Plate 6 never published
See Memo in Vol. I.

Vol. 46. July 1815. No. 207.

Published the Last Day of every Month,
[PRICE 2s. 6d.]

THE PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CCVII.

For JULY 1815.

CONTAINING THE FOLLOWING ENGRAVINGS:

1. Mr. T. Jones's New Reflecting Compass.
2. Mr. Woolf's Patent Boiler for Steam Engines and other Purposes.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by Cadell and Davies; Longman, Hurst, Rees, Orme, & Brown; Murray; Highley; Sherwood and Co.; Harding; Underwood; London: Constable and Co. Edinburgh: Brash and Reid, and D. Niven, Glasgow: and Gilbert and Hodges, Dublin.

Vol. 46. August 1815. No. 208.

Published the Last Day of every Month,
[PRICE 2s. 6d.]

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CCVIII.

For AUGUST 1815.

CONTAINING THE FOLLOWING ENGRAVING BY PORTER:

A Plate to illustrate Mr. Woolf's Improvement on the Steam-Engine, by which the possibility of Steam escaping past the side of the Piston is effectually prevented.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by Cadell and Davies; Longman, Hurst, Rees, Orme, & Brown; Murray; Highley; Sherwood and Co.; Harding; Underwood; London: Constable and Co. Edinburgh: Brash and Reid, and D. Niven, Glasgow: and Gilbert and Hodges, Dublin.

VOL. 46. SEPTEMBER 1815. No. 209.

Published the Last Day of every Month,
[PRICE 2s. 6d.]

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CCIX.

For SEPTEMBER 1815.

CONTAINING THE FOLLOWING ENGRAVING BY PORTER:
A Plate to illustrate some Electrical Experiments by M. DE NELIS; with an Extension of them by Mr. SINGER and Mr. CROSSE.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by CADELL and DAVIES; LONGMAN, HURST, REES, ORME, & BROWN; MURRAY; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD; London: CONSTABLE and Co. Edinburgh: BRASH and REID, and D. NIVEN, Glasgow; and GILBERT and HODGES, Dublin.

Mr. CHARLES SYLVESTER, of Derby, will publish, early in 1816,

AN ACCOUNT of some IMPROVEMENTS in DOMESTIC ŒCONOMY, adopted at the DERBYSHIRE GENERAL INFIRMARY, more especially relating to the Means of Warming and Ventilating the Apartments: with a Description of the Culinary Department, the Wash-House, Laundry, &c. and Observations on their Application to the Comfort and Convenience of Private Families, and other important Purposes.

The Work will be in Quarto, illustrated by Ten Plates, exhibiting Plans of the Infirmary, and Working Drawings of the different Apparatus.

*** The Price to Subscribers will not exceed *1l. 11s. 6d.*

☞ Subscriptions received by Mr. Sylvester, at Derby; and by Longman, Hurst, Rees, Orme, and Brown, Paternoster-Row, London.

ENGRAVINGS.

Vol. XXXVIII. Mr. DONKIN's Tachometer.—Mr. ALLAN's Mathematical Dividing Engine.—A 4to Plate of Mr. LOESCHMAN's Patent Piano-Forte, and Mr. LISTON's Patent Euharmonic Organ.—A Plate to illustrate Mr. HAUY's Paper on the Electricity of Minerals.—Mr. WALKER's improved Micrometer.—Mr. MOULT's Filtering Apparatus.—Mr. SMITH's Method of relieving a Horse fallen in the Shafts of a loaded Cart.—Mr. JOHN TAYLOR's Air-Exhauster for Mines.—A Representation of the Comet now visible in Ursa Major.—Sir HOWARD DOUGLAS's Patent Reflecting Semicircle.—A coloured Map to illustrate Dr. CAMPBELL's Paper on the inferior Strata of the Earth occurring in Lancashire.—Mr. SADLER's Apparatus for Smelting of Lead.—A Plate to illustrate Mr. WILLIAM SMITH's and Mr. EDWARD MARTIN's Reports on the State of the Collieries at and near Nailsea, in Somersetshire.—Mr. SADLER's Apparatus for Smelting of Lead.—A Plate to illustrate the different Theories of Arches or Vaults, and of Domes.

Vol. XXXIX. Mr. STEEVENS's New Levelling-Staff.—A Map illustrating the Great Derbyshire Denudation.—A Quarto Plate to illustrate the Optical Instrument called the Diacatoptron.—A Plate of Mr. STEEVENS's Contrivance for freeing Water-pipes from Air: engraved by Mr. PORTER.—Mr. ALLAN's improved Reflecting Circle.—Mr. WOOD's inclosed Grindstone for Pointing Needles, and Mr. WEBSTER's Method of carrying off Steam from Boilers.—Mr. BRUNTON's improved Pump for raising Water while Shafts or Pits are sinking.—Mr. HODGSON's improved Mariner's Compass for correcting the Magnetic Variations.—A Quarto coloured Plate to illustrate the Geology of Part of the Vicinity of Dublin.

Vol. XL. A Plate to illustrate the Foundation of Mr. WILLIAM JONES's temporary Corn Rick.—Mr. STEPHENS's Method of dividing Bricks.—Section of Mr. WILLIAM JONES's temporary Corn Rick.—Mr. WAISTELL's Improvement on the Acorn Dibble.—A Plate to illustrate Mr. TAWNEY's New Thrashing Machine.—Two Octavo Plates descriptive of a New Machine for Pumping Water.—Two Plates descriptive of the Muscular Motion of Snakes.—Figures to illustrate such Portions of a Sphere as have their Attraction expressed by an algebraic Quantity.—VAN HELMONT's Differential Thermometer.—Formation of Ovals for Gardeners.—CHAVASSE's Marine Transit.—A Plate to illustrate Mr. BRODIE's Experiments on the Influence of the Brain on the Generation of Animal Heat.

VOL. 46. OCTOBER 1815. No. 210.

Published the Last Day of every Month,
[PRICE 2s. 6d.]

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CCX.

For OCTOBER 1815.

CONTAINING THE FOLLOWING ENGRAVINGS BY PORTER:

1. A Plate to illustrate the Electrical Experiments of M. DE NELIS of Malines.
2. WOOLF's improved Steam-Engine.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by CADELL and DAVIES; LONGMAN, HURST, REES, ORME, & BROWN; MURRAY; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD: London: CONSTABLE and Co. Edinburgh: BRASH and REID, and D. NIVEN, Glasgow: and GILBERT and HODGES, Dublin.

ENGRAVINGS.

Vol. XL. A Plate to illustrate the Foundation of Mr. William Jones's temporary Corn Rick.—Mr. Stephens's Method of dividing Bricks.—Section of Mr. William Jones's temporary Corn Rick.—Mr. Waistell's Improvement on the Acorn Dibble.—A Plate to illustrate Mr. Tawney's New Thrashing Machine.—Two Octavo Plates descriptive of a New Machine for Pumping Water.—Two Plates descriptive of the Muscular Motion of Snakes.—Figures to illustrate such Portions of a Sphere as have their Attraction expressed by an algebraic Quantity.—Van Helmont's Differential Thermometer.—Formation of Ovals for Gardeners.—Chavasse's Marine Transit.—A Plate to illustrate Mr. Brodie's Experiments on the Influence of the Brain on the Generation of Animal Heat.

Vol. XLI. Pepys's Mercurial Voltaic Conductor.—Leslie's Differential Thermometer. Diagrams to illustrate Dr. Wollaston's Periscopic Camera Obscura and Microscope.—Count Rumford's improved Coffee-pots.—Mr. Goss's Instrument to work Addition of Numbers with Accuracy and Dispatch.—Mr. Davis's hanging Scaffold; and Mr. Martin's Method of relieving a Horse from a Cart when fallen down in its Shafts.— A Quarto Plate to illustrate Mr. J. Hicks's Account of the Situation and Means of raising the Royal George.—A Plate to illustrate Mr. Southern's Essay on the Equilibrium of a Combination of Beams, Blocks, &c. and on the Polygon of Forces.—A Plate to illustrate Mr. Steevens's Account of a Meteor seen at London and other Places in March last.—A Plate to illustrate Mr. Machell's Annular Saw.

VOL. 46. NOVEMBER 1815. No. 211.

Published the Last Day of every Month,
[PRICE 2s. 6d.]

THE PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CCXI.

For NOVEMBER 1815.

CONTAINING THE FOLLOWING ENGRAVINGS BY PORTER:

1. A Plate to illustrate Mr. EVANS's Paper on guiding Balloons through the Atmosphere.
2. A Plate illustrative of Dr. BREWSTER's Paper on Light.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

LONDON:

PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by CADELL and DAVIES; LONGMAN, HURST, REES, ORME, & BROWN; MURRAY; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD; London: CONSTABLE and Co. Edinburgh: BRASH and REID, and D. NIVEN, Glasgow: and GILBERT and HODGES, Dublin.

ENCYCLOPÆDIA BRITANNICA.

On Monday the 4th of December will be published, handsomely printed in Quarto, Price 1*l.* 5*s.* in boards, Volume First, Part First, of the

SUPPLEMENT to the ENCYCLOPÆDIA BRITANNICA; with a Preliminary Dissertation, exhibiting a general View of the Progress of *Metaphysical, Ethical,* and *Political Philosophy*, since the Revival of Letters in Europe.

By DUGALD STEWART, Esq. F.R.S. Lond. and Edin.

To the Second Volume there will be prefixed a similar View of the Progress of MATHEMATICAL and PHYSICAL SCIENCE by Professor PLAYFAIR; and the Publishers beg leave generally to mention, that the Work is arranged upon a plan calculated not only to complete the Encyclopædia Britannica in all its departments, but to afford within *itself* a comprehensive view of the progress and present state of the Arts and Sciences, and of General Literature. The List of the Contributors has been long before the public, and contains many of the most eminent literary names that this country can boast. The present Half Volume is illustrated with Fifteen Plates, engraved (from original Drawings) by the first Artists of London and Edinburgh.

Printed for Archibald Constable and Company, Edinburgh: Sold by Gale and Fenner, London; Thos. Wilson and Sons, York; Robinson sen. and Holdsworth, Leeds; John Cumming, Dublin; and to be had of every Bookseller throughout the United Kingdom. Of whom may be had, the ENCYCLOPÆDIA BRITANNICA, 20 Volumes, Fifth Edition, price 36*l.* boards.

ENGRAVINGS.

Vol. XL. A Plate to illustrate the Foundation of Mr. WILLIAM JONES's temporary Corn Rick.—Mr. STEPHENS's Method of dividing Bricks.—Section of Mr. WILLIAM JONES's temporary Corn Rick.—Mr. WAISTELL's Improvement on the Acorn Dibble.— A Plate to illustrate Mr. TAWNEY's New Thrashing Machine.—Two Octavo Plates descriptive of a New Machine for Pumping Water.—Two Plates descriptive of the Muscular Motion of Snakes.—Figures to illustrate such Portions of a Sphere as have their Attraction expressed by an algebraic Quantity.—VAN HELMONT's Differential Thermometer.—Formation of Ovals for Gardeners.—CHAVASSE's Marine Transit.—A Plate to illustrate Mr. BRODIE's Experiments on the Influence of the Brain on the Generation of Animal Heat.

Vol. XLI. PEPYS's Mercurial-Voltaic Conductor.—LESLIE's Differential Thermometer. Diagrams to illustrate Dr. WOLLASTON's Periscopic Camera Obscura and Microscope.—Count RUMFORD's improved Coffee-pots.—Mr. GOSS's Instrument to work Addition of Numbers with Accuracy and Dispatch.—Mr. DAVIS's hanging Scaffold; and Mr. MARTIN's Method of relieving a Horse from a Cart when fallen down in its Shafts.— A Quarto Plate to illustrate Mr. J. HICKS's Account of the Situation and Means of raising the Royal George.—A Plate to illustrate Mr. SOUTHERN's Essay on the Equilibrium of a Combination of Beams, Blocks, &c. and on the Polygon of Forces.—A Plate to illustrate Mr. STEEVENS's Account of a Meteor seen at London and other Places in March last.—A Plate to illustrate Mr. MACHELL's Annular Saw.

Published the Last Day of every Month,
[PRICE 2s. 6d.]

THE PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
GEOLOGY,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CCXII.

For DECEMBER 1815.

CONTAINING THE FOLLOWING ENGRAVING BY PORTER:

A 4to Plate to illustrate Sir H. DAVY's Paper on the Fire-damp of Coal-Mines, and on Methods of lighting the Mines so as to prevent its Explosion; and the Account given in the present Number of Mr. STEPHENSON's Safe-Lamp for Coal-Mines.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by CADELL and DAVIES; LONGMAN, HURST, REES, ORME, & BROWN; MURRAY; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD; London: CONSTABLE and Co. Edinburgh: BRASH and REID, and D. NIVEN, Glasgow: and GILBERT and HODGES, Dublin.

Vol XXXVII. Plates 1 and 2, Representations of Luminou
Illustrate Mr. Macartney's Paper on that Subject.—Quarto Plat
of the newly discovered Planets.—A Quarto Plate to illustrate
Paper on Burning Mirrors.—A Plate to illustrate Mr. Donc
on Electro-chemical Agency —Mr. ACCUM's New Mineralogica
—Mr. LEE's Thrashing Machine.—Mr. COOK's Apparatus for
and other Products from Pit-Coal; and Mr. WAY's Method
Turpentine from Fir-Trees.—A Plate to illustrate Dr. BREWS
on the Power of the Lever.

Vol. XXXVIII. Mr. DONKIN's Tachometer.—Mr. ALLAN's M
Dividing Engine.—A 4to Plate of Mr. LOESCHMAN's Patent I
and Mr. LISTON's Patent Euharmonic Organ.—A Plate to ill
HAUY's Paper on the Electricity of Minerals.—Mr. WALKER
Micrometer.—Mr. MOULT's Filtering Apparatus.—Mr. SMITH'
relieving a Horse fallen in the Shafts of a loaded Cart.—Mr. JOHN
Air-Exhauster for Mines.—A Representation of the Comet now
Ursa Major.—Sir HOWARD DOUGLAS's Patent Reflecting S
A coloured Map to illustrate Dr. CAMPBELL's Paper on the in
of the Earth occurring in Lancashire.—Mr. SADLER's Ap
Smelting of Lead.—A Plate to illustrate Mr. WILLIAM SMITH
EDWARD MARTIN's Reports on the State of the Collieries at and
sea, in Somersetshire.—Mr. SADLER's Apparatus for Smelting
A Plate to illustrate the different Theories of Arches or Vaults, and

Vol. XXXIX. Mr. STEEVENS's New Levelling-Staff.—A Map
the Great Derbyshire Denudation.—A Quarto Plate to illustrat
cal Instrument called the Diacatoptron.—A Plate of Mr. S
Contrivance for freeing Water-pipes from Air: engraved by Mr.
Mr. ALLAN's improved Reflecting Circle.—Mr. WOOD's inclo
stone for Pointing Needles, and Mr. WEBSTER's Method of
Steam from Boilers.—Mr. BRUNTON's improved Pump for rai
while Shafts or Pits are sinking.—Mr. HODGSON's improved
Compass for correcting the Magnetic Variations.—A Quarto col
to illustrate the Geology of Part of the Vicinity of Dublin.

Vol. XL. A Plate to illustrate the Foundation of Mr. WILLI
temporary Corn Rick.—Mr. STEPHENS's Method of dividing
Section of Mr. WILLIAM JONES's temporary Corn Rick.—Mr.
Improvement on the Acorn Dibble.—A Plate to illustrate Mr.
New Thrashing Machine.—Two Octavo Plates descriptive of
chine for Pumping Water.—Two Plates descriptive of the M
tion of Snakes.—Figures to illustrate such Portions of a Sph
their Attraction expressed by an algebraic Quantity.—VAN
Differential Thermometer.—Formation of Ovals for Gardeners.—
Marine Transit.—A Plate to illustrate Mr. BRODIE's Expe
the Influence of the Brain on the Generation of Animal Hea

Vol. XLI. PEPYS's Mercurial Voltaic Conductor.—LES
ferential Thermometer. Diagrams to illustrate Dr. WOLLAS
scopic Camera Obscura and Microscope.—Count RUMFORD'
Coffee-pots.—Mr. GOSS's Instrument to work Addition o
with Accuracy and Dispatch.—Mr. DAVIS's hanging Sea
Mr. MARTIN's Method of relieving a Horse from a Cart v
down in its Shafts.—A Quarto Plate to illustrate Mr. J. H
count of the Situation and Means of raising the Royal George
to illustrate Mr. SOUTHERN's Essay on the Equilibrium of a G
of Beams, Blocks, &c. and on the Polygon of Forces.—A Plate
Mr. STEEVENS's Account of a Meteor seen at London and oth
March last.—A Plate to illustrate Mr. MACHELL's Annular S

THE

PHILOSOPHICAL MAGAZINE

AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XLVI.

For JULY, AUGUST, SEPTEMBER, OCTOBER, NOVEMBER, and DECEMBER, 1815.

LONDON:

PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:

And sold by CADELL and DAVIES; LONGMAN, HURST REES, ORME, and BROWN; MURRAY; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD, London: CONSTABLE and Co. Edinburgh; BRASH and REID, and NIVEN, Glasgow: & GILBERT & HODGES, Dublin.

CONTENTS

OF THE FORTY-SIXTH VOLUME.

CONTENTS.

CONTENTS.

CONTENTS.

THE

THE

PHILOSOPHICAL MAGAZINE

AND JOURNAL.

I. *Facts and Observations relating to the Connexion between vascular and extra-vascular Substances in living organized Bodies. By* ANTHONY CARLISLE, *Surgeon Extraordinary to His Royal Highness the Prince Regent, and His Royal Highness the Duke of Gloucester, F.R.S. F.A.S. F.L.S. &c. &c.*

To Mr. Tilloch.

SIR, — THE following memoir having been partially made known to the public, I beg you to lay it before your scientific readers, as a means of preventing misrepresentation or piracy.

I am, sir,

Your most obedient servant,

Soho Square, July 4, 1815. ANTHONY CARLISLE.

General or comparative anatomy, the great branch of natural knowledge on which the rationale of medical art is founded, has lately risen in esteem, and is every day more accurately and more extensively cultivated. Considering how intimately the discoveries of new facts, their relation to each other, and the physiological inferences to be drawn from them, are connected with the previous establishment of definite views, of clear intelligible terms, and of strict physical methods; and feeling the importance of the present subject, I hasten to submit this memoir to competent judges. I am aware that premature generalization of facts as well as premature inductions from them, are seldom useful; and I should not have troubled the scientific inquirer with this communication had I not felt assured that the present state both of anatomy and physiology would authorise it. In my statements I shall purposely avoid all metaphysical pretension to dive into the hidden mystery of vitality, confessing myself wholly incompetent to reduce that power within the rules of physical science: a power which appears to my judgement as allied to the nature of an inscrutable First Cause or an emanation from it.

The vast variety in substances, textures, bulk, and combinations, which the living animal and vegetable kingdoms exhibit, render it difficult to define the essential residence of life as connected with any of the modes of organic structure. Some of the compounds and textures of animals are known to be more important for the maintenance of life than others; as the cerebral substance and the muscular textures: but there is a numerous tribe of living bodies that appear to be wholly destitute of those peculiar parts; of which the entire vegetable kingdom may be adduced as an instance. Habits of meditation and research have led me to conclude, that some benefit may arise to physiology from more accurate discriminations between the several substances of living bodies, especially as to the relative dominion of vitality or physical causes on those substances respectively.

The active phænomena of life appear to be generally distinct from those of physical causation, but the passive condition of living substances is not so obvious. The suspended action of torpid animals and vegetables, and the latent vitality of many of the more simply constructed animals and vegetables during the absence of heat and moisture, show the intimate connexions which subsist between vitality and physical causes. Difficult and intricate as these investigations may seem when extended to all the cases of vital phænomena, they are not so in the grosser examples to be now adduced; and if it should be found that many substances distinctly continuous with vital organic bodies are wholly subjected to physical dominion, and that several other substances are in part influenced by the one cause and by the other, it may perhaps open new and more precise views in the medical art. Those parts of organic bodies which have no power of self-repair, which hold no continuity with the circulating fluid material destined to replenish the waste, to augment the bulk, or repair the accidents of the living fabric, may be justly deemed extra-vital. The exuvial defences or coverings of animals are of this kind; namely, hairs, nails, feathers, and all cuticular structures, as well as the epidermoid coverings and husks of the vegetable kingdom: some of those substances which are destined to be worn away, retain a partial continuity with the organic system of circulating fluids, as the growing bulbs of hairs, the roots and laminæ of nails and hoofs; whilst other parts which are destined to be shed, as feathers and cuticular scales, are wholly detached from the vascular communion after their complete

complete formation, and only adhere mechanically to the living parts for a time.

The most apposite illustrations and the most positive instances of union between vital and extra-vital parts are to be found in the testaceous tribe of animals. After a long continued and careful investigation, I am fully convinced that the shells of all the *Vermes* of Linnæus are extra-vascular from their commencement, and remain so during the whole of their connexion with the living creature. The first production and the growth of those shells always depend upon a deposit of material thrown out from the surface of the body of the living animal. The figure and colour of the several parts of those shells in every species depend upon the shape and the colouring glands of the modelling organs. Fractures are repaired by spreading a cretaceous fluid over the inner edges, and never by any exudation from the fractured parts, since they retain always the squared broken surfaces after such repairs. Extraneous bodies are equally covered with shell, whether they are in contact with the parent shell or not. The first may be seen in the frequent envelopment of *Nereides* in the common oyster; the latter has been often ascertained by experiments made for the purpose of creating artificial pearls, and which might, if skilfully practised, yet prove very successful. The borings of parasitical vermes into shells are never filled up, or the bored surfaces altered, unless such borings penetrate into the cavity where the living animal dwells, and then the apertures are universally plugged up or smeared over with pearly matter. The water-worn external surfaces of old shells and other external abrasions are never repaired, which is to be seen in old living oysters exposed to the moving friction of currents or strong tides, in the worn-off spines of the *Pholas Dactylus* and in the convex points of the two valves of old *Mytili*; especially the *Mytilus anatinus*. I have sought in the most extensive collections of the metropolis for examples of fractures, and other injuries which have occurred to the shells of living vermes, and I have collected many remarkable specimens: they all demonstrate the same results without any exception. I have made numerous experiments upon the garden snail *Helix nemoralis*, by fracturing and breaking away the shell in various parts, and have always found the repairs to be effected from within and by smearing over an epidermoid varnish, and then by plastering the inner surface of that film with successive calcareous laminæ. I have in vain attempted to inject the shells of recent vermes from the vascular parts of their bodies, and am fully satisfied that none of their albuminous or gelatinous testaceous membranes were ever at any time traversed by vessels: indeed they do not possess any of the reticular texture or ar-

borescent

borescent pores which are common to all vascular parts, but microscopically examined they resemble the exuvial or epidermoid membranes. To these may be added, the notorious circumstance of the unchangeableness of the outer surfaces of testaceous shells during their growth, and the continued renewal of their other surfaces which admit of contact with the living inhabitant; next, the stains and coloured transudations which they often derive from metallic salts, and other colouring materials placed in their vicinity; and lastly, that such occurrences do not affect the living animal. The mechanical connexion or contact that subsists between the living animals which occupy the testaceous shells and their extraneous dwellings is in many instances very slender. The common oyster possesses its first pair of valves, consisting of single laminæ, before it quits the parental organs; a muscle passes between the centres of the concavity of each shell adhering to each, and it acts upon the valves nearly at right angles. The animal has no other continuity with the shell: at the hinge an elastic substance is wedged in, the spring of which is excited by compression; but it does not possess the property of extension beyond the passive state: when dried this substance cracks into cubes. As the animal grows it augments the margin of its shells, and thickens them by adding new laminæ on their insides; the muscular adhesion glides forward, still keeping the centre of the valves. The elastic substance at the hinge is augmented along the inner surfaces only, and must have been always deposited during the expanded state of the valves, since the limits of its elastic condition is exactly adapted to that state. As the laminæ of the shells increase, there is a gap at the outside of the hinge, filled with soft crumbling and decomposing worn-out elastic ligament. This gap presents two inclined planes which meet in an acute angle; and that space is kept free from pebbles and hard extraneous bodies by the presence of the decomposing ligament, as such an accident would prove fatal, by preventing the opening of the valves. The growth of all testaceous shells affords remarkable proofs of their extravascular formation. The muscular adhesions are generally the only points of continuity between the animal and its shell, and these are constantly changing with the augmentations of bulk. In all the conoid univalves which revolve upon spiral axes, the successive parts of the shell are merely spread upon the older parts without any intermixture of their substances, and the epidermoid or extraneous bodies are alike involved in the successive folds. In the other classes of animals similar phænomena occur. The calcareous shells of birds' eggs are merely deposited upon the *membrana putaminis*, and the inner portions are regularly crystallized prisms, the long diameters of which point to the

the centre of the egg. These shells are wholly extra-vascular, and their albuminous membranes are alike cuticular, whilst the inner true *membrana putaminis* is made reticular and capable of vascular organization. The order of deposit in these examples is like that of enamel in teeth, which appears to be precipitated upon the bone of the tooth under the guidance of a membranous case or mould. From a disordered fowl, I have seen eggs produced the calcareous crusts of which were inflated with bubbles, so as to form a cancellated shell in texture like pumice-stone. The most durable substances of animal bodies, such as the bones and teeth, are only partly vascular, since their calcareous materials are fixed by chemical precipitants, and remain under chemical laws. Injuries done to the horns of cattle, to the hoofs of animals, and to human nails, are never restored; those parts do not possess the power of self-repair, and it is only by mechanical wearing away that such injuries are obliterated. Indeed, the beneficent construction of animal nature is sufficiently manifested in the insensibility of all the exuvial coverings, and in the inorganic composition of many parts which are exposed to mechanical attrition; as the enamel of teeth, the horny beaks of birds, and the cuticular or horny coverings of feet. The same beneficence appears to be extended to many parts of the external organic substances by which painful sensations are obviated; while the substances themselves, being left directly under the dominion of the vital superintendancy, become more permanent:—such parts are the tendous ligaments, cartilages, cellular tissue, the gelatine and lime of bones. Even water is an essential constituent of the animal fluids, and affords the necessary softness and flexibility to the solids.—But this subject and its connexion with the vegetable composition and texture extends far beyond the limits of a memoir, and I must therefore suspend my observations.

[To be continued.]

II. *Description of a new Reflecting Compass.* By Mr. T. Jones, 62, *Charing Cross, London.*

Among the various instruments used for surveying, the compass appears to have been least considered; and this has probably arisen from its want of portability when furnished with a stand, and the conviction that under the most favourable circumstances it is far inferior to the theodolite; and yet requires equal care in its adjustment and use.

But in military sketching, in the delineation of roads, rivers, &c. and in all surveys where rapidity is more an object than *extreme* accuracy, the compass will be found a most valuable auxiliary, as it possesses the advantage peculiar to itself, of determining the point of station by the bearings of two objects only, the positions of which are known; and may even in certain confined circumstances be employed in determining the station of the observer by the bearing and estimated distance of one known object.

These considerations led to the construction of the instrument which I am about to describe. It was invented in 1811 by Captain Kater, F.R.S.: and on a survey made with it the same year, was found capable of a degree of accuracy beyond any expectation that had been previously formed of it.

The box containing the compass is about 2½ inches diameter, but may be made of any size that may be thought convenient. Its depth is such as to allow no more space than is sufficient for the card to float freely. The card is very accurately divided into 360 degrees from the *north* towards the *east*, and every tenth degree is distinctly numbered. The needle, which is rendered strongly magnetical, is fixed beneath the card with its north point at the 180th degree. It has an agate cap, and is made as light as is consistent with that form best calculated for retaining the magnetic virtue.

Fig. 1. Plate I. represents the instrument; A is the sight next the eye. It slides rather stiffly in a dovetail on the outside of the box, and is prevented by a shoulder from going beyond the bottom. Very near the top of this sight on the inside, and projecting over the edge of the compass-card, is fixed a small plane silvered glass or mirror B, at an angle of about 40°. The mirror is of the same breadth as the sight; and when the sight is pressed home to the shoulder, the lower edge of the mirror nearly touches the glass of the compass. Between the glass of the compass and the upper part of the mirror there is sufficient room for an opening which contains the segment of a convex lens of about three-fourths of an inch focus. In a line perpendicular to the centre of the lens, and immediately above the upper edge of the mirror a small hole is drilled, through which is seen the object the bearing of which is to be determined. On the opposite part of the box is an open sight with a vertical hair or wire: this sight is of such a length as to admit of its being folded down on the glass of the compass, and a contrivance is added, by which the needle is thrown off the point at the same time, thus securing it from injury when the instrument is not in use.

To use the Instrument.—Raise the sight, when the compass-card will vibrate freely; look through the small hole above the lens, and cover the object with the thread of the opposite sight; check the vibration of the compass-card by inclining the box, keeping

keeping at the same time the object steadily covered by the hair; and when the card is stationary, the degree indicating the bearing (which is seen by reflection, and considerably magnified through the lens), may be read off to fifteen minutes by means of the hair of the sight coming in contact with the reflected image of the divisions of the card.

Should not the divisions on the card appear distinct, the sight next the eye is to be drawn out till they are perfectly well defined. It is adviseable to tap the box gently with the finger, to lessen any friction which might prevent the needle from settling in the true direction.

In addition to all the usual purposes of surveying to which this instrument is applicable, it may be employed with very superior advantage as an azimuth compass, or to take bearings from a boat at sea where the motion might wholly prevent the use of a compass of any other form; and on such occasions it will be found far more accurate than the largest azimuth compass of the common construction, and its use unattended with those difficulties which frequently give rise to erroneous results.

It would be unnecessary to point out all the various uses to which this instrument may be applied; but I may briefly observe, that as an azimuth compass it stands unrivalled, and that in land-surveying, and in the construction of maps of a country, it will be found equal in accuracy to a large circumferentor, though sufficiently portable to be carried in the waistcoat-pocket.

III. *Of certain Causes which render more difficult the Discernment of the Character of the Mind from the Examination of the Organs of the Brain.* By Thomas Forster, *Esq.*

To Mr. Tilloch.

Sir,—In pursuing the anatomy and physiology of the brain and nervous system from the time of its first promulgation in England, I have been daily more and more convinced of the truth of the doctrine maintained by Gall and Spurzheim. With the evidence now before me of their correctness, it would be quite impossible that any arguments drawn from theory should set aside a systematic arrangement of facts and a theory deduced therefrom, in the support of which I could now advert my mind to such a numerous body of evidence. I have, however, in the course of my observations, noticed several causes why the apparent indication of the organs were found in a degree fallacious; and why we did not always find the mind possessed of energies commensurate to the expectation we had formed from an an examination of the size of the organs. I shall endeavour to point out a few of these, in order to facilitate the inquiries of those who are engaged in similar pursuits, by adverting to the causes of some apparent exceptions to the rules.

It has been already sufficiently explained in all the new books on the brain, that the character results not from the size of the organs alone, but from their proportion to each other, from established mutual influences, from the temperament of the individual, and from something peculiar in the internal activity of the organs. The two latter circumstances seem immediately connected with each other. I am at present disposed to attribute more to the particular activity of the organs than I was when first I learnt from Spurzheim's labours the very curious facts which support his doctrines. I have since seen cases in which very large organs, even in those whose education was good, and were very inactive from this habitual and constitutional ill-health of the subject. Besides the difference in the visible structure of the fibres which compose the organs of the brain in different persons, it seems that the constitutional differences of individuals, whether they are of such a permanent and innate nature as are called temperaments, or whether they assume the form of acknowledged diseases, or of constitutional tendencies to diseases,—as for instance, in scrofulous or consumptive habit,—that this peculiar diathesis or temperament must affect the functions of the brain, as well as other parts of the animal system. To differences in the actual condition of the nervous system, either original or acquired, we must attribute the constitutional tendencies to particular forms of disease, which external influences may modify in the production of various disorders. To me it appears that the state of the blood and fluids in general can only be secondary in the catenation of causes which produce changes in the system. If upon good or bad chylification depend the qualities of the blood; upon nervous influence must depend chylification. If blood by determination to particular parts produce disease, to nervous influence must we ascribe such action of the sanguiferous vessels. From whatever combination of external causes constitutional bad health and peculiarity of temperament depend, the nervous powers are the agents, in them consists essentially the disease. The nervous parts of the brain would suffer among the rest, and thus the activity of the organ would be diminished when its size remained the same. This circumstance, combined with the particular activity into which organs may be called by education, constitutes one of the chief causes why mental activity does not more regularly and uniformly correspond to the physical development of the organs of the faculties. I have endeavoured to ascertain by

extensive

extensive observation of individuals, whether particular organization of the brain were exempt from any of the calamities of constitutional diatheses; but have found no relation between the two circumstances. I have seen heads of various kinds in persons of all the various temperaments and constitutional diseases, and the faculties of their minds seemed modified by these circumstances of constitution. Sometimes, indeed, there are exceptions to this rule, wherein we find extraordinary power of mind in persons in other respects unhealthy, and whose bodies have been enfeebled and almost destroyed by disease. It will be adviseable in future to examine minutely the fibres which compose the nervous parts of the brain in persons of different kinds of constitution, to ascertain how far the demonstrable constitution of nervous parts can be shown to agree with circumstances of constitutional character. Whatever discoveries I should be fortunate enough to make in these investigations, I shall communicate in the Philosophical Magazine; and shall proceed in some future Number to point out some other difficulties we experience in judging of the activity of particular parts of the head.

Yours, &c.

Clapton, July 4, 1815. THOMAS FORSTER.

IV. *Correction of some Errors in Mr.* SINGER's *Paper on the Mechanical Applications of the Electric Column. In a Letter from the Author.*

To Mr. Tilloch.

DEAR SIR,—IN the paper on the mechanical applications of the electric column which you have lately published for me, there is another error besides that you have noticed in your last erratum. The reference to the engraving is not only defective in describing it as Plate VII instead of Plate VIII., but by some mistake of the engraver the reference figures are misplaced. The electric chime, which is referred to as fig. 1, has been numbered 3, and Zamboni's vibrating needle, which I have described as fig. 3, is numbered in the plate fig. 1.

I am sorry to observe by a note in your last Number that my mention of the electrical clock contrived by Mr. Ronalds has called forth an invective against me from that gentleman, whose ingenuity I have always been not only ready, but anxious, to notice; as the pages of your Journal, and my "Elements of Electricity," sufficiently testify.

I am not so fond of controversy as to trespass on your pages, or or the patience of your readers, by a lengthened discussion on the contrivance of an electrical toy, however ingenious it may be; and therefore feel it only necessary to observe, that I have endeavoured to state the facts with which I was acquainted, distinctly and impartially, without offering any insinuation, or drawing any inference but from my own experiments, which had previously indicated every conclusion I have ventured to offer on the occasion.

I am, dear sir,

Yours, &c.

London, July 4, 1815. G. I. SINGER.

P.S.—It appears to me that much misapplication of time and talent would be spared, and many trifling but vexatious disputes be prevented, if men would endeavour to estimate the value of their individual labours with more diffidence; and attend a little to the advice of Dr. Franklin, who has so properly pointed out, both by precept and example, the necessity of doubting one's own infallibility. The following remarks of a reverend contemporary of ours on this subject, may perhaps be quoted with advantage:

"I marvel much how mortals can spend their time in cavilling at each other—in murdering with their pens as well as their swords, all that is excellent and admirable in human nature—instead of curbing their passions, elevating their hopes, and tranquillizing their fears. Every evening, for at least one-third of the year, heaven has fixed in the sky yonder visible monitor to man. Calmness and splendour are her attendants: no dark passions, no carking cares, neither spleen nor jealousy seem to dwell in that bright orb, where, as has been fondly imagined, 'the wretched may have rest.'—And here we do nothing but fret and fume if our fancied merits are not instantly rewarded, or if another wear a sprig of laurel more verdant than ourselves.

* * * * * * * * * * * * * * * * *
* * * * * * * * *

"This is the dangerous consequence not so much of vanity and self-love as of downright literary Quixotism. A man may be cured of vanity as the French nobleman was,—*Ecoutez, Messieurs! Monseigneur le Duc va dire la meilleure chose du monde!*—But for this raving ungovernable passion of soaring beyond all human comprehension I fear there is no cure."—Rev. T. F. Dibdin. Bibliomania. pp. 6 & 7.

V. *Se-*

V. *Second Reply to Mr.* De Luc's *Observations on a Paper entitled "Reflections on the Inadequacy of the principal Hypotheses to explain the Phænomena of Electricity." By* M. Donovan, *Esq.*

To Mr. Tilloch.

Sir,—In my former reply to Mr. De Luc, I declared that I had no intention of supporting any electrical hypothesis; on the contrary, I wished to show that we are deceived when we suppose ourselves possessed of any knowledge on the subject beyond facts. I did not intend to affirm that Mr. De Luc's experiment, in which unelectrified pith-balls brought into a positive atmosphere appeared negatively electrified, could be *justly* explained by the hypothesis of Franklin; inasmuch as I conceived this hypothesis to be itself unfounded; but merely that it had as good claims as any other. My intention was to show that even Volta's doctrine is untenable, and that Mr. De Luc's experiment brought forward in support of it, is better explicable on the views of Franklin, even though the Franklinian system is in general erroneous, than on the views of Volta: and therefore, that the experiment in question did not establish the opinions of the latter philosopher. My reasons for supposing this I have stated to be as follows. When the unelectrified balls were brought into a room where a powerful electric machine was dispersing positive electricity, they diverged negatively; and this I conceived might as well be explained by the agency of *electric influences*, as an experiment of mine in which similarly electrified bodies attracted each other, and which Mr. De Luc accounts for by the same influences. I therefore considered that I had an equal claim to suppose the positive atmosphere to have repelled the natural electricity of the balls into their interior substance, and virtually to have left the exterior minus. With this I considered the sequel to correspond. When the balls were removed into the unelectrified room, they lost their divergence, because the cause which induced the minus state in the balls being removed, the quantity natural to the balls might receive its former distribution, and therefore appear in the natural state. But in using the expression *natural state*, I have not made myself understood to Mr. De Luc. I did not mean the natural state as laid down by Franklin. I conceived, even admitting the standard natural state assumed by Volta, that the balls should resume their non-electrified appearance. For admit this standard to be variable, is it probable that there was any variation in so short a period as that occupied by the experiment, when it appears that the balls had not time to absorb any electricity from the positive atmosphere;

mosphere; and not to speak of probability, what proof can be adduced that there was a variation?

As to the theory of electric influences, I am at a loss to understand upon what basis it is founded. It appears to me to be an adaptation of an hypothetical principle supported by facts which admit of a different explanation equally. In short, throughout all the hypotheses of electricity which have come within my limited knowledge, the proofs are derived from the thing to be proved, and have this property, that when the reasonings are pursued in all their evolutions, the inquirer invariably arrives at the place from whence he set out.

The other principal topic on which Mr. De Luc is not satisfied, is my assertion of the permeability of glass; and he states that his explanation of the Leyden phial is founded on a supposition of the contrary. Mr. De Luc also declares himself disinclined at this advanced and infirm period of his life to undertake any new experiments, but suggests one to me for trial. The least possible return that can be made to a philosopher who has employed a long life in labours so creditable to himself and serviceable to the cultivators of science, would be to execute his suggestions, but that the circumstances have rendered it unnecessary. Mr. De Luc will find by referring to my first paper on this subject, that I was aware of the objection, and had provided against it; he will then probably agree with me that glass is permeable to the electric fluid.

I have the honour to be, sir,
Your humble servant,
Dublin, June 24, 1815. M. Donovan.

VI. *Destruction of the Viewers and Overmen at Sheriff Hill Colliery.* *By* A Correspondent.

To Mr. Tilloch.

Sir, — Actuated by the same motive that prompted me to write you an account of the shocking accidents which befel the workmen in Heaton and Newbottle colliery a short time since, I now transmit the particulars of an explosion at Sheriff Hill; not so destructive of human beings as either of those catastrophes, though highly distressing in its consequences; for on the present occasion all the managing and experienced miners employed in the concern have lost their lives. The mine at Sheriff Hill is situated on the elevated ground forming the southern banks of the vale of Tyne opposite Newcastle, from whence its distance is between two and three miles. Here the low main coal

coal lies more than a hundred fathoms from the surface, and in it the excavations have been carried on for a considerable number of years, the pitmen being now employed in heaving away the pillars left at the first working to support the roof. On the morning of the 27th of June, the overman who first descended the Isabella pit, finding the air in the mine to be impure, with proper attention to the safety of the workmen about to follow him, prevented them from going to work as usual, till the cause of the obstruction had been ascertained and the ventilation restored to its proper course. For this purpose the three Mr. Foggets (resident viewers) with five overmen and three boys, ventured into the mine, and soon discovered the stagnation of the atmosphere to be occasioned by a body of water escaping from the five-quarter seam, situated eight fathoms above the low main coal, forcing away a large portion of roof, and bringing with it a quantity of carburetted hydrogen gas. While taking measures to overcome this accident, which had been long dreaded, the inflammable air by degrees displaced the atmospheric air, and a blast was the consequence, which in an instant destroyed all those who were near it.

Mr. G. Fogget, having been for some time in a bad state of health, had quitted the party before the fatal explosion; but venturing back in hopes of saving his brothers and a son-in-law, fell a victim to the azotic gas or after-damp with which the mine was now filled. Nearly eleven hours afterwards a boy who accompanied him was found lying among the water and mud in a state of insensibility, but, on the usual modes to restore respiration being administered, slowly returned to life.

Should the coal-owners of this district continue to pursue the practice of obtaining the *greatest quantity of coals with the least possible expense*, I fear you will have to record many more of these horrid catastrophes.

Newcastle-upon-Tyne, N.
July 6, 1815.

VII. *On the comparative Stiffness of Beams.* *By* A Correspondent.

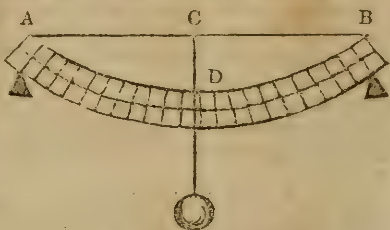
To Mr. Tilloch.

Sir,—The most useful part of the theory of the resistance of solids, is that which treats of the comparative stiffness of beams; yet in some of our best treatises on mechanics it is entirely omitted; and in others the calculations from theory are so much at variance with the results obtained by experiment, as to render them useless in practice.

A piece

A piece of Memel timber, eight inches in depth, two inches in breadth, and supported at each end, the supports being 18 feet apart, when a weight of 480 lbs. was applied to the middle, was depressed 0·73 inches. The same weight was suspended from the middle of a piece two inches square, the supports were four feet asunder, and it bent nearly 2·2 inches: but calculating from Cor. I. Prop. 79, Emerson's Mechanics, (3d edit. 4to,) the shorter piece ought to have bent only half an inch. This difference will, perhaps, be a sufficient excuse for an attempt to investigate the theory.

All bodies may be extended or compressed, and the extension or compression, in the same body, is as the force producing it.



Let AB represent a solid beam, supported at each end, and a weight suspended from the middle: by the action of the weight the lower side will be extended, and the upper side compressed; but the extension and compression, both follow the same law of variation; therefore we may consider the beam subject to extension only.

Conceive the beam to be composed of a number of equal and extensible parts, the extension of the parts being as the force producing it, is as the weight: but it is known by experiment that the deflexion is as the weight; therefore the deflexion is as the extension.

Now the effect of a force to produce extension is inversely as the breadth, and square of the depth; and the extension produced will vary in the inverse ratio of the distance from the centre of motion, or inversely as the depth; therefore, the deflexion is inversely as the cube of the depth. Also, the extension is as the strain, that is directly as the weight and length; and as the number of parts strained, that is as the length: hence, the deflexion is directly as the weight and square of the length.

Therefore, the deflexion of a rectangular prismatic beam is directly as the weight and square of the length, and inversely as the breadth, and cube of the depth; that is, if L, B, and D be the length, breadth, and depth of a beam, W the weight, and C D the quantity of deflexion; then, C D is as $\frac{L^2W}{BD^3}$. This gives 2·37 inches for the deflexion of the four-feet piece; and when the weight of the beams is taken into the calculation, it nearly agrees with the experiment.

The

The deflexion of a beam at the time of fracture, is directly as the length, and inversely as the depth.

For then, W is as $\frac{BD^2}{L}$.

Definition. "The stiffness of bodies is measured by their resistance at an equal linear deviation from the natural position." (Young's Nat. Phil. vol. ii. art. 332.)

The stiffness of a rectangular prismatic beam, is directly as the breadth and cube of the depth, and inversely as the square of the length.

Since CD is as $\frac{L^2W}{BD^3}$, by multiplication $BD^3 \times CD$ is as L^2W, and by division $\frac{BD^3 \times CD}{L^2}$ is as W; but by the definition CD is a constant quantity, therefore $\frac{BD^3}{L^2}$ is as W, or as the stiffness of the beam.

Several experiments were made; but the one described was preferred, because the difference in the length of the pieces was greater than in the others.

I am,

Your most obedient servant,

Red Barn, June 2, 1815. T. T.

VIII. *Report on M.* D'Arcet's *Method of extracting Gelatine from Bones, and on its Application to various œconomical Purposes. By Messrs.* Leroux, Dubois, Pelletan, Dumeril, *and* Vauquelin*.

M. D'Arcet has presented to the Philanthropic Society some gelatine extracted from bones, by a process which is peculiarly his own, inviting them to employ this substance in the broths and soups which they distribute to convalescents and to the poor.

The Society accordingly nominated a commission, to inquire into the advantages that may be derived from gelatine prepared in M. D'Arcet's manner. After several conferences, in which they were assisted by persons distinguished in chemistry and domestic œconomy, they were at length satisfied that the use of this substance would afford a considerable saving, and of the possibility of roasting for the nourishment of convalescents the greatest part of the meat that is otherwise employed in making the broth.

But as the Society never adopt any new article of food

* From the *Annales de Chimie.*

without previously taking the opinion of the medical faculty, it has submitted to that body the following questions: First, whether the gelatine extracted by M. D'Arcet be nutritive, and to what degree? Secondly, whether it would be salubrious as an aliment, and not liable to any inconvenience?

To these two points the committee have directed their attention; and although the method of preparing the gelatine is not of equal importance with its use as an aliment, we thought it our duty to become acquainted with it; and with this view we visited the manufactory of M. Robert, where it is extracted, and where we witnessed the series of operations to which the bones are submitted, in order to obtain from them the gelatinous matter in a state perfectly pure.

Hitherto the gelatine has been extracted from bones, by submitting them for a long time to the action of boiling water. By this method, which requires the pulverization of at least the larger bones, scarcely one-third of the gelatine contained in them is obtained: they are besides partly deteriorated by the long continued action of the water and heat. These difficulties have hitherto prevented the adoption in hospitals, of broths made from bones.

M. D'Arcet has followed a method entirely opposite. He deprives them, by means of diluted muriatic acid, of the phosphate of lime, and obtains the animal part in a solid state, still preserving the form of the bones. To take from this substance the small portions of acid and fat it retains, he puts it into baskets, and plunges it for a few instants into boiling water: lastly, after wiping it dry with linen cloths, he exposes it to a quick current of cold water, which by cleaning it perfectly, renders it white and demi-transparent.

Without entering into further details on this subject, it is enough to observe that M. Robert's establishment leaves nothing to be desired with respect to cleanliness and salubrity in the preparation of this substance.

When thus prepared and cut into pieces, the gelatine dissolves very quickly and almost entirely in boiling water. If it is wished to preserve it to use at a distant time, it is sufficient to expose it upon hurdles or nets, either entire or cut up, in a warm and dry place; then inclosed in casks or cases it undergoes no alteration, and may be preserved for a thousand years with all its qualities.

Let us now examine, with a view to œconomy, the advantages of employing M. D'Arcet's gelatine in the preparation of broth. Although this is not the principal object of the author, it is in itself of sufficient importance to merit attention.

It is known that about 100 kilogrammes of meat contain 80 kilogrammes of flesh and fat, and 20 kilogrammes of bones:

100

100 kilogrammes of meat make in our establishments 400 measures of broth of a demi-litre each. The bones which are thrown away or burnt would give 30 hundredths of dry gelatine; consequently, the 20 kilogrammes above-mentioned would furnish six kilogrammes, from which 600 measures of broth may be produced.

The quantity of broth produced from the bones is therefore in proportion to that from the meat as 3 to 2.

But pure gelatine having no taste of its own, is not sufficiently stimulating to the stomachs of invalids and convalescents: M. D'Arcet, therefore, proposes to season the broth with roots and herbs, to supply the place of the extractive matter, the *osmazone* and the salts of the meat; or, as appears preferable, to substitute the gelatine for three-fourths of the meat.

Thus with 50 kilogrammes of meat as much broth may be made of a good quality, as is usually made with 200 kilogrammes; so that the same expense will afford the same quantity of broth, and three-fourths of the meat, which may be roasted for the convalescents, who naturally prefer it to the *bouilli* of the hospitals, which is nearly reduced to the animal fibres deprived of all the nourishing juice.

Thus the food distributed will be considerably improved by adopting M. D'Arcet's method, without any additional expense. —We will show this advantage by a few examples.

First, 100 pounds of meat afford but 50 pounds of *bouilli*, and 100 pounds of the same meat furnish 67 pounds roasted; there is, therefore, nearly a fifth part gained by roasting it.

Second, 100 pounds of meat furnish 50 pounds of *bouilli*, and 200 measures of broth.

Third, 100 pounds of meat, of which 25 is made into broth, with three of gelatine, will give 200 measures of broth and 12 pounds and a half of *bouilli*, and the remaining 75 pounds will furnish 50 pounds of roast meat.

We see that by this means we have an equal quantity of broth of superior quality, and 50 pounds of roast meat, besides 12 pounds and a half of *bouilli*: indeed we spend seven francs 50 centimes in the gelatine; but this expense is more than covered by the 12 pounds and a half of *bouilli*. We must therefore conclude from these facts, that this process affords not only the means of greatly improving the subsistence of the indigent, but also a degree of œconomy which is not to be neglected.

This being demonstrated, we will now proceed to the principal object of our mission, which more particularly concerns the medical profession, and the only one on which the Society has consulted them; which is, the nutritive properties and salubrity of gelatine.

 With

With respect to the first part of this question, every one who is acquainted with the nature of meat, is convinced that the nutritive property it communicates to the broth, is derived principally, if not entirely, from the gelatine. If daily experience did not furnish undeniable proofs of this, we should find it attested by numberless authors who have written on this subject, and who all consider gelatine as the most nourishing of animal matters. Some persons may object that gelatine cannot supply the place of meat in the preparation of broth, because it is deprived of salts, and of the extract particularly denominated *osmazone*, which gives the colour, taste, and pleasant flavour to broth.

But we reply, that this principle does not exist in veal, poultry, or pork, and yet these meats are very nourishing; and, moreover, M. D'Arcet proposes, as we have before mentioned, to supply the portions of those substances that are deficient in the broth of gelatine, by a greater quantity of roots, such as onions, turnips, celery, carrots, &c. which are savoury, aromatic, and saline.

But the most conclusive experiment, and one to which every one must assent, was that which was made under our own inspection at the medical hospital. The broth was prepared with one quarter of the meat commonly used, gelatine and roots supplied the place of the other three quarters, which were roasted, and given to the invalids and convalescents, and even to the persons in attendance, who perceived no difference between this broth and that to which they had been used: they were also abundantly nourished, and perfectly satisfied to have roast meat instead of *bouilli*.

Here then is one part of the question resolved. The broth made according to M. D'Arcet's process, is at least as agreeable as the broth commonly made in hospitals; as to the other part, namely, the salubrity of the broth, we can affirm that of 40 persons who partook of it for three months, not one of them experienced any symptoms that could be reasonably attributed to the gelatine; the progress of the sick was the same as usual, and the convalescents were not longer recovering than in other circumstances. We may, therefore, without hesitation conclude, that gelatine is not only nourishing and easy of digestion, but also that it is very salubrious; and employed in the way proposed by M. D'Arcet, cannot have any bad effect on the animal frame.

Nor are these the only advantages to be derived from the gelatine extracted in the above manner: there are many others, on which I shall add a few words in this place.

1st. When reduced into thin cakes and dried, it may be used

by

by wine-merchants in their white wines, also in clarifying coffee, making jellies and creams; and lastly, it may be used instead of isinglass on all occasions.

2d. The gelatine simply dried and cut in pieces, contains a great quantity of nourishment in a very small compass; it may be rendered useful to make soup for sailors during long voyages, for soldiers in besieged towns, and even in camps and barracks.

3d. If made into cakes with a certain quantity of gravy and roots, it will make an excellent dish both for the naval and military officers. M. D'Arcet has shown us some specimens of this preparation, which surpass in beauty and quality all that we have hitherto seen of this kind.

4th and lastly. It can be employed to make glue with more advantage than any other substance that has been used for the purpose; the operation will be much shortened by it, and the glue infinitely better. The tenacity of the latter, according to some experiments made by Messrs. Cadet, Gassicourt, and Jecker, opticians, is to the best Paris glue as 4 to 3, a quality extremely valuable to joiners, cabinet-makers, &c. and especially to paper-makers, who frequently fail in their operations for want of good glue.

It is but justice to add, that M. D'Arcet, by applying to domestic œconomy a known principle in chemistry, has rendered a real service to humanity; since he has demonstrated the utility, for a number of purposes, of a matter which has hitherto been almost entirely lost.

IX. *Inquiries into the encaustic Painting of the Ancients. By* M. Chaptal *.

Pliny distinguishes colours as *colores austeri* and *colores floridi*, *i.e.* colours of a low value, and brilliant and clear colours: he adds that the latter were furnished to the painter by the person who made them; and he places in this class *minium*, *armenium*, *chrysocolle*, *indicum*, and *purpurissum*. The ochres, ceruse, sandaraque, and black, are of the former description.

Sinopis was a red earth with which minium was sophisticated.

Melinum, according to the characters indicated by Pliny, appears to us to have been a white clay. Nevertheless the ancients also employed in their fresco paintings the paste of lime, as I have ascertained by analysing some colours used by the ancients. These whites produced by lime have been preserved without alteration. The *melinum* was brought from Melos and Samos; but the latter was too fat, and the painters made but little use of it.

* *Annales de Chimie*, tome xciii. p. 298.

The ancients also distinguished two kinds of ceruse. That which Pliny called *cerussa cremata* or *usta*, seems to me to be only burnt ochre slaked in vinegar. It was used for painting shadows. The other kind of ceruse, which the Greeks called *psymmythium*, and the Latins *cerussa*, was obtained by the action of vinegar on lead. The ladies used it for the skin; the painters used it also: but Pliny places it in the third rank only among the white colours.

The black colour or *atramentum* of the ancient painters was successively the ivory-black of Apelles, the soot-black produced by the combustion of the resins, the charcoal of wood, and China ink. In order to make writing-ink, gum was added, and a fat substance (*glutinum*) in order to paint on the walls.

The *purpurissum* held the first rank among the fine colours. It was a lake which was prepared by absorbing the decoction of madder by means of the earth which Pliny calls *creta argentaria*. I presume that this earth, which was brought from England, far from being chalk, was a very white clay, because the chalk would give a vinous lake, and the argil, on the contrary, one of a very fine red. The best *purpurissum* is obtained from the first decoction: by increasing the quantity of water, the qualities were of course varied.

The ancients formed *purpurissum* also by collecting the froth which is formed on the solutions or decoctions of purple.

The *armenium* was a blue stone, which was long brought from Armenia; but a sand was found in Spain, which rendered this colour more plentiful and cheaper.

The green earths were also employed as a colouring principle.

Pliny observed that all the *chefs d'œuvre* of the ancient painters were composed with four colours; 1st. The white reduced to *melinum* alone; 2d. Ochre; 3d. The red earth or *Pontic sinopis*; 4th. The black *atramentum*.—It was, as he says, with those four colours that Apelles, Melanthus, Nicomachus, composed their *chefs d'œuvre:* "and, now-a-days," he adds, "that the purple covers our walls, and that India sends us the coloured slime of her rivers, we have more paint and less of true art."

Hence we find that the ancients employed almost none but natural colours in painting, and these were unalterable in air and water, and as we shall presently show, capable of being preserved without alteration and without degradation. But how does it happen that these very colours, most of which are used in modern paintings, change their hue on our canvass? Wherefore are our paintings incapable of preserving their colour even for a few years, while the pictures of the ancients have not lost their lustre after many centuries? This question, which is very important to the arts, deserves great attention; and I think that

that its solution can be found only in the different ways of painting practised by the ancients and moderns.

Pliny informs us that there still existed in his day, in a temple of Ardea, a town of Latium, paintings more ancient than the city of Rome; and he expresses his astonishment that the colours had preserved all their freshness, although they were exposed to the air. Some verses underneath these paintings announced that they were the work of one Ludius, a painter of Etolia. He also mentions some works of the same painter, such as an Atalanta and a Helen, which were to be seen at *Lanuvium*, and which were also in good preservation, although in the ruins of a temple. The same author also speaks of still more ancient paintings which were to be seen at Ceré, a city of Etruria.

We find, to our admiration, on the wrappings of some Egyptian mummies, and on the monuments of this ancient country of the arts, colours which have not lost much of their lustre. I have seen fragments of ancient paintings brought from Volsena, the ancient capital of the Etruscans; and also from the baths of Titus:—these were in admirable preservation, and they are composed of coloured earths only.

It remains to inquire, therefore, what was the method used by the ancients in painting, and to compare it with our present processes, before we can decide on the causes of the inalterability of the ancient paintings, and on those of the degradation of modern paintings.

Vossius informs us that the ancients formed the priming of their pictures with a coat of wax, and that this wax was coloured according to the subject which they wished to treat: he adds, that after having applied this coating of wax, they heated it and spread it. The painters, as Varro informs us, kept their coloured wax in boxes with compartments.

Vitruvius describes a process little different from that which we have described. He says that the coating was formed with melted wax, softened with a little oil, and applied with a pencil; it was afterwards made to penetrate the wall by means of a stove heated with charcoal, and with cloths a polish was given to it, as is practised with marble figures; vessels were also coated by this process.

Pliny speaks of a third modification on this method: it consists in melting wax and applying it with a pencil. These various processes were employed for painting walls, wood, &c.

Athenæus describes a festival of Ptolemy Philadelphus, in which were carried twenty amphoræ of gold, fifty of silver, and 300 others, which were painted in wax of various colours, "*trecentas vero omnis generis ceræ coloribus pictas.*"

 Wax,

Wax, therefore, was the vehicle of the colours of the ancients. The drying oils supplied its place, four centuries ago; and it is to John of Bruges that this discovery has been referred, which has been since generally adopted. Now that we know its merits and defects, we must be permitted a few reflections on its just value.

