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A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

VOL. VIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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THE Authors of Original Papers in the present Volume, are C. Wilkinson, Esq.; Peregrinus Proteus; A Constant Reader; E. P.; Mr. W. Jones, F. Am. P. S.; Alexander Henderson, M. D.; Mr. J. Haley, Jun.; R. B; Mr. Ezekiel Walker; G. A.; I. R. I.; A Correspondent; Mr. Cuthbertson; Mr. J. Bramah; Messrs. Harman and Dearn; Mr. Frederick Accum; A Carlisle, Esq.; R. T.; Mr. J. Dalton; H. G.; Mr. J. C. Hornblower; Mr. John Gough; M. le Comte de Bournon; Mr. A. Woolf; Right Hon. Sir Joseph Banks, Bart. P. R. S.; Thomas Thomson, M. D.; Wm. Hyde Woollaston, M. D. F. R. S.; Mr. Wm. Henry; T. S. T.

Of Foreign Works, H. Beaupoil; Cit. Berthollet; Fourcroy; Vauquelin; Collet Descotils; Brugnatelli; Professor Lichtenberg; Ritter; Haüy; J. Drapernaud; Klaproth; Van Marum; J. C. Bartholdi; Jérôme De Lalande; J. L. Guy Lussac; Dr. P. A. Nernich.

And of English Memoirs, abridged or extracted; R. Ramsden Bradley, Esq.; Mr. Robert Green; Rev. Edmund Cartwright; Dr. John Winterbottom; R. B.; Benjamin Smith Barton, M. D.; Mr. Edward Massey; Mr. David Charles; Smithson Tennant, Esq. F. R. S.

Of the Engravings the Subjects are, 1. Machine of considerable Power for clearing Roads of Mud, by Dr. Winterbottom. 2. Mr. Robert Green's Hand Drill for Peas. 3. The Rev. E. Cartwright's Three-Furrow Plough. 4. Diagram for investigating the Figure of the Earth. 5. New Galvanic

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vanic Apparatus, convertible at Pleasure into one or more Plates. 6. Hydraulic Machine operating by the rotation of two Pinions, in a Water Vessel. 7. An improved Jib for a Crane, by Mr. Bramah. 8. New Filtering Apparatus, by Messrs Harman and Dearn. 9. Galvanic Apparatus. 10. An ancient Lock used in Egypt and Western Asia for above three thousand Years. 11. Developement of the Mechanism of the Lock. 12. Sketch of the Orbits of the New Planets, by Jerome de Lalande. 13. Crystals of Arseniated Copper, by Hauy. 14. Telegraph by the Human Figure. 15. Machine for levelling the Surface of Land, by Mr. David Charles. 16. Shaded Sections of a Clock, which strikes the Hours by simpler Mechanism, and with greater regularity than usual, by means of a Pendulum substituted in the Place of a Fly. 17. Specimen of a very curious antique Composition or Painting in coloured Glass, by an Art at present lost. 18. Improved Lamp for producing a strong Heat in Chemical Experiments, by Mr. Accum.

Soho Square, August, 1804.

TABLE OF CONTENTS

TO THIS EIGHTH VOLUME.

MAY, 1804.

Engravings of the following Objects : 1. A Machine of considerable Power, for clearing Roads of Mud, by Dr. Winterbottom ; 2. Mr. Robert Green's Hand Drill for Peas ; 3. The Rev. E. Cartwright's Three Furrow Plough ; 4. Diagram for investigating the Figure of the Earth ; 5. New Galvanic Apparatus, convertible at Pleasure into one or many Plates ; 6. An Hydraulic Machine, operating by the Rotation of two Pinions in a Water Vessel.	
I. Letter from C. Wilkinson, Esq. on the Means of simplifying and improving the Galvanic Apparatus	Page 1
II. Observations and Communications on the Dry Rot in Timber, made to the Society for the Encouragement of Arts	5
III. On the Figure of the Earth. By Peregrinus Proteus	12
IV. Description and Drawing of a Hand Drill for sowing Peas, Beans, &c. Communicated to the Society of Arts, by the Inventor, Mr. Robert Green, of Westwratting, Cambridgeshire	19
V. Enquiries concerning the Methods of investigating the Course and Velocities of a Body supposed to be projected from the Moon to the Earth. By a Correspondent	22
VI. Drawing and Description of a Three Furrow Plough. By the Rev. Edmund Cartwright, of Woburn, Bedfordshire	24
VII. On the State of Science among the earlier Nations of Antiquity : and more especially of those Researches which constitute the Subjects of Alchemy. In a Letter from E. P.	27
VIII. Description of a Machine for clearing great Roads from Mud. Communicated to the Society of Arts, by the Inventor Dr. John Winterbottom, of Newbury, Berks	29
IX. Description and Drawing of an Hydraulic Machine, with an Account of several Inventions of early Date, which have been since brought forward by later Inventors. Extracted from a Foreign Work published early in the last Century. By a Correspondent	35
X. An Examination of Dr. Wollaston's Experiment on his Periscopic Spectacles. By Mr. W. Jones, F. Am. P. S.	38
XI. Experiments and Observations on the Change which the Air of the Atmosphere undergoes by Respiration, particularly with Regard to the Absorption of Nitrogen. In a Letter from Alexander Henderson, M. D.	40
XII. Observations and Experiments tending to ascertain the Causes of those Irregularities in Chronometers, which are generated during considerable Intervals of Time, and have been ascribed to external Causes. By Mr. J. Haley, Jun.	46
XIII. A Memoir concerning the Fascinating Faculty ascribed to the Rattlesnake and other American Serpents. By Benjamin Smith Barton, M. D. From the American Transactions, Vol. IV.	58
Scientific News, 62—Very extensive Table of Squares, ib.—On Phosphoric Rings. From a Correspondent, R. B. 64.	

JUNE,

JUNE, 1804.

Engravings of the following Objects: 1. An improved Jib for a Crane, by Mr. Bramah; 2. New filtering Apparatus, by Messrs. Harman and Dearn; 3. Galvanic Apparatus; 4. An ancient Lock used in Egypt and Western Asia, for upwards of Three Thousand Years; 5. Development of the Mechanism of the Lock.

- I. Letter from Mr. Ezekiel Walker, on the Methods of observing the Longitude at sea; with an Exhibition of the very great Accuracy of the mean Result from a Number of Chronometers - - - 65
- II. On Galvanism. By C. Wilkinson, Esq. - - - 70
- III. On the Formation of Snow. By G. A. - - - 73
- IV. Medico Chemical Researches on the Virtues and Principles of Cantharides. By H. Beaucaul - - - 76
- V. Description of a Galvanic Apparatus affording a large Surface for Oxidation, and convertible into one or more Plates at pleasure. By I. R. I. 79
- VI. On the Difference between the Effects of Electricity and of Heat. By Cit. Berthollet - - - 80
- VII. Letter from a Correspondent I. R. I. explaining some Facts in Galvanism, and on other Objects - - - 84
- VIII. On the Presence of a new earthy Phosphate, found in the Bones of Animals, which does not exist in those of Men. By Fourcroy and Vauquelin. Read before the National Institute - - - 85
- IX. On the Nature of Oxygen, Hydrogen, Caloric, &c. as deduced from Galvanic Experiments. By a Correspondent - - - 88
- X. Experiments on the Yolk of Wool, followed by some Considerations on the Cleansing and Bleaching of Wool. By Cit. Vauquelin - - - 90
- XI. Copy of a Letter from Mr. Cuthbertson to Dr. Pearson, communicating an important and curious distinguishing Property between the Galvanic and Electric Fluids. Communicated by Dr. Pearson - - - 97
- XII. Letter from a Correspondent, containing an Observation of the spontaneous Inflammation of Paper in Nitric Acid Gas - - - 98
- XIII. Description of a Jib on a new Construction; by Mr. J. Bramah, Engineer. Communicated by the Inventor - - - 99
- XIV. A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake, and other American Serpents. By Benjamin Smith Barton, M. D. From the American Transactions, Vol. IV. - - - 100
- XV. Some Account of an Egyptian Lock of very high Antiquity. Indicated from Denon's Travels. By a Correspondent. With Observations by W. N. 115
- XVI. On the Cause of the different Colours of the triple Salts of Platina, and on the Existence of a new Metallic Substance in that Metal. By Collet Descotils. Presented to the Class of Mathematical and Physical Sciences of the National Institute of France - - - 181
- XVII. Description of an Apparatus for filtering Water. By Mess. Harman and Dearn, of Redriff. - - - 126
- XVIII. Examination of a Stone containing Potash. By Frederic Accum, Teacher of Practical Chemistry, Pharmacy, and Mineralogy 127
- XIX. Letter from A. Carlisle, Esq. on the Temperature of the Sea 131
- XX. Observations upon the Doctrine of Count Rumford, respecting the Want of direct conducting Power in Fluids with regard to Heat. By Cit. Berthollet - - - 134
- XXI. On the Difficulty of obtaining Alumine in a State of Purity. By R. T. 141
- Scientific News, 142—New Earth, *ib.*—Suberic Acid from Paper, *ib.*—Easy and expeditious Method of preparing Copal Varnish, *ib.*—Large Piece of Amber, 143—Fluoric Ether, *ib.*—New Method of preparing Nitric Ether, by Brugnatelli, *ib.*—Accension of Sulphuretted Hydrogen Gas, by the Affusion of Nitrous Acid, by Professor Lichtenberg, 144.

CONTENTS.

iii

JULY, 1804.

Engravings of the following Objects : 1. Sketch of the Orbits of the New Planets, by Jerome de Lalande ; 2. Crystals of Arseniated Copper, by Haüy ; 3. Figures to illustrate a Process for Telegraphic Conversation at considerable Distances, in which the Human Figure itself constitutes the Telegraph ; 4. A Machine for levelling the Surface of Land, by Mr. David Charles ; 5. Shaded Sections of a Clock, which strikes the Hours by simpler Mechanism, and with greater Regularity than usual, by Means of a Pendulum substituted in the Place of a Fly.	
I. On the supposed Chemical Affinity of the Elements of Common Air ; with Remarks on Dr. Thompson's Observations of that Subject. In a Letter from Mr. J. Dalton	145
II. Easy Methods of completing Tables of Squares and Cubes. In a Letter from H. G.	150
III. Problems in Spheroidal Triangles. By Peregrinus Proteus	151
IV. Description of a striking Part for a Clock, in which the Intervals between Stroke and Stroke are not regulated by a Fly, but by a Pendulum. By Mr. Edward Massey	162
V. Description of a very simple Telegraph, consisting of the Human Figure, adapted to converse at a Distance by means of Signs	164
VI. Account of a Fact respecting Water heated in a Boiler of Stone ; with Observations. By Mr. J. C. Hornblower	169
VII. Facts and Observations tending to elucidate the Theory of Galvanism. By a Correspondent	171
VIII. Experiments with the Electric Pile, by Mr. Ritter of Jena. Communicated by Mr. Orsted	176
IX. Account of a Machine for laying Land level. By Mr. David Charles	181
X. Experiments on Magnetism ; by Mr. Ritter, of Jena. Communicated by Dr. Orsted, of Copenhagen	184
XI. Observations on Arseniated Copper. By Haüy	187
XII. Observations upon the Doctrine of Count Rumford respecting the Want of direct conducting Power in Fluids with Regard to Heat. By Cit. Berthollet	198
XIII. A Memoir on the Movements which certain Fluids receive from the Contact of other Fluids. By J. Draparnaud, Curator and Professor of Natural History, at the Medicinal School of Montpellier	201
XIV. Letter from Mr. Cuthbertson respecting his Galvanic and Electrical Experiments	205
XV. Chemical Examination of the Ochroites, a Mineral not hitherto well known, containing a New Earth. By Klaproth	207
XVI. Letter from Van Marum to J. C. Delamétherie, on Ritter's Galvanic Experiments	212
XVII. Experiments on Light ; by Mr. Ritter of Jena. Communicated by Dr. Orsted	214
XVIII. On Spontaneous Inflammations. By J. C. Bartholdi, Professor of Phycic and Chemistry	216
XIX. Discovery of Two New Metals in Crude Platina. By Smithson Tennant, Esq. F. R. S.	220
Scientific News and Account of Books, 222—Figure of the Orbits of the New Planets, By Jerome de Lalande, ib.—Philosophical Transactions of the Royal Society of London, for the Year 1804, Part I. 4to, 182 Pages, with 5 Plates and 26 Pages of Metereological Journal, ib.—Analytical Essays towards promoting the Chemical Knowledge of Mineral Substances. By Martin Henry Klaproth, Professor of Chemistry, &c. 223.	

AUGUST,

CONTENTS.

AUGUST, 1804.

Engravings of the following Objects: 1. Specimen of a very curious antique Composition or Painting, in coloured Glass, by an Art at present lost; 2. An improved Lamp for producing a strong Heat in Chemical Experiments, by Mr. Accum.		
I. On the Pastes, coloured Glasses, or Enamels, of the Ancients. By M. Klaproth	- - - -	225
II. On Spontaneous Inflammations. By G. C. Bartholdi, Professor of Physic and Chemistry	- - - -	236
III. On the Solution of Water in the Atmosphere; and on the Nature of atmospherical Air. By Mr. John Gough. From the Author	- - - -	243
IV. Reply to the Observations of M. l'Abbé Hauy, on Arseniated Copper. By M. le Comte de Bournon, Member of the Royal and Linnean Societies of London	- - - -	247
V. Process for separating Alumine from Alum. In answer to the Enquiries of R. T. By Mr. Frederick Accum	- - - -	260
VI. Short Account of Mr. A. Woolf's Improvement in the Construction of Steam-Engines	- - - -	262
VII. Description of a Chemical Lamp, with double concentric Wicks. Communicated by Mr. Frederic Accum	- - - -	266
VIII. On the mutual Precipitations of Metallic Oxides. By J. L. Guy-Lussac		270
IX. A Report of the State of his Majesty's Flock of Fine Woolled Spanish Sheep, for the Year ending Michaelmas, 1803. By the Right Honourable Sir Joseph Banks, Bart. P. R. S. &c. &c. From the printed Copy communicated by the Author	- - - -	277
X. On the Oxides of Lead. By Thomas Thomson, M. D. Communicated by the Author	- - - -	280
XI. On certain Chemical Effects of Light. In a Letter from Wm. Hyde Wollaston, M. D. F. R. S.	- - - -	293
XII. Illustrations of Mr. Dalton's Theory of the Constitution of Mixed Gases. In a Letter from Mr. Wm. Henry, of Manchester, to Mr. Dalton. Communicated by the Writer	- - - -	297
XIII. On the Disappearance of Oxygen and Hydrogen over Water, at the Heat of the Atmosphere. By T. S. T.	- - - -	304
XIV. Letter from Dr. P. A. Nennich, expressing Doubts with regard to the Death of the celebrated Humboldt	- - - -	303

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JOURNAL
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MAY, 1804.

ARTICLE I.

*Letter from C. WILKINSON, Esq. on the Means of simplifying
and improving the Galvanic Apparatus.*

To Mr. NICHOLSON.

SIR,

THE communication from your ingenious correspondent Introduction.
I. R. I. afforded me considerable pleasure. As you favoured
me with his idea how the galvanic power may be increased
to an immense degree, prior to its appearance in your valuable
Journal, I have been induced to reflect upon various modes
of galvanic arrangements.

If the whole apparatus be made to consist only of a single In apparatus of
one single plate,
the coating of
copper is un-
necessary.
plate reflected backwards and forwards, so as to expose an
immense surface, no plate of copper would be required, only
taking care, which in this instance would be easily effected,
that one side of the plate only should be exposed to the action
of the acid. For, from various experiments, I am persuaded
that the other metal answers no other electrical purpose, than
to guard and protect the zinc side, to which it is soldered from
being acted upon by the acid. Even cement answers very Cement will do
as well.
well, if we only preserve through the cement a good con-
ducting medium to the zinc. Thus with a zinc plate of eight
inches diameter, if only a piece of copper the size of a half-

penny be soldered in its centre, and all the rest of that surface, which is usually soldered to copper or silver, be well covered with cement, equal effects would be obtained.

Proof in the
Couronne de
Tasses;

In a Couronne de Tasses I find the same effects are produced, whether the copper be of a corresponding size to the zinc plates, or, whether they be merely simple copper or silver wires.

and in a pile.

Upon this principle I have constructed a pile, and find it produces the same effects, as if the whole surface were covered with copper; I purpose soon to arrange a trough upon the same principles, and I am persuaded, that the more tedious and expensive part of galvanic apparatus may thus be prevented.

The corrosion in
a trough is
greatest near the
air.

When a trough has been sometime employed, upon removing the plates, the loss of metal, I always observe, does not take place uniformly over the surface, but in the upper part, which in the galvanic action is the most exposed to the atmosphere, is the most acted on, and towards the bottom, the metal is very little altered. In order to preserve a more equal action, I am now preparing a battery formed of plates of ten inches by two and a half, the longest side placed horizontally, and I am persuaded, that this sized plate will produce more active effects than a plate of five inches square.

Hence a pile of
dishes would be
preferable.

It is well known, that in every galvanic operation, oxidation is produced, and if oxygen can be procured from the surrounding air, the effect would be more easy than producing it from the decomposition of water. I have no doubt, that if a series of zinc plates formed like large pewter dishes, were to be arranged in a pile-like form, insulated from each other, and the galvanic mixture to be poured in the hollow part; the lower side being covered with cement, excepting that from the centre a piece of copper should be soldered, and so projecting as to be in contact with the fluid in the cavity of the plate below; that a very powerful apparatus would thus easily be formed.

On the substitu-
tion of air in-
stead of wet
mediums by
Dyckhoff.

In the latter part of your Journal I perused with a considerable degree of surprise, some observations of a Mr. Dyckhoff relative to the substitutions of thin strata of air, instead of wet media. As the results of his experiments appeared so contrary in principle to any I had tried, I immediately repeated them in the manner he has described, with interposed

lents of glass, so as to render the separation very small. I then arranged several series of plates of one inch, to plates of seven inches in diameter; because he has not particularized the size of the plates he employed. I arranged piles of these different sizes to the number of twenty pairs in each arrangement, and observed not the slightest galvanic effect, either by my tongue or by any other test. I employed a very excellent condenser; The experiment did not succeed. no influence at all was evinced; and, lastly, I subjected it to the most delicate electrocope we have, viz. the muscular fibres of a frog. Not the slightest disturbance took place, although I fancy I have demonstrated, in the elements of galvanism I have published, that the sensibility of this animal electrocope is fifty thousand times greater than that of the condenser.

As to the charging of a Leyden phial, I am convinced it is perfectly erroneous to suppose it has ever been done. I have employed from fifty pair of plates to fifteen hundred, and never yet have produced any charge. Nor indeed could such be expected from the weak intensity of the state of electricity in galvanic operations; for a jar cannot be charged until a sufficient quantum is accumulated to overcome the resistance of the surrounding air, so absolutely requisite to the charging of a Leyden phial. Conviction of the author that galvanism cannot charge a Leyden jar.

I am, Sir,

Your's, &c.

C. WILKINSON.

ANNOTATION. W. N.

THE valuable observations in the preceding letter, will naturally suggest improvements to those who are employed in the construction of galvanic apparatus. It must be a great advantage, that the expence of copper and the work of foldering, or placing it, will be almost entirely saved. *Fig. 1. Plate III.* shews a simple method of disposing a single plate of zinc of large surface in a trough. It is supposed to have been made sufficiently hot to bed itself in cement at the bottom of the box in which it is placed, and its two ends A and B are secured in the same way. The shaded spaces represent the cavity occupied by acid, and the dotted space is left empty. Form of galvanic apparatus in a single piece,

B 2

A com-

A communication may be made by a copper wire from the wet or corroded surface to the opposite or dry surface, as shewn at A and C.

Another of many pieces acting as one.

As it may be difficult to bend a very long piece of laminated zinc, and to keep it flat, so as to give it a secure and close fixture in the cement, I should prefer a number of strait pieces screwed together with varnished pieces of wood between their extremities: every second piece being perforated on the acid side, to permit a free communication.

More particular account of the pile of dishes.

Mr. Wilkinson's pile of dishes promises to be very effectual, and of easy construction. I suppose the dishes to be of zinc, either cast in a metallic mould or stamped, and that each should be provided with three short copper legs, soft-folded on; after which, the lower face of the dish should be well defended by varnish or cement, and also the lower ends, but not the sides of the copper legs. Thus prepared, they might be easily builded up, and charged by a proper funnel, and an appropriate measure for dealing out the acid.

Galvanic trough of one metal convertible into a single plate, by varying the communication.

In *Fig. 2.* I have ventured to offer the sketch of a trough, consisting of zinc plates only, principally because it appears capable of being as speedily charged with acid as another trough, and may with facility and at pleasure be used either as one single plate, or as the usual series of plates, acting in succession upon each other. Let A B represent a trough constructed after the manner of Cruickshank; excepting that the plates are of zinc only: and let the alternate shaded cells be charged with diluted acid, while the dotted cells continue empty. This may, without difficulty, be done at one pouring, by means of a channel in the wooden side of the trough.

Arrangement to produce the usual effect of a large plate.

Whether this trough, so charged, shall act as one extended galvanic surface, or as a continued series, will be governed by the copper wires of communication. If the former, then a long copper wire having branches descending from it into every one of the acid cells must be duly placed; and another wire parallel to, but not in contact with, the former, and having double branches descending into all the empty cells, so as to touch both the dry metallic surfaces, must also be placed. Whenever a communication is made between these two principal wires or conductors, the galvanic energy will pass through the medium of communication. I have not given a diagram, because this disposition is very obvious.

If

If the latter effect be required, namely, that the trough should act like a continued series, the connection of the troughs must be differently made. A number of clipping pieces or spring forks must be provided of copper wire, as represented by the curved lines *a, b, c, d, &c.* from each of which proceeds a third leg or branch, as seen in the figure. These are applied (most conveniently above, but) not so as to touch each other; the clipping part of each embracing the dry surfaces of a pair of the zinc pieces, which are acted upon by the same mass of acid, while the third leg is immersed in the acid of the next adjacent cell. Every one of their legs or branches is disposed towards the same region or part of space; by which means the acid of each compartment acting upon a pair of zinc plates, on one surface only, is made to communicate, by the interposition of copper, with the uncorroded side of the next pair in succession; and so on, exactly as in the common trough or pile.

Arrangement
for the effect of
a succession of
plates.

II.

Observations and Communications on the Dry Rot in Timber, made to the Society for the Encouragement of Arts.

(Concluded from p. 318 of Vol. VII.)

Second Letter from BENJAMIN JOHNSON, Esq.

SIR,

THE observations I sent yesterday were taken from different parts of my note-book, in haste, because the second Tuesday in December was past; for it was by accident I saw the advertisement on Saturday; but wishing not to be deficient in information, I trouble you again.

The leaves of the plant appearing exhausted and dead, is owing to their having imparted all their juices to the wood, which changed it to a fungus, and not to a powder, like rottenness from length of time.

A more full account of the plant that occasions the dry rot;

The *Boletus Lachrymans* is of the fungus tribe, and is one of the few that have leaves, as the miseltoe, &c.

Nothing is more easy than to prevent the damage from the plant. Besides what I said yesterday, I am positive that a tile laid

laid close along the walls round the room, would prevent the growth of the plant, even without mortar; and perhaps it is only necessary where the walls are next to the air.

and the method
of cure.

Charring the ends of the joists for a few inches, and charring the side of the wainscot at bottom, next to the walls would be sufficient; for the plant cannot adhere to any thing but wood, and that possessed of its natural juices, to a certain degree; so that I question if old dry oak would receive it.

All the white soft woods, as beech, poplars, and deals, are for a long time ready to receive it. Repairing the damage with fresh wood, without removing the earth and plant, is only feeding the evil.

The plant is of the creeping kind, and cannot rise two inches; so that wood in all cases, must be in contact with the earth to support it.

A fungus broader than the palm of one's hand, and an inch or more in thickness, is commonly seen at the bottom of an old post, on the surface of the earth; but it is not easy to discern whether the wood or the earth furnishes the matter; so true is the observation of Muller:—" *Dans l'étude de la nature, on peut nous comparer à de petits enfans qui commencent à ouvrir les yeux; nous voulons parler beaucoup, et nous ne faisons que bégayer.*"

I am, Sir,

Your most obedient Servant,

BENJAMIN JOHNSON.

Ipſwich, Dec. 21, 1799.

To the Secretary.

N. B. The qualities of this plant are unknown to most English botanists, as appears from their publications; but they are known to the Germans, who have habitually used more wood in their buildings than we have.

Third Letter from the Same.

SIR,

Affured that the pursuits of the Society for the Encouragement of Arts, &c. aim at the full investigation of whatever they propose for the public benefit, I cannot persuade myself that I am troublesome in going a little further into this subject.

I had

I had lately a conversation with an old friend, who showed me two parcels of rotten wood, from an oak barn floor, laid about sixteen years ago. After lying twelve years it shook upon the joists. On examination, it was found to be rotted in various parts, and the planks, two inches and a half in thickness, were nearly eaten through, though the outside was glossy, and without blemish. The joists, and a large middle beam were laid at the ends, in brick and mortar, to create a firm level. No earth was near the wood; and he thinks that no air could find a passage. The rottenness was partly an impalpable powder, of the colour of Spanish snuff, and other parts were black, as if burnt; the rest was clearly a fungus.

Some account of a barn floor decayed.

This gentleman is a person of undoubted veracity; but a nice and exact observation is necessary in such examinations. He thought nothing of any plant, and it is likely there was none of the Boletus; so that my assertion that it was always to be found, was rather too systematic.

It does not appear to have been caused by any plant.

I asked him if the timber was dry when laid down. He could not however say that had been particularly adverted to. It had been sawed from a large oak, and was, as he thought, in all respects proper for a barn floor. As this seems not the operation of the Boletus, how did it happen?

We know that the oak, when in vegetation, is subject to what I shall call an exudation of juices, which produces the fungus, named the Agaric of the oak, with which the Druids of old played many tricks. The oak, then, if sawed into thick quantities, may emit these same juices, as the progressive course of nature to its entire decay.

On the decay of oak timber.

We have all seen oaks of vast size and ancient record, with a great part of the outside whole, and all the inside gone; perhaps the work of a century. In all hollow trees fungus is discoverable. To use a law term, it is a *misnomer* to call it dry-rot; for the rotting principle is in moisture.

I had never seen the rot upon so large a scale as in timber, till lately. The prevention, then, of beams, rafters, large joists, and posts, put into the earth, from decay by the rot, is in charring only, which will dry up all the fungus juices of wood in large substance. Paint, or a bituminous preparation, may probably stop up the pores, and prevent the rot in slight work, where the treatment I before observed, with fire, might be incommodious, as in half-inch wainscot, &c.

The preparation of wood for preserving it, should be either charring, or bituminous paint.

The

Durability of
charcoal.

The incorruptibility of charcoal is attested by undoubted historical facts, at the destruction of the famous temple at Ephesus. It was found to have been erected on piles that had been charred; and the charcoal in Herculaneum, after almost 2000 years, was entire and undiminished.

I am, Sir,

Your most obedient Servant,

BENJAMIN JOHNSON.

Ipswich, December 26.

Letter from RICHARD RAMSDEN BRAMLEY, *Esq. of Leeds,*
relative to the Dry Rot in Timber.

To CHARLES TAYLOR, Esq.

SIR,

Introduction.

I take the liberty of inclosing to your care an Essay on the Dry Rot in Timber, which you will be so obliging as to lay before the Society for the Encouragement of Arts, &c. Should this Essay be deemed worthy of attention, or should any farther notice be necessary respecting it, every information that may promote the views of your respectable Society will be given with pleasure by,

Sir,

Your most obedient Servant,

R. RAMSDEN BRAMLEY.

Leeds, Aug. 26, 1799.

As the Society for the Encouragement of Arts, &c. have for some years offered a premium for the discovery of the cause occasioning the dry rot in timber, of which, it seems, no satisfactory account has yet been received; should the following prove so, it will give the author much pleasure. To bring the matter to the test by experiments, would require the observation of a long period, and in selected situations.

Wood, used for the general purposes of man, is cut down at different periods; and although it may be felled at the proper season, or when most free from sap or moisture, it is not always to be effected.

Even admitting it to have been cut down in the most favourable situation, it still abounds with such an extra proportion of

moisture, as to require a regular exposure to the air, prior to its being applied to use, if we wish to guard against that shrinking which always takes place, where this precaution has not been taken.

Facts and observations concerning the dry rot in timber, and its cure.

Although the fir kind contains less of this watery portion, yet it assuredly possesses a considerable share; and it is in this species, I apprehend, that the evil called the dry rot most generally occurs, as from the facility of working the same, it is most generally applied in buildings.

But supposing it to be fir, or any other species; wood felled when abounding with any extra proportion of sap, and applied to use without the proper seasoning or exposure to a free current of air, until such extra moisture as has had time to exhale, is most liable to the disease in question; and the cure, or principal prevention against it, would be the precaution of felling all wood *only* at the proper season, or when the sap is not in circulation. The next mode of prevention would be to use such wood only as has been for a considerable period exposed to the influence of a free current of air, or where convenience will admit, to that of air heated to a moderate degree; such air extracting with greater facility the inclosed moisture, and in a more certain ratio than the irregularity of our atmosphere will allow.

In all rapidly-improving countries, this evil is likely to be an increasing one, as the current demand for wood generally exceeds the supplies laid by in store, so as to be applied to use in regular succession, after being properly seasoned.

Another cause that affects all wood most materially, when not fully dried, is the application of paint, the nature of which prevents all exhalation, and confines the inclosed moisture, till it occasions a fermentation through the whole fibrous system of the wood, and brings on a premature state of decomposition, or the dry rot.

A similar evil may be induced, in consequence of any newly-finished building having all the doors and windows shut up, and that for some length of time, particularly in moist weather. The wood, even though unpainted, is thus frequently placed in an atmosphere more charged with vapour than its own internal contents, and is consequently in an imbibing instead of an exhaling state, and tending to decay. Wood placed in dampish situations, and the ends of timbers near to moist walls, suffer from similar causes.

What

Facts and observations concerning the dry rot in timber, and its cure.

What particularly attracted my observation to the circumstances was this, that both oak and fir posts were brought into this premature state of decay, from their having been painted prior to the due evaporation of their moisture; and then extending the observation, and tracing the history of other wood affected in a similar manner, I am convinced that the evil frequently thus originates, and its prevention would be in using timber, previously well dried and seasoned.

RICHARD RAMSDEN BRAMLEY.

SIR,

A considerable time has elapsed since I furnished you with some observations relative to the dry rot in timber, and having been since engaged busily in draining from 4 to 5000 acres of ground, further ideas on the subject of the dry rot have in the interim recurred to me from the work I have been engaged in, which, if the respectable Society to which you are Secretary think worthy attention, they may add to, or connect with my former ideas, as may be deemed most useful. Where houses are troubled with damp walls, near to the earth's surface, it is generally, if not universally, occasioned by the percolation of water from the higher adjoining ground, which, thus intercepted in its current, attempts to follow the general hydrostatic law, of elevating itself, by the syphon line, to a height equal to that from whence it has its origin. Thus, in houses differently situated, we see the damp arising, to varying degrees of height, on the walls; and those are probably all corresponding to the height at which the moisture circulates in the adjoining ground. At its first entrance to the building, and whilst the moisture is in small quantity, the excavated part of the foundation wall may absorb, and gradually quit such proportion; but the excess, as is generally the case in moist weather, exceeding that power, the foundation stones are then saturated in a more rapid proportion than the adjoining rarified internal atmosphere can evaporate: the watery particles then creep up, in degrees proportionate to the ascent from which they originally descended, excepting when prevented, or driven off by the superior heat of the adjoining rooms, when, in addition to the disagreeable damp they cause, they frequently occasion considerable damage to pictures, furniture, &c. Drains laid out athwart the ascending ground, with a
very

very slight descent or fall, and made of the depth of one yard for each yard of ascent, and from the foundation until equal to the height that such damp ever rises, would, there is little doubt, completely secure the house and furniture from the inconveniences hitherto sustained, and would generally prove an effectual prevention to most cases of the dry rot, where it originates in extreme moisture. I am of opinion that the fungus which pervades decaying wood is not the first cause, but an attendant on the peculiar state to which such wood has been reduced by prior causes. The disseminated seeds finding a proper bed, or *nidus*, like the mushroom, toad-stool, &c. fix there their abode, and pervade the whole substance, thus accelerating the general law of Providence, which tends to make all matter re-productive.

Facts and observations concerning the dry rot in timber, and its cure.

Cellars, or such other places, should be drained in the manner I have above mentioned, by taking off the percolating water, prior to its gaining admission to or contact with the walls; and it is probable that, in most cases, a single drain will have complete effect; it would assuredly do so, if it was not for the variation of the earth's internal strata, which are not easily discernible. If attention to this rule was paid prior to the building any new streets in towns, it would prove essentially useful.

I am, with esteem,

DEAR SIR,

Your's truly,

Leeds, June, 1803.

R. RAMSDEN BRAMLEY.

To Mr. Charles Taylor.

The Society have been informed, that mortar made of lime from burnt chalk is much more destructive to timber than stone lime, or that burnt from lime-stone. Chalk lime attracts moisture; and communicating it to any timber which it touches, occasions its decay.

Sea sand is also prejudicial, if made into mortar, from a similar quality of attracting moisture from the atmosphere: this may in some degree be corrected by washing the sand well in fresh water, where good sand cannot be procured.

Good mortar, where any is required to be in contact with timber, may be made from a mixture of stone lime fresh burnt, and

and river sand, to which a very small quantity of common brown, or yellow iron ochre, should be added, and well incorporated therewith.

III.

On the Figure of the Earth. By PEREGRINUS PROTEUS.

To Mr. NICHOLSON.

SIR,

IN some of your late Journals I observe a paper on the figure of the earth, by Mr. John Playfair, professor of mathematics in the University of Edinburgh, containing several new theorems, and ingenious remarks, on a subject which has engaged the attention of the first mathematicians of Europe since the days of Newton. On reading it, I was led to examine the properties of spheroidal triangles, and to investigate the problem, proposed by the author, for determining the dimensions of the earth from the length of the straight line or chord joining two places whose geographical situations are given. These are intended to form the principal subject of this letter; but, before I proceed to them, I beg leave to make a few observations on that paper, without any view to cavil, or detract from its real merits.

Observations
upon Professor
Playfair's
memoir on the
figure of the
earth.

After taking notice of the disagreement in the compressions of the terrestrial spheroid, which result from the comparison of different measurements, he assigns, as the principal reason for this inconsistency, the local irregularities in the direction of gravity, arising in some situations from the attraction of mountains, and in others from the unequal density of the materials under, and not far from, the surface of the earth. That the first has a sensible effect on the plumb-line has been proved by accurate and undeniable experiments; the second is an ingenious and probable conjecture, which the surveys carrying on in Great Britain and France will afford data to refute or confirm. But though the former may operate in the general survey of a country, where the observer has not his choice of ground, it has always been avoided as much as possible in measurements made for the express purpose of determining the figure of the earth; and though the latter may produce some perceptible difference

difference in observations made in nearly the same latitudes, does the author think it sufficient to account for the great disagreement in the results from the comparison of distant observations? Is it not much more probable, without giving up the elliptic figure, that some of the observers may have used different standard measures from the rest, or not made proper allowances for the alteration of their lengths in different temperatures? In short, this circumstance appears to me sufficient to account for some small local irregularities, but wholly inadequate to explain the great differences in the general results.

Observations upon Professor Playfair's memoir on the figure of the earth.

The author then proceeds to point out several methods of calculating the dimensions of the earth from terrestrial measurements. The first applies to the case where two arches of the meridian are given in different latitudes, which, under the most favourable circumstances, is incomparably the most accurate that can be employed. The rules he gives are certainly very simple, and in some respects new; but he seems to be mistaken when he asserts, that the calculation must be made by rules quite different from those that have been hitherto given. Euler's * is essentially the same with his own; and Du Séjour, Legendre, Delambre, &c. have given many accurate theorems, which may be applied to this purpose. The second method is, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the same latitude; and the third from the measures of degrees of the curve perpendicular to the meridian in different latitudes. His theorems for both are very accurate and simple. But the principal novelty of Mr. Playfair's paper is, the method he proposes of finding the figure of the earth from the length of a straight line or chord joining two places whose geographical situations are given. As he has left the solution of this problem to some future occasion, the following perhaps may not be unacceptable:

Let PAO (*Plate III. Fig. 1.*) represent one quarter of the ellipsis, by the revolution of which round the semi-conjugate axis PC, half the terrestrial spheroid is generated. Let C be the center of the earth, P the pole, CO the radius of the equator = a , CP half the polar axis = b , and c = the compression at the poles, or the excess of a above b . Let A in the

Solution of the problem for finding the figure of the earth, from the length of a chord joining two known places, &c.

* *Memoires de L'Academie Royale des Sciences Belles Lettres a Berlin, 1753.*

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

meridian PAO be one of the extremities of the measured chord, and B in the meridian PB the other extremity; let AD, BF be drawn perpendicular to CP, BE perpendicular to the plane CPAO, and let AB, AE, FE be joined. Then will $AE^2 = (CD - CF)^2 + (AD - FE)^2 = CD^2 + AD^2 \times CF^2 + FE^2 - 2 CD \times CF - 2 AD \times FE$, and $AB^2 = AE^2 + BE^2 = CA^2 + CB^2 - 2 CD \times CF - 2 AD \times FE$.

Now let λ, ϕ be the latitudes of A and B expressed in decimals of the radius 1, ω the difference of longitude or the angle BFE, and D the length of the measured chord = AB. Then from the properties of the ellipsis we have

$$CA^2 = \frac{a^2 \operatorname{cof.} \lambda^2 + b^2 \operatorname{fin.} \lambda^2}{a^2 \operatorname{cof.} \lambda^2 + b^2 \operatorname{fin.} \lambda^2} = a^2 - 2ac \operatorname{Sin} \lambda^2$$

$$AD = \frac{a^2 \operatorname{cof.} \lambda}{\sqrt{(a^2 \operatorname{cof.} \lambda^2 + b^2 \operatorname{fin.} \lambda^2)}} = a \operatorname{cof.} \lambda + c \operatorname{cof.} \lambda \operatorname{fin.} \lambda^2$$

$$\text{and } CD = \frac{b^2 \operatorname{fin.} \lambda}{\sqrt{(a^2 \operatorname{cof.} \lambda^2 + b^2 \operatorname{fin.} \lambda^2)}} = a \operatorname{fin.} \lambda - c \operatorname{fin.} \lambda$$

$(2 - \operatorname{fin.} \lambda^2)$, neglecting the powers of c higher than the first, because c is very small in comparison of a . Whence by substitution, and putting $\delta^2 = 2a^2 (1 - \operatorname{fin.} \phi \operatorname{fin.} \lambda - \operatorname{cof.} \phi \operatorname{cof.} \lambda \operatorname{cof.} \omega)$, we obtain the following equation;

$$\delta^2 - c \left(4a (\operatorname{fin.} \lambda - \operatorname{fin.} \phi)^2 - \frac{\delta^2}{a} (\operatorname{fin.} \lambda^2 + \operatorname{fin.} \phi^2) \right) = D^2,$$

and by extracting the square root of each side, and rejecting the square, cube, &c. of c , there results,

$$\delta - c \left\{ \frac{2a}{\delta} (\operatorname{fin.} \lambda - \operatorname{fin.} \phi)^2 - \frac{\delta}{2a} (\operatorname{fin.} \lambda^2 + \operatorname{fin.} \phi^2) \right\} = D,$$

$$\text{or } a + c \left\{ \frac{1}{2} (\operatorname{fin.} \lambda^2 + \operatorname{fin.} \phi^2) - \frac{2a^2}{\delta^2} (\operatorname{fin.} \lambda - \operatorname{fin.} \phi)^2 \right\} = \frac{D a}{\delta}$$

This equation may be otherwise expressed thus; let a spherical triangle be constructed, having two sides equal to the polar distances of A, B, and contained angle = their difference of longitude; whence find the third side, which put = δ . Then will $\operatorname{fin.} \phi \operatorname{fin.} \lambda + \operatorname{cof.} \phi \operatorname{cof.} \lambda \times \operatorname{cof.} \omega = \operatorname{cof.} \delta$, and $1 - \operatorname{fin.} \phi \operatorname{fin.} \lambda - \operatorname{cof.} \phi \operatorname{cof.} \lambda \operatorname{cof.} \omega = 1 - \operatorname{cof.} \delta = 2 \operatorname{fin.} \frac{1}{2} \delta^2$;

therefore $\delta = 2a \operatorname{fin.} \frac{1}{2} \delta$, and $D = 2a \operatorname{fin.} \frac{1}{2} \delta + c \left\{ \operatorname{fin.} \frac{1}{2} \delta$

$$(\operatorname{fin.} \lambda^2 \times \operatorname{fin.} \phi^2) - \frac{(\operatorname{fin.} \lambda - \operatorname{fin.} \phi)^2}{\operatorname{fin.} \frac{1}{2} \delta} \right\} . \quad \text{The value of } \delta$$

is manifestly equal to the length of a straight line joining two places, whose latitudes are λ, ϕ , and difference of longitude ω , on a sphere, whose radius is a .

From

From this equation the following method of determining the figure of the earth is deduced. Let l be the length of a measured chord, and λ, ϕ, ω the latitudes and difference of longitude of its extremities; find \mathcal{D} as above, and let $m = 2 \sin. \frac{1}{2} \mathcal{D}$,

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

$$\text{and } n = \sin. \frac{1}{2} \mathcal{D} (\sin. \lambda^2 + \sin. \phi^2) - \frac{(\sin. \lambda - \sin. \phi)^2}{\sin. \frac{1}{2} \mathcal{D}}$$

Then if we reject all the powers of c higher than the first, we shall have the simple equation $ma + nc = l$. In like manner find a similar equation $m'a + n'c = l'$, corresponding to any other chord whose length is l' , and there will result $a = \frac{n'l - nl'}{mn' - m'n}$, and $c = \frac{m'l - m'l'}{mn' - m'n}$. The approximation may be easily carried further by including the second power of c , and thus finding an equation of the form $ma + nc + pc^2 = l$; but this labour would be useless, as the method itself does not admit of greater accuracy. If $\phi = \lambda$ the equation becomes

$$\frac{D}{2 \sin. \frac{1}{2} \mathcal{D}} = a + c \sin. \phi^2, \text{ as is found by Mr. Playfair in } \S 31.$$

From the first equation a rule may be easily derived for calculating the difference of longitude of two places, when their latitudes and distance are given. For by transposition we have

$$\delta^2 (1 + \frac{c}{a} (\sin. \lambda^2 + \sin. \phi^2)) = D^2 + 4ac (\sin \lambda - \sin \phi)^2,$$

and by division, and rejecting the powers of c higher than the

$$\text{first } \delta^2 = D^2 + \frac{c}{a} (4a^2 (\sin \lambda - \sin \phi)^2 - D^2 (\sin. \lambda^2 + \sin. \phi^2)):$$

but δ^2 is $= 2a^2 (1 - \sin. \lambda \sin. \phi - \text{cof. } \lambda \text{ cof. } \phi \text{ cof. } \omega)$, there-

$$\text{fore } \text{cof. } \omega = \frac{1 - \frac{D^2}{2a^2} - \sin. \lambda \sin. \phi}{\text{cof. } \lambda \text{ cof. } \phi} - \frac{c}{a} (2 (\sin. \lambda - \sin. \phi)^2 -$$

$$\frac{D^2}{2a^2} (\sin. \lambda^2 + \sin. \phi^2)) \text{ and putting } 1 - \frac{D^2}{2a^2} - \sin. \lambda \sin. \phi$$

$$= \text{cof. } \omega' \text{ we have } = \omega' + \frac{c}{a} \times \frac{2 (\sin. \lambda - \sin. \phi)^2 - \frac{D^2}{a^2}}{\sin. \omega'}$$

$(\sin \lambda^2 + \sin \phi^2)$, which rule may be thus expressed. Let

there be a spherical triangle, having two of its sides equal to the polar distances of the places, and the third side d such that

$\sin.$

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

$\text{fin. } \frac{1}{2} d = \frac{D}{2a}$: find the angle ω' contained between the polar distances, and the difference of longitude ω will be $= \omega' + \frac{2c}{a} \times \frac{\text{cof. } \frac{1}{2} d^2 (\text{fin. } \lambda^2 + \text{fin. } \phi^2) - 2 \text{fin. } \lambda \text{fin. } \phi}{\text{fin. } \omega'}$.

The latitude ϕ may also be found from the same equation, when λ , ω and D are given. For if the base of a spherical triangle be $= d$, the two other sides $= 90^\circ - \lambda$, $90^\circ - \phi'$, and the contained angle $= \omega$, the cof. ω will be $=$

$\frac{1 - \frac{D^2}{2a^2} - \text{fin. } \lambda \text{fin. } \phi'}{\text{cof. } \lambda \text{cof. } \phi'}$. Now let $\phi = \phi' + x$, where x must

be very small, and cof. λ cof. ϕ' there results cof. $\omega' = \text{cof. } \omega + \frac{(\text{cof. } \lambda \text{fin. } \phi' \text{cof. } \omega - \text{fin. } \lambda \text{cof. } \phi')}{\text{cof. } \lambda \text{cof. } \phi'} \times \text{fin. } x$: consequently

$\frac{(\text{cof. } \lambda \text{fin. } \phi' \text{cof. } \omega - \text{fin. } \lambda \text{cof. } \phi')}{\text{cof. } \lambda \text{cof. } \phi'} \times \text{fin. } x = \frac{c}{a}$

$\left(2 (\text{fin. } \lambda - \text{fin. } \phi')^2 - \frac{D^2}{2a^2} (\text{fin. } \lambda^2 + \text{fin. } \phi'^2) \right)$ nearly,

and $x = \frac{c}{a} \times$

$\frac{\text{cof. } \lambda \text{cof. } \phi' \left(2 (\text{fin. } \lambda - \text{fin. } \phi')^2 - \frac{D^2}{2a^2} (\text{fin. } \lambda^2 + \text{fin. } \phi'^2) \right)}{\text{cof. } \lambda \text{fin. } \phi' \text{cof. } \omega - \text{fin. } \lambda \text{cof. } \phi'}$.

From the investigation of Mr. Playfair's problem, therefore, we have obtained very accurate rules for finding ω from λ , ϕ and D , and ϕ from λ , ω , D .

Now in order to find an equation expressing the relation between the latitudes, difference of longitude, and one of the azimuths, let AL be perpendicular to the meridian PAO in A meeting FE in L , and BK perpendicular to AL . Join KE , and the angle BKE will be equal to the spheroidal angle OAB , and BFE equal to the angle APB or difference of longitude. Let $OAB = BKE = A$, $BFE = \omega$, and λ , ϕ as before, then will LE be $= (CD - CF) \cotang. \lambda + FE - AD$, $KE = (CD - CF) \text{cof. } \lambda + (FE - AD) \text{fin. } \lambda$, and $\cotang. A = \frac{KE}{BE}$.

Whence by substituting the values of CD , CF , AD , BF given above, and rejecting the powers of c higher than the first, there results

results $\cot. A = \frac{\text{cof. } \phi \text{ fin. } \lambda \text{ cof. } \omega - \text{fin. } \phi \text{ cof. } \lambda}{\text{cof. } \phi \text{ fin. } \omega} - \frac{2c}{a} +$

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

$\frac{(\text{fin. } \lambda - \text{fin. } \phi) \text{ cof. } \lambda}{\text{cof. } \phi \text{ fin. } \omega}$. But if ω be the vertical angle of a

spherical triangle, and $90^\circ - \phi$, $90^\circ - \lambda$ the sides; also A' the supplement of the angle opposite the side $90^\circ - \phi$; then

will $\cot. A' = \frac{\text{cof. } \phi \text{ fin. } \lambda \text{ cof. } \omega - \text{fin. } \phi \text{ cof. } \lambda}{\text{cof. } \phi \text{ fin. } \omega}$, and conse-

quently $\cot. A = \cot. A' - \frac{2c}{a} + \frac{\text{cof. } \lambda (\text{fin. } \lambda - \text{fin. } \phi)}{\text{cof. } \phi \text{ cof. } \omega}$,

Whence as A, A' are nearly equal, we obtain $A = A' + \frac{c}{a}$.

$2 \text{ fin. } A'^2 \times \frac{\text{cof. } \lambda (\text{fin. } \lambda - \text{fin. } \phi)}{\text{cof. } \phi \text{ cof. } \omega}$, which will be found

abundantly accurate in practice, but if the square of c be re-

tained, and $\frac{2 \text{ fin. } A'^2 \text{ cof. } \lambda (\text{fin. } \lambda - \text{fin. } \phi)}{\text{cof. } \phi \text{ fin. } \omega}$ be put $= M$,

and $\text{fin. } A'^2 \text{ cof. } \lambda \times \frac{\text{fin. } \lambda \text{ cof. } 2\lambda + 2 \text{ fin. } \lambda \text{ fin. } \phi^2 - \text{fin. } \phi}{\text{cof. } \phi \text{ fin. } \omega}$

$-\cot. A \times M^2 = N$, A will be $= A' + M \times \frac{c}{a} - N \times \frac{c^2}{a^2}$

more accurately. The rule may be thus expressed; let the colatitudes of the two places, and their difference of longitude form the sides, and contained angle of a spherical triangle, of which find the base angle at the place whose latitude is λ , and let it be $= \alpha'$, and the corresponding angle α of the

spheroidal triangle will be $= \alpha' - \frac{c}{a} \times 2 \text{ fin. } \alpha'^2 \times$

$\frac{\text{cof. } \lambda (\text{fin. } \lambda - \text{fin. } \phi)}{\text{cof. } \phi \text{ cof. } \omega}$. In like manner if β' be the angle of

the spherical triangle at the place whose latitude is ϕ , the corresponding angle β of the spheroidal triangle will be found to

be $= \beta' - \frac{c}{a} \times 2 \text{ fin. } \beta'^2 \times \frac{\text{cof. } \phi (\text{fin. } \phi - \text{fin. } \lambda)}{\text{cof. } \lambda \text{ cof. } \omega}$. Con-

sequently $\alpha + \beta$ is $= \alpha' + \beta' - \frac{c}{a} \times \frac{\text{fin. } \lambda - \text{fin. } \phi}{\text{cof. } \phi \text{ cof. } \omega} \times$

$2 \text{ fin. } \alpha'^2 \text{ cof. } \lambda - \frac{c}{a} \times 2 \text{ fin. } \beta'^2 \times \frac{\text{cof. } \phi (\text{fin. } \phi - \text{fin. } \lambda)}{\text{cof. } \lambda \text{ cof. } \omega} =$

$\alpha' + \beta' - \frac{c}{a} \times$

$\frac{2 (\text{fin. } \alpha'^2 \text{ cof. } \lambda^2 - \text{fin. } \beta'^2 \text{ cof. } \phi^2) (\text{fin. } \lambda - \text{fin. } \phi)}{\text{cof. } \lambda \text{ cof. } \phi \text{ cof. } \omega}$; but by

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

spherics $\sin. \alpha' \cos. \lambda = \sin. \beta' \cos. \phi$, therefore $\alpha + \beta = \alpha' + \beta'$ if we reject the powers of c higher than the first, which are insensible. Hence the principle laid down by Mr. Dalby, viz. that in a spheroidal triangle, of which the angle at the pole and the two sides are given, the sum of the angles at the base is the same as in a spherical triangle, having the same sides, and the same vertical angle, is verified, and therefore the concluding remark of Mr. Playfair is hasty and ungrounded. But perhaps Mr. P. in his solution retains the second power of c , and objects to Mr. Dalby's principle because its coefficient does not vanish except in particular cases. If so, the objection is frivolous, as the difference is so small as scarcely to be computed in the cases that occur in practice, and too small in any case to lead into error or deserve attention.

The preceding theorems for the solution of spheroidal triangles will be found extremely accurate, when applied to such as are described on the surface of the earth, on account of the smallness of c in comparison of a ; and in like manner others may be deduced, when different parts of the triangle are supposed given. Thus if λ , α and D be given; let a spherical triangle be constructed with one side $= 90^\circ - \lambda$, another $= d$, such that $\sin. \frac{1}{2} d = \frac{D}{2a}$, and the contained angle $= \alpha$; find the other side $90 - \phi'$, the angle at the pole ω' , the other azimuth β' and we shall have equations of this form $\phi = \phi' + Pc$, $\omega = \omega' + Qc$, and $\beta = \beta' + Rc$, where P , Q , R are functions of λ , α , D , which may be derived from the foregoing equations by proper artifices. But the formulæ, except in particular cases, will not be found so simple as the former. These, however, and some new theorems applicable to trigonometrical surveys, I shall delay to some future communication. In the mean time, it may not be foreign to the subject to remark, that the arch of the meridian, said to have been lately measured in the Myfore country in the East Indies, by Brigadier Major Lambton, gives the degree, in latitude $12^\circ.32' N.$ equal to 60494 fathoms; which compared with that of 60795 in latitude $47^\circ.24' N.$ gives $\frac{1}{300}$ for the compression at the poles, a quantity differing very little from the mean deduced from all the measures of degrees. But it must be confessed that there appear at present to be two very important objections against the accuracy of Major Lambton's measure. The Myfore, on account

Arch of the meridian measured in the Myfore country,

account of the irregularity of its surface and its uncertain elevation above the level of the sea, is an unfit country for ascertaining a nice point of this kind, however well situated for connecting the eastern and western sides of the peninsula by a geographical survey; and the Major, from his account in the 7th volume of the Asiatic Researches, seems to be somewhat doubtful of the exact length of his chain. Nevertheless it is probably to India that we must look for the means of finally deciding this long contested question. There, and there only, we find many tracts of country highly favourable to this purpose; and it is to be hoped that the East India Company, while anxious to ascertain the extent of its possessions, will not entirely neglect the interests of science.

—perhaps in some degree uncertain from causes stated.

I am, Sir, &c.

PERIGRINUS PROTEUS.

Portsmouth, April 7, 1804.

IV.

Description and Drawing of a Hand Drill for sowing Peas, Beans, &c. Communicated to the Society of Arts, by the Inventor, Mr. ROBERT GREEN, of Westwratting, Cambridgeshire.*

To CHARLES TAYLOR, Esq.

SIR,

I HAVE invented an engine to sow peas, with which I have sown all my peas, to the amount of 40 acres, at the price of 1s. per acre, and think that my peas are much better than those sown any other way. It is also on a very simple plan, and the expence of it when complete is not 2l. It is used by manual labour, without any horse; and it will draw the drill, sow the peas, and cover them at the same time, and will sow them much rounder than any other I have yet seen. I likewise find I can do it much cheaper than with any horse, and am of opi-

Very economical drill for sowing peas.

* To whom the silver medal and ten guineas were voted by the Society. A complete machine is placed in the repository of the Society.

Very economical drill for sowing peas. nion that it sows much better than any drill I have seen. If the Society wish it, I will send a model for their inspection.

I am, SIR,

Your obedient Servant,

ROBERT GREEN.

Westwratting, Cambridgeshire,

June 27, 1802.

SIR,

I HAVE sent the engine for sowing peas, in order that it may be laid before the Society for the Encouragement of Arts, &c. I intended to have sent a model of it, but afterwards thought that the engine itself would be more acceptable to the Society. I made it myself, and have sown with it 26 acres of land in my own occupation. Mr. Piper, a near neighbour of mine, has sown with it five acres; and Mr. Cock, of Blunt's Hall, Wratting, in Suffolk, 25 acres, at the expence of 1s. per acre. Several other gentlemen had drills of me for sowing peas. If I give my men 1s 6d. per acre, they will sow for me two acres in one day. I can with my own hand sow one acre in five hours, and at the same time sow the peas, draw the drill, and cover them, and make full twelve drills and a half to the rod. I likewise produce the plant much handsomer than any other seen in our country, and at a very trifling expence. By this too, the labour of horses is spared, which we find to be a very material circumstance. It will be a most excellent engine for gardeners in the neighbourhood of London; for I will be bold to say, that no man can sow with his hand, so as to equal this, at a very trifling expence.

I have spent much time in making implements of husbandry, but have made none so useful as this; for it is simple in its construction, may be purchased by any man, the expence being so trifling, and saves the labour of horses.

I remain, Sir,

Your most obedient Servant,

Westwratting.

ROBERT GREEN.

Description of the Engraving of Mr. ROBERT GREEN'S Hand-Drill, for sowing Peas, Beans, &c. Plate II. Very economical drill for sowing peas.

Fig. 3. a a, The wheels placed upon a wooden axis *b*, which is square at each end, but round in the centre. The square ends of the axle have holes throughout them, at different distances in order to deposit the feed at nearer or more distant intervals, as may be wanted.

c, The box in which the feed is placed : the axis *b* is cylindrical, and has holes made therein proper to receive the seeds, which by the revolution of the axis are carried forwards, and fall through an iron tube into the ground opened for them by the share *d*. When deposited in the ground, they are covered, or the earth drawn over them by two iron pins or scrapers *e*, fixed on each side of the tube, and extending some inches behind it.

f f, The handles of the drill-machine, by which it is pushed forwards.

Fig. 4. Shows an enlarged view of the interior of the feed-box *c*, above mentioned, and holes for the seeds placed in a spiral line, in order to drop the seeds more regularly.

g, Is a small brush within the box, which rubs against the cylinder, to keep the holes clear to receive the seeds.

Fig. 5. Is a section of the machine, where *a* is part of the feed-box ; *b*, the round part of the axle, which delivers the feed.

d, The share which opens the earth,

h, The tube through which the seed falls.

i, The mouth of the tube, and one of the fins which draws together the soil, and covers the feed.

k, Is a small door, to be opened occasionally, if the roller or tube are out of order;

l, A strong flat board, to which the iron work is screwed.

Fig. 6. Shows an enlarged plan of the iron work, when the machine is reversed,

d, Is the share,

i, The hole from which the seed is dropped.

e e, The two fins, or scrapers, which collect the earth and cover the feed.

N. B.

N. B. The length of the upper rim of the feed-box of the machine in *Fig. 3*, being fifteen inches, will serve as a standard for the measure of the other parts*.

V.

Enquiries concerning the Methods of investigating the Course and Velocities of a Body supposed to be projected from the Moon to the Earth. By a Correspondent.

To Mr. NICHOLSON:

SIR,

Theory of La Place, that stones fall from the moon.

AMONG the various theories concerning the stones fallen upon the earth in different parts of our globe, (the subjects of Mr. Howard's Analysis), that which De la Place has ventured upon, though apparently incapable of proof, seems the least improbable, namely, that they are projections from the lunar volcanos. Surely, Sir, whether this is or is not *possible*, may be mathematically demonstrated, certain data being allowed. If you, or any other able mathematician, could find time to do this, it would be a great gratification to see the solution of these questions.

Data for computation.

We must assume as data, That the density of the materials composing our satellite, is precisely similar to the density of our globe; and that the moon has no atmosphere to resist the projection from its surface, or so small a one as not to be calculated upon, since, probably, it is rare and low, perhaps not more than one-sixth of a mile high.

Now their relative bulks and distances are *pretty well* ascertained, and we will take them to be *precisely* known.

Their comparative centrifugal forces may easily be calculated, as the moon revolves about 27 times slower about its axis than the earth about hers.

These points must be settled, because particularly the last must have considerable effect in determining the line such projected body would describe.

* This account was also supported by a certificate from eight persons who had used the drill in sowing 113 acres of land, and of eleven farmers who witnessed and approved its operation.

Let us suppose too, that this body weighs one hundred weight.

What will be the velocity requisite to overcome the mutual attractions of the moon and this body, so as to project it beyond their powers; remembering that its velocity will be continually diminishing as long as any attracting power acts upon the projectile, and calculating the aid it would receive from the centrifugal force. Results to be investigated.

In the journey of 240,000 miles through which it has to travel in a direct line, is there any free space beyond the sphere of the moon's attraction and that of our planet's? If so, at what distance from the moon will that be found? and at what distance from the earth? and with what velocity may it be supposed to travel through that space? of course it will be much slower than when first projected.

As upon entering the limits of the earth's attraction its velocity will be again increased, *quære* its rate of travelling to the earth; and, taking the three reckonings into account, in how many days and hours can it be mathematically demonstrated that it would reach our solid globe?

Acted upon by the united forces of projection, centrifuge, (if I may coin a word) and the motion of the moon in its orbit, and the force of attraction and orbital motion of the earth, what will be the precise line it may be presumed to describe in its course?

Would it not have a revolving motion during some part of its course?

What are the calculations by which we may be enabled to judge that 3 or 5 times the velocity of a cannon-ball, at the moment of projection, would enable it to counteract these impediments?

It has surprized me, that the numerous late publications mentioning this theory, have not detailed the mathematic processes by which it seems capable of being solved, or of proving its fallacy.

I hope, Sir, you will not think this obtrusion impertinent. You obligingly and satisfactorily complied with my request in a note concerning Col. Blaquiere's gun to throw double headed shot; this has emboldened me to express my wishes on the subject of this letter. But I do not wish to have my ignorant questions

load

load your valuable Journal, though I shall feel myself much obliged by any further information upon this head.

I remain, Sir,

Your most humble servant,

A CONSTANT READER.

VI.

*Drawing and Description of a Three-Furrow Plough. By the Rev. EDMUND CARTWRIGHT, of Woburn, Bedfordshire.**

To CHARLES TAYLOR, Esq.

DEAR SIR,

Economical
three-furrow
plough.

I ENCLOSE you a certificate of the performance of a plough of my invention, which has occasionally been at work through the whole summer. For this last fortnight, it has been used for ploughing in wheat under furrow. Though a very useful instrument at all times, it is particularly valuable at the seed times, and the turnip season; because at those times it frequently happens you lose the most favourable opportunities, for want of ability to execute your operations with sufficient dispatch.

Weekly saving
4*l.* 16*s.*

I need not calculate to you the saving on the use of this plough. It is worked (on light land I mean) with a pair of horses, without a driver. A pair of horses and a ploughman cannot be laid at less than 8*s.* per day. As two sets of these are saved, the weekly saving by the use of this plough amounts to no less than 4*l.* 16*s.*

Applicable to
level lands in a
tolerable state of
cultivation.

Useful, however, as I find this instrument on our light level lands, I am not so partial to it, to suppose it is equally calculated for all soils, or all kinds of ground. For instance, where the ground is very uneven, or the ridges are narrow and steep, I would not use it; neither when the land is very foul with root weeds. In all these cases a single plough is certainly to be preferred: but in all cases where the ground is in a tolerable state of cultivation, and where it lies reasonably level, it will be found a most valuable acquisition.

* From the Transactions of the Society of Arts, who voted him the silver medal. A model is placed in their repository.

I will

I will thank you to communicate this letter, and the certificate accompanying it, to the Committee of Agriculture; and and if they are disposed to think favourably of this invention, I will send you a model for their inspection.

I am, DEAR SIR,

Your very obedient servant,

EDMUND CARTWRIGHT.

Woburn, Oct. 20, 1802.

This is to certify, that the three-furrow plough invented by Certificate; the Rev. Edmund Cartwright, ploughs a surface of twenty-seven inches each bout, and that on light land a pair of horses regularly ploughs three acres per day with it in a workmanlike manner.

JOHN DUCKITT, *as Bailiff to
his Grace the Duke of Bedford.*

WILLIAM BAXTER, *Assistant.*

June 21st, 1802,

DEAR SIR,

YOU herewith receive the model of my three-furrow Why this plough
saves power. plough.

The saving of hands, and consequently of expence, in a plough of this kind, is obvious; but why there should be a saving of power, may require to be explained.

I need not observe to you, nor to any man who considers the action of a common plough, that a very material part of the labour in ploughing, arises from the friction of *the land side* and *the sole*; of the one against the side of the furrow, of the other against the bottom. In a single plough a certain length and width are required in those parts of it, to make it go steady; and even then the effect would be imperfectly obtained, did not the ploughman assist by the leverage of the handles of the plough. Hence it is clear, that the less disposition any plough has to follow the draught in a strait line, the greater is the labour of working it, because the ploughman in that case is to exert a greater power of leverage to keep it steady. On the contrary, when two, three, or more ploughs are combined, they serve to steady each other, and require comparatively very little power of the lever to keep them in a strait line. Under these circumstances,

Economical
three-furrow
plough.

circumstances, neither the first nor second plough has any sole or land-side whatever; and even the third does not require so much of either as a single plough. I calculate the saving of power from the consideration alone, as equal at least to one plough. What farther power is saved, I attribute to the lightness and compactness of the instrument.

I am willing to think the simplicity of its construction, and the manner of fixing the plough (consisting but of two parts) to the beam, will not escape your observation. When the cutter (for as it is both coulter and share, I can give to it no other single name) requires to be sharpened, or new-laid with steel, by drawing the two bolts the whole is set at liberty.

I make the ploughs to fit each beam indiscriminately; because when the land is too strong, or too foul, to work the three, I take off the second plough, and transfer the third into its place.

You will observe the centre of the whiple-tree shifts. By this contrivance, the power of the horses is equalized, though they may be unequal in strength, the longer lever being given to the weaker horse.

Should the Society wish for any farther information, it will give me pleasure to furnish them with it,

I am, DEAR SIR,

Very truly and sincerely, yours,

EDMUND CATWRIGHT,

Woburn, December 14, 1802.

Charles Taylor, Esq.

Reference to the Engraving of the Rev. EDMUND CARTWRIGHT'S Three-Furrow Plough.—Plate II. Fig. 1, 2.

Description of
Mr. Cart-
wright's three-
furrow plough.

Fig. 1. A B, the two wheels of the plough, the wheel B being full one-seventh in diameter larger than the wheel A.

C D E, the three beams of the plough, of which C is the shortest and E the longest: these beams are fixed in the strong cross piece F, at equal distances from each other, and braced by another cross piece from C to E.

G H I, the three cutters which answer the purpose of both coulter and mould-board, each being formed together, or made of one piece of beaten iron. Each cutter is screwed to its beam by the flanging-iron K.

L M

L M, the two handles of the plough, the lower extremities of which are fixed in the two outer beams C E, and connected by a cross piece N, to make them firmer. The handle L is longer than the handle M, in the same proportion as the beam C is shorter than the beam E.

Description of Mr. Cartwright's three-furrow plough.

O P, two upright pieces of iron fixed in the cross piece F, having two holes at their summits for the reins to pass through which guide the horses.

S, an iron bar which slides up and down near one end of the cross piece F, to raise or lower the wheel A.

Fig. 2. Shows a detached portion of the strong cross piece F, to explain the manner in which the whipple-tree shifts (R) are fixed in front of that cross piece, so as to regulate or equalize the power of the horses.

S, a bar of iron, the lower part of which forms the axis of the wheel A, the upper part slides in a groove, in the cross piece, F, and has holes at different distances. It may be retained at any height by an iron pin T, which passes through the cross piece, and one of the holes of the iron bar. The real plough is nine feet long to the extremity of the handles and each cutter turns a nine-inch furrow; from centre to centre of the beams, being nine inches.

VII.

On the State of Science among the earlier Nations of Antiquity; and more especially of those Researches which constitute the Subjects of Alchemy. In a Letter from E. P.

To Mr. NICHOLSON.

Roscommon, April 1, 1804.

SIR,

IT is an authenticated fact, that much of our late scientific acquisition, and many of those facts which the experimental genius of the present age is daily bringing to light as original discoveries; were well known in more ancient periods of the world; and there is abundant reason for supposing that, in chemistry and metallurgy, the philosophers of those ages were superior to those of the present day.

Reasons for thinking that the modern sciences were known to antiquity.

But

But we must mount up much higher than what are called the dark and barbarous ages of modern Europe, or even of any of those revolutions in the East of which history has transmitted any detailed accounts.—Science had *began to decline previous* to the earliest historic relation which is extant, and there appears sufficient evidence that the Greeks and Egyptians, in their hieroglyphics, their allegoric devices, and in their mythologic mysteries, which they had blindly received from their enlightened predecessors, were recording for posterity a series of physics, of which they were ignorant, and which is now gradually unfolding.

Transmutation
of metals.

The universal rage for penetrating into the science of alchemy, not only indicates the scarcity and value of gold in all ages, but evinces, I think, that there has always existed some *tradition* of such a transmutation having been once effected.

Decomposition
of water.

The decomposition of water into different gases was certainly once known; and our recovery of that sublime phenomenon, which seems the key to the great laboratory of nature, bids fair to restore to mankind the most important facts which have lain in obscurity for so many centuries.

Alchemy.

Of these, alchemy will probably be one; it has deeply excited the attention of some excellent chemists in this island, with whom I have the honour of being connected: of any progress we may hereafter make, you shall be immediately apprised; and if you, or any of your ingenious correspondents are engaged in a similar course of experiments, we might mutually assist, and abridge each others labours,

I have the honour to be,

Your sincere friend and zealous well-wisher,

E. P.

P. S. A series of experiments on this subject will probably throw considerable light on the lunar (more properly lunatic) stones, the rational phenomena of which you have taken so much laudable pains to elucidate.

VIII.

Description of a Machine for Clearing great Roads from Mud.

Communicated to the Society of Arts, by the Inventor Dr. JOHN WINTERBOTTOM, of Newbury, Berks.*

IN a description of this machine, I shall briefly notice the five principal parts of which it is composed; the frame, the scraper, the chain, the sledge, and the pole; because a very accurate model accompanies this paper, made upon the usual scale of one inch to a foot.

Machine for
clearing roads
from mud.

The *frame* (see *Plate I. Fig. 1.*) consists of two pieces of timber AA, which at one extremity are formed into a pair of shafts BB, and at the other are strongly united by three transverse pieces CDE.

The *scraper* F is placed under this frame-work, in an oblique direction, at an angle of 30° , between two of the transverse pieces CD, and consequently forms an angle of 150° with the line of draught. By this position of the scraper, the machine, when used, actually clears itself from the mud as fast as it is collected, and removes it into a heap on one side, after the manner of a plough.

The *chain* G is connected with a piece of iron-work H, which projects from the lower end of the scraper; for here additional power is required, as the whole body of the mud, which has been collected, must pass off by this extremity. Some advantage has also been gained by making this end of the scraper shorter than the other.

The *sledge* II is constructed upon the upper part of the frame, that by inverting the machine it can be transported without injury to the scraper, over the most rough and stony roads, or pavements, to those places where its use is particularly required.

The *pole* K, which is moveable, serves the purpose of a rudder, that when the machine happens to be forced by any great weight of mud, or solid body of earth, &c. from its proper direction, it can be easily restored to its former position: and it may also be observed, that the moderate pressure

* To whom the silver medal was voted. There is a model in the Society's Repository.

Machine for
clearing roads
from mud.

of the hand upon the pole tends to make the machine steady, and therefore causes it to work to more advantage. In the model, the pole is made only ten inches long, instead of fifteen, that it might occupy less space in the box. The plates in front of the scraper, and upon the sledge, are made of cast-iron.

OPERATION.

For the operation of the machine, two men and four horses are required: one man to drive the horses, and another to take the management of the pole and the direction of the labour to be performed. The horses are to be worked double, as commonly practised, two being employed to draw by the shafts, and two by the chain above described. But the manner of using the machine will be best understood by the following sketch. *Plate I. Fig. 3.*

The first progress of the machine marked No. 1, commencing from the arrow-mark, will remove the mud in a line to the right; the first return, No. 2, will remove another part of it to the left. The second progress, No. 3, will take up what is left by No. 1, besides the quantity which is upon the space now to be passed over, and will remove it all to the right. The second return, No. 4, will operate in a similar manner with regard to No. 2, and remove that to the left. Thus, by four lengths, more than twenty feet wide of a road can be cleared; and this has been frequently performed in the presence of several persons. The number of lengths may be increased at pleasure, according to the width of the road.

In the neighbourhood of London, where there is incessant travelling, it would be adviseable to use two machines at the same time, one immediately following the other, as in No. 1 and 3, which will leave a space sufficiently wide for the largest carriage to pass, without disturbing the mud already scraped up.

There is one advantage in the operation of this machine worthy of being noticed, which is, that by the use of it the road is made more even and smooth, the small holes being filled up by the more solid parts of the mud; whereas, when roads are scraped in the usual way, by hand, all the irregularities are increased, and become the future deposits of water; and it is universally known that these puddles, as they are called, are the chief cause of the destruction of roads.

It has been observed, that stones are sometimes forced up by the machine; but it appears to be those only which project in such a degree as to be dangerous to the traveller, and which require to be broken for the more effectual mending of the road.

Machine for
clearing roads
from mud.

I can say nothing concerning the effect of the machine upon dusty roads, having had no opportunity of trying it at that season of the year. When, indeed, the roads are watered, as about London, there is no doubt but a great quantity of that dirt may be removed, which, in a few hours of scorching sun, would again be converted into a body of dust.

If it should be objected, that the machine is too large, and that a smaller one, which might pass over half the space of ground that this does, and might be worked by two horses, would be better; I must beg leave to answer, that, in my opinion, with a less one there would be much labour to little purpose; because this machine, which passes over a space of about six feet and a half, will not, in some places, when the roads are very wet and very deep, leave more than three feet clear, the mud on each side falling in and filling up, to a considerable extent, the space already passed over: it must therefore be obvious, that, under similar circumstances, the track of a smaller one would almost instantly be obliterated.

TESTIMONIALS.

I am so anxious that the Society should have ample satisfaction on this head, that I should be happy if they would, before finally determining on the utility of this machine, condescend to make some inquiries in this part of the country, where it has been publicly tried.

I can however mention, with some pleasure, that several gentlemen, acting as Commissioners of the Roads, have honoured me with their attendance during various experiments; and, having witnessed the very powerful effects of the machine, they have given it their public approbation at the last monthly meeting, when the following entry was made in their minute-book:—

“ At a meeting of Trustees of the London and Bath roads, held at the Globe Inn, Newbury, on Monday, the 21st of February, 1803. At this meeting were present, James Croft, Esq.

Machine for
clearing roads
from mud.

Esq. Frederick Cowslad, Esq. Rev. Thomas Best, Mr. Richard Baily, Mr. Thomas Clark, Mr. John Baily, Mr. Joseph Tanner, Mr. Thomas Pocock.

“Resolved, that the machine invented by Dr. Winterbottom, for scraping off mud from turnpike roads, will be of public utility, and save considerable expence of labour.”

After this public testimony in its favour, I might perhaps be excused from producing the certificates of a few individuals: it will, notwithstanding, be necessary to give some estimate of the probable saving to be expected from its use.

In all trials made previous to the 21st of February, the machine had been worked upon no measured extent of ground; but the general effects were such, that several persons of great experience in the management of roads, rated the daily work of one machine only as equal to the labour of fifty or seventy men: fifty being the lowest estimate ever named.

A few days ago I directed some work to be done by measure; and I can now state it as the opinion of two very competent judges, that one machine will clear three miles in a day, twenty feet wide (consisting of four lengths, and making the day's work twelve miles) which is considerably more than 120 men can do in a day.

	<i>l.</i>	<i>s.</i>	<i>d.</i>
120 men, at 2s. per day	12	0	0
Four horses and two men can here be hired to work the ma- chine for the day, at	-	1	5
		<hr/>	<hr/>
Difference	10	15	0

At a distance, where carriages run principally in the centre of the road, the chief business in the management of it consists in keeping the sides clear and open. One machine may therefore be occasionally employed in outside work only; that is, may go six miles, and return, (making twelve miles, as just mentioned) with the saving already given.

Whatever surprise these calculations may occasion, the Society will perhaps be satisfied that I have not over-rated them, when I produce the result of a fair experiment, made on the 25th of February, in the presence of four trustees (Frederick Page, Esq. Francis Page, Esq. Mr. Thomas Clark,

Clark, and Mr. John Baily) and others, by which it appears, Machine for clearing roads from mud. that two miles, by measure, on the road to Reading, were cleared from mud, to the extent of 18 or 20 feet wide, by two machines, in the space of two hours and a half, by the watch; and the work was judged to be equal to the labour of more than eighty men in a day.

The success of this experiment was so satisfactory to the above-named trustees, for I was not present on the occasion, that they directed, without my knowledge, the remainder of our district on this road, extending seven miles, to be cleared in the same manner; and I can now declare, with some degree of pleasure, that this was actually completed by two machines in one day, viz. on the following day, the 26th of February. Of this day's work I have heard it affirmed, by an experienced surveyor, that it could not have been done in one day by 400 men.

I confess that I am myself unable, from the want of practical knowledge on this subject, to form a comparative estimate between the work done by this machine and by hand: I have therefore sought for information from persons of respectable characters, who have been surveyors, or renters of roads for many years: and I have been assured, as well by those who were present at the experiments, as by others who examined the roads afterwards, that it would require sixty men a mile, to do the work in one day, which a single machine will accomplish at four lengths; and it has been already shown, that three miles can, without difficulty, be cleared in a day: one machine will therefore do the work of one hundred and eighty men. But I have taken the average at only two thirds of this estimate, viz. at forty men per mile instead of sixty, being more willing that the power of the machine should at present be under-rated, than that the public should be deceived or disappointed concerning it.

The trustees of the London and Bath roads, being desirous of having these two machines, which had been constructed on my account, and under my own inspection, for making the experiments, I have consented to dispose of them: and as far as I am now able to judge, the price of a machine complete will be about ten guineas.

Finally, I must beg leave to advise those who are inclined to make a trial of this machine, to be careful whom they in-

Machine for
clearing roads
from mud.

tend to employ in the construction of it; for I can assure them, that it is not sufficient to attend only to the form of the model; but it is absolutely necessary that the different parts, and especially the two braces behind, should be firmly put together, otherwise it will be impossible for it to withstand the force that must sometimes be exerted upon it by four, or perhaps by six horses. The scraper may be made of beech or elm, &c. but the other parts ought to be made of ash; and I must particularly recommend these materials to be well seasoned; all which circumstances were minutely attended to in the two machines which were made for me by Mr. Joseph Mofs, of Greenham, near Newbury.

JOHN WINTERBOTTOM.

Certificates from Mr. George Goddard, Greenham, near Newbury, Mr. Francis Page, Mr. Frederick Page, Mr. John Baily, and Mr. Thomas Clark, accompanied the above paper; stating, that on the 25th of February last, two miles had been cleared in two hours and a half, by two of Dr. Winterbottom's machines.

Reference to the Engraving of Dr. WINTERBOTTOM'S Machine for Clearing Roads from Mud.—Plate I.

Fig. 1, AA. Two pieces of ash timber, forming at one extremity a pair of shafts, *BB.*

CDE. Three transverse braces, to secure firmly the timbers above-mentioned.

F. The iron plate, or front of the scraper, fixed within the braces *CD*, at an angle of thirty degrees, extending on the further side two feet, and on the nearer side one foot and a half beyond the timbers.

G. An iron chain, one end of which is fastened to the outside of the timber *A*; the other end of the chain may be moved nearer to, or further from that end of the scraper which deposits the mud, by means of notches in the iron muzzle *H*, fixed to the scraper, and which regulates the draught of the horses attached to the ring at *G*.

K. The pole, or handle, to be made fifteen feet long, which passes through the strong holdfasts in the braces *CD*. This pole acts as a lever, as the scraper may be raised or sunk
by

by it, at pleasure. The person who holds it may direct the scraper in its proper line, and assist it in overcoming any obstacles it may meet with in its way, or in giving it additional pressure where necessary.

Machine for
clearing roads
from mud.

