambda-\cos^2\mu-\cos^2\nu+2\cos\lambda\cos\mu\cos\nu=k^2,$$

$$\begin{aligned} r^6 &- \frac{r^4}{k^2}\Big[a^2\sin^2\lambda+b^2\sin^2\mu+c^2\sin^2\nu-(bc\cos^2\lambda+ac\cos^2\mu+ab\cos^2\nu) \\ &\qquad +2(ab+ac+bc)\cos\lambda\cos\mu\cos\nu\Big] \\ &+\frac{r^2}{k^2}\Big[b^2c^2\sin^2\lambda+a^2c^2\sin^2\mu+a^2b^2\sin^2\nu-abc(a\cos^2\lambda+b\cos^2\mu+c\cos^2\nu) \\ &\qquad +2abc(a+b+c)\cos\lambda\cos\mu\cos\nu\Big] \\ &\qquad\qquad -a^2b^2c^2=0 \quad . \ . \ . \ . \ . \ . \ . \quad (7.) \end{aligned}$$

Now the squares of the three semiaxes of the surface are the three roots of this cubic equation, and as the last term is the product of the roots with the sign changed, we find, calling the semiaxes $r^{\prime}\, r^{\prime\prime}\, r^{\prime\prime\prime}$,

$$r^{\prime}\, r^{\prime\prime}\, r^{\prime\prime\prime} = a\, b\, c,$$

hence the volume of the ellipsoid $= \frac{4\,\pi}{3}\, r^{\prime}\, r^{\prime\prime}\, r^{\prime\prime\prime} = \frac{4\,\pi}{3}\, a\, b\, c.$

It is not difficult to show, that the areas of the sections of the surface made by the coordinate planes are $\pi\, a\, b$, $\pi\, a\, c$, and $\pi\, b\, c$ respectively, and in general that the area of any conic section whose equation is

$$A^2 x^2 + B^2 y^2 + 2\, A\, B \cos \nu \,.\, x\, y = 1,$$

is independent of ν, the angle between the axes of coordinates, where A and B are the reciprocals of the segments into which the line moving between the axes of coordinates is divided.

From this known property that if a line of constant length revolves between two fixed rectangular axes, the locus of the middle point is a circle, may be deduced a method of converting rectilinear into circular motion, rigorously exact, and simple in construction, admitting an unlimited length of stroke, and obviating the necessity of using a working beam or connecting rod; a change which would introduce a decided improvement in the construction of the steam-engine*.

LXXIX. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. FRANCIS *and* H. CROFT.

[Continued from p. 287.]

On Hæmatoxylin.

CHEVREUL examined Campechy wood (wood of *Hæmatoxylin campechianum*, L.) thirty years ago, and found in it a crystallizable colouring principle which he called Hæmatin, which name has been changed into Hæmatoxylin to avoid any confusion with the hæmatin of the blood. Chevreul probably did not procure the body in a state of purity. Erdmann has now examined it, and he proposes the following method for its preparation:—The common extract of logwood is pulverized and mixed with a considerable quantity of pure siliceous sand (to prevent the agglutination of the particles of the extract), and the whole allowed to stand several days with five or six times its volume of æther, the mixture being often shaken; the clear solution is poured off and distilled until there is only a small syrupy residue. This is mixed with a

---

* [The reader is requested to correct some oversights and errors in the preceding part of this paper, it having been printed from an unrevised proof. —EDIT.]

certain quantity of water and allowed to stand for some days, when the hæmatoxylin crystallizes out, and may be pressed between bibulous paper, &c. The residual extract itself contains more of the substance; from 2 pounds of extract treated with 10 pounds of æther, Erdmann obtained between 3 and 4 ounces of hæmatoxylin.

The colour of hæmatoxylin varies from a straw yellow to a deep yellow; when pulverized it is white or pale yellow. The crystals can be obtained some lines in length; their form has been studied by Wolff and previously by Teschemacher (Phil. Mag. S. 3. p. 28). It tastes like liquorice root without any trace of bitterness or astringency. Chevreul describes it differently, but he probably had an impure substance.

Hæmatoxylin dissolves slowly in cold water, but very easily in boiling water. It is necessary to employ water which has been previously boiled, for the smallest possible trace of ammonia causes the hæmatoxylin to become purple, and Erdmann proposes this substance as the most delicate test for ammonia: pure oxygen or air freed from ammonia does not alter the colour. The crystals must be dried by pressure in bibulous paper. The filtering paper which is used for the solutions of hæmatoxylin must be free from lime. Hæmatoxylin is soluble in alcohol and æther, but the solution in anhydrous æther does not yield crystals. By exposure to sunlight the substance acquires a reddish colour, but no change in its constitution is effected.

It does not sublime, leaves behind a great quantity of charcoal when heated in a tube; does not evolve ammonia when heated with potassa, and consequently contains no nitrogen. This hæmatoxylin loses water at ordinary temperatures, and the desiccation is completed at 100–120° C.; it contains 8 atoms of water, its formula is therefore $C^{40} H^{34} O^{15} + 8 H^2 O$. Another hydrate containing only 3 atoms of water is obtained by allowing a hot saturated solution of hæmatoxylin to cool in a closed vessel, when it separates in small granular crystals. It was impossible to determine the atomic weight of the body.

Caustic potassa colours a solution of hæmatoxylin violet, but by absorption of oxygen the colour passes into purple, brownish yellow, and at last dirty brown. These compounds appear to contain hæmatoxylin in different degrees of oxidation.

Ammonia has the same effect, but the presence of air is necessary to effect the change fully; the ammoniacal solution becomes deep red, almost black. If acetic acid be added to this solution until a precipitate begins to be formed, and it be then evaporated, the ammonia being carefully replaced from time to time (excess is to be avoided), a compound crystallizes out in dark violet grains which contains ammonia combined with *Hæmatëin*; these crystals must be quickly filtered off and dried by pressure and exposed to the air, but heat must not be employed. The mother liquor may be precipitated by means of acetic acid, hæmatëin falls down in the form of an ochre-red voluminous body like hydrated sesquioxide of iron; when dried it is dark green with a metallic glance, red by transmitted light; the powder is red. Slowly soluble in cold, easier in boiling water. Soluble in alcohol with a reddish brown colour, very little soluble in æther; dissolves in potassa with a blue colour, which exposed to the air passes through red into brown; with ammonia it gives a purple solution which soon turns into brown; formula $C^{40} H^{30} O^{16}$. Hæmatoxylin absorbs 3 atoms of oxygen under the influence of ammonia, and forms hæmatëin, and 2 atoms of water, $C^{40} H^{34} O^{15} + O^{3} = C^{40} H^{30} O^{16} + H^{4} O^{2}$.

No carbonic acid is formed during the change.

Hæmatëin-ammonia is a bluish-black or rather violet-black powder, which under the microscope is seen to consist of quadrilateral prisms. It is soluble in water with an intense purple colour, with alcohol it gives a reddish-brown solution. Heated to 100° C. it loses water and ammonia, it must therefore be dried over sulphuric acid. When dry it does not decompose of itself, but if moist or in solution a spontaneous decomposition takes place. Formula $C^{40} H^{44} N^{4} O^{17}$; consequently 1 atom of hæmatëin takes up 2 atoms of ammonia and 1 atom of water. Erdmann gives the ammonia compound the formula $C^{40} H^{28} O^{15} + 2 N^{2} H^{8} O$, and hæmatëin $C^{40} H^{28} O^{15} + H^{2} O$.

Hæmatëin-ammonia gives coloured precipitates with most metallic solutions. The lead compound is blue, but it is basic, for the supernatant solution is acid; at first the washings are colourless, but soon become brown-coloured: it is probable that under the influence of oxide of lead, air and moisture, the hæmatëin undergoes slow oxidation and decomposition. The blue compound was washed a little and then analysed: the organic part of it agreed pretty well with the formula $C^{40} H^{28} O^{15}$.

A reddened solution of hæmatoxylin is decolorated by sulphuretted hydrogen, and on evaporation pure hæmatoxylin is obtained; a solution of hæmatëin is also rendered colourless by sulphuretted hydrogen, but in this no reduction takes place, for on evaporating, as the gas is driven off the solution acquires its original dark colour, and crystals of hæmatëin are formed, but not a trace of hæmatoxylin. It is evident, therefore, that the sulphuretted hydrogen enters into combination with hæmatëin, as Chevreul has already stated. The lead and copper compounds of hæmatëin were also treated with sulphuretted hydrogen, but in no case was any reduction visible. A few experiments were made on the action of nascent hydrogen, which appeared to have better success.—(*Journ. für Prakt. Chemie*, vol. xxvi. p. 193.)

Opianic Acid.

Liebig and Wöhler have discovered that this body is produced when narcotine is exposed to oxidizing agencies. It is best prepared in the following manner:—Narcotine is dissolved in a considerable excess of dilute sulphuric acid, finely powdered peroxide of manganese is added, and the whole heated: it soon begins to assume a saffron-yellow colour and to evolve carbonic acid. It must be heated to boiling, and this temperature maintained as long as carbonic acid is evolved. At the end of the operation there must still be excess of oxide of manganese and sulphuric acid. It is filtered while hot; the liquid on cooling forms a mass of fine acicular crystals; these must be washed with cold water, and purified by re-solution in water and decoloration with animal charcoal. Opianic acid crystallizes in fine silky needles, whose form cannot be determined. Soluble in hot water but not in cold. Soluble in alcohol. Acts as an acid, but has only a weak bitter sourish taste. Fuses easily into an oil which crystallizes on cooling, but if the temperature has been raised above its fusing point, it remains amorphous. Is not volatile. Heated in the air it gives off the same aromatic odour as narcotine; it inflames easily, and burns with deposition of soot.

It expels carbonic acid from its salts, and forms soluble compounds with all bases; does not contain nitrogen. Liebig and Wöhler are at present engaged in its more accurate examination. — (*Journ. für Prakt. Chem.* vol. xxvii. p. 97.)

Quinoiline.

Gerhardt boiled one part of quinine with four parts of potassa and one of water in a small retort; the mixture became brown, and a heavy yellow oil passed over with the water. Hydrogen is evolved during the process. If the potassa is not allowed to fuse and the water continually replaced, no ammonia is formed. The oil is evidently alkaline, and forms crystallizable salts with acids; with bichloride of platinum it produces a compound soluble in boiling water, which on cooling crystallizes in golden-yellow needles. Its formula is $C^{16} H^{22} N^2 O^2$,

$H^2\ Cl^2 + Pt\ Cl^4$. Consequently one atom of quinine, by taking up four atoms of water and giving off $C^4\ O^4$ produces two atoms of quinoiline. This base also forms a crystalline double salt with bichloride of mercury. New liquid bases are also produced by acting with potassa on strychnine, narcotine, &c., but the process is more complex.

On Indigo-Nitric Acid (Indigotic Acid).

Marchand instituted a series of experiments on this acid with a view to determine its composition, without being aware that Dumas was engaged on the subject: Marchand's results have now been published, and they agree with those already obtained by Dumas. He found the crystallized acid to contain three atoms of water, two of which are given off at 150° C., or by long exposure to a dry atmosphere. The third atom is only displaced by bases. The formula of the hydrated acid is $C^{14}\ H^8\ N^2\ O^9 + 3\ H^2\ O$. The ammonia salt is anhydrous, as is also the silver salt. Neutral indigo-nitrate of baryta was obtained by boiling the acid with carbonate of baryta; it forms shining needles which are difficultly soluble in cold water, insoluble in alcohol and æther. It explodes when heated; it contains five atoms of water, of which it loses four at 200° C., By boiling with caustic baryta or by the addition of ammonia a basic salt is obtained. It contains two atoms of base and five of water. The potassa salt is anhydrous. The formula of the indigo-nitric acid has a great resemblance to those of the salicyle series. Marchand endeavoured to trace the connexion, and in the first place analysed some of the primary compounds, because it was possible that the new atomic weight of carbon might make some difference in their constitution. His analyses of salicine, as well as those of Piria, Mulder, Otto and Erdmann, agree very closely with the formula $C^{28}\ H^{38}\ O^{15}$, which explains the decompositions in a very satisfactory manner; the formation of salicylous acid (hyduret of salicyle) is very simple, $C^{28}\ H^{38}\ O^{15} = 2\,(C^{14}\ H^{12}\ O^4) + 7\ H^2\ O$. Gerhardt has remarked that traces of salicylous acid are formed among the products of the simple distillation of salicine; this method of preparing it is not, however, advantageous, inasmuch as very little is obtained. The best process is that of Piria as modified by Ettling. Three parts bichromate of potassa, three parts salicine, four parts and a half sulphuric acid, and thirty-six water. After twenty parts have passed over, twenty parts of water may be added and again distilled off. Marchand confirmed Piria's formula for salicylous acid, viz. $C^{14}\ H^{12}\ O^4$. Salicylic acid may be obtained by fusing salicylite of potassa with excess of potassa, or at once from salicine, as has been shown by Gerhardt; by fusing salicine with an excess of caustic potassa, hydrogen is evolved; the mass must not be allowed to become perfectly white, for then some of the salicylic acid is decomposed. Marchand employed two pounds and a half of potassa to half a pound of salicine. If too little potassa is used, resin and salicylous acid are produced. Marchand found the same formula as Piria. If this salicylic acid be mixed with strong nitric acid the action is exceedingly violent, and picrin-nitric acid is produced; if, however, it be treated with dilute nitric acid the so-called salicylo-nitric acid is formed, which Marchand has shown to be identical with indigo-nitric acid.—(*Journ. für Prakt. Chem.*, vol. xxvi. p. 386.)

On the Compounds of Sugar with Bases.

Berzelius determined the atomic weight of sugar from the analysis of the lead salt, which he considered to be a compound of one atom of sugar with two atoms of oxide of lead. Peligot analysed this salt, and also the compounds with baryta and chloride of sodium, and from them he deduced $C^{24} H^{36} O^{18}$ as the equivalent of anhydrous sugar, which combines with four atoms of base. But the true equivalent is not yet quite settled, for Berzelius threw out doubts as to Peligot's correctness; and the analysis of the baryta salt, upon which the latter chemist places considerable reliance, has been called in question by Liebig. With a view to clear up these mysteries Soubeiran undertook a series of experiments on the subject. As the compounds are very difficult to burn, he employed chromate of lead mixed with bichromate of potassa. Soubeiran found exactly the same formula for the baryta salt as Peligot; he could not obtain a compound containing less baryta. Brendecke prepared one with only 18·5 per cent. baryta, while the usual one contains 30 per cent.

Peligot has examined a combination of sugar with lime; he considers that it is always formed when lime is brought into contact with sugar; he found 14 per cent. of lime in it. Daniell however stated that he had obtained a compound containing one third of its weight of lime, by boiling fifteen parts of water with six of lime and ten of sugar for half an hour. Soubeiran could never obtain a compound with so much lime; the salt he found to be most generally formed is one in which the proportion of the ingredients is as 1 : 4; this is always produced when the lime is in excess, and the mixture is boiled, or else allowed to stand at ordinary temperatures. Brendecke prepares it by adding half a part of water to a mixture of equal parts of lime and sugar; a resinous mass is formed which is dissolved in water. This salt consists of $C^{24}H^{44}O^{22}+3\,CaO$. The compound containing two atoms of lime, 14 per cent., is much more difficult to prepare; an excess of sugar must be used (sugar thirteen parts, unslaked lime two parts), the salt must be precipitated from its solution by alcohol. Soubeiran could not obtain any other compound of lead but that with four atoms of base.

The compounds with potassa and soda have been examined by Brendecke, but are difficult to procure in a pure state, and are moreover deliquescent. Soubeiran did not make any experiments on them. From his researches he considers the constitution of an atom of sugar to be $C^{24}H^{36}O^{18} = S$, and the salts may be arranged as follows:—

Crystallized sugar $= S+4\,aq.$

$\left.\begin{matrix}\text{Potassa}\\ \text{comp.}\end{matrix}\right\} = S+RO$

...probably $= S+\left\{\begin{matrix}(KO.H^2O)\\ 3\,aq.\end{matrix}\right.$

Soda ...... $= S+\ NaO$

... probably $= S+\left\{\begin{matrix}(NaO.H^2O)\\ 3\,aq.\end{matrix}\right.$

$\left.\begin{matrix}\text{Chloride of sodium}\\ \text{compound}\end{matrix}\right\} = S+\left\{\begin{matrix}NaCl^2\\ 3\,aq.\end{matrix}\right.$

Lead comp. $= S+4\,PbO.$

Lime ... $= S+\left\{\begin{matrix}3\,(CaO+H^2O)\\ 1\,aq.\end{matrix}\right.$

Lime ... $= S+\left\{\begin{matrix}2\,(CaO+H^2O)\\ 2\,aq,\end{matrix}\right.$

Baryta ... $= S+\left\{\begin{matrix}2\,(BaO+H^2O)\\ 2\,aq.\end{matrix}\right.$

(*Journ. de Pharm. et de Chim.* Juin 1842.)

Plumbo-Sulphate of Ammonia.

Sulphate of lead is considerably soluble in sulphate of ammonia, particularly when boiled. A double salt crystallizes out on cooling; the best method of obtaining it is to precipitate a tolerably concentrated solution of acetate of lead with excess of dilute sulphuric acid; it is then neutralized with ammonia, and the whole boiled, by which the sulphate is dissolved. If this does not take place there is a want of sulphate of ammonia; if the solution does not deposit crystals on cooling, sulphuric acid must be added until turbidness commences. It appears as if the salt were easier formed when acetate of ammonia is present. The double salt forms small, but bright well-defined crystals. It is decomposed by water, and also by heat, when sulphate of lead and sulphite of ammonia are formed: the latter salt sublimes. It does not contain water of crystallization. According to the analysis of Professor Litton, its formula is $PbO, SO^3+N^2H^3O, SO^3$. —(*Ann. der Chem. und Ph.*, vol. xliii. p. 126.)

LXXX. *On a new Imponderable Substance, and on a Class of Chemical Rays analogous to the Rays of Dark Heat.* By John William Draper, *M.D., Professor of Chemistry in the University of New York*[*].

[With Figures, Plate I.]

IN the Number of this Journal for September 1841, I have pointed out several analogies which may be observed between the phænomena of the chemical rays and those of radiant heat.

In this communication it is my intention to show still more striking points of analogy, and also to direct the attention of chemists to equally striking points of discordance.

It will be seen from the remarkable facts detailed in this paper, that we are now forced to recognize the existence of a new imponderable agent, analogous in many of its properties to light, heat, and electricity, yet differing as much from them all as they do from one another.

So far as chemical analogies can direct us there does not appear any thing unphilosophical in the supposition of the existence of many imponderable agents analogous to those already known. The progress of science has indeed tended in different directions in the cases of the imponderable and ponderable bodies. Among the former, we have successively seen the agents that are concerned in galvanic phænomena and those of magnetism merged into electricity; but the ponderable bodies, especially those of a metallic kind, have greatly increased in number, though so far as their more obvious physical properties are concerned, the differences of many are almost undistinguishable. We have thus found it necessary to invert the maxims of the early cultivators of chemistry, who extended the number of æthereal agents very greatly, and believed that all metals and other ponderable principles were modifications of one or two primordial and elementary forms.

Centuries ago it was discovered that the sun's light had the property of effecting chemical changes in bodies, and it is stated that Scheele first noticed that this property was mainly due to the violet rays. Seebeck observed, that chloride of silver, exposed to the spectrum, varied its colour with the colour of the space in which it was held, and during the present century a very large amount of new observations has been accumulated. A new art, Photography, has come into existence.

The general supposition that obtains is, that the effects in question are due to the rays of LIGHT; hence all the words that

---

[*] Communicated by the Author.

have been introduced into use have reference to that supposition; such words as photography, photology, photometer, are derived from this erroneous hypothesis, and lead us to confound together things which ought to be kept essentially distinct.

As it is the object of this paper, and others which I am shortly to publish, to call the attention of chemists to the agent that is involved in photographic results as a clearly established and new imponderable substance, possessing striking analogies with light and heat, yet differing as much from them both as they do from each other, I am induced to propose for it a proper name, and to endeavour to establish for it a nomenclature that shall be free from ambiguity and keep the description of its phænomena separate from those of LIGHT. Whilst therefore I show that it undergoes radiation, reflexion, refraction, polarization, absorption, interference, &c. under the laws to which its radiant companions light and heat are subject, I wish to claim for it a separate and independent existence, to introduce it into the natural family of imponderable agents, with light, heat, and electricity. In that family it stands as the fourth member. Is there any reason that the progress of knowledge should not make known to us multiplied forms of imponderable substances as well as of ponderable matters? This agent differs from light and heat, as much as lead differs from zinc or tin.

When novel effects, brought about by novel causes, are met with, the purposes of science require new corresponding terms. In the case of the chemical rays of light it is so. I have experienced the need of a nomenclature of the kind from my earliest experiments. It is a rule of which modern philosophers know the value, that such names ought to be free from all attending hypothesis; for if this be not complied with, it soon comes to pass, as knowledge advances, that terms involving theoretical ideas lose much of their significance.

The chemical rays are associated with the rays of light, accompanying them in all their movements, originating with them, and unless disturbed continuing to exist along with them. But should a compound beam like this fall upon a sensitive surface, the chemical rays sink into it, as it were, and lose all their force, and the rays of light are left alone. Photographic results thus resulting from the reposing of the chemical rays on the sensitive surface are not however in themselves durable, as will be shown in this paper, for the rays escape away under some new form.

Tithonus was a beautiful youth whom Aurora fell in love with and married in heaven. The Fates had made him immortal, but unlike his bride, in the course of events he became feeble and decrepit, and losing all his strength was rocked to sleep in a cradle. The goddess, pitying his condition, metamorphosed him into a grasshopper.

The fact and the fable agree pretty well, and indeed the playful coincidence might be carried much further. The powers of photography, which bring architectural remains and the forms of statuary so beautifully and impressively before us, might seem to be prefigured by the speaking image of the son of Tithonus and Aurora that was to be seen in the deserts of Egypt. And besides this, such words as Tithonoscope, Tithonometer, Tithonography, Tithonic effect, Diatithonescence, are musical in an English ear. In this paper I shall therefore use the term Tithonicity and its derivatives in the same manner that we use electricity and its derivatives.

This communication takes up the consideration of three distinct facts:—

1st. The proof of the physical independence of TITHONICITY and LIGHT.

2nd. The proof of the physical independence of TITHONICITY and HEAT.

3rd. The proof of the existence of DARK TITHONIC RAYS, analogous to the rays of DARK HEAT. Under this head it will be shown, that tithonicity like heat enters transiently into bodies producing specific changes on them, and then slowly and invisibly RADIATES away. And the physical constitution of the new class of rays thus formed is entirely different from that of rays that come from incandescent sources; a distinction having a striking analogy in the case of heat. Tithonicity becomes *transiently* and *permanently latent* in bodies.

The Plate (Pl. I.), which accompanies this paper, serves to show that by the agency of absorbent media we may detect the existence of tithonic rays in every part of the spectrum unaccompanied by light. The results, there projected, were obtained by an arrangement such as that in Plate I. fig. 1. From a heliostat mirror *a a*, a beam of the sun's light was thrown in a horizontal position, and falling on a screen *b b*, a portion of it passed through a circular aperture one-fourth of an inch in diameter. At the distance of ten or twelve feet it fell on a glass trough *c c*, with parallel faces, into which any coloured solution could be placed; immediately behind the trough there was a double convex lens *d d*, of three feet focal length, and between them a second screen *f f*, with an aperture corresponding to the centre of the lens, half an inch in diameter. Behind the lens was situated a prism of flint glass *e*, which effected the dispersion of the incident beam. Now, the lens not being achromatic, the screen *r v* had to be placed in an inclined position in order to obtain a neat spectrum-image of the hole in *b b*, and this was attended with the great advantage of elongating the total length of the spectrum, and therefore increasing the measures. In order to obtain sensitive surfaces of great delicacy the silver plates were first iodized lightly, and then exposed to the vapour of bromine until they attained a full golden yellow.

In the Plate, the line No. 1, fig. 3, represents the *visible* colorific spectrum; it, with No. 2, serves as an index of comparison for all the others. No. 2 represents the effect of a spectrum that has not undergone the action of any absorbent medium on the bromoiodized plate, the extreme red tinges the plate white, the extreme violet brown, and all the intermediate space is of a rich brownish violet, with a point of maximum action nearly in its centre. The numerical subdivisions commence with 0 at the extreme red, and are graduated on a principle, which I shall explain in a future paper, which makes the spectra of different tithonographists comparable.

No. 3 shows the spectrum after absorption by the persulphocyanide of iron, and its corresponding tithonograph. This spectrum is divided into three portions, one of which is red and yellow, a second indigo, and a third violet. But the tithonograph exhibits an action far beyond the extreme red, half way through the dark space that intervenes in the middle of the spectrum, both ends of this lower part projecting into dark spaces; whilst the indigo ray, ordinarily so active, does not tithonize at all.

Without going into a long descriptive detail of the comparison of different spectras and their corresponding tithonographs, I shall here sum up the results which may be gathered from an inspection of the Plate.

By the absorbent action of the persulphocyanide of iron, we can prove the existence of invisible tithonic rays beyond the extreme red,—invisible rays corresponding to the green. We can also prove that the indigo-coloured rays of LIGHT may exist without tithonic effect.

By the absorbent action of neutral chloride of gold, we can insulate blue coloured rays of light that are not tithonic.

The green solution formed by a mixture of bichromate of potash, muriatic acid, and alcohol, enables us to insulate tithonic rays of the same refrangibility as the violet, but unaccompanied by any light.

The solution of sulphate of copper and ammonia enables us to insulate a visible red and yellow ray that are without tithonic power, and an invisible tithonic ray beyond the violet.

The solution of litmus enables us to obtain red and green light without action, and an invisible tithonic ray corresponding to the violet.

The solution of bichromate of potash enables us to obtain red and orange light without any tithonic effect.

Such results might be multiplied without end, for indeed there is scarcely an instance in which spectra of rays that have passed absorbent media are exactly coincident with their corresponding tithonographs. To set the matter plainly before the reader, the following tabular view, gathered from the Plate, may suffice.

Name of Solution.	Colour of LIGHT without Tithonic effect.	Invisible TITHONIC rays corresponding in refrangibility to the
Persulphocyanide of iron	Indigo	Extreme red, green.
Chloride of gold	Blue.	
Chrome solution	....	Violet.
Sulph. cop. and ammonia	Red, yellow	Extreme violet.
Litmus	Red, green	Violet.
Bichromate of potash ...	Red, orange.	

From this, therefore, I infer the entire independence *throughout the spectrum* of the luminous rays that give to the organs of vision the impression of colour, and the tithonic rays.

When I come to describe the dark tithonic rays that are analogous to the rays of dark heat, and which are unaccompanied by any kind of light whatsoever, no further doubt can be entertained on this subject. I have also some other proofs of a very remarkable kind, to be described hereafter, drawn from the phænomena exhibited by tithonic rays that have undergone polarization.

Next, as to the independence of these rays and the rays of heat.

One of the most striking proofs of this is the facility with which impressions of the moon's disc may be obtained on Daguerreotype and other sensitive plates. Even with lenses of comparatively small diameter, and in the space of a few minutes, strong impressions of the moon's surface may be taken. There is no more difficulty in obtaining these sketches than there is in copying a building or a statue, or any other object on which the sun is shining. But the moonbeams have hitherto given no trace of the presence of heat.

I found, moreover, by direct trial, that plates which had been carefully prepared so as to be exceedingly sensitive, were unaffected by the radiant heat of copper at any temperature up to a red heat. These dark rays therefore have no kind of effect on such surfaces. A sensitive plate may be made so hot that it cannot be touched, yet its surface remains unchanged, and even the radiant heat emitted by brightly incandescent bodies has no effect, as I also proved.

Lastly,—*Proof of the existence of* DARK TITHONIC RAYS *analogous to the rays of* DARK HEAT.

The experiments, now to be described, were made with Daguerreotype plates iodized at first to a pale lemon yellow, then brought to a golden hue by immersion in the vapour of bromine, and lastly exposed for a short time to the vapour of iodine again.

Having exposed such a plate, fig. 2, *a b*, to the action of weak daylight or lamplight for a period of time which would cause it to whiten powerfully *all over* if placed in the vapour of mercury, carry it into a room which is totally dark, and suspend at a distance of one-eighth of an inch from its surface a metallic screen *c d*, the under-surface of which is blackened. Let all remain in the dark four or five hours, and then remove the sensitive plate *a b*, and expose it to the vapour of mercury. All that portion of it which was not covered by the screen *c d*, will undergo no change, but that which was beneath *c d* will whiten powerfully.

From this remarkable result I infer, that the tithonicity that had originally disturbed the surface of the plate equally all over, has escaped away from those portions that were uncovered; but that its escape has been entirely prevented by the action of the screen; and this must be through RADIATION, for the screen is at a distance and has never touched the plate. And, further, that the rays that do thus escape away are absolutely invisible to the eye.

Now, suppose a piece of black cloth, placed in the rays of the sun until it has become warm, were carried into a cold room and half its surface screened by some material, as a piece of glass, at a short distance; there cannot be a doubt that the uncovered portion would cool fast by radiation, but the screened portion more slowly, for its radiation would be arrested by the glass plate.

The two cases are absolutely alike.

Tithonicity therefore radiates exactly after the manner of heat.

This also furnishes proof, in addition to those I have heretofore given in this Journal, that not only does tithonicity become LATENT in bodies, but that it becomes latent in two ways, *transiently* and *permanently*, exactly after the manner of heat.

The same result is obtained when other sensitive surfaces are employed, the period of time differing for different bodies. Guided, therefore, by the analogy of heat, I perceive that bodies have a relation to this imponderable agent corresponding to that of *specific heat*. It follows therefore with certainty that,—

The specific tithonicity of bodies is the prime function on which their sensitiveness depends. Under this point of view the sensitiveness is inversely as the specific tithonicity.

The circumstances under which this experiment is made serve also to show that metallic bodies are non-conductors of tithonicity.

This contrasts remarkably with their action towards heat.

Having exposed a sensitive plate *a b* to light until it would whiten if mercurialized, as before; and having prepared a second, *c d* (fig. 2), in total darkness, without allowing any light to have access to it, suspend this latter over the former at the distance of one-eighth of an inch, so as to cover it about half. Keep the two plates in darkness for several hours and then mercurialize both. That portion, *a c*, of the first, not covered by the second, will not whiten; that portion of the second, *b d*, not covered by the first, will also remain unchanged; but both on those parts that have looked towards each other will whiten.

From this I infer, that the portion of the first not overshadowed by the second does not whiten because its tithonicity escapes away under the form of dark tithonic rays.

I also infer, that as both plates are nearly equally whitened on those portions of their surfaces that have looked towards each other, the dark tithonic rays that have escaped from the first plate, notwithstanding their invisibility, have retained their peculiar chemical force, and have affected the second plate.

The analogy with heat is here perfectly observed. A hot *non-conducting* plate, set partially opposite a cold one, would warm that plate on the portion looking towards it, and through the consequent retardation of radiation would retain its own heat to a certain extent. But all those portions unopposed by the cold plate would cool down by radiation rapidly.

This experiment proves in a clear and undoubted manner the total physical independence of tithonicity and light.

Hence the absolute necessity of some such nomenclature as that proposed,—the chemical rays of light is a misnomer.

On the surface of a sensitive plate that has been suitably exposed, as heretofore, place a fragment of perfectly clean and colourless glass. Allow it to remain there for four or five hours in a dark room, then mercurialize, and it will be found that the portion on which the glass has been placed will whiten powerfully, but all the rest will remain unchanged.

This, therefore, proves that colourless glass is nearly opake to the dark tithonic rays, a result observed also in the case of the dark rays of heat.

I made a comparative trial of the relative permeability of colourless plate glass and common writing-paper. A sensitive surface was exposed until it had slightly but very plainly commenced to turn brown. On one portion I now laid a piece of clear glass, and by the side of it a piece of writing-paper; the arrangement was next placed in the dark for four hours; it was then mercurialized at 160° Fahr. for an hour, and the result was very striking. Notwithstanding the long exposure to the mercury vapour, all those portions that had not been covered were perfectly unaffected, the portion that had been covered by the glass was of an intensely deep brown colour, but the portion covered by the paper was marked by a distinct but very faint white stain. It was therefore plain, that from the uncovered portions all the tithonicity had radiated away,—from the portions covered by the writing-paper the same effect almost to the same extent had occurred, the paper, however, slightly obstructing the passage of the rays,—but radiation had been wholly prevented from those parts covered by the colourless glass.

Writing-paper is therefore far more permeable to the dark tithonic rays than the purest plate glass.

This property it will be hereafter convenient to speak of under the designation of Diatithonescence or Transtithonescence.

Blue, red and yellow glass obstruct to a great extent the process of radiation. In several trials it seemed as though the yellow was more transparent than the others, but there was not much difference.

Transparent rock-salt appears to hold very nearly the same relation of diatithonicity as plate glass.

In like manner the following substances in thin plates obstruct the radiation of tithonicity:—Sulphate of lime, beryl, agate, rock-crystal, calc-spar, mica, wafers, metallic bodies, cloth of cotton, wood, ivory, coloured glass, &c., &c.

The remarkable results described in the Philosophical Transactions by Sir John Herschel (1840, p. 44), but left by him without any explanation, are of the kind now under discussion. He found that paper washed with nitrate of silver, if exposed to the sun under a piece of glass, darkened much more rapidly than if the glass were away. This effect was by no means limited to that variety of paper, but was observable also with many other tithonographic compounds. Transparent minerals, such as topaz, selenite, Iceland spar, quartz, produced the same results as glass. But on gloomy days the phænomena did not appear, a bright sunshine being apparently requisite for their production. "When a piece of nitrated paper, for instance, was rolled round a cylindrical surface of moderate convexity, covered with black velvet, and the piece of glass laid gently in contact with it, the effect of sunshine was exalted at the line of contact, but on either side of that line as the interval increased the influence of the glass diminished, and at less than half an inch distance no difference could be perceived between the impressions under the glass and in the free air."