The drying oils may of course be mixed with great facility with colours, forming with them a very manageable article: the finest and most delicate tints may thus be given: the painting in oil dries quickly, and the work of the artist is never thereby suspended; but on the other hand, the drying oils become yellow when they come in contact with the air, and spoil the purest colours; the white becomes yellow; the blues, with the exception of ultra-marine which is almost indestructible, become green; the tints hold out irregularly, the transparent tints are obliterated by age. The coats of paint superposed, ac in a different way; speedily there is no more harmony in the tones, in the shadings, nor among the various parts. The oil which is dried, progressively and constantly becomes resinous, cracks, scales off, and falls from the canvass in consequence of shrinking. All these defects are inseparable from the use of drying oils; and if the works of some schools have partly escaped these alterations, it is to the use of a small number of inalterable colours that we owe their preservation. Most of the fine pictures of our museums now exhibit nothing to our admiration but correctness of drawing, fine harmony of composition, character and expression in the figures; for the painting, properly speaking, no longer exists, and the authors of some of these fine works would not know their own pictures, if they saw them at present.

We must therefore adopt some other method of painting, if we are animated with the noble ambition of working for posterity; and we shall hazard a few observations on the use of wax, as a method of preparing canvass, and afterwards serving as a vehicle for colours.

The ancients prepared the base of their pictures with a coating of coloured or colourless wax, which they made even by applying heat to it: they afterwards painted over it: "*Item muris obducebant ceræ loricam, in eaque pingebant*," says Vossius. It appears that they sometimes used oil to soften the wax; which was attended with the inconvenience of making the latter run when a warm body was brought close to it, with a view to unite and polish the coat. Thus Vitruvius, in speaking of this process, makes use of the words *sudare cogat;* which proves that the oil is detached, and forms an obstacle to the junction of the wax; an inconvenience which I have experienced on re-

peating

peating the process. Besides, this mixture of wax and oil retains a softness and glueyness too long, which does not admit of the celerity which is wanted in these kinds of works. Pliny says that the wax may be melted by means of heat, and applied with a pencil: but as it hardens in the air with great facility, it will be difficult to form in this way a very compact coating.

We must therefore invent another method of making wax manageable by the pencil, and which shall procure a prompt desiccation, without taking from its whiteness or consistence. We may find this desideratum, I think, in the employment of the volatile oils, or the colourless essences. It is sufficient in order to melt the wax in a volatile oil to employ it in stripes, as it is when bleached, and to sprinkle it after that with some drops of oil. A slight heat is then sufficient to operate the solution; and we thus obtain a very transparent liquid:—the same result is obtained with a fixed oil. The fixed oils well purified, or the colourless volatile oils, ought to be employed in this operation. This combination may be applied to canvass, wood, or marble, in the liquid state: it then adheres strongly to those bodies, because it penetrates them, and forms on their surface a white and slightly transparent coating. But the solution of the wax in the volatile oil is preferable, because, in addition to its being whiter, a heat of 20 or 25 degrees is sufficient to evaporate the oil, and give more consistence to the coating. Care must at the same be taken not to heat too strongly, lest the wax itself should be evaporated. We may also prime canvass intended for paintings in the following way:—

When the combination of volatile oil and of wax is fixed, it forms a soft paste which may be easily spread over canvass, wood, and marble. By means of a polished hot iron, the wax is made to penetrate these various substances. The heat then evaporates the volatile oil, and a coating of wax only remains. By priming canvass in this way we may cover both surfaces, and thus place them out of the reach of air and humidity, which will render their duration almost eternal.

If it be only wanted to apply one colour upon a base, and to produce what the ancients called a *monochrome* painting, it is sufficient to mix the colour which we wish to use, with the liquid combination of oil and wax, and to stir until the mixture is hardened. We should then apply the paste to the surface of the body upon which we wish to paint, and make it penetrate by means of an iron slightly heated. Some drops of olive oil spread over the surface will facilitate the operation of polishing which it is important to give to the coat of painting: this slight coat of oil will be afterwards removed by means of a piece of leather or fine linen. By this process the polish of the marble

statues

statues of the ancients, or of our modern stucco, is given to the painting.

If it is desired to paint in several colours (which truly constitutes painting), wax can be coloured, and all the different tones requisite can be given to it. But this employ becomes still more difficult, because these waxes can only be applied with a pencil: it is therefore necessary to keep them in a soft state approaching to fluidity, where there is the advantage of artificial heat, or the maintenance of a temperature kept up sufficiently in the workshop. It would be possible, however, by varying the proportions of the oil, to obtain coloured pastes which might be touched and retouched by the pencil, at the ordinary temperature of the atmosphere; only the drying up would then be very slow, and I think that the wax, being brought to a permanent state of fluidity by means of several drops of alkali poured on the melted wax, would form a preferable mixture, because the wax still remains liquid, and as white as milk. It is easy to incorporate on the palette the colours with this milk of wax (*lait de cire*); it gives them a suitable consistency, and they are used with a pencil like those which are prepared with drying oil. M. Bachelier, who suggested this vehicle for colours nearly forty years ago, composed in this way pictures which have not sensibly suffered in point of colouring.

M. Castellan lately communicated to the Institute a new method of painting, which strongly resembles that of the ancients: he begins by priming his base with a coat of melted wax, taking care previously to heat and dry the stucco and plaster: he spreads the wax with a brush, equalizes the surface by passing a gilder's stove over it, or the hot disk which the ancients used; pieces of new cloth and coarse brushes passed over this surface terminate the work of priming. All the modifications requisite for the priming of wood, plaster, and canvass, are detailed in M. Castellan's memoir. He paints on those primings with colours ground with olive oil and not drying oil. The painting is dried by passing the stove over the picture, or by raising the temperature of the workshop to 30° or 40° of heat; or lastly, by exposing the picture to the sun. Painting on canvass requires for its desiccation a heat of 20° or 30° only. M. Castellan glazes his pictures with a transparent varnish which is made by the solution of wax in a colourless volatile oil.

Several paintings executed by this process have been exposed for several years to all the inclemencies of the air, without being sensibly altered. Even the English lake, which fades so quickly in the sun, has not lost any of its intenseness of colour.

The process proposed by M. Castellan appears to possess the following advantages.

1. It

1. It incorporates, by means of heat, the painting with the base and priming, in such a way that we have only one single body; whereas in painting with drying oil, the coats of priming and of painting are not melted together, but superposed on each other. We may be convinced of this by observing closely the operations resorted to in giving a fresh canvass to an old painting: every coat of paint is found to be distinct, and adheres more or less closely, according to its thickness and the principles of which it is composed. The picture soon undergoes a great alteration by this process.

2. In the substance which serves for priming, or preparing the base, as well as in the vehicles for the colours or the varnishing, there is nothing capable of shrinking in course of time, or from progressive desiccation; so that the painting can neither warp nor crack, nor fall off in scales.

3. The colours being melted in the wax and covered with a coating of the same substance, are completely out of the reach of air and *humidity*, which are the most powerful destroyers of paintings.

M. Castellan's process for painting has this advantage over every other hitherto practised for imitating the encaustic painting of the ancients; namely, it does not overturn the customs which obtain in all modern schools for painting: there is in painting a merit so intimately connected with the mode of execution, that a sudden change in this would be repugnant to every painter, whose ideas are more intimately connected than is generally supposed with the mode of expressing them. Several landscapes which have been painted by M. Castellan, and two large portraits by M. Taunay, exhibit no difference from common oil paintings: we perceive in them the same freedom of penciling, the same boldness of touch, clean execution, lightness of colouring, and the same transparency of tone.

X. *Process for Curing Herrings in the Dutch Manner.* By *Mr.* H. E. Sievers, *of Lower Thames Street.**

Sir,—As a candidate for the Society's gold medal of the year 1814, I beg leave to present two samples of British herrings, cured in all respects after the manner of the Dutch. Those marked *A* having been caught and cured on the coast of Shet-

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures and Commerce*, vol. xxxii. for 1814.—The gold Isis medal of the Society was voted to Mr. Sievers, for herrings caught in the British seas and cured in the Dutch manner.

land

land on-board ship; those marked *B* were caught upon the coast of Yarmouth, and cured in my extensive warehouse there.

I beg leave to submit, that I have been employed in the cure of Dutch herrings near twenty years, twelve of which I passed in Holland, the remainder in England. That for the last four years, I have annually exported from 3 to 4000 barrels of herrings of this cure to the continent, and small quantities actually to Amsterdam, and large orders for the West Indies,—a decided proof I presume of the great superiority of my peculiar method of cure.

The whole of these fish were caught by vessels sent to sea for my account, and cured by my direction and positive instruction; certificates of which accompany the herrings.

Further, I have only to solicit the favour of an intimation when these casks are to be opened, in order that my cooper may attend to show them, a circumstance in some measure proper to their good appearance.

Subjoined you will receive the method of cure.

I am, sir,

Your obedient servant,

H. E. SIEVERS.

Dutch Herring Warehouse, No. 52, Lower Thames-st. 7th Feb. 1814.

To C. Taylor, M.D. Sec. to the Society of Arts, &c.

Method of Curing Dutch Herrings referred to.

As soon as the fish are taken they are gipped, that is, gutted, and afterwards sprinkled with salt in their own blood, in large square, round, or oval tubs, about 1½ feet deep, in England called stir tubs, in Holland warr backs, where the fish are well stirred together, that the salt may take equal effect. The barrels must be ready to use instantaneously for packing with salt, of which I generally use four barrels to 14 barrels (a last) of fish; they are then headed up and placed in the ship's hold, each barrel being marked with the date of heading: about four or five days after, wind and weather permitting, each barrel is again opened for filling up, and care must be taken that they are always kept in this state, otherwise they will become rancid, or what is termed rusty; in this state they are brought into port: here they are repacked as occasion may require, in various packages suitable to the market they are destined to. For home consumption and Baltic trade, they merely require to be filled up; for the West Indies they require repacking into small kegs of about one gallon each, with the addition of two pounds of salt to each keg.

I do

I do solemnly declare this to be the true method of curing Dutch herrings, as practised by me and my men in curing the samples now exhibited by me.

H. E. Sievers.

*** On a minute examination of the samples of herrings produced to the committee, it appeared that those caught in the deep sea of the coast of Shetland were fatter and fuller of milts and roes than those caught on the coast of Yarmouth. Mr. Sievers stated, that herrings are generally known under three denominations, viz. the St. Michael's herring, the Highland herring, and the long shotten herring. That the deep sea fishing is carried on in sloops, each carrying ten or twelve hands, that they go to Shetland to clear out according to act of parliament about 16th June, and have then to return to commence the fishery at Buchaness off Peterhead on the 24th of June; that by being thus at present obliged to go first to Sheerness to clear out, they suffer great disadvanges by delays from wind and weather, in a voyage of 300 miles before they can commence the fishery at Buchaness; and that the Dutch have an advantage from not being obliged to go to Shetland; that the herrings caught in the deep sea off Buchaness are large, fat, and full-bellied; they are also richer in flavour, and more esteemed for home consumption and the continent, but do not keep so well as the lanker herrings caught near Yarmouth, which last are better calculated for the West India markets; that the Yarmouth fishery is carried on in September and October, as the herrings come down the German ocean.

XI. *Memoir on the Nature of fat Substances.* *By* Henry Braconnot, *Professor of Natural History, and Director of the Garden of Plants at Nanci* *.

Hitherto chemists have considered the fat of organized bodies as being formed of one substance, having the same essential properties, and differing only by its firmer or weaker consistence: hence the various denominations of suet, lard, fat, marrow, &c. admitted by the ancients. This consistence of fat varies in fact in a manner truly remarkable: thus it is hard in the ruminating quadrupeds, softer in man than in the animals which live upon vegetables only, almost liquid in the amphibious mammiferæ, the cetacei, as well as in all the carnivorous animals, whether birds, reptiles, fishes, or insects. Not only does the consistence of the fat vary in the various kinds of animals, but also according

* *Annales de Chimie,* tome xciii. p. 225.

ing to the regions which they occupy, and according to the age, sex, and physical constitution of the individual. It is very hard in the neighbourhood of the kidneys, softer in the epiploon, the mesentery, the intestines, and around other moveable viscera: it is almost liquid when it coats the orbitary cavities. Certain accidental dispositions also produce remarkable changes in the consistence of the fat, as is observed in the *steatoma*, the old herniæ of the epiploon, and in certain sebaceous tumours, which are sometimes as hard as a calculus.

On reflecting on this infinite variation in the consistence of fat, and seeing besides that suet and oil seem to be the two extremes of this consistence, it appeared to me that with these two bodies mixed in various proportions, nature might produce that diversity of fat bodies which we observe in organized beings. Deeply penetrated with this idea, which appeared specious at least, I endeavoured to find among the chemical reagents some substances capable of separating the suet and the oil which I supposed to exist in all the fats, but I found none which could effect my purpose; when at last a very simple method presented itself to my mind, and fully confirmed my conjecture. It is founded on the physical property peculiar to oil, of being taken up greedily by gray paper, which is not touched by suet or tallow, in its state of purity. It was by applying this process to the analysis of fatty substances that I succeeded in discovering the two substances which compose them, and in determining their respective proportions in the following substances.

Melted Butter.

This article was forcibly compressed for several days at the temperature of zero, between several folds of coarse gray paper, taking care to renew it until it was no longer stained: when again pressed as at first, at a temperature of 15° Reaumur, a white brittle matter was obtained, at least as compact as the butter of cocoa or the hardest suet, of a very decided smell and taste of suet. Suspecting that it might still retain some traces of oleaginous matter, it was melted, and I mixed with it a small quantity of volatile oil of turpentine: the matter, when hardened and pressed to absolute dryness in blotting-paper, presented a substance which was kept in fusion during some time: when hardened, it was dry, broke with lustre, and melted at the same degree of heat as beef suet, a temperature which I found to be 46° of Reaumur, while we find that mutton suet requires a temperature of 49°. The suet of cow butter is, in fact, similar to that which exists in the various parts of the body of this animal: a result which might lead to some physiological consequences. In order to obtain the oil of butter, I moistened with

warm

warm water the gray paper, in which the butter had been compressed; then I made a ball or knot of it, which was submitted to the action of the press, and there resulted an oil perfectly fluid. We may also obtain a part of the oil of butter by pouring it into a vessel furnished at its lower part with an aperture which is to be uncorked when the butter freezes: after some time, and at a moderate temperature, a considerable quantity of the oil flows out, which may be advantageously used in the preparation of certain dishes. The oil of butter obtained at a low temperature is a liquid of a yellow colour, like most of the fixed vegetable oils, of a smell and a taste peculiar to the butter. I had tried, but without success, to take off from this oil its colouring matter, by treating it with ether. The sulphuric acid when properly employed seems to destroy this yellow colour, for the oil becomes colourless, particularly after having been treated with a little argil.

One hundred parts of melted butter of a good quality made during summer, yielded as a product at the temperature of zero:

Oil	60
Suet	40
	100

But these proportions are liable to variation, according to the physical constitution of the cow, the nature of her food, and the place where she is domiciled. Thus the butter which is brought to us from the Vosges mountains is of a fine yellow colour; it is much more oily and more esteemed than that which is procured in plain or low countries, which last is paler, firmer, and contains more suet. This last substance is still more abundant if the cows have been fed upon dry forage; when they furnish a hard, compact butter, of a dull white and of an inferior quality. One hundred parts of this winter butter from the Vosges yielded at the temperature of zero,

Oil	35
Suet	65
	100

We see by these respective quantities the enormous difference between summer and winter butter: but if the proportion of suet and oil varies in the same animal, according to different circumstances, *a fortiori*, ought it to vary in the different kinds of animals which yielded it. To judge from its consistence, the butter of cows and goats ought to contain a much greater quantity of suet than that of sheep, asses, and mares. That of women seems to be entirely formed of oil.

Of

Of the Fat of Pork, or Lard.

Like melted butter and the other fats, that of pork has been hitherto regarded as a homogeneous body. M. Vogel considering it as such, submitted it to a series of experiments in a memoir particularly directed to the examination of this substance*. Recent hogs-lard, well purified, was long pressed in gray paper, as we have directed in the case of butter, and a suet was obtained of the consistence of soft wax; as this suet seemed to me still to retain some oil, it was purified by melting it in oil of turpentine, and by submitting the mixture when cold to new compressions in gray paper, there remained a sebaceous matter, which was held in fusion some time to extract the small quantity of oil of turpentine which it might contain. This absolute suet at the ordinary temperature of the atmosphere is dry, brittle, and inodorous, like the other suets in their state of purity; but it differs from them in being almost transparent or semi-diaphanous like certain calcedonies; it is softened or reduced by the fingers with great difficulty, although more easily than beef suet, and has not such a greasy appearance, but it is soft and soapy to the touch like whale fat, like which it leaves a brilliant lustre when it is rubbed on bodies: in these respects it resembles spermaceti: it has, however, but a grainy crystallization, not decided; and it is less soluble in boiling alcohol, although the latter takes up enough to make the liquid turbid on cooling, and there are separated flakes of greasy matter on the addition of nitre.

The suet of pork requires for its fusion a temperature a little higher than that which is necessary for melting whale fat.

The same suet, like all those obtained from the different fatty substances, undergoes under the action of the acids and alkalis a very remarkable alteration, in so much as it is converted into two substances which did not exist before; namely, adipocire and oil. both extremely soluble in warm alcohol. This transformation is entirely and immediately affected by pouring into one part of pork suet melted, half a part of sulphuric acid, and by immediately diluting the mixture with a quantity of boiling water in order to take up the acid and to separate the two fat matters newly formed, which we shall presently make known. I shall content myself with observing at present, that the suet of pork on being converted into adipocire and oil is susceptible of forming with the sulphuric acid a soapy combination sufficiently intimate, and even soluble in water like soap, which does not precisely happen on treating beef or mutton suet in the same way, although all the fat substances, in changing their nature,

* *Annales de Chimie,* tome lviii. p. 154.

also

also saturate one part of the properties of the sulphuric acid, as they saturate that of the alkalies. Potash in saponifying pork suet transforms it also into adipocire and oil *. The gray paper in which the lard had been compressed was strongly penetrated with oil; it was moistened with hot water and subjected to the action of the press, after having been inclosed in a strong cloth, and we obtained an oil which was easily separated from the water. The oil of lard is a colourless fluid like water, of a peculiar taste. When exposed to a greater degree of cold it does not freeze, if it has been obtained at a low temperature. One hundred parts of hogs-lard at zero of the thermometer furnished me as constituent principles:

Oil	62
Suet	38
	100

If the compression of the lard has been effected at a middling temperature, we obtain a greater quantity of oil; but in this case the latter retains some suet, and is hardened into a gelatinous consistence when exposed to the cold.

Beef Marrow.

When slightly pressed between the fingers, this substance, taken from the interior of the thigh-bone, seemed entirely formed of an innumerable multitude of very distinct transparent bags containing the marrow. We shall see presently that this structure is not peculiar to the marrow, but that it is common to all fat substances in the animal kingdom. When well washed and freed from its vascular envelopes by fusion, beef marrow had a firm consistence at the temperature of —·2° R.; in this state it was pressed in gray paper until it ceased to stain it, and I obtained

Suet	76
Oil	24
	100

* I had dissolved in a great quantity of water, soap procured from hogs-lard and potash, with a view to examine the substance described by M. Chevreul under the name of Margarine; but instead of obtaining a pearly substance in brilliant scales, nothing was formed after some days but a very trifling precipitate, which when collected on the filter had the appearance of gelatinous albumen newly precipitated; when dried, it was of a dull white, which became shining when rubbed, and was reduced between the fingers to an impalpable powder, soft and soapy to the touch. This matter, insoluble in water, was composed of an adipocirous substance, potash, and a small quantity of lime. Probably it is not this substance which M. Chevreul has described as margarine: I confess candidly that I am ignorant of the circumstances which may have concurred in the formation of this last substance.

But the suet thus obtained was soft, and consequently still retained a little oil: in order to purify it completely, it was melted, and I added to it an equal quantity of volatile oil of turpentine.—The mixture when cooled and pressed as before, furnished a suet which did not differ much from beef suet: it seemed, however, to be a little more fusible. The oil of beef marrow is an almost colourless liquid, and has a disagreeable taste.

Mutton Marrow.

It is nearly of the same structure as that of beef; but it contains a much more considerable quantity of oil. Thus 100 parts of mutton marrow washed and melted gave me at the temperature of $-2°$ Reaumur: Suet 26; oil 74.

The suet of mutton marrow, purified as above, has the same appearance with common mutton suet; like which it is of a dry and brittle consistence, which admits of its being reduced to powder; but it is more fusible, for it melts at 41° of Reaumur, whereas common mutton suet from the kidneys fuses only at the temperature of 49°, which seems to establish a difference between those two substances; which have besides the property of waxing substances, like bees-wax.

Goose Grease.

One hundred parts of this grease well washed, after being obtained from a roasted goose, yielded as results, at the temperature of $-2°$ R: Oil 68; suet 32.

Goose grease purified with essence of turpentine has the whiteness, hardness, dryness, and fracture of common beef suet; but it is much more fusible, for it begins to melt at 35° R. The oil of goose is a liquid slightly coloured, and has the taste and smell peculiar to the goose.

Grease of Ducks.

This grease at the temperature of $10° + 0$ R. has the consistence of half-frozen olive oil; and at 20° of the same thermometer it is perfectly fluid. One hundred parts of this grease at $-2°$ R. produced: Oil 72; suet 28.

The oil of ducks possesses the taste, smell, and yellowish colour which is visible in the fat of the animals. The suet when very pure is white, not very sapid, inodorous, dry, and breaks like wax. It differs from goose suet in so far as it affects a decisive crystalline form: besides, it requires 42° R. to melt it. It is dissolved in boiling alcohol, but in a small quantity.

Grease of Turkeys.

One hundred parts yielded at the temperature of zero: Oil 74; suet 26. The oil is of a yellowish colour, and has the taste

taste and smell peculiar to the animal. The suet when purified by oil of turpentine, is not so dry and fragile as that of ducks: it melts at 36° R.

Olive Oil.

The oil extracted from olives is no longer, as has been supposed, an immediate simple principle: it is composed, like all the fat bodies, of two distinct substances. Olive oil of good quality was exposed for two days to the temperature of −5° R.: it formed into a mass of the consistence of honey: when pressed at the same degree of cold in gray paper, a suet was obtained of a brilliant white, inodorous, not very sapid, and as firm as beef suet, but of far greater fusibility, for it melted at 16° R. The alkalies by acting on this suet metamorphose it into oil soluble in alcohol and into an adipocire which melts at 50° R. only. The oil of olives thus isolated from its suet, and penetrating the paper in which it had been pressed, was separated from the paper by humecting it with warm water, and pressing it again. This oil has the smell and taste of olive oil; but when exposed to a temperature of 10° R. it does not freeze; although exposed to a more considerable cold, a small quantity of suet still leaves it. The property which this oil has of not freezing and not becoming rancid, renders it valuable in the arts, and particularly to watch-makers; but it has nevertheless a great inconvenience when employed in its common state; namely, as it freezes at a middling degree of cold, there must be a consequent irregularity in the movements of the delicate machinery to which it is applied. The oil of olives, however, when deprived of its suet is absolutely free from this complaint, and has all the qualities which we can wish. 100 parts of olive oil at the temperature of −5° R., produced:

Oil of a greenish yellow	72
Very white suet	28
	100

Without reckoning the frequent falsifications to which olive oil is subject in its purest state, it is not always identical in the proportion of its principles: thus the *virgin oil,* or that which is obtained by a slight pressure of the olives, contains much less suet than that which comes afterwards: it is therefore improper for the manufacture of soap, and we shall presently see that the suet or adipocire is indispensable in the manufacture of soap of good quality.

Oil of sweet Almonds.

One hundred parts of oil of sweet almonds, frozen at 8° or 9°

below

below zero, were treated as above; and yielded: yellow oil 76; very white suet 24.

The above suet resembles the rest in its colour and consistence; but it is distinguished from them by its extreme fusibility; for it melts at 5° of Reaumur, and freezes again on the temperature being lowered. This suet, which is so fusible, is transformed by the action of potash into oil, and into an adipocire which melts at 45° only; which is very remarkable. The oil of sweet almonds deprived of its suet resists the greatest cold without losing its fluidity.

Oil of Colsa.

By exposing this oil to the freezing temperature, and even above it, a part of its solid matter is separated from it in the form of round globules; it is a mistake in Fourcroy to allege that it is less capable of freezing than oil of sweet almonds, since the latter requires at least 7° or 8° below zero. At −3° R. the oil of colsa goes into a mass much firmer than melted butter, and of the yellow colour of wax. 100 parts of this substance, pressed at the same temperature in gray paper, yielded:

Oil of a fine yellow	54
Suet, very white	46
	100

This suet obtained after the first pressure had still a slight yellowish tinge; but when pressed a second time, after having been melted, and then frozen at the temperature of 2° R. a brilliant white was obtained: it is inodorous, not very sapid, and affects a spherical crystallization; it is a little less fusible than the suet of oil of sweet almonds, and melts at 6° R. Its chemical characters seem to distinguish it from the other suets; for, instead of being converted into adipocire and oil by the action of the acids, it only yields a thick and thready mass like turpentine: time produces nearly the same effects as the acids on these oils. The oil of colsa, deprived of its solid matter, is no longer susceptible of freezing: it merely possesses the colour, smell, and taste, which we recognise in the oil of colsa of commerce.

The experiments just detailed on the fat principles, authorize us to think that all the rest are similarly composed of fluid oil and a solid substance which we find even in the oils which have the least disposition to freeze, as recent linseed oil, which deposits during a cold night very regular conical crystals, as observed by Professor Goettling. Most of the volatile oils also contain concrete crystallizable substances, sometimes analogous to camphor: hence it would seem that camphor is to the volatile oils what suet is to fat. Certain volatile oils, however, seem to contain

tain concrete substances very different from true camphor, like those which probably exist in the volatile oil of roses and in that of the elder-tree which have the consistence of butter. We may say the same of the volatile oils furnished by the umbelliferous plants; such as the parsley, fennel, dill, cumin, and aniseed*, all of which have the property of freezing in a very slight degree of cold.

Experiments upon Suet.

Physiologists are of opinion generally, that the fat has its seat in the cellular texture. On examining with the naked eye suet as we meet with it in animals, *i. e.* that which envelops the kidneys in the ruminating quadrupeds, we find it arranged in conchoid, square, or orbicular masses, separated from each other by membranous laminæ. If we examine each of these masses attentively, we see them entirely formed of an innumerable multitude of oblong globules, transparent and brilliant like a crystallized salt, and which seem tied to each other by a very loose membranous texture, like the grains of starch in a boiled potatoe. We may separate these globules from each other by slightly macerating the suet in cold water, and shaking the whole over a hair sieve. We then obtain a powder which resembles starch when it has been dried on gray paper. These membranous vesicles containing the suet, did not appear to me to have the same structure as the cellular texture, which is formed, as we know, by the junction and mutual adherence of the cellules which communicate with each other.

In order to ascertain if those membranous cysts serving as a reservoir to the fat had any orifices to allow it to escape, they were exposed to different degrees of heat with water; but it was only at a heat of 65° of Reaumur that some bubbles of greasy matter melted, began to appear on the surface of the liquor, and even at the boiling point a great number of those isolated vesicles still existed without having given out any grease: hence it follows that every globule of suet is contained exactly in a membranous vesicular envelope, which hinders it from separating, even at a heat much greater than that which is necessary for melting it. The adipose glands of Malpighi, which no author since his day has discovered, are they the same organs with those vesicles filled with suet? I am not inclined to think so, since we observe the latter very distinctly without the assist-

* On examining some casks which had contained aniseed oil, I found the whole inside fringed with a brilliant dry crystallized matter, which it was easy to collect, even in a considerable quantity; and I thought of examining these crystals more minutely: but having left them during a whole summer exposed to the solar light, I found them resolved into a thick liquid which lost the faculty of crystallizing.

ance of the microscope, in the fats which are not too soft. However this may be, the fatty substance, liquid or solid, fixed or volatile, and wherever we meet with it in organized beings, presents nearly the same structure in the reservoir where nature has placed it.—To give some examples of this taken from the vegetable kingdom, I shall cite among the plants which secrete the *sebaceo-cirous* matter, the corpuscles which constitute the dust of the leaves and fruits which Proust has found analogous to wax. On examining this dust with the microscope, we see it formed of transparent grains, the form of which varies with the species: an analogous configuration may be remarked in the waxy substance which coats the surface of myrtle-berries. It is sufficient to press strongly the latter, in order to free them of the wax, which has a grainy and amylaceous appearance. The volatile oils are also contained in small membranous bladders, as is proved by the epithets of vesicular, miliary, globulous glands, which botanists have given to these small reservoirs filled with volatile oil. I shall not inquire, with physiologists, if the secretion of the suet or of the fat operates, as they pretend, by the extremities of the arteries, or by oozing through their tunics: one thing is certain, that this secretion takes place under certain circumstances with incredible celerity.

Suet does not exist in a state of absolute purity in animals, and hitherto it has been unknown any where in that state: the firmest which can be procured still contains a certain quantity of oil; that which incloses the kidneys in sheep contains more than that of bullocks: the tallow used for candles also furnishes a great quantity; for 100 parts at the temperature of 5° R. yielded 30 parts of oil. We succeed in depriving these fatty substances of their oil by melting them and adding spirit of turpentine newly distilled; the mixture, frozen and pressed in a cloth, or what is still better in gray paper, leaves the suet in a state of purity: it is only necessary to keep it in fusion for some time, and allow it to cool. Pure mutton suet thus obtained is a substance very analogous to wax, and seems even to be more compact, drier, and more brittle; but it has not the same ductility, for it is easily reduced to powder; it likewise waxes bodies, giving them a glossy appearance like bees-wax, and emitting the same noise on being rubbed*. Pure mutton suet melts at the temperature of 49° R: yellow wax melts precisely at the same de-

* Fatty substances as well as certain families of organized beings seem to contain a great quantity of species which pass to each other by shades so slight that it is difficult to group them well: thus we are frequently embarrassed, when it is wanted to range this or that fatty substance, whether to do so among the waxes, the suets, or the adipocires, and it must be admitted that the last three genera are purely artificial.

gree

gree of heat, according to Bostock; but this suet is less soluble than wax, and than the white fat of whales in boiling alcohol. The latter may, however, dissolve to make the liquor turbid on cooling. Boiling ether dissolves it better, and upon cooling a precipitate is formed in large gelatinous flakes.

Pure beef suet is white, although having naturally a yellow colour when it is not purified, which makes it be supposed that this colour is owing to oil: it has a greasier aspect than mutton suet; it is besides more fusible, since it is liquefied at 46° R. Thus it is less adapted for candles than pure mutton suet, which when obtained by some easy process will afford the means of supplying the place of wax in some circumstances.

In order to complete what I have said as to suet, I shall enter upon some details respecting its singular metamorphosis into several fat substances by the action of the acids and alkalies, which will lead me to examine the results of saponification and rancidity.

Action of the Sulphuric Acid upon Suet.

Chemists have contented themselves with saying, that the sulphuric acid decomposes and carbonizes fat, particularly with the aid of heat; but it does not appear that they have examined with care the results of this action. If we pour concentrated sulphuric acid into melted suet, there is immediately formed by agitation a true combination, in which the acidity has in a great measure disappeared. To one part of melted mutton suet was added half a pint of sulphuric acid, and the combination, which was of a reddish colour, was diluted in a great quantity of boiling water, which seized the sulphuric acid and abandoned the greasy matter. The latter, after having been washed several times, had not perfectly lost its weight, but it had acquired a less firm consistence than the suet employed: the latter was in fact transformed into a small quantity of oil, and into a substance very analogous to wax. In order to isolate these two substances from each other, the *oleo-cirous* mass, when well cleansed from sulphuric acid, was melted, and the half of its weight of alcohol was added to it; the union took place immediately with a promptitude truly extraordinary: upon cooling there resulted a hard mass, which was folded in new linen; by gradual pressure the alcohol was expelled charged with oily matter, and the compression was finished in gray paper. The solid matter thus obtained was not yet perfectly white, although treated several times with alcohol; but by means of warm ether, which also dissolves it with extreme facility, we succeed much better in obtaining it of a perfect whiteness. This substance is dry, sonorous like

 hard

hard bodies, susceptible of being reduced into powder, breaking with more lustre than white wax, and having the same taste, smell, and properties of waxing bodies, but it has not the same tenacity; and instead of receiving the various impressions which the fingers give it, when it is slightly softened, it is reduced into a lamellous powder: in this respect it resembles the Louisiana wax; but the latter melts at the temperature of 34° R. according to Bostock, whereas the matter in question does not enter into fusion sooner than 53°, and on cooling it does not crystallize better than common wax: it is also infinitely less fusible than the adipocire of dead bodies, although it seems in other respects to resemble it much in its chemical properties, and particularly in the faculty which it shares with the latter, of being combined immediately with the alkalies to form soap; whereas the other fatty substances do not become susceptible of uniting completely with it until after a long ebullition, and after undergoing an alteration which makes them closely resemble the waxy substance the properties of which we are endeavouring to ascertain. The latter has such an aptitude to combine with the alkalies, that it disengages the carbonic acid from the alkaline carbonates to form soap: this property is common to certain fat bodies only easily soluble in alcohol, like adipocire and the oil of soaps; that of churchyards, and perhaps also that of mushrooms, as well as their oil.

The same substance contracts with ammonia a combination soluble in water, but the soapy liquor deposits a part of this combination.

When this fat substance is melted, it unites with and is dissolved in alcohol in every proportion and with astonishing facility. If the liquid contains but a small quantity of alcohol, it goes into a mass more or less solid upon cooling; but when the liquor is diluted with a greater quantity of alcohol, a white flaky and abundant precipitate is formed. In the cold, alcohol dissolves but a very small quantity, because it has to overcome the force of cohesion which this substance opposes to it: thus 100 parts of alcohol at the temperature of 10° R. only dissolved 21·2. Warm ether dissolves it at least as rapidly as warm alcohol, *i. e.* in every proportion: when reduced to powder and put aside to digest with 100 parts of ether, the latter dissolved 12 parts of *cirous* matter at 10° R. The volatile oil of turpentine at the same temperature, only takes the twentieth part of its weight.

If we compare the totality of the physical and chemical properties of this substance with that of the other fat bodies, we shall find that among the latter the adipocire of the churchyards

seems

seems to be that which resembles it the most: consequently, and without hesitation, I regard it as a particular species in the genus adipocire, already very numerous in species and varieties.

[To be continued.]

XII. *Process for curing Herrings, Pilchards, Mackerel, Sprats, &c. by Immersion in Brine of British "solid Salt." By Mr.* Phillips London, *of Cannon Street.**

Sir,— I beg of you to present my respectful compliments to the gentlemen of the Committee, and also to the Society, for their vote of thanks conveyed to me in your letters of 9th March and 21st of April last, expressing also their hopes that I would persevere in preserving mackerel upon the principle which I had the honour to communicate to them in May 1813, and which, they were pleased to say, they regard as an object of much national consequence.

I have not, sir, been inattentive to the object; on the contrary, I have caused to be cured this last season at Ramsgate, upwards of twenty-five thousand mackerel.

There were cured at the same place this autumn, and sent to the West Indies, upwards of three hundred barrels of herrings, and in the last spring, about fifty barrels of sprats, all preserved upon the principles which the Society did me the honour so highly to approve, and which, I am fully persuaded, is the most effectual, œconomical and expeditious method of curing fish ever practised; and I am strengthened in this hope by observing in the fishing regulating bill, now depending, that encouragement is held out by a new clause introduced expressly for the purpose.

In expectation that the mode of curing fish by immersion in fully saturated brine of solid salt may become very general, particularly for herrings, I am induced to inclose to the Society the process more fully explained than it is in their volume for 1813, and calculated for curing fish on shore, and in any situation however exposed, even without any covering, as you will readily conceive that a surplus of salt on the surface of the brine will always meet and quickly saturate any rain whatever that may fall into the reservoirs.

I am, with respect and esteem, sir,

Your obedient servant,

57, Cannon Street, Dec. 6, 1814. Phillips London.

To C. Taylor, M.D. Sec.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures and Commerce*, vol. xxxii. for 1814.—The thanks of the Society were voted to Mr. London for this communication.

The

The Process.

Reservoirs of any required size are to be provided in form of tanners' pits, or backs, or vats, or casks, perfectly water-tight, which should be about one-half filled with brine made with the said salt of the spec. gravity of 1·206, water being 1·000, by dissolving about 28 parts of the salt in 72 parts fresh water.

The fish, as fresh as possible, are to be gutted or not, and without delay plunged into this fully saturated brine in such quantity as nearly to fill the reservoirs, and after remaining therein quite immersed for five or six days, they will be effectually struck, and so fully impregnated with salt, as to be perfectly fit to be repacked as usual with large grained "solid salt," and exported to the hottest climates.

Brine is known always to be weakest at the upper part. To remedy this, and in order that the brine be kept up to a uniform saturation, a wooden lattice-work frame, of such size as easily to be let into the inside of the reservoirs, is sunk an inch or two under the surface of the brine, for the purpose of suspending upon it lumps of 1 or 2lbs. or larger, of "solid salt," which effectually saturate whatever moisture may exude from the fish, and thus the brine will be continued of the utmost strength, and so long as any part of the lumps remain undissolved. The solidity of the lumps admits of their being applied several times, or whenever the reservoirs are replenished with fish; and the brine, although repeatedly used, does not putrify, nor do the fish, if kept under the surface, ever become rancid.

All provisions are best preserved by the above method, particularly bacon, which, when cured by that process, is not so liable to become rusty, as when done by the usual method of rubbing it with salt, and yet is more effectually cured.

The solid salt may be procured in any quantity or of any size of Messrs. Londons, at the salt pit, Norwich, Cheshire; Messrs. Smith, Marten, Smith and Co. America Square, London; or Messrs. Whitehouse and Galan, Liverpool.

*** At the examination of Mr. London's pickled mackerel before the Committee, they were of opinion, that the best method of rendering them useful to the lower classes of people, would be by preparing them with potatoes in the following manner:—The raw potatoes to be scraped and boiled, and when nearly boiled sufficiently, one or more of the pickled mackerel to be then laid in the pan upon the boiling potatoes, and the boiling process continued till the mackerel is properly done, when the mackerel and potatoes are to be taken out of the water for use. On this plan, the potatoes will be rendered very palatable by the salt extricated from the boiling mackerel, the

mackerel

mackerel become tender and nutritious, and the mixture form a valuable and cheap diet.

The sprats will also answer prepared in a similar manner with potatoes.

XIII. *Some further Particulars respecting* WOOLF'S *Patent Boiler. By the* EDITOR.

IN our seventeenth volume, page 40, we gave an account of Mr. Woolf's invention for producing strong and durable boilers for steam-engines and other purposes, with an extract from his specification, and a quarto plate; but we confined ourselves principally to his boilers composed wholly of tubes, and calculated to bear a very high pressure of steam. In the article alluded to, we informed our readers that Mr. Woolf had in his specification also stated that his invention was applicable to the boilers of steam-engines already in use, and that he had given in his specification illustrations of some of the methods by which this might be effected.

A correspondent having requested to be furnished with further information on this part of Mr. Woolf's invention, we willingly devote a page or two to the subject.—In his specification Mr. Woolf remarks, that "in cases where it may be wished to increase the power or production of steam without employing the very high degrees of heat, which boilers constructed on the principles before described, [viz. entirely of tubes,] are capable of sustaining, my invention may be applied to the common oblong boiler now in very common use for steam-engines." He then points out one method, by placing a row of tubes horizontally under and connecting them with the oblong boiler; and afterwards describes a method by which a very powerful oblong boiler may be produced, by dividing it into an upper and lower part, and connecting the two by vertical tubes. Of such a boiler we have given a vertical and horizontal section in Plate I. (fig. 2 and 3). The horizontal section is taken through the middle of the vertical tubes.

Mr. Woolf remarks that "from the examples he has given" in his specification "no person acquainted with the nature and uses of boilers can be at a loss to apply his invention modified according to circumstances to any case that may present itself." This is abundantly evident from the description and accompanying plate given in our seventeenth volume, and in the present short article: for instance, the benefit of Mr. Woolf's invention, so far as concerns an increase of surface and consequence of steam by means of tubes, may be secured to the common oblong boiler

boiler with the longitudinal flue, by introducing vertical tubes into the flue connecting the part above the flue with that below, by means of the said vertical tubes, in the same manner as the upper and lower part of the boiler, represented in Plate I. are connected with each other: or, if the boiler be constructed for the purpose, instead of one flue two flues may be carried through it with vertical tubes inserted in each; (see fig. 3:) and it will be advantageous that the vertical tubes should not range in a straight line behind each other, but rather take a zig-zag direction as in fig. 2. We are however of opinion, that this boiler, which would be more troublesome to construct than fig. 2 and 3, will not prove more efficient; for it is the vertical surfaces *opposed to the direct course of the flame and heated air*, that receive and transmit the principal part of the heat to the water, the vertical sides (which in this case form the sides of the flue) contributing but little to the effect, especially where there is room enough to construct a long boiler. There is not, after the furnace of a boiler is once fairly heated, so much waste of heat even by the sides of a brick-work flue as some may imagine; for the effort of the flame and heated air is to find the most direct course to the chimney; nor can any method better calculated to prevent this be easily imagined than the one pointed out by Mr. Woolf, namely, to interpose tubes in the way of the draft, that the flame and hot air may be obliged to impinge against them, and give out their heat before they reach the chimney.

XIV. *On the Metallic Salts.* *By* A Correspondent.

To Mr. Tilloch.

Sir,— The proper arrangement of the parts of a science is attended with considerable benefit towards its advancement, and the just definition of each particular part will not, I presume, be unattended by its corresponding utility.

To set a doubtful point in our present chemical nomenclature at rest, alone induces me to trouble you with the following brief remarks: not that I am an advocate for generally received opinions, or biassed by any particular theory, but solely actuated by a desire of eliciting truth.

Your correspondent H. p. 463, vol. xlv. of your valuable Journal, says "that he has continually observed metallic salts to have an excess of acid;" and concludes "that it is essential to them, and therefore, that there is no neutral metallic salt." Fourcroy corroborates this, when he says, in his admirable work, (p. 69, vol. v. Nicholson's Translation) that "the metallic salts have always an excess of acid," and H. endeavours with very ingenious

genious arguments to prove that position; but in this, however, he appears to me to have failed.

Admitting the correctness of his experiments, which, having tried them myself, I do explicitly, still I am inclined to consider that he has drawn wrong conclusions from the data before him, and that his reasoning thereon is fallacious and in some measure contradictory. He says that any dry metallic salt, moistened with water, and indicating acidity by the test, cannot in the dry state be considered neutral, it must in the dry state be an acid salt with excess of oxide caused by the evaporation of part of the acid in the process of drying and depositing its oxide upon solution:—Can the existence of a salt be imagined with each of its constituent parts in excess (*i. e.* acid and oxide)? Certainly not.

I cannot conceive any period during the formation of a salt, that the action towards chemical union would cease, whilst a portion of both its constituents remain in contact and uncombined; though it is easy to conceive that either one of the component parts may preponderate as in the super and sub salts. It is impossible to try the experiment in a direct way, the salt being in a dry state or combined only with the water of crystallization; when, therefore, we cannot refer to direct experiment, we must be contented in drawing conclusions from analogy.

Metallic salts are generally, if not universally, more soluble according to the greater proportion of free acid, and it is absolutely necessary to have a little excess to induce solution: this is not only the case with metallic, but also obtains with many of the earthy salts, such as the sulphates, phosphates, &c. And were his argument allowed, that the salt was not neutral, because it required excess of acid for its solution, these also would be considered super salts: now, it is not doubted that sulphate of lime is not neutral, because it is scarcely soluble without excess of acid. But, he says, before the free acid can be neutralized by an alkali, the oxide is precipitated: this is indeed the fact, and did it not take place there would be no solution: it only proves, therefore, that its tendency towards solution decomposes part of the salt to support the remaining part in the water.

Besides, the very evidence both of acid and oxide upon solution clearly proves that they were actually combined, and formed part of the salt when crystallized, and not caused by the evaporation of part of the acid in drying: were this the case, there would then only be a superabundance of oxide, and consequently, rather a sub than a super salt.

I am decidedly of opinion; First, from the acid or super-salts being more soluble than the neutral or than the sub-salts; Secondly, from the salt dissolving without any precipitation on the affusion of acidulated water; and Thirdly, from the decomposi-

tion

tion of part of the salt, and consequently a deposition of oxide to afford a sufficient quantity of free acid to induce solution; that it is rational to conclude, that the free acid in those metallic salts now called neutral, does not in the least militate against their neutrality when crystallized.

With these observations, which I submit to the consideration of more able proficients, I trust, the correctness of your correspondent's opinion or mine will be so established as may be most for the advantage of the chemical nomenclature.

I am, sir,

Yours obediently,

Stoke Newington, July 15, 1815. G. S.

XV. *Experiments introductory to an Attempt to exhibit the comparative Anatomy of Animals and Vegetables.* By Mrs. Agnes Ibbetson*.

To Mr. Tilloch.

Sir, — I have had the happiness to make what appears to me rather a curious discovery, respecting the brains of animals in general, which I hope and trust in this age of inquiry may be of some use in clearing up a little of the darkness that still exists respecting the powers and formation of the brain; especially its connexion with the *nerves* and *muscles*, and the consequent effect *arising to them*.

Having been some time employed in studying *comparative anatomy*, merely for the purpose of enabling me to draw up a comparison between the *vegetable* and *animal* frame, and having at last conquered it sufficiently to send you the sketch you will receive with this letter, which in some measure (though poor) will, I flatter myself, show the exact powers of *plants*, I went to Dawlish to recover the effects of a *too severe study;* but resolved to make use of that opportunity to examine the *Zoophytes.* While dissecting them, I remembered to have heard that it was once supposed that the brain might possess a sort of vapour; and it occurred to me to try. Finding in the anemone a quantity of *brain*, taking a small quantity of it on the point of a knife, placing it on the glass of my double compound microscope, and applying the second power; how great was my astonishment to perceive a strong vapour flow from it, so strong as to

* We have received the enlarged details alluded to in this introductory letter, and shall feel much pleasure in laying them before our readers next month. In the mean time we hasten to comply with our learned and indefatigable correspondent's request, by giving publicity to her important and valuable discoveries in their present shape.—Edit.

cover

cover the glass with bubbles of air! It continued to flow for about a couple of hours, then gradually lessened; but that change seemed rather to be occasioned by the *drying up* of the *medium* through which the air was seen, than the decrease of it; for as soon as a little water was added with the finger, the air began again to flow, and continued a couple of hours more; then died away. But as I depended on knowing the brain of the anemone by its *peculiar conformation*, I thought it better to try a creature that had a decided *head* and brains; and therefore took the prawn, the shrimp, the lobster. The two first gave much air, but not so much as the anemone; the lobster gave more, and had some curious particulars respecting it. However, I was not yet quite satisfied; but, the moment I returned to Exeter, contrived to get my butcher to send me a *sheep's head* the moment it was killed:—I had it within half an hour. I took a small slice not so thick as a card, and placed it on the glass of my *compound microscope*, under a *double power*; there was liquid *enough* to show the quantity of air that escaped for the first *two hours*, and it could be compared to nothing but an apple when placed in water under the receiver of an air-pump, which by pumping released the air within. I afterwards tried the brain of a pig, a lamb, and many kinds of fish; it was all the *same:* only that the animal gave most, and that the mackerel, &c. had a triangular bladder: I cannot help thinking it *vapour*, rather than *air only*, as after the bubbles have run from small to large, and burst,—they leave a very diminutive *speck behind*, which appears to me *to be matter*, remaining after the decomposition. A number of *gentlemen*, *physicians*, and *surgeons*, have seen it: some suggested it might occasion the *vis insita* of the muscles; but this cannot be, since in the account I here add, the same *vis insita* is found in the muscles of the plant; and they have neither *nerves* nor *brain*. But I will venture to add one idea which has occurred to me,—whether the sudden stopping of that vapour by a concussion of the brain, might not occasion the total torpidity of limb, and a loss of the organ belonging to that *side of the brain*, thus injured. Or irritating the brain might increase the *quantity* of *vapour*, and thus produce convulsions to the corresponding limb. But this is a consideration for wiser heads than mine, and to the order of physicians and surgeons I leave it, well assured that they will make use of every accidental fact thus presented to them. Perhaps no study has proceeded further in *practical utility* and scientific research than anatomy; and to no order of men are we so much indebted for the alleviation of the sufferings of mankind, and to whom I feel myself peculiarly happy in dispensing, though from an *obscure individual*, my humble meed of *admiration* and *praise*. No other

other part of the head gives a vapour, and I know not any other part except the lungs from which air may be drawn in this peculiar manner. That Cuvier was perfectly wrong in supposing this part to be the *intestines* of the *zoophyte* is certain; the brain has that peculiar shape which no other part in the smallest degree resembles, and which is alike in all animals, insects, zoophytes, &c. I was indeed most anxious to prove whether the *actenia* possessed a *brain* and *nerves;* for I never yet *saw* or *dissected* an *insect* (ever so small) torpid or insignificant that had *not* those *parts;* or any being that deserved the name of a *feeling animal*, which had not both brain and nerves; for surely the being that is *sensible* of *pain* or *pleasure*, must have those *parts*, from which those *feelings result*; and as there is in each part, that peculiar *mark*, which distinguishes them when seen, they cannot be mistaken: thus the muscle is known by its *contracting power*, by its *increasing hardness* when *cut*, and by its *vis insita* possessed by no other part of the body, in either *insect*, *animal*, *plant*, or *human being*. The nerve is known by its being that remarkable link which is the *source* or *vehicle* of *communication* between the *brain* and *muscle;* and serving probably as a passage for that gas, which may produce so many curious phænomena in the human frame, while the brain is that peculiar formed matter which in every creature that *feels*, has the same appearance and consistency, and may always be likened to a great fat worm.

I am, sir,

Your obliged servant,

Exeter, July 19, 1815. AGNES IBBETSON.

XVI. *On Citric Acid.* *By* SAMUEL PARKES, *F.L.S. M.G.S. &c.**

THERE is a peculiar acid in the juice of lemons, citrons, limes, and a variety of other fruits, different in some of its properties from all others, and known to chemists by the name of citric acid. The ancients, it appears, made no use of the juice of these fruits except as an antidote against poison. Formerly the citric was supposed to be identical with the tartaric acid; but the citric acid does not decompose muriate of potash, nor sulphate of lime, like oxalic acid. Georgius in the Stockholm Memoirs proposed the separation of this acid from the mucilage of the juice by cold; but the ingenious Scheele was the first who exhibited this acid in a solid form. To accomplish this,

* From the "Chemical Essays," in 5 vols. 12mo. just published; and of which we shall give an analysis in our next.

he

he adopted merely the same method which he had employed to purify and crystallize the tartaric acid. It consists in separating the real acid by means of carbonate of lime, and then decomposing the citrate of lime by the intermedium of diluted sulphuric acid; a process which has since been followed throughout Europe; for we have no other means of putting this acid into a crystalline form. Having myself had some experience in this process by frequently superintending its progress, and more than once to the extent of several hogsheads of juice in one operation, I shall now, with some confidence in their correctness, proceed to give what appear to me to be the best directions for obtaining a successful result.

The juice, which is imported in pipes and hogsheads, should be drawn off into a large open vessel; and a square vat made of fir is very proper for the purpose. In this vessel the juice is to be saturated with lime, by the gradual addition (lest the effervescence by the disengagement of carbonic acid should occasion an overflow) of clean soft chalk, or whiting. Care must be taken to note down the quantity of carbonate of lime employed, which should be added till it occasions no further effervescence, and until the liquor shows no signs of acidity on test paper. When there is room, the better way would be to put the intended quantity of whiting, or nearly the whole of it, all into the vat at first; to wet it well with water, and then to pump in the acid by degrees upon it; for in this way the effervescence is less, and the operation more manageable. If the cask containing the juice be rolled near to the vat, a small hand pump (of copper should be preferred) may be put in at the bung-hole; and while one man is slowly pumping it into the vat, another should not cease to agitate the whiting; and whenever there is any danger of the mixture rising too high in the vessel, the operators will have only to rest a moment, and all will be safe. By this mode the saturation will be complete, and it will be done in the least possible time. In this process all the real acid of the lemon juice is absorbed by the calcareous earth; and when the combination is completely formed, the citrate of lime, which is an insoluble salt, precipitates to the bottom of the vessel. When the whole of this has fallen, the supernatant liquor, which is nearly tasteless, will be found to contain nothing besides the mucilage, except a portion of the essential oil of the rind of the fruit, some extractive and saccharine matter, and a little malic acid *. This supernatant

* Were it deemed necessary for any part or the whole of the *malic* acid to be saved, it can readily be distinguished from the citric by the following properties: The malic acid does not crystallize; it precipitates silver, mercury, and lead from their solutions in nitric acid, while citric acid has no such effect; and it forms a *soluble* salt when combined with lime.

pernatant liquor is therefore to be drawn off*, and the precipitate preserved for use.

When the supernatant liquor is removed, the precipitate, or citrate of lime, is to be passed through a sieve, and then frequently washed with warm water, till all the remaining mucilage and other soluble impurities be entirely washed off. This will be known, when the whole has remained at rest for some time, by the water coming off clear and tasteless. The precipitate having been thus well washed, let it be treated with diluted sulphuric acid, in the following manner: For every ten pounds of chalk or whiting, which has been employed in the process, it is necessary to take the quantity of nine pounds of the best oil of vitriol of commerce of the specific gravity of 1·84, or 1·845, which is to be diluted with about seven gallons, or 56 lbs. of water †. This diluted sulphuric acid is to be gradually poured upon the citrate of lime ‡, and the whole mixture well stirred, for a considerable time, with a strong wooden spatula, that there may be no chance of any of the citrate remaining unbroken, and consequently unacted upon by the mineral acid. The more effectually to guard against this, it is advisable, just before the last portions of oil of vitriol are added, to pass the whole mixture a second time through the sieve: otherwise, notwithstanding the utmost care, some lumps of citrate of lime may still remain unpenetrated by the sulphuric acid, which would occasion a loss, and probably deception.

In conducting this part of the process, I have noticed an appearance worth mentioning to the practical chemist. It being necessary to stir up the mass very frequently during the gradual addition of the sulphuric acid, I have observed that whenever the citrate of lime became nearly all decomposed, and the earth mostly saturated with sulphuric acid, the sulphate of lime precipitates quicker and quicker after every addition of this acid, and that the supernatant liquor considerably increases in quantity. The latter circumstance is occasioned by the increased density of the precipitate; and both appearances should always be looked for, as they afford a good criterion for judging of the completion of the operation. It is however proper to remark,

* For this purpose some use plugs at different heights in the vat; but a syphon with a stop-cock is better, as with it the liquor may be drawn off perfectly close, and without any danger of accident.

† Proust employed diluted sulphuric acid of 1·150 sp. grav. But on looking to my table of the sp. grav. of diluted sulphuric acid, (see Phil. Mag. Sept. 1812.) I see that four parts water will reduce the strongest sulphuric acid of commerce to that sp grav.; and I have found that this is far short of the dilution necessary to produce the best result.

‡ Some apply heat in this case, but it is superfluous, as that evolved by the mixture of water and sulphuric acid is quite sufficient to decompose the citrate.

that

that it would not be prudent for the operator to rely solely on these appearances; for, if he is to ensure the best possible management of the process, he must have recourse to the agency of chemical tests, as they will afford the only sure criterion by which the real state of the saturation of the lime can be known.

In many chemical operations, preparations of barytes, or a simple solution of this earth in water, are employed to detect the presence of sulphuric acid. But as citrate of barytes would immediately precipitate on the addition of barytic water to a solution of citric acid, this reagent cannot be conveniently employed to show when the exact quantity of sulphuric acid has been used. It is necessary, therefore, to have some other test, and this may be found in the joint employment of acetate of lead and nitric acid: and although this might have suggested itself to any chemist who had thought on the subject, I confess I am indebted for the idea to a person who has for several years done much in the business, and who tells me he has long employed these two articles for this purpose with perfect success. Whenever the saturation of the citrate of lime is nearly completed, and the operator is fearful of supersaturating it, all that is necessary is to take out a little of the clear liquor after the precipitate has subsided, to filtrate it through paper, and then treat it with a few drops of acetate of lead. In this instance a citrate of lead will be formed and precipitate, which precipitate will be immediately redissolved on the addition of a little of the nitric acid. On the contrary, should too much sulphuric acid have been used, there will be a mixed precipitate of citrate of lead and sulphate of lead; and the latter of these salts not being soluble in nitric acid, leads to the detection of an excess of the decomposing acid.

But to return to the process. The consequence of this affusion of the citrate of lime with sulphuric acid is, that the latter having a more powerful affinity with the lime, it immediately combines with that earth, and expels the citric acid, which will then be found in the supernatant fluid. The insoluble part of the mixture is now sulphate of lime; and when sufficient time has been allowed for the whole of this to precipitate, the fluid, which is the pure citric acid in a state of solution, may be drawn off for use. In order to procure the whole of the acid, more water is to be poured on the sulphate of lime *, and repeated time after time, till the last portion comes off clear and tasteless. The whole of these washings are to be put together, and

* In opposition to some directions which I have seen, I would advise that *cold* water be employed for these washings, as it will separate the fluid citric acid from the precipitate, equally well with hot water, and will dissolve less of the sulphate of lime, which is a substance very unfavourable to the success of the process of crystallization.

the mixed liquor is afterwards to be concentrated by evaporation. Where the citric acid is made as an article of manufacture, a boiler of lead* will be the most convenient for this purpose. In this the acid may be kept continually steaming without any danger, until it acquire the specific gravity of about 1·130. It will then be prudent to withdraw the fire† entirely, and proceed to remove the acid into a smaller vessel of lead, which should be so fixed within an iron pan of boiling water, as to form a complete *balneum mariæ*. In this bath the fluid may be further concentrated by steaming, until it become so much reduced in bulk as to render it necessary to remove it into another water-bath still smaller, but constructed in the same manner as the former. In this last vessel the liquor is to be further evaporated until it acquire the consistence of very thin molasses, and then it should be watched with the greatest attention for the pellicle to appear, which may now be expected to arise on the surface. This pellicle will first show itself in small detached spots, and by degrees will increase in size and number till the whole of the liquor is nearly covered with them. It should then be immediately taken out of the bath, and laid aside to cool and crystallize‡. It is of the utmost consequence to attend to this circumstance; for so much of the fluid is evaporated before the pellicle appears, that, if it were to remain half an hour longer, the whole might be carbonized, and reduced to a dry mass.

It is likewise proper to remark, that however carefully the process may have been conducted, the whole of the calcareous earth will not precipitate totally while the supernatant liquor is loaded with the citric acid; for we know that this is capable of holding the citrate of lime in solution§. To remedy this, therefore, it will be requisite, as the acid becomes concentrated, to

* The bottom of this boiler should be made of one continued sheet of lead, not less than ten pounds to the foot, though five-pound lead will be sufficient for the sides. But where saving of expense is not the first object, it is best to have the vessel of one entire sheet of lead, by which all solder may be avoided.