II. Show the two parts of the machine which form the feet, or sledge part of the machine, on which it slides when reversed, and which enable it to be removed from place to place, when the scraper is not in use. These feet are strongly fixed to the timbers AA, and strengthened by a transverse brace betwixt them.

L. Is the iron chain, or back band, which lies upon the cart-saddle of the horse in the shafts, and which supports the shafts.

Fig. 2. Shows, on an enlarged scale, the iron-work, fixed on the outside of the shafts, to which the chain and horse are attached.

Fig. 3. Describes, in a small extent, the track usually made by the scraper in a large way, in four rows, commencing at the arrow mark, in the track No. 1, returning after it has gone any length required by the track No. 2, proceeding again by the track No. 3, and forcing the mud collected by the tracks No. 1 and 3 to the right side of the road, and, on its return by the track No. 4, depositing the mud of the tracks No. 2 and 4 on the left of the road, as is more fully described in the preceding account, and thus clearing from mud a breadth of road twenty feet wide, by four passages of the machine.

IX.

Description and Drawing of an Hydraulic Machine, with an Account of several Inventions of early Date, which have been since brought forward by later Inventors. Extracted from a Foreign Work published early in the last Century. By a Correspondent.

To Mr. NICHOLSON.

SIR,

I HAVE lately met with a work in French, entitled *Recueil d'Ouvrages curieux de Mathematique et Mecanique, ou Description du Cabinet de Mons. Grollier de Serviere*, in quarto, Account of the publication of the Museum of the Museum of Mons. de Serviere.

Turnery.

printed at Lyons in 1719. The book is divided into three parts, the first of which contains curious engravings of delicate, eccentric, swash and rose-work turnery, the methods of producing which are to be found in Moxon's *Mechanic Exercises*, and in other later works on that subject. Upon this part I shall make no other remark, than by demanding of your correspondents, whether the oval chuck, or engine for turning ovals, was unknown so late as the period above specified. If *Monf. de Serviere* had known it, I should suppose he would have introduced ellipses among the various figures he has exhibited. The second part of the work consists of clocks or time-pieces, more remarkable for some singularity in figure or structure, than any improvement in the art of measuring time; and, the third part contains models of hydraulic machines, with some engines for military and other purposes. Many of the hydraulic machines appear better calculated to be shewn in a model, than carried into effect on a larger scale, and, among the other engines, I see nothing which, at the present day, would much conduce to the entertainment of your readers. I have, therefore, sent you a drawing of the hydraulic engine exhibited in his 49th plate, and after the description, I will mention a few other objects which have been thought of more modern invention.

Time-pieces.

Hydraulic machines.

Hydraulic machine described. It consists of two pinions working in an elliptical box.

In *Plate IV. Fig. 2.* A and B represent two solid pinions made in wood or metal, and occupying all the interior space of the oval box or chamber CD; in which they turn freely and take into each other. The chamber CD is to be well and solidly made, having an opening below at D, as in the figure, and also at E, where the aperture corresponds with the bore of a pipe F, applied and fixed to the same. Every other part is well closed and secured. *Fig. 3.* represents the cover or cap.

This chamber is properly fixed under the water of the well, or cistern, out of which the supply is to be obtained, and in this situation, the elbow or handle G, *Fig. 1. Plate IV.* is fixed on the square of the axis A. This handle is connected with another H, by the iron sliding piece I, which moves upon the fixed pin K, and obliges its two extremities constantly to move alike. Whenever therefore the handle H is turned by the first mover M L, the other arm G must also revolve together with its pinion, and by consequence the other pinion B.

When

When the two pinions turn (so that their upper teeth may ^{Its action,} constantly approach each other, while the lower recede,) the water which lies between their teeth in the lower part D of the chamber, will be carried round till it arrives at the part C, where it will be compressed by the continual augmentation of water, which is brought thither between the teeth, (or rather by the perpetual diminution of the space between the two uppermost teeth that touch the chamber). So that the fluid will enter the pipe F, and be forced to the intended place.

Thus far I have translated from Monf. de Serviere. ^{The Remarks on the engine.} The machine appears to be ingenious, and though liable to the objections of wear which you very properly urged against the schemes of O. B. and others*, seems preferable to them in several respects. I think it would be an improvement, to cause the axis A and B to drive by a connection of external wheel-work, instead of depending upon their interior teeth, which require to be well figured and fitted, and kept so, by not loading them with pressure of one surface against the other. It seems scarcely necessary to remark, that we have better methods of connection for work at a distance in our cotton gear and elsewhere, than the sliding piece I; and lastly, I would propose it as a mathematical exercise to determine the law of the velocity of the fluid through F, when the rotation of the machinery is uniform.

Among the machines which I believe have been considered ^{Old engines re-invented.} as of later date, but are found in this work, are the engine mentioned in Desaguliers, for raising water by a losing and ^{Losing and gaining bucket.} gaining bucket, and regulated by a fly; the chain pump, for ^{Chain pump.} which Cole had a patent, but which is known to have been of very ancient use in the Chinese Empire; the horse mill ^{Adam Walker's horse mill.} worked by the wheels of a carriage, as lately proposed by the ingenious Adam Walker; and the gouty chair of Merlin, worked by two small handles connected with a pair of small wheels attached to the fore feet.

I am, SIR, with esteem,

Your obliged Reader,

R. B.

X.

An Examination of Dr. Wollaston's Experiment on his Periscopic Spectacles. By Mr. WILLIAM JONES, F. Am. P. S.

To Mr. NICHOLSON.

SIR,

Preliminary observations.

THE inferences that Dr. Wollaston has thought it best to publish in your last month's Journal, instead of a direct reply to my refutation of his new principle of spectacle glasses, are of themselves sufficient to convince any impartial person of the validity of the objections advanced by me in your preceding Journal; and, notwithstanding an extraordinary experiment he has therein related, as made only by himself, I should not have thought it requisite to trouble your readers again, but for the unfounded imputation that he has declared against me, that of having by an experiment deceived myself. I trust, Sir, I may be allowed, in contradiction to this, to observe, that, after more than 20 years experience in the practice of my profession, such as daily administering to decayed vision, and employment in the construction of all kinds of optical instruments, I should not be acquainted with all the various properties of lenses, singly or combined, and especially of so simple and well known a form of lenses as adopted by him, is an idea, that I am confident he will not be able to impress upon the minds of the public. I suggested no new experiment, nor was any one wanting; the definitive laws I adduced, were contained in the works of the best writers on optics, and were sufficient to evince the want of originality and improvement of his meniscus shaped lens. In respect to the experiment by which he attempts to enforce a proof of an advantage in his spectacles, its value will be known by the following account of a repetition of it.

Experiment with a pair of periscopic spectacles compared with a pair of the usual construction.

I am possessed of a pair of his periscopic glasses, mounted in a single steel frame, which cost 10s. 6d. The glasses, I must observe, are different to his proposed form, having of each, the inner side, or that next to the eye so little incurvated, that by any person but an optician, they would be called plano convexes. The focus is four inches, the same as used by Dr. Wollaston in his experiment. In a similar mounting, with

with double convex glasses of the same diameter and focus, I provided a pair of our own manufacture, and as sold by us at 3s. 6d. These two pair of spectacles were attentively compared together, by myself and several judicious and impartial persons, in the manner as stated by Dr. Wollaston of his. The result was as follows.

The convex glasses being applied as close as possible to the eyes, with the frame attached to the head, the print of a large quarto page was viewed through them, at a distance for distinct vision at their centres, the letters at the distance of about 25 lines, appeared quite distinct or well defined; giving the axis of the eyes a little obliquity to discriminate more lines, an indistinctness or confusion of letters commenced, increasing towards the extremity of sight, and from the lateral aberration of the lenses, the letters were tinged with the prismatic colours. Keeping the head fixed in the same position, the periscopic glasses were substituted. The extent of distinct letters without distortion was nearly as great, but the coloured letters were evidently nearer to the centre, and more numerous than by the other glasses. By inclining the axis of the eyes still more than in the former case, or looking extremely askint through the glasses, a greater extent of lines was observed, but blended with colour and confusion. The optic nerves felt a sensible irritation, evidently from the squinting position of the eyes, a refraction of many superfluous rays, and the consequent increased and unusual magnitude of the images on the retina. The pain in the eyes mentioned by Dr. Wollaston, must have arisen only from this circumstance, and not from the one he represented it to be. By a trial of the old meniscus glass I before mentioned, which is of four inches focus, and corresponds with what he has a patent for, in comparison with one of the above plano convexes, the view of letters was still more extended, but illegible and with much colour, and like the others towards the extremity, of no sort of use for the purposes of vision. Now all this is conformable to the laws of optics, and manifests a property different to that advanced by Dr. Wollaston.

These several glasses are also at the public service for inspection in our shop in Holborn.

By making the glasses of the above periscopic spectacles nearly planos, Dr. Wollaston's principle is destroyed, and my opinion

Result. With the double convex lenses. Extent of distinct vision. Indistinct.

With the periscopic glasses, more colour.

Trial of another meniscus.

Concluding remarks.

opinion evidently verified; that the nearer a meniscus approaches to a plano, the more perfect it will be, as the spherical surface, for the same focus, is diminished, and consequently the aberration besides; admitting that there were any advantage desirable from a great obliquity of the axis of the eyes to those of the miniscus shaped spectacle glasses. I would ask, for what reason has man his head moveable? was it not, that he should place his eyes directly before the object to be viewed, and not subject himself to fallacious ideas of them, by an aukward and revolutionary squinting. From what I have advanced, I doubt not of the public decision, (from a fair comparison of the two kinds of spectacles) in favour of the established double convex spectacle-glasses, for

“ *Magna est veritas et prævalebit.*”

I am, SIR,

Your respectful humble Servant,

W. JONES.

Holborn, April 10, 1804.

XI.

Experiments and Observations on the Change which the Air of the Atmosphere undergoes by Respiration, particularly with Regard to the Absorption of Nitrogen. In a Letter from ALEXANDER HENDERSON, M. D.

To Mr. NICHOLSON.

SIR,

Whether nitrogen be absorbed in respiration.

I TAKE the liberty of communicating to you a brief detail of some experiments on respiration, which were undertaken chiefly with a view to determine the absorption or non-absorption of *nitrogen*, a point which has been hitherto much controverted, but which I flatter myself to have now sufficiently ascertained.

The gasometer.

These experiments were performed by means of a gasometer, capable of containing about 2200 c. inches, and graduated so as to shew a difference of 2 c. inches. In breathing from this apparatus, the inconveniences from friction were very inconsiderable. Towards the end of the experiment, however,

ever, when the air in the gafometer became vitiated, and the respirations fuller and quicker, some disagreeable effects were experienced from the smallness of the breathing tube, and from it becoming, in some measure, choaked by the vapor from the lungs, which was condensed in it.

After closing the nostrils, and making a forced exhaustion of the air in the lungs, a full inspiration was made of the atmospherical air in the gafometer, and this inspiration as well as the subsequent expiration, was measured, by means of the graduated scale. The air of the gafometer was then respired as long as possible, and till the oppression about the chest became so great as to oblige the experimenter to desist, and before closing the stopcock, and separating the tube from the mouth, the magnitude of the last full inspiration and expiration was accurately observed. Method of respiring,

In all our experiments, the bulk of the atmospherical air was considerably diminished by respiration, the quantity that disappeared varying from five to eight c. inches per minute. This diminution, however, is partly to be attributed to the condensation of the oxygen gas, when converted into carbonic acid gas, in which form its bulk is diminished about one fourth. In order to obviate any objections that might apparently arise from the difference of the inspirations and expirations at the commencement, and at the conclusion of the experiments, they were, as we have already mentioned, carefully noted; and if any difference did occur, it was deducted on the side where the preponderance took place. and general effect, as noted;

In proceeding to examine the chemical qualities of the air, considerable difficulties were at first experienced for want of an accurate eudiometer. Several trials were made with *nitrous gas*, which affords the most easy and expeditious mode of analysing atmospherical air. But, independent of the inaccuracies which may arise from the number of vessels it is necessary to have recourse to in employing it, this test is liable to still greater objections, from differences in its degree of purity, from its absorbing a portion of nitrogen, and from its combining with a greater or less proportion of oxygen, according to the diameter of the vessels employed, the length of time which it is allowed to remain in contact with atmospherical air, and the degree of agitation used in mixing them. Eudiometrical examination.
Nitrous gas exceptionable.

The

Seguin's eudiometer with phosphorus objected to;

The eudiometer with *phosphorus*, as recommended by Seguin, is by no means free from these disadvantages. It is subject to the same inconveniences as the nitrous gas, with respect to the frequent change of vessels; and, if we may credit the observations of V. Humbold, is liable to several sources of fallacy from the different proportions of oxygen, which the phosphorus absorbs, and from its combining also with nitrogen. A portion of carbonic acid may also be generated, if the phosphorus be not perfectly free from impurities. The eudiometrical test, which has been lately recommended by Davy, viz.

and also the impregnated sulphate of Davy.

a solution of the *pale sulphate of iron*, saturated with nitrous gas, is not altogether exempt from the inconveniences which attend the use of the latter substance, and has the additional objection, that a portion of nitrogen is generally disengaged from the solution, after the absorption of oxygen is completed.

Sulphuret of alkali or lime absorb oxygen very slowly;

The *sulphurets of alkalis and lime* have hitherto been regarded as among the most accurate tests of the purity of atmospheric air. They, however, absorb oxygen but slowly, and several days may elapse, before the absorption be completed. It is therefore, in general, necessary to have recourse to complicated and uncertain calculations, in order to adjust the result of the experiment to the variations of the surrounding atmosphere. To remedy this objection to their use, the application of heat was proposed by Guyton, by which means a speedy absorption was effected; but, besides, that some difference of the product may result from the method employed, the apparatus itself is objectionable, from its size and inequality of dimensions.

and are not completely amended in this respect by heat.

The apparatus of Dr. Hope affords great advantage.

Luckily for the progress of eudiometrical science, these obstacles to the attainment of an accurate knowledge of the constituents of the atmosphere, seem to be now, in a great measure, removed, by the invention of an apparatus by Dr. Hope, which unites the advantages of neatness and simplicity with those of extreme accuracy and expedition, and of which you have already given a very full description in your Journal*. In the trials which I have made with this instrument, the substance found to answer best as a test of the atmospheric air was the *sulphuret of lime*, which is more

Sulphuret of lime was used.

* Vol. VI. pages 61, 210.

readily decomposed than the sulphuret of potash, and is consequently better adapted for the purpose of expedition. In most of the experiments, the absorption was finished in about twenty minutes, if sufficient agitation had been employed. The only objection, to which the above test may seem liable, is, that, according to some, it absorbs a small proportion of the nitrogen, as well as the oxygen of the atmosphere. This opinion, however, does not appear to be well founded, for in several of our experiments, where the quantity of oxygen absorbed appeared to be unusually small, and where the agitation was continued for a much longer time than necessary, no perceptible alteration in the bulk of the air was observed*.

It does not appear to absorb nitrogen.

The method of proceeding in the analysis of the air, was briefly as follows: after having ascertained the purity of the atmospherical air by means of the eudiometer above-mentioned, and knowing the exact bulk of the air contained in the gasometer, the total quantities of oxygen and hydrogen in it were calculated by a very simple process. This air was then respired, and its diminution marked as has been already described. After respiration, a portion of it was introduced into the eudiometer, and its carbonic acid was absorbed by means of lime water, (for which the above-mentioned instrument was found extremely convenient). Freed from the carbonic acid, the air was now subjected to the action of the sulphuret of lime, and the relative quantity of nitrogen contained in it was thus discovered. Then, by deducting the quantity of carbonic acid, and of oxygen gas, contained in the air of respiration, from the total quantity that remained after respiration, we procured the proportion of nitrogen, which abstracted from the total quantity before respiration, gave the proportion of nitrogen absorbed. Thus, let *a* represent the original quantity of nitrogen; *b* the carbonic acid, and *c* the oxygen of the air of respiration, and *M* the bulk of the residual air; $M - b + c = n$, or residual nitrogen, and $a - n = x$ or nitrogen absorbed.

The process. Previous examination of the air,

and after respiration.

Experiment I. June 16, 1803.

600 c. inches of atmospherical air were respired, for four minutes, at the temperature of 63°.

Exp. 1, 2, 3.
The bulk of nitrogen absorbed in respiration was about 1-40th of the whole, or

* De Marti is of opinion that the hydrogenated sulphurets absorb nitrogen only when recently formed. Vide Journal de Physique. Tom. LIII. p. 176.

Before

about 1-32d of
the nitrogen.

Before respiration, 100 parts contained of

Oxygen	-	-	-	-	.22
Nitrogen	-	-	-	-	.78

After respiration, quantity diminished to 570 c. inches,
100 parts found to contain of

Carbonic acid	-	-	-	-	.07
Oxygen	-	-	-	-	.14
Nitrogen	-	-	-	-	.79

$$570 - 39.7 + 80 = 450.3.$$

$468 - 450.3 = 17.7$ c. inches or quantity of nitrogen
absorbed.

Experiment II. June 18, 1803.

600 c. inches atmospherical air were respired, for four
minutes, at the temperature of 64° .

Before respiration, 100 parts contained of

Oxygen	-	-	-	-	.22
Nitrogen	-	-	-	-	.78

After respiration, quantity diminished to 570 c. inches,
100 parts found to consist of

Carbonic acid	-	-	-	-	.08
Oxygen	-	-	-	-	.12
Nitrogen	-	-	-	-	.80

Quantity of nitrogen absorbed, therefore, = 12 c. inches.

Experiment III. February 11, 1804.

1000 c. inches atmospherical air were respired for the
space of $4\frac{1}{2}$ minutes, at the temperature of 57° . Barometer
= 28.78.

Before respiration, 100 parts contained

Oxygen	-	-	-	-	.22
Nitrogen	-	-	-	-	.78

After respiration, quantity diminished to 962 c. inches.

After respiration, 100 parts contained

Carbonic acid	-	-	-	-	$.07\frac{1}{2}$
Oxygen	-	-	-	-	.13
Nitrogen	-	-	-	-	$.79\frac{1}{2}$

Quantity of nitrogen absorbed, therefore, = 15.1 c. inches.

Comparative re-
marks on these
experiments and
those of Davy.

These different experiments, (which I have selected from
among many others) agree in the general result, viz. that a
portion of nitrogen is abstracted from the atmospherical air,
by the blood in its passage through the lungs, although the
amount

amount is somewhat less than has been stated by Davy, who makes it equal to about 5 c. inches per minute. Yet I am inclined to think, that this is more a difference in appearance than reality; for if we consider that the most of Davy's experiments give the result of the changes produced on the air by a single inspiration, or by a small number of respirations, while in the experiments just described, a large portion of air was breathed for a considerable length of time, so as to become, at last, unfit for the due performance of respiration; it is probable that the blood could no longer produce the same alterations in its properties, that took place when a purer atmosphere was inspired. It is also natural to suppose, that the quantity of air consumed in respiration varies in different persons, and in the same person at different times. An approximation to the truth, therefore, is all that we can expect to obtain in the determination of this question; and we must rest satisfied with the knowledge of the important fact, that nitrogen is absorbed by the human body in respiration.

The striking uniformity in the analysis of the atmospherical air, by means of the sulphuret of lime, occurred also, as I have with pleasure observed, to Dr. Thompson in the numerous trials which he has made on this subject; and furnishes a most interesting problem for the investigation of the chemist and natural philosopher.

Dr. Thompson also found sulphuret of lime a very excellent test.

I am, with much Respect,

SIR,

Your most obedient Servant,

AL. HENDERSON, M. D.

Edinburgh,
April 12, 1804.

XII.

Observations and Experiments tending to ascertain the Causes of those Irregularities in Chronometers, which are generated during considerable Intervals of Time, and have been ascribed to external Causes. By Mr. JOHN HALEY, Jun.

To Mr. NICHOLSON.

SIR,

HAVING seen in the last number of your very useful Journal some remarks on chronometry, I have thought the accompanying observations might not be unacceptable. If you should consider them worthy of insertion, I shall take the liberty of communicating the results of some experiments I am now making in the same line.

I am, SIR,

Your most obedient and humble Servant,

JOHN HALEY.

March 29,
1804.

25, Cleveland Street,
Fitzroy Square.

Latent causes of error in time-pieces.

MY design in writing the following pages is to develop certain latent causes of error, and their mode of operation, which have been found in a greater or less degree to affect every machine hitherto constructed for measuring time.

If the errors of the machine be ascribed to foreign causes, improvements will cease.

It is certainly the part of a watch-maker to search for and remove every mechanical cause that may affect the going of his machines, before he hastily concludes that the causes of those errors which are generated in a length of time, (and which have escaped his notice,) are foreign to its structure; such as the influence of the oil, the varying different density of the atmosphere in which the wheels move, &c. For such opinions when once adopted, must greatly tend to slacken his pursuits and put a stop to his hope of farther improvement.

The foreign causes are of little effect: For many machines have performed wonderfully well, while subject to their action.

That these foreign causes do in fact produce errors deserving much notice in the best adjusted time-keepers is probably an erroneous notion. For if so, how could so many of different constructions have been found to perform for very long periods of time, exposed to all these causes, with a wonderful degree of

of accuracy. Indeed Mr. Cumming, in his treatise on clock and watch work, has supposed that the influence of the oil may be in some cases even beneficial to the performance of the machine; though it must be allowed he there speaks of such as have no provision against the effects of heat and cold. And though Mr. Mudge in some of his letters to his Excellency the Count de Bruhl does write, that when the air was moist in Devonshire (where he made and tried his machines) they retarded their rates; yet the quantity of error produced by this supposed cause was so small, that it gave him no uneasiness with regard to their fate. But the very different performance of machines on the same construction, strongly induce me to believe that most of the errors hitherto found in the best time-keepers have been produced by latent mechanical causes coexistent with the machines, or rather, with the times they were first put in motion.

Various opinions have been entertained by gentlemen of scientific and mechanical acquirements, and by artists upon this subject. One in particular has very generally prevailed, namely, that the errors have arisen from inequality of power derived from the main spring, and the train of wheels. That very great errors will be produced by these causes, (if not removed by good workmanship,) must on all hands be admitted; but where the execution has been correct, the errors will be trifling indeed, and must always remain nearly the same. The late Mr. Arnold, on being asked by a Committee of the House of Commons his opinion of the Remontoire, said, that it was only a help to bad workmanship.

Mr. Harrison's opinion of the Remontoire was different from that of Mr. Arnold; for he expected to arrive at perfection in his machine from introducing it, though by his method of application, he did not detach his escapement from the whole of the train of wheels. The invention was undoubtedly a great proof of his superior ingenuity, and merited high praise. However, on finding the going of his watch not to answer his expectation, he attributed its irregularity to the thermometer not having its due effect; and asserted, "that if it could properly be put into the balance, the watch would go within a few seconds a year," which assertion has since been proved erroneous.

Mr.

Opinions respecting the causes of irregularities. Variation in the maintaining power, inconsiderable.

Remontoire condemned by Arnold;

but esteemed by Harrison.

Mudge's Remontoire for winding up every vibration.

Mr. Mudge's comprehensive mind suggested a Remontoire that should be wound up at every vibration of the balance, and he attached it to a 'scapement which was certainly very superior to Mr. Harrifon's, and for uniformity of excellence in its going, is more to be depended upon, in my opinion, than any other which has hitherto been applied to a portable machine. Mr. Mudge in the construction of this 'scapement, had almost rendered his Remontoire usefess with respect to one purpose for which he introduced it. His son, in a book entitled a Narration of Facts, (pages 46 and 47, in the margin) has published an extract from a manuscript of his father's, wherein he writes as follows, "was I to make a watch myself upon this reasoning, I should not expect it to be by any means perfect, but I cannot help thinking, that I should in the first essay get rid of so many, and so great errors, that the cause of those that remain would be more comeatable than when blended as they are now, &c." I say, that he had, in the formation of his admirable 'scapement, nearly annihilated those errors, the cause of which he proposed to arrive at the knowledge of by the introduction of the Remontoire; it is much to be lamented that this great mechanic's powers failed him through age and infirmities, at the time when he had almost arrived at the knowledge of those causes of error which he so earnestly fought after. And we may presume from the superiority of his penetration, that he would also have accomplished the means of removing them, and consequently of giving to his machines all the perfection of which their principles were susceptible.

He supposed it would detect unknown causes of error.

Mudge's watches accelerated their rates at first.

It is observable that Mr. Mudge's watches, (which I consider from their construction as having the causes of error which I shall endeavour to develope, operating in them in a less degree than any other time-keepers,) in almost every instance accelerated their rates of going, while under trial at the Royal Observatory. The Rev. Doctor Maskelyne has, I think, done mechanics an essential service, by publishing his very judicious and accurate remarks upon their going while under his care. He observes, among other facts, "that the watches did generally accelerate their rates of going with sometimes retarding them a little, that they accelerated their rates less in the second trial than in the first, and least of all in the last trial, and that towards the latter part of that trial the

Dr. Maskelyne's account of them.

the

the watch *green* retarded its rate." If we carry our observations farther, we find, that, in their subsequent trial which took place at Mr. Dutton's, (the watches having had nothing done to them,) they both retarded their rates throughout. I will endeavour to shew why these watches did accelerate and retard their rates in this manner.

The errors of opposite tendency that did exist in these machines were so small, and I may add, were such a length of time in generating, that it became very difficult for Mr. Mudge to ascertain their cause. Where was he to look for them? It was not to the 'scapement only his search was to be confined, but he had also to traverse through the whole train of wheels: For though he had applied his Remontoires beyond the train, yet the last wheel (if there was any inequality in the power derived from the main spring or train) would press sometimes stronger, and at others weaker upon the pallets of the Remontoires; which pallets the balance in its vibrations (by means of the pins in the crank affixed to it) had alternately to unlock, before it could be impelled by the unbending of the Remontoire springs—therefore, as he supposed the causes were not determined to the 'scapement, he knew not where to fix them*.

It was an opinion (in writing) of Mr. Mudge's, "that the simple principles of all watches are the same and perfect, the errors found in them are therefore not errors arising from the principles, but from imperfections, inseparable from all mechanic operations."

The late Mr. Arnold asserted before a Committee of the House of Commons, that, though he put no oil to his 'scapement, yet it wore less than any other part that was subject to wear.

Mr. Emery told the same Committee, that the 'scapement which he made for his Excellency the Count de Bruhl, after a model of Mr. Mudge's, was very difficult to execute, but when made, it would not easily wear out. It is a known fact, that this watch went with an unusual degree of excellence.

Great difficulty of ascertaining the causes of error in Mudge's time-pieces, if in the train.

Opinions of mechanics. 1. Of Mudge, that time-pieces err not from principle, but imperfect execution.

2. Arnold: that his 'scapement wore less than other parts.

3. Emery: that Mudge's 'scapement would not easily wear.

* For drawings and descriptions of Mr. Mudge's 'scapement, the detached 'scapement now in general use, and several others, see *Philos. Journal*, quarto series II. p. 49.

4. General observation; that when the 'scape-ment wears there is no more regularity.

Inference: that the errors in the best time-pieces are thus caused.

Distinct statement: that the alterations of rate are caused by wear in the 'scapement.

Short description of the usual detached scape-ment. 1. Scape wheel, 2. (face of) detent, 3. passing spring, 4. unlocking pallet, 5. impulse pallet.

Consequences of wear. 1. If the wheel (point) should wear, it will sooner escape and strike the pallet.

And still more it is the observation of every watch-maker, when speaking of the verge and horizontal 'scapements, that when once the verge or cylinder begins to wear, there is an end of all good performance, and that the going of the watch becomes from that period totally incorrect.

These several testimonies tend to induce a belief, that the errors which still remain in the best time-keepers, are produced chiefly by this cause; and this cause must exist from the moment the machine is first put in motion, although the errors will not manifest themselves to any considerable extent, (particularly if the materials are of the best kind,) until the machine has been going for some time.

I therefore submit it as a fundamental principle, that the principal cause of the errors found in the best adjusted time-keepers, consist in the wearing of the different parts of the 'scapement; that so long as by the act of wearing the relative proportions of its parts are preserved, that errors of contrary kinds compensate each other in the general action, the machine will go correctly; but so soon as those ratios of the parts are altered by wearing, the watch will go either too fast or too slow.

The scape wheel of what is termed the detached escape-ment, say of the late Mr. Arnold, or any other of them in use, (for their principles are the same, and differ only in modification,) is looked upon a jewel in the detent. The detent has a small spring fastened to it, which reaches considerably beyond the locking jewel. Some watch-makers have given the name of passing spring to it, because, the little pallet in one vibration of the balance passes it without disturbing the detent, and in the next vibration, the same pallet by laying hold of its point, lifts the detent out of the wheel, which immediately impels the balance by means of the great or impulse pallet*.

To simplify our deductions, let it be supposed that the point of the passing spring does not wear at all, and I think it will appear to every person acquainted with the 'scapement, that if the points of the teeth of the wheel should wear, the watch must go faster. For the wheel must then escape the locking jewel of the detent sooner, and will necessarily impel the balance sooner also; consequently each impulse given to

* Philof. Journal. Quarto II. 54.

the balance must be began quicker and quicker, as the points of the teeth of the 'scape wheel wear more and more.

But on the contrary, let us now suppose that the points of the teeth do not wear, and the passing spring wears; in this case, as the balance in its vibration will have a greater space or portion of a circle to move, before it can arrive at the spring, and unlock the wheel, in order to receive the impulse, every such impulse will be made later and later, and the watch must necessarily go slower and slower as the point of that spring wears away more and more. There is another part in this 'scapement, namely, the striking face of the tooth, in which, if wear takes place, the tendency I presume will be to lose, in consequence of the wheel acquiring a greater-drop, or having farther to go before it can overtake and strike the pallet. This part of the tooth I here supposed to wear, and which first comes into contact with the pallet, is not the point, but a little nearer the centre of the wheel; and as the wheel follows the pallet, it must be some portion of time longer before it can arrive to begin the impulse. In the above example I have supposed the detent and both pallets to be jewelled: if they were not, the errors would be greater.

Now we find that, according to the above reasoning, although there are two parts of the 'scapement in which, if wear takes place, the tendency will be to lose; leaving out of the question the oil becoming glutinous, the springs losing their elastic force, &c. and only one part, in which if wearing takes place the tendency will be to gain, yet, we find in most machines the tendency to gain is generally predominant. I will shew why I think the teeth of the wheel wear more than either of the other two parts: there is not only friction, but percussive force also takes place in the three different parts of the 'scapement before-mentioned; the little roller or pallet that lifts the detent out of the wheel strikes against the point of the passing spring on coming into contact in order to unlock the wheel; in the next place the wheel strikes against the pallet when it begins the impulse, and lastly, the wheel again strikes against the jewel of the detent when it locks upon it: I will not attempt to determine which of the two former percussions is the greatest, or which will occasion the greatest retardation of rate, but will say, that the last is greater than either of the other two by much, and in general we find that the wheel is locked upon

If the passing spring wear, the unlocking, and consequently the impulse will be later.

If the face of the teeth should wear, the drop will be more and the impulse later.

Thus there are two kinds of wear which tend to produce loss, and one to produce gain.

—but the gain predominates,

—and this because the stroke of the wheel in locking is much greater, that

those of the
unlocking and
the impulse.

that part of the face of the tooth nearest the point; now as repeated strokes will wear as well as friction, I take it that the wheel wears more from this cause (particularly where the execution has been favourable for it) than either of the other two parts, and therefore most time-keepers made upon this construction have a greater tendency to gain than they have to lose.

The advantage
of clocks or ma-
chines with short
vibrations de-
duced from the
smaller per-
cussion.

If it should be admitted that percussive will wear in any material degree, I submit that it will lead to the discovering why clocks whose pendulums make the shortest vibrations go better than others, and also the cause of the great disparity there is between the going of the common verge watch and that of table and long eight-day clocks; in both clocks and watches constructed with two pallets upon the recoiling principle, each pallet, with the accumulated force it has obtained by the vibration of the balance or pendulum, meets the wheel which is coming a contrary way when it comes into contact, and a percussive takes place; therefore the longer the vibration of the pendulum or balance, the greater must this percussive be, and consequently the wear also: hence may be supposed one reason why spring or table clocks that make shorter vibrations go better than verge watches, and long clocks, which make shorter vibrations still, go better than either. I readily admit there is one, and perhaps many weighty objections to this reasoning, viz. that some clocks upon the recoiling principle are found to go as well or even better than others on the principle of the dead beat, though the execution in each has been of equal excellence. For an answer to this objection, I refer the reader to Mr. Cumming's elaborate treatise upon clock making; who in some part of the work, asserts, the reason to be, that the plane of action of the pallet does not sufficiently subtend the angle of vibration; by which means the power applied is administered too suddenly in the dead beat; to which I add that the percussive is also greater in this case in the dead beat than in the recoil, although the pallet does not meet the wheel in opposition, and the wearing of the teeth of the wheel may be increased by that means. There will be among many others one very serious objection to my supposition that the wearing of the wheel can produce error in a long pendulum clock, viz. the power of the pendulum being so much superior to the motive force, it cannot be affected by it; to which I shall only observe that the results in practice are sometimes very different from theoretical conclusions.

Some objections
considered.

The

The mode of operation by which friction is said to accelerate the rate of machines, is thus pointed out by the trade in general, viz. that whenever it takes place by the action of the wheel upon the pallet or pallets, (in watches or clocks constructed with one or two pallets), or in the verge holes by the action of pivots therein, it impedes the motion of the balance or pendulum, shortens its vibrations, and in consequence the machine goes faster: how does this agree with Mr. Harrison's opinion? which was, that large arcs are naturally performed in less time than small ones. Mr. Cumming, in the work of his before alluded to, has intimated, that nothing conclusive can be drawn from different arcs, because they are effects and not causes of error.

Whether gain be occasioned by shortened vibrations.

In the Repertory of Arts and Manufactures, No. 33, in the year 1796 or 7, there is a description of a Remontoire 'scapement, which is completely detached from the train of wheels. The errors of this machine must be determined to the 'scapement alone, because no errors in the going can possibly arise in any other part of the watch. For if any additional power, however great, be applied to the train, the arcs of the balance will not be in the least affected by it: in the opinion of all or most who understood its principles, its going must be more perfect than that of any machine before invented.

An escapement completely detached.

In the following remarks upon that 'scapement, I shall call what is in the before-mentioned description of it termed a snail pallet, a locking pallet. In the commonly termed detached 'scapement, every tooth in the wheel is locked in succession upon the jewel of the detent, and the effect arising from wearing of the teeth does not very soon shew itself, there being 12, 15 or more (at pleasure) in number, but in this remontoire 'scapement before every impulse there is only this individual locking pallet to supply the place of a wheel, (if I may so express it) the wear therefore must be 12 or 15 times as great, and its effect must manifest itself very soon.

In this the wear on the locking is greater because the action is on a single face.

In the years 98 and 9, I executed four or five of these 'scapements, and tried the going of each. They all went in the same manner, viz. for the first twenty-four or thirty hours the machine would vary nothing; the day following it would gain one or two seconds, the next day seven or eight, and on the fourth or fifth day it would gain 30 or 40. I observed, the more the machine gained, the larger were the arcs of the balance;

Account of the construction and early performance of several of these.

lance; if the machine was set a-going without oil being applied to the locking pallet, it would not go many days before it would stop, and if set in motion the balance would perform only three or four vibrations, when it would come to rest again: in this state, if oil was applied to the locking pallet, it would set a-going more lively than ever, the balance making considerably larger arcs, and the machine gaining upon its former rate in a very surprizing manner.

Explanation of the irregularities;—from wear or change in the parts, and not from friction only.

Should it here be enquired, how were these effects produced? I think it would be answering in a vague and indefinite manner to say merely,—that it was occasioned by friction; for the mode by which it operated is certainly necessary to be pointed out: I suppose that by friction the point of the locking pallet (which was made of steel) wore shorter, by which means it locked shallower upon the jewel in the detent, therefore the balance unlocking the Remontoire sooner, the impulse to the verge was given quicker as the point of the locking pallet became shorter by wear; also, the shallower the locking pallet locked upon the jewel of the detent, the less was the resistance to the balance in unlocking it, and the drop upon the impulse pallet became less, therefore the arcs of the balance became larger in proportion as the locking pallet wore shorter: I can never believe that the above effects, viz. acceleration of rate and large arcs were produced by friction abstractedly; for had it been so, they would have ceased the moment oil was applied to the locking pallet, instead of which the effects became more apparent.

Remedy. An intermediate wheel was introduced, to render the locking faces more numerous.

It was not till after various examinations of the 'scapement alluded to, and making two or three others of the same kind, that I formed the foregoing conclusions respecting the cause of the error that had puzzled me; when I had fixed upon the cause in my own mind, I determined upon a remedy, which was, by applying an intermediate wheel between the remontoire and the balance, instead of the locking pallet; I contrived to fix a short weak jewelled detent upon the remontoire axis, which moved round with it and locked upon the intermediate wheel, which had twenty-four teeth; as the locking pallet had before locked upon the detent, I placed also a detent upon the other side of the intermediate wheel, to lock it, and by a particular contrivance made the intermediate wheel to shift one tooth at every vibration of the balance; after which alteration,

alteration, the former effects totally ceased; in order to make the intermediate wheel (which was loose) shift one tooth at every vibration, I made the detent which locked it strong, and inserted a long jewel into it, having an inclined plane that filled the space between each two teeth that always kept the wheel in a certain position, the consequence of applying the strong detent was, that the point of the passing spring (or in other words the unlocking part) wore very fast, and occasioned a very considerable retardation of rate, which continued without intermission until the watch stopped from the point of the passing spring wearing so much, that the balance could not unlock it. I must observe, that the arcs of the balance were in this case not perceptibly altered, and the retardation not nearly so great as the acceleration in the former. I afterwards, by another contrivance, which it would be difficult to explain, remedied the evil attendant upon a strong detent, and produced an escapement perfectly to my wishes, that had no propensity either to accelerate or retard its rate.

It succeeded.—
Strong detent to
shift the wheel

—produced wear
in the passing
spring;

—and retarda-
tion.

Remedy.

It is clear to me, that friction or wearing will, in some cases, make a machine accelerate, and in others retard its rate of going, therefore I presume if we could hit upon some method of preserving the ratios of the detached 'scapement permanent, by giving the different parts a durability, they have not hitherto possessed, we might produce machines of this simple construction that should retain an equal rate of going for long periods of time, and not look to chance for their success; if the actions of the 'scapement were unalterable (which I imagine might be made by jewelling) we should then have only the influence of the oil to oppose our success, which by becoming glutinous, will, I think occasion, if any change, a retardation of rate, yet if it be good, the effect, I suppose, will not be very great in a moderate length of time.

The simple de-
tached 'scapement
would perform well
for long periods of
time if preserved
from wear.

Having been on the coast of Coromandel and Ceylon, and other parts of the East Indies, for a series of eight years, I had there an opportunity of trying an oil to watches, which artists here are unacquainted with; being in camp in the Mysore country, in the years 1791 and 2, for a period of about eighteen months, I could scarcely procure a drop of olive oil that was not rancid; I therefore substituted in room of it what is there called cocoa nut oil, which retains its fluidity to the last. I never knew an instance of its becoming glutinous, nor

Cocoa nut oil
recommended
for time pieces.

does

does it evaporate soon; I have found it in jewelled, verge, and horizontal watches, in that warm climate, after a period of three years, in a very fluid state, and in sufficient quantity. That oil may be easily procured here by breaking the shell of the cocoa nut, drying the nut in a very gentle manner, and afterwards expressing the oil from it, which is yielded in a great quantity; it assumes a concrete form in a very moderate degree of cold, but how far that may be a good objection against its use in this climate remains to be determined.

Mudge's first watch stopped for want of oil on the pallets.

I will conclude these observations, after a few remarks upon the stopping of Mr. Mudge's first watch, while under trial at the Royal Observatory; Mr. Mudge found, after a good deal of pains taken in investigating the cause, that it was for want of oil being put to the pallets (or locking part) of his 'scapement: I think this case so parallel to that of Mr. Haley's machines stopping from the same cause, that it tends in some measure to prove the truth of what I have advanced: Mr. Mudge had thought when he finished this machine, that it was altogether unnecessary to apply oil to this part, as he mentions in a letter to his excellency the Count de Bruhl, upon the subject of its stopping; and in the same letter he writes to the following effect, that when he had applied oil to the pallets, the watch went as well as before, or, as it did when first set a-going; that the arcs of vibration had fallen off but a few degrees, yet the watch went ten seconds a day faster than the last-mentioned rate, and from any thing that suggested itself to him, he could not see why it should not have gone rather slower than faster through length of time. I will venture to suppose the following to have been the cause of the machine going faster, and of the balance measuring smaller arcs in consequence.

Effect when oil was applied.

Explanation: The teeth of the wheel wore shorter; escaped sooner, and (qu.)

The points of the teeth of his 'scape wheel had become shorter* by constant wear, however small, and escaped the pallets (or rather hooks of the pallets, as they have been termed) sooner; therefore the opposite pallets which impelled the crank gave the impulse sooner to the balance, and in my mind, the watch may be supposed to have accelerated its rate from this cause; on the other hand, by the wearing of the teeth, the wheel did not wind up the remontoire springs quite so

* Refer to the figure, *Philos. Journ.* 4to series, Vol. II. p. 49,

high,

high, therefore there arose in some degree an impediment to the balance, which might occasion the arcs to be shortened, because, before the wheel was set at liberty, or unlocked from one pallet, in order to wind up the other remontoire spring, the balance (by means of the crank affixed to it) had alternately to wind up each remontoire spring a little higher, and in short, so much higher than the wheel had before wound it, as the momentum of the balance was equal to; therefore, as I said before, when the teeth of the wheel became shorter by wear, they would not wind up the remontoire springs quite so high; in which case the crank would engage the pallets on the opposite side of the remontoire axis a little sooner, by which means the balance would receive a check in its vibrations, and the arcs would certainly fall off. These, which I have mentioned, are the only reasons which have occurred to me to shew why the remontoire machine I made should have its arcs of vibration increased when the watch went faster, and, on the contrary, Mr. Mudge's should have its arcs shortened when it went the same way.*

—and the remontoire springs would be less wound up: whence the balance would have more to do alone.

As to Mr. Mudge's watches, *green* and *blue* retarding their rates while under trial at Mr. Dutton's, after having gone through their public trials at the Royal Observatory, it should be remembered they had not been cleaned, and that the oil being thicker, must have had a share in producing that effect, and perhaps a principal one; for as to friction in the impulse or unlocking part, there was none, it was totally annihilated, by the verge and remontoire axis moving on the same centre; if any wear could take place there, it must be by percussion and pressure only: likewise it may be considered, that the teeth of the wheel (or locking part) having been blunted by wear with former going, would have larger surfaces to resist the wear and its accelerating effect during this trial; besides, it will be readily believed by those acquainted with Mr. Mudge's scape-ment, that the wearing of his wheel and its pernicious effect, cannot be so great as in that of the detached scape-ment.

The retardation of Mudge's other time pieces ascribed to inspissation of the oil.

* It appears to me, that if the impelling pallet came to a stop before the instant of unlocking, no other than the last effect, in Mudge's machine, would take place; except that the resistance to unlocking might become less. Would the crank and levers wear and diminish the effect of the acting pallet?

— N.

XIII.

A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake and other American Serpents. By BENJAMIN SMITH BARTON, M. D. From the American Transactions, Vol. IV.

(Continued from p. 285 of Vol. VII.)

Experiment of
fascination by a
snake.

THE facts which came under the notice of Mr. Vosmaër, at the Hague, are curious, and deserve to be mentioned. But they do not appear to me to be proofs of the existence of an infectious or mephitic vapour proceeding from the mouth of the rattle-snake. I am not at all surpris'd that the birds and mice that were put into the cage, along with this reptile, should exhibit the motions which were observed by the Dutch naturalist. When the little animals squatted down in a corner of the cage, they were, most probably, impelled by the instinct of fear, which is so powerful, and so extensive, in the vast family of animals. When they run towards the serpent, it may have been fear that actuated them.

Perhaps partially
observed.

In conducting a series of experiments, it is ever a matter of importance, that the mind of the experimentalist should be free from the dominion of prejudice and system. Perhaps, facts are never related in all their unadulterated purity except by those, who, intent upon the discovery of truth, keep system at a distance, regardless of its claims. The strong democracy of facts should exert its wholesome sway. I cannot help thinking, that if Mr. Vosmaër had disbelieved the fascinating faculty of serpents, the conclusions which he would have drawn from his experiments, just mentioned, would have been somewhat different. But of this I cannot be certain, and, therefore, I shall not avail myself of the supposition.

Other facts
which contradict
the notion.

Some experiments which have been made in this city, do not accord with those of Mr. Vosmaër. The birds, which were put into the cage that contained the rattle-snake, flew or ran from the reptile, as though they were sensible of the danger to which they were exposed. The snake made many attempts to catch the birds, but could seldom succeed. When a dead bird was thrown into the cage, the snake devoured it immediately. He soon caught and devoured a living mole, an animal much more sluggish than the bird. A few days
since,

since, I had an opportunity of observing the following circumstance. A small bird, our snow-bird*, had been put into a cage containing a large rattle-snake. The little animal had been thus imprisoned for several hours, when I first saw it. It exhibited no signs of fear, but hopped about from the floor of the cage to its roof, and frequently flew and sat upon the snake's back. Its chirp was no ways tremulous; but perfectly natural: it ate the feeds which were put into the cage, and by its whole actions, I think, most evidently demonstrated, that its situation was not uneasy.

I do not relate this latter fact with any intention to disprove the notion, that the rattle-snake possesses the faculty of charming. For the observation was made on the seventeenth of last month, which is somewhat earlier than the time when our snakes usually come out of their dens. The snake, too, which was the subject of the experiment, appeared to be very languid, and had not eaten any thing for a considerable time. We ought not, therefore, to suppose him possessed of the fascinating faculty at this period; since, I presume, that this faculty, did it exist at all, is subservient to the purpose of procuring the reptile its food. The fact is, perhaps, valuable in another point of view. It seems to show, it does show, that the mephitic vapour proceeding from the rattle-snake, allowing that such a vapour really existed, was, in no respect, injurious to the bird.

No mephitic vapour was emitted by the snake in the experiment,

If the mephitic vapour of the rattle-snake were productive of the effects attributed to it by Mr. de la Cépède, and other writers; and, especially, if this vapour extended its influence to animals situated at a considerable distance from the reptile, the atmosphere of the rattle-snake would often be a kind of Avernus, which many animals would avoid, and which would generally occasion the sickness or death of those that were so unfortunate as to come within its sphere. But how different is the case! The abodes of the rattle-snake are the favourite haunts of frogs, and many species of birds, which often pass the seasons of their amours and generation in clouds of mephitism; uninjured, and undestroyed. How often has the rattle-snake been known to continue, for days, at the bottom of a tree, or even a small bush, upon the branches of which

nor in the woods at large.

* The *Emberiza hyemalis* of Linnæus.

the thrush or cat-bird are rearing their young! This would be a suitable situation for the mephitic vapour to exert its noxious influence; but, in our woods, such influence has never been perceived.

Other instances
in proof.

Birds of the eagle and the hawk kind have been seen to soar, for a considerable time, above the spot occupied by a rattle-snake, and at length to dart upon the reptile, and carry it to their young. Neither the parent-bird nor its young ones, have ever been known to receive any injury from the snake's vapour. Possibly it may be said, this vapour was dissipated, or greatly diluted, in passing through the air.

Whether other
animals emit
mephitic va-
pour.

A mephitic, or fetid, vapour emanates from the bodies of many animals, besides the rattle-snake; from the opossum*, and the pole-cat †, for instance. The vapour of these quadrupeds would be as likely to affect birds, &c. with asphyxy, as that of the rattle-snake. And possibly it does. There is, certainly, one thing in favour of the supposition. The opossum, in particular, is noted for his cunning in catching birds.

I shall conclude this part of my memoir by observing, that the odour of the rattle-snake is said to be agreeable to some persons.

Question, if the
agitated animals
may not have,
been already
bitten.

Mr. de la Cépède's second mode of explanation is much more plausible. I have already observed ‡, that it was the system of Sir Hans Sloane, who affected to ground it upon experiments. It is adopted by the author of the well-written account of de la Cépède's *Natural History of Serpents*, in the *Monthly Review* §.

Mr. de la Cépède presumes that, "for the most part, when a bird, a squirrel, &c. has been seen precipitating itself from the top of a tree, into the jaws of a rattle-snake, it had been already bitten; and that its whole conduct, such as its crying, its agitation, its leaping from branch to branch, &c. are all effects induced by the violent operation of the poison, thrown into its body, by the reptile.

* Didelphis Opossum.

† Viverra Putorius.

‡ See pages 30 & 34, *note*.

§ Appendix to the second volume of the *Monthly Review* enlarged. p. 511.

An attention to facts constrains me to reject this attempt towards a solution of the question, which I am considering. I shall arrange my chiefest objections under two heads.

Most probably not:

First. We are pretty well acquainted with the most prominent effects produced by the poison of the rattle-snake, in various species of animals. It must be admitted, that there is a considerable variety in these effects, and a great difference in the strength of these effects. In one animal, the poison produces an high degree of inflammatory action in the system; in another, the most striking primary effect is a somnolency, or drowsiness. In one animal, the poison does not produce any obvious effect upon the system for many minutes; in another the effects are almost instantaneous*. But in almost every instance in which the poison of the rattle-snake has been successfully thrown into the body of an animal, there ensue a set of symptoms, very different from the actions of birds and squirrels when under the supposed fascinating influence of the serpent-kind. It is not necessary to detail, in this place, these various symptoms, because I have already done it in a paper, which is printed in the third volume of the *Transactions* of our Society †, and because these symptoms cannot be unknown to the members of the Society. It will be sufficient to observe, that two of the most universal effects of the poison of the rattle-snake, I mean the extreme debility and the giddiness, which commonly almost immediately succeed the bite, will preclude the possibility of a squirrel's, or a bird's, dancing from branch to branch, flying about, and running to and from the serpent, for a considerable time, before it becomes a prey to its enemy. Besides, the farce of fascination is often kept up for a much longer term of time than any small animals are known to live after a successful bite by the rattle-snake. But, perhaps, it may be said, that the rattle-snake, like some of our wasps, knows how to inject into the animal, which he means to devour, any given quantity of his subtle poison. Here, the analogy will not apply: but I have not time to point out the various instances in which its failure is conspicuous.

—because the effects are more speedy and very different from what are related of fascination.

* A small dog that was bitten in the side by a large rattle-snake, reeled about, and expired, seemingly suffocated, in two minutes. This was in the month of August.

† No. xi. p. 110 and 111.

The system of fascination implies that the animal may be relieved and escape.

Kalm mentions a well-known fact, which will be admitted to have considerable weight in destroying the force of this part of Mr. de la Cépède's system. "The squirrel being upon the point of running into the snake's mouth, the spectators have not been able to let it come to that pitch, but killed the snake, and as soon as it had got a mortal blow, the squirrel or bird destined for destruction, flew away, and left off their moanful note, as if they had broke loose from a net. Some say, that if they only touched the snake, so as to draw off its attention from the squirrel; it went off quickly, not stopping till it had got to a great distance." "Why," continues our author, "do the squirrels or birds go away so suddenly and why no sooner? If they had been poisoned or bitten by the snake before, so as not to be able to get from the tree, and to be forced to approach the snake always more and more, they could however not get new strength by the snake being killed or diverted*."

(To be continued.)

SCIENTIFIC NEWS.

Very extensive Table of Squares.

ABOUT three years ago, a large quantity of mathematical papers were brought to Oxford, which had belonged to Mr. Councer, an attorney at Bloxam, in the north of Oxfordshire. They consisted chiefly of mere transcripts and collections from different publications: But there were two works among them, which must be excepted from this description, and which prove Mr. C. to have been a man of singular industry.

The one was a table of sines and tangents for every second of the quadrant. It is probable, that this table was completed before the year 1760; but it would have been of no value, even if it had not been superseded by Mr. Taylor's publication. For Mr. C. seems, in this instance, to have been ignorant of the true method of calculating, and to have

* Travels into North-America, &c. vol. ii. p. 209 & 210. It will be easy to discover what part of Kalm's reasoning, in the above quotation I admit.

only

only interpolated some old tables, without allowing for the variation of the differences. The other work is really valuable, it is a table of squares and cubes to a much greater extent than that, which was published by the Board of Longitude in 1781, under the care of Dr. Hutton*. This table was calculated for the squares of all numbers from 1 to 128540, and for the cubes of all from 1 to 26560. But there is an interval in the squares from 28261 to 29061. It appears to have been originally left in the manuscript, although it is impossible to conjecture the reason of the omission. The grocer, who had purchased the papers, had begun to tear this table before it could be rescued out of his hands; but as he had not got beyond the number 6000, this circumstance is comparatively of little consequence. In all other respects the work is complete. It is written in a clear hand, and (having been examined in several hundred places) appears to possess a degree of correctness, which may in general be depended upon.

Among the papers were proposals for printing this table; they are dated 1758. There were, likewise, two sheets of an introduction to them, printed in 1761. At this distance of time, it is impossible to ascertain the reason, which prevented the publication; but it is not improbable that Mr. C. might have been stopped by the great expence of the undertaking. Had Dr. Hutton been in possession of this manuscript, it might have saved much of his valuable time, and if any man of science should be employed in continuing his work, it will be useful to him to know that Mr. Cuncer's has been preserved.

P. S. Mr. Cuncer had, likewise, calculated all the powers from the 1st to the 13th (inclusive) of the series of numbers from 1 to 20. And he has continued this, for the nine digits, as far as the 26th power.

S. R.

* * I received the preceding notice from the respectable gentleman whose initials are subjoined; and am enabled to give such farther information as may conduce to render the papers of public utility.

W. N.

* Dr. Hutton's table is for the squares of all numbers from 1 to 25000, and the cubes of all from 1 to 10000.

Phosphoric

On Phosphoric Rings; from a Correspondent: R. B.

Phosphoric
rings.

I DO not know that any one has explained the phenomenon of those dense cloudy rings which are produced when phosphorated hydrogen arises through water, and explodes in the air; an effect similar to what is sometimes seen on the firing of cannon. Upon close inspection the fact appears to take place thus. The bubble of hydrogen, containing phosphorus in solution, rises and takes fire. Phosphoric acid is separated in the form of white smoke; through which dense mists the expansive action is directed upwards. A fluid opaque ring, denser than the atmosphere is thus formed, the inner surface of which is made to ascend by the rapid stroke, while the external part has received little or no impulse. The natural consequence is a quick rotation of the ring, from within outwards, which shews itself to the sight, and seems, in some manner or another, as if it kept the parts together.

How to measure the Contents of any Pipe by a very ready Method.

Contents of any
pipe in pounds
or gallons.

MY ingenious correspondent, Mr. Woolf, sometime ago gave me a ready method of measuring the contents of a pipe, as follows:

Square the diameter in inches, and the product will be the number of pounds of water in every yard length of the pipe. Or if the last figure be cut off or considered as a decimal, the remaining figures will give the ale gallons in the yard.

Dr. Winterbottoms' Machine for clearing Roads from Mud.

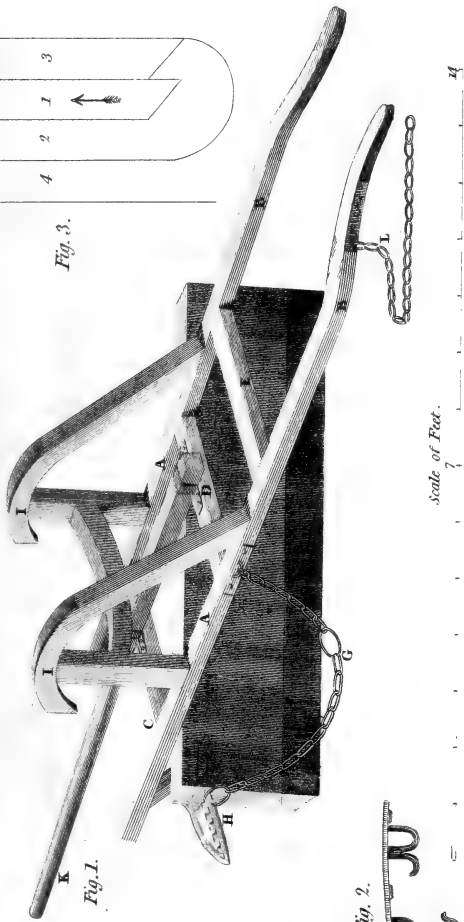


Fig. 1.

Scale of Feet.

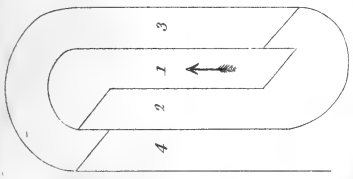


Fig. 3.

Fig. 2.





M. Robert Greens Hand Drill for Tons &c.

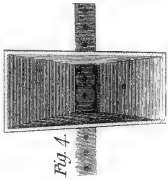
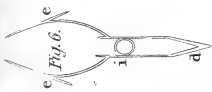


Fig. 3.

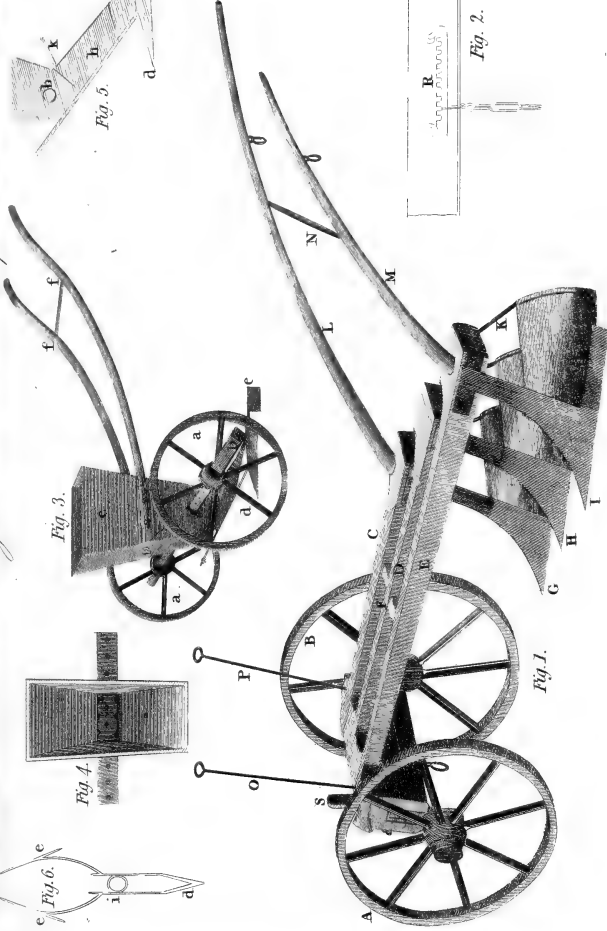


Fig. 1.

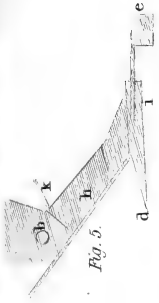


Fig. 5.

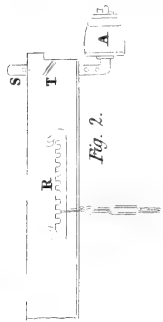
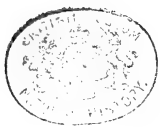


Fig. 2.

The Rev. E. Cartwright's three furrow Plough.



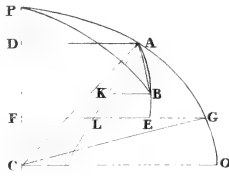


Fig. 1.

Figure of the Earth.

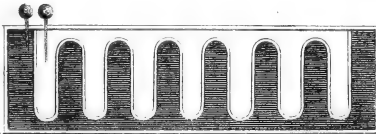


Fig. 2.

Galvanic Apparatus.

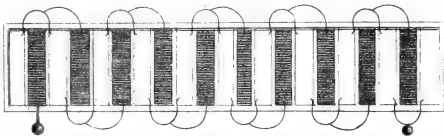


Fig. 3.

New Galvanic Trough, W. N.

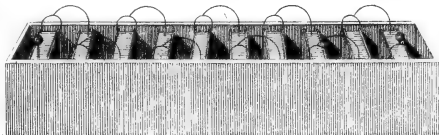
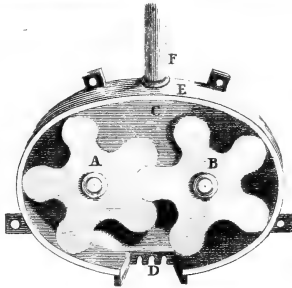
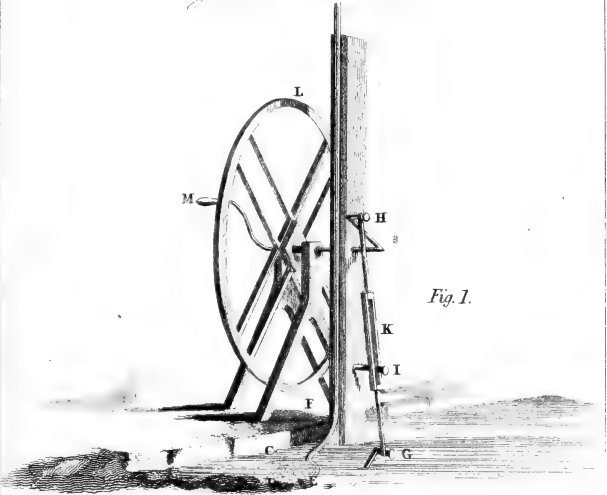


Fig. 4.

Perspective view of Fig. 3.



Hydraulic Machines.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1804.

ARTICLE I.

Letter from Mr. EZEKIEL WALKER, on the Methods of observing the Longitude at Sea; with an Exhibition of the very great Accuracy of the mean Result from a Number of Chronometers.

To Mr. NICHOLSON.

SIR,

THE two most approved methods of finding the longitude at sea, are those by lunar observations and by time-keepers. Although these methods are now brought to considerable perfection, yet they can only be looked upon as in a progressive state towards that point of precision, at which the astronomer and the artist have long been labouring to arrive.

Longitude found by lunar observations and by time-pieces.

The lunar theory is still but imperfectly understood; but perhaps the time is at no great distance, when it shall be as well known as the theory of any other planet. Nor is it unphilosophical to suppose, that some property of matter, which is at present unknown, may hereafter be discovered, by means of which a time-keeper may be so constructed as to perform as well at sea as a regulator on shore.

Both methods still improveable.

These are conjectures, though probable, that I shall not insist upon any further, but proceed to inquire into the merits of the two methods in their present state of perfection.

The lunar method will be for ever subject to those intervals wherein the moon cannot be seen.

The imperfection of the lunar method is, that in consequence of bad weather, or the moon's not being above the horizon, it sometimes happens that the necessary observations cannot be taken for many weeks. And these long intervals between one lunar observation and another, must for ever happen from the same causes; and those causes are such as no human art can remove.

Instances of the much greater frequency of observations by time-pieces.

La Perouse, in his voyage round the world, was at one time 31 days without taking a single lunar observation; but he had, within that time, 20 observations by his time-keeper. At another time during the same voyage, 24 days elapsed without a lunar observation; but he had 20 observations by his time-keeper. Lord Hugh Seymour, in his voyage to the West Indies in the year 1796, had been at sea near six weeks before the first lunar observation was obtained, in which period the longitude had been obtained 30 times by his time-keepers.

Time-keepers may stop; or may alter; or be neglected in winding up; or may suffer from accident.

What has just been mentioned sets time-keepers in a very favourable light, but it remains to inquire into their imperfections. 1. A time-keeper may stop. 2. It may alter in its rate of going. 3. It may not be wound up through forgetfulness. And, 4. It is liable to other accidents, even in the most careful hands. But these are defects which may be removed, and a result obtained, which will be more perfect than by any time-keeper that has yet been made.

Method of obtaining a more perfect result from time-keepers; proposed upwards of 20 years ago.

The method which I have to mention must be known to many gentlemen, as I had the honour to lay it before the commissioners of longitude in the year 1783. The method, however, seems either very little known, or not much attended to. La Perouse made no use of it in his voyage round the world; hence it may be supposed that it was unknown in France at that time. Nor is it once mentioned by Mr. Wales in his excellent treatise on the method of finding the longitude at sea by time-keepers; although he had "been pretty intimate with the subject for near forty years"*.

It consists in taking the mean result of a number of machines.

The method which I have recommended depends upon a particular use of time-keepers as they are now constructed. Although the best time-keeper be too imperfect a machine to be depended upon for determining the longitude in long

* Preface to Wales on the longitude, p. 5.

voyages, yet if five, six, or more, be taken to sea in the same ship, the longitude computed by each separately, and the mean of their results taken, it will come exceedingly near the truth, even at the end of three or four months.

Another very great advantage resulting from this method is, that, should one of the time-keepers stop, it may be set going again by the rest; or should one of them be found to go very incorrectly, its rate may be rejected, and the longitude still determined by those which shall be supposed to go well; in short, this method is almost intirely free from all those accidents to which a single time-keeper is liable.

The two following examples, which are taken from the rates of chronometers that have been determined in fixed observatories, will show the perfection of this method more clearly. In the first example, which contains a period of 30 days, there are only two instances in which this method differs so much as a single second from mean time. In the second example, containing a period of six weeks, the greatest error on any one day, during that time, is only $5''.9$, and the total error at the end of that period, is only $1''.9$.

It may be said that these are favourable specimens, but supposing that these chronometers had gone *ten times worse* than they did, the greatest error in the first example would have amounted to no more than $11''.3$; and the greatest error in the second example would not have exceeded a single minute.

This method saves the time in case of neglect in winding up,

and affords a wonderful degree of precision. Examples.

If the chronometers had performed ten times worse, the error would have been less than one minute per month.

FIRST EXAMPLE.

Table of the going of three chronometers, and their mean result.

Day of the Month.	<i>Mr. Arnold's Chronometers.</i>			Mean Daily Rate of Three Chronometers.	Total Error of Three Chronometers.
	Daily Rate of No. 36.	Daily Rate of No. 51.	Daily Rate of No. 59.		
	Feb. 1779	Feb. 1782	Feb. 1783		
1	+ 0'' .79	- 0'' .3	- 1'' .5	- 0'' .33	- 0'' .33
2	- 0 .15	+ 1 .1	1 .5	0 .18	0 .51
3	+ 0 .15	- 0 .4	+ 0 .3	+ 0 .02	0 .49
4	0 .00	+ 0 .5	0 .7	0 .40	0 .09
5	- 1 .16	0 .0	0 .0	- 0 .39	0 .48
6	+ 1 .34	- 0 .7	- 1 .6	0 .32	0 .80
7	0 .81	1 .3	0 .4	0 .30	1 .10
8	- 0 .26	0 .0	+ 1 .4	+ 0 .38	0 .72
9	+ 0 .56	0 .5	0 .2	0 .09	0 .63
10	0 .07	0 .0	0 .6	0 .22	0 .41
11	- 0 .45	+ 0 .2	0 .0	- 0 .08	0 .49
12	0 .39	0 .3	- 1 .2	0 .43	0 .92
13	0 .25	0 .6	0 .4	0 .02	0 .94
14	0 .35	0 .9	+ 1 .4	+ 0 .65	0 .29
15	1 .48	- 0 .9	1 .2	- 0 .39	0 .68
16	+ 0 .09	0 .2	0 .2	+ 0 .03	0 .65
17	0 .30	0 .5	2 .2	0 .67	+ 0 .02
18	- 0 .58	0 .7	1 .4	0 .04	+ 0 .06
19	0 .77	0 .1	1 .6	0 .24	0 .30
20	0 .90	0 .9	- 0 .6	- 0 .80	- 0 .50
21	0 .00	0 .0	+ 0 .2	+ 0 .07	0 .43
22	1 .55	0 .0	1 .8	0 .08	0 .35
23	0 .48	+ 1 .0	0 .6	0 .37	+ 0 .02
24	0 .76	0 .7	1 .6	0 .51	0 .53
25	0 .29	0 .9	1 .2	0 .60	1 .13
26	0 .68	1 .0	- 1 .2	- 0 .29	0 .84
27	1 .74	1 .1	0 .8	0 .48	0 .36
28	0 .49	0 .1	+ 0 .5	+ 0 .04	0 .40
Mar. 1	1 .86	0 .4	0 .7	- 0 .25	0 .15
2	- 0 .80	- 0 .7	+ 1 .8	+ 0 .10	+ 0 .23

SECOND EXAMPLE.

	<i>Mr. Mudge's Chronometers.</i>		<i>Mr. Ar- nold's No. 86.</i>	Mean Daily Rate of Three.	Total Error of Three Chronome- ters.
	Green.	Blue.			
	Daily Rate.	Daily Rate.			
1	- 1''.12	- 0''.27	- 0''.3	- 0''.56	- 0''.56
2	.61	+ .06	0 .1	.22	0 .78
3	.97	- .40	+ 0 .1	.42	1 .20
4	.64	.40	0 .3	.24	1 .44
5	.16	.16	0 .1	.07	1 .51
6	.11	.05	—	.05	1 .56
7	.64	.41	- 0 .8	.62	2 .18
8	.47	.57	0 .7	.58	2 .76
9	.35	.13	1 .1	.53	3 .29
10	.99	.45	0 .7	.71	4 .00
11	.05	.30	0 .1	.15	4 .15
12	.74	.37	+ 0 .1	.33	4 .48
13	.36	.12	0 .7	+ .07	4 .41
14	+ .04	+ .08	0 .2	.11	4 .30
15	+ .05	- .38	0 .5	.06	4 .24
16	- .26	.34	- 0 .6	- .40	4 .64
17	.00	.45	1 .2	.55	5 .19
18	+ .43	.18	0 .8	.18	5 .37
19	- .36	.49	0 .6	.48	5 .85
20	+ .07	.19	0 .0	.04	5 .89
21	+ .39	.23	0 .0	+ .05	5 .84
22	.44	.17	+ 0 .2	.16	5 .68
23	.59	.04	+ 0 .0	.18	5 .50
24	.49	.46	- 0 .2	- .06	5 .56
25	.65	.46	0 .3	.04	5 .60
26	.20	.74	+ 0 .1	.14	5 .74
27	.86	+ .08	0 .3	+ .41	5 .33
28	.53	- .41	1 .5	.54	4 .79
29	.74	.85	1 .3	.40	4 .39
30	+ .67	.07	0 .2	.27	4 .12
31	1 .50	+ .85	0 .6	.98	3 .14
1	.80	- .03	1 .8	.86	2 .28
2	.87	.23	1 .0	.55	1 .73
3	.63	+ .02	2 .6	1 .08	0 .65
4	1 .28	.16	0 .4	.61	0 .04
5	.43	- .51	2 .2	.71	+ 0 .67
6	.84	.42	0 .8	.41	1 .08
7	.61	.18	0 .6	.34	1 .42
8	.25	.68	0 .2	- .08	1 .34
9	.38	.54	- 0 .6	.25	1 .09
10	.41	.39	0 .2	.06	1 .03
11	- .83	- .27	+ 2 .2	+ .92	+ 1 .95

Table of three
other chronome-
ters.

From

Hence the longitude at sea may be well determined,

From these examples we may see the nature of fortuitous events, how regularity rises out of irregularity; the error of one time-keeper correcting that of another in a wonderful manner; and as the number of time-keepers increases, the error will decrease, until it be almost annihilated. Consequently the method of finding the longitude at sea is no longer doubtful, as it may, by this method, be found to any degree of precision that may be useful in navigation.

and the expence is inconsiderable.

The only objection that can be advanced against it is the expence; but the sum of two or three hundred pounds bears no proportion to the value of a British Squadron, nor even to the value of a single East India ship; and this sum would purchase as many time-keepers, at their present reduced prices, as would, I presume, be sufficient to secure any ship from that danger which might arise from the want of knowing the longitude.

EZEKIEL WALKER.

Lynn, April 19, 1804.

II.

On Galvanism. By C. WILKINSON, Esq.

May 6, 1804.

To Mr. NICHOLSON.

SIR,

Introductory letter.

BEING now engaged in some philosophical lectures at Bath, I have had frequent opportunities of conversing with Dr. Gibbes, a gentleman of considerable scientific information, relative to his opinions as to the apparent decomposition of water by the galvanic process. As many gentlemen of eminence in this department of philosophy are converts to his doctrine, I beg leave, through the medium of your valuable Journal, to state the outlines of Dr. Gibbes's theory to the public, with a few cursory observations which have occurred to me.

I am, Sir,

Your's with great respect,
C. WILKINSON.

No. 19, Soho-Square.

Soon

Soon after the publication of the valuable discovery of the decomposition of water by the galvanic process, by Messrs. Nicholson and Carlisle, Richter, an eminent German philosopher, observing that water becomes decomposed by wires placed at a considerable distance from each other, could not conceive that the same identical particle of water could be influenced by two wires at the same instant of time; and hence he conjectured that the Lavoisierian theory relative to water being a compound substance must be incorrect.

In the decomposition of water by galvanism the effects are produced at great distances asunder.

Dr. Gibbs supposes water to be an elementary principle, and that the two gases which are produced are compounds. Thus the oxygen gas which is disengaged from the zinc end of the battery, is taken to be a compound of water and positive electricity; the water constituting its ponderable part, while the hydrogen gas given out by the other wire connected with the copper end of the battery is concluded to have water also for its ponderable part in a state of combination with negative electricity.

Dr. Gibbs supposes the oxygen to be water and plus electricity, and the hydrogen to be water and minus electricity.

Dr. G. considers positive and negative electricity as two distinct principles, which when in a state of union constitute caloric. Thus in the explosion of a mixture of oxygen and hydrogen gases, the two electricities enter into union and form caloric, while the respective ponderable bases are precipitated in the state of water.

That plus and minus electricity compose caloric.

Upon this hypothesis, the circumstances of the evolution of flame and heat, together with the production of water, are capable of explanation.

Hence flame and heat,

Also upon this supposition some notion may be formed, why the gases are produced in the vicinities of each wire, and why oxygen gas must be evolved from the wire connected with the zinc end of the battery, and hydrogen gas from the wire united with the copper end.

and the galvanic phenomena.

This ingenious supposition, which does great credit to the well-known abilities of Dr. Gibbs, in the first point of view seems to receive additional support from its apparent simplicity. But upon more minute investigation, I am induced to think it will be found more complicated than the Lavoisierian theory, and by no means so general in its application.

Examination of the theory.

In the first place it should be proved that there are two such distinct principles as positive and negative electricity; and in support of this, the experiments of Eeles, Symner, Atwood, &c. are by no means conclusive.

The two electric principles not proved.

When

Nor the decomposition of ammonia explained.

When pure ammonia is exposed to the galvanic action, we have, as before, hydrogen from one wire, and nitrogen from the other. How can these productions be explained upon the principle of Dr. Gibbes?

This theory supposes oxidation to be the addition of water and subtraction of electricity. But the pile requires oxygen from the atmosphere.

This able physician further observes that oxydation is a combination of water with the metallic body, consonant to the opinions of Priestley, Watt, and others.

When a galvanic pile is placed under a receiver situated over water, while the galvanic process goes on, by having a metallic circuit from the top to the bottom plate, a loss of air ensues, and which is found to be the pure part of atmospheric air: if the pile be placed in an exhausted receiver, the galvanic process is very trivial, and the plates very slightly oxydated. How upon the principles of Dr. Gibbes can we explain these phenomena? If oxydation be merely the combination of water with the metal, why should the pure part of atmospheric air be thus separated while the metals are in immediate contact with so much water?

Oxydation appears to require the disengagement of electricity.

Whether galvanism be the cause or the effect of the chemical change induced in the metallic substance, they appear to me to be contemporaneous results, similar to what appears in the immersion of iron in a solution of the sulphate of copper, and whether the iron be dissolved prior to the copious precipitation, no experiments I am aware of can decide. From various circumstances it appears that oxydation cannot take place unless the combined electricity of the metal be capable of being disengaged. This is rendered evident in a very happy experiment of Dr. Ash, who having been the associate of the much-lamented Humboldt* in his scientific pursuits, may now be deemed more acquainted with the minutiae of galvanism than any other person. This gentleman has remarked that when a plate of zinc is immersed in a weak solution of sulphuric acid and water, a decomposition takes place, the oxygen base combines with the metal, while the hydrogen is disengaged in a gaseous state. When a plate of silver is immersed, no decomposition takes place; the very instant a contact is effected between the two metals, whether by portions out of the fluid or in the fluid, then the silver is immediately acted on and disengages hydrogen also, and itself becomes oxydated.

Dr. Ash's experiment.

* Humboldt died lately at Acapulco, of the yellow fever, while attempting to perfect his geological observations.

In the first instance the evolution of electricity appears to be the result of a chemical action of the sulphuric mixture on the zinc, while in the latter, no action is induced on the silver till some alteration is effected as to its electrical state; as we well know from other galvanic experiments, that of zinc and silver, when thus combined, the zinc evinces positive signs, and silver negative; it is hence evident that an abstraction of electricity from silver is requisite prior to its capability of being acted on by the sulphuric acid.

If I might presume to submit to some of your chemical readers, an examination of such compound of silver as may thus be effected; a sulphate of silver is actually formed, it might lead to some important discoveries. I am every day more and more persuaded that light and electricity are the two most active agents we possess, and a more minute enquiry into their respective energies, I have no doubt, would enable us to explain many physical changes in the material world, with which we are at present perfectly unacquainted.

The very intense light disengaged from charcoal by means of my extensive apparatus, has occasioned me to be frequently honoured with the attendance of Dr. Herschell; from some conversation I have had with him, I am in hopes he will be induced to institute some experiments relative to the evolution of light, so nearly approaching to solar light. If these should be conducted, I shall do myself the honour of stating them to the public through the medium of your valuable Journal.

III.

On the Formation of Snow. By G. A.

To Mr. NICHOLSON.

SIR,

THE remarks contained in the enclosed pages, may not, perhaps, evince any originality of thought, nor more than would occur to the most superficial observer; yet the suggestions of some persons, frequently lead to the observation of valuable facts by others. Therefore, Sir, you will make whatever application of the paper you consider best.

If

If you think it worthy of insertion in your valuable Journal, it will be considered as an honorary obligation conferred on,

Your's, most respectfully,

23d April, 1804.

G. A.

On the formation of snow;—

The frequent changes of the weather that have taken place during the last winter, having induced me to direct my attention to meteorology; I confess, that the manner in which philosophers account for some of the phenomena that occur, is not, to me, altogether satisfactory.

—supposed to be effected by electricity.

It is not surprizing, that electricity (with the immediate agency of which we are so little acquainted) should be resorted to, as the grand agent in all meteorological phenomena. Accordingly we find, that snow, and indeed every variety of weather we experience, is considered to be more or less effected by the electric fluid.

Snow is generally supposed to be the vapours of the atmosphere, disengaged by the electric fluid, and frozen.

Questions respecting the mode, &c.

But it appears to me, that before we receive so vague an explanation, the following questions might be asked:—

What are the vapours of the atmosphere composed of?

By what laws, and in what manner does the electric fluid act, either in the formation of snow, or as a component part of it?

Electricity supposed not essential to snow;

I shall now offer a few remarks to strengthen a supposition, that the electric fluid is not engaged in, or in the least essential to the production or existence of snow.

—but a change of wind.