Now all this is precisely what should happen if the tithonographic compound radiates whilst it is undergoing decomposition. The rays, which come from the sun, pass through the glass with but little loss from absorption, falling upon the nitrate they decompose it, and now it commences radiating, but the physical character of these rays is very different from the character they possessed before impinging on the nitrate. *Now* they cannot get through the glass, *before* they passed without difficulty. So it is precisely in the case of heat. Much of the heat of the sun passes through plate glass, and if it falls on a dark surface that can absorb it that surface becomes presently warm and commences radiating; but the physical constitution of these rays is changed, they cannot get through the glass, and if a non-conducting black surface, half covered by a piece of glass and half in the free air, were exposed to the sun, the covered half would for these obvious reasons become the hotter. For the same reason, precisely, in the tithonic experiment the glass increases the final effect by obstructing radiation.

It is very obvious why such effects cannot be produced on gloomy days. If at such times we were to expose a piece of black cloth, partially covered by glass, no difference of temperature would be perceptible in its covered and uncovered portions. The reasons are analogous in each case.

An experiment the same in principle as Sir John Herschel's may be easily made. Upon a sensitive plate, that has been exposed a short time to a feeble light, place a convex lens; the arrangement being left for a time in a dark room. When you have mercurialized, you will find a central dark point corresponding with the point of contact, and round it a white areola that shades gradually and imperceptibly away. With a lens with which I have occasionally made this experiment, the areola is nearly an inch in diameter, the lens being a double convex of about two inches focus.

LXXXI. *On Thermography, or the Art of Copying Engravings, or any printed Characters from Paper on Metal Plates; and on the recent Discovery of Moser, relative to the formation of Images in the Dark. By* ROBERT HUNT, *Secretary of the Royal Cornwall Polytechnic Society*[*].

THE Journal of the Academy of Sciences of Paris, for the 18th of July, 1842, contains a communication made by M. Regnault from M. Moser of Königsberg, "Sur la formation des images Daguerriennes[†];" in which he announces the fact, that "*when two bodies are sufficiently near, they impress their images upon each other.*" The Journal of the 29th of August contains a second communication from M. Moser[‡], in which the results of his researches are summed up in twenty-six paragraphs. From these I select the following, which alone are to be considered on the present occasion.

"9. All bodies radiate light even in complete darkness.

"10. This light does not appear to be allied to phosphorescence, for there is no difference perceived whether the bodies have been long in the dark, or whether they have been just exposed to daylight, or even to direct solar light.

"10. Two bodies constantly impress their images on each other, even in complete darkness.

"14. However, for the image to be appreciable, it is necessary, because of the divergence of the rays, that the distance of the bodies should not be very considerable.

"15. To render the image visible, the vapour of water, mercury, iodine, &c. may be used.

"17. There exists *latent light* as well as latent heat."

The announcement at the last meeting of the British Association of these discoveries naturally excited a more than ordinary degree of interest. A discovery of this kind, changing, as it does, the features, not only of the theories of light adopted by philosophers, but also the commonly received opinions of mankind, was more calculated to awaken attention than any thing which has been brought before the public since the publication of Daguerre's beautiful photographic process. Having instituted a series of experiments, the results of which appear to prove that these phænomena are not produced by *latent light*, I am desirous of recording them.

I would not be understood as denying the absorption of light by bodies; of this I think we have abundant proof, and it is a matter well deserving attention. If we pluck a Nastur-

---

[*] Read at the Cornwall Polytechnic Society, Tuesday, Nov. 8, 1842.

[†] *Comptes Rendus*, tome xv. No. 3. folio 119.

[‡] Translations of M. Moser's papers containing the full details of his researches and discoveries will be published in the course of the present month (December) in Part XI. of Taylor's Scientific Memoirs.

tium when the sun is shining brightly on the flower, and carry it into a dark room, we shall still be enabled to see it by the light which it emits.

The human hand will sometimes exhibit the same phænomenon, and many other instances might be adduced in proof of the absorption of light; and, I believe, indeed of the principle that light is latent in bodies. I have only to show that the conclusions of M. Moser have been formed somewhat hastily, being led, no doubt, by the striking similarity which exists between the effects produced on the Daguerreotype plates under the influence of light, and by the juxtaposition of bodies in the dark, to consider them as the work of the same element.

1. Dr. Draper, in the Philosophical Magazine for September 1840, mentions a fact which has been long known, "That if a piece of very *cold* clear glass, or what is better, a *cold* polished metallic reflector, has a little object, such as a piece of metal, laid on it, and the surface be breathed over once, the object being then carefully removed, as often as you breathe again on the surface, a spectral image of it may be seen, and this singular phænomenon may be exhibited for many days after the first trial is made." Several other similar experiments are mentioned, all of them going to show that some mysterious molecular change has taken place on the metallic surface, which occasions it to condense vapours unequally.

2. On repeating this simple experiment, I find that it is necessary, for the production of a good effect, to use dissimilar metals; for instance, a piece of gold or platina on a plate of copper or of silver will make a very decided image, whereas copper or silver on their respective plates gives but a very faint one, and bodies which are bad conductors of heat, placed on good conductors, make decidedly the strongest impressions when thus treated.

3. I placed upon a well-polished copper plate, a sovereign, a shilling, a large silver medal, and a penny. The plate was gently warmed by passing a spirit lamp along its under surface; when cold, the plate was exposed to the vapour of mercury; each piece had made its impression, but those made by the gold and the large medal were most distinct; not only was the disc marked, but the lettering on each was copied.

4. A bronze medal was supported upon slips of wood, placed on the copper, one-eighth of an inch above the plate. After mercurialization, the space the medal covered was well-marked, and for a considerable distance around the mercury was unequally deposited, giving a shaded border to the image; the spaces touched by the [mercury?] were thickly covered with the vapour.

5. The above coins and medals were all placed on the plate, and it was made too hot to be handled, and allowed to cool without their being removed; impressions were made on the plate in the following order of intensity,—gold, silver, bronze, copper. The mass of the metal was found to influence materially the result; a large piece of copper making a better image than a small piece of silver. When this plate was exposed to vapour, the results were as before (3, 4). On rubbing off the vapour, it was found that the gold and silver had made permanent impressions on the copper.

6. The above being repeated with a still greater heat, the image of the copper coin was, as well as the others, most faithfully given, but the gold and silver only made permanent impressions.

7. A *silvered* copper plate was now tried with a moderate warmth (3). Mercurial vapour brought out good images of the gold and copper; the silver marked, but not well defined.

8. Having repeated the above experiments many times with the same results, I was desirous of ascertaining if electricity had any similar effect; powerful discharges were passed through and over the plate and discs, and it was subjected to a long-continued current without any effect. The silver had been cleaned off from the plate (7), it was now warmed with the coins and medals upon it, and submitted to discharges from a very large Leyden jar; on exposing it to mercurial vapour, the impressions were very prettily brought out, and strange to say, spectral images of those which had been received on the plate when it was silvered (7); thus proving that the influence, whatever it may be, was exerted to some depth in the metal.

9. I placed upon a plate of copper, blue, red and orange-coloured glasses, pieces of crown and flint glass, mica, and a square of tracing paper. These were allowed to remain in contact half an hour. The space occupied by the red glass was well marked, that covered by the orange was less distinct, but the blue glass left no impression; the shapes of the flint and crown glass were well made out, and a remarkably strong impression where the crown glass rested on the tracing paper, but the mica had not made any impression.

10. The last experiment repeated, after the exposure to mercurial vapour; heat was again applied to dissipate it; the impression still remained.

11. The experiment repeated, but the vapour of iodine used instead of that of mercury. The impressions of the glasses appeared in the same order as before, but also a very beautiful image of the mica was developed, and the paper well

marked out, showing some relation to exist between the substances used and the vapours applied.

12. Placed the glasses used above (9, &c.) with a piece of well-smoked glass for half an hour, one-twelfth of an inch below a polished plate of copper. The vapour of mercury brought out the image of the smoked glass only.

13. All these glasses were placed on the copper and slightly warmed; red and smoked glasses gave after vaporization, equally distinct images, the orange the next; the others left but faint marks of their forms; polishing with Tripoli and putty powder would not remove the images of the smoked and red glasses.

14. An etching, made upon a smoked etching ground on glass, the copper and glass being placed in contact. The image of the glass only could be brought out.

15. A design cut out in paper was pressed close to a copper plate by a piece of glass, and then exposed to a gentle heat; the impression was brought out by the vapour of mercury in beautiful distinctness. On endeavouring to rub off the vapour, it was found, that all those parts which the paper covered, amalgamated with mercury, which was removed from the rest of the plates; hence there resulted a perfectly permanent white picture on a polished copper plate.

16. The coloured glasses before named (9, 12) were placed on a plate of copper with a thick piece of charcoal, a copper coin, the mica and the paper, and exposed to a fervent sunshine. Mercurial vapour brought up the images in the following order: smoked glass, crown glass, red glass, mica beautifully delineated, orange glass, paper, charcoal, the coin, blue glass; thus distinctly proving that the only rays which had any influence on the metal, were the calorific rays. This experiment was repeated on different metals, and with various materials, the plate being exposed to steam, mercury and iodine; I invariably found that those bodies which absorbed or permitted the permeation of the most heat gave the best images. The blue and violet rays could not be detected to leave any evidence of action, and as spectra imprinted on photographic papers by light, which had permeated these glasses, gave evidence of the large quantity of the invisible rays which passed them freely, we may also consider those as entirely without the power of effecting any change on compact simple bodies.

17. In a paper which I published in the Philosophical Magazine for October 1840, I mentioned some instances in which I had copied printed pages and engravings on iodized paper, by mere contact and exposure to the influence of the

calorific rays, or to artificial heat. I then, speculating on the probability of our being enabled by some such process as the one I then named, to copy pictures and the like, proposed the name of THERMOGRAPHY, to distinguish it from Photography.

18. I now tried the effects of a print in close contact with a well-polished copper plate. When exposed to mercury, I found that the outline was very faithfully copied on the metal.

19. A paper ornament was pressed between two plates of glass, and warmed; the impression was brought out with tolerable distinctness on the under and warmest glass, but scarcely traceable on the other.

20. Rose leaves were faithfully copied on a piece of tin plate, exposed to the full influence of sunshine, but a much better impression was obtained by a prolonged exposure in the dark.

21. With a view of ascertaining the distance at which bodies might be copied, I placed upon a plate of polished copper a thick piece of plate glass, over this a square of metal, and several other things, each being larger than the body beneath. These were all covered by a deal box, which was more than half an inch distant from the plate. Things were left in this position for a night. On exposing to the vapour of mercury it was found that each article was copied, the bottom of the deal box more faithfully than any of the others, the grain of the wood being imaged on the plate.

22. Having found by a series of experiments that a blackened paper made a stronger image than a white one, I very anxiously tried to effect the copying of a printed page or a print. I was partially successful on several metals, but it was not until I used copper plates amalgamated on one surface, and the mercury brought to a very high polish, that I produced any thing of good promise. By carefully preparing the amalgamated surface of the copper I was at length enabled to copy from paper, line-engravings, wood-cuts and lithographs, with surprising accuracy. The first specimens produced (which I have the satisfaction of now submitting to your inspection), exhibit a minuteness of detail and sharpness of outline quite equal to the early Daguerreotypes and the photographic copies prepared with chloride of silver*.

The following is the process at present adopted by me, which I consider far from perfect, but which affords us very delicate images.

A well-polished plate of copper is rubbed over with the nitrate of mercury, and then well washed to remove any nitrate

---

* The first faithful copy of the lines of a copper-plate engraving was obtained by Mr. Cantabrana, who has since succeeded in procuring some tolerable specimens on unamalgamated copper, which cannot be rubbed off.

of copper which may be formed; when quite dry a little mercury taken up on soft leather or linen is well rubbed over it, and the surface worked to a perfect mirror.

The sheet to be copied is placed smoothly over the mercurial surface, and a sheet or two of soft, clean paper being placed upon it, it is pressed into equal contact with the metal by a piece of glass, or flat board; in this state it is allowed to remain for an hour or two. The time may be considerably shortened by applying a very gentle heat for a few minutes to the under surface of the plate. The heat must on no account be so great as to volatilize the mercury. The next process is to place the plate of metal in a closed box, prepared for generating the vapour of mercury. The vapour is to be slowly evolved, and in a few seconds the picture will begin to appear; the vapour of mercury attacks those parts which correspond to the white parts of the printed page or engraving, and gives a very faithful, but a somewhat indistinct image. The plate is now removed from the mercurial box, and placed into one containing iodine, to the vapour of which it is exposed for a short time; it will soon be very evident that the iodine vapour attacks those parts which are free from mercurial vapour, blackening them. Hence there results a perfectly black picture, contrasted with the gray ground formed by the mercurial vapour. The picture being formed by the vapours of mercury and iodine, is of course in the same state as a Daguerreotype picture, and is readily destroyed by rubbing. From the depth to which I find the impression made into the metal, I confidently hope to be enabled to give to these singular and beautiful productions a considerable degree of permanence, so that they may be used by engravers for working on.

It is a curious fact that the vapours of mercury and of iodine attack the plate differently, and I believe it will be found that vapours have some distinct relation to the chemical or thermo-electrical state of the bodies upon which they are received. Moser has observed this, and attributes the phænomena to the colours of the rays, which he supposes to become latent in the vapour on its passing from the solid into the more subtile form. I do not however think this explanation will agree with the results of experiments. I feel convinced that we have to deal with some thermic influence, and that it will eventually be found that some purely calorific excitement produces a molecular change, or that a thermo-electric action is induced, which effects some change in the polarities of the ultimate atoms of the solid.

These are matters which can only be decided by a series of well-conducted experiments, and, although the subject will not be laid aside by me, I hope the few curious and certainly important facts which I have brought before you, will elicit the attention of those whose leisure and well-known experimental talents qualify them in the highest degree for the interesting research into the action of those secret agents which exert so powerful an influence over the laws of the material creation. Although attention was called to the singular manner in which vapours disposed themselves on plates of glass and copper, two years since by Dr. Draper, Professor of Chemistry at New York, and about the same time to the calorific powers of the solar spectrum, by Sir John Herschel*, and to the influence of heat artificially applied, by myself (17), yet it is certainly due to M. Moser of Konigsberg, to acknowledge him to be the first who has forcibly called the attention of the scientific world to an inquiry which promises to be as important in its results as the discovery of the electric pile by Volta.

As to the practical utility of this discovery, when we reflect on the astonishing progress made in the art of photography since Mr. Fox Talbot published his first process, what may we not expect from thermography, the first rude specimens of which exhibit far greater perfection than the early efforts of the sister art?

As a subject of pure scientific interest thermography promises to develope some of those secret influences which operate in the mysterious arrangements of the atomic constituents of matter, to show us the road into the yet hidden recesses of nature's works, and enable us to pierce the mists which at present envelope some of the most striking phænomena, which the penetration and industry of a few "chosen minds" have brought before our obscured visions. It has placed us at the entrance of a great river flowing into a mighty sea, which mirrors in its glowing waters some of the most brilliant stars which beam through the atmosphere of truth.

Falmouth, Nov. 7, 1842. ROBERT HUNT.

LXXXII. *On the Elevation and Denudation of the District of the Lakes of Cumberland and Westmoreland. By* WILLIAM HOPKINS, *Esq., F.G.S.*†

THE general structure of this district has been long known to geologists through the labours of Professor Sedgwick and other geologists. The object of this paper is to afford theoretical expla-

---

* Philosophical Transactions, Part I. for 1840, page 50.

† From the Proceedings of the Geological Society, vol. iii. part ii. p. 757; having been read on June 1st, 1842.

nations of the observed phænomena of elevation and denudation. The general boundary of tract may be considered as sufficiently defined on the north by the band of mountain limestone which runs from Kirkby Stephen by Heskel, on the west by the coast, and on the south by the discontinuous and irregular band of limestone, which again nearly meets the great mountain limestone ridge of Yorkshire, by which, and the great fault along its base, the district is bounded on the east. The general strike of the limestone beds at any point, as well as that of the new red sandstone reposing upon them, coincides with the direction of the boundary at that point, except on the east, where the boundary is the great fault just mentioned. Consequently the dip is nearly perpendicular to the boundary, and round the western side is divergent from the extremity of the axis of the district, which may be considered to extend from near Scaw Fell over Kirkstone and Howgile Fells. On the west the dip frequently amounts to between 20° and 30°; and it should be remarked, that it appears to be very nearly as great in the new red sandstone beds as in those of the subjacent limestone. The mountain limestone reposes unconformably on the older formations which, within the limestone band, occupy the surface. The general strike appears to be somewhat north of N.E. and south of S.W.

The *surface of junction* of the mountain limestone and the older formations beneath can be well examined in many places, and the author concludes that the surface on which the limestone was deposited must have been an *even* surface in the same sense in which the expression may be now applied, for instance, to the bottom of the German Ocean. He also concludes that this surface must have been *horizontal*. This will necessarily follow from the previous inference, unless it be contended that those animals whose remains are now found in the lower limestone beds could exist in the perfect performance of all the functions of life, at the depth of several thousands of feet, under an enormous pressure and in darkness, as well as at small depths, under small pressure and in the light of the sun.

This surface of junction wraps round the outer portion of the district, and, if continued as an imaginary surface, over the central portion in the manner which the inclination of the existing portion would obviously suggest, it would pass considerably over the tops of the highest mountains of the district, to which it would form a complete envelope. Hence it follows that if the movement which produced the geological elevation of the existing portion of the surface of junction affected the central portion of the district in the same manner as in all analogous cases in which the evidence is complete, it will follow that the present surface of the Cumbrian mountains must have been beneath the surface of the ocean at the commencement of the deposition of the mountain limestone. The truth of this conclusion involves that also of the original horizontality of the surface of junction.

The *stratification* of the older rocks of the district can afford no direct evidence on this point on account of the previous disturbance to which they had been subjected; but the great *faults* of the district prove to demonstration that its central portion must have been submerged in the ocean subsequently to the formation of those faults; for, if an enormous denudation had not taken place after their formation, every large fault must have given rise to a mural precipice, or great ridge (such as that which the Penrin and Craven faults have produced), by the elevation of the mass on one side of the fault relatively to that on the other. The total absence of any such precipice or ridge where enormous faults unquestionably exist, prove incontrovertibly the submergence above asserted.

Faults.—The faults of this district may be arranged in three classes, according to the evidence we possess of their existence:—

(1.) Those which offer conclusive evidence of dislocation. Such are those of the Dudden, Coniston Water, one between Coniston Water and Windermere, Trentbeck and Kentmere.

(2.) Faults along the Lake valleys. The existence of these faults is inferred from that of the lakes, the formation of which it would appear impossible to account for without referring them to dislocations along the valleys in which they are found. The bottom of Wastwater, for instance, is probably at a considerably lower level than the surface of the sea, and it has not been formed by the filling up of the lower end of the valley, for the bottom of it consists of the solid rock *in situ.* It appears inconceivable that such a lake should have been scooped out by the action of water.

(3.) Faults along the upper portions of other valleys. If the Lake valleys have originated in dislocations we seem justified in inferring, *from analogy*, that other valleys differing from the former only in the circumstance of not containing lakes, have had a similar origin. It should be remarked, however, that this evidence can probably be depended upon only in the upper parts of the valleys, where denuding agencies must probably have acted for a much shorter period than at lower levels, where they may have formed valleys much more independently of previous dislocation.

Theory of Elevation.—If we allow the conclusiveness of the above evidence of faults, we have here a system of which the law is obvious. Round the western extremity of the district they diverge from its highest point and extremity of its axis of elevation. On the north side they assume northerly, and then north-easterly, directions; and on the southern side they take southerly directions. If we conceive a stratum of the mountain limestone, or the surface of junction above described to be continued over the central portion of the district, its dip along the faults would very nearly coincide with their directions.

This is one of the laws connecting the directions of dip and of dislocation, resulting from the theory which the author has elsewhere developed, supposing the faults to have been caused by the elevation which gave to the limestone beds their present position. This theory would therefore appear to assign these faults to the epoch of the disturbance of the carboniferous system. There is also, however, another law pointed out by that theory, viz. that a system of dislocations may also exist having the same directions as the *strike* of the disturbed beds. Consequently those faults which are in the direction of the strike of the beds of the older formations, may, according to this theory, be assigned to the epoch of the elevation and dislocation of those beds. The great faults of the Dudden, Coniston Water and Troutbeck are of this class, since their directions coincide very nearly with the mean strike of the older beds. Theory, therefore, leaves the epoch of these faults undetermined; nor has the point been settled by observation, since there is no direct evidence to prove whether these faults have affected the mountain limestone or not.

It may be thought that the mountain limestone must have been more decidedly disturbed by the great faults above mentioned had they been produced at the epoch of the disturbance of the carboniferous system. It must be remarked, however, that the direct evidence of these faults is found only at a considerable distance from the existing portions of mountain limestone, and that if they originated in that *central and local elevation* to which the actual configuration of this tract must be due (at whatever epoch it took place), the diverging faults, however great near the centre of the district, would disappear as they approached its boundary. The author, however, is disposed to refer the four great faults above mentioned to the disturbance of the older rocks. They appear to have produced such enormous relative displacements of the masses on opposite sides of them, as may be more probably referrible to the more intense action of the elevatory forces which disturbed the older formations than to that which subsequently took up the mountain limestone.

But, it may be urged, the directions of these great dislocations do not coincide with that of the actual strike of the older beds. The author shows that if this coincidence existed (as it ought according to theory) after the elevation of the older beds, but previously to that of the limestone, it could not possibly exist after the latter elevation in those parts in which the deviation from such coincidence is now recognised, viz. along the band of limestone interstratified with the older beds, and crossing the above faults in its course from the Dudden to Troutbeck. To one who has a distinct conception of the geometry of the subject, it will easily appear that the elevation which gave its present position to the beds of mountain limestone, and (as the author conceives) its dome-like configuration to the district, would necessarily give to the strike of the beds along the above line, a direction approximating more to east and west than the original strike, while it would have no effect on the direction of a vertical fault as determined by its intersection with the surface. This accounts for the actual difference between the directions of the above faults and that of the strike.

Upon the whole, the author considers it probable that the four great parallel faults above mentioned are due to the elevation of the older rocks, the fractures having been probably renewed by the elevation of the carboniferous series. The divergent faults he conceives to be unquestionably due to the movement which impressed upon the district its peculiar configuration, and the *geological elevation* to which that configuration is due, whatever be the epoch to which that movement may be referred. If this be the case, these faults are entirely in accordance with theory.

It appears to the author that this movement commenced with the breaking up of the carboniferous series, and was continued, or rather perhaps resumed, after the deposition of the new red sandstone. If the beds of these formations were originally horizontal, as above contended, this conclusion must necessarily be true, as shown by the present inclination of these beds. Whether the limestone beds were strictly sedimentary, or formed in the manner of coral reefs, the author contends equally for the original horizontality of the surfaces of stratification ; and that such was the original character of the beds of new red sandstone, no geologist, he conceives, can doubt for a moment. If this be allowed, the above conclusion respecting the epoch of elevation appears as incontrovertible as the nature of geological evidence will admit of.

Series of Geological Events.—After the elevation of the older rocks, including the old red sandstone, the whole district must have been under the surface of the sea, and subjected to the powerful action of denuding causes, by which the upturned edges of the disturbed beds were worn to an even surface, and the existing masses of old red conglomerate washed into the hollows.

The mountain limestone was deposited on the worn and even surface of the older rocks, and, if the conditions were sufficiently favourable for its formation, may have extended over the whole district.

The great movement which broke up the carboniferous series gave, in part, its dome-like form to the district, and elevated its surface very nearly to, or perhaps above, the surface of the ocean.

The deposition of the new red sandstone afterwards took place, but did not probably extend over the district on account of the elevation already given to it. This formation probably thinned off as it approached the central elevation, but was deposited in much greater thickness than it has at present in the Vale of Eden. From the present height and thickness of the sandstone near Penrith, the author thinks it probable that the depth of the submarine valley immediately west of Stainmoor was not more than 300 or 400 feet, and perhaps considerably less, measuring from the level of the lowest part of the Stainmoor pass.

To this period of repose succeeded another of disturbance, in which the new red sand was dislocated and elevated. It was during this period, the author conceives, that the surface of the district first began to acquire any permanent and considerable elevation above the surface of the sea. The denudation of the red sand would commence with these movements, but was probably completed only as the whole tract of country emerged slowly from beneath the surface of the sea. If we reject the glacial theory in its application to the transport of blocks, as totally inadmissible in the case before us, this emergence must necessarily have taken place subsequently to the transport of blocks from the Cumbrian mountains across Stainmoor.

The author conceives the valleys of the district to have been formed during this gradual emergence; the action of denuding causes being facilitated by previous dislocations, the masses, the removal of which formed the valleys, would at the same time be transported and spread over the surrounding country. The formation of the existing lakes must have been one of the most recent events in the geological history of this region.

Period of Transport of Erratic Blocks.—The author thinks that geologists have frequently limited too much the period during which the transport of blocks may have taken place. When blocks are found reposing on an undisturbed formation, the only conclusive inference which can be drawn from the fact is, that the *last stage of their movement* was posterior to the deposition of the beds on which they rest. If the beds be much disturbed, but all the irregularities and asperities of its external surfaces worn away by long-continued attrition, we may generally conclude that the same action would have worn away any blocks previously existing on its surface, and therefore any blocks now existing on such surface must have been lodged there subsequently to its denudation. Also, when diluvial gravel contains organic remains, we may conclude that the *last stage* of its movement must have been subsequent to the existence of the animals whose remains are entombed in it. To contend, for instance, that the diluvial gravel of Norfolk was not removed from its original site till the post-tertiary period, is to draw an inference which the author deems altogether inadmissible.

The great mass of diluvium from the Cumbrian mountains reposes on nothing more recent than the new red sandstone, and the author conceives that its transport might begin with the elevatory movements which disturbed that formation, when the surface of the present mountainous district began to rise permanently above the surface of the ocean, and the valleys began to be formed. This is the more remote limit of the period to which the transport of diluvium and blocks can be referred; the other limit is the emergence of Stainmoor (over which so many blocks passed) from beneath the surface of the ocean, assuming the total inadequacy of the glacial theory to account for that transport. The present height of Stainmoor is stated to be about 1500 feet above the sea; consequently an elevation of from 1500 to 2000 feet must have taken place in these regions since the transport of the Cumbrian blocks across the Penim ridge—a fact which appears corroborative of the author's opinion, that the district had scarcely emerged from the ocean at the more remote of the above-mentioned limits of the possible period of transport.

Modes of Transport—Glacial Theory.—This theory, in its application to the transport of blocks across Stainmoor, involves such obvious mechanical absurdities, that the author considers it totally unworthy of the attention of the Society. Polished and striated rocks were, however, detected by Dr. Buckland, and pointed out by him to the author in various places. The author does not feel himself called upon to offer any decided opinion as to the cause of such phænomena; he here speaks of the glacial theory only with reference to the solution it offers of the problem of the transport of blocks or detritus to distant localities.

Iceberg Theory.—There appears to be no doubt that floating ice may have played an important part in some cases in the transport of large blocks, but the author doubts whether such agency has been at all employed in the case under consideration. In the first place, he cannot but consider it absurd to attribute the formation of a bed of diluvium spread out with approximate uniformity over an extended area to the action of floating ice. Such a distribution of the transported matter is the *necessary* effect of broad currents of water, which, at most, is the *merely possible* effect of floating ice. Secondly, there appears no adequate reason why blocks transported by floating ice should diminish in size as their distance from their original site increases; why the Cumbrian blocks on the eastern coast of Yorkshire should be generally much smaller than those less remote from the place whence they came. Thirdly, the theory in its application to the case before us involves a great physical difficulty—a depression of temperature, for which no adequate cause has yet been assigned. The author does not admit the parallel which has been drawn between this case and that of places in equal latitudes in South America or that of the island of Georgia.

Transport by Currents of Water.—It has already been remarked that the spreading out of diluvial matter into a horizontal stratum may be regarded as the necessary consequence of broad general currents, and that this has been the agency by which the mass of diluvium covering the surface of Lancashire has been carried there does not admit, in the author's opinion, of the smallest doubt. He accounts for the existence of currents diverging from the centre of the district in question by a repetition of paroxysmal elevations.

Suppose a certain area at the bottom of an ocean to be *suddenly* elevated; and, for the greater clearness, suppose the elevated area to be a circle of twenty miles in diameter, its elevation to be 50 feet, and the depth of the ocean 300 or 400 feet. If the elevation were sufficiently gradual no sensible *wave* would result from it, but if it were *sudden* the surface of the water above the uplifted area would be elevated very nearly as much as the area itself, and a diverging wave would be the consequence. Its front would be steep, like that of the tidal wave in some rivers called the *bore*, so that the highest part or summit of the wave would not be far from its front. The height at its summit would be approximately equal to the elevation of the uplifted area, or, in the case supposed, nearly 50 feet. The velocity with which the front would diverge would depend on the height of the wave and the depth of the ocean. In a certain time the water first raised above the general level of the ocean immediately over the elevated area would run off, leaving the surface of the ocean there at its original level; and when this should be completed the posterior boundary of the wave would be immediately over the periphery of the elevated area. During the same time the front of the wave would move on through a certain space, still forming a circle of which the centre would be immediately over that of the elevated area. Thus the whole wave would at the instant now referred to be comprised between two concentric circles, the distance between which would be the breadth of the wave. Afterwards, as the front or anterior boundary of the wave spread outwards, so would the posterior boundary move forward in a similar manner, always preserving the annular form just mentioned. As the wave diverged its height would gradually diminish till it became finally insensible.

The motion of the wave here spoken of is altogether distinct from the motion of translation of the aqueous particles. This latter motion, however, accompanies the former in the kind of wave here described, producing a *current* like that of a tidal river opposite to the usual course of the stream. Each particle begins to move onward the instant when the anterior boundary of the wave has reached it, but its motion being always slower than that of the wave, it will afterwards be overtaken by the posterior boundary of the wave, which will then leave the fluid particle behind and at rest. Thus, at any proposed point, the *current* will begin when the front of the wave reaches that point, will increase there till the highest part of the wave is directly over it, and will then gradually decrease till the posterior boundary of the wave has reached the point in question, where the current will then cease altogether. There will be no *reflexion* of this great solitary wave unless it meet with some obstruction in the course of its motion.

We are indebted to Mr. Russel for our knowledge of the properties of these great waves of translation. He has further ascertained, experimentally, that the *velocity of the wave* is equal to that which would be acquired in vacuum by a stone falling under the action of gravity through a height equal to half the depth of the ocean measured from the crest of the wave. He has also found that the *velocity of the current* at any point is independent of the *depth* of that point, being the same at the bottom as at the surface*. From these data it is easy to calculate the velocity of the current which attends the wave, when the depth of the ocean and original height of the wave are known. And hence it appears that there is no difficulty in accounting for a current of twenty-five or thirty

---

* Mr. Russel's experiments were made with much smaller waves and at much smaller depths than those above spoken of; but he expresses a conviction (and, as the author conceives, a well-founded conviction) that the same results will hold for much greater depths than those experimented with.

miles an hour, if we allow of *paroxysmal elevations*[*] of from 100 to 200 feet. This velocity will decrease as the wave expands, unless the current be constrained to pass through a comparatively narrow channel, like that which must have been formed by the pass Stainmoor when just submerged beneath the surface of the ocean. In such case the velocity of the current might be much increased.

With respect to the magnitude of the blocks which might be moved by a current of given velocity, the author remarks, that the facility with which the transport of a block may be effected depends principally on its form. The more it approximates to perfect sphericity, the less, *cæteris paribus*, will be the force necessary to remove it. The author conceives that there is no doubt whatever but that blocks, not more spherical than many rolled blocks are observed to be, of five tons weight and upwards, might be moved under favourable circumstances, by a current of ten miles an hour. That the force of a current increases in the ratio of the square of its velocity has been distinctly established by experiment for all velocities up to eleven or twelve miles an hour; nor does there appear to be any reason for doubting that the same law holds for much greater velocities. Assuming this law, the author states it as the result of a simple calculation, that if a certain current be just able to move a block of given weight and *form*, another current of double the velocity of the former would move a block of a *similar form*, whose weight should be that of the former in the ratio of $2^6 : 1$, *i.e.* of 64 to 1. If the velocity of the second current were treble that of the first, the weights of the two similar blocks would be in the ratio $3^6 : 1$, *i. e.* of 729 to 1, and so on for other velocities. Hence, if a current of ten miles an hour would move a block of five tons, a current of twenty miles an hour might, under similar circumstances, move one of 320 tons. No transported blocks approximating to this weight appear to have been moved from the Cumbrian mountains. The author, therefore, does not hesitate to affirm the entire adequacy of the cause now explained to transport all the erratic blocks which have been identified as being derived from that region, nor can he therefore hesitate to conclude that such has been the agency by which that transport has actually been effected.

It has been urged that no current could carry boulders up the escarpment of the Eastern Wolds of Yorkshire, nor does the author contend for any such effect of currents. Whether the blocks now found on the wolds were transported there by currents or by floating ice, the transport must have taken place before that region emerged from the ocean. But the author contends that the formation of such an escarpment as that referred to, or like the oolitic

---

[*] If the extent of country elevated be considerable (like that of the district of the Lakes, for instance) the elevation might occupy several minutes and still produce the great wave above described. If the elevation were produced more slowly, the height of the wave, and consequently the velocity of the current, would be proportionably less.

escarpment which overlooks the valley of the Severn, could not possibly be formed by oceanic currents, except under very peculiar conditions, which we have no reason to believe to have existed in those localities. On the contrary, the formation of such escarpments during the gradual emergence of the land would be a necessary consequence of that emergence under conditions which must have obtained in numerous instances. Hence the author concludes that the escarpment of the wolds was formed subsequently to the transport of the blocks which are now found in that region. He conceives that, with respect to the theory of transport by currents, difficulties founded on existing inequalities of surface have been far too strongly contended for on the one hand, and too easily admitted on the other.