† In building the fire-place, it should be entirely covered with a continued row of iron plates, which are to be supported at each end by the walls of the fire-place. These should not be less than one inch thick; and if they are cast, only six inches broad: it will be less expense to repair them whenever any of them are worn out by the action of the fire. It is by these plates the leaden boiler is to be supported.

‡ When removed, it should remain undisturbed for three or four days, that the largest possible quantity of crystals may be obtained from the liquor. After this time has elapsed, the mother-water may be drained from them, as, after that period, it will be in vain to expect a further increase without a further abstraction of water from the mothers by a fresh evaporation.

§ Dize having dissolved 100 parts of crystallized citric acid in pure water at 212°, found that it would take up 50 parts of calcareous citrate. *Journ. de Phys.* 1794, tom. ii. page 231. Hence some nicety is required in separating the lime, without adding an unnecessary and injurious quantity of sulphuric acid.

add

add occasionally a small portion of diluted sulphuric acid*, which will arrest the lime, and precipitate it. To this end, the steaming should every now and then be discontinued for some hours, to allow time for the sulphate of lime to separate, which otherwise would prevent the crystallization of the pure citric acid †; for Crell has shown, that a very small quantity of calcareous earth will have this effect ‡. Where the product is designed for crystallization, it will not be necessary to attend much to the temperature of the room; for I have found that the crystals shoot equally well in a warm as in a cold situation.

The crystals thus produced will sometimes, though not generally, be needle-formed §; and, as the process is usually conducted, they will be nearly as dark as the brownest sugar. Indeed, every writer on citric acid, whom I have consulted, tells us, that the first crystals will be either dark brown, or black; but I am confident, if the article has not been burnt during the evaporation, that this is chiefly owing to the mucilage not having been sufficiently washed out of the citrate of lime before it is treated with the sulphuric acid; for I have more than once seen crystals of the first crop, of a very bright light brown, and even distinct in their form; and such I conceive they always will be when the proper precautions have been taken. However, though the crystals in any of these states may very well answer most of the purposes to which they are applied by the calico-printer, it is sometimes desirable to have them quite pure; and by dissolving and crystallizing the acid three or four times successively in pure water ‖, very white and beautiful solid crystals of citric

* A single drop of this acid will be a sufficient test, at any time, to show whether the concentrated citric acid holds any citrate of lime in solution.

† A small excess of sulphuric acid is not injurious, but will rather favour the crystallization of the citric acid, though, perhaps, only inasmuch as it tends to destroy the mucilage.

‡ In opposition to this, Westrumb says that most of the citric acid, even when crystallized, contains a considerable quantity of lime. However, where this is suspected, it may quickly be known by dissolving some of the crystals in water, and, when the solution has been saturated with ammonia, by treating it with an oxalate of that alkali, which, if there be lime, will immediately separate it in a palpable form.

§ It has been repeatedly said by the writers on citric acid, that the crystals produced by the first evaporation are always needle-formed; but I am inclined to think that this will never be the case if the citrate of lime has been properly freed from the mucilage, and the solution of citric acid not evaporated beyond the due point of concentration, before it be laid aside for crystallization. Indeed, by proper management, I know that perfectly formed rhomboidal crystals may always be procured from the liquor of the first boiling.

‖ The most œconomical way of conducting this part of the process is to employ very little more water than is absolutely necessary to dissolve the crystals, and then allow sufficient time for the impurities to subside before the solution is again set apart for crystallization. It would add to the perfection of the crystals, in point of figure, were not only the first, but every subsequent solution passed through a clean leather filter, previous to its being allowed to shoot.

 acid

acid may at any time be produced *, and if the first solution of the brown crystals be passed through a skin of wash-leather, stretched upon a frame, so much of the colouring matter will be abstracted that two or three solutions in clear water will be sufficient to give crystals of the desired purity.

There is another point respecting the conducting of this business which deserves our notice, and that is the management of the mother-waters. In the manufacture of every kind of salt, the profit very much depends upon the œconomy which is observed in reducing these to a solid or a marketable form. But in this article, the high value of the crystals, and the peculiar difficulties attendant on the reduction of these residuary liquors, render this part of the subject important to those who operate on a large scale. Whenever this acid is first boiled to a sufficient strength for crystallization, the small excess of sulphuric acid carbonizes a portion either of the acid or the mucilage; and therefore, when several parcels of juice have been concentrated, there will be a considerable quantity of very black uncrystallizable mother-liquor. This kind of residuum has given much trouble to some operators, and it has often been thrown away as useless. By the following management, however, the whole of the citric acid which it contains may be turned to a good account, and produce fair and clean crystals. The process is as follows:

Whatever the quantity of mother-liquor may be, let it be diluted with ten or twelve times its measure of pure water, and the water intimately mixed with it. Then add whiting to it, by degrees, till it is completely saturated, and proceed with it in all respects as before directed, and as if it were fresh lemon juice. Here it will be necessary to wash the product of citrate of lime with several successive portions of water; and if this be attended to, the crop of crystals which it will yield will be as fine as any before obtained.

On referring to my notes, while operating on some large parcels of lime-juice, I find the following entry, which is given in the hope that it may furnish some useful hints to those who have had less practical experience, and enable them to avoid some errors into which I had fallen before I had that knowledge of the process which I have since acquired. Three casks of lime-juice, which measured together 270 gallons, were drawn off into a large wooden vessel; and to this were added 118 lbs. of best whiting, which completely saturated it. When the citrate of lime had precipitated, the supernatant liquor was taken away

* Though the first product may be in needle-shaped crystals, the acid when further purified will always appear in rhomboidal prisms.

and

and reserved for another purpose*. The precipitate was then carefully washed with hot water from the worm-tub of the laboratory still, and this was drawn off the next morning. It was afterwards washed several times with cold water; in all seven times, the first washing only being with *hot* water. The water which was drawn off at the third washing was considerably coloured. While the citrate of lime was precipitating completely, after the last washing, and I allowed it due time to subside, in order to separate as much as possible of the water which rested on its surface, I drew off into another vessel the contents of five pipes more of lime-juice, which in the whole measured 450 gallons. This quantity required 149 pounds of whiting for its complete saturation†.

The precipitate now obtained was treated and washed with the same attention and care as the former, to separate the whole of the malate and acetate ‡ of lime, the extractive matter, and the mucilage. Both the precipitates were then removed to a leaden cistern, and 500 lbs. of soft water were poured on them, which enabled us very thoroughly to mix the whole. 240lbs. of oil of vitriol of the specific gravity of 1·845 were now diluted with 256lbs. or 32 gallons, wine measure, of soft water, and this diluted sulphuric acid was immediately, whilst hot from the mutual action of the two fluids on each other, gradually poured in successive portions into the leaden cistern upon the above-mentioned mixture of citrate of lime and water. While this was doing, two of the workmen never ceased stirring up the whole with large wooden spatulas, the more effectually to prevent any part of the citrate from escaping the action of the sulphuric acid.

Notwithstanding this, I perceived that the precipitate conglomerated very much on every addition of the sulphuric acid; a circumstance which was occasioned, as I afterwards discovered, by using this acid in too concentrated a state. There is besides another evil attending this; for as sulphuric acid, fully concentrated, will convert the citric into acetic acid, there will always be a danger of lessening the quantity of the produce, when

* It was designed to separate the water by freezing or evaporation, and then treat the remaining malic acid and mucilage with nitric acid, with the view of obtaining oxalic acid.

† Lime-juice is generally stronger than that of lemons; and when it is of the full standard strength, it will take half a pound of chalk to every gallon of the juice.

‡ It may be worth remarking, that ordinary juice not only takes less lime, but that sometimes even a considerable portion of what is used does not remain in the vat, for it is drawn off with the malic and acetic acids; the malate and the acetate of lime being both soluble salts. Hence the necessity of always employing a chemical test to determine the quantity of sulphuric acid required for the subsequent operation.

the oil of vitriol is not sufficiently diluted before it be employed. Indeed, from the last operations which I superintended, I am led to believe that it cannot be œconomical to use this acid in a higher state of concentration than one part of the sulphuric acid of commerce to six parts of water. The whole quantities used, were lime-juice 720 gallons, carbonate of lime 267 lbs., sulphuric acid 240 lbs., water (besides the quantity which was before in the nitrate of lime *) 756 lbs.

Several other parcels of citrate of lime were afterwards treated with sulphuric acid properly diluted, and these inconveniences avoided; but it is not in my power to furnish the amount of the crystals that were produced from any of these parcels, as I disposed of my share of the whole of the citric acid in a state of solution, and the task of concentrating it for crystallization fell into other hands.

From other experiments, however, I have reason to conclude that 20 gallons of good juice will generally give 18 lbs. of dry citrate of lime, and that if the whole process be well conducted, these 18 pounds of citrate will furnish 10 pounds of good white crystals of citric acid †.

A different method has been proposed by Richter for procuring the crystallized citric acid; and the author of the process says (Mr. J. Thomson's Notes to Fourcroy) that the crystals produced in this way are much finer than those which are furnished by the old method. It consists in saturating lemon-juice with potash, and in adding to the filtrated solution, a solution of acetate of lead so long as any white precipitate continues to be formed.

In this process the citric acid unites of course to the metal, and the acetic acid with the alkali. The citrate of lead, after being well washed with water, to separate any of the acetate which may adhere to it, is to be digested with a sufficient quantity of diluted sulphuric acid, and stirred frequently. During this digestion sulphate of lead is formed, and the disengaged citric acid exists in the supernatant liquor. By evaporating this with a gentle heat, beautiful crystals of citric acid will be formed.

As the citric acid takes the lead directly from the acetous acid, the citrate of lead may be obtained by adding lemon-juice to the acetate of lead. But the mucilage and other impurities

* If the citrate be treated with oil of vitriol *immediately* after it has been washed, as is frequently the case, a dilution of three parts of water to one part of oil of vitriol may be sufficient, as there will be much water in a state of mixture with the citrate of lime.

† It has been stated that "the quantity of solid citric acid in a gallon of lime-juice varies from 14 to 18 ounces." I confess, however, that I have not been able to procure this proportion from any of the parcels on which I operated.

are

are not so completely separated, as when the juice is first saturated with an alkali.

I cannot speak of this process from my own experience; and knowing that the most beautiful crystals may be procured by Scheele's process, I have felt no desire of repeating this of Richter, especially as it is more expensive, and the use of lead is objectionable where the crystallized acid is intended for internal and domestic purposes.

Fourcroy proposed sending proper persons over to America, to collect the vast quantities of limes and lemons which are annually lost there; to saturate the expressed juice with chalk, and to send the washed and dried precipitate closely packed in barrels to France, where it might be decomposed, and the citric acid obtained pure. Whether any Frenchmen ever embraced this proposal, I have not learned; but I know that some few persons have gone from England to different parts of Italy for this purpose, and that considerable quantities of the citrate of lime have been imported from thence to Great Britain.

Having been introduced to a person, who some years ago formed an establishment of this sort in Sicily, I have learned several important circumstances relating to this business, which have never yet been given to the public. I have also been favoured with the perusal of a great part of the correspondence which passed on the occasion; and having obtained leave to print the whole, or any part of these letters, I shall subjoin such extracts as I conceive will be interesting to those who may be desirous of acquiring more information on the subject.

In the first letter, which is dated Messina, the 6th of September 1808, the writer says, "The time of pressing is generally in the latter end of the month of November or December, for till that period* the lemons yield little or no juice."

"The country round Messina consists of mountains of immense height, rising one above another, and thickly covered to the very tops with fruit-trees, chiefly olives and lemons, which render this place the very best in the world for procuring lemon-juice. The quay surpasses most others, extending for a mile and a half, close to the edge of which ships lie in 20 fathoms water. Formerly a range of superb houses, perfectly uniform, extended the whole length of this marino, or quay; but ever since the earthquake these magnificent mansions lie in ruins.

"As soon as the country people press the juice, they bring it in here† for sale. The buyers do not afford it warehouse-room, but roll it into the street, exposed to the weather and to the heat of the

* At this time of the year 200 lemons are generally required to furnish a gallon of juice: there are, in consequence, presses so constructed that they can squeeze many thousands of lemons at once.

† The farmer brings it to Messina in his own casks, and the merchant must provide casks to rack it into, when he takes it away.

the sun, where it remains till an opportunity offers for shipping it*. It is therefore not surprising that so much is imported that is musty and perished, and that the English merchants often find it so bad on its arrival in England as to create a difficulty in procuring for it even the amount of the import duty."

In a letter dated Messina, September 30, 1808, he says, "The pressing continues till the following March, the quantity of juice produced increasing every month, in proportion as the fruit become riper, and the necessity of gathering them greater. The juice is sold by the salm, which is a measure equal to 21 gallons English." Again, April 15th, 1809. "I have hitherto found no difficulty in saturating the chalk in any quantity; but the drying is extremely tedious and difficult; it requires very hot and dry weather, which we have had little or none of as yet. I have had parcels lie upwards of a fortnight before they became perfectly dry. When spread out for this purpose it occupies a vast deal of room; and when the drying places are all full, I am obliged to stand still, although I have procured the use of a vast terrace belonging to a convent not far distant. The citrate, when taken from the baskets in which I drain it, is of a consistence and grain as fine as butter, and so delicate of touch when about half dry, that if, as on some occasions, when the weather looked uncertain, it became prudent to remove it into the warehouse to be under cover, it could scarcely be touched without breaking and crumbling into dust."

May 8, 1809.—"I have had many difficulties to contend with in bringing the preparation of the citrate of lime to any perfection; and this chiefly in the drying it, an object of the most material importance; and so much have I been perplexed on this score, that I have felt my expectations quite damped upon the occasion. I observed to you before, that the weather had not been settled enough to admit of any attempt at drying out of doors; for the high mountains hanging over us are until the present month continually bringing down showers; and the atmosphere, during this time, is very unfavourable to drying†." "I therefore made use of the upper rooms of my house for this purpose, where I spread out the citrate, and constantly attended to turning it and exposing it to the air for two months: it now appeared perfectly dry, and as hard as in its original state of chalk; and wanting the rooms to bring forward more, I pro-

* Goods are laid in here by the merchant in anticipation of a demand, for it is not possible to purchase any quantity, whatever may be the species, at the moment you require them. The custom is to pay the sellers at the time of making the contract, one third of the amount in advance to enable them to bring their produce to market. You then make further payments in proportion as they send in their goods, till the whole be delivered, and the contract completed.

† These facts and circumstances are inserted to moderate the too sanguine expectations of adventurers.

ceeded

ceeded to pack it in large casks, to be ready for shipping. However, at the expiration of two or three days, when I began to fill up and cooper the casks, I found the article so heated that I was obliged immediately to turn it all out again to dry it better. The cistern* in which I work is capable of containing 12 pipes; but notwithstanding the large size of the apparatus, the effervescence occasions me much trouble; and this I attribute to the chalk being sifted to so fine a powder. Neither the monks nor any of the inhabitants seem to have the slightest curiosity to know what the article is, although, until it is dry, it continually occasions a most agreeable odour." "According to your directions, I have made inquiry respecting the fruit which grows on the coasts of Barbary, and I find there is a considerable quantity of lemons, but they yield very little juice, being of the bergamot species, and have very thick rinds†."

June 22, 1809.—"The storing of lemon-juice is here a most difficult and expensive thing, since the soldiers occupy all the places formerly used for this purpose, and also many of the convents‡, which have all very large repositories."

July 4, 1809.—"I have been under the necessity of hiring a large room, formerly the refectory of the convent, the door of which opens on the terrace where I dry. This serves also to store the citrate when dry, and at times, when the weather looks squally, to bring it under cover; a very necessary precaution in this climate, as the rain descends in torrents unknown to us in England, and would very soon wash away every thing; so that you see, this business, when conducted on a large scale, requires plans and precautions which nothing but actual operations can point out to us. With each shipment I have furnished the measures of the juice I have used, and the exact quantity of chalk employed in each batch, from a book I keep for this purpose; and the measuring, the weighing, and the whole process has been constantly under my own inspection; for without my presence the men will do nothing. This hot country destroys in a great measure the energies of mind and body."

* In another letter he says, "It is impossible at any price to procure a cistern of wood in this place capable of holding the necessary quantity. There is no wood to be obtained in the island fit for it, and much less could a workman be found who would be capable of putting it together. Even baskets for draining must be sent from England, as none but very small ones are to be had in Sicily, and those very poor and slender."

† The Baron Lahontan relates that there are citrons in some parts of Canada, which instead of a rind have only a single skin, and that the fruit is as wholesome as the root is dangerous, the juice of the latter being a subtile poison. He proceeds to say that in the year 1684 he saw an Iroquoise woman, who was following her deceased husband, swallow a portion of this juice, which produced instant death. See Lahontan's Voyages to North America, 8vo, 1735, vol. i. p. 250.

‡ These are chiefly decayed establishments, which derive a considerable part of their revenues from letting out large apartments for storehouses and other purposes.

June

June 22, 1810.—"I find as much difference in lemon-juice as in wine, and both have more or less body according to the particular soil on which the fruit grows. When fresh squeezed, all seems equally sharp and good. The hot weather, however, is the test, and much will not bear it; it often changes very quickly, and a mawkish vinegar-sort of taste alone remains. Though the early juice resists the heat the best, I can positively assert that, with the utmost care, there is no certainty of preserving its native sharpness in the hot months but by the addition of lime or some other agent."

From a table of the produce of this establishment in the years 1809 and 1810, it appears that 74,964 gallons of juice were used, and 35,017 lbs. of chalk, which gave 49,902 lbs. avoirdupois of citrate. The quantity of citrate produced by every lb. of chalk varied from 19 to 27 ounces, and from every gallon of juice from 8¼ to 12¼ oz. avoirdupois. The disparity in the products from the chalk is ascribed to the use of an equal quantity to every kind of juice, which was very injurious. Dize says that 64¼ lbs. of chalk gave him 20 lbs. of dry citrate; but I have never been able to procure this quantity, and I conceive that he had not freed it entirely from the mucilage by washing. Proust (who operated on the fruit of Andalusia) speaks of 30 oz. of dry citrate from a pound of chalk, and this, I am confident, is the utmost that can be procured.

Before I conclude this part of the Essay, it will be right to apprize those who may intend to embark in this business, that it will be necessary to send the whiting from England, as neither lime, nor chalk, nor any other suitable ingredient (the burning of marble being expensive) for the purpose is to be found in Sicily. In sending out whiting or chalk, it will be advisable not to rely entirely on one shipment; for, should the calcareous earth be lost, it is probable the whole purchase of juice would be spoiled before a fresh supply could be written for, and actually arrive in the island*.

[To be continued.]

* It should also be known that all the resident merchants have an inveterate jealousy against any new settlers amongst them, and therefore strangers will be disappointed if they go with an expectation of receiving the usual facilities in bill negotiations, whatever may be the recommendations which they may take out with them. Every article which is the produce of the island, must be paid for in hard cash, and the purchasers should be provided accordingly. Considering these, and some other difficulties, which I have already stated, (and I have felt no desire to conceal any circumstance, however unfavourable, which is connected with this subject,) I am inclined to think that, by purchasing lemon-juice in London or Liverpool, at favourable opportunities, citrate of lime might be made in England nearly as cheap as it could be imported from Sicily, with all the inconveniences and disadvantages which must inevitably attend its formation in a distant country. However, having stated the different facts, the reader will be able to form his own opinion.

XVII. *On*

XVII. *On the Ventilation of Coal Mines.* *By* A Correspondent.

The frequent accidents that have lately occurred in the collieries in the neighbourhood of Newcastle and Durham, in consequence of the inflammation of the carburetted hydrogen so abundant in the coal-mines of those districts, have justly excited the attention and sympathy of the public at large for the fate of the unfortunate sufferers.

It is extremely probable that the increase of accident arises from a growing difficulty in ventilating the coal-mines as their depth increases from the surface; an evil which must be unavoidably increasing in all old coal-fields throughout the kingdom. Unless, therefore, some radical mode of circulating an abundant supply of pure atmospheric air be devised, so as to replace daily and hourly the gas as it becomes generated, these explosions and concomitant scenes of misery and distress are more likely to increase than to diminish.

Some severe censures have lately appeared against the abilities and judgement of the directors of the principal coal-mines of the North. It is evident that these remarks have been hazarded by persons otherwise highly respectable for talent, but who seem little conversant with the practical details and difficulties of one of these works. Strictures thus freely and indiscriminately bestowed are not likely to be productive of much of that general benefit which it seems the wish of all to obtain.

Feeling with many others an earnest wish to diminish the chances of that frightful calamity which hangs over the finest coal-mines in the world, I submit to the public what seems to me a probable remedy.

Let every large colliery have an air-pit; the place and situation of it in relation to the pumping or winding pit or pits, I leave to the judgement of the viewer. In sinking, let the joints and openings be properly filled with mortar that will harden in damp situations so as to make it as air-tight as possible. Near to it place a steam-engine of about ten-horses power: this will work an air-pump or cylinder of six feet in diameter with a power of compression equal to half a pound upon the square inch of the piston. Let the air-pump be placed in the mouth of the air-pit, and the apparatus so contrived as to produce an exhaustion in the pit both in the ascending and descending stroke;—the mouth of the pit of course to fit close to the cylinder, to prevent the air from passing inwardly.

If this engine travels at the rate of 200 feet in one minute, it will

will pump out of the pit and its workings in that time 6000 feet of contaminated air: consequently an equal volume of pure atmospheric air will descend by the down-cast pit, and be circulated through the air courses.

If the extent of the under-ground workings be equal to a mile; 400 yards in the direction of the rise; and the height of the excavated opening, after allowing for the space occupied by the pillars in a vein six feet in thickness, be taken at four feet,—this will give a total excavation of 25,000,000 of cubical feet, or a magazine of air of this extent composed of the fatal gas, atmospheric air and carbonic acid gas: this quantity, though great, would by means of the engine be entirely removed in three days, and the void replaced by pure air.

I shall now proceed to show that a supply of 6000 feet of fresh air per minute, is much more than is necessary for the exigencies of the under-ground workings, so far as combustion and respiration are concerned.

According to the accurate experiments of Messrs. Allen and Pepys, a man requires 310 cubical inches of atmospheric air in one minute for the purpose of respiration. Let us suppose that one hundred men are necessary to carry on the work of the mine, and that 200 candles are required to give sufficient light to all the departments of the work: let the quantity of air necessary for each candle be taken at twice the quantity requisite for a man; let the complement of the horses be taken at 30, and the quantity of air necessary for their respiration be estimated as equal to that consumed by the men and candles. These items of waste taken collectively will not amount to more than 200 cubical feet per minute. Whereas the engine has powers to replace into the mine in the same space 30 times as much, or 6000 cubical feet.

As the subject occurs to me at present, only two objections are likely to be urged to this mode of ventilation; namely, its first cost and subsequent expense; and the probability of its being urged, that an upcast pit with a funnel will produce equal effects, and at a smaller expense.

If the former objection be urged, I hope it will occur to the coal-owner, that the investment of 1000*l.* of extra capital, together with the weekly wages of two boys and the consumption of 20 tons of small coal per week that would otherwise go to waste, or in the best situations be not worth more than 40*s.*, are matters of comparative small importance and expense compared with the ruinous derangement which takes place throughout the whole of the work when an explosion occurs.

If the second objection which I have anticipated be urged against

against my plan, and the merits of the upcast pit set in opposition to the powers of the steam-engine and air-pump, I hope it will be accompanied with satisfactory data to determine the question. At any rate it may excite discussion, and lead to experiments on the best mode of rendering perfectly safe to humanity and to the proprietors at large a highly important branch of our national wealth and comfort. C.

XVIII. *Report of the National Vaccine Establishment, for the Year* 1814.

To the Right Hon. Lord Viscount Sidmouth, principal Secretary of State for the Home Department, &c. &c.

National Vaccine Establishment, Leicester Square,
June 19, 1815.

My Lord,— The Board of the National Vaccine Establishment has the honour to report to your lordship, that a greater number of individuals has been vaccinated in the course of last year than the preceding; that several thousand more charges of vaccine lymph* have been distributed to the public, whence the destructive ravages of small-pox have been diminished.

It appears from the bills of mortality of London, that the deaths occasioned by small-pox have decreased in a larger proportion than one-fourth, six hundred and thirty-eight having fallen victims to that malady during the last year, eight hundred and ninety-eight during the former. Large indeed is this melancholy catalogue, which is attributable to the dissemination of variolous matter by a few interested individuals, who, from sordid motives, continue the practice of inoculating with small-pox virus, and diffusing this fatal disease through the metropolis.

With the view of augmenting the benefits of this establishment, the Board has lately appointed a class of extraordinary vaccinators, in addition to the stationary surgeons of respectability, who have voluntarily stepped forward to contribute their assistance gratuitously, compose this class, from which it is intended hereafter to elect the stationary vaccinators.

Another class, denominated corresponding vaccinators, has also been established, from which a very material extension of the benefits to be derived from the Vaccine Institution is confidently expected. Each person will in his own neighbourhood be a point, from which the practice will continually diverge, and through whom any communication of importance may at once be made to this Board.

* At the different stations 4,686 persons have been vaccinated, and 32,199 charges of lymph have been distributed.

The

The stationary and extraordinary vaccinators must reside in London, or the suburbs; but the corresponding may live at any distance, or in any part of the world.

The official communications from the Medical Colleges of Edinburgh, Glasgow, and Dublin, evincing their confidence in vaccination, and the annihilation of small-pox in the settlements of the Cape of Good Hope and of Ceylon, by its introduction, as formerly reported, have been insufficient to convince some individuals of the security against the infection of small-pox; but it is to be hoped that the strong additional facts hereafter stated will produce the fullest conviction of its benefits in their minds.

From the official documents transmitted by the Right Hon. the Secretary of State for Foreign Affairs, to this Board, respecting the effects of vaccination in the islands of Mauritius and Bourbon, it appears that the inhabitants have been secured against the visitation of one of the severest scourges incident to the human race, as the sequel shows. In the year 1728, the small-pox swept off nearly one-half of the population; in 1756, about one-fourth; in 1771 and 1772, it occasioned a comparatively less, though very great mortality; and in 1792 it destroyed one-third; and of those who survived the disease, one-third lingered out a short and miserable existence, afflicted with dropsy, marasmus, consumption, &c. It is worthy of remark, that in three times out of the four, the disease was introduced by slave ships. Let the contrast now be drawn between the introduction of variolous infection and vaccine inoculation.

In 1802, vaccination was introduced from the British possessions in India, but its general use was prevented by the prejudices of the people, and the lymph, after a short time, could not be procured. In 1805, it was re-introduced, and the French government, seeing the necessity of regulations, framed some accordingly; but vaccination was only partially adopted, for it did not exist in many parts of the island when the British took possession of it. In 1811, the small-pox re-appeared in the island, and about 220 persons became infected, of whom thirty died. The alarm excited by the progress of this disease, prompted his excellency Governor Farquhar to issue his mandate, compelling all the inhabitants to be immediately vaccinated; which energetic measure at once arrested the progress of small-pox.

In 1813 an opportunity was offered of putting to the test the security of vaccination, by a slave (who came from the island of Madagascar, and was afflicted with the confluent form of small-pox) having been landed and received into the hospital; many slaves and other vaccinated persons were exposed to the infection, but no one became the subject of the disease.

From the introduction of vaccination in 1802, to the 28th of February

February 1814, it is computed that 200,000 persons have been vaccinated; and the medical practitioners unanimously declare, that no instance has occurred of small-pox being contracted after regular vaccination.

In the island of Bourbon the calamitous effects of variolous disease, and the beneficial consequences of vaccination, though detailed in a more abridged form, as forcibly corroborate the utility of vaccine inoculation, from its having banished the small-pox from that settlement.

The National Vaccine Board cannot omit to direct your lordship's attention to the meritorious conduct and zealous exertions of private individuals, not of the medical profession, who have not only vaccinated many hundreds every year; but who have studiously maintained a supply of lymph in their districts for the perpetual renewal of vaccination. It has particularly noticed the ardent zeal of Mr. Ellis, of Rhos Farm, who has vaccinated several thousands in North Wales, without a single failure; and also of the late Rev. Mr. Holt, and the Rev. Mr. Finch, two of the earliest friends to the practice; who resided, the one in Buckinghamshire, the other in a populous part of Lancashire, and who met with undeviating success;—a success which puts to shame the negligent practice of some belonging to the medical profession. For, strange as it may appear, it is not the less true, that there are many places in the united kingdom, where, though vaccination is professed, it is in effect never fairly practised. Lymph, indeed, has been procured from the National Establishment, but it would seem only for present, and not prospective purposes; as no pains have been taken to preserve it by a succession of cases; and that too, where the population in many of the districts supplied, has exceeded some thousands, and even in some of these where county hospitals are established.

In many instances the Board has had reason to lament, that throughout entire districts the lymph has been lost altogether; and when thus suffered to be lost, there must surely be just grounds of suspicion that interested motives have actuated individuals to throw serious impediments in the way of the general adoption of vaccination.

In confirmation of this conclusion, the Board adds the plain and unaffected narrative of a person who, humbly situated in life, has in the true spirit of philanthropy been the instrument of dispensing the benefits of vaccination to an extensive neighbourhood, and of preserving a regular supply of lymph, with which many medical practitioners have been liberally furnished.

Mr. W. Jeffrey, of Cambus-baron, near Stirling, after reciting the history of his practice, which from the purest motives of benevolence he commenced under circumstances not necessary to be detailed, thus proceeds in his communication to the Board:— "Vaccine inoculation has acquired such a character for ten miles round where I dwell, that the natural small-pox is not heard of. In this village there is not one child which has not been vaccinated, (excepting in two families,) so universal has the practice become; and it is remarked by the inhabitants of the village, that the children are more numerous owing to their being vaccinated; and among the children that I have vaccinated, I challenge all the country round to produce any instance in which the inoculation failed to preserve them from variolous contagion, notwithstanding their being exposed to lying in bed, eating and drinking with those infected with the small-pox. I am much surprised when I hear of such backwardness in and about London to the performance of such a salutary benefit to the human race. If any one should speak against it in any village, or in the large parish of St. Ninians, he would expose himself to the contempt of all the people."

The National Vaccine Board has it in contemplation to enrol the names of such steady and exemplary friends, under the denomination of Honorary Vaccinators, as they cannot with propriety be included in the other orders; and it hopes to receive from this class a continuance of their valuable communications Though it cannot be supposed that any stimulus is required to incite the active labours of such highly commendable persons, yet the Board cannot in justice silently pass over such zeal without giving some signal mark of its approbation.

Notwithstanding the accumulated and accumulating proofs of the utility of vaccination, there is reason to apprehend that variolous inoculation will still be persisted in, whereby the baneful effects of small-pox must be continually propagated.

The Board has with regret to observe, that although the punishment of three months imprisonment was awarded against Sophia Vantandillo, for carrying her child whilst under the influence of small-pox through the public streets, (which infected many others, eight of whom died,) the unwary and uninformed are still enticed by the hand-bills of shameless empirics, to submit their children to variolous inoculation. It is however yet to be hoped, that the above sentence so recently passed by the Court of King's Bench, which the Board of the Vaccine Establishment has taken every method of promulgating, may produce considerable benefit. But if inoculation of small-pox be permitted, the promiscuous intercourse of the infected with society at large, ought to be as speedily as possible prevented, and a receptacle* established, to which the diseased should be immedi-

* The Small Pox Hospital has been lately purchased, for the use of the sick poor afflicted with fevers.

ately

ately removed; for the narrow alleys and confined courts in which most of the poor reside, must tend to concentrate contagion, to render it extremely virulent, and eventually to disseminate this disease under its most malignant form.

The Board selected Sophia Vantandillo as a proper example, on account of the extent of the mischief occasioned by her misconduct; and that this prosecution, followed by a lenient punishment, may prevent any further wilful exposure of inoculated persons, is its fervent wish. It at the same time prosecuted Mr. Burnet, who inoculated the child of Sophia Vantandillo, and who has long circulated the most mischievous and offensive hand-bills, offering to inoculate persons with small-pox gratuitously, and stigmatizing vaccination as productive of the most loathsome diseases.

This practitioner, having suffered judgment to go by default, has been recently sentenced by the Court of King's Bench to six months imprisonment.

The Board has a duty of the highest nature to perform;—and that it is resolved to discharge faithfully and energetically.

The Board has endeavoured to form a system, regular and consistent in all its parts, conducing uniformly to one main end, namely, the universal adoption of the practice introduced by the immortal Jenner. It entertains the confident expectation that so great a blessing will be no longer under-valued, and that the labours of the good and powerful will not be rendered impotent by the ignorant and the interested. It trusts that the wisdom of Parliament will not be set at nought by the most unfeeling and worthless of the medical profession, and a disease even more destructive than the plague, allowed to be fostered by them with impunity, and continually propagated among the unsuspecting multitude of the united kingdom.

The whole of the expenses incident to this establishment, for the year 1814, were defrayed by the vote of Parliament which passed last year; but the Board regrets, that in consequence of the recent prosecutions and convictions of the persons mentioned in this Report, and the measures adopted for the more effectual extension of the practice of vaccination throughout the empire, an addition of five hundred pounds to the annual grant will be necessary.

J. LATHAM,
(President of the Royal College of Physicians) President.
WILLIAM BLIZARD,
Master of the Royal College of Surgeons.
HENRY AINSLIE, M.D. JAMES HAWORTH, M.D.
THOMAS HUME, M.D. HENRY JAMES CHOLMELEY, M.D.,
Censors of the Royal College of Physicians.
HENRY CLINE, WILLIAM NORRIS,
Governors of the Royal College of Surgeons.
By order of the Board, JAMES HERVEY, M.D. Registrar.

XIX. *Notices respecting New Books.*

Mr. Donovan is preparing for the press, "An Essay on the Origin, Progress, and present State of Galvanism; containing Investigations experimental and speculative of the principal Doctrines offered for the Explanation of its Phænomena, and a Statement of a new Theory." This essay was honoured with the prize by the Royal Irish Academy.

Optical Outlines; or, A new Theory of Vision, Light, and Colours, with Experiments on radiant Caloric. By Joseph Reade, M.D. of Cork.

This work amongst others will embrace the following subjects: The present theory of vision;—retina not the seat of vision;—the real seat pointed out, and confirmed by conclusive experiments;—blackness a real colour caused by the condensed reflection of blue, red, and yellow;—primary colours;—the composition of blackness shown by analysis and syntheses of opaque paints, and also by analysis and syntheses of the prismatic rays themselves;—absurdity of visual angles taken from refracted foci;—a new theory of refraction;—yellow rays shown to be the most refrangible, and blue the least;—a new rationale of reflecting and refracting telescopes, &c. &c.;—incident light never yet decomposed;—reflected light alone capable of decomposition;—spectric phænomena;—spectrum not an image of the sun;—rationale of the prism, lens, &c.;—Sir Isaac Newton's theory of colours shown to be fallacious;—concentric rings, with the colours of thick and thin plates, soap bubbles, &c. investigated, and their real causes ascertained by numerous new and interesting experiments;—a new theory of the rain-bow, &c.;—colour of the clouds;—apparent distance of objects;—double vision;—radiant caloric;—observations and experiments on Dr. Herschel's investigation of the solar ray;—identity of heat and light.

Some copies will be published with coloured plates.

Dr. Spurzheim has just published a second edition of his work on the Anatomy and Physiology of the Brain, with considerable additions. The science of the organology of the brain is rapidly advancing, and every month some new cases of corroboration, or some new discovery, have engaged the Doctor's attention. The medical gentlemen who have given their time to this subject have many of them become very zealous pupils, and on the whole the science is rapidly gaining ground in Great Britain. Several Reviews have mentioned the work; but no arguments, according to the opinion of the craniologists, have yet been able to set aside the proofs of the truth of the theory.

XX. *In-*

XX. *Intelligence and Miscellaneous Articles.*

AGRICULTURE.

At the Holkham sheep-shearing, held at the beginning of this month, Mr. Coke, of Norfolk, alluding to that destructive insect the wire-worm, observed, that as rooks devoured great quantities of these insects, it was impolitic to wage indiscriminate war against these birds.

The Earl of Albemarle then communicated the following highly useful information:

"I think I may with confidence state, that oil-dust destroys the wire-worm.—I ground my assertion on the successful result of two experiments, one on turnips, the other on wheat. The first was an old sainfoin layer for turnips, a small part of which was sown with oil-dust, at the rate of half a ton to an acre, before the last ploughing for turnips.—When they came up, there was a perfect plant all over the field, but soon went off in every part except where the dust was sown; *they* continued to look strong and healthy. The portion which failed was ploughed and sown again with turnips—again the wire-worm destroyed them. They were, for the third time, sown with coleworts, which shared the same fate as the turnips—the dusted part remaining at this time uninjured, and produced a fine crop.

"In order," continued the noble lord, "more fully to prove that it was the oil-dust, and no incidental change of soil, which thus preserved my turnips, I must explain to you, that the dust was not sown as the steches were drawn out, but, most fortunately for the success of my experiment, *diagonally*. This direction was, in the first instance, purely accidental—I verily believe that you might see exactly the path in which the seedsman walked. —The next was a piece of wheat, which had also partially been sown with oil-dust. In the spring the wire-worms attacked it; but on examining the roots, I found five wire-worms dead in one piece of oil paste. It sustained but little injury, produced eight coombs per acre, and I have no reason to doubt was preserved by the oil-dust."

OXALIC ACID.

Although it is an unquestionable fact that all concentrated acids are destructive of animal life if taken in any considerable quantity; yet they have not in general been denominated poisons, owing, perhaps, to the vague application of this term. Some chemists have even ventured to deny their poisonous quality, notwithstanding the destructive powers of prussic and other acids; and Guyton Morveau has recently denied that the oxalic

acid

acid can act as a poison. The experiments, however, of Mr. A. T. Thomson, "Medical Repository," remove all doubts on this head. A solution of ten grains of oxalic acid thrown into the stomach of a rabbit killed the animal in a few minutes; a drachm administered in like manner to a dog, killed him in ten minutes. The appearances on dissection were a pulpy state into which the coats of the stomach were reduced. This state is ingeniously conjectured by Mr. Hume to arise from a decomposition of the phosphate of lime contained in the coats of the stomach. But however this may be, it appears undeniable that oxalic acid taken into the stomach acts as a virulent poison. Mr. Royston has related a plain case: "A woman took about four drachms of oxalic acid mistaking it for sulphate of magnesia. Excessive tormina shortly ensued, and in forty minutes after swallowing it she expired." A mixture of chalk and water, if taken shortly after the oxalic acid, proves an antidote, by forming an oxalate of lime in the stomach. The knowledge of this fact is much more important than may be generally supposed, as there is a number of persons in the habit of using a solution of oxalic acid regularly, supposing or alleging it to be a cooling beverage, an excellent remedy for the effects of wine or other stimulants, and imagining that it improves their complexion, and renders them proof against any contagion or infectious disease. Now that it is proved to be a potent poison, perhaps they may discover that its greatest virtue is in tempting the hand of death before nature designs it.

Oxalic acid is very frequently sold in the shops for citric acid; and as if to sanction this deception, it is vulgarly called the "salt of lemons," although sorrel and sugar with nitrous acid are the chief articles used in its formation. It crystallizes in slender four-sided rhomboidal prisms, bevilled at each extremity; the crystals are white, of an agreeable sour or tartish taste, and permanent in the air or slightly efflorescent. The crystals of citric acid are not so white, of a sharp acid taste, almost caustic, and permanent in the air, although inclining rather to deliquesce than effloresce. The crystals are rhomboidal prisms acuminated by four planes, and much more soluble in water than those of oxalic acid, boiling water dissolving double its weight of citric acid and only its own weight of oxalic.

DIAMONDS.

At the third annual course of lectures of the Birmingham Philosophical Society which closed a short time since, a very curious description and estimate of diamonds known to be in existence, were given by Mr. Thonson. The number of known diamonds of 36 carats and upwards, he stated to be no more than

than 19, two only of which were in England, the Piggott diamond weighing 45 carats, and worth 16,200*l.* and one in the possession of the Hornsby family of 36 carats, worth 8000*l.* Holland has but one, which weighs 36 carats, and is valued at 10,368*l.*; its form is conical, and it was for some time in the possession of Messrs. Rundell and Bridge, of London.—France has two; the largest was bought by the Duke of Orleans during his Regency, and thus called the Regency Diamond; its weight is 166½ carats, and value 149,038*l.*—Germany has one weighing 139½ carats, and worth 155,682*l.*—Russia is rich in these gems; its largest is that of the Sceptre, which is said to weigh 779 carats. If this be true, it must be worth, according to the general mode of estimating them, the enormous sum of 4,854,728*l.*—The history of this diamond is rather curious. For a long time it formed the eye of an East Indian Idol, from which post it was removed by a European soldier. From him it passed through several hands, and was finally sold to the Empress Catherine for 90,000*l.* a handsome annuity, and a patent of nobility.—Russia has several others, one of which is estimated at 369,800*l.*—The Great Mogul has one of a rose colour, and valued at 622,728*l.* The two principal ones belonging to Persia are called in the hyperbolical language of the East "The Mountain of Splendour," &c. and "the Sea of Glory:" one is worth 145,800*l.* and the other 34,848*l.*—The Portuguese Royal Family have two, one of which is still uncut; and, if we may credit the Portuguese accounts, is the largest ever found: it is said to weigh 1680 carats: and supposing it to lose half its weight in cutting, it would be worth 5,644,800*l.* upwards of a million more than the Sceptre Diamond of Russia. There is a small part broken off, which was done by the man who found it; who, ignorant what stone it was, struck it with a hammer upon an anvil. It was found at the Brazils. It must not be concealed that some persons conversant in these things doubt the existence of this stone. According to the model exhibited, it is somewhat like the shape and size of an ostrich's egg. The other diamond in the possession of the House of Braganza is worth 3,698,000*l.*

M. Van Mons, of Brussels, observes in a letter recently received from him, addressed to the Editor of this Journal:—"My friend Brugnatelli has ascertained that ice is electrified positively while it is scraped: he always found it a very good conductor, whereas water exempt from salts, or when distilled and very pure, is almost a non-conductor. Electricity is checked in bodies in which it finds hydrogen to displace or oxygen to take up; and the ice which, by the abstraction of caloric, has acquired a new excess of oxygen, is not in this situation. M. Brugnatelli has also made several experiments, in the hope of awakening in ice an electro-motive power, by associating it with the metals and other bodies of a nature opposite to its own, but without success. He thinks that the qualities of ice strongly resemble those of sulphur.

"M. Zamboni has modified the dry pile so as to obtain from it violent shocks and strong sparks. I shall send you by and by a description and drawing of his improved apparatus.

"In Germany some chemists have succeeded in uniting borax with carbon, by treating both bodies in a platina crucible. A strong crackling and swelling up was observed, and a *borure of carbon* was obtained, shining and striated. The borax had in fact a strong resemblance to the carbon; it became fixed in the fire, it had the same colour, was insoluble in the alkalies, refused to combine with the oxygenated acids, it was incombustible at a black heat, it had an evident affinity for a single metal, and a metal a bad conductor of caloric, and susceptible of becoming of a bright red before melting, having a great intensity of oxidation; besides its oxide, of a feeble intensity, having scarcely the physical characters of acidity, forming alkalescent salts, uniting itself with dry fluoric acid, as the carbonic acid unites with the dry muriatic acid, &c.: the combination between borax and carbon completes this parallel of their properties, and this union may be compared to the analogies between sulphur and phosphorus, and between iodine and chlorine. With respect to the latter combination (that between iodine and chlorine) we have succeeded, by a strong and rapid heat, in extricating oxygen from it; after which it sustains a red heat without being decomposed. This fact is very curious and important, on account of the great number of new bodies to which the double hyperoxygenated acid gives birth."

M. Vauquelin, in a recent number of the *Annales de Chimie*, thus relates an accident which happened on mixing the chlorine of barytes with acetate: "I had purposed preparing a great quantity of chloric acid in order to form combinations and study their properties. With this view I followed the process of M. Chenevix, which consists in making the chlorine of barytes act on the phosphate of silver: but this action being very slow, I made use of a small quantity of acetic acid to accelerate it, as recommended by M. Chenevix. The operation then took place with the greatest facility, particularly when a slight heat was applied.

"When I had carefully separated the chlorine of barytes from the chlorate, I evaporated the latter to dryness; and after having redissolved it in water, I set aside a part of the solution, and

and decomposed the other by sulphuric acid: I obtained the chloric acid, with which I made many combinations, the properties of which I intend to describe on a future occasion.

"I evaporated and crystallized the portion of chlorate of barytes which I had kept in reserve. This being done, I dried a certain portion of it in order to inspect the water of crystallization. I put two grammes of the latter into a platina crucible, which I exposed to the fire in order to determine the loss which this salt should undergo on being decomposed, which would have given me the quantity of oxygen, at least in an approximative manner, for I already knew the barytes. But shortly after this salt was exposed to the fire, a detonation took place which resembled a musket-shot at least. The furnace was broken; the platina crucible, although very thick, was rent in several places; the bottom, which was flat, was driven in like a cone; and the lid was forced against the chimney, to which it stuck, assuming the same form.

"Although the circumstance astonished me at first, yet when I reflected on the way in which chlorate of barytes thus prepared acts on burning coals, my surprise was lessened. In fact, it does not fuse like the chlorate of potash; it detonates rapidly and with great noise, even upon the parts of the charcoal where no more sign of combustion is observed.

"When reflecting on the cause of this detonation, it occurred to me at first that it consisted in a rapid disengagement of oxygen; but chlorate of potash, prepared directly with my chloric acid, having presented the same phænomenon, whilst that prepared by the common process does not present it, we must seek for it elsewhere; and I soon found that it was to the acetate of barytes that it ought to have been attributed.

"We must therefore admit that the acetic acid employed to expedite the operation does not confine itself to dissolving the phosphate of silver, as generally supposed: it also decomposes a part of the chlorate of barytes, from which it should seem that it drives off the acid or decomposes it.

"From what I have said, it appears that the acetic acid ought not to be employed for preparing the chloric acid; for, independent of the troublesome accidents which might result, the acid obtained is not fine, any more than the combinations made with it.

"The process of Mr. Chenevix being very tedious when vinegar is not employed, I am endeavouring to find one more expeditious, and I expect to succeed."

The French Class of Physical and Mathematical Sciences has proposed the following as the subject of a prize memoir; viz. To determine, 1st. The march of the mercury thermometer, at least from

from zero to 200° of the centigrade; 2d. The law of cooling in vacuo; 3d. The laws of cooling in the air, hydrogen gas and carbonic acid gas, at different degrees of temperature and for different states of rarefaction.

The prize will be a gold medal 3000 francs in value.

Another medal, of the same value, is offered for the solution of the following phænomenon:

"Fruits acquire new properties in attaining maturity, even when they are taken from the tree: they afterwards pass speedily into another state, and yet we are still unacquainted with the changes in their composition, and the causes which produce them."

The Class is desirous of ascertaining, therefore, "the chemical changes which fruits undergo during their ripening and beyond that term. With this view, it will be necessary to examine with care the influence of the atmosphere surrounding the fruits, and the alterations which they receive from it. The observations may be confined to some fruits of different species, providing the consequences drawn be sufficiently general."

The papers on the above subjects must be given in before the 17th of October 1816, and the prizes will be awarded in January 1817.

TRAVELLERS IN AFRICA.

Extract of a Letter from the Cape of Good Hope, dated the 26th of April last.

"I have the satisfaction of announcing to you the safe return to this place of that very extraordinary young man, Mr. William Burchell, after an absence of more than six years, traversing and exploring the extreme parts of South Africa. The dangers and privations he has thus voluntarily endured to gratify his thirst after knowledge, far surpass all I have ever met with in the history of other travellers for similar purposes. The joyful and interesting reception he met with from his old friends and acquaintances at the Cape, who went out to meet him, and who had several times given him up as lost, was most gratifying to his feelings, and must also afford great consolation to his relatives and friends in England. He has brought home with him an immense collection of the natural productions of that heretofore unexplored country, particularly birds; and as there are in an eminent degree combined in this man the philosopher, the botanist, the artist, and the scholar, the public may fairly calculate upon receiving, on his return, information of the most valuable and interesting description. I understand he is preparing to sail with the first ship; he may therefore be expected in England about the latter end of August."

Erratum.

Erratum.—We inadvertently stated in our last Number, that the steam-boat now plying between London and Margate, and of which a drawing was given in a previous number, was *planned* by Mr. Robertson Buchanan. This is a mistake—that gentleman had no concern in planning that vessel, having merely selected the drawings of it as best calculated to illustrate his valuable paper on steam-boats given as above.

LIST OF PATENTS FOR NEW INVENTIONS.

To Jonathan Ridgway, of Manchester, in the county of Lancaster, plumber, for a new method of pumping water or other fluids.—26th May, 1815.—6 months.

To John Pugh, of Over, in the county of Chester, salt-proprietor, for a new method of making salt-pans upon an improved principle, to save fuel and labour.—26th May.—2 months.

To John Lingford, of Woburn Place, Russel Square, gent., for an anatomical self-regulating truss, consisting of a three-quarter or circular spring with an angular moveable joint and end-piece, with joint and additional spring to act occasionally with a moveable pad of various shapes, agreeably to the form of the afflicted parts of the body, and with elastic spring covering.—1st June.—2 months.

To John Kilby, of the city of York, brewer, for improvements in the art of brewing malt liquors.—1st June.—6 months.

To Benjamin Stevents, of No. 42, Judd Street, St. Pancras, for an improved method of making marine and domestic hard and soft soap.—6 months.

To Richard Trevithick, of Camborne, in the county of Cornwall, esq., for certain improvements on the high pressure of steam-engines, and the application thereof with or without other machinery to useful purposes.—6th June.—6 months.

To Julien Jorett, of Wells Street, Oxford Road, sweepwasher; John Postee, of Great Suffolk Street, Charing Cross; and Lewis Contesse, of Bateman's Buildings, Soho, jeweller; in consequence of a communication to them by a foreigner residing abroad, for a method of extracting gold and silver from the cinders of gold-refines and other substances by means of certain curious machinery.—8th June.—2 months.

To Charles Whitlow, now of the New York Coffee-House, Sweeting's Alley, botanist, for the working or making of certain manufactures from certain plants of the genus *Urtica* and *Asclepias* growing in North America, and not heretofore used in this realm, whereby the fabrics or products usually obtained from hemp, flax,

flax, cotton, silk, and other fibrous materials, or the seeds or the parts thereof may be beneficially obtained.—14th June.—6 mo.

To James Gardner, of Banbury, in the county of Oxford, machine-maker, for improvements on a machine for cutting hay and straw.—14th June.—2 months.

To William Pope, of St. Augustin's Place, in the city of Bristol, perfumer, for certain improvements in wheeled carriages; and also a method of making them go with or without the assistance of animals—which may be applied to other purposes.—14th June.—12 months.

To Robert Brown, of Burnham Westgate, in the county of Norfolk, iron-founder, for certain improvements upon the swing of wheel-ploughs, plough-carriages, and plough-shares.—14th June.—6 months.

To John Taylor, of Stratford, in the county of Essex, manufacturing chemist, for a mode of producing gas to be used for the purpose of affording light.—14th June.—2 months.

To Grace Elizabeth Service, of Arnold Place, Newington, in the county of Surrey, spinster, for a new method of manufacturings traw with gauze, net, web, and other similar articles, for the purpose of making into hats, bonnets, work-boxes, bags, toilet-boxes, and other articles.—17th June.—2 months.

To Charles Silvester, of Derby, engineer, for improvements in the texture of bobbin lace.—22d June.—2 months.

To Robert Dickenson, of Great Queen Street, Lincoln's Inn Fields, esq. for his means for facilitating the propulsion and for the safety of boats or other vessels through the water.—22d of June.—6 months.

To John Taylor, of Stratford, in the county of Essex, manufacturing chemist, for certain methods of purifying and refining sugar.—22d June.—2 months.

To Robert Raines Baines, of Kingston-upon-Hull, glue-manufacturer, for an improvement in the construction of vertical wind-mill sails.—22d June.—6 months.

To Samuel Balden, of Ridditch, in the county of Worcester, miller; and John Burton Shaw, of Green Street, Bennet's Row, Blackfriars Road, in the county of Surrey, oven-builder, for their machine or instrument for the better heating ovens.—24th June.—6 months.

To Samuel John Smith, of Manchester, for his improved method of staining, printing, or dyeing silk, woollen, cotton, yarn, or goods manufactured of cotton.—24th June.—6 months.

To Sir William Congreve, baronet, of Parliament Street, Westminster, for his new mode of manufacturing gun-powder.—3d July.—6 months.

To

To William Beavan, of Morriston, Glamorganshire, and Martin Beavan, of Riscor, Monmouthshire, for certain improvements in the construction of furnaces and their contingent apparatus for the purpose of smelting copper and other ores, and the making of copper and other metals.—12th July.—6 months.

To Charles Coldridge, of the city of Exeter, for his grate and apparatus.—18th July.—2 months.

To William Lewis, of Brimscomb, Gloucestershire, for his improved principle of erecting racks for the purposes of racking woollen cloth and other articles.—15th July.—2 months.

To Robert Copland, of Liverpool, for his means to effect a saving in the consumption of fuel.—21st July.—10 months.

To John Manton, of Dover Street, for his improvement in the construction of hammers and pans to the locks of all kinds of fowling-pieces and fire-arms.—21st July.—2 months.

---

*Meteorological Observations made at Clapton, in Hackney, from June 27 to July 12, 1815.**

June 27.—Fine clear day; *cumuli* and some *cirrus* above them: towards afternoon the *cumuli* increased and condensed, but the night was fair, with haziness. There was a soft appearance in the western horizon occasioned by the disposition of the clouds, and by their edges being softened down in appearance by the interposed mist. I noticed this morning a whirlwind which carried up much dust in the Leebridge road.

June 28.—Fair hot day. Thermometer 73° at midday.—*Cumuli* form and rapidly evaporate again. Some features of *cirrostratus* in the afternoon. Wind southerly. The evening was beautiful; a pale golden colour above the set sun, and with a clear greenish blue to the N. and a fine pink blush above.

June 29.—More cloud in elevated masses. The day was fair and warm, and the evening somewhat cooler. A fine clear sunset. The wind veering eastward with dry air†.

June 30.—Early the sky was clouded; but a fine warm day with *cumuli* succeeded. The evening sunset presented a clear yellowish-golden colour, with a pinkish blush above it. Wind S and SE. A light gale with cloudiness came up suddenly at about midnight.

July 1.—Fair day: *cumuli*, &c. Towards evening copper-coloured refractions in *cumulostratus* breaking up; some light features of the cirrocumulative *cirrostratus* assumed at one end

* I have been prevented by absence from home from continuing this journal the last month. The weather has been variable, but generally warm and fair. It is reported that a brilliant meteor was seen in the neighbourhood of London on the 18th of June, and another on the 22d.

† The edges of the *cumuli* were not very well defined to-day.

of

of the mass the form of alternate bars. Wind easterly, with cool breeze, and cloudiness towards midnight. Thermometer midday 73°; midnight 51°. Barometer falling 30·20.

July 2.—There was a cool NE wind, and cloudy sky in the morning; the edges of the clouds in general were not well defined*; the day became warmer; in the evening *cumulus*, rocky appearance, &c.; features of *cumulus*, and large low *cirri* condensing with copper-coloured and afterwards crimson colours in their under parts. The colour of the western horizon pale yellow. At 11 P.M. Thermometer 48°. Barometer 30·15. Sky clouded. There was a whirlwind about four to-day.

July 3.—Cool easterly wind, and cloudiness early; sun at intervals through the day. The clouds were ill-defined in their edges in general. Clear cool night. At midnight Thermometer 45°. Barometer 30·11.

July 4.—A great deal of *cumulostratus* obscured the sky: very slight nimbification took place about two o'clock. The wind was gentle, and the weathercock indicated a southern direction. The quantity of cloud continued; some lofty and large *cirrocumulus* in the evening. Thermometer at midday 63°, at 11 P.M. 54°. Barometer 30·12.

July 5.—Clear morning; *cumuli* formed, and in the afternoon obscured the sky almost. Fair evening, with lofty clouds with the cirrocumulative arrangement. Thermometer midday 74°, at 11 P.M. 53°. Barometer 30·18. Wind gentle and northerly.

July 6.—The day was fair and warm, but there were light irregular gales from the north and east with *cumuli* below, and much *cirrus*, whose fibres were assuming horizontality, above in a higher air; forms of *cirrocumulus* too occasionally broke out. Towards evening the subsidence everywhere of the fibres of *cirrus* into the denser and more horizontal *cirrostratus* foreboded the gentle rain which came down at night. At 11 P.M. while raining. Thermometer 54°. Barometer 30·04.

* I am now convinced by repeated observation, that nervous and susceptible persons who are much influenced in their feelings by the atmosphere, are in general worse when the clouds are characterized by an indefinite and confused margin. There are probably many varieties in the electric state of the air (not discoverable by our present instruments) which act on the constitutions of different individuals, and produce irritation; and the periodicity noticed in many disorders by physicians of old, and the periodical affection of the health of persons in general noticed by Dr. Spurzheim, must, I think, be referred to the atmospheric electricity. But we cannot always detect even in the clouds marks of its unwholesome states. There is, however, usually a very uncomfortable feeling when the clouds have ill-defined edges, their circumscriptions not being distinctly marked, but when they are confused with the surrounding atmosphere.

July 7.—A showery tendency appeared in the atmosphere by the disposition of the clouds, with a strong northerly wind towards evening; night cool and fair.

July 8.—N wind early, then westerly. *Cumuli* under a higher veil of cloud, which showed now and then features of cirrocumulative *cirrostratus;* the latter cloud also in bars, &c. Some drops of inosculation towards evening. At 11 P.M. Thermometer 55°, with a clouded sky.

July 9.—This morning I witnessed a phænomenon, of which (though not uncommon) I shall relate the particulars, as it serves to point out the nature and causes of some atmospheric changes. Early abundance of filiform *cirri* were strewed aloft, and *cumuli* formed in a lower region; by and by the *cirri* disappearing, the *cumulus* was changed for *cumulostratus;* and the progressive conversion of the former into the denser and upward-spreading features of the latter cloud, seemed to clear the upper air of the *cirrus*. The *cumulostratus* when formed, preserved the tendency to have its prominences curl inward, which the previous *cumuli* had. Afterwards much cloud obscured the sky.

July 10.—Fair warm day, with *cumuli*, &c. Remarkably fine evening, almost cloudless.

July 11.—Fair hot day, with *cumulus;* the *cirrus* appeared toward evening. The night was damp, and there was a haziness in the horizon at sunset. About 11 at night, I noticed the electricity of the blades of grass on a grass-plot behind the house. It appeared as a whitish and very faint and almost momentary augmentation of light on certain parts, and now and then a faint scintillation. I have noticed this phænomenon before on similar fair damp summer evenings, after hot days. Thermometer in the day 75°, at midnight 54°. Barometer 30·20. Wind south-westerly.

July 12.—Hot day, light cirrocumulative *cirrostratus* was the first cloud to appear to-day; *cumulus* prevailed during day, but did not abound. Fine clear sky in evening, with few *cirri*, and gentle gales from SW. Thermometer 78° and 58°. By night I saw a very minute falling star*.