By an attentive observation of all the circumstances that have attended the fall of snow, during the last winter, I have, in almost every instance, found that it is accompanied with, or rather preceded by a change of the wind; and that the wind, previous to the fall of snow, blew from some point between the *South*, and the *West*; and afterward from some point between the *East*, and the *North-West*.*

* If it is observed, that we sometimes have snow, without the wind changing to any of the points above-mentioned, or, even without a visible change to us; yet it does not militate against the following remarks; for it has been observed by aeronauts, that different strata of air blow from *opposite points* at the same time.—Therefore, notwithstanding a south wind may prevail at the surface of the earth, a superior stratum may blow from the North.

Such

Such being the facts, is it not probable, that a change of the wind is the cause of snow?

Now let us examine, whether such a cause will produce such an effect.

The winds that blow from any of the points between the South and the West, by coming from warm climates, and passing over, perhaps, a very large tract of water, where there is a powerful evaporation going on, must possess a very great degree of humidity, and are most commonly, of a temperature between 45° and 60° of Fahrenheit.

Warm winds meeting cold deposit their vapours in snow.

The winds which blow from any of the points between the East and the North-West, by coming mostly from such high latitudes, and passing over immense fields of ice, where evaporation is undoubtedly greatly impeded, cannot be supposed to contain much water in solution, but must bring with them very great degrees of cold.

Namely, north-easterly meeting south-westerly winds.

Now let us suppose that a north wind of any temperature between 32° and 0° (which it generally is, in superior strata of the atmosphere) meets a south-west wind, as before-mentioned, the consequence will be, that the intense cold which accompanies the former will convert the water with which the latter is impregnated into ice; and the instantaneous application of cold is probably the reason why snow is produced in what we call *flakes*; for before the vapour can concentrate itself into large particles, or drops, it is arrested by the intense cold.

In this view, the formation of snow appears to be a beautiful chemical phenomenon; for the warmer air, having a greater affinity for the colder air than it has for the water which is held in solution, the water is disengaged, crystallized by the cold, and precipitated in the form of snow.

Formation of snow.

It is generally observed, that it is unusually cold for half an hour or an hour before the fall of snow, and warmer afterwards. Might not this be accounted for, by considering that the adverse wind must meet with considerable resistance, in effecting either a union with, or a passage through a stratum of air surcharged with water, and consequently must be in a great degree reflected back again, not in the perpendicular, but as radii from a center, in an oblique direction, part of which must descend to the earth. And it will undoubtedly be warmer, after

after

Formation of
snow.

after the stratum of north wind has either forced a passage through or effected a union with the south-west wind.*

Though I have not, in the preceding observations, considered the electric fluid as at all essential to the production of snow, yet I do not deny the presence of it. That snow contains the electric fluid, cannot be doubted; but it does not follow, that the latter is *necessary* to the existence of the former. We know of no substance in nature, that is impervious to that subtle fluid; it seems to pervade *all bodies* with nearly the same facility as caloric. Therefore, though snow indicates electricity, it is probably no more than it has acquired in its passage through an electrified atmosphere.

If those who are much more competent to the task than myself, would direct their attention to this most interesting branch of natural philosophy, I am inclined to think, they would find the result of their enquiries highly gratifying. Meteorology cannot be considered, but as yet in a state of infancy; for the greater part, our knowledge of it is hypotheses, which we cannot support by experiment. Therefore, it is only by a close observation of facts, accompanied by just inferences drawn from them, that we can arrive at any degree of certainty on this complicated subject.

IV.

Medico Chemical Researches on the Virtues and Principles of Cantharides. By H. BEAUPOIL.†

Experiments and
observations on
cantharides.

THOUGH the animal kingdom presents us with only a small number of substances of use in medicine, it must nevertheless be admitted, that among those we possess, there are some of which the effect is so certain, so constant, and so definite, that if we were deprived of them, it would be difficult, and perhaps impossible, to find others to supply their place.

Cantharides are of this number; their mode of action is universally known, together with the advantages they afford in many disorders. It is not, therefore, to be wondered that the

* The water gives out heat in congelation. *Vide* Irwine, Black, Crawford, &c.

† *Annales de Chimie*, XLVIII. 29.

examination of these insects should have engaged the attention of celebrated physicians, and that their analysis should have been frequently attempted by chymists.

Experiments and observations on cantharides.

The principal aim of all those who have operated upon this material, has been to discover whether the blistering property which it so eminently possesses does generally appertain to all the parts of the animal, or whether it do not rather reside in some peculiar matter, which, independent of the parts which accompany it, is capable of acting alone, and producing the effects which are observable by the entire cantharides.

It is undoubtedly needless to repeat in this place, what has been said and done respecting this object; but it is essential to remark, that no one before Thouvenel pursued that course which could lead to the solution of the problem offered for consideration; and accordingly, we must consider the period in which that physician published his experiments on cantharides, as the earliest time in which philosophers could indulge the hope of ascertaining some positive information respecting the nature and properties of the immediate material of those insects.

But while we render justice to the labours of Thouvenel, we must confess that he has not carried them to an extent answerable to his happy commencement. For he has neglected some of the most important questions, and among others, those which relate to the vesicatory, diuretic, and aphrodisiac properties of cantharides.

It was to supply in some measure the deficiency of that respectable philosopher upon the above three points, that Citizen Beaupoil has thought fit to undertake a new examination of cantharides. The paper in the annals consists of an extract from his memoir, by Citizen Deyeux.

The author divides his dissertation into four parts.

In the first he gives a rapid sketch of the specific properties of cantharides; the methods used for collecting them, and the preparations to which they are subjected previous to their introduction to the market as an article of commerce.

In the second he gives a slight history of the use and application of these insects from the time of Hippocrates to the present period.

In the third we find an accurate outline of the attempts made by chymists to analyze the cantharides, as well as an account of his own particular experiments and their results.

The

Experiments and observations on cantharides.

The fourth contains every thing which relates to the physiological essays made with these animals; the effects produced by their exhibition, whether internally or externally; and lastly, observations relative to the opening of the bodies of several dogs, to which the author had given either the entire cantharides, or the different immediate materials, which he separated by means of his processes.

As the first and second parts contain nothing more than is to be met with in various authors, it is unnecessary to attend to them, and accordingly, the abridger has confined himself to the third part, in which the chemical facts are given.

Thouvenel, who was the first rational experimenter on cantharides, made use of water and alcohol to separate the soluble parts of these insects, and the results he obtained were,

1. A yellow reddish extractive matter, of a sharp bitter taste, resembling, as he says, that of ants, but less acid.
2. Another yellow matter, of a paler colour than the former, and nearly insipid.
3. A fatty matter, of a green colour and acrid taste, possessing the smell by which the entire cantharides are distinguished.
4. Lastly, A parenchymatous matter.

Citizen Beaupoil obtained similar products; but he not only ascertained their existence, but examined them separately, and in this it is that the difference between his labours, and those of Thouvenel principally consists.

He first observed that the aqueous solution of the peculiar extractive matter afforded by cantharides, does not fail to undergo a kind of alteration when exposed to the air; that the fluid becomes turbid, affords a yellowish precipitate, which acquires a peculiar odour; becomes covered with a viscid pellicle, emitting a foetid ammoniacal smell; and that when it has arrived at this term, the fluid no longer exhibits any sensible change. He afterwards remarked that the solution here mentioned before it undergoes those changes which are produced by exposure to the air, strongly reddens the tincture of turnsole; that when mixed with rectified alcohol or ether, it becomes divided into two parts, nearly equal; the one possessing the form of a black adhesive precipitate, insoluble in alcohol, and the other that of a yellow brown matter, very soluble in that fluid.

(The Conclusion in our next.)

Description

V.

Description of a Galvanic Apparatus affording a large Surface for Oxidation, and convertible into one or more Plates at pleasure. By I. R. I.

To Mr. NICHOLSON.

SIR,

Edinburgh, May 10, 1804.

I AM much flattered by the notice that you and Mr. Wilkinfon have been pleased to take of my late communication, and I think I have done no small service to science by drawing forth from both of you, the very ingenious disquisitions towards the œconomy of galvanism, which appeared in your last Journal.

Your description of what may be called a Polychrest trough, Improvement in the galvanic trough. seems to reach as near perfection, in every requisite property for giving the shock, or deffagrating metals, as could be wished: Yet there is still a further improvement which has suggested itself to my mind; but as it is probable the waste of the zinc would be more considerable, I mention it with diffidence.

It appeared to me in some experiments I made with the *couronne de tasses*, that the metals were more corroded to produce the same effect, than happened either in the pile or the trough; but if Mr. W.'s idea is just (which is most likely to be, from his great experience in the science), that the effect is in proportion to the oxigenation of the metal, the following plan appears the best adapted for both œconomy and power. Corrosion greatest in the *couronne de tasses*.

In Fig. 3, Plate VI. A represents, by a side view or section, Description. a plate of zinc of six inches by three; in its centre is a square piece, from which rises another piece of a smaller diameter, either square or round. These projecting bits are both sunk in a block of wood somewhat larger than the whole plate, but not so as to pass through it; and the plate must stand clear of the wood and every part of the cell, hanging entirely by the knob, and strongly cemented. A bore is made down through the thickness of the wood, in a line with one through the small knob of the zinc.

The copper wire B, passed through that hole, is the medium of communication from cell to cell; and the same mode may be

be employed in forming them into one plate, as suggested, in your last, when a deflagrating power is required.

The intermediate parts express the wooden blocks, which must be well covered with cement on every side, and inserted into grooves, as the zinc and copper plates usually are.

The engraving that referred to my last communication, was inaccurately explained, as the dotted work was said to express the zinc, whereas the acid liquor was denoted by it.

I remain, Sir,

With much respect,

Your obedient servant,

I. R. I.

VI.

On the Difference between the Effects of Electricity and of Heat.

By Cit. BERTHOLLET.*

Difference between electricity and caloric.

I HAVE thought it important to determine the difference which exists between the action of the electric fluid and that of caloric, and the cause which renders their effects similar, more especially, as in the lessons of the Normal schools this similitude of effect made me adopt the opinion of those who have considered the electric fluid to be caloric itself; I consequently requested permission of Citizen Charles to make use of his powerful apparatus in the experiments which appeared to me to be necessary on this subject. With that civility which those engaged in similar pursuits are always sure to experience from him, he undertook to perform them himself. I now give the result, such as it was communicated to me by Guy Lussac, who assisted in the experiments.

Platina was not much heated by an electric shock nearly capable of burning it.

A wire of platina was submitted to shocks which were nearly strong enough to effect its combustion; and to be satisfied of this, a shock was excited by which a great part of the wire was melted and dispersed; afterwards the shocks employed were a little weaker, and immediately after each the wire was touched to judge of the temperature it had acquired: a heat was felt, which was dissipated in a few minutes, and which,

* From his *Essai de Statique Chimique*.

at the utmost, was estimated to resemble that of the boiling point of water. If electricity liquefied metals and brought them into combustion by the heat it excites, the platina wire must, after a shock which differed but little from that which would have produced its dispersion and its combustion, have approached the degree of temperature which occasions its liquefaction: now this degree, which is the most elevated that can be obtained, would, according to the valuation, more or less accurate, of Wedgwood, be 32277° of Fahrenheit.

When the shock is sufficiently strong to destroy the aggregation of the platina wire, it begins by detaching *moleculæ* from its surface, which exhale like smoke; if it is strong enough to produce combustion, the remains of the wire appear to be torn into filaments.

Electricity detaches the parts of platina, but does not fuse it.

A thermoscope, blackened with ink, and placed in the stream of a strong electric spark, only experienced a dilatation which was nearly equal to one degree of Reaumur's thermometer; and this slight effect might depend on the oxidation of the iron of the ink: placed beside the current, it did not shew any dilatation, although the air was necessarily affected by the electric action: it was the same when it was placed in contact with a metallic conductor which received a stream less powerful than in the preceding experiments.*

The electric current of sparks causes little heat.

A cylinder of glass filled with air, with an exciter at each of its extremities, to one of which was fixed a tube, communicating with another cylinder filled with water, produced an impulse at each shock which raised the water more than a decimeter above its level; but its effect was instantaneous.

Electric shock through air.

These experiments seem to me to prove that electricity does not act on substances, and on their combinations, by an elevation of temperature, but by a dilatation which separates the *moleculæ* of bodies. The slight heat observed in the platina wire is only the effect of the compression produced by the *moleculæ* which first experience the electric action, or which experience it in a greater degree; it must therefore be compared to that excited by percussion or compression.

Hence it is inferred, that electricity acts not by heating, but dilating.

If the dilatation was the effect of heat, that experienced by a gas in the experiment related above, would not have been

* A small thermometer in the luminous current between two balls of wood, is raised 32 degrees.—Nairne.

instantaneous, it would only have experienced a progressive diminution by cooling, as when its expansion is owing to heat.

When ammoniacal gas is decomposed by electricity, it remains cold.

In the experiment by which ammoniacal gas is decomposed, the gas indubitably receives the electric action, and nevertheless it is not heated; and as soon as the decomposition is finished, its volume remains unchanged, because the electric action which is employed in this experiment, is not sufficiently energetic to cause a perceptible dilatation. No sensible dilatation is produced in a gas by a shock which is not very strong, because the impulse not being gradual, like the expansion arising from caloric, and being excited instantaneously, the resistance of the liquid becomes too great, and cannot be overcome unless the dilatation has great energy.

Lead exploded in azote continues metallic, and is not fused.

An experiment of Dieman and his learned associates confirms this explanation: They caused a shock to pass through lead placed in a vessel filled with azote gas, which could not oxidate it; it was reduced into powder retaining all its metallic properties: If it had experienced a liquefaction similar to the action of heat, it would have cooled gradually, and would have congealed into one, or at least into several masses.

No heat is produced by the electric dilatation of metals: the ignition and heat are from oxidation or combustion.

When a metal is submitted to the electric action, the effects produced immediately by the electricity must be distinguished from those which are owing to its oxidation: The first are limited to the diminution or destruction of the effects of the force of cohesion, to removing and dispersing the molecules; (if by this a little heat is disengaged, it is only owing to the compression sustained by some of the parts); but those which are occasioned by the oxidation, produce a high degree of heat, and then the effects assume all the appearances of an ordinary combustion; hence it arises that the most oxidable metals are those which become red with the greatest facility, and which most shew the properties of a metal liquefied by heat.

Electricity favours oxidation by diminishing cohesion.

Electricity favours this oxidation, in as much as it diminishes the force of cohesion: it is thus that an alkali renders the action of sulphur on oxygen much more powerful, by destroying the force of cohesion opposed to it, and that a metal dissolved in an amalgam is oxidized much more easily than when it is in a solid state. It is only by destroying the effects of the force of cohesion that heat itself produces the oxidation of metals, but the expansive action of electricity will have a great advantage

advantage over that of caloric, because its action is confined to the solid which it encounters in its course, so that the gas itself will not experience a dilatation in opposition to the condensation which accompanies the combination: To this circumstance may be applied what is observed in the action of hydrogen gas, which is capable of completely reducing an oxide of iron placed in the focus of a burning glass, although water, whose two elements receive the heat equally, is decomposed by this metal.

It is probable that it is also to the expansive effect of an electric current established between two metals, having a stratum of water interposed, that the oxidation observed by Faraday between these substances, placed in contact in water, is owing, and which, in this case, appears to be confined to the combination of the oxygen which is held in solution in this liquid*.

Oxidation of metals by electricity and water.

All the chemical effects produced in substances submitted to the action of electricity, seem capable of being deduced from these considerations, and of being explained by the diminution of the force of cohesion, which is an obstacle to the combinations which their molecules tend to form; but the differences which may be offered by positive electricity and negative electricity remain to be determined: the chemical effects of the pile of Volta may be much more considerable than those of the common electricity, although the latter possesses a much greater tension, because its action being necessarily interrupted, the chemical effects which require time to be accomplished, can only begin to be effected, and may even be destroyed, by the sudden re-establishment of the first state of the body; while the permanence of the action of the electromotive apparatus, although weaker at each instant, may give rise to the chemical changes which it promotes, by diminishing the effects of the force of cohesion.

This explanation of the agency of electricity is general, but does not shew the difference between plus and minus.

Galvanism may owe its greater energy to its constancy.

I do not myself consider the explanations I have now hazarded as more than conjectures, which observation may confirm or destroy.

* Journ. de Phys. Vendem. An. X.

VII.

Letter from a Correspondent I. R. I. explaining some Facts in Galvanism, and on other Objects.

To Mr. NICHOLSON.

SIR,

Edinburgh, May 19, 1804.

Observations on galvanism.

I SENT you, a few days ago, some hints towards a farther improvement of the galvanic apparatus*, and I hope that the following observations will arrive in time to be added as a supplement to my last communication.

There seem to be two very marked laws in galvanism; the first is, that the phænomena are produced by moist oxidation, which seems a general one; but, by the second law, that they remain latent till one part of the conducting metal be made perfectly dry: this is at least strictly correct where two metals are employed.

From the tendency of galvanic effects to become latent, by the use of moist conductors, a pile cannot be formed unless one side of the oxidated metal be covered from moisture by cement, besides having a conductor of a different metallic substance. The trough is, therefore, best adapted to æconomize galvanism, as I have shewn, that nearly as large a surface can be exposed to the action of the fluid in it, as in the *couronne de tasses*, over which it has many advantages of steadiness, portability, &c.

From the necessity of dry conductors to alternate with moist to produce the more striking and perfect galvanic effects, it would appear that the former give the necessary celerity to their action; it might therefore be useful to enquire, whether making them of a considerable length, and inclosing them in some substance that would insure them from the moisture inseparable from the galvanic apparatus, might not considerably increase that celerity.

It is much to be wished that the end conductors were improved; jointed wires seem to lose much of the power, by not being in perfect contact; spiral wire seems better adapted to that use, and more capable of varying in its direction.

* See page 79.

It does not seem impossible to take a shock of its full force from piles or troughs of different powers, by bringing their conductors very near, but keeping them from actual contact by pieces of ivory or baked wood: it does not seem so clear that the full power of combustion could be preserved in the same manner.

Observations on galvanism.

I was lately rather surprized to find that galvanism had the effect of making silver remarkably brittle: this looks as if its action was somehow connected with the malleability of the perfect metals, and to shew that the conducting metals should be occasionally passed through the fire; the silver plates were nearly clean.

I will conclude with an observation on another subject. Having kept a considerable variety of Reeves's water-colour cakes in a damp closet; many of the cakes were in some degree affected by it so as to acquire mold; but the colour that Reeves sells under the name of Royal Smalt, has a most surprising power of attracting moisture, as it was reduced most completely to a soft mass; as almost all the vegetable, animal, and mineral substances used in painting were in the same box, we may pretty fairly conclude that an oxide of cobalt surpasses them all in its power of attracting moisture, and would make a most delicate hygrometer if suspended from one arm of a fine beam.

Attraction of moisture by oxide of cobalt.

I am, Sir,

With much respect,

Your obedient servant,

I. R. I.

VIII.

On the Presence of a new earthy Phosphate, found in the Bones of Animals, which does not exist in those of Men. By FOURCROY and VAUQUELIN. Read before the National Institute. †

WE flatter ourselves the Institute will recollect our laborious essays on the analysis of urine, and stony concretions found in the body of man and animals; with the striking difference existing between them, and the cause to which the latter is

Recapitulation of the analysis of urine, &c. No phosphate in the urine of animals,

* Gehlen's New Journal of Chemistry, I. 555.

owing.

owing. We endeavoured to prove in those essays that none of the salts called phosphates, (which exist in abundance in the urine of man) are to be found in the urine of mammiferous animals, that their kidneys are not the concretory of these saline combinations; but that the hair which covers their skin, and the corneous appendages which defend their extremities, are the organs and repositories in which nature secretes and deposits these salts in the body of animals.

Nor the morbid concretions called urinary calculi.

We have also shown that the morbid and preternatural concretions, called urinary calculi of man, contain besides uric acid, also phosphate of lime and phosphate of magnesia; none of which are met with in those of animals; and that on the contrary, the stony concretions found in the intestinal canal of animals, always contain phosphates of different kinds, whereas the concretions met with in the intestines of man, do not contain a vestige of them.

The bones also differ. Those of animals contain phosphate of magnesia; those of man do not.

We shall now endeavour to prove that the bones of animals also differ in composition from those of men. We have found that the former contain, besides phosphate and carbonate of lime, also phosphate of magnesia; the latter of which has hitherto escaped the notice of chemists. This salt which we have detected in the bones of all the animals we hitherto examined, does not at all exist in the bones of man. We shall first state the method we employed for detecting and separating it, and then point out the relative proportions in which it exists in different animals.

Method of separating magnesia from the bones of animals.

Take a quantity of bones of adult animals, burn them to whiteness in an open fire, and reduce them to a fine powder. Upon one part of this powder, after having been put into a convenient earthen or glass vessel, affuse an equal quantity by weight of concentrated sulphuric acid; stir the mixture intimately together, and then suffer it to stand for five or six days. Having done this, dilute the mass with at least ten times its bulk of water, agitate it well and transfer it on a strainer. When no more fluid passes, diffuse the mass again through five times its quantity of water, and strain again as before, and repeat this process till the water runs tasteless. The fluids thus obtained, are to be added together, and mingled with liquid ammonia, taking care the latter be in excess. The precipitate which is obtained consists of phosphate of lime, phosphate of ammonia, and phosphate of magnesia. To separate these

these salts, wash it in as little cold water as possible, and boil it in a solution of potash so long, till the odour of ammonia is no further perceptible. The potash in this process decomposes the phosphates of ammonia and magnesia, leaving the latter and phosphate of lime. To separate these two, let the whole be boiled in acetic acid, the magnesia will be dissolved, and the phosphate of lime remain untouched.

Method of separating magnesia from the bones of animals.

To obtain the magnesia, let the solution of acetite of magnesia be carefully evaporated to dryness, re-dissolve the salt in water, and decompose the solution by carbonate of soda; the precipitate obtained is carbonate of magnesia. If it be pure carbonate of magnesia, it will be completely soluble in sulphuric acid; if it contains lime, the solution will be cloudy, and a precipitate will gradually be deposited.

Such is the method we employed for detecting and separating the magnesia contained in the bones of animals; it is perhaps tedious, but it is easy and certain.

The bones of the ox examined in this manner yielded something less than $\frac{1}{16}$ of its weight of sulphate of magnesia, which is equal to about $\frac{1}{14}$ of phosphate of magnesia, or in the burnt bone to $\frac{1}{24}$.

The bones of the horse and sheep afforded $\frac{1}{38}$ of phosphate of magnesia.

Those of fowls and fish yielded nearly the same quantity as those of the ox.

The results of a general analysis of the bones of the ox were:

Dry gelatine	-	-	-	-	51	0	
Phosphate of lime	-	-	-	-	37	7	
Carb. of lime	-	-	-	-	10	0	
Phosphate of magnesia	-	-	-	-	1	3	
<hr/>							
						100	0

The presence of phosphate of magnesia in the bones of animals, and its total absence in those of man, calls upon the physiologist to point out the source whence this salt is derived in the former, and why it is not met with in the latter. That it forms a constituent part of the food of both, we have proved elsewhere; that phosphate of lime enters into the composition of wheat, barley, oats, peas, &c. Why then is it not to be found in the bones of men? The nature of the human urine may perhaps assist in explaining this problem. We have proved that

that the urine of men contains phosphate of magnesia, and that the urine of animals is free from it. This salt is therefore ejected in man, by the kidneys; it therefore does not enter into the composition of bones; moreover the urinary calculi of man frequently contain phosphate of ammonia and magnesia, but no such salts are ever found in the stony concretions of the intestinal canal.

IX.

On the Nature of Oxigen, Hidrogen, Caloric, &c. as deduced from Galvanic Experiments. By A CORRESPONDENT.

On the existence of oxigen.

SINCE we are told that oxygen is one of the most essential substances in the production of the most striking phenomena of nature; that heat and light result from its change of combination; nay, that animal life is dependant on it, it becomes a matter of very great utility to investigate its nature, much more to prove whether it has any existence. It is well known that this substance, which has now so long and so generally been admitted, has never yet been exhibited except in combination, and that the evidence of its existence in combination has never been more than presumptive. It appears from some late experiments to be possible to substitute known for unknown principles, and to relieve science from those agents which are merely hypothetical.

Oxigen and hidrogen taken to be water with the power from each respective end of the galvanic wire.

The wires from the galvanic combination of metals produce different effects when placed in the same vessel of water. One produces inflammable air, the other vital air. When the circuit is made by the human body, a shock similar to an electrical one is perceived. It therefore appears that those powers which thus affect the human body, change water into inflammable and vital air. One wire always produces one air, and the other wire another air from water. Is it not therefore philosophical to refer the production of one of these airs to the power proceeding from one end of the pile and water; and the production of the other air to the power proceeding from the other end of the pile and water? In this experiment we are made sensible of no other principle, power, or substance than the above-mentioned. Why should we therefore have recourse to two hypothetical substances, oxygen and hydrogen, which have

have never in any experiment been made sensible to us? Is it not philosophical to refer phenomena to causes which are objects of our senses, rather than to account for them by agents which are merely hypothetical?

Under certain circumstances water is converted into two ^{Development of the facts and inferences.} airs, which airs have peculiar properties; in the galvanic experiment we are made acquainted with no other agent but those powers which are elicited by the particular arrangement of metals, and these powers we are made sensible of; they produce different effects on various substances; and therefore I contend that these powers are different agents: for the same powers under the same circumstances should produce the same effects. The zinc side of the galvanic arrangement produces vital air, whilst the copper side produces inflammable air.— Does it not appear from this experiment that there are other causes besides caloric that give aeriform elasticity to bodies? And do not the two powers of the pile here seem to be real principles? Each of them produces a real and decided effect on water. At all events we have not in this experiment any reason for asserting that water is a compound body, formed of two distinct and solid substances, oxygen and hydrogen. I do not mean at present to inquire whether negative electricity be a mere negation or not. We know that it is as much a cause of repulsion as what is called positive electricity, and that in experiments of a different kind from the one we are now considering. When a substance has in one instance been clearly proved to be formed of certain principles, it is consistent with philosophical accuracy to refer in all other instances to the same principles as the causes of the production of such substance. Inflammable air therefore is water rendered aeriform by negative electricity or galvanism; and vital air is water rendered aeriform by positive electricity or galvanism. This is nearly the enunciation of the fact, and I contend that in this, and in all the reasonings respecting water and fire, we have no occasion for the two hypothetical principles, oxygen and hydrogen.

In the above experiment with the pile of Volta, it appears ^{The effect not caused by caloric.} that it is not caloric which causes the elastic aeriform state of either the vital or the inflammable air, at least we are not made sensible of it. If it were caloric that proceeded from the two wires of the pile, why should each wire uniformly produce the same air, and one different from the other? There should be

be some reason why the solid base hydrogen attaches itself to the caloric of the one wire, and why the solid base oxygen is always so ready to enter into combination with the caloric of the other wire.

Compound nature of heat.

Scheele has asserted the compound nature of the matter of heat, and that all inflammable bodies contain a principle of inflammability, which principle, by combining with his igneous air, produces the phenomena of combustion. It does not appear that this opinion has been controverted by any decisive fact. We now know that some principle besides caloric is necessary in the production of one inflammable body, namely, of inflammable air; which principle, by combining with vital or igneous air, produces combustion. The re-produced water is common to both.

Generation of f.c.

I think upon a further prosecution of this inquiry, it will appear, that fire is generated during combustion, that it is fire alone, (that is, the principle which causes the sensation of heat) that causes all the phenomena of combustion, and that it is, as Scheele observes, the water of his igneous air which forms the additional weight of bodies after they are burned. I know of no chemical fact that contradicts this explanation, in which no new substance that is not sensibly discovered is introduced.

A CORRESPONDENT.

X.

Experiments on the Yolk of Wool, followed by some Considerations on the Cleansing and Bleaching of Wool. By CIT. VAUQUELIN.*

Yolk of wool.

SEVERAL philosophers have thought that the yolk of wool was a fatty matter; others from its dissolving in water could not adopt the same opinion. Chemical analysis alone could decide this question, and this is what I proposed to myself in the work, the result of which I now offer.

Action of water upon wool.

1st. Water deprives wool of much of its colour, and the liquid acquires colour, odour and taste.

2d. The washings of the wool is milky like an emulsion of gum-resin, and passes through paper with difficulty.

* From the Ann. de Chimie, Fructidor, An. XI. No. 141.

3d. In time it gives a deposition of sand, carbonate of lime, and several other foreign bodies; it lathers by agitation and heat like a solution of soap.

4th. The water with which wool has been washed, filtered and evaporated, yields a brown extract, thick like a syrup, of an acrid, salt, and bitter taste; in this state it still retains its peculiar odour.

Aqueous solution and extract.

5th. Alcohol, applied to this extract, dissolves a part which communicates a reddish-brown colour to it: if the alcohol be separated from this substance by evaporation, it assumes the form of a transparent, thick, and viscous honey.

Extract exposed to alcohol.

The following are some of the properties which it offered: To acids.

1st. It dissolves easily in water, and its solution is speedily coagulated by the acids, which separate a fat substance from it, insoluble in water. The matter thus separated by the acids, collects very slowly; its colour is yellowish. The acids, as will be seen lower, retain a great quantity of it in solution, which gives them a reddish brown colour. By evaporation, the greatest part of this substance, dissolved by the acids, is deposited in the form of a black bitumen, and salts are obtained with base of potash and of lime. The greasy matter is so adherent to these salts that they cannot be obtained in a state of purity and whiteness, until after several calcinations and solutions.

It yields salts with base of lime and potash:

At the same time that the acids precipitate this fat matter, they drive off a certain quantity of acetous acid, very distinguishable by its odour. Concentrated sulphuric acid blackens the inspissated yolk, and disengages some vapours of muriatic acid.

Acetous acid, and muriatic acid.

2d. Lime-water renders the solution of the yolk turbid and milky, but it does not form a coagulum in it as in a solution of common soap.

Lime-water.

3d. Caustic alkalis or quick-lime do not demonstrate the presence of ammonia.

Caustic alkalis and quick-lime.

4th. Nitrate of silver produces a yellow precipitate in it, which attaches itself to the sides of the vessel, like a fat substance. Great part of this precipitate is dissolved in nitric acid.

Nitrate of silver.

The part of the yolk which is insoluble in alcohol has still a salt taste, but less distinct than the part which is soluble in this re-agent. After having been thus treated with alcohol, it does not entirely re-dissolve in water; there remains a glutinous matter,

The insoluble part is still salt.

Is not entirely soluble in water after having been treated with alcohol.

Contains an alkaline carbonate.

Action of reagents.

matter, of a grey colour, with which the acids produce a pretty brisk effervescence, which shows the presence of an alkaline carbonate. The portion which retains its solubility in water communicates a reddish colour and a saline taste to this fluid; its solution is not disturbed by the acids, as it was before having been treated by alcohol. Caustic alkalis do not disengage any ammonia; the muriate of barites forms a very abundant deposition in it, the greatest part of which is soluble in water: the nitrate of silver also occasions a precipitate in it, which dissolves partly in nitric acid. Alcohol precipitates this matter in the form of a mucus, which is deposited quickly.

Nitrate of iron being mixed with the solution of this substance, formed a brown precipitate in it, and at the end of some days, the liquor furnished a pretty large quantity of nitrate of potash.

The yolk being decomposed by dilute sulphuric acid, and the liquor filtered, it blackened by evaporation, exhaled vapours of sulphuric acid, and became carbonaceous, as the concentration of the sulphuric acid took place. The residue being afterwards washed with water, and the solution suitably evaporated, yielded crystals of neutral sulphate of potash, but a good deal remained in the solution on account of the superabundant acid which brought it to the state of an acidulous salt: by a longer evaporation, this salt crystallizes in needles and plates of a pearly white.

Yields sulphate of lime by sulphuric acid;

During the course of these successive evaporations, another species of salt was offered, in the form of flattened needles, of a fatty white, and without any sensible taste.

This salt examined with care, appeared to me to be only sulphate of lime; it however differed from it in some respects: for example, it melts much more readily by the flame of the blow-pipe into a globule, transparent while it is in fusion, and which becomes opaque by contracting: it is also much more soluble in water, and nevertheless does not contain the acid in excess; as I have satisfied myself. Its solution in water precipitates muriate of barites and oxalate of ammonia abundantly: one of these precipitates is sulphate of barites, and the other oxalate of lime. Neither lime-water nor ammonia disturb its solution. It appears therefore that this salt is a modification of sulphate of lime, which is probably produced by the proportion of its elements. It may also be possible that this salt still contains

tains some portions of fat matter, which, by decomposing the sulphate of lime, and forming a little sulphuret would facilitate the fusion. I regret my not having had a sufficient quantity of this salt, to examine its properties more minutely.

The yolk dissolved in water, filtered, and inspissated, having Acetic acid; been distilled with dilute sulphuric acid, furnished a liquor in which I easily recognized acetic acid, by its odour, its taste, and the properties of the salts which it formed with different bases, particularly with lime and potash.

Thus the yolk contains acetic acid, which without doubt is combined with part of the potash.

It also contains muriate of potash, for, with the solution of silver it forms an abundant precipitate, which is not entirely soluble in nitric acid; and, by distillation with sulphuric acid, it gives sensible indications of muriatic acid, which is mixed with the acetic acid.

The yolk evaporated to dryness, and strongly heated in a silver crucible, swells, chars, and exhales fetid ammoniacal vapours; afterwards oily fumes arise which take fire, and when the greatest part of the oil is dissipated, it reddens, and enters into quiet fusion. If, at this moment, it be poured on a marble, it yields a substance which contracts by cooling, of a greyish colour, and a very caustic alkaline taste: if this substance be afterwards dissolved in water, there only remains an infinitely small quantity of carbonaceous matter, and, by evaporation, the liquor yields a true potash slightly carbonated.

It results from these experiments that the oil or grease, whose presence in the yolk has been demonstrated by means of the acids, is combined in it with potash, in the state of a true animal soap; that, besides, there is a portion of carbonate of potash in excess, since the acids produce a pretty brisk frothy effervescence in the concentrated solution of the yolk. In addition to the substances which I have just mentioned, the yolk contains a certain quantity of animal matter; for, by distillation, it gives very sensible traces of ammoniac, and an oil whose fetid odour resembles those furnished by animal matters.

The yolk is therefore formed, 1st, Of a soap with a base of potash, which makes the greatest part; 2^d, Of a small quantity of carbonate of potash; 3^d, Of a perceptible quantity of acetate of potash; 4th, Of lime, whose state of combination

I am

Recapitulation
of the compo-
nent parts :

I am unacquainted with; 5th, Of an atom of muriate of potash; 6th, finally, Of an animal matter to which I attribute the peculiar odour of the yolk.

which are not accidental.

I am of opinion that all these matters are essential to the nature of the yolk, and are not found in it by accident; for I have constantly found them in a great number of samples, as well of Spanish wool as of French.

I do not here speak of the other matters, insoluble in water, which are also met with in wool, such as the carbonate of lime, sand, and filth of every sort, these being evidently accidental.

Are they the products of cutaneous transpiration?

It remained now to enquire if all the matters in the yolk were the product of cutaneous transpiration, accumulated and thickened in the wool, or if they were taken up in the folds and other places in which the sheep lie. It is very certain that all the elements fit for the formation of the matters contained in the yolk, are found in the excrements of these animals, and in the vegetables which serve them for litter. Nevertheless, I could not believe that all of it was the effect of dung; on the contrary, I am of opinion that the humour of the transpiration is the principal source of it.

The analysis of the dung offers nothing certain in this respect, because the matters found in it may have been deposited there by the sheep themselves.

In what state are they emitted by the skin?

But admitting that the principles of the yolk arise from the cutaneous transpiration, which is very probable, are these matters emitted by the body of the animal in this state, and do they not experience some change while they remain in the wool? This is a question on which it is difficult to decide positively; we can only presume that changes are produced in it, as in all very complex substances deprived of motion, of which, in the present case, we neither know the cause nor the manner.

Washing the wool in running water is not enough to cleanse it.

The yolk, as we have seen above, being a true soap, soluble in water and alcohol, it would seem that nothing better can be done for scowering the wool than to wash them in running water. But I should observe that there is a small quantity of fat matter in the wool, which is not in combination with the alkali, and which, remaining attached to the wool, keeps it a little glutinous (*poisseux*), notwithstanding the most careful washing.

But

But if the wool be put into buckets, and only as much water as will moisten it poured in, and if it be suffered to remain some time in this bath, pressing it often, it scours much better, and becomes much whiter afterwards, by washing in running water.

The scourers have a custom of macerating the wool in putrefied urine, and it is generally believed that it is the ammonia which is developed that effects the scouring; but I have some reason to think that this alkali is of no value. This effect is rather owing to the yolk itself, or to some other principle of the urine, to the urée, for example. The following are the grounds of my opinion in this respect; I put wool washed in running water into a mixture of sal ammonia and common potash; the mixture had a strong smell of ammonia, and nevertheless the wool was in no respect cleansed, because this alkali does not form, or at least with great difficulty, a saponaceous combination with the greasy matter of wool. From these observations, therefore, I believe putrid urine to be nearly useless in the scouring of wool, at least as far as respects its ammonia.

Putrid urine does not promote the scouring.

Though the utility of putrid urine be in some degree doubtful, it is, on the contrary, very certain that fresh urine would be greatly injurious to the proposed object, for the soap contained in the yolk would incontestibly experience a decomposition by the acid of the urine, which would precipitate the grease on the wool.

Fresh urine would precipitate the grease.

I suspect that the same effect would take place from washing the wool in water containing earthy salts, which are known to decompose alkaline soaps. For which reason it is always prudent to employ the purest water which can be procured for this purpose.

As would water containing earthy salts.

This is not the case with soap-suds, which accomplish the scouring of wool perfectly, at the same time giving it more whiteness. It, therefore, after having washed the wool in running water till it loses no more, it be suffered to macerate for a few hours in only one twentieth of its weight of soap dissolved in a sufficient quantity of warm water, squeezing it often, it will be entirely purged of the small portion of grease which still adhered to it, and will then have a softness and degree of clearness which it could not have had without this operation.

Soap-suds the best menstruum.

The

The yolk exercises an action on the uncombined greafe.

Danger of continuing its action too long; or of using strong soap-suds.

The acrimony of the yolk probably hurtful to the living animal.

Washing recommended.

Loss of weight by scouring.

Bleaching of scoured wool.

The yolk itself, when a little concentrated, as I have already mentioned, has an efficacious action on the portion of greafe which is not in a saponaceous state; for I have found that, in putting to the wool only the quantity of water necessary to cover it, it scours better, particularly with a little heat, than when it is washed in running water. But I also found that, when wool has remained too long in its own yolk, it swells, splits, and loses its strength: this effect also takes place with soap-suds which are too strong.

Since the solution of the yolk occasions this swelling and splitting of the wool, is it not possible that this accident may happen on the sheep's back, particularly in hot, moist seasons, or when they are shut up in folds in which the litter is not often enough renewed? Nor would it be impossible that the acrimony of the yolk should occasion an irritation in their skins, and, by that means, be the cause of some of the disorders to which this organ is liable in these animals, which must principally happen in hot and damp weather: fortunately, in these seasons, they are from time to time exposed to rains which wash them, and carry off at least a portion of this matter. On this subject I cordially agree with those who think that washing sheep in hot and dry weather, would be useful to their health and the quality of their wool.

The loss experienced by scouring wool is very variable; the greatest I met with was 45 per cent. and the least 35; it is true, those which I washed were very dry. This loss is not wholly owing to the yolk; the humidity, the earth, and the filth of every species, also contribute to it.

I have made some attempts to bleach scoured wool, but I confess that they have not been carried so far as they ought to have been. I have remarked, generally, that those which had been washed with soap-suds whitened better, by every method, than those which had not. Sulphureous acid dissolved in water whitened it pretty well, but it did not destroy the yellow colour which the wool, growing in the groin and under the fore-legs of the sheep, had contracted. In liquid sulphureous acid the wool acquires the property of crackling between the fingers like brimstoned silk, and, at the same time, contracts a very powerful fetid smell, which is not dissipated in a long time.

I did

I did not try the vapour of burning sulphur, but all the world knows that it whitens wool well, and that the woollen manufacturers use it to give the finishing degree of whiteness to their goods. Of all the methods which I tried, I found none better for bleaching the wool, than exposing it, on the grass, to the dew and the sun, after being well scoured with weak soap-suds: the yellow spots of that from the groin, however, were not entirely destroyed; they had only diminished in intensity.

XI.

Copy of a Letter from Mr. CUTHBERTSON to Dr. PEARSON, communicating an important and curious distinguishing Property between the Galvanic and Electric Fluids. Communicated by Dr. PEARSON.

To Dr. PEARSON:

DEAR SIR,

I THINK it right to inform you, that yesterday evening I resumed the experiments with the galvanic batteries; the result was—

1. Charcoal was deflagrated and ignited for about one inch in length.
2. Iron wire $\frac{1}{40}$ inch diameter, was melted into a ball $\frac{1}{10}$ inch diameter.
3. Platina wire $\frac{1}{60}$ inch diameter, was melted into a ball $\frac{1}{10}$ inch diameter.
4. Brass wire $\frac{1}{20}$ inch diameter, $\frac{3}{4}$ inch in length was ignited.
5. Ditto $\frac{1}{10}$ inch diameter, was red-hot at the extremity.
6. Iron wire $\frac{1}{30}$ inch diameter, was red-hot for 16 inches in length.
7. Ditto 12 inches, deflagrated and melted into a ball.
8. Ditto six inches in length, were deflagrated.
9. Ditto 8 inches in length, were ignited.

Two troughs, each trough containing 30 pair of plates six inches square, were used for the first seven experiments, and one of these troughs only for the two last experiments.

Galvanic deflagrations.

The four last experiments prove, I think, that *double quantities of galvanic fluid only burn double lengths of wire, and not the square, as electrical discharges do* *.

I am, with the greatest respect,

Sir,

Your very humble servant,

JOHN CUTHBERTSON.

Poland-Street, Soho,

March 27, 1804.

XII.

Letter from a Correspondent, containing an Observation of the spontaneous Inflammation of Paper in Nitric Acid Gas.

To Mr. NICHOLSON.

DEAR SIR,

London, May 22, 1804.

Paper inflamed
in nitrous gas.

THAT several different inflammable bodies, while in a state of inflammation or of ignition, burn with an enlarged flame, and continue ignited when immersed in nitric acid gas, is, I suppose, commonly known; but that paper itself would take fire and flame most beautifully in this gas, and at not a very elevated temperature, has not, that I recollect, been already observed. By the following accidental circumstance this phenomenon was seen this morning in the public lecture-room, while reading on the subject of nitrous acid. In putting together the different parts of the Wolfe's apparatus, having ready only a bent tube much smaller than the lateral aperture of the globe condensing receiver, I filled up that aperture, partly with a piece of writing paper which projected into the receiver, and partly with almond paste. Soon after the acid had begun to distil, and while the apparatus was filled with

* It is not said whether the two troughs were used collaterally or longitudinally. Electrical jars may be considered as being always combined in the former mode. This subject seems to require comparison with the facts given in Mr. Wilkinson's letter in our Journal, Vol. VII, p. 207; but the communication came too late for me to offer any remarks upon it.

W. N.

reddish coloured nitrous acid gas, I was surprized by the bursting forth of flame from the paper, which was consumed by it in less than a minute, without cracking, as I expected, the receiver. I think it unnecessary to make any comments, or give the *rationale* of this fact.

Always your's faithfully,

AMICUS.

XIII.

Description of a Jib on a new Construction; by Mr. J. BRAMAH, Engineer. Communicated by the Inventor.

JIBS of the usual construction turn on two solid gudgeons. The rope by which the goods are raised, passes over the upper gudgeon, and is confined between two small vertical rollers, in order that it may constantly lead fair with the pulley or sheave at the extremity of the jib. According to this construction, whenever the crane turns round its axis, the rope is bended so as to form an angle more or less acute, which causes a great increase of friction, and produces a continual effort to bring the arm of the jib into a parallel position to the inner part of the rope. These inconveniences may appear to be trifling on paper, but in actual practice they are of no small importance, for they necessarily imply a much greater exertion of power in raising goods, and the application of a constant force to keep the jib in the position that may be requisite; while the partial stress which is exerted on only a few strands of the rope, when bended into an acute angle, infallibly destroys it in a very short time.

Description of a jib on a new construction.

The simple construction exhibited in Plate V. obviates all these defects, and at the same time possesses the very desirable property of permitting the jib of what is termed a camphut or landing crane, wholly to revolve round its axis, and to land goods at any point of the circle described by the arm of the jib.

It consists in perforating the axis or pillar of the crane, and in conducting the rope through this perforation by means of an additional pulley fixed on the top of the arm of the jib.

Description of a
jib on a new
construction.

The nature of the contrivance cannot fail to be sufficiently understood by an inspection of the figures; the one of which represents a jib attached to the wall of a warehouse, the other a camshut or landing crane fixed on the edge of the wharf. Each of these jibs turns on a perforated axis or pillar. The rope proceeds from the goods which are hoisted, through a pully fixed as usual at the extremity of the jib; it then passes over another pully fixed at the opposite extremity of the jib, and is, by this pully conducted through the perforated axis or pillar to a third pulley; whence it is immediately directed to the crane by which the weight is elevated.

It is almost unnecessary to state that the lower axis is usually fixed in an oil box, and that friction rollers are applied to the axis wherever the circumstances may render it necessary.

The importance of this improvement, in an article of such extensive use, must be evident even to those who are the least acquainted with the subject. Mechanics who are aware that simplicity of construction and certainty of effect are among the most valuable characters to be sought in engines, will most probably observe this crane with pleasure; and the advantages to the community at large must be measured by the convenience and saving of labour it is calculated to afford.

XIV.

A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake, and other American Serpents. By BENJAMIN SMITH BARTON, M. D. From the American Transactions, Vol. IV.

(Concluded from Page 62.)

Other snakes
(particularly the
black-snake
which is not
poisonous) are
said to charm.

SECONDLY. It is a fact well known in this country, that the rattle-snake is not the only kind of serpent that is said to be endued with the faculty of fascinating birds, squirrels, and other animals. As far as my inquiries have extended, it does not appear to me that, in general, the rattle-snake is thought to have so large a portion of this faculty as some other species of serpents. Of this, at least, I am certain, that persons residing in our country-situations tell us many wonderful tales of the bewitching eyes of the black-snake, the coluber constrictor of

of Linnæus, as they do of the boiquira, or rattle-snake. Now let it be supposed, for a minute, that the poison of this latter serpent, when thrown into the body of a bird, a squirrel, &c. is capable of producing, in these animals, those piteous cries, those singular movements, those tremulous fears, which are mentioned by Kalm, by de la Cépède, and by other writers,—in what manner are we to account for the similar cries, movements, and fears, in those birds which are frequently seen under the fascinating influence of the black-snake? For we Americans all know, that the bite of the black-snake is perfectly innocuous. This, indeed, is also the case with the greater number of the species of serpents that have, hitherto, been discovered in the extensive country of the United States. And yet almost every species of serpents is supposed to be endowed with the power of fascinating such animals as it occasionally devours.

These facts, and this mode of reasoning, certainly involve, and consequently the supposition of poisonous influence is unfounded. in some difficulty, Mr. de la Cépède, and those writers who espouse his opinion, which I have examined, under the first head of my objections. An attempt is made to account for the imaginary fascinating faculty of the serpent from the powerful influence of a subtle poison. But, upon inquiry, it is found, that the power of bewitching different animals is not an exclusive gift of those serpents which nature has provided with envenomed fangs: it is a gift which as extensively belongs to that more numerous tribe of our serpents, whose bite is innocent, and whose creeping motion is their only poison*.

These

* If there is any impropriety in this mode of expression, the impropriety has its source in my feelings, with respect to the serpents. Perhaps, no man experiences the force and the miseries of this prejudice in a greater degree than I do. It is the only prejudice which, I think, I have not strength to subdue. As the natural history of the Serpents is a very curious and interesting part of the science of zoology; as the United-States afford an ample opportunity for the farther improvement of the history of these animals, and as I have, for a long time, been anxious to devote a portion of my leisure time to an investigation of their physiology, in particular, I cannot but exceedingly regret my weakness and timidity, in this respect. I had meditated a series of experiments upon the respiration, the digestion, and the generation of the serpents of Pennsylvania. But, I want the fortitude which it is necessary to possess in entering on
the

These objections will, I am persuaded, be sufficient to convince every unprejudiced reader, that the system of explanation offered by Mr. de la Cépède is unfounded in facts; and, consequently, that the problem still remains to be solved, in another way.

Professor Blumenbach admits the fact; but ascribes it to a moral cause.

Among the number of ingenious men who have amused themselves with speculations on the subject of this memoir, and who, rejecting the commonly received notion of the existence of a fascinating power in the rattle-snake, have attempted to explain the phenomenon upon other principles, it is with pleasure I recognize the respectable Professor Blumenbach, of Gottingen. This gentleman, in a late publication, speaking of the rattle-snake, makes a few remarks on the fascinating faculty which has been ascribed to this reptile. These remarks I shall translate at length.

Toads, hawks, &c. are said to have the power of fascinating by the eye.

“That squirrels, small birds, &c.” says he, “voluntarily fall from trees into the jaws of the rattle-snake, lying under them, is certainly founded in facts: nor is this much to be wondered at, as similar phenomena have been observed in other species of serpents, and even in toads, hawks, and in cats, all of which, to appearance, can under particular circumstances, entice other small animals, by mere stedfast looks. Here the rattles of this snake (the rattle-snake) are of peculiar service; for their hissing noise causes the squirrels, whether impelled by a kind of curiosity, misunderstanding, or dreadful fear, to follow it, as it would seem, of their own accord. At least,” continues Mr. Blumenbach, “I know from well-informed eye witnesses, that it is one of the common practices among the younger savages to hide themselves in the woods, and by counterfeiting the hissing of the rattle-snake to allure and catch the squirrels.”*

the task. Instead of slowly and cautiously dissecting and examining their structure and their functions, with that attention which the subject merits, I am more disposed, at present, to obey the injunction of the Mantuan poet, in the following beautiful lines:

—Cape saxa manu: cape robora, pastor,
Tollentemque minas et sibila colla tumentem
Dijice: jamque fuga tumidum caput abdidit alte,
Cum medii nexus, extremæque agmina caudæ
Solvuntur, tardosque trahit sinus ultimus orbes.

GEORG. Lib. iii. 420—424.

* Handbuch der Naturgeschichte, P. 253. Gættingen: 1791.

I do not intend to take up much time in examining the foregoing explanation. I shall offer my objections to it, in as concise a manner as I can.

First. The faculty of fascinating is by no means peculiar to the rattle-snake, but is attributed as extensively to the black-snake, and other serpents, which are not furnished with the crepitaculum, or set of bells *, by which this serpent is supposed to be enabled to ring for its prey, when it wants it.

Examination of the facts stated by Blumenbach, which are contested.

Secondly. Some persons, who have seen the rattle-snake in the supposed act of charming, assure me that the reptile did not shake its rattles, but kept them still. It is true, that Mr. Vosmæer's rattle-snake, already mentioned, continually shook its rattles.

Thirdly. With regard to the practice of the young savages, spoken of by Mr. Blumenbach, I know nothing. I have inquired of Indians, and of persons who have resided for a considerable time, among the Indians, and they appear to be as ignorant of the circumstance as I am myself. I am inclined to think that Mr. Blumenbach has been imposed upon: or, perhaps, the following circumstance may have given rise to the story. The young Indians put arrows, across, in their mouths, and by the quivering motion of their lips upon the arrows, imitate the noise of young birds, thus bringing the old ones so near to them, that they can be readily shot at. In like manner, the *Lanius Excubitor*, or great shrike, hiding itself in a thicket, and imitating the cry of a young bird, often succeeds in seizing the old ones, which have been solicited, by the counterfeited noise, to the assistance of their young.

Facts and observations respecting the power of fascination ascribed to snakes.

Ever since I have been accustomed to contemplate the objects of nature with a degree of minute attention, I have considered the whole story of the enchanting faculty of the rattle-snake, and of other serpents, as destitute of a solid foundation. I have attentively listened to many stories, which have been related to me as proofs of the doctrine, by men whose veracity I could not suspect. But there is a stubborn incredulity often attached to certain minds. In me it was strong. The mere force of argument never compelled me to believe. I always suspected, that there was some deficiency in the extent of observation, and the result of not a little attention to the subject has taught me, that there is but one

* *Serpent à sonnette* is the French name for the rattle-snake.

Facts and observations respecting the power of fascination ascribed to snakes.

wonder in the business;—the wonder that the story should ever have been believed by a man of understanding, and of observation.

In conducting my inquiries into this curious subject, I thought it would be proper, and even necessary, previously to my forming a decided opinion, to ascertain the two following points, viz. first: what species of birds are most frequently observed to be enchanted by the serpents? and, secondly, at what season of the year has any particular species been most commonly seen under this wonderful influence? I was induced to believe that the solution of these two questions would serve as a clue to the investigation of what has been long considered as one of the most mysterious operations in nature. I am persuaded that I have not been mistaken. Possibly, the credulous may not think as I do.

It is a curious circumstance in the history of birds, that almost every species, in the same country at least, has an almost uniform and determinate method of building its nest, whether we consider the form of the nest, the materials of which it is constructed, or the place in which it is fixed*. Some observations on this subject are necessarily connected with the point under investigation, in this memoir;—indeed, they are involved in the question concerning the species of birds which have most generally been observed to be enchanted by the rattle-snake, &c.

Some birds build their nests on the summits of the loftiest trees; others suspend them, in a pendulous manner, at the extremity of a branch, or even on a leaf†, whilst others build them

* I do not mean, by this observation, to assert, that birds are necessarily impelled to construct their nests of the same materials, or to place them in the same situations; yet such is the language of some writers on natural history, and on morals, who talk of the “determinate instinct” of animals, and who think it impossible that “animals of the same species should any where differ.” “The grouse in America, we are told, perch upon trees; the hare burrows in the ground; and we have, in these instances, sufficient reason to deny that the species of either is the same with those of a like denomination, with which we are acquainted, in Europe.” These are the words of the celebrated author. See Dr. A. Ferguson’s Principles of Moral and Political Science, vol. i. p. 59 & 60. quarto edition.

† See a very interesting account of the *Motacilla futoria*, or Taylor-

them on the lower branches, among bushes, and in the hollows of decayed, and other trees. Many species, again, are content with the ground, laying their eggs, and hatching them, in the cavity of a stone, an excavation from the earth, among the grass of fields and meadows, or in fields of wheat, rye, and other grains. Thus, to confine myself to our own country, the eagle, the vulture, the hawk, and other birds of this extensive family, make choice of the loftiest oaks, and other trees of our forests; the baltimore-oriole *, commonly called, in Pennsylvania, the hanging bird, suspends a beautiful nest to the extremity of a branch of the Liriodendron †, or some other tree; the migrating thrush ‡, called robin, is content with the lower branches; the red thrush §, the cat-bird ||, the red-winged oriole, called the swamp-black-bird ¶, and many others build in the low bushes; the wood-peckers **, the blue motacilla (blue-bird) ††, the torchepot ‡‡, and others, build in the hollows of trees, the chattering plover §§, and the whippoor-will ||||, take advantage of a hollow place in the ground, or in a stone, which the great lark ¶¶, the marsh-wren ***, &c. place their nests in the grass; and, lastly, the partridge ††† builds in the corn-fields.

Facts and observations respecting the power of fascination ascribed to snakes.

Of all these birds, and of a great many others, those which build their nests upon the ground, on the lower branches of trees, and on low bushes (especially on the sides of rivers, creeks, and other waters, that are frequented by different kinds of serpents), have most frequently been observed to be under the enchanting faculty of the rattle-snake, &c. Indeed, the bewitching spirit of these serpents seems to be almost entirely limited to these kinds of birds. Hence, we so frequently hear tales of the fascination of our cat-bird, which builds its nest in the low bushes, on the sides of creeks, and other waters, the most usual haunts of the black-snake, and

Taylor-bird, by my learned friend Mr. Pennant, in his Indian Zoology, pages 44, 45 & 46.

- | | |
|----------------------------|----------------------------|
| * Oriolus Baltimore. | † Liriodendron tulipifera. |
| ‡ Turdus migratorius. | § Turdus rufus. |
| Muscivora carolinensis. | ¶ Oriolus phœniceus. |
| ** Pici | †† Motacilla Sialis. |
| ‡‡ Sitta. | §§ Charadrius vociferus. |
| Caprimulgus. | ¶¶ Alauda magna. |
| *** Motacilla Troglodytes? | ††† Tetrao virginianus. |

other

Facts and observations respecting the power of fascination ascribed to snakes.

other serpents. Hence, too, upon opening the stomachs of some of our serpents, if we often find that they contain birds, it is almost entirely those birds which build in the manner I have just mentioned.

This fact I had long remarked. It had made some impression upon my mind before I had turned my attention to the subject of this memoir. Lately, when I came to take a view of the subject, the fact appeared to me to be of some consequence. I shall now avail myself of it.

The rattle-snake seldom, if ever, climbs up trees*. He is

* Some respectable writers assert, that the rattle-snake does climb trees, and that it does it with ease. Mr. de la Cépède is of this opinion. After telling us that this reptile lives upon worms, frogs, and hares, this naturalist proceeds: "il fait aussi sa proie d'oiseaux & d'écureuils; car il monte avec facilité sur les arbres, & s'y élance avec vivacité de branche en branche, ainsi que sur les pointes des rochers qu'il habite, & ce n'est que dans la plaine qu'il court avec difficulté, & qu'il est plus aisè d'éviter sa poursuite." *Histoire Naturelle des Serpens.* p. 409. At the conclusion of his account of the boiquira, or crotalus horridus, the eloquent author has run into the same error, in the following beautiful, though rather poetical, apostrophe. "Tranquilles habitans de nos contrées tempérées, que nous sommes plus heureux, loin de ces plages où la chaleur & l'humidité règnent avec tant de force! Nous ne voyons point un Serpent funeste infecter l'eau au milieu de laquelle il nage avec facilité; les arbres dont il parcourt les rameaux avec vitesse; la terre dont il peuple les cavernes; les bois solitaires, où il exerce le même empire que le tigre dans ses déserts brûlans, dont l'obscurité livre plus sûrement sa proie à sa morsure. Ne regrettons pas les beautés naturelles de ces climats plus chauds que le nôtre, leurs arbres plus touffus, leurs feuillages plus agréables, leurs fleurs plus suaves, plus belles: ces fleurs, ces feuillages, ces arbres cachent la demeure du Serpent à sonnette." *Histoire Naturelle des Serpens.* p. 419 & 420. I have been at some pains to discover whether the rattle-snake does climb up trees. The result of my inquiries is that it does not. Although I have had opportunities of seeing great numbers of rattle-snakes in the western parts of Pennsylvania, &c. particularly in the vicinity of the river Ohio, I never saw one of them except on the ground. The black-snake I have often seen upon trees. I ought not, however, to conceal that in the summer of the last year, a Choktah-Indian told me, that the rattle-snake does climb trees and bushes, to a small height. He said, that he had once seen one of these snakes upon a reed. I am not very willing

is frequently, however, found about their roots, especially in wet situations. It is said that this reptile is often seen, curled round a tree, darting terrible glances at a squirrel, which after some time is so much influenced by these glances, or by some subtle emanation from the body of the serpent, that the poor animal falls into the jaws of its enemy. This story is, I believe, destitute of foundation, though it is related by the good Cotton Mather*. The rattle-snake is, indeed, sometimes seen at the root of a tree, upon the lower branches of which, at the height of a few feet from the ground, a bird or squirrel has been seen exhibiting symptoms of fear and distress. Is this a matter of any wonder? Nature has taught different animals what animals are their enemies; and although, as will be afterwards shewn, the principal food of the rattle-snake is the great frog, yet as he occasionally devours birds and squirrels, to these animals he must necessarily be an object of fear. When the reptile, therefore, lies at the foot of a tree, the bird or the squirrel will feel itself uneasy. That it will sometimes run towards the serpent, then retire, and return again, I will not deny. But that it is irresistibly drawn into the jaws of the serpent, I do deny: because it is very frequently seen to drive the serpent from its hold; because the bird or squirrel often returns, in a few minutes, to their

Facts and observations respecting the power of fascination ascribed to snakes.

willing to deny this Indian's story: yet it is opposed to every information I have been able to procure from persons well acquainted with the reptile of which I am speaking. However, it is not impossible that where trees and bushes grow very close together, the snake may climb them to a very small height. Most species of serpents move in a spiral manner: the rattle-snake moves straight on; and this is the reason why he cannot climb trees. In the quotation which I have made from Mr. de la Cépède, another mistake is involved. He speaks of the agility with which the rattle-snake moves. This is not, however, merely the mistake of Mr. de la Cépède. We find it in Piso. Speaking of this reptile, our author says: "In triviis juxta ac deviis locis cernitur, tam celeriter proreptans ut volare videatur, idque velocius per loca saxosa, quam terrestria." *De Indiæ utriusque re naturali et medica.* p. 274. Now the truth is that the rattle-snake is one of the most sluggish of all our serpents. Linnæus was well informed, when he asserted that Providence had given "the Crotalus a very slow motion." See *Reflections, &c.* quoted p. 84 of this memoir.

* Philosophical Transactions of the Royal Society, No. 339.

habitations

Facts and observations respecting the power of fascination ascribed to snakes.

habitations. Sometimes the bird or squirrel, in attempting to drive away the snake, approach too near to their enemy, and are bitten, or immediately devoured. But, from what will afterwards be said, it will appear that these instances are not so common as is generally imagined.

My inquiries concerning the season of the year, at which any particular species of birds has been seen under the fascinating power of a serpent, afforded me still more satisfaction. In almost every instance, I found that the supposed fascinating faculty of the serpent was exerted upon the birds at the particular season of their laying their eggs, of their hatching, or of their rearing their young, still tender, and defenceless. I now began to suspect, that the cries and fears of birds supposed to be fascinated originated in an endeavour to protect their nest or young. My inquiries have convinced me that this is the case.

I have already observed, that the rattle-snake does not climb up trees. But the black-snake and some other species of the genus coluber do. When impelled by hunger, and incapable of satisfying it by the capture of animals on the ground, they begin to glide up trees or bushes, upon which a bird has its nest. The bird is not ignorant of the serpent's object. She leaves her nest, whether it contains eggs or young ones, and endeavours to oppose the reptile's progress. In doing this, she is actuated by the strength of her instinctive attachment to her eggs, or of affection to her young. Her cry is melancholy, her motions are tremulous, She exposes herself to the most imminent danger. Sometimes, she approaches so near the reptile that he seizes her as his prey. But this is far from being universally the case. Often, she compels the serpent to leave the tree, and then returns to her nest*.

* Horace, though he has not, like his contemporary, Virgil, given any great proofs of his knowledge in natural history, appears to have known, full well, the anxiety of birds for the preservation of their young:

“ Ut affidens implumibus pullis avis
“ Serpentium allapsus timet.”

EPOD. 1.

The author of these two fine lines, had he lived in America, the land of fascination, would, I am inclined to think, have disbelieved, the whole story. They would have been a clue to light and truth on this subject.

It

It is a well known fact, that among some species of birds, the female, at a certain period, is accustomed to compel the young ones to leave the nest; that is, when the young have acquired so much strength that they are no longer entitled to all her care. But they still claim some of her care. Their flights are awkward, and soon broken by fatigue. They fall to the ground, where they are frequently exposed to the attacks of the serpent, which attempts to devour them. In this situation of affairs; the mother will place herself upon a branch of a tree, or bush, in the vicinity of the serpent. She will dart upon the serpent, in order to prevent the destruction of her young: but fear, the instinct of self-preservation, will compel her to retire. She leaves the serpent, however, but for a short time, and then returns again. Oftentimes, she prevents the destruction of her young, attacking the snake, with her wing, her beak, or her claws. Should the reptile succeed in capturing the young, the mother is exposed to less danger. For, whilst engaged in swallowing them, he has neither inclination nor power to seize upon the old one. But the appetite of the serpent-tribe is great: the capacity of their stomachs is not less so. The danger of the mother is at hand, when the young are devoured. The snake seizes upon her; and this is the catastrophe, which crowns the tale of fascination!

Facts and observations respecting the power of fascination ascribed to snakes.

An attachment to our offspring is not peculiar to the human kind alone. It is an instinct which pervades the universe of animals. It is a spark of the divinity that actuates the greater number of living existences. It is a passion which, in my mind, at least, declares, in language most emphatic, the existence, the superintendance, the benevolence, of a first great cause, who regards with partial and parental, if not with equal eyes the falling of a sparrow and the falling of an empire.

Among the greater number of the species of birds, the attachment of the parent to the young is remarkably strong. We have daily instances of this attachment among our domestic birds, and I believe, it is stronger among these birds in their wild state: for there are some reasons for suspecting, that this amiable instinct is diminished and weakened by culture*.

* This question will be examined in my memoirs upon the force, or affections, of animals.

Facts and observations respecting the power of fascination ascribed to snakes.

The instances which I have already mentioned, as well as a fact, which remains to be mentioned, point out, in a striking view, the attachment of the mother-bird to her offspring. She often guards her nest with the greatest attention, fearful of the insidious glide of the serpent. She endeavours to prevent the destruction of her eggs or young, by this enemy. When he has succeeded in obtaining them, she attacks him either alone, or calls other birds to her assistance. We ought not to be surprised, that sometimes she falls a victim to her affection. For it is a well known fact, that some species of birds will suffer themselves to be taken upon their nests, rather than relinquish their young, or their eggs.

In the study of natural history, I am always happy to discover new instances of the wisdom of providence, and new proofs of the strong affections of animals. And for the discovery of such instances of wisdom, and such proofs of affection, the contemplation of nature is an ample field. In the instances now before us, the strength of the instinct of affection in birds is illustrated, in a striking point of view; and I cannot help observing, that I feel an high degree of pleasure in being able to do away, in some measure at least, a prejudice, not less extensive than it is unfounded, by bearing my slender testimony in favour of the existence and the powerful dominion of a benevolent principle in animals.

The following fact was communicated to me, some time since, by our president, Mr. Rittenhouse. I think it strikingly illustrates and confirms the system which I have been endeavouring to establish. I relate it, therefore, with pleasure, and the more so, as I have no doubt, that the authority of a cautious and enlightened philosopher will greatly contribute to the destruction of a superstitious notion which disgraces the page of natural history.

Some years since, this ingenious gentleman was induced to suppose, from the peculiar melancholy cry of a red-winged-maize-thief*, that a snake was at no great distance from it, and that the bird was in distress. He threw a stone at the place from which the cry proceeded, which had the effect of driving the bird away. The poor animal, however, im-

* Commonly called, in Pennsylvania, the Swamp-Black-bird. It is the *Oriolus phœniceus* of Linnæus.

mediately

mediately returned to the same spot. Mr. Rittenhouse now went to the place where the bird alighted, and, to his great astonishment, he found it perched upon the back of a large black-snake, which it was pecking with its beak. At this very time, the serpent was in the act of swallowing a young bird, and from the enlarged size of the reptile's belly it was evident, that it had already swallowed two or three other young birds. After the snake was killed, the old bird flew away.

Facts and observations respecting the power of fascination ascribed to snakes.

Mr. Rittenhouse says, that the cry and actions of this bird had been precisely similar to those of a bird which is said to be under the fascinating influence of a serpent; and I doubt not that this very instance would, by many credulous persons, have been adduced as a proof of the existence of such a faculty. But what can be more evident than the general explanation of this case? The maize-thief builds its nest in low bushes, the bottoms of which are the usual haunts of the black-snake. The reptile found no difficulty in gliding up to the nest, from which, most probably in the absence of the mother, it had taken the young ones. Or it had seized the young ones, after they had been forced from the nest, by the mother. In either case, the mother had come to prevent them from being devoured.

We are well acquainted with the common food of the rattle-snake. It is the great-frog* of our rivers, creeks, and other waters. The snake lies insidiously in wait for his prey, at the water-edge. He employs no machinery of enchantment. He trusts to his cunning and his strength.

A very ingenious † friend of mine, who has devoted considerable attention to the natural history of the rattle-snake, and who has dissected many of them, assures me, that he never saw but one instance in which a bird was found in the stomach of this reptile, and this bird was the chewink, or ground-robin ‡. In another instance, he saw a ground-squirrel § taken out of one of these reptiles. In every other case, so long as the food retained enough of the form to be distinguished, the stomach was found to contain the great-frog, which I have mentioned.

* *Rana ocellata* of Linnæus. † Timothy Matlack, Esq.

‡ This is the *Fringilla erythrophthalma* of Linnæus.

§ The *Sciurus striatus* of Linnæus.

Facts and observations respecting the power of fascination ascribed to snakes.

Another argument against the fascinating power of the serpent-tribe still remains to be considered.

It is natural to inquire, for what purpose nature has endued serpents with the supposed powers of fascinating birds, and other animals? The answer to this question is uniform. It is said, the power is given that the serpents may obtain their food. Let us examine this opinion.

Admitting the existence of this power, I should have no hesitation in believing, that its use is what is here mentioned, though, indeed, it ought not to be concealed, that snakes are supposed, by some foolish people, to have the power of charming even children. And yet, I believe, there are no instances recorded of our American snakes devouring children. If, then, nature, in the immensity of her kindness, had gifted the serpents with this wonderful power, we should, at least, expect to find that the common and principal food of these serpents was those animals, viz. birds and squirrels, upon which this influence is generally observed to be exerted. This, however, is by no means the case.

As connected with this part of my memoir, it will not be improper to observe, that all our serpents are the food of different kinds of birds. Even the rattle-snake, whose poison produces such alarming symptoms in man, and other animals, is frequently devoured by some of our stronger and more courageous birds. As far as I can learn, the birds which most commonly attack and destroy this reptile, are the swallow-tailed hawk *, and the larger kinds of owls. The owl often feeds her young with this snake, whose bones are frequently found in her nest, at considerable heights from the ground. Even a hen has been known to leave, for a minute, her affrighted chickens, and attack, with her beak, a rattle-snake, the greater part of whose body she afterwards devoured †.

* *Falco furcatus*.

† It is commonly believed, that the rattle-snake is a very hardy animal; but this is not the case. A very small stroke on any part of its body disables it from running at all; and the slightest stroke upon the top of the head is followed by instant death. The skull-bone is remarkably thin and brittle; so much so indeed, that it is thought that a stroke from a wing of a thrush or robin would be sufficient to break it.

The black-snake is a serpent of much more activity than the rattle-snake. The latter, as I have already said*, seldom, if ever, climbs up trees. But the former will sometimes ascend the loftiest trees, in pursuit of the object of his appetite. The rattle-snake, it has been just observed, subsists principally upon the large frog, which frequents the waters of our country. He has, therefore, but little occasion for activity. But the black-snake, feeding more upon birds, stands more in need of activity. He frequently glides up the trees of the forest, &c. and, commonly in the absence of the mother, devours either her eggs or her young ones. The difficulty of obtaining his prey upon the tree is sometimes very considerable, as will appear from a fact which will be related immediately. Now, if this serpent is gifted with the faculty of fascinating, why is he not content to continue at the bottom of the tree, and bring down his object? And if he can employ this machinery of fascination at his pleasure, how comes it, that he so seldom succeeds in capturing old birds? For it is a fact that when birds are found in his stomach, they are principally young birds.

Facts and observations respecting the power of fascination ascribed to snakes.

I have said, that the black-snake sometimes finds great difficulty in obtaining his prey upon the tree. In support of this assertion I could adduce many facts. But my memoir has already exceeded the limits which I originally prescribed to it. I shall content myself, therefore, with relating a solitary fact, which strikingly illustrates my position.

A black-snake was seen climbing up a tree, evidently with the view of procuring the young birds in the nest of a Baltimore-bird. This bird, it has been already observed, suspends its nest at the extremity of the branch of a tree. The branch to which the bird, of which I am speaking, had affixed its nest, being very slender, the serpent found it impossible to come at the nest by crawling along it: he, therefore, took the advantage of another branch, which hung above the nest, and twisting a small portion of his tail around it, he was enabled, by stretching the remainder of his body, to reach the nest, into which he insinuated his head, and thus glutted his appetite with the young birds.

The importance of this fact, in the investigation of the subject of my memoir, appears to me to be great. An

* See page 106.

Facts and observations respecting the power of fascination ascribed to snakes.

American forest is not the silent residence of a few birds. During the greater part of the spring and summer months, our woods are alive with the numerous species of resident and visitant birds. At these times, if the black-snake possesses the faculty of fascinating, it cannot be a difficult thing for him to procure his food. Yet, in the instance which I have just related, we have seen this reptile climbing up a tree, and there obliged to exert all his ingenuity to obtain his prey.

I cannot well conclude this memoir without observing, that in the investigation of the subject which it involves, I have experienced much pleasure. For to the cultivators of science, the discovery of truth must, at all times, be a source of pleasure. This pleasure will even rise to something like happiness, when, in addition to the discovery of truth, we are enabled to draw aside the veil, which, for ages, has curtained superstition and credulity. Under the influence of various species of superstition, we fall from our dignity, and are often rendered unhappy. It should be one of the principal objects of science to rear and prop the dignity of the mind, and to smooth its way to comforts, and to happiness. The ills and the infirmities of our earthly state of being are numerous enough. It is folly, if not vice, to increase them. He who seriously believes, that an hideous reptile is gifted, from the sacred source of universal life and good, with the power of fascinating birds, squirrels, and other animals, will hardly stop here. He may, and probably will, believe much more. He will not, perhaps, think himself entirely exempted from this wonderful influence. He may suppose, that the property belongs to other beings, besides the serpents; and he will, perhaps, imagine that it forms a part of a more extensive plan, the effects of which, he will assert, are prominent, and unequivocal, though its ways, he will confess, are incomprehensible to mortal minds.

Historia naturalis non bene digesta abit in fabulum; præjudicia vero et nimia credulitas Veritatem, etsi cominus satis cognitam, longissime aliquando propellunt.

JACOBUS THEODORUS KLEIN.

XV.

Some Account of an Egyptian Lock of very high Antiquity. Indicated from DENON'S Travels, by a Correspondent. With Observations by W. N.

To Mr. NICHOLSON.

SIR,

A FEW weeks ago I saw, with particular pleasure, a model, or wooden lock, made from the description in Denon's travels, which was exhibited in the lectures at the Royal Institution. I beg leave to propose the insertion of the same in your excellent Work, and should hope for your opinion as to its security.

Letter concerning a lock shewn at the Royal Institution.

I am, Sir,

Your obliged reader,

New Broad Street,
May 12, 1804.

P. Q.

I HAVE given engravings of this lock, copied from Denon's book. *Plate VII.* shews the lock as applied to a door, and its developement is made in *Plate VIII.* The passage in Denon (translated) is as follows:

Description of the lock,

“No. 2, 3, 4, and 6. The Egyptian lock. It secures the gates of towns, of houses, and the apertures of the smallest articles of furniture or use. I have placed it among the antiquities, because it is the same as was in use four thousand years ago. I found one sculptured among the bas-reliefs which decorate the great temple of Karnack. It is simple in conception, easy of execution, no less sure than any other lock, and deserves to be applied on all our rural occasions. Fig. 2 is the key, which is capable of thousands of different combinations. In Fig. 3 the lock is shewn closed, seen as to its interior; the key being in the act of lifting up certain pins which had fallen into holes in the bolt, and kept it in its place. In Fig. 1 the bolt is drawn back, and the lock consequently open.” See *plate 139 of the said work.*

sculptured on the great temple of Karnack.

Eton, in his Survey of the Turkish Empire, mentions this lock as follows:

“Nothing can be more clumsy than the door-locks in Turkey, but their mechanism to prevent picking is admirable.

Eton's account of the same lock as of universal use in Turkey, &c.

It is a curious thing to see wooden locks upon iron doors, particularly in Asia, and on their caravansaries and other great buildings, as well as on house doors. The key goes into the back part of the bolt, and is composed of a square stick with five or six iron or wooden pins about half an inch long, towards the end of it, placed at irregular distances, and answering to holes in the upper part of the bolt, which is pierced with a square hole to receive the key. The key being put in as far as it will go, is then lifted up, and the pins entering corresponding holes raise other pins, which had dropt into these holes from the part of the lock immediately above, and which have heads to prevent them falling lower than is necessary. The bolt being thus freed from the upper pins, is drawn back by means of the key, the key is then lowered, and may be drawn out of the bolt: to lock it again the bolt is only pushed in, and the upper pins fall into the holes of their own weight. This idea might be improved on, but the Turks never think of improving."

Observations.

Simplest structure of this lock is a kind of latch.

The present lock.

Probable improvement.

This lock cannot be picked;

THE simplicity and other advantages of this lock or bolt, are too obvious to require much remark; for which reason I shall confine my present observations to its degree of security or inviolability. I think we may contemplate it in three several stages of perfection or improvement. 1. If it be constructed with one pin of considerable size to fall into the bolt, and the finger be supposed to be introduced for the purpose of raising it and setting the bolt at liberty, we shall have a fastening of nearly the same effect as the common latch. 2. Or if, instead of one falling pin, there be many, and an instrument be used to lift them, we shall have the bolt before us: and this, as far as we are informed, is the present state of the invention, though of so long standing. 3. Or thirdly, in case the present bolt should, on examination, be found to admit of being opened without extreme difficulty, it will become a question whether the principles of its structure can be so applied as to render it absolutely safe. This last question requires that we should first examine the ancient bolt a little more closely.

We may admit that the ancient lock, with many pins falling independently of each other, cannot be picked or opened without its key: and therefore we must ask whether the key can

can be made from an examination of the lock alone? In answer to this it may be noticed, that since all the pins in the key must be of equal height, the secret will consist in their relative distances and positions on the face of the key; and that these distances and positions can be easily known by introducing a stick, or key without pins, into the hollow of the bolt, and taking an impression (by means of a facing of soft clay) of the holes intended to receive the pins. After this a key may be made without difficulty; so that we arrive at our conclusion, that though this ingenious piece of mechanism cannot be violated without its key, yet it is easy to construct a key for that purpose.

but its key may be easily made from the lock.

We now come to the enquiry after that application of its principles which may render it absolutely safe. This is certainly possible; but not without considerably diminishing its simplicity. Besides several others, the following may be proposed: Let the dropping pin have an enlarged part, and a tail of wire above and below. Let the lower tail fall into its hole in the bolt, while the enlarged part falls into a socket made for its reception. Under these circumstances the bolt becomes fast: But when by raising the pin the enlarged part is clear of its socket, the bolt becomes free, and the lower tail is prevented from stopping it by a groove left for its reception. The bolt must have a covering piece of board, having a hole of the size of the enlarged part of the pin, directly above the socket into which it falls, and a groove for its upper tail; the interval between the covering-piece and the bolt itself being equal to the height of the enlarged part of the pin. By this means, when the pin is pushed up, just out of its socket, the bolt will move freely; but if it be pushed the least quantity farther, the enlarged part will enter the hole in the covering-board, and set it fast, as if it were in the socket; so that a very precise distance of elevation will be requisite. Lastly, the lower pin may be shorter or longer at pleasure. Now, if there be a number of these pins so placed and adjusted as to fall into their respective sockets at the position of the bolt when shut; if their lower tails be of different lengths, and a key be made to correspond with them, and lift them all to the proper height at once; the combination will be such as cannot be made out by any impression or tentative process upon the lock itself. For the evidence of a due length of any

Improvement by which it is rendered safe.

one of the lifting pieces of the key, will consist in the actual opening of the lock; and this cannot be had unless the due length of all the pins be obtained at once; against which the probability will be as the number of permutations of the pins, multiplied into the number of possible lengths of pins practically differing from each other in effect. Thus if the pins were six, the permutations would be $1 \times 2 \times 3 \times 4 \times 5 \times 6 = 720$; and if the length of a whole pin were one quarter of an inch, and a sensible difference would in practice arise from making the pins one-sixtieth of an inch longer or shorter; there would be 15 possible lengths for every pin. Whence $720 \times 15 = 10800$, the number of chances against the discovery.