The author is anxious that his views should not be misunderstood as respects the glacial theory, or that which would refer the transport of blocks to floating ice. He is quite prepared to believe in the possible extension of glaciers beyond the boundaries to which they now extend, wherever such greater extension can be accounted for consistently with the conclusions of collateral branches of physical science; and also to believe that such more extensive glaciers, where they have existed, may have been the means of transport of erratic blocks, provided sufficient mechanical cause can be assigned for their movement. With respect to the iceberg theory, though he rejects its application to the case investigated in this communication as altogether unnecessary to account for the observed phænomena, he conceives that floating ice may probably have been the most efficient agent in transporting the larger blocks of colder regions from their original localities.

LXXXIII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 401.]

March 11, 1842. THE following communications were read:—

1. On an Instrument adapted for observing Right Ascensions and Declinations of Stars independently of time, accompanied by Drawings made with the Camera Lucida by Captain Basil Hall, R.N. By M. Wettinger. Communicated, with a Letter of Description, to Sir J. F. W. Herschel, Bart., by Capt. Basil Hall, R.N.

The instrument contrived by M. Wettinger is so fully described in Captain Hall's letter, that an independent abstract of M. Wettinger's paper is unnecessary. The following is a copy of the letter, dated Malta, Dec. 6, 1841:—

"My dear Sir John,—I have had my attention lately called to an invention which appears to me so ingenious, and grounded upon such good principles, that I think a description of it may interest you, and perhaps be considered by you as worthy of being brought to the notice of the Astronomical Society. Of this, however, you are the best judge; and I shall therefore merely give you the means of examining the pretensions of the instrument. In this view I have made three sketches of the model with the camera lucida, and I have added to each the same letters of reference to the same parts. I transmit to you also the opinion of Carlini of Milan, and of his colleagues, as to this instrument, which was submitted to their examination some time ago.

"I may begin by stating that the chief object of the instrument is to determine the difference of right ascension between any two stars, without the agency of time as an element, the equatoreal angular difference between them being measured directly, *in arc*, on an hour-circle, graduated in degrees and minutes for that purpose. It is true that *time* does enter as an element into the principle of the instrument, inasmuch as a certain part of the machinery is moved by clock-work, in the manner used in many equatoreals; but this agency is purely mechanical and subsidiary, and does not require that the absolute time should either be exactly known, or its march be exactly kept.

"The instrument is essentially an equatoreal arc, in its structure,—that is to say, its principal axis is directed to the pole, and it carries a telescope capable of being directed to any star which is above the horizon. [I should mention, in passing, that the clock-work machinery is not included in the model; and there may be observed some other mechanical omissions, it not having been thought worth while to encumber either the model or the description with more details than are necessary to an explanation of the principle and workings of the instrument.]

"The principal or polar axis of the instrument is made hollow—in fact, is a telescope, having at its upper extremity a small reflector or speculum capable of being directed at any angle into the tube of this axis telescope. The object-glass of this telescope is fixed not at its extremity, but about half-way between the upper end and the centre. At the centre there is placed another reflector, which stands at an angle of 45° with the length of the tube, to receive the image of a star formed by the object-glass from the rays reflected from the upper speculum. The side of the axis telescope is perforated, in order to allow the image of the star which is reflected from the central speculum to pass into the middle of another telescope, which, for distinction, may be called the *declination telescope*, as it is attached to, and carries with it, a declination circle. In the middle of this declination telescope there is fitted a very small reflector, at an angle of 45° to its length, on which the image of the star reflected from the central speculum is received and transmitted to the eye of the observer, in every position of the declination telescope.

"The further arrangements of the instrument will perhaps be more readily understood by describing the manner of using it, than by giving a detailed explanation of the parts.

"In commencing an observation, the upper speculum is directed to a standard star of the first or second magnitude, partly by moving it on its own axis of rotation, so as to direct the rays into the principal axis telescope, and partly by the equatoreal motion, either of the whole apparatus, or by the rotatory movement of the principal axis, by means of the declination telescope. This motion, I may mention by the way, of the principal or polar axis may be made at pleasure, independently of a large frame-work attached to the instrument, which is moved by the clock-work, There is an hour-circle in the plane of the meridian, fixed to this outer frame-work, and another circle fixed to the lower extremity of the polar axis, which may be clamped or freed from that which belongs to the frame-work. The speculum, at the other extremity of the axis, is so contrived that it moves along with the frame-work.

"It will therefore be understood, that if the upper speculum be so directed towards a star that the rays reflected from it pass down the polar axis telescope, they will be received and reflected, first, from the central speculum, and secondly, from the speculum in the declination telescope, to the eye, in whatever position the declination telescope may be. Now, if the hour-circles be clamped, so as to form one, and the frame-work be put into gear with the clock-work, the whole will move round at the rate observed by the heavens, and, consequently, the image of the star reflected from the upper speculum will continue in the centre of the field of the declination telescope, for any required length of time, and in every possible position of that telescope.

"Suppose, now, that the relative position of the equatoreal circle, fixed to the frame, and that carried by the polar axis, be carefully ascertained by reading off their graduated circumferences, by microscopes or otherwise, and that then the circle carried by the polar axis be unclamped, that axis will be left free to revolve and to carry with it the declination circle, and likewise the declination telescope, but without interfering with what may be called the *celestial movement* of the frame, or that of the upper speculum, which, by going along with, continues to reflect the rays from the star to which it was originally directed; and, consequently, to preserve the image of that star constantly in the centre of the field of the declination telescope. This declination telescope is now directed to any other given star whatsoever, the image of which, viewed directly, is to be brought into coincidence with that seen by reflection from the upper speculum. If now the equatoreal circles be clamped, and a second set of readings be made, it is obvious that the difference between the two will be the difference, in arc, of the right ascensions of the stars.

"When the observation commences, the declination telescope is directed to the standard star, as well as the upper speculum, so that the images, seen direct and by reflection, are made to coincide in the centre of the field of view of the declination telescope. The graduations of the declination circle are then read off, to be compared with those when the second observation is made, or that of the star whose place is to be determined. The difference of these readings will give the difference of the declinations of the two stars, in the same manner that the difference of the readings of the two concentric hour-circles (as they may be called) at the lower end of the polar axis, gives the difference of the right ascensions.

" If clock-work machinery be not in such perfect adjustment as to keep the standard star, first observed, correctly in the centre of the field of view, it may be brought to that point by a tangential movement of the frame-work, to be made by hand, at the moment of making the second observation, without in any respect vitiating the integrity of the observation, for this small movement does no more than compensate for any error in the going of the clock.

" As it may not be always convenient to move the whole frame which is attached to the clock-work, the upper speculum, at the upper end of the axis, is so fitted as to be capable of being turned round independently of the frame, to which it is fixed by a moderately stiff collar. This movement, which may be made roughly by hand, or more nicely by a tangent screw, enables the observer, without stopping the clock-work machinery, to direct the speculum to any given standard star; and I may observe that only those of the first and second magnitudes are named for this purpose by M. Wettinger, as he fears the light lost by the three successive reflections might render any less brilliant stars invisible. This, however, does not affect stars viewed through the declination telescope, which looks directly to its object, and is supposed to be capable of seeing small stars as readily as large ones.

" Observations for determining the differences of right ascension and declination, in arc, between a standard star and any other, both being above the horizon, may be repeated as often as required; and it does not appear how, supposing the machinery perfect, any error can enter into these determinations, except what arises from the false position in which refraction places celestial objects. In the determination of right ascensions by an instrument placed in the meridian, this source of error is avoided; but it remains in full force as to declinations. The question with respect to right ascensions, therefore, resolves itself chiefly (if I rightly understand M. Wettinger) into the fact of its being both easier and more exact to determine the difference of right ascension in arc, by a leisurely and direct observation of the angle formed by the two meridians in which the stars lie, than to *infer* that difference of arc by the uncertain agency of a clock, which is further vitiated, he thinks, by the uncertainty of marking the exact moments when the stars respectively pass the wires of the meridian instrument. To these sources of error he adds that of the ear in appreciating the beats of the clock.

" M. Wettinger is of opinion, that, although only experience can determine the degree of accuracy with which such an instrument could give the desired results, very fair estimates may be formed by practical astronomers familiar with the difficulties and errors of the existing methods, of the probable advantages of his invention. Whether, for example, the effects of refraction on stars above a certain altitude, on their right ascensions and declinations, are not sufficiently well known to admit of such exact corrections being applied to the determinations made by his instrument, as would render their results more worthy of confidence than those made with the existing instruments. It being taken into account, also, that, while only one observation can be made in the day on all stars which are not circumpolar, and only two on some of those which never set, with an instrument fixed in the meridian, the number of observations which may be made with M. Wettinger's instrument is unlimited; and as these observations might be made at all altitudes from that when the stars passed the meridian to the moment of their rising or setting, many curious inferences might possibly be deduced from it on the subject of refraction, while the observations might be so arranged as to counteract the vitiating effects of refraction, and, by the combination, to give correct results.

"It would seem that this instrument would be very useful in determining the place of a comet by direct observation, instead of inferring it, as is usual, even with an equatoreal instrument. For this purpose any standard or other star sufficiently brilliant to bear the triple reflection may be used.

"It will be observed in Signor Carlini's report, that, a doubt having been expressed as to the possibility of applying the principle of this instrument to the sun, M. Wettinger, in order to try the experiment, fixed the small reflector or speculum of his model to the great equatoreal at Milan, in such a way that, while Sirius was observed *directly* by the telescope, the image of the sun, duly darkened and submitted to one reflexion, was observed in the same apparent direction; and both, as he informs me, with such perfect precision, that the star could be seen on the disc of the sun, or be brought in contact with the limb with the utmost certainty.

"It will be observed that Signor Carlini and his colleagues, in their report, advert to the multiplicity of parts and variety of movements in M. Wettinger's instrument, as contrasted with the fixed nature and simple operations of the large meridian instruments now in use. But still they appear to be disposed to look with a favourable eye to the capabilities of M. Wettinger's invention, and they seem anxious that one of sufficient dimensions should be made; but for this, in their opinion, there are no means in Italy, and they recommend Munich or Vienna. Why not London?

"M. Wettinger is of opinion that prisms of glass might probably be substituted with advantage in place of the reflectors.

"As I may probably have omitted some material points in this explanation, I have requested M. Wettinger to draw up a description of it in Italian, the only language which he speaks; and I have asked him to employ the same letters of reference which I have used, so that the same sketches may do for both.

"I ought to add, that M. Wettinger is one of the professors of the university established here, and that he has long been highly esteemed for his knowledge and ability, and he is a person well acquainted both with the principles and the practice of astronomy.

"Should you wish it, or should you think it would prove interesting to the Astronomical Society, to see the model which M. Wettinger has constructed, I have no doubt he would readily allow it to be sent to England; or should you wish any further information respecting it, you will do him a favour by writing to him at Malta. I shall not be here above a month longer, as I go on to Egypt with my family in January; but M. Wettinger being fixed to this spot, will always be available.—I remain, &c.

"BASIL HALL."

II. A Letter from Professor Henderson to the Secretary, dated Edinburgh, January 31, 1842, on the Determination of the Parallax of α Centauri, by recent Observations made by Mr. Maclear at the Cape of Good Hope[*].

"My dear Sir,—Within these few days I have received from Mr. Maclear a series of observations of α^1 and α^2 Centauri, made with a view to ascertain the parallax; and I find that they confirm the existence of a parallax amounting to about one second. The observations are of the double altitudes of the stars made with the mural circles, and they extend from April 16, 1839, to August 12, 1840. Twenty-six observations of the double altitude of each star were made with the old circle between April 16 and June 16, 1839; and 108 observations of the double altitude of α^1, and 112 of α^2, were made with the new circle between August 4, 1839, and August 12, 1840. In each observation the star was observed both by direct vision and by reflexion at the same transit. The results which I have obtained are as follow:—

"From the 272 observations made with both circles,

Parallax = 0″·91. Weight 147·93 observations.
Coefficient of Aberration = 20·55. ... 142·47 ...

"From the 220 observations made with the new circle,

Parallax = 0″·92. Weight 138·81 observations.
Coefficient of Aberration = 20·53. ... 127·97 ...

"The observations made with the old circle extend over too short a period to warrant any results beng deduced from them alone for parallax and aberration which could be relied upon.

"On computing the observations of each star separately, I find for α^1,—

"From the 134 observations made with both circles,

Parallax = 0″·86. Weight 70·37 observations.
Coefficient of Aberration = 20·61. ... 70·02 ...

"From the 108 observations made with the new circle,

Parallax = 0″·91. Weight 65·83 observations.
Coefficient of Aberration = 20·54. ... 63·71 ...

"And for α^2,—

---

[*] Former observations on this subject are noticed in Phil. Mag. S. 3. vol. xvi. p. 148; vol. xviii. p. 599.—EDIT.

"From the 138 observations made with both circles,

Parallax = 0″·96. Weight 77·55 observations.
Coefficient of Aberration = 20·48. ... 72·44 ...

"From the 112 observations made with the new circle,

Parallax = 0″·93. Weight 72·99 observations.
Coefficient of Aberration = 20·52. ... 66·27 ..

"If the coefficient of aberration be assumed = 20″·36, as in the Astronomical Society's Catalogue, then, from all the observations with both circles, parallax = 0″·98, the separate results for the two stars being 0″·95 and 1″·00; and, from all the observations with the new circle, parallax = 0″·99, the separate results being 0″·98 and 0″·99.

"I believe that the observations are still continued to be made at the Cape; and I will write to Mr. Maclear immediately, requesting him to send the additional observations.

"The two stars appear to be approaching each other, the difference of declination being in 1826 = 18″, in 1833 = 15″, and in 1840 = 11″. When all the observations are collected, an attempt may be made to determine the orbits, and thence the masses of the stars.

"I will as early as possible prepare a detailed memoir on the subject, and transmit it to the Admiralty for presentation to the Astronomical Society.—I am, &c. "T. HENDERSON."

III. Positions of 78 Fixed Stars contained in the A. S. C., represented by Mr. Baily as not determined with sufficient accuracy, deduced from Observations made with the Meridian Circle of the Observatory of Kremsmünster. By M. Köller, Director of the Observatory.

IV. Observations of Falling Stars made at Hereford on the night of Nov. 12, 1841. By Henry Lawson, Esq.

Three observers were employed in watching for these phænomena, from seven o'clock in the evening till half-past four o'clock of the following morning, each taking a distinct portion of the heavens. The whole number observed was 79, and the greatest number observed in any one hour was 20, between the hours of three and four in the morning. The result the author considers to be so far satisfactory, that it tends to confirm the fact of the appearance, at about this period, of a greater number of meteors than usual.

V. A List of Falling Stars observed Nov. 12, 1841, at St. Helena. By J. H. Lefroy, Esq., R.A., Director of the Magnetic Observatory at Longwood.

The whole number observed was 102, between the hours of eight in the evening and five of the following morning. The Greenwich mean solar time of the appearance of each is noted to the nearest second, and the place of its appearance as referred to the bright stars nearest it. The direction of the motion of each is also given, with remarks on its appearance, rapidity, and other circumstances connected with the phænomenon.

VI. Path of the Moon's Shadow over the Southern part of France, the North of Italy, and part of Germany, during the Total Eclipse of the Sun on July 7, 1842 (July 8, Civil Time). By Lieutenant W. S. Stratford, R.N. This paper will be found, entire, at p. 346 of the preceding volume.

VII. A letter from Professor Hansen, dated March 1, 1842, in acknowledgement of the communication of the Foreign Secretary, announcing the award of the Society's Gold Medal at the last Annual General Meeting.

"Sir,—I have just now received your letter, by which you announce to me that the Royal Astronomical Society have honoured me with their Gold Medal. I recognise in it a valuable sign of the kind attention of this Society towards me and my labours; and I beg you to present to them my sincerest thanks.

"Pray have the goodness to allow the medal to be sent to M. Prætorius, Secretary and Librarian of His Royal Highness Prince Albert, who will undertake to send it to me.

"Of late my labours in the lunar theory have been considerably advanced. The calculation of the perturbations is finished, and I am now engaged on the calculation of provisional tables for the purpose of comparing my results with observations, and of determining the correction of the elliptic elements which result from them. I am now giving to these tables the necessary extension, that they may afterwards serve as definite tables, after having applied to them the necessary corrections which are required by the new determination of the elliptic elements. To combine with exactness in these tables the most convenient mode of calculating the places of the moon, I have chosen the form that M. Carlini has given to the tables of the sun, as much as it is possible to do so. However, the labour of calculation of the tables themselves is much increased by this arrangement.

"Repeating my request that you will present my respects to the Royal Astronomical Society, I beg you will accept the sentiment of high consideration with which I have the honour to be, &c.

"P. A. Hansen."

LONDON ELECTRICAL SOCIETY.

[Continued from p. 405.]

Nov. 15, 1842.—A note from Mr. Weekes was read, accompanied by specimens of *Acarus galvanicus*, developed in solution of ferrocyanuret of potash.

The following notices were communicated by W. G. Lettsom, Esq., M.E.S.:—1st. Of a new and important application of galvanism, by which Jacobi succeeds in extracting gold and silver from their respective ores. 2nd. Of the employment of electro-magnetism for the movement of machinery, in which it is stated that M. Wagner, to whom the German Diet promised 100,000 florins if his plan really succeeded, now reports that he has surmounted all difficulties. 3rd. On M. Peclet's new condenser, an instrument calculated to test the most minute amount of electric tension. It consists of an electroscope surmounted by a disc A of glass coated with gold and varnished on its upper surface; the disc B, varnished on both sides, is placed on this; it has a glass handle; the disc C has a handle of glass tube so constructed that the handle of B can pass through it. The delicacy of this instrument was shown by the results which followed the touching of the upper disc with an iron wire once, twice, thrice, four, five, and ten times.

A paper by J. P. Gassiot, Esq., F.R.S. M.E.S. &c., was then read, "On the Polarity of the Voltaic Battery." After alluding to the confused descriptions of voltaic batteries which have emanated from the varied modes of arranging the elements, Mr. Gassiot mentions that the electric tension of the water battery has been described as differing from that of other batteries; the end we have been accustomed to regard as positive is designated resinous, and the other vitreous; and this result presented itself to him in his early experiments; upon closer investigation, however, it appears that these conflicting results are due to want of attention to the mode of manipulating with the electroscope. When the excited rod is applied to the *side* of this instrument, the leaves are affected in a manner precisely the reverse of what happens when it is applied *above*. The anomalous results occur in the former case, and are due to the effect of the glass rod on the instrument itself, disturbing not only the natural electricity contained in the leaves, but also the surplus acquired by being in contact with, or charged by, the battery. The charge is driven upwards into the plate, and the leaves approach the normal condition. When the rod is applied above the converse occurs. These experiments were made with a new double electroscope. In conclusion, the author offers a few remarks on electrical nomenclature, and conceives that so long as we are content to continue the terms positive and negative, vitreous and resinous, in application to the machine, we should not object to use them in reference to the battery.

Mr. Walker then concluded the reading of his translation of M. Becquerel's paper " On the Electro-Chemical properties of Gold," in which are given some interesting applications of theory to practice. In extracting ore from a solution of several metals, another solution is made of all the metals but that one; it is made as nearly as possible of the same specific gravity; the two form the exciting liquids to a single cell arrangement, and the effect is the release of the metal required. Modifications of the same principle are applied to gilding, the author giving the preference to the single cell apparatus.

Mr. Weekes's Electro-Meteorological Register for October was read.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

Nov. 14, 1842.—Professor Fisher read a paper on the Development of the Spinal or Intervertebral Ganglia, and on various Malformations of the Nervous System. This communication was one of several which Professor Fisher intends to bring forward, the general object of which may be thus expressed:—

Researches on certain forms of disease, considered in their connexion with the process of formation, the growth and maintenance, and the decline of the human frame.

The tendency which the human œconomy has to accomplish the scheme of its organic existence, is the vital law by which the author has been directed in these researches. Deriving his method from an idea of Galen, Professor Fisher distinguishes in an organ two processes, the plastic and the functional. Under the first he comprises the formation, the growth, and maintenance of an organ, as well as the alterations of structure, normal or anormal, which it may present. Under the second, those acts of an organ by which it effects results which have reference to the œconomy.

The physiological portion of Professor Fisher's communication consisted of an account of some embryological researches he had made on the development of the spinal ganglia, in order to throw light on the anomalous conditions which some of them present in *Spina bifida,* when that disease is limited to the lower region of the spinal column[*]. Before stating the result of these researches, it may not be inappropriate to mention, that those anomalous conditions consist in a coalescence of the last lumbar with the first sacral ganglion, or in a coalescence of some of the sacral ganglia with each other[†]. In some instances a comparatively strong band is found to pass from the fourth to the fifth lumbar ganglion[‡].

Finding no mention made of the development of the spinal ganglia by the physiologists whose works Professor Fisher consulted, he was induced to make researches on the subject, of which the following statement comprises the general results:—

That the white, rounded or pyriform bodies which are situated on the side of the furrow which occupies the site of the future spinal cord of the embryon constitutes the rudiments of the spinal or intervertebral ganglia[§].

---

[*] In every case of *Spina bifida* which the author has met with affecting the upper part of the spinal column, it was accompanied by a defective formation of the head.

[†] The author has not met with any instance in which this coalescence did not exist. He has now examined sixteen cases. In one case the subject presented a club-foot, on the same side as that on which the two first sacral ganglia were united. He could not recognise any trace of the anterior roots on the united ganglia, but unfortunately the thigh was so lacerated as not to enable him to ascertain with any degree of security whether any part of the nervous or muscular system of the limb was deficient or not. In the same case the fourth sacral ganglia on each side were united into one mass, which was supplied by a single artery. In this case, as indeed in all others which Professor Fisher has observed, the lumbar and sacral nerves presented, as they emerged from their respective foramina, a natural appearance. The sacral plexus always seemed to be duly formed.

[‡] The author at first thought this band might be a vessel, but careful dissection convinced him that it was continuous with the sheath of the ganglia with which it was connected. Its internal structure presented a granular appearance.

[§] Professor Fisher, at the commencement of these researches, was im-

That whilst the edges of the furrow are closing, a white line having a filamentous appearance arises between it and each ganglion, the connexion of which with the central parts corresponds with the swellings which give to those parts a sinuous appearance.

That another white line arises between the ganglia, and connects them together, so as to cause them to offer collectively an arrangement somewhat analogous to that which the ganglia of some invertebrate animals present.

That another line appears to proceed from each ganglion externally, and to join one which runs parallel with the axis of the body and communicates with the cardiac ganglion.

Resuming the pathological part of his subject, Professor Fisher gave the following statement of the views he entertained on the subject of *Spina bifida*, when that disease is situated in the lumbo-sacral region*:—

That the coalescence already described of the ganglia constitutes the primary irregularity to which all the others that the disease presents may be directly or indirectly referred.

That this coalescence is favoured by the position those ganglia occupy, and by their volume, the comparative greatness of which may be due to their connexion with the sacral plexus †.

That the roots of the nerves appertaining to the united ganglia, by virtue of their passing through the dura mater in one bundle, become so irregularly connected with the pia mater of the cord, as to give rise to adhesions between that membrane and the arachnoid, and between the latter and the dura mater.

That this disordered condition of the pia mater has for its consequence the anomalous position of the cord (which always adheres to the inner surface of the posterior wall of the tumour), and even in some instances a deficient development of that organ.

That the beginning of the bifid state of the osseous canal corresponds above to the point where the cord becomes attached to the posterior wall of the tumour ‡.

---

pressed with the feeling, that since his results differed from those of other embryologists, he might be mistaken about the nature of these bodies. He finds, however, that they are confirmed in part by the observations of the late Professor Rolando, and therefore he has felt more confidence in communicating them. But whether the observations he has made, or the conclusions he has drawn from them, be correct or not, the development of the spinal or intervertebral ganglia ought not to be lost sight of.

* Although these views coincide with those the author communicated on a previous occasion, and which were recorded in the London and Edinburgh Philosophical Magazine (vol. x. p. 316), still it may not be considered inappropriate if they be presented again, in association with the additional matter he has brought forward.

† The spinal ganglia are, at least about the middle of fœtal life, richly supplied with blood-vessels, which may also assist, along with the hypertrophy of the ganglia, in favouring their coalescence.

‡ In all cases of *Spina bifida*, the defective formation of the osseous canal corresponds with that of the cord; where the latter assumes its natural conformation, the canal becomes complete.

That the branches of the lumbar and sacral vertebræ are not absent in the region affected, but are more or less everted by the presence of the tumour.

The researches which Professor Fisher has made on the defective formation of the spinal cord have led him to adopt the following general view regarding the plastic process of that organ:—

That although the spinal cord possesses, like every other organ, a plastic process peculiar to itself, yet that process may be so influenced by the anomalous condition of some of the roots of the spinal nerves as to lead to a partial malformation, or even to a partial deficiency of the organ *.

The following are the therapeutical inferences that Professor Fisher has drawn from his investigations of the disease in question:—

That as the fluid which the tumour contains is a natural product †, and destined by its pressure to protect the parts with which it is in relation, the removal of it, either by a natural or artificial opening, is to be avoided; for an opening is not only liable to occasion inflammation of the lining membrane of the tumour, by the introduction of air and by other causes, but also to allow the continual escape of the fluid, so as to lead to death, either by exhaustion or by depriving the blood of its serous fluid; for according to an observation recorded by Morgagni, and one made by Professor Fisher himself, the suppression of urinary secretion coincided with the constant discharge of the fluid.

That if, in puncturing the tumour, the operation be performed in the upper and middle part of it, the spinal cord will almost necessarily be wounded.

That if the skin covering the tumour be in a natural state, then an equable pressure, in the application of which regard must be had to the situation of the spinal cord, may be used with advantage; but if the walls of the tumour be thin and membranous, then astringent lotions, tending to corrugate them, may be applied; in this case, however, the disease generally proves fatal.

LXXXIV. *Intelligence and Miscellaneous Articles.*

USE OF SULPHATE OF AMMONIA IN AGRICULTURE‡.

FOR the full development of the capacity of the soil, and to afford a greater amount of nitrogen than what is af-

---

* The author has applied the idea involved in this view to the consideration of Anencephalus, and it is his intention to communicate, on another occasion, the results of his observations on that subject, and on the defective formation of the upper part of the spinal column.

† The fluid is secreted by the pia mater, but its quantity is probably increased by the veins, which often appear unusually distended, a condition that may be owing to the want of resistance in the containing parts. As regards the author's views on the subject of this secretion, see Phil. Mag. S. 3. vol. x. p. 316.

‡ Communicated by the Engineer of the Chartered Gas Company.

forded either by the ordinary manure, or the ammonia &c. of the atmosphere, sulphate of ammonia has been introduced, and found to be a most valuable auxiliary, as a top dressing, to the farmer.

It has been found to impart a greater degree of fructification to grass, wheat, and other grain, than any other dressing yet discovered, and at a less cost by 50 per cent.

The mode of application as adopted by Mr. C. Hall of Havering-atte-Bower, Essex, is as follows:—

Having selected several fields of grass, peas, turneps, and wheat, he had sown broad cast on parts of these fields quantities at the cost of 5*s.* 3*d.*, 11*s.* 4*d.* and 21*s.* per acre; the sulphate having cost him 17*s.* per cwt.

The produce was kept and threshed separately, when the increase from the wheat land was found to be as follows:—

The part that was sown at the rate of 5*s.* 3*d.* per acre gave an increase of 3 bushels; 11*s.* 4*d.* gave 6 bushels, and 21*s.* upwards of 9 bushels, besides a considerable increase of straw.

CHLORIDE OF GOLD AS A TEST OF CERTAIN VEGETABLE ALKALIES.

MM. Larocque and Thibierge find, that perchloride of gold is a more decisive test of certain vegetable alkalies, than the double chloride of sodium and gold already employed for this purpose. The following are the colours of the precipitates which it produces with the salts of the annexed alkalies dissolved in water:—Quina, buff-coloured: Cinchonia, sulphur-yellow: Morphia, yellow, then bluish, and lastly violet; in this last state the gold is reduced, and the precipitate is insoluble in water, alcohol, the caustic alkalies, and sulphuric, nitric or hydrochloric acids; it forms with aqua regia a solution which is precipitated by protosulphate of iron: Brucia, milk-, coffee-, and then chocolate-brown: Strychnia, canary-yellow: Veratria, slightly greenish yellow.

All these precipitates, with the exception mentioned, are very soluble in alcohol, insoluble in æther, and slightly soluble in water. These precipitates appear to be combinations of gold, chlorine and the vegetable alkali, for their alcoholic solutions treated with tannin give a greenish blue precipitate of reduced gold; if the solution be filtered, and the alcohol be evaporated by heat, a precipitate of tannate of the alkali employed is formed. The liquor again filtered, gives with nitrate of silver a white precipitate insoluble in nitric acid, but soluble in ammonia.

Among the reactions of chloride of gold, there are two which to the authors appear to be especially important, they are those which occur with morphia and brucia; they are sufficiently marked to prevent these alkalies from being mistaken for each other, and also yield pretty good characteristics for distinguishing brucia from strychnia.

MM. Larocque and Thibierge detail also various experiments on the modes of detecting opium proposed by Dr. Christison, and they mention that their results differ much from his. They state that these differences may arise from three causes,—1st, the inequality of the composition of the opium of commerce; 2ndly, the analytical process employed by Dr. Christison, which consisted in decomposing the meconate of lead by sulphuretted hydrogen—this the authors show frequently masks the meconic acid, and that it could only be detected by decomposing the meconate of lead with dilute sulphuric acid; 3rdly, the variable nature of the liquids with which opium is mixed.

The authors have also, as the results of their experiments, arrived at the following conclusions:—

1st. By the aid of reagents it is possible to determine the presence of morphia, strychnia and brucia in substances, which, after being mixed with the salts of these alkalies, have undergone the vinous, acetic or putrefactive fermentation. M. Orfila has already shown that the putrefactive fermentation does not alter morphia.

2ndly. Crystallized iodic acid, or a concentrated solution of this acid, is susceptible of being decomposed by neutral azotized bodies; but a dilute solution of this acid cannot be decomposed by them unless there be added concentrated sulphuric acid, crystallizable acetic acid, oxalic, citric or tartaric acid.

3rdly. Iodic acid should not be employed as a test of morphia without the greatest caution.

4thly. Perchloride of gold produces such effects with the vegetable alkalies, as serve to distinguish morphia, brucia and strychnia from each other.

5thly. The reagents on which the greatest reliance may be placed as tests of morphia are, nitric acid, neutral perchloride of iron, and perchloride of gold.

6thly. By the use of reagents, morphia which has been mixed with beer, soup or milk may be detected.

7thly. It is also easy to prove by reagents the presence of meconic acid in soup or milk, especially when the meconate of lead is decomposed by dilute sulphuric acid.—*Journal de Chimie Médicale*, Octobre 1842.

NON-DECOMPOSITION OF VEGETABLE ALKALIES BY EXPOSURE TO FERMENTING BODIES.

It appeared to MM. Larocque and Thibierge a subject of some interest to determine by experiment, whether the vegetable alkalies suffered decomposition when in contact with fermenting substances. It had, indeed, been proved by Orfila and Lesueur that acetate of morphia suffered no change discoverable by reagents, under these circumstances; and M. Merck detected strychnia, morphia and brucia after they had been exposed to fermenting animal and vegetable matters during twenty days.

The following experiments were made by MM. Larocque and Thibierge:—

To 3080 grains of blood there were added 5·14 grains of [sulphate of?] brucia; this mixture was exposed to the air from the 2nd of June to the 3rd of August; at this period the blood was putrefying and fetid. It was evaporated to dryness; the residue was treated with boiling alcohol; the solution obtained was filtered and evaporated to dryness, and treated with water acidulated with acetic acid. The solution thus procured was filtered and evaporated to a syrupy consistence. In this state it was reddened by nitric acid, and become of a violet tint by the successive application of nitric acid and protochloride of tin.

Mixtures of the following substances were made on the 2nd of June:—7700 grains of distilled water, 154 grains of yeast, and 462 grains of sugar. To four such mixtures were separately added 5·14 grains of sulphate of brucia, 5·14 grains of sulphate of strychnia, and 5·14 grains of acetate of morphia. These mixtures soon began to ferment, and when after standing several days the evolution of carbonic acid had ceased, they were evaporated to dryness, then treated with boiling alcohol, and after evaporating the spirit, the residue was treated with water acidulated with acetic acid, and in this liquor, evaporated to a syrupy consistence, the characteristics of the alkali introduced before fermentation were determinable.

Some red wine holding hydrochlorate of morphia in solution had been kept in a bottle loosely corked from July 1841 to the 15th of June 1842; the liquid exhaled a strong odour of acetic acid; after treating in the manner above described, and decolorized by animal charcoal, it did not yield crystals, but by evaporation to a syrupy consistence it gave a residue which was reddened by nitric acid, rendered blue by perchloride of iron, was precipitated by tannin, and reduced the chloride of gold.—*Ibid.*

PREPARATION AND COMPOSITION OF PEPSIN.