Five Houses, Clapton,
July 13, 1815.

THOMAS FORSTER.

* These very minute, and to an unattentive observer almost imperceptible, meteors often abound in clear weather. They may probably be as common as the larger kind, or falling stars, but being very small are not noticed. They seem to move horizontally.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For July 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June	27	56	69	60	30·12	67	Fair
	28	59	71	57	·21	70	Fair
	29	60	74	55	·25	61	Fair
	30	56	72	59	·15	56	Fair
July	1	58	73	55	·12	47	Fair
	2	55	67	52	·10	47	Fair
	3	55	66	53	·03	40	Fair
	4	58	66	56	·01	43	Cloudy
	5	57	69	56	·09	47	Fair
	6	58	70	55	·07	46	Fair
	7	55	67	50	·01	44	Fair
	8	56	68	60	·08	47	Fair
	9	61	67	60	·10	52	Fair
	10	62	69	61	·14	49	Fair
	11	60	70	63	·12	56	Fair
	12	62	76	65	·08	64	Fair
	13	64	76	63	·01	67	Fair
	14	64	76	65	30·00	56	Fair
	15	67	75	67	29·99	47	Slight Shower
	16	68	76	64	30·01	66	Fair
	17	65	74	63	29·91	57	Fair
	18	60	70	60	·85	57	Fair
	19	60	61	56	·64	0	Rain
	20	55	63	55	·83	46	Showery
	21	55	66	54	·86	52	Fair
	22	56	67	60	·90	56	Fair
	23	57	63	55	·91	46	Cloudy
	24	55	70	62	30·00	56	Fair
	25	63	72	63	·10	50	Cloudy
	26	62	60	54	·12	30	Showery

N.B. The Barometer's height is taken at one o'clock.

XXI. *Comparative Anatomy; or a slight Attempt to draw up a Comparison between Animal and Vegetable Life. By Mrs.* Agnes Ibbetson.

To Mr. Tilloch.

Sir,—After having dissected most parts of vegetables, shown their different powers, and endeavoured as far as able to inquire into and explain the various uses to which each different part is applied, it struck me that it was now time to draw up something of a comparison between animal and vegetable life. I have often during the course of this work lamented the injury done to the anatomy of botany, from comparisons drawn only from human anatomy: because a surgeon would be but too apt to add many more powers than really exist in the vegetable, unless he had very exactly *compared* and *studied* the two *dissections together*. But as a perfect assistance may be procured in the description of the human body, that, joined to a *thorough knowledge* of the *vegetable*, would not, I think, be liable to the same objection. I shall, however, try it: and if I should make any mistake, it will be pardoned in one who can claim but very imperfect knowledge in *human anatomy*, and who will (but in a very few cases) *attempt* (*in animal anatomy*) to borrow from *her own dissections*.

In reading books of anatomy, and viewing the dissections of animal life, when contrasted with a vegetable being, it is impossible not to be struck with the studied care and elaborate pains *seemingly* exhausted in forming the first, and the clearness of invention and simplicity of formation observed in the second. To display this, I shall first give a slight sketch of the human figure, contrasting it, as I proceed, with a more finished picture of the vegetable frame; touching most lightly on those parts which admit not of being compared with plants, but dwelling as *strongly* and *clearly* as *possible* on *those features which do, or are supposed* to, *admit* of some resemblance to the human structure. In viewing the figure of a *man* and of a *tree*, it is hardly possible to conceive two things more completely dissimilar; and in beholding the *skeleton* of *each*, they would appear too much *unlike* to be compared, not seeming at a first view to offer *any* of *those similitudes* which are necessary in such cases.

But though no general resemblance can be discovered, yet a sort of comparison may be drawn up, perhaps advantageous to both, as more clearly impressing on the mind the beautiful diversity to be found *in two beings*, both possessed of a different kind of existence; showing also a *few instances* in which there is some resemblance; fixing the similitude which *does* or *does not*

 exist;

exist; but which in contemplation must continually remind us, that the same *great* and *benevolent* Being formed and created both.

To give some idea, though an imperfect one, of the nature of the two different beings to be compared, "the human form and vegetable," it is first necessary to fix on those parts which can in any measure be assimilated to each other. To the brain, spinal medulla, and heart of an animal, we can only with justice contrast *the line of life of a tree.* But how different! how inferior! The spiral wire is, however, a more faithful image of the muscle: indeed, as the plant is apparently a mechanical being, we might expect to find the strongest similitude in these parts; and as the muscle is in *both* the *only* part which seems to possess a *self-existing motion,* we can be in no danger of mistaking in the vegetable the wrong *situation,* since the *spiral* is the only part which is *vis insita.* The wood and the bones also have some curious analogies. The blood, with the bark, juice, and sap, may *be compared.* The *oil* of the *muscles* and *spiral wire* are *the same.* The womb and seed-vessel are something alike. The stomach, the place of digestion; the liver, the secreter of bile; the kidneys, the bladder,—all belong to a being that digests; which the plant does not. The skin and the upper cuticle have no resemblance whatever. The plant has no *nerves;* nor are the *muscles at all attached* to the line of life, or *brain* of the *plant.* Both beings have *lymphatics,* both have *glands,* but the vegetable is *different* from the animal gland; since the vegetable has no *secretions* from the blood, but receives the juices of *this kind* all pure from the *hairs.* Does not every part of this selection prove the vegetable to be a mere *mechanical agent?* a great and bold line, drawn by the Almighty hand to diversify and beautify creation, and fill up the link which would otherwise be wanting between the organized and unorganized matters! Let us then descend to the minutiæ of the existing difference.

First then (to borrow Hunter's words and plan), we must form a human being. The MIND (the immaterial agent) must be provided with a place of *immediate residence,* which shall have all the requisites for the union of spirit and body: accordingly she is provided with a brain, where she dwells as governor and superintendant of the whole fabric. In the next place, as she is to hold a correspondence with all the material beings around her, she must be supplied with organs fitted to receive the different kinds of impressions which they will make: in fact, therefore, we see that she is provided with the organs of sense; the eye to see, &c. &c., Further, she must be furnished with other organs between herself in the brain and those organs of sense; and she must have other organs *between herself* in the *brain* and *every other*

other part of the *body, fitted* to convey her commands and influence over *the whole*. For both these purposes, *nerves* are *adapted*; they convey all the sensations to the mind, and give notice of all suffering. It is evident, therefore, that all sensations arise from the impression of some active substance on some *nerve* of the *human body*; and that the same is represented to the mind by the means of the nerves connected with the brain: every nerve, therefore, that is irritated produces a sense of pain; and if certain parts of the brain are *compressed*, from which *particular nerves arise*, then those *senses* are *lost* which depend on *those nerves*. But *in* the *injuries* of the spinal medulla (which is a continuation of the brain down the *vertebræ* of the back) this is *still more evident;* for those parts which receive their nerves arising from the place injured in the medulla, are either *convulsed* if that be *irritated*, or rendered *paralytic* if that be *compressed* (and if the greater part of the brain *suffers*, the *whole body loses* its power of motion). These things considered, there seems to be *no doubt* that the *cause* of all *sensibility* arises from the *nerves* being connected with the *brain* or *spinal medulla;* and that all *voluntary motion* equally arises from the *connexion* of the *nerves* with the muscles, thus conveying its orders to and from the brain. Therefore it cannot reside in the *nerves* themselves; because otherwise the moving cause would continue *to act* after being *separated* from the *brain*, nor would it be increased by its irritation.

Of all this important and admirable formation of the human body, what part does the vegetable possess? Scarcely any that can deserve to be assimilated to so wonderful and marvellous a structure of *senses* and *feelings;* though it has certainly great beauty, not only from its simplicity, but arising also from the fitness of its form, so admirably adapted to its situation in the grand scale of *beings;* but without *volition*, without *sensibility*, even without *senses*. The line of life can only be said to be the *brain, spinal medulla*, and *heart* to the plant, *to stand for all three*, though it resembles the heart only as the first visible life, and the brain alone as the chief source of existence, and the spinal medulla as passing like the line of life through the body. But how inferior an image of all! It has not the action of the heart, having neither *arteries* nor blood to circulate; nor the *brain*, having no *nerves*. I have entered more minutely into this than I intended, in order to impress on my readers a conviction I feel myself, that, *thus formed*, there can be no *volition* in plants.

To begin then with the description of the *line of life* (the only part that can be compared to the brain and spine of an animal). The line is formed of a matter firmer than the bark, not so hard as the wood, generally of a white colour, but sometimes green,

 sometimes

sometimes yellow, always *black* when *dead.* When a vegetable is really killed, this *line* is the *first part* to *die*:—thus, when struck with lightning or a *hard frost*, in a moment this line turns *black*, and can never again be revived, though the bark and leaves may for a time retain their verdure; as, like the hair and nails of an animal, they possess but little life. The line of life consists of three or four rows of cylinders hollow in the middle, and within them are generally found the juices of the nectary; but this mixture formed in the middle root, seldom, I believe, flows up the vessels till after the *flower* has *passed up them:* about the end of February the *flower* (formed in the root) enters *these vessels*, which *enlarge* for the purpose, and run *up them*, till they arrive at the buds, which were empty till then, having only their scales (*prepared* some time *before*), and smeared with an oil which protects the interior from all danger. The embryo then quits the vessels, and enters those buds which are properly their cradles in the bark, situated in the axillæ of the leaves. The vessels, after this service done, are restored to their usual size, and remain all the rest of the year unaltered, still conveying the nectareous juices. It is only three weeks of the year that this phænomenon can be seen; it appears in every plant nearly a month before the flowering season. With respect to life, this line more resembles the *heart* than brain of a human being; as it is *evidently* the *first place* which *exhibits life:* and though it does not show it by *motion*, having neither *arteries* nor *blood* in this part, yet it is *this line which gives life to the bud*, to *the seed*, and *to the flower*.

There is a curious similitude between the animal and vegetable that should be displayed here, as belonging to this part principally. I have shown that the brain and spinal medulla are the sources of life in the animal, as the line of life is to *the vegetable.* I have also mentioned that the muscle and *spiral wire* are the sources of *involuntary motion to both.* Now if they are each struck with a quick death, that is not lightning, the irritability in the muscle will remain in both objects for a time after death; but the *animal struck* with *lightning* or the *plague*, and the *vegetable* with *lightning* or a *hard frost*, will *directly* lose that irritability, and immediate decomposition will come on; each body will grow soft, *pappy*, and *watery*, *putrefaction* will suddenly succeed; nay, this is often the case in both even *before death.* Many have supposed that there might be some volition in plants. But how can that be? What is the cause of *will* in the human frame? It is alone *supported* by the connexion of the nerve with the brain; and in motion, that nerve being also fixed on the muscle, by its *immedate action* proves *its obedience to orders sent it from that superior power:* but there is no *nerve* in the *vegetable;* nor does the muscle (if it could be substituted)

stituted) at all join to the *line of life*, or form any connexion with it. How then can *will* be *announced* or *executed?*

There can therefore be no volition in plants, and they can only be a *piece* of *living mechanism.* No person who has seen and dissected a plant killed by *electricity*, can doubt which is the line of life, since this line is *directly marked* from the root to every fibre, and turned completely black in a moment.

I shall now turn to the animal and vegetable muscle. And here the strongest *similitude* is to be discovered; since it is the *only part* that in *both beings* can be said to move by *involuntary motion* or *vis insita.* It is well known that in the human body there are three sorts of motion; the voluntary, the involuntary, and the mixed: but the first two will serve our purpose here. The first is caused by the will, *enforced* by the *nerve*, and executed by *the muscle;* but the last is spontaneous, proceeds from the muscle alone, the cause *unknown.* It remains often in the muscle after death, and, after it is divided from the brain, even exercises its full force in this situation. The human muscle forms part of the flesh; it consists of two parts, the wide (which alone is active), and the thin shining extremity called the tendon. The only purpose of the last is to fix the muscle to the moveable bones, which it does with a force not to be conceived but by feeling it. They are thus fastened in a concentrated manner, in which a greater power is permitted to act, as labours are assisted by ropes in moving weighty bodies; each muscle *contracts both ends* towards the centre, which serving as a *fixed point*, draws the bones with it. The wide part of the muscle is the moving part, and is most curiously formed. The muscles differ in the number of their cases, some animal muscles have two, or even three; but the vegetable *never but one.* In the human muscle a number of long, soft, fleshy fibres are seen inclosing many bundles of hair-like figures, which are the *nerves*, and which *split without end.* They appear to me to pass through every part of the muscle, *case* and *all;* when the muscle is quite fresh, they seem to move with *light* and *moisture* if *suddenly applied.* But I am not *sure of this*, having but *once* been able to get them fresh enough for this examination. It is difficult to discover how solid the *nerves* are, since they contract at the end the moment they are cut, and with such instantaneous effect, that though I have divided them with the microscope at my eye, I could not catch a view up the pipe. There is no part of the body which may be properly called fleshy (and which is in truth the muscle) which is not *penetrated* in every *direction* by *nervous fibres;* their sensibility *pervades the whole muscular* substance, even its smallest portions; and though they appear not to move themselves, yet they are supposed to be by some means

the

the cause of all *voluntary motion* to the muscles. Cuvier thinks that they are the cause of *all* motion, both voluntary and involuntary; but *that* certainly *cannot be*, since the muscles of a *plant* have the same *vis insita*, without possessing *any nerves*. But there may undoubtedly be a sort of *vapour*, which may pass through the nerves from the brain, and cause a most powerful effect, but still be entirely *separate* from that *vis insita* which is *so peculiar* to the *muscles*, that no one who has *seen it act* both in the *human body* and *vegetables* can confound it with *any other sort of motion*. The human muscle is a bundle of vessels: the oil lubricates them, that they may not be hurt by constant friction: and this is exactly the case also with the vegetable; but the cases of *both muscles* are even more wonderful than the hair-like fibril they each contain. The human case appears to be formed of two sorts of vessels; the fibril already described (which also enters into the formation of the case, and runs *longitudinally in it*), and another sort of vessel which forms a line and space, and passes across the muscle, and gives such a beautiful shining *lucid* appearance to the *muscle case*, and by its help draws up both vessels. The crossing vessels are alternately filled with a white oily matter, and the spaces between (being perfectly empty) by the help of the nerve which gathers up longitudinally, the cross vessels are drawn into *ribs* one above and one below, which running in a forked manner produce that *lucid appearance* admired by surgeons. When a piece of the *muscle case* is drawn out, and left to dry, it becomes *clear* and *horny;* and if when in this state it is bent to and fro *very often*, the place *swells up*, and appears of a *blueish milky colour*, which I *supposed to be caused by the oil* given out *from the cross vessels*. The *nerves* also (which pass through the muscles) are with this management *broken* and dispersed out of their cases; and it is *then* they are seen running in an undulating manner through the oily compound, which, now the vessels are destroyed, retain only a matter much resembling the impervious skin of the vegetable, only a more oily composition. The vegetable muscle-case, like the human, has also *the fibril* muscle in it: it is formed of straight vessels, replete with oil; but often so *brittle* they will not bear to be bent. These vessels are at some distance from each other, and laced up by the spiral wire. Though the figure of each case does not possess a very strong resemblance, yet their effects are *the same:* they have both the *moveable fibrils within*, the oil to lubricate, the case to protect the white oily vessels also; no point is wanting to prove that they are intended to produce the same effect, though perhaps set in motion by a *different cause*. There can be no doubt that light and moisture are the *hidden causes* of the vegetable motion, since their direct obedience

dience *when excited by those powers speaks for itself.* But what is the human cause I leave to better heads than my own. The involuntary motion of the human muscle seems always to take place with a jerk or by *impulses.* This is exactly the case with the vegetable, nor can they either move again till the *relaxation* of the *muscle* has *again taken place.* It would be curious to ascertain whether the measure is as exact in both as in *the vegetable**. In the plant there are two impulses, *a long* and *short* one; the latter, half the length of the former; the first from ball to ball, the second from knot to knot. Both these measures are seen in the *Mimosa sensitiva;* when the whole *leaflet drops*, the spiral is moved from ball to ball; but when it is a *leaf only*, then the spiral moves from knot to knot. When the stamens rub together to throw their dust on the pistil in the mosses, then it is a single impulse; and when a leaf is moved it is the same; and *the moisture* it *occasions undoes* the *spiral*, and prepares it for a *new contraction*; and so delicate and sensible is it to each increasing light or *decreasing moisture*, that each minute almost brings its *variation*; certainly each *passing cloud over the sun, its change.* A few words, however, I must add on the unknown cause of the muscular motion. The human muscle lying so near the outward skin, (a cuticle which though doubled, nay trebled, is so pierced with pores which must admit much light and moisture,) would *seem* to *suggest* some *cause* for its motion, of the same nature as the vegetable, *did not* the peculiar sort of cross action in the *muscle-cases* seem to suggest a difference: and yet there is one circumstance which is so exactly contrived respecting the nerves, as it is in the vegetable in the *muscles*, as to carry some impression with it: "The sense of touch is supposed to arise from the *nervous extremities* of the *papillæ* being more numerous, or covered with a *thicker* or a *thinner skin* in some places than in others, and thus bestowing a *grosser* or a *finer* degree of feeling to the different parts. Now this is exactly the case with the *muscles:* they increase their irritability in proportion to their advancement to the *outward cuticle*, and as they become *more sensible* of the *effects* of the *atmosphere.*

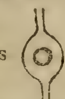
In all trees and shrubs whose branches live through the winter, and are therefore exposed to *severe changes*, the muscles are *concealed within the wood:* but in other plants (rising each year from the earth) they are discovered next the alburnum vessels; and in all very *sensitive plants* the outward cylinders of the rind are few in number and very thin, and the more irritable the muscle the thinner they are. Now there is some analogy here,

* But in the human, I believe, it depends on the length of the muscle.

which if followed up and closely investigated might perhaps lead to a discovery of the causes of motion.

I shall now turn to the bone of the animal and wood of the vegetable, which may from some curious particulars be compared together. The first has a skeleton or system of bones which gives firmness and shape to the fabric, keeps the soft parts in their places, affords fixed points to the organs of motion, and gives them their proper direction, and protects some of the most tender parts from external injury. To connect the bones together, there are *ligaments;* and to afford smooth surfaces for them to play easily on one *another*, their extremities are furnished with *cartilages* or *gristle.* The bones consist of an interior, which is a species of honeycomb, and an exterior surface, which has many *layers* formed in a particular manner; the last if cut with a very sharp knife, and very thin, appears composed of many meandering vessels *filled with atoms*, which seem to be *rather solid*, from the clear spaces they show between them; while the honeycomb parts are *plates*, *thick* in the middle and meeting in their *thin edges.* This is also the same as the formation of the large bones of the skull, which increase also towards their centres; but the most extraordinary part of the wood is, that the foundation *exactly resembles* the thin surface of the bone, and is really composed of diminutive vessels filled *with atoms:* but this is only to be *seen in both* when excessively *magnified*, then they exactly resemble each other. But the system of wood, though it gives firmness to the fabric, yet obeys most *strictly* the *laws* of the *line of life* in its shape: it preserves, however, this important part from injury, but is within every other *soft ingredient*, and therefore cannot defend it.

In trees and shrubs it serves as some *support* to the *muscle*, but in annuals and herbaceous plants the *muscle changes its place*, and is at the *external part* of *the wood.* It cannot be said to afford a fixed point for the organs of motion; for it is the *gristle* which does this, which forms the *balls* round which the spiral winds itself, and which covers the ends of the wood in such a manner as to enable them to slip *over* or *into each other*, like a *cup and ball.* I never saw the *knee* or elbow of a human *skeleton* that it did not remind me of a *Galium*, *Selene*, or some of the stems of those plants which *swell* at *the joint.* It is a sort of gristle which serves *this purpose* and sheathes the wood, a kind of *thick matter*, which performs, in its *solid* or *liquid* state, many *important functions* in the *vegetable œconomy*, and makes a part, I doubt not, of that resinous compound Dr. Thomson discovered in the wood.

But how curious it would be to possess and be able to examine *that liquid* which performs that *extraordinary office* of *clearing the way* through the *hard wood* for the buds; probably it might serve as a good comparison to the *gastric juice;* for great must be its *dissolving powers.* When the bud is first formed on the line of life in the root, and runs up to that part from which it is to *start* and make its way to the bark, the wood opens it a passage; and this is apparently done, even through *iron wood,* by a *liquid* which no sooner *touches* the *fibres,* than they divide and bend, and thus make a sort of *covered way* for the bud. That the bud passes from the interior no one can, I think, *deny,* who examines a *tree after* it has been *barked:* at every time of the year, but particularly in the *autumn,* a quantity of buds may be seen running from the line of life, to fix themselves in the bark. This is merely the embryo of the bud, or empty seed-vessel, with the nectareous passages and the corolla, all *rolled up together* into a little ball. These, when they start from the line of life, possess a juice of a peculiar kind which *precedes them,* and arranges the wood-vessels as it passes, m-king them all bend from it thus : so that there is actually a large space between the *bud* and the *wood* filled up with sap-jelly: nor is it possible the bud can be *even touched,* much more injured; since this hollow space is left for it till it reaches its scales, already prepared in the bark for its *reception.* There is not, perhaps, any of the contrivances of Nature more beautiful than this, or which has been so *strangely misunderstood:* if the bud had had to force its way, it would have been *destroyed.* In following the process, it is but cutting away the wood near where there *are buds;* as you approach one it becomes softer and softer, at last the liquid is discovered, and in the midst of it the *little delicate embryo,* lying in its bath or jelly-case, while a covered way near an inch before it is already prepared for its advancement. As it approaches the bark, its inclosure or cradle is ready for it, with all the conveniences selected for its preservation; and generally a quantity of hairs or points covering the outward scales of the bud, to introduce into the interior, *or the ball,* all the nourishment the most delicate creature can require; for there are few buds that are not at that time of the year loaded with hairs and instruments. Though I can find no analogy in the human frame for this process, yet I doubt not, *if* we possessed the *liquid,* it would assimilate well with the gastric juice. That so delicate, so soft a matter as a bud should pass unhurt through perhaps a foot or two of hard wood (for it must do so in the stem of a very large tree) is really almost miraculous till the method is explained.

Having

Having now described those parts of the human body and vegetable frame, on *which their figure, support, strength,* and *motion depend;* we must next examine those *juices* which are intended to replace the waste of both the machines, and to supply them with new energies. In the animal body are many more sorts of liquid than in the vegetable, because these are so many secretions from the blood: now this is unknown in plants. The animal blood is a rich store of nutritious fluid, fine enough to *penetrate* its *minutest parts,* and which constantly circulates through its whole machine: the elements of its composition are furnished by all our food. Now in the vegetable there are but *two sorts* of liquid that can be said to replace the waste of its substance,—the bark-juice and sap: the first, far from being formed in the body, is merely collected there from the atmosphere by the hairs, in one of the layers of the leaf, from which place it runs to the inner bark-vessels. It has no circulation like the blood; nor is it formed by the food, but taken in when separate, and mixed in the pabulum of the leaf. The bark-juices are so far from being capable of circulating, that their tendency to *coagulate* is *such,* that the bark is full of large species of matter escaped from its liquid state; and even in the vessels of the inner bark (if it was not for their *peculiar construction*) it would never *retain* its *liquid* form, since it is forced through apertures not half the size of the vessels in which it runs; and this *impetus,* by *increasing* the *violence* of the *action, tends to its fluidity.* The sap is, however, better adapted to play the part of the blood, were not a *circulating medium totally unfit* and *unsuited* to the *vegetable form.* A *being increasing* at each of its numerous extremities, receiving its blood from another body, and that *blood coagulating into a jelly* by *rest* and *suspension,* before it can form its *compound,*—can such a make be compatible with a constant circulation of blood? which cannot stop without destroying life. The proving that the sap-juices do not circulate is of the greatest consequence: if they circulate, and all the juices that should (when they arrive at the extremities) instead of forming the new shoot, run down again through the bark,—how is the new shoot to be completed? since all *that matter* which was to form it is employed in returning through the bark, leaving the only part which at that time required support and moisture. Is it not more natural to suppose that the juices (as they always become a jelly before they are wood) are arrested to be converted into that matter, and then complete their change? The idea of circulation is drawn from the comparison of animal life, *just only* in a *few instances, misleading* in almost every other. Is there not a great difference between an animal which grows not at its extremities, but which after the first few years ceases to increase, and

and one which increases every season at every joint, whose juices are independent of itself, and taken from another, and may naturally be supposed, *therefore*, to draw up *only that liquid* which is necessary *for that increase?* But if there were a circulating law established by Nature for the sap, it would most probably be a *universal law*, general to all plants: What then would become of those *trees* and *shrubs* which lose their bark in the winter? whose vessels are supposed by some botanists to convey the sap? If they remain, they would be doubly conspicuous, when the rest had *departed*; and they are certainly not to be found. A trifling piece of the green bark is discovered in the winter, and the rest of the space *is empty*, and the inner bark vessels are generally deprived of their juices. How is then the sap to convey its circulation? If we are to assert that the sap descends again after its ascent, we must *show* in *what vessels*. If in the wood-vessels? the bark could not hold a tenth part, as I shall show. If in the bark-vessels? then must we discover the anastomoses or joinings of both series of vessels, that is, the communication between the *cortical* and *ligneous* cylinders, the meeting of the wood and bark-vessels. I injected a weak quantity of sugar of lead into a plant, and sent the German test after it; but no joinings of vessels were to be perceived: they gave the result indeed, but traced it to the terminations of the wood-vessels in each fresh forming twig. This is always the method I pursue when puzzled respecting the terminating vessels, and it rarely fails me. It is often given as a proof, that the sap circulates in trees, instead of flowing up, as, I think, only to form the different productions it has to complete: "that when the fellers and peelers set about barking, they continually find the young shoot so full of juice as to be ready to pour over, while in the larger shoot, or lower part of the tree, they are almost dry, at least there is a much smaller quantity of liquid; so that the men often exclaim that the sap enters at the ends of the twigs: but this is *the very circumstance* that proves the truth of what I advance. It must be recollected that they do not begin to bark till after a quantity of liquid has passed up the tree *from the root*, loosening the bark all the way from the wood: therefore it is almost at *the end* of its *flowing time* when they begin to peel, or it is very difficult to *tear off* the bark. The liquid is therefore already arrested in the young shoots. Now its arresting is always the time it takes to become jelly, and soon to receive the wood-vessels and complete the shoot. This, therefore, is the cause of the quantity of sap being always found in the young twigs at this time; *three weeks sooner* the sap would have been discovered in the middle root. I have often drawn a pint from them *at this time*.

The

The quantity of sap always found in the sap-vessels plainly shows their office: if then there were *returning* sap-vessels, would they not be equally *plain* and *visible*, whether in the *bark* or *wood?* Besides, *except* the *inner-bark vessels*, all *the cylinders* that can convey juices pass either *round the tree* or in a *transverse* direction.

I have but one more argument with which I shall trouble the reader. It is said that the sap after mounting to the top of *the tree all descends again*, and returns through the bark. Now the bark is in summer one-tenth or eighth part of the wood, and preserves in its *decrease* (as it mounts the tree) nearly the *same ratio.* Suppose, therefore, *a a* to be the bark; and *b b* the wood; and as the wood was full of sap-vessels, it is not possible to cram them in less than *half that space:* the ratio *b c* must, therefore, be added to *a a*, yet not increase *its size.* This is indeed adding much to a little and making it less. Had Nature intended the sap-vessels or juices to *return into the bark*, a conspicuous place would have been allotted for them; but they are now sought in vain. It will be remembered that it is at all times the sap is supposed to circulate when the bark is quite full of its own juices, the sap must indeed then be *invisible.*

The absorbent vessels are perhaps both in the animal and vegetable one of the most curious parts of their formation. In the human body they are various: those that open their extremities under the different surfaces of the skin are called exhalants—of these the vegetables possess but a few, since they perspire not, and it is only under the first cuticle of the leaf they are discovered: but there are *other absorbent* vessels which take in from the atmosphere (even in the human frame) many *juices* and *gases* favourable to health, and which I doubt not increase the nourishment of the body; and though they are not near so *numerous* as the hairs of vegetables (which are of such a variety of kinds), yet they so increase with the *vigour of the body*, and in hard-working people, that I doubt not they are intended to *supply much nutriment.* In the vegetable it is well known that they do so, and that they have absorbent vessels leading to each hair, to throw the matter thus acquired into the nourishing vessels; and probably they would not increase under such existing circumstances in the human body, but for some such purpose. I doubt not but that those who eradicate hairs, from the silly notion of beautifying their Maker's work, acquire many disorders in the skin that would never be theirs but for this foolish custom. All hairs arise from two distinct capsules, one within the other, having a certain degree of moisture between them; and the only difference between the human and *common* vegetable hair is, that the *latter* has many more valves, and has frequently

a spiral

a spiral within the interior *cylinder*. The human hair shows not that variety of shape; three or four sorts are all that are ever discovered. But in the vegetable there is scarcely a form possible that is not imitated: but these I call *instruments, not hairs*, since they much more resemble the various invented glasses of a laboratory than the simplicity of a hair; to each of these, however, an absorbent vessel is added to convey the juices thus acquired to their various places of destination; while those vessels in the human body are thin pellucid cylinders arising from the various surfaces of the skin, and running to a common duct called the thoracic duct. They are of two kinds, the *lymphatics* and the *lacteals*. The first the vegetables possess, but not the last. The lymphatics take up the colourless fluid called lymph (whence they have received their name), and convey it from all the parts of the body to the same point. Thus the parts of the blood, which either from their thin oily or nutritive qualities had been separated from the red blood or circulating mass and thrown out by the exhaling arteries, are absorbed after having performed their various uses, and are again conducted by the lymphatics into the circulation. But the vegetable having no secretions from the blood, the lymphatics receive from the absorbent vessels those juices introduced from the atmosphere by particular hairs, and which are partly secreted in the glands till wanted. The rest (after performing their various purposes) are carried into the nourishing or sap-vessels. But in one point of resemblance both agree;—absorption in both bodies helps to remove those injuries which happen to the frame by accidents. If a tumour proceed from a blow or cut, the absorbents will soon begin to *act:* a fluid poured from its ruptured vessels will be absorbed by its lymphatics and carried again into circulation. The black or green spot which is left will disappear, and be taken up in both; fresh supplies of wholesome nourishment are brought from every part to recruit the bad flesh or wood that *has been injured:* and in the plant, so great is the haste made to remedy the defect of this kind, that it rarely happens that a wound is made without such a quantity of nutriment being sent to the part that it generally finishes by being a nursery for new buds. The absorbents of both beings are full of valves; and as to the glands which secrete the oil and other matters, they are certainly in the vegetable as well as the human form, though not so numerous: the two nectaries of the flower are of this kind. Some *excretory ducts* also *plants must possess;* since much matter is conveyed away that would otherwise impede the flow of the sap: for when the new wood is formed, all the old divisions of the piece which had been before *bark*, disappear quickly, but not so hastily as not to show it is drawn off by degrees by some vessels *appropriated* for the

purpose.

purpose. The broken wood also is in the same predicament, and equally well managed.

The analogy between the animal and vegetable *fœtus*, in respect to their *production*, nutriment, and *oxygenation*, is indeed very striking and forcible.

The ovum in the *female* is discovered at an early period in the virgin state, as is the seed of the tree; and like that, many years pass before the tree arrives at its proper maturity; but they both attain that point of perfection at an earlier period in warm countries than in cold. The ovum in the female resembles a *bunch of grapes* fastened as they are to the umbilical cord; and when the seed is first formed in the side roots, it is an apparent *gross powder*, which soon becomes *regular balls*, and which tied to the line of life, may well be likened to grapes also;—but in one respect they evidently differ. The *ovum moves* not till after impregnation has taken place; whereas the seed has travelled far and gone almost all its journey, before it receives the powder of the stamen.

Mirbel says, the vessels in the style unite with those in the peduncle and form the umbilical cord. But Mirbel was not aware that the seed rising from the root had already received (from the first moment of its existence) its umbilical cord (which is indeed its life), when it is fixed in the flower (at the time he alludes to), a long season after its real birth; it then first runs up the interior of the style (which had till then been empty) and increases greatly; when the seed is to be impregnated it swells, and serves as the vehicle to receive the pollen, which soon dissolving in the sweet juices of the line of life, runs down again to each seed, which it touches; when the heart is directly filled up, and the seminal leaves (the lungs of the embryo) proceeding from the interior cavity under the heart, *begin to grow*.

In the first months of pregnancy the *involucra* bear a large proportion to their contents, but afterwards this proportion is *reversed*. This is also the case with the seed, which evidently changes in this respect, though much quicker than the *fœtus*. The *placenta* (which is the medium through which the blood is conveyed from the mother to the fœtus) is a soft spongy substance like a cake, *closely adhering* to the inner surface of the womb; and the arteries of the uterus discharge their contents into this substance of the cake, and thus form the umbilical cord which is to nourish the child. Now this is most exactly imitated in the seed, except in the last *circumstance*, where *another vessel* called the nourishing vessel supplies the place of the umbilical cord, in *respect to yielding nutriment to the embryo:* since there is a broad spongy substance on the interior surface of the seed, *exactly resembling a cake;* and before the seed is divided from its

mother

mother tree, a certain set of veins arising in the *branch* from which the *seed proceeds*, and passing through the cover into the interior, is *thoroughly filled* with a *proper liquid*, to be used as soon as the division takes place: the seed is then divided from the *tree*, and the vein closed, which has in the mean time spread all its *meandering branches on the cake*, and now open their tiny mouths to produce (with the joint juices of that spongy matter) the sort of *milky fluid* to nourish the young plant, till the root is formed for the purpose. But the seed has one advantage unknown to the fœtus: "*if* at the first shooting of the root it is *destroyed*, the cake again receives the last juices remaining in the nourishing vessels, and forms with its assistance milk sufficient to supply the young one with support till the growth of the root is renewed." It may however be said, that the child is not exposed to a similar disaster, and requires not therefore such a remedy. This exact imitation of the fœtus with the embryo shows that I was right in persisting that the seminal leaves had nothing to do in nourishing the plant, and were merely formed to protect the embryo when leaving its exterior cover, and to serve *as lungs* to the *young plant*; and that, so far from bestowing nutriment, it required more itself than any other part. I showed this before I knew *how* the human fœtus was managed in this respect, and could not therefore be misled by the analogy, which is but too often the case when the knowledge of human anatomy precedes the dissection of botanical objects. The different covers also of the seed may be compared to the true and spongy chorion; the last is opaque and thick like the white cover of the seed, and the interior clear, as is the first *envelope* of the young one, while the heart of the seed first floats in a liquor that may well be compared to the *liquor amnii*. It is curious that in the latter months of pregnancy the spongy chorion becomes gradually thinner and more connected with the true chorion, which is also the case with the stony fruit and seed. The seed is so easily divided from the mother plant, is obliged to receive that assistance from the *cover of its seed* the *child* receives from its *mother*, and which nutriment the seed took in from its *parent plant* as long as it preserved its hold. The innumerable open-mouthed vessels in the outward exterior of its case, take in both *juices* and *gases*, to be seen flowing into the interior and changing constantly. It is truly astonishing to see the excessive *assistance* of this sort the seed receives daily before it is again placed in the ground, and before it forms *its radicle*: in opening one seed *each day*, I have rarely found it without some *new vessels* running from the exterior. The seed is of course infinitely quicker in forming than the fœtus; for

scarcely

scarcely a fortnight elapses after the *impregnation* of the *seed*, before all the important parts that can precede *maturity are completed.*

There are some curious changes in the form of a child before it breathes, and there is also a most curious difference in the seminal leaves in a plant when compared with other plants. They have no spiral wire, and of course no motion, and no midrib or side-ribs: this is very conspicuous in the *Mimosa sensitiva*, since it is the only part that does not move if touched. It was one of the proofs that enabled me to ascertain that the spiral wire was the real muscle of the plant.

There is little resemblance between the lungs of a human being and the leaves of trees—nevertheless they perform for *each being* the same office; and yet it is difficult to draw a comparison between them. The human lungs fill the greater part of the cavity of the breast; they are of a soft spongy texture, and are divided into two lobes, which are separated from each other by the mediastinum, and externally covered by a production of the pleura: each of these is divided into two or three lesser lobes. are commonly three in the right and two in the left side of the cavity. There are two series of arteries which carry blood to the lungs, and it is the exposition of this blood on the surface which changes its tint from a darker colour to a lighter red. The pulmonary artery and veins are not intended for the nutriment of the lungs; but the blood in its passage *through them* is destined to undergo those changes, and become florid by a greater mixture of oxygen. Respiration constitutes one of those functions which are properly termed vital, as being essential to life: it consists of an alternate *contraction* and dilatation of the thorax, by first *inspiring air* into the lungs, and then expelling it from them *by exspiration.* When an animal has once breathed, it is easy to see how the motion may be continued; but what occasioned the first inspiration is not so easy to discover.

Now the difference to be discovered in the lungs of plants is, that they have little inspiration of air except in water-plants,—but most of the oxygen given out is taken in as water, then decomposed; the hydrogen drawn off, and secreted for the seeds, while the oxygen is again given out to the amosphere to *purify* and *invigorate it.* The two upper cuticles of the leaf yield oxygen, the under cuticle *takes in carbonic acid gas* in the night, which is quickly converted into a purer air as soon as the sun has power sufficient in the morning, and again *returned* to the *public stock.* Many plants fold their leaves at night; but the atmosphere loses not, since they give no gas at that time, but the edges and hairs of their leaves are taking in quantities of water;

water; nor do they open them in the morning till the sun has power enough to *decompose* the *liquid*. Then what clouds of apparent smoke pour from every hedge or tree, filling the morning air with vapour! I doubt not also that much of this pure gas is given out when the leaf forms its pabulum, from the decomposition of the several juices the hairs have collected to compose this new compound, in which all vital air is banished. Nor is it improbable that the hairy points may help in a bright day to decompose and set at liberty the oxygen of the bubbles of water without further process, as it certainly occurs to the green matter *or fresh water conferva* in Priestley's experiment, which is owing to the fine points of both. As those insects which have spiracula or breathing apertures, as *wasps* and *flies*, are immediately suffocated if oil be poured on them, so the leaves (if a brush is merely passed over them, giving them a simple varnish of oil) directly *grow black* and *die*. The confining the air within them, and stopping all their natural functions, is sufficient to kill them. That some thick flowers serve as respiratory organs as well as the leaves, I have no doubt, since there are in most of them *open air-vessels* for the purpose: when without leaves, therefore, the petals serve as lungs to the plant, as they always have that circular vessel alternately holding air or water surrounding them.

A tree in the winter will bear the closest confinement; but the moment its leaves appear and open, if it has *not* plenty of air it *dies*: hence the number killed in a hot day in spring. Nothing can be more different than the skin of a human being and the cuticle of a plant: in one point only they agree—that they have both many layers. The human *cutis* which covers the flesh is composed of *fibres* closely compacted, through which the ends of the nerves appear which give out perspiration, and on which skin innumerable *papillæ* are discovered coiled up, or spread out, and which seem to be calculated to receive the impressions of the touch, being the most in quantity where the sense of feeling is the most delicate, as in the palms of the hands and the fingers. This skin which will stretch to any extent and *contract again*, has been known in cases of dropsy to stretch a foot or two, and reduce itself when the water was *discharged*. It is always of a white colour, while the matter which covers it and lies between the cutis and epidermis is that which gives the tint to the fair, brown, or black person. This is called the *rete mucosum*; it is a mucous substance which may be dissolved in water, and which in *negroes* is *quite black*. Next to this is the epidermis or *scarf skin*. It is a fine transparent *insensible pellicle*, destitute of nerves and blood-vessels, and every where covering the true skin, which invests the body. It is in reality composed of several laminæ or scales, and is full of pores, through which the perspiration passes

from

from the ends of the nerves on the cutis. The use of this last skin is to protect the delicate nervous fibres which stand out from the true skin: it increases by pressure; and the more scales are added, the less that *nervous sensibility* is *felt*. This is the reason that hard-working people can put their hands in the fire or in extreme hot water, without being sensible of pain: the more this skin is increased, the less feeling they possess.

As the plant has no *nerves*, it can have no *feeling*; and as it has no *perspiration*, it can have no *pores*. The *marks* seen in the skin are merely the effect of the opening of the balls in the second and third cuticle, which contain first the water and then the oxygen. The first cuticle is perfectly impervious to water, and completely like gold-beater's skin, without figure. Air, however, passes it freely: but for this skin, how could the water be retained to be decomposed?

I have said that there is no perspiration in plants; indeed it was perfectly *impossible* that the vegetables could decompose water to give out oxygen, and at the *same time* perspire such *quantities*—and yet have juices enough *left* to support the plant. It seems to me that the incomparable Mirbel almost owned his unbelief long ago:—that those bubbles taken for water have proved to be *instruments* of various kinds and forms, and show themselves to be *undoubtedly* the most beautiful and *complicated part* of the vegetable arrangement. As in the animal body there are such a variety of secretions from the blood, which require an *immense mechanism*, not suited to the general simplicity of the form of a plant, so in its stead Nature has contrived that it should *receive* from the *atmosphere*, by means of these instruments or hairs, all the juices necessary to the vegetable, in lieu of forming them by *secretion:* but they are not received in their perfect state, but often alter greatly between their entering the *valve* and flowing into the plant: nay, I have frequently known two liquids enter the *hairs* and secrete themselves between the two different valves; then meeting explode, and compose a *third juice*, and thus form by chemical affinity the matter which the plant requires. This is really the most *beautiful part* of the study of plants; and to chemists would (I should conceive) serve as an *elucidation to many points*, if they would condescend to try them. Even oil is received by the hairs in a sort of vapour, and only becomes oil when it reaches the first liquid; and they both enter the third valve purified and completed. I have watched the various liquids innumerable times; nay, I might truly say, for months together—their manner of filling and emptying their hairs—the variety of their juices—the curious changes made in them—and the getting rid of the obnoxious part by means of the outward cuticle of the hair, and then sending down into

into the plant that which is completed. This account would fill volumes instead of a few pages, if thorough justice were done to so beautiful and inimitable a contrivance. But what pen or what tongue can properly display the works of Him who can conceive and form such productions!

Between the skin or cutis, and the muscles or flesh, there is interposed a loose oily substance. It is continued without interruption over the whole exterior of the muscles, and is called the *cellular substance*, filling up their depressions, and affording a smooth and regular surface for the skin to lie upon. This substance is composed of a cellular texture and *fat:* the latter is fluid in the body, and is *deposited* in *cells* in the former for facilitating muscular motion; and though found in the greatest quantity in the cells of the *membrane* filling up the space *between* the most external muscles of the skin, yet it may be met with in most other parts of the body. Just so is the *vegetable pith*, composed of a cellular texture, filling up the depressions the line of life frequently makes in its curious undulations, which every other part is obliged to follow: the only difference is, that it is not placed in the *same situation*, but owing to the softness of its texture it admits of the wood in very vigorous trees *increasing* both ways, and allowing also of the yearly swelling of the line of life, when the bud passes up its cylinders, from the root upwards. The pith serves also to contain water, instead of fat; and by its quick growth supplies each vacancy till the wood is able to fill its situation.

Having now brought forward (though in *so imperfect a sketch*) each different ingredient possible to compare together in the *human form* and *vegetable frame;* let me now see *what results may be drawn* from the comparison. First it proves, I think, that the vegetable frame is a mere *muscular creature, having life only*, but *no sensation—no sensibility*, but possessing alone the property of the muscles (*irritability*). The comparison proves also a matter of *great consequence* to the vegetable, *which is*, that as the only part in the *human* body possessing self-motion (independent of all other parts) or *vis insita* is *the muscle*, so the only part in the plant possessing the *same motion or* vis insita *is the spiral wire: that part* therefore *must be* the *muscle of the vegetable*, as I have long ago endeavoured to show by other arguments, as I never yet could find motion in any part that did not possess the spiral wire, and that the motion was always in proportion to the *quantity* or peculiar formation of the muscle: as the tendril of the vine admirably shows, which is composed of little else. We learn also that there are no secretions either from bark-juice or sap, but that Nature supplies them in the vegetable in a *different way*, drawing

them

them from the atmosphere, and then completing them in the hairs by chemical affinity.

Most of the accounts descriptive of the human body are selected from our best surgical books; Hunter, Haller, Monro, Burk, and Cooper. I have since, however, regretted I did not form the comparison between the *insect* or *amphibia tribe* and the vegetable, as, certainly approaching nearer together, they would admit a more perfect similitude; but I was fearful the dissection of the insect tribe would not be *sufficiently known, to interest*;—or the lungs of the frog's fœtus are so like the part of the *leaf* which serves as lungs *to the plant*; and the admirable manner in which the muscles of the snail display themselves, so well discriminate its *different parts* of tendon, muscle or *moving* part, and the nerve to which it is attached, as to take away all obscurity from the subject; which is however with the least attention plain enough. The inimitable Swammerdam, who studied the muscles most deeply, thinks their internal motion may arise from a similar cause with those of the vegetable.

I am, sir,

Your obliged servant,

Agnes Ibbetson.

XXII. *On Citric Acid.* *By* Samuel Parkes, *F.L.S. M.G.S. &c.*

[Concluded from p. 60.]

The juice of several other fruits, besides those of limes and lemons, will furnish the citric acid, either alone or mixed with other vegetable acids. Thus the cranberry (the *vaccinium oxycoccus** of Linnæus), the red whortleberry (*vaccinium vitis idæa*), the birds-cherry (*prunus padus*†), the berry of the nightshade (*solanum dulcamara*‡), and the hep, or fruit of the wild briar (*cynosbatus* vel *rosa canina*§), yield chiefly the citric acid ||, while in the red gooseberry (*ribes grossularia*), the garden currant

* As the fruit of this shrub is largely employed in most families, some persons may be glad to be informed that these berries may be preserved perfect for several years, merely by drying them a little in the sun, and then stopping them closely in dry bottles.

† Although this fruit is of itself nauseous, yet when bruised and infused in wine or brandy, it gives it an agreeable flavour.

‡ Linnæus says that an infusion of the young twigs of this plant is an admirable medicine in acute rheumatism.

§ The pulp of these berries, beat up with sugar, makes the conserve of heps of the pharmacopœia. Mixed with wine it is an acceptable treat in the north of Europe.

|| Westrumb has shown that the juice of the common cherry contains scarcely any other acid than the citric.

(*ribes*

(*ribes rubrum*), the bilberry (*vaccinium myrtillus* *), the hawthorn (*cratægus aria* †), the black cherry (*prunus cerasus* ‡), the wood strawberry (*fragaria vesca* §), the knout or cloudberry (*rubus chamæmorus* ‖), and in the raspberry (*rubus idæus*, which has also the property of rapidly dissolving the tartarous incrustations on the teeth), it is mixed with nearly an equal proportion of malic acid. From all of these the citric acid may, however, be separated by means of chalk, if ever a scarcity of limes and lemons should occur.

Simon Pauli has published the method of making a concrete salt from the juice of the barberry; and directions for making barberry-punch of a beautiful red colour are given by Weigleb. It may also be worth mentioning, that sometimes there is a large quantity of damaged tamarinds in the market, which might be bought cheap enough for the use of calico-printers, and which would afford their citric acid to calcareous earth, in the same way as the common lemon juice. Vauquelin, who has made direct experiments upon the tamarind, (*Ann. de Chim.* v.) has shown that one pound of the pulp contains one once and a half of solid citric acid ¶.

The chemical affinities of this acid, and which were determined by Bergman and by Mons. de Breney, of the Academy of Dijon, are in the following order:

	Oxides of	
Barytes,	Manganese,	Mercury,
Lime,	Iron,	Antimony,
Magnesia,	Lead,	Silver,
Potash,	Cobalt,	Gold,
Soda,	Copper,	Platina,
Ammonia,	Tellurium,	and
	Arsenic,	Alumina.

* The juice of this berry has been employed to stain paper, or linen, purple. In autumn the moor-game chiefly live upon the product of this shrub.

† The wood of the cratægus aria, on account of its great toughness, is much used for making wheels and axletrees; it is also so hard and smooth a wood that it is highly esteemed for making the handles of sundry tools used by carpenters and others.

‡ The gum which exudes from this tree is extremely nutritive; indeed, it is equal in every respect to gum Arabic. Hasselquist, in the account of his Travels, octavo, London, 1766, relates that a hundred men, during a siege, were kept alive for nearly two months without any other sustenance than a little of this gum taken occasionally into the mouth, and suffered gradually to dissolve. The wood is in great estimation with the turner.

§ It has been said that this fruit has the property of dissolving the tartarous incrustations upon the teeth, and that hence, those who have been afflicted with gout and nephritic disorders have found great relief by eating them very freely.

‖ The inhabitants of Norway pack the cloudberry in casks, and send them to the capital of Sweden, where they are served up in desserts. The Laplanders bury them under the snow, and thus preserve them fresh from one year to another. Withering, 8vo, vol. i. Birm. 1776, page 303.

¶ The rubus idæus has also the property of dissolving the tartarous incrustations on the teeth, but in this respect it is inferior to the strawberry.

It would be improper to dismiss this subject before we speak of the application and general uses of the citric acid.

Besides the medicinal* uses to which the citric acid is applied, such as the making of saline draughts, &c., it is often combined with opium to prevent the stupifying effects of that very powerful narcotic; and I have been told that it has been given with good effect to counteract such an over-dose of it as would otherwise have proved fatal. In domestic œconomy the citric acid is employed with advantage as a condiment for a variety of food, it being more grateful than vinegar on account of the fine aroma derived from the essential oil peculiar to the rind of this fruit. Likewise for taking out iron-moulds or other stains of iron; as this metal is not only soluble in citric acid, but the solution is so permanent that it is not precipitated again by the application of soap, which sometimes happens when other salts are used for this purpose. It is useful also for removing spots occasioned by alkalies, or the alkaline earths, from scarlet cloth;—for making a solution of iron for bookbinders, to give the mottled appearance to the surface of calf-skin;—for improving that peculiar solution of tin which is employed for producing the most exquisite specimens of the scarlet dye;—for altering the hue of some colours which are used in the dyeing of silk;—for the preparation of the best Morocco-leather;—for improving candles, by whitening and hardening the tallow which is employed in making them.

A very considerable quantity of citric acid, in the state of lemon-juice, is also consumed by the seamen of the British navy†. It was formerly bought in this market, having first been submitted to the judgement of a respectable chemist, who was regularly employed for the purpose. As the managers of this department have found that it can be brought from Sicily at a much more reasonable price, and of the best quality, they now procure it from that island, and decline all other sources. To preserve it on long voyages, about ten gallons of brandy are generally put to every 100 gallons of juice, which has the effect of precipitating the mucilage, that otherwise would not fail, during a voyage, to throw the whole into a state of fermentation, and convert much of it into vinegar. Dizé, in his Memoir read to the Institute of France, says, that a lemonade of the most agreeable taste and appearance may be made by dissolving forty grains of citric acid in a pint of water, with the addition of a sufficient quantity of refined sugar, and that it may be rendered fragrant by dissolving in it a small quantity of *oleo-saccharum*, prepared by rubbing a

* It is said that although citric acid curdle the milk of most animals, it has not that effect on woman's milk, whether used hot or cold.

† Lord Anson and Captain Cook were, I understand, the first navigators who employed lemon-juice on their ships' crews for the prevention and cure of the sea-scurvy.

lemon

lemon on a lump of sugar. The sugar, by imbibing the volatile oil of the lemon, renders it soluble*.

Other domestic and œconomical purposes might be enumerated to which citric acid is applicable, especially now that it can be kept in its crystallized state, in every family, and that it is in such form liable neither to total decay nor even the slightest alteration.

An aceto-citric acid has been much employed in various processes of dyeing by Mr. Guhliche. He calls it vegetable acid spirit, and prepares it in the following manner:

He takes any quantity of lemons (those of which the rind is decayed will do), he removes the peel and the skin that adheres to it, and slices them into a vessel, which ought not to be made of wood. He then sprinkles them with a quantity of good vinegar; squeezes out the juice through a flannel, by means of a press, and filtrates the expressed liquor through paper. It may be used in this state; but as it is apt to grow mouldy, he directs it to be purified and concentrated as follows:

The liquor is to be exposed to the sun, until it forms a sediment, and grows clear. It is then to be filtrated, and distilled in a sand-bath. The receiver is to be changed when the liquor that drops becomes acid, and the distillation continued till oily streaks are perceptible in the neck of the retort. The acid now found in the receiver is to be kept for use.

The largest quantity of citric acid is consumed by the calico-printer, who uses it in several processes of his art, especially to discharge iron, and to preserve white grounds on certain parts of any pattern, by means of the property it has of resisting the operation of sundry dyes, as is more fully explained in the Essay on Calico-printing.

The person who first prepared this acid in large quantities in England, and principally for the use of the calico-printers, was Mr. Coxwell, late of Fleet-street, who sold it at a high price, in the form of neat white crystals. With this the printers were supplied for many years; but when the demand became greater than he could well answer, he began to furnish them with the brown crystals, that is, with the acid in the first stage of crystallization, and consequently at a much more reasonable price.

This urged the printers pretty generally to turn their attention to the preparation of the citric acid; and now most of the houses

* By this means it is easy to preserve the volatile oil of any number of lemons. The oleo-saccharum thus obtained, may be mixed in a mortar with a sufficient quantity of sugar; and when this has been dried by a very gentle heat, it may be kept in well stopped bottles, unimpaired, for any length of time. This is preferable to the volatile oil of lemons, obtained by distillation, for the fire generally imparts an unpleasant or empyreumatic flavour to this and most other essential oils.

buy lemon-juice for themselves, and use it in different states of concentration, according to the nature of the processes to which it is to be adapted.

For most of the styles of work in which citric acid is employed, they find that the common impurities in lemon-juice are of very little consequence; and that when the juice itself has undergone no other process than that of being concentrated by evaporation, it very well answers their purpose, and supersedes the use of the crystallized acid.

As there is always abundance of spare heat in every large calico-print work, the process of evaporation may be conducted at no expense; and if it be continued very slowly for some considerable time, the acid will be sufficiently concentrated, and this without waste. For when the evaporation is thus managed, the vapour which arises will carry off none of the acid; and when it arrives at a certain point of concentration most of the mucilage will be separated, one part of which generally rises to the surface, and the other precipitates.

It is time, however, to come to the particular point which first induced me to think of writing on this subject, namely, the knowledge of this circumstance, that the printers depend solely on the hydrometer for ascertaining the goodness of lemon-juice, which made me very desirous of recommending to them some less fallacious guide.

Various reasons may be adduced to show the uncertainty of the hydrometer, but one or two will suffice. It is possible, for instance, that two parcels of juice may appear to differ very much when examined by the hydrometrical test, from the difference of the quantity of mucilage, though the same parcels may be of equal goodness as to the produce of the crystals, or real acid that may be procured from them. In like manner, a pipe of lemon-juice may be sophisticated with other acids, so as to make it pass the hydrometer, and yet be worth little for the printer's use.

On these accounts it appears to me that there can be no safety in purchasing this article, unless the buyer avail himself of the use of chemical tests *.

Whenever, therefore, a parcel of lemon-juice is offered for sale, the first thing should be to take its specific gravity by the hydrometer, or by a gravity bottle, and note that down. Should it prove to be only of the specific gravity of about 1·0156, which is 16 ounces 4 drachms to the wine pint, it may at once be rejected as not worth purchasing; for, good juice varies from a

* An experienced calico-printer has assured me that fine wheat flour is sometimes employed to adulterate lemon-juice. It certainly may be so incorporated with the mucilage as to affect the hydrometer considerably.

specific

specific gravity of 1·0312 (16 ounces 8 drachms) to that of 1·0625, or 17 ounces avoirdupois the wine pint.

Should the parcel appear good by the hydrometer, take then, for the next experiment, a wine glass of the juice, dilute it plentifully with pure water, and add to it a few drops of a solution of acetate of lead, as directed at page 51: this will produce an immediate precipitate, chiefly of citrate of lead, which should then be treated with a little pure nitrous acid, and the effect carefully observed; for, if the nitrous acid dissolve the whole of the precipitate, it will prove that the juice under examination contains no sulphuric acid. By proceeding in the same way with an acidulous solution of nitrate of silver *, instead of acetate of lead, it will be seen whether the juice contains any muriatic acid; this and the sulphuric being the only two acids, except the acetic, that are ever likely to be employed in the adulteration of lemon-juice.

There were indeed instances, when lemon-juice was scarce in the market, of its being adulterated with vinegar; and should this occur again, it may be readily detected by saturating a sample of the juice with carbonate of lime (whiting or pure chalk), and when the calcareous citrate precipitates, taking the specific gravity of the supernatant liquor. For, as lime is entirely soluble in acetic acid, part of the whiting employed will be held in solution by the vinegar, and occasion the supernatant liquor to be specifically heavier than it would have been had the sophistication not been practised.

To render this fact more certain, I made the following experiment. Having purchased a few good lemons, and pressed out the juice, I found its spec. gravity to be 1·0384. This was put into a capacious phial bottle, and when loosely corked was placed in a secure place in my common sitting-room, the temperature of which was usually at about 60 degrees. When it was examined at the end of sixteen days, the mucilage had in a great measure deposited, and the specific gravity of the juice was found reduced to that of 1·0352.

The weight of the juice under experiment, and after it had passed the filter, was 2400 grains, and the deposited mucilage weighed 133 grains, or one nineteenth of the whole.

The clear juice being divided into two equal portions, to one of them a fourth of its weight of common vinegar was added. Finding that the unadulterated portion amounted to 1200 grains,

* To prepare the nitrate of silver, dissolve a drachm of lunar caustic, which may be procured from any respectable apothecary, in one ounce of soft water, and to this add half an ounce of strong nitrous acid, previously diluted with an equal portion of water. When the impurities have precipitated, pour off the clear supernatant liquor, and preserve it in a stopped phial for use.

an

an equal quantity was taken of that which contained vinegar mixed with it, and both these parcels were accurately saturated with carbonate of lime*, and left 24 hours undisturbed until the citrate of lime thus composed should be deposited.

On pouring off the supernatant liquors from the two parcels of calcareous citrate, and drying both their precipitates equally, the following results presented themselves. The citrate of lime from the pure juice, when perfectly dried, weighed 138 grains, while that from the adulterated parcel weighed only 129 grains. The specific gravity of the supernatant liquor from the former was 1·0076, or 16 ounces and 2 drachms to the wine pint; but that from the latter, in consequence of the lime taken up by the vinegar, was 1·0135, or 16 ounces and 3 drachms and a half to the pint. By a similar experiment, the adulteration of lemon-juice with vinegar may at any time inevitably be detected.

When it has thus been found that the lemon-juice contains no admixture of other acids, the next step ought to be to ascertain how much real citric acid any given quantity of it actually holds in solution. For though it may not have been designedly sophisticated by any extraneous matters, it may be very inferior from other causes, the extent of which can only be accurately discovered by the application of another chemical test.