W. N.

XVI.

On the Cause of the different Colours of the Triple Sets of Platina, and on the Existence of a new Metallic Substance in that Metal. By COLLET-DESCOTILS. Presented to the Class of Mathematical and Physical Sciences of the National Institute of France.*

Precipitation of the solution of platina. **A**LL chemists know that crude platina is easily soluble only in nitro-muriatic acid, that the solution is decomposable by muriate of ammonia and other salts with alkaline bases; and that the result of their former decomposition is a triple compound, consisting of oxid of platina, muriatic acid, and ammonia, or the alkali employed. The colour of this precipitate varies from a light yellow to a dark brown. It is sometimes also greenish. The latter is the case if the solution of platina is precipitated by a salt with base of soda.

Remarks. Before I say any thing further concerning the causes which influence the colour of this precipitate, I shall point out some phenomena which characterise the solution of the metal itself.

Foreign admixtures in crude platina. The grains of platina of commerce always contain more or less of foreign mixtures from which it should be previously freed as much as possible. The foreign bodies met with are most frequently minute stones, on which the acid employed for dissolving the metal, has little or no action, and two sorts of ferrugi-

* Gehlen's Chemical Journal, Vol. II. Part I. p. 73.

nous sand, one of which is obedient to the magnet, and another which is not attracted by the magnet, and which is only partially acted on by acids. I shall say no more in this place concerning these bodies, but that the first contains titanium and the second chromic acid, in considerable quantity.

The ferruginous sands contain titanium and chromic acid.

The best method to free platina of commerce from these admixtures, is that recommended by Proust, which consists in spreading out the platina on a sheet of paper, and carefully blowing away the lighter parts by means of a pair of bellows.

Mechanical purification by bellows.

Platina thus purified I introduced into a porcelain retort, to which a glass receiver had been fitted, previously filled one-third full of water. After having placed the retort in a reverberatory furnace, I raised the fire gradually, and increased the heat to the utmost I could produce, which was kept up for two hours. Nothing particular attended this process, except that the water with which the receiver had been partly filled, acquired a greenish hue towards the end of the process. On the roof of the retort, a fine blue powder was sublimed, of which I shall say no more at present than that it was soluble in water. The water in the receiver, after having been suffered to stand for a few days, had acquired a beautiful blue colour, which surpassed the colour of the best ultramarine.

Crude platina by violent distillation affords a blue sublimate soluble in water.

It was impossible to get the platina out of the retort. On breaking the distillatory vessel, the metal was found agglutinated, the upper surface of the mass had a rusty appearance, the middle was less discoloured, and the lower part had suffered no perceptible change.

The platina grains were agglutinated.

In order to examine the colouring matter which tinged the water of the receiver, I dropt into it a solution of an alkali; this produced instantly a blue precipitate. Sulphuric and muriatic acids, when mingled with this fluid, occasioned no change. Nitric and oxiginized muriatic acids changed this blue fluid, first to a lilac, but it soon lost this colour, and the whole became limpid. Water holding in solution sulphuretted hydrogen gas, occasioned no precipitate; but hidro-sulphuret of ammonia threw down a grey precipitate, which became blue by the affusion of acids, and then was rendered soluble again in hidro-sulphuret of ammonia.

The aqueous solution of the blue matter afforded a precipitate by alkali. Habitudes with acids, &c.

A small quantity of the blue precipitate collected from the roof of the retort, when urged with the blow-pipe in contact with borax, imparted no colour to this salt. When heated *per se*, it disappeared completely.

The blue sublimate disappeared before the blow-pipe: it did not colour borax.

Having

When platina is dissolved in n. m. acid, a black powder is separated. Having so far proceeded, I made a solution of platina in nitro-muriatic acid (the platina had been freed from iron as much as possible by muriatic acid.) During this solution, a glittering black powder became separated, as is always the case when platina of commerce is dissolved in nitro-muriatic acid. If the operator be careful in collecting this powder as fast as it is separated, the quantity which may be collected, amounts to about 0,03 of the platina employed. But if this powder be not removed as fast as it is deposited in the solution, part of it becomes again acted upon, and a much less quantity is obtained.

Muriate of pl. was decomposed by mur. of ammonia.

The muriate of platina obtained, after having been suffered to repose and being decanted, I decomposed by adding by a saturated solution of muriate of ammonia; the precipitate was separated by decanting the supernatant fluid, and repeatedly washed till the water which passed, did not become green by the addition of prussiate of potash. The precipitate obtained was of a *yellow colour*. The decanted fluid from which the precipitate had been separated, and the first quantity of water employed for its ablution, after having been mingled together and concentrated by evaporation to one third of its bulk, were again mixed with a solution of muriate of ammonia; the precipitate now obtained was of a *dark red colour*. On treating the fluid separated from it, as before, the precipitate which did fall down was of a very *dark brown*. All these different coloured precipitates were carefully washed till they contained no vestige of copper or iron.

Yellow precipitate.

The decanted fluid being concentrated, and more muriate of amm. added, a *dark red* precipitate was obtained.

The fluid a second time decanted gave by mur. amm. a *dark brown* precip.

The colour of the precip. of platina is darker the more of the black powder in the solution.

I have remarked that if the solution of platina be slowly prepared, that is to say, if the platina be introduced into the nitro-muriatic acid, in small quantities at a time, and the subsequent solution of each quantity be respectively separated, and separately be decomposed by muriate of ammonia, the colour of the precipitate or triple salt obtained, is darker in proportion to the quantity of the black powder which was contained in the solution.

The black powder is (difficultly) soluble in nitr. m. acid; and decomp. by mur. of am.

This black powder which is deposited during the solution of platina of commerce* in nitro-muriatic acid, is soluble (though difficultly) in nitro-muriatic acid, composed of much nitric, and little muriatic acid; its solution is also decomposable by

* This black powder is likewise separated during the solution of *malleable platina* in nitro-muriatic acid.

muriate of ammonia, and the colour of the precipitate is more or less intense, according to the quantity of powder contained in the solution.

From what has been so far stated, it appears, that this black powder is the cause of the different colours which the different precipitates or triple salts of platina exhibit, under different circumstances.

The black powder is the cause of colour in the precipitates.

In order to learn the nature of this substance, I shall detail the experiments which were undertaken for that purpose. The precipitates or salts I made use of were, the triple ammoniacal muriate of platina, and the triple muriate of platina and soda; the former salt I preferred on account of its easy decomposibility, and the latter on account of being very soluble.

Enquiry into the nature of this substance.

Experiments on the triple ammoniacal Muriate of Platina.

Equal quantities of the before-obtained yellow and dark red precipitate, being separately dissolved in equal quantities of water, the first salt furnished a solution of a gold yellow colour, whereas that of the latter was orange red. On adding to the latter solution a minute quantity of green sulphate of iron, or sulphureous acid, it became instantly of a gold yellow colour; the same effect was produced, though slowly, by the addition of alcohol.

Aqueous solutions of the yellow and dark red precipitates; the first gold-yellow, the second orange red.

The latter was rendered yellow by a sm. portion of gr. sulph. of iron, or by alcohol.

It was natural to suppose that the colour of the red salt might be owing to the higher degree of oxidization of the dissolved platina. In order to convince myself of this conjecture, I attempted to transfer oxygen to the yellow salt, by means of nitric and oxygenized muriatic acids. This however failed; the colour of the salt remained yellow as before. On repeating the application of nitric and oxygenized muriatic acids alternately, the result was only a very pale red coloured precipitate.

The red colour was not owing to greater oxidization.

Equal parts of the yellow and red precipitate, desiccated at equal temperatures, and under the same circumstances, when decomposed by heat, yield also unequal quantities of fixed residue. That of the first, amounted to 0,44 of the weight of the salt employed, and that of the latter was 0,4 and 5.

The yellow and red precipitates do not leave equal residues, when decomposed by heat.

If yellow precipitate be reduced by heat, the platina obtained is uncommonly soluble in a comparatively small quantity of nitro-muriatic acid, and the solution yield a yellow precipitate with muriate of ammonia.

The yellow precipitate if reduced, affords very soluble platina; which again affords a yellow precip.

If

The red precipitate affords a less soluble metal which leaves a black powder, and affords a red precip.

If the red precipitate be reduced, the metal obtained is of a different nature from the former. It is far more insoluble in nitro-muriatic acid, and whatever quantity of acid may be applied, there remains constantly a quantity of black powder, which is absolutely insoluble in the acid. This solution, on being decomposed by muriate of ammonia, yields a red precipitate.

This precipitate treated with oxygenized muriate of potash with heat.

To learn the nature of this precipitate, I reduced a quantity of it, introduced it into a porcelain tube, connected with a small retort, containing hyper-oxygenized muriate of potash, and applied heat to the retort, after having first adapted to the other extremity of a receiver containing a little water. On increasing the heat so as to decompose the hyper-oxygenized muriate, the tube became lined with a blue powder, which was also observed in the empty part of the receiver.

Blue powder obtained;

—soluble in nitro-muriatic acid.

The red colour arises from a peculiar metal.

After all the salt in the retort had been decomposed, I collected the blue sublimed powder. The minute quantity of it however did not permit me to submit it to many experiments. It was easily soluble in nitro-muriatic acid. Its nature will become more obvious hereafter.

From what has been so far stated, it appears to follow that the red colour of the triple precipitate of platina, obtained by muriate of ammonia, or other salts with alkaline bases, is owing to the presence of a peculiar metal contained in the platina, which has hitherto been considered as simple.

Examination of the triple Muriate of Platina and Soda.

Triple muriate of platina and soda.

This triple compound is very little known.* It may easily be obtained by pouring into a solution of platina, a salt with base of soda. It is very soluble in water, and even in alcohol. The solutions are capable of crystallizing in long prisms, on three-sided tables, of a yellowish red colour. It is decomposable by muriate of ammonia; the precipitate is a muriate of platina and ammonia. It is likewise decomposed by a solution of soda; on adding this alkali in excess, the formed precipitate becomes again dissolved.

Reducible on charcoal, by the blow-pipe.

Muriate of platina and soda is reducible upon charcoal before the blow-pipe. The reduced metal possessed a considerable lustre.

* Muffin-Puschkin has pointed out some of its properties in Crell's Annales, 1800, Vol. I. p. 93. of which a short abstract is to be found in the Annales de Chimie. p. 277.

If crystallized muriate of platina and soda, free from all adhering or excess of acid, be exposed for some time to the air, its beautiful red colour becomes changed into a greenish hue. If the salt in that state be dissolved in water, and oxiginized muriate of lime be added to it, a dark brown precipitate falls, which after having been washed and dried, is soluble in muriatic acid, with which it forms a *beautiful blue solution*. This colour becomes again destroyed by the admixture of alcohol, and re-appears by the addition of oxiginized muriate of lime.

Its crystals from a beautiful red, become green by exposure to the atmosphere. Its solution precip. by ox. muriate of lime.

This precipitate is somewhat soluble in water; it is reducible when fused with borax, without imparting to the latter any colour. The reduced metal is very porous: it appeared not to be soluble in any of the acids.

Precipitate somewhat soluble; reducible into a porous metal insoluble in acids.

If the solution of muriate of platina and soda, contain an excess of acid, it then is not disturbed by letting fall into it oxiginized muriate of lime; but on evaporation the mixture acquires a fine *green colour*.

No precipitate if acid be in excess.

Examination of the Yellow and Red triple Muriate of Platina.

If we add to the red triple muriate of platina, a solution of carbonate of soda, till it becomes completely dissolved the solution is of a yellow colour. On exposing it for some time to the contact of air, a green substance becomes deposited.

The red triple muriate becomes yellow by carbonate of soda; —and deposits a green precip. by expos- to the air.

The yellow triple muriate of platina, treated in a like manner with carbonate of soda, yields a solution of a dark yellow, or orange colour, which suffers no change whatever on exposure to air.

The yellow thus treated becomes darker, and does not afterwards change.

The separation of the green substance from the red triple muriate, is much accelerated by the admixture of oxiginized muriatic acid. It seems as if the separation of this substance is owing to the action of oxygen; for no other acid is capable of producing it; at least the precipitate which they produce, is a mere oxid of platina.

The green deposit from the red triple muriate is from oxidizement.

The green substance may likewise be instantly obtained by evaporating the solution by heat.

It is instantly had by evaporation.

If the solution of the yellow triple muriate be heated and evaporated, a very minute quantity of the green substance is indeed, sometimes deposited, and the remaining solution then acquires a more beautiful yellow colour. On continuing the evaporation of the yellow solution, a precipitate is deposited, which

The yellow triple muriate may afford a minute portion of green precip. by evap.—but this is an impurity. Its precip. is yellow.

which is not green, but *yellow*. This precipitate on being again dissolved in muriatic acid, and saturated with carbonate of soda, yields no farther green substance.

The green precipitate was reduced (by heat with borax) to a white brittle metal; difficultly acted on by nitro-muriatic acid;

more so when powdered.

The solution of the green substance became yellowish by sulphureous acid or green muriate of iron.

Ox. mur. of lime restored its green.

The green substance gave a blue sublimate, and left metal.

A portion of the same substance heated with nitre till the acid was decomposed. The alkali was dissolved in water, and not changed by acids.

The residue was not acted on by acids.

Hence chrome and molybdena were absent.

Separation of the platina from the triple muriate by alcohol.

I mingled a quantity of this green substance with vitrified borate of soda, and exposed the mixture for twenty-five minutes in a double crucible to the most intense heat I could produce; the result was a white brittle metal, which was difficultly acted upon by nitro-muriatic acid. The weak solution which had been obtained by this acid, had a violet colour; it yielded, on being evaporated, a dark green residue, which was soluble in muriatic acid, with which it formed a green fluid.

On pulverizing another quantity of this white brittle metal, and then exposing it to the action of nitro-muriatic acid, the solution was effected more easily, it was now more concentrated, and of a reddish-yellow colour. Muriate of ammonia let fall into it, threw down a little brownish-red precipitate, a proof that this metal still contained platina.

A quantity of the before-mentioned green substance being dissolved in muriatic acid, acquired a yellowish colour on being mingled with sulphureous acid, or with a solution of green muriate of iron. Oxigenized muriate of lime restored the original green colour.

A third part of the green substance, on being exposed to a violent heat in a porcelain retort, yielded a blackish-blue sublimate: the unsublimed part was a metallic substance, very difficultly soluble in nitro-muriatic acid.

A fourth portion of the green substance was urged, in a crucible head, with nitrate of potash. After the decomposition of the nitrate had been completed, the mass was diffused through water. The alkaline solution was colourless, and acids produced no change of colour in it. The residue, after repeated ablutions, was hardly acted upon by acids, the nitro-muriatic acid not excepted. This experiment precludes the presence of chrome and molybdena.

It is possible to separate nearly all the platina which is contained in the red triple muriate, by the mere admixture of alcohol, and the subsequent addition of dry potash or soda; for doing this much heat is evolved, and the reduction of platina takes place almost instantly. The same may be effected by means of carbonate of potash or soda; but in that case the solution must be highly concentrated. The reduction of the

platina

platina then takes place even without the application of heat, but only after the space of some days.

The supernatant fluid of the reduced platina, when heated, acquires a lilac colour, which becomes blue on long exposure to air, depositing at last a greenish substance, resembling that hitherto treated of. Oxigenized muriatic acid favours the separation of this substance.

The supernatant fluid affords the blue substance.

Sulphuretted hydrogen may also be employed for separating the platina from the red triple muriate; the platina becomes separated in the form of a brown powder; the other metallic substance remains undisturbed in the solution. It may, however, almost totally be precipitated by liquid ammonia. The precipitate obtained is brown. When fused with potash in a silver crucible, the mass acquires a green colour: On pouring muriatic acid on it, no complete solution could be effected; there always remained a powdery sediment which resisted likewise the action of nitro-muriatic acid. Carbonate of potash separated from this solution a small quantity of iron. The clear fluid from which the iron had been separated, remained perfectly transparent when heated; but it acquired a bluish hue, which increased on concentration, and after the exsiccation of the salt. On adding a little nitric acid to the salt, the blue colour became changed into a dark red.

Sulphuretted hydrogen also separates the platina.

CONCLUSION.

I now flatter myself with having proved, by the above experiments, that the red coloured triple salts of platina owe their colour to the presence of a peculiar metal, oxidized to a certain degree.

Conclusion. The red colour is owing to a peculiar oxide,

That this metal is nearly wholly insoluble in acids; that it becomes soluble in combination with platina; that when oxidized, it appears in the form of a blue oxide inclining to green; that its oxides, when combined with platina, are soluble in alkalies; that its acid solutions are not decomposable by sulphuretted hydrogen; that it imparts no colour to borax; that its oxides are reducible and volatilizable by heat, which volatilization becomes favoured by a stream of oxygen gas; and, lastly, that oxygen gas, assisted by heat, is capable of oxidizing this metal, and of volatilizing it in the form of a blue oxide.

nearly insoluble in acids, unless combined with platina, &c.

These

The new metal
not yet named.

These properties do not characterize any of the known metals, and I am therefore authorized to consider it as a new one, to which I shall give a name when I have explored its nature more fully.

XVII.

Description of an Apparatus for filtering Water. By Messrs. HARMAN and DEARN, of Redriff.

Apparatus for
filtering water.

THE waters which run near or upon the surface of the earth, are usually contaminated by the remains of animal and vegetable substances in their progress towards entire decomposition, as well as by the minute powder of earthy or mineral bodies, which render it turbid and less fit for the purposes of domestic life. Spring or pump waters, by a natural filtration through the sandy strata of the ground, are mostly cleared from these mechanical admixtures; but in many places, as is the case with those of our metropolis, they are rendered impure, or, as it is called, hard, by an actual solution of sulphate of lime or plaster of Paris, which prevents their lathering with soap, and probably renders them less wholesome; besides which, they usually carry a portion of the drainage water in great towns, which renders them offensive at certain seasons, and at all times less worthy of confidence. For these and numerous other reasons, it has always been considered as a desirable object to clear waters, by filtration, from those impurities which render them less limpid, and a variety of apparatus have been offered to the public for that purpose.

In all these the process of nature has been imitated; namely, by causing the water to percolate either through sand or a sand-stone; the latter of which, though costly, seems at present to be almost the only method in use among us.

The contrivers of the simple and cheap apparatus delineated in Plate VI. are Messrs. Harman and Dearn, potters at Redriff, who remark, that the filtering-stone is not only expensive and liable to be clogged up and spoiled by the bodies deposited in its pores from the water, but that, as these bodies are actually in the progress to decay and decomposition, they are in some cases found actually to change the flavour and affect the purity
of

of the fluid they are made use of to ameliorate. In consequence of which, they have been induced to apply their art to the simplest method of affording an apparatus for the filtering process, which shall not be liable to these objections. Apparatus for filtering waters

Plate VI. Fig. 1, represents the whole apparatus. Fig. 2 shews a shaded section. A is a vessel of stone-ware perforated with holes, *m*, at bottom, upon which coarse gravel, *h*, is laid, and upon that a stratum of fine gravel, and lastly fine sand, *g*. Or otherwise, the bottom may be covered with a coarse cloth, which will render the graduated fineness of the gravel and sand less necessary. Upon the top of the sand is laid a perforated and loaded board or plate of earthen ware, to prevent the sand from being disturbed when the water is poured in. B is a lower vessel, into which the filtered water from A drops, together with any sand that may escape from above. The clear water flows out through the neck *a c* into the vessel D for use.

The structure, uses, and effects of this apparatus are so obvious, that it is needless to enlarge upon them. The fineness and depth of the siliceous sand will regulate the perfection and expedition of the process; and the requisite cleanness and delicacy of the vessels and sand may be insured by changing the latter from time to time; for example, once in a fortnight or three weeks.

W. N.

XVIII.

Examination of a Stone containing Potash. By FREDERIC ACCUM, Teacher of Practical Chemistry, Pharmacy, and Mineralogy.

To Mr. NICHOLSON.

SIR,

SINCE Klaproth has detected potash in the lepidolite, leucite, and sonorous porphyry, chemists have sought for this alkali in other minerals, and their enquiries have not been disappointed: Tromsdorff has found it lately in the augite, and no doubt this alkali will be met with in many other minerals in which it was not expected. Being called upon by the company of potters of Staffordshire to examine a variety of Introductory letter.

of stones employed in the manufactory of earthen-ware, a particularly exact analysis was demanded of a stone, labelled *grey Cornish* *, in which I detected this alkali. I shall not detain you with a circumstantial detail of my reiterated experiments, which were undertaken with a view to learn the nature of the stone under consideration, which is sought for by the potters with avidity; I shall merely confine myself to point out its characters, as well as that examination which may serve to establish the credit of my assertion.

I am, Sir, with respect,

Your most obedient humble servant,

FREDERICK ACCUM.

Old Compton Street, Soho.

Physical Characters of a Siliceous Stone containing Potash.

Physical characters of a stone containing potash.

THIS stone is found in amorphous masses, forming irregular strata, under the surface of a blue clay. Its colour, when newly taken from the bed, is a greenish-grey interspersed with black spots; but when left exposed to the air, it acquires an ash-grey colour. It is not very hard; its powder is white. When rubbed, it exhales a faint argillaceous odour. Its substance is coarse or uneven, having many small, sharp, abrupt, irregular elevations and irregularities. Its fracture is very irregular. It may be easily scratched with a knife. It scintillates with steel. It is absolutely opaque in small fragments. Urged before the blow-pipe, it froths and melts into a white enamel. Treated with borax in a similar way, it forms a reddish bead.

Its specific gravity is 2,465.

ANALYSIS.

Analysis of a stone containing potash.

Experiment 1.—One hundred grains of the finely levigated stone, after having been previously ignited †, were mixt with a solution of potash containing 400 grains of alkali, the mixture was evaporated in a silver capsule to dryness, transferred

* I am not permitted to state the exact place in Cornwall where this stone is found.

† 100 parts lost, during ignition, 6 parts.

into a crucible of silver, and fused for half an hour. The fused mass had rather a pasty appearance, and could not be rendered perfectly fluid. Analysis of a stone containing potash.

Exp. II.—As soon as the crucible was nearly cold, it was removed from the furnace, its contents were softened by water, and the effusion of this fluid renewed from time to time, till all the fused mass was detached from the crucible: about 18 times its quantity of water were expended for that purpose.

Exp. III.—Into the obtained alkaline imperfect solution of muriatic acid was gradually poured, and the whole evaporated to dryness.

Exp. IV.—The mass was then transferred into a flask containing dilute muriatic acid, the whole was suffered to boil for a few minutes, and the insoluble part separated by the filtre. The siliceous earths thus obtained had a greyish appearance, but it acquired a white colour after having been again digested in muriatic acid, dried and ignited. It weighed 58 grains.

Exp. V.—The fluid from which this quantity of flex had been separated, together with the muriatic acid employed for purifying it, and the water expended for ablution, were concentrated to about 20 cubic inches, and then mingled, boiling hot, with a solution of carbonate of soda; the precipitate deposited was collected, and washed as usual.

Exp. VI.—As soon as the precipitate was so dry that it could be removed from the filtre without losing part of it, it was transferred into a solution of potash, and the mixture boiled for about half an hour. On suffering the alkaline solution to stand undisturbed for twenty-four hours, a powder was deposited, which, on being again boiled in a more concentrated solution of potash, remained unaltered. It was therefore collected, washed, dried, ignited, and put aside for further examination.

Exp. VII.—Into the alkaline solution, freed from this powder, I now dropt muriatic acid, till the precipitate which first appeared again vanished, and then decomposed it by the addition of carbonate of ammonia. The precipitate obtained in this experiment, after repeated ablutions and ignition, amounted to 28 grains. Experiments not essential to be stated here, convinced me that it was alumine.

Analysis of a stone containing potash. *Exp. VIII.*—The precipitate of this experiment was dissolved in nitro-muriatic acid, and then mingled with liquid ammonia. A brown precipitate became deposited, which, after being dried, amounted to 0,25 grains. It was oxide of iron.

The fluid from which this iron had been separated, yielded no other products, on being carefully examined, except two grains of flex which escaped separation.

Thus far the stone had yielded siliceous earth, alumine and oxide of iron. I shall suppress those enquiries which proved fruitless for learning its farther composition, and state merely those which were attended with success for that purpose, and which were as follow :

Exp. IX.—One hundred grains of the stone reduced to an impalpable powder, were triturated with 600 grains of crystallized nitrate of barytes : the mixture fused till the flame of a piece of ignited wood did not become enlarged when held close over the surface of the fusing mass.

Exp. X.—The melted mass was softened with water, and digested in muriatic acid ; the fluid filtered, and the residue washed.

Exp. XI.—The liquid obtained in this process I neutralized with carbonate of ammonia, and added the latter till no further cloudiness ensued. After having separated the precipitate, I evaporated the fluid to dryness : the obtained salt I transferred into a glass tube closed at one end, and committed it to sublimation.

Exp. XII.—After the sublimation had ceased (which was known by a metallic wire introduced into the tube during the process, not becoming in the least covered with a coat of salt), I broke the tube and separated the salt. This being dissolved in water and crystallized, yielded nine grains of muriate of soda.

Exp. XIII.—To decompose this salt, I redissolved it in water, and mingled the solution with nitrate of silver till no farther cloudiness appeared : I then separated the muriate of silver formed, and decomposed the nitrate of potash by heat, which yielded 4,50 of potash.

The

The analysis being thus completed, I learnt that 100 grains of this siliceous stone from Cornwall contain, Analysis of a stone containing potash.

Silex,	60
Alumina,	28
Oxide of iron,	0,25
Potash,	4,50
Water,	6
	98,75
Loss,	1,25
	100

XIX.

Letter from A. CARLISLE, Esq. on the Temperature of the Sea.

To Mr. NICHOLSON.

DEAR SIR,

Soho Square, May 28, 1804.

THE following table was made by Mr. R. Perrins, surgeon on the Honourable E. I. Company's establishment, during a voyage to Bombay in the year 1800. The temperatures in this table were noted at my request, from a desire to determine whether fishes possess any other temperature than that of the water in which they live, the negative being asserted by Linnæus. As, however, this imperfect journal may assist in similar researches, I beg leave to offer it for the use of your periodical work.

Table of the temperature of the sea, &c.

I am, Sir,

Your much obliged servant,

ANTHONY CARLISLE.

Table of the temperature of the sea, &c.

"Register of Atmospheric Temperatures, collated with those of the Sea-Water taken on board the Honourable E. I. Company's extra Ship Skelton Castle."

1800.	Atmo- sphere.	Sea-Water.	Lat. N.	Long. W.
Feb. 28	*	52°	42° 34'	13° 25'
March 2	*			
4	*	57	36 47	15
7	*	62	33 19	17 34
9	*	64	31 58	50
11	*	66	30 26	20 49
13	*	67	No observ.	59
15	71°	†68	26	23 24
16	72	69½	25 49	24
17	72	70	21 14	55
18	74	72	18 16	26 12
19	74	72	15 17	47
20	75	72	13 52	24 49
21	76	74	11 58	23 6
22	78	†76½	9 46	21 12
23	80	78	7 43	20 11
24	82	80	5 25	20 13
25	82	80	4 24	20 6
26	84	82	3 33	19 41
27	84	82	2 58	19 27
28	85	82	2 35	19 17
29	86	83	2 7	19 10
30	86	83	35	20 1
31	85	82	33 S.	19
April 1	84	81	2 27	20 41
2	83	80	4 44	22 31
3	82	80	7 12	23 47
4	83	80	9 50	24 50
5	82	79	12 30	25
6	80	76	15 17	25 59
7	78	76	18 13	26 5
8	78	76	20 41	26 3
9	80	78	22 22	56
10	80	78	23 28	27 20
11	80	78	24 23	37
12	78	76 Shark 88†	24 48	32
13	76	74	26 29	22
14	74	72	27 50	24 14
15	72	70	28 42	35
16	72	70	31	45

* The atmospheric temperature was not set down during these days.

† Each trial upon the sea-water was repeated three or four times, and the same results followed.

‡ Caught a shark, and found the heat in its stomach, after being suddenly killed, 88°.

[Table continued.]

1800.	Atmo- sphere.	Sea-Water.	Lat. S.	Long. W.	Table of the temperature of the sea, &c.
April 17	70°	68°	33° 10'	23° 57'	
18	70	66	34 20	22 46	
19	70	66	35 51	19 38	
20	68	66	36 2	18 35	
21	68	66	36 3	15 11	
22	68	64	35 56	11 22	
23	68	64		6 47	
24	66	64	36 47	3 15	
25	64	62	36 34	41 East	
26	64	62	39	3 40	
27	62	58	37 25	6 20	
28	61	58	53	7 40	
29	60	58	45	8 40	
30	60	58	36 30	12 11	
May 1	60	58	7	16 13	
2	60	58	30	18 13	
3	60	60	34	10 16	
4	60	60	37 22	22 25	
5	60	62	10	26 38	
6	58	64	36 54	50 51	
7	62	66	30	34 51	
8	64	66	37	39 39	
9	62	64	5	43 21	
10	62	64	20	45 21	
11	62	64	35 43	48 14	
12	64	66	33 15	49 38	
13	66	66	30 35	51 18	
14	68	68	27 35	52 12	
15	72	70	25 2	31	
16	76	74	21 21	39	
17	78	76	18 52	53 16	
18	78	76	16 25	54 12	
19	80	78	13 27	53 29	
20	78	78	12 41	50 55	
21	80	78	10 56	49 41	
22	80	78	7 8	49 41	
23	80	80	4 9	50 32	
24	81 $\frac{1}{2}$	80	1 46	52 27	
25	84	81	13 N.	54 2	
26	86	82	2 32	50 59	
27	84	82	4 36	56 10	
28	85	82	9 26	60 51	
29	85	82	9 26	60 51	
30	85	82	14 31	64 41	
June 1	84	82	15 56	66 59	
2	86	82	17 39	69 40	
3	85 $\frac{1}{2}$	84	18 57	72	
4	Arrived in Bombay Harbour.				

Observations

XX.

Observations upon the Doctrine of Count Rumford respecting the want of direct conducting Power in Fluids with regard to Heat. By CIT. BERTHOLLET.*

The doctrine of Count Rumford on the non-conducting property of fluids deserves examination.

COUNT RUMFORD has published several memoirs, by which he has endeavoured to prove that liquids and elastic fluids are not conductors of heat, and that they only transmit caloric by means of the contact with solid bodies, which is owing to the motion of their parts: as this property would make a difference between the states of a substance much greater than there is occasion to suppose in the explanation of the other phenomena; as, besides, this celebrated philosopher has fixed the attention on an object which had been neglected, and has drawn applications from it, beneficial in the arts, and in the uses of life, I think it proper to offer some doubts on the principles which he has deduced from his observations: I shall, in the first place, examine whether the facts on which he relies, cannot admit of a natural explanation from the properties which I have already analysed, or whether it will be necessary to have recourse to particular properties. But I shall attend only to the considerations which may serve to elucidate this discussion, without introducing the details it would require, if I were to examine it more fully.

Detail of Count Rumford's experiments on heated fluids.

The experiments which the author made were performed with an apparatus, of which it will be proper to insert a description. "He employed a cylindrical glass jar of 4.7 inches in diameter, and 13.8 in height; he put a known quantity of water (about two pounds) into the jar, which was intended to form a cake of ice at the bottom of the vessel. For this purpose, the jar with the water was put into a frigorific mixture of salt and ice, the action of which was not long in converting the water into a solid disk adhering to the bottom and sides of the jar; the jar was then removed, and plunged into a mixture of ice and water, to the level of the interior cake, which gave it the temperature of melting ice, or of the zero of the common thermometer. Then, after having covered the surface of the ice with a disk of paper,† hot water was poured

* From his *Essai de Statique Chimique*.

† *Bibl. Brit.*

on it, as gently as possible, to about the quantity of 74 ounces; this water was about eight inches above the surface of the disk." Detail of Count Rumford's experiments on heated fluids.

"The paper was then removed very gently, and after having suffered the water to remain a certain number of minutes in contact with the ice, it was poured off, and the jar with the ice which it still contained immediately weighed; its difference from the primitive weight established the quantity of ice which had melted while the hot water remained above it."

Having observed that the motion occasioned by pouring on the hot water produced an effect which was considerable, and foreign to the communication of heat, the author successively devised several modes of diminishing it. "He introduced the hot water through a wooden tube, closed at the bottom and pierced laterally with several small holes, through which the water issued upon a wooden disk, also pierced like a sieve, and floating on the water as it rose in the vessel. This disk was removed as soon as the water was poured in, and the vessel was covered with a wooden lid, in the centre of which was suspended a thermometer; finally, by previously covering the ice with a stratum of cold water, about half an inch in thickness, in which the perforated wooden disk floated, the author succeeded in greatly diminishing the irregularity of the results."

Besides, these precautions, the author separated from his results the quantity of ice, which liquefied at the first instant, and which exceeded that which melted in the succeeding spaces of time: in these different experiments, while that part of the cylinder which contained the ice was kept constantly at the temperature of melting ice, the upper part was left in contact with the surrounding air, or surrounded with a bad conducting substance, or plunged into the mixture of water and ice: the water poured on the ice received different temperatures. I make three divisions of the results of all the experiments: 1st. Water which was only about four degrees above zero, melted a little more ice in the same space of time than boiling water: 2d. When the upper part of the cylinder was wrapped in a bad conducting substance, the hot water melted more ice than when it was in contact with the air: 3d. When the upper part of the cylinder was plunged into the mixture of ice and water, more ice was melted than when it was left in contact with the atmosphere at 61° Fahrenheit's thermometer.

Explanation.
 1. More heat will be communicated the greater the difference of temperature.
 2. Locomotion increases this effect in fluids, and ought to be separately considered. 3. The locomotion does not follow the difference of temperature.

To explain these observations, the properties which we have recognized in liquid substances, and in elastic fluids, and from which we have inferred the changes which are effected in their different states of combinations, must be applied to the phenomena observed by Rumford.

We have seen, 1st. that the liquid particles enter so much the more rapidly into combination as they were at a greater distance from saturation, because then the force which solicits the saturation is greatest; so that the effects which depend on the communication of the temperature must be very weak, when the differences between them are but small.

2d. Locomotion, which serves to bring particles together which are at a greater distance from saturation, accelerates the effect of the mutual action by which its equilibrium is established, so that it is necessary to separate the effect which depends on this cause from that which is owing to immediate communication.

3d. Water and some other substances acquire a greater specific levity on approaching the term of congelation; whence it follows that the locomotion produced by the variations of temperature in other circumstances will be subject to modifications, which must be allowed for when water and the other liquids which possess this property, approach the term of congelation.

To apply these properties, we must also take into consideration the direction in which the heat is communicated; for the combination of effects will be different accordingly as it is applied to the inferior or superior part of a liquid.

Locomotion is greatest when the difference of temperature is small.

In order that a ready motion may be established between the particles which are at the bottom of the vessel, and those at the surface, there must be but little difference between their temperature; the particles which are near the ice, and become expanded, will then raise themselves above those which have a temperature barely greater; but if the temperature should cause a great difference between the specific gravities, this motion will be much more confined, so that the ice will remain surrounded with water of its own temperature, or which is very little removed from it. It is evident, therefore, that that part of the effect which depends on the motion will be much less, when there is a great difference in the temperatures.

The effect of the heat by a fluid will be

But when this distance in fact exists, the effect produced by the communication of the heat, independently of the motion, will

will vary according to the manner in which the temperature is preserved in the liquids. If the vessel has a non-conducting covering, the heat will be retained, and a greater quantity will be communicated than if it had been allowed to pass into surrounding bodies.—But when the difference of the temperature of the liquid is not considerable, as in the experiment in which water at 16° was employed, it is more advantageous to augment the effect owing to the translation of the particles, by cooling all the cylinder, than to preserve that which is owing to the simple communication of caloric. It appears to me that this explanation naturally flows from the known properties of fluids, and that Rumford's observations do not lead us to new inductions

It must be remarked that by separating the effect which took place at the first, when a considerable difference in the temperature could occasion a quick communication, he only observed that which was produced when there were but very slight differences between successive strata of the liquid and the ice itself: now, when there is but a small difference of saturation, either between chemical combinations, or between the temperatures, the equilibrium is established very slowly, and it becomes difficult to appreciate the effects.

The experiments which Rumford made by plunging a small cylinder of iron, heated to the degree of the ebullition of water, into water and mercury standing over a small piece of ice, without producing its liquefaction, only prove that when two bodies differ but little in their temperature, the equilibrium is established with difficulty; for it must be observed that the iron, which had but a little specific heat, and is a good conductor, must have lost the greatest part of its heat rapidly, in that part of the liquid which it passed gently through, and nevertheless have raised that of the liquid but little, or even that of the mercury, considering the mass of it.

But in these experiments of Rumford I find proofs of the property which he denies to liquids.

1st. In all the experiments which I have quoted, except in those made with the heated cylinder of iron, the liquefaction of the ice took place in a considerable degree, and each part liquified supposes a quantity of heat which would have raised an equal weight of water from the term of congelation to 75 degrees of the centigrade thermometer.

greatest where the difference of temperature is most favourable to both the causes of its communication. C. Rumford's experiments require no new law to explain them.

Additional remark.

Experiments with mercury and heated iron.

Other facts to shew that fluids are proper conductors.

2d. He froze the water at the surface of mercury cooled by a frigorific mixture; the temperature of the mercury was therefore communicated to the water, and the latter yielded its caloric to the mercury, to replace that which it lost.

If the communication of heat was only the effect of the particles of a liquid, the mercury of a thermometer would scarcely change its temperature when it had arrived at the freezing point of water: in fact in several of his experiments (*Essay 7.*) Rumford supposes, that at this degree, the mercury no longer communicates heat: now a thermometer takes the temperature of neighbouring bodies very rapidly, and indicates it several degrees below the freezing point of water, and as far as its own congelation; then it conducts itself like the solid bodies, and its dilatations become proportionably smaller than the preceding.

Mercury conducts better than water, but being denser, has less locomotion, ergo, &c.

Rumford has proved that the conducting power of mercury is to that of water as 1000 to 313.

This effect of the mercury, which takes the temperature of the system in which it is placed more rapidly than the water, although it has a much greater specific gravity, and is much less dilatable by the same degrees of heat, and consequently the heat will cause much less locomotion in its particles than in those of water; this effect I say, proves that the changes of temperature do not depend on the immediate communication and the changes of specific gravity which produces the approximation of the particles of unequal temperatures, but also on the better or worse conducting property of each substance.

Rumford neglects the radiant heat.

3d. Rumford paid no attention to the radiant caloric, nor did he make any allowance for it; nevertheless the communication of heat established by its means between solid bodies and liquids, through the gases, cannot be doubted, and it may be remarked that when he brought a heated bullet near to ice and tallow, a communication of heat took place which melted the surface of both, without it being possible to attribute this communication to a circulation such as he thinks is necessary.

Experiments of other philosophers.

The ingenious experiments of Rumford have employed the talents of several philosophers, who have already proved that the principles to which they led were not conformable to the true results of observation.

Nicholson found heat to pass downwards through a fluid.

Nicholson, in conjunction with Pictet, made some experiments by which he proved, that, on heating a liquid at the surface, by the superposition of a body, the heat penetrated, and

and raised a thermometer placed at the bottom of the liquid : to avoid communication by the sides of the vessel, a bad conducting substance was made choice of, and he ascertained, by means of a thermometer placed in the same liquid near the side of the vessel, that no current was established which differed in the temperature : finally, the motion of the bubbles which were disengaged, and the other appearances of the liquid, convinced him that currents were not formed.

In these experiments * it was proved, that liquids were different in their conducting faculty ; the penetration of the heat from the top to the bottom, was five times slower in oil than in mercury. —and that oil conducted much worse than mercury.

Rumford supposed that the slightest changes of specific gravity were accompanied by a locomotion, which produced a current, and he endeavoured to render it visible, by exposing an alkaline liquor, in which were suspended very small fragments of amber, which he found had the same specific gravity as the liquid, to a change of temperature : but Thomson has shown †, that the motions observed in these molecularæ were illusory, and that, in these variations of temperature, which are gradual, they appear to be owing only to the difference of specific gravity which they acquire, and to the adherence of air-bubbles, so that some of these molecularæ move in contrary directions, and run against each other without following the direction of the currents, he has also shown that these floating corpuscles might receive different motions while the strata of the liquid maintained a perfect tranquillity : he put water, tinged blue by juice of red cabbage into a glass vessel ; he afterwards poured clear water on it with great precaution, by means of a tube with a capillary extremity ; thus he kept the two liquids separate and distinct ; he then heated the vessel gently at the bottom : it is manifest that if a current had been established, it would have been marked by the coloured liquid, but the separation of the two liquids was preserved unconfused ; moreover the corpuscles put into the first liquid moved upwards and downwards, and crossed the line of separation.

Thomson found that the motions of solids do not prove any currents in the fluids,

—the floating solids rose and fell thro' two quiescent fluids of different colours.

* Bibl. Brit. Tom. XVIII. or Philosophic Journal, Quarto series. V. 197.

† Nicholson's Journal, Octavo, for Feb. 1802. See also a memoir by this philosopher, in the Journal for March, 1801, containing the earliest experimental examination of the Count's doctrine.

ration without producing the mixture of the two fluids, so that their various motions were not the effect of a current which carried them with it, and nevertheless, the heat was communicated to all the liquid. The propagation of the heat, and the agitation of corpuscules, which have nearly the same specific gravity, may therefore take place, independently of the circulatory motion, which is only established when there is a difference of temperature of a certain intensity between the different strata of a fluid.

Murray made experiments in a vessel of ice, containing a fluid. The heat passed downwards,

Murray has opposed Rumford's opinion with experiments still more direct, and not less conclusive; * he placed the bulb of a thermometer in a cylinder of ice, which he filled alternately with oil and mercury; he afterwards brought a heated body near the surface of the liquid; the thermometer rose several degrees in both experiments; but the heat could not have been conveyed by the sides of the ice whose surface would have absorbed and liquefied; no current was established, for the molecules of the liquid having become lighter, could not take a contrary direction, and the author avoided using water, which contracts on passing from the degree of congelation to a temperature a little more raised: the heat must therefore have been communicated to the bulb of the thermometer without the establishment of such a current as is supposed to be requisite; and that which served to dilate it was only the excess of what had liquefied part of the ice.

—and mercury conducted better than oil.

The observations of Murray prove at the same time that mercury is a much more effective conductor of heat than oil, for the elevation of the thermometer was manifested by its intermedium in a much shorter time, and more ice was liquefied.

* Ann. de Chim. Floreal, An. X. or Philos. Journal, Octavo, I. 165. 241. The experiments of Thomson and Murray, originally appeared in our Journal, and those of Count Rumford are also given, for which see the Indexes.

(To be continued.)

XXI.

On the Difficulty of obtaining Alumine in a State of Purity.

By R. T.

To Mr. NICHOLSON.

SIR,

YOUR Journal being open to every disquisition which may contribute to the progress of science, it may not be deemed impertinent to ask you, or some of your correspondents, to point out the best method of preparing pure alumine.

You may, probably, refer me to our modern authors on chemistry, but I aver that the methods therein recommended do not answer the purpose. For if a saturated solution of alum of commerce, be decomposed by a like saturated solution of a carbonated alkali, the alumine obtained is harsh to the touch, rather spongy, and strongly adheres to the tongue.

Alumine obtained by mixing saturated solutions of alum and alkali,

This earth, although washed as often as you please, always reddens the blue juice of the flowers of mallow, as well as of other delicate vegetable blues. It may be wholly dissolved in about 100 parts of boiling water: and the solution becomes very turbid by muriate of barytes.

appears to contain acid, and is soluble.

If, on the other hand, a dilute solution of alum be decomposed by another of alkali, a quite different product will be obtained. The alumine produced, on being desiccated is not porous, but splits into pieces like starch; and has, before it is nearly dry, a certain degree of transparency; it breaks with a smooth and nearly conchoidal fracture; it does not adhere to the tongue like the former, and has no earthy appearance. This, like the former, cannot be freed from the adhering acid. It also changes fine vegetable blues to red, although ever so much washed. What is the reason of this? Is there no method of forcing this earth from the adhering acid? or is it perhaps a characteristic of the earth itself, to redden vegetable blues. If you will please to answer this question, or point out a better method for procuring this earth pure, you will much oblige,

Dilute solutions afford alumine of another appearance,

likewise acid.

Camden-Town,
May 23, 1804.

SIR, Your constant reader,
R. T.

REPLY.

THIS letter having arrived so late in the month, I can only for the present offer it to my other correspondents.

SCIENTIFIC

SCIENTIFIC NEWS.

New Earth.

New earth.

A NEW earth has been discovered by Professor Klaproth of Berlin, in an ore which was hitherto supposed to contain tungsten, to which he has given the name of *ochroit* earth, the mineral which contains it he has called *ochroit*, (*ochroïtes*.) This earth seems to form the connecting link between the earths and the metallic oxides. Like yttria, it produces a reddish coloured salt with sulphuric acid; and is precipitable by all the prussiates; but it is distinguished from yttria by not forming sweet salts, and by not being (at least much less) soluble in carbonate of ammonia, and by acquiring when ignited, a light brown colour. This earth farther differs from yttria, by not being soluble either by borax nor by phosphates, with which yttria fuses into a colourless pellucid bead. Its other characteristics, and method of obtaining will be given in our next Journal.

Suberic Acid from Paper.*

BRUGNATELLI has observed, that when nitric acid is made to act upon paper, a large quantity of suberic acid, mixt with oxalic acid, is obtained. This proves that Fourcroy was right, in placing cork among the immediate principles of vegetables.

Easy and expeditious Method of preparing Copal Varnish †.

DEMMENIE, an ingenious glass blower, has noticed that the solution of copal may easily be effected, by exposing it to the vapours of alcohol or oil of turpentine. For that purpose an alembic may be filled $\frac{1}{4}$ with either of these fluids, and some pieces of copal suffered to be suspended by threads in it, over the surface of the fluid. After having made the alcohol or oil of turpentine to boil, the copal becomes liquefied, drops into the fluid and becomes dissolved. When no farther

* Gehlen's *Journal of Chemistry*: Vol. I. p. 3. page 340.

† *Ibidem*.

solution takes place, the whole is suffered to cool, and the solution of copal is decanted from the undissolved part. The varnish has no more colour than the copal itself.

Large Piece of Amber *.

A PIECE of amber weighing 13lb. 7 oz. 9scr. and measuring $318\frac{1}{2}$ cubic inches, has lately been found at Schlapacken, near Gumbinnen and Insterburg in Germany, which is the largest mass of amber hitherto found. Its colour is a pale yellow, intersected with several lines. Its value is estimated at about 40,000 dollars. Large specimen of amber.

Fluoric Ether †.

15 oz. Previously ignited and pulverized fluuate of lime, were introduced into a retort containing 10 oz. of highly rectified alcohol, and an equal quantity of sulphuric acid of 1,860 spec. grav. and the mixture distilled to dryness. During the distillation, a large quantity of gas was evolved, which burnt with a beautiful blue flame, and diffused an odour resembling phosphorated hydrogen. During the combustion of this gas, vapours of fluoric acid were precipitated. The product which had been obtained during the distillation was again distilled to one half, and the product which passed over, was poured into a vial containing water. No heat was produced, nor did the two fluids mix. It was therefore ether. But as its taste was sour, I added to it a solution of potash; this instantly separated a considerable quantity of flux, and the whole became converted into a gelatinous mass. The whole mass, on being again distilled, yielded pure fluoric ether. It greatly resembled sulphuric ether; its specific gravity was 0,720; it burnt with a blue flame; its taste was bitter, and greatly resembling bitter almonds.

New Method of preparing Nitric Ether. By BRUGNATELLI ‡. Nitric ether.

INTRODUCE into a tubulated retort, one ounce of sugar, and pour over it two ounces of highly concentrated alcohol. Adapt to the retort a capacious receiver surrounded with

* Ibid.

† Ibid.

‡ Ibid.

cloth

cloth dipt into water; and secure the junctures of the vessels by surrounding them with slips of paper only. Having done this pour through the tubulure of the retort 3oz. of concentrated nitrous acid; a violent action takes place, the sugar becomes dissolved, and the alcohol converted into ether, passes over into the receiver; its quantity is nearly equal to the alcohol employed.

Ascension of Sulphuretted Hydrogen Gas, by the Affusion of Nitrous Acid. By Professor LICHTENBERG.*

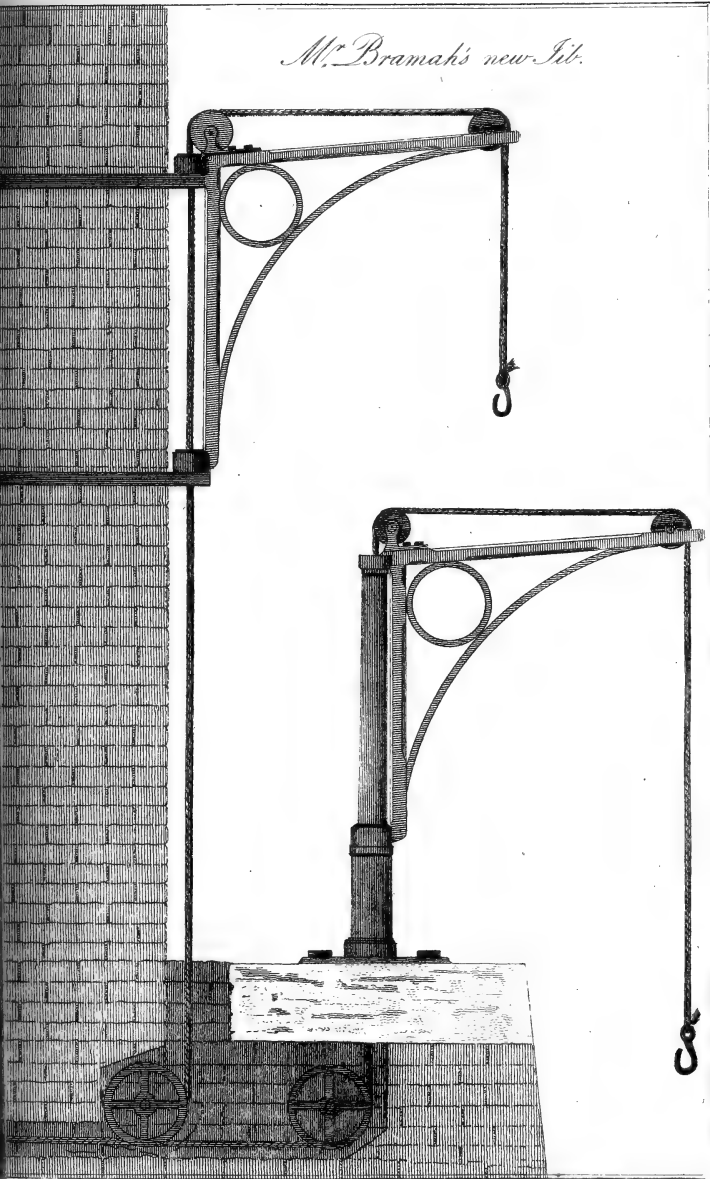
Ascension of sulphurated hydrogen gas.

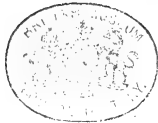
ATTEMPTING to illustrate the decomposition of sulphuretted hydrogen gas, I filled a bottle with it, capable of holding about 18oz. of water. Having done this, I poured into it at once, $\frac{3}{4}$ of an ounce of nitrous acid; a hissing noise took place, and much red vapour was disengaged, which in order not to molest my auditors, I confined in the bottle, by corking it. No sooner had this been accomplished, the mixture exploded with a loud report, accompanied with a blue flame †. The pieces of glass were thrown to a considerable distance, the larger ones were covered with sulphur.

* Ibid.

† For the success of this experiment, it seems to me to be necessary that the gas must be obtained by decomposing water by means of sulphuret of iron and an acid; for it always failed in my hands, if an earthy or alkaline sulphuret had been made use of for the production of it. F. A.

Mr. Bramah's new Lib.





Filtering Apparatus.

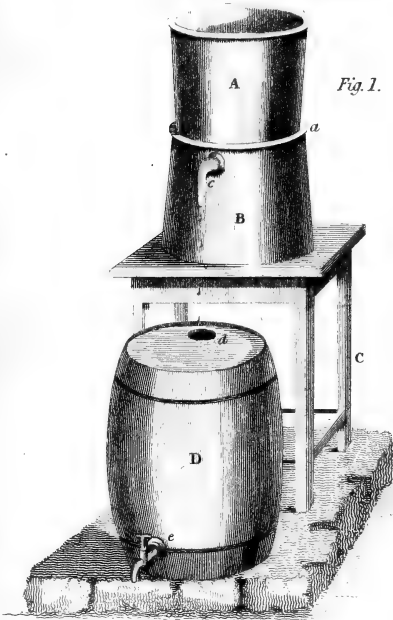


Fig. 1.

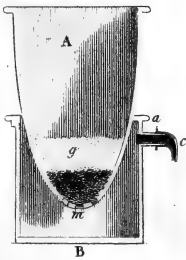


Fig. 2.

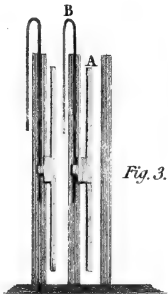
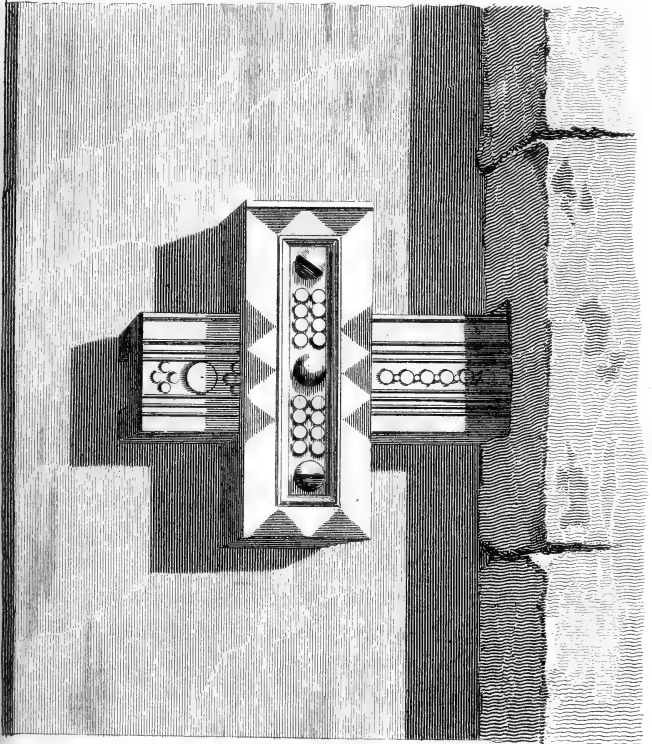


Fig. 3.

Galvanic Apparatus.



*Ancient wooden Lock,
used in Egypt, and in most parts
of the Turkish Empire.*





Ancient Egyptian Lock, developed.

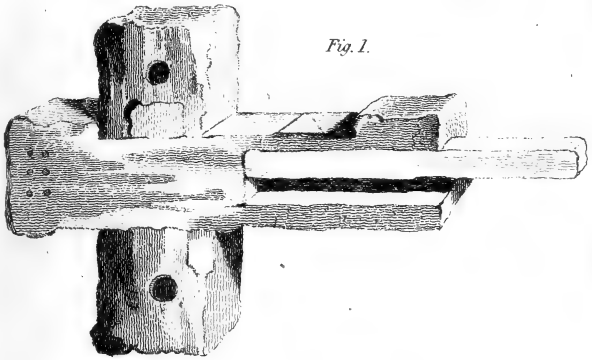


Fig. 1.



Fig. 2.

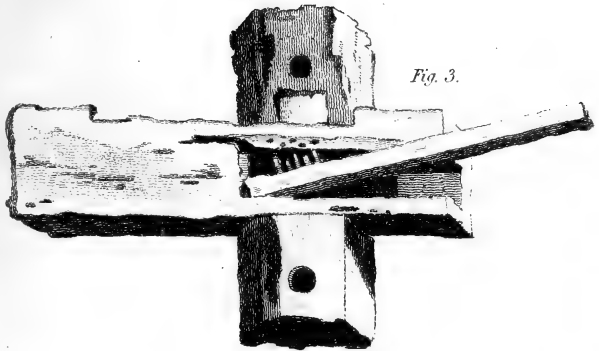


Fig. 3.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1804.

ARTICLE I.

*On the supposed Chemical Affinity of the Elements of Common Air;
with Remarks on Dr. Thomson's Observations of that Subject.
In a Letter from Mr. J. DALTON.*

To Mr. NICHOLSON.

SIR,

IN a former letter, inserted in your Journal (new series, vol. III. page 267) I endeavoured to shew the absurdity of the notion of atmospherical air being a chemical compound of azotic and oxygenous gases. Besides the difficulty, or rather impossibility on the one hand of conceiving how two elementary particles, constantly repelling each other, should notwithstanding be held together by a principle of cohesion or chemical affinity; or on the other hand, supposing the two atoms to combine, and form one centre of repulsion, how atmospheric air should differ from nitrous gas, &c. There are a variety of facts which oppose the doctrine so forcibly that I have for some time wondered on what grounds those who are still its adherents defended it. Dr. Thomson, in the second edition of his chemistry, vol. III. page 316, after reviewing the opinions of different philosophers on this head, and amongst others my own, concludes that air is a chemical compound; he assigns the four following reasons for the conclusion, which, from his extensive

Difficulties respecting the position that atmospheric air is a chemical compound.

Dr. Thomson's arguments in favour of that doctrine.

acquaintance with authorities, may fairly, it is presumed, be deemed the most cogent that have been offered on that side of the question. It is the object of this communication to shew their insufficiency.

1. The constant proportion of oxygen and azot supposed to be regulated by affinity, or increased attraction of the abundant gas,

—but this may with more probability be ascribed to diminished repulsion of the deficient gas: For, &c.

2. Humboldt and Morozzo's experiments; that a mere mixture of the gases differs from atm. air:—not credited.

3. Different combustibles absorb different quantities of oxygen from common air.

1. The constant proportion of azot and oxygen in the atmosphere is considered as an argument for their being held by affinity. So indeed it may; but it is equally in favour of my hypothesis, and therefore nothing tending to decide the question can be obtained from it. For, let part of the oxygen be abstracted any where from the atmosphere; then the azot may be supposed to attract the oxygen from the vicinity, and thus the equilibrium be restored: but it is certainly equally satisfactory to suppose that the oxygen in the vicinity, meeting with a less repulsive power from the deficient quarter, nothing prevents its diffusion into that quarter but the azot previously there, which, by hypothesis, can only retard, but by no means prevent the effect. Thus then, whether the azot attract the oxygen, or the oxygen repel itself, the effect is precisely the same. From this fact simply, it is impossible therefore to decide the merits of either theory; but if it be found that any one gas diffuses itself in any other, with nearly the same celerity, it will be a presumption in favour of my hypothesis; if otherwise, it may be urged that the quicker diffusion is owing to the stronger affinity. I have made a great number of experiments on this head, but could not find any remarkable difference in the time and circumstances of diffusion of the same gas.

2. It is said the experiments of Morozzo and Humboldt shew that air possesses different properties from a mere mixture of its two component parts. I do not credit the experiments.—Humboldt finds a variable quantity of oxygen, from 25 to 30, or more per cent. in the air; whereas others who are more accurate, find but 21, or at most 22, and that constant. It is no wonder then, if he mix 28 oxygen and 72 azot, that the mixture diminishes nitrous gas more than air, and supports combustion and animal life for a longer time.

3. Different combustibles are capable of absorbing different portions of oxygen from a given quantity of air. Phosphorus 22 per cent. Sulphur, 8, &c.—The only inferences I draw from these facts are, that phosphorus will burn in oxygen of any density, that sulphur will not burn in oxygen unless it be of $\frac{1}{2}$ of atmospheric density or more. The difference in the pheno-

mena of combustion in air, and in oxygen is not to be ascribed to the combination of azot and oxygen, but to the less density of the latter, ($\frac{1}{2}$) of what a pure atmosphere of the same gas would be. From an incidental but imperfect trial I made, in conjunction with Mr. Davy last winter, I have no doubt but iron wire would burn in common air of five times the density with brilliancy as in an atmosphere of pure oxygen of common density. At any rate it is notorious that as the density of common air is increased, combustion in it becomes more vigorous. Though I have never attempted combustion in an atmosphere of pure oxygen of $\frac{1}{2}$ the common density, I can scarcely doubt that the appearances would be much the same as in the open air. It is probable therefore that the facts under this head, if duly investigated, would turn out in favour of the hypothesis of air being a mixture.

Ascribed merely to the various densities of oxygen in which they cease to burn.

Proposed experiments to decide this point.

4. "A gas no way distinguishable from common air frequently makes its appearance during the preparation of nitric acid; and Mr. Davy decomposed nitrous oxide, by passing it through a red hot tube, and converted it into nitric acid and a gas, which possessed the properties of common air; now if air were a mere mixture, it is infinitely improbable that its two constituent parts should be evolved during such processes exactly in the proportion that exists in common air."—Granted; but as the force of this argument rests upon the *exact proportion* of oxygen and azot in the gases so evolved, that is, upon their being constituted always of 21 per cent. oxygen, and 79 azot, the facts should be made out accordingly. Dr. Priestley is the only one I know of, who has particularly examined the gas produced in the preparation of nitric acid, and he found it to have *much more* oxygen than common air. Mr. Davy in his analysis of nitrous oxide, found the gas analogous to atmospheric air always to contain *less* oxygen, though it was nearly of the atmospheric standard.

4. Gas resembling common air, obtained in preparing nitrous acid:

Answer. The precise resemblance, or proportion of parts, has not been shewn.

The quick ascent of hydrogen and the descent of carbonic acid, have been objected to my hypothesis as facts that prove the operation of the laws of specific gravities on elastic fluids. No doubt can exist that a portion of elastic fluid completely insulated, as a balloon, or a bubble of carbonic acid or hydrogen, surrounded by a film of water, is subject to the laws of gravitation, and rises or falls in elastic fluids on the same principle

The statical ascent or descent of the gases affords no proof against the law of their general diffusion.

ciple as it rises in water; the same must be allowed when a vessel, containing a considerable portion of elastic fluid, is suddenly exposed at some surface to the atmosphere; in this case, the fluids *must operate upon each other* for a few moments in a collected capacity, as in elastic bodies; because the diffusive or repulsive force by which they constantly tend to dispersion, is comparatively slow in producing the ultimate effect, being in this respect exactly similar to chemical affinity, the operation gradually diminishing as the effect draws towards a conclusion.

For the separate masses are carried by their relative gravity more speedily than the diffusion can take place.

Nothing more therefore can be inferred from the facts above-mentioned, than that gravity overpowers, and for a moment obliterates the effect of that cause which in other cases slowly produces the dispersion of the fluid, whether it be attraction, as commonly supposed, or repulsion, as I suppose. Chemical philosophers have not enquired sufficiently into the effects of exposing gases in different circumstances to the atmosphere; all that we are usually told is, that a jar filled with hydrogen and uncovered, loses its gas in a few seconds; but if inverted, it remains nearly pure for a considerable time, &c. I find that a cylindric jar of 7 inches depth and $2\frac{1}{2}$ diameter, being filled with hydrogen, and inverted, loses more than half of its gas in two minutes, and there is so little left as scarcely to explode in

But the diffusion is much speedier than is generally taught.

five minutes. If a tube, 12 inches long and $\frac{1}{4}$ inch diameter, be filled with hydrogen, and exposed in like manner to the atmosphere, it will lose half its gas in five minutes, and that the same, whether it be held up or down or horizontal. Here we see effects that cannot be accounted for by gravity, that are produced in opposition to its agency, and where indeed it is almost obliterated by the action of some more powerful cause. Let the advocates for the atmosphere being a chemical compound attend to such facts as these, and they will soon find themselves reduced to acknowledge that *all gases have the same affinity for one another*, a position which their doctrine ultimately tends to establish. Indeed it is the same with regard to air and vapour, of water, ether, or of any other fluid; that is, all kinds of gas or mixtures of gases, have the same affinity for the same vapour, and even a torricellian vacuum possesses just the same affinity as any of them, judging from the quantity evaporated, and force of the vapour in a given volume. If any one doubt it, he may easily satisfy himself by throwing up a drop

The facts, if ascribed to affinity, would shew that it is the same between all the gases—and even between a gas and a vacuum.

a drop or two of ether into the vacuum of a common barometer; if the temperature be 68° , the mercury will fall 15 inches nearly; at the same time, if ether be admitted to a given bulk of any kind of gas, subject to the pressure of the atmosphere, the volume will be doubled, clearly shewing that the elastic vapour from the ether is the same in both cases, namely, an independent fluid of 15 inches force.

I cannot dismiss this subject without observing, in justice to Dr. Thomson, that he has entered more clearly into my views of these subjects than any other of our own country who has animadverted upon them. There are certain principles, however, which he, with most chemists of the present day, embraces, which are, according to my experience, decidedly erroneous. One of these is, *that water dissolves air*. An excellent paper of Mr. W. Henry, on the absorption of gases by water, in the *Philos. Transactions* for 1803, has shewn us sufficiently in what light we should view the supposed solution of air in water. Certainly air that is retained in water by mechanical force, and which always escapes when that force is withdrawn, cannot with any propriety be said to be held by chemical affinity.

Water does not dissolve air.

Dr. Thomson has been misinformed respecting my opinions on the expansion of liquids. In vol. i. page 343, he gives it as my suggestion, that all liquids expand the same quantity from their freezing to their boiling temperatures. I never entertained such an opinion; and it is certainly erroneous. My idea is, that pure and homogeneous liquids, such as water and mercury, expand according to the square of the temperatures from the points at which they congeal; but I have not yet found a law to regulate the *relative* expansions of these and other liquids.

The author does not affirm that the expansion of all liquids by heat is the same between conge-lation and boiling; but that any single fluid expands according to the square of the temperature from its freezing point.

I am your's, &c.

J. DALTON.

Manchester,
June 16, 1804.

Easy

II.

Easy Methods of completing the Tables of Squares and Cubes. In a Letter from H. G.

To Mr. NICHOLSON.

SIR,

Computation of squares: **I**N the last number of your Journal, Mr. Councer's extensive table of squares is stated to be deficient from No. 28261 to 29061. This deficiency may very easily be filled up by this rule. To the square of any given root add twice that root + 1; the product will be the square of the next root.

Of cubes; half the series; **T**he table of one half the cubes in Mr. Councer's table, may easily be made, examined, and added to, by the following rule: Multiply the cube of any given root by 8; the product will be the cube of twice the next root.

the other half. **T**he other half of the cubes in the same table, and also all those already found as above, may easily be found from any two cubes and their roots in succession being given. Thus: From the given cube of the largest of the given roots, subtract the given cube of the next less given root; to the remainder add six times the largest given root, and also the given cube of the largest given root; the sum will be the cube required; as will appear from the following example:

	Roots.	Cubes.	
Give {	9999=A=	999700029999	} Required the cube of the root 10001.
	D=10000=B=	1000000000000	
	B - A = C =	299970001	} Remainder.
	6 D =	60000	} Six times the largest given root.
	B + C + 6 D =	1000300030001	} Cube required of root 10001.

I am, Sir,

Your most humble servant,

East Smithfield.

H. G.

III.

Problems in Spheroidal Triangles. By PEREGRINUS PROTEUS.

To Mr. NICHOLSON.

SIR,

THE solution of the problem relating to the figure of the earth, in my first letter*, led to some new properties of spheroidal triangles, from which I shall endeavour, in this, to deduce a few rules, that may be applied with success in trigonometrical surveys. It will be found that, though the general formulæ be complex, yet, in the cases that occur in practice, they admit of being sufficiently simplified; for not only may the terms involving the second and higher powers of the compression, but also frequently the differences between the longitudes and latitudes of the stations, be rejected.

New problems in spheroidal triangles.

1. 'Having given the latitudes of two places on the surface of the earth, and the length of the straight line or chord joining them, it is required to find their difference of longitude?'

Prob. I. Given two latitudes and chord of arc: Required diff. longit.

Let λ, ϕ be the latitudes of the two places, D their distance in fathoms, a the radius of the equator in fathoms, $\frac{c}{a} = \delta$ the

compression at the poles, d such that $\sin \frac{1}{2}d = \frac{D}{2a}$, and ω' the difference of longitudes of two places, whose latitudes are λ, ϕ , and distance d on the sphere. Then by the formula, page 16,* the true difference of longitude ω is equal to $\omega' +$

$Q \times \frac{c}{a} = \omega' + Q \delta$, if we reject the higher powers of δ , in which \dagger

$$Q = \frac{2(\sin \lambda - \sin \phi)^2 - 2 \sin \frac{1}{2}d^2 (\sin \lambda^2 + \sin \phi^2)}{\cos \lambda \cos \phi \sin \omega'}$$

Now because in trigonometrical surveys, d, ω' , and $(\lambda - \phi)$

are small, we may take $\frac{2(\sin \lambda - \sin \phi)^2}{\cos \lambda \cos \phi \sin \omega'}$

* Journal for May.

† There are some typographical and other errors in the paper referred to, which I have taken notice of at the end of this letter.

$$\frac{8 \operatorname{cof.} \left(\frac{\lambda + \phi}{2} \right)^2 \operatorname{fin} \left(\frac{\lambda - \phi}{2} \right)^2}{\operatorname{cof.} \lambda \operatorname{cof.} \phi \operatorname{fin} \omega'} = 4 \operatorname{fin} \frac{\lambda - \phi}{2} \times \frac{\lambda - \phi}{\omega'}, \text{ and}$$

$$\frac{2 \operatorname{fin} \frac{1}{2} d^2 (\operatorname{fin} \lambda^2 + \operatorname{fin} \phi^2)}{\operatorname{cof.} \lambda \operatorname{cof.} \phi \operatorname{fin} \omega'} = 4 \operatorname{tang.} \lambda \operatorname{tang.} \phi \times \frac{\operatorname{fin} \frac{1}{2} d^2}{\operatorname{fin} \omega'}$$

$2 \operatorname{tang.} \lambda \operatorname{tang.} \phi \operatorname{fin} \frac{1}{2} d \times \frac{d}{\omega'}$; wherefore Q is equal to $4 \operatorname{fin}$

$$\frac{\lambda - \phi}{2} \times \frac{\lambda - \phi}{\omega'} - 2 \operatorname{tang.} \lambda \operatorname{tang.} \phi \operatorname{fin} \frac{1}{2} d \times \frac{d}{\omega'}, \text{ and}$$

$$\omega = \omega' - \delta. \left\{ \operatorname{tang.} \lambda \operatorname{tang.} \phi \times \frac{d^2}{\omega'} - \frac{2 (\lambda - \phi)^2}{\omega'} \right\} \text{ nearly; where}$$

the correction $Q \delta$ will be given in seconds of a degree, if d , ω' , and $(\lambda - \phi)$, be expressed in seconds.

By spherics $\operatorname{cof.} \omega' = \frac{\operatorname{cof.} d - \operatorname{fin} \lambda \operatorname{fin} \phi}{\operatorname{cof.} \lambda \operatorname{cof.} \phi}$, whence we find,

$$\text{when } d \text{ and } \omega' \text{ are small, } \omega' = \sqrt{\left\{ \frac{(d + \lambda - \phi)(d - \lambda + \phi)}{\operatorname{cof.} \lambda \operatorname{cof.} \phi} \right\}}$$

without any sensible error, which value substituted in the formula $\omega = \omega' + Q \delta$, will give the true difference of longitude required.

When the measured chord is perpendicular to the meridian,

ϕ is nearly equal to λ , and consequently $\omega' = \frac{d}{\operatorname{cof.} \lambda} = \frac{D}{a \operatorname{cof.} \lambda}$,

and $\omega = \frac{D}{a \operatorname{cof.} \lambda} \times (1 - \delta \operatorname{fin} \lambda^2)$, or $\omega = \frac{D}{\operatorname{cof.} \lambda (a + c \operatorname{fin} \lambda^2)}$; but

$a + c \operatorname{fin} \lambda^2$ is equal to the radius of curvature of the perpendicular to the meridian, in the latitude $\lambda = AM$ in the figure (*Journal for May*), which put $= R$, and there results $\omega =$

$\frac{D}{R \operatorname{cof.} \lambda}$, which corresponds exactly with one of Legendre's

theorems; (*Mem. Acad.* 1787).

Example. Let $\lambda = 50^\circ 44' 23''.71$, the latitude of Beachy Head, $\phi = 50^\circ 37' 7''.31$, the latitude of Dunnofe, $D = 56566.57$ fathoms, and $d = 3496740$. Then because $\operatorname{fin} \frac{1}{2} d =$

$\frac{D}{2a}$, d is $= \frac{D}{a}$ nearly $= 3336''.73$, $\omega' = 1^\circ 27' 0''.65$, and $\omega = \omega'$

$- 3105''.74 \delta$; in which, if we suppose $\delta = \frac{1}{252}$, we shall have

$\omega = 1^\circ 26' 47''.93$.

2. ' Having

2. 'Having given the latitude of a place, and its longitude and distance from another place, it is required to find the other latitude?' Prob. II. Given one lat. and long. and distance from another place. Req. lat. of this last.

Let λ be the given and ϕ the required latitude; D the measured distance, $d = \frac{D}{a}$, ω the difference of longitude, and ϕ'

the latitude on the sphere found from λ, ω, d . Then by spherics $\text{cof. } \lambda \text{ cof. } \phi' \text{ cof. } \omega + \sin \lambda \sin \phi' = \text{cof. } d$, whence, when d, ω , and $\lambda - \phi'$ are small, there results $\lambda - \phi' = \pm \sqrt{(d^2 - \text{cof. } \lambda \text{ cof. } \phi' \cdot \omega^2)} = \pm \sqrt{(d^2 - \omega^2 \text{cof. } \lambda^2)}$ nearly. But by the theorem, page 16, if $\phi = \phi' + x$, we find $x = Q \delta$, in which the variable quantity Q is
$$= \frac{2(\sin \lambda - \sin \phi')^2 - \frac{1}{2} d^2 (\sin \lambda^2 + \sin \phi'^2)}{\text{cof. } \lambda \sin \phi' \text{ cof. } \omega - \sin \lambda \text{ cof. } \phi'}$$

$$\frac{\frac{1}{2} d^2 (\sin \lambda^2 + \sin \phi'^2) - 2 \text{cof. } \left(\frac{\lambda + \phi'}{2}\right) \cdot (\lambda - \phi')^2}{2 \text{cof. } \lambda \sin \phi' \sin \frac{1}{2} \omega^2 + \sin(\lambda - \phi')}$$

; or

$$Q = \frac{\frac{1}{2} d^2 (\sin \lambda^2 + \sin \phi'^2) - 2 \text{cof. } \left(\frac{\lambda + \phi'}{2}\right) \cdot (\lambda - \phi')^2}{\text{cof. } \lambda \sin \phi' \sin \frac{1}{2} \omega' \times \omega' + (\lambda - \phi')}$$

Now when λ and ϕ are nearly equal, this formula becomes,

$$Q = \frac{\sin \lambda^2 \cdot d^2 - 2 \text{cof. } \lambda^2 (\lambda - \phi')^2}{\omega \cdot \text{cof. } \lambda \sin \phi' \sin \frac{1}{2} \omega + (\lambda - \phi')}$$

$$\frac{\sin \lambda^2 \cdot d^2 - 2 \text{cof. } \lambda^2 (\lambda - \phi')^2}{\text{cof. } \lambda \sin \lambda \sin \frac{1}{2} \omega \cdot \omega + (\lambda - \phi')}$$

3. 'Having given the latitude λ , the horizontal angle α , and the distance D , it is required to find the difference of longitude?' Prob. III. Given one lat. with the horiz. angle and distance: To find diff. long.

Let ϕ be the latitude, and ω the longitude of the place required; with the latitudes λ, ϕ , and distance $d = \frac{D}{a}$, on the sphere, to find the difference of longitude ω' , and the horizontal angle α' at λ .

Then will $\sin \omega' = \frac{\sin d \sin \alpha'}{\text{cof. } \phi}$, or $\omega = \frac{\sin \alpha' \cdot d}{\text{cof. } \phi}$; but by first

question $\omega = \omega' - \delta \left\{ \text{tang. } \lambda \text{ tang. } \phi' \times \frac{d^2}{\omega} - \frac{2(\lambda - \phi')^2}{\omega} \right\}$;

wherefore $\omega = \frac{d \sin \alpha'}{\text{cof. } \phi} \left\{ 1 - \delta \left(\frac{\text{tang. } \lambda \sin \phi \text{ cof. } \phi}{\sin \alpha'^2} - \frac{2 \text{cof. } \phi^2 (\lambda - \phi')^2}{d^2 \sin \alpha'^2} \right) \right\}$

$$= \frac{d \sin \alpha'}{\text{cof. } \phi} \left\{ 1 - \delta \left(\frac{\sin \lambda^2}{\sin \alpha'^2} - \frac{2 \text{cof. } \lambda^2 (\lambda - \phi')^2}{d^2 \sin \alpha'^2} \right) \right\}$$

Now

Now by spherics $\left(\frac{\lambda - \phi'}{d}\right)^2$ is = $\text{cof. } \alpha'^2 \text{ feré}$, and $\sin \alpha' = \sin \alpha - 2 \sin \alpha'^2 \text{ cof. } \alpha' \text{ cof. } \lambda \cdot \frac{\phi - \lambda}{\omega} \cdot \delta$, by theorem, page 17; or because $\frac{\phi - \lambda}{\omega} = \text{cof. } \lambda \text{ cotang. } \alpha \text{ feré}$, the $\sin \alpha'$ is = $\sin \alpha - 2 \sin \alpha' \text{ cof. } \alpha'^2 \text{ cof. } \lambda^2 \delta$; consequently ω is = $\frac{d \sin \alpha}{\text{cof. } \phi} \left\{ 1 - \delta \cdot \frac{\sin \lambda^2}{\sin \alpha^2} \right\} \times \left\{ 1 + \frac{2 \text{cof. } \lambda^2 \text{ cof. } \alpha^4}{\sin \alpha^2} \cdot \delta \right\}$.

From the theorem $\sin \alpha' = \sin \alpha - 2 \sin \alpha' \text{ cof. } \alpha'^2 \text{ cof. } \lambda^2 \cdot \delta$, it is manifest that α is nearly equal to $\alpha' + \sin 2\alpha' \text{ cof. } \lambda^2 \cdot \delta$.

When α is nearly equal to a right angle, we have $\omega = \frac{d \sin \alpha}{\text{cof. } \phi} \left\{ 1 - \delta \sin \lambda^2 \right\} = \frac{D \sin \alpha}{R \text{cof. } \phi}$, which corresponds with one of Legendre's theorems.

Prob. IV.
Given two lats.
and diff. long.
To find horiz.
angles.

4. 'Having given the latitudes of two places, and their difference of longitude, it is required to find the horizontal angles?'