In order to prepare pepsin in quantity, M. Vogel, jun. employed the following process:—The glandular skin of the fresh stomach of the hog was separated from the serous part, and after having cut it into small pieces it was treated with cold distilled water; after twenty-four hours' immersion, the water was poured off and fresh portions added. This operation was repeated during several days, until a putrid odour was perceptible. The aqueous infusion thus obtained was precipitated by acetate of lead, the white flocculent precipitate formed containing the pepsin mixed with much albumen; this precipitate being diffused through water, it was decomposed by hydrosulphuric acid gas. When the liquor is filtered, the solution contains pepsin and sulphuric acid, while coagulated albumen and sulphuret of lead remain on the filter. A very small quantity of hydrochloric acid, added to the solution of pepsin and acetic acid, is sufficient to render it capable of artificial digestion.

In order to procure solid pepsin, the filtered liquor must be evaporated to a syrupy consistence, carefully avoiding ebullition, and afterwards adding absolute alcohol to it. After some time a whitish bulky precipitate is formed, which is to be dried by exposure to the air; and it is then a yellowish viscid mass of a peculiar animal odour and a disagreeable taste. Pepsin thus obtained has an acid reaction, because it always contains a small quantity of acetic acid, to deprive it of which various processes were tried, and that which succeeded was heating it in a salt-water bath for some hours, by which a white powder soluble in water and possessing no acid reaction was obtained. It is to be remembered that pepsin loses some of its power of assisting digestion by the action of a high temperature, but as it is not at the same time altered in its chemical constitution, M. Vogel employed it for analysis; the mean of several experiments gave

Hydrogen	5·666
Carbon	57·718
Oxygen..........	16·064
Azote	21·088
	100·536

M. Vogel remarks, that the results of this analysis show that pepsin is not identical with modified albumen, as has been supposed; he further states that the action of pepsin in digestion may be compared to that of disastase, which changes fecula into grape sugar, without itself undergoing any alteration; this opinion was supported by the fact, that of two grains of pepsin which had acted upon dressed beef so as completely to dissolve it, 1·98 grain was recovered.

The pepsin of the sheep possessed only in a slight degree the power of favouring digestion.—*Journ. de Pharm. et de Chim.*, Oct. 1842.

ACTION OF CHLORIDES ON SOME MERCURIAL COMPOUNDS. BY M. MIALHE *.

A solution of 60 parts of common salt, and 60 of sal-ammoniac, is termed by M. Mialhe the *assay liquor*: in this 60 parts of various mercurial compounds were digested, during twenty-four hours, at the temperature of the air, and in the heat of a stove; the former varying from 59° to 68° Fahr., and the latter from 104° to 122° Fahr.

I. *Protobromide of Mercury.*—The alkaline chlorides behave with this salt as with calomel, with this difference only, that out of the contact of the air the small proportion of the bisalt of mercury which is formed is, at least momentarily, bibromide and not bichloride of mercury; whereas, while reacting in the presence of air, the greatest proportion of the mercurial bisalt formed is bichloride.

1*st Experiment.*—At the temperature of the air, corrosive sublimate produced 0·6 part.

2*nd Experiment.*—By the heat of a stove, corrosive sublimate produced 1·5 part.

II. *Protiodide of Mercury.*—This is one of the mercurial salts in which the solution of alkaline chlorides acts with the least intensity.

1*st Experiment.*—At the temperature of the air, corrosive sublimate produced 0·5 part.

---

* M. Mialhe's researches on the action of chlorides upon protochloride of mercury will be found at p. 320 of the present volume.

2*nd Experiment.*—By the heat of a stove, corrosive sublimate produced 0·6 part.

III. *Binoxide of Mercury.*—This substance is scarcely at all soluble in water, and yet it produces a considerable proportion of corrosive sublimate with the alkaline chlorides.

1*st Experiment.*—At the temperature of the air, corrosive sublimate produced 4·7 parts.

2*nd Experiment.*—By the heat of a stove, corrosive sublimate produced 15·4 parts.

The quantity of bichloride of mercury obtained by this last reaction is certainly very considerable, and nevertheless it was nearly the same with a much smaller quantity of the binoxide, the greater part of which remained unacted upon.

The reaction which takes place between the binoxide of mercury and the alkaline chlorides is certainly remarkable; it is however very easily explained. The oxide of mercury behaves with the alkaline chlorides exactly in the same manner as the oxides of lead and silver, that is to say, by simple substitution between the chlorine and the oxygen there are produced bichloride of mercury and an alkaline oxide. It is at first more difficult to account for the non-decomposition of the corrosive sublimate by the alkali produced. M. Mialhe accounts for this by the unquestionable affinity existing between bichloride of mercury and the alkaline chlorides. It is at any rate certain that magnesia, which decomposes sublimate readily, has no action upon it when combined with excess of an alkaline chloride.

IV. *Black Oxide of Mercury.*—The experiments of M. Guibourt have proved that this compound is not a true protoxide, but a mixture in definite proportions of binoxide of mercury and metallic mercury. Nevertheless its reactions with the alkaline chlorides more nearly resemble those which are produced with the compounds containing protoxide of mercury than the peroxide. This fact, however, cannot be considered as singular, it being well known that black oxide of mercury yields salts with most acids which really contain the protoxide of the metal.

1*st Experiment.*—At the temperature of the air, sublimate produced 1·1 part.

2*nd Experiment.*—By the heat of the stove, sublimate produced 1·9 part.

V. *Protosalts of Mercury.*—The action of the alkaline chlorides upon these is always the same; protochloride of mercury is at first formed, which acts, as has been already described, when alkaline chlorides are present.

The following protosalts, employed in the quantities already stated with the assay liquor, gave the annexed proportions of sublimate:—

	Temperature of the Air.	Stove Heat.
Protonitrate	0·4 part.	1·3 part.
Protosulphate	0·7 ...	1·4 ...
Protoacetate	0·8 ...	1·1 ...
Prototartrate	0·4 ...	0·8 ...

VI. *Bisalts of Mercury.*—All the salts of binoxide of mercury, when in contact with the alkaline chlorides, immediately yield corrosive sublimate and a new alkaline salt by double decomposition; but as this reaction is not always perceptible, the following experiments, among others, were performed to prove it.

Biniodide of Mercury.—Is chlorine under certain circumstances capable of separating iodine from its combination with mercury? Do the alkaline chlorides, when reacting on the biniodide of mercury, produce corrosive sublimate?

1*st Experiment.*—At the temperature of the air, mercurial salt dissolved 11 parts.

2*nd Experiment.*—By the heat of the stove, mercurial salt dissolved 19·3 parts.

When it is recollected how slightly biniodide of mercury is soluble in water, and considering the enormous quantity of bisalt here shown to exist in the solution of the alkaline chlorides, it is difficult not to suppose that a portion at least of the mercurial salt is in the state of corrosive sublimate.

VII. *Bicyanide of Mercury* is decomposed by the alkaline chlorides, and converted into corrosive sublimate; it is, however, worthy of remark, that potash, soda, hydrosulphuric acid, free or combined, a plate of copper and Smithson's pile, are almost the only reagents which discover the presence of mercury in solutions of alkaline chlorides.

It is always easy to prove that the mercury exists in them in the state of bichloride; it is sufficient for this purpose to evaporate the solution, and to treat the saline residue with alcohol: this solvent takes up a salt which is not bicyanide, but bichloride of mercury.

VIII. *Pernitrate of Mercury.*—Like all the other salts of binoxide of mercury, this is converted into sublimate by the alkaline chlorides; this is proved incontestably by the fact, that no trace of subnitrate of mercury is obtained by pouring pernitrate of mercury into a boiling solution of chloride of sodium, which would inevitably occur if this curious reaction did not take place. Moreover, when the mixed solution was treated with pure sulphuric æther, it exhibited the reactions of chlorine and the bisalts of mercury. Then as pernitrate of mercury is instantly decomposed by æther, it follows that the reactions mentioned certainly belong to corrosive sublimate.

IX. *Turbith Mineral.*—This salt is powerfully attacked by the alkaline chlorides, as will appear by the following experiments.

1*st Experiment.*—At the temperature of the air, sublimate produced 11·2 parts.

2*nd Experiment.*—By the heat of the stove, sublimate produced 22·8 parts.

X. *Pertartrate of Mercury.*—This salt is much more soluble in water than the prototartrate, and yet the proportion of sublimate which it produces in solutions of the alkaline chlorides is truly surprising. This reaction affords one of the best examples which can be cited in favour of the difference which exists between the modes of action of the alkaline chlorides with the two classes of mercurial salts.

1*st Experiment.*—At the temperature of the air, sublimate produced 31·2 parts.

2*nd Experiment.*—By the heat of the stove, sublimate produced 36·2 parts.—*Ann. de Chim. et de Physique,* Juin 1842.

ON A NEW MODE OF FORMING AMMONIA. BY M. REIZET.

The researches of M. Kuhlmann have shown that under the influence of spongy platina, nitric oxide mixed with excess of hydrogen produced ammonia. On repeating the experiments of M. Kuhlmann, M. Reizet substituted several metallic oxides for spongy platina. The results which he obtained are very interesting, and throw great light on the obscure cause of catalytic action*. M. Reizet states, that with an apparatus, consisting of two bottles, each of the capacity of about 60 cubic inches, for evolving hydrogen gas and nitric oxide, and 145 grains of sesquioxide of iron, heated in one end of an analysis tube, he obtained sufficient ammonia to completely saturate 360 grains of commercial hydrochloric acid.—*Ibid.*

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1842.

Chiswick.—October 1. Clear and fine. 2. Foggy: fine. 3. Foggy: overcast. 4. Very fine. 5. Cloudless and very fine. 6—8. Cloudy and fine. 9. Light haze: cloudy. 10. Overcast. 11. Foggy: clear and very fine. 12. Cloudy. 13. Overcast. 14. Hazy. 15. Overcast. 16. Light haze: very fine. 17. Hazy: overcast and fine. 18. Very fine: heavy rain at night. 19. Fine. 20. Clear and frosty: fine: frosty at night. 21. Sharp frost: fine: frosty. 22. Densely overcast: heavy rain. 23. Rain: heavy showers. 24. Boisterous: clear and fine at night. 25. Rain: stormy at night. 26. Very clear. 27, 28. Cloudy and fine. 29. Frosty: cloudy and fine: clear and frosty at night. 30. Frosty: clear and fine. 31. Overcast: clear at night.—Mean temperature of the month 5°·94 below the average.

Boston.—Oct. 1. Cloudy: rain early A.M. 2. Cloudy. 3. Cloudy: rain A.M. 4. Cloudy. 5—8. Fine. 9, 10. Cloudy. 11. Fine. 12, 13. Cloudy. 14. Fine. 15—17. Cloudy. 18. Cloudy: rain P.M. 19. Stormy. 20, 21. Fine. 22. Stormy: rain A.M. 23. Cloudy. 24. Windy: rain A.M. 25. Cloudy: rain P.M. 26—31. Fine.

Sandwick Manse, Orkney.—Oct. 1. Showers: cloudy. 2. Showers. 3. Cloudy. 4. Showers. 5. Clear: cloudy. 6. Showers: rain. 7. Damp: cloudy. 8—13. Cloudy. 14, 15. Drizzle: cloudy. 16. Cloudy. 17. Cloudy: showers. 18. Rain: sleet. 19. Hail-showers: sleet. 20. Snow: hail. 21. Sleet-showers: cloudy. 22. Rain. 23. Showers. 24. Snow: aurora. 25. Rain: aurora. 26. Rain: showers. 27—29. Showers. 30, 31. Damp.

Applegarth Manse, Dumfries-shire.—Oct. 1—3. Fair and fine. 4. Frost: fair and clear. 5—8. Fair and fine. 9, 10. Fair and fine, but cloudy. 11. Fair and fine: clear. 12. Fair and fine. 13. Fair and fine: frost A.M. 14. Fair and fine, but cloudy. 15. Fair and fine. 16. Fair and fine: cloudy. 17. Cloudy, but fair. 18. Shower. 19. Shower of snow. 20, 21. Fair and clear. 22, 23. Heavy showers all day. 24. Fair and clear. 25. Heavy fall of snow. 26. Snow A.M.: melting P.M. 27. Fair and clear. 28. Fair and clear: snow gone. 29, 30. Fair and clear: frost. 31. Fair and clear: no frost.

Sun shone out 28 days. Rain fell 4 days. Frost 4 days. Snow 3 days.

Wind North 3 days. North-east 1 day. East-south-east 1 day. South-east 2 days. South-south-east 1 day. South 1 day. South-west 4 days. West-south-west 4 days. West 3 days. West-north-west 6 days. North-west 3 days. North-north-west 2 days.

Calm 12 days. Moderate 5 days. Brisk 9 days. Strong breeze 4 days. Boisterous 1 day.

Mean temperature of the month .... 44°·45
Mean temperature of October 1841 .... 45 ·75
Mean temperature of spring-water .... 49 ·60

---

* On this subject see Professor Grove's paper in the present Number.

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

Days of Month.	Barometer.								Thermometer.											Wind.					Rain.					Dew-point.
1842. Oct.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mm.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	30·268	30·275	30·200	29·76	30·25	30·23	30·31	30·28	50·7	56·5	46·8	59	31	50	65	38	52	50½	nw.	ne.	calm	ne.	nw.	......	......	·19	......	·07	48
2.	30·336	30·300	30·149	29·82	30·20	30·12	30·16	30·22	46·2	57·6	42·8	60	32	50	59	44	51	43½	nnw.	ne.	calm	wnw.	w&n.	......	......	......	......	......	46
3.	30·106	30·089	30·010	29·50	30·10	30·09	30·27	30·22	50·2	56·7	45·8	57	38	50	54	41	44	44	nnw.	ne.	calm	wnw.	nw.	......	......	......	......	......	47
● 4.	30·096	30·123	30·052	29·57	30·11	30·10	30·09	30·08	50·3	59·4	45·2	57	29	48·5	54½	30	51	49	nnw.	ne.	calm	nw.	wnw.	......	......	·04	......	·10	46
5.	30·236	30·238	30·190	29·70	30·12	30·19	30·12	30·21	43·3	56·0	42·3	61	31	41	59	35½	52	51½	nnw.	n.	w.	nnw.	nw.	......	......	......	......	·03	43
6.	30·334	30·288	30·250	29·77	30·23	30·20	30·20	30·06	46·0	48·3	42·7	60	34	50	56½	41½	50	53	nw.	ne.	calm	nw.	wnw.	......	......	......	......	·06	42
7.	30·314	30·298	30·258	29·74	30·18	30·25	30·16	30·36	51·4	59·3	46·8	61	39	49	58	48	52½	52	w.	ne.	calm	nnw.	nw.	......	......	......	0·00	·17	47
8.	30·416	30·436	30·348	29·83	30·39	30·40	30·43	30·46	53·9	60·7	51·0	58	39	52	58	32	52	52	n.	ne.	calm	se.	wnw.	......	......	......	......	......	50
9.	30·540	30·490	30·447	30·00	30·41	30·36	30·45	30·34	51·3	50·2	48·5	59	49	53	55½	39	51	51	n.	ne.	calm	se.	sw&s.	......	......	......	......	......	48
10.	30·510	30·451	30·386	29·94	30·30	30·25	30·15	30·01	53·7	58·2	51·0	64	34	51·5	55½	48	54½	54½	n.	ne.	calm	wsw.	sw.	......	......	......	......	·02	48
☽ 11.	30·410	30·363	30·282	29·82	30·26	30·28	30·24	30·38	47·8	56·3	46·3	64	44	47	63	49	52	51	nnw.	ne.	calm	wsw.	wnw.	......	......	......	......	·02	46
12.	30·332	30·286	30·244	29·74	30·30	30·24	30·40	30·31	52·4	59·6	48·3	54	47	50	58½	34½	51	51½	nnw.	ne.	calm	sse.	wnw.	......	......	......	......	......	49
13.	30·302	30·258	30·220	29·80	30·19	30·12	30·20	30·07	51·7	58·3	49·2	55	37	50	56	29½	51½	52½	n.	ne.	calm	s.	w.	......	......	......	......	·06	48
14.	30·252	30·208	30·204	29·67	30·08	30·09	29·99	29·99	48·8	56·3	45·9	57	42	50	56½	41½	54	54	nnw.	w.	calm	wsw.	w.	......	......	......	......	·07	47
15.	30·312	30·263	30·233	29·73	30·13	30·15	30·04	30·16	51·0	54·7	49·5	61	48	51	57	48	54	53	w.	se.	calm	wnw.	w.	......	......	......	0·00	·11	49
16.	30·288	30·235	30·171	29·77	30·18	30·14	30·21	30·13	51·7	55·8	51·3	55	48	51	53	46	54	53	w.	ne.	calm	wnw.	w.	......	......	......	......	·05	48
17.	30·070	30·045	29·664	29·56	29·90	29·51	29·73	29·40	51·7	57·6	51·0	56	45	51	53	46	53	45½	nnw.	e.	calm	sw.	sw&w	......	......	......	......	......	51
18.	29·482	29·465	29·202	28·94	29·24	29·20	29·15	29·40	52·0	54·2	49·6	57	35	49	54	38	42	39	s.	sw.	w.	sw.	n.	......	.40	......	......	·69	48
○ 19.	29·280	29·518	29·250	28·84	29·30	29·46	29·40	29·43	42·3	57·8	40·8	59	25	41	46	33	35	37½	w.	nw.	nw.	n.	n.	·488	·02	·18	......	·50	42
20.	29·672	29·739	29·647	29·22	29·50	29·65	29·47	29·71	37·3	48·2	36·6	50	22	36	47	20½	33	39	w.	w.	calm	n.	n.	......	......	......	......	·35	38
21.	29·912	29·980	29·883	29·47	29·85	29·80	29·88	29·66	38·0	47·0	34·3	50	20	34	47	24	36½	41½	nw.	nw.	w.	w.	n&sw.	......	......	......	......	·24	33
22.	29·700	29·726	29·025	29·29	29·05	28·70	28·88	28·85	40·0	45·7	32·7	48	38	37	49	33	43	45	s.	sw.	w.	w.	se & e.	......	·50	......	0·45	·58	36
23.	28·792	29·956	28·802	28·50	28·75	28·92	29·08	29·30	44·7	48·8	39·7	52	30	40	50	39	43	42	s.	sw.	calm	w.	ne.	·444	·07	·17	......	·55	40
24.	29·194	29·546	29·171	28·80	29·23	29·33	29·31	29·20	42·3	50·6	37·5	46	27	43	46	27½	40	38	w.	sw.	w.	nw.	n&nw	......	......	......	......	·17	39
25.	29·634	29·623	29·333	29·25	29·34	29·06	29·25	29·33	41·3	46·7	35·7	47	27	36·5	45	30	35	29½	sse.	sw.	calm	ese.	nw.	·013	·72	·21	......	·15	37
26.	29·590	29·625	29·572	29·12	28·96	29·08	28·90	29·13	37·8	49·0	34·0	45	36	34	48	36	35	39	s.	sw.	w.	sw.	e&ne.	·344	......	·09	......	·52	38
☾ 27.	29·700	29·691	29·677	29·23	29·28	29·40	29·27	29·54	42·4	45·3	38·3	52	33	43	46	38	42	44	s.	w.	calm	wnw.	n.	......	......	......	......	·73	38
28.	29·552	29·942	29·743	29·17	29·50	29·57	29·66	29·78	40·5	51·0	40·0	53	24	41·5	48	36	42½	42	w.	w.	calm	n.	n.	......	......	......	......	·20	39
29.	29·806	29·807	29·770	29·40	29·78	29·82	29·84	29·75	37·3	50·6	35·2	51	23	40	47½	32	40	42½	ssw.	sw.	calm	wnw.	calm	......	......	......	......	·06	36
30.	30·116	30·158	30·101	29·72	29·79	29·92	29·65	29·79	37·4	49·0	34·8	51	39	37	52	30½	49	47½	sw.	sw.	calm	wsw.	w.	......	......	......	0·60	·18	36
31.	30·268	30·234	30·223	29·75	30·04	30·05	29·87	29·86	47·7	49·7	37·6	52	33	47·5	51½	44½	50	51	s.	sw.	calm	sw.	w.	......	......	......	......	......	40
Mean.	29·994	30·053	29·893	29·49	29·836	29·697	29·830	29·851	46·2	53·4	42·9	55·19	34·80	45·2	53·2	37·2	46·62	46·41						Sum. 1·289	1·71	0·88	1·05	5·68	Mean. 43

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.
SUPPLEMENT TO VOL. XXI. THIRD SERIES.

LXXXV. *On the Currents produced by the Actuation or Induction of instantaneous Electric Currents. By* Professor Stefano Marianini*.

Currents produced by the Leyden-electrical Induction.

I. THE facility with which, by means of the instruments described in the preceding memoir†, the presence of an instantaneous electric current in a metallic wire is detected, encouraged me to seek indications also of the currents which are derived from actuation or induction.

I had surrounded a little cylinder of iron with two copper wires covered with silk, which formed two parallel coils, and having placed the cylinder upon the cap of a needle, I made use of this apparatus as a re-electrometer, the delicacy of which varied as I made the currents pass through one only of the said coils, or through both at the same time, or joined them so that the current had to pass through one first, and then through the other; with these two parallel coils I made my first attempt on the Leyden-electrical induction. Having however taken that re-electrometric cylinder from the cap of the needle, I connected one of the wires which surrounded it with the ends of the coil of a re-electrometer, and the Leyden jar being discharged upon the other wire, connecting its extremities with the two coatings, I have seen the needle of the instrument deviate a similar number of degrees.

---

* Translated from *Memorie di Fisica Sperimentale scritte dal Professore Stefano Marianini dopo il* 1836. *Anno Primo*, 1837. Modena, 1838. It was from the *Anno Secundo*, 1838, of this work that the memoir by the author inserted in Phil. Mag. S. 3. vol. xviii. p. 193 was translated. Prof. Henry's researches on the induced currents of common electricity, to which the memoir now given relates, will be found in Phil. Mag. S. 3. vol. xvi. p. 551. The experiments of these philosophers were we believe quite independent, and must have been nearly contemporaneous; but the priority is probably due to the Italian.—Edit.

[† The preceding memoir here referred to relates to an instrument for measuring the force both of instantaneous and non-instantaneous electric currents.—Edit.]

I suspected that the deviations obtained in this and other similar experiments might proceed, not from induction occasioned immediately by the instantaneous current of the jar, but from magneto-electric induction, produced by the temporary magnetization of the iron cylinder surrounded by the two coils. However, the cylinder being taken away, and a small glass rod substituted, the same phænomenon took place.

II. As I made use of a jar highly charged and the wires presented some metallic points uncovered, it was probable that part of the current might make itself a passage from one wire to the other, and that the effects observed might proceed from a real current transferred into the wire of the re-electrometer, and not from a movement of the electricity of the wire itself excited by the current which was made to pass near it.

A coil of large uncovered copper wire was enveloped in four layers of silk; I then inclosed it in sixty coils of fine copper wire covered with silk; with this apparatus I made many trials, and I observed,—1st, that with the small jar (having little more than half a square decimeter of coating) the needle always deviated from the part, whence it also deviated when I discharged the jar itself, and in the same manner upon the wire of the re-electrometer, as much as when I discharged it upon the coil of uncovered copper wire (keeping the fine wire in communication with that of the re-electrometer), as when I put the coil of uncovered copper wire in communication with this, and discharged the jar upon the fine wire; 2nd, that with the large jars (nineteen square decimeters of coating), if they were highly charged, the needle deviated in the same manner as when they were discharged upon the re-electrometric wire; but if they were slightly charged the deviation was different.

III. I suspected that the copper coil might exhibit induction in proportion as it was itself magnetized, and that thence the electricity might act instantaneously, as a magnet does when introduced into a coil [or helix]. I wished then to prove whether the copper might be magnetized by the discharged electricities.

Having covered a copper cylinder with silk I inclosed it in a coil, as I was accustomed to do with the iron cylinders. Having then placed it upon a magnetized needle, I caused some slight discharges of Leyden jars to pass through the coil itself, both weak and strong; but I never had the smallest indication of magnetism in the copper.

IV. Having taken the said copper cylinder from the needle, I attached to its extremities two metallic wires, and these I connected with the ends of the wire of a re-electrometer; then discharging the Leyden jar by means of the coil which surrounded the cylinder, the re-electrometric deviations were no longer wanting.

I suspended the connexion between the little copper cylinder and the wire of the instrument, and having connected it instead with the ends of the coil which surrounded it, and then discharged the Leyden jars upon the cylinder itself, the usual deviations no longer failed. Are not these phænomena dependent upon Leyden-electrical actuation? As I invariably saw that, when I made the discharges pass through two points of the coil itself, there were very nearly the same deviations; and in the other case (namely, when the copper cylinder was connected with the wire of the re-electrometer) there was exactly the same effect when I discharged the jar connecting it with the two ends of the cylinder, I doubted whether, instead of inductions, I might not hitherto have seen only the effects of a division and subdivision of the discharge.

V. It appeared to me that I had taken the most scrupulous precautions that there might be no metallic contact between the actuating wire and that to be actuated; but might it not be that even through the silk the electricity might make itself a passage? The doubt was so much the more reasonable, as from the experiments related in the preceding memoir I had learned that the currents of the Leyden jars might divide themselves between good and bad conductors; I sought therefore to clear up this doubt by the following experiments.

I put the ends of the wire itself in metallic communication by means of a band of lead two centimeters broad and eight decimeters long; I covered one part with a small piece of very dry wool; then upon this wool and above the band itself I extended for the space of a decimeter, another band of the same metal, which rose at a right angle from both parts, for the space of four centimeters. Having many times discharged the Leyden jar, now in one manner, now in the other, putting the coatings in contact with the extremities of the second band, deviations of two, three, and even six degrees in the needle of the re-electrometer were always obtained.

I repeated the same experiments after having put four small pieces of wool between the band of lead adjoining the ends of the re-electrometric coil and that upon which the jars were discharged, and the results were pretty nearly the same.

The deviations were somewhat smaller (though they never failed) when, besides the said pieces of wool, I also placed between the two metallic bands a large cake of sealing-wax having a strongly insulating power, and six good millimeters in size. And as in these experiments I made use of a small jar highly charged, and the spark passed thence to a great distance, so to remove my doubt that some part of the discharge might fall upon the neighbouring bodies instead of the extremity of the metallic band upon which I intended the spark to pass, I attached two long copper wires to the extremities of the said band, and I went to some meters distance to discharge the jar; but the effects upon the re-electrometer were not different from those already observed.

If these experiments do not serve to show that the current of the Leyden jar passing through a metal causes in a neighbouring metal an electric current by induction, they would yet prove a very singular and unexpected property of the discharge of the Leyden jar; that I mean of dispersing itself in part in the worst conductors, even after having begun to traverse the best, since it must be said that the electric fluid descending by the vertical band scarcely reaches the point where this touches the wool or the sealing-wax, when a part of it quickly passes through the wool itself, and finding the band of lead underneath, a fraction of this part of the current passes into the band itself which is under the wool, and reaches the external coating of the jar by the shortest way; whilst the other fraction makes a much greater turn to traverse the coil around the re-electrometric iron, and at length reaching the wool from the opposite part, and there passing through the confining stratum, rejoins the other part of the larger band communicating with the external coating of the jar. That such a dispersion and division of discharge does not take place, seems proved by the experiments which I shall now describe.

VI. I made the experiments of the preceding section, but instead of laying the plate upon which the jar was discharged upon the confining portion for the space of two decimeters, I caused it to touch it only at a few points, bending it upwards at both sides; and I observed that if that part of the plate parallel to the under one was distant from the latter only a few centimeters, the effects upon the re-electrometer were still visible; but when the distance was two decimeters, there were no longer any deviations in the re-electrometer.

Having again laid the said plate upon the confining stratum, I cut in two the leaden band which communicated with the ends of the re-electrometer, and I placed near, between them, the sections, without their touching; and this was done, because, if in discharging the Leyden jar a part of the current were carried upon this plate, crossing the confining part, all this portion must pass through the coil of the re-electrometer, and thence produce much greater deviations than when the band offered a continued conductor. But, on the contrary, the effects in this case were nothing; consequently, in these trials, either the suspected dispersion does not take place, or if it does, the movements of the magnetized needle do not proceed from it.

VII. Instead of placing the plate, which we will call the *actuating* plate, parallel to, and above the other which closes the re-electrometric circuit, and which we will call the *actuated* one, I placed it parallel to it laterally, and at the horizontal distance of two centimeters. The currents produced by Leyden-electrical induction were somewhat weaker; but nevertheless sufficiently great to be seen, and measured.

VIII. Having replaced the actuating plate* above the other, and turned it somewhat in the horizontal plane round its central point, so that it might form an angle with the under one, the induced current was weaker, and still more so when the angle was greater. If the angle was of 60 degrees, the deviation caused in the instrument by the induced current was scarcely perceptible. And these experiments also prove, that the deviations do not proceed from a current transferred by dispersion, but rather from true Leyden-electrical induction.

IX. Having joined the ends of the re-electrometric coil by means of a fine copper wire, silvered, and then covered as usual with the little pieces of wool, and with the sealing-wax upon which was the usual plate of lead or copper, or zinc, upon which the jar was discharged, the induction took place as usual. It took place equally when the ends of the coil were connected with one of the said plates, and the jar was discharged upon the metallic wire duly placed upon the cake of sealing-wax.

X. I placed upon the said cake two insulating supports of glass, covered with sealing-wax, five centimeters high, and upon them the usual metallic plate, duly placed under one part of the re-electrometric wire. I then discharged the Leyden jar from one end to the other of the plate itself, and I had the deviation by actuation which at that distance I usually obtained.

Having inclosed in many folds of silk ribbin all but the extremities of the actuating plate upon which I wished to discharge the jar, and which I held in my hand parallel to the actuated plate, and carried to the height of six, ten, and even fifteen centimeters, the signs of electric induction never failed.

XI. The position and direction of the jar when discharging

---

* For the sake of brevity, I call a band or plate *actuating* through which the electricity passes, or ought to pass, its extremities being connected with the coatings of a Leyden jar or of a Franklinian square [see note, p. 509.]; and I call a plate *actuated*, in which (being near the former) arises, or may arise, an induced electric induction.

it, never produced any difference in the results; certainly the effects were less when the distance between the two points of the plate upon which the jar was discharged was less: and if such distance was sufficiently small, as for example, one centimeter, there was no indication of an induced current, although the actuating plate might only be separated from the actuated by a very fine little portion of silk or wool.

XII. A band of lead, one meter long and two centimeters broad, was for the space of six decimeters inclosed in silk ribbin, leaving two decimeters of it bare at each extremity. I applied over this, but only for the space of five centimeters in the middle, a longer band of the same metal, and in order that they might be in better apposition to each other, I surrounded them both with silk thread in an open coil for all the said space. The uncovered band was then folded back, half a decimeter of the covered band remaining on each side to prevent the danger of any metallic contact between the two bands. I connected the extremities of the uncovered band with the metallic wire of a re-electrometer, and then discharged the Leyden jars, putting the two coatings in contact with the uncovered extremities of the band covered with silk ribbin, as above mentioned. During these experiments the currents produced by the Leyden-electrical induction caused much greater deviations in the magnetized needle than those which occurred in the experiments hitherto described, in which I caused the actuating current to act upon a part of the actuated band which was not longer than two decimeters.

The uncovered extremities of the leaden band partially covered with silk, having been connected with the wire of the re-electrometer, and the Leyden jar being discharged upon the extremities of the other band, the effects, in similar circumstances, were perfectly equal.

Again, with the weakest discharges and even with the first two or three residual charges, manifest indications of induction followed on experimenting with similar bands of lead, placed adjacent, in the manner I have mentioned.

XIII. Seeing that I could obtain induced currents by means of the weakest actuating currents, also by means of currents which when made to act directly upon the re-electrometer produced deviations less than those which followed from the actuated currents produced by strong discharges of Leyden jars, I thought it would not be time lost to attempt to obtain the inductions of the said induced currents.

A band covered with silk for the space of seven decimeters, with the extremities bare, had attached to all that part of it covered with silk, with the exception of two centimeters on each side, a second uncovered band; this remained on one side free for one decimeter; and on the other for nine, the two ends being connected together. Seven decimeters of this second band (in the space not attached to the first) were covered with silk, and to all this part, except two decimeters, was attached a third uncovered band, having also a space of one decimeter free on one side, and nine decimeters free on the other. Things being thus disposed, the ends of the first band were connected with those of the coil of the re-electrometer, and the ends of the third band were destined to be connected with the two coatings of the Leyden jar.

The trial being made, the deviations of the needle of the re-electrometer never failed. The usual small Leyden jar being charged as highly as possible, that is to about 80 degrees of the quadrant electrometer, there was a deviation of almost seven degrees. When charged to twenty degrees, there was a deviation of one degree and a half; and also with weak charges, and on many occasions with mere residual charges, I obtained visible deviations. Then the instantaneous current which passed into the third band of lead caused an induced current in the second, which formed as it were a large closed ring, and this current in its turn caused a second Leyden-electrical induction in the first band, which formed as it were a second large ring or closed circuit, being joined, as I have stated, to the re-electrometric coil.

I closed also the circuit formed by this third band, by connecting the ends of it, and covered a free space of seven decimeters with silk, and attached to it a fourth uncovered band in the manner now described; the Leyden jar being discharged upon this, there were unequivocal signs of an induction which we will call *of the third order*, and I thus obtained also the induced currents of the fourth and fifth orders, by adding another ring similar to those described, and then another still.

XIV. After the result here stated, there appeared to me no longer any doubt that the instantaneous currents of the confining armatures cause currents by true induction or actuation in the conductors near which they pass; and I addressed myself to study the properties of this action. I wished first to see whether the induction would take place even if there should be some metallic stratum as well as the confining strata between the actuating and actuated conductors.

Between two bands of lead, both covered with silk, I placed another, with care, in order that there might be no metallic contact between the bands. Having connected one of the covered bands with the wire of the re-electrometer, and discharged the Leyden jar upon the other similarly covered, the needle deviated, as when there was no naked band between the actuating and the actuated plates.