For instance, the fruit of which it was made may have been gathered in an unfavourable month, after the descent of the heavy rains; or deteriorated by the spontaneous decomposition which is occasioned by age; or it may have been diluted with water; all of which may be apprehended, when it is not known how many hands the juice has passed through before it comes into the possession of the consumer.

The mode which naturally suggests itself of ascertaining this point, is by means of an alkali; for, if it be proved by the foregoing tests that the juice contains no admixture of other acids, the amount of the crystals of citric acid which it will yield will be in exact proportion to the quantity of alkali which it requires for its saturation.

To adapt this theory to practice, all that is necessary is to select some alkali, such as pearl ash, or the crystals of soda, of which a solution is to be made, something below the point of saturation; and when this has been filtrated through blotting paper, and its specific gravity noted down, it will be ready for use, and should be carefully laid by, to have recourse to whenever occasion requires.

It will be necessary also to procure some of the test-paper

* The 1200 grains of pure lemon-juice required 98 grains of carbonate of lime, whereas the same weight of acid previously sophisticated with vinegar took 102 grains for its complete saturation.

which

which is generally used for acids and alkalies. For this end, I know of none better than the common blue litmus paper, which may be bought in London by the sheet. This paper should be kept in a stoppered bottle, or closely folded in paper, to preserve it from the action of the atmosphere, as well as from the light, and this will always prove an excellent test to discover the presence of an acid, which changes the blue to a red; and the same paper, being slightly tinged by means of a very dilute acid, becomes equally delicate as a test for alkalies. A very *weak* mixture of vinegar and water answers this purpose.

The use which the printer will have to make of these papers, is to ascertain when the portion of lemon-juice under examination is exactly saturated with the alkali. He may, therefore, proceed with it in the following manner:

Let one thousand grains, or any given quantity, of the lemon-juice be weighed out; and having previously balanced a phial of the solution of alkali in an accurate pair of scales, let a portion of the alkali be gradually added to the juice, which should be well stirred with a glass rod, or poured from one vessel to another, between every addition of the alkali. When the effervescence seems to grow less, a slip of the blue test-paper should be dipped in to ascertain whether the mixture approaches the point of saturation. If the paper comes out red, it indicates that the acid still predominates, and more alkali must therefore be cautiously dropped in. After waiting a minute or two, to allow time for them to unite, another slip of test-paper is to be applied, and in this way repeated until the liquor has lost the power of occasioning any change in the colour of the test.

When this is the case, a slip of the reddened test-paper should be dipped in, which will acquire a blue colour if too much of the alkali has been used; but if the liquor be brought to that state that it will cause no visible change of colour in either the red or the blue litmus paper, the lemon-juice may then be considered to be accurately saturated, and brought to the neutral state.

Now, replace the phial of dilute alkali in the scale, note down how many grains in weight are requisite to balance it, and call this the quantity of alkali expended in saturating the juice. When the operator has by previous experiment determined how much of his standard alkali is necessary to saturate any given quantity of the pure crystals of citric acid, he may at once, by the common rule of proportion, say how much solid citric acid is contained in any particular sample of lemon-juice which has been submitted to the tests already described.

The method now recommended of examining the goodness of lemon-juice, will answer the purpose of any individual so far as respects his own purchases; but as the pearl ash of commerce contains

contains different proportions of impurity, and the crystals of soda are liable to effloresce, and thence to hold variable quantities of the water of crystallization, neither of these is fit for forming a standard by which both the buyer and seller may arrive at the same estimate of the value of any given parcel of juice.

Conceiving, however, that this would be a very desirable thing, I have thought of various expedients, and know of nothing which would be so generally convenient for effecting this purpose as the common salt of tartar of the shops, which, if bought of a respectable chemist, will be found to be a tolerably pure sub-carbonate of potash; and if this be preserved in a well-stoppered bottle from the action of moisture, it will be a useful test for the purpose under consideration.

In order to lay the foundation for a table of the quantities of pure alkali which are taken up by this acid in various states of concentration, I prepared some of this sub-carbonate of potash, and found that 544 grains of such alkali are required to saturate one ounce avoirdupois, or 437·5 grains of pure dry solid crystals of citric acid; or, that one ounce and a quarter, omitting fractions, will render neutral one ounce of the crystallized citric acid.

Conceiving, however, that, for the purposes of business, a solution of potash in pure water might be more generally useful than the dry salt of tartar, I formed an aqueous solution of that alkali, somewhat below the point of complete saturation, by dissolving two ounces and a half of the salt of tartar in thirteen ounces and a half of pure water, making in the whole one pound avoirdupois, and by means of this solution* the table No. II, at the end of this Essay, has been constructed.

Notwithstanding the general certainty that must result from this mode of procuring the test, it occurred to me that it would be more convenient to some calico-printers to use soda. I therefore prepared some crystals of this alkali in a state of purity; and having carefully abstracted the water of crystallization†, I found that 352 grains of this dry sub-carbonate of soda will accurately saturate an avoirdupois ounce of the pure crystals of the acid of lemons; and on this result has the table been formed which is marked No. III.

* These proportions of water and alkali were chosen, in order that the solution might be of such a strength that eight ounces of it would accurately saturate as much lime-juice as should contain one ounce of pure solid crystals of citric acid. The specific gravity of such a solution at 60°, when filtrated, will be 1·120, or 17 oz. 12¾ drachms to the wine pint.

† In order to separate the water of crystallization entirely, so as to procure a perfectly dry and impalpable powder of sub-carbonate of soda, it is necessary to expose the crystals to a very gentle and long continued heat; for when dried hastily the pulverized crystals will be apt to melt in their own water of crystallization, and become reconverted into a hard mass.

It

It is worthy of notice, that these tests may be employed, not only in purchasing lime-juice, but also in ascertaining the quantity of real acid in acid liquors of different degrees of concentration; a matter of considerable consequence to those printers who are desirous of being accurate in the proportion of acid which they design to use in any particular style of work.

As some of the calico-printers in the North of England have found it advantageous to buy lemon-juice for themselves, as mentioned at page 85, and use it, after it is concentrated by evaporation, without further purification, I would here take the opportunity of suggesting, that when the juice has been partially concentrated by being exposed for some time to a very moderate heat, it would be useful to remove it to a situation where the evaporation will be suspended, and where it can remain undisturbed for a day or two, to give time for the thickened mucilage* entirely to subside. It will then be advisable to draw the fluid off from the precipitated impurities, by a syphon, or other means, and to pass it through a filter before it be submitted to any further process of concentration.

If it be treated in this manner, I am of opinion that it will not only be sufficiently concentrated, but that it will also be sufficiently pure for almost every purpose to which the calico-printer can possibly have occasion to apply it.

However, should the printer be desirous of purifying his juice by the chalk process, as described at page 12, I see no reason why he may not evaporate the fluid by some such method as is directed above, and use it in any stage of its concentration that may be most suitable, without being at the expense of reducing it to the solid form.

To render this more easy, I have constructed a table of the quantities of purified acid in a given portion of liquor of different strengths, as indicated by their specific gravity; and another table of a similar nature for the unfinished acid, or brown crystals†, which I presume will be used with more propriety than the former, whenever it is necessary to ascertain the quantity of acid in the liquor of the first boiling. Both these tables will be found in the subsequent pages marked No. IV. and V.

* Fourcroy, who has examined this precipitate, says that "it is not a simple vegetable mucilage, for that when dried it is not soluble in boiling water; that if treated by nitric acid, it yields azotic gas, and is converted into malic and oxalic acids; and that thus it appears to have some analogy with gluten.

† It will be perceived, that the specific gravity of the solution of brown crystals is greater than that of the pure crystals. This I apprehend arises from the brown crystals generally retaining a portion of sulphuric acid.

No. I.

No. I.

A Table of the quantity of pure crystals of citric acid which may be obtained from different parcels of lime- or lemon-juice, as indicated by the proportions of *dry salt of tartar* required for their saturation. See page 108.

Quantity of Juice saturated by the Test.	Quantity of dry Salt of Tartar employed.	Quantity of Crystals of Citric Acid which may be obtained from one Gall. of such Juice.
1 Ounce Measure of Lemon-juice.	Grains.	Ounces avoirdupois.
	17	4
	21¼	5
	25½	6
	29¾	7
	34	8
	38¼	9
	42½	10
	46¾	11
	51	12
	55¼	13
	59½	14
	63¾	15
	68	16

No. II.

A Table of the quantity of pure crystals of citric acid which may be obtained from different parcels of lime- or lemon-juice, as indicated by the proportions of the solution of *salt of tartar* required for their saturation. See page 108.

Quantity of Juice saturated by the Test.	Quantity by weight Avoirdupois of the Solution employed	Quantity of Crystals of Citric Acid which may be obtained from one Gall. of such Juice	
1 Ounce Measure of Lemon-juice.	Drachms.	Ounces.	Drachms.
	4	4	0
	4½	4	8
	5	5	0
	5½	5	8
	6	6	0
	6½	6	8

To avoid extending this table, it may be remarked, that the solution of sub-carbonate of potash has been made of such a strength, that whatever number of drachms of it may be required to to saturate the acid in an ounce measure of lemon-juice, the same number of ounces of pure rhomboid crystals of citric acid may be obtained from every gallon, wine measure, of the said juice.

No. III.

A Table of the quantity of pure crystals of citric acid which may be obtained from different parcels of lemon-juice, as indicated by the quantity of *dry sub-carbonate of soda* required for its saturation. See page 108.

Quantity of Juice saturated by the Test.	Number of Grains of the dried Sub-carbonate of Soda employed.	Quantity of Crystals of Citric Acid which may be obtained from one Gall. of such Juice.
	Grains.	Ounces avoirdupois.
1 Ounce Measure of Lemon-juice.	11	4
	$13\frac{3}{4}$	5
	$16\frac{1}{2}$	6
	$19\frac{1}{4}$	7
	22	8
	$24\frac{3}{4}$	9
	$27\frac{1}{2}$	10
	$30\frac{1}{4}$	11
	33	12
	$35\frac{3}{4}$	13
	$38\frac{1}{2}$	14
	$41\frac{1}{4}$	15
	44	16

To prevent the possibility of any misconception respecting the nature of these tables, it may be proper to state an example of the use of each. For instance, if it be required to examine a parcel of lemon-juice by the dry salt of tartar, all that is necessary is to take an ounce measure, or the sixteenth part of a wine pint of such juice, and having noted the number of grains required for its saturation, to look for the same number in the second column of the first table, and opposite thereto, in the third column, will be seen the quantity of crystals of citric acid which may be obtained from a gallon of such juice. The Table No. II. is constructed in the same manner, for the use of a solution of salt of tartar to be prepared as directed at page 108. The Table No. III. is contrived in a similar way, for the use of the sub-carbonate of soda to be dried in the manner prescribed at page 108.

No. IV.

No. IV.

A Table of the solution of white crystals of citric acid.

Crystallized Acid.	Ounce Measures of Water.	Sp. Grav. at the Temp. 55 Degrees.	Avoirdupois Weight of the Wine-pint.		Quantity of Crystals dissolved by one Gall. of Water.		
			Oz.	Dms.	lbs.	Oz.	Dms.
1 Oz. Avoir-dupois.	Oz. 1	1·2187	19	8	8	0	0
	2	1·1395	18	$3\frac{3}{4}$	4	0	0
	3	1·1016	17	10	2	10	$10\frac{1}{2}$
	4	1·0799	17	$4\frac{1}{2}$	2	0	0
	5	1·0663	17	1	1	9	$9\frac{1}{2}$
	6	1·0575	16	$14\frac{3}{4}$	1	5	$5\frac{1}{4}$
	7	1·0494	16	$12\frac{1}{4}$	1	2	$4\frac{1}{2}$
	8	1·0433	16	11	1	0	0
	10	1·0352	16	9		12	$12\frac{3}{4}$
	12	1·0298	16	$7\frac{1}{2}$		10	$10\frac{1}{2}$
	14	1·0250	16	$6\frac{1}{2}$		9	$2\frac{1}{4}$
	16	1·0216	16	$5\frac{1}{2}$		8	0
	20	1·0162	16	$4\frac{1}{4}$		6	$6\frac{1}{4}$
	24	1·0135	16	$3\frac{1}{2}$		5	$5\frac{1}{3}$
	28	1·0115	16	3		4	$9\frac{1}{7}$
	32	1·0101	16	$2\frac{1}{2}$		4	0

No. V.

A Table of the solution of brown crystals of citric acid.

	1	1·2656	20	4	8	0	0
	2	1·1640	18	10	4	0	0
	3	1·1171	17	14	2	10	$10\frac{1}{2}$
	4	1·0937	17	8	2	0	0
	5	1·0820	17	5	1	9	$9\frac{1}{2}$
	6	1·0664	17	1	1	5	$5\frac{1}{4}$
	7	1·0566	16	$14\frac{1}{2}$	1	2	$4\frac{1}{2}$
	8	1·0526	16	$13\frac{1}{2}$	1	0	0
	9	1·0488	16	$12\frac{1}{2}$		14	$3\frac{1}{2}$
	10	1·0448	16	$11\frac{1}{2}$		12	$12\frac{3}{4}$
	12	1·0390	16	10		10	$10\frac{1}{2}$
	14	1·0332	16	$8\frac{1}{2}$		9	$2\frac{1}{4}$
	16	1·0312	16	8		8	0
	18	1·0273	16	7		7	$1\frac{3}{4}$
	20	1·0254	16	$6\frac{1}{2}$		6	$6\frac{1}{4}$
	24	1·0214	16	$5\frac{1}{2}$		5	$5\frac{1}{3}$
	28	1·0195	16	5		4	$9\frac{1}{7}$
	32	1·0156	16	4		4	0

The

The Tables Nos. IV. and V. are founded on actual experiments, undertaken for the purpose of ascertaining what would be the specific gravities of the solutions of this acid at different degrees of concentration. A single example will be sufficient to explain their design.

Suppose a printer, who uses purified crystals of citric acid, should find that a solution of the sp. gr. of 1·0162 is suitable for any particular purpose, he will learn, by reference to the Table No. IV., that he may prepare a similar solution by dissolving one ounce of crystals in 20 ounces of water, or 6 ounces $6\frac{1}{4}$ drachms in one gallon of water. The Table No. V. is constructed upon the same principle, and is intended for the use of those printers who employ the brown crystals of citric acid.

XXIII. *Proposal for a new Regulation of Weights and Measures. By* A Correspondent.

To Mr. Tilloch.

Sir,—Perceiving that you sometimes devote a portion of your valuable publication to the consideration of weights and measures, and as doubtless the bill at present before a select committee of the House of Commons has been perused by the greater part of your readers, I shall take the liberty of making a few remarks on the subject, and lay before you a plan for the simplification of an uniformity of weights and measures.

The great inconvenience which exists at present, owing to the intricate state of our ponderal system, must be obvious to every observer. This complicateness has long called for a remedy to prevent the inconceivable trouble arising therefrom, and to establish one weight and one measure throughout the British empire. The committee of the House of Commons of 1758 paid most laudable attention to the subject, to establish a standard universal and uniform. The committee of the present time, under whose directions the above-mentioned bill was prepared, have followed the former, and have even paid greater attention to it, by endeavouring to simplify the system; but they also have not attempted entirely to abolish the system at present in use, though such a thing would be highly desirable. The Royal Society also have exerted themselves, and have even offered a premium for the discovery of an invariable standard,—but none have appeared to be sufficient for establishing an universal standard: the only ones which have been at all approved are, the length of a pendulum vibrating seconds of time, and the arc of the meridian. The latter is that lately established in France, and is said to be superior to the other,

other, as being on a larger scale. But the objection to it is the inequality of the earth's surface; for it has been found that the degrees of the meridian vary in different longitudes even in the same latitude. The mathematicians who established this system objected to that of the pendulum, as depending on *gravitation* and *time*. But gravitation is uniform in the same latitude; and with regard to time, it is universally so, as it solely depends on the earth's diurnal revolution round its axis, which has never been found to vary. Thus, therefore, a superiority cannot be ascribed to the meridian system; and as that of the pendulum is more easily performed with accuracy, it must enjoy a higher approval. Another plan that has been proposed, is the space through which a heavy substance will fall in a second of time, which has been proved in the latitude of London to be $16\frac{1}{120}$ feet: this is, however, obviously too difficult to be performed with the requisite accuracy.

The pendulum system is that approved of and taken as the standard by the committee of the House of Commons, the length of which in the latitude of London has been found to be 39·13047 inches, of which 36 are established as the legal standard for the yard, and 12 such inches a foot. These subdivisions are certainly far from deserving any great approbation; but the committee are not desirous of completely altering the present system, and therefore still retain its subdivisions. From this measure that of liquid and the weights are effluent. And as the subdivisions and multiples of our present system are too well known to require a recapitulation here, I shall proceed to give a scale that might be adopted in establishing an uniformity of weights and measures. It will be easily seen that it is founded on the system established by the French Republic. The latter, as I have before remarked, is calculated from the arc of the meridian, and which, by order of the French Government, was measured between the parallels of Dunkirk and Barcelona. But as that of the pendulum is chosen by our Government, I shall make it the source of the plan:—the system of the pendulum is evidently more correct than the other, and that is more approvable.

The length of a pendulum oscillating seconds in the latitude of London is, as I have before stated, according to the committee of the House of Commons, 39·13047 inches, of which 36 were to make the standard yard. Now it would be better to make the pendulum itself the unit for measures retaining that name, and so increasing and decreasing by decimals, dividing the pendulum into decimal parts progressively by 10, and multiplying it after the same manner. Then shall we have:

1 Kilopendulum = 1000 pendulums
1 Hecto ...... = 100 do.

1 Deca

1 Deca	= 10	pendulums	
1 Pendulum ...	= 1	pendulum	= 39·13047 inches
1 Deci	= ·1	do.	
1 Centi	= ·01	do.	
1 Milli	= ·001	do.	

From this long measure proceeds that of liquid. The gallon at present in use contains 268·803 cubical inches, and is recommended by the present committee to contain 276·48 cubical inches, or 10 lbs. avoirdupois, being 3 per cent. larger; thus making the half-pint to contain 10 oz. or 17·28 cubical inches of pure water of $56\frac{1}{2}°$ by Fahrenheit's thermometer. This gallon is by the above bill divided into four quarts, eight pints, sixteen half-pints, though it would be a much enlargement of this gallon: yet to have a strict comparison we might make it to contain ten cubical pendulums, or equal to 391·3047 cubical inches. With this scale,

1 Kilogallon ..	= 1000	gallons	
1 Hecto	= 100	do.	
1 Deca	= 10	do.	
1 Gallon	= 1	gallon	= 10 cubic pendulums
1 Deci	= ·1	do.	
1 Centi......	= ·01	do.	
1 Milli	= ·001	do.	

From these measures proceed weights. It has been already seen that the gallon contains 10 lbs. pure water, whence 1 lb. pure water is equal to 27·648 cubical inches, according to the standard named by the said bill; which pound is by the same divided into 16 oz. each ounce of 16 drachms 48 scruples, or 480 grains. Now, for the reasons before stated, let this pound contain 1-10th of the gallon, or one cubical pendulum of pure water, thus:

1 Kilopound..	= 1000	pounds	
1 Hecto	= 100	do.	
1 Deca......	= 10	do.	
1 Pound.....	= 1	pound,	or 1 cubical pendulum
1 Deci	= ·1	do.	
1 Centi......	= ·01	do.	
1 Milli	= ·001	do.	

From the above measures the square measures might be derived. With regard to assaying metals, let both the carat and the ounce be abolished, and have another carat to be subdivided into 1000 parts, to be used both for gold and silver.

The next point I have to notice is the coinage. In this case also I would recommend a regular conformity with the weights and measures. This however might be laid over for the direction of the Government; since the consequence would be little even were the coins continued as they are. However, the moneys of account might be altered amongst the commercial world themselves, without any inconvenience, as making the guinea the unit, and dividing it into 10, 100, and 1000 parts, by a similar decimal division as that contained in the weights and measures. With regard to dry measure, that might be regulated by the liquid measure, retaining the same dimensions; thus, the bushel containing 10 gallons, or 3913·047 cubic inches, or 100 cubical pendulums; and other decimal divisions in proportion.

Should this article meet your acceptance, I shall take the liberty of offering you a further communication on the subject; in which, however, I shall confine myself to a review of the bill now in the committee of the House of Commons. In recommending this to your protection,

I am, sir,

Your very obedient servant,

August 7, 1815. R. W.

XXIV. *Some Observations on Steam-Engines; with a Table of Work done by certain Engines in Cornwall, from August* 1811 *to May* 1815, *both Months inclusive: showing the monthly Consumption of Coals by the Engines reported; the Number of Pounds of Water lifted one Foot high by the Coals consumed; and the average Work performed by each Bushel of Coals, expressed in Pounds of Water lifted one Foot high.*

In the year 1811, a number of the respectable proprietors of the valuable tin and copper mines in Cornwall resolved to have the real work ascertained which their respective steam-engines were performing, as it was suspected that some of them might not be doing duty adequate to the consumption of fuel; and for the greater certainty of attaining their object it was agreed that a counter should be attached to each engine*, and all the engines be put under the superintendance of some respectable and competent engineer, who should report monthly the following particulars in columns: viz.

The name of the mine; the size of the working cylinder; whether

* The *counter* is a train of wheels, working like clock-work, commonly attached to the beam in such a manner that every stroke made by the engine moves one tooth, so that the index tells how many strokes have been made since last examined. This is so shut up in a box that no person can get at it but the one intrusted with the key.

whether working single or double; the load per square inch in cylinder; length of stroke in the cylinder; the number of pump lifts; the depth in fathoms of each lift; diameter of pumps in inches; time worked; consumption of coals, in bushels; number of strokes during the time; length of stroke in pump; load in pounds; pounds lifted one foot high by a bushel of coals; number of strokes per minute; and lastly, a column for names of engineers, and remarks.

Messrs. Thomas and John Lean were appointed to the general superintendance; and the different proprietors, as also the regular engineers of the respective mines, engaged to give them every facility and assistance in their power. Their first monthly report was for August 1811, and included eight engines, which had in that month consumed 23,661 bushels of coals, and lifted 126,126,000 pounds of water one foot high, being an average duty of 15,760,000 pounds lifted one foot high with each bushel of coals. In the months of September and October the engines reported were nine, and in November and December twelve; and it now evidently appeared that the regular publication of Messrs. Leans' very useful Tables had already been attended by some improvements in the condition of the engines; for the average duty for December 1811, extracted from these tables, appears to have been 17,075,000 pounds.

In January 1812 the number of engines reported was 14, and by the end of that year they were increased to 19; and the average duty performed by all the engines in the last-mentioned month had advanced to 18,200,000.

In 1813 the number of engines included in the monthly reports continued to increase till in December they were 29, and the average work 20,162,000.

During some of the months of 1814 the engines reported were 32, and the average duty performed during December was 19,784,000 pounds lifted one foot high with each bushel of coals.

The Table which is subjoined is an abstract from Messrs. Leans' Reports, and has been formed by first counting how many engines are reported, as in January 1815, 32 engines; then adding up the column containing the quantity of coals consumed by all the engines during the month, and putting down the amount, 110,824; in like manner adding up the column of pounds lifted by each engine one foot high by one bushel of coals, the amount of which was 637,320,990; and lastly, dividing the latter quantity by 32, the number of engines at work, to obtain the average duty performed, viz. 19,916,250 pounds.

TABLE.

		Number of Engines reported.	Bushels of Coals consumed by all the Engines.	Pounds of Water lifted one foot high by all the Coals consumed.	Average of Pounds lifted one foot high with each Bushel of Coals.
1811.	August - -	8	23,661	126,126,000	15,760,000
	September -	9	25,237	125,164,000	13,900,000
	October- -	9	24,487	121,910,000	13,540,000
	November -	12	30,998	189,340,000	15,770,000
	December -	12	39,545	204,907,000	17,075,000
1812.	January -	14	50,089	237,661,409	16,972,000
	February -	15	54,349	260,514,000	17,900,000
	March - -	16	59,140	274,222,000	17,138,000
	April - -	16	62,384	276,233,000	17,260,000
	May - -	16	51,903	273,546,000	17,096,000
	June - -	17	50,410	288,076,000	16,940,000
	July - -	17	51,574	300,441,000	17,677,000
	August - -	17	44,256	314,753,000	18,510,000
	September -	18	46,536	348,396,000	19,355,000
	October -	18	53,941	321,900,000	17,883,000
	November -	21	57,176	381,460,000	18,160,000
	December -	19	55,784	341,803,000	18,200,000
1813.	January -	19	60,400	363,906,000	19.153,000
	February -	22	58,044	438,737,000	19,940.000
	March - -	23	73,862	440,612,000	19.157,000
	April - -	23	61,739	431,032,000	18,700,000
	May- - -	24	58,890	463,346,000	19,300,000
	June - -	24	53,110	470,157,000	19,590,000
	July - - -	23	56,709	443,462,000	19,281,000
	August - -	21	50,110	416,898,000	19,852,000
	September -	22	58,008	427,148,000	19,415,000
	October- -	26	74,796	488,671,000	18,795,000
	November -	28	77,135	537,958,000	19,212,000
	December -	29	86,273	584,721,000	20,162,000
1814.	January -	28	91,753	550,751,000	19,670,000
	February -	26	78,986	536,677,000	20,641,000
	March - -	28	109,904	565,406,000	20,193,000
	April - -	29	91,607	576,617,000	20,325,000
	May - -	28	79,437	569,319,000	20,305,000
	June - -	30	75,343	626,669,000	20,888,000
	July - - -	27	85,224	573,208,000	21,229,000
	August - -	26	70,443	545,019,000	20,961,000
	September -	27	78,167	560,608,000	20,763,000
	October -	32	75,080	630,704,000	19,709,000
	November -	32	82,000	637,322,000	19,916,000
	December -	29	84,669	573,744,006	19,784,276
1815.	January -	32	110,824	637,320,990	19,916,250
	February -	33	101,667	710,271,250	21,523,370
	March - -	34	117,342	706,071,990	20,766,820
	April - -	35	105,701	695,212,340	19,863,210
	May - -	34	107,530	669,299,140	20,479,350

From

From the foregoing Table it appears that the average duty of the engines reported, exclusive of Woolf's patent engine, is at this time about 20 millions.

We have purposely kept out Woolf's patent engine*, because one of the ends intended to be gained by the monthly report of work actually done by the engines employed in the mines, particularly in pumping, was to know the comparative merit of Woolf's engine with two cylinders when contrasted with the steam-engines in common use. One of Mr. Woolf's engines has been lately erected at Wheal Vor mine, of 53 inches diameter in the great cylinder (the smaller cylinder being about one-fifth of the contents of the great one) and nine-feet stroke. According to Messrs. Leans' Report for May, the duty performed by the engine alluded to, was 49,980,882 pounds lifted one foot with every bushel of coals consumed; and by letter we are informed (for the printed Report has not yet reached us) that the duty performed by Woolf's engine in the month of June was 50,333,000.

Thus it appears that the average duty of the Patent Engine for the months of May and June was *fifty millions*, while the aggregate average duty of the other engines is only *twenty millions*. From this it is evident that Mr. Woolf's improvements on the steam-engine will be productive of much benefit to the mining interests of the kingdom. On some of the large mines when this engine shall have come into general use, which it must do sooner or later, the saving in fuel only, will add to the yearly dividends among the proprietors, several thousand pounds sterling. Nor is this all: the expense that will thus be saved will prevent numbers of mines from stopping work; and will be the means of setting many again to work which have ceased on account of the expense necessary to keep them free from water.

Those of our readers who are not acquainted with the magnitude of some of the mining concerns in this kingdom, can form no adequate opinion of the importance of such a saving in the article of fuel, as is effected by Woolf's engine. They may, however, form some idea of it, when informed that the expense of one of the large mines for coals only, to work their engines, and keep the mine free from water, is about twenty-five thousand pounds a year!

The improvement by which this great saving is effected, is not the only one that Mr. Woolf has made upon the steam-engine. He has, since the date of that patent, applied for and obtained a patent for another improvement, by which he expects to be able to effect a still greater reduction in the quantity of fuel required

* For a description of Woolf's Steam Engine, see vol. xix. p. 133; vol. xxiii. p. 123 and 335; and vol. xlvi. p. 43.

quired to perform a given quantity of work. Of this improvement we shall give some account in the article immediately following.

XXV. *Specification of Mr.* Arthur Woolf's *latest Patent for certain Improvements in the Construction and Working of Steam-Engines.*

"I the said Arthur Woolf do hereby describe and ascertain the nature of my said invention, and the manner in which the same is to be performed, as follows; that is to say: the working cylinder of my said steam-engine has no bottom, but is inclosed in another cylinder, of such dimensions that the space between the two (which space I call the receiver) is equal to at least the contents of the working cylinder. The inclosing cylinder has a bottom, and the two cylinders are joined together at the top by flanches, or any other suitable means; and the lower rim of the working cylinder is about as far distant from the bottom of the inclosing cylinder as the distance between the sides of the two cylinders. (N. B. Though I have called the inclosing vessel *a cylinder*, it may be of any other figure fit to receive the working cylinder, as will be rendered sufficiently obvious by explaining my mode of working, which is as follows.) Instead of having a void space for receiving steam below the piston, or whatever may be used instead of a piston, I introduce below it, and into the receiver, such a quantity of oil, fat of animals, wax, or any other substance not too volatile, and which may be kept fluid by such a temperature as may conveniently be commanded, as shall, when the piston is at its greatest height in the working cylinder, fill all the space below it, and also fill the receiver up to the height of a few inches above the lower rim of the working cylinder. Things being thus arranged, if the engine is to be worked by the pressure of the atmosphere, the receiver has a communication with the boiler. This communication being opened, steam is admitted into and fills the receiver above the oil, or other fluid body. If the communication between the receiver and the boiler be now shut off, (by any of the means in common use, or by any other means,) and a communication be opened from the receiver to the condenser, (previously exhausted by the usual means,) a vacuum will be formed in the receiver, and then the pressure of the atmosphere, acting upon the piston, will cause it to descend in the working cylinder, pushing the oil or other fluid body before it, and causing the fluid body to ascend into the receiver, after which the steam is again admitted for the next stroke. If the engine, instead of being worked by the pressure of the atmosphere,

mosphere, is to be worked by the action of steam upon the piston, then the working cylinder must, as usual in steam-engines so worked, be furnished with a cover. In this case, whether the engine is to be worked as a single or as a double engine, instead of the communications usual in the engines in common use, viz. for the alternate admission and condensation of steam above and below the piston, the communications of my engine are to the upper part of the working cylinder, and to the upper part of the receiver; the receiver in my engine answering to the space below the piston in other steam-engines, so that when the receiver is open to the condenser, and the upper part of the working cylinder open to the boiler, the piston descends, and *vice versâ*. It may be proper to remark here, that though I have described my receiver as containing and surrounding the working cylinder, because I prefer this arrangement, yet the receiver may be a separate vessel connected with the working cylinder at the lower part. It is also proper to remark, that to prevent waste of steam by unnecessary condensation, and to keep up the temperature of the oil, or other fluid body employed, the receiver may be inclosed in a steam-case, or heat may be applied to it externally. Lastly, whatever arrangement or mode of working be adopted, there should always be some oil, or whatever other fluid body may be used, above the piston, to the height of a few inches, to prevent the passage of the atmospheric air or of the steam downwards by the side of the piston; and as the quantity of oil, or other fluid body, above the piston, may deviate from a given height by the working of the engine, means must be provided to restore it to the requisite height, as cocks, valves, or any suitable contrivance, regulated by a float or floats upon the surface of the oil, or by means of a pump or pumps, worked by the engine itself, or otherwise. By the interposition of the oil, or other fluid body, between the piston and the condenser as before described, all waste of steam, by passing the piston, is effectually prevented, and a consequent saving of fuel is effected.

"N.B. As it is usual for the pistons of steam-engines to work in steam-vessels of a cylindric form, I have in the preceding description, for the sake of perspicuity, spoken only of the piston working in a cylinder; but the piston may work in a four-sided or any other polygonal prismatic-formed vessel.

"In witness whereof," &c.

The accompanying sections (Plate II) which we have laid down from Mr. Woolf's Specification, will give those of our readers who may not be perfectly acquainted with steam-engines, a pretty correct idea of the arrangement and mode of working which he has specified.

Fig.

Fig. 1. In this the inner cylinder represents the working cylinder filled with oil, or any other fluid body, (perhaps water might be found to answer) up to the piston, with a portion of the same fluid over the piston. This cylinder having no bottom, the same fluid stands between it and the next cylinder (which Mr. Woolf calls his receiver) at a height proportioned to the distance of the piston from the top of its stroke. The outer cylinder in this figure shows the steam-case.—This is his arrangement for working by the pressure of the atmosphere. The steam being admitted only into the receiver, causes the oil or water to descend therein and rise into the working cylinder carrying the piston before it; when (the communication with the condenser being opened) a vacuum being formed, the oil or other fluid ascends in the receiver, and the piston then descends, when a fresh charge of steam is admitted, and the stroke of the engine repeated.

Fig. 2 differs from fig. 1 only in having the receiver placed beside the working cylinder, instead of inclosing the latter. For our part, we prefer the first arrangement.

When the engine is to be worked by steam upon the piston, instead of the atmosphere, then the working cylinder as well as the receiver must be covered, and the working cylinder, like the receiver, must have a communication with the condenser.

The advantage which Mr. Woolf's new improvement promises is, the impossibility of any steam being wasted by passing the piston—a source of more waste than is generally imagined.

*** In the plate of Mr. Woolf's boiler given in our last, the top of the boiler, fig. 3, was, by a mistake of the engraver, carried over the top of the brick-work. It should have been carried only to the inner edge of the brick-work.

XXVI. *On the Size best adapted for Achromatic Glasses; with Hints to Opticians and Amateurs of astronomical Studies, on the Construction and Use of Telescopes in general.* By W. Kitchener, *M.D.**

In reflecting-telescopes, Dr. Herschel says, the maximum of distinctness is much easier obtained in a speculum of six inches and a quarter aperture than in larger ones; and this was the size of the telescope he made his astronomical catalogues with, and in his hands it has worked wonders. Dr. H. observes, that the seven

* The greater part of this paper was originally published in "Practical Observations on Telescopes," by the same Author, which was noticed in the Philosophical Magazine for December 1814. The present paper is intended to form part of an enlarged and improved edition of the above work now preparing for the press by the learned author.

seven feet Newtonian has sufficient light with a single eye-glass, which gives it a magnifying power of 287, to show the belts and double ring of *Saturn* completely well. What can we wish for more? How many have expended large sums of money on telescopes, without having ever seen such an all-repaying sight!

Query: Can the acme of perfection be obtained in metals of larger diameter? Several of our first-rate practical and working opticians have candidly declared to me, they would not, for general sale, undertake to make speculums of larger size than nine inches, that would show a star round and neatly: and unless they will bear this grand ordeal, it has been the fashion, lately, to suppose its figure cannot be depended on for exhibiting any object with that faithful accuracy which is the *sine quâ non* of astronomical instruments.

That distinctness of vision appearing to be so limited, may not create one sigh from the breast of any minute philosopher, that further optical assistance cannot be given to his eye; and that art is, as I have before said, so circumscribed; I will venture to account for these impediments and boundaries from the operations of Nature herself; *i. e.* the rapid rotatory motion of the earth preventing the application of a higher power than 300 times being used with any advantage. This is so true, that, until this obstacle is removed, we need not hunt after monstrous telescopes, unless it be in the true hobby-horsical spirit, *for the sake of the pleasure arising from the trouble of using them*, and being disappointed. Beyond a certain size, telescopes *are only just as useful, as the enormous spectacles which are suspended over the doors of opticians.*

When the inventors of the achromatic glasses fixed the powers of their telescopes, it was no doubt done after due deliberation, and a conviction arising from experiment, that for planetary uses the proportion of the pencil of rays to the diameter of the object-glass was most proper when as forty to one, *i. e.* for common telescopes and common observers. Thus the thirty-inch, with two inches aperture, magnifies eighty times; and it may be considered a general rule, that to find the most effective magnifying power of a telescope for planetary use, multiply the diameter of the object-glass by forty or fifty: to bear more it must be a very fine instrument, and the planet near the meridian; by the proximity of the object to which, the application of magnifying power must always be governed. When the pencil is much less than one-fiftieth of an inch diameter, it is too diluted to perfectly excite the action of the eye: and, when applied to the planets, we lose in distinctness more than we gain, by the magnifying being in too high a ratio to the illuminating power. But we must take into the account not only the bigness but the brightness

ness of the pencil of rays, which will of course be in proportion to the brilliance of the object observed.

Some stars I have observed with a power which diminished the diameter of the pencil to nearly one hundred-and-twentieth of an inch; *i. e.* a power of 420, with an aperture of three inches and five-eighths diameter in the clear. I have never yet seen any object that appeared to require a greater power; and it requires a most perfect telescope, and every other favourable circumstance, to admit of this being used with any advantage. From the rapidity of the diurnal motion of the earth, the limited excitability of the eye, the impediments to vision arising from our magnifying the atmospheric medium we look through, in proportion as we magnify the object we look at, increase in so high a ratio to the magnifying power, that more than 100 for terrestrial and 300 for astronomical use rather impede than assist vision. And again, when we charge our telescopes with a higher power than 300 times, what very uncommon dexterity is required either to find the object, or manage the instrument! It is indeed fortunate that a higher magnifier is rarely needful, as it cannot be used to much advantage *till the atmosphere be removed, and the earth stands till.*

With a good achromatic of forty-six inch focus, and a treble object-glass of three inches and five-eighths in the clear aperture, I have seen that most minute point of light near the pole-star, with the following powers, measured by a dynameter invented and made by the late ingenious Mr. Ramsden: 40, 80, 150, 250, 350, 420, 700; and even with 1123 times the small star was still visible. Mr. William Walker, the astronomer, was observing with me, and also saw this. Mr. Charles Fairbone, mathematical-instrument maker, of Great New-Street, Fetter-Lane, saw it again very distinctly on the 30th August 1807. Mr. Samuel Pierce, telescope-maker, at Mr. Berge's, optician, Piccadilly, observed the same on the 26th May 1811. I believe the polar star is as proper as any, for a test of the perfection of a telescope, as to its light and distinctness; and as it is easily found, and always visible, it is the more desirable, as it is a more universally attainable test.

I mention the foregoing observations merely as an authenticated and curious fact, how far magnifying power could be carried on this object, as it was with evident detriment to vision when higher than 80, which showed this star more pleasantly, and the illuminating and magnifying powers for this object appeared to be in more perfect proportion than with any of the higher or the lower powers.

We should never use a greater magnifier than we absolutely want; the lower the power, the more beautiful and brilliant the object

object appears: the field of view is proportionately large, and uniformly good and distinct, and the motion of the objects passing it proportionately less: thus they may be observed with greater ease and quiet attention. But here it may be well to observe, there is no use in the pencil of rays being of larger diameter than the optic pupil, which is commonly calculated at one-tenth of an inch, varying in magnitude according to the brightness or obscurity of the object presented to it. The natural state appears to be that of dilatation; and the contraction, a state of violence produced by an effort originating in the mind: when the light is too strong, or the object too bright, we contract the pupil to intercept that excess of light which would injure the eye: when the light is faint, the pupil expands, that a greater quantity of light may enter the eye, and thus make a stronger impression upon it. This contraction and dilatation of the pupil may be observed by holding a looking-glass (or, what is still better, the lowest small speculum of a Gregorian telescope) before the eye at a window, and turning gradually from it, continually looking at the eye. It may be more easily and perfectly seen by attentively watching the eye of another. I think it is most agreeably observed in a fine full blue eye.

To ascertain the magnifying power of a telescope, if we measure the diameter of the aperture of the object-glass or speculum, and that of the little image of it which is formed at the end of the eye-piece, the proportion between these will give the ratio of the magnifying power. To measure the diameter of the pencil of rays with great ease and accuracy, Mr. Ramsden about the year 1775 contrived a clever little instrument, which he called a dynameter; for though, when single lenses are used, the power of a glass is readily discovered by dividing the focal length of the object-glass by that of the eye-glass,—in eye-pieces of the common construction, especially those of a negative focus, it is very difficult to measure in this manner; nor can it be done with any accuracy with those eye-pieces which are made for erect vision with four eye-glasses.

The dynameter is principally composed of a fine micrometer screw, and a divided plano-convex glass; by means of which the image of the pencil of rays is completely separated, and the diameter of it known to the greatest nicety. The wheel or head of the micrometer is divided into a hundred equal parts, and a figure engraven over every fifth division, which is cut rather longer than the others; 1, 2, 3, and so on to 20: but adding an 0 to each figure in calculating, it will then read off, 10, 20, 30, and so on to 200. The nonius is divided into 15, 10, towards 0, and 5 on the contrary side.

The revolutions of the micrometer head will bring the edge of the

the circle round it, and the division on the nonius, to coincide at 10: each division, therefore, is equal to the ten thousandth part of an inch.

Applying this little instrument to the eye-glass of a telescope, when adjusted to distinct vision at any distant object, and turning the micrometer head, the emergent pencil will begin to separate; and when the extreme edges are brought into contact, the number of divisions will show the diameter of it in thousandths of an inch; then reduce the diameter of the object-glass into thousands, and divide that sum by the diameter of the pencil, the quotient will be the real magnifying power. But as it is requisite for the emergent pencil of rays to be in the focus of the divided glass, a thin transparent piece of ivory, precisely one-tenth of an inch in diameter, is set in the sliding cover, to adjust for that distance, which must always be done before it can be used with accuracy.

When this transparent piece of ivory is brought over the hole in the cover of the dynameter, and appears perfectly round, the nonius will then be at 0, and is properly adjusted. Five revolutions of the micrometer screw will make a complete separation of the diameter of its aperture, which is one-tenth of an inch: and when the opposite sides are brought into contact, the nonius will coincide at the fifth division of it, which is five two-hundredths of an inch; thus dividing each tenth of an inch into a thousand equal parts. Another method of discovering the magnifying power, is to set the telescope in such a position opposite the sun, that the rays of light may fall perpendicularly on the object-glass; and the pencil of rays may be received on a piece of paper, and its diameter measured: then, as the diameter of the pencil of rays is to that of the object-glass, so is the magnifying power of the telescope. Or, thirdly, a thin piece of mother of pearl, with a very acute angle two inches long marked thereon, and only one-tenth of an inch at its base marked thereon; the length being divided into ten equal divisions, making a visible line to each division, with a figure over it,—these divisons will express or show the hundredths of an inch: apply this scale to the eye-tube of the telescope, observe where the emergent pencil of rays fills up a certain space at or near any of the divisions; multiply the diameter of the object-glass into hundredths on the scale, and the quotient will be the magnifying power.

Before any of these methods of finding the magnifying power be made use of, remember to look through the tube, and observe carefully if some of the object-glass be not cut off, and part of the original pencil intercepted by the stops in the tube, &c. This is a very common trick, and will render your calculation on the whole aperture erroneous; for in all cases the magnifying power of of telescopes, or microscopes, is measured by the proportion of the diameter of the original pencil to that of the pencil which enters the eye.

The degree in which magnifying power may be advantageously applied, depends so much on the perfection of the telescope and the state of the atmosphere, that it is hardly possible, by any general rules, to fix precise limits to it: but, to afford an opportunity of trying this and many other entertaining experiments, the day eye-piece should have a pipe-drawer; and the screw, which receives the tube containing the two first glasses, should be the same as the screw which fixes the eye-drawer to the telescope: and the two first eye-glasses should be made to separate (by a sliding tube within the pipe-drawer) from the third and fourth. This will give a very pleasing variety, and be extremely convenient to those who wish to obtain a certain, exact degree of magnifying power.

For large adjustments, and also that the telescope may be used for near objects, and occasionally be made to supply the place of a microscope, it should have a sliding tail-piece; and the tooth and pinion for the fine adjustment should be made carefully, so as to move easily and smoothly, that it may not shake the glass while adjusting it. This is one of those defects we must expect to find in instruments which are so very rarely used by those who make them—the workman not being aware of the great importance of the telescope being perfectly steady during the adjustment of the focus. For this purpose, there should be two steadying sliding tubes applied from the eye-end of the telescope to the stand. These will in great measure prevent the vibrations, which are such impediments to vision. When the eye is perfectly satisfied with the adjustment of the focus, let the telescope be so placed that the object may pass through the field, the instrument remaining at rest during the time: this answers better than running after it with rack-work.

The telescope should be suspended in the centre of gravity, and mounted on a portable and folding mahogany stand, with divided circles, and an universal polar adjustment. If the instrument be then placed in the plane of the equator, only one motion will be required to follow the object; which, when large magnifiers are used, is a very great advantage, as the tremors occasioned by the movement of the rack-work are of course proportionably diminished. And be it always remembered, that steadiness is of the first importance. When high magnifiers are used, we need every assistance that can be contrived; as, even with the best constructed stands, a person walking in the room will prevent our seeing distinctly; nay, the very pulsation in the body

body of the observer will sometimes agitate the floor enough to produce this effect.

The atmosphere always appears most diaphanous on those evenings when there is least wind; and vision seems better, perhaps, because the instrument is still. For this reason, and to avoid currents of air passing before the glass, whenever the weather will permit, let the telescope be taken out of doors; for it will never do its utmost unless it is placed on the ground, in the open air. If the instrument has been kept in a room, the temperature of which is much warmer than the open air, I usually take off the cap of the object end, and take out the eye-piece, and let the air pass through the tube for ten minutes; and for at least the same space of time we must carefully avoid all stimulating and bright objects; so that the pupil may be in its most expanded state. When the eye is thus prepared, the sensibility of the visual organ will be much increased. I have also found it very advantageous to occasionally rest the eye for a few minutes: this will restore its irritability, which is soon exhausted when stimulated by an intensely bright object: and when a light is necessary to find an eye-piece, or rectify the instrument, to prevent the adjustment of the eye being disturbed, I use a small lantern, which gives a very faint light only on one side, and that may be made dark.

For those who have not courage, or constitution, to brave the inclemency of midnight frosts and damps, the most steady way of supporting a telescope within doors, is by a clamp made to fasten on the sashes when the top sash is put down: the object-end of the telescope is then in the open air, and out of reach of the undulating motion occasioned by looking through a medium of atmosphere which is undergoing a change of temperature, by the cold air rushing into the warm room. By this contrivance we have almost all the steadiness of being on the ground, without being exposed to the cold, &c.

I must here endeavour to impress on the mind of my readers another most important observation: when they have done using the telescope, let the object-glass be taken out and laid in a dry warm place, for a sufficient time to evaporate the damp air, which on dewy evenings too plentifully condenses on the object-glass; and however closely the lenses constituting the object-glass are burnished into the brass cell, unless they are very carefully kept dry, the damp air will penetrate between the glasses, and produce a sort of fog, or sometimes an arborescent vegetation like sea-weed, which I have seen spread all over the object-glass. Unless these evils exist in a very extreme degree, experience has proved the only detriment they do to the perform-

ance

ance of the glass is, that it does not transmit quite so much light: and if the instrument be a very fine one, it is more advisable to put up with an almost imperceptible diminution of its brilliancy, than run the risk of destroying the telescope—for the object-glasses cannot be separated from each other, without perhaps irreparably disordering the adjustment; from the perfect harmony of which, the instrument may, possibly, in a great measure derive its superior excellence, from certain circumstances, which once disturbed can never be restored—the acme of perfection being always accidental.

Moreover, it is equally indispensable, when we wish to discern those delicate and minute objects which are the most interesting and curious exhibitions our telescopes display to us, and with the finest instruments are only discernible with the most favourable circumstances, that we should be in a position of the greatest ease: no cramp or painful posture must distort the body, or irritate the mind; the whole powers of which must be concentrated in the eye: for such is the sympathy between the various organs of the human body, that we may as well attempt to think accurately on two subjects at the same time, as to use two senses at the same moment:—each must be used alone. As our immortal Shakespeare has observed of listening, with such profound attention, that "each other sense was lost in that of hearing."

The smallest achromatic that can be used with effect for astronomical purposes is the three-and-a-half feet. These telescopes were originally furnished with three object-glasses of three inches and three-quarters diameter; but they are now usually made with two object-glasses of two inches and three-quarters aperture. With this telescope all the principal and most interesting celestial phænomena may be pleasantly observed: and indeed, if exquisitely perfect, it will discover the minutest objects in the heavens: and as there are more made of this than of the larger-sized telescopes, it is proportionably more easy to obtain a good one. In astronomical as well as in all other concerns, truth and perfection are the first *desiderata:* our telescopes only delude us, unless, like the juryman's oath, they display the truth, and nothing but the truth. And, in future, I hope astronomical amateurs will rather seek for *perfect* telescopes than *large* ones; for, as the pupil of the eye contracts and dilates *pro re-natâ*, bright objects would often be better seen by reducing the aperture, than by loading the telescope with magnifying power to save the eye from being drowned in light.

XXVII. *Memoir on the Nature of fat Substances.* By Henry Braconnot, *Professor of Natural History, and Director of the Garden of Plants at Nanci.*

[Concluded from p. 41.]

Action of the Nitric Acid upon Suet.

Messrs. Fourcroy, Alyon, and Vogel have already made some experiments on the action of the nitric acid upon fat; but these chemists seem to have considered the results of this action only as furnishing a medicament, which has been called, with sufficient impropriety, oxygenated pomatum; for, to speak the truth, this fat contains suet approaching the state of adipocire, fixed oil with nitric and acetic acids combined with these fat matters.

Twenty grammes of beef suet were put into a retort, and there were added 120 grammes of the nitric acid of commerce at 39°. The reaction between these two substances was not so prompt as might have been expected; for the acid was in full ebullition without any red vapours being extricated: at length the latter appeared, and distillation was continued until almost the whole of the acid had passed over: there was extricated but a small quantity of azotic gas mixed with carbonic acid, and a volatile oil which had the smell of coriander.

The suet thus altered and remaining at the bottom of the retort was melted several times in boiling water; the water was uniformly set apart: and we shall examine them presently. In the mean time we direct our attention to the fatty substance which refused to melt in boiling water; this weighed 13 grammes, had an acrid styptic taste, partly owing to the nitric acid which it still retained: it was much softer than the suet employed, and contained a quantity of fixed oil of a brown colour, very soluble in alcohol, besides a solid substance which was isolated from the oil by pressure in gray paper. This concrete substance was dissolved in a small quantity of warm alcohol; the mixture became hard on cooling; it was then pressed in gray paper, and a snow-white substance was obtained in fine needles of a pearly appearance: it is inodorous, not very sapid, pulverulent, soft and unctuous to the touch, precisely like spermaceti; but it is infinitely more soluble in warm alcohol, which takes up such an immense quantity that the liquor runs into a solid mass upon cooling. If the solution be sufficiently diluted with alcohol, snowy-like flakes are precipitated, formed of a net-work of very fine needle-like crystals. Ether and oil of turpentine also dissolve perfectly this substance, which crystallizes by the spontaneous evaporation of those solvents. It does not experience any sensible alteration from

from the acids. When distilled it passes into the receiver without changing its nature, and leaves but a slight charry residue in the retort. It enters into fusion at the temperature of $54\frac{1}{2}°$ R.; white wax in its state of purity melts at the same degree of heat, according to Bostock. From the properties which we have recognised in this substance, we cannot mistake it for a distinct species of adipocire; for it differs sensibly from that which we have obtained by the action of sulphuric acid upon suet, since the latter substance presents but a grainy crystallization like bees'-wax, and which is besides a little more fusible.

I now return to the waters which have served to wash the greasy matter proceeding from the distillation of the nitric acid on suet. These waters, when collected, deposited a flaky white matter; but as the liquor appeared still to retain a great deal in solution, doubtless in consequence of the nitric acid which existed in this liquor, it was evaporated to the consistence of honey, which became very solid upon cooling. This residue well washed with cold water, left nearly two grammes of a very white flaky and not very soluble powder; the water which had been used in the washing, kept in solution but a small quantity of yellow matter, but no trace of oxalic acid. M. Vogel had likewise observed, that by boiling nitric acid with fat or with beef suet, a white powder was formed upon cooling. M. Vogel considered it as mucous acid; but the properties which it presented to me do not permit me to adopt the conclusion of this respectable chemist. This powder, as I obtained it, is an acid of a sourish taste, but it has not the granulated and crystallized aspect of mucous acid. When exposed to heat, it melts like suet, and is entirely volatilized on a burning coal, giving out a white vapour. When distilled in a small retort, this acid passes into the receiver in the form of an oily liquid, which on cooling assumes the consistence of wax, without however losing its acid taste and other properties.

Warm oil of olives dissolves this acid, and a part is precipitated upon cooling: it is soluble in cold water but in small quantities. Warm water has a much stronger action over it; and when it is boiling, we may obtain a solution which upon cooling goes into a white mass, in which we can distinguish no appearances of crystallization. This acid dissolved in water precipitates several metallic solutions; with the acetate of lead, it produces a flaky white precipitate soluble in weak acetic acid: it forms also deposits in the nitrate of mercury and sulphate of iron. The latter is of a reddish colour; but it does not produce any change in the nitrate of silver, the sulphate of manganese and the sulphate of copper, although it produces with oxide of copper a salt which is soluble but in a moderate degree.

When joined with ammonia, this acid yields a salt unalterable in the air, soft, fusible like suet, and easily soluble in water: this solution does not becomes frothy by agitation; the nitrate of silver forms in it a magma like pitch, and which is coloured in the light. The sulphate of iron occasions a deposit which collects in clots like a fatty matter.

This same acid combined with lime furnishes a soluble salt, the solution of which evaporated spontaneously leaves a very white, shining, opaque and enamel-like residue. This salt is sapid, and unalterable in the air. With magnesia it gives a transparent combination, which leaves a coat of varnish on the vessels, from which we may detach it with the point of a knife: it then resembles mica.

To conclude: this salt is unalterable in the air, and soluble in water: we know that the mucites of lime and magnesia are insoluble. It forms with oxide of zinc a white salt tolerably soluble. We see that this acid has by no means the characters of the mucous acid: it seems, on the contrary, to resemble closely the pyro-sebacic acid of M. Thenard.

Action of the weak Nitric Acid upon Suet.

Twenty grammes of nitric acid at 39°, diluted with one-half its weight of water, were put in ebullition for three quarters of an hour with twenty grammes of beef suet: the latter was entirely decomposed, although but a small quantity of nitrous gas was extricated, mixed with azotic gas and carbonic acid: the greasy matter when well washed in boiling water had not sensibly lost its weight; but it was much softer than the suet employed, and was of a slight yellowish colour. This substance when heated with alcohol was dissolved in it with astonishing promptitude; the solution in cooling went into a white mass, which when contained in a fine cloth was subjected to the action of the press: I obtained an alcoholic liquid, which left after its evaporation about five grammes of a yellowish oil very soluble in alcohol, in ether, and in the alkalies. The solid matter left in the cloth formed the greater part of the suet thus altered by the nitric acid. It was very white and pulverulent: but in order to be very certain of obtaining it in all its purity, it was treated with essence of turpentine and pressed between some folds of gray paper: a substance was obtained analogous to white wax, but much drier, falling into powder between the fingers, and capable of waxing bodies. When melted and cooled, it presents a lamellated crystalline structure like certain kinds of adipocire, but not in fine and pearly needles like those which we have obtained by the action of a great quantity of nitric acid at 30° on the suet.

To

To conclude: This sebaceo-cirous substance, as well as all those formed by the action of the acids and alkalies on suets, is united instantaneously with the alkalies, and is dissolved with excessive promptitude in ether, in warm alcohol, and is precipitated from these solvents, sufficiently diluted, in the form of gelatinous flakes, as wax will do.

Action of the Muriatic Acid on Suet.

The muriatic acid has not exactly the same action on suet with the sulphuric and nitric acids: nevertheless it converts it in part into an oil not very soluble in alcohol. I boiled ten grammes of muriatic acid with as much beef suet, until the latter had acquired a fawn colour; when well washed in boiling water and cooled it was of a soft consistence, owing to two grammes and two decigrammes of oil formed during the reaction; but a very small quantity of adipocire only was produced, and the greater part of the suet remained without appearing to have undergone any remarkable alteration.

Action of the Alkalies on Suet.

We have seen that the acids, in acting on the suet, metamorphose it into a substance very analogous to wax, and into oil very soluble in alcohol. The action of the alkalies will offer to us nearly the same results. Beef suet has been saponified by potash; this combination dissolved in a great quantity of water did not deposit pearly matter such as has been described by M. Chevreul. I poured into this soapy liquor muriatic acid in a slight excess, which separated from it the fat matter, which when well washed in boiling water was kept in fusion during some time, in order to vaporize the water which it retained in its molecules: when cooled, it was softer than the suet which had served to produce it, and contained nearly one-third of its weight of an oil formed during saponification. This matter was melted, then mixed with a weight of alcohol equal to its own; the union immediately took place, and by cooling a concrete white mass was obtained, which was strongly compressed in a towel: there passed over a yellowish alcoholic liquid, which when left to the air for some time deposited flakes of adipocire; this liquid when evaporated left a fluid oil of considerable rancidity: it differs from most of the other fixed oils by the property which it possesses of being dissolved in alcohol and in ether with the greatest facility. It is also united instantly with the alkalies, and forms a very thick mucilage. With ammonia it gives a soap which water dissolves very easily; but when exposed to heat it is decomposed, and gives off a part of the ammonia: the oil becomes liquid again as before, but it retains pretty

 strongly

strongly a quantity of ammonia, which the potash renders very sensible. The adipocirous matter which remained in the cloth after expression, was redissolved in warm alcohol: the mass, when cooled and again squeezed, was afterwards pressed in gray paper, and we obtained a white adipocire greatly resembling those produced by the action of the acids on suet; nevertheless, it seems to have a greasier aspect, since it can also be reduced to powder: it is also a little more fusible, since it enters into fusion at 50° R. Besides, it is dissolved with the same facility in alcohol, ether, and the alkalies; which we do not observe in most of the fats, if it be not the adipocire of dead bodies.

When combined with potash it produces a very hard soap, which when diluted in water does not form a thick mucilage like that which results from the union of the oily matter of the soap of suet with potash. With ammonia it furnishes a soapy emulsion, which is precipitated in large flakes by an excess of ammonia, probably because the water has more affinity with this alkali than with this ammoniacal soap: nevertheless, the latter appeared to me to form a more permanent solution with water than the ammoniacal soap with the basis of adipocire, proceeding from the alteration of the suet by the acids. We see that all those adipocirous substances, whether they have been formed by the action of the acids or the alkalies upon suets, differ among themselves by shades so slight that we may safely consider them as so many varieties of one and the same species.

Action of Potash on Spermaceti.

This substance, which we may regard as a species of crystallizable suet, appeared to me to have less aptitude to saponify than the other suets, which require in general a long interval of time. In the soap of potash and of spermaceti, this last substance has absolutely lost all its characteristic properties; it does not crystallize any longer, becomes infinitely more soluble in alcohol, ether, and the alkalies, and partakes precisely of all the properties of the adipocire formed by the action of the alkalies on the suets. But what appears to me very singular is, that the spermaceti did not undergo the same metamorphosis from the application of the concentrated sulphuric acid, which appeared to have little action upon it.