Let λ, ϕ be the latitudes of two places, ω their difference of longitude, and α, β the horizontal angles at λ, ϕ respectively on the spheroid; also α', β' the corresponding angles on the sphere, which may be found from the data by the rules of spherical trigonometry. Then by the theorem, page 17, if we put,

$$M = 2 \sin \alpha'^2 \times \frac{\text{cof. } \lambda}{\text{cof. } \phi} \times \frac{\sin \phi - \sin \lambda}{\sin \omega} = 2 \sin \alpha'^2 \times$$

$$\frac{\text{cof. } \lambda \text{ cof. } \frac{\lambda + \phi}{2}}{\text{cof. } \phi} \times \frac{\phi - \lambda}{\omega} \text{ feré,}$$

$$N = M \cdot \left\{ \sin \lambda (\sin \lambda + \sin \phi) - \frac{1}{2} \right\} + \text{cotang. } \alpha' M^2, \text{ and}$$

$$N' = M \cdot \left\{ \frac{1}{2} - \sin \phi (\sin \lambda + \sin \phi) \right\} + \text{cotang. } \beta' M^2,$$

we shall have $\alpha = \alpha' + M \delta + N \delta^2$, and $\beta = \beta' - M \delta + N' \delta^2$.

Example. Let $a = 3496740$, $b = 3477210$, $\lambda = 49^\circ 40'$, $\phi = 50^\circ 0'$, and $\omega = 30'$. (See the Account of the Trigonometrical Survey, &c. Vol. 1, Page 158). Then the two colatitudes, and the included angle $30'$, will give the spherical angles $\alpha', \beta', 43^\circ 51' 48'' \cdot 3$, and $135^\circ 45' 16'' \cdot 2$ respectively. The remaining part of the calculation is as follows:

$2 \sin \alpha'^2$	- Log.	9.98242	-	$\sin \lambda (\sin \lambda + \sin \phi) - \frac{1}{2}$	0.665036	
Cof. λ	- -	Log.	9.81106	- -	Cotang. α' . M - - - 0.432644	
Cof. $\frac{\lambda + \phi}{2}$	Log.	9.80957	- -	Cotang. β' . M - - -	0.426906	
Sec. ϕ	- -	Log.	0.19193		_____	
$\phi - \lambda$	- - -	Log.	1.30103	N - Log.	9.65937 - N' Log. 9.65937	
ω cour.	- Log.	8.52288		206264'',8	- Log.	5.31443
		_____		δ^2	- Log.	5.49408
M	- - - -	Log.	9.61889		_____	
206264'',8	Log.	5.31443		M' δ^2	- Log.	0.46788 - 2'',937
δ	- - - -	Log.	7.74704		_____	

$$M''\delta - - - \text{Log. } 2.68036 - 479'',025$$

$$+ 2,937$$

$$481'',962 = 8' 1'',962 = \alpha - \alpha' = \beta' - \beta.$$

Therefore $\alpha = 43^\circ 59' 50'',262$, and $\beta = 135^\circ 37' 14'',238$.

Mr. Dalby makes $\alpha = 43 \ 59 \ 51, 55$, and $\beta = 135 \ 37 \ 12, 95$.

I have already remarked, that the sum of the horizontal angles on the sphere and spheroid are very nearly equal, and that they would be perfectly so, if we were permitted to reject the terms of the formulæ involving the powers of δ higher than the first. We shall now, by retaining the square of δ , ascertain the probable error of this theorem.

To ascertain the probable error of the theorem; that the sum of the horiz. angles on the sphere and spheroid are nearly equal.

We have then by the formulæ, $\alpha + \beta = \alpha' + \beta' + (N + N')\delta^2$.

Now $N + N'$ is equal to $M \left\{ \sin \lambda^2 - \sin \phi^2 + \frac{\sin (\alpha' + \beta') M}{\sin \alpha' \sin \beta'} \right\}$

$= M (\sin \lambda - \sin \phi) \cdot \left\{ \sin \lambda + \sin \phi - \frac{2 \sin (\alpha' + \beta')}{\sin \omega} \right\}$; but in

all the cases that occur in practice, λ and ϕ are nearly equal, and the sum of α', β' differs little from two right angles, wherefore the $\sin \lambda^2 - \sin \phi^2$, and $\frac{\sin (\alpha' + \beta') M}{\sin \alpha' \sin \beta'}$ must be small, and the sum of these is not only to be multiplied by M , which is also small, but by δ^2 , which is about $\frac{1}{90000}$. So that $(N + N')\delta^2$ is insensible, and therefore $\alpha + \beta = \alpha' + \beta'$.

When ϕ is nearly equal to λ , the formula may be considerably

ably simplified. For then $M = 2 \sin \alpha'^2 \cos. \lambda \times \frac{\phi - \lambda}{\omega}$, and $N = M (2 \sin \lambda^2 - \frac{1}{2}) + \cotang. \alpha' M^2$; and if $\alpha = 90^\circ$ feré, M is $= 2 \cos. \lambda + \frac{\phi - \lambda}{\omega}$, and $N = M (2 \sin \lambda^2 - \frac{1}{2})$.

The particular states of the data, when the term involving the second power of δ is rigorously equal to nothing, may be thus determined:

In the first place, if $\delta = 0$, or the eccentricity of the spheroid be infinitely small, α, α' , and β, β' , are exactly equal to each other.

Secondly, when $M = 0$, N is $= 0$, but M is $= 0$ when $\phi = \lambda$, or the sine $\alpha' = 0$, that is when the triangle is isofceles, or the directions of the places due north or south of each other.

It is almost rigorously exact.

It appears then that this property of spheroidal triangles, first advanced by Mr. Dalby, and objected to by Mr. Playfair, is almost rigorously exact; and it might easily be shewn, that its application will never occasion any material error, even in the most unfavourable case that can be proposed. And it is not merely an elegant and curious theorem, but is highly valuable, as affording a method of determining the longitudes of places from terrestrial measurements, almost independent of all hypothesis. For whether the earth be an exact ellipsoid or not, any small portion of its surface may certainly, without error, be considered as pertaining to one of small eccentricity, which supposition is all that is necessary for demonstrating the theorem.

Determination of the eccentricity at place of obs.

Our solution also affords an easy method of determining the eccentricity at the place of observation. For if we have the latitudes and difference of longitude given, we shall also have the horizontal angles on the sphere. But from observations of the pole star, we may find the horizontal angles on the spheroid, and consequently the difference between them; but this difference is equal to a certain function of δ in our solution, whence we shall have an equation, from which δ may be determined. Thus if α be the observed horizontal angle on the spheroid, and α' the computed one on the sphere, we have $M \delta^2 + N \delta^2 = \alpha - \alpha'$. Now if we reject the term $N \delta^2$ as insensible, we obtain a near value of $\delta = \frac{\alpha - \alpha'}{M}$, which substituted for δ in

$M + \delta$, gives $\delta = \frac{\alpha - \alpha'}{M + \frac{\alpha - \alpha'}{M}}$ very nearly. Here a question

naturally arises; *viz.* To determine the value of α' such that δ may be obtained in this manner with the highest degree of accuracy, of which the method is susceptible.

This will evidently be the case when M is about its maximum value. So that if we put the differential of $M = 0$, and substitute the value of the differential of α' resulting from the properties of the spherical triangle in this equation, we shall have the required value of the horizontal angle. Let us suppose λ and

ω given, and there will result $\sin \alpha'^2 \times \frac{\sin \phi - \sin \lambda}{\text{cof. } \phi}$ a maxi-

mum; whence $2 \cotang. \alpha' \text{ cof. } \phi (\sin \phi - \sin \lambda) d\alpha' + (1 - \sin \lambda \sin \phi) \times d\phi = 0$. But by spherics,

$$\text{Cotang. } \alpha' = \frac{\text{cof. } \lambda \text{ tang. } \phi - \sin \lambda \text{ cof. } \omega}{\sin \omega},$$

and $d\alpha' = -\frac{\sin \alpha'^2 \text{ cof. } \lambda}{\text{cof. } \phi^2 \sin \omega} \times d\phi$; wherefore by substitution,

$$2 \sin \alpha' \text{ cof. } \alpha' = \frac{\text{cof. } \phi (1 - \sin \lambda \sin \phi)}{\text{cof. } \lambda} \times \frac{\sin \omega}{\sin \phi - \sin \lambda}.$$

Also when ω and $\phi - \lambda$ are small, the $\sin \omega = \frac{\text{tang. } \alpha'}{\text{cof. } \phi}$.

$\left\{ \sin (\phi - \lambda) + 2 \sin \lambda \times \text{cof. } \phi \sin \frac{1}{2} \omega^2 \right\}$, and $\sin \phi - \sin \lambda$

$= 2 \text{ cof. } \frac{\phi + \lambda}{2} \times \sin \frac{\phi - \lambda}{2}$; consequently $2 \text{ cof. } \alpha'^2 =$

$$\frac{(1 - \sin \phi \sin \lambda)}{2 \text{ cof. } \lambda \text{ cof. } \left(\frac{\lambda + \phi}{2} \right)} \times \frac{\sin (\phi - \lambda) + 2 \sin \lambda \text{ cof. } \phi \sin \frac{1}{2} \omega^2}{2 \sin \left(\frac{\phi - \lambda}{2} \right)},$$

and $2 \text{ cof. } \alpha'^2 = 1$ if we suppose $\phi = \lambda$ nearly, and $\sin \frac{1}{2} \omega^2$ very small.

When the horizontal angle α' , therefore, is equal to 45° or 135° , the observations will be nearly in their most favourable state for determining the compression by means of our theorem.

We might illustrate this method by examples taken from the trigonometrical survey of Great Britain, but on reference to it I have found fewer complete sets of observations than might be expected, and such as are complete in every respect, are not well calculated for this purpose, the horizontal angles being

ing nearly right ones. The observations at Beachy Head and Dunnose give $\frac{1}{148,2}$ for the compression; but it must be remarked, that the state of the data is very unfavourable in this example.

The rules which our solution gives for computing the horizontal angles from the latitudes and difference of longitude, will be found, I apprehend, much shorter than Mr. Dalby's, besides the advantage they possess of affording us the means of ascertaining the figure of the earth by a very simple process, from observations made with the same instruments and by the same observers.

The preceding rules applicable to an ellipsoid; but the managers of the trigonom. survey avoid this assumption.

The theorems we have been detailing, with some others which may perhaps form the subject of another letter, would give us the relative position of one place to another on the surface of the earth, were its figure an ellipsoid of known dimensions; but as this is still considered as problematical, the method adopted by the gentlemen who have so ably conducted the survey of our island, is certainly preferable. They first obtain the length of a degree upon the meridian, and its perpendicular in a given latitude, and employ these data for computing the geographical situations of all the places near that parallel, and not far distant from a known meridian. In the smaller triangles the truth may be thus obtained to the fraction of a second, and in the larger ones they have very successfully employed the beautiful property of spheroidal triangles, which we have so often mentioned.

How to apply the rules.

But though we give the preference to their method of computation, I conceive the preceding rules will be found equally accurate, if we make use of the values of c , a , and δ deduced from their observations; or if we assume near values of them, and note the agreement or disagreement of the computations with observations made at a place considerably distant from the first station. We may thus ascertain nearly the error of our suppositions, and then correct the intermediate stations. This cautious method of proceeding is rendered necessary by the anomalies which have been discovered in the measures of degrees in different latitudes, as well as by the general rule, which ought to be our guide in all philosophical inquiries, to frame as few hypotheses as possible, but to make accurate experiments, and infer the truth from them by fair and genuine induction.

I mean

I mean not, however, to support the opinion, that the earth is not an ellipsoid; but, on the contrary, should be very loth to be obliged to give up an hypothesis, which is so beautiful in theory, and has stood its ground so long. Many of our objections to it may very probably arise from errors in observations, or from other causes which have not yet been fully examined. The remarks of Mr. Playfair on this subject are very ingenious, and I hope will be confirmed by the phenomena: but if not, I am convinced we have not the plea of inaccuracy to set up in this instance. One of the strongest objections, however, has been lately done away. The degree of the earth measured in Lapland in the year 1736, has been found, by some Swedish gentlemen sent there for that purpose, to err in excess by no less than 208 toises. Now if we advert to the number and character of the astronomers who originally measured this degree, it will be difficult for us to set limits to the errors of other observers. Perhaps the anomaly in the degree at the Cape of Good Hope arises from the same cause.

The earth probably an ellipsoid;

particularly as the error of the Lapland degree removes much of the doubts entertained on this subject.

Fortunately, however, the great improvements, which have since been made in the instruments for astronomical observations and geodetical mensuration, afford us the means of bringing the probable errors of observation within very narrow limits. We may thus obtain a number of measures in different latitudes of equal accuracy, and by comparing them together, the question about the earth's figure may be put beyond a doubt. If this comparison shall be found to give different ellipses, we shall then be fully warranted in rejecting the hypothesis entirely and for ever. But till this is done, we may be allowed to adopt an hypothesis, which is so simple, so good in theory, and supported by so many strong arguments and accurate observations.

Late improvements will clear up this matter.

We have already remarked, that the degree lately measured in the Mysores, compared with that in France and England, gives $\frac{1}{300}$ for the compression at the poles: the corrected degree in Lapland gives $\frac{1}{313}$, and that measured in Peru, $\frac{1}{314}$. There is a considerable difference between the compressions deduced from other measures, but the mean falls between these limits. From the best observations of the length of the pendulum that swings seconds in different latitudes, the same conclusion is also drawn; the second pendulum near the pole compared

Mean compression from measurement;

and from the pendulum;

compared with that at the equator, gives $\frac{1}{304}$ for the compression. We may therefore assume $\frac{1}{303}$ as being very nearly its true value.

from celestial irregularities ;

It is a curious circumstance to find the figure of the earth, deduced from the measurement of lines and angles on its surface, confirmed (perhaps corrected), by observations of the stars and planetary bodies in the heavens, combined with the theory of universal gravitation. But such is certainly the case.

e. g. of the moon.

Among others may be mentioned two small inequalities in the moon's motion, which the industry of modern mathematicians have unfolded. One of them was first taken notice of by Mayer, and fixed by Maſon at $7''\cdot7$, but was neglected by astronomers, as it did not sufficiently appear that such an equation should arise from the theory, till Laplace traced it to the oblateness of the earth's figure. Its argument is the longitude of the moon's node, and its value has been found by Burg, from the observations of Dr. Maskelyne, to be equal to $6''\cdot8$, which answers to a compression of $\frac{1}{303,03}$. There is also another inequality of the moon's motion in latitude, which depends on the sine of the true longitude, and results from a nutation in the lunar orbit, produced by the action of the terrestrial spheroid. Burg has also determined the coefficient of this inequality, from a great number of observations, to be equal to $8''\cdot0$, which results from a compression of $\frac{1}{304,7}$.

Remarks upon Newton's discovery and investigation of this subject.

The precession of the equinoxes, and the nutation of the earth's axis, were discovered by Newton to arise from the oblateness of the earth's figure. This famous problem is acknowledged to be one of the most abstruse in physical astronomy, and its complete solution requires the utmost resources of the modern analysis. The compression thence arising is equal to $\frac{1}{304}$, agreeing exactly with the results from the two lunar inequalities, the lengths of the second pendulum, and the best measurements on the earth's surface. It is well known that Newton failed in attempting to solve this problem, and some French mathematicians have been disposed to pride themselves on being the first to detect it. It ought however to be remembered, for the honour of that great man, that his mistake did not arise from any error in principle, but from his taking for granted, without demonstration, a circumstance which appears highly probable, but is really erroneous. He seems to

have

have first made this assumption in order to shorten his solution, and on finding the calculations to agree with observation, to have never after returned to the subject. The mathematicians of his time were unwilling or unable to follow him, and the question remained as it came from his hands, till the middle of the last century. Had doubts arisen and objections been started, the genius of Newton might have been once more roused to action, and continued to enlighten the sciences to the last. But unfortunately no such incentive was given, and Newton stopped short in the career of his glory. The evening of his life was devoted to other studies; but however usefully he may have been employed, there are few who will not be inclined to lament that he ever laboured in the Mint or the Revelations.

Thus much I have thought it not entirely foreign to observe on one of the most remarkable effects of the oblateness of the earth's figure, and in justice to its immortal discoverer, the inventor of the modern analysis, the father of physical astronomy, and the preceptor of Europe.

I am, Sir,

Your most obedient servant,

Portsmouth,
May 6, 1804:

PEREGRINUS PROTEUS.

Corrections to be made in the first Letter.

In page 12, line 20, after *paper* insert *but*. In page 15, line 23, &c. divide the coefficient of the term $\frac{c}{a}$ by $\cos. \lambda \cos. \phi$; also in page 16, lines 3 and 12. In page 16, line 9, *dele* and $\cos. \lambda \cos. \phi$; and also in line 14. In page 17, line 6, &c. for $\cos. \omega$ read $\sin \omega$. In page 18, line 25, for Pc, Qc, Rc , read $P \frac{c}{a}, Q \frac{c}{a}, R \frac{c}{a}$.

IV.

*Description of a striking Part for a Clock, in which the Intervals between Stroke and Stroke are not regulated by a Fly, but by a Pendulum. By Mr. EDWARD MASSEY.**

SIR,

New striking
part for a clock.

HAVING for a number of years considered that a method of striking a clock, at certain regular intervals, might be of great service in making observations on the heavens, and ascertaining the velocity of sound, &c. I beg leave to lay before the Society for the Encouragement of Arts, &c. a striking part of an eight-day clock, which I have no doubt will answer the purpose intended; and if, upon examination, the Society should be of opinion that it may be useful, I trust they will reward it according to its merit. They will find that the work of this model is less than that of the common striking movements, and may be made by a common workman, with less expence and trouble; the weight required is also considerably less. The principle I act upon is the pendulum, by which I regulate the stroke, instead of the fly; the advantage of which must be obvious to every one. The machine consists of a toothed wheel A, one pinion B, a pin wheel C, pallets DD, pendulum E, and locking detent G. The hammer-work F is as usual, and strikes on the bell at H. The weight hangs to the cord I. See Plate XII. Fig. 1 and 2, where a front and side view of the machinery are given, and where similar letters denote the same parts in each view.

Description of the Machinery.

I consider it is only necessary for me to give the description of the wheels, so as to be a direction to a mechanic, who wishes to manufacture clocks on this principle. The main-wheel A, with seventy-eight teeth, is to act in a pinion of eight leaves, B. The pin wheel C should be large, so that the pins on which the pallets D and the locking G act, may be flung as far from the center as possible, which pins may be eight or sixteen in

* From the Transactions of the Society of Arts, 1803. This invention was honoured with a reward of 20 guineas.

number. If eight, the pendulum E should be about nine inches long, and it will vibrate twice betwixt each blow of the hammer; but if sixteen pins are put in the wheel, the pendulum must be about three inches long, and will make four vibrations betwixt each blow. The pins for drawing the hammer must be eight in number, and be fixed in a circle of about half the diameter of the aforesaid pins. The locking-plate is on the main wheel. The stop is against the pins on which the pallets act, and may be discharged by a flirt piece.

New striking
part for a clock.

As I have described the model, I beg leave to point out the method of striking a clock by the common pendulum; true seconds, without any additional pendulum or pallets for the striking part.

Description.

Fix a cantrite wheel with sixty teeth on the same arbor with a swing wheel of thirty teeth. Now suppose a striking part to be made in the common way of making an eight-day clock, so far as the pallet pinion, leaving out the warning and fly pinions. A crank piece must be fixed on the pallet pinion, which must come into contact with the cantrite wheel, which is fixed on the swing wheel arbor. Then suppose the clock to be set a going, and the rack discharged, the pallet pinion will make a revolution on every vibration of the pendulum, by which means a clock will strike seconds as true as a pendulum vibrates, which I hope will be considered as useful for the purposes I have described. I also beg leave to observe; that a great advantage arises in both the above machines, from their not being liable to foul, as the stroke is given by the certain and regular vibration, instead of the uncertain motion of the fly. Its advantage likewise depends on the cleanness of the work; and church clocks will be much benefited from the decrease of weight.

I am, Sir,

Your most obedient servant,

EDWARD MASSEY.

Charles Taylor, Esq.

Hanley, in Staffordshire,

Jan. 12, 1803.

V.

Description of a very simple Télégraph, consisting of the Human Figure adapted to converse at a Distance by means of Signs.*

Preliminary observations.

AMONG the great advantages of which those who do not enjoy sight are deprived, we may reckon telegraphy, which it would be difficult to supply by another sense. Such an art for the use of the blind would doubtless be very imperfect. On the contrary, the advantages which it affords to those who enjoy light, appear to me to be so important, that it ought to be rendered more general, and brought within the reach of all men. For this purpose I have offered some notions on the subject. I do not pretend either to be the first, or the only one who has entertained them: I even believe the contrary; and, in this instance, they will have that in common with telegraphy in general, which, though new in its execution, was not so in its invention †.

* From a small pamphlet in French, extracted from *Mémoires sur les Aveugles, &c.*

† It is not extraordinary that the same invention should have been thought of by several persons. Thus the notion of telegraphy may be found in the preface to one of the German works of the celebrated Chr. Louis Hoffman, a native of Rheda, and physician to the Elector of Mentz, which, however, does not lessen the merit of the French inventor. A description of the telegraph invented and executed by Citizen Chappe, is found in the interesting work of Mr. Meyer, intitled, *Fragmente aus Paris, im. IV ten Jahr der Französischen Republik; Hamburg, 1797.* The author, who was in the telegraph office at the Louvre with Cit. Chappe, affirms, that the latter had made his discovery before the revolution, that he communicated it to the National Assembly in 1792, and that the Convention, on the report of Lakanal, decreed, July 25, 1793, the establishment of a telegraphic correspondence, under the direction of Citizen Chappe, as telegraphic engineer. A notion may be formed of the rapidity of the telegraphic correspondence, by the following example, of which Mr. Meyer was a witness. He says, that during his presence in the office at the Louvre, and at the appointed hour in the evening, enquiry was made of the office at Lisle, by a single signal, if any thing new had happened in the army of the north, and the answer, no, was received in 88 seconds.

Perfused of the importance of communicating our ideas at distances too great for the voice or hearing, I have employed myself in enquiries for a telegraph which should be at once cheap and sufficiently perfect to be easily used. I believe I have discovered it; nature herself has given it to all the world.

This telegraph is the human body, its branches are the arms, which, with each other and with the perpendicular line of the trunk, may form a great number of figures, sufficiently distinct to be readily seen, at considerable distances, by simple vision or by the assistance of a telescope. It would certainly be very agreeable for two friends, living opposite to each other in an extensive place or on the banks of a large river, to be able to converse together. Of what utility might it be to the inhabitants of open countries to have a method of communication which is at once speedy and requires no expence!

I hope, therefore, that the generality of readers will see with pleasure that a method of communication is opened to them, susceptible of being varied and brought to perfection by themselves. In the annexed *Plate X.* are found the signs for all the characters in the alphabet, for the figures, and for the punctuation. To simplify the writing of these signs, the perpendicular and immoveable line, which represents the trunk of the body, may be omitted, as I have done, only indicating it in some signs or characters by a point, as in the *e* and *u*; and to write them with more rapidity, they may perhaps be joined in the manner of short-hand writers.

Three different positions of the right arm, and as many of the other, form the signs for the vowels. The right arm stretched out, and a little raised, forming an angle of about 45° with the line of the trunk, gives the sign which expresses *a*; the same arm extended and more elevated, or horizontal, forming a right angle with the trunk, gives *e*; more elevated, and forming an obtuse angle of about 135° , it furnishes *i*. The left arm extended, and forming an angle of about 45° with the trunk, gives us the sign for *o*; more elevated, or horizontal, it signifies *u*; still more elevated, and forming an angle of about 135° , it gives *y*.

It may be observed, were it only to assist the memory, that *a* and *o*, whose sounds have some resemblance in several words, are indicated by the same sign, and likewise *i* and *y*, which in the French language often express but a single sound; with this

Telegraphic
signs by the hu-
man figure.

this difference, however, that the signs for *a* and *i* are formed by the right arm, and those for *o* and *y* by the left arm. The two other vowels, *e* and *u*, are likewise indicated by the same sign, but with the same difference.

To form *b*, both arms describe an angle of 45° . To form *c* and *d*, the right arm is kept in the same position, and the left arm is raised to the height of 90° for the first, and of 135° for the latter.

To form the letters *f*, *g*, and *h*, the right arm is extended horizontally, and the three different angles of 45° , 90° , and 135° , are formed with the left arm.

To express *j*, *k* or *q*, and *l*, the right arm is raised to the height of 135° , and the same positions are repeated with the left arm as were employed for the six preceding consonants.

To form *m*, the upper part of the right arm is placed in a horizontal line, and the fore-arm is raised at the same time, so as to form a right angle with this part. To designate *n*, the left arm is placed in the same position. To form *p*, both arms describe the preceding figure at the same time.

To express the letters *r*, *s*, *t*, the upper part of the right arm is placed in an horizontal line, and a right angle is formed with the fore-arm, while the extended left arm successively describes the three different angles of 45° , 90° , and 135° .

For *v*, *x*, and *z*, the same is done with the left arm as was done, for the three former letters, by the right, which then forms the three angles of 45° , 90° , and 135° .

If it is intended to shew that a telegraphic signal is terminated, the two arms are withdrawn, so as to form but one line with the rest of the body.

The right arm put at rest, so that the hand is supported by the hip, indicates the termination of a word.

The left hand placed in the same position, is the sign of a comma, (,).

The two arms put in this position, signify a point and a comma, (;).

By putting the right arm into this position, and at the same time making the sign of *n* with the left hand, two points are indicated, (:).

By resting the left arm on the hip, and making the sign of *n* with the right arm, a point is indicated, (.)

By

By holding the right arm in such a position that the hand shall be above the head, or touch it, the point of interrogation is made, (?). Telegraphic signs by the human figure.

The same motion performed with the left arm, furnishes a sign for the point of admiration, (!).

The three signs of the termination of a word, of a comma, and of the point of interrogation, each combined with the three different angles made with the other arm, form the nine figures.

By putting the right arm at rest on the hip, and forming the three angles of 45° , 90° , and 135° , with the extended left arm, the signs are given for the figures 1, 2, and 3.

By resting the left arm on the hip, and making the same three angles with the right arm, the figures 4, 5, and 6, are formed.

By bringing the right arm to the sign of the point of interrogation, and repeating the three angles with the left arm, the figures 7, 8, and 9, are formed.

To indicate zero, (0), the two arms are raised so that the hands shall be above the head, or touch it.

To express 10, the signs of 1 and 0 are made without making either the sign of the termination of a word, or of a number, or that of a comma, between these two signs. To express 11, the sign of 1 is twice made, with the assistance of the sign for the termination of a character, or of a figure. To indicate 12, the signs of 1 and 2 are made, and so on: 10, 11, and 12 may be expressed at pleasure, each by a single sign, by placing the left arm in the position of the point of admiration, and by successively forming with the right arm the three angles of 45° , 90° , and 135° . It would not be difficult to devise a great number of signs to express 13, 14, 15, and many other numbers, each by a single sign.

If it be intended to make use of this natural telegraph at the distance of one, or even of several leagues, or to do without a telescope at less considerable distances, it is only necessary to add to each natural arm an artificial one, that is to say, an oblong frame, of the breadth of a foot or thereabout, and of the length of one or more yards. This frame must be covered with oiled silk, or any other stuff of a dark colour, and must be provided with a handle to hold and direct it. Our telegraph would here no longer be called *natural*, but it is still among the Mechanical addition for great distances.

the most simple of the artificial. I am of opinion that this kind of telegraph may be employed with very great advantage: it is simple and cheap; it may be used in all directions, and is removed with ease. In this latter point of view, it would doubtless be well calculated for the formation of moveable telegraphic lines, or what might be called a flying telegraphy, which in my opinion would be very useful in war, to keep up a quick and constant communication between the different bodies, and with the fixed and common telegraphic lines. A machine of easy carriage, and calculated to raise a man to a considerable height, would, no doubt, contribute to render the flying telegraphy more perfect. The telegraphs with the frames may be used by night as well as by day, by adding lanterns to them, as in the common telegraphs. One must be placed in the middle, that is to say, on the breast of the person who forms the telegraphic signals with his arms and the frames, one at the handle, and one at the extremity of each frame. To prevent the person from being affected by the vapours of the lantern in the middle, it should be placed at a small distance before him; and when the movements are intended to be visible on both sides, it would be still better to place this lantern on the head, or to put one on each shoulder.

May be used by
night as well as
by day.

Perhaps this kind of telegraph might be advantageously employed to establish immoveable telegraphic lines, in every case in which the telegraphs employed at present would be too expensive. The same telegraph might frequently serve for several lines in different directions. By means of this telegraph a communication might be maintained, with little cost, even in small districts, or in the provinces and departments, with the capital or the chief place, and the other considerable points. Of what importance may it be to be able to indicate instantaneously conflagrations, inundations, or other events which require speedy succour.

It would be well to place the telegraph, or the person who makes the telegraphic signals, on a kind of stool, to facilitate his movements. Placed in this manner, with his arms and the frames he could make a prodigious number of figures, sufficiently distinct not to be confounded with each other.

VI.

Account of a Fact respecting Water heated in a Boiler of Stone; with Observations. By Mr. J. C. HORNBLLOWER.

To Mr. NICHOLSON.

DEAR SIR,

THOUGH it must be admitted by every one who has attended to the critique contained in your note on the experiments of Count Rumford, in your first volume * of the Philosophical Journal, concerning the non-conducting power of fluids with regard to heat, that it is an erroneous conclusion, and which several experiments, particularly that of Mr. Murray, have since confirmed; yet I cannot be quiet without informing the gentlemen who have so constantly opposed the Count's doctrine, of a circumstance which accident brought forth on a very large scale, about forty years ago, in the county of Cornwall.

Fluids not strictly non-conductors.

Incident.

It had long been a desideratum in the mining interest in that county, to reduce the consumption of fuel in draining the mines by the means of steam engines, and every expedient that carried any tolerable face of probability was brought to a trial.

Attempt to reduce the consumption of fuel in working steam engines.

Among the rest it was suggested, that as, in the several operations of smelting the produce of the mines, much heat must be carried off, from the intense fires of their furnaces, which might be employed to some purposes requiring but a subordinate temperature; a resolution was formed by a company of gentlemen, with Mr. John Weston at the head of it (to whom mining was an ungovernable hobby-horse), to erect an engine on a copper mine in the parish of Camborne, and to put up a set of furnaces so attached to the engine that they might avail themselves of this superabundant heat to raise the steam.

Trial to work by the smelting heat.

To effect this they had their engine-boiler made of masonry, of what is called in Cornwall *moor-stone*, well wrought and put together with Aberthaw lime, which has the property of setting

A stone boiler connected with water-lime, &c. heated by copper tubes.

* I. Page 291, Quarto Series.

like

like the Dutch trafs *; and to convey the heat, two or three copper tubes were placed in it from end to end, and the furnaces connected to one end of the boiler, and the engine at the other; which, though it was a prodigal way of saving heat, yet it was competent enough to raise steam for the use of the engine in certain cases.

When the water boiled, a cock near the bottom gave water, but little heated.

At the bottom of the boiler was placed a cock, as usual, to tap it as occasion required, either when it was necessary to clean it, or in case it should be over-charged with water, which I think was the case which brought the following fact to light; namely, that when the fire had been lighted, and the heat had to circulate through all this mass of stone-work, and the water brought to boil, and the engine had been at work for the first time, which is usually attended with many unforeseen delays and hindrances, the cock being turned, the water was not hotter than to admit the hand without any painful sensation of heat. Many deep and grave hypotheses were formed on this extraordinary discovery, which, for the sake of the profession at this advanced state of science, I must forbear to mention. But at that period many a valuable truth had come and gone like a worthless mendicant, whom few regarded, and whom none would receive.

The tubes were near the surface, and the vessel a bad conductor.

The most striking circumstances in the detail of this discovery are, that the temperature downward (for the tubes were near the surface of the water) must be greatly accelerated by the conducting power of the sides of the boiler, and the agitation of the water in a state of ebullition: notwithstanding all, it must have occupied four or five hours to bring the lower part of the water to the temperature of 90°, or perhaps 100°; and no doubt can be entertained but that they deemed it a matter of no consequence as to the place of the tubes, so long as they were covered with water; believing with every body else, that the whole mass would attain the boiling point at the same time.

Hail the man who throws if but one grain of truth into the scale against popular prejudice or vulgar error; and notwithstanding the Count did not obtain the extreme of precision in this particular instance, he has laid the foundation for all the subsequent experiments which have gone to confirm or reject

* This lime is found on the beach on some part of South Wales.

the position he advanced. This I may be permitted to observe, without the imputation of detraction from those whose labours and talents demand my respect.

I am, Sir,

Your very obedient servant,

J. C. HORNBLOWER.

City Road,

June 11, 1804.

VII.

Facts and Observations tending to elucidate the Theory of Galvanism. By A CORRESPONDENT.

THE pile of Volta has disclosed many new properties which were not known to belong to the two powers of the electric fluid, for by its operation, two sets of chemical phenomena are produced, differing essentially from each other, thereby determining the real or positive nature of each power. It has introduced electricity as a chemical agent, and has shewn us that it acts as such in its two-fold character very generally throughout nature. Electricity has hitherto been considered mostly as an adventitious power excited upon the surfaces of bodies, it is now known to attach itself to, and to enter into combination with various substances.

General effects
and theory of
galvanism.

If the spinal marrow of a frog be coated with tin foil, and the muscles of the lower extremities be laid bare, if the frog, thus prepared be drawn through water, at the bottom of which there is a piece of silver, when the tin foil touches the silver, the limbs of the frog are thrown into strong convulsions. If the tin foil be kept steadily at the first place of contact, the convulsions soon cease; but if it be removed to the smallest distance from where it first touched the silver, and is again in contact with it, the convulsions are renewed with their original violence. The same thing happens when the silver is connected with the muscles by means of a metal, though that metal touch the muscle in a point, as when the water acts as the conducting arch.

These experiments shew that there is a power in each assignable particle of the metal which excites the action of the muscles, and that nothing but the contact of the metals seems necessary for the production of the effect.

It

General effects
and theory of
galvanism.

If the power which excites the motion of the muscles be electricity, it is in a state not to be conducted from one particle to another, for the same effect is renewed whenever the tin-foil comes in contact with another part of the silver, however contiguous it be to the one at first touched. We find farther that the superficial particle does not take off the power of the one immediately beneath.

It is thus, I apprehend, that the renewal of surfaces gives a permanency to the powers of the pile, and in proportion to the quickness of succession in the presenting new surfaces, the power of the pile is found to increase.

It is needless to explain these appearances by the hypothetical principle of Mr. Lavoisier's oxygen, since Mr. Volta has shewn that the metals themselves are electro-motors, even without the intervention of water, or the action of acids or alkalis. Taking it for granted then that the animal muscles are a species of electrometer, we may conclude that some disturbance of electricity takes place when two metals are brought in contact, and a circuit is made. This disturbance varies according to the substances employed. Mr. Volta has found this to be the case, for he has rendered this small portion of electricity perceptible by accumulating it on his condenser. The pile of Volta accumulates the power which is called forth by the contact of two metals, and enables us to apply it in such accumulated state in various chemical experiments.

One of the most striking effects is the disengagement of two elastic aeriform fluids from water, by introducing wires, from which are produced the two states of the electric fluid. Thus it happens that the wire connected with the zinc produces, when immersed in water, one gas, and the wire connected with the silver or copper plate another gas. The properties of these gases are perfectly distinct, and evince that the producing powers have peculiar effects, for the water is common to both. What takes place from the wires in water at the extremities of the pile is more or less apparent between the plates themselves, where wetted paper is interposed, for there the action of the gases is to be seen in their effects on the metallic plates. The effect produced by the pile may be greatly increased by arranging it in the following order; silver, wet paper, zinc, a plate of glass, &c. and by placing wires on each side of the glass, and putting those into water; from each

pair of wires the two airs will be produced. Might not Mr. Wilkinfon's experiment of burning charcoal be in this manner greatly increased in splendour? General effects
and theory of
galvanism.

In the prosecution of the present inquiry, I shall especially notice the two different effects produced in water by the wires of Volta's pile. It is known that these two wires throw out two powers similar to what are called positive and negative electricity; and as water is common to both, I am led to conclude that the two gases are produced in one instance by the union of one of these powers with water, and in the other by the other power and water, and to consider each gas as thus compounded, because we have no evidence of any other power, principle or substance being accessory to the production of either of the gases. Volta's pile causes combustion in atmospheric air, and therefore vital air is absorbed; it causes another species of combustion in vacuo, because the two principles of fire are supplied by it. Volta's pile does not act in vacuo, because the resistance necessary to accumulate the powers is withdrawn in the same manner as the Leyden jar does not charge in vacuo.

If the gas which is produced from one of the wires communicating with the pile in water be united and inflamed with the other in a just proportion, the water which is common to both is re-produced, and common fire is generated in great abundance; now as we have had no evidence of the presence of fire till this point of time, does it not appear that these two principles which are thrown out from the wires of the pile, are by their union transformed into ordinary fire, and does it not appear in this experiment that ordinary fire is generated by and compounded of these two powers?

Lavoisier has triumphed over the friends of Phlogiston. He overturned a system which was built upon the assumption of a principle, upon a mere name of hypothetical entity, endued with attributed or imaginary properties. But in the place of the system he destroyed, he has erected one of his own. Has he not built upon the same weak foundation with his late adversaries? Does not his system rest altogether upon the assumed distinct existence of things, the distinct existence of which cannot in any way be evinced? Do not all his solutions of chemical phenomena depend on such assumption? May not all these phenomena be explained with equal ease, nay more simply, by referring them to the action of known agents?

General effects
and theory of
galvanism.

If we examine Mr. Lavoisier's theory of combustion, we shall find that it depends upon the assumption of a principle whose existence has never been proved by direct experiment. It is the union of the basis of oxygen gas with the combustible body which in every instance is the cause of the combustion. We have seen that this gas is produced by positive electricity and water, and we conclude that the ponderable part of this gas is water, because we can perceive the agency of no other principle or substance. The simple condensation of oxygen gas, according to Mr. Lavoisier, occasions the evolution of heat and light. But little heat is excited by the mechanical condensation of this gas; but we find that when a chemical union takes place between a combustible and the gas, a great portion of heat and light is disengaged.

The accension of charcoal and metallic bodies by the galvanic battery, even in vacuo, seems to shew that the electricities thus produced are the peculiar agents in the phenomena of combustion. They seem not only to be the excitors of combustion, but they furnish with water those gases by which combustion has been conceived to be upheld.

The theory of the Lavoisierian school, of the combustion of hydrogen gas, in conjunction with oxygen gas, is as follows: "When both gases are mixed at a lower temperature than that of ignition or red heat, the attraction of their respective bases to the caloric is, in that case, stronger than their attraction to each other, and therefore they are not decomposed. But in the heat of ignition, their bases, namely concrete oxygen and hydrogen, again attract each other more strongly, and hence they unite to produce water; and both the caloric and light by which they are retained in their aerial form, are again disengaged from them, and constitute the fire."

Now where is the evidence of the heat and light being necessary to the formation of these gases, when they are produced from the wires connected with the extremities of the pile, and placed in water? Also during the oxidation of a metal in the moist way, the hydrogen discharged is at first very hot, so as to heat immoderately the tubes used in collecting it. If the condensation of oxygen gas be the cause of the production of heat and light, how comes it that charcoal and nitre desflagrate together with the production of so much light and heat, although the compound formed, namely, carbonic acid gas, occupies so much

much more space than the substances from which it is formed? Does it not seem an assumption to say that the two conditions of oxygen in the aeriform state, and oxygen in the solid state, as in nitre, should hold the same quantity precisely of heat and light? This fact disproves the opinion that the production of heat and light is commensurate with the condensation that takes place in combustion.

General effects
and theory of
galvanism.

An established law of chemical affinity would be reversed, if oxygen, when in combination with another substance, were to have a greater, or even the same attraction for the matter of heat as it has when oxygen is simply combined with the matter of heat.

There are many instances of combustion, deflagration, &c. wherein the compounds formed seem to hold more heat in their composition after than before combustion. When gun-powder is inflamed, does not the production of steam seem to shew that a great quantity of heat is generated during the explosion. If the matter of heat was held to the particles of the component parts of gun-powder, in consequence of their great capacity for containing it, should we have any reason by analogy to suspect that the electric spark would entirely reverse the order of the existing attractions? Have we not evidence of the accumulation of one power of the pile in nitrous and muriatic salts, for in the formation of their acids we do not find combustion to be necessary, and there is no known expenditure of this their igneous principle. These are the salts which cause the combustion of inflammable bodies, and burn them, in consequence of their both containing igneous and inflammable principles. Thus does it appear that fire is generated during combustion.

From all the observations and experiments I have made on this subject, I conclude that water is still to be considered as a simple substance, that its two assumed component parts are non-entities, that fire is generated in every species of combustion, whether by acids or otherwise, and that heat is generated in the lungs during respiration. I take the liberty also to suggest, that the admission of the hypothetical substances of the Lavoisierian system may have retarded the progress of science, by diverting the mind from real objects. Analogies drawn from imaginary data must tend to perplex and confound, and thus do names arise without existing realities. Perhaps it
would

would be as proper to question the existence of azote, if oxygen and hydrogen were to fail, as it was to admit it, from the supposition of the existence of those two substances.

A CORRESPONDENT.

VIII.

*Experiments with the Electric Pile, by Mr. Ritter, of Jena.
Communicated by Mr. ORSTED.**

WHILE the great inventor of the electric pile was proving the identity of galvanism and electricity, many of the philosophers of Germany were busied in the same pursuit. On this subject the celebrated Ritter undertook a very extensive series of experiments, the results of which are so remarkable as to merit attention, even after the publication of the labours of the philosopher of Pavia.

To compare galvanism with electricity we must attend to the kind, chemical action, spark and shock.

1. Kind.
The pile has a positive and a negative pole: and the intensities decrease gradually from the extremities to the centre.

But the whole pile becomes negative, when the positive pole is made to communicate with the earth by means of a conductor, and *vice versa*.

Yet this change in the kind of electricity does not affect its chemical action.

A collateral fact is the increase of the action by adding a salt to the water without changing the kind of electricity.

In order to compare galvanism properly with electricity, four different phenomena are to be distinguished; the kind of electricity, chemical action, spark, and shock.

As to the kind of electricity, it is known to every person, that the pile has two electrical poles, one positive, the other negative. On a more attentive examination we discover, what was not difficult to foresee, that the respective intensities are strongest at the extremities of the pile, and regularly decrease from the extremity to the centre, where the intensity is at its minimum. But it was never yet suspected, that the whole pile becomes negatively electrified, when a communication is made between the positive pole and the earth, by means of some conducting substance; and on the contrary, the whole pile becomes positive; when the electricity is abstracted from the negative pole. We have here a phenomenon, that shews the theory of electricity to be still in its infancy: When the state of the pile is thus changed, its chemical action is not destroyed, but continues as before. This fact is perfectly consistent with the augmentation of the chemical action of the pile, on the addition of salts, acids, or alkalis, to their wet strata, in which case the kind of electricity remains constantly the same as when simple water was employed.

* *Journal de Physique*, December, 1803, Vol. LVII, p. 401.

The assertion of Volta, since repeated by Van Marum and Pfaff, that the electrical pile charges a jar or a battery instantaneously, requires some limitation.

With regard to common piles, the assertion is true: but if the pieces of pasteboard, instead of being thoroughly wetted, contain but very little moisture, that of the damp air for example, the action takes place much more slowly. At first, indeed, the action is still tolerably prompt, but in proportion as the pasteboards lose their moisture, the action becomes gradually slower; so that a pile of six hundred pairs of zinc and copper, used immediately after it was made, was ten or fifteen minutes charging a battery of thirty-six square feet to the same degree, to which it would have charged it instantaneously, had it been constructed with pieces of pasteboard thoroughly wetted. Each of the wet strata too may be composed of a piece of glass, armed on each side with wet pasteboard; and such a pile of six hundred pairs would require twelve hours to charge the battery to the same degree, as would be done by a common pile, with a solution of a salt, in an imperceptible space of time. The law of this retardation therefore is, that the action of a pile is more slow, in proportion as it is a worse conductor.

Ritter has made a great number of experiments in particular, which prove, that the state of the pile, *on all occasions*, obeys the same laws, as that produced by the electrical machine: but we cannot here enter into particulars, without exceeding the bounds allowable to an abstract.

It is a well known fact, that electricity produces the same change in water as galvanism. Ritter has shown, that positive electricity, like positive galvanism, disengages from it oxygen gas; and that negative electricity, like negative galvanism, disengages from it hydrogen.

Inquiries into the action of the pile on metals have taught him, that its negative pole disposes them to combine with the hydrogen of water, as the positive pole disposes them to combine with oxygen. The hidrogenation has different degrees with respect to the same metal, as well as the oxidation. Silver with a large quantity of hydrogen assumes the state of gas: with a smaller quantity it remains solid. He also found, that the electricity does not produce oxygenations and hydrogenations in the humid way alone, but in the dry way also. The oxida-

Volta, Van Marum, and Pfaff, say, that the pile charges a jar instantaneously.

This is true with the common pile; but if the pasteboards be little moistened, the jar will be charged more slowly.

Such a pile of 600 pairs was 10 or 15 minutes charging a battery of 36 feet as high as it would have done instantaneously with thoroughly wet pasteboard. By interposing glass armed with wet pasteboard between each pair of metals, the time would be protracted to twelve hours.

The action of the pile therefore is quick in proportion to its conducting power.

Ritter has proved by many experiments that the electricity of the pile obeys the same laws with respect to kind as that produced by the machine.

2. Chemical action.

Galvanism and electricity produce the same change in water; each in the positive state extricates from it oxygen; in the negatives, hydrogen.

The negative pole of the pile disposes metals to

combine with the hydrogen of water; the positive with its oxygen.

The hydrogenation of metals has different degrees, as well as their oxygenation.

Silver highly hydrogenated becomes a gas: less so, remains solid.

Galvanism hydrogenates and oxygenates metals in the dry way also.

If the positive pole of the pile be armed with gold leaf, and the negative with a bit of charcoal, on making the contact, the gold leaf will burn with a bright flame, and the charcoal remain unaltered: if the situation of the two be reversed, the charcoal will burn, and the gold melt.

The hydrogenation is less distinct: but if mercury, in a vessel of iron or platina, be placed in contact with the negative pole, and its surface touched by a conductor from the positive, a spot or circle of a powder different from the black oxide, which is produced under the opposite circumstances, will be found on the surface.

tion produced by the positive pole is easily observable. Nothing more is necessary, than to arm it with a leaf of gold, and the negative pile with a bit of charcoal; when, on bringing them into contact, the gold leaf will burn with a bright flame, and the charcoal remain untouched. If the charcoal be placed in contact with the positive pole, and the gold leaf in contact with the negative, the charcoal will burn, and the gold melt. The hydrogenation produced by the negative pole is less distinct, so that it is seldom perceived: but facts may be adduced however, that prove its existence. If a little vessel of iron or platina, filled with mercury, be placed in contact with the negative pole, on touching the surface of this fluid metal with the positive conductor, we obtain a point or circle of a powder very different from the black oxide of mercury, which is produced when the mercury is placed in contact with the positive pole, and touched with the negative conductor. The oxide produced in the latter case arranges itself in the figure of little stars, equal to those produced by positive electricity with the powder: and the circular figures on the mercury at the negative pole are likewise equal to those produced by the powder electrified with the negative conductor.

In a pile the poles of which are not made to communicate by means of a conducting substance, the chemical action of the strata composing it is very unequal. The plates of zinc are oxidized less in proportion to their distance from the positive pole; so that those nearest the negative pole have frequently no traces of oxidation, and seem rather to have been protected from the action of the water by which they are wetted, than attacked by the action of the pile. This may be rendered still more evident by placing every fifth pair in contact with an iron wire, the other end of which is plunged in water. In this experiment the oxidation of these wires will be in the inverse ratio of their distances from the positive pole; in the centre the wire will not be more oxidized than another simply plunged in water for an equal length of time, and all the wires beyond this will be still less oxidized. Hence it is evident, that another action, the reverse of oxidation, has taken place.

Of all the effects of the pile, its action on the human body has been least examined. The shock, or rather palpitation, that it excites, has been considered as too simple, to be subjected to strict inquiry; and the flash, as well as its action on

the

the tongue, has drawn but slight attention. It is true these inquiries, like all others relating to organized beings, are very difficult, particularly when they concern an action that is frequently injurious to the living subject. Ritter has more than once paid for the following discoveries, by long and even dangerous fits of illness.

It is well known, that the skin, being a bad conductor, must be wetted, to make it a good one: it is likewise found in practice, that a surface of considerable extent must be wetted and armed with metal, to have all the possible effect of a pile. The reason is not difficult to discover, though it may lead to many important consequences: we have only to advert to the known fact, that conductors can only convey a quantity of electricity proportionate to their surface; whence it follows, that, to produce the greatest effect, a considerable extent of skin must be made a good conductor. If one of the surfaces wetted and armed with metal, which is touched by the conductors of the pile, be larger than the other, the sensation is less distinct than that which takes place on the smaller, where there is a more perceptible, and often painful sensation: so that we are masters of the magnitude of the effect that we would produce on any part of the body, a very important circumstance in employing galvanism medicinally.

The following is an application of what has just been said. All the difference between the shock obtained from the pile, and that received from a jar, arises from the different states in which we are when we touch them: if a very large pile be touched with dry hands, we experience the same sensation, as if we had touched a charged jar: on the contrary, if with hands thoroughly wetted and armed with metal we touch a jar previously discharged by dry hands, we receive a shock similar to that of the galvanic pile.

Ritter reduces all the effects of the pile on the animal body to expansions and contractions. By the positive conductor he has made several parts of the human body assume a greater bulk; and by the negative he has made the same parts contract. When the tongue is brought into contact with the positive conductor, applying the negative to some other part of the body, and they be all left a few minutes in this state, a little rising is produced on the tongue: the negative conductor, placed in contact with it in the same manner, produces a little

In the latter case the black oxide arranges itself in stars, as when electrified by the machine positively; and the circular figures of the former resemble those produced by electrifying the powder negatively. When the poles are not made to communicate by means of a conductor, the chemical action of the strata is very unequal. The plates of zinc are oxidized less in proportion to their distance from the positive pole; and those nearer the negative pole appear to have been protected from this action.

If every fifth pair be in contact with an iron wire communicating with water, the oxidation of the wires will be in the inverse ratio of their distance from the positive pole; the central wire will not be more oxidized, than one simply standing in water, and the wires toward the negative pole will be preserved freer from oxidation.

3. & 4. Spark and shock.

The action of the pile on the human body has been least examined.

It is in fact most difficult, and Ritter has brought on himself long and dangerous fits of illness by his experiments. The skin, to form a good conductor, must be wetted; and this to a considerable extent, to have all the effect of a pile. If the skin be wetted and armed with metal in two places, and touched with conductors from the pile, the sensation on the smaller surface is most perceptible, and often painful: hence we can command the magnitude of the effect we would produce on the body. The difference between the shock from the pile, and that from the jar, arises from the state in which we are when we touch them. If we touch a large pile with dry hands, we receive a smart shock; but if with hands wetted and armed with metal, we touch a jar discharged previously by dry hands, we feel the same sensation as from the pile.

depression. If the wet hands be kept a few minutes in contact with the poles of the pile, the pulse of the hand in contact with the positive pole becomes sensibly stronger, and that of the hand which touches the negative pole weaker. Mr. Ritter has given us many particulars on this subject, and is employed in making additions to them; but we shall content ourselves with observing, that the expansion is followed by a sensation of heat, the contraction by a sensation of cold.

The action of the pile on the organs of sense is modified by the particular nature of each; but it is remarkable, that the two poles of the pile produce in some sort the two extremes of each species of sensation. I have already observed, in the abstract I gave of Mr. Ritter's discoveries on light some time ago, that the pile produces in the eyes those red and blue colours, which are nearly the extremes of those of the prism; and if it were not so difficult to distinguish the violet from the blue, undoubtedly we should have nothing to wish in this respect. In these experiments, the eye in the positive state, while it sees every object of a red colour, sees them at the same time larger and more distinctly: in the negative state, on the contrary, it sees them at once blue, smaller, and less distinct than they usually appear.

Thus the expansive power of the positive pole, and the contracting power of the negative, seem to exert their action here likewise.

The tongue is equally affected by the pile: the acid taste produced by the positive conductor, and the alkaline by the negative are sufficiently known.

The effect of the negative conductor on the nose is an ammoniacal smell; that of the positive is a depression of the sensibility of the organ, similar to what is produced by the oxygenated muriatic acid.

The ears, touched by the positive conductor, hear a grave sound, and with the negative, a sound more acute.

These experiments require much care: to repeat them properly it is necessary to read the descriptions at large, which the author has given in different tracts, where the particulars are minutely detailed.

Ritter reduces all the effect of the pile on the body to expansion and contraction; the positive conductor expanding, the negative contracting the part. The pulse is made stronger by the positive conductor; weaker by the negative. The expansion is followed by a sensation of heat, and *vice versa*. The action of the pile on the organs of sense depends on the nature of the organ; but the two poles produce the extremes of the sensation. The eye in the positive state sees objects red, large, and distinct; in the negative, blue, small, and more obscure. On the tongue the positive pole produces an acid taste; the negative, an alkaline. In the nose the negative produces an alkaline smell; the positive, that of oxygenated muriatic acid. In the ears the positive produces a grave sound; the negative, an acute.

IX.

Account of a Machine for laying Land level.

By Mr. DAVID CHARLES.*

THIS simple machine, which is the invention of my Steward, and of which I have seen nothing similar, appears to me necessary, even in the most fertile parts of England, where the new system of drill-husbandry has been introduced, or even where there is any attention to the waste of time, or to the ease of cattle in the act of ploughing; in order to get rid of crooked or unequal ridges, without either a summer fallow by cross ploughing, or else by frequent repetitions of ploughing in the winter and spring, which the humidity of our climate will not allow in every kind of soil.

Account of a
machine for lay-
ing land level.

I reduced fourteen acres of land last spring to a perfect level, where the crowns of the ridges were above two feet higher than the furrows, and where they were crooked and of unequal breadths. Six acres of this is now under turnips, a crop that gives sufficient time to ameliorate the under-strata of soil that had perhaps never before been exposed to the influence of the sun and air; and by the adoption of the Northumberland mode of sowing that root on dunged drills, it is almost immaterial where the upper strata is, provided the seed vegetates, as it soon strikes into the manure, and rapidly flourishes.

My chief success, however, has been upon a field of eight acres, which lay in the unprofitable state already described. This land, which is a deep clay, and which had produced a crop of wheat from an old lay sod the former year without any manure, was winter ploughed, and lay in that state until the leveller was introduced the first dry weather in April. It was preceded by two horse-ploughs, taking perhaps a square of an acre at once: these loosened the soil the depth of a common furrow, and twice the breadth across the ridges. The leveller followed, drawn by two oxen and two horses, with a man at each handle, to press it down where the weight is to be removed, and to lift up the body by the handles

* From the Transactions of the Society of Arts, who rewarded the Inventor with their silver medal. The communication was made by Lieut. Col. Hardy, of Westmead, Carmarthenshire.

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Account of a
machine for lay-
ing land level.

where it is to be discharged. Thus four men, one driver, and eight head of cattle, will more effectually level from half an acre to three roods in one day, according as the earth is light or heavy, than sixty or eighty men would accomplish with barrows and shovels, &c. even with the assistance of a plough. In sandy ground where the depth of one furrow will bring all to a level, as much will of course be done in one day as two ploughs can cover; but my ground required to be gone over several times. After this field was levelled, the backs of the ridges, as they are termed, which were stripped of their vegetable mold, were ploughed up, the furrows not requiring it. They were also harrowed, and the field copiously manured with lime compost; harrowed in, and broke into nine-foot ridges, perfectly strait, in order to introduce Duckit's drill. It was sown under furrow, broadcast, the last of it not until the 13th of May, and was cut down a reasonable crop the 4th of September. I am now thrashing it, and a sample shall be sent, as well as a return of the eight acres if necessary.

The field now lies in proper form, well manured, with the advantage of a fair crop from heavy tenacious ground, without losing a season, and in a year by no means favourable.

I am well aware there are many shallow soils, where it may be hazardous to remove the enriched surface, and trust perhaps one half of your land for a crop that had never before been exposed to the atmosphere; but where the soil is sufficiently deep, or you have good under-strata, and there is manure at hand to correct what is sour from want of exposure and tillage, it is evident from this experiment that no risk is run.

To avoid the expence of a fallow, and to lay out ground in strait and even ridges, even where drill husbandry is not practised, should be objects to every rational farmer. But where the new system is intended to be adopted, it becomes indispensably necessary. In laying down lawns, parks, &c. where furrows are an eyesore, or places inaccessible to wheel carriages from their declivity, and from which earth is to be removed, it will be found equally useful.

Should the society consider the inventor, David Charles, worthy of any remuneration, honorary or otherwise, it will be gratefully acknowledged by

Your obedient Servant,

JOSEPH HARDY.

Westmead, Jan. 1, 1803.

Certificates

Certificates from Mr. Owen Edwards, of Brook, and Thomas Bynan, carpenter, of Westmead, accompanied the above letter, confirming the statement made herein.

Account of a machine for laying land level.

Description of the Machine for laying Uneven Land level, invented by Mr. DAVID CHARLES.—Plate XI. Fig. 1, 2.

Fig. 1.—A, Part of the pole to which the oxen or horses which draw the machine are fastened, and which is attached to the machine by a pin at B.

CC, The two wheels, shod with iron, which run upon the axle D.

EE, The upper frame-work of the machine, extending from the axle to the extremity of the handles FF, and secured firmly by the cross pieces.

GG, The curved iron sliders of the machine, which may be raised or depressed a little by means of the pins HH, which pass through holes in the wood-work, and also in the iron sliders; these sliders form one piece with the back iron scraper I, in the manner more fully explained in *Fig. 2*.

K, The wooden back of the machine, which should be made strong, to resist the weight of the earth when collected therein. The iron scraper should be firmly secured to this by screws and iron-work.

LL, The wooden sides of the machine firmly connected with the back and frame-work, in order to assist in collecting the earth to be removed.

M, A strong cross piece into which the ribs which support the back are well morticed.

Fig. 2.—K, The interior part of the back of the machine.

I, The iron scraper, sharp at the bottom, and firmly screwed to the back of the machine.

GG, Parts of the side irons or sliders, showing the mode in which they are united with the scraper I.

M, The cross piece above described.

X.

Experiments on Magnetism; by Mr. RITTER, of Jena. Communicated by Dr. ORSTED, of Copenhagen.*

THE phenomena of magnetism have frequently been compared with those of electricity, and many facts seem to justify the comparison. These facts, however, are neither numerous enough, nor sufficiently conclusive, to compose a complete theory. A series of experiments, exhibiting the magnetic needle in all its relations to electricity, at present better known by means of the pile, would undoubtedly throw much light on

a subject heretofore so obscure. Ritter felt the importance of such an undertaking, and began fresh inquiries concerning magnetism, with the same ardour and sagacity that have ever distinguished his labours. Though these experiments did not always answer the full extent of his designs, they notwithstanding exhibit a sufficient number of interesting facts to excite the curiosity of every natural philosopher.

Ritter has instituted new inquiries on the subject. His experiments did not always answer, but exhibited many interesting facts.

A magnetic wire, and another not magnetic, excited galvanic palpitations in frogs; the south pole more strongly, the north less, than iron not magnetic. Hence he inferred, that the south pole has a greater affinity for oxygen; the north, less.

This confirmed by experiment. Magnetic iron wires being placed in weak nitric acid, the south pole was most oxidized. The south pole of a magnetic wire being immersed in one phial of water,

Mr. Ritter's first experiments with the magnet were on frogs. He found that a magnetic iron wire, with another not magnetic, excited a galvanic palpitation in these animals. Presently he observed, that the south pole excited stronger palpitations, and the north pole weaker, than the iron not magnetic. Having constantly noticed, that the metals most susceptible of oxidation excited the strongest palpitations, he inferred, that the south pole possesses a greater affinity for oxygen than simple iron, and the north pole less.

This supposition he confirmed by means of several chemical re-agents. He placed a magnetic iron wire on pieces of glass in a plate of earthen ware, and poured upon it very weak nitric acid. The south pole was attacked by the acid much more powerfully than the north; and was soon surrounded by a deposition of oxygen, the quantity of which greatly exceeded that of the other pole.

The different oxidability of the magnetic poles is very well exhibited likewise, by taking three small bottles of equal size, filled with water, either pure or slightly acidulated, and putting

* Journal de Physique, December, 1803, Vol. LVII. p. 406.

into one the south polar end of a magnetic wire, into a second the north polar end of a similar wire, and into the third the end of an equal wire not magnetic: the south pole will first begin to deposit oxide, the unmagnetic iron a little after, and the north pole last. This experiment requires considerable care. The surface of the water must be covered with very fresh oil of almonds, to exclude all access of air. Care must be taken too, that one of the bottles is not more exposed to the sun than the others, because light accelerates oxidation. Ritter convinced himself of this by direct experiments; exposing two iron wires in water to the sun, but covering one of the phials with black paper, when that in the phial left uncovered was oxidized much the more quickly.

If infusion of litmus be substituted instead of the water in the three phials in the preceding experiment, the relative oxidations will be the same; but they will be attended with a change of colour, shewing, that an acid is produced proportional to each oxidation; so that the south pole not only undergoes the greatest oxidation, but likewise reddens the infusion of litmus most*. The action that takes place in this experiment is very feeble, and frequently requires a week or more to produce a distinct effect; and indeed to accelerate it so much as this, it is necessary to add, previously to the infusion, as much acetic acid as will incline it to red, without completely changing its colour. The infusion reddened in this experiment resumes its blue colour on exposure to the air: but we must not hence conclude, that the acid produced by the action of the magnet is very volatile, for infusion of litmus reddened by phosphoric acid, or any other, exhibits the same phenomenon.

The following experiment exhibits some things peculiar, and therefore I shall give it more at large. It has not been repeated, but the harmony of its results is in favour of its accuracy. Sixteen magnetic wires, of equal size and power, were placed in six vessels, all equally full of a mixture of one part nitric acid and thirty-six parts water, in the following

* Ritter has remarked, that the oxidation of zinc, and several other metals, in pure water, produces an acid. Mr. Jager, a celebrated physician of Stuttgard, made the same discovery, without knowing any thing of Ritter's.

the north pole of one in another, and the end of a simple wire in a third; the south pole first deposits oxide, and the north pole last. This experiment requires particular precautions. Air must be excluded; and light accelerates oxidation.

If infusion of litmus be used, the relative oxidations will be the same; but an acid in proportion to each will be produced, as appears by the change of colour.

This action is very slow; and to accelerate it a small portion of acetic acid should be added to the infusion. The infusion thus reddened becomes blue again on exposure to the air: but it is the same when reddened with any other acid.

Experiment.

manner:

Ritter and Jager have found, that the oxidation of several metals in pure water produces an acid.

In phials containing water 36 parts, nitric acid 1 part, were immersed: 1. a N. and S. pole, at $\frac{1}{2}$ a line distance: 2. the same $1\frac{3}{4}$ inches, at $\frac{1}{2}$ a line: 3. 3 S. poles, at $\frac{1}{2}$ a line: 4. the same at $1\frac{3}{4}$ inch: 5. 3 N. poles, at $\frac{1}{2}$ a line: 6. the same at $1\frac{3}{4}$. The oxide deposited in these with respect to quantity was in the following order: No. 2, 3, 4, 5, 6. The loss of fluid by evaporation, was in the inverse ratio of the oxidations. All the magnets were weakened in power: No. 2 least; 1 more: in all the rest one wire was weakened more than the others. On immersing two south poles in one phial, two north in another: the latter were most oxidized. Mr. R. attempted unsuccessfully to construct a battery with 120 magnetic wires, their poles placed in opposition, and separated by a drop of water; but he has not given up the design: and means to pursue his inquiries on the subject at large very extensively.

manner: In the first glass were placed two wires, one with the north pole immersed in the fluid, the other with the south, and not more than half a line asunder: In the second, the same, but the wires an inch and three quarters apart: In the third and fourth were each three wires, with the south poles of all immersed, but their distances in the two glasses different, as in the first and second: In the fifth and sixth were wires similarly arranged, but with the north poles immersed. Different quantities of oxide were gradually deposited; and to express the whole in few words, we will call the south pole S, the north pole N, their greater distance *g*, and their less *p*; and we will express the order of oxidations as follows: S N *g* > S N *p* > 3 S *p* > 3 S *g* > 3 N *p* > 3 N *g* >. On the nineteenth day it was observed, that the loss of fluid by evaporation had not been equal in all the vessels, but took place in the inverse order of the oxidations. All the magnetic wires were weakened in power; N S *g* least; N S *p* more: of the wires 3 S *p*, two had lost less power than the third; and in like manner 3 S *g*, 3 N *p*, 3 N *g*, had each two left more powerful than the third; the strongest were equal to N S *g*.

In another experiment, where two little vessels filled with infusion of litmus were employed, one of them containing two magnetic wires, the south poles of which were immersed in the fluid; the other two similar wires, of which the opposite poles were immersed; the oxidation was greatest in the latter vessel*.

Lastly, Mr. Ritter endeavoured to construct a battery of magnets, but he did not succeed. For this purpose he employed a hundred and twenty magnetic wires, placed so that each pole had its contrary opposite to it, and separated from it by a drop of water; but this apparatus produced no effect. The ingenious author, however, has not relinquished the hope of being able to compose a magnetic battery, though other experiments, not less important, have hitherto prevented him. This series of experiments he considers only as the commencement of a very extensive labour, the results of which we hope soon to obtain.

* This appears contradictory to the experiment adduced in the third paragraph.

T.

XI.

Observations on Arseniated Copper. By HAUVY*.

THE only ores of arseniated copper which are well known, are those from the county of Cornwall in England. The determination of their true composition followed closely on the discovery of this metallic substance, for which we are indebted to the fortunate chance which threw some specimens into the hands of the celebrated Klaproth. It was in 1787 that he published in the *Journal de la Société des Curieux de la Nature* †, the result of the examination which he had made of this new mineral.

History of the discovery of the ores, and their composition.

The authors who have spoken of arseniated copper since that period, had only described it under the form of acicular crystals, when Citizen Lelievre, member of the Council of Mines, having suspected the existence of a peculiar substance, from the inspection of a group of green hexagonal bevelled laminæ which were given to him, made an essay of it, and discovered the presence of oxide of copper and arsenic acid. Citizen Vauquelin soon afterwards confirmed this indication, and determined the proportion of the relative quantities of the two principles contained in the same substance.

About this time the opening of a second mine in the county of Cornwall occasioned the re-appearance of arseniated copper, the vein which had been formerly explored being exhausted. This discovery was the more important, as the substance appeared, in its new situation, with characters altogether peculiar, and under forms hitherto unknown.

M. de Bournon, who was at hand to participate in this increase of riches which resulted to mineralogy, sent to Citizen Gillet Laumont and me, several specimens chosen from among those he possessed; and that which added to the value of his gift, was his haste to communicate to us the interesting work which he had prepared, on the crystallography of arseniated copper, before he published it.

* Translated from a pamphlet in quarto, sent by the author to the Count de Bournon; probably forming part of a journal.

† Tom. VIII. p. 160.

Mr. Chenevix was employed at the same time on the analysis of this substance. Soon after they both published the results of their researches in the *Philosophical Transactions**; and Mr. Chenevix testifies his admiration at such a perfect agreement between two sciences which employ two methods so different to interrogate nature. M. de Bournon, on his part, says that the analyses of Mr. Chenevix have given the most satisfactory sanction to the division which he had himself made, of the arseniated copper into four distinct species.

Description of
the varieties.

Before going farther, it is necessary to make known the varieties of arseniated copper which I have been enabled to examine. I shall confine myself to giving a description of them, succinct and independent of the laws to which the structure of crystals is submitted, the actual state of our knowledge on this subject only admitting of hypothetical views, of which I shall speak hereafter.

1. Obtuse octahedral arseniated copper, (*Plate IX. Fig. 2*): incidence of P on p , $50^{\circ} 4'$; of P' on p' , $65^{\circ} 8'$; of P on P' , $139^{\circ} 47'$ †. The colour of the crystals is sometimes a fine celestial blue, and sometimes a green, which varies between a grass-green and a pale green. The octahedron sometimes becomes cuneiform, lengthening so that the terminal edge is parallel at D .

2. Lamelliform arseniated copper. In hexagonal laminæ, whose narrow faces are inclined alternately in contrary directions; the incidence of two of the narrow faces, situated on the same side, on the correspondent base, 135° nearly, according to M. de Bournon; incidence of the third on the same base, 115° nearly.

* For 1801, p. 199, *et seq*:

† I adopt here, very nearly, the results of M. de Bournon, who indicates 50° for the incidence of P on p , and 65° for that of P' on p' . I have only endeavoured to find limits capable of facilitating the calculations which I propose to make. Let bac , gac , (*Fig. 4*) be the same faces as P and P' , (*Fig. 2*); let ao (*Fig. 4*) be the height of the pyramid which has its summit in A , (*Fig. 2*); on (*Fig. 4*) a perpendicular to bc , and or perpendicular to cg :—if $ao = \sqrt{588}$, $on = \sqrt{2695}$, and $or = \sqrt{1440}$, we shall have $50^{\circ} 4'$ for the incidence of P on p (*Fig. 2*), and $65^{\circ} 8'$ for that of P' on p' ; whence is deduced, by calculation, $139^{\circ} 47'$ for that of P on P' ,

The laminæ divide parallel to the large faces with great ease. Their colour is a fine grass-green.

3. Acute octahedral arseniated copper, (*Fig. 3*): Incidence of r on r' , 96° , according to M. de Bournon; of l on l' , 112° . The colour is a brown green more or less deep.

a, cuneiform. The preceding octahædron lengthened so that the terminal edge is parallel to n . This form, which is the most common, offers the appearance of a long rhomboidal prism, more or less acute, and is terminated by dihedral summits.

4. Trihedral arseniated copper. In a strait triangular prism, which is at the same time equilateral, according to M. de Bournon.

When the crystals have not been long exposed, their colour is a fine bluish green; but their surface is subject to change and take a blackish tinge. If they are scratched, their primitive colour will re-appear.

5. Capillary arseniated copper. This is properly the olivenerz of the German mineralogists.

6. Mammellated arseniated copper. In mammellated masses, striated in the interior. These two last varieties are susceptible of a great diversity of tints, which shew the transitions from grass-green to olive-green, to greenish-brown, to tawny (*mor-doré*), to yellow, to bluish, and to white, which is frequently fatty.

The following is the manner in which M. de Bournon classed the different modifications which have been just mentioned, according to the differences which they offer with respect to form, specific gravity, and hardness.

He divides them, as I have said, into four distinct species. The first is derived from the obtuse octahedron: the second is the lamelliform crystal, in hexagonal laminæ, with bevels inclined alternately in contrary directions. He takes the acute octahedron for the primitive form of the third, and connects the acicular crystals and mammelary concretions with it, as varieties: in the fourth he places the equilateral triangular prism, and several other forms which offer the same prism, truncated on its solid angles or on its edges.

On the other hand, Mr. Chenevix has given six results of the analysis of arseniated copper, which I shall detail them conformably to the order established by M. de Bournon.

First

ON ARSENIATED COPPER.

First Species, in obtuse octahedra.

Oxide of copper	-	-	-	49
Arsenic acid	-	-	-	14
Water	-	-	-	35
Lofs	-	-	-	2
				<hr/> 100 <hr/>

Second Species, in lamelliform Crystals.

Oxide of copper	-	-	-	58
Arsenic acid	-	-	-	21
Water	-	-	-	21
				<hr/> 100 <hr/>

Third Species, in acute octahedra.

Oxide of copper	-	-	-	60
Arsenic acid	-	-	-	39.7
Lofs	-	-	-	0.3
				<hr/> 100.0 <hr/>

Variety of the same Species, in capillary Crystals.

Oxide of copper	-	-	-	51
Arsenic acid	-	-	-	29
Water	-	-	-	18
Lofs	-	-	-	2
				<hr/> 100 <hr/>

Another Variety, in mammellated Concretions.

Oxide of copper	-	-	-	50
Arsenic acid	-	-	-	29
Water	-	-	-	21
				<hr/> 100 <hr/>

Fourth Species, in trihedral Prisms.

Oxide of copper	-	-	-	54
Arsenic acid	-	-	-	30
Water	-	-	-	16
				<hr/> 100 <hr/>

Towards

Towards the conclusion of his memoir, Mr. Chenevix remarks that the natural arseniate of copper exists in three different combinations, the first of which contains 14 per cent. of arsenic acid, (first result above); the second contains 21, (second result); and the third 29, (third, fourth, fifth, and sixth results). It is true, the third result gave 39.7 of acid in the 100 parts, but as the remainder of the mass was composed of 60 parts of copper without water, he found that the proportion of the acid with the copper did not differ much from that which takes place in the varieties in which water forms a part: this induced Mr. Chenevix to comprehend this result in the same division. Yet he considers this combination as the only true arseniate of copper, while the other three are arseniates of hidrate of copper.

Only one true arseniate of copper: the others are arseniates of hidrate of copper.

I feel the value of the double work from which I have given this extract so much the more, because, having read the memoirs which contain the developement, I am enabled to judge of the advancement which it has produced in our knowledge on a subject which was in a great measure new when MM. de Bournon and Chenevix began to be employed with it. The exposition which I shall add of some enquiries I have made on the crystallization of arseniated copper, and of the reflections which they have given rise to, have no other object but that nothing may be neglected which tends to elucidate in a greater degree, every thing connected with an object of such importance as the distinction of mineralogical species.

After having read the crystallographic part of the work in question, I was desirous to know if it was not possible to bring some of the crystals, described by M. de Bournon as belonging to different species, to the same form of the integrant molecule; but not being able to make all the direct observations which would have guided me in this enquiry, I was obliged to confine myself to simple hypotheses.

Can the varieties be reduced to one primitive form?

I therefore considered the obtuse octahedron as performing the functions of the primitive form; and I was the more warranted in conceiving this opinion, because the celebrated Karsten, in a supplement to the excellent memoir which he had published before*, on the combinations of copper with different principles, says that the octahedron in question is lamellated,

* *Journ. de Physique*, Brumaire, An. X. p. 342, et suiv.

Comparison of
the acute and
obtuse octahe-
dra.

in a direction parallel to the faces of the two pyramids, of which it is the aggregate*. Proceeding on this datum, I was curious to know if it was not possible to connect with the form of the obtuse octahedron in question, that of the acute octahedron, which M. de Bournon has taken for the type of his third species. Let P, P' (Fig. 2) be still an obtuse octahedron, in which the incidence of P on p is reckoned to be $50^\circ 4'$, and that of P' on p' , $65^\circ 8'$, conformably to the measures indicated above: if we imagine another octahedron (Fig. 3) the sign of

which is $\begin{matrix} 24 \\ DF \\ 24 \\ lr \end{matrix}$, we shall find that the incidence of l on l' is

109° , and that of r on r' is $93^\circ 36'$. Now the correspondent incidences determined by M. de Bournon, are one 112° , and the other 96° ; which in one case makes a difference of 3° , and in the other of $2^\circ 24'$.

The differences
in the angles ac-
counted for.

If the measures had been taken on crystals so well defined, that the differences could be considered as real, we must have concluded that they formed two distinct species, because even these differences could only have been done away by supposing the laws of decrement to be much too complicated to be admissible.

But if the crystals were not capable of very accurate measurement, we can the better conceive that the differences were simply apparent, and that it may be possible that the error did not wholly arise in one observation, since it was necessary to make two, in which small deviations might have been produced on opposite sides; and then mechanical division alone, by giving different results with respect to the two octahedra, would have shown that a conformity between the angles observed and those measured, would be purely accidental.

Comparison of
the lamelliform
variety with the
obtuse octahe-
dron.

I afterwards compared the lamelliform variety with alternate bevils, which is the second species of M. de Bournon, with the same octahedron with obtuse summits. Now, if we suppose two intersecting planes, parallel to the face P' , and which meet the center, they will detach an octahedral segment which cannot be supposed to have much thickness, and whose two large faces will be hexagons, and the six lateral faces, trapeziums,

* *Journ. de Physique, Pluviose, An. X. p. 131.*

inclined to the great faces *. But these trapeziums will not be situated alternately in opposite directions. The three which will form obtuse angles with the large faces, will be contiguous to each other, and the same will be the case with those which form acute angles with the same faces. For example, those of the trapeziums which form obtuse angles with the large face analogous to P' , will correspond to the two faces of the adjacent octahedra at B, B' , and to the face situated behind A , parallel to P . The inclination of this latter face on P is, according to M. de Bournon, 115° ; and the two others, as I have indicated them above according to my calculations, are each about $139\frac{1}{2}^\circ$.

Now, of the three lateral trapeziums in the lamelliform arsenical copper, one has the same inclination of 115° on this base, according to M. de Bournon, and the two others have 135° ; an estimate which he only gives as an approximation, and which only differs by $4\frac{1}{2}^\circ$ from that corresponding to it in the obtuse octahedron, (*Fig. 2*).

The great difference consists in this, that the three lateral trapeziums which look towards the same base, in the octahedral segment I have described, are contiguous to each other, as I have said; while those of the lamelliform arseniated copper alternate with the three others which look towards the opposite base †.

But there is a method of removing this difficulty. Let us conceive that the two sections made in the octahedron (*Fig. 2*), instead of being parallel to the face P' , are so to the face P . In this case the lateral trapeziums, situated on the two sides of the edges B, B' , will always have an inclination of $139\frac{1}{2}^\circ$ to the superior base. Now, if the segment parallel to p makes an angle of 115° with the base analogous to P , the three segments will preserve, with respect to those turned towards the opposite base, the alternation indicated by M. de Bournon.

* Several substances, among others the spinelle, offer examples of similar segments.

† The figure given by M. de Bournon, of which that is a copy in *Plate XI. Fig. 5*, seems to have been traced according to the condition that the three trapeziums turned towards the same base should be contiguous. This was doubtless an oversight of the draughtsman.

But the incidence of p on P gives, on the contrary, an acute angle of 50° . Now, let us imagine a decrement indicated by D , which acts on the face p and on that which is opposite; the faces produced will be situated vertically; whence it follows that that which will mask the face p will form an angle with P equal to 90° plus 25° , which is the half of the inclination of p to P , that is to say, the angle in question will be 115° , conformably to observation*.

Their division into four species is not admitted by the laws of structure.

I shall not urge these results farther, which, as I have already stated, I only offer as purely hypothetical; and I shall abstain from adding my opinion with respect to the fourth of the species admitted by M. de Bournon, which, according to him, has the equilateral triangular prism for a primitive form. It is enough for me to have shown that, with respect to the division of arseniated copper into four distinct species, the laws of the structure may give rise to doubts which deserve some attention. If they can be removed, as it is not impossible they may, another proof will be obtained in favour of an opinion on which no obscurity should remain, that it may be worthy of being unanimously adopted.

Comparison of Mr. Chenevix's analyses and M. de Bournon's division.