But if the two ends of the metallic band between the actuating and the actuated plates were connected, then the Leyden jar being discharged as above, there was no deviation in the re-electrometer, or it was very small, and only took place when the jar was considerably charged; which appears to me to prove, that when the actuating current operates upon a closed metallic circuit, it induces in it a contrary current, which either wholly or in part destroys the effect of the actuating current upon the second actuated band.

I connected the ends of the middle band with the re-electrometer, and now connected the ends of the third band with them, now employed them unconnected. In the first case, the jar being discharged upon the first band, the deviations were somewhat small, and in the second evidently greater. There was not so much difference in the two cases as in the preceding experiment, in spite of the small interval between the actuating band and the immediately actuated one.

XV. Two silvered copper wires were placed parallel to each other, each by means of two pegs of wood covered with sealing-wax, at one meter distant from each other, and moveable, so as to allow the distance between the wires to be varied, while they still remained parallel. Having connected one of these with the ends of the wire of a re-electrometer, and discharged the Leyden jar upon the other, I saw that the induction visibly took place, even when the distance between the actuating and actuated wires was seven centimeters.

I have already observed in § XI., that when the actuation only took place on a small space of the actuated conductor, the effect was less. Now, applying the ends of the re-electrometric wire to two points more or less distant from each other, I have observed that the signs of an induced current began to appear when the extent of wire subject to actuation was about a centimeter and a half, and the distance from the actuating wire two millimeters. When the actuated wire was three centimeters long a degree of deviation was obtained, and this increased to ten degrees when the extent of actuated wire was six or seven decimeters. It did not increase on the wire being considerably lengthened, which I tried to the extent of a meter. I have observed the same on varying the length of the actuating wire.

XVI. I connected the ends of a perfect Nobili's multiplier with those of the actuated metallic wire, and discharged the

Leyden jar upon the actuating wire under the most favourable circumstances for obtaining the induced current; but the galvanometer did not make the smallest movement; nor was the result different when experimenting with the bands of lead instead of the copper wires; and this proves that such actuated currents are instantaneous, like the actuating.

XVII. On obliging the actuating current to pass through a metallic wire several hundred meters in length, the induced current was excited; nor did I perceive any different effect when it did not pass through that long wire. The same result also occurred when I forced the induced current to pass through the long wire before reaching the re-electrometer.

The induction did not fail either, when, instead of one end of the actuated wire being in metallic contact with one end of the coil of the re-electrometer, both were immersed in water, and distant from each other more than a decimeter; nor was the result different when the actuating current was made to pass through a stratum of water more or less thick.

The inductions of the second and third orders also, did not fail when made to pass through a long metallic wire or a stratum of water, although under such circumstances they appeared much weaker.

XVIII. Making use of a small iron cylinder surrounded by two parallel coils, similar to that mentioned in § I., I connected the two ends of one of them, and having discharged upon the other the small Leyden jar charged to fifteen degrees, no movement of the magnetized needle ensued; and the reason appears to me to be that the current induced in the circuit so formed was contrary, and nearly equal in magnetizing force to the immediate current.

I have also constantly observed, that the somewhat weaker currents of the first two or three residual charges were indicated by one or two degrees of deviation; and this, as it appears to me, arises from the weakest current not being sufficient to cause a sensible induced current in the wire circuit already mentioned.

Thus I have seen that a stronger charge, for example of forty or fifty degrees, was indicated by several degrees of deviation; never, however, so many as were observed when the other wire was unconnected: and this shows that when the wire was joined the immediate current prevailed over the induced.

XIX. The phænomena of induction of which I have hitherto spoken, presented no anomalies to me; but this was not the case with respect to the direction of the induced current. In most of the experiments on this subject I made use of three small Leyden jars not having more than a square decimeter of exterior coating, and with each of these I observed that the re-electrometric deviations indicated that the induced currents were directly contrary to the actuating. If, for example, the discharge of the jar in the direction of the band, or of the actuating wire parallel to the actuated, proceeded from right to left; in the band, or in the neighbouring actuated wire, the induced current passed from left to right: and, seeing in this an analogy to the volta-electric induction of Faraday*, I felt more and more persuaded that the phænomena observed proceeded really from Leyden-electrical induction. But I quickly began to doubt it when I applied myself to confirm, with the large jars, the results obtained with the small; for, on using these, the deviations of the instrument indicated that the induced current, and that which caused it, proceeded in the same direction in the two parallel and neighbouring conductors.

I doubted whether, from the quantity of electricity being different in proportion to the tension, a different distance might not be required between the actuating and actuated conductors, in order to produce the direct current in a given manner in the latter; and whether in such phænomena there might not be something analogous to the inversions of magnetization observed by Savary in steel needles, placed at different distances from the conductors through which he made the discharges of great electrical batteries to pass. But whatever was the distance between the actuating and actuated conductors, which I have varied from one millimeter to a hundred, the inductions of the smaller jars were always directly opposite to those of the larger.

I turned my attention to the construction of the little jars, and I observed that they had the internal coatings formed of cuttings of tin-foil and silvered paper; I conjectured that the difference of effects in the large and small jars might depend on that discontinuity of the coating; and I long held this opinion, from having observed that two large jars, having the external coatings formed of so many small squares of tin-foil, of about one centimeter square, attached to the glass, so that between them might be a band of bare glass, of two or three millimeters broad, acted in precisely the same manner as the said small jars. But finally, having had some small jars prepared with internal and external coatings adhering to the glass, as in the large ones, I saw that they produced the currents by induction in the same direction as the large and small jars with discontinuous coatings; whence I was convinced that

---

[* See Faraday's Experimental Researches in Electricity (26.), or Phil. Mag., Second Series, vol. xi. p. 300.—Edit.]

the cause of those different effects, although remote, depended on the different capacity of the jars; for the large jars, with discontinuous external coatings, may be considered as furnished with small electric capacities only, as many of these small squares remain idle in charging or discharging the jars themselves.

Considering, then, that the least capacity of the confining coatings involves as a consequence, that with similar charges, that is, furnished with an equal quantity of electricity, the spark must pass to a greater distance, and thence find a similar expenditure in the longer space of air which it must traverse, I wished to see, if by effecting a retardation in the discharges of the large jars, there might perhaps be the same direction of the induced current that was observed with jars of less power.

I therefore caused the discharges of the large jars to pass through the water in a glass before reaching the actuating wire; and I saw that in this case these jars acted as those furnished with much less capacity. It is singular to see how the same quantity of electricity, put in motion by the same jar, induces a current, either in one direction or in that precisely opposite, according as it is, or is not, made to pass through a liquid stratum.

The different velocity, then, with which the electricity discharges itself from the one coating to the other, seems to give rise to the said inversions of phænomena: and in this opinion I was confirmed by having many times observed, when experimenting with the bands of lead described in § XII., that a jar of great power (*capacità*) strongly charged and discharged upon the actuating band, caused the induction contrary to that of the little jars; and with the first residual charge there was no effect, with the second and third an opposite one.

XX. A glass tube of about two centimeters in diameter, and twenty in length, was filled with spring water, and closed with two corks, through the axis of each of which passed a brass wire so far as to touch the water; both these wires projected out for the space of some centimeters, and terminated in a little globe. Having duly dried the exterior of the tube, and surrounded it with a band of lead two centimeters wide, which was twisted round it three times in the middle part of the tube, the ends of the bands were put in metallic communication with the extremities of the re-electrometric wire. Having discharged the Leyden jar so that it must pass through the water of the tube, I brought the external coating into contact with the little globe of one of the said wires, and the internal* with the little globe of the other, and the needle deviated two degrees.

[* *Armatura externa* in the original, but obviously in error.—Edit.]

Saline water being substituted for the spring water, a deviation of five degrees was obtained.

In order the better to assure myself of the insulation, I twisted round the tube a band of lead covered with silk; I repeated the experiments several times, and the results were always such as to lead to a conclusion that it was not necessary that the electricity should pass through a metal to cause the Leyden-electrical induction; it being sufficient that it should pass through some conductor, in order that the passage might be accomplished with sufficient celerity.

XXI. I connected the liquid contained in the tube which was used in the experiments above described, with the ends of the re-electrometric wire, and I then discharged the Leyden jar upon the extremities of the band which surrounded the tube itself. The needle deviated almost a degree. I renewed the experiment; but on repeating the same discharge in the same direction six times, I found the needle deviated four degrees. Hence we see that it is not necessary that the actuated conductor should be metallic, in order that the Leyden-electrical induction should take place.

From the experiments of this and the preceding paragraph, it may be deduced, that the induction would take place if neither the actuating nor the actuated conductor were metallic; which, I believe, I have also verified by apposite experiments.

XXII. Having interrupted the actuating wire in another place, I connected one end of it with the external coating of a Leyden jar not charged, and the other with the internal*. I afterwards discharged a jar equal to that in power upon the wire itself. The induction took place, and the charge was divided between the two jars, which proves that it is not necessary that the identical fluid of one coating should pass to the other, to produce the phænomena of Leyden-electrical induction.

XXIII. Also the simple sparks drawn from the prime conductor may produce currents by actuation. Whilst one of the wires described in § XV. was connected with the re-electrometric coil, I let pass some sparks upon one extremity of the other, keeping the other extremity in communication with the ground; and I observed some movement in the magnetized needle at every spark that appeared.

Once, with fifteen sparks directed upon the actuating wire, a magnetization in the iron of the re-electrometer was obtained with the induced currents, which caused the needle to deviate three degrees.

[* *Armatura externa* in the original, but obviously in error.—Edit.]

But reserving to myself to treat, on another occasion, of the induced currents produced by sparks and by other electric currents, artificial or natural, it appears to me that we may in the mean time conclude,—

1st. That the instantaneous current of the Leyden jar, or of the Franklinian square* passing through a metallic current, causes an electric current, also instantaneous, in another metallic conductor, near to it, and forming a closed circuit,—a phænomenon which I call *Leyden-electrical induction*, because analogous to that called by Faraday *volta-electric induction*.

2nd. That the same induced current may cause in another conductor a second current of induction; and this second, again another, and so on; whence may be produced currents of Leyden-electrical induction, of the second and third orders, &c.

3rd. That the Leyden-electrical induction also takes place when the circuit through the metallic actuated conductor is closed by a very long metallic conductor, or even by a conductor not wholly metallic.

4th. That such induction also takes place when the discharge of the Leyden jar traverses a very long metallic conductor, and also a non-metallic conductor; nor does the phænomenon fail to appear, when it is not the identical fluid of one coating which passes to the other.

5th. That the induced current takes in the actuated conductor the same direction which the inducing current takes in the actuating conductor whenever the jar has great electric capacity, and is not too weakly charged. But the direction is opposite when the charge of the jar is rather weak, or when the electricity has to pass through a bad conductor, or when the jar is of small electric capacity.

6th. That the phænomena of induction may be seen, although neither the actuating nor the actuated conductor is metallic.

7th. That finally, such inductions are not exclusively from Leyden jars and the Franklinian square, but are obtained also with instantaneous electric currents from other sources.

[* *Quadro Frankliniano*; meaning, we presume, the pane of glass with tin-foil coating on both sides; but this, we believe, was the device, not of Franklin, but of Smeaton.—Edit.]

LXXXVI. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY*.

Extracts from the Report of the Council to the Twenty-second Annual General Meeting, held this day.

Feb. 11, 1842. AMONGST the losses by death, the Council have here to notice one among the Foreign members, which was announced at the last anniversary, namely, that of Professor Littrow of Vienna, who was one of the earliest members of this Society. He contributed several papers which were read at the meetings, and which have been printed in the first four volumes of the Memoirs, exhibiting a spirit of research and inquiry into a variety of subjects connected not only with astronomy, but also with other branches of physical science. He was also the author of a valuable Treatise on Astronomy, in three volumes octavo, in the German language, and continued, till the time of his death, to conduct the affairs of the Imperial Observatory at Vienna.

The Council have also to regret the loss of Mr. Frend, lately one of the Members of the Council of the Society.

William Frend was the son of George Frend, an alderman of Canterbury, in which city he was born, November 22, 1757. He received his education in his native place, at the King's School; and, after staying some time at St. Omer, was placed in a mercantile house at Quebec; but the breaking out of the disturbances in America destroyed his commercial prospects, and he returned to England. His wishes being directed towards the Church, he was placed at Christ's College, Cambridge, in 1775, and took the degree of B.A., with the honour of second wrangler, in 1780. After taking his degree, he almost immediately removed to Jesus College, of which he was elected fellow and tutor. In 1783 he was ordained, and afterwards obtained the living of Madingley, near Cambridge. In 1787, a change in his religious opinions took place, which ended in his adoption of the views of the Unitarians. The resignation of his living and the loss of the tutorship followed of course; but the laws of the University still allowed him to retain his fellowship. After some years of travel he returned to Cambridge, and occupied himself further in the study of Hebrew and divinity. In 1793, a pamphlet, entitled "Peace and Union recommended to the Associated Bodies of Republicans and Anti-republicans," was published by him, which contained distinct expressions of dislike to the doctrines and discipline of the Established Church. Immediately upon the publication of this pamphlet, both his college and various members of the senate commenced proceedings against Mr. Frend. The master and fellows of the former (by seven to four) "removed" him from residence in college, until proof of "good behaviour," and this sentence was confirmed by the visitor. Thirty-four members of the senate cited the author of the pamphlet before the Vice-chancellor

---

[* A notice of the proceedings of the Society for January will be found at p. 397, and of those for March at p. 477 of the present volume.]

(Dr. J. Milner), and a trial took place in his court, which lasted eight days. The result was, that a form of recantation was proposed to Mr. Frend, which he refused to sign; and sentence of "banishment" from the University was passed. This banishment is not expulsion, as persons unacquainted with the University generally believe, but a deprivation of the right to reside within the limits of the University; and, accordingly, though the sentence was confirmed on appeal, Mr. Frend continued to hold his fellowship till his marriage, and remained to the day of his death a Master of Arts, and a member of Jesus College. He retired of course from Cambridge, and came to London, where he maintained himself till 1806, by adding the profits of teaching and writing to the income derived from his fellowship. When the Rock Life Assurance was founded (1806), Mr. Frend, who had previously been consulted in the formation, was appointed actuary of that company, a post in which he remained until a severe illness compelled him (in 1826) to retire from active life. His health, however, recovered, and he continued his mental employments with an activity very unusual at his age, until the beginning of the year 1840, when he was attacked by paralysis, under which he lingered with almost total loss of speech and motion, though with the smallest possible decay of mind or memory, until February 21 of the last year, when he closed a life, which is regarded, even by those who differed from him, as a splendid example of honesty in the pursuit of truth, and of undaunted determination in the assertion of all that conscience required.

The losses and inconveniences which attended his banishment from college were not among the greatest risks which he ran. At a subsequent period, when the political struggle was at its height, and government prosecutions were frequently directed against the mere expression of opinion, Mr. Frend was one of the foremost among the despised minority which advocated emancipation and enfranchisement for all who were under religious or political disqualifications. At the time of certain of the prosecutions alluded to, it was currently said, that had the government succeeded in obtaining convictions, there was an intention of instituting several more; and Mr. Frend, it was stated, was to have been one of the defendants. This supposition cannot now be verified, even if it were true; but the rumour itself constitutes its object one of the leading opponents of the system which has since been so materially modified. With his political writings*, of which there were several, we have here nothing to do, any more than with those of a religious character. A true account of his scientific views cannot be easily given in a short space; nor can reasons for enlargement be better given than in the description itself of these views.

It generally happens that in recording the career of our departed members, we have little to say on their opinions, but only to specify the manner in which they carried them into practice; and small

---

* The titles of these will be found in the Gentleman's Magazine for May 1841 (pp. 541–543).

space may serve for great results. In the present instance we have to point out the singularities of thought which made Mr. Frend the last, we should suppose, of the *learned* Anti-Newtonians, and a noted oppugner of all that distinguishes algebra from arithmetic. Opposition to the theory of gravitation must in future be left to those whose mechanics do not distinguish velocity from force; and the rejection of the distinctive principles of algebra to those who would teach like philosophers what they have learnt like schoolboys, without going through any intermediate stage. But the subject of the present memoir stands in neither of these predicaments; and it would be highly interesting in itself, and no less than due to expiring tenets, to specify the probable influences under which such a mind as that of Mr. Frend directed him to stand quite alone among men of his philosophical acquirements; especially when it is considered that, up to the age of thirty-six, he had been a successful teacher of those scientific doctrines which he afterwards opposed, both by serious argument and ridicule *.

Undoubtedly the prime mover of this curious change was the alteration which took place in his doctrinal views of religion. Having been led to conclude that he had been betrayed by authority into the belief of propositions both inexplicable and false, the tendency to think that the inexplicable must be false, or at least to regard the former with strong suspicion, was a necessary ingredient of his future reflections on all subjects. The manner in which several leading doctrines of physics and mathematics had been handled by names of celebrity, was highly calculated to call out this disposition. The doctrine of attraction,—a mysterious connexion between matter and matter, with no existence but in its results; the theory of quantities less than nothing, a phrase which, arithmetically considered, is a simple contradiction of terms, were adopted at the time when Mr. Frend taught in a most positive and substantive sense, by the majority of investigators and all elementary writers.

It was in vain that Newton, obviously hoping for some further elucidation of his great regulator, concluded the *Principia* with a caution that he had *not yet* (*nondum*) found out the source of gravitation; his successors and commentators, with one voice, pronounced him the discoverer of the final mechanical cause of the planetary motions; and popular writers, who seldom refuse to say B when their leaders have said A, added that Newton had found out *why* water runs down hill. With respect to algebra, the matter was still worse. Euler asserted downright that a penniless man, fifty crowns in debt, has fifty crowns less than nothing; and offered proof. He assumes that a gift of fifty crowns would make this man richer; and

---

* In a magazine which lasted for a few months of 1803, 'The Gentleman's Monthly Miscellany,' of which Mr. Frend was editor, or co-editor, is an article by him, entitled "Pantagruel's Decision of the Question about Nothing," in which the manner of Rabelais is so well caught, that any one on a first perusal would think it likely to be an actual adaptation or parody, until a search through the writings of Rabelais satisfied him that it was simple imitation. It is a satire against some parts of algebra.

supposing him to employ the gift in the payment of his debts, then concludes that he *had* less than nothing, because, being now richer than before, he *has* only nothing. Others admitted the negative and impossible quantities as mysteries, and, reversing Mr. Frend's process, brought them forward as auxiliaries to the mysteries of the orthodox forms of Christanity; a practice not extinct in our own day, even after all that was inexplicable about impossible quantities has disappeared. At the time when Mr. Frend first thought on the subject, the assertion of mystery was the escape from the confession of incompleteness; the great mass of readers followed with implicit confidence, while, of those who thought for themselves, an enormous majority was too sensible of the value of the results of algebra to abandon it on account of difficulties. Some few rejected the peculiar doctrines of algebra altogether; among whom those of most note were, in succession, Robert Simson, Baron Maseres, and Mr. Frend. Most of those who were independent of authority united in blaming the method of the elementary writings, and were content to hope that a palpable guide to truth would not always be without rational connexion with undeniable axioms. Woodhouse, the restorer of thought on first principles at Cambridge, in a letter to Baron Maseres, preserved among Mr. Frend's papers, and dated November 16, 1801, distinctly lays it down that, in these matters, it is not the principles which prove the conclusions, but the truth of the conclusions which proves that there must, somewhere or other, be principles. "Whether or not," says he, "I have found a logic, by the rules of which operations with imaginary quantities are conducted, is not now the question: but surely this is evident, that, since they lead to right conclusions, they *must have a logic*." And he goes on thus: "Till the doctrines of negative and imaginary quantities are better taught than they are at present taught in the University of Cambridge, I agree with you that they had better *not* be taught; and the plan of our system of mathematical education, much as it is praised, needs, in my opinion, considerable alteration and reform; and perhaps you think that our late mathematical publications will not much increase the love or improve the taste for luminous and strict deduction." As concerns the mystics, then, there is no need to object to Mr. Frend's entire abandonment of their principles, but the reverse; for it may be asserted that most of those who thought about first principles did the same. Those who imposed on matter, in the name of Newton, a primary power of attracting other matter, with those who could, on their own definitions, be made to say that *a command to subtract* 2, *repeated as many times as there are units in a command to subtract* 3, *gives a command to add* 6, ought to have been surprised that they found so little opposition.

But the circumstance relative to Mr. Frend's ultimate views which is peculiar to himself and which cannot be remembered without surprise, is, that in clearing the trammels of mystery he had to force so thick an enclosure, that he left behind him not only the mysterious explanation, but the very facts which were professed to be explained, and which, it may be thought, could have admitted of no doubt. It seems to any one who reads his writings, that he means that Newton had done nothing out of mathematics, and that the *results* of algebra are all delusion. That the planets, attraction or no attraction, move about the sun, and are disturbed, precisely as it would be if there were attraction; that the truth of an equation though produced by aid of impossible quantities, may be verified by numerical computation—may be made purely experimental realities, and would, to most minds as well acquainted with the subject as that of Mr. Frend, remain true, even though attraction were the atheism which some formerly called it, and the doctrine of negative quantities were a part of the black art. Nor would it have been wonderful if he had rejected incomplete explanations in elementary writing, the object of which is to teach clear results of clear principles. But there was more than this: sometimes, though rarely, he seemed to have a power of admitting the facts as facts; but for the most part, when they were presented to him in conversation, his mind did not appear capable of dwelling on them long enough to decide whether an answer was required or not; they seemed to slip like water through a sieve. In this there was neither affectation nor evasion; it was a peculiar state of mind with regard to what could be contemplated as a scientific truth, and may be partly explained.

Mr. Frend had an admiration of simplicity, and an indisposition to arrive at complex results, which was perhaps a consequence of the desire to have no secret in philosophy. Next to the abandonment of all that was difficult to explain, followed the practical rejection of every thing in which the mind could not hold the full explanation at once before itself, in all its parts. The simple theory of numbers, that is, of integer numbers, was therefore naturally a favourite study; and this branch of mathematics is well known to be an extremely powerful stimulant of that disposition which leads to its pursuit. Legendre has said that it almost always becomes a species of passion with those who give themselves to it at all. With Mr. Frend it went still further; an equation with a fractional root, even if commensurable, was a pseudo-equation: and $x^2+y^2=1$, x and y being rational fractions, was an illegitimate child of $x^2+y^2=z^2$, x, y, and z being integers. In this particular Mr. Frend differed greatly from another remarkable person, his own most intimate friend Baron Maseres, whose leading idea it seems to have been to calculate more decimal places than any one would want, and to reprint the works of all who had done the same thing.

There was also another peculiar circumstance which no doubt had considerable effect. Mr. Frend had studied Hebrew thoroughly, and was, in the opinion of learned Jews, better versed in that language than any English Christian of his day. No one who became acquainted with him could long avoid noticing the interest which he took in every matter directly or indirectly concerning the history and progress of Christianity. This knowledge of their language, history, and customs, with a community of opinion on the nature of the Deity, led him much into the acquaintance of his *elder brethren,* as he frequently termed them, of the Jewish race; and he would have held any biography of himself very imperfect which omitted to note how strongly he felt toward their persuasion. It seldom happens that any person devotes himself so keenly to any history without imbibing some opinion of the superiority of its subjects; and Mr. Frend carried to the very verge of paradox, or it may be a little beyond, the notion that the mathematical and astronomical science of ancient Judea was substantially equal at least to that of any period of modern Europe, not excepting the present. Their lunar calendar was as good as if it had been made from modern observations, and much better adapted to represent a long period than any other; as much of pure mathematics as any one ought to admit flourished among them in the time of Solomon. It is needless to say, that not a vestige of historical evidence was ever produced in favour of these opinions, nor did we ever hear of any modern Jew who had carried his notions of the learning of his ancestors to such a length. Among modern nations, Mr. Frend had a peculiar respect for the Chinese, and was impressed with the opinion (not by any means peculiar to himself) that their government and social state is a model. The rudiments of science which he found among these nations, the ancient Hebrews and the modern Chinese, were easily magnified by his temperament, which was both sanguine and contemplative, into as much of astronomy and arithmetic as he had been able to save from the pollution of attraction and negative quantities; consequently, these countries were the depositories of real science, uncorrupted by sophistry. For the ancient Greeks and their writings he had an open contempt; they were children who had learned of the Jews, and spoiled their masters' doctrines: the good was due to their teachers, the bad was their own. All this time, and in the midst of such strange singularities of opinion as were never long absent from his mind, there was an eagerness to see the good of every thing actually present, which made his approbation very easy to gain. No one who talked with him could soon fathom the wide difference of sentiment between the two; for whatever might be the subject, there was a side on which it could be favourably viewed; and for that side Mr. Frend's mind, or that part of it which regulated his first expressions, had the quality (we must not say the attraction) of a magnet. His persuasion of the rapid advances which his contemporaries were making in morals, arts, and even sciences (however corrupted), was a spring of comfort to his age which never ran dry; and his interest in every thing new, which promised improvement to any class of mankind, in any one of those particulars, was, even after he was unable to speak or move, a commanding instinct, which he could not have disobeyed if he would. This unvarying effort to detect good in whatever came before him was essentially linked to his religious feelings, the source of his daily comfort, by the view which he never ceased to take of the ultimate consequences of Christianity; which he looked upon as the gradual restorer of mankind to a state of perfect goodness and knowledge. Every advance in art, learning, or science,—every amelioration of social evils,—every improvement in the law,—every evidence, however slight, of disposition to act, think, or hope, for the better, brought before him his cherished prospect of the final state of mankind, and was, in his opinion, only a step towards it. The consequence was, that any one who would wish to describe his age, must simply invert each and all of the characteristics which Horace* makes significative of the advanced periods of life.

Mr. Frend's scientific writings were particularly distinguished by simplicity and earnestness. The greater part of the whole consists in short pamphlets, or communications to periodical publications; and many proofs might be given, both of the extreme importance he attached to truth, and of his conviction that error, even in matters of science, is a noxious weed in the field of morals. His principal distinct writings on subjects of science are his 'Algebra' (Part i. 1796, Part ii. 1799), and his 'Evening Amusements' (1804–1822). The latter was an astronomical elementary work of a new character, which had great success; and the earlier numbers went through several editions. It embraces a metonic cycle, and therefore describes the places of the moon, in a manner which would make it useful for a considerable time to come, in the elementary instruction for which it was intended. This present year is that which answers to 1804, so that the opportunity to repeat the process of instruction, so far as the moon is concerned, has just commenced. The phænomena of the different months are described, and to each month is usually attached a short religious reflection, an account of some astronomical process or discovery, a hit at the Newtonian philosophy, or some such preface. We do not see much acquaintance with the new doctrines of physics, which had then excited attention for some years; but it must be remembered that a man, who took his degree at Cambridge in 1780, had very little training in experimental deduction apart from mathematics.

Mr. Frend's scientific peculiarities strongly illustrate what those who have carefully considered the reading of that time will perhaps think to be the natural consequence of it, upon an exceedingly honest, clear, and decided mind, placed in circumstances favourable to the development of opposition. The Cambridge student was isolated from experimental physics by the habits of his university, and from the progress of mathematics by its adherence to the fluxional notation. In essentials, the academic system was nearer to what it might have been at the death of Newton than those who now see its state could readily imagine to be possible: the theory of gravitation was taken wholly and solely from the *Principia*; no Englishman had made the smallest addition to it; and Clairaut, D'Alembert, &c. were only known by name as *French philosophers*,

---

* "Multa senem circumveniunt incommoda; vel quod
Quærit, et inventis miser abstinet, ac timet uti,
Vel quod res omnes timide gelideque ministrat;
Dilator, spe longus, iners, avidusque futuri,
Difficilis, querulus, laudator temporis acti
Se puero, censor castigatorque minorum."

the most odious appellation of the time. One question might be asked which would, perhaps, add some force to the preceding remarks, if reasons for an answer were sought:—How came the men of science, who were bred at our English universities, to let Priestley, whose life was one turmoil of controversy, and who visibly must have written four pages a-day, or thereabouts, of theological discussion during his whole experimental career, run off with such a splendid portion of the first-fruits of real chemistry?

The other work of Mr. Frend, his 'Elements of Algebra,' will lead every one who peruses it to think, with sincere regret, of his having preferred rejection to amendment; and will be a lesson to writers yet to come, that they should let that stand which appears to lead to truth, whatever warning they may think it necessary to give that the reason why it does so lead is imperfectly understood. It is, on the points which it treats, the clearest book in our language. Something of this is due to the rejection of difficulty; something to the use of no problems except those which can be answered in integers; but there remains enough to show that a work from such a writer, which should have taken algebra as it stood, distinguishing the part of which the logic was then complete from that of which the principles remained insufficiently understood, would have been the most valuable present which could have been made to the elementary student, and would, perhaps, have greatly accelerated the transition to the present state of the science, in which none need find a mystery. In all probability, the attack of Mr. Frend did materially influence this result. Among his papers is preserved a letter to him from M. Buée, a Frenchman residing in England, dated June 21, 1801, containing the form in which the perusal of Mr. Frend's work made the writer put together his own views of the subject; and admirably expressed. Of course it cannot be said how much suggestion was derived from the necessity of replying to specific objections; what is certain is, that in a few years from that time, this same M. Buée was, though in an imperfect manner, what Dr. Peacock calls the first formal maintainer of that exposition which removes the long standing difficulty.

Finally, whatever may be our opinion on the peculiarities of Mr, Frend's views, we must remember with high satisfaction that he was, during the last years of his life, one of our Fellows; and, also, that no narrow idea of the necessity of conformity of opinion prevented a man of his intellectual station from being called to the Council of the Society. The sincere regret with which the Council announces the loss which our Body has sustained is materially lessened by the reflection that his extensive learning, practical wisdom in the affairs of life, chivalrous assertion of all that he thought true, and extraordinary benevolence of feeling, were permitted a long and useful career, terminated only by natural decay, and followed by the love of many, and the respect of all.

It is well known to many of the Members of this Society that an enlarged and improved Catalogue of the Stars, arranged after the manner of the Catalogue of this Society, has been a long time in progress, under the auspices of the British Association. That work is now nearly completed, and ready for the press, and will contain above 8000 stars. To each star will be annexed not only the annual precession, but also the secular variation of such precession, and the proper motion when it can be ascertained. The usual constants for determining the apparent positions of the stars at any required epoch will also be given. This work cannot fail of being a valuable addition to the resources of the astronomer.

The Members may be interested in learning that the Standard Scale of this Society has been reported to Her Majesty's Government, as one of the best means of regaining an accurate determination of the Standard Yard that was destroyed in the conflagration of the two Houses of Parliament; and that an indirect overture has been made for the acquisition of it, should the Government eventually consider it desirable. The Council apprehend that the Members would readily accede to any arrangement in this respect, which would promote the object that the Government has in view, and at the same time not be injurious to the interests of the Society.

The British Association having appointed a Committee to consider the propriety of revising and re-arranging the constellations in the heavens, Sir John Herschel has drawn up an interesting paper on this subject, which has been read before the Society, and printed in the forthcoming volume of the Memoirs. As it was considered desirable that an early and extensive circulation of his views on this subject should take place, the Council ordered an additional number of copies of this paper to be printed, which have been generally distributed, with a view of drawing the attention of astronomers to this branch of the science. Sir John's revision has been confined to the southern hemisphere, where the greatest confusion prevails in the nomenclature of the stars and in the distribution of the constellations; and if the reform, which is here suggested in the south, should meet the approbation of astronomers, it may become a matter of consideration, whether the principle may not be extended into the northern hemisphere, which has been sadly confused by modern innovations.

Since the last Anniversary, Her Majesty's Government has put the Society in possession of two rooms on the basement story of the present building; which have been cleaned out and appropriated for the erection of any apparatus that may be required for pendulum experiments, or for prosecuting any other investigations that may be carried on in such apartments.

It had long been a subject of regret that the immense magazine of facts contained in the Annals of the Royal Observatory from the time of Bradley's appointment, downwards, till a very recent epoch, should remain in a great degree unavailable for astronomical use. Our illustrious associate Bessel, in his *Fundamenta Astronomiæ*, corrections to the solar tables, and finally by his *Tabulæ Regiomontanæ*, rendered this vast labyrinth permeable, and extracted and exhibited in a finished shape much of its valuable contents. Some years ago, the British Association proposed to the Government the reduction of all the Greenwich *planetary* observations under the gratuitous superintendence and responsibility of the present Astronomer Royal, and at his own suggestion. That work is now completed, and it is understood that the funds required for printing the results will be furnished by the Board of Admiralty. The planetary places are compared with the best existing tables, and the difference in heliocentric longitude and latitude given exactly as in the recent volumes of the Greenwich Observations, with a term which takes into account the errors of the solar tables, should any sensible errors be therein found. It need not be said to the members of this meeting that every care has been taken, by duplicate computations and frequent comparisons, to attain all practicable accuracy. The geometer who undertakes the revision of the theory of a planet will now have no labour which could be spared, and will be freed from every difficulty which is not inherent in the problem itself; so that we may feel tolerable confidence a few years will see us in possession of tables very far indeed advanced towards perfection.

But this work, laborious as it has been, yields in importance to that which has been subsequently undertaken by the Astronomer Royal (also gratuitously), the reduction of all the Greenwich observations of the moon, from Bradley downwards, together with a comparison of the observed places with those deduced from Plana's theory. Considerable progress has already been made. The R. A. and N. P. D. of the moon's bright limb, with the corresponding mean solar time, are computed; MS. tables, consisting of an extension of Damoiseau's tables for 1824, modified by the introduction of Plana's coefficients and new terms, are nearly ready. The skeleton forms are prepared, and some steps in the computations taken. The liberality of Her Majesty's Government has enabled the Astronomer Royal to employ fourteen calculators on the work, which is consequently advancing with all possible speed and œconomy. Let us hope that no pause will be made until a new set of lunar tables of home manufacture are produced, which shall define the place of our hitherto incorrigible satellite with the accuracy of the best observations, and sufficiently for the nicest purposes of geography. Your Council feel that you will heartily join with them in their respect for the talents, disinterested activity, and *official* piety of the Astronomer Royal, and in thanks to the Government for its discriminating and liberal patronage of our science.