Analysis of Marseilles Soap.

Twenty-five grammes of white soap of oil of olives, as sold in the shops, and of a good consistence, were cut into thin slips, and exposed to the heat of a sand-bath for several days, until it became very dry and brittle. It lost by this desiccation five grammes 34 centigrammes of water. It was dissolved in boiling

water,

water, and muriatic acid was poured into it in slight excess, shaking well the whole; there was separated a greasy matter which became fixed upon cooling, and which was much firmer than olive oil fixed at the same temperature. This greasy matter well washed and melted weighed 17 grammes one decigramme. The liquor from which it had been separated gave by evaporation four grammes of muriate of soda, made red hot, which represents, according to Kirwan's valuation (and which appears exact), two grammes 56 centigrammes of soda, which was in combination in the 25 grammes of soap employed.

The 17 grammes one decigramme of greasy matter were pressed for twenty-four hours, at the temperature of $+4°$ R., between several folds of gray paper, which absorbed the yellowish oily matter, very soluble in alcohol, ether, and the alkalies, and left two grammes three decigrammes of pure white adipocire: nevertheless, it was dissolved in a small quantity of hot alcohol, and the liquor was passed through a filter: on cooling it was concreted into a solid white mass, which was pressed in gray paper, and which was melted. This substance greatly resembles white wax: it has the external appearance, the granulated fracture, and the semi-transparency; but its consistency is drier, for it is reduced into a white powder between the fingers rather than melt; nevertheless, it may be cut, presenting in its section shining surfaces as in common wax, and like the latter it can wax bodies; but it is a little less fusible, and melts precisely at $50°$ R. like the adipocire obtained by the action of potash on beef suet. To conclude: this adipocire of soap strongly resembles all those of which we have spoken formerly, and like them it is dissolved with the greatest facility in alcohol, ether, and the alkalies.

It results from this analysis that 100 parts of white soap of oil of olives contain:

Water	21·36
Adipocire	9·20
Oil very soluble in alcohol	59·20
Soda	10·24
	100·00

Of the Soap of Wool.

I thought it right to examine the results of the action of potash upon wool. Consequently, to a solution of this alkali kept boiling in a glass vessel, I added successively ten grammes of white wool: it was dissolved with a very perceptible extrication of ammonia; the liquor diluted in water was of a brown colour and limpid, after having deposited a slight sediment: I poured into this liquor, still hot, some muriatic acid, which occasioned a considerable extrication of sulphuretted hydrogen gas, and there was precipitated a fatty mass of a brown colour, and having the smell of burnt wool: it resembled pitch; when melted in water to wash it, it allowed itself to be drawn into threads like turpentine. When cold it is dry, brittle and pulverulent, like a resin; it contains a notable quantity of sulphur, as I ascertained by fusing it on a silver plate, which became a deep black. Cold alcohol has no action on this substance; but when it is boiling it dissolves it nearly like wax, and upon cooling a yellowish flaky precipitate is formed; the liquor separated from this precipitate becomes milky by an addition of water, and contracts a smell and taste very strongly of spirit of cochlearia, owing to the presence of the sulphur.

Boiling oil of turpentine, as well as the fixed oils, exhibited no disposition to dissolve this substance, and in this respect it seems to be removed from fat bodies.

It is dissolved very easily in cold potash, and the acids may again precipitate it: it differs therefore from the oil which Papin's digester can extract from hair, which is very insoluble in the alkalies. When distilled it gave a brown grease, some sulphuretted hydrogen gas, and a liquid which renders the blue vegetable colours green: but no carbonate of ammonia is sublimed. This substance therefore contains azote, but much less than wool. From the ten grammes of wool dissolved in potash, I could precipitate by the acids only one gramme and two decigrammes of fatty sulphuro-azotic matter: hence the soapy liquor ought to contain some other parts of the wool soluble in water. In fact, by evaporation we obtained a yellowish residue in considerable quantity, composed of muriate of potash and a substance forming the greater part of the soap of wool. As alcohol easily dissolves it, it was easy to separate it from the muriate of potash. Thus isolated it was of a shining yellow colour, perfectly transparent like a gum, but of a peculiar odour, which I cannot better compare than to that of the grain of trigonella, *Fœnum græcum*; its taste has nothing remarkable. This substance is dissolved in ether, but better in alcohol; and particularly in water. The infusion of gall-nuts precipitates it entirely from its aqueous solution, under the form of a flaky white sediment soluble in alcohol.

The aqueous solution of the same substance is not disturbed either by the nitrate of barytes or the sulphate of iron: only the latter raises the colour; but the acetate of lead forms in it a flaky white precipitate entirely soluble in nitric acid.

When burned on a capsule of silver, it swells up, giving out the

the smell of burnt wool, and forms a black spot on the silver, owing to the presence of sulphur.

When submitted to distillation, it gave out ammonia and sulphuretted hydrogen gas, but much less than wool. In short, this animal substance contracts no combination with the alkalies, and it exists in the state of simple mixture only in the soap of wool.

Of Saponification.

Now that we have examined the results of the action of the acids and the alkalies on a fat substance, the nature of which is homogeneous, it is easy to conjecture what passes in saponification in general. When we make an acid or an alkali act on suet, the three principles which constitute it, viz. hydrogen, oxygen, and carbon, which were in a state of equilibrium, are separated and combined in another order to form adipocire, and an oil very soluble in alcohol. These two materials are indispensable to the making of soap of good quality; for it is to be noted, that when the fixed oils do not contain suet in a sensible degree, they cannot produce adipocire during their saponification, and for this reason they only furnish soft soaps, like those which we obtain with pure olive or other vegetable oils, and even with fish oil; but by adding to them a certain quantity of suet, they produce solid soaps.

We may also infer from the foregoing, that most of the oils and suets are not susceptible of uniting immediately with the alkalies, since this union cannot be effected but by a change of equilibrium in the elements of tallow or suet or of oil, which besides requires a prolonged ebullition during a considerable time, as the soap-makers very well know. It would even be a great advantage, and save much time and fuel, if by an œconomical and speedy method we could dispose the fatty substance to unite immediately with the alkalies; hitherto I have found only the concentrated sulphuric acid which can attain this object: it is sufficient to pour this acid into oil or melted tallow to dispose them to unite immediately with the alkalies and form a perfect soap on the instant: this mode appears to me so much the more profitable, that the last portions of alkalies, even those which are combined with the carbonic acid, may enter into combination with the fat matter; which is far from being the case in the common preparation of soap, since the water which passes off still contains alkali, which will not act upon fat.

Of Rancidity.

The results of the rancidity of fat had not yet been well appreciated, since all chemists have admitted that an acid was formed;

formed; but this fact has been called in question by Messrs. Thenard and Parmentier, and the latter throught that oxygen in combining with fat was the agent of its rancidity. This opinion did not appear to me to be supported by irrefragable experiments: we see that in the course of time fat undergoes nearly the same alteration with that which the acids and alkalies introduce, *i. e.* that it tends insensibly to approach the state of adipocire.

Some mutton suet, as it is employed in the manufacture of candles, had been left for five years in an open vessel, and at a mild temperature; its surface had acquired a yellow colour, but the interior of the mass was of a shining white, and of a very rancid taste and smell. This suet, when slightly melted and rubbed upon a paper coloured blue by turnsole, reddened it instantly: an acid therefore was formed. In order to separate the latter as well as the odorous principle which the rancid suet seemed to contain, it was boiled for some time with water in a retort, and I obtained by distillation a product of a very pungent and disagreeable smell of rancid grease, owing probably to the presence of a very expansible volatile oil; this same product reddened turnsole tincture, and consequently contained a volatile acid which exhibited to me the characters of acetic acid. The water remaining in the retort, and separated from the greasy matter, furnished by its evaporation a slight yellowish acid residue, transparent like a gum, and attracting humidity a little. This residue, which had a very strong taste of rancid grease, dissolves in water and in alcohol; although strongly dried, it still reddened blue paper: its aqueous solution was precipitated by the acetate of lead. The infusion of gall-nuts disturbed its transparency, and in a few minutes slight flakes were collected.

This residue was therefore formed of an animalized matter, (perhaps a foreign substance,) and of a fixed acid in a proportion in too small a quantity to be examined.

I returned to the greasy matter proceeding from the rancid suet boiled in water:—it had entirely lost its rancidity. Boiling in water is therefore a very good way of taking from fat that part which has become rancid, since the latter consists of substances soluble in water, and of a volatile rancid principle. Demachy had already advised the use of alcohol to take off the rancidity of fat, but boiling water answers the purpose equally well.

Rancid tallow after being boiled in water seemed to me to be of a firmer consistence than recent tallow, from which it differs, besides, in most of its properties: in fact, it is more soluble in alcohol; but it is above all, the action of the alkalies on this tallow thus changed, which is remarkable: they unite with it almost

almost instantly to form a perfect soap; if we repeat the same experiment by treating in a similar way, and in the same space of time, any recent tallow with an alkali, scarcely shall we be able to produce any perceptible traces of soap.

If we do not succeed in bringing a rancid fat back to its primitive state, we may at least retard its rancidity: a slight alkaline solution seems to enjoy this property: hence the practice in certain countries of keeping fresh water in cloths moistened with a lixivium.

The disposition of the rancid fats to be saponified in a few minutes, might perhaps present some useful results for soap-makers: it also shows the profound alteration which they have undergone, and the different degrees by which they become adipocire—time alone can operate this change, as Fourcroy proved in his examination of the churchyard of the Innocents. This chemist also ascertained that fat persons furnished more adipocire than lean, and that the marrow of the long bones was converted wholly into adipocire.

XXVIII. *An Investigation of the Pressure sustained by the fixed Supports of flexible Substances.*

To Mr. Tilloch.

SIR,—THE formidable accident, which occurred some time since from the failure of the hoops of a vat of great size, has led to an inquiry respecting the strength required in structures of this kind: and its results are comprehended in the following propositions. It must be remembered that they are only correctly true upon the supposition that the resisting points are absolutely fixed, and that in actual practice the forces will be somewhat more equally divided: it would, however, be always prudent to make the strength great enough for the most unfavourable supposition that can be made respecting its employment.

A. If a flexible bar, equably loaded throughout its length, be supported at each end and in the middle by fulcrums perfectly fixed, the middle point will sustain $\frac{5}{8}$ of the whole pressure.

Let the half length be a, the distance of any point from the middle x, and the pressure on the end y; then the strain at the point, being the joint result of all the forces acting on either side of it, as on the arm of a lever of which it is the fulcrum, will be $y(a-x)-(a-x)\frac{1}{2}(a-x)$, since the weight of the portion $a-x$ acts at the distance $\frac{1}{2}(a-x)$; and the curvature will

will be as $ay-xy-\frac{1}{2}a^2+ax-\frac{1}{2}x^2$, the curve being supposed to differ but little from a straight line: hence the fluxion of the inclination will be as $ay\dot{x}-yx\dot{x}-\frac{1}{2}a^2\dot{x}+ax\dot{x}-\frac{1}{2}x^2\dot{x}$, the fluent $ayx-\frac{1}{2}yx^2-\frac{1}{2}a^2x+\frac{1}{2}ax^2-\frac{1}{6}x^3$, which requires no correction: and in the same manner the fluent of the ordinate will be found $\frac{1}{2}ayx^2-\frac{1}{6}yx^3-\frac{1}{4}a^2x^2+\frac{1}{6}ax^3-\frac{1}{24}x^4$, which must vanish when $x=a$, since the ends are supposed to be absolutely fixed, or $0=\frac{1}{2}a^3y-\frac{1}{6}a^3y-\frac{1}{4}a^4+\frac{1}{6}a^4-\frac{1}{24}a^4=\frac{1}{3}y-\frac{1}{8}a$, and $y=\frac{3}{8}a$, which is $\frac{3}{16}$ of $2a$, the whole pressure; so that the two ends support $\frac{3}{8}$ of the whole pressure, and leave $\frac{5}{8}$ for the middle.

B. In order that a flexible bar, equably loaded, may rest equally on each of three fixed supports, their distance must be ·3472 of the whole length.

If the half length be a, and the distance m, the strain, between the supports, will be $\frac{2}{3}a(m-x)-\frac{1}{2}(a-x)^2$; the inclination, by taking the fluent, is found $\frac{2}{3}amx-\frac{1}{3}ax^2-\frac{1}{2}a^2x+\frac{1}{2}ax^2-\frac{1}{6}x^3$, and the ordinate $\frac{1}{3}amx^2-\frac{1}{9}ax^3-\frac{1}{4}a^2x^2+\frac{1}{6}ax^3-\frac{1}{24}x^4$, which must vanish when $x=m$, and $\frac{1}{3}am-\frac{1}{9}am-\frac{1}{4}a^2+\frac{1}{6}am-\frac{1}{24}m^2$, or $\frac{7}{18}am-\frac{1}{4}a^2-\frac{1}{24}m^2$ must vanish also; whence $m^2-\frac{28}{3}am=-6a^2$, $m-\frac{14}{3}a=\pm\sqrt{\frac{142}{9}}a$, and $m=\frac{14-\sqrt{142}}{3}a=·6944a$; that is, about $\frac{1}{72}$ of the whole length more than if the bar were composed of three pieces, and each point supported an equal share.

C. If a flexible bar, equably loaded, rest on five fixed points, at the distance of $\frac{1}{5}$ of the length from each other, and $\frac{1}{10}$ from the ends, the pressures will be as 59, 52, and 58, or as 2107, 1857, 2071, 1857, and 2107; and if the middle support be removed, the pressure on the remaining points will be as 11 and 21, or as 1719, 3281, 3281, 1719.

Calling the whole length $10a$, and the pressure on the lateral fulcrums,

fulcrums, y and z, the strain at the distance x from the middle will be, for the portion next the middle, $y(2a-x)+z(4a-x)-\frac{1}{2}(5a-x)^2$; the inclination $2ayx-\frac{1}{2}yx^2+4azx-\frac{1}{2}zx^2-\frac{25}{2}a^2x+\frac{5}{2}ax^2-\frac{1}{6}x^3$; and the ordinate $ayx^2-\frac{1}{6}yx^3+2azx^2-\frac{1}{6}zx^3-\frac{25}{4}a^2x^2+\frac{5}{6}ax^3-\frac{1}{24}x^4$, which must vanish when $x=2a$, and $0=4a^3y-\frac{4}{3}a^3y+8a^3z-\frac{4}{3}a^3z-25a^4+\frac{20}{3}a^4-\frac{2}{3}a^4=\frac{8}{3}y+\frac{20}{3}z-\frac{57}{3}a$, and $8y+20z=57a$. In the next portion the pressure y is not concerned, and the expression for the nclination becomes $4azx-\frac{1}{2}zx^2-\frac{25}{2}a^2x+\frac{5}{2}ax^2-\frac{1}{6}x^3+b$; and for the ordinate $2azx^2-\frac{1}{6}zx^3-\frac{25}{4}a^2x^2+\frac{5}{6}ax^3-\frac{1}{24}x^4+bx+c$: here b must be determined from the first portion, the final inclination of the one coinciding with the initial inclination of the other; and making in both expressions $x=2a$, $8a^2z-2a^2z-25a^3+10a^3-\frac{4}{3}a^3+b=4a^2y-2a^2y+8a^2z-2a^2z-25a^3+10a^3-\frac{4}{3}a^3$; consequently $b=2a^2y$: then the ordinate vanishing at the beginning of the second portion, when $x=2a$, we have $0=8a^3z-\frac{4}{3}a^3z-25a^4+\frac{20}{3}a^4-\frac{2}{3}a^4+4a^3y+c$; and when $x=4a$, and the ordinate once more vanishes, $0=32a^3z-\frac{32}{3}a^3z-100a^4+\frac{160}{3}a^4-\frac{32}{3}a^4+8a^3y+c$; and by subtraction, $24a^3z-\frac{28}{3}a^3z-75a^4+\frac{140}{3}a^4-\frac{30}{3}a^4+4a^3y=0=\frac{44}{3}z-\frac{115}{3}a+4y$; whence subtracting $4y+10z-\frac{57}{2}a$, we have $0=\frac{14}{3}z-\frac{59}{6}a$, and $z=\frac{59}{28}a$; consequently $y=\frac{52}{28}a$, and for the pressure supported at the middle, there remains $\frac{58}{28}a$.

If we now suppose the fulcrum at the middle to be removed, the equations for the second part of the figure and for the inclination of the first part remain unaltered, and we have also $y+z=5a$, and $4y+4z=20a$, which, subtracted from $\frac{44}{3}z-\frac{115}{3}a+4y=0$, leaves $\frac{32}{3}z-\frac{55}{3}a=0$, and $z=\frac{55}{32}a$, and $y=\frac{105}{32}a$.

D. In

D. In a flexible stave, forming part of the side of a cistern, and supported only at the ends, the inclination at the top is $\frac{7}{8}$ as great as at the bottom.

The centre of pressure being at one-third of the height, the upper support must withstand $\frac{1}{3}$, and the lower $\frac{2}{3}$ of the whole force, which, if a be the height, may be called $\frac{1}{2}a^2$; and the strain at the distance x from the surface will be the difference of the strains produced by the pressure of the fluid and the resistance of the support, that is $\frac{1}{6}a^2x - \frac{1}{6}x^3$, since the pressure of the fluid above the given point, that is $\frac{1}{2}x^2$, may be considered as united in the centre of pressure, and therefore acting at the distance $\frac{1}{3}x$. Hence, for the fluxion of the inclination of the stave, we have $\frac{1}{6}a^2x\dot{x} - \frac{1}{6}x^3\dot{x}$, and the corrected fluent is $\frac{1}{12}a^2x^2 - \frac{1}{24}x^4 + b$: again, for the ordinate of the curve we find, by a second integration, $\frac{1}{36}a^2x^3 - \frac{1}{120}x^5 + bx$, which must vanish when $x = a$, so that $\frac{7}{360}a^4 + b = 0$, and $b = -\frac{7}{360}a^4$: hence, when $x = a$, the inclination becomes $\frac{1}{24}a^4 - \frac{7}{360}a^4 = \frac{8}{360}a^4$, while the initial inclination is represented by $b = -\frac{7}{360}a^4$.

E. If a stave be supported by three fixed fulcrums or hoops, one at each end, the other in the middle, the upper one will sustain $\frac{1}{48}$ of the whole pressure, the middle $\frac{30}{48}$, and the lowermost $\frac{17}{48}$.

If we call the distance from the surface x, the pressure at the top y, and at the middle z, the strain will be first $yx - \frac{1}{6}x^3$; and below the middle, calling half the height a, $yx + z(x-a) - \frac{1}{6}x^3$, whence the inclination will be first $\frac{1}{2}yx^2 - \frac{1}{24}x^4 + b$; and secondly, $\frac{1}{2}yx^2 - \frac{1}{24}x^4 + \frac{1}{2}zx^2 - azx + c$, and the ordinate first $\frac{1}{6}yx^3 - \frac{1}{120}x^5 + bx$, and secondly $\frac{1}{6}yx^3 - \frac{1}{120}x^5 + \frac{1}{6}zx^3 - \frac{1}{2}azx^2 + cx + d$. Now the ordinate must vanish in the first expression

expression when $x = a$: hence $\frac{1}{6}ya^2 - \frac{1}{120}a^4 + b = 0$, and the inclination in the middle is $\frac{1}{2}ya^2 - \frac{1}{24}a^4 + \frac{1}{120}a^4 - \frac{1}{6}ya^2 = \frac{1}{3}a^2y - \frac{1}{30}a^4$, which must be the value of the inclination in the second expression when $x = a$; so that $\frac{1}{2}a^2y - \frac{1}{24}a^4 + \frac{1}{2}a^2z - a^2z + c = \frac{1}{3}a^2y - \frac{1}{30}a^4$, and $c = -\frac{1}{6}a^2y + \frac{1}{120}a^4 + \frac{1}{2}a^2z$: when, therefore, $x = a$ in the second expression for the ordinate, $\frac{1}{6}a^3y - \frac{1}{120}a^5 + \frac{1}{6}a^3z - \frac{1}{2}a^3z + -\frac{1}{6}a^3y + \frac{1}{120}a^5 + \frac{1}{2}a^3z + d = 0 = \frac{1}{6}a^3z + d$, and $d = -\frac{1}{6}a^3z$: when also $x = 2a$, $\frac{4}{3}a^3y - \frac{4}{15}a^5 + \frac{4}{3}a^3z - 2a^3z - \frac{1}{3}a^3y + \frac{1}{60}a^5 + a^3z - \frac{1}{6}a^3z = 0 = a^3y - \frac{1}{4}a^5 + \frac{1}{6}a^3z$, and $z = \frac{3}{2}a^2 - 6y$. Again, the pressure on the third point will be $2a^2 - y - z$, the three hoops having to sustain the pressure $2a^2$: this third pressure must also exceed the force y by $\frac{2}{3}a^2$, in order that it may hold in equilibrium the whole pressure $2a^2$, acting at the distance $\frac{1}{3}a$ from the middle point, considered as the fulcrum of a lever, so that $y + \frac{2}{3}a^2 = 2a^2 - y - z$, and $z = \frac{4}{3}a^2 - 2y$; whence, subtracting the former value of z, we have $4y - \frac{1}{6}a^2 = 0$, $y = \frac{1}{24}a^2$, $z = \frac{5}{4}a^2$, and the third pressure $\frac{17}{24}a^2$.

F. If a stave be supported at the ends, and by two intermediate hoops at equal distances, the respective pressures will be $\frac{1}{45}$, $\frac{9}{45}$, $\frac{24}{45}$, and $\frac{11}{45}$.

For the first and second portions of the staves, the values of the inclinations and ordinates are determined from those of y and z, as in the last proposition: for the third, the inclination will be $\frac{1}{2}yx^2 - \frac{1}{24}x^4 + \frac{1}{2}zx^2 - azx + \frac{1}{2}ux^2 - 2aux + e$, which, at the origin of this portion, where $x = 2a$, becomes $2a^2y - \frac{2}{3}a^4 + 2a^2z - 2a^2z + 2a^2u - 4a^2u + e$, and this must be equal to the final inclination in the second part, or to $2a^2y - \frac{2}{3}$

$\frac{2}{3}a^4 + 2a^2z - 2a^2z - \frac{1}{6}a^2y + \frac{1}{120}a^4 + \frac{1}{2}a^2z$, whence $-2a^2u + e = -\frac{1}{6}a^2y + \frac{1}{120}a^4 + \frac{1}{2}a^2z$, so that the ordinate will be $\frac{1}{6}yx^3 - \frac{1}{120}x^5 + \frac{1}{6}zx^3 - \frac{1}{2}azx^2 + \frac{1}{6}ux^3 - aux^2 + 2a^2ux - \frac{1}{6}a^2yx + \frac{1}{120}a^4x + \frac{1}{2}a^2zx + f$; and this must vanish when $x = 2a$ and $x = 3a$, or $0 = \frac{4}{3}a^3y - \frac{4}{15}a^5 + \frac{4}{3}a^3z - 2a^3z + \frac{4}{3}a^3u - 4a^3u + 4a^3u - \frac{1}{3}a^3y + \frac{1}{60}a^5 + a^3z + f = \frac{9}{2}a^3y - \frac{81}{40}a^5 + \frac{9}{2}a^3z - \frac{9}{2}a^3z + \frac{9}{2}a^3u - 9a^3u + 6a^3u - \frac{1}{2}a^3y + \frac{1}{40}a^5 + \frac{3}{2}a^3z + f$, and by subtraction $3y - \frac{7}{4}a^2 + \frac{7}{6}z + \frac{1}{6}u = 0$, or $36y - 21a^2 + 14z + 2u = 0$. We have also, as before, from a comparison of the evanescent ordinates of the second portion, $z = \frac{3}{2}a^2 - 6y$, or $6z = 9a^2 - 36y$, and by addition, $-12a^2 + 8z + 2u = 0$. On the other hand, considering the stave as a lever with its fulcrum at its lower end, we have $\frac{9}{2}a^3 = 3ay + 2az + au$, and $2u = 9a^2 - 6y - 4z = 12a^2 - 8z$, and $6y = 4z - 3a^2 = \frac{3}{2}a^2 - z$, whence $5z = \frac{9}{2}a^2$, $z = \frac{9}{10}a^2$, and the second hoop sustains one-fifth of the pressure; consequently $y = \frac{1}{4}a^2 - \frac{1}{6}z = \frac{1}{10}a^2$, $u = \frac{24}{10}a^2$, and there remains for the force at the bottom, $\frac{11}{10}a^2$. In a similar manner the calculation may be extended step by step to a greater number of points; but as the number increases, the inequality of the distribution between the neighbouring points must of course diminish, and if it became infinite, the pressure on each would be simply as the depth.

August 3, 1815. A. B. C. D.

P. S.—The "Radiatrix" mentioned as a "new curve," in the last number of Dr. Thomson's Annals, p. 154, is the conchoidal epitrochoid described, by Lahire and Maclaurin, and more particularly in Young's Natural Philosophy, vol. ii. p. 561.

Your correspondent, who has endeavoured in your last number to remove some difficulties respecting the stiffness of timber, seems to be mistaken where he asserts, that the stiffness is inversely as the square of the length only; as he will immediately perceive, if he reflects that when the curvature is uniform, the depression is as the square of the length, supposing the strain to remain unaltered: but that from the properties of the lever the strain,

strain, and consequently the curvature, must also vary as the length, and the ultimate depression must be as the cube of the length. This correction is the more necessary, because, from the manner in which he has introduced his proposition, after a definition professedly copied from a classical work on the subject, it appears as if the proposition itself were also borrowed from that work, and supported by its authority.

XXIX. *Notices respecting New Books.*

The Pharmacopœia of the Royal College of Physicians of London, 1809*: translated into English, with Notes, &c. Third Edition. By* Richard Powell, *M.D. Fellow of the College, Physician to St. Bartholomew's and the Magdalen Hospitals, &c.* 8vo. London 1815. Longman and Co.

Our readers have been from time to time apprised that the learned and indefatigable translator of the London Pharmacopœia was busily employed in collecting materials for an improved and enlarged edition of that valuable work. In a modest and well-written preface, Dr. Powell thus describes the various objects which it was his study to attain:—

"Since the publication of the last edition of the London Pharmacopœia, various objections have been published to its general principles as well as to its processes. These, whatever be the language in which they are expressed, whatever the motives by which they are in many instances dictated, deserved at least a calm consideration of their subject matter, and even the adoption of such parts thereof as appeared to be necessary and useful. Under this impression a committee of the college went through the Pharmacopœia, and have made those alterations therein which have by them been judged to be requisite, and which, having received the sanction of the college, are now published.

"It will be seen that the alterations adopted refer, first to some important processes, to which reasonable objections have certainly been urged, on the score either of manipulation or of product, as, for instance, in the preparation of Antimonium tartarizatum, which, though it answered repeatedly according to the former process, upon a small scale, before the committee, has certainly failed upon a large one, and under other circumstances: secondly, to some changes in the names of substances, as in giving up that principle (which was before considered to be sufficiently distinctive) by which *sub* and *super* were only employed where both the salts were used pharmaceutically for the purpose of distinguishing between them, without regarding the actual relation of their constituent parts; so that the salt which was at first named carbonate of ammonia is now named, as it really is, a subcarbonate: thirdly, to the introduction of new articles, which have been sparingly adopted: fourthly, to the restoration of some which had stood in the Pharmacopœia of 1787 and been omitted: and lastly, in a very few omissions from the last edition. Although these alterations have been made after a mature and impartial deliberation, there probably will be many persons to whose ideas they may be neither sufficient nor satisfactory, and who will have sufficient confidence in their own opinion to hold it as matter of faith, that no other can be right or deserve to be adopted.

"A Pharmacopœia is in its very nature ephemeral, and requires certain changes after intervals of no very long duration; nor should there exist in a well-educated profession, such as that of medicine in all its branches ought to be, any difficulty in receiving and adopting the alterations which are thought necessary. It seems not improbable but that circumstances will again demand such a revision at no distant date. Parliament are now employed in the consideration of the standard weights and measures of the kingdom; and if those alterations which are expected therein shall be established by law, it will be for the college to consider whether they will claim any peculiar exemption for the compounder of medicines, or will not rather promote and assist in the general adoption of one uniform standard, to which there can be no other objection than the ideal difficulty of its first introduction. Whenever too that period shall arrive, I most sincerely hope that an increase of intercourse and of mutual respect and good-will between the constituted authorities in medicine in the different parts of the British empire will have laid a foundation for the establishment of one national Pharmacopœia, which I am daily more and more convinced will be a measure of the utmost importance to the public good, and remove the evils which exist in the shops from the incorporation of the three.

"After thus speaking of the original work of the college, it is necessary for me to add a few words respecting the translation, which in the first instance cost me no small time and trouble, and which I have in the present edition endeavoured to correct and improve, measuring however my notions of the necessity of either, not by the abuse of others, but by my own judgement. I will repress as much as I can the impulse which I feel to enlarge upon this subject, and will in this respect accede to the judgement of my friends.

"I could, in undertaking the translation originally, look to no praise beyond that of having collected together useful information upon an important subject, and of having endeavoured to render it it correct. My object did not preclude on the one hand, nor on the other did it demand, originality: if this had been necessary, my engagements and occupations would have rendered it impossible for me to have undertaken the work. But still so much of the added matter has been repeated and adopted by others, whether with or without acknowledgement is of no consequence, that I can infer therefrom, with tolerable safety, that my labours have been of some utility in the judgement of others.

"It is unnecessary to repeat what I have stated in the former preface, that for any chemical facts beyond the mere processes I have chiefly referred to Thomson's Elements of Chemistry, which in my own estimation is the best; and it is more particularly valuable on account of its reference to the original authorities upon whom its statements ultimately rest; and it would be childish to complain of that common trick which omits any statements, as if they had not been made, when they happen to militate against the purpose of an objector.

"I hope I shall preserve as long as I live the power as well as the will to resist personal attacks, and to defend and protect my own character; but I see no reason why I should formally enter the lists with every one who chooses to assail me from a dark corner as I pass, or why like Erasmus I should hold myself compelled θηριομαχεῖν*. As I wish, on this and every other occasion, like him to go on my road peaceably, it has certainly been matter of regret to me that I have been attacked with a virulence beyond even that of sectarian controversy, though, thank God, I have not been, and I hope never shall be, provoked by it to a contest, in which victory itself must be disgraceful.

"The present edition of the translation of course includes the alterations made in the original; and all the processes therein which have undergone any change, or are restored from the Pharmacopœia of 1787, are marked thus, †.

"I see my own error in attempting to mark the pronunciation of words with which prosody has so little to do by the prosodial characters, and have therefore employed the method of accentuation used by Dr. Young (Medical Literature 1813), and which is not subject to the same objections, instead of them.

"To the botanical generic and specific names I have subjoined the synonyms of the parts of the plants used in the French, German, and Spanish languages, as far as, with only a very imperfect knowledge of either, I could glean them from Dr. Swediaur's Materia Medica (Paris, An. 8.) because, under the prospect of an increasing intercourse with the continent, which re-

* "Ego ille pacis et quietis semper amantissimus, cogor esse retiarius; nec hoc satis, cogor θηριομαχεῖν.

"*Epist. ad Polum*, 4 Mar. 1526.

 cent

cent events have unhappily again interrupted, I expected that such a reference might be occasionally useful in the shops, and that its defects might easily be pardoned, and hereafter removed by those of our professional brethren who are better acquainted with the application of these substances in the countries referred to.

"The table of doses has been revised, but not much altered. I stated its necessary imperfections in the former preface, which I shall not again repeat; but I beg leave to offer my opinion that doses can be ascertained by experience alone, and that nothing can be more fallacious than to infer those of compound substances from those of their constituent parts. I have had some little experience upon this subject, and I believe the table to be as correct, practically, as the nature of the subject will admit. Many of the articles, it is true, will allow of increase with perfect safety beyond the limits which are here assigned, especially when they are intended to produce some particular definite effects; and upon the whole, general practice may be thought, in fact, and perhaps wisely, to keep itself below the standard rather than exceed it; and my chief object has been not to lead the young practitioner into error by what I have stated.

"I have allowed the preface to the first edition to remain unaltered in the present one; and the only principle stated therein, which is now superseded, I have marked by inverted commas.

"I find that some errata exist, particularly in the accents, which easily shift, even after they have once been right; any others which have struck my eye on revision I have put together in a table, and if more exist I shall be glad to have them pointed out, and to remove them hereafter.

"I deem it also of importance here, to recommend to the apothecary a most useful, and indeed necessary, new instrument for the purposes of his profession; I mean Dr. Wollaston's Scale of Chemical Equivalents, described in the Philosophical Transactions.

"In acknowledging with unfeigned gratitude a variety of communications which I have received respecting the present work, I cannot but particularize the aid of Mr. Hume, chemist, of Long Acre, to whose practical skill the committee had repeated occasions to express their obligation. Indeed, I am not conscious that in any instance I have purloined the observations of others, or used them without due acknowledgement: if I have, it has not been wilfully done; nor need it have been mentioned, but that this proceeding am I charged withal. And if, as the result of the whole, I may fairly reflect hereafter within my own mind, that it has been my lot, in any degree, to contribute to the promotion of a profession of so much importance to mankind,

kind, as that of medicine, when it is liberally and fairly exercised, my labours will be amply repaid, and my highest ambition most abundantly gratified."

We are far from expecting perfection in the early editions of a work comprehending such an immense variety of chemical and pharmaceutical preparations. That there is however a still nearer approximation to this desirable requisite in the third edition of the Pharmacopœia now before us, a slight perusal will convince every candid reader. In his praiseworthy labours Dr. Powell has judiciously availed himself of the assistance of the best practical chemists of the metropolis; among whom he has occasion more than once to particularize an old and zealous contributor to the Philosophical Magazine, Mr. Hume of Long Acre. The very scientific and delicate process for preparing "*Antimonium tartarizatum*," as communicated by that gentleman in a preceding number of this Journal, is embodied into the present edition of the work of the College, with a few modifications. How far these changes may or may not benefit the preparation, we are not from actual experience qualified to decide: of this we are certain, however, that Mr. Hume's process, as described in this Journal, and above alluded to, has since been adopted, and with the most complete success, by almost every practitioner in pharmacy in the metropolis. In such a very delicate process, therefore, and of the result of which practical chemists are alone best qualified to judge, deviations from their formulæ ought to be cautiously introduced.

Before concluding, we ought to add that the present edition has to boast of a very valuable Appendix, containing every requisite table respecting weights and measures, proportion of doses, &c.; without an intimate and familiar acquaintance with which, no practitioner in medicine or pharmacy can answer for the consequences of his prescriptions.

Mr. Accum has in the press a second edition [stereotype] of his Practical Treatise on Gas Light; exhibiting a summary description of the apparatus and machinery best calculated for illuminating streets, houses, and manufactories, with coal gas; with remarks on the utility, safety and general nature of this new branch of civil œconomy. Illustrated with seven coloured plates, showing the construction of the large machinery employed for illuminating the streets and houses of this metropolis, as well as the smaller apparatus employed by manufacturers and private individuals.

A second edition has just appeared of the late Mr. Bramah's ingenious Dissertation on the Construction of Locks; accompanied with a well engraved plate. This dissertation being well known to every curious mechanician, we notice it only to correct an error which has been so often repeated as to have received very general credit—that when Mr. Bramah applied to Parliament for an extension of the term of his patent, two gentlemen, examined as witnesses against the grant, demonstrated to the committee that Mr. Bramah's lock might be picked with a piece of wood and a quill. Prefixed to this edition is an extract from the Minutes taken before the committee.—Mr. George Hawks being asked, "When you made that key of wood, had you not the real key in your possession, from which you copied it?" replied "Yes."—"Does the witness mean to say that he could make that key of wood, in such a manner as to open the lock, without having the real key to copy from?"—"I don't mean to say so."—Mr. Henry Downer, cross-examined—"Had you not the real key in your possession when you made what you call the mutilated key? and did not you copy it?"—"I had, and I did copy it."—The extract is signed "Hen. Coles, Committee Clerk, May 24, 1815."

XXX. *Intelligence and Miscellaneous Articles.*

ROYAL SOCIETY.

In our report of the proceedings of this learned body for May last, we briefly alluded to a paper by Sir H. Davy, On a Compound of Oxygen and Chlorine. This compound, we now find, consists of two parts in volume of oxygen and one part in volume of chlorine. It is a permanent gas, very soluble in water, of a bright yellow colour, detonates, and is resolved into its two constituent parts by the heat of boiling water. It has not acid properties; from which it may be concluded that the acid properties of the hyper-oxygenized muriatic acid are owing to combined hydrogen. According to the doctrine of definite proportions, this new gas may be regarded as composed of one proportion of chlorine, and four proportions of oxygen.

M. Sementini, Professor of Chemistry at Naples, stated in a recent Italian journal, that "the yellow tincture of *curcuma*, which is changed into a red colour by the alkalies, undergoes the same change by the action of the phosphoric acid." M. Guyton Morveau thus controverts this conclusion in the *Annales de Chimie*:—"The author of this observation must have been led into an error by some accident. A phosphoric acid which reddened strongly even the blue paper in which sugar loaves are wrapped,

wrapped, had no more effect than distilled water on paper coloured by curcuma. How could M. Sementini bring himself to publish this observation before having repeated the experiment, and taken all means of ascertaining the purity of the substances which he employed, or rather to reproduce the accidental composition which had produced the phænomenon?

"If it be true, as the illustrious Bergman observes, that Nature never conducts more securely to discoveries than when she presents us with facts which are in complete opposition to our opinions, it is not by precipitate judgements founded upon slight observations that truths are demonstrated."

DESCRIPTION OF A CALCULUS FOUND IN A TUMOUR IN THE LEFT HYPOCHONDRIA.

M. Penada, an Italian surgeon, has published in the Memoirs of the Italian Society of Sciences the following:—"The calculus existed in a female fifty years of age, and came out through an ulcer which was formed in the tumour. It was precisely of the shape of a hen's egg: the colour was dark gray, and its surface granulated and shagreened. On removing the external coating, the calculus presented a lamellated structure and a bright yellow colour: its consistency was compact, but not very hard; and it weighed in all one ounce two drachms. When thrown into water before opening it, it floated for a few seconds, and then fell to the bottom of the liquid, giving out some air bubbles.

"It was cut into two parts parallel to its axis: interiorly it presented from ten to twelve concentric layers, which decreased gradually in thickness, analogous to walnut-tree wood. These layers were separated by a black streak around a circle of a deep yellow, which diminished in breadth as it approached the centre, and the surface of which was as brilliant as marble or highly polished wood. In the centre of these ellipses a perfectly spherical body was remarked, of a fine white, formed of a substance less compact than the rest of the calculus; of a crystalline scaly texture, and having nearly the form and consistence of the crystalline of a bull's-eye coagulated by heat. This nucleus was surrounded by a black circle more deeply coloured than the ellipses just mentioned."

On the chemical analysis of this calculus, Professor Melandri of Padua obtained the following results:

"The central substance, or white nucleus, was entirely soluble in ether, crystallizable and combustible; when thrown upon red-hot coals, it emitted the smell of incense. The cortical part was only partially soluble in alcohol and ether. The soluble part crystallized, and burned with a white flame and a smell of wax. The insoluble part burned with the smell of animal matter. Pro-

 fessor

fessor Marabelli concludes from these experiments, that the calculus was formed entirely of adipocire modified by a resinous principle, and the cortical part of adipocire very pure, and of an animal substance."

We have been favoured with a continuation of the valuable correspondence of our scientific friend M. Van Mons, of Brussels, from which we present the following extracts:

"M. Dobereiner of Jena, the same gentleman who metallized carbon, has succeeded in seizing upon hydrogen in the metallic form. He galvanizes water put in contact with mercury, and obtains at the positive pole oxygen; and at the negative pole where the mercury is placed, instead of hydrogen he obtains an amalgam consisting of this metal. We may knead this amalgam and make it take different forms without its being decomposed; but when exposed to heat it gives off the hydrogen, and the mercury once more becomes fluid. M. Dobereiner thinks that hydrogen gas is a metal dissolved in caloric. His experiment is very curious, and deserves to be repeated.

"M. Dobereiner has also had indications of metallization in phosphorus. He burns it under a bell-glass, and directs over it a ray of light direct from the sun. The phosphorus is formed in pellets of a gold colour, and with great metallic lustre. Subhydrogenated sulphur exhibited a blue substance similar to ultramarine. M. Dobereiner considers all the substances hitherto undecomposed, as metals.

"Since my last, I have obtained hydrargyrio-chlore at two different degrees of saturation, and even the one with the double of metal of the other. The most saturated is black, the least saturated is red. These are true oxidule and oxide of mercury by the dry muriatic acid, instead of being so by oxygen. Sweet mercury furnished me the first compound, and corrosive sublimate the second. I increased the fire during the sublimation of the mixture of corrosive sublimate and quicksilver in one phial, and sweet mercury ready prepared, but incompletely sublimed, in the other phial. The latter gave the black powder, and the other the red. Some reduced mercury was separated in the first phial, and oxygen must have been extricated from both, because metallic mercury was combined with the dry acid. When heated in oxygen gas, the one compound was converted into sweet mercury, and the other into corrosive sublimate. Chlorine neither acts upon the one nor the other when it is dry; but with the addition of water it takes the mercury from them, the dry acid combining with water; unless we mean to say that the oxygen of the chlorine passes to the metal, and that its dry acid is taken up by the water under the pile, and in the actual decom-

decomposition of water the two compounds are resolved into chlorine, which proceeds towards the positive pole, and into reduced mercury, which conjointly with hydrogen proceeds to the negative pole. Water in steam directed on the compounds very much heated, takes from them the dry acid, and the mercury remains reduced. This effect is much more easily produced on the red hydrargyrio-chlore than on the black.

"I know not how far the announcement of Mr. Accum is well founded* with respect to the metallization of charcoal; but in the correspondence of the author of this discovery (M. Dobereiner) with me, nothing of the kind is mentioned. I may not, however, have received all his letters, owing to the war. His colleague, Professor Kieser, with his pupils, like the rest of the members of all the German colleges, is with the allied armies.

"M. Dobereiner had already resolved silex in a great measure into carbonic acid; and some carbon proceeding from it was sent, but it never reached me. According to Mr. Accum's information, M. Dobereiner must have operated by the phosphuret of iron and liquid alkali, which must have produced phosphuretted hydrogen gas, whereby the carbon will be hydrogenated, and at the same time reduced, if this combustible does not exist already without oxygen in carburetted iron. The carbon is reduced in the alcohol of Lampadius, which by water is regenerated into sulphur and into carbon, and requires for its acidification more oxygen than its elements separated; and the sulphur is more or less reduced in the sulphuro-phosphorus, as is proved by the water collected during the formation. A detonation takes place, and hydrogen is disengaged."

Brussels, Aug 7. 1815. B. Van Mons.

[To be continued.]

To Mr. Tilloch.

Sir,—In your Magazine for May, page 387, I am requested by Mr. John Farey sen. to explain a mistake, or otherwise throw some light on my extraordinary account of rain upon Blackstone Edge in the nine months observation of 1813, as inserted in one of your former Numbers. The extraordinary notation of 86,085 inches is certainly an error. My friend Mr. Leadbetter (the gentleman who notes the fall of rain upon Blackstone Edge) and I are convinced that the error arose from the rain, which was driven against the outside of the funnel, getting access to the vessel underneath; but which since January 1814 has been effectually prevented by a very simple contrivance. On the outside of the rain-funnel (which is made of copper, and surmounted with a cylindrical rim two inches broad and six inches diameter,) is soldered, about the middle of the conical part, another piece of

* See vol. xlv. p. 156.

conical

conical copper having its greatest diameter downwards: of course it will form an inclined shed; so that, if the rain should trickle down the outside, it will be prevented from entering the bottle. We could not at first account for so great a difference between the two places of observation, till I suggested the probability of the rain entering from the above cause.

Yours very respectfully,

Manchester, Aug. 16. 1815. THOMAS HANSON.

ON METEORIC STONES.

SIR,—As every fact relative to the subject of meteoric stones,—now that the existence of such stones is no longer a matter of doubt either among philosophers or the vulgar,—is entitled to attention; and as some of the most interesting observations on this subject are contained in different Numbers of your valuable Philosophical Magazine; I hope that you will not deem the following observation unworthy of republication. I give it to you entire in the words of the author, Michael Bernhard Valentin, from the *Ephemerides Naturæ Curiosorum*, for the year.

"Crystallus inter grandines è nubibus decidens."

"Anno superiori 1724, Opilio in præfectura Alsfeldiensi ruri pede suo innitens sub oborta tempestate observat lapidem pellucidum inter grandines delabentem, quem, cum vitrum secaret, Judæi, quibus eundem ostendebat, pro adamante habentes, pretium non contemnendum illi offerebant. Inventor autem majus inde lucrum sperando Francofurtum se conferens, thesaurumque suum gemmario offert, qui lapidem hunc non tam pro adamante, quam optimæ notæ crystallo habendum esse judicabat. Rem certissime ita gestam esse minister Verbi Divini illius loci sancte mihi asseveravit. Possibilitatem ejus suo jam tempore adstruxit Cel. Bohnius, qui crystallum habuit, in cujus meditullio gutta aquæ limpidæ continebatur: unde concludit gemmas pellucidas ex aqua generari. Idem confirmat pluvia gemmea à Balbino in Bohemia observata, de qua *Armamentario Art. et Nat.* à me olim edito."

"*Giessa Augustam Vind. d.* 18 *Maj. An.* 1727, *missa**."

Michael Bernhard Valentin, the writer of the preceding observation, was a professor at Giessen in Germany, and the author or editor of many works or memoirs, of more or less importance, in the latter end of the 17th and the earlier part of the 18th century. Several of his papers, published in the *Ephemerides Academiœ Naturæ, &c.* I have read with much satisfac-

* *Acta Physico-Medica Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum exhibentia Ephemerides, &c.* Vol. ii. *Observatio* cxxii. *p.* 280.—Norimbergæ, 1730.

tion,

tion. I regret that I have not been able to procure his *Armamentarium Naturæ et Artis, s. Compendium Physicum*, published at Giessen in 1709. In this work, it seems, the learned author has recorded, or mentioned, a rain of crystals, or pellucid stones, "pluvia gemmea," which was observed by Balbinus in Bohemia. B. S. B.

It is to be regretted that Valentin has not given us some more minute information concerning the size and form of the crystal, the exact season of the year in which it fell, &c. These are points of some importance, and should have been attended to. But notwithstanding those omissions, the general truth of the fact ought not, perhaps, to be called in question: and, at any rate, the falling of a solitary rock-crystal from the clouds, or even a whole shower of such stones, are circumstances not more improbable than the falling of masses of iron, of nickel, and other metallic and similar substances. And who knows, as we live in the days of bold and ingenious speculations, but that some naturalist may step forward, and endeavour to prove that millions of those beautiful hexagonal and other siliceous crystals, which are so abundantly distributed upon, and immediately under, the surface of the earth, in North America and in other countries, have had similar, or *meteoric*, origin?

With much respect, Sir, Yours, &c.

London, Aug. 28, 1815. BENJAMIN SMITH BARTON, M.D.

FALL OF METEORIC STONES IN INDIA.

A phænomenon of this description, which was witnessed in November last, is thus described by a native philosopher: "A singular phænomenon has occurred in the Doab; I have heard the facts related by various persons, who all concur in the same account; the circumstances are as follow: On the 5th of November 1814, being Saturday, while half a watch of the day still remained (*i. e.* half past four P. M.) there was first of all heard a dreadful peal of thunder, and then stones rained down in sight of the inhabitants of the country, each stone being 13 to 15 seer * in weight. In the first place, wheresoever they fell a great dust rose from the ground; and after the dust subsided, a heap of dust (chakri) was formed, and in that dust (chakri) were found the stones, a piece of one of which is sent herewith.

"In the district of Lapk seven stones were found; in the district of Bhaweri, dependant on Bezum Sumroo, four; in the district of Chal, belonging to the pergunnah of Shawlif, five; at Kabout, belonging to the pergunnah of Shawlif, five. In all nineteen stones were found."

* The Bengal seer weighs 2lb. and 2 dr.

LECTURES.

LECTURES.

Dr. Clutterbuck will begin his Autumn Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Wednesday, October 4, 1815, at Ten o'clock in the Morning, at his House, No. 1, in the Crescent, New Bridge-Street, Blackfriars; where further particulars may be had.

Theatre of Anatomy, Bartlett's Court, Holborn.—Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S. Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and *treatment of surgical diseases*, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

The Autumnal Course will commence on Saturday, October 7, 1815, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.—Particulars may be had on applying to Mr. Taunton, 87, Hatton Garden.

Medical School of St. Thomas's and Guy's Hospitals.—The Autumnal Courses of Lectures at these adjoining Hospitals will commence the beginning of October; viz.

At St. Thomas's.—Anatomy, and the Operations of Surgery, by Mr. Astley Cooper and Mr. Henry Cline. Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's.—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other Particulars may be learnt at the respective Hospitals.

Medical, Chirurgical, and Chemical Lectures at No. 9, George Street Hanover Square, and No. 42, Great Windmill Street.

1. The first Monday in October next, at No. 9, George Street Hanover Square, a Course of Lectures on the Practice of Physic, with the Laws of the Animal Œconomy, will commence at Nine in the Morning, and be continued every succeeding Monday, Wednesday, and Friday, till February next, by George Pearson, M.D. F.R.S. Senior Physician of St. George's Hospital, of the College of Physicians, &c. &c.

2. On Tuesday, October 3d, at Eight in the Morning, on Chemistry, No. 42, Windmill Street; to be continued every succeeding Tuesday, Thursday, and Saturday, at the same hour, by Augustus Granville, M.D.

3. On Tuesday, October 3d, at Nine in the Morning, on Therapeutics, with Materia Medica, and Medical Jurisprudence; to be continued every succeeding Tuesday, Thursday, and Saturday, at the same hour, by Richard Harrison, M.D. Fellow of the College of Physicians, and Physician to the Northern Dispensary.

4. On Monday, October 2d, at Seven in the Evening, at No. 42, Great Windmill Street, on the Theory and Practice of Surgery; to be continued every succeeding Monday, Wednesday, and Friday Evenings, at the same hour, by B. C. Brodie, F.R.S. Assistant Surgeon to St. George's Hospital.

☞ Proposals at St. George's Hospital, and the respective Theatres.

Meteorological Observations made at Clapton, in Hackney, and at other Places, from July 13 to August 10, 1815.

July 13.—Hot day. Very early came over in the higher stratum of air spreading *cirrus*, and continued through the day on and off, breaking out into cirrostrative and cirrocumulative forms of spots, streaks, &c. or into dependent tufts of *cirri*, while *cumuli* and *cirrostrati* had a rainy appearance below. Wind from SW in gales, which swept the roads and carried along the dust;—the falling of smoke and dim light of the stars in the clear intervals at night, foreboded that the occasional drops of rain would end in more copious wetness. Barometer falling 30·10. Thermometer 76°, and at night 62°.

July 14.—Fair weather,—various clouds.

July 15.—(At Dover.)—Fair with strong cool NW wind, and a cloudy evening.

July 16.—(At Calais.)—Fair weather with gentle gales from north-west. I noticed the very partial nature of these gales in crossing

crossing the Channel in a sailing-boat to France. We had alternate gales and calms. In the evening much *cirrocumulus, cirrus,* and *cirrostratus.*

July 17.—A strong NW wind, with much cloud, and some rain. Fine evening.

July 18.—Fine day; *cirrus,* &c.

July 19.—Cooler wind from N. Cloudy, and showers in the evening.

July 20.—(At Dunkirk.)—Fair morning; *cirrostratus* and showers at night.

July 21.—Fair gentle wind from westward, afterwards north.

July 22*.—(At Calais.)—Strong westerly wind in the morning, which was fair. Calm evening, not a breeze on the sea the whole way from Calais to Dover, and much cloud, which showed the *rabdoi divergentes* at sunset.

July 23.—(At Clapton.)—Fair day and gentle NW wind.

July 24.—Fair warm close day. Wind NW.

July 25.—Wind still NW, and an inclination to nimbification, which however went off till night, when it rained.

July 26.—NW wind and some showers; a dead orange-red colour at sunset below the clouds in the west.

July 27.—Fair day and warm, with northerly wind.

July 28.—Fair warm day. NW. *Cumuli,* &c.

July 29.—Fine day. Wind NW. but somewhat variable. The colour of the western horizon at eventide has of late been clear, and without much colouring. The distances have been very clear, but there has been often a haziness above.

July 30.—Fine dry air. *Cumuli,* &c. Wind NW, though at times from S.

July 31.—The same kind of weather. Very dry with northerly winds; cooler in the evening.

August 1.—Fine dry day, and clear. In the evening a few clouds stretched in the west but gray, and there was very little colour in the haze. At night some small meteors of the common kind. NE.

August 2.—Fair dry warm morning; a faint haziness aloft obscured the sun's light. *Cumuli* formed below this veil, and by inosculation there was nimbification, and a shower about half after four: but the blackness which threatened a storm went off towards midnight, and the stars shone. Thermometer in the day 75°, at 11 P.M. 59°. The Barometer kept steady through the afternoon at 30·24. Wind NNW in gentle gales, but at times a still closeness.

* It is said that lightning and thunder are much less frequent on the shores of France than on the opposite coast of Britain.

August

August 3.—Fine morning, with dry air, and gentle gales varying NW and W.; flimsy cirrocumulative masses of *cirrostratus* aloft: *cumuli* form below;—afterwards *cumulostratus* threatened rain and obscured the sky; but it cleared, and the *cumulostratus* subsided in many horizontal masses*, &c. The Barometer remained all day stationary at 30·24. The afternoon cleared and became calm, and the air close and oppressive;—few clouds, and from NW. Dim starlight at times. Thermometer 76° and 56°. Small meteors.

August 4.—Hot hazy autumnal morning. The wind blew gentle gales from the west, the haze thinned off, and *cumuli* were rather rocky and copper coloured. Thermometer 78°.—Barometer falling 30·00 about noon. The evening became fair and light, and there were small meteors. Barometer falling, at 11 P.M. 29·95.

August 5.—The morning was clear, flying fleecy *cumuli* forming and dispersing again sailed over from the west; streaks of *cirrus* remaining higher up. In the afternoon the sky clouded and rain came down; the Barometer falling to 29·70. Thermometer 75° and 52°. The night cleared again.

August 6.—Rocky *cumulostratus* and followed by *nimbi* and thunder and lightning, with gentle showers. Fine evening; the haze at sunset as of late very little coloured. At 11 P.M. Thermometer 48°. Barometer 29·82. W.

August 7.—Fair day, with much *cumulostratus*, &c.

August 8.—Much *cumulostratus* of rocky appearance, A.M. The sky was nearly veiled by it, P.M. Bar. 29·95°. Towards evening forms of *cirrostratus* appeared. Wind calm from S. Dim starlight. Sounds heard at a great distance this evening.

August 9.—Obscured hazy gray morning; after it cleared off, cirrocumulative *cirrostratus* and *cirrus* in the higher regions; *cumuli* and lastly *cumulostrati* below. Wind SW. and still. Thermometer at max. 72°. Barometer 30·01.

August 10.—Clear weather; a few *cumuli* form; air dry from SW: afterwards *cirrus* in light bands, &c.

Five Houses, Clapton,
Aug. 11, 1815.

THOMAS FORSTER.

* There is something peculiar in the weather of late; the repeated processes which threatened rain so often, arrested as it were before its fall, and the steady state of the barometer, the great dryness, &c. indicate a peculiar state of the air. People generally complain of an oppressive and unhealthy feeling from a sort of closeness in the air.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For August 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July	27	54	60	55	30·19	52	Cloudy
	28	56	63	54	·18	61	Fair
	29	55	71	60	·10	59	Fair
	30	60	67	58	·01	64	Fair
	31	58	66	54	·08	54	Cloudy
Aug.	1	54	70	57	·20	56	Fair
	2	57	69	60	·16	48	Fair
	3	62	74	63	·23	66	Fair
	4	64	75	62	·01	72	Fair [the evening
	5	60	66	54	29·75	56	Cloudy, rain in
	6	58	67	52	·62	57	Slight Thunder
	7	59	66	55	·87	59	Fair
	8	56	68	57	.88	62	Fair
	9	57	69	62	.89	60	Fair
	10	56	70	58	·82	66	Fair
	11	57	69	54	·52	57	Slight Showers
	12	55	66	56	·54	42	Slight Showers
	13	61	70	59	·84	66	Fair
	14	58	71	60	30·13	60	Fair
	15	59	74	61	·05	62	Fair
	16	62	74	56	·05	47	Showery
	17	58	72	55	30.00	49	Fair
	18	57	66	57	29·90	40	Cloudy
	19	56	68	56	·99	56	Fair
	20	55	67	56	30.01	55	Fair
	21	56	68	57	29.94	51	Fair
	22	57	70	66	.81	60	Showery
	23	66	67	68	.75	42	Showery
	24	69	76	67	30·00	64	Fair
	25	69	75	66	.10	60	Fair
	26	68	74	63	.08	57	Fair

N. B. The Barometer's height is taken at one o'clock.

XXXI. *Account of some Electrical Experiments by M.* De Nelis, *of Malines in the Netherlands: with an Extension of them. By* George John Singer, *and* Andrew Crosse, *Esqrs. Communicated by Mr.* Singer.

During the last fifteen years, M. De Nelis has devoted considerable attention to the subject of electricity, which he states he was led to study in consequence of a thunder storm, which accidentally struck his house whilst his family were assembled round a table in the dining parlour.

The experiments he has made are very numerous, and present some facts of an interesting nature. The present account of them is selected in part from his recent communications to Mr. Tilloch, and partly from his correspondence with M. De la Metherie.

The first experiments were made with hollow cylinders of metal, open at top, and partly filled with water. In the centre of each cylinder a needle was insulated, by surrounding it with wax in such a manner as to keep it at an equal distance from the sides of the cylinder, whilst a thin slip of lead, or a piece of leaden wire attached to the lower end of the needle, was thus kept in the axis of the cylinder, and with its point resting on the bottom thereof.

In this disposition of the apparatus the metallic cylinder was connected with the outside of a large electrical battery, and the circuit was completed by means of the discharger, by connecting the internal coating of the battery with the upper end of the needle; the charge consequently passed through the small leaden wire, which was thus melted under water in the centre of a metallic vessel.

The effects produced by this experiment are, 1st, The needle and part of the water contained in the cylinder are projected from it with considerable force; and, 2dly, The cylinder itself is expanded more or less in proportion to its power of resistance. Very thin cylinders yield to the first explosion; but thicker ones require many repetitions of the experiment, and usually become undulated on the surface, whilst they expand gradually: at each explosion the expansion increases, and at length the cylinder is cracked or burst open.