If we now consider the results of the analyses which Mr. Chenevix has made of the different modifications of arseniated copper, we find, that in such of these analyses as have had for their object the types of the four species admitted by M. de Bournon, three have given sensible differences in the relative quantities of copper, arsenic acid, and water. These analyses correspond with the first, third, and fourth species. Another analysis, made on the second species, gave only copper and arsenic acid, without water. Thus, supposing that the relations between the quantities of the three principles contained in the modifications which Mr. Chenevix calls *arseniates of hydrate of copper*, constitute true limits, and that, in the modification which he calls simply *arseniate of copper*, the absence of water depends on the nature of the substance itself; we shall, in this respect, find an agreement between the results of

* I have a lamelliform crystal, on which, instead of a simple bevil, there are two, situated in contrary directions on the sides of the same edge; but they are too small for it to be possible to determine the positions exactly.

analysis and those of crystallography, very favourable to the subdivision of the mineral in question, into four distinct species.

But Cit. Vauquelin, on analysing a piece of lamelliform arseniated copper, whose crystals were quite fresh, obtained a very different relation between the quantities of the three principles*. His result was as follows :

Oxide of copper	-	-	-	39
Arsenic acid	-	-	-	43
Water	-	-	-	17
Loss	-	-	-	1
				100

It is remarkable in this case, that the quantity of acid exceeds that of the copper, while in the result obtained by Mr. Chenevix it forms only a little more than a third of the quantity of copper. It is not therefore evident that the limits indicated by this celebrated chemist are essential to the substances analysed.

The experiments of the same philosophers on the capillary crystals, offer differences not less striking. According to Cit. Vauquelin these crystals contain,

* Mr. Chenevix, in his memoir, gives a passage from a letter which Citizen Vauquelin had written to him, and in which he informed him, that having analyzed crystals of the lamelliform variety, he found that they were composed of 59 of oxide of copper and 41 of arsenic acid. Mr. Chenevix adds, that the great difference between this result and that which he had himself obtained from the same substance, induced him to repeat his analysis with great care and attention, and that he constantly found the same proportions of oxide of copper, arsenic, and water. It is very probable that this result announced by Cit. Vauquelin, and so different on the other hand from that now given, was obtained in a first essay, or that this chemist, when he wrote to Mr. Chenevix, trusted to his memory, which was not so faithful as it generally is. However that may have been, the only result avowed by Cit. Vauquelin is that we now publish, and which he has inserted in the *Journal des Mines*, No. 55, p. 562.

Silex	-	-	-	-	2
Water	-	-	-	-	5
Arfeniate of iron	-	-	-	-	7 to 8
Arfeniate of copper	-	-	-	-	86
					100

This chemist adds, that if the arfeniate of copper did not contain any foreign matter, it would be formed of about 69 parts of oxide of copper and 31 of arfenic acid.

Klaproth's analysis.

We have another result on the same subject, obtained by M. Klaproth, whose labours have concurred so advantageously with those of Vauquelin, to procure an exact knowledge of the composition of minerals. His result gave,

Oxide of copper	-	-	-	50.62	
Arfenic acid	-	-	-	45.00	
Water	-	-	-	3.50	
Loss	-	-	-	88	
					100.00*

The quantity of copper is nearly the same as in Mr. Chenevix's result; but on one side we find 45 of acid with 3.5 of water, and on the other only 29 of acid and 18 of water; which is very different.

Mr. Chenevix's results do not agree with those of M. de Bournon.

Besides, we need only keep to Mr. Chenevix's own results to find difficulties and causes of uncertainty; for while this celebrated chemist obtained a very sensible quantity of water from the capillary crystals and the mammellated masses, these two modifications were considered by M. de Bournon as simple varieties of the third species, which is the acute octahedron, and which gave only copper and arfenic acid without water. Further, if the analyses of the capillary crystals and of the mammellated masses, are compared with that of the crystals in trihedral prisms, it will be seen that the differences do not exceed those which are frequently met with between the analyses of several pieces which evidently belong to the same species of mineral.

I add, that M. de Bournon seems to have had more authority for considering the capillary crystals and the mammel-

* *Additions à la Connoissance chimiques des Minéraux*, p. 192.

lated concretions as simple varieties of the acute octahedron, since he indicates the intermediate modifications which connect these varieties with their type; so that, according to him, there are crystals which are perfectly determined in one part of their length, and fibrous at their extremity.

M. de Bournon, doubtless struck with the exception which the agreement between the two sciences, announced by Mr. Chenevix and himself, seemed liable to, in this instance, has since inserted in Mr. Nicholson's Journal * a note, in which he proposes to establish a fifth species of arseniated copper, composed of the capillary crystals and mammellated masses, which seems to operate less in removing the difficulty than in bringing it to light.

It cannot be denied that the modifications of arseniated copper offer sensible differences in their aspect, their exterior forms, and their colours. M. de Bournon also indicates some in their hardness and in their specific gravities. But the reduction of natural beings to the smallest possible number of species, really distinct, is an advantage of such value to science, which it perfects by simplifying it, that, before separating substances, according to those diversities which seem to be at variance with the relations which they otherwise have, and before seeking particular specific names for them, which would be necessary, all the means of ascertaining that the diversities in question are not purely accidental, should be exhausted. Even though the researches which still remain to be made for the accomplishment of this object, should have no other effect but to cause the disappearance of one single species, admitted by the two celebrated men whose results I have set forth, from the system, they will not be unprofitable to the progress of mineralogy.

A fifth species proposed.

External differences should not be admitted alone as evidence of a different species.

* * A reply communicated by the Count de Bournon will appear in our next.

* Philosophical Journal, new Series. Vol. VII. p. 577.

XII.

Observations upon the Doctrine of Count Rumford respecting the want of direct conducting Power in Fluids, with regard to Heat. By CIT. BERTHOLLET.

(Concluded from page 140.)

Nicholson, Thompson and Murray have proved, that heat passes through the particles of fluids; that currents are often imaginary; and that fluids differ in conducting power.

I AM of opinion, that the experiments of Nicholson, of Thompson, and of Murray, leave no doubt on the communication of heat between the particles of liquids: some of them show that the motions of the solid corpuscles which are agitated in a liquid, may often mislead, with respect to the currents which are believed to be perceptible: but their existence must not, for this reason, be denied, when a difference between the specific gravities is suddenly established, and when the heat is communicated at the lower part of a vessel. The others prove that the communication of heat may be made through a liquid in which no current can be supposed to transport it immediately to a solid body, and they prove that liquids are possessed of a conducting faculty which differs in its intensity; but it is not to be inferred from this, that the locomotion of the particles of liquids does not contribute to establish an equilibrium of temperature more speedily: it is even probable, that the latter effect is generally the greatest.

These general facts account for all the phenomena.

The foregoing considerations, into which I have admitted the application of the faculty of communicating heat common to all bodies, of the conducting difference, and of the more speedy distribution of heat by means of the difference of the specific gravity which it introduces between the particles of a fluid, seem to me to account for all the phenomena which the discernment of Rumford has made public.

That gases receive heat very rapidly, is seen in the expansion of air thermometers and air balloons.

These considerations lead me to an opinion very different from his; it is known with what rapidity the thermoscopes, or air thermometers, indicate the variations of temperature: Pictet could not observe a second of difference between the elevation of a thermometer of this description, and the emanation of radiant heat by a body placed at a distance: it has been observed, that aerostats experience a sudden dilata-
tion

tion by the appearance of the sun*; these phenomena seem to me to indicate that the elastic fluids, far from being bad conductors, on the contrary, receive the temperature of other bodies very quickly; for, can it be supposed that all the particles of the gas take the temperature which they acquire by the contact of the covering of the balloon alone, and how can it be conceived, that the lower particles, which are contiguous to that portion of the covering which does not receive the solar emanations, should be carried towards that which is exposed to it? And since these particles at each contact only receive a part of the temperature to which they attain, what a prodigious whirlwind must there be supposed to be in the gas!

It appears to me, that the elastic fluids, instead of being bad conductors, possess this property in a high degree, although they probably differ from each other in this respect; and if air which is confined produces effects which seem to prove the contrary, they are owing to some circumstance which modifies this property.

I think it is probable, that this circumstance is the state of compression produced in a gas which cannot acquire a dilatation suitable to the temperature it receives; we have seen that caloric, in combining with the gases, only raises the temperature because the dilatation meets with an obstacle (107); hence it results, that the further the air is removed from the state of dilatation, which it should have, to be in equilibrium of temperature, the greater resistance will it oppose to the combination of the caloric, and the more will it lose of its conducting faculty, so that the air which would take the temperature of the surrounding bodies with facility, if it could acquire suitable dimensions under a given pressure, becomes a worse and worse conductor in proportion as it receives a temperature farther removed from the dimensions which it can take. The air then experiences an effect, which may be compared to that of a body in which the force of cohesion obstructs the action of a liquid, which can effect its solution as soon as this obstruction begins to be diminished.

This explanation is applicable to the conservative property of heat, which Rumford has proved to belong to the air which adheres to particles, such as those of the eider-down; The effect of eider down, &c. is less to prevent the currents, than to oppose the ex-

* Descrip. de l'aérostat de l'Acad. de Dijon.

panfion by its attraction, by which the air adheres to it.

this air only adheres by a true affinity, which probably reduces its dimenfions, or at leaft, oppofes its dilatation; and if the water can drive it off, it is only becaufe it combines with thefe fubftances, and adheres to their furface by its affinity; fo that the air will then experience the fame effect from the action of the affinity of the bodies to which it is adherent, as is produced on its elastic effort, by the fpace within which it is confined, and in which it receives a higher temperature without having the power to dilate.

So that elastic fluids being more alterable in their volume, are alfo more difpofed to give and take heat.

Thus the elastic fluids which dilate much more by a fimilar change of temperature than liquids and folids, muft have the correfponding faculty of entering more eafily into combination with caloric: they offer but little refiftance to compreffion; they heat by the reduction of their volume; and they cool when they dilate: do not thefe effects announce a great difpofition to combine with caloric, or to abandon it, and to receive different degrees of faturatation from it? and nevertheless, according to the opinion of Rumford, there muft be an infurmountable barrier between the moft diftant temperatures, of the different particles of a gas, when the particles do not meet with a folid body.

The fame doctrine applied to liquid water: Sudden accumulation or abforption by its fufion; it conduéts better than ice, &c.

It is poffible, that liquid fubftances may be much better calculated to condué heat than when they are in a folid ftate; the properties of the reciprocal affinity which produces cohesion, feem to point this out: for fince this affinity oppofes the dilatation, it will offer an obftacle to the combination of the caloric: this refiftance to its introduction is alfo proved by the quick accumulation which is made of it, as foon as the force of cohesion is deftroyed, fo that it is oppofed to the combination of caloric, as well as to that of other fubftances; in fact, water feems to take the common temperature more eafily, independent of the locomotion of its particles, than ice, which is a very bad conduétor, and it is perhaps from this difference, that ice, and all the folids which pafs to the liquid ftate, liquefy at the furface, inftead of taking the common temperature.

I only offer thefe laft explanations as conjectures, which may invite to experimental enquiries on a fubject which is not indifferent to chemical theory.

XIII.

A Memoir on the Movements which certain Fluids receive from the Contact of other Fluids *. By J. DRAPARNAUD, Curator and Professor of Natural History, at the Medicinal School of Montpellier.

I HAD observed that alcohol attacks, and finishes by even, at length, destroying the calcareous covering of the molluscæ, which are put into it to be preserved. Supposing that this might arise from the alcohol, particularly that which is not well rectified, containing a little acetous acid, I put a little tincture of turnsole into a glass capsule, and poured into it a few drops of alcohol. The tincture did not change colour, but to my great surprize it moved towards the circumference with great vivacity, leaving the bottom of the capsule uncovered: when it had reached the maximum of dispersion, it returned again, and covered the bottom of the vessel which it had abandoned. It is evident, therefore, that missing what I sought, I found that which I did not seek, which frequently occurs in the course of experiments.

Experiment on the action of alcohol which led to the discovery of this impulse.

This curious experiment induced me to make a multitude of others, and to try a great number of substances. At the moment of putting these experiments in order, and of composing this memoir, I recollected having read that *M. Benedict Prevost* had produced this repulsion of water by means of volatile oils, and even of many solid odorant bodies. I therefore consulted the two memoirs which this ingenious philosopher has inserted in the *Annals*, and whose subject is, *The methods of rendering the emanations of odorant bodies sensible to the sight* †. Although my experiments were made with another view, I pass over, in silence, all which are conformable to those of *M. Prevost*, and which I made by employing the same substances: I shall only speak of those which differ from his, either in the results obtained, or in the means employed.

Prevost's memoirs on the emanations of odorant bodies.

1. If a thin stratum of water is put on the bottom of a vessel, and a drop of alcohol is brought to the centre of this

Action of alcohol on water.

* From the *Annales de Chimie*. *Frustridior*, An. XI.

† *Annales de Chimie*. Tom. XXI. et XXIV.

stratum,

stratum, with a glass rod, the water flies in an instant with vivacity, leaving the bottom of the vessel uncovered; when it has reached the maximum of dispersion it returns, and covers again the bottom of the vessel which it had quitted.

Disk of dispersion.

2. I call that part of the vessel which is abandoned by the water the *disk of dispersion*. In the preceding experiment, this disk shows a perfect dryness and all its natural polish.

The repetition of the experiment lessens its effects.

3. The repulsion becomes less considerable, and the disk of dispersion smaller, in proportion as the experiment is repeated in the same water. This arises from the water becoming gradually saturated with alcohol.

Influence of the vessels.

4. The nature of the vessels has no influence on the preceding experiment, nor on those which follow. They take place equally in vessels of porcelain, earthen ware, glass or metal.

5. The form of the vessels has much influence on the second period of the phenomenon, that is to say, on the return of the water, and on the disappearance of the disk of dispersion.

If the vessel is a little concave, the water always comes back, and covers the bottom of the vessel again. It will be obvious, that this is the necessary effect of its gravity.

If the vessel is flat, the water only returns when the disk of dispersion has not attained too great a diameter.

If the bottom of the vessel is a little convex, the water does not return after having been dispersed, and it must be evident, that to do so it would act contrary to the operation of its own gravity.

Motion of the expelled fluid;

6. Being desirous to render the observation of the motions of the expelled fluid easier and more perfect, I substituted tincture of turnsole, which, as is known, is only water coloured by turnsole, for pure water. The results were the same, but much more sensible; and I could then readily distinguish an undulating or trembling motion on the internal edge of the water which surrounds the disk of dispersion; a motion which proves the continual emission of the alcoholic particles against this interior edge, and determines the removal of the water.

and of the impelling fluid.

7. But to complete the proof of the explanation which I have just given of the phenomenon, it was also necessary to render the motions of the impelling fluid sensible: I succeeded by

by an analogous process. I coloured alcohol by means of turnsole; with this substance it takes a very beautiful colour which is not at all similar to the violet blue of tincture of turnsole, but, on the contrary, is of a very vivid blue, analogous to that of indigo, or prussian blue. I then wetted the bottom of a plate with pure water, and, with a glass rod, brought a drop of this coloured alcohol to the centre: the water was driven back with vivacity. In the centre of the disk of dispersion was a blue spot, formed by the coloured alcohol, and the rest of the disk was white like the bottom of the plate. But the proof of the actual continual emission of the coloured alcoholic particles, is that as the water retired, its internal edge, which touched the disk of dispersion, became more and more of a violet colour, analogous to that of tincture of turnsole prepared with water. It is evident, therefore that in natural philosophy, facts are explained by facts, and, that this experiment confirms the consequence I had deduced from the preceding experiment.

8. If the plate is wetted with the alcohol, and a drop of water is put into the centre, the alcohol does not experience any motion; the drop of water flattens, it retains its orbicular form for some moments, at length it finishes by spreading irregularly, mixing with the alcohol, and uniting with it. Water does not repel alcohol.

9. If the bottom of the plate is covered with a very thin stratum of olive oil, and a drop of alcohol is brought to the centre, the oil is repelled, though more slowly than the water on account of its viscosity, and the bottom of the plate is left dry. Oil is repelled more slowly than water.

If the stratum of oil is too thick, it will not quit the bottom of the plate, and the expansive motion of the alcohol takes place only on the superficies of the oil.

10. If a morsel of the fresh rind of lemon or orange is placed in the centre of a wetted plate, the water is sensibly repelled, and the disk of dispersion is agreeably tinged with the prismatic colours, which depends on the disengagement of the essential oil. But this motion has not nearly so much intensity as that produced by means of alcohol. The essential oil of lemon or orange rind, produces repulsion, but weaker.

11. Convinced by the preceding experiments that every volatile fluid at the atmospheric pressure, was capable of producing this repulsive movement, I employed liquid ammonia. I therefore wetted the plate in the usual manner, and brought a drop The affinity of ammonia for water prevented its repulsive action,

a drop of volatile alkali to the centre. What was my surprize to observe, that the slightest motion was not manifested in the water; I however suspected the cause, and believed that this apparent anomaly was owing to the ammonia, which, having a very great affinity with water, combined with it at the instant of their contact.

which was exercised in olive oil.

12. I resolved, therefore, to substitute a fluid to the water, which had less affinity with ammonia, and I chose olive-oil, which I had at hand. I again covered the bottom of the plate with a thin layer of this oil, and brought a drop of the ammonia to the centre: the oil was instantaneously repelled, as it had been by the alcohol.

Ammonia does not repel alcohol:

13. If the plate be wetted with alcohol, and a drop of ammonia is put into the centre, the alcohol is not repelled, the drop of ammonia flattens, and the two liquors evaporate. I thought I perceived a slight tremulation on the edge of the drop of ammonia.

but alcohol repels ammonia.

14. If the plate be wetted with ammonia, and a drop of alcohol is brought to the centre, the ammonia is repelled like pure water. It appears, therefore, from this experiment, and the preceding, that the horizontal or lateral force of expansion of the alcohol is superior to that of the ammonia.

Olive-oil and water have no repulsive action.

15. A drop of olive oil put into a wetted plate, did not produce any motion in the water. It was the same with water beat up with oil to the consistence of an unguent. *M. Prevost*, in his memoirs, seems to announce results obtained with the fixed oils, which are contrary to my experiments, and particularly to these.

I shall not enlarge farther on experiments which some may, perhaps, think more curious than useful. But when the attractive powers of yellow amber, or of the loadstone were first observed, neither their importance, nor the astonishing discoveries to which they have since led, were suspected.

XIV.

Letter from Mr. CUTHBERTSON respecting his Galvanic and Electrical Experiments.

To Mr. NICHOLSON.

DEAR SIR,

IN consequence of the note which you have been so obliging Introduction.
as to add to my letter, addressed to Dr. Pearson, inserted in your Journal for this month, I have to say, that *the troughs were used collaterally*: I shall now be happy to see your remarks. It appears, that I ought to have been more explicit, and therefore, I beg leave to offer the following additional observations*.

The two last mentioned experiments in the letter alluded to, were compared with common electrical discharges, with a view to prove what quantity of coated glass would be requisite to ignite the same lengths of wire.

Two jars, each containing about 170 square inches of coated Experiment.
Deflagration of
wire by jars.
surface, were set to the conductor of a 24 inch single plate electrical machine, with my universal electrometer loaded with 31 grains, (see Quarto Journal, Plate XXII. Vol. II.)

* I ought certainly to have mentioned the arrangement of the troughs, and likewise I ought not to have said so vaguely, that double quantities of galvanic fluid, only burn double lengths of wire, because I am strongly of opinion, that the reason why galvanic discharges from troughs do not act upon metals in the same ratio as common electric discharges do, proceeds from some defect in the arrangement, and also construction.

I find in my notes of improvements for the 6th of June, 1803, Pile of large
plates.
that I had made a pile of 16 pairs of plates of 10 inches diameter, and that eight of them laid upon each other in the usual manner, with cloths wetted with diluted muriatic acid, burned one inch of wire of 1-195th part of an inch in diameter, and that 16 pairs burned four inches of the same wire. This experiment was repeated on the 8th of June, with the same result, with respect to metals, but gave strong and loud sparks from metal to metal, very loud gal-
vanic sparks.
sufficient to be heard at 300 yards distance, which result, I believe, has never been obtained from troughs, so as to be heard, indeed, at any distance. For the last experiment, the cloths were wetted in a strong solution of muriate of ammonia.

Eight

Eight inches of the same sort of wire were laid in the circuit, 57 revolutions of the plate caused the electrometer to discharge the jars which ignited the wire perfectly, as in the ninth experiment. Then six inches of the wire were laid in the circuit, and the above number of revolutions caused the discharge, the wire being deflagrated and fused into balls in the same manner as in the eighth experiment.

Deduction as to the quantities of electricity in a jar and a pile.

Probability that Mr. Wilkinson's wire was thinner than stated.

Hence I conclude, that 340 square inches of coated glass properly constructed, will bear a charge equal to a galvanic battery of 1080 square inches of surface.

The result of the above experiments gives me reason to think, that there is a mistake respecting the diameter of the wire ignited by Mr. Wilkinson's batteries, as mentioned in your Journal, Vol. VII. p. 297, to which you refer, because to ignite one half inch of steel wire of *one seventieth of an inch in diameter*, will require a power sufficient to ignite 120 inches of wire $\frac{1}{150}$ part of an inch in diameter, by common electrical discharges, which is a power equal to two of my common electrical batteries, (see your Quarto Journal, Vol. II. p. 525.)

The greatest power of 60 pairs of 6 inches square plates that ever has been known, was that of igniting 16 inches of wire of $\frac{1}{150}$ part of an inch in diameter. Mr. Wilkinson's trough of 100 pairs of plates of 4 inches square is of much less surface, and as he says, it is a less favourable size, from which, and from the above experiments, I conclude, that such a battery has not the power of igniting one half an inch of wire of *one seventieth of an inch in diameter*, unless galvanic discharges act upon metals in some manner different to common electrical discharges, but with which I am unacquainted; perhaps Mr. Wilkinson will be kind enough to clear up this remark.

I am, with due respect,

Dear Sir, Your very humble Servant,

JOHN CUTHBERTSON.

Poland Street, Soho,

June 19, 1804.

XV.

Chemical Examination of the Ochroites, a Mineral not hitherto well known, containing a New Earth. By Klaproth.*

THE fossil which forms the subject of the present analysis, and to which I have given the name of ochroites, for reasons to be stated hereafter, is found in the mine of Bafnætes, near Riddarhytta in Westmannland. History of the fossil.

The first account of this mineral we owe to Cronstedt, who furnished a description of it, together with that of another mineral, found at Bispsberg in Delecarlien †. Scheele considered both as species of iron ores, and gives to them the name of lapis ponderosus, ponderous stone, (schwerstein) or tungsten, which he describes in his mineralogy as *terrum calciforme, terra quadem incognita intime mixtum* ‡. He likewise examined this tungsten, and made us acquainted with its true nature. The mineral which he examined, was, however, the pearl-coloured tungsten of Bispsberg, and from this he concluded, that the examination of the tungsten of Riddarhytta was necessary, considering it a mineral of the same nature, he distinguished it by the name of *reddish tungsten*. First noticed by Cronstedt; and examined by Scheele

Soon afterwards D'Elhuyar analysed both minerals, he verified the analysis of the true tungsten, but proved that the conjecture of Scheele concerning the other mineral was founded in error; the results of his analysis showed that the fossil known by the name of reddish tungsten, was composed of 54 parts of lime, 24 of iron, and 22 siliceous earth. From what follows, it will, however, become obvious, that this mineral contains neither lime nor tungsten, but a new earth hitherto unknown. and D'Elhuyar.

External Characters of the Ochroites.

The colour of this mineral is between carmoisin red, clove-brown, and reddish-brown. It is compact, breaks, splinter- External characters of Ochroites.

* Gehlen's new Journal of Chemistry, Vol. II. part. iii. p. 303.

† Transactions of the Swedish Academy of Sciences, 1751. p.

235.

‡ And also in Cronstedt's Mineralogy by Magellen, Vol. I. p. 46.

ing

ing in irregular, not very sharp or angular pieces. It is perfectly opaque. Its powder is reddish-gray. It is not very hard, but brittle, and very ponderous.

Its specific gravity is 4,660. Cronstedt states it to be = 4,988.

A.

Analysis of the
Ochroites.

a. A piece of the mineral after having been ignited to redness, lost 2 per cent. Its reddish colour had been changed to brown. Its figure had suffered no alteration.

b. One hundred grains of the finely levigated mineral ignited for half an hour, lost five grains. Its colour was changed to a dark brown.

B.

a. One hundred grains of ochroit, after being mixt with 200 grains of carbonate of potash, were strongly ignited, the mass which could not be rendered fluid, was reddish, grey and brittle. On being diffused through water, as usual, the obtained solution was colourless. It remained perfectly transparent; a proof that it did not contain tungsten oxide; nitrate of silver, mercury, lead, barytes, &c. proved the absence of acids.

b. The insoluble residue of the last process was boiled in nitro-muriatic acid, the siliceous earth being separated, the solution was decomposed by potash, and the whole boiled for some time. The alkaline fluid after being neutralized with muriatic acid, and then mingled with carbonate of potash, suffered no change.

C.

a. 200 grains of the finely pulverized mineral, were first boiled in two ounces of muriatic acid, to which half an ounce of nitric acid was gradually added, and the digestion continued for some time. The whole became thus dissolved except the filix contained in the mineral. Its quantity amounted to 68 grains.

b. To the solution obtained in the last process, carbonate of ammonia was added so long, till no permanent precipitate was produced. On letting fall into it succinate of ammonia, a curdly precipitate fell, which vanished again on agitation,
leaving

leaving merely a pale red precipitate of succinate of iron, This being collected, washed, dried, and strongly ignited, yielded nine grains of oxide of iron.

c. The fluid thus freed from iron, and now colourless, was decomposed by carbonate of ammonia. The precipitate obtained was white, and weighed 168 grains, on being deprived of water and carbonic acid by heat, its white colour changed to cinnamon-brown. It weighed 109 grains.

d. All the water employed for washing the different precipitates were mingled, evaporated to dryness, and the ammoniacal salt volatilized; a minute quantity of a muriate was obtained, the basis of which could not be determined.

From what follows it will become evident, that the cinnamon-brown precipitate (c.) which forms the principal part of the fossil is a peculiar earth, distinct from all the others hitherto known. The characteristic property which it possesses of acquiring a light-brown colour after being heated, has induced me to call it ochroit earth*, which may also serve for the mineral itself.

According to this analysis, 100 parts of the ochroite of Ridderhytta contain,

Ochroit earth	-	-	-	-	54,50
Silex	-	-	-	-	34
Oxide of iron	-	-	-	-	4
Water, &c. (A. b.)	-	-	-	-	5
Lofs	-	-	-	-	2
					100.

Characteristic Properties of Ochroit Earth.

1. Ochroit earth is capable of combining with carbonic acid during its precipitation from acids by carbonated alcalies, and strongly consolidating a portion of water.

Ochroit earth,
1. combines
with carbonic
acid.

100 grains of the earth precipitated by carbonate of ammonia, and strongly dried, lost on being neutralized by nitric acid, 23 grains: 100 grains of the same earth lost after being strongly ignited, 35 grains, 100 parts of carbonate ochroite therefore consists of

Ochroit earth	-	-	-	-	65
Carbonic acid	-	-	-	-	23
Water	-	-	-	-	12
					100.

* From the Greek word *οχρως*, (Havefcens,) brownish yellow.

2. Ochroit earth, after being freed from carbonic acid and water, by heat, always appears in the form of a cinnamon-brown powder. The intensity of the colour is in proportion to the heat applied. This colour is not owing to the presence of iron, or manganese, &c. but it is a characteristic property of the earth.
3. Ochroit earth included in a charcoal crucible, and exposed to the heat of the porcelain furnace, suffered no change whatever.
4. Urged by the blow-pipe, it becomes phosphorescent; fused with phosphate of soda and ammonia, it becomes tinged by it, without effecting a solution of the earth. The salt acquires merely a marbled lemon yellow colour. Borax has likewise no chemical effect upon it. This salt only effects a mechanical division. The earth always appears diffused through the borax in minute *floculi*.
5. Ochroit earth mixed in different proportions with proper fluxes, and applied for painting of porcelain, proved unsuccessful. The painted articles were light brown, but the colour was not uniform; a proof that no combination had been effected.
6. Ochroit earth combined with carbonic acid is easily soluble with effervescence in acids. The taste of the solution is very rough and astringent. The concentrated solution is of an amethyst red colour; diluted with water, it becomes colourless. Ignited ochroit earth, on the contrary, is difficultly soluble in acids in the cold; if nitric acid be employed, the solution is yellowish red.
7. The combination of ochroit earth with sulphuric acid, is crystallizable. The figure of the crystals formed in the mass of the fluids is the octahedron. They are heavy, of a pale amethyst colour, and difficultly soluble in water; but the sulphate of ochroit with excess of acid, is more soluble; the figure of the crystals formed on the sides of the vessel, are needle-shaped, radiating from a centre. They are more soluble than the former.
8. If a solution of sulphate of soda be mingled with a solution of muriate or nitrate of ochroit, a mutual decomposition takes place. A white insoluble precipitate is formed, consisting of sulphuric acid united to the ochroit earth. This combination

1. It is brown.

3. Not reducible.

4. Not fusible as the microcosmic salt nor borax.

5. Gives an uneven brown as a porcelain colour.

6. Difficultly soluble in acid if the earth be pure; but easily if carbonated.

Nitric acid.

7. Sulphate of ochroit is crystallizable, and pale amethyst colour.

Sulphate of soda decomposes muriate or nitrate of ochroites. The insoluble sulphate of ochroites,

bination

bination may be decomposed by boiling it with double its weight of carbonate of soda. By this means ochroit earth may be obtained very pure.

9. Ochroit earth is likewise soluble in sulphureous acid, the solution crystallizes in needles of a pale amethyst colour.

10. Muriatic acid dissolves ochroit earth, and yields crystals, the figure of which is the prism. It is soluble in alcohol without imparting to its flame any particular colour.

12. Acetite of ochroit could not be crystallized, but yielded an adhesive mass.

13. Nitrate and muriate of ochroit is decomposable by carbonated earths and alcalies, the precipitate is milk-white. Alcalies and earths freed from carbonic acid, occasion a yellowish grey precipitate.

14. Prussiate of potash precipitates ochroit from all its neutral solutions, milk white. The precipitate is soluble in muriatic and nitric acid.*

15. Tincture of galls occasions no change in the solutions of this earth.

16. Hidroguretted-hidrosulphuret of ammonia precipitates the solution of ochroit earth, yellowish white.

17. Water impregnated with sulphuretted hydrogen occasions no change in the solutions of ochroit earth.

18. Succinales precipitate ochroit earth white.

19. Phosphate of soda, occasions in the solutions of this earth a white precipitate, which again vanishes by the addition of nitric or muriatic acid.

20. Tartrites of potash also precipitate this earth white.

21. Oxalates effect a like decomposition, the oxalate of ochroit, however, is not soluble in nitric or muriatic acids.

22. Alcalies and alkaline carbonates do not act on ochroit earth.

23. Ammonia feebly acts on it, under certain circumstances, as may be evinced from the following experiment :

A solution of nitrate of ochroit, prepared by dissolving 100 grains of carbonate of ochroit (not absolutely free from iron,) in nitric acid, was decomposed by carbonate of ammonia, and digested in the fluid, containing a considerable quantity of carbonate of ammonia in excess, for some days. The fluid

may be decomposed by boiling with carb. of soda; and the earth obtained pure.

9. Sulphureous acid dissolves ochroit earth; crystallizable.

Muriatic acid also dissolves it, and crystallizes.

The alcohol solution does not colour flame.

Acetite of ochroit not crystallizable.

The nitrate and muriate precipitable by earths and alkalis, and by prussiate of potash; white.

Not by galls, but by hidrog. sulph. of ammonia.

Not by hidrog. sulph. water, but by succinates.

Phosphate of soda precipitates, &c.

Tartrites precipitate it, and oxalates.

Alcalies do not act on the earth.

Ammonia feebly.

* If the earth contained the muriates and quality of iron, it becomes by this means manifested.

which had acquired a yellow colour, was separated and neutralized by sulphuric acid, and then placed in a warm place. A grey precipitate was thus obtained, which on being dried, weighed $1\frac{1}{4}$ grains. This precipitate, after being dissolved in nitric acid, yielded a blue precipitate by prussiate of potash, this being separated, a white flocculent precipitate fell down by dropping into the remaining fluid carbonate of potash. This method is therefore applicable for separating a minute quantity of iron, that may be contained in the fluid.

General remarks and characters of the ochroit earth.

From what has been stated, it becomes obvious, that the ochroit earth bears the nearest relation to yttria, for like this it forms a connecting link between the earths and the metallic oxides. Like yttria it has the property of forming a reddish coloured salt with sulphuric acid, and is precipitable by prussiate of potash, but it differs from yttria, that it does not form sweet salts, that it is not (at least very sparingly) soluble in carbonate of ammonia, and that when ignited it acquired a cinnamon brown colour. It farther differs from yttria by not being soluble in borax or phosphate of soda when urged upon charcoal before the blow-pipe, which salts easily effect a solution of yttria, and melt with it also into a pellucid pearl.

XVI.

*Letter from VAN MARUM to J. C. Delam  therie, on Ritter's Galvanic Experiments.**

MR. ORSTED of Copenhagen, on his way through Harlem, having shewn me, by means of the apparatus in Teyler's Museum, some discoveries of his friend Ritter of Jena; † I made at the same time a new experiment with him, which affords a fresh proof of the identity of the fluid set in motion by Volta's pile and the common electrical machine, and an account of which I imagine will be acceptable for your Journal.

Mr. Orsted having shown me, that two wires of platina, after having been kept five minutes in the chain of communication between the two extremities of a galvanic pile, acquired

* *Journ. de Physique*, December, 1803, Vol. LVII. p. 471.

† See Ritter's experiments, as communicated by Dr. Orsted, in this present and some other numbers of the Journal.

New experiment in proof of the identity of electricity and galvanism.

Two wires of platina, kept 5 minutes in the chain of communication between the two poles of the pile, convulsed the legs of a frog,

thereby

thereby the faculty of throwing the legs and thighs of a frog into convulsive movements, according to the discovery of Mr. Ritter; I proposed to him, to pass the stream of fluid from a powerful electrical machine through these same wires of platina, in order to observe, whether it would have the same effect on them as the steam from the galvanic pile. We employed the plate machine of 35 inches diameter, and of the new construction, described in the *Journal de Physique* for June, 1791, vol. 38, (or Philosophical Journal, quarto series, No. 2.)* Having passed the stream from it through the wires of platina, held a quarter of an inch from the conductor, for the space of five minutes, we touched with them the crural nerves of a frog prepared in the usual manner, and observed immediately the same convulsive movements in its legs and thighs, though not so strong as in the preceding experiment. This less powerful effect of the stream from the machine perfectly answered my expectations, since my experiments in November, 1801, described in my letter to Mr. Volta, shewed me that the stream from the machine had not more than five sixths of the velocity or strength of the stream set in motion by a similar pile.

We then repeated the experiment, keeping the wires of platina in contact with the conductor of the machine, while we passed the stream through them. Then holding the wires one in each hand, as in the preceding experiment, and in contact with the two crural nerves, but keeping their opposite extremities separate, their effect on the same frog, the sensibility of which was greatly weakened, was scarcely perceptible: but on bringing the upper extremities of the wires together, while the lower remained in contact with the crural nerves, we noticed very striking convulsions in the legs of the frog, every time the extremities were made to touch. A little sealing wax, which had been used to keep the wires of platina insulated when held to the conductor, and which still adhered to one of the upper ends, rendered the experiment still more evident; for when we brought these ends together in such a manner, that a little of the wax prevented them from being in perfect contact, the legs of the frogs exhibited none of the

and exposed to the stream of a plate machine for a like time did the same, but more weakly.

The experiment was repeated, the wires having been kept in contact with the conductor; when the sensibility of the frog being much weakened, the effect was scarcely perceptible, unless the upper extremities of the wires were brought into contact. When a little sealing wax prevented the absolute contact of the wires, no effect took place.

* It is a machine, that produces positive and negative electricity, like the pile of Volta. *Delamétherie.*

convulsive

convulsive movements, that took place, notwithstanding every time the wires were made to touch each other completely.

Thus we have a fresh proof of the identity of the fluid set in motion by the pile of Volta, and by a common electrical machine.

XVII.

Experiments on Light; by Mr. RITTER, of Jena. Communicated by Dr. ORSTED.†

THE important discovery of invisible solar rays, with which Herschel has enriched natural philosophy, has given occasion to another little known, even in the country where it has been made.

Our knowledge of light had made no perceptible advancement since Newton, when Herschel found, that all the phenomena occurring during the decomposition of light by means of the prism had not yet been noticed. Philosophers had contented themselves with perceiving different colours, without examining by other processes, whether phenomena imperceptible to the eyes did not take place. Herschel, by means of the thermometer, discovered invisible rays exterior to the solar spectrum, that possess the property of raising the mercury. Mr. Ritter repeated his experiments with success; but considering, that the different rays of light produce very different chemical changes in bodies susceptible of them, he conceived, that light likewise contained invisible rays, which acted chemically. He exposed muriate of silver to the action of the solar spectrum, and found his conjecture substantiated: the muriate of silver very soon became black beyond the violet edge of the spectrum; blackened a little less in the violet itself; and this action was still less in the blue, diminishing thus more and more in proportion to the distance from the violet, till it became null. On exposing muriate of silver a little blackened, that is to say, a little disoxygenated, to the same action of light, its white colour was partly restored by the red ray, and still more by the invisible ray beyond it.

Herschel's discovery of invisible solar rays has given rise to another.

Our knowledge of light at a stand since Newton;

till Herschel found, that invisible rays, beyond the spectrum, raised the thermometer. Ritter, repeating the experiment, conjectured, that light contained invisible rays acting chemically likewise; and found, that muriate of silver, exposed to the spectrum, grew blackest beyond its violet edge, and the effect diminished in proportion to the distance from this, and the muriate a little disoxygenated had its colour partly restored by the in-

* *Journal de Physique*, December, 1803, Vol. LVII. p. 409.

The solar spectrum therefore is accompanied by two invisible rays, one on the red side, which favours oxigenation, and the other on the violet side, which favours difoxigenation. The visible and coloured rays partake more or less of the properties of these invisible principles, whence we ought to infer, that all the coloured rays contain more or less of these principles.

These experiments succeed very well with phosphorus. On letting the invisible ray adjoining the red fall on it, it immediately emits white fumes: but if the invisible ray adjoining the violet be thrown on the phosphorus in a state of oxigenation, it is instantly extinguished, with the same rapidity as a frog is convulsed in galvanic experiments.

These experiments readily accord with some others made by the same gentleman. He kept one of his eyes for some minutes in contact with the negative conductor of Volta's pile, and after this operation all objects appeared red to him: but after having kept it in contact with the positive conductor, he saw every thing blue. It is to be observed here, that the retina and optic nerve, when the external part of the eye is brought into the negative state, become positive, and *vice versa*; because the eye is filled with a fluid, in which the same distribution of electricity must take place, as in water and other fluids. It is in the positive state therefore, that the optic nerve perceives all objects of a red colour; and in the negative state they appear violet. The chemical action of positive electricity likewise is the same as that of red light; that is, they both favour oxigenation. Negative electricity and the violet ray possess the same analogy, both promoting difoxigenation: as the experiments with the galvanic pile have sufficiently shown.

If I might be allowed to add any observation to these important discoveries, I would mention one of the most commonly known facts; that the opposite electricities, when united, produce light; which seems to demonstrate synthetically, what the preceding experiments have shewn by analysis.

This account was read to the philomathic society several months ago, since which time Mr. Ritter has published some new observations that deserve notice. He has found, with all the prisms he used, that the solar rays give two coloured spectrums, which spread in proportion to their distance from the prism, so that at a certain distance one nearly covers the other.

The two spectrums;

visible ray beyond the red. Thus there is an invisible ray promoting oxigenation, and another the contrary, and the visible and coloured rays partake more or less of the properties of both.

The invisible ray on the red side likewise kindles phosphorus; while that on the violet side extinguishes it instantly.

One of the eyes being kept some minutes in contact with the negative conductor of Volta's pile, all objects appeared red to it; in contact with the positive blue.

In the former case the optic nerve is in the positive state; in the latter, negative.

The chemical action of positive electricity likewise favours oxigenation, like red light; and negative, the contrary.

The two electricities too produce light by their union; which seems to prove synthetically their identity with the chemical rays.

Mr. R. has since found, that the prism affords

distinct at a little distance, as 4 inches, but confounded together when farther off.

And the chemical rays may be completely separated from the coloured, without destroying them.

So that we may produce a coloured spectrum devoid of chemical action.

The calorific rays have not been proved to be separable from the chemical; but it may be presumed they are, since the former differ in degree in winter and summer, the latter do not.

That they accompany each other, though separable, no greater difficulty than attends electricity.

The experiment must be made at a very small distance from the prism, that of four inches for example, to distinguish the two spectrums, which become confounded together in proportion to their distance from the prism. No doubt this is the reason why the phenomenon has so long escaped the notice of philosophers. This observation was accompanied by another, still more important; namely, that the chemical rays may be completely separated from the coloured rays. If the invisible rays of the violet side be made to fall on the red part of the solar spectrum, the process of oxidation may be completely suspended there, and even disoxidation produced, without destroying the red colour; and by means of several prisms we may even separate all the coloured rays from the chemical rays. We may thus produce a coloured spectrum devoid of chemical action; and a series of chemical rays, analogous to the spectrum, without any mixture of coloured rays.

We have not yet any accurate experiments, to determine whether the calorific rays be likewise separable from the others, particularly from the chemical rays: but the comparison of different experiments made in winter and summer, when the degrees of heat are different, though no difference in the force of the chemical rays at different seasons has been observed, lead to a belief that the calorific rays are separable from the chemical.

It may be asked, why the different rays found in light frequently accompany each other, though they can subsist separately: and this question no doubt may be answered, when we are able to say, why the different functions of electricity accompany each other, though they likewise are separable.

XVIII.

On Spontaneous Inflammations. By G. C. BARTHOLDI, Professor of Physic and Chemistry.*

Definition of spontaneous inflammation.

SPONTANEOUS inflammation is that which is manifested in a combustible body, without the immediate contact of any ignited matter.

* From the *Annales de Chimie*, Vol. XLVIII. p. 249, or No. 144.

These

These kinds of combustion may be occasioned by different Causes. causes, the principal of which are:

1. Considerable friction.
2. The action of the sun.
3. The disengagement of caloric, produced in bodies, which, though not combustible, by being brought near to other combustible bodies, may communicate such a degree of heat to them, that they inflame by the contact of the air.
4. The fermentation of animal and vegetable substances heaped up in a large mass, which are neither entirely dry, nor too wet, such as hay, dung, &c.
5. The accumulation of wool, cotton, and other animal and vegetable substances, covered with oily matter, particularly drying oil.
6. The preparation of linseed oil for printers' ink, of varnishes, and in general of all fat.
7. The torrefaction of different vegetable substances.
8. The sulphurated and phosphorated hydrogen gases which are disengaged in many of the operations of nature, and of which, the latter generally inflames by the sole contact of atmospheric air, even at a low temperature, and which is often seen at the surface of the earth like a small flame, known by the name of Jack o'Lantern, in places in which there are animal substances in a state of putrefaction: if other combustibles are met with where the disengagement takes place, they readily catch fire.
9. The phosphuret of lime and potash, which may be formed in the preparation of charcoal, particularly in that from turf, and from some sorts of wood which grow in marshy situations. This charcoal by being wetted, or by simply attracting the humidity of the air, forms phosphureted hydrogen gas, which, by the contact of the atmospheric air inflames, and may set fire to the whole mass of charcoal.
10. The phosphorus which is sometimes, though rarely, formed in the carbonisation of different sorts of wood, without combining either with lime or with potash in the state of phosphuret. These charcoals do not inflame spontaneously at the common temperature of the atmosphere, but they produce a detonation by percussion with nitrate of potash, or with some other nitrates and metallic oxides to which the oxygen adheres but feebly, and which, being in the state of thermoxide, retain much latent caloric.

I. *Friction.***Friction.**

It is generally known that by rubbing two bodies against each other, they are heated; the intensity of the produced heat depends on several circumstances, and chiefly varies in proportion to the hardness of the friction, to the nature, and to the surface of the rubbing bodies: if the friction takes place between combustible bodies, such as the woods, the heat which it excites is frequently sufficient to inflame them: if the bodies are not combustible, such as stones, or but little combustible, like the metals, they do not themselves inflame, but they can communicate such a degree of heat to other combustibles which surround them, that these can inflame by the contact of atmospheric air.

Dr. Palcani having repeated the experiments which have been long known for obtaining fire by the friction of two pieces of wood, to one of which he gave the form of a tablet, and to the other that of a spindle or a cylinder, he has allowed me to give the results of some of his experiments here, to shew that more attention should be paid to the choice of the woods which are destined to rub against each other, in the construction of machines and instruments.

Experiments with a tablet between two cylinders,	Two cylinders of	Tablet.	Duration.	Effect.
	Box-wood	Box-wood ^{rubbed against}	5 Minutes	Sensible heat
	<i>Idem</i>	Poplar	<i>Idem</i>	<i>Idem</i>
	<i>Idem</i>	Oak	<i>Idem</i>	<i>Idem</i>
	<i>Idem</i>	Mulberry	3 Minutes	Cons. heat and smoke
	<i>Idem</i>	Laurel	<i>Idem</i>	<i>Idem</i>
	Laurel	Poplar	2 Minutes	<i>Idem</i>
	<i>Idem</i>	Ivy	<i>Idem</i>	<i>Idem</i>
	Ivy	Box-wood	3 Minutes	<i>Idem</i>
	<i>Idem</i>	Hazle	<i>Idem</i>	<i>Idem</i>
	Olive	Olive	<i>Idem</i>	<i>Idem</i>
	Mulberry	Laurel	2 Minutes	Con. he. fm. & black
	Ash	Oak	5 Minutes	Sensible heat
	<i>Idem</i>	Fir	<i>Idem</i>	<i>Idem</i>
	Pear-tree	Oak	<i>Idem</i>	<i>Idem</i>
	Cherry-tree	Elm	<i>Idem</i>	<i>Idem</i>
	Plum-tree	Apple-tree	<i>Idem</i>	<i>Idem</i>
	Oak	Fir	<i>Idem</i>	<i>Idem</i>

On changing the experiment, and rubbing a cylinder of one of these woods between two tablets of the other, for example, a cylinder of poplar between two tablets of mulberry, the augmentation of the rubbed surfaces which are in contact with the air, produced a much more considerable heat, and nearly all the woods mentioned above took fire.

The effect of the friction also varies according as the woods employed, particularly if they are of the same species, are rubbed in the direction of the grain of the wood, or when the grains of the woods cross each other. In the first case the friction and the heat are much more considerable than in the second.

In large machines, in which there is a great deal of friction, the heating is prevented by directing a continual current of cold water on the rubbing surfaces: in common machines, and in coaches, waggons, &c. it is diminished by covering the rubbing surfaces with some oily matter. There have been many examples, during the great heat of summer, of coaches, and other machines subjected to a rapid motion, having taken fire, because the greasing them had been neglected. The grease, by hardening on the rubbing surfaces, instead of diminishing the friction, increases it; and as this covering is very combustible, it renders spontaneous inflammation still more easy. It is also preferable, in many circumstances, to rub machines with soap, talc, plumbago, or other substances, which, without being oily, are very unctuous to the touch.

2. Action of the Sun.

By concentrating the solar rays with convex glasses or concave mirrors, the strongest heat is produced, all sorts of combustibles are set on fire, and the most refractory substances are melted: it may happen that other bodies may be found in circumstances, in which, without our concurrence, they produce the effects of glasses and burning mirrors: although these effects are rather physical than chemical, it is nevertheless essential to make them known, to guard against their danger. There are examples of fires produced by large glass bottles, filled with water and exposed to the sun, in an apartment. Whenever the form of the vessel is nearly similar to that of a lenticular

lenticular or spherical glass, the rays are refracted, and by uniting in a focus, produce a heat capable of setting fire to the combustible bodies within it.

3. *The Heat excited in Bodies not combustible.*

Heat from in-
combustible
bodies.

It is known that quick-lime plunged into water, or simply moistened, produces a considerable heat. This method has even been employed with success for heating apartments, green-houses, hot-beds, &c. at little expence. This property of quick-lime of disengaging much heat by the contact of water, and that, not less dangerous, of dissolving or corroding animal substances immersed in it, require the greatest precautions when a considerable quantity of quick-lime is kept together. To preserve it, it must be protected from the contact of the air and of every species of humidity, and it must be carefully kept at a distance from all combustible bodies, such as wood, hay, straw, &c. which might inflame spontaneously, if the lime contracted the least humidity. The *Journal de la Haute-Saône* gave an account, last year, of the destruction of a barn, one of the wooden partitions of which took fire from a heap of quick-lime, intended for the repairs of the farm, having been carelessly laid against it.

In nature a great number of analogous phenomena occur, in which bodies, by changing their composition, or by contracting new combinations, heat so much, or disengage such a quantity of caloric, that other combustibles which are near them may take fire.

(*To be continued.*)

XIX.

Discovery of two new Metals in crude Platina. By SMITHSON
TENNANT, Esq. F. R. S.

New metal
Iridium.

AT the last meeting of the Royal Society a paper of Mr. Tennant was read, on the analysis of the black powder which remains after dissolving platina, shewing that it contains two new metals. Mr. Tennant's first experiments were made last
summer,

summer, and had been communicated to Sir Joseph Banks, after which an account of one of these metals appeared in France, by M. Descotil *, and also by M. Vauquelin. The properties ascribed to it by the French chemists are, 1. That it reddens the precipitates of platina made by sal ammoniac; 2. That it dissolves in marine acid; 3. That it is precipitated by galls and prussiate of potash. The properties mentioned by Mr. Tennant are, that it dissolves in all the acids, but least in marine acid, with which it forms octahedral crystals. The solution with much oxygen is deep red, with a smaller proportion green or deep blue. It is partially precipitated by the three alkalies when pure. All the metals, excepting gold and platina, precipitate it. Galls and precipitate of potash take away the colour of this solution, but without any precipitate, and afford an easy test of its presence. The oxide therefore loses its oxygen, by water alone. When combined with *gold or silver*, it cannot be separated by the usual process of refining these metals. As the French chemists have not given a name to the metal, Mr. Tennant inclines to call it *Iridium*, from the various colours of it in solution.

The second new metal is obtained by heating the black powder with pure alkali in a silver crucible. The oxide of this metal unites with the alkali, and may be expelled by an acid and obtained by distillation, being very volatile. The oxide has a very strong smell, from which Mr. Tennant has called it *Osmium*. It does not redden vegetable blues, but stains the skin of a deep red or black. The oxide in solution with water has no colour, but by combining with alkali or lime becomes yellow. With galls it gives a very vivid blue colour. All the metals, excepting gold and platina, precipitate this metal. If mercury is agitated with the aqueous solution of the oxide, an amalgum is formed, which, by heat, loses the mercury, and leaves the osmium pure as a black powder.

* See our Journal, Vol. VIII. p. 118.

SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

Figure of the Orbits of the new Planets. By JEROME DE LALANDE.

Figure of the orbits of the new planets. THE mean distance of each of these planets from the sun is 2,77, which answers to 227 million of geographical miles nearly.

Piazzi or Ceres, discovered Jan. 1, 1801.

Revolution 4 years, 7 months, 10 days.

Mean longitude, Jan. 1, 1804	-	-	-	10 ^s 11 ^o 59'
Annual motion	-	-	-	2 18 14
Aphelion	-	-	-	10 26 44
Node	-	-	-	2 21 6
Equation of the orbit	-	-	-	9 3
Eccentricity	-	-	-	0, 079
Inclination	-	-	-	10 37

Albers or Pallas, discovered March 28, 1802.

Revolution 4 years, 7 months, 11 days.

Mean Longitude, Jan. 1, 1804	-	-	-	9 ^s 29 ^o 53'
Annual motion	-	-	-	2 18 11
Aphelion	-	-	-	10 1 7
Node	-	-	-	5 22 28
Equation of the orbit	-	-	-	28 25
Eccentricity	-	-	-	0,2463
Inclination	-	-	-	34 39

Philosophical Transactions of the Royal Society of London, for the Year 1804. Part I. 4to, 182 Pages, with five Plates and 26 Pages of Metereological Journal.

Philosophical Transactions of the Royal Society.

THIS Part contains—1. The Bakerian Lecture, Experiments and Calculations relative to Physical Optics; by Thomas Young, M. D. F. R. S. 2. Continuation of an Account of a peculiar Arrangement in the Arteries distributed on the Muscles of slow moving Animals, &c.; in a Letter from Mr. Anthony Carlisle to John Symmons, Esq. F. R. S.

3. An

3. An Account of a curious Phenomenon observed on the Glaciers of Chamouny; together with some occasional Observations concerning the Propagation of Heat in Fluids; by Benjamin Count Rumford, V. P. R. S. Foreign Associate of the National Institute of France, &c. &c. 4. Description of a triple Sulphuret of Lead, Antimony, and Copper, from Cornwall; with some Observations upon the various Modes of Attraction which influence the Formation of Mineral Substances, and upon the different Kinds of Sulphuret of Copper; by the Count de Bournon, F. R. S. and L. S. 5. Analysis of a triple Sulphuret of Lead, Antimony, and Copper, from Cornwall, by Charles Hatchett, Esq. F. R. S. 6. Observations on the Orifices found in certain poisonous Snakes, situated between the Nostril and the Eye; by Patrick Russell, M. D. F. R. S.: with some Remarks on the Structure of those Orifices, and the Description of a Bag connected with the Eye met with in the same Snakes; by Everard Home, Esq. F. R. S. 7. An Enquiry concerning the Nature of Heat, and the Mode of its Communication; by Benjamin Count Rumford, V. P. F. R. S. Foreign Associate of the National Institute of France, &c. 8. Experiments and Observations on the Motion of the Sap in Trees; in a Letter from Thomas Andrew Knight, Esq. to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. Appendix, Metereological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

Analytical Essays towards promoting the Chemical Knowledge of Mineral Substances. By MARTIN HENRY KLAPROTH, Professor of Chemistry, Assessor to the Royal College of Physicians, Member of the Royal Academy of Sciences at Berlin, and various other learned Societies. Vol. II. 8vo, 267 Pages. Translated from the German. Cadell and Davies.

THIS work is translated by the same learned chemist to whom the scientific world is obliged for the former volume. Its valuable contents are as follow. 73. Examination of the Auriferous Ores from Transylvania. 74. Analysis of the sulphated Oxyd of Manganese from Transylvania. 75. Examination of Tungstate of Lime (Scheelium). 76. Gadolinite. 77. Examination of the Egyptian Natrum (Soda). 78. Striated

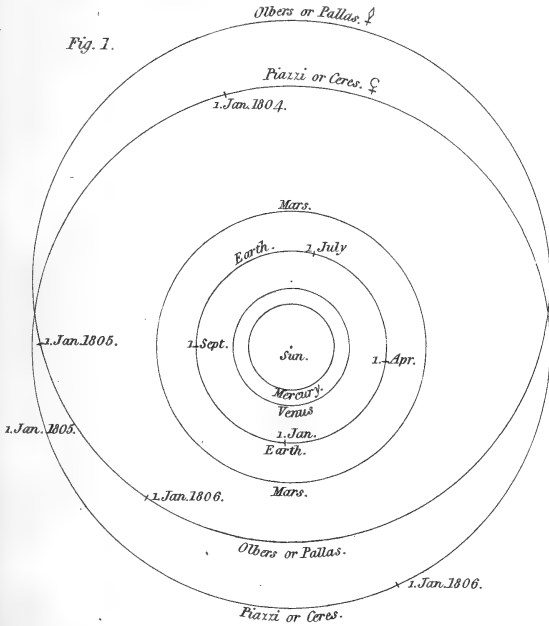
Analytical
Essays.

Analytical
Essays.

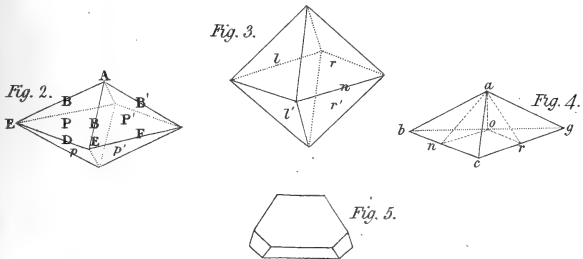
ated Soda. 79. Analysis of the native Muriate of Ammoniac. 80. Examination of Saffolin. 81. Examination of the Plumose Alum from Freyenwalde. 82. Capillary Salt (Halotrichium) from Idria. 83. Elastic Bitumen, from Derbyshire. 84. Examination of Mellilito. 85. Umbra (Umber). 86. Examination of the muriated Lead Ore. 87. Phosphated Lead Ores. 88. Sulphated Lead Ores. 89. Tabular, White Lead Ore, from Leadhills. 90. Examination of the native Reguline Antimony, from Andreasberg. 91. Antimoniated Silver, from Andreasberg. 92. Fibrous red Antimonial Ore. 93. White Ore of Antimony. 94. Arseniated Olive Copper Ore. 95. Muriated Copper Ore. 96. Phosphated Copper Ore. 97. Krijolite. 98. Beryl. 99. Emerald. 100. Examination of Klingstone (Echodolite). 101. Basalt (Figurate Trapp). 102. Pitch Stone. 103. Addition to the Analysis of Pumice Stone (Essay 33). 104. Examination of the Jargon (Zircon) from Norway. 105. Examination of Madrepomite. 106. Pharmacolite. 107. Scorza. 108. Examination of the Fibrous Sulphate of Barytes. 109. Tabular Spar (Safel-spath). 110. Examination of Miemite. 111. Examination of the prismatic, Magnesian Spar, from the Territory of Gotha. 112. Examination of the striated grey Ore of Manganese. 113. Earthy, black Oxyd of Manganese. 114. Examination of the Asphaltum from Albania. 115. Earthy brown Coal. 116. The Hungarian Pearl Stone.

* * W. N. acquaints G. S. that the supposition of Du Hamel, that what was called mild volatile alkali contains chalk, arose from a want of knowledge of the carbonic acid at the early period alluded to in his letter. The additional weight in the carbonate of ammonia arises from carbonic acid, and there is no reason to suspect any volatilization of the earth during its preparation. If any were present, it might be detected by sulphuric acid, or, in preference, by oxalic acid, either of which would carry down the lime.

Sketch of the Orbits of the new Planets,
by Jerome de la Lande.



Arseniated Copper.





Telegraph by the human figure.



<i>a</i>	<i>e</i>	<i>i</i>	<i>o</i>	<i>u</i>	<i>y</i>
<i>b</i>	<i>c</i>	<i>d</i>	<i>f</i>	<i>g</i>	<i>h</i>
<i>j</i>	<i>k</i>	<i>l</i>	<i>m</i>	<i>n</i>	<i>p</i>
<i>r</i>	<i>s</i>	<i>t</i>	<i>v</i>	<i>x</i>	<i>z</i>

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*Machine for Laying—Lond. level,
by M^r. David Charles.*

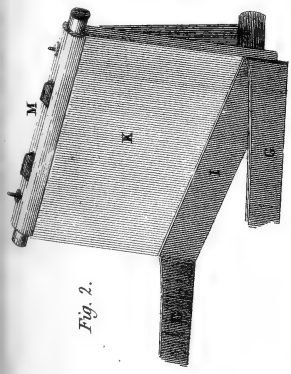


Fig. 2.

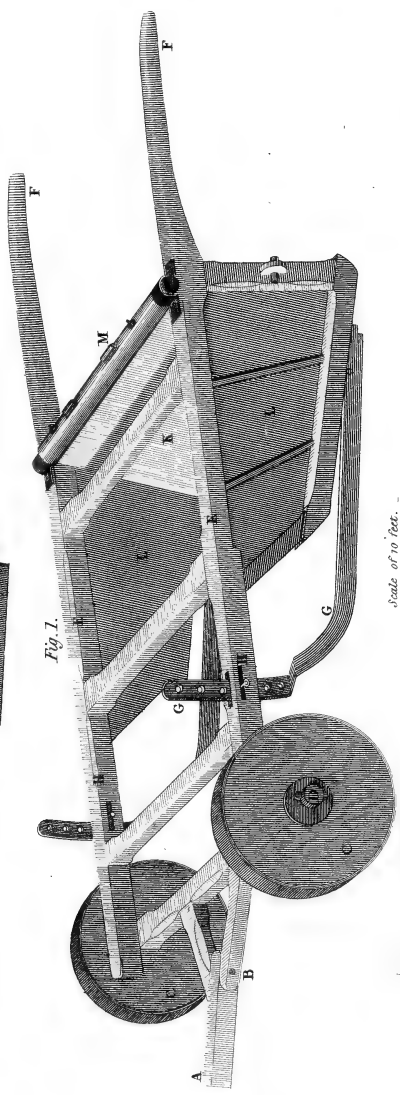
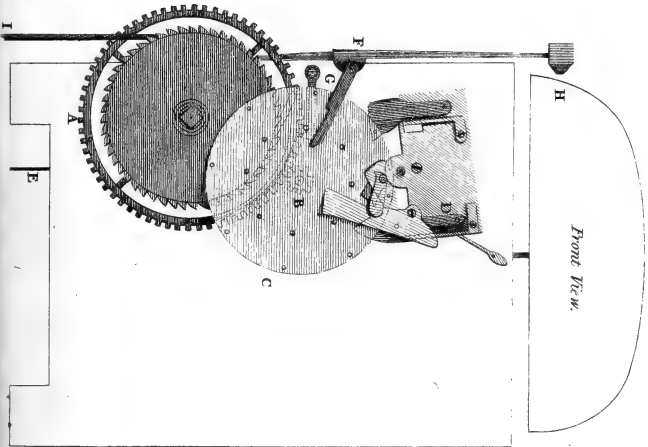
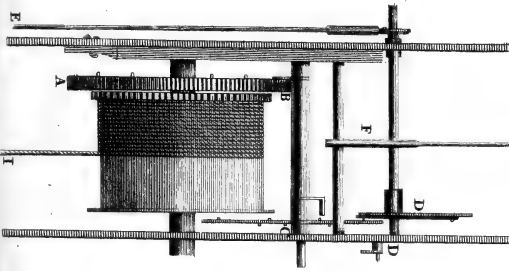


Fig. 1.

Scale of 10 feet.



Lock to strike without a Fly,
by Mr. D. Haysop.
Side View.





A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1804.

ARTICLE I.

On the Pastes, coloured Glasses, or Enamels, of the Ancients.

By M. KLAPROTH.*

SECTION FIRST.

THE invention of glass, which, in various respects is so highly valuable a production of art, is among those ancient discoveries of which history has transmitted us some account. Pliny relates it as follows: † “*Fama est, adpulsa nave mercatorum nitri, cum sparsi per litus epulas pararent, nec esset cortinis attollendis lapidum occasio, glebas nitri e nave subdidisse. Quibus accensis, permixta arena litoris, translucentes novi liquoris fluxisse rivos; et hanc fuisse originem vitri.*” If we suppose this account to be merely an unsupported tradition, still it contains in itself no circumstance that might render it questionable. It deserves rather the more credit, as it is hardly possible to imagine this invention could have had any other origin than that of accident.

History of the invention of glass. Pliny's Account.

* Read in the Royal Academy of Sciences at Berlin, October 4, 1798.—Translated by N. N. who received a copy from the author.

† Lib. XXXVI. Cap. 65.

Glass-houses at
Diofpolis in the
most remote
ages,

and among the
Phenicians.

The nitrum or
natrum of the
ancients was
soda;

or rather it con-
sisted of any salt
left upon the
ground (not
muriatic);

This impure salt
was not detri-
mental to the
glass.

The art of co-
louring glass is
of nearly the
same antiquity as
glass itself.

Though Pauw and some other antiquaries are more inclined to ascribe this discovery to the Egyptians, who are said to have constructed the first glass-house, in the remotest ages, at Diofpolis, the ancient capital of Thebais; yet it likewise appears, from the writings of the ancients, that this art must have arrived at a considerable degree of perfection, chiefly among the Phenicians; as also, in general, this nation seems, in her flourishing age, to have been almost in the exclusive possession of manufactures. Sidon, that colony of theirs so flourishing by commerce, arts, and manufactures, was not less famous on account of her glass-houses. These, according to the testimony of Pliny, obtained for some hundred years the chief ingredients of their glass from the sea-shore near the Phenician town Acco, afterwards called Ptolemais, and now St. John d'Acre, in the vicinity of the small river Belus, which there empties itself into the Mediterranean.

The substance which the ancients employed for the purpose of vitrifying the sand, is comprehended by the early authors under the name of *nitrum*; but it has long been generally agreed, that they did not mean by it our nitrate of potash, but the mineral alkali or soda; consequently their *nitraria* were not nitre-works, but, strictly speaking, refineries of soda. And from the descriptions which Pliny and others have given of their *natrum* and its properties, it is rendered probable, that in those times all saline substances, whether efflorescing upon the soil or left by dried lakes, if not belonging to the muriatic genus, were considered as *natrum*. Hence undoubtedly, among those salts often occurred real nitre as well as native sulphate of soda.

However, such confusion in their use in manufacturing glass has not produced any real detriment; since the longer time during which the ancients exposed their materials for glass to the action of the fire, has been more than sufficient to decompose those neutral salts, and to expel from them their acid constituent parts.

The art of colouring glass seems to be of nearly the same antiquity as the invention of making it; as is evident not only from several passages in the ancient writers, but may also be proved by actual documents, and, among others, by the variously coloured glass-corals, with which several of the preserved Egyptian mummies are decorated. This art supposes the possession of some chemical knowledge of the metallic oxides, because these are the

only

only substances capable of producing such an effect. But it would be a problem of difficult solution to determine, what were the means and processes employed by the ancients for this purpose, as they had no acquaintance with the mineral acids, which, at present, are usually employed in the preparation of metallic oxides. It is nevertheless certain, that the art of giving many various colours to glass must, at least in later times among the Greeks and Romans, have reached an eminent degree of perfection; for they knew how to imitate, by their pastes of glass, even those gems which have a deep colour, so as to deceive the eye very considerably. A proof of this, among others, is afforded by the following words of Pliny *, relating to the artificial imitation of the *carbuncle*, a gem then in the highest estimation: “*Adulterantur vitro similime: sed cote deprehenduntur, sicut aliæ gemmæ factitiæ.*”

Though the ancients were ignorant of our mineral acids, they made coloured pastes in high perfection,

It was in the time of Augustus that the Roman architects began to make use of coloured glass in their mosaic decorations, besides the several species of marble and other coloured stones, which, before, were usually employed with that design. Such an application of the glass-pastes was resorted to in a villa built by the emperor Tiberius on the island of Capri, as is shewn by specimens lately found among its ruins. I subjected some of these in my possession to chemical analysis, chiefly for the purpose of discovering what metallic substances the ancients employed to tinge those variously coloured masses of glass.

Roman mosaic work of glass,

I. *Antique Red Glass.*

The colour of this glass-paste is a lively copper-red. The mass is perfectly opaque, and very bright at the place of recent fracture. This is probably the very same glass, of which Pliny says: * “*Fit et totum rubens vitrum, atque non translucens, Hæmatinon adpellatum.*”

Antique red glass; opaque.

(a) Two hundred grains of this red glass were finely triturated, and, together with 400 grains of *caustic potash*, ignited for half an hour; by which management the mixture soon entered into a thin fusion. After cooling, the whole mass was softened with water, then supersaturated with *muritic acid*; and, after this mixture had been again inspissated to a saline

Analysis, 200 grains, triturated and fused with 400 gr. potash;

softened with water; supersaturated with muritic acid; inspissated; diffused in much mass, hot water.

* Lib. XXXVII. Cap. 26.

mass, it was again diffused in a large quantity of boiling water, to which a slight portion of muriatic acid had previously been added. *Siliceous earth* separated; which, collected,edulcorated and ignited, weighed 142 grains.

Silex fell down.

The remaining solution deposited crystals of muriate of lead by evaporation.

(b) The filtered solution possessed a green colour; and when concentrated by evaporation, it deposited needle-shaped crystals. When on continuing the evaporation no more such crystals would appear, the remaining fluid was diluted with spirit of wine, and thrown upon the filter. The collected crystals were washed with spirit of wine and dried in a warm temperature, upon which they weighed $32\frac{1}{2}$ grains. They consisted of muriated lead, equivalent to 28 grains of gently ignited *oxide of lead*.

The liquid was then superaturated with ammonia, and *alumine* was separated:

The rem. fluid being sat. with mur. acid, then evap. and precip. by oxalate of potash, gave ox. of lime.

Copper was then precip. by iron.

(c) I then superaturated with *caustic ammonia* the solution thus freed from its contents of lead. It was now of a dark-blue colour, and let fall a grey precipitate; which being separated, the solution was again neutralized with muriatic acid, reduced by evaporation, and upon this combined with *oxalate of potash* as long as any turbidness ensued. The precipitate thence formed was *oxalate of lime*, which after strong ignition yielded three grains of *pure calcareous earth*.

(d) The ingredient copper was now precipitated from the solution, by immersing into it a polished piece of iron. The reguline copper obtained by this process amounted to twelve grains, for which fifteen grains of oxidized copper must be put in the account.

Purification of the precipitated alumine.

(e) The above grey precipitate (c) thrown down by the caustic ammoniac, was mixed and digested with liquid *caustic soda*. When to the filtered solution, again superaturated with muriatic acid, *carbonate of soda* was added, *aluminous earth* fell down, which after washing and ignition amounted to five grains.

Insoluble part was iron.

(f) The remaining part that was left undissolved by the caustic lye, appeared of a black-brown tinge. This, after washing and exposure to red heat, weighed two grains, and was *oxidized iron*.

Hence, according to this analysis, the sum of the constituent parts of the *two hundred* grains of the *red antique glass-paste* decomposed, consists of,

Silex

<i>Silex</i>	-	-	(a)	-	142 grains.
<i>Oxide of Lead</i>	-	-	(b)	-	28 —
— of <i>Copper</i>	-	-	(d)	-	15 —
— of <i>Iron</i>	-	-	(f)	-	2 —
<i>Alumine</i>	-	-	(e)	-	5 —
<i>Lime</i>	-	-	(c)	-	3 —

Composition of
the red glass.

195

On comparing the external characters of this red glass-paste with the cupreous scoriæ of a lively brown-red, such as is sometimes obtained on melting copper ores, it is rendered highly probable, that the ancients did not compound the above paste directly from its simple constituent parts, but instead of them have perhaps employed copper scoriæ. On that supposition they had nothing more to do, than to select the best coloured pieces to fuse and cast them into plates.

It was probably
a scoria from
copper works.

II. *Antique Green Glass-Paste.*

The colour of this green paste is a light verdigris. Its mass like that of the preceding, is opake, and of a scoriaceous splendent fracture.

Antique green
glass; opake.
Analysis as in
§. 1.

For its chemical analysis I employed *two hundred grains*, which, having been treated in exactly the same manner as the foregoing, I found to consist of the following ingredients:

<i>Silex</i>	-	-	-	130 grains.
<i>Oxide of Copper</i>	-	-	-	20 —
— <i>Lead</i>	-	-	-	15 —
— <i>Iron</i>	-	-	-	7 —
<i>Lime</i>	-	-	-	13 —
<i>Alumine</i>	-	-	-	11 —

Component parts
of the green
enamel;

196 grains.

This green glass-paste, then, contains the same constituent parts as the red, only in different proportions. Both receive their colour from copper: But the reason why this metal produces in the one a red, and in the other a green colour, depends on the different degrees of its oxidation or saturation with oxygen.

Differ only in
proportion of
parts from the
green.

It is one of the chemical properties of copper, that in the state of a sub-oxide, that is only half saturated with oxygen,

it produces a copper-red enamel; while, on the contrary, when perfectly oxidized or fully saturated with that acidifying principle, the enamel which it yields is green. Pliny mentions several preparations of copper that were in use in his time; he only dwells too long on enumerating their pretended medicinal virtues. Of such artificial preparations of copper some might have been serviceable in making green glass-pastes, in the case that, perhaps, the native oxides of copper, of which in particular the copper-mines on the island of Cyprus could afford copious quantities, were not then employed for this purpose.

III. *Antique Blue Glass.*

Whether the ancients coloured glass by cobalt.

My leading object in chemically decomposing this glass, was the solution of the question: What was the colouring matter which the ancients employed in order to tinge their glass blue? The striking similarity of the colour of the blue antique glass to that of our modern, which, as is well known, is tinged by means of cobalt, has induced several learned men to conjecture, that even the ancients must have been acquainted with this fossil, as well as with its properties of colouring glass blue. This was, likewise, the opinion of Ferber, when in his *Letters from Italy*, page 114, he says: "In the villa Adriani near Tivoli, near Frascati, and in several places, antique mosaic works have been found which exhibited some cubes of a blue vitreous composition, and serve as a proof, that the ancients must have known the use of cobalt and the preparation of smalt." This opinion he repeats in various places.

The affirmative generally,

but erroneously supposed.

This opinion being supported by no chemical proof, rests solely on the supposition, that cobalt is the only substance which is capable of affording a blue enamel. However, it is certain the ancients knew the art of giving, by means of iron, a blue colour to glass resembling that which we produce by cobalt.

The contrary shewn by Gmelin,

A chemical demonstration of this fact has been given by Gmelin of Göttingen, in his *Chemical Examination of a Blue Glass from an antique Mosaic Fragment**, which was found in digging a garden at Mümpelgard, and is probably of Roman

* *Commentat. Götting. Vol. II.*

origin. It is true that Gmelin could, in his examination, employ no more than the small quantity of a few grains; but the results were sufficient to shew, that the colouring principle in his specimen originated not from cobalt but from iron.

A like result is afforded by the following decomposition of the blue glass from the ruins at Capri. and by our author.

Its colour is a *sapphire-blue* verging towards that of smalt. Antique blue glass; almost perfectly opaque.
 It is transparent on the edges only. Its fracture, as well as that of the preceding, comes nearer to the scoriaceous and conchoidal than to the splintery. Some of these blue glass-plates are particularly distinguished by this circumstance, that they are not coloured blue throughout the whole of their mass, but only to about two-thirds of their thickness. Each of the strata is so nicely distinct from the other, as to give the appearance of two plates adhering at their broad surfaces; the one blue, the other colourless. Some plates are blue only to a certain depth.

(a) Two hundred grains of the above blue paste were reduced to a subtile powder, and fused with 400 grains of *caustic soda*. Fusion with soda, &c. gave flux. The obtained mass, softened with water, was saturated to excess, and evaporated to a moderate dryness. When redissolved in boiling water, it deposited *siliceous earth*, which, after washing and ignition, amounted to 163 grains.

(b) The fluid was then supersaturated with *caustic ammonia*. Alumina by the process with ammonia, &c. A brown precipitate thence ensued, which, uponedulcoration, I digested with a solution of *caustic potash*. The slight portion taken up of it by this alkali was again, after saturating the liquor with an excess of muriatic acid, precipitated by means of *carbonated soda*, and proved, uponedulcoration and red heat, to be *aluminous earth*, amounting to three grains.

(c) What remained undissolved by the caustic potash, was merely *oxidized iron*, weighing nineteen grains when ignited and washed. Oxidized iron.

(d) The liquor that had been supersaturated with caustic ammonia and possessed a blueish tinge, was by slow evaporation so far reduced to a smaller volume, that the greatest part of the muriate of soda, which had been generated and contained in it, could separate in crystals. The fluid separated from these, in which the acid predominated, and which now hardly exhibited any perceivable greenish colour, was in vain examined The liquid contained a little copper and lime; but no cobalt.

mined for cobalt *. It contained only a slight trace of copper and lime. The first of these was made to appear, by combining the fluid with *prussiate of potash*, and the brown-red precipitates thus obtained amounted to a little more than two grains, which are to be considered equivalent to about one grain of *oxidized copper*.

(e) At last, carbonated soda threw down about half a grain of *calcareous earth*.

Component parts
of the blue paste
as enamel.

In consequence of this decomposition, those *two hundred* grains of the antique blue glass-paste, must have contained the following earthy and metallic constituent parts:

<i>Silex</i>	-	-	(a)	-	163.	grains.
<i>Oxide of Iron</i>			(c)	-	19.	—
<i>Alumina</i>	-	-	(b)	-	3.	—
<i>Oxide of Copper</i>			(d)	-	1.	—
<i>Lime</i>	-	-	(e)	-	0.5	—
					186.5	

Other experiments were made but shewed no cobalt.

Iron can afford a blue enamel; as is seen in the smelting works.

As I have subjected the above blue glass to several other experiments, merely with an intention of discovering the cobaltic portion it might possibly contain, yet without finding the least trace of it, there appears to be no doubt, that its blue colour entirely depends on the ingredient iron. That iron, under some circumstances, is capable of producing a blue enamel, is clearly shewn by the beautifully blue coloured scoræ of iron, which frequently are met with in the high furnaces on smelting siliceous iron-stones. But we are not sufficiently acquainted with the circumstances and conditions under which this colour is produced; for the assertion of Henckel, and some other earlier authors, that by means of iron, cemented with arsenic, the same blue tinge can be given glass which it acquires from cobalt, has not yet been sufficiently

* It is well known that nature tinges the *sapphire, lapis-lazuli, blue clays, &c.* by means of *iron* without cobalt; but man is not possessed of her means. A chemical friend, with whom the translator had a conversation on this subject and on the difficulty of proving the accuracy of the above *analysis* by a *synthetical process*, suggested the idea, that such a blue paste could, perhaps, be made without cobalt by the intermedium of *lapis-lazuli*; an idea which may afford a subject for experiment.—*Transl.*

confirmed.

confirmed. Whence, after the discovery of the blue from cobalt, the art of tingeing glass blue by means of iron has had the same fate with several other attainments now lost; namely, to have been discarded and forgotten on the account of new invented, more commodious, and certain expedients and methods.

SECOND SECTION.

THESE coloured mosaic glass-pastes of the ancients agree, with respect to their opacity and scoriaceous fracture, with our modern enamels. On the other hand, the deceitful imitations of gems already mentioned before shew, that the ancients likewise knew how to prepare beautiful, high-coloured, and transparent glass-pastes.

But however well known those works in glass of the ancients may be, since both earlier and later writers have given sufficient information of them, and several specimens preserved in the collections of antiquaries afford a direct knowledge of this subject; it is, on the contrary, very surprising, that antiquaries are so little acquainted with that entirely peculiar and by far more remarkable painting on glass, which is formed of variously coloured delicate glass fibres, joined with the greatest nicety, and by subsequent fusion conglutinated into an homogeneous compact mass. In the earlier works on antiquities this scarce production of art is not at all mentioned; the reason of which is probably this, that the specimens now existing of it were found, perhaps, only about the middle of this (last) century.

Among later antiquaries Count Caylus appears to be the first, who in his *Collections of Antiquities* has given information, accompanied by rather inadequate drawings, of this singular species of mosaic work. Winkelmann has afterwards, in his *Annotations on the History of the Art among the Ancients*, (page 5, *seq.*), more accurately described two other antiques of this kind; with the appellation, *Pictures made of Glass-Tubes*, in the following passage: "The works of the ancients in glass, which are not noticed in the *History of the Arts*, deserve particularly to be mentioned in this place; more especially, because the ancients carried the art of working in glass to a much higher degree than we have arrived at; a fact which, to those who have not seen their works of this kind, might

The preceding glasses resemble our enamels;

but the ancients possessed another method of painting, little known to antiquaries:

It is formed of delicate fibres of glass united by fusion:

First mentioned by Count Caylus.