The Council are glad to have it in their power to report to the meeting, that the difficulties which seemed to lie in the way of successful completion of the Cavendish experiment have been removed, by new precautions against the radiation of heat from the large balls. Though many experiments may, in the early investigations, have been apparently wasted, yet in reality much good must result from the new light thus thrown upon the details of the operation itself, and on the torsion-balance, which is the principal instrument employed. Considering the nature of the quantity required, the results begin to assume a degree of accordance with each other which promises a very accurate determination of that great element of the solar system, the mean density of the earth. The slight discrepancies which still remain, and which appear to show that something depends on the substance employed, and more on unknown circumstances connected with the torsion-balance itself, are not such as to throw any reasonable doubt on the density obtained being true within less than a hundredth part of the whole. So much can safely be said at the present time; and it is not improbable that a still smaller limit of error may be substituted for the one just named. Mr. Baily's final report may be soon expected, and in the meantime some detail of the history of the experiment is actually in the hands of the Secretaries, and will shortly be read at an ordinary meeting of the Society. The work itself will form the fourteenth volume of the Memoirs, and a portion of the tables is already in the hands of the printer.

The Council have the satisfaction of announcing that the thirteenth volume of the Memoirs will be ready, perhaps, before the completion of the twelfth; Mr. Baily, having been lately engaged in reprinting, at his own expense, the catalogues of Ptolemy, Ulugh Beigh, Tycho Brahé, Halley, and Hevelius, in the type and form of our Memoirs, has offered the whole to the Council, to form the volume in question. As might have been expected, these catalogues have undergone such a revision and comparison as will materially increase their utility, and make these integrant portions of the history of astronomy familiar to the observer of our own day, who now looks upon them as difficulties, and refers to them (if, indeed, he have so much as the means of doing so at all) as little as he can help. The outlay saved to the Society by the manner in which this volume comes to us, though deserving and obtaining our warm acknowledgements, is the least part of the benefit; nor could the Council have omitted one word of the preceding testimony, if the manuscript, being, as it is, such as would gladly have been received, had been presented in the usual manner.

The whole of the volume is printed, excepting the preface, of which a circumstance well known to the Society at large has delayed the execution. And here, though it may be unusual to refer to the incidents of private life, yet the Council are sure that this meeting would feel disappointed if some opportunity were not given to the members of the Society to congratulate each other, and Mr. Baily, upon his most welcome and providential escape from the consequences of one of those accidents to which the inhabitants of crowded cities are daily exposed: an accident which, as all present remember, almost removed all hope of recovery, and made it seem next to impossible that life, if spared, should have been again occupied in the promotion of knowledge, and least of all in active research. Seeing him once more among us, in perfect health of mind and body, and remembering how much more probable it lately appeared that we should now be commemorating his innumerable services to the Society than anticipating their continuance, the Council drop the subject with the expression of their earnest hope that a life preserved against all expectation may be preserved beyond all expectation, and that a distinguished career may yet await one of the earliest and the most indefatigable friends of the Society.

In the Address of the President at the last Anniversary of the Society, honourable mention was made of Mr. Henderson's investigations relative to the presumed parallax of α Centauri. These investigations have been continued to the present time; and from some observations recently received by him from Mr. Maclear, at the Cape of Good Hope, he is confirmed in his opinion relative to this subject, and considers the parallax to be about $1''$. The Council trust that they shall soon receive from Mr. Henderson a detailed memoir on this important subject, which will then be read at the ordinary meeting of the Society.

The Council regret that they have to announce the retirement of Lieut. Raper from the office of Secretary to this Society; an office which he has filled with the greatest zeal and attention, and which calls from this meeting the expression of their best thanks. Nothing but the love of science and the talents which he possesses could have induced him to take so active and important a duty, oftentimes at a sacrifice of private ease and convenience: but this remembrance is at once the source of our approbation and the cause of our regret.

The Council trust that the award of the medal to Prof. Hansen will meet the approbation of the Society. The labours of M. Hansen are well known to those astronomers and mathematicians who have attended to, and cultivated, that branch of inquiry which more especially relates to those abstruse and intricate points of investigation that require the greatest exercise of mental exertion. The grounds on which this award has been made will be more fully explained in the Address of the President at the close of this Report.

The President (the Right Honourable Lord Wrottesley) then addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—Since the great discovery of the law of gravitation, the means by which the astronomy of the solar system has been advanced to its present state of perfection are of two distinct kinds. The first consists in the collection of facts from observation,—or, it may be said, in the application of that complicated and refined system of operations whereby the practical astronomer is enabled not only to assign the exact positions which the several bodies belonging to the system occupy at the moment of observation, but also to determine the paths they describe in space, and the laws by which their motions are governed. The second is that which is employed by the geometer. Setting out from the law of gravitation as established by Newton, and borrowing only from observation the elements which are necessary for the institution of his calculus, his object is to deduce from theory alone the whole of the phænomena of the system, even to their minutest details, and, by a comparison of his results with observation, to determine the masses of the different bodies, the influences which they exercise on the motions of each other, and the amount by which the elements of their fluctuating orbits deviate from their average conditions;—to express in formulæ the state of the system and the position in space of every body belonging to it at any given instant in past or future duration; and, finally, to convert his formulæ into numericalt ables, for the uses of navigation and the other important purposes to which astronomy is subservient.

It is for researches in this second department of our science, undoubtedly the most arduous and difficult of the two, that your Council have awarded the Society's Gold Medal for the present year to Professor Hansen, the Director of the Observatory at Seeberg, and, according to annual custom, the duty devolves on me of stating to you the grounds of their decision. The subject is not very susceptible of popular explanation; in fact, the especial services which M. Hansen has rendered to astronomy consist in the development of new formulæ, and the exhibition of new artifices of calculation, in the remotest and most abstruse departments of mathematical analysis. Nevertheless, I trust I shall be able to convey such an idea of their nature and object as will enable you at least to appreciate the motives which have influenced your Council in conferring on our illustrious Associate this testimony of the Society's approbation.

In proceeding to determine the motions of a celestial body urged by a central force, and disturbed by the action of other bodies, the accelerating forces in the direction of rectangular coordinates are expressed by three differential equations of the second order, which, as is well known, can only be integrated by approximation. To obtain approximate integrals, two methods have been principally followed. The first consists in deducing from the differential equations, expressions for the variations of the radius vector, longitude, and latitude of the disturbed body in a function of the disturbing force and its partial differentials; and in integrating these expressions, either by developing them in series which proceed according to the powers of the eccentricities and inclinations, or else by the method of parabolic quadratures. This is the most obvious method of determining the perturbations, and also the simplest when the approximations are only carried to terms of the order of the eccentricities and inclinations; but when a closer approximation becomes necessary, and terms of a higher order are required to be included, the expressions become complicated, and the method accordingly loses its advantages.

The other method of obtaining approximate results is known in analysis as the method of *variation of arbitrary constants*. This method, though undoubtedly entitled to be regarded as one of the most ingenious artifices of modern analysis, is suggested in a manner by the peculiar constitution of our solar system, in which the disturbing forces which act upon any body bear so small a proportion to the principal force which determines the general orbit, that the body may be regarded as moving always in an *ellipse*, but in an ellipse whose elements are in a state of continual though extremely slow change. In accordance with this idea, the origin of which may be referred to Newton himself, the accelerating forces which act on a celestial body are conceived to be divided into two parts, one of which renders integrable the differential equations between the coordinates and the time, and gives the elliptic orbit which the body would describe about a centre of force if there was no disturbance; while the arbitrary quantities introduced by this first integration are supposed to be rendered variable by the other part, and their variations determined by means of differential equations of the first order, whose integrals (usually obtained by successive approximation) give the elements of the true perturbed orbit, from which the radius vector, longitude, and latitude of the body at any given time are computed.

The first example of this method of computing the planetary perturbations was given by Euler in the Berlin Memoirs for 1749, where he obtains the differential equations of the first order of the inclination and longitude of the node by varying the arbitrary constants which express these two elements in the elliptic orbit. But though Euler afterwards succeeded in finding expressions for the variations of some of the other elements, the complete development of the method, and its application not only to physical astronomy, but to the general theory of mechanics, is due to Lagrange; and it forms the distinguishing feature, so far as dynamics are concerned, of the beautiful system of mathematical analysis which that illustrious geometer has bequeathed to science in the *Mécanique Analytique.*

The method of analysis which we are now considering, is attended with peculiar advantages when applied to the determination of the *secular* inequalities of the orbits, in the development of which the greatest triumphs have been achieved of which physical astronomy can boast since the discoveries of Newton. It was by this means that Lagrange demonstrated that the greater axes of the planetary orbits are affected by no inequalities independent of the configuration of the bodies, and consequently that amidst all the fluctuations of the system, the mean distances of the planets from the sun, and therefore also their mean motions, remain for ever and unchangeably the same. It was by the same means Laplace formed exact expressions for the secular variations of the eccentricities and inclinations, and thence proved that the changes of those elements must always be inconsiderable; that they do not increase indefinitely with the time, but after a longer or shorter period again resume their former values. These conclusions, which were confirmed by the subsequent and more complete analysis of Poisson, lead immediately to what may be regarded as the most remarkable triumph of modern science, namely, the stability of the solar system; for they show that, however the motions and positions of the several planets and satellites may be deranged and disturbed by their mutual perturbations, the variations which take place in the magnitudes and forms and positions in space of the different orbits are not only periodic, but confined within narrow limits.

But, although in the hands of these great masters of analysis the method of varying the elliptic elements led to the sublime discoveries now alluded to, it is not without defects, which become particularly sensible in the numerical computations. Among these is to be reckoned the length of the calculations which it renders necessary for two reasons; first, because the number of elements of an orbit being twice the number of the coordinates which determine the place of the body, the calculation of a much greater number of quantities is required than by the first-mentioned method; and, secondly, because when the perturbations of the elements have been computed, there still remains the labour of substituting the altered elements in the expressions of the coordinates derived from the elliptic motion, in order to obtain the disturbed coordinates and the place of the body in its actual orbit. The principal defect of the method, however, consists in this, that the coefficients of the different terms of the series which express the disturbed elliptic elements have, in general, much larger values than the corresponding terms of the expressions of the disturbed coordinates which determine the position of the body, so that the series expressing the disturbed elements converge slowly, even when they correspond to small perturbations of coordinates. If we conceive, for example, a system of forces of short period to disturb the curvature of an orbit many times in a single revolution, it will be easy to see that in each of these periods the elements of the orbit may have been greatly altered, while the disturbance of coordinates (of the longitude and radius vector, for example) may have been trifling. But in order to obtain these small disturbances, it is necessary to pass through the perturbations of the elements, which, relatively, are very considerable, and of which the calculation is rendered laborious by reason of the slow convergence of the series; and this inconvenience exists not merely in the case of the perturbations of the first order with respect to the masses, but in a still greater degree in the case of those of the second and of the higher orders. For these reasons the calculation of the perturbations has hitherto been in some respects imperfect and unsatisfactory; the computer always finding himself obliged to omit a number of the smaller terms without having any assurance that the error resulting from the omission is insensible; or, as M. Hansen has remarked, rather from a sort of presentiment that the omitted terms have no appreciable influence, than from a mathematical demonstration of their influence being insensible.

It was with a view to remove these defects from the lunar and planetary theories that M. Hansen undertook the series of remarkable investigations which have appeared from time to time, during a considerable number of years (partly in Professor Schumacher's invaluable Repertory, the *Astronomische Nachrichten*, and partly in two separate publications,—one on the perturbations of Jupiter and Saturn, and the other on the lunar theory), for which the Council has now awarded the Society's medal. His method of expressing the perturbations is based on that of Lagrange; but the modifications which he has introduced are of an important kind, and lead, in fact, to an entirely new mode of conducting the numerical calculations; so that, if it cannot be said that he has furnished us with a new instrument wherewith to attack the difficulties of the problem, he is at least entitled to the merit of having taught us a new method of applying that of which we were already in possession.

On taking a general view of Hansen's method*, the point which first presents itself as remarkable, and that indeed in which the novelty of his process essentially consists, is the original and highly ingenious artifice which he employs in order to arrive at the expressions for the perturbed coordinates,—namely, the longitude, radius vector, and latitude. In the usual method of proceeding, the arbitrary constants introduced by integration are determinate functions of the elliptic elements and time, and the perturbations of coordinates are obtained by supposing the elements to vary. Instead of the true time, M. Hansen introduces into the functions an analogous, but indeterminate quantity, and considers the elements as invariable. He then determines the variations which this quantity must undergo (in other words, he finds what alteration must be made in the *time*, in the place where it enters explicitly into the elliptic formulæ), in order that the elliptic formulæ, with altered time and invariable elements, may give the same value of the indeterminate functions as would be found by using the true time and variable elements. Suppose, for example, the function of elements and time to be the true longitude; then the problem, according to M. Hansen's method of viewing it, amounts to this:—To find the perturbations which must be applied to the mean longitude, in order that the true longitude deduced from it with the use of invariable elements, may be the true perturbed longitude.

It is evident, that the use of invariable elements, and time altered so as to give the correct value for longitude, would not, with the elliptic formulæ, give a correct value of the radius vector; but this difficulty is surmounted in an extremely ingenious manner by the introduction of subsidiary terms, which, being applied as corrections to the radius vector of the unaltered elliptic orbit (*i. e.* unaltered except in time), give its true perturbed value. By similar considerations an expression is found for the latitude in the disturbed orbit. It would be impossible, however, without the aid of algebraic symbols, to give an idea of the analytical processes employed for determining these subsidiary terms; and for the same reason I must content myself with a bare allusion to the still more remarkable artifice to which he has recourse in order to obtain an expression for the continuous variation of the perigee and node of the lunar orbit, for which, by reason of their rapid revolution, invariable elements will clearly not suffice, and a departure in some degree from the original principles becomes necessary.

These deviations from the usual methods lead to very important advantages in the calculation of the tables, for the series expressing the perturbations of coordinates are not only rendered more conver-

---

* [On the subject of M. Hansen's method see Phil. Mag., Third Series, vol. xix. p. 82.—Edit.]

gent, whereby a smaller number of terms is required to be computed, but the coefficients of the individual terms are obtained with a smaller amount of labour than was necessary in the methods hitherto employed.

It will be readily seen from what has now been said, that the general aim of M. Hansen's researches is the improvement of the methods of expressing the lunar and planetary perturbations, so as to render the numerical calculations more easy and more certain. There is, however, one advantage which M. Hansen states to belong to his method, of by far too important a kind to be passed over without particular notice. It is this:—In the series which express the values of the disturbed coordinates, every term exceeding a certain numerical value, assumed at pleasure, can be immediately recognised, so that all those terms which fall below the assumed value may be rejected from the first, with the certainty that their sum falls within a given limit. The certainty thus acquired that every term having a sensible value is retained in the calculation, is an improvement in the theory on which it would be difficult to set too high a value; and in fact it removes the principal defect which has hitherto attended all the methods of approximation which have been proposed. Nor is this advantage obtained by any sacrifice of generality; for neither with respect to the eccentricity and inclination, nor powers of the mass, is any other restriction introduced than is inseparable from the nature of the problem.

Besides these principal advantages of more rapidly converging series, and certainly with respect to the value of the omitted terms, there are some minor advantages incidental to the new method, which, however, are still of great importance. Among these may be mentioned certain relations subsisting among the analytical expressions of the coordinates, pointed out by M. Hansen, from which equations of condition are deduced which not only facilitate the calculations but afford a ready means of verification.

The applications which M. Hansen has as yet made of his method are to the inequalities of Jupiter and Saturn*, in a memoir which obtained the prize of the Royal Academy of Sciences of Berlin; and, to the lunar theory, in a work recently published†. In the former memoir the theory is worked out to a numerical result. The expressions for the differential values of the longitude, latitude, and radius vector, are integrated by the method of quadratures, and results obtained which agree with those derived from the ordinary methods of approximation founded on the smallness of the eccentricities and inclinations. The approximations are, indeed, only carried to terms of the second order inclusive, with respect to the masses; but in the case of Saturn, all the terms of this order exceeding a certain numerical value are calculated. His theory of the

---

* *Untersuchung über die gegenseitigen Störungen des Jupiters und Saturns.* Berlin, 1831.

† *Fundamenta nova Investigationis Orbitæ veræ quam Luna perlustrat.* Gothæ, 1838.

lunar perturbations, which presents difficulties of a peculiar kind, is not so far advanced, and much is still wanting to render it complete even as a symbolical theory. But in a recent number of the *Nachrichten** he has announced that the calculations, on which he has been for some time engaged, are now proceeding towards a conclusion; and he has given some results which show that the new methods apply with as much advantage to the moon as to the planets.

Thus, gentlemen, I have endeavoured to place before you a sketch of M. Hansen's researches, which, brief and imperfect as it is, will enable you to understand their object, and appreciate their importance. Should it be thought that these investigations refer only to matters of detail, and that the results at which he has arrived include none of those grand discoveries which enlarge the boundaries of science, and give us, as it were, a new insight into the constitution of the universe, let it be remembered that the progress already made in physical astronomy has narrowed the field to the present inquirer, and that, in proportion as science advances, its processes become more and more intricate. The problem of the universe, difficult as it is, is still a limited problem; and the successive steps in its solution may be assimilated to the terms of one of those converging series expressing the perturbations we have been speaking of, in which each succeeding term is not only smaller in value than the preceding, but also more difficult of calculation. It is with the smaller terms only that the theoretical astronomer has now to concern himself; but his labours, though necessarily attended with less brilliant results, are not on that account the less necessary or useful. On the contrary, no more valuable service remains to be rendered to astronomy, in the present state of the science, than the improvement of the existing methods of computing the lunar and planetary perturbations. The labours of M. Hansen have been steadily, and perseveringly, and successfully directed to this end. Whether the new methods which he has so ingeniously developed will be found in all cases preferable to those with which we are already familiar, or whether they will ultimately be adopted by astronomers as affording the most convenient forms under which the conditions of the solar system can be expressed, is a question which your Council do not venture to decide, and on which, perhaps, it would at present be premature to form an opinion. But with respect to the profound ingenuity and consummate analytical skill which he has brought to bear on the intricate subjects of his investigation, there can be but one voice. His researches, which have been of the most laborious and abstruse kind, have been directed to the highest and most important questions of astronomy; and the means by which he has sought to conquer the still remaining difficulties, present more of novelty and originality, and afford stronger hopes of removing the differences which still exist between the tables and observation, than any which have been employed since the variation of arbitrary constants was propounded by Lagrange. On the whole, having respect to the importance of the subject, the results which

---

* No. 403.

have already been obtained, and the promise afforded of future improvements, the Council doubt not that the Society, and astronomers in general, will ratify its decision.

The President then, addressing the Foreign Secretary, continued as follows :—

Mr. Rothman,—In transmitting this medal to Professor Hansen, assure him of the lively interest which this Society takes in the continuance of his important labours; and convey to him our warmest wishes for his health and happiness, that he may be enabled to complete those improvements in the most arduous departments of our science which he has so auspiciously commenced, and thereby acquire a still stronger title to the gratitude of astronomers, and to a place among those who have most signally contributed to the development of the theory of Newton.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: the Right Hon. Lord Wrottesley, M.A., F.R.S.—*Vice-Presidents*: Francis Baily, Esq., F.R.S.; Rev. George Fisher, M.A., F.R.S.; Sir John F. W. Herschel, Bart., K.H., M.A., F.R.S.; Rev. Richard Sheepshanks, M.A., F.R.S.—*Treasurer*: George Bishop, Esq.—*Secretaries*: Rev. Robert Main, M.A.; Richard W. Rothman, Esq., M.A.—*Foreign Secretary*: Thomas Galloway, Esq., M.A., F.R.S.—*Council*: George Biddell Airy, Esq., M.A., F.R.S., *Astronomer Royal*; Rev. W. Rutter Dawes; Augustus De Morgan, Esq.; Thomas Jones, Esq., F.R.S.; John Lee, Esq., LL.D., F.R.S.; Major-General C. W. Pasley, R.E., F.R.S.; Lieut. Henry Raper, R.N.; Edward Riddle, Esq.; Lieut. William S. Stratford, R.N., F.R.S.; Charles B. Vignoles, Esq.

April 8.—The following communications were read:—

I. On the Aggregate Mass of the Binary Star 61 Cygni. By S. M. Drach, Esq.

The truth of universal gravitation having been confirmed by the elliptic form of the orbits of binary stars, it follows that knowing the absolute distances of the component members and their period of revolution round each other, we are able to deduce their aggregate mass compared with that of our sun and a planet, by exactly the same process which acquaints us with the various masses of the planets which are attended with satellites.

The ratio of the sums of the masses of the component bodies in two such systems being then that of the cubes of the mean distances of the components, multiplied into that of the inverse squares of their periods of revolution round each other, we may assume that one system is composed of the earth and sun, and we have then two cases to consider: 1st, when this binary star is of very small mass compared with the sun, in which case the system would revolve about the sun, the centre of gravity being near the sun's centre; and, 2ndly, when the star's mass is much superior to that of the sun, in which case the orbital motion of the star would be only apparent, and owing to the real revolution of the solar system round it.

Applying these remarks to the case of the star 61 Cygni, and assuming Bessel's value of the parallax, and the usually assumed elements of the orbit of this binary system, it appears evident that this system is unconnected with the solar system. It does not, however, appear impossible that both systems revolve round a third at an immensely greater distance than that of the sun from the earth.

The author, in conclusion, adverts to the great importance, in the present advanced state of practical astronomy, of noting the positions of the stars having the greatest proper motions with all possible accuracy, and of rigorously comparing the deduced proper motions at equal intervals of time, for the purpose of discovering whether the motions are performed in one plane, and whether they are uniform; and also to the importance of having a catalogue of stars accurately arranged in order of brilliancy by means of photometrical observations, as an essentially requisite element in the determination of their relative distances from the earth.

II. Second Note on the Mass of Venus. By R. W. Rothman, Esq.

In a Note on the Masses of Mercury and Venus, read at the Meeting of this Society on the 14th of January*, I stated that a consideration of the motion of the perihelion of Venus had led me to conclude, that it was necessary to diminish the mass of Mercury by a quantity estimated approximately at $\frac{4}{10}$. This would make the mass in question $\frac{1}{3182843}$. I may observe in passing, that in the notice of the meeting of the 14th of January, page 132, there is a misprint in the algebraical formula for the motion of the perihelion; but this is merely a typographical error, and the calculations are correct. At the same meeting there was read an extract of a letter from Professor Encke to the Astronomer Royal, from which it appears that Professor Encke, guided by very different considerations, has been led to fix the mass of Mercury in the first instance at $\frac{1}{3091947}$, and subsequently at $\frac{1}{4865751}$.

At the end of my Note I stated that the secular equations affecting the orbit of Mercury appeared to confirm the necessity of an augmentation of the mass of Venus, to which I have been led by an examination of the secular motion of the node of the latter planet. But, in fact, this deserves somewhat further development.

I have calculated the secular equation of the node of Mercury with the same planetary masses as those assumed in my first node, excepting that of Mercury, which I have supposed equal to $\frac{1}{3182843}$.

---

[*] See present volume, p. 398.

I have used the following values of the greater axes which are slightly different from those employed before :—

☿ = 0·38709888
♀ = 0·72333228
♁ = 1·
♂ = 1·52369210
♃ = 5·20115524
♄ = 9·53797320
♅ = 19·18251740

With these data I obtain for the annual sidereal motion of the node of mercury :—

$$\frac{d\,\text{☊}_0}{dt} = -\,7''{\cdot}264 \quad -\,0''{\cdot}0621\,\mu_0 - 3''{\cdot}8665\,\mu_1 - 0''{\cdot}8915\,\mu_2$$
$$-\,0{\cdot}0991\,\mu_3 - 2{\cdot}2292\,\mu_4 - 0{\cdot}1129\,\mu_5 - 0{\cdot}0022\,\mu_6$$

If we assume Encke's second value of the mass of Mercury, namely $\frac{1}{4865751}$, and suppose $\mu_2, \mu_3, \mu_4, \mu_5, \mu_6$, each $= 0$,

then $\frac{d\,\text{☊}_0}{dt} = -7''{\cdot}242 - 3''{\cdot}867\,\mu_1$.

Now, according to Lindenau, the tropical motion of the node from 1631 to 1802 is $42''{\cdot}534$ annually; hence, with a precession of $50''{\cdot}21$, the annual sidereal motion is $7''{\cdot}676$,

$$\therefore\ -\,0''{\cdot}434 = -\,3''{\cdot}867\,\mu_1$$
$$\mu_1 = +\,0''{\cdot}11.$$

With the same data as before I have calculated the motion of the perihelion of Mercury, for which I find the following expression :—

$$\frac{d\pi_0}{dt} = +\,5''{\cdot}44335 \quad +\,2''{\cdot}88796\,\mu_1 + 0''{\cdot}86028\,\mu_2$$
$$+\,0''{\cdot}02881\,\mu_3 + 1''{\cdot}59026\,\mu_4 + 0''{\cdot}07604\,\mu_5.$$

The mass of Mercury does not enter into this expression. The coefficient of μ_6 is insensible. Supposing now $\mu_2, \mu_3, \mu_4, \mu_5$, each $= 0$,

$$\frac{d\pi_0}{dt} = +\,5''{\cdot}44335 + 2''{\cdot}8876\,\mu_1.$$

Now Lindenau gives for the tropical motion of the perihelion $56''{\cdot}354$; or, with a precession of $50''{\cdot}21$, an annual sidereal motion $= +\,6''{\cdot}144$.

$$\therefore\ 6''{\cdot}144 = 5''{\cdot}443 + 2''{\cdot}888\,\mu_1$$
$$\therefore\ \mu_1 = \frac{0\,{\cdot}701}{2\,{\cdot}888} = 0''{\cdot}25.$$

The node of Venus, as given in my first note, furnishes us, assuming Encke's second mass of Mercury, and neglecting the terms which contain $\mu_2, \mu_3, \mu_4, \mu_5, \mu_6$, with the equation

$$-\,1''{\cdot}60 = -\,5''{\cdot}174\,\mu_1$$
$$\therefore\ \mu_1 = +\,0''{\cdot}31.$$

The three values of μ_1 are then

$$\mu_1 = +0''\cdot 11$$
$$\mu_1 = +0\cdot 25$$
$$\mu_1 = +0\cdot 31$$

or, taking the mean $\mu_1 = 0''\cdot 22$.

This, of course, is only given as an approximate estimation; but it seems difficult to resist the conclusion that the mass of Venus should be augmented by a quantity which cannot be put lower than one-tenth, and is probably considerably larger. An augmentation of one-tenth would make this mass $\frac{1}{365308}$, of two-tenths, $\frac{1}{334866}$.

III. On a Method of Determining the Latitude at Sea. By M. C. L. von Littrow, Adjoint-Astronomer at the Imperial Observatory at Vienna. Communicated by the Rev. W. Whewell, Master of Trinity College, Cambridge.

IV. On the Rectification of Equatoreals by Observations of Stars on the Meridian and at an Hour-Angle of Six Hours. By M. C. L. von Littrow. Communicated by the Rev. W. Whewell.

V. The Parallax of α Centauri deduced from Mr. Maclear's Observations at the Cape of Good Hope in the years 1839 and 1840. By Professor Henderson.

An abstract of the principal contents of this paper will be found in Professor Henderson's letter, contained in the last Monthly Notice, viz. that for March 1842 *. In addition, the author gives the following facts relating to the history of the observations of the star α Centauri. The earliest recorded observations which he has found are those of Richer, at Cayenne, in 1673, and of Halley, at St. Helena, in 1677; but neither of these astronomers mentions it as being double. Feuillée appears to have been the first person who observed it to be double, his observations being made at Conception, in Chili, in July 1709, with a telescope of 18 feet focal length. He estimates their magnitudes as being of the third and fourth, the smaller star being the more westerly, and their distance as equal to the apparent diameter of the smaller star (*Journal des Observations Physiques*, &c., par Louis Feuillée, tome i. p. 425; Paris, 1714).

La Condamine observed the star during the expedition to Peru for measuring an arc of the meridian (see Philosophical Transactions for 1749, p. 142). He estimated it as being of the first magnitude, and recognised its duplicity; and he remarked that the larger star was northward of the other, and to the east of it. From La Caille's observations in 1751–2, the distance of the two stars appears to have been 22″·5. Maskelyne observed them at St. Helena in 1761 (see Philosophical Transactions for 1764, p. 383), and estimated them as being of the second and fourth magnitudes. Their distance, as observed with a divided object-glass micrometer, he found to be from 15″ to 16″. From this time to the time of the institution of the Paramatta Observatory, the author has met with no observations of the distance of the stars. Mr. Dunlop, in the years 1825–6, found

---

* See present volume, p. 482.

the distance to be 23″ (see Memoirs of the Royal Astronomical Society, vol. iii. p. 265), since which time it has been decreasing at the rate of more than half a second per annum. The angle of position scarcely appears to have changed since the time of La Caille; whence it may be inferred that the relative orbit is seen projected into a straight line, or a very eccentric ellipse; that an apparent maximum of distance was attained in the end of the last or the beginning of the present century; and that, about twenty years hence, the stars will probably be seen very near each other, or in apparent contact; but the data are at present insufficient to give even an approximation to the major axis of the orbit and time of revolution.

VI. Observations of the beginning and end of the Solar Eclipse of July 18, 1841. By Dr. Cruikshank. Communicated by G. Innes, Esq.

The eclipse was observed at Fyvie Castle, in latitude 57° 26′ 40″·7 north, and longitude $9^m\ 32^s{\cdot}6$ west, where there is a good clock by Hardy and a fine transit instrument. The magnifying power of the telescope used was about thirty.

	h	m	s		s
Time of the beginning of the eclipse.	2	15	4	; uncertain to	10
Time of the end..................	2	57	30	....	2.

ROYAL IRISH ACADEMY.

[Continued from p. 397.]

May 24, 1841 (Continued).—The Rev. Charles Graves, F.T.C.D., read a paper "On the Application of Analysis to spherical Geometry."

The object of this paper is to investigate and apply to the geometry of the sphere, a method strictly analogous to that of rectilinear coordinates employed in plane geometry.

Through a point O on the surface of the sphere, which is called *the origin*, let two fixed quadrantal arcs of great circles O X, O Y be drawn; then if arcs be drawn from Y and X through any point P on the sphere, and respectively meeting O X and O Y in M and N, the trigonometric tangents of the arcs O M, O N are to be considered as the *coordinates* of the point P, and denoted by x and y. The fixed arcs may be called *arcs of reference*. An equation of the first degree between x and y represents a great circle; an equation of the second degree, a spherical conic; and, in general, an equation of the nth degree, between the spherical coordinates x and y, represents a curve formed by the intersection of the sphere with a cone of the nth degree, having its vertex at the centre of the sphere.

Though it is not easy to establish the general formulæ for the transformation of spherical coordinates, they are found to be simple.

Let x and y be the coordinates of a point referred to two given arcs, and let x', y' be the coordinates of the same point referred to two new arcs, whose equations as referred to the given arcs are

$$y - y'' = m(x - x''),$$
$$y - y'' = m'(x - x''),$$

x'', y'' being the coordinates of the new origin; then the values of

x and y to be used in the transformation of coordinates would be

$$x = \frac{x''(a x' + b y' - 1)}{p x' + q y' - 1},$$

$$y = \frac{y''(c x' + d y' - 1)}{p x' + q y' - 1}.$$

In which a, b, c, d, p, and q are functions of m, m', x'', and y''. It is evident that the degree of the transformed equation in x', y', will be the same as that of the original one in x and y.

The great circle represented by the equation

$$\alpha x + \beta y = 1,$$

meets the arcs of reference in two points, the cotangents of whose distances from the origin are α and β; and, if the arcs of reference meet at right angles, the coordinates of the pole of this great circle are $-\alpha$, and $-\beta$. It appears from this, that if α and β, instead of being fixed, are connected by an equation of the first degree, the great circle will turn round a fixed point. And, in general, if α and β be connected by an equation of the nth degree, the great circle will envelope a spherical curve to which n tangent arcs may be drawn from the same point. Thus, the fundamental principles of the theory of polar reciprocals present themselves to us in the most obvious manner as we enter upon the analytic geometry of the sphere.

A spherical curve being represented by an equation between rectangular coordinates, the equation of the great circle touching it at the point x', y', is

$$(y - y')\, d x' - (x - x')\, d y' = 0;$$

the equation of the normal arc at the same point is

$$(y - y')\, [d y' + x' (x' d y' - y' d x')] + (x - x')\, [d x' + y' (y' d x' - x' d y')] = 0.$$

Now, if we differentiate this last equation with respect to x' and y', supposing x and y to be constant, we should find another equation, which, taken along with that of the normal arc, would furnish the values of x and y, the coordinates of the point in which two consecutive normal arcs intersect: and thus, as in plane geometry, we find the evolute of a spherical curve.

Let $2\,\gamma$ be the diametral arc of the circle of the sphere which osculates a spherical curve at the point x', y', Mr. Graves finds that

$$\tan \gamma = \pm \frac{[d x'^2 + d y'^2 + (x' d y' - y' d x')^2]^{\frac{3}{2}}}{(1 + x'^2 + y'^2)^{\frac{3}{2}} (d x' d^2 y' - d y' d^2 x')}.$$

For the rectification and quadrature of a spherical curve given by an equation between rectangular coordinates, the following formulæ are to be employed:—

$$d s = \frac{\sqrt{d x'^2 + d y'^2 + (x' d y' - y' d x')^2}}{1 + x'^2 + y'^2},$$

and $$d\,(\text{area}) = \frac{y\, d x}{(1 + x^2) \sqrt{1 + x^2 + y^2}}.$$

In the preceding equations the radius of the sphere has been supposed $= 1$.