M. De Nelis has employed cylinders of various metals, and of different thickness; some of wide and others of narrow bore. In general the effect is greatest with those of narrow bore when the thickness remains the same; but in comparative experiments with different metals and alloys, he did not find that their relative resistance corresponded with the tenacity assigned to them in the usual tables. The alloys appeared to resist more than the

 simple

simple metals; for a cylinder of pure silver was cracked at the fourth explosion, whilst a similar cylinder of sterling silver resisted eleven explosions without injury; and when any soldered metal was employed, the solder was found less susceptible of injury than the metal soldered. From these circumstances M. De Nelis is disposed to ascribe the effects produced, rather to the attraction of the electric fluid for the metals, than to the expansion which its rapid passage through imperfect conductors so invariably produces.

Anxious to ascertain the extent to which these effects might be carried, he employed a cylinder of iron, 18 lines diameter* and 27 lines high, having a hole drilled in its axis of one line and a half diameter and 18 lines deep; and it was torn open by 70 discharges of a battery of 100 feet of coating. The results of former experiments having rendered it probable that this large cylinder had yielded to a lower power than would have been requisite had the iron been perfect, another cylinder of the same size was made with great care, and 70 explosions produced no effect upon it. The interior of this cylinder was then filled with olive oil instead of water: in 30 explosions the bottom was perforated, though nine lines thick; the lateral expansion was very inconsiderable. M. De Nelis supposes that in these experiments the leaden wire is minutely divided, and combines with the oil or water and with the electric fluid to form a gaseous product, which exerts a powerful action on the metal cylinder by which it is surrounded. By this he appears not merely to express an expansive effect, but a peculiar attraction which he conceives to exist between the electric fluid and other bodies, and to which he applies the general term molecular attraction. This peculiar action he thinks decisively evinced by the last experiment; in which the bottom of the cylinder, a thickness of nine lines of excellent iron, was perforated, whilst the sides remained nearly without injury. By an extension of this experiment still more remarkable effects were produced: it was imagined that by increasing the thickness of the cylinder the lateral expansion would be entirely prevented, and the action of the charge be confined to its molecular attraction on the bottom of the cylinder. An iron cylinder of 28 lines diameter was drilled to within five lines of the bottom, and a cylinder of pure silver of the same size was procured; they were submitted in succession to the action of the battery. 200 explosions perforated the bottom of the iron cylinder, and produced an excavation of three lines diameter. With the silver cylinder 20 explosions occasioned an expansion at bottom, which con-

* One inch and a half English; the line being one-twelfth of an inch nearly.

tinued

tinued to increase to the 200dth explosion when it had produced a projection of 18 lines diameter and three lines high, but no perforation was apparent. This cylinder was sawed through, and the expansion which had taken place in the interior appeared to have been produced by the action of some power determined in every direction from a centre. M. De Nelis appears to consider these experiments as a demonstration of the different molecular attraction of the electric fluid for various metals: but I must confess that to me they seem to present no such evidence, nor do they appear to differ materially from the usual expansive effects of the charge. When electricity passes luminously through any fluid, it invariably produces a sudden and temporary expansion therein, and the expansion is greater in proportion to the quantity and momentum of the electricity, and to the nonconducting power of the fluid through which it passes: hence the spark is brighter and the expansion more considerable in oil, alcohol, or ether, than in water; more feeble in hot water than in cold, and still less evident in saline fluids and concentrated acids; but in every case the expansion is greatest when the bulk of the fluid in which it occurs is least, and when the spark by which it is produced passes through the greatest interval. The arrangement employed by M. De Nelis is well calculated to obtain these essentials; for the small leaden wire which he has substituted for the interrupted circuit of former electricians, admits the passage of a much more considerable charge than could be conveyed through the oil or water itself by the same power; whilst the fluid employed is so arranged as to occupy the least possible space compatible with the luminous passage of the charge through it.

In the experiments I have made on this subject I have been favoured with the assistance of my friend Andrew Crosse, Esq.; and it was by the aid of his powerful apparatus and active exertions that I was enabled to shorten the time of their performance materially. M. De Nelis generally employed a battery of 100 feet of coated surface, which he charged to about 60 of Henley's quadrant electrometer; this was most probably equal to 15 grains of Cuthbertson's discharging electrometer, at least it is so when the index of Henley's quadrant is terminated by a pith-ball; but a cork ball will only indicate about 40 with the same charge. 15 grains appeared therefore the most convenient power, and was employed in these experiments most frequently. The battery we made use of consists of 50 coated jars, exposing about 75 feet of coated surface; it was charged by two cylinder machines, one of them 52 inches circumference, and the other 40 inches only. The large machine I constructed in the year 1808: it is on the whole the best I have yet seen. The smaller

 one

one was adapted to the purpose of these experiments for the occasion, and by a simple mechanical arrangement was made equal in charging power to the large one.

The joint action of these machines enabled us to charge the whole battery of 75 feet coating in two minutes; and this rate of charging was maintained through the whole of our experiments.

The metallic cylinders were all of the same size; namely, six lines diameter, and 24 lines high, with a bore of $2\frac{1}{2}$ lines diameter and 18 deep. We were provided with two of each of the following metals,—Bismuth, zinc, tin, lead, iron, copper, and brass. A steel wire or needle, about the 40th of an inch thick and three inches long, was then coated with wax in two places, to such a thickness as to admit of its sliding freely into the bore of either of the cylinders. The length of the needle was then prolonged by the addition of a piece of leaden wire $\frac{1}{100}$th of an inch diameter and nine lines long to its lower extremity, in close contact with which it was secured by the same wax. Fig. 1, Plate III, represents this needle. W is the fine leaden wire, and CC the coatings of wax. All the precaution necessary in preparing such needles, is to place the wax round them with care, so as to keep the needle in the centre of the metallic cylinder when it is placed therein, during which process it is necessary to keep the leaden wire as straight as possible, that its end may rest on the bottom of the cylindrical cavity without approaching any where in contact with its sides. Several of these needles may be prepared before the commencement of the experiments, as it is necessary to employ a fresh one at every explosion. Fig. 2 represents one of the cylinders; its cavity (which is shown by dotted lines) is to be filled with water or oil, and a needle prepared as above, introduced carefully into it until the point of the leaden wire rests on the bottom of the cylinder. To prevent the oil or water from being thrown about, the cylinder should be placed in a wooden box, open on one side and lined with lead at bottom: the bottom being connected with the outside of the battery, a communication is made by a wire, between the upper part of the needle and the receiving ball of Cuthbertson's discharging electrometer, the electrometer being connected with the inside of the battery. This arrangement is shown in fig. 3.

In this disposition of the apparatus, a cylinder of bismuth filled with water was subjected to the action of the battery of 75 feet, charged to 15 grains. A single explosion shattered it to pieces, and dispersed the parts to a distance with considerable violence.

Another cylinder of bismuth the same size as the preceding was

was exposed to a charge of eight grains only: it was dispersed with nearly the same violence in one explosion.

A cylinder of zinc of the same dimensions and under the same arrangement, with a charge of 15 grains was slightly cracked at the first explosion, and at the ninth explosion was opened by a wide rent, as shown in fig. 4.

A similar cylinder of zinc was filled with olive oil instead of water; one explosion broke a large piece out and threw it to a distance. This cylinder is represented in its present state by fig. 5.

A cylinder of tin of the same size filled with olive oil, was split open by one explosion of a charge of 15 grains; the rent being very similar to that of fig. 4.

A second cylinder of tin, the same size as the preceding, and under the same arrangement, gave a different result: it was expanded by the first explosion, and more so by the second and third; at the fourth it burst open: the rent was more considerable than in the last experiment.

A cylinder of iron of the same dimensions was arranged in the usual manner with olive oil, and a charge of 15 grains: the first explosion produced two cracks nearly the whole length of the cylinder; the second explosion detached a portion of the cylinder (nearly one-third) in the direction of its length, and threw it with violence to a distance.

The experiment was repeated with another cylinder of iron of the same size: the first explosion produced three cracks; the second explosion divided the cylinder in three parts in a very curious manner, as represented by fig. 6.

The thickness of these cylinders is greater than that of the strongest muskets; the expansive power of electricity acting in this way is therefore vastly superior to the most potent gunpowder.

A cylinder of copper became undulated on the surface at the first explosion; it expanded at the second, and continued to increase with every discharge until the twelfth, when it was burst open nearly in the same manner as fig. 4.

A second cylinder of copper exhibited nearly similar phænomena: it did not crack until the 14th explosion; but at the 15th it was torn with a wide rent.

A cylinder of brass, under similar circumstances, was undulated on the surface, and had a small perforation at the first explosion: these effects and the expansion of the cylinder continued to increase with each discharge until the sixth, when a wide rent was produced.

Another cylinder of brass, of the same description, exhibited

 nearly

nearly similar phænomena, and was burst with analogous appearances at the fifth explosion: it had opened at the fourth.

A leaden cylinder of the same size was very much swelled by the first explosion, and perforated in three places: these effects increased greatly with the second; and by the third it was burst open in a very curious manner, as is shown by fig. 7.

This experiment was repeated with another cylinder of lead, with precisely the same result, and in the same number of explosions.

A cylindrical tin vessel, four inches diameter and six inches in height, was suspended by strings a short distance from the ground in the open air, on the lawn in front of the electrical room: the can was filled with water, and a leaden wire of two inches long suspended within it, with its end touching the bottom of the can. A charge of the battery of 75 feet, to 15 grains, being passed through the leaden wire, it was melted; about half the water was projected into the air to a considerable height, and the flat bottom of the can was bulged outwards, so as to assume a spherical surface.

M. De Nelis states that he made an experiment of this kind with a cylinder of two inches diameter; and that the water was projected to the height of 40 feet.

He also exploded a wire in a leaden tube of an inch diameter, which was filled with water, and its ends stopped with corks; the corks were expelled with violence, and the tube much expanded.

Fig. 8 represents a thick cylinder in which the lead wire, fig. 9, was introduced: the cylinder had a hole drilled in its centre like a flute, so that the oil might be permitted to escape when expanded; but by three explosions of a battery of 75 feet the rents exhibited in the figure were produced.

These experiments certainly present very remarkable and permanent evidence of the expansive power of the electric charge; and it is difficult to contemplate such extraordinary mechanical effects, without admitting that the power by which they are produced has at least the leading characteristics of a material substance.

Broomfield, near Taunton, August 28, 1815. G. J. Singer.

☞ Some other experiments of M. De Nelis will form the subject of another communication.

XXXII. *On the Diffusion of Heat at the Surface of the Earth.* By John Murray, *M.D. F.R.S. Ed.**

An argument which I had stated against the Huttonian Theory of the Earth, in so far as it relates to the operation of a central heat, was honoured some time ago with a reply by a distinguished member of the Society. Respect for the opinion of Mr. Playfair led me to consider attentively the reasoning he employed; and still feeling some confidence in the grounds on which the original argument rests, I propose to offer a few observations with regard to it. The question further involves the consideration of the mode in which heat is distributed at the surface of the globe. This, when minutely investigated, presents a very perfect arrangement, by which the escape of caloric is prevented, while its equal distribution is more effectually attained; and the subject, under this point of view, may have some interest, independent of its relation to any controversial discussion.

The argument which I had advanced is, That if a heat exist in the interior part of the earth, operating for an indefinite period, as is assumed in the Huttonian theory, it cannot for such a period remain locally accumulated. It must diffuse itself through the entire mass, and become at length equal, or nearly so, over the whole. An equilibrium of temperature must therefore be established, incompatible with that system of indefinitely renewed operations which is represented as the great excellence of the system. This has always appeared to me conclusive; and an argument such as this, derived *à priori*, and directed to the first principles of a geological theory, if successful, is of greater weight than arguments derived from its adaptation to natural phænomena, which, even when they appear to be just, amount only to probability, and, from our imperfect knowledge of the relations of the mineral kingdom, leave often some degree of uncertainty †.

To obviate this argument, the following reasoning has been employed by Mr. Playfair. The diffusion of temperature, he remarks, is a consequence of the tendency of heat to pass from bodies

* From the Transactions of the Royal Society of Edinburgh.

† I ought to remark, that this view of the constant existence of a central heat is not considered by all the defenders of Dr. Hutton's system as a necessary part of it, nor do they even regard it as a position which he himself maintained. They suppose the existence only of interior local heat, which may cease for a time, and be again renewed; and to this hypothesis the above argument does not so strictly apply. I must only add, that if even this view of the subject be adopted, much of the difficulty will still remain in its original force; it becomes liable too, if I mistake not, to other objections peculiar to itself, equally important; and by adopting it, much

bodies where the temperature is higher to those where it is lower. It is not, therefore, a necessary result, but is only contingent, requiring the presence of another condition, which may be wanting, and actually is wanting, in many instances;—this is, that the quantity of heat in the system should be given, and should not admit of continual increase from one quarter, nor diminution from another. When such increase and diminution take place, no such equilibrium can be attained. In proof of this, he mentions the fact, that a bar of iron thrust into the fire, though red-hot at one extremity, will not become so at the other in any length of time, but each part of it will have a fixed temperature, lower as it is further from the fire, but remaining invariable while the condition of the fire, and of the surrounding medium, continues the same. He illustrates it also more fully by the following example: Let A, B, C, D, &c. be a series of contiguous bodies, or let them be parts of the same body; and let us suppose that A receives from some cause, into the nature of which we are not here to inquire, a constant and uniform supply of heat. It is plain that heat will flow continually from A to B, from B to C, &c.; and in order that this may take place, A must be hotter than B, B than C, and so on; so that no uniform distribution of heat can ever take place. The state to which the system will tend, and at which, after a certain time, it must arrive, is one in which the momentary increase of the heat of each body is just equal to its momentary decrease, so that the temperature of each individual body becomes fixed, all these temperatures together forming a series decreasing from A downwards. This is then applied to the argument in the following manner: "If heat be communicated to a solid mass, like the earth, from some source or reservoir in its interior, it must go off from the centre on all sides towards the circumference. On arriving at the circumference, if it were hindered from proceeding further, and if space or vacuity presented to heat an impenetrable barrier, then an accumulation of it at the surface, and at last a uniform distribution of it through the whole mass, would inevitably be the consequence. But if heat may be lost and dissipated in the boundless fields of vacuity, or of ether, which surround the earth, no such equilibrium can be established.

much of the beauty and unity of the system is lost. These seem to me to require the assumption of a *central heat*, or general reservoir of heat capable of extending its action to every part of the circumference, always existing, though not equally active in its *apparent* effects. It is to this view of the subject, consistent, I believe, with the original statement of the theory by Dr. Hutton, that the argument applies. It is this which Mr. Playfair admits, and on the admission of which, indeed, his reasoning is founded: and, strictly speaking, it is to *his* reasoning only that the observations in this paper are directed.

The

The temperature of the earth will then continue to augment, only till the heat which issues from it every moment into the surrounding medium become equal to the increase which it receives every moment from the supposed central reservoir. When this happens, the temperature at the superficies can undergo no further change, and a similar effect must take place with respect to every one of the spherical and concentric strata into which we may conceive the solid mass of the globe to be divided. Each of these must in time come to a temperature at which it will give out as much heat to the contiguous stratum on the outside, as it receives from the contiguous stratum on the inside; and when this happens, its temperature will remain invariable*."

The principle on which this reasoning rests will not be disputed. Admitting it therefore, my objects in attempting to support the original argument will be to show, *first*, that such a discharge of heat from the surface of the earth as is here supposed does not take place; and, *secondly*, that if it did, this would be as subversive of the system, as if the heat were retained.

There are two modes in which caloric may be supposed to be conveyed from the surface of the earth; one is by radiation, the other by slow communication by the medium of the elastic fluid which surrounds it. Each of these may be briefly considered.

The great expansibility and mobility of an elastic fluid, such as the atmosphere, give rise to a peculiarity with regard to the communication of temperature through it. Its proper conducting power appears to be inconsiderable; but whatever it may be, it is principally by the motion of its parts that heat is diffused through its mass. When the temperature of a lower portion of such a fluid is raised, it ascends rapidly; a fresh portion comes in contact with the surface communicating heat; and by this successive application and retrocession of the air, and this movement of the heated portion, rather than by the direct communication of caloric from one part to another, the temperature of the whole is raised. It is in this manner that heat is diffused from the earth into the atmosphere. The air incumbent on any part of the surface communicating heat, is, by the elevation of its temperature, rarefied, whence an ascending current is formed; and the common opinion with regard to this is, that it carries the excess of heat to the higher regions of the atmosphere, and may allow it to be diffused into the interplanetary space. It is on the assumption of this that the heat must be supposed, in Mr. Playfair's argument, to be discharged from the earth, so far at least as it is conveyed from the surface by the surrounding

* Transactions of the Society, vol. vi. p. 356.

elastic medium. The just view of the operation of the atmosphere in diffusing heat, is however more complicated; other conditions connected with it are to be taken into account, and lead, I believe, to a very different result.

The air heated at any part of the earth's surface, may, as it ascends, impart, especially at first, a portion of heat to the air with which it comes in contact. But as it rises, becoming subject to less pressure, it expands; by this its capacity for heat is augmented, and its temperature, therefore, falls proportionally. As it recedes from the surface, with its temperature thus constantly falling, from its increasing capacity, its tendency to part with heat is always becoming less; and as it must continue to rise in the atmosphere until it attain an equilibrium of temperature with the air around, any tendency to communicate heat to that air must at length cease. If it retained its high temperature, or if it were to lose this gradually only from the communication of its heat, it might be conceived to convey caloric onwards; but its capacity for caloric increasing from its rarefaction as it ascends, it is enabled to retain the excess of heat it had received, without having a corresponding elevation of temperature. It is only when it returns towards the surface, in consequence of that circulation which the constant ascent of portions of heated air establishes in the atmosphere, that this is evolved. As it descends, it becomes progressively subject to greater pressure, its capacity gradually diminishes; and continuing to do so as it falls, it gives out in the same gradual manner the excess of heat which it contains. Thus, for every portion of heat conveyed by the ascent of a stratum of heated air from any part of the circumference of the globe, a corresponding portion of heat is given out by a descending stratum at some other part; and as this communication of heat from the atmosphere will happen principally at the colder parts of the earth's surface, both as the descent of the air will be there greatest, and the disposition to receive heat also greatest, the whole forms an admirable arrangement to counteract local inequalities of temperature, to diffuse heat equally over the globe, and to prevent any dissipation of it beyond the sphere of the atmosphere.

It is thus, I conceive, demonstrated, that by the principal mode in which heat is propagated through the atmosphere,—that, by the motion of the heated portion of air, it can only be withdrawn to comparatively a short distance from the surface of the earth, and that there is a constant return of it. By direct communication its conveyance must be equally limited; the conducting power of an aëriform body is extremely imperfect, and, there is every reason to believe, becomes less as the fluid becomes more rare; and in the higher regions of the atmosphere the

the subtilty of the medium is so great, that beyond a certain height it cannot be supposed to be the vehicle of the conveyance of heat, far less that it can convey it into the boundless fields of vacuity.

But, further, were the conducting power of the most perfect kind, no effect could arise from it in the conveyance of caloric beyond a certain height. The communication of heat from one part of a mass of matter to another, or from one body to another, depends, as Mr. Playfair justly observes, on difference of temperature; and if there is no difference, there will be no such communication. Now, a little reflection will show, that this completely limits the diffusion of heat through an elastic fluid. receiving it under such conditions as our atmosphere. The air heated at the surface expanding as it rises, but at the same time from this expanion falling in temperature, must come at length to be in equilibrium, both in density and in temperature, with air at a certain height; its further ascent will then cease, and its temperture being the same with that of the air around it, it will yield none of its heat. It is only in its descent, as a fresh portion of air rises, that it will evolve caloric, and it will continue uniformly to do so, as it returns to the surface of the earth.

We thus trace a curious provision to prevent any discharge of heat by the atmosphere into the interplanetary space; the excess at particular regions is only withdrawn, is more equally distributed over the surface, but is ultimately communicated to the earth; and there is no other apparent arrangement by which this could have been attained, than by an atmosphere constituted as ours is,—expanding as it is heated, and falling in temperature from augmentation of capacity as it expands. To admit of an unlimited conveyance of heat, a solid mass, however subtile, would have been required, or an atmosphere the capacity of which for caloric should not increase with its rarefaction.

The passage of heat is thus from the circumference towards the centre of the globe, instead of the opposite direction, and it is prevented from accumulating at the surface only by being conveyed into the mass of earth.

These conclusions may be applied to the present argument. If the view I have stated be just, we have demonstration that the atmosphere conveys no heat into unlimited space; our planet, in relation to the discharge of caloric from it, is bounded as it were by a wall of non-conducting matter, and no arrangement can be imagined more perfect, by which the heat belonging to it could be confined. If the heat from the centre, Mr. Playfair observes, on arriving at the circumference, were prevented from proceeding further, and if space or vacuity presented to heat an impenetrable barrier, then an accumulation of it at the surface, and

and at last a uniform distribution of it through the whole mass, would inevitably be the consequence. It appears that the atmosphere does present such a barrier, and the consequence, therefore, necessarily follows*.

This is true, at least, so far as relates to any power of the atmosphere to convey heat. There is still to be considered, however, another mode in which caloric may be supposed to be discharged from our planet,—that by radiation. Rays possessed of heating power are thrown off from a body at a high temperature, and by the discharge of these its temperature is reduced. But various considerations show, that this would be a very inadequate source of the escape of heat from the interior part of the earth.

Thus the radiation of caloric is inconsiderable, except from a body which is heated; and the quantity radiated increases as the temperature rises in a much higher ratio than the increase of temperature itself. At low temperatures, therefore, it must be extremely small. At 100° it is scarcely apparent, from experiment; and at 50° is not sensible. Not only, too, does the quantity diminish rapidly with the temperature, but the projectile force of the rays emitted becomes less, so that those which are discharged at low temperatures are incapable of penetrating *media* such as glass, which those at high temperatures penetrate with facility. And as even the latter are, to a certain extent, intercepted by the atmospheric air, the former must be more completely arrested. At 50°, therefore, the medium temperature of the globe, and which prevails over so large a portion of its surface, we have no reason to believe that a discharge of caloric by radiation, into remote space, can take place to any extent.

At the parts of the surface of the earth which are at still lower temperatures, and at those where intense cold prevails, the supposition of any radiation is of course still more precluded, and the negative even may be proved. When a solid body, and the surrounding elastic medium, are at the same temperature, there

* If the sphere from the centre of which heat is supposed to be diffused, be not exposed to an unequal external source of heat, the heat will be communicated equally at its circumference to the incumbent atmosphere, and produce ascent and descent of the air, with little of that circulation of it from one part to another, which is the consequence of inequality of temperature at the surface of the solid. But still the same changes of capacity for heat will accompany the ascent and descent of the aërial mass, and the principal effect will be, to accumulate temperature at the surface. The condition is one, however, which need not to be taken into account, as it is one which has never existed with regard to our planet; and the argument remains as is stated above, while there is inequality in the distribution of heat over the circumference of the globe.

seems

seems to be no discharge of radiant heat from the former; and still more, when the temperature of the solid is lower than that of the elastic medium, (and this is usually the case in colder regions,) it is rather disposed to absorb than to emit caloric by radiation. The resistance, too, opposed by the atmosphere to the discharge of rays having the weak projectile force which those emitted at such a low temperature must have, would in a great measure prevent their escape, if the power to radiate did actually exist.

At the hotter parts of the earth's surface there may be some emission of caloric by radiation, but it is not difficult to show, that the quantity of this cannot be equal to the quantity communicated by the solar rays; for of the heat derived from the latter source, a portion is absorbed by the earth, and conveyed to the interior, as is apparent from the decreasing temperature, as we recede from the surface to a certain depth; and another portion is carried off by the ascending current of heated air, and conveyed to colder regions, where it is also absorbed.

Thus, even from those parts of the surface of the earth where the circumstances are those most favourable to radiation, the quantity radiated cannot be equal to the quantity received by the solar rays. Over the whole surface, the difference must be still greater; and instead of the conclusion, that this planet discharges an excess of heat by radiation, there is every reason to draw the opposite conclusion,—that part of the heat which it receives from the sun is retained.

The only reason, at least, that would lead to hesitation in admitting this last inference, is a hypothetical one,—that it is inconsistent with that perfect arrangement, which we are disposed to believe exists among all the heavenly bodies, whence the stability of the entire system is maintained, and whence, therefore, in relation to any communication of caloric among them, the quantity radiated by each will, on the whole, be equal to what it receives. This, whether we restrict the view to the different parts of our solar system, or extend it even to the universe, is no doubt the conclusion which the imagination is naturally disposed to embrace, though it may not be supported on any decisive evidence, or may even be in some measure apparently in opposition to the fact. But to suppose that this planet discharges more caloric than it receives, would be not only in opposition to evidence, but in opposition to the very principle which alone leads to the admission that the quantity discharged may be equal to the quantity received.

Concluding, then, as it appears to me may be strictly inferred from the preceding reasoning, that our earth does not discharge caloric from its surface into the regions of space, or at least does not

not discharge more than it receives from the other heavenly bodies, if it discharge even this, the argument recurs, I believe, in its original form, as sufficiently established, that if an interior heat exist, it must be diffused through the substance of the earth, and an equilibrium of temperature be at length established, incompatible with those operations ascribed to its action in the Huttonian theory.

But further, though the heat were not retained,—though it were granted that it is propagated into the regions of space, this does not render the argument I have urged less conclusive. The strength of that argument lies in the circumstance of the diffusion of heat from the interior, not in the accumulation of that heat at the surface. The latter is a contingent event, which may or may not happen, without being of essential importance in the conclusion to be drawn. The former is a necessary result, which must prove subversive of the arrangement, the assumption of which constitutes the leading principle of Dr. Hutton's system.

Mr. Playfair, accordingly, in the general case which he takes for the illustration of his argument, assumes *a constant supply of heat at the centre, as well as a constant escape of it from the circumference.* But in applying the argument, where is the proof of such a supply of heat at the centre of the earth? It can scarcely be necessary to remark, that none is attempted to be given. And were it allowable to make an assumption, without evidence, merely to obviate an argument, the force of which cannot otherwise be avoided, no hypothesis, even the most extravagant, could ever be overturned. Heat cannot originate in nothing; and if we assume its constant discharge from the circumference of this planet, what cause is it possible to assign for its constant supply from the interior? Admit even its existence in any degree of intensity, still it is obvious that it must be in limited quantity. If we speak of a *spring* of heat, as conveying the idea of an unlimited supply, we deceive ourselves by the use of a term to which no definite signification can be affixed, but what is subversive of the reasoning it is designed to support. A source or spring means nothing more than a hidden reservoir connected with external supplies. There can be no reservoir of heat in the centre of the earth, which, without being recruited by constant supply, shall continue to furnish it, to be discharged from the circumference through indefinite time into unlimited space. And it is not possible to imagine any circulation by which it shall be restored.

It is, therefore, I conceive, of little importance in the discussion, whether the heat supposed to be conveyed from the centre to the circumference is accumulated there, or is discharged into the regions of space. Its propagation to the surface must be sub-

subversive of its accumulation in the interior. Either the diffusion alone, therefore, or the diffusion attended with the final result of equilibrium of temperature, is sufficient for the refutation of the hypothesis. Both I conceive are established. And I would still regard the argument in the light in which I first advanced it, as a demonstration of the fallacy of the leading principle of the Huttonian system, which assumes the existence of an internal heat operating at renewed periods for indefinite time.

There are some other points of view under which the subject may be considered, in which the difficulties attending the assumption of an internal heat, when connected with the law which Mr. Playfair has illustrated, appear in a very strong light.

It has always been found difficult to give any account of the origin of the supposed internal heat, to the operation of which the consolidation of minerals, and the elevation of the habitable land from the bed of the ocean, have been ascribed. If the view be restricted to the production even of one world, such an intensity and continuance of heat are necessary for this single effect, that no adequate cause can be even hypothetically assigned for it. If it be extended to the successive production of three or four worlds, each embracing a period between its formation and destruction in which millions of years must elapse,—an event which, according to the theory, is not only possible, but has actually occurred,—the heat required is altogether beyond what the imagination can conceive. But if, in addition to this, caloric is also diffused from the interior, and discharged from the circumference of the globe into unlimited space, we are lost in the magnitude of the result, and are unable to acquire a just conception of the force of the argument, from the impossibility of contemplating clearly the difficulty in all its extent.

The difficulty, from the intensity of the heat which must be assumed to exist, is not less great than that from its continuance and waste. It is sufficiently apparent, when we consider that the highest mountains of the globe run in extensive chains, and are so connected, that they must have been formed at one time, and that they are composed of materials which a very intense heat does not fuse. But this is nothing compared with the statement which must be made, in consequence of the law, that the internal temperature is a *decreasing* one from the interior to the circumference of the globe.

If we can discover the rate of this decrease, by knowing the temperature which exists at two distant points, we may of course form some calculation of the intensity of the heat which exists at the commencement of the series. Now this we have the means of determining with considerable precision. At the bottom

tom of the sea, or within a short distance from it, the heat from the interior must be at a degree of intensity sufficient to produce mineral fusion and consolidation from the disintegrated materials of a former land, which may be estimated from our knowledge of the fusibility of these bodies. It is propagated from this onwards, with such a decrease that at the surface there is no sensible high temperature. Its diffusion from the central regions to the bottom of the sea must of course have been at a similar rate of diminution. If we were to calculate the rate of progression, and compare it with the distances in the two portions of space,—that from the central region to the bottom of the sea, and that from the bottom of the sea to the surface of the land,—we shall find an intensity of heat in the interior, compared with which the heat necessary to melt mountains of quartz, formerly supposed to present so great a difficulty, is a mere atom in the scale, scarcely affording even a point of comparison.

Some idea may be formed of this, by recurring to the illustration of the iron bar, with a decreasing temperature, making the most liberal allowance in favour of the Huttonian hypothesis, with regard to the respective portions of space. Thus, the bar being one thousand inches in length, if its temperature at the one extremity be 50°, and if within five inches of this it is at a white heat, then the heat increasing at the same rate, through every succeeding five inches, what must be its intensity at the other extremity? No effort of the imagination can form the most remote conception of it, nor can any argument be wanting to prove, that no such heat can exist in the interior of the earth.

If, to avoid the difficulty, a less rapid decrement of temperature be supposed; then, from a heat of that intensity which must be assumed to exist at the bottom of the ocean, to produce the effects ascribed to it, the decrease in the short space between that and the surface cannot be such as to bring the temperature within that which is at all compatible with the established œconomy of nature. The difficulty is, therefore, insurmountable; it must occur on the one hand or on the other; and it is not merely connected with Mr. Playfair's argument; but, as that argument is founded on a law perfectly just with regard to the diffusion of temperature, it is a difficulty which necessarily follows from the assumption of a central heat, or of any internal heat such as that which must be assumed in the Huttonian theory,—a heat which is to operate around the whole circumference of the globe, continue its operations for such immense periods, and renew it for indefinite time.

Leaving the consideration of this subject, in so far as it is connected with the argument on the Huttonian theory, I may add

add a few observations of a more general nature, which the preceding reasoning suggests with regard to the temperature of the globe, and its relation to solar heat. From the nature of the agency of the atmosphere in diffusing heat, the conclusions seem to me almost necessarily to follow,—that there is a tendency to equalization of temperature over the whole surface of the earth, —that this continues to operate in such a manner, that in the progress of time the difference at different parts must become less than what existed at a preceding period; and that, ultimately, a temperature nearly uniform shall be established over the whole.

At the hotter parts of the earth's surface, the temperature cannot increase, or must increase very slowly, and to no great extent; for, if it were to rise higher, the ascent of heated air from it, and the transfer of this to the colder parts of the surface, would only become more rapid. But the temperature at the colder regions may rise higher; for the direct ascent of heated air is there less abundant, and what recedes in a lateral direction, does so, deprived of caloric, which it has yielded to the earth. Whatever discharge of caloric, too, may take place by radiation, must be principally from the hotter parts of the surface; from the colder it must be much less considerable, for the quantity radiated is less as the temperature is low: it increases, too, at a higher ratio than the increase of temperature; and hence, if increased radiation from increased temperature did take place at both, being greater at the former than at the latter, its effects would be more considerable in retarding a further augmentation of temperature. Thus it appears, that the causes counteracting rise of temperature at the surface of the globe, act most powerfully at those parts where it is high, and any progressive rise, therefore, must be principally at those where it is low.

The effect of this arrangement may be most obvious, perhaps, from comparing the two extremes. At the equator, the ascending current of warm air, carrying off heat, is constant or nearly so; towards the poles, the descending current imparting heat, must be equally constant. The heat directly communicated at the former by the solar rays, and the heat communicated at the latter by the current of warm air, will both be in part conveyed through the solid mass towards the interior; but this will always be with a decreasing temperature as it proceeds,—that is to say, the first or exterior layer will be at a higher temperature than the second, the second than the third, and the accumulation, therefore, will be at the surface, to a certain extent. At the poles this may continue to proceed, because heat is there communicated without an equal abstraction. But at the equator it will

will remain stationary, or nearly so, as no increase can take place without the abstraction both by the atmosphere and by radiation becoming proportionally greater.

This is aided by another effect, that with regard to the diffusion of heat through the solid mass itself. This diffusion from the surface proceeds in part towards the centre, or each layer, as the surface is receded from, receives a portion of heat from the exterior one, and this continues onwards, though with a gradual diminution. But there must be at the same time a diffusion more nearly horizontal, proceeding through these layers from the equator towards the poles, while there can be none in the opposite direction to counteract this effect.

The ocean, too, serves to convey a considerable portion of heat from the warmer to the colder regions of the earth, by the movement of currents, somewhat similar to those which exist in the atmosphere, and the course of which it is even possible to trace.

Thus, by these various arrangements, whatever excess of heat may be received by this planet from the sun, and retained at the surface, will be permanently accumulated towards the poles, and the temperature there will increase. In all the portions of the intermediate space, between the poles and the equator, the same law will operate, though with decreasing force; and over the whole surface, there is a tendency to equality of temperature, which, however slow the progression to it may be, must, as the result of general causes constant in their operation, be finally established.

The speculations, then, on which some have dwelt,—that the northern parts of our globe have suffered a gradual refrigeration, and which Bailly, in particular, applied to the fanciful system which he supported with so much ingenuity,—that civilization and science have descended from the elevated regions of the north of Asia,—have probably no foundation. It is always interesting to trace the succession of opinions which mark the progress of knowledge, and to observe how far what at one period is considered as established, is at another rendered doubtful, or proved to be false. The refrigeration of the globe from the loss of its interior heat, is a fact, says the author just referred to, of which there can be no doubt; and this refrigeration, he adds, must have been principally towards the poles, partly from the flattening of the sphere there, in consequence of which the heat from the centre must escape sooner, but still more from the unequal action of the solar rays, which is more intense towards the equator. Hence the countries round the poles must have been the first that were habitable, and this gradual cooling has caused the

the same temperature to proceed successively over all the regions of the globe*. Not only are these conclusions unfounded, but the changes which have occurred are probably the very reverse. No heat can escape from this planet, but in consequence of a high temperature being kept up at its surface by the communication of heat from an external source. Until this be attained, it must retain the heat it receives from the sun; and this must accumulate principally towards the poles.

That the temperature at the surface has risen above the original temperature of the earth, may be inferred not only from the consideration, that solar heat has been communicated to it, which could not be discharged until a certain elevated temperature were attained, but also from this, that there is no natural operation actually generating cold; there is only the production of heat; and cold prevails where this is less powerful, or is counteracted. To account, therefore, for the low temperature at the colder regions of the earth, only two suppositions can be advanced. Either the original temperature must have been as low as this, or lower, and have been raised higher, where it is actually superior, by the reception of the sun's rays: or it must be assumed, with Buffon and Bailly, that heat can escape from this planet to an indefinite extent, and that it is in a state of progressive refrigeration, the effects of which are prevented from being apparent at the equatorial regions by the direct communication of solar heat, but towards the polar circles are evident, as not being counteracted by the same cause. This assumption, however, is precluded by the proof, that heat is not capable of being carried off by the atmosphere, but can escape only by radiation, which is dependent on an elevated temperature. The conclusions seem, therefore, necessarily to follow, that the original temperature of the earth must have been at least as low as the lowest natural temperature, and of course that the mean temperature has been raised.

While there is this rise, and this equalization of temperature at the surface, there must be a corresponding rise in the internal mass. It is obvious that no permanent rise can take place at the external layer, without a portion of heat being diffused to the internal matter at a lower temperature. It is obvious also, that a portion of the heat received necessarily must be conveyed to that matter. And as this diffusion is limited at the centre, the temperature must rise until it become equal, or nearly so, through the whole, and as high, or nearly as high, as that which the solar rays can excite.

Lastly, the rise of temperature must observe certain limits; it

* *Lettres sur l'Origine des Sciences*, p. 329.

must continue until that of the surface is such, that as much caloric is discharged from it by radiation, as is received by the solar rays, and it cannot proceed beyond this. The first conclusion necessarily follows, when it is proved that radiation is the only mode by which caloric can escape from this planet. The second conclusion is equally evident. Thus the tendency is to an equal and uniform temperature. How far this may be from that which at present exists, it is difficult to determine. If the increase of heat is accompanied with its more equal diffusion, so as to establish nearly the same temperature over the entire surface, it may not proceed much beyond that which now prevails at the hotter parts of the earth; for at this, when extended over the whole, the quantity of caloric radiated may be equal to that received. And even if it were to rise higher than this; still, from gradual changes in the laws of organized matter, or in the species of living beings, not greater than what seem to have occurred in the past revolutions of the globe, the existing temperature might be sufficiently compatible with the continuance of animated existence, and with an order not very different even from that which now prevails.

Different views have been presented of the relation of the temperature of the globe to solar heat. Some have imagined that the earth is in a state of progressive refrigeration; and while it was believed that the atmosphere could convey heat onwards without limitation, this conclusion might be drawn. Others, from considering merely the constant communication of solar heat, have drawn the opposite conclusion, that its temperature must increase, and this indefinitely. While a more probable opinion than either, founded on the apparent uniformity of natural operations in those past periods to which any records reach, is, that the temperature has always been the same, or nearly the same, as that which now prevails. The view which follows from the preceding observations is different from all these, and presents a more perfect arrangement. The temperature of the globe must, from the mode in which heat is communicated to it, rise, and at the same time, as it advances, must become more equal over the whole surface. And this rise has its limits; there cannot be either unlimited increase of heat, or indefinite refrigeration; but the final result will be a state of permanence and uniformity, the continuance of which is secured by the very circumstance, that, if it is deviated from, the deviation must check itself.

No view has been presented in physical science of equal grandeur with that established from astronomical observations, that, amid all the revolutions of the heavenly bodies, an order exists whence the irregularities arising from their mutual actions do not

not increase, but are so adjusted, that when they reach certain limits they recede, and within this state of oscillation the stability of the entire system is for ever secured. It would gratify the mind, could it be shown that a similar system exists with regard to the subordinate parts; or, if not, that a state of permanence in these parts will be ultimately established, compatible with the operation of that more general law under which the order of the universe is maintained. In the structure of the globe, however, and in the operations to which it is subject, there are evident causes of disintegration, which seem incompatible with a state of permanence, and from which, in the progress of time, those arrangements which constitute it a habitable world must apparently be subverted. For the Huttonian theory has been claimed the praise (with what justice need not here be inquired) of unfolding a system of renovation corresponding to this waste. In the opposite theory, no similar attempt has been made; its object has been merely to trace the arrangements which exist in the mineral kingdom, and from these to infer the order and mode in which they have been framed; nor have any causes been pointed out, as, indeed, none seem to follow from the principles of the theory, by which that disintegration, the occurrence of which in past periods is so clearly marked, and the operation of which, even at present, is to be traced, may be repaired.

If the view, however, which has been given of the relation of the temperature of the earth to solar heat be just, this deficiency may perhaps be supplied. Inequality of temperature is the great source of change and of disintegration at the surface; the expansion and contraction from alternations of heat and cold, the absorption and expulsion of humidity from the same causes, the distending force of congelation, and the rapid precipitation and flow of water, which are the principal, or rather the sole, disintegrating processes of general or uniform operation, being the results of it. When inequality of temperature, therefore, shall cease, or be restrained within much narrower limits, an order may be established of less vicissitude, and less waste, than that which now prevails, and the stability of which may even remain unimpaired for indefinite time.

This view, if it is not carrying the speculation too far, may even be extended to all the parts of our solar system, and the condition of each planet may be connected in permanence with that law which appears to regulate the constitution of the universe. Considering this earth as passing through a series of revolutions from its chaotic state to a more permanent and perfect form, the different planets may be regarded as in a similar progression. Astronomers have often traced the analogy which exists among them, not only in the laws of their motions, but in the figure of their masses; they have not failed to remark the flattening at the poles, which can be observed in some of them, similar to that of our globe, proving a similar state of fluidity from which this figure has originated; and they have endeavoured even to draw from the aspect which they exhibit, indications of the stage of progression in which they now are*. But they have presented no pleasing prospect of the final adjustment of this series of revolutions. Regarding the planets as extinct suns, or fragments of suns, or at least as masses which have been hot and luminous, they have supposed them to be in a state of gradual refrigeration, which will terminate in the total cessation of movement and animated existence. The assumption on which this gloomy hypothesis is built,—that of the unlimited escape of heat from each planetary mass,—is fortunately as false, as the view to which it leads is unworthy of the order and magnificence which the system of nature displays; and instead of this termination, in what one of these philosophers emphatically calls the state of ice and death, of silence and repose, we may with more confidence look to the equal diffusion of heat through the mass of each planet, as the state of permanence under which it will exist, and to the equal interchange of heat among all, as the perfection of the system they form.

XXXIII. *Geological Observations.* *By* W. H. Gilby, *M.D.*

To Mr. Tilloch.

Sir,—In the present communication, I shall offer a few observations as to the situation which the mountain limestone and red sandstone usually associated with it hold in the Wernerian system. I shall then proceed to make some remarks upon the red ground, chiefly for the purpose of showing how repeatedly the beds of this formation have been confounded with the red sandstone accompanying the mountain limestone; as also to point out the great mistakes that prevail in regard to the beds which may be considered as forming a part of the red ground. Such being the object of my communication, if it is likely to be of any interest to you or your readers, you will oblige me by inserting it.

I am, sir,

Yours very respectfully,

W. H. Gilby, M.D.

* *Histoire de l'Astronomie Moderne*, tom. ii. p. 726.

I be-

I BELIEVE it is an opinion with many, that the limestone which so constantly incloses the coal-fields in England belongs to the transition series. This opinion is most distinctly stated in a recent publication by Dr. Kidd, who has at some length endeavoured to show the correspondence of the gray mountain limestone of Somersetshire, and elsewhere, with the transition limestone of the Wernerian arrangement. This opinion appears to me to be very erroneous; for, in the country which I have described, there are, I think, sufficient grounds for referring it to the first flœtz series. In geological examinations it frequently happens that individual rocks are so indifferently characterized, that it is impossible, without attending to the formations with which they may be accompanied, to refer them to their true position in a systematic arrangement. When a rock, as limestone, occurs in all the grand divisions of a system, it is most hopeless, without such correlative evidence, to guess at its true geological rank; for limestone, whether from a primitive transition or a flœtz country, must still be carbonate of lime; and it is hardly to be expected that the same substance should assume characters decisive of the class to which it belongs. Thus the primitive or granular limestone, which is well characterized by its highly crystalline fracture, sometimes occurs among flœtz and transition strata; but no geologist, in seeing the granular limestone *in situ* with mica slate or gneiss, would hesitate in calling it primitive; for these rocks never occur among those of the flœtz or transition description. In the same manner, no one upon seeing limestone associated with gray wacke, would doubt as to its being transition, for gray wacke is a rock exclusively of transition formation. Provided therefore our system be correct, however puzzling it may be by external characters alone to refer any rock, common to all the divisions of the system, to its true position, by attending to its geological relations we soon find a solution of our difficulties. I have made these remarks, because Dr. Kidd, in considering the mountain limestone to be transition, has been a good deal guided by its sometimes (though never where I have seen it) agreeing, as to its external characters, with those which Mr. Jameson has ascribed to transition limestone, while he has completely misunderstood or overlooked its relation with other rocks.

It will be seen, by referring to my Description of the Neighbourhood of Bristol, that the mountain limestone rests upon a red sandstone. I have only, indeed, mentioned two places where this connexion can be observed; but this I am persuaded is owing to the sandstone occupying the champaign and cultivated parts of the country, so that it is difficult to ascertain its existence and relation with the limestone, excepting where the stratifica-

 tion

tion is displayed by some such section as I have described. My friend Dr. Prichard of Bristol informs me, by letter, that he has observed the same arrangement, *i. e.* the red sandstone dipping under the limestone at the north part of the basin about Cromhall.

Herefordshire and Monmouthshire, as I have remarked in my former paper, exhibit the same stratification, which, according to Dr. Prichard, continues into Brecknockshire*. Other writers have mentioned the same disposition of strata. It appears from Mr. Bakewell's section and account of the geology of Northumberland and Durham, in the Philosophical Magazine for February last, that in crossing the island from the coast of Durham to the Irish Channel, the mountain limestone is immediately succeeded by the red sandstone. Mr. Aikin, in his account of the great coal-field of Shropshire, says that the limestone, which from its lying immediately beneath the coal can be no other than the mountain limestone, is associated to a very considerable extent with sandstone; and in another part he considers this sandstone as identified with the old red sandstone. From these authorities it appears that the mountain limestone is pretty constantly accompanied by the red sandstone, which as constantly is situated below it. I have little doubt that this arrangement will be found still more general through the coal districts in England; but hitherto, most unfortunately, the red sandstone has been perpetually confounded with the red marl, and has been extravagantly considered as one of its beds †. With regard to the geological position of the red sandstone, there can, I think, be but one opinion. It answers exactly to the description of the old red sandstone of Werner, and precisely resembles the red sandstone in the neighbourhood of Edinburgh, and in the isle of Arran, which are considered by Professor Jameson as characteristic localities of the formation in question. This opinion, as I have just now mentioned, is distinctly stated by Mr. Aikin. If, then, this be the old red sandstone of Werner, the first of the flœtz series, we may fairly presume that the limestone,

* Vide Thomson's Annals for June 1815.

† The siliceous pudding-stone, or more properly conglomerate, consisting of pebbles of quartz fixed in a basis of sandstone, which I have described as intervening between the red sandstone and limestone, is found, as I am informed by Professor Jameson, very generally accompanying the red sandstone. It is by him considered merely a variety of this rock. He is of opinion, that the red sandstone is a pure chemical deposit, and that the pebbles which occur so abundantly in particular strata, are to be viewed not as the result of mechanical attrition, but as contemporaneous with the basis in which they are set, being formed by some particular modification in the process of crystallization, by which the siliceous particles were attracted together in the form of pebbles, instead of grains of sand, This

stone, which in so many districts is associated with and lies upon it, is the first flœtz limestone. The bituminous marl-slate which is said to characterize this particular formation, I have found, in its usual situation under the limestone, in the neighbourhood of Bristol. But any thing I can say will be of far less consequence than the opinion of Professor Jameson, who allows me to state that, as far as he can judge from the description I have given and the specimens I have shown him, he considers the red sandstone and mountain limestone as members of the first flœtz formation.

It is a point yet to be ascertained, whether the limestone associated with the coal-field of Derbyshire is to be considered as belonging to the flœtz or transition series. The red sandstone which so commonly accompanies the mountain limestone in other parts of England, is here wanting, and its constant alternation with amygdaloid distinguishes it from the mountain limestone of other quarters. The strata too, or measures accompanying the coal, as far as I can make out from the perplexing language and confused description of those who have seen most of the Derbyshire coal-field, seem to be quite different from those usually associated with the coal in England.

Dr. Prichard, in a late number of the Annals of Philosophy, has endeavoured to show that the mountain limestone belongs to the transition series; because the organic remains which it contains are similar to those which are assigned to transition rocks. And hence he concludes that the first flœtz limestone of Werner, to which another description of fossils belongs, is more recent than the rocks of the coal formations in England. The fact, however, that the fossils of the mountain and transition limestone are of the same description, does not appear to me to warrant the conclusion which he wishes to deduce from it; for, if the mountain limestone be transition, it is very evident that the red sandstone which lies under it must also be transition. But we have seen, and he himself allows it, that this rock

This opinion Mr. Jameson extends to many rocks which appear to exhibit marks of mechanical action, as gray wacke, trap tuff, breccia, &c.; and I think it is only necessary to see the specimens and hear the argument which he advances in support of this opinion, to be convinced of the truth of it. Dr. Kidd in his recent publication, I find, entertains the same opinion, and has advanced many ingenious arguments in favour of it, apparently without being aware that it had long before been proposed by Professor Jameson, in his paper on Conglomerate Rocks, published in the Wernerian Transactions, and has also been announced in Dr. Thomson's Annals of Philosophy. This appears the more strange, because Dr. Kidd seems to have read every author from whom he could derive information on geological subjects, and of course could not but have given the Wernerian Transactions a diligent perusal.

agrees

agrees with the old red sandstone of Werner, the first of the flœtz series. The only legitimate conclusion, therefore, that can be drawn from the fact above stated is, that the same fossils which occur in transition rocks may also occur in those of the first flœtz series.

If then we regard the mountain limestone as a member of the first flœtz series, it may be asked, Does transition limestone occur in England? Geologists in several places have described limestone which certainly widely differs from mountain limestone; and in several instances such limestones appear decidedly transition. The limestone of Plymouth Dr. Berger describes of a blueish colour frequently intersected by veins of calc spar, of a crystalline grain and destitute of organic remains; though, according to a notice in the Annals of Philosophy for February, it contains shells in quarries at Stonehouse-hill and dock-yard. Dr. Berger speaks in the most decided manner of its being transition; and Dr. Thomson, in his Account of the Geology of Cornwall, says that it is associated with clay-slate, and "that there can be no hesitation in considering it transition" Mr. Horner, in his Account of the Geology of the Malvern Hills, has described a formation of limestone resting on the west part of the range. This I have seen; and it appears to me very different in its general structure from the mountain limestone; which, as far as I have seen, generally occurs in strata of considerable magnitude: the strata however of the Malvern limestone seemed to me never to exceed the thickness of a foot or a foot and a half. Mr. Horner considers them as decidedly transition; and as it appears that they rest in the sienite of the Malvern Hills, which is indisputably transition; and since, as Dr. Prichard informs us, they dip under the old red sandstone of Herefordshire,—there can, I think, be no doubt of their being entitled to this denomination. In a paper of Mr. Horner's lately read before the Geological Society, and of which Dr. Thomson has given an extract in his Annals of Philosophy, the Quantock Hills are described as being composed of a gray wacke formation alternating with strata of clay-slate. Within the latter strata beds of limestone are included, which are therefore manifestly transition. Mr. Allan, in his Remarks on the Transition Rocks of Werner, (Thomson's Annals, p. 416, vol. i.) says that the limestone at the lakes of Windermere and Coniston, which contains organic remains, is transition.

In Thomson's Annals of Philosophy, vol. v. p. 456, there is a notice of a paper of Mr. Buckland's, read before the Geological Society, On the Rocks situated to the east of Appleby, between Malmerly and Morton in Cumberland; in which it appears that these rocks are chiefly composed of greenstone and slate,

slate, and that a few thin beds of *transition* limestone occur in the slate. These, as far as I have been able to collect, are the only instances of the occurrence of transition limestone in England: it is yet to be ascertained, whether the primitive or foliated limestone anywhere exists in this country. The only primitive districts in England are Cornwall, the Isle of Man, and North Wales. The two former have now been minutely examined, without any locality having been given of this rock:—the probability of its occurrence is therefore confined to North Wales.

Having made these remarks upon the classification of the limestones in England, I proceed to offer some observations on the red ground, in order to show that much confusion prevails in regard to the beds that properly constitute this formation. If we attend to the general succession of rocks in England, we observe a striking conformity to the truly admirable system of Werner. In Cornwall, and in the Isle of Man, we have granite and clay-slate; and in North Wales, according to Mr. Aikin (see his Tour through North Wales), we have granite, mica-slate, hornblende-slate, serpentine, and other primitive rocks. In the above-mentioned districts, upon the primitive rocks rest those of the transition formation, consisting of gray wacke, limestone, and gray wacke-slate, which, however, sometimes, as at Malvern, in Cumberland, &c. are found unassociated by the older rocks.—Upon the transition series, as has been well remarked by Dr. Prichard in the paper above referred to, repose in almost every district the members of the first flœtz formation, the old red sandstone and mountain limestone, upon one of which all the coal formations in England, with the exception of that in Derbyshire, seem to recline.—Independent of these formations, an extensive series of horizontal strata is observed in many parts of England to cover the coal-fields, and the mountain limestone and red sandstone by which they are inclosed. Of this class of strata, the lowest, or those immediately covering the coal-fields, and their inclosing strata, are the beds of what is called the Red Ground. Above these occurs the lyas limestone, which is followed by several varieties of oolite, and then again by the chalk. From this statement it is obvious, that the red ground or rock marl must be of later origin than the coal-measures or the first flœtz formation. But such have been the inaccuracy and confusion of those who have had the best opportunities of ascertaining the true nature of the red ground, and its relative position with other formations, that not only members of the flœtz but even of the transition series have been referred to this formation. Thus, Mr. Farey in his account of Derbyshire, when enumerating the beds, or, as he calls them, the products of the red ground, mentions after gypsum and sandstone strata, sienite and

roof

roof or clay-slate, and probably, says he, the sienite of Malvern belongs to this formation. (Report, p. 152.) It is quite unnecessary for me to make any comments upon so strange an error as that of placing the sienite and slate of Derbyshire and Malvern hills (which certainly belong to the transition series, but which some have even called primitive,) in the same formation with the gypsum of the red ground, between which and the sienite, in point of regular succession, there intervene the red sandstone, mountain limestone, and coal deposit with all its accompanying measures.—As well, I apprehend, might Mr. Farey, upon seeing a mountain of granite rising 3000 feet above the surface of sand or gravel which surrounds its base, call the granite a bed in such matters, as the sienite a bed or product of the red ground. But Mr. Farey, in fact, seems to despise every thing like a systematic arrangement, and calls, forsooth, the confusion which he wishes to introduce, English Geology.

Mr. Townsend in his work upon Moses vindicated for veracity, &c. has committed a mistake not less important. At p. 156, he describes the quartz sandstone of Brandon hill near Bristol, which, as I have stated in my former paper, always lies in a conformable position on the mountain limestone, as a bed in the red ground; whereas the least observation might have convinced him that the true beds of the red ground have a position quite unconformable to this quartzy sandstone, and that between it and the former there intervenes, as to the æra of their formations, the whole of the coal deposit. Not less inconsistent is he when he asserts the micaceous sandstone to be a part of the red ground. Upon this belief, it seems strange that he did not follow up his mistake, by saying that every stratum of coal in the Gloucestershire and Somersetshire basis belonged to the red ground; for the micaceous sandstone, or Pennant stone, is universally a coal measure; and in whatever light we consider one, under the same point of view must we regard the other.

Dr. Kidd in his late publication has likewise, I conceive, fallen into a mistake respecting this formation. He there confounds the red ground with the old red sandstone, and gives it as his opinion, that they both belong to the same formation. Nothing, however, as will plainly appear from what I have above stated, can be more different than the situation of the beds of the red ground and the red sandstone. No spot is more favourable for observing this difference than the neighbourhood of Ness, which the Doctor says he has examined. He would there most distinctly see the old red sandstone dipping under the mountain limestone,—a situation, as every one will acknowledge, perfectly inconsistent with the red ground formation. A similar error, I conceive, has been committed by Mr. Horner in his Account

Account of the Salt-works near Droitwich, published in the second volume of the Geological Transactions. The prevailing rock around Droitwich, says Mr. Horner, is a fine-grained calcareo-argillaceous sandstone of a brownish red colour, which contains beds of a greenish-gray colour, in which are slender veins of crystallized gypsum. This sandstone, continues he, is the same as that which Mr. Aikin has described as occurring to so great an extent in Shropshire and Staffordshire, and which he considers to be the old red sandstone of Werner. The sandstone which Mr. Horner here describes, answers exactly to the description of that which I have mentioned in my former paper, as forming one of the beds of the red ground near Bristol, and is characterized by its strongly effervescing with acids, and by its being sometimes associated with gypsum: whereas the old red sandstone, independent of its different geological situation, as Mr. Aikin very justly remarks, does not effervesce with acids, contains spangles of mica, and, as far as I have seen or read, is never accompanied by gypsum. The occurrence of gypsum and rock-salt in England is, I am convinced, in every instance to be referred to the red ground; even in Scotland, where the old red sandstone is so widely distributed, its occurrence with gypsum is so rare, that Professor Jameson, in his Lectures, if I remember right, only gives two instances of it. Having made these remarks upon Mr. Horner's paper, I quote with great pleasure some passages from a notice of a paper of his lately read before the Geological Society, On the Structure of the Quantock Hills*, where the red ground is described to cover the old red sandstone (in the same way, I apprehend, as I have described in my former paper), so that the distinction between these two formations is clearly made manifest. "Where the hills of gray wacke (says he) sink down into the lower country, their sides are covered with beds of conglomerate passing into red sandstone. These beds appear to consist of the same materials as the gray wacke formation, but decomposed to a considerable degree." This conglomerate is merely a common variety of the old red sandstone, and is covered by a red agillaceous sandstone containing a variable proportion of calcareous matter. It is of a fine texture, never contains fragments, is in some places traversed by veins of gypsum, and appears to be the same rock as that in which the salt-beds of Cheshire and Droitwich are situated. To this succeed the strata of lyas limestone, which are sometimes seen distinctly resting on the red gypsum rock.

Having, as I trust, now shown that many strata have been referred to the red ground, which have not the smallest relation

* Vide Thomson's Annals for May 1815.

to

to that formation, it may be asked, What are the true beds of the red ground? by which term I mean that deposit which, as to the æra of its formation, is later than all the coal strata in England, and of course than their inclosing rocks, and which lies immediately under the lyas. As far as I have seen, the lowest stratum of the red ground is a very coarse breccia, or rather conglomerate, which becomes finer as we ascend. The contained masses are rounded and angular, and consist of limestone, hornstone, quartzy sandstone, &c. To this succeeds a fine-grained calcareous sandstone, which is sometimes of a deep red and sometimes of a white colour. Above these, I believe, lies the great deposit of red clay, abounding in many districts with gypsum both foliated and fibrous, and sometimes containing sulphate of strontites. I do not pretend to say that no other beds can with propriety be referred to this formation; but, as far as I may be allowed to speak from my own experience, and the descriptions of others, these only appear to me to belong to this formation. It may be worth while mentioning, that the conglomerate which I have formerly described as one of the red ground beds near Bristol, has also been noticed by writers as occurring in other districts. In a short account of the stratification of Glamorganshire in the xlivth vol. of the Philosophical Magazine there is the following passage:—"About a mile west of Dunraven, a calcareous breccia similar to that described by Mr. Gilby underlies the lyas." The following is a note which I have extracted from some author, but unfortunately have neglected to take down the name, but I think it is from Dr. Kidd.—"A circumstance in the history of rock marl is its alternating with and transition into beds which would usually be called conglomerate. Thus the red cliffs at Budleigh, Saltstoun near Teignmouth, and those also of Teignmouth itself, consist of argillaceous beds with beds of sandstone and breccia."