Ample description by Winkelmann.

might have the appearance of a groundless assertion." After which he mentions a floor formed of green glass-plates discovered in the Farnese-island, as well as some fragments of glass-cups, which must have been turned on the lathe, and then proceeds as follows: "But the art strongly claims our admiration in two small pieces of glass, which last year (1765) were brought to Rome. Each of them is not quite one inch long, and one-third of an inch broad. One plate exhibits, on a dark ground of variegated colours, a bird representing a duck of various very lively colours, more suitable to the Chinese arbitrary taste, than adapted to shew the true tints of nature. The outlines are well decided and sharp, the colours beautiful and pure, and have a very striking and brilliant effect; because the artist, according to the nature of the parts, has in some employed an opaque, and in others a transparent glass. The most delicate pencil of the miniature painter could not have traced more accurately and distinctly, either the circle of the pupil of the eye, or the apparently scaly feathers on the breast and wings, behind the beginning of which this piece had been broken. But the admiration of the beholder is at the highest pitch, when, by turning the glass, he sees the same bird on the reverse, without perceiving any difference in the smallest points; whence we could not but conclude, that this picture is continued through the whole thickness of the specimen; and that, if the glass were cut transversely, the same picture of the duck would be found repeated in the several slabs; a conclusion which was still farther confirmed by the transparent places of some beautiful colours upon the eye and breast that were observed. The painting has on both sides a granular appearance, and seems to have been formed, in the manner of mosaic works, of single pieces; but so accurately united; that a powerful magnifying-glass was unable to discover any junctures. This circumstance, and the continuation of the picture throughout the whole substance, rendered it extremely difficult to form any direct notion of the process or manner of performing such a work. And the conception of it might have long continued enigmatical, were it not that, on the section of the fracture mentioned, lines are observable, of the same colours which appear on the upper surface, that pervade the whole mass from one side to the other; whence it became a rational conclusion, that this kind

Very curious antique enamel of a duck;

traced and finished with extreme accuracy and effect;

and continued through the whole thickness of the piece, minutely the same on both surfaces!

It is found to consist of threads of glass seen endwise.

of painting must have been executed by joining variously coloured filaments of glass, and subsequently fusing of the same into one coherent body. The other specimen is of about the same size, and made in the same manner. It exhibits ornamental drawings of green, white, and yellow colours, which are traced on a blue ground, and represent volutes, beads, and flowers, resting on pyramidally converging lines. All these are very distinct and separate, but so extremely small that even a keen eye finds it difficult to pursue the subtle endings, those in particular in which the volutes terminate. Notwithstanding which, these ornaments pass uninterruptedly through the whole thickness of the piece."

Another specimen no less admirable.

Of the same glass-paste, which has been here described, mention is made by Sulzer in his *Theory of the Polite Arts*, under the article *Mosaic* (mosaïch). Having seen the piece itself in the house of its then possessor, Casanova, at Dresden, he confirms, in the capacity of an eye-witness, the description given by Winkelmann, and calls it "a remnant of antiquity, which indicates the existence of an art brought to the highest degree of perfection."

The same were seen by Sulzer.

Mr. Townley, of London, enumerates, among the principal rarities of his celebrated cabinet of antiquities, the stone of a ring of a similar antique glass-paste, which represents a bird so small a delineation, that it cannot be distinctly seen but by means of a magnifying lens.

Extremely minute bird in Mr. Townley's collection.

As very few specimens of this species of glass-painting, which undoubtedly must be reckoned among the lost attainments of art, and of which even the existence is still so little known, are met with; I think it not superfluous to give the following notice of two new samples which I possess of this class of antique subjects. Both pieces have a heart-shaped form. The principal front is flat, the reverse is convex, and has from eight to ten prominences (*Ecken*). The length of one of them is one inch, the breadth four-fifths, and the thickness two-fifths of an inch. The other specimen is *two-thirds* smaller. As to colouring and manner of drawing, they are both nearly alike. The principal mass of the *larger* is of a dark-blue, and wholly opaque; but that of the *smaller* is a sapphire-blue, and in some places transparent. The blue ground is embellished with voluted, stellular, minute flowers, of so very small a delineation as to be hardly imitable by the

Two specimens in possession of the author.

Description.

pencil of the miniature painter. The colours of these flower-like ornaments, which are red, green, brown, sky-blue, and white, are pure and lively. The delineations pervade the whole substance, and upon a broken part it is seen by mere inspection, that these delicate figures have been formed of parallel glassy fibres of various colours, conglutinated by means of gentle fusion.

Coloured plate of the larger specimen.

As the drawings given by Count Caylus afford but a very imperfect idea of that ingenious enamel-painting, I submit here a delineation of the larger of my two specimens. *Fig. 1, Plate XIII.* represents it in its true size; *Fig. 2* exhibits it magnified.

Invention of Meyer to multiply copies of paintings.

On this occasion the following passage from a letter of M. Kästner*, concerning an invention of the celebrated Tobias Mayer, may well deserve a place here. "Mayer possessed the art of making a number of perfectly similar copies of a painting. He compounded his picture of coloured wax-crayons, in the same manner as a prism may be composed of thinner prisms of the same length. Every transverse cutting afforded then a copy." Meyer might probably have been led to this method of imitation in wax, from the inspection of a mosaic work of the kind here described.

II.

On Spontaneous Inflammations. By G. C. BARTHOLDI, Professor of Physic and Chemistry.

(Concluded from Page 220.)

4. *The Fermentation of Animal and Vegetable Substances.*

Heat from fermentation.

THE greater part of animal and vegetable substances, when they still retain humidity, and are accumulated into large masses, enter into fermentation, a change in their composition is effected, and they frequently heat to the point of inflammation. It is thus that magazines of hay, of turf, of flax, of hemp, stacks of hay or straw, heaps of linen-rags in paper-mills, &c. take fire spontaneously.

* *Allgemeine Geographische Ephemeriden*, by Zach, 1798.

It is principally hay which requires precaution; if the hay-harvest happen in rainy weather, it is commonly stacked before it is thoroughly dry, and in this state is more disposed to ferment and to heat. If a hay-stack is observed to be in fermentation, great care must be taken not to throw it down too suddenly, the exterior layers must be carefully detached one after the other. It almost always happens, that when a hole is made in the middle of a stack of heated hay, it takes fire suddenly.

Nothing, however, is more easy than to prevent these fatal accidents: when there is any reason to fear that the hay which is intended to be housed or stacked, is not sufficiently dry, it is only necessary to scatter a few handfulls of common salt (muriate of soda) between each layer. It would be very ill judged to regret this trifling expence; for the salt, by absorbing the humidity of the hay, not only prevents the fermentation and consequent inflammation of it, but it also adds a taste to this forage which stimulates the appetites of cattle, assists their digestion, and preserves them from many diseases.

Utility of adding salt to hay.

During the great heats of summer, it frequently happens that heaps of dung inflame spontaneously: great care should be taken to sprinkle them frequently with water in the summer season, and to keep them at a certain distance from habitations, as well to prevent fires as with a view to salubrity.

5. *The Accumulation of Animal and Vegetable Substances covered with an Oil.*

If animal and vegetable substances heaped into a large mass, can take fire from the heat produced by their decomposition, this accident is still more to be dreaded when they are covered with oily matters, and particularly with a drying oil.

Heat from greasy animal and vegetable matters.

Besides the accident which happened at the manufactory of Lagelbart, and of which our colleague Hauffman gave an account to the Society, and the fire which took place in one of the finest manufactories at St. Marie-aux-Mines, there are many other examples of wool, stuffs, and pieces of cloth, which were not freed from grease, taking fire in the warehouses when they were folded together, and even while moving them from one place to another when they were in large quantities: this is principally to be dreaded when linseed oil, or any

any

ny other oil, drying in itself, or rendered so by oxide of lead, is employed in the preparation of these stuffs.

In the manufacture of cloths, only olive oil, or oil of colza, should be used to greafe the wool.

It sometimes happens on boiling flowers and herbs in oil, which occurs in several pharmaceutical operations, that, after being taken out, the herbs dried in the oil inflame spontaneously: care should therefore be taken when these herbs are thrown away, not to heap them near other combustible bodies.

There have been several examples of vessels having been burnt in sea-ports, either by the spontaneous combustion of heaps of cordage coated with tar, or by a mixture of linseed oil boiled with lamp-black, and inclosed in a bag.

6. *The boiling of oily Matters.*

Fire from boiling
oily matters;

In the preparation of some varnishes, such as printers' ink, in which linseed-oil, boiled to a certain consistence, is generally made use of, it frequently happens that the oil takes fire, unless the necessary precautions are employed: the same effect takes place in melting butter, lard, or any other greafe, if they are heated too much; so that, in these operations, it is always necessary to remove every other combustible substance, to have a lid at hand to cover the vessel as soon as the fire has caught, and particularly to take care not to pour water upon it, which instead of extinguishing it, would spread it more and give it greater activity.

7. *Torrefaction.*

and from roasted
vegetable sub-
stances.

There are many vegetable substances which increase their power of inflaming spontaneously by torrefaction, if they are inclosed in sacks of cloth, which leave them in contact with the surrounding air; such are saw-dust, roasted coffee, the meal of grain, and leguminous fruits, such as French beans, lentils, pease, &c.

There have been several instances of stables having taken fire from a bag of roasted bran which had been applied to the neck of a sick animal, and had inflamed spontaneously. The inhabitants of the country who, in some disorders of their beasts, are obstinate in applying this remedy, to which others
of

of more efficacy and less danger might be substituted, should at least be careful not to inclose the bran in the cloth, either too hot or too much roasted.

Brewers, after having made their barley and other grain, which they employ in making beer, germinate, dry it in a kiln, except what is intended for pale beer, and they generally roast it more or less highly, to give the beer a deeper or paler colour. If, therefore, when the grain is brought from the kiln, it is put, still hot, into sacks, it frequently happens that it heats and takes fire, and this has occasioned several fires in breweries.

8. *Sulphurated and phosphorated Hydrogen Gas.*

The cause of subterraneous fires and volcanoes is generally attributed to the decomposition of pyrites, or metallic sulphurets, buried in the interior of the earth. These pyritous masses are decomposed by the contact and concurrence of water and air, and the decomposition is always accompanied by a great expansion of caloric, and a disengagement of a very inflammable gas, called sulphurated hydrogen gas. This gas inflames at an elevated temperature, and can communicate the inflammation to the sulphur of the pyrites, to the coal and other bituminous matters which generally accompany it.

Inflammation of sulphurated and phosphorated hydrogen gas.

Similar inflammations are sometimes observed near coal-pits. In exploring the coal, veins and insulated masses of pyrites are frequently met with: since these pyrites always communicate a bad quality to the coal, the miners generally lay them aside, and throw them out of the pit: if these heaps of pyrites, mixed with coal, are then exposed to the alternate action of the sun and rain, they heat and inflame. Great care must therefore be taken that these accumulations of pyrites should be kept at a distance from all combustible bodies to which they would necessarily communicate the inflammation.

There are many operations of nature in which sulphurated hydrogen gas is formed, but it often enters into other combinations as it forms, it dissolves in water, or disengages at a temperature too low for it to inflame.

By boiling phosphorus in a solution of potash or lime, phosphorated hydrogen gas is disengaged, which, being much more combustible than sulphurated hydrogen gas, inflames at a low temperature as soon as it comes in contact with atmospheric air.

This

This gas, which in chemical experiments offers the beautiful spectacle of a fountain of fire over water, is produced naturally by the putrefaction of animal substances which are buried. The lights which are frequently seen to come out of the earth, and which are known by the name of Jack o' Lanterns, are only owing to the disengagement of this phosphorated hydrogen gas: as these lights generally appear moving about in places where they do not touch dry combustibles, they seldom occasion disagreeable accidents; but they are also disengaged in forests, and it may happen that in hot summers, when the grass and brush-wood are thoroughly dry, the gas in combustion may meet with these combustible materials, and set fire to them, and thus produce the conflagration of a whole forest: we should not therefore, too lightly, and without sufficient proofs, attribute to the malevolence or to the connivance of mankind, those dreadful events which are sometimes only the result of causes purely natural.

9. *Sulphuret and Phosphuret of Lime and of Potash formed in the Combustion of several Vegetables.*

Sulphuret and phosphuret of lime or potash.

Sulphur is always formed when gypsum (sulphate of lime) or any other sulphate, whether earthy or alkaline, is strongly heated with charcoal, wood, or, generally, with any combustible which is reduced to charcoal by heat. The same salts form sulphureous waters if the remains of animal or vegetable substances are left in a water in which it is dissolved; so that it frequently requires only a little sulphate of lime, or some other sulphuric salt, to communicate the odour and taste of sulphur to water which is stagnant.

Pyrophorus.

Pyrophorus is obtained by calcining the alum of commerce, or sulphate of potash with sugar, meal, or any other substance which is reducible to charcoal.

The inflammation of pyrophorus which takes fire by the sole contact of humid air, is only owing to the sulphuret of potash, which by attracting the humidity of the air, heats to that point that it kindles the carbonaceous matter which surrounds it, and which being in a state of greater tenuity, is so much the more disposed to burn.

A pyrophoric matter may be formed in ordinary combustion.

But since many of our common combustibles contain sulphuric salts, it may happen that, in their combustion, a pyrophoric matter is sometimes formed by chance, which remains in the residue

residue of the combustion, especially if the combustible is not entirely consumed, and a part is not reduced into charcoal, which frequently happens in the fire-places in which the combustibles are not burned in grates, and the ashes are not separated from the charcoal. There have been instances of houses having been burnt by ashes intermingled with the charcoal which had been taken too early from the fire-place and put into places where they were surrounded with combustibles, which they set on fire by a spontaneous inflammation. Happily these causes of conflagration rarely occur; for pyrophorus does not retain its property of inflaming for any length of time, and it is frequently decomposed shortly after its production, without occasioning any unpleasant event: nevertheless, care should always be taken not to put ashes newly burned, and which are still intermingled with charcoal, in places where they can communicate with combustibles.

The formation of a pyrophoric matter is principally observed in the preparation of the soda of commerce, which is obtained by the incineration of different maritime plants containing much sulphate of soda, and which, in the combustion, sometimes furnish a certain quantity of sulphur, greater or less, according to the manner in which the operation is directed.

The formation of phosphuret of lime has great analogy with that of sulphuret of lime. Although the phosphoric acid is not found so often in vegetables as the sulphuric acid, it nevertheless exists in them in greater quantity than has hitherto been believed: it is principally found in most plants which grow in marshy places, in turf, and in several species of the white woods. In reducing these woods into charcoal, a small quantity of phosphorus is sometimes formed, which may remain combined with the same bases as retained the phosphoric acid before the combustion: the phosphorus, by forming other combinations, may be rendered incapable of occasioning any accident, but it may also happen from a concurrence of several circumstances, that charcoal impregnated with any phosphuret whatever, may, by exposure to the action of a warm and humid air, disengage phosphorated hydrogen gas, which, by the contact of the atmospheric air, will take fire and communicate the inflammation to the mass of the charcoal.

Phosphoric acid is abundant in vegetables.

Two examples of this kind of spontaneous combustion have taken place in the powder magazine of Elstone, in the years 8

Spontaneous inflammation of charcoal in a powder magazine.

and 10. The first time the receiver of the machine for sifting the charcoal caught fire, and the second time it began in the magazine of charcoal, without a suspicion of any other cause except that of a spontaneous inflammation. The different reports made on these two events, have been inserted in the public journals; but the explanations that were given of them were not satisfactory. It seems very probable that they were occasioned by some phosphorus contained in the charcoal; and this explanation has the more weight, because willow (*bourdaine*), which is used at Essone, as well as in most other powder-manufactories, and which, in many respects, deserves the preference over other woods in the preparation of powder, contains phosphoric acid, at least that does which grows in our neighbourhood.

Charcoal from turf begins to be employed in some domestic and other operations; but as it is much disposed to spontaneous inflammation, its use should be prevented, or at least it should be stored with great precaution. It has happened at Paris and other places, that magazines of this charcoal, which were unsheltered, have taken fire by the combined action of the heat and rain.

10. *Phosphorus sometimes contained in Charcoal.*

Detonation from the phosphorus in charcoal.

It may also happen that the small quantity of phosphorus which is sometimes formed in the carbonization of different sorts of wood, without uniting either with the lime or the potash, remains combined with the charcoal, which then does not disengage phosphorated hydrogen gas, nor does it readily inflame by the sole action of water or of a humid air, but which, by percussion with salt-petre (nitrate of potash) may produce a powerful detonation. It is very probable that the three successive explosions which took place in the powder-mill at the manufactory of Vonges, were partly owing to a similar cause.

Charcoal probably one cause of the bad quality of cold-short iron.

Charcoal has, in general, great influence on the different productions of nature and the arts. It is frequently observed in forges and founderies, particularly in those of iron, that the products vary according to the nature of the charcoal employed. The bad quality which is sometimes found in iron, of being brittle when cold, is generally attributed to the phosphoric acid contained in the ores: but since the same ore, by the same processes,

cesses; furnishes better iron in one foundery than in another, the difference seems frequently to arise in part from the charcoal.

Such are the principal causes of spontaneous combustions, whose effects are so much the more dangerous by being least expected. The Society of Emulation thought they should render an important service to every class of proprietors, and particularly to the inhabitants of the country, by developing the physical knowledge which might guard them against dangers, of which they are too frequently the victims, from ignorance and a fatal want of foresight. I trust I have fulfilled the wish of the Society, and of the first Magistrate, whose intelligence and constant sollicitude extend, without exception, to every object which may contribute to the prosperity of the country and the happiness of the governed.

III.

On the Solution of Water in the Atmosphere; and on the Nature of atmospherical Air. By Mr. JOHN GOUGH. From the Author.

To Mr. NICHOLSON.

SIR,

I DO not recollect any philosopher or meteorologist, who has attempted to demonstrate the chemical union of atmospherical air and water, by help of the following facts and arguments. Should the present endeavour, to establish the proposition, appear deserving of a place in your Journal, the insertion of it will oblige,

Yours, &c.

JOHN GOUGH.

Middleshaw, July 16, 1804.

Exp. 1. If a cylinder of dry porous wood be put into a strong glass tube, nearly of the same diameter with itself, and water be poured into the vessel, the particles of the fluid will penetrate the wood, and cause it to swell, so as to burst the glass. Some writers affirm, that the same artifice has been used with success to split rocks, an operation which is commonly performed by the elastic power of gun-powder. A quantity of

Dry vegetables
attract water.

motion is generated in this experiment, which cannot be referred to the action of gravity. We must therefore ascribe it to another force, namely, the mutual attraction of wood and water in a liquid form.

Aqueous vapour attracted by dry vegetables, &c.

Exp. 2. If a piece of whip-cord or tharm be stretched by the heaviest weight, it can support, the pendent body will ascend, as oft as the string or gut contracts, in consequence of an accession of water derived from the atmosphere; on the contrary, it will descend, when the cord begins to relax from the loss of moisture. The motion generated in this instance, proves atmospherical vapour to be powerfully attracted by the dry fibres of vegetables and animals; consequently these substances have a strong affinity to water, not only in a liquid form, but also when it is diffused through the air. This affinity or force will be called the hygrometrical attraction in the sequel of the essay, for the sake of perspicuity.

The force of affinity permanent.

The preceding experiments have not the least claim to novelty; but they are the preliminaries of an inference, which is of moment in the present question. For affinity is a fixed relation of bodies, creating a disposition to coalesce, in such as are thus mutually related, as often as water is combined with another substance. The union must therefore be permanent, unless it happens to be dissolved by an external cause. Now as any certain force only gives way to another superior and contrary to itself, it is evident that a moist body, which discharges a portion of the water it contains, is obliged to part with it by a more powerful attraction, existing in its neighbourhood. It is to be remarked, that temperature is one of the external causes alluded to above; but it is disregarded at present, because the effects of its changes may be obviated in the following experiment, which is intended to throw additional light upon the hygrometrical attraction.

Hygrometrical attraction is diminished by the accession of water.

Exp. 3. Take two bibulous substances, such as two slices of sponge, or a piece of sponge and shred of woollen cloth. Make the one wet and keep the other dry; then put them both into a close vessel of glass or metal, placing them either in contact or apart: the wet body will grow lighter in a short time, and the dry one will gain more weight; this process may be prolonged, until the two substances find the equilibrium of their attractive powers; which will be accomplished, when their respective weights become stationary. This equilibrium proves the

the

the hygrometrical attraction to diminish with the absorption of water, and to increase with the loss of the same; consequently an union produced by this force, may be dissolved by the presence of a body, which contains less water, and therefore attracts it more powerfully.

Atmospherical air may be concluded to possess the power described above, from the changes and effects, which are observable in the following instances: First, Atmospheric air takes the water of crystallization from various salts; it therefore overcomes the affinity, which unites the component parts of these crystals. Secondly, The same hygrometer denotes a greater degree of humidity at one time, than at another, though the height of the thermometer be the same; consequently the hygrometrical attraction of the atmosphere is variable under equal degrees of temperature; because this force is evidently constant in an instrument kept in an uniform heat. Third, If two vessels be exposed, at the same time to the air; one of which contains dry potash, and the other a dilute solution of the same; the former will acquire weight, while the other grows lighter. The last fact shows, that atmospherical air may be saturated with moisture, in respect of one body, and be at the same time in a very different situation relative to another; so that evaporation evidently arises from an excess of hygrometrical attraction in the atmosphere; on the contrary, the production of dew depends upon a similar excess in the bodies on which it is formed.

I may be asked, after making this open declaration of my sentiments, which of the constituent gases of the atmosphere combine with water? The proper reply to the question appears to be this: It is atmospherical air; which I consider to be a homogeneous gas, for the following reasons:—First, The atmosphere is diaphanous; which could hardly be the case, were it a mass of uncombined fluids of different specific gravities; for, had such an arrangement been formed, the rays of the sun would have suffered a multiplicity of refractions, in their approach to the earth; and total darkness, or at best a dim twilight, would have been the consequence, had our planet been shrouded by a covering of heterogeneous gases. Thus the atmosphere appears to be homogeneous, from the consideration of its transparency.—Secondly, A given measure of oxygen is heavier than an equal bulk of azote, under similar circumstances;

Atmospherical air possesses this attraction.

Atmospherical air a simple gas,

because it is diaphanous,

because smart reports are not double to sense.

Atmospherical
air a simple gas.

circumstances; consequently the density of the former exceeds that of the latter, supposing their elastic forces to be equal. On this account sounds will move, in all cases, with less celerity in oxygen than they do in azote. If then our atmosphere consisted of two independent masses of these fluids, mutually pervading each other, every momentary report would have been double to sense, at a sufficient distance from the seat of sound; because such a report would arrive at the ear more expeditiously through the medium of the azote, than it would through that of the oxygen. But sounds of the shortest duration are not repeated at the greatest distances; consequently the air is homogeneous, because it is the vehicle of sound.

If the preceding arguments be just, the homogeneity of atmospherical air cannot be controverted; because the conclusions which result from the contrary hypothesis are repugnant to common experience. We come in the next place to the specific nature of this gas; but this is a difficult enquiry in the present unsettled state of chemistry, when the phenomena of galvanism are daily bringing new truths to light, and threaten to subvert the prevailing theory. Conjectures, however, will naturally spring up in the midst of uncertainty; and as a diversity of sentiment has its use in times of scientific anarchy, I will venture to propose the following hypothetical questions relative to the constitution of common air. Is not this fluid a chemical compound, having the gas called azote for its basis; to which the positive energy of the galvanic pile is united, together with water, but in a manner which distinguishes this compound from the gaseous oxide of azote? May not a gas, thus constituted, oxidate other substances through the interposition of the water, which it holds in solution by the hygro-metrical attraction? Though the aqueous part of the atmosphere cannot of itself decompound common air; may not it perform the office of an intermediate agent, when assisted by the body to be oxidated, and in this manner deprive the azote of the galvanic energy, more or less perfectly, according to circumstances? Will not the aqueous vapour unite with the matter separated from the air, and produce oxygen gas, which will enter into composition with the third substance, and complete the business of oxidation? The hints suggested in the preceding queries, would have been by no means admissible in a time of more perfect uniformity in the sentiments of philosophers;

losophers; and nothing can be pleaded in its excuse but the revolution of opinion, which is apparently ready to take place in the theory of gaseous fluids.

IV.

*Reply to the Observations of M. l'Abbé Hauy, on arseniated Copper. By M. LE COMTE DE BOURNON, Member of the Royal and Linnean Societies of London.**

IT is but a few days, Sir, since I had the honour to receive Introductory remarks. from you the observations which you have made on the different species of arseniated copper, described by me, in a memoir on that subject, read to the Royal Society of London, on the 19th of February, 1801. I have read these observations with the greatest interest, but not being able to adopt the opinion respecting them, to which the investigations you have submitted them have led you, I feel great obligation for the opportunity you have afforded me of explaining myself more particularly than I have hitherto done, on what relates to this interesting subject. Besides, you offer these observations with that diffidence which usually characterizes real merit, accompanied with a doubt which calls for a new examination.

You oppose my opinion, Sir, on the division which I have made of the arseniated copper into four species, with a delicacy and a politeness, which renders the slight mineralogical discussion that becomes the necessary result of it, of infinite value to me. It is very desirable that those facts on which differences of opinion may prevail should always be discussed in this manner: the sciences would certainly gain by it, and those who cultivate them would lose nothing by yielding a little to each other.

Like you, Sir, when I employed myself on the substance Notice of the former enquiries. which, since the first essays of the celebrated Klaproth on it, has been called a combination of the arsenical acid and copper, I thought it right to consider, under the same point of view, the different crystalline forms which it offered, deriving them all from one common base, and my first enquiries were

* Translated from the original, communicated by the author. For the paper of the Abbé, see p. 187 of our present vol.

directed

directed to determining this base, or the primitive generating crystal of all those of this substance. I was not long in discovering that among the crystals which I had subjected to this examination, there existed two forms which could not, in any way, be connected with the others: analysis has since shown, that one of these belonged to an arseniated iron, which had been improperly cited as belonging to copper, and the other to a combination of copper and iron with the arsenical acid, which had not been known before. With respect to the other crystals, as the appearance offered by each of them contradicted the opinion which connected them, it became necessary to depend in the best possible manner on all the other exterior characters which this substance could offer to the mineralogist, to attain to some result respecting it. This is precisely what I have done, and when the aggregate of these characters forced me to recognize four very distinct species in the mass of substances, which I suspected might belong to the combination of the arsenical acid and copper, I confess I experienced some satisfaction in observing that the analysis of a chemist, so justly esteemed as Mr. Chenevix is, sanctioned, in some measure, the division to which observation had led me. You remark, Sir, that these analyses, on being repeated by M. Vauquelin, varied in their result: it follows necessarily, that this support fails, or at least becomes uncertain for me: I abandon it therefore, and leave to chemistry the discussion of a fact which belongs to it, and was to me only a powerful auxiliary, to confine myself within the strict limits of mineralogy properly so called.

Preliminary observations on the mode of conducting mineralogical enquiries.

But allow me, Sir, first to make some previous observations on the method which, it appears to me, should be followed to determine the union or separation of substances, and afterwards, on the possibility of finding several species placed under the combination of the same acid with the same base, but without doubt, having essential differences in the manner of combination.

The methods to be employed by the mineralogist in the study of mineral substances, are comprized in the examination of the peculiar marks which nature has impressed on each of the individuals which decorate and enrich its bosom, and which his great habit of observing has taught him to recognize. Of these marks, which we designate by the expression of exterior specific

cific characters, some are too delicate to be described; but custom enables the naturalist to seize them; their action on his sight is sudden; the most rapid glance embraces the whole of them, and the naturalist has frequently formed his opinion long before he has thought of accounting to himself for it. He is not, however, secured by them from the errors which other bulkier and more comparable characters may afterwards rectify; but the first impression received from these first traces, very often serves him as a guide in the method of employing the second. Among these latter characters, some are of easy application and almost always possible, others require attention and particular circumstances to be capable of being employed. Those which are in most common use, and easiest, are the form, the fracture, the hardness, the specific gravity, and the colour. Perhaps in a skilful hand, directed by the habit acquired from their use, these characters are almost always sufficient for the knowledge and classification of mineral substances. In stones, the colour is the most variable of all: nevertheless, it is certain, though the true cause cannot yet be assigned, that each of those which have been examined hitherto, affects one only of the known colours more readily than it does any of the others. But in the metals this character becomes more constant and more essential, and it very seldom varies without the cause of its variation being a change in the nature of the metallic substance itself.

This fact granted, when the naturalist employs the exterior specific characters, to ascertain the subject which determines his enquiry, from the moment at which the agreement of these characters, or their differences with those shown by known substances, puts him in a situation to pronounce on the identity or the difference of their nature, do not you believe that he has then the liberty of retrenching, on the one hand, those which do not agree with the opinion which he had previously thought it right to embrace; and, in the second place, to subject the others to suppositions which may occasion a change in their aspect to connect them with that which he wishes, when nature itself has not offered traces, free from doubt, of the probability of the modification which he admits in these characters?

Permit me to observe to you, Sir, that this is precisely what appears to me to be the substance of your observations on the arseniated

The consideration of some of the characters may be omitted when they do not lead to general inferences.

This has been done by M. Haüy.

arseniated copper. You seem to consider as nothing the very sensible differences which exist in the divers species which I have established, with respect to hardness, specific gravity, and colour; and, stopping at the single character of form, you make suppositions for each of them, which, in fact, terminate by connecting all those which they offer with one primitive crystal: but nature does not exhibit any of the decrements which you suppose. I have never discovered the slightest trace of them in any of the immense quantity of crystals of arseniated copper which have passed through my hands. Do you believe that these suppositions are only susceptible of being admitted in a case in which, all the other characters being agreed in the most perfect state of these substances, which is that of regular crystallization and transparency, they would become necessary only to add an accumulation of proofs to those already acquired of their identity.

Exact distinction of the species necessary to the progress of the science.

Never was more attention paid than at this moment to the great truth, that the progress of the sciences which lead to the study of nature, depends principally on the exact distinction of each of the species whose union forms the aggregate to which the science is applied. No one is more convinced of this important truth than I am. But this exact knowledge of the species, which perhaps your calculation or the analysis of improved chemistry may one day attain in a simple and accurate manner, rests at present on the agreement of the exterior specific characters. Whenever this agreement exists, we are compelled to conclude that there is a similitude in the species, and, on the contrary, a dissimilarity when they differ essentially from each other. I, however, agree perfectly with you, that before separating one of these substances from the other to make a species of each, it is requisite to be previously convinced that the differences which they offer, and on which their division rests, are not purely accidental. It appears to me, therefore, that nothing can be more undeserving of the reproach of having neglected these precautions, than, on the contrary, the establishment of the division on the invariable constancy in the difference of their exterior characters.

Chemical analysis does not supersede the mineralogical characters.

The only reason, which, in the substances in question, can raise any doubt on their difference, is the result obtained from them by chemical analysis, which constantly found the arsenical acid combined with the copper in each of them; but if the analysis

analysis had not been possible, certainly no naturalist would have hesitated to separate them from each other, according to the exterior characters shown by each of them.

Why therefore, because these substances all belong to the combination of the same acid with the same metal, should there not be found several species among them? This, I believe, is a fact which occurs much more frequently than has been hitherto supposed. Do not all the metals show various instances of striking differences in the oxides, in consequence of that which exists in the combination of oxygen with them? The octahedral attractive oxidized iron, that which is rhomboidal, that not attractive, are not all these so many species? In a memoir which was inserted in the 75th number of the *Journal des Mines*, I have endeavoured to show that the octahedral sulphurated iron, and that in cubes, formed two very distinct species, and I do not believe that these are the only ones which exist in it. How many species are offered by sulphurated copper! I am myself acquainted with six, all perfectly distinct and characterized, which I have long intended to give the description of, but my occupations and want of time have not yet permitted me. Finally, have not you yourself been compelled to form a particular species of carbonated lime, of the arragonite, from the difference alone, which exists in its exterior specific characters, although chemistry can only find carbonic acid and lime in it?

Variations in the species of the same chemical combination.

One reason which may be alledged against the division of arseniated copper into species, is that the combination of copper with the arsenical acid being already a species in the genus of ores of copper, it would be making species of a species: and this objection, which at the first blush appears well founded, would bear equally against the various oxides, sulphurets, &c. But this difficulty seems to me to be more specious than solid: it takes its rise from the impossibility in which we still are of ascertaining every thing connected with the different causes which may produce a variation of the species. Without doubt, in this instance, for example, it is not the simple combination of the arsenical acid with the copper which forms the species, but the particular combination of this acid with the metal. Thus it is not the simple combination of oxygen, hydrogen, carbon, azote, &c. which constitutes the particular species of animal, but the manner of the combination itself.

The

Mineralogy is not yet sufficiently perfect to admit of the separation of the species by the form of the primitive molecule alone.

The mineralogical species are very accurately determined by the agreement or difference which exists in the first molecule of the formation of the different substances, but until we have fixed data to appreciate, in a determinate and invariable manner, all which relates to these molecule, the constancy or the difference in these exterior specific characters will always be the only means within our power of uniting or separating the species. I acknowledge, however, that in this case, it is necessary that this division should be established as much as possible on striking and essential characters; and I agree, at the same time, that latterly, this method has perhaps been much abused by giving importance to simple and casual characters; this has frequently placed substances in the number of species, which should only have been considered as simple varieties of those already known.

I shall now beg of you, Sir, to compare with me the different species of arseniated copper which I have described.

Comparison of the First and Second Species.

Comparison of the first and second species of arseniated copper.

The form of the first species is an obtuse rectangular octahedron, whose faces are unequally inclined. Two of them meet at the summit under an angle of 139° and at the base under one of 50° . The two others meet at the summit in an angle of 115° , and at the base in one of 65° . This octahedron is usually cuneiform: I have never perceived any modification of it.

The form of the second species is a hexahedral plate, always very thin, whose vertical planes are inclined alternately in opposite directions, so that two of them, on the same side, make an angle of 135° with the terminal faces to which they incline, and the third, one of 115° .

The most usual colour of the first species is a deep and very brilliant sky-blue, which sometimes changes to green.

That of the second species is a fine emerald green: I have never seen any other.

The specific gravity of the first species is 2881: that of the second 2548.

The hardness of the first is such that it readily cuts carbonated lime: the second is no harder than is sufficient to cut gypsum.

In your observations, Sir, you have raised to $50^{\circ} 4'$ and $65^{\circ} 8'$ the measures which I had established at 50° and 65° ; measures which you have fixed from the relations established by you between the height of one of the pyramids and the perpendiculars drawn from its bottom on the edges of its base, which corresponds with the adjacent and unequally inclined, pyramidal faces. These measures are so near to mine, that I have proved them again, and the instrument is so little capable of marking this difference, that I do not make any difficulty in adopting them.

To connect the form of the second species with the obtuse octahedron of the first, you afterwards suppose two sections made parallel to one of the most inclined faces of the octahedron, so as to detach a very thin segment, in which the centre of this octahedron is to be contained; that is to say, this octahedron is to be considerably increased on all its faces, with the exception of one alone, taken on each pyramid, and in an opposite direction in each of them. You suppose at the same time a decrement of a single row along the edges of the base, but which acts only on two of the faces of the octahedron, and that the segment which results on two of the three sides inclined to each of the terminal faces, makes with them an angle of $130\frac{1}{2}^{\circ}$ and the third 115° , measures which only differ $5^{\circ} 30'$ in the angle of $130^{\circ} 30'$ from those which I have given for the crystal.

The following is the answer dictated by the new examination which I have made of this substance.

Agreeably to what I have said in my memoir on the arseniated coppers, the obtuse octahedron frequently shows slight streaks in its faces parallel to its edges, which indicates a lamellated texture in the direction of these faces. The fracture also indicates the same texture, but its fractures are always more or less irregular. I have never been able to obtain a clear one. In the second species, on the contrary, the laminae are as easily raised from the hexahedral terminal faces, as could have been done on a prism of mica. These terminal faces are sometimes streaked parallel to the edges of the sides which are inclined to them, and these streaks, which are continued strongly on the sides, never appear upon them except in this direction. This texture, very analogous to that of mica, seems to me to be totally different from that of the obtuse octahedron of the first species.

Objections to their separation refuted.

I have

A new variety of
the second
species.

I have submitted some new crystals of this species to measurement, and have found them agree perfectly in the measures of 115° and 135° with those which I had examined before. The angle supposed by you of $130^\circ 30'$, which I have tried on a number of crystals always appeared to me to be much too small. These crystals have offered me a new variety, in which the sides of the hexahedral laminæ are less inclined on the terminal faces, with which they form an angle of about 105° . The crystal which afforded me this new variety is four lines in diameter: it is only in perfect preservation in one of its halves, but it admits of a judgment from it, that all its sides must have the same inclination. These new faces are perfectly smooth, and do not show any striæ. On another crystal, instead of these inclined sides, two planes are observed, one of which belongs to that which made an angle of 105° with the terminal face to which it inclines, and the other belongs either to that of 115° or that of 135° .

There was not any thing that I could discover connected with any of the planes of the obtuse octahedron of the first species.

Comparison of the Third Species with the First.

Comparison of
the third species
with the first.

The colour of the first is either a deep sky-blue or a grass-green. That most usual in the third is a yellowish green, more or less deep, but very frequently it can only be perceived by placing the crystal between the eye and the light, the intensity of the colour making the crystals appear black in every other position.

The specific gravity of the first is 2881; that of the third 4280.

The hardness of the first is not more than sufficient to scratch carbonated lime; that of the third is such as to cut fluated lime.

The first species has an obtuse rectangular octahedron for a unique and primitive crystal, whose dimensions have been given above: the figure of the third is an acute rectangular octahedron, in which each pyramid has two faces more inclined than the other two; the two most inclined faces meet at the summit, in an angle of 84° , and at the base in one of 96° , and the two others meet at the summit in an angle of 68° , and at the base in one of 102° . This octahedron is most usually cuneiform, and its prolongation is sometimes very considerable;

derable; it then takes the appearance of a rhomboidal tetrahedral prism of 84° and 96° , terminated at its extremities by a dihedral summit with isosceles triangular planes, the summit of which is placed on the edges of 84° , and the bases meet with each other in an angle of 112° . This form has not hitherto shown any other modification except being replaced by a plane, larger or smaller, on the edges of 96° . Its planes are usually very smooth and brilliant, and I have never been able to discover an appearance of division (*clivage*) in any of them.

This third species passes by the greatly lengthened octahedron to the determinate capillary variety, as well as to that which is indeterminate, and in this case the colour appears either to tend more to green or to take a more defined yellow, which sometimes has the brilliancy of gold.

The first species does not exhibit any thing which resembles these various transitions; it is always the same obtuse octahedron, and only varies by a very slight prolongation of its crystals parallel to the least inclined faces. To make the formation of the acute octahedron of the third species, secondary to the obtuse one of the first, you suppose a decrement at the base of the latter of two rows above and below the edges of the union of the least inclined faces, and another of four rows at that of the union of the most inclined faces, and by this, you get an acute octahedron, whose most inclined faces meet at the summit, in an angle of $86^\circ 24'$ and at the base in an angle of $93^\circ 36'$; and the others meet at the summit in an angle of 71° , and at the base in one of 109° .

I acknowledge that this approximation to the measures which I have given is seducing, and, considering the natural smallness of the crystals of this species, it would perhaps be difficult for me to pronounce determinately whether the measures which I have taken are much more exact than those to which you have attained by calculation; but this I can assure you, that no indication whatever, in either of these two octahedra, leads to the supposition which has given you this result.

From the details I have now given, it is easy to deduce the reasons which impel me to adhere to the division which I have thought it right to make in the arseniated copper, and prevent me from adopting the approximation to which your ingenious hypotheses have led you. Every thing still seems to me to tend

Reasons for
adhering to this
division of the
species.

to indicate a difference in the species into which I have separated them, while to bring them to a single one, you have been obliged to consider as nothing; all the exterior specific characters, with the exception of the form alone, and you have used the latter only in establishing an hypothesis respecting it, to which, neither artificial means, such as splitting, nor natural indications, such as secondary planes on a primitive crystal, nor the retaining of primitive planes on the others have led you. If in the explanation of embarrassing facts, it were permitted thus to make nature speak when she is silent, I have no hesitation in asserting that a naturalist so well informed, and so practised in the art of calculation as you are, would find very few obstacles in resolving all the species into each other whenever he chose.

Of the four species of arseniated copper which I have described, there still remains one, in respect of which you have not made any calculation of approximation; it is the fourth, which, as I have stated, has for a primitive crystal a tetrahedral prism with an equilateral triangle for a base. Nevertheless you do not exclude it when you draw your conclusions on the doubt which you believe to exist on the division of the arseniated coppers into four species, and you direct this doubt equally to the fourth. Since according to your supposition this can only be as a primitive to the first crystal, that it may also be in a state to be brought to it, I have thought it right to add likewise the comparison of this fourth species with the first.

Comparison of the Fourth Species with the First.

Comparison of
the fourth
species with the
first.

The colour of the first species is a deep sky-blue, which sometimes changes to grass-green. That of the fourth is a brilliant deep verdigris, but its surface is very readily discoloured, doubtless by oxidating, and it then becomes black: this renders the crystals opaque, which, when they have not experienced this alteration, are beautifully transparent; this is very unusual among those which have been naturally exposed to the free air during a certain time. This change however exists only at the surface; by scratching the crystals lightly, their fine colour is readily restored to them. I have never perceived any thing resembling this fact, which unquestionably depends on the nature of the substance of this species, either among the crystals of the first, or among those of the second and third.

The

To prove what has been stated before, let any quantity of the obtained alumine be heated in contact with charcoal powder; introduce the mixture into a tabulated retort, connected with the pneumatic apparatus, and add to it muriatic or sulphuric acid; the result will be sulphuretted hydrogen gas in abundance, particularly if heat be applied to the mixture. The production of this gas will become evident on considering the philosophy of the process. By means of the following method, alumine may be obtained in a purer state.

Take any quantity of alum of commerce, dissolve it in four parts of boiling distilled water, and mingle this solution with liquid ammonia till no further cloudiness ensues. Then heat the mixture nearly to the boiling point for a few minutes, and transfer it on a filter. In proportion as the fluid passes off, pour more water over the precipitate, and continue the ablution till the water runs off tasteless. Having done this, let the precipitate while yet in a pasty state, be transferred into a basin or flask, and add to it muriatic acid, in small quantities at a time, until the whole is dissolved; then evaporate the solution till a drop of it, when suffered to cool on a plate of glass, yields minute crystals. If it now be suffered to cool, crystals of alum will be deposited. Remove these crystals, by decanting the fluid, and renew the evaporation, until on further cooling, no more crystals are formed. Nothing now but nearly pure alumine remains in solution; the potash and sulphuric acid being got rid of, at the expence of a little alumine in the crystals. The fluid may therefore be diluted with water, and then decomposed by liquid ammonia, taking care to add this alkali in excess. The precipitate thus obtained, when washed and dried, will be alumine in a state of considerable purity.

The alumine thus obtained does not yield sulphuretted hydrogen when heated with charcoal power; it emits no odour when breathed upon, it is somewhat unctuous to the touch, insipid, and white as snow.

The precipitate or supposed earth, if heated with charcoal, and then treated with an acid, gives sulphuretted hydrogen.

Method of separating purer alumine.

Precipitate by ammonia; and wash well. Dissolve in muriatic acid. Evaporate and crystallize for a contaminating portion of alum. Then dilute and precipitate again by ammonia.

The earth that now falls is considerably pure.

VI.

Short Account of Mr. Arthur Woolf's Improvement in the Construction of Steam-Engines.

Discovery that heated steam may be suffered to expand, with a very powerful effect;—

MR. WOOLF founds his improvements on an important discovery he has made respecting the expansibility of steam when increased in temperature beyond the boiling point, or 212° of Fahrenheit's thermometer. It has been ascertained for some time by Mr. Watt, that steam acting with the expansive force of four pounds per square inch against a safety-valve exposed to the atmosphere, is capable of expanding itself to four times the volume it then occupies, and still to be equal to the pressure of the atmosphere. Mr. Woolf has discovered and established by proof, that steam of the force of five pounds the square inch, may expand itself to five times its volume; that masses or quantities of steam of the like expansive force of six, seven, eight, nine, or ten pounds the square inch, may be suffered to expand to six, seven, eight, nine, or ten times its volume, and will still be respectively equal to the atmosphere, or capable of producing a sufficient action against the piston of a steam-engine to cause the same to rise in the old engine (with a counterpoise) of Newcomen, or to be carried into the vacuum part of the cylinder in the improved engines first brought into effect by Messrs. Boulton and Watt;—that this ratio is progressive, and nearly if not quite uniform, so that steam of the expansive force of 20, 30, 40, or 50 pounds the square inch of a common safety-valve will expand itself to 20, 30, 40, or 50 times its volume with like effect; and that, generally, as to all the intermediate or higher degrees of elastic force, the number of times which steam of any temperature and force can expand itself is nearly the same as the number of pounds it is able to sustain on a square inch exposed to the common atmospheric pressure: provided always that the space, or vessel in which it is allowed to expand itself, be of the same temperature as that of the steam before it was allowed room to expand.

—the increase of temperature not being considerable.

Respecting the different degrees of temperature required to bring steam to, and maintain it at, different expansive forces above the weight of the atmosphere, Mr. Woolf has

found, by actual experiment, setting out from the boiling point of water, or 212°, at which degree steam of water is only equal to the pressure of the atmosphere, that in order to give it an increased elastic force equal to five pounds the square inch, the temperature must be raised to about 227½°, when it will have acquired a power to expand itself to five times its volume, still being equal to the atmosphere, and capable of being applied as such in the working of steam-engines, according to his invention: and with regard to various other pressures, temperatures, and expansive forces of steam, the same are shown in the following table :

Table of the relative pressures per square inch, temperatures and expansibility of steam at degrees of heat above the boiling point of water, beginning with the temperature of steam of an elastic force equal to five pounds per square inch, and extending to steam able to sustain forty pounds on the square inch.

Pounds per square Inch.	Degrees of Heat.	Expansibility.
Steam of an elastic force pre-dominating over the pressure of the atmosphere upon a safety-valve.	227½	5
5	230¼	6
6	232¾	7
7	235¼	8
8	237½	9
9	239½	10
10	250½	15
15	259½	20
20	267	25
25	273	30
30	278	35
35	282	40
40		

Table of pressures, temperatures and expansions of steam equal in force to the atmosphere.

And so in like manner, by small additions of temperature, an expansive power may be given to steam to enable it to expand to 50, 60, 70, 80, 90, 100, 200, 300, or more times its volume, without any limitation but what is imposed by the frangible nature of every material of which boilers and the other parts of steam-engines have been or can be made; and prudence dictates that the expansive force should never be carried to the utmost the materials can bear, but rather be kept considerably within that limit.

Upon this discovery, Mr. Woolf has obtained a patent for various improvements in the steam-engine, from the specification

tion of which, the following extract is made, which will be sufficiently instructive to those who are acquainted with the subject.

Extract from
Mr Woolf's
specification,

“ If the engine be constructed originally with the intention of adopting my said improvement, it ought to have two steam vessels of different dimensions, according to the temperature or the expansive force determined to be communicated to the steam made use of in working the engine; for the smaller steam vessel or cylinder must be a measure for the larger. For example, if steam of forty pounds the square inch is fixed on, then the smaller steam vessel should be at least one fortieth part the contents of the larger one; each steam vessel should be furnished with a piston, and the smaller cylinder should have a communication both at its top and bottom (top and bottom being here employed merely as relative terms, for the cylinders may be worked in a horizontal or any other required position, as well as vertical): the small cylinder, I say, should have a communication both at its top and bottom with the boiler which supplies the steam, which communications, by means of cocks or valves of any construction adapted to the use, are to be alternately opened and shut during the working of the engine. The top of the small cylinder should have a communication with the bottom of the larger cylinder, and the bottom of the smaller one with the top of the larger, with proper means to open and shut these alternately by cocks, valves, or any other well-known contrivance. And both the top and bottom of the larger cylinder or steam vessel, should, while the engine is at work, communicate alternately with a condensing vessel, into which a jet of water is admitted to hasten the condensation, or the condensing vessel may be cooled by any other means calculated to produce that effect. Things being thus arranged, when the engine is at work, steam of high temperature is admitted from the boiler to act by its elastic force on one side of the smaller piston, while the steam which had last moved it has a communication with the larger steam vessel or cylinder, where it follows the larger piston now moving towards that end of its cylinder which is open to the condensing vessel. Let both pistons end their stroke at one time, and let us now suppose them both at the top of their respective cylinders, ready to descend; then the steam of forty pounds the square inch entering above the smaller piston will carry it down-

wards,

The specific gravity of the fourth species is 4280, and is consequently perfectly analogous to that of the arseniated copper of the third species, but at the same time greatly inferior to that of the first, which is 2881.

Its hardness, much below that of the third species which we have just seen that it resembles in weight, is also less than that of the first species, by which it is cut.

Its forms, which are greatly multiplied, while there exists but one in the first species, differ essentially from those of the first.

All these forms appear to me to be derived from the right tetrahedral prism with equilateral triangles for bases; and all those which I have endeavoured to recognize and have given in my memoir, seemed to me to be very readily derived from that; as I have also said: these crystals are always extremely small, and I was unable to measure them; a few groups of them exhibited this prism in such a manner that I could observe it perfectly complete. The scarcest crystals, after those belonging to the varieties which I have represented at figures* 15, 16 and 17, and which are usually so grouped as to penetrate each other, and thus to become very difficult to be known, are the very acute complete rhomboid and its incomplete varieties, such as are represented at figures 22, 23 and 24 of my memoir. I even hesitated, the division (*clivage*) not having shown any thing to guide me, whether I should not take this rhomboid for the primitive form. In this instance, the only manner which seemed to me natural and simple of connecting this crystal with the obtuse octahedron of the third species, is to suppose this octahedron become rhomboidal by an increase having taken place by the superposition of laminæ, or by the collection of rows of molecuæ, growing smaller, on only one of the faces of each pyramid, and in an opposite direction on each of them, as occurs in the spinel, and a number of substances having a right octahedron for a primitive crystal, and which I have also seen in the diamond. But in this case, either the increase must be made on the most inclined faces, and then calculation shows that the planes of the rhomboid should have $57^{\circ} 39'$ and $122^{\circ} 21'$ for the measure of the angles of its plane; or the same increase must be made on the least inclined faces, and then the measures of the angles of the plane of the rhomboid

Reasons for forming a fifth species.

* Phil. Trans.

Reasons for forming a fifth species.

should be $47^{\circ} 4'$ and $132^{\circ} 56'$: now although from its minuteness, the rhomboid which exists in this species is not measurable by instruments, yet I can affirm with certainty that it is much more acute than either of the two which this supposition gives rise to.

It is very true, Sir, that since my memoir on the arseniated copper was printed, I have thought it right to separate the species described in it, and to make a fifth of one of the substances included among them; a substance which is entirely different from the others in its exterior characters, and seems to lead naturally to a belief that water must be a principal in the number of its component parts: but you are in an error with respect to which of these substances I believe to be really of a different nature from the others. In my first work on the arseniated copper, I found it necessary to make several subdivisions or varieties in the fourth species: of these, the three first are the determinate capillary, the indeterminate capillary, and that which is solid at one of its extremities, and divided into very delicate fibres at the other. You seem to believe that I comprehend these three varieties in the new species which I have been led to consider as an hidro-arsenate. This opinion would indeed be, as you very justly observe, completely contradictory of every thing which I have said on these varieties, in the description I have given of them, and I have no doubt, must have astonished you. The only species of arseniated copper to which this opinion is directed, is that which includes the two varieties to which I have given the names of hematiform and amiantiform: they are very certainly the same, with this difference, that one is the product of the decomposition of the other.

This arseniated copper, when it is unmixed, forms very compact mamellæ, but striated from the centre to the circumference, and very often also forming different concentric layers: their colour is brown, sometimes with a very slight tinge of green. This substance, in its aspect, greatly resembles the hematiform oxide of tin, which, in *Cornwall*, bears the name of *wood-tin*; this has caused the miners of the same country to give the name of *wood-copper* to this species of arseniated copper. Its hardness, notwithstanding its fibrous texture, is sufficiently great to scratch fluated lime with facility. Its specific gravity is from 4100 to 4200.

This

This substance very readily changes; it then passes to an ash-grey, and loses considerably of its hardness: it also frequently experiences a more advanced decomposition; and then becomes perfectly white, and so soft that the nail is sufficient to cut it and separate its fibres. If the mamellæ which have passed to this state are broken, their decomposition is frequently found to have reached their centre; but very frequently also the centre has perfectly retained its brown colour and its hardness, and both are observed to diminish gradually on approaching the circumference; if in this case the attention is directed to the fibres near the circumference, it will be seen that they are detached from each other, and that the surface itself of these mamellæ has the appearance of that of madrepore, from the immense number of fissures in different directions, which are occasioned by the contraction. At length this substance arrives at such a degree of decomposition, that the mamellæ open completely, their fibres are entirely separated from each other, and, in this state, frequently become so slender and flexible as perfectly to resemble small portions of papyraceous amianthus.

Reasons for forming a fifth species.

Such, Sir, is the nature of the arseniated copper, in which I believe I have found properties and a mode of existence which differ from those of the others, and which I have had opportunities of examining with greater facility and accuracy since the impression of my memoir. This easy decomposition, the prodigious contraction, and the great change which this substance experiences in itself, have led me to suppose that the loss of water has great influence on it. But this fact is, however, nothing more than an opinion, which experience will either reject or confirm; and I have so stated it. It may also be very possible that this arseniate is no more than a variety of the third species, as I at first considered it. You must, however, acknowledge that it offers very singular characters, of which it would, therefore, be interesting to know the cause.

The dealers in minerals from London, and principally Mr. Mawe, have, I believe, taken a collection of arseniated copper to Paris. This substance appears to me worthy of engaging the attention of chemists, who, I am of opinion, should repeat the analyses of it. Perhaps they may one day throw a clearer light on a substance which has greatly interested me, and to which I am at this moment indebted for the pleasure of having entered with you, Sir, into a discussion, agreeable in its manner and instructive in its effect.

V.

Process for separating Alumine from Alum. In answer to the Enquiries of R. T. By Mr. FREDERICK ACCUM.

To Mr. NICHOLSON.

SIR,

IF you have not received a better answer to the queries of your correspondent R. T. (p. 141) who finds so much difficulty in obtaining alumine in a state of purity, &c. I request you will give a place to the following lines in your next. I have endeavoured to be as concise as possible, and have the honour to be,

Sir,

Your most obedient humble servant,

FREDERICK ACCUM.

Old Compton-Street, Soho,

July 17, 1804.

Common process for separating alumine from alum, throws down a salt, and not a mere earth.

The process in general recommended by systematic writers for obtaining alumine, (which R. T. adhered to) is absolutely erroneous, for no pure earth of alum can be expected if a solution of alum of commerce be decomposed by a solution of carbonated alkali, and subsequent ablution and expulsion of the carbonic acid. For alum of commerce is a triple compound, consisting of alumine, potash and sulphuric acid in excess. If we attempt to saturate this excess of acid, by the addition of an alkali, or even by pure alumine, a highly insoluble salt, (sulphate of alumine) is generated, differing from alum principally in the proportion of its constituent parts. When we therefore gradually add (as R. T. did) to a solution of alum, a carbonated alkali, the first effect of it is, to saturate the excess of the sulphuric acid, and the precipitate consists principally of the salt, which is insoluble in water. A farther addition effects instantly a decomposition of part of the salt, which, in proportion as it takes place, becomes mixed with the alumine, which is thus covered or defended from the further action of the alkali. This being the case, it is obvious that no subsequent washing can do more than separate the sulphate of potash, and therefore the residuum, instead of being pure alumine (as R. T. imagines) contains also a variable portion of true sulphate of alumine.

T.

1. For obtaining oxygen gas, by means of the glass retort, Description, &c.
of an improved
chemical lamp. from a mixture of four parts of finely powdered black oxide of manganese, and three of concentrated sulphuric acid; or by merely heating in a retort, hyper-oxygenized muriate of potash.

2. For disengaging nitrogen, or azote, from animal substances, by affusing two parts of nitric acid of commerce, diluted with an equal bulk of water, upon one part of muscular flesh (a piece of beef or veal cut into small pieces), and heating the mixture to ebullition in a glass retort.

3. For obtaining hydrogen, sulphurated and phosphorated hydrogen, in the usual manner; or for producing heavy carbonated hydrogen by strongly heating, in a retort, a mixture of three parts, by weight, of concentrated sulphuric acid, and one of sulphuric ether, or two of alcohol; nitrous gas, by any of the usual processes; gaseous oxide of nitrogen, by decomposing nitrate of ammonia by heat in a retort; sulphureous acid gas, by causing four parts of concentrated sulphuric acid to act on one of lump-sugar, assisting the action of the mixture in the retort by heat; muriatic acid gas; oxygenized muriatic acid gas, and carbonic acid gas, according to any of the usual methods; ammoniacal gas, by heating in a retort a mixture of two parts of finely powdered lime and one of muriate of ammonia.

4. For the distillation of nitric, muriatic, oximuriatic, acetic, oxalic, arsenic, prussic, suberic, mucous, and camphoric acids, according to the methods recommended by systematic writers.

5. For the production of metallic, earthy, and alkaline sulphurets; such as sulphuret of potash, soda, barytes, strontia, ammonia, iron, copper, tin, mercury, &c.

6. For performing the analysis of ores of gold, silver, copper, lead, zinc, tin, &c. and for examining mineral and native salts, earth and stones, according to the methods pointed out in the "Practical Essay on the Analysis of Minerals," and for a variety of other operations, too numerous to be detailed.

*Old Compton-Street, Soho,
July 18, 1804.*

VIII.

On the mutual Precipitations of Metallic Oxides. By
 J. L. GAY-LUSSAC.*

The subject is
 hitherto but
 little known.

Utility of the
 investigation.

THAT we have still so little information on the mutual precipitations of metallic oxides, can only be attributed to the complication of the results which they offer. Indeed, a little reflection will convince us that the oxidation, the affinity, the reciprocal action of the oxides, and the property which they have of neutralizing the acids unequally, are so many causes which must join in the production of the phenomena. It would, nevertheless, be very useful to know the order in which the metallic oxides precipitate from their solutions: the chemical analyses, and more especially the purification of metallic salts, would thence become easier. It was with this intention that I have made some experiments, and if they have not been sufficiently numerous to have enabled me to distinguish the influence of every cause, they, at least, show that of some, and will serve to draw the attention to a subject which is still obscure and very complicated.

I shall begin by relating the results which I have obtained, and shall afterwards endeavour to determine their causes.

Experiments
 with the green
 and red muriates
 of iron.

Having taken a solution of green muriate of iron, I added to it a little red muriate of the same metal, and I poured potash into the mixture in a quantity at least sufficient to decompose all the red muriate separately. After agitation, the first portions of alkali yielded a precipitate of iron very much oxidized, without any mixture of black oxide; but by adding more and more of the alkali, it finished by being composed of the two oxides. The filtered liquor was then perfectly limpid, and no longer produced a blue with the prussiates, nor a black with the gallic acid: this proves that the very oxidized iron had been precipitated from it. On making the inverse experiment, that is to say, on putting a little green muriate of iron into a larger quantity of red muriate, and precipitating it by the alkali, the black oxide was retained in the solution to the last, and was not precipitated until after all the red oxide. Hence therefore it results that the black oxide

* From the *Annales de Chimie*, No. 145. or Vol. XLIX. p. 21.

of iron precipitates the red oxide, and that it is, consequently, very easy to have green solutions of iron without red oxide.

Black oxide of iron precipitates the red.

Into a solution of sulphate of zinc of commerce, which is known to contain much iron, I poured a little potash, to produce a precipitate, and I agitated and heated the mixture.

Experiments with the sulphate of zinc of commerce.

On examining the precipitate I found oxide of zinc and a little iron very much oxidized, and nevertheless the liquor still contained much iron, but it was at the minimum of oxidation: an addition of alkali only separated oxide of zinc. I divided the filtered liquor into two portions; into one I poured oxygenated muriatic acid, and I boiled the other with a little nitric acid. Potash then poured into the two liquors separated all the iron from them, so that there only remained a very pure sulphate of zinc, containing only a little sulphate of potash, which it is easy to avoid by employing oxide of zinc, recently prepared and well washed, to separate the iron.

On making the same experiments on other solutions of zinc, I constantly found that oxide of zinc precipitated red oxide of iron, and that, on the contrary, it was precipitated by the black oxide.

Oxide of zinc precipitates red oxide of iron, but is precipitated by the black.

A solution of zinc in nitric acid may be directly obtained, sufficiently pure, by dissolving it very rapidly: great part of the very oxidized oxide of iron is precipitated; and that which remains in solution requiring a great excess of acid, is precipitated by dissolving another quantity of zinc. But if the solution has been made slowly, it retains much iron, which being but little oxidized, is retained very strongly.

It is well known that, in all experiments of this kind, the precipitate may be composed of one or of two oxides, according to the quantity of alkali employed; but the better to observe what passes, it is advisable to put very little alkali into the metallic solution, the precipitate being in that case composed of only one oxide.

The quantity of alkali employed should be small.

By continuing to follow the same processes I found, that when the iron is very much oxidized it is precipitated by oxide of copper, and that the inverse takes place when it is but little so. Here are two very important consequences, because they may be very frequently applicable in the arts: the first is, that all the iron may be separated from a solution of copper; the second, that all the copper contained in a green solution of iron may be abstracted.

Importance of the mutual precipitations of copper and iron.

Several

Sulphate of copper may be freed from iron :

Several colours are prepared with sulphate of copper, but the iron which it always contains, and which hitherto has never been completely separated, alters the shade. If, to accomplish this last object, the iron is strongly oxidized by means of nitric acid, or, which is better, of oxygenated muriatic acid, the sulphate of iron may be entirely precipitated by pouring a sufficient quantity of potash into it, and by heating and agitating the liquor.

and sulphate of iron, from copper.

Green sulphate of iron is also frequently employed in the arts, and in many of them it is desirable that it should not retain any copper. Iron has the property of separating it, but it appears that it only does so imperfectly, and requires much time. It would doubtless be more advantageous to employ potash, and to pour a little into the green sulphate: the precipitate of black oxide of iron would be speedily re-dissolved by agitation, and would precipitate, at the same time, the oxide of copper and the red oxide of iron, if there was any in the green sulphate.

Ammonia dissolves oxide of iron at a minimum.

I shall here remark that, having employed ammonia to discover the copper in the green sulphate, I observed that, on adding an excess of alkali, the oxide of iron was dissolved in great abundance, though it is known that, in the same circumstances, it does not dissolve the very oxidized iron. The solution left in the air is decomposed, the ammonia escapes, and a black crust is formed on the surface of the liquid which soon defends it from the contact of the air. In analyses, ammonia is frequently employed to separate iron, but this method is not good unless it had been strongly oxidized before. This induces me to believe that this circumstance might have prevented Bergman from separating iron from nickel by means of ammonia; for he found that his solution contained the oxides of both metals, and this could only have arisen from the iron not having been sufficiently oxidized.

Oxygenated muriate of mercury precipitates the red oxide of iron and those of zinc and copper
Oxide of silver precipitates oxide of copper.

I also discovered, by the same means, that the oxide of oxygenated muriate of mercury precipitates the red oxide of iron with the greatest facility, and those of zinc and copper from their muriatic solution.

Having dissolved a piece of silver in nitric acid, I obtained a blue liquor composed of copper and silver. A little potash poured into the solution formed a flocculent precipitate, composed,

wards, while the steam below it, instead of being allowed to escape into the atmosphere or applied to any other purpose, will pass into the larger cylinder above its piston, which will take its downward stroke at the same time that the piston of the smaller cylinder is doing the same thing; and while this goes on, the steam which last filled the larger cylinder, in the upward stroke of the engine, will be passing into the condenser to be condensed during the downward stroke. When the pistons in the smaller and larger cylinder have thus been made to descend to the bottom of their respective cylinders, then the steam from this boiler is to be shut off from the top and admitted to the bottom of the smaller cylinder, and the communication between the bottom of the smaller and the top of the larger cylinder is also to be cut off, and the communication to be opened between the top of the smaller and the bottom of the larger cylinder; the steam, which in the downward stroke of the engine filled the larger cylinder, being now open to the condenser, and the communication between the bottom of the larger cylinder and the condenser shut off; and so on alternately, admitting the steam to the different sides of the smaller piston, while the steam last admitted into the smaller cylinder passes alternately to the different sides of the larger piston in the larger cylinder, the top and bottom of which are made to communicate alternately with the condenser.

“ In an engine working with the improvements which have been just described, while the steam is admitted to one side of the piston in the smaller cylinder, the steam on the other side has room made for its admission into the larger cylinder, on one side of its piston, by the condensation going on on the other side of the large piston which is open to the condenser; and that waste of steam which takes place in engines worked only by the expansive force of steam, from steam passing the piston, is prevented; for all steam that passes the piston in the smaller cylinder is received into the larger.

“ In such an engine, where it may be more convenient for any particular purpose, the arrangement may be altered, and the top of the smaller made to communicate with the top of the larger, and the bottom of the smaller with the bottom of the larger cylinder; in which case the only difference will be, that when the piston in the smaller cylinder descends, that

in

Extract from
Mr. Woolf's
specification.

in the larger will ascend, and while the latter descends the former will ascend, which for some particular purposes may be more convenient than the arrangement before described."

Mr. Woolf then proceeds to describe various other modifications of his invention, and points out means for applying his improvements to the working of steam-engines already constructed and now in use. It is obvious that the advantages of this discovery promise to be very considerable, and I shall take the earliest opportunity of communicating the facts as they shall be brought forward in practice.

VII.

*Description of a Chemical Lamp, with double concentric Wicks.
Communicated by Mr. FREDERIC ACCUM.*

Description, &c.
of an improved
chemical lamp.

THOSE who are familiar with Chemistry will readily allow, that one of the principal obstacles which frequently impede the progress of the young chemist in the prosecution of his science, is the want of proper apparatus: he is at a loss to select from the number of instruments displayed in the lectures of his teacher, those which are calculated for rendering, *in his own apartment*, the most important truths of the science legitimate by experiment.

It must naturally be so, as long as chemists are anxious to exhibit a variety of costly and complicated apparatus, and continue to pay that frivolous regard to *show* which characterises so many public lectures; and as long as the student is told in the introductory lesson, that the science cannot be learned but in the laboratory, fitted up with furnaces, stills, bellows, water-baths, sand-baths, &c. It is true indeed, that many chemical phenomena cannot be accurately observed without the help of instruments calculated to assist the imperfection of our senses; but it is equally true, that many of the brilliant apparatus which are daily displayed in the laboratories of teaching chemists, as instruments of research, serve more to divert the attention of the auditors, than to elucidate the fundamental truths of the science.

The modern processes of philosophical enquiry differ so much from what they formerly were, and the instruments of experi-
ment

ment have been so much improved during our own time, that by means of a comparatively small number of them, the most complicated processes may very commodiously and conveniently be carried on in the closet of every cultivator of chemistry.

Description, &c.
of an improved
chemical lamp.

To awaken the industry of the junior class of chemists, I gave in the twenty-fourth number of this Journal, a description and sketch of a convenient and portable universal furnace, and in the twenty-third number of the same Journal, I ventured to recommend to the young analyst a cheap and useful apparatus for drying the products of his analysis, and also convenient for experiments on artificial cold, &c. The favourable reception which these trifles have met with, amongst those amateurs of the science for whom I have caused them to be constructed, has encouraged me to point out to them an improved lamp-furnace, by means of which, in the small way and on the table of the student, almost every operation may be performed at a cheap rate, as well as with facility and dispatch. In order to enable the intelligent reader to judge for himself, I shall first detail the construction of the instrument, and then point out to the inexperienced operator some of the most capital processes which serve to unfold the fundamental truths of the science, and for which the lamp may be applied according to the conditions laid down.

Fig. 1, Plate XIV. is a perspective view of the improved chemical lamp-furnace. It consists of a brass rod screwed to a foot of the same metal, loaded with lead. On this rod (which may be unscrewed in the middle, for rendering it more portable) slide three brass sockets with straight arms, terminating in brass rings of different diameters. The largest measures four inches and a half. These rings serve for supporting glass alembics, retorts, Florence-flasks, evaporating-basins, gas-bottles, &c. for performing distillations, digestions, solutions, evaporations, saline fusions, concentrations, analyses with the pneumatic apparatus, &c. If the vessels are not wished to be exposed to the naked fire, a copper sand-bath may be interposed, which is to be previously placed in the ring. Each of the brass-rings may, by means of a thumb-screw acting on the rod of the lamp, be set at different heights, or turned aside, according to the pleasure of the operator. Below these rings is a fountain-lamp on Argand's plan, having a metallic valve within,

Description, &c.
of an improved
chemical lamp.

within, to prevent the oil from running out while the reservoir is put into its place. This lamp also slides on the main brass rod by means of a socket and thumb-screw. It is therefore easy to bring it nearer, or to move it farther, at pleasure, from the vessel which may remain fixed; a circumstance which, independent of the elevation and the depression of the wicks of the lamp, affords the advantage of heating the vessels by degrees after they are duly placed, as well as of augmenting or diminishing the heat instantly; or for maintaining it for several hours at a certain degree, without in the least disturbing the apparatus suspended over it. It may therefore be used for producing the very gentle heat necessary for the rectification of ethers, or the strong heat requisite for distilling mercury.

The chief improvement of this lamp consists in its power of affording an intense heat by the addition of a second cylinder added to that of the common lamp of Argand. This additional cylinder incloses a wick of one inch and a half in diameter, and it is by this ingenious contrivance, which was first suggested to me by Mr. Webster, that a double flame is caused, and more than three times the heat of an Argand's lamp of the largest size is produced. This part of the construction of the lamp is clearly shewn in *Fig. 2*, which represents the concentric wick-holders of the lamp; the distance between the exterior and interior cotton is half an inch, the circumference of the largest wick is $4\frac{3}{4}$ inches, and that of the smaller two. Both the wick-frames are connected by a fine screw cut upon a piece of pinion-wire.

Fig. 3. is a section of the concentric cylindrical tubes in which the wicks move.

The superior advantages of this lamp, above all others I am acquainted with, consists therefore in quickly producing, if required, a very low as well as intense heat, and in regulating its power instantaneously; by means of which the operator may observe a number of minute circumstances essential to be known, but which cannot be noticed when the same process is carried on within a furnace.

Use of the Lamp-Furnace.

From what has been stated it is obvious, that this lamp may be used for a variety of chemical operations, if conducted under the conditions here pointed out; a few of which are—

1. For

posed, in great part, of oxide of silver, because the precipitation took place only where the alkali was poured in; but this precipitate was gradually covered with oxide of copper, and, by agitation, it was in a little time replaced by the latter. An addition of alkali having given me a precipitate of oxide of silver which was not re-dissolved, I filtered it, and obtained a liquid perfectly colourless, which did not contain any more copper. If it is wished to avoid the potash in the solution, a part of the impure nitrate of silver may be decomposed separately, and the precipitate, well washed, may be employed to separate the copper of the other part. This simple method of separating copper from a solution of silver may be very useful in laboratories, and even in large works.

The oxide of silver also decomposes the nitrate of zinc; and the oxide of manganese the muriate of copper.

In what precedes I have, for shortness, supposed that the precipitates were pure oxides; but I am far from believing this to be the case; on the contrary, I consider nearly all of them as true salts. Copper, for example, was always precipitated of a bluish-green, although the shade varied with the oxides which were precipitated; and it is now well ascertained by Proust and the younger Berthollet, that the green and blue oxides of copper retain some of the acid.

The precipitates are not pure oxides.

Such are the facts as I have observed them, and on which alone I shall make some reflections. Although they are too few to have enabled me to fix on all the circumstances which concurred in their production, the examination of them will, however, develope some of them.

In fact, if we direct our attention to the acidity of the different salts noticed above, we shall see,

Inferences from a consideration of the state of the acidity.

1 β . That the iron which is but little oxidized, and the highly oxidized mercury which precipitates the red oxide of iron, the oxide of zinc, and that of copper, approach nearer to neutralization than the latter.

Neutralization of the acids by oxides.

2d. That the zinc and manganese which precipitate the copper, neutralize the acids better than it*.

* By neutralizing the acids more or less, I mean that property possessed by the metallic oxides, and some earths, such as glucine and alumine, of approaching more or less, in their combinations with the acids, to the term of neutralization.

3d. That the oxide of silver which precipitates those of zinc and copper, neutralizes the acids better than them.

Alumina is precipitated by metallic oxides;

and by glucine.

Magnesia precipitates the earths and oxides.

If, besides, we reflect, that alumina, whose solutions are very acid, is precipitated by several metallic oxides which neutralize the acids better than it; that, according to the experiments of Vauquelin, glucine decomposes aluminous salts, and that its solutions are more neutral than those of alumina, although they are not entirely so; and, finally, that magnesia, which neutralizes the acids perfectly, precipitates the preceding earths, and a very great number, not to say all, the oxides from their solutions; we cannot abstain from allowing that, if the property possessed by the metallic oxides and several earths of neutralizing the acids unequally, is not the only cause of the decompositions which I have detailed, it is at least one of the principal.

The affinity for oxygen is not the cause.

We may also conclude from the same experiments, that the metals which have a great or a weak affinity for oxygen, do not enjoy any particular property with respect to their mutual precipitations; for we see that iron, in a state of great oxidation, is precipitated by a number of oxides which it precipitates when it is less so; and, that there are several oxides which contain less oxygen than that of zinc, which precipitate the latter, while there are others which are precipitated by it.

The affinity of the different metals for oxygen is therefore rejected as the cause of the mutual precipitations of their oxides; but can the greater or less oxidation of the same metal occasion a variation in the affinity of the oxide for the acids?

This opinion has been promulgated by Cit. Berthollet in his *Chemical Statics* *, and he has grounded it upon several facts, in which the metal, by losing a little of its oxygen by any means whatever, forms another salt with less acid. This happens to the oxygenated muriate of mercury, which, by exposure to light, or by being brought into contact with iron, is changed into white muriate by abandoning some of its acid. Although these, and other similar facts, are capable of a different interpretation, other considerations, which I shall omit here, because they would lead me too far, induce me to participate in the opinion of Citizen Berthollet; but I do not be-

The state of the oxidation influences the affinity of the oxides for the acids;

* A translation of which will be published by Mawman, in the *Poultry*, about the middle of the present month.

lieve that this cause can have much effect, it being strongly counteracted by the acidity which almost all the metallic solutions possess, and by the insolubility of the oxides. Thus, although it appears to me that iron, when little oxidized, has more affinity for muriatic acid than when it is greatly oxidized, I should rather attribute the precipitation of the latter by the former to the great excess of acid which its solution requires, than to its weaker affinity.

Neither, for the same reasons, do I believe that the affinity of the different oxides for the acids, an affinity which I measure, with Cit. Berthollet, by the capacities for saturation, can be considered as the cause of their mutual precipitations. but is not the cause of their mutual precipitations.

Besides, there is one consideration of some importance which should be taken into the explanation of the mutual precipitations of the oxides; it is that, in a case where the precipitation of a metallic solution is produced by means of an alkali, the precipitate retains some of the acid which can favour its solution; so that an oxide which could retain much of the acid, would dissolve more readily than that which could only retain less. It must really be so with iron, which, when it is precipitated from a green solution, retains much more acid than when it is precipitated from a red solution, and which dissolves much better in the acids in the first case than in the second. This more ready solution cannot, however, be considered as a cause of the mutual precipitations of the metallic oxides; it may be very favourable to them, but cannot determine them. In fact, we see that the oxide of copper, which retains much acid, is nevertheless precipitated by oxide of silver, which does not sensibly retain any, when they are precipitated from their nitric solutions by potash. Influence of the retained acid.

CONCLUSION.

THE metallic oxides are mutually precipitated from their solutions. Several causes may contribute to this; but among the number of the principal must be placed the property which they have of neutralizing the acids unequally. General inferences.

This property has furnished us with the means, 1st. Of freeing a green solution of iron from the red oxide which it may contain; 2^d. Of separating the sulphate of zinc and that of copper from the iron which is always found in them; 3^d. Of having a green sulphate of iron free from copper; 4th. Of

General inferences.

readily separating the copper from a solution of silver. It is easy to accumulate these applications by extending them to a greater number of substances. Thus, the oxides of cobalt and nickel do not neutralize the acids equally; that of the two which neutralizes it most, will be able to precipitate the other, and remain alone in the solution. Thus, also, since glucine neutralizes the acids much better than highly oxidized iron, it will be easy to separate this metal from its solutions, by first oxidating it strongly, and afterwards precipitating one part of the solution, to employ it, after being well washed, to precipitate the iron of the other part.

The greater or less affinity of the metals for oxygen does not give them any particular property with respect to the mutual precipitation of their oxides.

Oxidation produces a variation in the affinity, or the capacity for saturation of the oxides for the acids; nevertheless, the results are only sensible inasmuch as they produce a change in the neutralization, and in this case they may be attributed to the latter cause.

The affinity of the oxides for the acids may indeed contribute to their mutual precipitations, but its effects are very limited.

It appears, therefore, in general, that, all circumstances remaining otherwise the same, the substances which neutralize the acids best, may precipitate the others from their solutions.

I repeat in concluding, that it is only on the facts which I have related that I have established my reasoning, and that it was not my object in this note to speak of the precipitations by the metals, nor of those which are owing to the reciprocal action of the oxides, or to that of the latter and the alkalis.

IX.

*A Report of the State of his Majesty's Flock of Fine Wooled Spanish Sheep, for the year ending Michaelmas, 1803. By the Right Honourable Sir JOSEPH BANKS, Bart. P. R. S. &c. &c. From the printed Copy communicated by the Author.**

THE wether lambs of the last year having been fold in their wool, and the rams wool retained, in order that two years growth might be prepared for sale together, his Majesty's Spanish flock consisted, when shorn in June 1802, of ninety-six ewes only; the fleeces of these, after having been washed on the sheeps' backs as usual, weighed as follows:

State of his Majesty's flock of Spanish sheep, June 1802.

In wool, as shorn from the sheep	-	-	352	lbs.
Lofs in scowering	-	-	96	
Amount of scowered wool	-	-	256	

This wool, when sorted, produced as follows:

Prime wool, or R.	221	lbs. at	5s. 9d.	£.63	10	9
Choice locks, or F.	32	—	3 6	-	5	12 0
Fribs, or T.	3	—	1 9	-	0	5 3
				£.69	8	0

After deducting the expence of sorting and scowering, at the high rate which an individual who is not a manufacturer must pay for these processes, this wool is worth about 5l. a tod, or 43l. 5s. a pack, as clipped from the sheeps' back.

The prime wool was purchased by John Maitland, Esq. member of parliament for Chippenham, whose mercantile house, established for more than a century, has always dealt largely in the importation of Spanish wool, and who from the first introduction of Merino sheep into this country by the King in the year of 1787, has uniformly given the most liberal and zealous aid to the promotion of his Majesty's patriotic views, though doubtful in the beginning of the ultimate success of the project.