The method of coordinates here employed by Mr. Graves is entirely distinct from that which is developed by Mr. Davies in a paper in the 12th vol. of the Transactions of the Royal Society of Edinburgh. Mr. Graves apprehends, however, that he has been anticipated in the choice of these coordinates by M. Gudermann of Cleves, who is the author of an "Outline of Analytic Spherics," which Mr. Graves has been unable to procure.

The President communicated a new demonstration of Fourier's theorem.

A letter was read from Professor Holmboe, accompanying his memoir, *De Priscâ Re Monetariâ Norvegiæ*, &c., and requesting to know from the Academy whether any of the coins described in that work are found in Ireland*.

July 12†.—Part I. of a "Memoir on the Dialytic Method of Elimination," by J. J. Sylvester, Esq., A.M., of Trinity College, Dublin, and Professor of Natural Philosophy in University College, London, was read.

The author confines himself in this part to the treatment of two equations, the final and other derivees of which form the subject of investigation.

The author was led to reconsider his former labours in this department of the general theory by finding certain results announced by M. Cauchy in *L'Institut*, March Number of the present year, which flow as obvious and immediate consequences from Mr. Sylvester's own previously published principles and method.

Let there be two equations in x,

$$\begin{aligned} U &= a\,x^{n} + b\,x^{n-1} + c\,x^{n-2} + e\,x^{n-3} + \&c. = 0, \\ V &= \alpha\,x^{m} + \beta\,x^{n-1} + \lambda\,x^{n-2} + \&c. \qquad\quad = 0, \end{aligned}$$

and let $n = m + \iota$, where ι is zero or any positive value (as may be).

Let any such quantities as x^{r} U, x^{e} V, be termed augmentatives of U or V.

To obtain the derivee of a degree s units lower than V, we must join s augmentatives of U with $s + \iota$ of V. Then out of $2\,s + \iota$ equations

$$\begin{aligned} &x^{0}\,.\,U = 0, \quad x^{1}\,.\,U = 0, \quad x^{2}\,.\,U = 0, \ldots\ldots\, x^{s-1}\,.\,U = 0, \\ &x^{0}\,.\,V = 0, \quad x^{1}\,.\,V = 0, \quad x^{2}\,.\,V = 0, \ldots\ldots\, x^{\iota+s-1}\,.\,V = 0, \end{aligned}$$

we may eliminate linearly $2\,s + \iota - 1$ quantities.

Now these equations contain no power of x higher than $m + \iota + s - 1$; accordingly, all powers of x, superior to $m - s$, may be eliminated, and the derivee of the degree $(m - s)$ obtained in its prime form.

Thus to obtain the final derivee (which is the derivee of the degree zero), we take m augmentatives of U with n of V, and eliminate $(m + n - 1)$ quantities, namely,

$$x,\ x^{2},\ x^{3},\ \ldots\ldots \text{ up to } x^{m+n-1}.$$

---

[*] The Committee of Antiquities, having been consulted on this point, reported in the negative.

[† An abstract of Prof. Lloyd's paper read on June 14th, will be found in the present volume, p. 395.]

This process, founded upon the dialytic principle, admits of a very simple modification. Let us begin with the case where $\iota = 0$, or $m = n$. Let the augmentatives of U be termed $U_0, U_1, U_2 U_3, \ldots$ and of V, $V_0, V_1, V_2, V_3, \ldots$ the equation themselves being written

$$U = a x^n + b x^{n-1} + c x^{n-2} + \&c.$$
$$V = a' x^n + b' x^{n-1} + c' x^{n-2} + \&c.$$

It will readily be seen that

$$a' . U_0 - a . V_0,$$
$$(b' U_0 - b V_0) + (a' U_1 - a V_1),$$
$$(c' . U_0 - c . V_0) + (b' U_1 - b V_1) + (a' U_2 - a V_2), \&c.$$

will be each linearly independent functions of $x, x^2, \ldots\ldots x^{m-1}$, no *higher* power of x remaining. Whence it follows, that to obtain a derivee of the degree $(m - s)$ in its prime form, we have only to employ the s of those which occur first in order, and amongst them eliminate $x^{m-1}, x^{m-2}, \ldots . x^{m-s+1}$. Thus, to obtain the *final* derivee, we must make use of n, that is, the entire number of them.

Now, let us suppose that ι is not zero, but $m = n - \iota$. The equation V may be conceived to be of n instead of m dimensions, if we write it under the form

$$0 . x^n + 0 . x^{n-1} + 0 . x^{n-2} + \ldots . + 0 . x^{m+1}$$
$$+ \alpha x^m + \beta x^{m-1} + \&c. = 0.$$

and we are able to apply the same method as above; but as the first ι of the coefficients in the equation above written are zero, the first i of the quantities

$$(a' U_0 - a V_0), (b' U_0 - b V_0) + (a' U_1 - a V_1), \&c.$$

may be read simply

$$- a . V_0, \quad - b . V_0 - a V_1, \quad - c V_0 - b V_1 - a V_2, \&c.$$

and evidently their office can be supplied by the simple augmentatives themselves,

$$V_0 = 0, \quad V_1 = 0, \quad V_2 = 0 \ldots . V_{\iota - 1} = 0;$$

and thus ι letters, which otherwise would be *irrelevant*, fall out of the several derivees.

The author then proceeds with remarks upon the general theory of simple equations, and shows how by virtue of that theory his method contains a solution of the identity

$$X_r . U + Y_r . V = D_r;$$

where D_r is a derivee of the rth degree of U and V, and accordingly, X_r of the form

$$\lambda + \mu x + \nu x^2 + \ldots . + \theta x^{m-r-1},$$

and Y_r of the form

$$l + m x + \ldots . + t x^{n-r-1},$$

and accounts *à priori* for the fact of not more than $(n - r)$ simple equations being required for the determination of the $(m + n - 2 r)$ quantities λ, μ, ν, &c. l, m, n, &c., by exhibiting these latter as *known* linear functions of no more than $(n - r)$ unknown quantities left to be determined.

Upon this remarkable relation may be constructed a method well adapted for the expeditious computation of numerical values of the different derivees.

He next, as a point of curiosity, exhibits the values of the secondary functions,

$$a' . U_0 - a V_0,$$
$$b' . U_0 - b V_0 + a' . U_1 - a V_1,$$
$$c' . U_0 - c . V_0 + b' . U_1 - b V_1 + a' . U_2 - a V_2, \text{ \&c.}$$

under the form of symmetric functions of the roots of the equations $U = 0$, $V = 0$, by aid of the theorems developed in the London and Edinburgh Philosophical Magazine, December 1839, and afterwards proceeds to a more close examination of the final derivee resulting from two equations each of the same (any given) degree.

He conceives a number of cubic blocks each of which has two numbers, termed its *characteristics*, inscribed upon one of its faces, upon which the value of such a block (itself called an *element*) depends.

For instance, the value of the *element*, whose *characteristics* are r, s, is the difference between two products: the one of the coefficient rth in order occurring in the polynomial U, by that which comes sth in order in V; the other product is that of the coefficient sth in order of the polynomial V, by that rth in order of U; so that if the degree of each equation be n, there will be altogether $\frac{(n+1)}{2}$ such elements.

The blocks are formed into squares or flats (*plafonds*) of which the number is $\frac{n}{2}$ or $\frac{n+1}{2}$, according as n is even or odd. The first of these contains n blanks in a side, the next $(n-2)$, the next $(n - 4)$, till finally we reach a square of four blocks or of one, according as n is even or odd. These flats are laid upon one another so as to form a regularly ascending pyramid, of which the two diagonal planes are termed the planes of separation and symmetry respectively. The former divides the pyramid into two halves, such that no element on the one side of it is the same as that of any block in the other. The plane of symmetry, as the name denotes, divides the pyramid into two exactly *similar* parts; it being a rule, that *all elements lying in any given line of a square (plafond) parallel to the plane of separation are identical*; moreover, the sum of the characteristics is the same, for *all* elements lying *anywhere* in a *plane* parallel to that of separation.

All the terms in the final derivee are made up by multiplying n elements of the pile together, under the sole restriction, that no two or more terms of the said product shall lie in any one plane out of the two *sets* of planes perpendicular to the sides of the squares. The *sign* of any such product is determined by the places of either set of planes parallel to a side of the squares and to one another, in which the elements composing it may be conceived to lie.

The author then enters into a disquisition relating to the *number* of terms which will appear in the final derivee, and concludes this first part with the statement of two general canons, each of which affords as many tests for determining whether a prepared combination of coefficients can enter into the final derivee of any number of equations as there are units in that number, but so connected as together only to afford double that number, *less* one of independent conditions.

The first of these canons refers simply to the number of letters drawn out of *each of the given equations* (supposed homogeneous); the second to what he proposes to call the *weight* of every term in the derivee in respect to *each of the variables* which are to be eliminated.

The author subjoins, for the purpose of conveying a more accurate conception of his Pyramid of derivation, examples of the mode in which it is constructed.

When $n = 1$ there is one flat, viz.

1, 2

When $n = 2$ there is one flat, viz.

2, 3	2, 4
2, 4	3, 4

Let $n = 3$, there will be two flats:

2, 3

1, 2	1, 3	1, 4
1, 3	1, 4	2, 4
1, 4	2, 4	3, 4

Let $n = 4$, there will still be two flats only:

2, 3	2, 4
2, 4	3, 4

1, 2	1, 3	1, 4	1, 5
1, 3	1, 4	1, 5	2, 5
1, 4	1, 5	2, 5	3, 5
1, 5	2, 5	3, 5	4, 5

Let $n = 5$, there will be three flats:

3, 4

2, 3	2, 4	2, 5
2, 4	2, 5	3, 5
2, 5	3, 5	4, 5

1, 2	1, 3	1, 4	1, 5	1, 6
1, 3	1, 4	1, 5	1, 6	2, 6
1, 4	1, 5	1, 6	2, 6	3, 6
1, 5	1, 6	2, 6	3, 6	4, 6
1, 6	2, 6	3, 6	4, 6	5, 6

Let $n = 6$, there will be three flats:

3, 4	3, 5
3, 5	4, 5

2, 3	2, 4	2, 5	2, 6
2, 4	2, 5	2, 6	3, 6
2, 5	2, 6	3, 6	4, 6
2, 6	3, 6	4, 6	5, 6

1, 2	1, 3	1, 4	1, 5	1, 6	1, 7
1, 3	1, 4	1, 5	1, 6	1, 7	2, 7
1, 4	1, 5	1, 6	1, 7	2, 7	3, 7
1, 5	1, 6	1, 7	2, 7	3, 7	4, 7
1, 6	1, 7	2, 7	3, 7	4, 7	5, 7
1, 7	2, 7	3, 7	4, 7	5, 7	6, 7

Thus the work of computation reduces itself merely to calculating $n \cdot \frac{n+1}{2}$ elements, or the $n(n+1)$ cross-products out of which they are constituted, and combining them factorially after that law of the pyramid, to which allusion has been already made.

GEOLOGICAL SOCIETY.

[Continued from p. 378.]

Dec. 15, 1841. A paper "On the Glacia-diluvial Phænomena in Snowdonia and the adjacent parts of North Wâles," by the Rev. Prof. Buckland, D.D., F.G.S., &c. was first read.

A paper was afterwards read, "On the occurrence of the Bristol Bone-Bed in the Lower Lias near Tewkesbury," by Hugh Edwin Strickland, Esq., F.G.S.

After alluding to the occurrence of the bone-bed at various places between Westbury and Watchett, also at Golden Cliff and St. Hilary in Glamorganshire, and at Axmouth, Mr. Strickland proceeds to describe its characters at three newly-discovered localities, many miles to the north of the points previously known, namely, Coomb Hill, between Tewkesbury and Gloucester, Wainlode Cliff, and Bushley.

1. *Coomb Hill, four miles south of Tewkesbury**.—In lowering the road through the lias escarpment during the summer of 1841 a considerable surface of the bone-bed was exposed, and its contents were rescued from destruction by Mr. Dudfield of Tewkesbury. The following section is given by Mr. Strickland:—

	Ft.	in.
1. Yellow clay	2	0
2. Lias limestone	0	3
3. Yellow clay	5	0
4. Nodules of lias limestone	0	6
5. Brown clay	14	0
6. Impure pyritic limestone with Pectens and small bivalves	0	6
7. Black laminated clay	8	0
8. Hard, grey pyritic limestone	0	2
9. Black laminated clay	1	0
10. Greyish sandstone	0	2
11. Black laminated clay	1	6
12. *Bone-bed*	0	1
13. Black laminated clay	3	6
14. Compact, angular, greenish marl	25	0
15. Red marl	3	0
Dip about 12° east.	64	8

The bone-bed, No. 12, rarely exceeds one inch in thickness, and frequently thins out to less than a quarter of an inch. It consists in some places chiefly of scales, teeth and bones of fishes, and small coprolites cemented by iron pyrites, but in others the organic remains are rare, and are replaced by a whitish micaceous sandstone. The osseous fragments, Mr. Strickland states, have the appearance of having been washed into the hollows of a rippled surface of clay,

---

* Mr. Murchison has noticed the section formerly exposed in this escarpment, but at the time he examined the district, Mr. Strickland says, the banks were obscured by debris, and the bone-bed did not attract his attention. See Mr. Murchison's Account of the Geology of Cheltenham, p. 24, plate, fig. 1, and Silurian System, pp. 20, 29, pl. 29, fig. 1.

and of having been subjected to slight mechanical action. The existence of gentle currents is further proved, he says, by the presence of small rounded pebbles of white quartz, a substance of very rare occurrence in the liassic series. The only shell found in the bed at Coomb Hill is a smooth bivalve, but too imperfect to be generically determined.

2. *Wainlode Cliff, three miles west-south-west from Coomb Hill.*—The section exposed at this locality has been laid open by the action of the Severn, and consists of the following beds:—

	Ft.	in.
1. Black laminated clay, inclosing, near the top, a band of lias limestone with Ostreæ	22	0
2. Slaty calcareous sandstone, with a peculiar small species of Pecten	0	4
3. Black laminated clay	9	0
4. *Bone-bed,* passing into white sandstone	0	3
5. Black laminated clay	2	0
6. Light green angular marl	23	0
7. Red marls, with zones of a greenish colour	42	0
Dip very slight to the south.	98	7

The bone-bed is far less rich in organic remains, accumulations of fragments of bones and coprolites occurring at rare intervals; and its prevailing character is that of a fissile, white, micaceous sandstone, sometimes acquiring a flinty hardness. The upper surface of the bed is ripple-marked, and in some cases presents impressions considered by Mr. Strickland to have been probably made by the claws of crustacea. A small bivalve is also the only shell found in the bed. The stratum No. 2, the author says, is evidently a continuation of No. 6. of the Coomb Hill section.

3. *Bushley, two miles and a half west of Tewkesbury.*—The intersection of the lias escarpment by the Ledbury road near Bushley afforded Mr. Strickland the following section:—

	Ft.	in.
1. Black laminated clay, about	10	0
2. Lias limestone	0	4
3. Black laminated clay	6	0
4. Compact slaty bed with numerous small bivalves, and the Pecten of Wainlode and Coomb Hill	0	3
5. Black laminated clay	9	0
6. *White micaceous sandstone,* with impressions of two species of bivalve shells	1	0
7. Black laminated clay	2	6
8. Greenish marl, about	20	0
9. Red marl	—	–
Dip about 8° east.	49	1

The sandstone bed, No. 6, agreeing precisely with that at Wainlode Cliff, Mr. Strickland does not hesitate to consider it the representative of the bone-bed, though organic remains are wanting; and he points out the identity of the stratum No. 4. with the beds Nos.

2. and 6. of the preceding sections. The author also refers to the railway section near Droitwich*, and identifies with the bone-bed the two-feet band of white micaceous sandstone six feet above the top of the green marl, as it contains the same indeterminable small bivalve. He has also examined sections of the lias escarpment at Norton near Kempsey, and Cracombe Hill near Evesham, and has invariably detected, a few feet above the base of the lias clay, a thin band of white sandstone containing the same shell.

The bone-bed at Axmouth, Watchett, Aust, Westbury, and other southern localities, occupies precisely the same geological position, or a few feet above the top of the greenish marls which terminate the New Red system, though much more rich in organic remains; and Mr. Strickland draws attention to this remarkable instance of a very thin stratum ranging over a distance of about 112 miles.

The great abundance of fossils in some parts of this stratum the author considers an indication that a much longer period probably elapsed during its deposition, either on account of the clearness of the water or of a gentle current which prevented the precipitation of muddy particles, than while an equal thickness of the less fossiliferous clays above or below it was accumulated.

The list of organic remains given in the paper includes scales of *Gyrolepis tenuistriatus*? and *Amblyurus*; teeth of *Saurichthys apicalis*, *Acrodus minimus*, *Hybodus minor*, *Pycnodus*?; others bearing an analogy to those of Sargus; portion of a tooth with two finely serrated edges, and considered as probably belonging to a saurian allied to the genus *Palæosaurus*; a tooth of *Hybodus De la Bechei* (*H. medius*, Ag.), a ray of *Nemacanthus monilifer*; small vertebra of a fish; bones of an Ichthyosaurus; coprolites; and the casts of the bivalve before mentioned.

Mr. Strickland next alludes to Sir Philip Egerton's paper on the Ichthyolites of the bone-bed†, and he states that the bed cannot be of the age of the muschelkalk, as it overlies the red and green marls, which he considers to have been satisfactorily shown to be equivalent to the Keuper sandstein of Germany; and that the occurrence of muschelkalk fishes associated with lias Ichthyolites only justifies the inference that certain species survived from the period of the muschelkalk to that of the bone-bed. There are yet stronger grounds, Mr. Strickland states, for placing the bone-bed in the liassic series in the remarkable change a few feet below it, from black laminated clay to compact "angular" marl, greenish in the upper part and red below; and he adds, the transition is so sudden that it may be defined within the eighth of an inch; moreover no marl occurs above the line nor black laminated clay below it; and although, in the case of the bone bed, an arenaceous deposit similar to the Keuper sandstein is repeated, accompanied by some triassic organic remains, yet, the author adds, this does not invalidate the evidence of the commencement of a new order of things, or of an interesting passage into the liassic series from the triassic system.

[* Phil. Mag. S. 3., vol. xviii., p. 523.]

[† Ib. vol. xix., p. 522.]

Lastly, Mr. Strickland notices the occurrence of precisely analogous bone-beds in the Upper Ludlow rock, described by Mr. Murchison in the 'Silurian System' (p. 198), and in Caldy Island, near the junction of the carboniferous limestone with the old red sandstone; and he offers some remarks on the bone-beds being found in all the three cases near the passage from one great geological system of rocks to another.

January 5, 1842.—"A Notice on the Fossil Bones found on the surface of a raised Beach at the Hoe near Plymouth," by Edward Moore, M.D., F.L.S., was first read.

At the Meeting of the British Association at Plymouth, Dr. Moore read a paper on the same subject as that which forms part of the present communication*. In this notice he first alludes to the discovery of the beach by the Rev. R. Hennah in 1827†, and to Mr. De la Beche's account of numerous anciently raised beaches in Devon and Cornwall‡; he then briefly describes the characters of the beach, its position in a hollow in the limestone rock, 100 feet wide, 70 feet deep, and, at its base, 35 feet above the present high water mark. He also notices a projecting ledge of limestone stretching several hundred feet southward from this spot, and which sustained a mass of sand, with rolled pebbles and blocks, some of them two or three feet in circumference, and forming a hill twenty to twenty-five feet high, containing patches of loose sand with fragments of *Patella* and *Buccinum*. It was, says the author, easily traced by several patches along the rocks, and proved, by its structure and contents, to be a continuation of the same beach. Dr. Moore likewise briefly describes another deposit 100 yards westward of the beach, and at a greater elevation, being 88 feet above high water, 50 feet in extent, and 10 in thickness, covered irregularly by soil.

The animal remains more particularly enumerated by Dr. Moore consist of a molar and part of the jaw of a young elephant; a femur of a rhinoceros; maxillary bones of a bear, with the malar and palatine processes, and two teeth in each; an entire right lower ramus with teeth and tusks, the latter much worn; four separate tusks; several fragments of long bones; fragments of jaws of the horse containing teeth, numerous loose teeth, portions of long bones, and two caudal vertebræ; likewise portions of a deer's jaw containing teeth. The quantity of the bones which has been found is stated to be equal to several bushels. The vertebræ of a whale, much rounded, were also discovered, with undeterminable portions of ribs. The animals to which the above remains belonged, are considered by Dr. Moore to have coexisted with those which inhabited the caves of Devonshire.

The author then enters upon a defence of the opinions contained

---

* Athenæum, No. 721, and the volume of Reports of the British Association for 1841, Trans. of the Sections, p. 62 (published 1842).

† See also "A Succinct Account of the Lime Rocks of Plymouth," by the Rev. R. Hennah, 1822, p. 58.

‡ Manual of Geology, 3rd Edition, p. 173, 1833; also Report on the Geology of Cornwall and Devon, p. 423, 1839.

in his paper read at Plymouth, respecting the mode of accumulation of the bones. He states that these osseous remains cannot have been derived from the emptying of some cave, because the mass of superincumbent matter which has been removed from above the beach proves that the bones must have been deposited where they were found at a very ancient period, and long before they could have been affected by human agency. There are also no known caves containing bones sufficiently near. On the contrary, says Dr. Moore, if the sea was at one time at the level indicated by the beach, the Hoe must have been an island accessible by animals at low water, and there appears no obstacle to the supposition that the bears might have selected the beach to devour their prey; and the stranded whale may have added to the banquet. Whether the bones were drifted or not, their occurrence on the top of the beach, and not in it, prevents, the author says, any identity of time in their origin; but that the beach previously existed, and was of marine origin, is proved by the resemblance of the deposit to a modern beach, and its containing sea-shells of the existing period, although few in number.

That the deposit is not the result of glacial action, the author observes, is probable from the want of any indication of such action in the neighbouring district; and though he does not presume to assert that this may not be a cause of drift generally, and even of the upper deposit in the same locality, yet he contends that the dissimilarity in the composition of the lower deposit sustains him in the supposition of its being of different origin, and really a deposit from the sea. Lastly, Dr. Moore, in reference to the present position of the beach far above any point attained by the sea during the greatest storms, states that the deposit must have been elevated by natural causes; and that, however uncertain the exact period of such an event, it seems to have occurred at a time probably more recent than the epoch when the extinct animals disappeared.

Appended to the paper, is a notice of a specimen of perforated limestone taken from the Hoe Lake quarries, eighty-five feet above the present level of high water, and Dr. Moore maintains his belief that the perforations were formed by Pholades, and not by snails.

A paper was next read, entitled "An Account of the Contortions and Faults produced in the Strata underneath and adjacent to the great Embankment across the Valley of the Brent, on the Great Western Railway," by J. Colthurst, Esq.; communicated by George Bellas Greenough, Esq., F.G.S.

The author was induced to lay this paper before the Society, because he conceives, that, in the phænomena exhibited by the subsidence in the Brent embankment, there may be found the cause of many of the contortions, faults and dislocations of strata, especially among sedimentary rocks, and which are commonly attributed to the agency of forces acting from below rather than to pressure from without.

The embankment is fifty-four feet in height, and rests on vegetable soil, beneath which are four feet of alluvial clay; then occurs a bed of gravel varying from ten to three feet in thickness, but which thins out in some places, and under it is the regular London clay, traversed in almost every direction by slimy joints. The surface of the country gradually slopes towards the Brent, the difference of level between the south side of the embankment and the Brent being about twenty feet.

On the night of the 21st of May 1837 the embankment began to settle, and in the morning it was found that the foundation had given way, and that on the south side, or towards the Brent, a mass of ground, fifty feet long and fifteen feet wide, had protruded from under the earthwork. During the four succeeding months this mass continued to increase in dimensions, and the disturbance to extend, so that the surface, for a considerable distance from the base of the embankment, had assumed an undulated outline, and the subjacent beds, where cut into, exhibited corresponding curvatures, overlappings and cracks, the whole of which are described in the memoir, but cannot be rendered intelligible without diagrams. In the embankment itself the symptoms of failure were confined to a settlement of about fifteen feet, and a large fissure near the top, on the side opposite to that where the foundation had yielded, and which extended the whole length of the slip. To this fissure, and its dip towards the disturbance at the base of the embankment, the author particularly directs attention, as he infers from it the nature and inclination of a fault exhibited in the diagrams which illustrate the memoir.

At the end of twelve additional months, during which the embankment continued to slip, and the disturbance at the base to increase, Mr. Brunel directed a supplementary earthwork or terrace to be thrown down upon the swollen surface, and it was an effectual remedy. Up to this time the total subsidence had exceeded thirty feet; and the swollen ground, which extended nearly 400 feet in length, and from seventy to eighty feet in width, had attained an average height of ten feet, with a horizontal motion of fifteen feet; but the general disturbance ranged to a distance of 220 feet from the foot of the slope, or to the Brent, the bank of which was forced five feet forwards: the faults varied from thirty feet to two feet, and the contortions had attained a curvature, the semi-axis of which was in many places eight feet.

The author then dwells on the magnitude of the disturbance, and on the effects which may have been produced in the strata composing the earth's surface, by pressure from above. He says, that in consequence of the great inequality in the thickness of the sedimentary rocks, due to the conditions under which they were deposited, great inequality of pressure must have arisen, and consequently contortions and faults have been produced, varying in amount according to the thickness and the degree of consolidation in the strata themselves. In support of his argument, the author quotes a passage contained in Mr. Greenough's 'Critical Examination of the Principles of Geology,' and which asks the question whether contortions may not have taken place where clay alternates with limestone or silex, in consequence of an unequal rate of consolidation (p. 77). The author also alludes to the theory of Sir James Hall, but chiefly to prevent its being "mixed up in any way with the subject of this paper, or the inferences it contains;" and lastly, he wishes it may be clearly understood, that while he advocates the explanation of many geological phænomena by means of pressure from without, he does not propose that all geological disturbance should be attributed to it; nor does he deny that many, and more especially the most considerable, irregularities in the structure of the earth may and must be assigned to other causes.

"Notice on the occurrence of Plants in the Plastic Clay of the Hampshire Coast," by the Rev. P. B. Brodie, F.G.S., was then read.

The cliffs to the east and west of Bournemouth are composed of horizontal strata belonging to the plastic clay formation. East of the town they consist of white and yellow sands, the former containing fragments of wood. Further along the shore the cliffs are higher, and beds of clay full of vegetable remains appear under the sands. About half a mile beyond, a stratum of fine white sand, three or four feet thick, situated near the middle of the cliffs, contains impressions of ferns; and a layer of sand and clay is full of small leaves. The subjacent strata of clay are separated by thin layers of vegetable matter. Somewhat further, beds of white and yellow sand and sandy clay abound with beautiful leaves, and the surface of the strata is in some places covered with a thin layer of iron-sand containing impressions of ferns. In most cases, the various-coloured sands are divided by beds of clay, and their fossil contents are distributed in layers at rather distant intervals. Mr. Brodie did not discover any shells. Several of the fossil plants are stated by the author to belong to the *Lauraceæ* and *Amentaceæ*; but he says that these, as well as others which he arranges among the *Characeæ* and Cryptogami, and some of which he has not determined the characters, are all generically distinct from any British plant, and belong to those of a warmer climate. When the sandstone is freshly broken the epidermis of the fossil frequently peels off, leaving the impression of only the fibres. These remains often form masses of some thickness; and, from their state of preservation, must, the author states, have been deposited tranquilly beneath the waters.

A paper "On the Mouths of Ammonites, and on Fossils contained in laminated beds of the Oxford Clay, discovered in cutting the Great Western Railway, near Christian Malford in Wiltshire." By J. Chaning Pearce, Esq., F.G.S., was lastly read.

Mr. Pearce commences by stating, that his attention was first directed to this part of the railway by the impression of a crushed Ammonite procured at Cheltenham in April 1841, but that he was prevented from examining the locality for three or four months.

The following section of the beds is given by Mr. Pearce:—

1.	Alluvial soil	2 feet.
2.	Gravel	8 ...
3.	Four or five bands of laminated clay, alternating with sandy clay, almost entirely composed of broken shells	6 ...
4.	Clay, containing *Gryphæa bilobata*.	

The objects of the author are, first, to draw attention to the organic bodies discovered in the laminated clay; and secondly, to describe the various forms which the mouth of the Ammonite assumes in different species and in different stages of growth in the same species.

The fossils obtained from the laminated clay are stated to be as follows:—1. A succulent plant. 2. Lignite, with oysters sometimes affixed to it. 3. Crustaceans, supposed to have inhabited the dead shell of the Ammonite*. The specimen described is stated to have a finely tuberculated and delicately thin covering; the tail to have the appearance of being divided into three portions, finely corrugated towards their edges; the body to have on each side internally five or more processes; and the head to be furnished with several short arms and two long ones jointed a little above the head and terminated in two claws, the longer being serrated on its inner edge. 4. Another allied crustacean is stated to have also an extremely thin and finely tuberculated covering; to be furnished with two long arms of similar shape, each terminated at its extremity by one claw, and two others projecting from about the centre; and passing off posteriorly are two fan-like processes of similar shape. 5. Trigonellites, two species. 6. One valve of a Pollicipes. 7. The remains of an animal considered to have been probably allied to a Sepia. 8. Shells of the genera Unio, Cyclas, Astarte, Avicula, Gervilla, Pinna, Nucula, Rostellaria, Turritella, Ammonites†, Belemnites, and an animal to which he has applied (since the paper was read) the name of Belemnotheutis. In describing the last fossil, he states that the lower part is conical, blunt at the apex, and chambered internally like the alveolus of a Belemnite, with an oval siphunculus near the edge of the chambers; that it has a brown thick shelly covering which gradually becomes thinner towards the superior part; that immediately above the chambers is an ink-bag resting on what resembles the upper part of a sepiostaire, and composed of a yellow substance finely striated transversely, being formed of laminæ of unequal density; that in some specimens, broken longitudinally through the middle, are exposed long, flat, narrow processes of a different structure; that immediately beneath the superior contraction are two long feather-like processes, and one or more which are short, indicating, the author thinks, probably the situation of the mouth. With reference to the first part of the paper, Mr. Pearce also notices an animal allied to Sepia or Loligo, one side being covered by a pen resembling that of the Loligo, and having immediately underneath it, at the junction of the middle with the lower third, an ink-bag

---

* To this organic body Mr. Pearce has given since the paper was read the name of Ammonicolax.

† Since the paper was written Mr. Pearce has consulted Mr. Pratt's account in the Annals of Natural History for November 1841, of Oxford clay Ammonites, and ascertained that he possesses [*A. Lonsdalii*, *A. Brightii*], [*A. Gulielmi*, *A. Elizabetheæ*], *A. Comptoni*, and *A. Königii*. The fossils included between brackets the author considers to belong to one species.

resting on what resembles a sepiostaire. He mentions likewise ten or twelve species of fishes, but without giving names; also coprolites.

2. Respecting the form of the mouth of the Ammonites and the changes at different periods of growth, Mr. Pearce states his belief, that the terminal lip or mouth has a different shape in the young shell of almost every species, but assumes in the old a straight outline, and that he has been aware of this circumstance several years. Of cases of young shells with differently shaped lips, he mentions *Ammonites Brongniarti* (Inf. oolite), *A. sublævis* (Oxf. clay), *A. obtusus* (Lias), *A. Kœnigii* (Kelloway Rock, the mature shell is stated to have a straight mouth), *A. Calloviensis* (Kelloway Rock, the lip of the old shell is stated to be slightly contracted and to terminate with gently undulating sides), *A. Walcottii* (Lias), and *A. Goodhalli*, furnished in the mature state with a single horn-like projection at the front of the mouth. In addition to these species he enumerates those noticed in the preceding part of the paper. Mr. Pearce is further of opinion that at different periods of the formation of the shell the lateral processes were absorbed and reproduced, and that therefore they are found in various stages of growth, but are invariably wanting in the mature shell. In some species in which the successive mouths were much contracted or expanded, the new shell the author says was continued without the absorption of the lip, leaving a highly projecting rib or a deep furrow*.

After a careful examination of upwards of twenty species in his collection, with perfect mouths of all ages and from different strata, not including the Oxford clay, Mr. Pearce has found the external chamber to vary considerably in extent, occupying in some specimens the whole of the last whorl, but in others less than one-third, and without reference to age or species; and he therefore suggests that the young animal of the Ammonite filled the whole of the outer chamber, extending also to the extreme points of the lateral processes in those species which were provided with them; and thereby not only received support but afforded protection to a portion of the shell extremely liable to injury. In old individuals he is of opinion that the animal when quiescent was entirely contained within the last chamber.

Jan. 19th.—"A Memoir on the Recession of the Falls of Niagara," by Charles Lyell, Esq., V.P.G.S., was read.

The general features of the physical geography of the district traversed by the Niagara between Lakes Erie and Ontario, Mr. Lyell says, have been described with a considerable approach to accuracy by several writers. Prof. Eaton, in a small work published in 1824†, gives a correct section of the formations between Lewistown and the Falls of Niagara, and also refutes the hypothesis of the Lewistown escarpment being due to a fault by an exposition of the true

---

* The author was not acquainted with M. Al. d'Orbigny's work, *Pal. Francaise*, when he wrote the paper, and was not aware of the views given in it respecting the mouth of the Ammonite.