Sale of the wool;

* For the original project of this important undertaking and its subsequent progress, see the former reports by the same Right Hon. Gentleman, inserted in our Journal, quarto, vol. IV. p. 289, and octavo, vol. V. p. 65.

manufactured
into cloth;

It was made into cloth by Mr. Edridge, a manufacturer of Chippenham, whose skill and respectability in his line is exceeded by no man. He inspected its quality with the most minute exactness, and with an eye more inclined to expect symptoms of degeneration than of improvement, during the whole of the numerous processes to which wool is subjected in the making of broad cloth, and he found that in every one of them it answered to his complete satisfaction.

The cloth made from this wool proved so excellent in its kind, that the King was graciously pleased, at the desire of Mr. Maitland and Mr. Edridge, to permit these gentlemen to explain, in his Majesty's presence, its qualities and peculiarities.

of which samples
may be seen.

Samples of this cloth may now be seen in Mr. Maitland's warehouse in Basinghall-street; and it will be found, in conversing with Mr. Maitland and his partners, that in their opinion the R.'s of his Majesty's wool, considered as a pile, are inferior to but few of the best of those imported from Spain, though it is probable that no pile in Spain throws out so small a proportion of F.s and T.s. From this opinion it may fairly be deduced, that his Majesty's wool has improved since the sheep were imported from Spain; indeed there is every reason to believe that it is still improving, and will in a very few years equal if not excel the very best piles that have hitherto been imported into this kingdom.

The wool has
improved.

Improvement of
the carcase of
Merino sheep,
by Mr. Tollet,
without da-
maging the
wool.

Mr. Tollet, a gentleman of Gloucestershire, who has purchased Merino sheep both from the King and from Lord Somerville, has been very successful in improving the carcase without damaging the wool; he possesses a ram, bred from a ram and a ewe both purchased from the royal flock in 1801; which, when clipped in June last, yielded 11 lbs. 12 oz. of unwashed wool. The carcase of this sheep was then estimated by good judges at 16 lbs. a quarter, and it was admitted to be a handsome sheep.

For this animal Mr. Tollet has refused an offer of 200 guineas, or of 100 for the next season's use of him; he also refused 30 guineas each for the sire and the dam, though old and infirm, being unwilling to part with animals which had belonged to the royal flock; he however sold their ram lamb of the last year for 30 guineas, and thus made some progress in ascertaining the value of this important breed.

These

These facts, which prove an amelioration in the King's Merino sheep, are fully confirmed by the improved shape and weight of his Majesty's sheering rams of the present year, and give a justifiable hope, that by a due selection of rams and ewes, and a correct judgment in matching them, Merino sheep will in time be produced, with carcases perfectly fashionable, and wool as perfectly fine.

Confirmation by the improvement of his Majesty's flock.

No purchaser having been last year found for the lambs wool at a price adequate to its value, it was made into light ladies cloth, which proves excellent, and promises to be a valuable article. A speculation, however, has offered for manufacturing the lambs wool into superfine woollen hose, which seems likely to yield a still better price for the raw article than the cloth.

Lamb's wool.

The demand for his Majesty's Merino sheep increases at present beyond all calculation. The best informed clothiers in Gloucestershire, enlightened no doubt by the useful labours of the Bath Society, and the valuable experiment of Dr. Parry, as well as by the Doctor's, and by Lord Somerville's publications, are among the most anxious applicants to purchase. The Bath Agricultural Society, whose attention has been most particularly directed to the improvement of English wool, humbly requested the King to give them a Spanish ram, which request his Majesty most graciously complied with last Autumn, and they returned thanks in the warmest terms of respectful gratitude and satisfaction.

The demand from the growers proves that these facts are well established;

As speculation on the value of Spanish sheep is evidently on the increase, and a reasonable probability now appears that his Majesty's patriotic exertions in introducing the breed, will at last be duly appreciated and properly understood, it would be palpably unjust should the views of those who wish to derive a fair advantage from the sale of the progeny of Spanish sheep purchased by them from the royal flock, be in future impeded by a continuation of the sale of the King's sheep at prices below their real value.

and render it just that the sheep should be distributed by the competition of a public sale.

This circumstance having been stated to the King, his Majesty was graciously pleased to permit the rams and ewes that are to be parted with from the royal Merino flock this year, to be sold by auction, in the same manner as is done at Woburn by his grace the Duke of Bedford, and at Holkham by Mr. Coke, on the presumption of this being the most likely manner of placing

placing the best individuals of their improved breeds in the hands of persons most likely to preserve, and further to improve them.

JOSEPH BANKS.

August 17, 1803.

POSTSCRIPT.

Postscript,

AS the publication of this report has been delayed by unavoidable circumstances to so late a period, it is proper to add, that the wools of 1803 have yielded, both raw and scowered, much as usual. The prime or R. of the ewe flock were sold for 6s. 9d. a pound, and that of the Rams for 6s. 6d. These enormous prices, however, depended on a scarcity of imported Spanish wool, and are highly distressing to the manufacturer: they ought not, therefore, to be allowed to enter into the speculation of the grower.

Notice of sale of the sheep that can be spared.

The sheep that can be spared from the royal flock will be sold by auction this year at a barn opposite the Pagoda in Kew-lane, on the 15th of August next. Notice of the particulars will be given as soon as possible.

July 10, 1804.

X.

On the Oxides of Lead. By THOMAS THOMSON, M. D.
Communicated by the Author.

Great importance of the analysis of metallic oxides:

PERHAPS there is no practical branch of chemical investigation of more importance than the analysis of the metallic oxides. Almost every thing relating to the metalline paints and salts depends upon it; and it involves, either directly or indirectly, most of the interesting questions in the theoretical department of chemistry.

first applied with effect by Bergman and Scheele.

Bergman and Scheele were, I believe, the first persons who applied chemical analysis, in the modern sense of the phrase, to the metallic oxides. Notwithstanding the difficulties with which these illustrious chemists had to struggle, their experiments were made with so much care and sagacity, that they still furnish us with the best data for ascertaining the composition of several of these bodies. No modern chemist has laboured

boured so successfully in this department as Mr. Proust. To Analyses of Proust. him we are indebted for the analysis of the oxides of zinc, iron, tin, copper, antimony, and arsenic. His dissertations are all stamped with the character of originality, and display so much skill and candour, that they never fail to command the confidence of the reader. If he sometimes pushes his consequences a little too far, he more than compensates for this by the originality of his views, and the new light which he throws upon every subject that he discusses.

I intend at present to offer some observations on the oxides Subject of this memoir: the oxides of lead. of lead, a subject more than once slightly touched upon by Proust, but never fully discussed by him. I trust the difficulty of the subject will plead my excuse, if I shall be unfortunate enough to fall into mistakes.

We are acquainted with three oxides of lead sufficiently Three distinct oxides; yellow, red, brown; and two others. distinct from each other. The first is of a yellow colour, and forms the base of almost all the salts of lead; the second is a paint well known by the name of *red lead*; the third a brown powder discovered by Scheele, and examined more lately by Proust and Vauquelin. Besides these three, a fourth has been announced by Proust; and *litharge* has been considered by some as constituting a fifth. Let us examine these oxides.

I. *Yellow Oxide.*

The yellow pigment called *massicot* consists essentially of this Yellow oxide of lead. oxide; but the easiest method of forming it, is to dissolve lead in nitric acid. Pure lead dissolves completely in that acid; but the lead of commerce usually leaves a small quantity of grey powder, which consists for the most part of oxide of antimony, sometimes mixed with a little silica. When the solution is concentrated by evaporation, we obtain crystals of nitrate of lead, a salt too well known to require any particular description.

1. When the crystals of nitrate of lead thus obtained by Nitrate of lead, loses acid by low heat: evaporation, and well dried upon blotting-paper, are exposed to a temperature of about 300°, they lose, at an average, three *per cent.* of their weight. This loss is not to be ascribed to the escape of mere water, for the fumes smell strongly of nitric acid.

2. When 69 grains of lead are dissolved in nitric acid, and — lead contained therein. the solution evaporated to dryness, the nitrate of lead, after being

being dried at the temperature of 300° , weighs 112 grains. Hence 100 grains of lead yield $162\frac{1}{3}$ grains of nitrate of lead. From this we learn that 100 parts of nitrate of lead consist of

$61\frac{2}{3}$ lead,
 $38\frac{1}{3}$ foreign bodies.

—
100.

Carbonate of lead: white lead.

3. When 112 grains of nitrate of lead (dried at 300°) are dissolved in water and mixed with a solution of carbonate of potash, a copious white powder precipitates, which is a *carbonate of lead*. Bergman shewed long ago, that the white lead of commerce is precisely the same with this carbonate. When washed, collected on a filter, and dried at 300° , it weighs 90 grains. This shews us that 69 grains of lead yield 90 grains of carbonate: of course, 100 grains of lead would yield $130\frac{1}{2}$ grains of carbonate. From this experiment we learn, that 100 parts of precipitated carbonate of lead are composed of

$76\frac{2}{3}$ lead,
 $23\frac{1}{3}$ foreign bodies.

—
100.

Carbonate of lead by heat leaves yellow oxide; 9 lead + 1 oxygen:

4. When 90 grains of precipitated carbonate of lead are exposed in a retort to a heat gradually raised to redness, the acid and water which they contain are driven off, and a yellow coloured oxide remains behind. This oxide weighs 77 grains, and contains, of course, 69 grains of lead. Hence it follows, that the yellow oxide of lead is composed of 69 lead + 8 oxygen, or *per cent.* of

89.7 lead,
10.3 oxygen.

—
100.

— very fusible.

It is well known that the oxides of lead very easily melt and run into glass. This happens in the preceding experiment, unless particular care be taken. In that case the lead acts with great energy upon the retort; but the loss of weight is the same, unless the heat has been a great deal too high. When the oxide is fused in an earthen vessel, it covers the surface with a yellow glass, as in the coarsest kinds of pottery. In that case some of the oxide may be dissipated, unless the pro-
per

per precautions are taken. It deserves attention, that when carbonate of lead is slowly heated in a glass retort till it begins to melt, the melted portion has a fine yellow colour, while the colour of what remains in the state of a powder is a dirty pale brick-red; whereas in a platinum crucible the melted portion is red and the unmelted yellow.

5. From the preceding experiments it follows, that the yellow oxide of lead contains 10.3 per cent. of oxygen. Mr. Proust has deduced nine per cent. as the proportion of oxygen, from his experiments. This result does not differ much from mine. If I have committed an error, the oxygen I think is rated too high; for the lead which I used contained $1\frac{1}{2}$ per cent. of antimony, the oxides of which have much more oxygen than the yellow oxide of lead. Perhaps we shall come nearer the truth by taking the mean of the two results: we may therefore consider the yellow oxide of lead as composed of

$$\begin{array}{r} 90\frac{1}{2} \text{ lead,} \\ 9\frac{1}{2} \text{ oxygen.} \\ \hline 100. \end{array}$$

6. The preceding experiments enable us to state the constituents of nitrate of lead as follows:

1. Nitrate dried on Blotting-Paper.

$$\begin{array}{r} 66 \text{ yellow oxide,} \\ 34 \text{ acid and water.} \\ \hline 100 \end{array}$$

2. Nitrate dried at 300°.

$$\begin{array}{r} 68.5 \text{ yellow oxide,} \\ 31.5 \text{ acid and water.} \\ \hline 100.0 \end{array}$$

They give us also the precipitated carbonate of lead, dried at 300°, as follows:

$$\begin{array}{r} 86 \text{ yellow oxide,} \\ 14 \text{ acid and water.} \\ \hline 100. \end{array}$$

The native carbonate of lead contains about 16 per cent. of carbonic acid. Precipitated carbonate then either contains less acid than native, or it loses a part at a low heat. It is well known that carbonates, when in crystals, frequently contain more acid than when in the state of powders.

7. Yellow oxide of lead is a powder of a lively yellow colour, tasteless, insoluble in water, but soluble in fixed alkalies and acids.

Singular fact.
Corrected elements of the carbonate.

Comp. parts nitrate of lead, &c.

Characters and habitudes of the yellow oxide.

acids. The alkaline solutions have a yellow tinge; but the acids are most frequently colourless. It readily melts when heated, and forms a yellow, semitransparent, brittle, hard glass. It does not lose oxygen gas when heated. In violent heats a portion of the oxide is dissipated. When kept heated in the open air, its surface becomes brick-red. When mixed with metallic lead it runs, according to Proust, into a green glaze.

Yellow oxide by mere heat from the nitrate.

8. The yellow oxide may be obtained directly from the nitrate of lead, by exposing that salt to a sufficient heat; but the loss of weight sustained is usually greater than it ought to be. I suspect that this is one reason why Proust found so small a proportion of oxygen in yellow oxide. One hundred grains of nitrate of lead (obtained by evaporation) were put into a small Wedgewood crucible furnished with a lid, and enclosed in a common earthen-ware crucible. They were exposed for half an hour to an intense red heat in a wind-furnace. The salt was converted into a very hard, yellow, brittle glass, nearly opaque: It had sustained a loss of 40 *per cent.* or about six *per cent.* more than it ought to have lost. On breaking this glass to pieces the reason of this became obvious: It contained a great number of globules of lead reduced to the metallic state, some of them of considerable size. From this experiment we learn, that lead is reducible directly from the nitrate merely by the application of heat, without adding any combustible matter.

II. *Supposed first Oxide.*

Ashes of lead, or supposed first oxide. It is yellow oxide and metallic lead.

When lead is kept melted in the open air, it is soon covered with a dirty coloured powder, formerly called the *ashes* of lead. When this powder is heated sufficiently, it melts into a greenish yellow glass, in which globules of lead may be detected. Mr. Proust has shewn, that these ashes are a mixture of the yellow oxide of lead with lead in the metallic state. They do not, therefore, constitute a peculiar oxide.

Neither is the *white oxide* of the French chemists entitled to a place among the oxides of lead; being in all cases nothing more than the yellow oxide combined with an acid, usually the carbonic.

Proust's oxide by boiling lead in the nitrate.

But Mr. Proust, in his observations on the *Connoissances Chimiques* of Fourcroy, has mentioned the method of forming an oxide of lead containing less oxygen than the yellow. When
lead

lead is boiled in a solution of nitrate of lead, the liquid gradually assumes a yellow colour, and, on cooling, deposits crystals in scales. These crystals, according to Proust, contain the oxide in question: But his conclusions, as far as appears, were formed from the single experiment related. He does not seem to have decomposed the salt, nor to have examined its base.

1. When 100 grains of nitrate of lead are dissolved in water, The experiment, and boiled in a phial with a cylinder of lead (weighing $64\frac{1}{2}$ grains), the metal soon loses its brilliancy and is covered with a white crust, while the liquid assumes a yellow colour. The boiling was continued (water being added as fast as it evaporated) till the liquid seemed to exert no farther action on the lead. The cylinder being then taken out and weighed, was found to have lost 44 grains. From this we learn, that 100 grains of nitrate of lead dissolved in water, are capable of uniting with 44 grains of lead, or almost half their weight. The whole, however, was not dissolved. A bluish-grey powder fell to the bottom, and increased in quantity as the cylinder diminished. If these 44 grains were oxidized at the expence of the yellow oxide of the nitrate, we should have a new oxide containing much less oxygen; and it would be easy to assign the proportion of its constituents; for 100 grains of nitrate contain 66 grains of yellow oxide, composed of $59\frac{1}{4}$ lead and $6\frac{1}{4}$ oxygen: Therefore the new oxide contains $59\frac{3}{4}$ + 44 lead and $6\frac{1}{4}$ oxygen, or, *per cent.*

94.3 lead,
5.7 oxygen.

100.0

But it is extremely unlikely that the 44 grains of lead should receive the whole of the oxygen necessary to enable them to dissolve from the oxide, while an excess of nitric acid is present in the solution. Let us therefore examine the new salt.

2. When the solution cools, it deposits thin scaly crystals of a light yellow colour: They have the same sweet astringent taste as common nitrate, but are less soluble in water. If the yellow liquid which remains be farther concentrated, it deposits, on cooling, small needles of a pale yellow colour, not unlike *sugar of lead*. Their taste is sweet and astringent; they are not altered by exposure to the air. When thrown into cold water they fall to the bottom, the liquid gradually becomes

Deposition of scaly crystals by cooling, and small needles by evap. White depos. of carbonate by cold water.

comes milky, and deposits a white powder. This powder does not disappear, though the solution be heated boiling hot; but the liquid acquires the property of dissolving an additional quantity of the salt, without depositing any more white powder. Boiling water dissolves the salt without any similar deposition. Hence I think we may conclude, that the white powder is owing to the presence of some carbonic acid in the cold water, and that our salt in this respect resembles acetate of lead.

The salt deposited in the boiling, contained yellow oxide and an under proportion of acid;

3. When 30 grains of the salt deposited during the boiling of the lead in the nitrate, were cautiously heated to redness, they melted into a yellow mass, which weighed 24.5 grains. The loss of 5.5 grains must be ascribed to acid and water. Hence this salt is composed of

$$\begin{array}{r} 81.5 \text{ oxide,} \\ 18.5 \text{ acid and water.} \\ \hline 100.0 \end{array}$$

These 24.5 grains of oxide being dissolved in nitric acid, yielded 35 grains of common nitrate of lead (dried at 300°). But 35 grains of nitrate contain 24 grains of yellow oxide, which scarcely differs from the quantity dissolved. From this experiment it seems to follow, that the salt in question contains only yellow oxide, and that it differs from common nitrate in containing a smaller proportion of acid. But it will be said, perhaps, that the oxide of the salt absorbed oxygen from the nitric acid during the application of the heat, and was thereby oxidized up to the state of yellow oxide.

as also the crystals do.

4. Twenty-three grains of the needle-form crystals were dissolved in water and decomposed by carbonate of potash. The carbonate had the common appearance, and, when dried in 300° , weighed 24 grains. But 24 grains of common carbonate contain about $21\frac{1}{2}$ of oxide, and ought therefore, when dissolved in nitric acid, to yield about $31\frac{1}{2}$ grains of common nitrate of lead (dried at 300°); and, upon trial, I found this to be the case very nearly. The oxide in the needle-form crystals then is the yellow; for there is no apparent course from which, in the above experiment, oxygen could be drawn. And if this be the case with the needles, it must be so also with the fcaly crystals; for the two salts are obviously the same.

5. Sixty-

5. Sixty-three grains of the salt, partly in scales and partly in needles; were fused with carbonate of potash in a Wedgewood crucible. By solution and filtration a flesh-coloured powder was obtained, which was a mixture of oxide of lead and filica. It weighed 53 grains; but a portion which I could not estimate adhered to the crucible. The filica was obviously abraded from that vessel. Thirty grains of this powder digested in nitric acid, left $3\frac{1}{2}$ grains of filica; of course, $26\frac{1}{2}$ were dissolved. The solution yielded $39\frac{1}{2}$ grains of nitrate of lead. Now $39\frac{1}{2}$ of nitrate contain 27 grains of yellow oxide, or almost the very quantity dissolved. The oxide obtained by this experiment, then, was the yellow; of course, it coincides exactly with the preceding ores.

The oxide of Proust's salt is not different from the yellow.

Proust's salt, then, does not appear to contain a different oxide from common nitrate; but its new properties were owing to the different proportion of its acid. It is completely neutralized, whereas common nitrate contains an excess of acid, and is, in fact, a super-nitrate. But if this conclusion be well founded, Proust's nitrate may be formed by exposing common nitrate to a heat sufficient to expel the excess of acid. It was requisite to verify this presumption by experiment.

6. One hundred grains of nitrate of lead (dried in 300°) were exposed to a graduated heat in a flask. Fumes of nitrous acid separated in abundance, and the salt lost five *per cent.* of its weight. On increasing the temperature the salt melted into a transparent glass of a very pale yellow colour. The weight of the mass was now reduced to 85 grains. Hence it was composed of 68.5 oxide and 16.5 acid, or, *per cent.* of

Proust's salt made by abstracting acid instead of adding lead.

80 oxide,

20 acid.

100.

On pouring water into the flask and digesting, I obtained a yellow solution similar to that formed by boiling lead in nitrate of lead, but not so deep. A yellow powder refused to dissolve; it consisted chiefly of the portion of salt at the bottom of the flask, which had been exposed to a higher temperature. It was tasteless, and not unlike sub-muriate of lead. When heated to redness it melted into a yellow glass, and lost 14 *per cent.* It was therefore composed of

86 oxide,
14 acid and water.

100.

The solution being evaporated, deposited two sets of crystals; one set consisting of common nitrate of lead, another set resembling those obtained by Proust.

Three nitrates of lead; viz. neutral, and with excess or with defect of acid.

7. From the preceding details we learn, that there are three distinct species of nitrated lead: The first is a *super-nitrate*, or contains an excess of acid; the second is neutral; the third contains an excess of base, and is, of course, a sub-nitrate. The first species includes the common nitrate of chemists in all its varieties; the second, the nitrate of Proust; the third, the yellow powder obtained by heating common nitrate sufficiently.

III. *Brown Oxide.*

Brown oxide; left when minimum is dissolved in nitric acid, &c.

Though this oxide contains a maximum of oxygen, I beg leave to introduce it here, because the knowledge of its composition is necessary to enable us to analyse the *red oxide* of lead. It was discovered by Scheele, and described by him in his dissertation on manganese. When diluted nitric acid is poured upon red lead, the greater part of the oxide is dissolved, but a brown powder remains behind, which is not acted upon by the acid. This brown powder is the brown oxide of lead. Proust discovered that it may be formed also by causing a current of oximuriatic acid gas to pass through red lead suspended in water.

Its habitudes;

1. This oxide is a tasteless powder, of a flea-brown colour, and very fine and light. It is not acted on by sulphuric nor nitric acid. To muriatic it gives out oxygen, and converts it into oximuriatic acid. Oximuriatic acid dissolves it, and forms two salts, muriate and hyper-oximuriate of lead. The vegetable acids reduce it to the state of yellow oxide. Fourcroy, on the authority of Vauquelin, affirms, that sulphur takes fire when triturated with brown oxide of lead. With me the experiment did not succeed: I suspect, therefore, that the oxide used by Vauquelin contained a portion of hyper-oximuriate of lead mixed with it.

contains one-tenth more oxygen than yellow

2. When 100 grains of this oxide, prepared from red lead by nitric acid, are exposed to a red heat, they lose nine grains

of

of their weight, and are converted into yellow oxide: These ^{oxide; easily} nine grains are oxygen gas. Hence brown oxide is composed ^{disengaged.} of 91 yellow oxide and 9 oxygen. But 91 of yellow oxide contain 9.4 of oxygen. Therefore 100 parts of brown oxide are composed of

$$\begin{array}{r} 81.6 \text{ lead,} \\ 18.4 \text{ oxygen.} \\ \hline 100. \end{array}$$

3. Mr. Proust, from his experiments, states the proportion of oxygen in this oxide at 21 *per cent.* If we take the mean of the two results, we obtain 19.7. We may, therefore, lay down 20 *per cent.* as the proportion of oxygen in brown oxide of lead: This cannot deviate far from the truth.

IV. Red Oxide.

Red lead being one of the most common of pigment, is un-Red lead; known, I presume, to no person. The method of manufacturing it has been described by Dr. Watson in his *Chemical Essays*, by Jars in the *Memoires* of the French Academy for 1770, and by Ferber in his *Mineralogy* of Derbyshire.

1. It is a tasteless powder, very heavy, and of an intense ^{its characters;} red colour, often inclining to orange. I have never met with any specimens of it absolutely pure, but not unfrequently the foreign bodies do not exceed one or two *per cent.* They consist of seven grains of sand and oxide of antimony. Dr. Watson found traces of silver in it. It loses no sensible weight in a heat of 400°.

2. When 50 grains of red lead are digested in diluted nitric ^{contains 88 lead} acid, they leave 12 grains of brown oxide. The solution eva- ^{+ 12 oxygen;} porated to dryness, yields 56 grains of nitrate of lead. Now, 56 grains of nitrate contain 38.36 grains of yellow oxide. Red lead, therefore, is composed of 38.36 yellow oxide and 12 brown oxide, or, *per cent.* of

$$\begin{array}{r} 76.72 \text{ yellow oxide,} \\ 24.00 \text{ brown oxide.} \\ \hline 100.72 \end{array}$$

The excess must be ascribed to the imperfection of our methods. I shall omit it in the calculation: Not that red lead

is a mixture of yellow and brown oxides, but that it contains all the lead and oxygen in the above proportions of these bodies. Now

76 grains yellow oxide is composed of	68.8 +	7.2	
24 grains brown oxide of - - -	19.2 +	4.8	

Therefore red lead is composed of - 88. + 12. = 100.

not easily de-
composed by
mere heat.

3. It is well known that red lead gives out oxygen gas when heated, and that it approaches to the state of yellow oxide. The loss of weight ought to give us the portion of oxygen which it contains more than is necessary to constitute it yellow oxide: But, upon trial, I could obtain no satisfactory results this way. In one experiment 100 grains of red lead lost $4\frac{1}{2}$ per cent. in another seven per cent. The experiments were made in small covered earthen-ware crucibles. The oxide was melted into a dark-brown transparent glass, not unlike glass of antimony, but much harder. On breaking this glass, I found in it globules of lead reduced to the metallic state: This accounts in part for the loss of weight, and shews us also, contrary to the opinion of chemists, that the red oxide of lead is reducible, at least in part, by mere heat. In all probability nothing prevents the complete reduction but the readiness with which it unites with the vessel in which the experiment is made. I twice varied the experiment, by enclosing the small earthen crucibles containing the red lead in a crucible of platinum; but in neither case did I obtain any visible metallic globules; yet the loss of weight was the same. This renders it probable that a portion of lead had been reduced and afterwards diffused through the oxide.

Not soluble as
red lead in acids,
nor in alkalis.

4. The red oxide of lead does not seem capable of combining with acids. Many acids indeed act upon it, but they always begin by reducing it to the state of yellow oxide. The fixed alkalis do not alter its colour, but they gradually dissolve it. From this solution it is thrown down always in the state of yellow oxide: Hence it must lose oxygen during the solution.

V. Litharge.

Though litharge is very far from being the same with red lead, yet, as the mode of preparation is analogous, a few remarks on it may not be improper in this place.

Litharge

Litharge consists of scales, partly of a golden yellow colour Litharge; and partly red: They possess a certain degree of elasticity. The method of making litharge has been described by Dr. Watson, by Gmelin, and by other chemical writers.

1. When 100 grains of litharge are exposed to a red heat, they melt into a yellow glaze, and lose, at an average, four grains of their weight. When 50 grains of pounded litharge are thrown into nitric acid, they dissolve with effervescence, and lose two grains of their weight. The effervescence and loss of weight are owing to the escape of carbonic acid gas. From this we may conclude, that litharge contains four per cent. of carbonic acid.

2. When 50 grains of litharge were dissolved in nitric acid, and the solution evaporated to dryness and re-dissolved in water, $1\frac{1}{2}$ grains of a grey powder remained behind in my trials, which proved to be oxide of antimony. Therefore, litharge contains three per cent. of oxide of antimony.

3. The solution evaporated to dryness, gave 68.5 grains of nitrate of lead; but this nitrate contains 46.72 grains of yellow oxide of lead. Of course, we have litharge composed of

93.44	yellow oxide of lead,
3.00	oxide of antimony,
4.00	carbonic acid.

100.44

The small excess must be ascribed to unavoidable errors in the analysis.

4. Fifty grains of litharge dissolved in nitric acid, deprived of its oxide of antimony, and then thrown down by carbonate of potash, gave $52\frac{1}{2}$ grains of carbonate of lead. Hence 97 grains of litharge (supposing the antimony a foreign body) would have given 105 grains of carbonate. But 97 grains of litharge contain nearly four of carbonic acid. Hence we have the carbonate formed of 93 oxide and 12 acid.

In this experiment the carbonate produced was too small by about a grain. This was partly owing to the loss of a small quantity of the powder while separating it from the filtre. As I could not estimate the loss, I left it out in the calculation, and stated the amount precisely as I found it.

From the preceding experiments it follows, that litharge is a sub-carbonate of lead, since it consists essentially of about 96 parts of yellow

96 yellow oxide,
4 carbonic acid.

100.

Probably it varies somewhat in the proportion of its constituents, according to circumstances: But all my trials were made on one parcel of litharge. I have observed traces of carbonic acid also occasionally in red lead, but too little to affect its weight.

VI. Conclusions.

Summary recapitulation.

From the preceding experiments and observations we are entitled, I think, to draw the following conclusions:

1. Three oxides of lead only are at present known. The constituents of these oxides may be seen in the following table:

Oxides.	Colour.	Constituents.	
		Lead.	Oxygen.
Protoxide.	Yellow.	90.5	9.5
Deutoxide.	Red.	88	12
Peroxide.	Brown.	80	20

Lead. Oxygen.

100 + 10.6 = 110.6 protoxide.

100 + 13.6 = 113.6 deutoxide.

100 + 25. = 125. peroxide.

2. The ashes of lead are a mixture of protoxide and lead in powder.

3. White lead and litharge are combinations of protoxide with carbonic acid: the first is a carbonate, the second a sub-carbonate of lead.

* As colour is a very ambiguous criterion for distinguishing metallic oxides, I have been accustomed for some time to denote the oxide with a minimum of oxygen by prefixing the Greek ordinal number to the term oxide: Thus, *protoxide of lead* is lead united to a minimum of oxygen. The oxide with a maximum of oxygen I call *peroxide*: Thus, *brown oxide of lead* is the *peroxide* of lead. I denominate the intermediate degrees of oxidizement by prefixing the Greek ordinals 2d, 3d, 4th, &c. Thus, *deutoxide* is the second oxide of lead, *tritoxide* of cobalt, the third oxide of cobalt, and so on,

4. The yellow nitrate of Proust contains the same oxide as common nitrate. But in it there is no excess of acid; whereas common nitrate is in fact a *supernitrate* of lead. In a strong heat it is partly converted into a *subnitrate*.

5. Protoxide of lead unites with all acids, deutoxide with none, and peroxide only with hyperoxymuriatic acid.

6. The protoxide of lead may be formed by combustion; but the other two cannot, and indeed lose oxygen in a strong heat. The deutoxide is formed by keeping protoxide in contact of air at a given temperature: the peroxide by the action of nitric or oxymuriatic acid on the deutoxide.

XI.

On certain Chemical Effects of Light. In a Letter from Wm. HYDE WOLLASTON, M.D. F. R. S.

To Mr. NICHOLSON.

SIR,

HAD I foreseen the publication of Mr. Ritter's '*Experiments on Light*' in the last number of your Journal,* I would have requested you to accompany them with a few observations of mine on the same subject; not with a view of claiming any priority in the observation of those invisible rays, that have chemical effects, which I believe occurred to Mr. Ritter and myself very nearly at the same time; but for the purpose of inserting a caution against the theory implied by the term "*disoxidating*" as applied to those rays.

In my note upon a communication to the Royal Society,† which you did me the honour to reprint in the 4th Vol. of your Journal, ‡ I was careful to express the power exerted by the most refrangible rays on muriate of silver, in general terms as *chemical*, not merely from a doubt whether they would in other cases produce a corresponding effect, but because I had at that time made the following experiments, which proved that the same rays, which cause the emission of oxygen by muriate of silver, occasion its absorption by the resin usually called gum guaiacum.

Introduction on the invisible rays that have chemical effects.

The power of these rays was termed *chemical* by Dr. W. and not *disoxidating*, because they do not constantly disoxidate.

* Page 214. † Phil. Trans. 1802, p. 379. ‡ 8vo. series, p. 99.

My

My reasons for withholding these experiments at that time were, that they appeared somewhat irrelevant to the primary subject under discussion, and that I was also in hopes of increasing their value by additional trials on other substances.

Whether the solar rays tend to extinguish fire; —not proved.

Upon considering the power which these rays possess of expelling oxygen from the muriate of silver, I thought it not impossible, that there might be more truth than I had been accustomed to suppose, in the popular observation that the sun has a tendency to extinguish fire, as the same rays might retard combustion by opposing the absorption of oxygen. Accordingly I made various experiments on different substances in a state of slow combustion, but without any apparent confirmation of that hypothesis.

Light or solar rays did not affect vegetable blues;—

I also tried the action of light on several vegetable blue colours, which are known to be affected by union with oxygen, and upon the same colours previously reddened; but on these also I did not succeed in producing any effect at either boundary of the prismatic spectrum.

Trial of guaiacum,

After failure of these endeavours, I had recourse to guaiacum, which I had long known to acquire a green colour by exposure to light; but that the presence of air is also requisite for this purpose I had ascertained in the following manner.

No change by solar rays without air.

Two plates of glass were heated with a small piece of guaiacum interposed, and thereby cemented together in their centers for a circular space about $1\frac{1}{2}$ inch diameter. In this state they were exposed for several weeks during summer to the sun, without the smallest apparent alteration in the colour of the guaiacum.

—but when the air had access;

The plates were then forcibly separated; and as they were both similarly coated with a portion of the resin adhering to their surfaces, one of them was preserved for comparison in a dark place, where it had free access of air alone, while the other was again exposed uncovered to the mid-day sun.

—it was affected in five minutes by the noon-day sun.

The latter was in five minutes perceptibly rendered green, and in a few hours had acquired the full colour, which it seemed capable of receiving; but the former, in the course of many months that it was kept confined from the light, seemed not to have been discernibly altered.

The prismatic spectrum was too weak to show whether all the rays were effective or the contrary.

Since by later experiments it appeared probable, that the whole of the sun's rays were not active in this process, with a view to determine on what part of them the effect might depend,

pend, I dissolved some guaiacum in alcohol, and after having washed a card with the tincture, I exposed it for some time to different parts of the common prismatic spectrum, but without producing any apparent change. It therefore became necessary to have recourse to other expedients for increasing the power of the spectrum.

Over the surface of a lens 7 inches in diameter, was pasted a circular piece of paper having its radius $\frac{1}{10}$ of an inch less than that of the lens. I had consequently remaining uncovered a prismatic annulus, corresponding in the length of its circumference to a prism 22 inches long, so arranged by its circular form that any one of the colours might at pleasure be brought to a focus, or the spectrum might be received as a ring of any diameter required, by mere variation of the distance of the lens. At short distances the exterior margin of the spectrum of course was red, and the violet within. The focus of brightest illumination was at $24\frac{1}{4}$; at greater distances the spectrum again became an annulus with its colours in an opposite order to the preceding, having the violet on the exterior margin.

With this apparatus the effect produced on muriate of silver is much accelerated. At distances short of $22\frac{1}{4}$ inches a ring is produced; at $22\frac{1}{2}$ a circular dark coloured spot; and at about 23 inches appears to be the focus of these rays, as the spot is then smallest; at $23\frac{1}{2}$ it is larger, at $24\frac{1}{4}$ it again becomes a ring shaded to the center; and at $24\frac{1}{2}$, (unless the paper has been wetted,) the center remains completely white though strongly illuminated. I have not however been able in any situation to restore the white colour to muriate of silver, after it has once been tinged, however slightly, by exposure to the most refrangible rays.

The experiments on guaiacum nevertheless will prove distinctly, that the powers of the two extremities of the spectrum are not only different, but opposite in their chemical effects.

A sufficient quantity of paper having been tinged with the solution of guaiacum, was cut into small pieces, some of which were exposed to the sun-shine till rendered completely green; the rest were kept confined from the light till taken out for each experiment.

The first endeavour was to ascertain the focal distance of those rays which gave the deepest colour in a given time; and it was found to be about 23 inches distant from the lens. At shorter

A broad convex lens being covered all but a narrow exterior ring, gave, at pleasure, according to the distance, a focal spot of any one colour, or an annular spectrum, &c.

Muriate of silver very speedily affected by these concentrated rays;—

but not restored to whiteness.

Guaiacum was affected both ways.

At a short focal distance (namely of rays most refrangible) the deepest green was produced;—

shorter distances the surface coloured was larger, but in the same time paler. At $22\frac{1}{2}$ a green ring was formed, having its center without colour.

At a medium focus no effect was produced;

When the guaiacum was exposed at distances greater than 23 or $23\frac{1}{4}$, the surface coloured was also larger, but much paler than at equal distances short of the focus; inasmuch that at $24\frac{1}{4}$, which corresponds with the principal focus of illumination, little or no effect was produced in the space of one minute, which was the time employed in other experiments. It was manifest therefore, that the chemical effect of the most refrangible rays (which were now diverging beyond their focus) was in this situation counteracted by an opposite action equally powerful, of the most refrangible (not yet arrived at their focus;) and as it appeared probable that the power, which could in one instance prevent discoloration, might also, when duly applied, remove the same colour after it had been produced.

At a longer focal distance (namely of rays least refrangible) the original yellow was restored.

I next exposed to the condensed spectrum, at various distances from the lens, portions of the paper that had been previously rendered uniformly green. A second focus was now found at the distance nearly of $25\frac{1}{4}$, in which the green colour was completely removed, and the guaiacum restored to its pale yellow colour.

It is unnecessary to describe minutely the consequences resulting from variations of the distance, as the effects in this instance were necessarily the counterpart to the preceding; the circle of yellow was larger, when the paper was placed at a greater distance from the lens, and at the distance of $25\frac{7}{8}$ the center remained green, surrounded by a yellow ring, corresponding nearly to the red and orange interior margin of the annular prismatic spectrum.

In carbonic acid gas the disoxidation, but not the oxidation was practicable.

The same experiments being afterwards repeated in carbonic acid gas, only confirmed the opinion before entertained, of the causes to which the changes of colour were owing, but afforded little additional information. In this gas the guaiacum could not be rendered green at any distance from the lens, but was speedily restored from green to yellow by exposure to the focus of red rays.

The removal of colour (or disoxidation) was effected by heat from a piece of metal.

Since the removal of colour was observed to take place in the situation of the principal focus of heat, it seemed desirable to ascertain whether the presence of light, or the circumstance of radiation had any influence in promoting this effect. A piece

piece of paper was therefore stained with guaiacum and after being rendered green by exposure for a sufficient length of time to light, was pressed on its posterior surface with a silver spoon previously heated over the flame of a candle; and the green colour was thereby as effectually removed, as in the focus of solar heat.

The last experiment may possibly appear to have been unnecessary; but until it is explained why the heat, that accompanies the sun's rays, penetrates the substance of transparent or semi-transparent bodies, while the radiant heat from a fire has scarcely power to enter even the most transparent, but principally scorches the surface, and is thence slowly conducted into the interior parts: no degree of caution upon a subject so imperfectly understood, should be deemed superfluous.

Radiant solar heat penetrates transparent bodies; culinary radiation scarcely at all.

I remain, Sir, &c.

W. WOLLASTON.

XII.

*Illustrations of Mr. Dalton's Theory of the Constitution of Mixed Gases. In a Letter from Mr. WM. HENRY, of Manchester, to Mr. Dalton. Communicated by the Writer.**

To Mr. DALTON.

DEAR SIR,

IN the first enunciation of a new theory, it is not unusual that some links are omitted in the chain of reasoning, which led to its formation; and thus the doctrine fails of that ready and general acceptance, which immediately follows its more distinct development. Such an omission appears to me to have taken place in your Theory of the Constitution of Mixed Gases; for, according to your own candid confession, several persons, versed both in chemical and mechanical science, have declared their inability fully to understand the scope of the hypothesis, and consequently to judge of its merits or defects. In the discussions also, which took place in this Society, on your several papers, the doctrine was opposed by almost every member interested on such subjects, and by no one more strenuously

Causes why a new theory may not readily produce conviction and adoption.

* Read before the Manchester Society.

nuously

nuously than myself. Subsequent attention, however, to the evidences of the theory, and still more the results of experiments which were made under impressions very unfavourable to the hypothesis, have satisfied me that the opposition to it arose chiefly from an imperfect comprehension of the argument; and that your theory is far better adapted than any former one, for explaining the relation of mixed gases to each other, and especially the connection between gases and water.

Distinguishing principle of Dalton's theory.

The distinguishing principle of your doctrine I apprehend to be, that *mixed gases neither attract nor repel each other, and that every gas is as a vacuum to every other gas.* It is not my intention to recapitulate your proofs of this position, but merely to add to them the evidence of a few facts, which have occurred to me, and which strongly tend to establish the same conclusion.

Since gases are held in water (always) mechanically in quantities as the pressure, different gases do not press each other if they do not prevent escape.

From a series of experiments, which I communicated to the Royal Society, and which appeared in their Transactions for 1803, it may, I think, be safely inferred, that the relation of gases to water is altogether a mechanical one; for the quantity absorbed follows exactly the ratio of the pressure. If then it can be shewn that a gas, absorbed by water, is not retained in its place by an atmosphere of any other gas, we shall be furnished with a strong presumption that different gases do not gravitate on each other.

Carbonic acid quits water exposed to the atmosphere,

It is well known that water may be charged with its own bulk, or rather more, of carbonic acid gas, under a pressure of 30 inches of mercury. The gas, thus absorbed, is retained so long as the water is preserved from contact with any other gas; but, when exposed to the atmosphere, the carbonic acid gas rapidly escapes. Now this effect can be only ascribed to one of two causes, 1st, the affinity of carbonic acid for atmospheric air may surpass that of its affinity for water; or, 2dly, the air of the atmosphere does not press on the gas in the water, which is therefore placed under similar circumstances, as if exposed under the exhausted receiver of an air-pump.

—because attracted, or else because not pressed by the atmosphere.

Not from attraction; for atmospheres of different gas cause no difference.

Were the first explanation the true one, it might be expected that equal quantities of various gases would detach different quantities of carbonic acid from like volumes of impregnated water; because the affinities of these gases, as in all other cases of chemical affinity, differing in force, would occasion their combining with different quantities of carbonic acid, and in a certain

certain order. But on making the experiment, with all the attention I could bestow, this did not prove to be the fact: for similar measures of impregnated water gave up equal bulks of carbonic acid, to like quantities of all the different gases.

The reverse of this fact also occurred to me in the course of a series of experiments, to which I have already referred; viz. that the admixture of common air with carbonic acid gas diminishes considerably the proportion of the latter gas taken up by water. Thus, when 20 measures of pure carbonic acid gas are agitated with 10 of water, at least 10 measures of gas are absorbed. But from a mixture of 20 measures of carbonic acid with 10 of common air, 10 parts of water take only 6 of carbonic acid. That chemical affinity between the mixed gases is not the cause of the diminished amount of absorption, is perfectly clear; since it is indifferent, as to the effect, what gas is added, and the proportion alone influences the result. The effect is therefore to be ascribed to the diminished density of the superincumbent carbonic acid by mixture with another gas; and the pressure of gases being directly as their density, and the quantity absorbed by water being as the pressure, the absorbed carbonic acid must necessarily quit the water. This escape continues till the carbonic acid *above* the water has a density equal to that *in* the water, and no longer.

If carbonic acid be mixed with another gas, the absorption by water is governed by the density of the carbonic acid and not by the nature of the gas.

Previously to my acquiescence in your theory of mixed gases, I undertook an extensive series of experiments, with a view to ascertain the order of affinities of gases for water. But, after a great variety of trials, made with all the accuracy in my power, I could discover nothing like a series of elective attractions. Each gas, it was found, displaced every other, and reciprocally was dislodged by them.

Water has no preference of attraction as to the gases.

It may be urged against the doctrine of the non-gravitation of gases on each other, that from water impregnated with carbonic acid gas, and exposed to the atmosphere, the gas ought, on this principle, to escape as rapidly as under an exhausted receiver. It must be remembered, however, that the escaping gas constitutes, by admixture with the air of the atmosphere, a gas of diminished density, but still of such density as to retard the escape of farther portions. All that the air-pump effects is to remove these as fast as they are liberated.

The slower escape of a gas from water exposed to the atmosphere compared with a vacuum arises from the less rapid exhaustion of the superincumbent gas.

There are various facts, satisfactorily explained on this doctrine, which are irreconcilable to any former hypothesis. Of these

Facts explained by Dalton's theory.

these I shall mention only a few; since the theory will receive from yourself all the elucidation that its establishment can require.

Light and heavy gases mix spontaneously.

1. If each gas be a vacuum to every other, a heavier gas should ascend into a lighter one, without the aid of agitation; and on the contrary a lighter one should descend into a heavier one. That this is actually the fact, and under circumstances very unfavourable to their mixture, your own experiments have fully proved.

Sulphuret takes oxygen from air not agitated.

2. The hypothesis explains why sulphuret of potash withdraws oxygen from the air without agitation, and whether placed at the top or at the bottom of a jar; for it acts as if the absorbed gas were the only one present in the vessel.

Aborbable gases expel the last portions of common air from water.

3. It explains why the last portions of common air are expelled from water by carbonic acid, and other aborbable gases. For these gases act as a vacuum to the air contained in the water, which must therefore necessarily quit its place. It solves also the problem how to expel completely any gas from water; for to effect this, the water must successively be agitated with portions of some other gas of the greatest attainable purity. Thus to expel atmospherical air entirely from water, it may be agitated with pure carbonic acid gas; but as the liberated common air presses on that remaining in the water, according to the proportion it bears to the superincumbent carbonic acid, the gas thus employed must be removed, and fresh and pure portions used in succession.

Best method of impregnating water with a gas.

4. By applying the same general law, we are taught how to effect the highest attainable impregnation of water with any gas. There could be no difficulty in accomplishing this object, if the gas and water were both absolutely uncontaminated by admixture with other gases; but when pure carbonic acid is agitated with water, atmospherical air is extricated, which, mingling with the carbonic acid, lessens its density. To obviate this difficulty as much as possible, a quantity of water, to be impregnated fully with carbonic acid, should be agitated with several successive portions of the purest possible gas. The unabsorbed residuum should also be very large, in order that the carbonic acid may bear a large proportion to other aeriform substances accidentally mixed with it.

These are, doubtless, only a few of the phenomena, to the explanation of which your theory may be successfully applied; and

and I confidently expect that many facts, hitherto referred to chemical principles, will be brought, in consequence of your discoveries, within the pale of mechanical philosophy.*

I am, Dear Sir,

Your's very truly,

WILLIAM HENRY.

Manchester, June 20, 1804.

XIII.

On the Disappearance of Oxigen and Hidrogen over Water, at the Heat of the Atmosphere. By T. S. T.

To Mr. NICHOLSON.

SIR,

SOME months ago, I read in your excellent Journal an account of an experiment, which tended to shew that oxygenous and hydrogenous gases, when mixed together, and allowed to remain over the surface of water for a long time, spontaneously united and formed water. Slow absorption of oxigen and hidrogen over water.

Having long been accustomed to consider a temperature considerably higher than that our atmosphere ever attains, necessary to this union, I was naturally led to investigate this phenomenon; and for that purpose undertook the following experiments:

1. I prepared oxygen gas from black oxide of manganese, by means of concentrated sulphuric acid, aided by heat, and in order to render it more pure, I washed it well with milk of lime. I prepared likewise a quantity of hydrogen gas, by passing a few drops of water through a gun-barrel, filled with iron filings, and passed through the body of a small furnace, I introduced nearly equal quantities for both gases into a bell-glass jar, placed on the shelf of a common pneumatic trough, which stood in a room without fire, and almost without light. The mixture was suffered to remain in that situation for about five months; at the end of which time, the volume of the gases had diminished $\frac{1}{3}$.

* As the author had not seen Mr. Dalton's letter, published in our last number, at the time when these illustrations were written, he has mentioned a few circumstances contained in the letter.

2. On

Slow absorption
of oxigen and
hidrogen over
water.

2. On reading the account above alluded to, it occurred to me that it was possible, that the diminution in bulk, might have arisen from a partial absorption of one or of both gases, by the water of the trough. In order to ascertain this, I introduced like proportions of both gases, into a jar, placed in a mercurial trough, which was in the same room with the other; and after suffering this experiment to continue as long as the other, I found that of 12 cubic inches of both gases introduced into the jar, $3\frac{1}{2}$ had disappeared; but I could scarcely perceive any moisture on the sides of the jar, owing to the small quantity of water which had been formed.

The decrease in volume, in these experiments, could not be owing to any condensation of the gases, by the coolness of the surrounding air; for I found that it took place gradually; and the mixtures were made in the beginning of January, and stood till the end of May; consequently there should have been rather an increase than a decrease in bulk, if the temperature of the air was the cause.

The residuary air contained in the jars, still consisted of oxygen and hydrogen gases; for when received into a phial, on the application of a lighted taper, a smart explosion took place, and the sides of the phial grew dim. On adding sulphuret of lime to another portion of the residue, a rapid absorption of the oxygen gas took place, and hydrogenous gas was left behind.

From these facts we may fairly conclude, that the decrease in volume was owing to the spontaneous combination of the two gases to form water.*

T. S. T.

Orkney, June 20th, 1804.

* It deserves to be considered whether the absorption of the purer gases within, and the escape at the surface of the water exposed to the atmosphere, according to the doctrine explained in Mr. Henry's paper (page 297) may not have occasioned the deficiency.

W. N.

XIV.

Letter from Dr. P. A. NEMNICH, expressing Doubts with regard to the Death of the celebrated Humboldt.

To Mr. NICHOLSON.

SIR,

Hamburgh, June 29, 1804.

YOUR Journal of Natural Philosophy, &c. June 1, 1804, Inquiry concerning the death of Mr. Humboldt. page 72, mentions positively the death of Mr. Humboldt, which, as we had here in Germany no other advices, I immediately communicated to my countrymen through the channel of our newspapers. There are however in Germany, as well as in France, many doubts about the validity of the said notice, and many objections made. Having quoted your Journal, as above-mentioned, I should be very much obliged to you for a more circumstantial and positive account of this report of Mr. Humboldt's death, with the day of his decease, and the way by which this notice reached England, &c. which as soon as I have received, I will instantly make public, in order to maintain that credibility your valuable Journal deserves.

I am, Sir,

Your's most respectfully,

P. A. NEMNICH,
Licentiate.

Extrait du Publiciste.

Paris, 20 Juin, 1804.

VOTRE feuille de ce jour contient à l'article de Hambourg la nouvelle de la mort le Mr. de Humboldt. Il m'est permis d'en révoquer en doute l'authenticité, et de rassurer les amis des sciences et de l'humanité. Je fais positivement que Mr. Guil. de Humboldt à Rome, a reçu de son frere des lettres datées de la Havane du 28 Mars, dans lesquelles il lui marquoit que sous 12 jours il seroit rendu à Charlestown d'ou il s'embarqueroit de suite pour le Havre, et qu'il comptait être à Paris avant la fin de Juin.—La nouvelle est donc plus que douteuse et nous pouvons esperer, que le sort ne se seroit pas fait un jeu cruel de rendre vain le dévouement sans bornes et les nobles efforts de l' illustre voyageur. (Signé) Mendelssohn.

Extract from the Publiciste.

Extract from the Publiciste.

Paris, 20th June, 1804.

Translation.

YOUR number of this day contains under the article *Hamburgh*, an account of the death of Mr. de Humboldt. I have reasons to doubt the authenticity of this article, and to encourage the friends of science and of humanity. I know positively that Mr. William de Humboldt received from his brother letters dated from the Havannah, of the 28th March, in which he informs him that in twelve days he should go to Charlestown and embark for Havre, with the expectation of arriving in Paris before the end of June. Your article of news is therefore more than doubtful, and we may hope that the course of events have not been so unfavourable as to render the unlimited sacrifices and efforts of this illustrious traveller of no use to society.

* * Dr. Gibbes, the author of the note in question, will, no doubt, have the goodness to mention his authority, when he sees this.

W. N.

From the extraordinary Number of valuable Communications this Month, (every one of the Articles, but those of GAY LUSSAC and BARTHOLDI, being original) it has been necessary to postpone an excellent Memoir on Haüy's System, by the Abbé Briel, and a Paper by E. O. on the Computation of Tables of Squares and Cubes, both which, and some Abridgements and Collections of interesting Matter from the Phil. Transactions, together with the Scientific News, will appear in our next.

Antique painting composed in Glass.

Fig. 2.

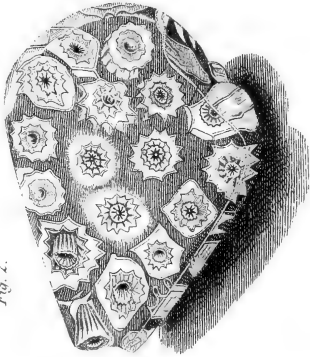
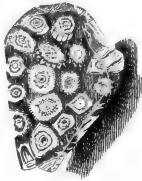
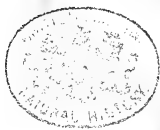


Fig. 1.





Improved Chemical Lamp.

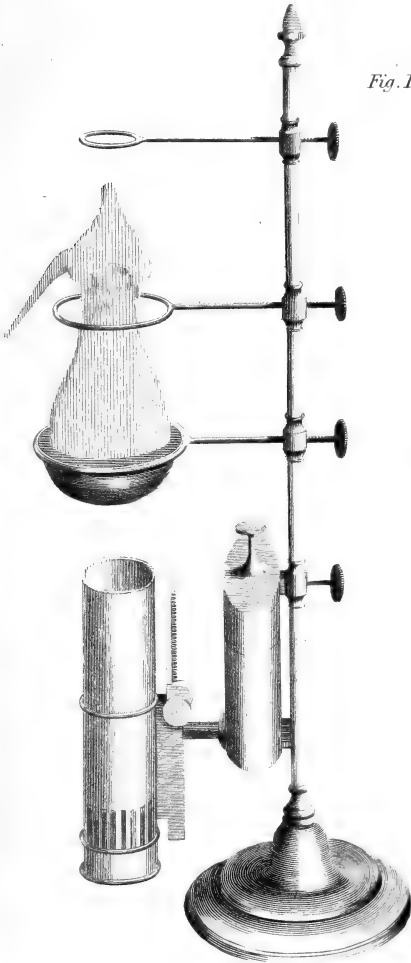


Fig. 1.



Fig. 2.

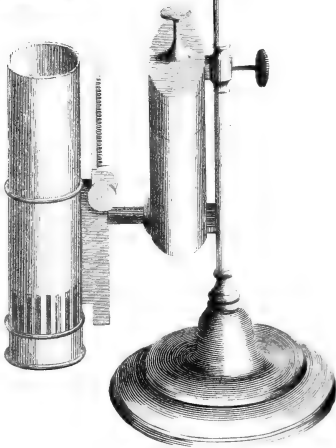


Fig. 3.

1 Foot.



INDEX.

A.

ACCUM, Mr. his examination of a stone containing potash, 127—His process for obtaining alumine pure, 260—Description of his improved chemical lamp with double concentric wicks, 266

Acid, nitrous, accension of fulphuretted hydrogen gas by, 144

— phosphoric, abundant in vegetables, 241

— suberic, contained in paper, 142

Acids, neutralization of, by the metallic oxides, 273

Affinity, chemical, of common air, examined, 145—Of the gases for each other, is alike in all, 148

Air, on the change which it undergoes by respiration, 40—On the supposed chemical affinity of its elements, 145—Its density is the cause of the various phenomena of combustion, 146—Is not dissolved by water, 149—Possesses hygrometrical attraction, 245—Reasons for considering it an homogeneous fluid, *ib.*

Alchemy, on the researches which constitute the subjects of, 27

Alcohol, its mutual repulsive action with other fluids, 201

Alumine, on the difficulty of obtaining it in a state of purity, 141—A method pointed out, 260—Is precipitated by the metallic oxides, 274

Amber, large specimen of, 143

Ammonia, its repulsive action on other fluids, 204—Dissolves oxide of iron at a minimum, 272

Vol. VI.

Animal body, effects of galvanism on the, 179

Apparatus, for filtering water, description of an, 126

— galvanic, on the means of simplifying and improving, 1—In a single piece, 3—Affording a larger surface for oxidation, and convertible into one or more plates, 79

Arch of the meridian measured in the Mysore, 18

Arnold, Mr. 47

Arseniated copper, Haüy's observations on, 187—Varieties of, 188—Analyses of, 189—Count de Bournon's reply, 247

Ash, Dr. 72

Ashes of lead, composition of, 284

Atmosphere, comparison of its temperature with sea water, 132

Attraction hygrometrical, is diminished by the accension of water, 244

Azote, electricity does not fuse lead placed in, 82

B.

Banks, Sir Jos. 221—His report of the state of his Majesty's flock of fine-wooled Spanish sheep, 277

Bartholdi, Prof. on spontaneous inflammations, 216, 236

Barton, Dr. on the fascinating faculty ascribed to the rattle-snake and other American serpents, 58, 100

Beauvoil, on the virtues and principles of cantharides, 76

Bedford, Duke of, 279

Bergman, 272, 280

Berthollet on the differences between heat and electricity, 80—His observations on

INDEX.

- Count Rumford's doctrine respecting fluids as non-conductors of caloric, 134, 198, 274
- Berthollet the younger, 273
- Black, 76
- Blacquiere, Col. 23.
- Bleaching of wool, experiments and observations on, 96
- Blumenbach, Prof. 102
- Boiler of stone, fact respecting water heated in a, 169
- Boiling oily matters, spontaneous inflammation from, 239
- Bones of animals, contain a phosphate not found in those of men, 85—Method of separating magnesia from, 86
- Books, account of new, 222
- Bournon, count de, 187—His reply to the Abbé Hauy's observations on arseniated copper, 247
- Bramah, Mr. I. description of his jib on a new construction, 99
- Bramley, R. R. Esq. on the dry rot in timber, 8
- Brown precipitate from platina, 120
- Brugnatelli, 142—His new method of preparing nitric ether, 143
- Bruhl, Count de, 47
- Bucket, gaining and losing of an early date, 37
- Burg, 160
- C.
- Caloric, a compound of *plus* and *minus* electricity, 71—On its nature as deduced from galvanic experiments, 88
- Cantharides, on the virtues and principles of, 76
- Carbonate of lead, analysis of 282—Singular fact in its fusion, 283
- Carlisle, A. Esq. 71—On the temperature of the sea, 131
- Cartwright, Rev. E. description and drawing of his three furrow plough, 24
- Cafanova, 235
- Caylus, Count, 233
- Chain-pump, of an early date, 37
- Chappe, 164
- Characters, exterior, the only means of separating the mineralogical species, 252
- Charcoal, durability of, 8—Emits an intense light in galvanic combustion, 73 — Spontaneous inflammation of, in a powder magazine, 241—Contains phosphorus, 242
- Charles, Cit. 80
- Mr. D. account of his machine for laying land level, 181
- Charring prevents the dry rot in timber, 6
- Chenevix, Mr. his analyses of arseniated copper, 188—248
- Chronometers, causes of irregularities in, 46—Great accuracy of the mean result from a number of, 65—Tables of the going of three, 68
- Cleansing of wool, experiments and observations on, 94
- Clock, new striking part for a, 162
- Cobalt, superior hygrometric properties of its oxide, 85
- Cock, Mr. 20
- Coke, Mr. 279
- Cole's chain pump, 37
- Collet-Descoitils on the cause of the different colours of the triple salts of platina, and on a new metallic substance in that metal, 118, 220
- Coloured glass, antique, 227
- Combustibles, absorb different quantities of oxygen from common air, 146
- Combustion, its intensity depends on the state of the oxygen, 147
- Compression of the earth, mean, 159
- Copal varnish, easy method of preparing, 142
- Copper, unnecessary in a galvanic apparatus, 1—Observations on the arseniated, 187, 247—Yields different coloured enamels according to its state of oxidation, 229—Mutual precipitations with iron, 271

Counce,

INDEX.

- Cooncer, Mr. mathematical tables constructed by, 62
 Course of a body projected from the moon to the earth, method of investigating, 22
 Crawford, 76
 Cronstedt, 207
 Cruickshank, Mr. 4
 Cubes, a very extensive table of, 62—
 Easy method of completing them, 150
 Cumming, Mr. 47
 Cuthbertson, Mr. on a distinguishing property between the galvanic and electric fluids, 97—Respecting galvanic and electrical experiments, 205

D.

- Dalby, Mr. 18, 156
 Dalton, Mr. on the supposed chemical affinity of common air, 145—Illustrations of his theory of mixed gases, 297
 Davy, Prof. 42, 147
 Decomposition of water, known to the ancients, 28
 Deflagrations, galvanic, 97
 De la Cépède, 59, 101
 Delambre, 13
 Delametherie, 213
 D'Elhuyar, 207
 De Marti, 43
 Demmenie, 142
 Denon's account of an ancient Egyptian lock, 115
 Density of the air influences combustion, 147
 Defagulier's gaining and losing bucket, 37
 Deyeux, 76
 Dieman, 82
 Dilatation produced by electricity, 81
 Disk of dispersion, 202
 Draparnaud on the movement which certain fluids receive from the contact of other fluids, 201
 Dry rot in Timber, observations and communications on, 5
 Duckitt, 182

- Du Hamel, 224
 Du Sejour, 13
 Dyckhoff, 2

E.

- Earth, the, on its figure, 12—On the course and velocity of bodies projected from the moon to, 22—Is probably an ellipsoid, 159
 — a new one, chemical examination of, 209
 Eccentricity of a place of observation, method of determining, 156
 Edridge, Mr. 278
 Eeles, 71
 Electricity, the positive and negative are not proved to be distinct principles, 71
 — Differences between it and heat, 80—
 Acts by dilating bodies, 81—Favours oxidation by diminishing the cohesion, 82—Method of comparing it with galvanism, 176—Comparison of its phenomena with magnetism, 184—New experiment in proof of its identity with galvanism, 212
 Ellipsoid, the figure of the earth is probably an, 159
 Emery, Mr. 49.
 Enamels of the ancients, memoir on the, 225—Very curious and antique one of a duck, 234—Extremely minute one of a bird, 235
 Engines, old, re-invented, 37
 E. P. on the state of science among the ancients, and on alchemical researches, 27
 Ether, fluoric, process for obtaining, 143
 — nitric, new method of preparing, 143
 Eton's account of an ancient Egyptian lock, 115
 Eudiometers, comparative examination of different, 41
 Euler, 13
 Expansion of liquids corresponds with the square of their temperatures, 149
 Experiments, galvanic, by Ritter, 176—
 Van Marum's observations on, 212

INDEX.

F.

- Fabroni, 83
- Fascinating faculty ascribed to the rattlesnake and other American serpents, memoir on 58—100
- Ferber, 230, 289
- Ferguson, Dr. A. 104
- Fermentation, spontaneous inflammation from, 236
- Fire, generation of, 90
- Fluid, electric is not necessary to the formation of snow, 76—Curious and important property which distinguishes it from the galvanic, 97
- Fluids, examination of Count Rumford's doctrine of the non-conducting properties of, 134—Are proper conductors of heat, 137, 198—Experiments on the currents in, 139—Their expansion corresponds with the square of their temperature, 149—Differ in the power of conducting heat, 198—On the movements produced in them by the contact of other fluids, 201
- elastic, the effects of their specific gravity examined, 147
- Fourcroy, on a new earthy phosphate found in the bones of animals, but not in those of men, 85—142
- Friction, spontaneous inflammation from, 218
- Fuel, attempt to reduce its consumption in steam-engines, 169
- Furnace, use of the lamp, 268

G.

- G. A. on the formation of snow, 73
- Galvanism, cannot charge a Leyden jar, 3—Outlines of Dr. Gibbs's theory of, 70—Disengages an intense light from charcoal, 72—The great energy of its chemical effects may arise from its constancy, 83—Two very marked laws in, 84—Makes silver remarkably brittle, 85—Facts and observations tending to elu-

- cidate the theory of, 171—Method of comparing it with electricity, 176—Hydrogenates and oxygenates metals in the dry way, 178—Its effects on the animal body, 179—New experiment in proof of its identity with electricity, 212
- Gas, ammoniacal, decomposition of by electricity, 82
- nitric acid, spontaneous inflammation of paper in, 98
- nitrous, objections to its use in eudiometrical examinations, 41
- phosphorated hydrogen, spontaneous inflammation of, 239
- resembling common air, obtained in preparing nitrous acid, 147
- sulphuretted hydrogen, accension of, by nitrous acid, 144—Spontaneous inflammation of, 239
- Gases, have all the same affinity for each other, 148—Receive heat very rapidly, 198—Illustrations of Mr. Dalton's theory of mixed, 297
- Gay-Lussac, 80—On the mutual precipitations of metallic oxides, 270
- Gibbes, Dr. outlines of his theory of galvanism, 70—304
- Gillet Laumont, 187
- Glass, history of the invention of, 225—Antiquity of the art of colouring, 226—Roman mosaic work of, 227—Method of painting in, practised by the ancients, 233
- Gmelin, 230—291
- Gough, Mr. J. on the solution of water in the atmosphere, and on the nature of atmospheric air, 243
- Gravity, specific, of elastic fluids, effects of, 147
- Greasy animal and vegetable matters, spontaneous inflammation from, 237
- Green, Mr. R. description and drawing of his hand-drill, 19
- Guaiacum, experiments on the action of light on, 294
- Guyton, 42

Haley,

INDEX.

H.

- Haley, Mr. on the causes of irregularities in chronometers, 46
- Hand-drill, description and drawing of a very economical, 19
- Hardy, Lieut. Col. 181
- Harman and Dearn, Mess. their apparatus for filtering water, 333
- Harrison, Mr. 47
- Hauffman, 237
- Hauy's observations on arseniated copper, 187—Count de Bournon's reply to, 247
- Hay, utility of adding salt to, 237
- Heat, differences between it and electricity, 80—Compound nature of, 90—Passes through the particles of fluids, 198—That from incombustible bodies occasions spontaneous inflammation, 220—Difference in the radiation of solar and culinary, 297
- Henckel, 232
- Henderson, Dr. on the change of air by respiration, particularly with regard to the absorption of nitrogen, 40
- Henry, Mr. W. 149—On Mr Dalton's theory of mixed gases, 297
- Herschell, Dr. 72, 214
- H. G. his method of completing the tables of squares and cubes, 150
- Hidrate of copper, arseniates of, 191
- Hydrogen, a compound of water and *minus* electricity, 71—On its nature as deduced from galvanic experiments, 88—Disappearance of a mixture of it and oxygen over water, 301
- Hydrogenation of metals has different degrees, 178
- Hippocrates, 77
- Hoffman, 74
- Hope, Dr. 42
- Horace, 108
- Hornblower, Mr. I. C. account of a fact respecting water heated in a boiler of stone, 169
- Horse-mill of an early date, 37

- Howard, Mr. 22
- Human figure, description and use of a telegraph formed by the, 164—Mechanical additions to, 167
- Humboldt, 42—Notice of his death, 72—Question respecting it, 303
- Hulton, Dr. 63
- Hydraulic machine of an early date, description and drawing of, 35
- Hygrometer of oxide of cobalt, proposed, 85

I.

- Jager, 185
- Jars, 289
- Jib, description and drawing of one on a new construction, 99
- Inflammations, spontaneous, causes of, 217
- Inventions of an early date which have been since brought forward by later inventors, 35
- Johnson, B. Esq. observations and communications respecting the dry rot in timber, 5
- Jones, Mr. W. his examination of Dr. Wollaston's experiment on periscopic spectacles, 38
- I. R. I. on a galvanic apparatus of large surface, and convertible into one or more plates, 79—On some facts in galvanism, and on other objects, 84
- Iridium, a new metal found in platina, 221
- Iron, can yield a blue enamel, 282—The bad quality of the cold-short, probably caused by the charcoal, 242—Mutual precipitation with copper, 271
- Irwine, 76

K.

- Kalm, 62, 101
- Kaftner, 236
- Klaproth, 127, 142, 187—His analyses of arseniated copper, 196—Chemical examination of the ochroites, 207—On the pastes, coloured glasses, or enamels of the ancients, 225, 47
- Klein, J. T. 114

Lakanal,

INDEX.

L.

- Lakanal, 164
 Lalande, Jerome de, on the figure of the orbits of the new planets, 222
 Lambton, Brig. Maj. 18
 Lamp, description and drawing of an improved chemical, with double concentric wicks, 266
 La Perouse, 66
 La Place, 22, 160
 Lavoisier, 172
 Lead, placed in azote gas is not fused by electricity, 82—Memoir on the oxides of, 280
 Legendre, 13, 152
 Lelievre, 187
 Levelling of land, description of a machine for, 181
 Light, intense, disengaged from charcoal by galvanism, 73—Experiments on, 214—On certain chemical effects of, 293
 Linnæus, 107, 131
 Litharge, analysis of, 291
 Lock, account of an Egyptian one of great antiquity, 115—Improvement by which it is rendered safe, 117
 Longitude, on the methods of finding it at sea, 65

M.

- Machine for clearing roads from mud, 29
 ———— early hydraulic, 35
 ———— for levelling land, 181
 Magnesia, method of separating it from the bones of animals, 86—Precipitates earths and oxides from their solutions, 274
 Magnetism, experiments on, 184
 Maitland, J. Esq. 277
 Maskelyne, Dr. 48, 160
 Mafon, 160
 Maffey, Mr. E. description of his new striking part for a clock, 162
 Mather, Cotton, 107
 Matlack, T. Esq. 111
 Mawe, Mr. 259
 Mayer, 160, 236

- Mercury is a better conductor of heat than water, 138
 Meridian, measure of an arch of, in the Myfore, 18
 Merlin's gouty chair, 37
 Metallic substance, on the existence of a new one in platina, 118—Chemical examination of it, 125
 Metals, antiquity of a belief in the transmutation of, 28—Effects of electricity on, 82—Their hydrogenation has different degrees, 178—Their oxidation in water produces an acid, 185—Two new ones, 220
 Meyer, 164
 Mineral which contains a new earth, 207
 Moon, on the course and velocity of a body projected to the earth from, 22—Inequalities in her motion, 160
 MoroZZo, 146
 Mortar, its effects on timber, 11
 Mosaic work of glass, Roman, 227
 Moxon, 36
 Mudge's remontoire, 48
 Muller, 6
 Muriates, triple of platina, experiments on the, 121
 Murray, 140, 169, 198
 Muffin-Puschkin, 122

N.

- Nairne, 81
 Natrum of the ancients, 226
 Neinnich, Dr. P. A. letter from him respecting Humboldt's death, 303
 Newton, Sir I. 12, 160, 214
 Nicholson, 71, 131, 198
 Nitrate of lead, component parts of, 283
 —Three varieties of, 288
 Nitrogen, on its absorption in respiration, 40
 Nomenclature for metallic oxides, proposed, 292

O.

- Oak-timber, causes of its decay, 7
 Observation, method of determining the eccentricity of the place of, 156

Ochroit

INDEX.

- Ochroit earth, notice of, 142—Chemical examination of, 207
- Odorant bodies, emanations of, 201
- Oil, is a worse conductor of heat, than mercury, 139—Mutual repulsion with alcohol, 203
- essential, repulsive action of, 203
- Optic nerve, effect of the galvanic influence on the, 215
- Orbits of the new planets, 222
- Orsted, Mr. 176, 184, 212, 214
- Osmium, a new metal found in platina, 221
- Oxidation, requires a disengagement of electricity, 72—Of metals in pure water, produces an acid, 185—Its influence on metallic precipitations, 274
- Oxide of cobalt, superior hygrometric properties of, 85
- Oxides, metallic, on the mutual precipitations of, 170—Neutralization of the acids by, 273—Great importance of the analysis of, 280—Proposed nomenclature for, 292
- Oxygen, a compound of water and *plus* electricity, 71—On its nature as deduced from galvanic experiments, 88—Disappearance of a mixture of it and hydrogen over water, 301
- P.
- Painting on glass, method of, practised by the ancients, 233
- Paintings, invention to multiply copies of, 236
- Palconi, Dr. 218
- Paper, spontaneous inflammation of, in nitric acid gas, 98—Yields suberic acid, 142
- Parry, Dr. 279
- Pastes of the ancients, memoir on the, 225—Resemble our enamels, 233
- Paum, 226
- Pearson, Dr. communication from him respecting a distinguishing property of the galvanic and electric fluids, 98—205
- Pendulum for regulating the striking part of a clock, 162
- Pennant, Mr. 105
- Periscopic spectacles, examination of the, 38
- Perrins, Mr. his table of the temperature of the sea compared with the atmosphere, 132
- Pfaff, 177
- Phosphate of magnesia is found in the bones of animals but not in those of men, 85
- Phosphoric rings accounted for, 64
- Phosphorus in charcoal may cause detonations, 242
- Piclet, 138, 198
- Pile of dishes, recommended for galvanic experiments, 2, 4
- Pipe, ready method of estimating the contents of, 64
- Piper, Mr. 20
- Piso, 107
- Planets, orbits of the new, 222
- Plant which occasions the dry rot, 5
- Platina, action of electricity on, 80—Cause of the difference of colour in the triple salts of, 118—Foreign admixtures in, *ib.*—Blue sublimate obtained by distilling, 119—Chemical examination of a metallic substance peculiar to, 125—Two new metals discovered in, 220
- Playfair, Prof. observations on his memoir on the figure of the earth, 12, 156
- Pliny, 225
- Plough, description and drawing of a three-furrow, 24
- Potash, examination of a stone containing, 126
- P. Q. on an ancient Egyptian Lock, 115
- Precipitates, metallic, are not pure oxides, 273
- Prevost, 201
- Priestley, Dr. 72, 147
- Problem for finding the figure of the earth, solution of, 13—In spheroidal triangles, new, 151

Proteus,

INDEX.

Proteus, Peregrinus, on the figure of the earth, 12—New problems in spheroidal triangles, 151

Proust, 273—His analyses of metallic oxides, 281

Pyrophorus, cause of its spontaneous inflammation, 240

R.

Rattle-snake, on the fascinating faculty ascribed to the, 58

Rays, invisible, which act chemically, 214—May be separated from the coloured, 216

R. B. on early inventions, 35—On phosphoric rings, 64

Red-lead, analysis of, 289

Red precipitate obtained from platina, 120

Remontoire, various opinions respecting the, 47

Repulsion, mutual, of fluids, 202

Respiration, on the change effected in the air by, 40

Richter, 71

Rings, phosphoric, accounted for, 64

Rittenhouse, Mr. 110

Ritter, Mr. his experiments with the electric pile, 176—His experiments on magnetism, 184, 212—His experiments on light, 214, 293

Roads, machine for clearing them from mud, 29

R. T. on the difficulty of obtaining alumine in a state of purity, 141

Rumford, Count, observations on his doctrine respecting fluids as non-conductors of heat, 134, 169, 198

S.

Sale of his Majesty's spare sheep, 280

Salt, common, utility of adding it to hay, 237

— of lead, Proust's, 286

Salts, triple, of platina, cause of the different colours of, 118

Scapement, detached, description of a, 50

Scheele, 90, 207, 280

Science, on its state among the earlier nations of antiquity, 27

Scientific news, 62, 142, 222

Sea, table of its temperature compared with the atmosphere, 131

Seguin, 42

Serpents, on the fascinating faculty ascribed to, 58

Serviere, Mons. Grollier de, 35

Seymour, Lord Hugh, 66

Shark, temperature of the stomach of one when taken, 132

Sheep, washing them recommended, 96

—Report of the state of his Majesty's flock of fine woolled Spanish, 177—Improvement in the carcase of the Merino, 278

Signs, telegraphic, by the human figure, 165

Silver is made brittle by galvanism, 85

Sines and tangents, table of, 62

Sloane, Sir H. 60

Snow, on its formation, 73

Soap-suds the best menstruum for cleansing wool, 95

Solar spectrum, is separable into two series of rays, the one coloured, the other chemical, 216

Somerville, Lord, 278

Species, mineralogical, can only be separated by the exterior characters, 252

Spectacles, examination of the periscopic, 38

Spheroidal triangles, new problems in, 151

Squares, very extensive table of, 62—Easy method of completing them, 150

Steam, powerful effect of its expansion, 262

Steam-engines, attempt to reduce the consumption of fuel in, 169—Improvement in the construction of, 162

Stone, containing potash, examination of a, 127—Fact respecting water heated in a boiler of, 169

Sublimate, blue, of platina, chemical examination of, 120

Sulphuret

INDEX.

- Sulphuret of lime, the best test for atmospheric air, 42
- Sulzer, 235
- Sun, spontaneous inflammation from its action, 219
- Symner, 71
- T.
- Table, of the going of three chronometers, 68
- of the relative pressures of the atmosphere, temperatures and expansibility of steam, 263
- of sines and tangents, 62
- comparative, of the temperature of the sea and atmosphere, 131
- very extensive, of squares and cubes, 63—Easy method of completing, 150
- Tangents, tables of sines and, 62
- Telegraph, formed of the human figure, 164—Mechanical additions to, 167
- Temperature of the atmosphere and sea, comparative table of, 131
- Tennant, Smithson, Esq. discovery of two new metals by, 220
- Thompson, Dr. 45, 139, 145, 198—On the oxides of lead, 280
- Thouvenel, 77
- Time-pieces, of an early date, 36
- Tollet, Mr. 278
- Torrefaction of vegetables increases their tendency to spontaneous inflammation, 238
- Townley, Mr. 235
- Transmutation of metals, antiquity of a belief in the, 28
- Triangles, spheroidal, new problems in, 151
- Tromsdorff, 127
- Trough, galvanic, improvement in, 79
- T. S. T on the disappearance of a mixture of oxygen and hydrogen over water, 301
- Turnery of an early date, 36
- V.
- Van Marum, 177—On Ritter's galvanic experiments, 212
- Vapour, a mephitic probably emitted by some animals, 60
- Varnish, easy method of preparing copal, 142
- Vauquelin, on a new earthy phosphate found in the bones of animals, but not in those of men, 85—On the yolk of wool, and on the cleansing and bleaching of wool, 90—His analysis of arseniate of copper, 195, 221, 248, 288
- Velocity of a body projected from the moon to the earth, methods of investigating, 22
- Vegetables, dry, attract water, 243
- Virgil, 108
- Volta, 172, 177, 203
- Vofmaer, Mr. 58, 103
- U.
- Urine, its action in scouring wool, 95
- W.
- Wales, Mr. 66
- Walker, Mr. A. his horse-mill, 37
- Mr. E. on the methods of observing the longitude at sea, and on the accuracy of the mean result from a number of chronometers, 65
- Water, its decomposition known to the ancients, 28—Its action on the yolk of wool, 90—Does not cleanse wool alone, 94—Apparatus for filtering, 126—Does not dissolve air, 149—Heated in a boiler of stone, fact respecting, 169—Action of alcohol on, 201—Is repelled by essential oils, 203—Its action on dry vegetables, 243—Is attracted by dry atmospheric air, 245—Best method of impregnating it with a gas, 300—Slow absorption of a mixture of oxygen and hydrogen over, 301
- Watson, Dr. 289, 291
- Watt, 72, 262
- Webster, Mr. 268
- Weston, Mr. I. 169
- Wilkinson, C. Esq. on the means of simplifying and improving the galvanic apparatus,

INDEX.

- paratus, 1—On galvanism, 70, 172, 206
- Wind, its influence on the formation of snow, 75
- Winkelmann, 233
- Winterbottom, Dr. description of his machine for clearing roads of mud, 29
- Wire, comparative deflagration of, by electricity and galvanism, 205
- W. N. on the arrangement of different galvanic apparatus, 3—On an Egyptian lock of very high antiquity, 115
- Wollaston, Dr. examination of his periscope spectacles, 38—On certain chemical effects of light, 293
- Wood-copper, a species of arseniated copper, 258
- Wool, considerations on the cleansing and bleaching of, 90—Loss of weight by scouring, 96—Improvement in, 278
- Woolf, Mr. his method of measuring the contents of a pipe, 64—Account of his improvement in the construction of steam-engines, 262

Y.

- Yellow precipitate obtained from platina, 120
- Yolk of wool, experiments in the, 90—Component parts of, 93—Its principal source is the humour of the transpiration, 94—Its acrimony probably hurtful to the living animal, 96



THE END OF THE EIGHTH VOLUME.