† Mr. Lyell's attention was called to this work by Mr. Conrad.

structure of the country. Mr. R. Bakewell in 1830*, published an account of the country adjacent to the Falls, and Mr. De la Beche in 1831†, endeavoured to point out the gradual manner in which the receding Falls, if they should ever reach Lake Erie, would discharge the waters of the lake; Prof. D. Rogers also in 1835‡ showed distinctly, that, as the Falls retrograde, they would cut through rocks entirely distinct from those over which the waters are now precipitated, and correctly represents the superior limestone at Buffalo as newer than the limestone of the Falls, though he omits the intervening saliferous formation. Mr. Conrad likewise, in his Report for 1837§, first assigned all the formations of the country to the Silurian system; but to Mr. James Hall (1838)‖ is due the merit of having shown the true geological succession of rocks of the district.

The contents of the memoir may be divided into two parts: I. an account of the successive strata of the Niagara district; and II. a description of the phænomena exhibited by the Falls.

I. His sketch of the geology of the district, the author states, is derived either from the published surveys of Mr. Hall, or from the information he obtained while travelling with that gentleman in the State of New York during the autumn of 1841; and he acknowledges the great advantage he derived from the facilities thus afforded him. The strata between Lakes Erie and Ontario appear to belong to the middle and lower portions of the English Silurian system, and they are divisible into the following five principal formations: 1st. the Helderberg limestone; 2nd, the Onondago salt group; 3rd, the Niagara group; 4th, the Protean group; and 5th, the Ontario group,

1. *The Helderberg limestone,* which has derived its designation from the range of mountains of the same name, and is the newest formation of the country, is exposed where the Niagara flows out of Lake Erie, and on account of the organic remains with which it abounds, it is considered to be the equivalent of the Wenlock rocks of Mr. Murchison's Silurian system. The correctness of this stratigraphical position Mr. Lyell has verified by an examination of the succession of formations from the coal-field on the borders of Pennsylvania to the group in question, the intervening deposits consisting, first, of old red sandstone, having at its bottom a large development of shales and sandstones called the Chemung and Ithaca formations, but containing organic remains which resemble those of the Devonian system; and then 1000 feet of Ludlowville shales with fossils analogous to those of the Ludlow rocks of Mr. Murchison. The superposition of this vast horizontal series is beautifully exposed in the banks of the Genessee and other rivers; and near Le Roy as well as elsewhere, the Helderberg limestones crop out from beneath them. On account of the middle portion containing nodules

---

* Loudon's Magazine of Natural History, 1830.

† Manual of Geology, three editions, 1831, p. 55; 1832, p. 55; 1833, p. 60.

‡ Silliman's Journal, vol. xxvii. p. 326.

§ States' Report of the Geology of New York.

‖ Geological Report of the State of New York for 1838.

and layers of chert, the whole deposit was first called the corniferous formation by Prof. Eaton. In this part of the State of New York, and still further to the west, in Upper Canada, the limestone is only 50 feet thick, whereas at Schoharie in the Helderberg mountains, 300 miles to the eastward, its thickness is 300 feet.

2. *The Onondago salt group.*—This series of beds, Mr. Lyell says, is extremely unlike any described member of the European Silurian group. With the exception of a stratum of limestone at the top containing Cytherina, it consists of red and green marls with beds of gypsum, the former being undistinguishable from the marls of the new red system of England; and they are also destitute of fossils. Salt springs are of frequent occurrence, but no rock salt has been discovered in the group. The breadth of the zone of country occupied by the deposit is not less than 16 miles, and Mr. Hall infers from it and the slight southerly dip of the strata, that the entire thickness in the neighbourhood of the Niagara is at least 800 feet, an estimate confirmed by the nearest sections eastward of the river. In some parts of the State of New York the thickness is not less than 1000 feet. Along the Niagara the formation has been greatly denuded, and is covered by superficial drift, except at a few places.

3. *The Niagara group.*—This series of beds commences near the rapids, above the great cataract. It comprises, 1st, the Niagara, or Lockport limestone, and 2ndly, the Niagara, or Rochester shale; and it contains in both divisions fossils identical with those of the Wenlock limestone of England, with others peculiar to North America. The limestone at the rapids and the Falls is 120 feet thick; the upper 40 feet, being thin-bedded, have given way to the frost and the action of the stream; but the lower 80 feet, being massive, forms at the cataract a precipice, beneath which occurs the shale, also 80 feet thick.

4. *The Protean group.*—Under the water at the base of the Falls crop out the higher beds of this formation, the name of which has been derived from the variable nature of its component strata. In the district more particularly described in this paper the group is only 30 feet thick, but farther to the eastward it attains thrice those dimensions. On the Niagara it consists of 25 feet of hard limestone, resting on 4 feet of shale; while at Rochester, eighty miles to the eastward, it comprises, among other beds, a dark shale with graptolites, or fossiliferous iron ore, and beneath them a limestone full of *Pentamerus oblongus* and *P. lævis*, considered by Mr. Conrad to be one species. On account of the occurrence of this shell, the whole of these strata have been separated from the Niagara series.

5. *Ontario group.*—About half a mile below the Falls the uppermost beds of the Ontario group crop out. At the whirlpool they have a thickness of 70 feet, and at Queenstown of 200, but to the latter dimension must be added 150 feet of inferior beds, exposed between Queenstown and Lake Ontario. The entire group consists of

1. Red marl with beds of hard sandstone in its upper division .... } 70 feet.

2. White quartzose strata, so hard as to form at Queenstown a ledge projecting beyond the face of the escarpment	25 feet
3. Red marl and sandstone	250 ...

Other divisions of the group, concealed beneath the waters of the lake, may be studied in the cliffs of its eastern and north-eastern shores.

Mr. Lyell next proceeds to give a brief account of the geographical distribution of the formations or groups. The strike of the beds being east and west, and the dip very slight towards the south, the sections exposed along the Niagara afford a key to the structure of a large portion of the State of New York, the same deposits having been traced eastward through a region 40 miles in breadth by 150 in length, and westward to a much greater distance. The Helderberg and the Niagara limestones constitute platforms which terminate in parallel escarpments, from twenty to twenty-five miles apart, about sixteen miles of the intervening space being occupied by the saliferous group. The Helderberg escarpment, to the east of Buffalo, is 50 feet high; but in the neighbourhood of the Niagara it has been denuded and is half buried beneath drift; it is however resumed in Upper Canada, and eastward it may be followed to the river Hudson. The Niagara limestone escarpment presents at Lewistown and Queenstown a cliff 300 feet high, which may be traced eastward nearly 100 miles and westward for a much greater distance. The limestone series, however, constitutes only the uppermost third of the escarpment, the remainder being composed of the Protean and the Ontario groups; the whole section being as follows:—

1. Niagara limestone, lower beds	30 feet.
2. Niagara, or Rochester shale	80 ...
3. Protean beds	30 ...
4. Ontario group: red marl, with hard beds in the upper part	70 ...
5. ————: quartzose grey sandstone, with Lingulæ, &c.	25 ...
6. ————: red marl	100 ...
	335 feet.

Though only the lower beds of the Niagara limestone occur in the escarpment at Lewistown, yet, in consequence of the gentle rise of the strata to the north, the summit of these lower beds is at a higher level than that of Lake Erie. The whole of the Niagara platform is covered irregularly with hillocks of drift, beneath which the limestone is polished and furrowed.

From the foot of the Queenstown escarpment to Lake Ontario, a distance of six or seven miles, is a low tract, consisting of sandstones belonging to the Ontario group, and dipping like the preceding beds slightly to the south.

A section which accompanied the memoir to illustrate the preceding details corresponds, the author says, in all essential particulars with one previously published by Mr. Hall; but the whole succession of beds has been verified by Mr. Lyell in more than one line of section, from north to south. He is induced to believe, from a comparison of English Caradoc and Llandeilo fossils with suites of organic remains examined in America, that a series of beds which underlie the Ontario group, and termed by American geologists the Mohawk group, may be older than the lower Silurian rocks, and wanting in England.

II. *On the Recession of the Falls.*—The following measurements, Mr. Lyell says, are of great importance in speculating on the past or future recession of the Falls. The distance from the point where the Niagara flows out of Lake Erie to the Falls is sixteen miles, thence to the limestone escarpment seven miles, and from this point to Lake Ontario about seven more. From Lake Erie to the commencement of the rapids, fifteen miles and a half, the river falls only 15 feet; but from the top of the rapids to the great cataract the descent is 45 feet; and the height of the Falls is 164 feet, perpendicular. From the base of the Falls to Queenstown, seven miles, the difference of level in the river is about 100 feet; but from that place to Lake Ontario, seven miles further, it is only 3 or 4 feet. If the Falls were ever at Queenstown, they must, the author observes, have been about twice their present height, having lost a small portion of the difference by the southern inclination of the strata, and rather more than 100 feet by the rise of the bed of the river.

With respect to the opinion of the Queenstown escarpment being due to a fault, Mr. Lyell states, that the strata on the banks of the Niagara, both above and below Queenstown, presenting the same relative position as at Lockport or Rochester, the escarpment must be entirely due to denudation; and he has no hesitation in attributing this escarpment, as well as the Helderberg, to the action of the sea; these great inland cliffs having far too great a range to have resulted from a former extension and higher altitude of Lake Ontario.

The next question, whether the ravine through which the Niagara flows is to be regarded as a prolongation of the Queenstown escarpment and referable to the same period, or has been cut through by the river, is, the author states, of greater difficulty. From his own observations, he concludes that the ravine has been formed by the river; but he assumes, that a shallow valley pre-existed along the line of the present defile, resembling the present one between Lake Erie and the Falls. His reasons for conceiving that the river has been the excavating agent, are, 1st, the ravine being only from 400 to 600 yards wide at the top, and from 200 to 400 at the bottom, between Queenstown and the Whirlpool; 2ndly, the inclination of the bed of the river, $14\frac{1}{2}$ feet per mile, being everywhere cut down to the regular strata; 3rdly, the fact that the Falls are now slowly receding; 4thly, that a freshwater formation, which the author ascribes to the body of water which flowed along the original shallow valley, exists on Goat Island and half a mile lower down the river, and could not have been deposited after the Falls had receded farther back than the Whirlpool. Mr. Lyell considers that the indentation of about two acres on the American side of the Niagara, and not referable to the action of that river, is no objection to the theory of the recession of the Falls, because he conceives that the stream flowing down it could have effected the denudation, aided by atmospheric agents; and because a similar objection might be founded on a ravine on the Canada side opposite the Whirlpool, where several parallel gullies have been deeply eaten into by streams. The characters of this ravine were carefully examined by Mr. Lyell and Mr. Hall, and appear to have escaped previous observers. What was anciently a ravine joins the defile of the Niagara at this point, but it is entirely filled with horizontal beds of drifted pebbles, sand and loam; the first, near the bottom of the deposit, having been cemented into a conglomerate by carbonate of lime. This is the only interruption of the regular strata along the course of the Niagara; and Mr. Lyell observes, it is desirable to ascertain if it be a prolongation of the ravine which intersects the great escarpment at St.David's, west of Lewistown.

The author states, that he is by no means desirous of attaching importance to the precise numerical calculations which have been made respecting the number of yards that the Falls have receded during the last half century, as there are no data on which accurate measurements could be made; and because fifty years ago the district was a wilderness. Mr. Ingrahaw of Boston has, however, called his attention to a work published by the French Missionary, Father Hennipen, in which a view is given of the Falls as they appeared in 1678. Goat Island is represented dividing the waters as at present; but besides the two existing cascades, a third is depicted on the Canada side, crossing the Horse-shoe Fall at right angles, and appears to have been produced by a projection of the Table Rock. In the description Father Hennipen states, that this smaller cascade fell from west to east, and not like the other two, from south to north.

Seventy-three years afterwards, in 1751, a letter on the Falls, by Kalm, the Swedish botanist, was published in the 'Gentleman's Magazine.' It is illustrated by a plate, in which the third Fall is omitted; but the writer states in a note, that at that point the water was formerly forced out of its direct course by a projecting rock, and turned obliquely across the other Fall *.

Mr. Lyell then proceeds to show what are the geological evidences of the former prolongation of the river's bed, on a level with the top of the ravine through which the Niagara now flows. The existence on Goat Island of strata of marl, gravel and sand, containing fossil freshwater shells, was known before Mr. Bakewell's paper on the Falls was published, and they have been more recently described by Mr. Hall †; and Mr. Lyell states, that he was very desirous of ascertaining how far they extend on the banks of the river, or

---

* The author has observed distinct signs of recession in strata of the Silurian and Devonian epochs at the Falls of the Genessee in Rochester and at Portage, at the Fall of Allen's Creek below Le Roy, near the town of Batavia, and at the Falls of Jacock's river, three miles north of Genessee.

† Report for 1838.

whether they could be detected below the present Falls. On the south-west side, in a cliff 12 feet in perpendicular height, a bed of gravel, 7 feet from the surface, contains eight species of fluviatile and one of terrestrial shells, determined for the author by Dr. Gould of Boston, the whole of the former now living in the waters of the Niagara, and some of them even in the rapids. At the south-west extremity of Goat Island this deposit must be 24 feet thick, and it rests on the Niagara limestone. On the right bank of the river, opposite the island, are two river-terraces, one 12 feet above the stream, and the other 12 feet higher; and both have been cut out of this freshwater formation. In making a mill-dam some years ago, the same species of shells as those on Goat Island were thrown out, and Mr. Lyell had still an opportunity of collecting them. He was also shown a tooth of the "*Mastodon Americanus,*" which, with another tooth and a bone of the same animal, were discovered in the deposit 13 feet from the surface. From information given to the author by Mr. Hooker, the guide, the formation was found half a mile farther down the river, at the summit of the lofty precipice, 6 feet deep and composed chiefly of gravel. It contained in abundance *Cyclas rhomboidea, Valvata tricarinata* and *Planorbis parvus*. This patch of gravel demonstrates, therefore, the former position of the river at a level corresponding to that of the present summit of the cataract, and half a mile below the existing Falls. It proves however, Mr. Lyell says, much more; for in order that such a fluviatile deposit should have been accumulated in water tranquil enough to allow those shells to exist, there must have been a barrier farther down; and he is of opinion it may be safely placed as low as the Whirlpool, or three miles from the present Falls. If this be admitted, then, the author says, "we may be prepared to concede that the still narrower ravine beyond the Whirlpool was excavated by the river cutting back its course."

A similar terrace, consisting of the Goat Island deposit, is distinctly seen also on the Canada side, and at about the same level between the Falls and the Whirlpool; but its extent, height and fossil contents have not been investigated.

If, Mr. Lyell observes, the river continue to intersect its way back, the sediment now depositing in its bed, above the Falls, will be laid dry in places, and cut into in the same manner as the Goat Island deposit.

Assuming that the cataract was once at the Queenstown escarpment, allowance must be made, in speculating on the probable time which has elapsed in cutting the ravine, for a very different rate of retrocession at different periods, dependent on the changes in the formation intersected, especially of those which successively constituted the base of the precipice. At Queenstown and Lewistown the fundamental rock, at the period when the Falls were there, was a soft red marl, and the river acted upon the same deposit for about three miles, where the rise in the channel, combined with the dip of the strata, caused the superincumbent hard quartzose beds, 23 feet thick, to form the base of the precipice. From this point the retrocession must have proceeded much more slowly for about a mile, or to the Whirlpool, where a small fall of 6 or 8 feet still marks the place of the highest beds of the sandstone. After, Mr. Lyell says, the cataract had remained nearly stationary for ages at this point, it next receded more rapidly for two miles, having soft red marl 70 feet thick to erode its way through; but beds of greater solidity, consisting of grey and mottled sandstone and Protean limestone, amounting in all to 30 or 40 feet, then offered a greater resistance, and continued to retard the backward movements of the Falls, the Protean limestone occurring at the base of the present precipice.

Lastly, the author offers some observations respecting the future retrocession of the Falls, quoting the opinions entertained by Mr. J. Hall (Report for 1838) on the effects which the strata above the existing cataract will have on the progress of the river, and pointing out results similar to those given by Mr. De la Beche in his 'Manual of Geology.' But all predictions, Mr. Lyell says, regarding the future history of the Falls may be falsified by the disturbing agency of man. Already a small portion of the waters of Lake Erie is carried off to supply the Welland canal, and another canal on the American side of Niagara; and numerous mill-races have been projected and others will be required along both sides of the river, as the population and wealth of the country increase. Many cities also, situated to the eastward of the great escarpment and at a lower level, may in after times borrow water from Lake Erie, especially as the continued felling of the forests causes streams which were formerly constant to become dry in summer; and it must not be forgotten that Lake Michigan has lately been made by a cutting to feed the Illinois river, and that whatever quantity of water is abstracted from the upper lakes is taken away from the Niagara.

Feb. 2nd.—"Sketch of the Geology of the South of Westmoreland." By Daniel Sharpe, Esq., F.G.S.

The object of this communication, the author says, is to describe the Silurian rocks and the old red sandstone of the south of Westmoreland, to define approximatively their geographical boundaries, and to compare their lithological structure and stratigraphical phænomena with the equivalent formations previously noticed in other parts of the kingdom.

The author, in alluding to the published labours of those who preceded him in the same district, mentions the memoir of Mr. J. Phillips on a group of slate rocks between the Lune and Wharf*, Prof. Sedgwick's on the Cumbrian mountains†, Mr. J. G. Marshall's on a section between the Shap granite and Casterton Fell‡, and Prof. Sedgwick's Geological Map of Westmoreland; also the abstract of his memoirs on the English stratified rocks inferior to the old red sandstone§.

---

* Geol. Trans., 2nd Series, vol. iii. part i. p. 1, 1829.

† Ibid, vol. iv. part i. p. 47, 1835.

‡ Proceedings of British Association for 1839.

§ Proceedings, vol. ii. p. 675 [Phil. Mag. S. 3. vol. xiii. p. 299.]; Athenæum, No. 736; Proceedings, vol. iii. p. 541.

The different formations are described under the heads of,—1. Coniston Limestone; 2. Blue Flagstone Rock; 3. Windermere Rocks; 4. Ludlow Rocks; and 5. Old Red Sandstone.

1. *Coniston Limestone.*—This calcareous band, which has been laid down in great detail by Prof. Sedgwick, was adopted by Mr. Sharpe as the base of his inquiries. It usually rests upon dark brown shale, and consists, in its lowest part, of a hard, dark blue, slaty limestone, from fifty to sixty feet thick at Low Wood; and in the upper, of thin beds of dark brown shale, alternating with others of blue limestone, which gradually diminish in thickness, and totally disappear towards the top of the formation. The bottom bed of limestone contains very few organic remains, but the shales and thinner calcareous bands abound with casts. A list of fossils given by the author includes fifteen Silurian species, seven of which belong to the lower Silurian rocks of Mr. Murchison; and the author places the Coniston limestone and associated shales on the parallel of that division of the Silurian system, but without attempting to define its exact relative position. Mr. Marshall, on the authority of Mr. J. Sowerby, places the Coniston limestone on the parallel of the Caradoc limestone. An exact account of the strike and dip of the rock, the author says, will be found in Prof. Sedgwick's memoir, but the general bearing of the strike of the beds throughout the western part of their course is stated to be north-east, though on approaching Shap more nearly east and west; and the ordinary dip is stated to be south-east, with an inclination rarely less than 30°, and frequently exceeding 60°.

2. *Blue Flagstone Rock.*—The shales of the last deposit pass upwards into a dark blue flagstone, the strike of which is parallel to that of the Coniston limestone, and the dip is conformable. It is stated to range from the west of Coniston by the village of Torver, the head of Coniston Lake, also south of the Ambleside road to Low Wray, and thence from the east side of Windermere, by Trout Beck and Kentmere, to the neighbourhood of the Shap granite. The faults which affected the Coniston limestone series extend into this deposit. No organic remains were found by the author, but he is of opinion that their absence may be owing to the rearrangement of the constituent particles of the rock when they assumed the slaty structure.

3. *Windermere Rocks.*—This vast series of beds, to which Mr. Marshall applied the name of Blawith slate, succeeds conformably to the blue flagstone, and is arranged by the author into three groups, which he calls the lowest, middle, and upper divisions. A line drawn from Coniston Water Head to Lindale, a distance of twelve miles, would cross the beds at right angles to the strike; and though the same strata are, according to the author, frequently repeated in a succession of parallel anticlinal ridges, yet he is of opinion that the total thickness of the formation exceeds 5000 feet.

3*a*. *Lowest Division.*—This portion of the Windermere rocks consists of gray schistose grits and argillaceous slates, containing thin beds of limestone on the banks of Coniston Lake. The strata are stated to be much affected by cleavage lines. The usual strike of the beds at the foot of Coniston is said to be north-east, but great variations are shown to occur in other portions of the district, in consequence of anticlinal ridges which range north and south. The boundary between this division and the middle one passes from the foot of Coniston Water to the ferry on Windermere, and thence by the foot of the valley of Kentmere, across Long Sleddale at Murthwaite Crag, south of Tebay Fell, Langdale Fell and Ravenstone Fell, to Rathay Bridge, but it is much affected by dislocations. The general range of the division, Mr. Sharpe states, may be traced by the grits and slates forming a series of bold hills which stand out in relief above the tame rounded masses of the argillaceous schists of the middle division.

The author alludes to a band of calcareous slates shown by Prof. Sedgwick to range from Blawith to the south-west, but he states that he failed to find its eastern continuation; he alludes likewise to Mr. Marshall's account of having found lower Silurian fossils in it; and he is induced, on this account, to conceive that the calcareous band may form the uppermost portion of the lower Silurian rocks. The lowest division of the Windermere series is stated to be well exposed on the shores of Coniston Lake.

3*b*. *Middle Division.*—This deposit consists of hard argillaceous rocks, usually striped or banded gray, blue, or white, and sometimes brown; it contains also beds of soft shale and hard grits similar to those of the lowest division. On the west side of Windermere the usual strike is north-east, but to the eastward of the lake the strata are stated to be thrown into great confusion by faults ranging north and south. The boundary between this and the upper division is drawn by the author from Newby Bridge to Witherslack; but from Whitborrow to the Lune, the southern edge of the deposit is overlaid unconformably by various rocks of more modern date. East of the Lune the Windermere rocks are stated to be less concealed by other formations, the southern boundary ranging from a little east of Barbon to Barbon Fell House, where it is again overlaid by carboniferous limestone. The only traces of organic remains mentioned by the author are some crushed specimens, one of which he considers to be a Phragmoceras.

3*c*. *Upper Division.*—This division consists of hard, compact, purplish greywacke, little affected by cleavage, and can be distinguished from the Ludlow rocks only by the absence of fossils. The strata are greatly disturbed by north and south anticlinal faults. The division is exposed in only two limited districts; one south of Windermere, and the other east of the Lune, constituting Barbon Beacon and the western end of Casterton Fell, all the intermediate district being occupied by newer formations.

4. *Ludlow Rocks.*—This series rests, the author says, unconformably on the Windermere beds; but the want of conformity is stated to be inferred, not from the usual evidence of irregular deposition at the passage beds, but from the relative position of the two formations, the Ludlow rocks resting, in different places, on the middle and

upper divisions of the Windermere series. The deposit is composed of hard, purplish gray, argillaceous strata, and though intersected by several cleavage plains, does not possess a slaty structure. The lines of stratification are usually well-marked by thin rotten layers full of casts of shells, the intermediate portions being devoid of organic remains. The range of the Ludlow rocks, as limited by the author to beds which contain fossils, and commencing west of Kendal Fell, is stated to be a narrow strip at the base of Underbarrow Scar; and on the east of Kendal Fell, is a patch on the Tenter Fell, north-west of Kendal. In the valley of the Kent, the Ludlow rocks are concealed by newer deposits; but east of the valley they constitute the high anticlinal ridge of Benson Knot and Helme, the top of the latter, however, being old red sandstone; they occupy also all the country thence to the Lune, except the highest point of Lupton Fell, where the Windermere rocks are brought to the surface, being bounded on the west, south, and east by mountain limestone or old red sandstone. The usual strike of the beds is said by the author to be north and south, and the dip either east or west, the strike conforming to the direction of the principal faults. The chief anticlinal north and south ridges are stated to be Benson Knot, Helme, Old Hutton Common, and Lupton Fell: several east and west faults are likewise mentioned in the paper; as in Lambrigg Park and Fell, in Mansergh Common, west of Lunesdale, and at Old Town.

A gradual passage from the upper beds of the Ludlow rocks into the tilestone of the old red sandstone is exposed at the top of Helme at Old Town and the southern part of Mansergh Common; and the author is induced to infer, from eleven of the twenty-five species found in the bottom beds of Herefordshire occurring also in the upper Ludlow rocks of that district, and from seven of the remaining fourteen species occurring low in the Ludlow rocks of Westmoreland, that the beds which have been considered to form the bottom of the old red sandstone ought to be included in the Silurian system. A further argument in support of this arrangement is drawn from the fact, that where the old red sandstone rests on the Windermere rocks these doubtful beds are wanting, the shells being found only where the Ludlow rock occurs.

A list of thirty-four species of fossils is given in the paper, consisting almost solely of Ludlow Testacea figured in Mr. Murchison's work, but the author does not state positively to what portion of the Ludlow series the Westmoreland beds ought to be assigned.

5. *Old Red Sandstone.*—The following distinct districts, composed of old red sandstone, occur within the area described by the author: (*a.*) that in the valley of the Lune and the neighbourhood of Kirkby Lonsdale; (*b.*) those near Kendal and in the valleys of the Kent, Sprint, and Mint; and (*c.*) that near Shap and Tebay.

5*a.* To the old red of the valley of the Lune, above Kirkby Lonsdale, the author assigns the bed of loose conglomerate and red clay, which he says dips under the scar limestone of Casterton, the limestone being inclined to the south-east at an angle of 30°, and the conglomerate to the east by north at an angle of 25°. The want of conformity is stated to be more manifest to the westward; for where the limestone bends round by Kirkby Lonsdale bridge it dips 25° or 30° to the south-south-east; at Catshole quarry the strata are arched with a north-west strike; at Hollin Hall quarry the dip is south-west 30°, and at Teamside 40° south-east; but the old red sandstone dips throughout, as far as the beds can be seen, to the east. At Casterton the loose conglomerate is 100 feet thick, and passes downwards into red marl, occasionally mottled blue, and estimated to be fifty feet thick. This marl rests on alternating beds of red marl and red sandstone, beneath which is a considerable deposit of dark red tilestone and light-coloured sandstone, forming the passage beds into the Ludlow rocks. The total thickness is estimated at 1000 feet. To the north of the Casterton fault, the lower beds of the old red sandstone are stated to be raised up and exposed, far to the eastward of their position below Casterton; and above this spot the right bank of the river is said to be composed of the lowest beds of the tilestones and the passage beds into the Ludlow rock, but the left bank to consist of tilestones and red sandstones. The dip is east, at an angle of 25°. Mr. Sharpe also assigns to the old red sandstone, but not definitively, the bed of brown gravel, or of brown clay full of pebbles, which covers the whole of the valley of the Lune to its junction with the Rathay, and up that valley nearly to Sedbergh. It forms a line of low hills on each side of the Lune, resting on the northern edge of the tilestones above Barbon Beck, and conceals the junction of the Ludlow rocks on the right of the Lune with the Windermere rocks on the left of that river.

5*b*. Several limited patches of old red sandstone occur in the neighbourhood of Kendal, the remnants, in the author's opinion, of a once continuous mass. They consist, near Kirkby Lonsdale, of red conglomerates, red marls, and red and light-coloured sandstones, with tilestones, which pass downwards into the Ludlow rocks. Some of these patches, as on the top of Helme and at Monument Hill, two miles north-east of Kendal, have been raised to a considerably higher level than the rest of the formation. Three miles above Kendal the old red sandstone is well-exposed on the banks of the Sprint, consisting of

Loose conglomerate	60 to 80 feet.
Red marl	50 ...
Thin-bedded red sandstone	30 ...

The strike of the beds is north by west, and the dip east by north 10°, and they are unconformable to the adjacent older rocks. Similar beds are slightly exposed in the banks of the Mint, near Lavrock Bridge, striking east, and dipping 5° north, a bearing different from that of all the neighbouring rocks. They are separated from a more extensive patch about Greyrigg by an anticlinal ridge of the middle division of the Windermere rocks, but they cover a considerable area capped by nearly horizontal beds of mountain limestone. Around Kendal is another doubtful deposit of brown gravel, and the castle stands upon it.

5*c*. *Shap and Tebay.*—The course of the Birkbeck, from its rise above Shap Wells to its junction with the Lune at Tebay, intersects a deposit of old red sandstone, and the same deposit extends for some distance eastward up the valley of the Lune. It consists of the usual triple division, but the passage beds into the Ludlow rocks are entirely wanting, and the lower beds thin out in ascending the valley from Tebay. It rests on the lowest portion of the Windermere series. The dip is only 5° or 10° to the north-east. On the opposite side of the ridge which separates the Lune from the Lowther, the old red again occurs in the valley of the latter river, the intervening ridge being occupied by masses of the doubtful brown gravel. Throughout this district the lowest beds of the mountain or scar limestone rest conformably on the old red sandstone.

General Remarks; or comparison of the Westmoreland strata with the equivalents in other parts of the kingdom.—The triple division of the Westmoreland old red sandstone, the author says, agrees remarkably with that of Herefordshire, as already stated by Mr. J. Phillips in his work on the Fossils of Devonshire; the only differences being the disaggregated state of the conglomerates, and the absence of the cornstones as well as of the Ichthyolites. The gradual passage from the bottom of the old red sandstone into the Ludlow rocks also coincides with the phænomena described in Herefordshire by Mr. Murchison. The Ludlow rocks of Westmoreland will also bear comparison with those of the border counties of England and Wales; but, owing to the absence of the Aymestry limestone, it is not possible, the author states, to fix the exact relative position of the former with respect to the latter, but he says that they exactly agree with the upper division of the upper Silurian rocks of Denbighshire, as described by the late Mr. Bowman*. With respect to the Windermere series, the author likewise hesitates to place it on an exact parallel with any of the subdivisions of the Silurian as described in Mr. Murchison's work, but he states that it precisely agrees in part with lower divisions of the Denbighshire upper Silurian rocks, both in general characters and the details of the component strata. The Coniston limestone Mr. Sharpe, as already stated, prefers to consider as a lower Silurian deposit, than as the equivalent of any one of the members of that series of rocks.

The author then enters upon the inquiry of the principal epochs of disturbance and elevation of the Westmoreland rocks; and he shows, 1st, that the earliest period of disturbance was connected with the outburst of the Shap granite; inferring, from the conformity of the Windermere rocks with the Coniston limestone, that all these series were deposited before the outbreak of the granite; 2nd, that the old red sandstone resting horizontally on the elevated rocks of Shap Fell, proves that this formation was accumulated after the disturbance consequent upon the protrusion of the granite; 3rd, that all the faults which affect the old red sandstone, or any newer formation, are more modern than the outburst of the granite. Although difficulties attend the fixing of the age of the Ludlow rocks relative to the outburst of the granite, on account of the complicated

---

* Athenæum, No. 719, Aug. 7, 1841.

irregularity of the position of the former, yet the author thinks, that from the want of conformity of the Ludlow rocks to the Windermere, and from the faults which traverse them extending into the old red sandstone, that they were deposited subsequently to the protrusion of the granite. Having thus defined the limit of that event, Mr. Sharpe proceeds to show its effects. In the south of Westmoreland, he says, it threw into a high angle the strata of Coniston limestone and Windermere schists, and produced the great east and west faults around Coniston and Windermere, as well as in Middleton and Casterton Fells; likewise the dislocations of the Coniston limestone, with their prolongations in the valleys of Coniston, Esthwaite, Windermere, Kentmere, Long Sleddale, &c., which are not continued into the Ludlow rocks. These valleys, or lines of cracks, Mr. Sharpe says, are quite distinct in character from the north and south synclinal valleys in those rocks; he is also of opinion that the valley of the Lune had a similar origin, but the older rocks being concealed by newer deposits, its resemblance to the other valleys is less complete.

Mr. Sharpe did not observe any proof of the Ludlow rocks having been disturbed anterior to the deposition of the old red sandstone, but, he says, there is abundant evidence of both those formations having been dislocated before the accumulation of the mountain limestone, as the limestone of Kendal Fell rests in a nearly horizontal position upon the upraised edges of an anticlinal ridge of Ludlow rocks, from which a covering of old red sandstone is considered to have been partially denudated: the anomalous manner in which the limestone overlies the old red sandstone of Kirkby Lonsdale is, he says, another instance. The principal north and south faults of the Ludlow rocks, and a portion of the Windermere schist, between Windermere and the Lune, are, however, considered by the author to be of later origin than the mountain limestone, and he particularly refers to the disturbances at Natlands, Farleton Knot, Hutton Roof, Lupton Fell, Witherslack, Whitbarrow and Kendal Fell. Lastly, the author calls attention to the successive elevation of hills in one direction by forces acting at different periods as a phænomenon which has not received the thought it deserves; and he points out as an instance the Windermere schists forming the high chain of Middleton and Casterton Fells, which chains, he says, were elevated from the north at the period of the eruption of the Shap granite, nearly as they a re at present, for they formed, he states, the boundary of the great hollow in which the Ludlow rocks were deposited; and the great faults which cross the Fells in an east and west direction were, he is of opinion, formed at the same period, the mountain limestone not having been broken through by the faults in which the Rathay, the Dee, and the Barbon traverse the chain: yet this chain of hills has been elevated, he adds, in the same north and south direction subsequently to the deposition of the mountain limestone, the whole band of limestone resting upon their eastern flanks having been thrown up to a high angle, and in some places much disturbed.

INDEX TO VOL. XXI.

END OF THE TWENTY-FIRST VOLUME.

PRINTED BY RICHARD AND JOHN E. TAYLOR,
RED LION COURT, FLEET STREET.

ALERE FLAMMAM.

6