The calcareous sandstone has also been noticed by other authors. I have mentioned above Mr. Horner's description of its occurrence in his account of the Quantock Hills.

Mr. Buckland, in the paper above referred to, on the rocks situated to the east of Appleby, mentions that on the west part of the range there occur strata of limestone, either by themselves, or with thin seams of coal interposed nearly vertical. Then comes in a *sandstone* in nearly horizontal beds, forming, in his opinion, part of the great deposit which overspreads the vale of Carlisle, a large part of Cheshire, and vale of York, in which are contained the great quarries of *gypsum* and beds of *rock salt*.

Before closing this paper, it may be interesting to state the occurrence of a trap rock in the neighbourhood of Bristol. I had frequently remarked, in travelling from Bristol to Gloucester, that

that the road is repaired about Newport with an amygdaloid, but I had never an opportunity of visiting the spot from whence it was brought. My friend Dr. Prichard, however, informs me, by letter, that it is quarried from a hill at Micklewood to the east of Newport, a few miles beyond the northern boundary of the limestone ellipsis. The hill itself is of very inconsiderable extent, and perfectly unconnected with the surrounding high country, which to the north of it is of oolite; and in walking south of it we first come on the red sandstone, then on the sandstone conglomerate, both of which we find further south at Cromhall, dipping under the limestone.—From this account, there can be no doubt, I think, that the amygdaloid rests upon the red sandstone. The specimens that I have seen of the amygdaloid consist of a basis of a sort of iron-clay containing almond-shaped portions of calc spar and green earth.

With regard to the point at issue between Mr. Farey and me, it would be a most unprofitable waste of time to dissect his sesquipedalian sentences, and to answer each part separately: I shall content myself with stating, that after reading Mr. Farey's letter, I referred to the original account of Mr. Strachey in the Philosophical Transactions, on the coal-pits near the Mendip Hills, and find the unconformable position of the red ground is most distinctly mentioned both in his section and description; so that the merit of having first observed this appearance belongs neither to Mr. Farey nor to myself, but exclusively to Mr. Strachey. My ignorance of this circumstance arose from not having had an opportunity, while writing my paper, of consulting the Philosophical Transactions themselves, but was obliged to content myself with referring to Dr. Thomson's Abridgement, where the particulars relative to the occurrence of the coal are accurately given, but this point of the unconformable position of the red ground is altogether omitted.

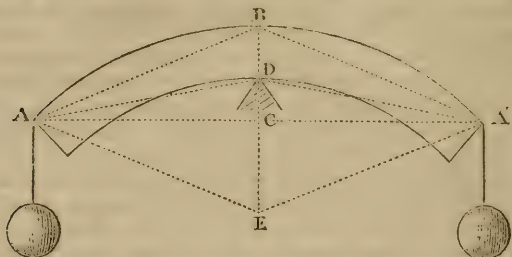
XXXIV. *On the Strength of Beams.* *By* A Correspondent.

To Mr. Tilloch.

Sir,—Buffon found by his experiments, that the strength of beams decreased in a much quicker proportion than the inverse ratio of their lengths. The cause has not been satisfactorily explained; but may it not be accounted for by the tendency of the weight to compress the beam, in the direction of its depth, at the place of fracture? To ascertain the effect of this kind of pressure, I fixed one end of a bar of deal $\frac{3}{4}$ of an inch square, and suspended a weight of 17 lbs. at 12 inches distance from the point

point of support. After it had remained in this state about half an hour, without being any more depressed than it was five minutes after the weight was laid on, I subjected the fixed end to a considerable pressure, and the depression increased in proportion to the pressure, and ultimately the bar was broken without any additional weight at the other end. The experiment was repeated with the same results.

When a beam is supported in the middle, and a weight suspended at each end; the weights and the extending forces are to each other as BD : AB; the compressing forces will be nearly,



if not exactly, equal to the extending forces, because there is a considerable degree of pressure transmitted to the lower part of the beam D, by the exertion of the parts of the upper side to retain their rectilineal position. Now the effect of the extending forces to compress the beam is measured by BE, and this force is resisted by the compressing forces AD, and A′D, and by the support at D: therefore, BD : BE : : W (= the weight at A or A′) : $\frac{BE.W}{BD}$, equal the force compressing the beam at B; and the strength of the beam is inversely as this pressure.

Put L = the length, D = the depth, B = the breadth, and 2E = BE = twice the deflexion; then the weight the beam will sustain, is as $\frac{BD^2}{L}$, and as $\frac{D}{2EW}$, that is as $\frac{BD^3}{2EWL}$, or W is as $\sqrt{\frac{BD^3}{LE}}$.

If the deflexion is equal to the depth, the proportion becomes W : $\sqrt{\frac{BD^2}{L}}$; and when it is less than the depth, we may safely neglect it, and make the proportion W : $\frac{BD^2}{L}$.

In order to compare the rule with Buffon's experiments, I have taken the first experiment on the beams five inches square as a standard, and the following Table shows how far it agrees with the longer lengths.

The

The column No. 1 contains the length of the beams in feet (French).

The column No. 2 contains the deflexion of each beam in inches and lines (French).

The column No. 3 contains half the weight of each beam added to the weight that broke it, in lbs. (French.)

The column No. 4 contains the weight that ought to have broken each beam, if the strength is as $\sqrt{\frac{1}{LE}}$.

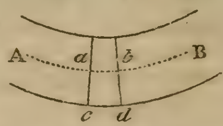
The column No. 5 contains the weight that ought to have broken each beam, if the strength had varied in the inverse ratio of the length.

1	2		3	4	5
	in.	line.			
7	2	6	11802		
20	8 10	10 0	3406·5 3304·5	3709 3489	4130
22	11	3	3115·5	3138	3755
24	11 13	0 6	2355 2278·5	3035 2740	3442
28	18 22	0 0	1982 1930	2198 1989	2950

Your correspondent objects to the conclusion that the deflexion of beams is as the square of their length, (No. 208, page 144,) because the deflexion is as the square of the length, and as the strain which is as the length; but he does not say why the deflexion is as the square of the length. In beams of the same length, the deflexion is as $\frac{W}{BD^3}$; for when beams are of the same length, the weights that will break them are directly as BD^2, and the deflexion at the time of fracture is as the curvature, which is inversely as the depth: therefore, if W be any other weight, then,

$BD^2 : \frac{1}{D} :: W : \frac{W}{BD^3} =$ the deflexion corresponding to the weight W.

Now let us conceive the depth and breadth constant, and the length variable, and let AB represent the neutral line, or that part of the beam which is neither extended nor compressed: then the curvature at any point, is as the extension of the indefinitely small part cd, is to its original dimension ab, which varies as the strain, that is as the length and weight; but the deflexion is as the curvature and as the sum of the extensions: therefore it is as L^2W.



Suppose a beam, of a given length L, to be supported in the middle, and loaded at each end, and let the deflexion be E; then conceive a part of each end of the beam to be taken away, and at the same time let the ends of the remaining beam be loaded so that the strain in every part may be exactly the same it was before the ends were taken away. Let l be the length of the remaining beam; then the deflexion will be as $L^2 : l^2 :: E : \frac{l^2E}{L^2}$. It is obvious the curvature is exactly the same in both cases. If I do not mistake, this imaginary experiment is the same with the uniform curvature of your correspondent. But it is evident, that when we say the deflexion is as the square of the length, we do not suppose the weight to have been varied, but it was varied as the length, that the strain might remain the same: therefore, the deflexion is as the length and weight. Hence the deflexion is as the length and weight when the curvature is the same; and the curvature will vary as the strain: therefore, $\frac{L^2W}{BD^3}$ is as the deflexion.

The deflexion of beams at the time of fracture, when the depth is very small in proportion to the length, is subject to several irregularities: there is one which I do no recollect having seen noticed, that is, the sliding of the ends of the beams on the supports: this motion sometimes gives such a momentum to the weight, that the beam breaks with a much less weight than it would have done had the deflexion been gradually increased.

In practice, where we wish the deviation from the natural position to be as small as possible, the rule given for the stiffness of beams will be found as correct as the nature of the subject will admit of.

I am, sir,

Your obedient servant,

Sept. 5, 1815. T. T.

XXXV. *Ob-*

XXXV. *Observations upon the different Hypotheses that have been proposed respecting the Nature of Light.* *By* William Crane, *Jun. M.D. Boston, Lincolnshire**.

By light, we are enabled to distinguish the objects that surround us; to view the beauties of creation, and to determine the pleasing vicissitude of day and night. When mankind first turned their attention to philosophical pursuits; to account for, and understand the various changes that are continually taking place on the globe which they inhabit, and the heavens surrounding it; one of their earliest inquiries was to attempt the discovery of that agent which enabled them to perceive these phænomena; an agent which, after the lapse of centuries, and the exertions of the greatest philosophers that have appeared, still remains to be discovered. Empedocles and Plato rank amongst the earliest philosophers that treat upon this subject. They supposed vision to be occasioned by particles flying off from the object seen, and meeting with others that proceeded from the eye. This crude conjecture, unsupported by argument or experiment, was soon rejected, and another advanced by Pythagoras, who maintained that particles were emitted from the body that became luminous, and entered the pupil of the eye; a doctrine which still holds a place in the writings of many philosophers of the present day. Aristotle appears to be one of the first that considered light and heat not to be the same. Light he imagined to be the act of a transparent substance considered as such, and not arising from any matter proceeding from the luminous body:—this idea of Aristotle's gave rise to the Cartesian doctrine. In this hypothesis it was considered that light was only a property of the luminous body, exciting a clear and vivid sensation in the observer; or that an invisible fluid was everywhere present, and set in motion by an ignited body, or other means, to make objects visible. Nearly similar to this is the doctrine of Huygens, who conceived that light was a subtile elastic fluid filling all space, and rendering bodies visible by the undulations into which it is thrown. According to him, when the sun rises it agitates this fluid; the undulations gradually extend themselves, and at last striking against the eye cause us to perceive the sun. As this hypothesis has occupied the attention of philosophers more than any other, except Newton's, we shall state the objections to it, after having noticed what Dr. Young has said on the same subject. Euler was a strenuous advocate in favour of this opinion of Huygens; but appears to have been unable to support it against the theory of Newton.

* Communicated by the Author.

 This

This great philosopher, no less celebrated and revered for the superior powers of his mind than for the modesty with which he gave his opinion on any subject, supposed light not to be a fluid *per se*, but that it was projected from the luminous body with an immense velocity in all directions, consisting of a great number of very small particles, and always proceeding in right lines, until they were turned from that path by some particular cause. Several objections have been brought against this opinion of Newton's, as will be presently noticed. But in the first place let us turn our attention to the Huygenian theory, or more properly its modification by Dr. Young in his Bakerian Lecture for 1802. The Doctor observes, 1st, That a luminiferous ether pervades the universe, rare and elastic in a high degree. 2dly, Undulations are excited in this ether whenever a body becomes luminous. 3dly, The sensation of different colours depends on the frequency of vibrations excited by light in the retina. 4thly, All material bodies have an attraction for the ethereal medium, by which it is accumulated within their substance, and for a small distance around them, in a state of greater density, and not of greater elasticity. Dr. Young first quotes several passages from Newton's works, to prove that Newton himself had recourse to an extremely elastic and subtile medium, which reflects and refracts light by fits of easy transmission; that this medium is far more elastic, rare, and active than air. He then proceeds to prove that the undulations of this elastic medium are light; and in his 9th prop. he states, that radiant light consists in undulations of the luminous ether.

To this hypothesis of Dr. Young's there appear to be many objections, and some of them, I am inclined to think, almost insurmountable. Newton has stated very clearly, "that a pressure on a fluid medium, *i. e.* a motion propagated by such a medium beyond any obstacle which impedes any part of its motion, cannot be propagated in right lines, but will be always inflecting itself every way to the quiescent medium beyond that obstacle." He further adds, "The waves, pulses or vibrations of the air, wherein sound consists, are manifestly inflected, though not so considerably as the waves of water; and sounds are propagated with equal ease through crooked tubes as through straight lines; but light was never known to move in any curve, nor to inflect itself *ad umbram*." In reply to this objection Dr. Young observes, "Sounds are propagated through crooked passages, because their sides are capable of reflecting sound, just as light would be propagated through a bent tube if perfectly polished." The statement in the latter part of this passage can carry but little weight with it, as it is merely an assumption. Sound, it is well known, passes with ease through a tube consisting of many

many spiral turns, but experiments prove directly the contrary with respect to light. It may be said that it is for want of the tubes having a due polish: but we know that straight metallic tubes which are highly polished reflect light in great quantity, and with great ease, but cannot reflect it so as to cause it to pass through them when they are curved. If light consists of a series of undulations, it is difficult to conceive how we could perceive objects clearly and distinctly: from what we observe with respect to bodies thrown into a state of vibration, as when a musical chord is struck, it almost appears impossible that the edges of bodies should be distinctly seen. Again, if light depended on the undulations of this ethereal medium, caused by the vibrations of the luminous body, as stated by Dr. Young when he accounts for the phænomena of solar phosphori—why does not light always accompany sound? For the sounding body not merely imparts its vibrations to the air, but also to fluids and the most solid bodies.

It is therefore difficult to conceive that an elastic subtile fluid should not also partake of this vibratory motion; particularly as it is a fluid capable of being affected by a very slight cause so as to produce light, as when light is perceived by means of many pyrophori; yet the loudest tones are not accompanied with light. And on the contrary, the most intense light has not the least effect on the ear, although this beautiful organ is truly formed to convey the slightest vibrations to the sensorium. When we contemplate the mechanism of the ear, we find an organ formed so as to convey any vibratory motion; but the eye is a piece of mechanism entirely different, and appears but badly calculated to propagate tremors. These observations appear to be in direct opposition to what might be expected if light consisted in the undulation or vibration of a highly elastic medium. In short, the very existence of such a medium is entirely gratuitous. The same objections also hold good against the opinion advanced by Malbranche; who supposed that there was an analogy between sound and light, and that bodies appeared more or less luminous according to the vibrations of a subtile matter between the eye and the object, which matter was put into motion by the rapidity with which the particles of the body moved.

In the latter part of the lecture Dr. Young accuses Sir Isaac Newton of want of candour towards Huygens, which we may here notice, in endeavouring to account for the singular refraction of the Iceland crystal. There are several passages in the writings of this great man, which will show that the Doctor has fallen into an error on this subject. Newton expressly states in his 28th query, at the end of his Treatise on Optics, "That to

 explain

explain the unusual refraction of the Iceland crystal by pression or motion propagated, has not been hitherto attempted, to my knowledge, except by Huygens, who for that end supposed two several vibrating mediums within that crystal. But when he tried the refraction in two successive pieces of that crystal, and found them such as is above mentioned, he confessed himself at a loss for explaining them." In his 25th query, he also states, "Are there not other original properties of the rays of light, besides those already described? An instance of another original property is the refraction of Iceland crystal, described first by Erasmus Barteline, and afterwards more exactly by Huygens." Sir Isaac then gives his own opinion, which, being well known, need not be repeated. Indeed, with respect to the Iceland crystal, Huygens was himself by no means satisfied with his own views upon this subject: he observes in his *Tractus de Lumine*, page 67, when treating upon its double refraction, "Quo artem pacto id fiat, nihil reperire potui, quod mihi satisfaceret." Also Haüy in his Natural Philosophy gives the preference to Newton's explanation. He states*: "The theory of Huygens," alluding to his theory of double refraction, "which in general agrees with this mode of viewing the matter, is connected in another way with an hypothesis not at all natural, respecting the elliptical figure of the waves of light, to which this great man attributes the refraction of aberration; and he himself acknowledges his difficulty to reconcile with this hypothesis the result of observations made with two rhomboids, in which the rays sometimes merely change their functions, and sometimes are subdivided anew, in passing from one rhomboid into another."—"We are much more disposed to range ourselves here again on the side of Newton, when we consider the extreme simplicity of the law admitted by this celebrated geometrician for the refraction of the ray aberration." In short, so many difficulties present themselves with respect to the undulating doctrine, that I am inclined to think it ought to be entirely abandoned.

Having thus given a rapid view of the objections to the Cartesian doctrine and its modifications, let us turn our attention to that proposed by Sir Isaac Newton, which is, in fact, a modification of the Pythagorean. By this philosopher light is supposed to consist of particles emitted from the luminous body in every direction. The first objection is, that as the rays of light pass from every part of a luminous body, they must interfere and destroy each other; or that perception must become confused, and objects be represented indistinctly. Canton has removed

* Dr. Gregory's Trans. of Haüy's Nat. Phil.

this,

this, by supposing a small instant of time to take place between the emission of each particle of light that passes off in the same direction; which is by no means a crude supposition; for, as the impression of light does certainly remain for a short space of time on the retina, it is easy to conceive that there must be an interval between two particles of light entering the eye; for, if the impression of the second particle of light were to affect the eye before the first had ceased, a confused vision must ensue, from that organ being affected with several impressions at the same time. Chevalier D'Arcy has attempted to prove that the sensation of a particle of light will remain for the seventh part of a second; and this, by computing the velocity with which light moves, will give an interval of 20,000 miles between two particles of light. But as I have not seen the original paper written by D'Arcy, which is to be found in the Academy of Sciences for 1765, I do not know upon what experiments he makes the above estimation. That the impression lasts the interval he mentions is a correct estimate, I am inclined to think, from an experiment I made with a stick having its point made red hot. When the stick made less than eight revolutions in one second, the circle of light was not perfect, but appeared to consist of a series of luminous points extremely near to each other.

I have stated above, that the impression of light according to D'Arcy continues for the seventh part of a second, which is taken from Dr. Hutton's Mathematical Dictionary. But Dr. Brewster in his edition of Ferguson's Astronomy, vol. ii., says, "It appears from the accurate experiments of D'Arcy, that the impression of light upon the retina continues *two minutes and forty seconds.*" Which of these two respectable writers is right, respecting the estimation made by D'Arcy, I am unable to state; but it appears to me, from the experiment just mentioned, that Dr. Hutton's is the most correct statement. We may also observe, if the impression remained for so long a time as two minutes and forty seconds, that upon shutting out the light in the day-time, or extinguishing a lighted body, a room would not appear so instantaneously dark, but objects would remain for a short time visible.

The second objection is, that if there be such a constant emission of particles from luminous bodies, they would in a short time be much wasted, and therefore the stars ought to decrease and the bulk of the planets increase. This objection Dr. Horsley has endeavoured to remove; by first proving that the sun and stars receive mutually a great quantity of light from each other directly, and from the planets by reflection. He by mathematical calculation shows that in the space of 385,130,000 years the sun would lose only the $\frac{1}{13232}$ part of the matter it now contains. From hence it is evident, that no diminution in the one or increase in the size of the other can as yet, or ever will be, perceived. Dr. Priestley also, when reasoning upon an experiment made by Mitchell, respecting the force of light, has calculated "that in the space of 6,000 years there would be only 670 lbs. of the matter of light issue from the sun. But little reliance however can be placed upon a calculation founded upon such an experiment; as it is very likely the motion obtained by Mitchell in his machine might be owing to the rarefaction of the air at that point where the rays of light were collected into a focus producing a stream sufficiently strong to put in motion so delicate a piece of machinery. The quantity of heat too, he observes, was so great as to warp the piece of copper considerably, and almost to melt it; and this would also tend to produce motion, by altering in some measure the centre of gravity of the instrument. In the third place, it has been supposed that the amazing velocity with which the particles of light must move, would, although almost inconceivably minute, have a force exceeding that of a cannon ball. This also Dr. Horsley has ingeniously answered: and he not only proves that the force of a particle of light would not be equal to a cannon shot, but that it will when emitted from the sun, and received directly on the eye, on a bright day, not exceed the force of a shot a quarter of an inch in diameter, moving at the rate of sixteen inches in a year; or in short, the force is less than can be imagined. The celebrated Euler also maintained, that if light were material, its velocity would disturb the motion of the planets. But this is an objection which may also be urged against the doctrine of vibration: for how could the planets move steadily in the midst of a medium that is perpetually in a state of vibration? This philosopher further urged, that if light consisted of particles passing in straight lines; then those bodies through which it passes must consist of right-lined canals in all directions, and that such a structure would deprive transparent bodies of all solidity and coherence amongst their parts. This upon investigation does not appear so strong an objection as at first sight. Indeed it is by no means improbable, though undoubtedly hypothetical, that transparent substances have their moleculæ or atoms so arranged as to form right-lined canals.

The discoveries made in chemistry since the time of Euler, clearly prove that the ultimate particles of matter are not in absolute contact. The expansion and contraction of bodies by the aid of heat sufficiently show this.

Lavoisier, in the first chapter of his Elements of Chemistry, observes, when treating upon heat, "that the particles of bodies

bodies do not touch each other in any state hitherto known; which, though a very singular conclusion, is yet impossible to be denied." And Newton in his Optics, part iii. book ii. prop. 8, appears to have entertained a similar idea; for he says, "A concave sphere of gold filled with water, and soldered up, has, upon pressing the sphere with great force, let water squeeze through it, and stand on all its outside in multitudes of small drops, like dew, without bursting or cracking the gold, as I have been informed from an eye witness. From all which we may conclude, that gold has more pores than solid parts, and by consequence that water has above forty times more pores than parts." And he further adds, "that he who shall find out an hypothesis by which water may be so rare, and yet not capable of compression by force, may doubtless by the same hypothesis make gold and water, and all other bodies, as much rarer as he pleases, so that light may find a ready passage through transparent substances." Hence, as it appears that even the most solid body is porous, it is not, I think, too hypothetical to suppose that the particles of matter may be differently arranged in different bodies; and that those bodies which are transparent, or transmit light freely, may have their atoms so arranged as to form an indefinite series of right-lined canals; which is effected by these atoms having for each other a certain degree of polarity.

Crystallization, which affords the most transparent bodies, is a strong argument in favour of this supposition. If it were not for some peculiar arrangement of the particles of a crystal, how can we account for the same substance always assuming a determinate form upon going from a fluid to a solid state? So correctly does this take place, that we are able in many instances to determine the composition, by knowing the form of the crystal of the subject under examination. The only means of accounting for this regularity is by supposing that the particles of matter composing the crystal, have with respect to each other a certain polarity. It is only upon this principle that many of the phænomena of magnetism can be at all explained. For how otherwise can the fragments of a magnet instantly observe a polarity; and magnets, which have been struck by lightning, or by having the electric shock passed through them, have their poles changed? An instance is recorded of a ship's compass being struck by lightning, ever afterwards turning east and west. Perhaps in this case the poles of the atoms forming the needle were turned so as to lie transversely instead of longitudinally.

To account for bodies being opaque, according to this hypothesis, we have only to observe, that in such bodies the particles are

are promiscuously arranged, if such an expression may be allowed; or that they attract the light they receive with a degree of force so as not to allow it to pass through. In the first case it would be reflected, in the latter absorbed; and lastly, it is both transmitted and reflected, by those particles of light being reflected which fall upon the moleculæ themselves, and by those passing through which fall in the direction of the canals through which it is transmitted.

By the term absorption, used as above, will be understood light combining chemically with the substance which is exposed to it; and hence, when a body gives out light, it is acted upon by some chemical agent, such as is observed in combustion, &c. An exception to this observation, at first, appears in those animals which are phosphorescent, and have the property of emitting a great quantity of light; but this is probably owing to some chemical change taking place in the animal, in a similar manner as we observe carbon to be emitted from the lungs by means of the organs of respiration. From what has been advanced it therefore appears, that, of the two theories, the one adopted by Newton is the more applicable to the phænomena of light.

By many philosophers heat and light have been considered only as modifications of each other. A concise history of the opinions upon this subject may be seen in Dr. Frank's Treatise on the Theories of Chemistry*. It has also been advanced, that heat is only the effect of a vibratory motion amongst the particles of the body that is heated, and that light is only heat in a state of radiation. If this were the case, we should expect to find that the point of greatest illumination in the prismatic spectrum should also be the hottest: the contrary of which is shown by Herschel in his experiments on the prism. Also the focus of light and focus of heat ought to be the same; but Dr. Herschel has likewise shown that these differ, and his experiments are further confirmed by those made by Sir Henry Englefield. Sir Henry observes, in the account he gives of his experiments†, that "Mr. Cary, optician in the Strand, and Dr. Hunter, were present at these experiments, and repeatedly saw the thermometer sink when carried into the light, and rise again when carried into the dark. Dr. Hunter also received the focus on the palm of his hand, where the heat was sensibly felt; and shutting his eyes, and pointing with a long pen to where the heat was greatest, he always touched his hand beyond the visible light." As another objection, it may be observed, that phosphorescent bodies that emit light very freely have a temperature

* Medical and Physical Journal, vol. i.

† Journal of the Royal Institution.

very little, if at all, elevated above that of the surrounding atmosphere in which they are placed. And further, many substances are capable of attaining a very high degree of temperature, and becoming highly attenuated, (as for example, when water is converted into steam,) without emitting the least degree of light. Heat passes through all bodies, with greater or less facility; whereas light only passes through those bodies which are termed transparent:—and many substances that transmit light very freely, are at the same time very bad conductors of heat. Hence it would appear, that heat and light are two distinct agents. Whether light be a fluid, or only a property of matter, or consists of an innumerable quantity of small particles emitted from a luminous body, remains yet to be determined. The phænomena of light appear to admit of the best explanation, according to the latter supposition; and the late discovery of Malus, respecting light having a polarity, may be adduced as another argument in its favour.

XXXVI. *On Correcting the Rate of an Electric Clock by a Compensation for Changes of Temperature.* *By* F. Ronalds, *Esq.*

To Mr. Tilloch.

Sir,—Having at different intervals, since my communication to you in April last, made fresh attempts to render Mr. De Luc's column applicable to the measure of time, and being now obliged to discontinue them by a long absence from London, permit me to describe that which has been most successful, in the hope that some person better qualified for the subject may deem it worthy of attention.

I have, by the following method, procured a better compensation for the effect of an increase of temperature, which, by increasing the power of the column, accelerates the velocity of the vibrations.

A beam is suspended like a dipping needle, as nearly as possible in its centre of gravity, carrying at one end the clock, and at the other a weight almost counterpoizing it. A spirit thermometer of nearly the same kind as those used in France (to mark the degree of heat by an index) is placed under the beam; the part containing the spirit is 35 inches long, and one inch bore, and the part which contains in its lower end the mercury is half an inch bore. A small ivory rod and piston sliding very freely in the latter tube rests upon the surface of the mercury, and sustains at its upper end the preponderating weight of that

arm

arm of the balance which carries the clock. Two flat plates of brass (four inches long and one inch in breadth) insulated and connected with the opposite extremities of the columns are placed at such distances, viz. about one inch from the ball of the pendulum, which is also insulated, that it vibrates between them by the joint forces of gravitation and electric attraction in half seconds; and they are inclined to such angles with the pendulum, that the degree of heat, which causes the columns to act more powerfully, at the same time, by expanding the spirit, raises the piston, the beam, and the pendulum into a position where the vibrations of the latter are proportionally longer; and consequently, these two different effects of heat are thus made to compensate each other; and the instrument actually keeps tolerably good time:—how long it will continue to do so, must depend upon the constancy of the columns. But the regularity and steadiness of the vibrations are also much improved by enveloping the columns in cement, which was laid on in a liquid state, so that no moisture can be driven out of the squares of paper, and by the ball or bob (which is of brass, with a fine gold girth,) being very considerably heavier than those of cork or thin silver which I had hitherto employed.

I believe the principal defects are now those arising from a difference between the rates of heating and cooling of the spirit thermometer and the columns, and from some unknown circumstance which has been attributed to the electric state of the ambient air. The larger the series employed, and the heavier the pendulum, the more correctly it seems to vibrate*.

Amongst other interesting intelligence from your learned correspondent M. Van Mons, it appears that Sign. Zamboni has been enabled to produce strong sparks and shocks by the column, the construction of which he seems to have thus very greatly improved. With the help of a condenser I have advantageously substituted it for the electrophorus in Volta's inflammable air-lamp, which is apt to get out of order; but Sign. Zamboni's columns could be used without this assistance.

I remain, sir,

Your obliged and humble servant,

Hammersmith, Sept. 15, 1815. FRANCIS RONALDS.

* As Mr. Singer's account of the invention of this instrument seemed very inconsistent with mine, I wished it to be clearly understood, for the information of his friend in the note to which he has referred, that I derived no kind of suggestion from any person whatever respecting the employment of an inflexible pendulum. That Mr. Singer should honour me with the title of his pupil, I certainly did not at all expect. The other facts stated in that note can be verified without difficulty, by any who may think them of sufficient consequence to repeat the experiments upon which they are founded.

XXXVII. *On*

XXXVII. *On the Fracture of Electrical Jars by spontaneous Discharges.* By Mr. THOMAS HOWLDY.

To Mr. Tilloch.

SIR,—THE perforation or fracture of Leyden jars, which sometimes takes place when they explode spontaneously, and at other times when they are intentionally discharged, is a phænomenon which has often obtruded itself upon the notice of electricians; and yet, so far as I can learn, its cause has not been investigated with much care. My attention was forcibly attracted to this subject a few years since, by the following circumstance:—A small electrical battery of nine jars, that had been charged pretty high, exploded spontaneously at the moment when I was going to discharge it. On examining the jars after the explosion, it was found that three of them had been perforated nearly in the middle between the top and bottom of the coating. The jars of this battery were four inches in diameter and twelve high, having uncoated margins two inches broad. As I was not then aware that similar accidents had occurred to other experimenters, and as I had never before had more than one jar broken by any single explosion of the battery, I was greatly surprised at the fracture of three on this occasion, and could not account for it in any manner satisfactory to myself. The most plausible conjecture that I was able to form concerning its cause was, that the communication between the charging wires and the inside coatings of the jars was not sufficiently extensive; so that, when the discharge of the battery was made, the whole charge of each jar, rapidly converging to the single and very limited point of contact between its coating and the charging wire, experienced a proportionate condensation and resistance on entering it, which caused a momentary retardation of its velocity, and consequently a momentary reaction of the charge from that point against the sides of the jars; which reaction, when the charge had considerable intensity, might be sufficient to burst one or more of them.

I determined, therefore, to make some experiments with the least injured of the fractured jars, in order to ascertain the probability or improbability of this conjecture. With this intention I removed the damaged jars from the battery; and finding that one of them was simply perforated without having any lateral cracks extending from the hole, I took from round it nearly an inch in breadth both of the exterior and interior coating, and made each side of the glass perfectly clean. The jar was then placed directly below the ball and wire at the extremity of the prime

prime conductor of the electrical machine, one end of a chain being annexed to the wire, while the other end of it descended into the jar and came in contact with the middle of its bottom, so that the suspended part of the chain was at equal distances from the opposite sides of the jar. The chain was thus connected with the bottom only of the inside coating, in order that the jar might be charged and discharged as nearly as possible under the same circumstances as when it was in the battery; the jars of which have their charging wires in communication with the interior coating at the bottom of each, and nowhere else. The jar being disposed as described above, the object of the first experiment was to ascertain how many turns of the wheel were necessary to make it explode spontaneously through the perforation in its side.

Exp. 1.—The wheel being turned, on completing its eighth revolution an explosion took place through the perforation in the jar. The charge with which the jar exploded through the perforation being thus known, I now wished to communicate to it the greatest charge it would bear without so exploding; in order that the jar might be discharged in the usual way, and that I might observe at the moment of its discharge whether any appearance worthy of notice would occur at the perforation.

Exp. 2.—The jar was next charged by something more than seven turns of the wheel; one ball of an insulated discharging rod was then put in contact with its exterior coating at the same height as the perforation, and at about three inches distance from it: on making the other ball of the rod approach the ball communicating with the inside of the jar, a vivid spark was seen to pass between the two balls, and at the same moment a similar spark with respect to brilliancy and strength was likewise seen to pass between the interior and exterior coatings of the jar through the perforation in its side. Although the result of this experiment seemed to confirm my conjecture as to the cause of the bursting of the jars before related, yet it occurred to me that possibly the spark through the perforation might have been occasioned by the mode in which the jar had been discharged: to ascertain this, the jar was repeatedly charged as in Exp. 2, and was afterwards discharged by putting one ball of the discharging rod in contact at different times with different parts of its exterior coating, more or less distant from the perforation; and by bringing the other ball either more slowly or more quickly towards the ball communicating with the inside of the jar; but in all these instances the same phænomena occurred. Two sparks were always seen to pass simultaneously; one between the two balls, and the other between the

two

two coatings of the jar through the perforation in its side. Being now satisfied that the latter spark did not depend on the mode in which the discharge was effected, I proceeded to connect the upper part of the inside coating of the jar in six different points with the chain, by means of three pieces of metallic wire placed in the jar diameterwise, their opposite ends pressing somewhat stiffly against its opposite sides. The jar was then charged and discharged twice, as before; and at each discharge the spark again occurred at the perforation without any sensible difference in its appearance: but on attempting to repeat the experiment the third time, the charge exploded before it had arrived at its former intensity; and on examining the perforation there were now found several fractures proceeding from it, through one or other of which the jar, during every succeeding attempt to charge it, continued to explode; notwithstanding several expedients were employed to prevent it. The jar having thus become unable to retain a charge sufficient for the purpose in view, and the other damaged ones appearing to be so fractured as to forbid any attempt to employ them, I was unable to proceed further in the inquiry, and obliged to rest satisfied with having ascertained the following facts:

1. That when a Leyden jar, under the circumstances already described, is charged to a certain height, and then discharged in the usual way,—the charge will separate or divide itself into two portions, one of which will pass through the metallic medium employed to produce the discharge; while the other portion will pass from one coating of the jar to the other, through the perforation in its side.

2. That this separation of the charge takes place with every mode of discharge by which a spark is elicited at the ball of the charging wire.

3. That when the charging wire communicates in several points with the upper part of the coating, all other circumstances remaining the same, the separation of the charge still ensues; and the ratio of its separated portions to each other experiences no sensible alteration.

Should future experiments show that this phænomenon depends on the limited and imperfect communication with the inside of the jar, it would afford information of much practical usefulness. If, on the contrary, it should be found to be wholly independent of that circumstance, the investigation of its cause would still be interesting, as its discovery would perhaps throw additional light upon the other singular phænomena of the Leyden jar. These considerations, together with the uncertainty whether I may hereafter have the opportunity and means of resuming and completing the inquiry, have induced me to lay it before

before your readers in its present imperfect state, and to take the liberty of suggesting the following hints for its further prosecution.—Three jars of equal electrical capacity with one another might be taken, and a hole full a quarter of an inch in diameter be drilled in each. One of them should have the hole near the top of its coating; the second in its middle; and the third near its bottom. The holes being of this size and the glass tolerably thick, the explosions would pass through them from coating to coating without injury to the jars. If, however, stronger charges should be required than might with safety to the jars be employed in this manner, the coating round the holes might be removed to a greater distance, and two wires having a small ball on one extremity of each might be connected with the opposite coatings of the jars; the opposing balls being placed at a small distance from each other and opposite the centre of the holes: the explosions might then be made to pass through them from ball to ball without acting on the glass, and the experiments might be repeated as often as necessary without fear of damaging the jars. Cuthbertson's balance electrometer would likewise be useful for regulating with accuracy the strength of the charges.—A series of experiments made with each jar separately, and another series with the jars combined, under every variety of circumstance which might influence the results, would, it is probable, add something more to our knowledge of the laws which regulate the action of the electric fluid.

Should any of your ingenious and able correspondents be induced by this communication to investigate the subject of it, I shall have obtained by making it the only object I had in view.

I am, sir,

Your obliged servant,

Hereford, Sept. 14, 1815. THOMAS HOWLDY.

XXXVIII. *New Outlines of Chemical Philosophy.* *By* EZ. WALKER, *Esq. of Lynn, Norfolk.*

[Continued from vol. xlv. p. 432.]

THE phænomena of electricity are investigated in a much more satisfactory manner by low degrees of excitement, than by more powerful artificial means. For when very large electrical machines are used, all the air contained in a room soon becomes electrified, which puts a total stop to all investigations upon its natural properties; nay, even a single candle burning in a room soon alters the state of the air which it contains*.

* "The repellency between the cork-ball and the shot (the prime conductor which the Doctor made use of) is destroyed by candle-light, even though the candle be at a foot distance."—Franklin's Works, vol. i. p. 172.

The

The apparatus that I make use of for excitation is a barometer tube only 3-10ths of an inch in diameter, and a silk handkerchief; but these low electrical states require a very perfect mode of insulation. The material generally used for this purpose is glass; but this is known to be a conductor in some degree, and to remove this inconvenience, varnish and sealing-wax have been used. But as these do not render glass a perfect non-conductor, some more perfect mode of insulation is necessary when low degrees of electricity are the objects of investigation.

I have frequently made use of thermometer tubes, because they contain less surface than the glass rods generally used; but these only lessen the inconvenience, without removing it.

White flint glass contains much lead; whence I supposed that green-bottle glass might be better for this purpose, because it contains no metal, and a hint to this effect is given in these Outlines *. How far my supposition was well founded will appear from the following experiments:

Exp. 9.—1. Having fixed two slips of Dutch gold-leaf to the end of a thermometer tube ten inches long, I suspended it in the axis of an open-necked bell-glass, by means of sealing-wax; one-half of the tube being within the glass, and the other half above it.—2. A solid stick of green-bottle glass, seven inches long, and about the same thickness as the thermometer tube mentioned above, was fitted up in the same manner.

The excited barometer tube being held over the top of the stick of bottle glass, at the distance of an inch, the leaves diverged to an angle of about 30 degrees; but the effect was transient, the leaves soon collapsed.

The barometer tube, without receiving any further excitement, was then held over the top of the thermometer tube of flint glass, at the same distance as before; the leaves gradually diverged to an angle of more than 100 degrees, and continued electrified for 30 hours.

From these experiments we may infer, that green-bottle glass is greatly preferable to flint glass for insulation, and may be substituted for it in many parts of an electrical apparatus.

A thermometer tube seven inches long, having its surface uniformly covered with black sealing-wax to the thickness of 1-10th of an inch, is still a conductor: and when compared with a solid stick of green-bottle glass of the same dimensions, their conducting powers appear nearly equal,—but with this difference, the green glass conducts electricity much slower than the other †.

* Phil. Mag. vol. xlv. p. 425.

† "The smallest wire will be a sufficient conductor, covered a foot thick with sealing-wax."—Ency. Brit. Sup. vol. ii. p. 615.

A stick of black sealing-wax is the best non-conductor of any substance that I have examined, but it is far from being perfect. It may, however, be used with advantage in many electrical experiments, though not in such as require perfect insulation.

As a perfect insulating stand is a valuable part of an electrical apparatus, and as all the materials just mentioned were found to be conductors, I was induced to try what effect might be produced by combining glass and sealing-wax. I took a thermometer tube nine inches long, and joined it to a glass foot, like the foot of a wine-glass, with black sealing-wax. Upon the top of this tube a stick of sealing-wax an inch and a half in length and half an inch thick is fixed, having a circular piece of plate-glass three inches in diameter fixed horizontally upon the top of it. The upper surface of this glass is gilt with gold-leaf, and upon its centre one end of a slip of Dutch leaf is fixed with gum water. This serves as an index or electroscope; for, as soon as the gilt surface of the glass cap is charged, this index stands erect, being repelled equally on every side by the charged surface of the cap.

The perfection of this stand, as a non-conductor, was proved thus:

The wire upon the top of one of my best electrometers was applied to the glass foot of the stand, and then to the thermometer tube fixed upon it, without the least effect being produced upon the Dutch leaves: but as soon as the electrometer was brought near the cap the leaves began to diverge; and on coming into contact, either with the under or upper side of it, the leaves instantly diverged to an angle of 90 degrees or more, according to the height of the charge which the cap had received.

In my fifth experiment, (Phil. Mag. vol. xliii. page 430,) which was made to prove the permeability of glass by the electrical elements, the neck of a Florence flask was closed with an animal substance communicating with the earth, by which means no electricity could enter the flask without passing through the glass.

The truth of this conclusion may be evinced by means of the following experiment.

Exp. 10.—I took the same flask mentioned in the fifth experiment, and after having removed the animal membrane from it, I dipped the top of its neck and stopper into melted sealing-wax, by which means the neck of the flask was closed *more completely* than if it had been hermetically sealed; for it has been proved in the preceding experiments, that sealing-wax is a much worse conductor than glass.

An excited barometer tube being held over the top of the neck of the flask at a short distance, no effect was produced in the

the Dutch gold-leaves within it; but the excited tube being held under the bottom of the flask, without touching it, the leaves diverged, and continued electrified for eight days.

Lynn, Aug. 4, 1815. EZ. WALKER.

XXXIX. *An alphabetical Arrangement of the Places from whence Fossil Shells have been obtained by Mr.* JAMES SOWERBY, *and drawn and described in vol.* i. *of his "Mineral Conchology;" with the geographical and stratigraphical Situations of those Places, and a List of their several Fossil Shells, &c. By Mr.* JOHN FAREY, *Sen. Mineral Surveyor.*

To Mr. Tilloch.

SIR,—IN page 274, of your last volume, the attention of your Readers was called to the subject of *Fossil Shells;* since then, three events of considerable importance to the progress of British Geology have occurred, viz. the publication of Mr. William Smith's very long expected *Map of the Strata*, with a short Memoir explaining the same, the completion of Mr. Aaron Arrowsmith's very large and minute *Map of England and Wales*, (by which the localities of Places* can now, in so superior a degree be ascertained, which is of the first importance to geological observations), and the completion of vol. i. of Mr. James Sowerby's "*Mineral Conchology*," with an Index to the 57 genera, and 212 species† of fossil Shells, described therein.

I have availed myself without delay, of the facilities thus afforded me, to draw up an arranged List of Mr. Sowerby's Shells, according to the Strata to which they severally belong, and mentioning the Place or places where each shell is found; which arrangement he intends to print, as a supplementary Index to his first

* Mr. A. is now engaged on "An Index to Maps," which is to contain all the Towns, Villages, Houses, Mines, Rivers, Hills, &c. &c. which are mentioned in his great Map of England and Wales, or in any others, or that his Friends may communicate, with their exact bearings and distances from known Towns: a work greatly wanted.

† I think it far more than probable, that repeated and more minute research and examination, will extend the *species* of fossil Shells described, or partly so, in this volume, to the number of 244; because in 21 instances the localities mentioned, are found to belong to two different, and mostly to very distant Strata, in the series, and in 6 other instances, to three such strata; and it is remarkable, that, without being aware of these circumstances, Mr. Sowerby has, in far the greater number of these instances, mentioned striking differences in the Shells, so, for the present, referred by him to the same Species, although, as it now seems, belonging to different Strata.

The Species, which I conceive will require dividing, are as follows; viz.

 Ammonites

first volume: and I hasten to transmit for insertion in your Magazine, a list of all the *Places* mentioned, as the localities of these Shells, with the Situation, Strata and Shells, of each of these places of observation.

After the many pressing invitations which I have given in your work, and elsewhere, to the free and candid examination and correction of what I may have ventured to state, concerning the Stratification of Britain, it may be almost superfluous to add, that it would particularly gratify me, to see these lists of Shells, stratigraphically arranged, discussed, and fully corrected, or marked in every erroneous or doubtful particular.

An alphabetical Arrangement, &c.

Aldborough, 20 m. NE of Harwich, Suff. in Crag Marl; on or in the London Clay?

Murex corneus, tab. 35, u. | Turbo rudis, t. 71, fig. 2.

Alton, 1¼ m. NE of Ashover, Derbs. in 3rd Coal-shale.

Ammonites Listeri (Brit. Min. t. 455.)

Amestry, 9 m. SW of Ludlow, Heref. in Derbyshire-peak Limestone.

Pentamerus Aylesfordii, t. 29.

Armswell-Hill, 5 m. NE of Cerne Abbas, Dorset, in Green Sand, below the Chalk.

Pecten quadricostata, t. 56, f. 3 to 8.

Ashford, 2 m. NW of Bakewell, Derb., in black Marble in the Limestone-shale.

Orthocera Bregnii, t. 50, f. 5.

Ashley, 5 m. ENE of Market Harborough, Northam. in Blue Marl or upper Lias Clay.

Orthocera .. .. p. 128.

Aynhoe, 5 m. SW of Brackley, Northamp. in Clay, &c. between the Bath-stone.

Terebratula concinna, t. 83, f. 6.	Terebratula media, t. 83, f. 5.
———— lateralis? t. 83, f. 1.	———— tetraëdra, t. 83, f. 4.

Ammonites planicosta, into 3; Cardium Plumstediense 2; Ellipsolithes funatus 2; Euomphalus catillus 2; Melania striata 2; Modiola depressa 2; Pecten quinquecostata 2; Perna aviculoides 2; Plagiostoma gigantea 2; P. spinosa 2; Productus aculeatus 2; Scaphites obliquus 2; Terebratula biplicata 3; T. carnea 2; T. crumena 3; T. digona 3; T. intermedia 2; T. lateralis 2; T. obsoleta 2; T. ornithocephala 2; T. subrotunda 3; Trigonia clavellata 3; Turrilites costata 2; Turritella conoidea 2; Unio uniformis 2; Venus equalis 2; and Vivipava fluviorum 2.—And in the arranged Lists of Shells now presented it will be found, that I have indicated all these doubtful Species, by a note of interrogation (?): leaving the Name to apply to the best defined Shell and Stratum, as far as I have been able to judge, but which Mr. Sowerby will, I trust, clearly settle, and place beyond doubt, in his future volumes.

Bakewell,

Bakewell, Derbyshire, in 1st Limestone.
Cardium elongatum, t. 82, f. 2. | Productus aculeatus, t. 68, f. 4.

Banbury, Oxford., in Clay, &c. between the Bath-stone.
Terebratula tetraëdra, t. 83, f. 4.

Barry Island, in Bristol Channel, Glam. in Blue Lias Limestone.
Modiola lævis, t. 8, le, lo.

Barton Cliff (the habitat of *Brander's* Shells) 5 m. E of Christchurch, Hants. in the London upper Clay.

Ancilla aveniformis, t. 99, m.
——— turritella, t. 99, la.
Dentalium striatum, t. 70, f. 4.
Fusus longævus, t. 63.
Infundibulum obliquum, t. 97, f. 1.
——— ——— spinulosum, t. 97, f. 6.
——— ——— tuberculatum, t. 97, f. 4, 5.
Murex Bartonensis, t. 34, lo.
Mya subangulata, t. 76, f. 3.
Ostrea gigantea, t. 64.
——— ——— ? t. 63.
Rostellaria rimosa, t. 91, f. 4, 5, 6.
Scalaria acuta, t. 16, lo.
——— semicostata, t. 16, m.
Serpula crassa, t. 30.
Solarium discoideum, t. 11. u. r.
Strombus amplus, t. 30.
Trochus agglutinans, t. 98, sm.
——— Benettiæ, t. 98, la.
Turritella brevis, t. 51, f. 3.
——— conoidea, t. 51, f. 1 and 4.
——— edita, t. 51, f. 7.
——— elongata, t. 51, f. 2.
Vivipara concinna, t. 31, f. 4, 5.
——— lenta, t. 31, f. 3.

Bath (near), Somers. on the heights, in the Great Oolite or freestone.
Mactra gibbosa, t. 42.

Ditto, in Clay and Fullers'-earth, occasioning vast slips.
Terebratula media, t. 83, f. 5.

Ditto, in under Oolite.
Nautilus lineatus, t. 41. | Terebratula digona, t. 96.

Ditto, in the valleys, in Blue Lias Limestone.
Plagiostoma gigantea, t. 77.

Ditto, in white Lias Limestone.
Plagiostoma gigantea? t. 77.

Ditto, W of, in Coal-measures, 9th Coal-shale?
Unio uniformis, t. 33, f. 4.

Bedford SE of, in Alum-shale, in lower part of the Clunch Clay.
Perna aviculoides? t. 66.

Ditto NE, near the House of Industry, in Bedford Limestone.
Ammonites discus, t. 12.

Benthal-Edge, 1 m. NW of Broseley, Shrop. in Derbyshire-peak Limestone.
Euomphalus angulosus, t. 52, f. 3.

Bethersden, 5 m. WSW of Ashford, Kent, in Clay, imbedding Sussex Marble.

Vivipara fluviorum, t. 31, f. 1.

Black-down Hills, (Whetstone-pits.) E of Cullumpton, Devonshire, in Green-sand, below the Chalk, with chalcedonic Shells.

Cardium Hillanum, t. 14, u.	Venus angulata, t. 65.
Cucullæa glabra, t. 67.	——- equalis? t. 31.
Dentalium medium, t. 79, f. 5.	——- lineolata, t. 20, u.
Pecten quinquecostata, t. 56, f. 3 to 8.	——- plana, t. 20, l.
Trigonia spinosa, t. 86.	Vivipara extensa, t. 31, f. 2.

Black-rock, m. SE of Cork Town, Ireland, in Derbyshire-peak Limestone.

Cardium Hibernicum, t. 82, f. 1 & 3.	Ellipsolithes funatus, t. 32.
Ellipsolithes compressus, t. 38.	———— ovatus, t. 37.
	Orthocera striata, t. 58.
	Terebratula lateralis, t. 83, f. 1.

Black-rock, of Limerick Town, Ireland, in Derbyshire-peak Limestone.

Amplexus coralloides, t. 72.	Terebratula biplicata? t. 90.

Bognor, 7 m. SW of Arundel, Sussex, in the London upper Clay.

Dentalium planum, t. 79, f. 1.	Mya intermedia, t. 76, f. 1.
Lingula tenuis, t. 19, f. 3.	Natica similis, t. 5, m.
Modiola elegans, t. 9, le u, m, lo.	

Boulogne (near), Depart. of Streights of Calais, France, in the Aylesbury Limestone.

Trigonia clavellata, t. 87, u.

Brackesham Bay, 11 m. E of Portsmouth, Hamp. in the London upper Clay.

Venericardia planicosta, t. 50.

Bradford, Somers. in Clay under Bedford Limestone.

Terebratula digona? t. 96, f. 1, 2, 3.

Bradford, S of (Low Moor?), Yorks. in Ironstone of 9th Coal-shale.

Unio acutus, t. 33, f. 5, 6, 7.

Bramerton (hill), 5 m. SE of Norwich, in Crag Marl.

Ammonites binus, t. 92, f. 3.	Scalaria similis, t. 16, u.
Balanus tesselatus, t. 84, f. 1.	Turbo littoreus, t. 71, f. 1.
Mya lata, t. 81.	

Brandestone, 4 m. SW of Framlingham, Suff. in superficial Gravel, belonging to some unascertained Stratum.

Ammonites quadratus, t. 17, f. 3.

Brentford, Middx. in the London upper Clay.

Murex trilineatus, t. 35, f. 4, 5.	Nautilus imperialis, t. 1, u.

Brighton,

Brighton, Sussex, in soft, upper Chalk.
Plagiostoma spinosa, t. 78, f. 1, 2.
Ditto, E of, in hard, lower Chalk.
Scaphites obliquus ?, t. 18, f. 4 to 7.

Brockenhurst, 4 m. N of Lymington, Hamps. in the London upper Clay.
Infundibulum obliquum, t. 97, f. 1.

Buildwas, $2\frac{1}{2}$ m. NW of Broseley, Shrops., in Derbyshire-peak Limestone.
Pentamerus lævis, t. 28, r.

Bury St. Edmunds (near), Suff., in the Woolwich Loam ?.
Cardium Plumstediense ? t. 14, m. | Serpula . . . . t. 14, m.

Buxton, N of ? 10 m. NW of Bakewell, Derbs. in Limestone-shale.
Euomphalus catillus ? t. 45, f. 3, 4.
Ditto, SE (Mill-dale), in 3rd Limestone.
Ammonites sphæricus, t. 53, f. 2.
Productus aculeatus ?, t. 68, f. 4.
——— scabriculus, t. 69, f. 1.
Ditto, SW (Pool's-hole), in 4th Limestone.
Ammonites striatus, t. 53, f. 1.

Cambridge (Castle-hill), in Chalk Marl.
Terebratula biplicata ? t. 90.

Cærphilly Castle (near), Glamor. in Blue Lias Limestone.
Modiola lævis, t. 8, le, lo.

Cardiff (Castle-hill), Glamor. in Blue Lias Limestone.
Plagiostoma gigantea, t. 77.

Castleton, SW of, 4 m. N of Tideswell, Derbys. in 3rd Limestone.
Ammonites sphæricus, t. 53, f. 2.
Ditto, in 4th Limestone.
Ammonites striatus, t. 53, f. 1.

Cathrines, St., Mount, m. of Rouen, in dep. of Lower Seine, in France, in Chalk Marl ?.
Ellipsolithes funatus ? t.32 | Turrilites costata, t. 36.
| ——— tuberculata, t. 74.

Charmouth Cliff, 1 m. ENE of Lyme-Regis, Dorset, in Blue Marl, above the Lias, imbedding large Bones, Teeth, &c.
Ammonites ellipticus, t. 92, f. 4.

Chatley, 1 m. SE of Norton St. Philip, Somers., in Cornbrash of the Bedford Limestone.
Terebratula digona, t. 96.
——— obovata, t. 101, f. 5.
——— intermedia, t. 15, f. 8.
——— ornithocephala, t. 101, f. 1, 2.
——— subrotunda ? t. 15, f. 1 & 2.

Christchurch (near), Hamps., in the London upper Clay.
Turritella elongata, t. 51, f. 2.

Christian-Malford, 4 m. NNE of Chippenham, Wilts, in Kelloway Limestone.
Ammonites sublævis, t. 54.

Chute Farm (Shoot), 3 m. SW of Warminster, Wilts, in Green Sand, below the Chalk.
Chama canaliculata, t. 26, f. 1.
——— conica, t. 26, f. 3.
Dianchora striata, t. 80, f. 1.
Pecten quadricostata, t. 56, f. 1, 2.
——— quinquecostata, t. 56, f. 3 to 8.
Terebratula biplicata, t. 90.
——— intermedia ? t. 15, f. 8.
——— ovata, t. 15, f. 3.

Colebrook-dale, 2 m. N of Broseley, Shrop. in Derbyshire-peak Limestone.
Euomphalus discors, t. 52, f. 1.
——— rugosus, t. 52.
Pentamerus Aylesfordii, t. 29.

Comb-down, m. of Bath, Somers., in the under Oolite.
Nautilus lineatus, t. 41.

Cotswold Hills, E of Stroud, Gloucest., in the Great Oolite.
Terebratula carnea? t. 15.

Craymouth, m. of Dorset. ?, in Blue Marl, above the Lias.
Ammonites planicosta ?, t. 73.

Crick Tunnel, 6 m. N of Daventry, Northamp., in Blue Marl, above the Lias.
Orthocera .. .. p. 128.

Croft-Ambrey Park, 8 m. SSW of Ludlow, Heref., in Derbyshire-peak Limestone.
Pentameris Aylesfordii, t. 29. | Pentameris Knightii, t. 28, u.

Croom-hill, 3½ m. SSE of Buxton, Derbys., in 4th Limestone.
Productus Martini ( ), p. 158.

Derbyshire, in 12th Coal-shale ?
Unio subconstrictus, t. 33, f. 1, 2, 3.

Ditto, in 9th Coal-shale.
Unio acutus, t. 33, f. 5, 6, & 7.

Ditto, in one of the four Peak Limestone Rocks.
Euomphalus nodosa, t. 46.

Devizes, NE of, Wilts, in soft, upper Chalk.
Terebratula carnea, t. 15, f. 5, 6.

Ditto, in (Canal, near the Town), in Green Sand, below the Chalk.
Arca carinata, t. 44, io. | Pecten quinquecostata, t. 56, f. 3 to 8.
Mya mandibula, t. 43. | Turrilites obliqua, t. 75, f. 4.

Dilton,

Dilton, 2 m. S of Westbury, Wilts, in Green Sand, below the Chalk.
Vermicularia concava, t. 57, f. 1 to 5.

Dinton, 9 m. W of Salisbury, Wilts, in Green Sand.
Chama haliotoidea, t. 25.

Donhead St. Mary, 4 m. NE of Shaftsbury, Wilts, in Green Sand.
Chama haliotoidea, t. 25.

Downton, 5 m. W of Ludlow, Shrops. in Derbyshire-peak Limestone.
Pentamerus Knightii, t. 28, u.

Dublin (near), Ireland, in Derbyshire-peak Limestone.
Euomphalus pentangulatus, t. 45, f. 1, 2.
Terebratula lateralis, t. 83, f. 1.

Dudley (Castle-hill), N of the Town, Staff., in Derbyshire-peak Lmestone.
Orthocera circularis, t. 60, f. 6, 7.

Elmsett, 7 m. NW of Ipswich, Suff., in Crag Marl.
Venus equalis, t. 21.

Evershott, 8 m. S of Yeovil, Dorsetshire, in Chalk Marl.
Ammonites planicosta ? t. 73.

Exmouth, 9 m. SSE of Exeter, Devons., in Blue Marl, above the Lias.
Ammonites planicosta ? t. 73.
Dentalium cylindricum, t. 79, f. 2.

Farnham, SE of, Surry, in Sussex Marble in Clay above the Woburn Sand.
Vivipara fluviorum, t. 31, f. 1.

Felmersham, 6 m. NW of Bedford, in alluvial Clay, on the surface.
Unio uniformis ? t. 33, f. 4.

Ditto, in the Bedford Limestone.
Ostrea Marshii, t. 48.
Terebratula digona, t. 96, f. 4, 5.
Terebratula intermedia, t. 15, f. 8.
——— obsoleta, t. 83, f. 7.

Filley-Bridge, or Point, 6 m. S of Scarborough, Yorks., in Blue Marl, on the Aylesbury Limestone.
Perna aviculoides, t. 66.

Folkstone, NE of, 5½ m. SW of Dover, Kent, in Chalk Marl, below the hard Chalk and Firestone.
Ammonites minutus, t. 53, f. 3.
Dentalium ellipticum, t. 70, f. 6, 7.
Hamites adpressus, t. 61, f. 6.
——— attenuatus, t. 61, f. 4, 5.
——— compressus, t. 61, f. 7, 8.
Hamites gibbosus, t. 62, f. 4, r.
——— intermedius, t. 62, f. 2, 3, 4.
——— maximus, t. 62, f. 1.
——— rotundus, t. 61, f. 2, 3.
——— tenuis, t. 61, f. 1.
Nautilus inequalis, t. 40, lo.

Fonthill,

Fonthill, 2 m. SE of Hindon, Wilts., in Green Sand.
Modiola pallida, t. 8, r, lo.

Gisleham, 4 m. SW of Lowestoft, Suff., in green Sandstone Lumps (belonging to some unascertained stratum) in superficial Gravel.
Terebratula lata, t. 100, lo. | Terebratula ovoides, t. 100, u.

Goatsacre, m. E of Bath, Wilts, in Coral-rag, under the great Bath Oolite?
Melania striata? t. 47, l.

Great Bowden, 1 m. NE of Market Harborough, Leicest., in Blue Marl above the Lias.
Orthocera .. .. .. p. 128.

Gunton, 1 m. N of Lowestoft, Suff., in Crag Marl, in or on the London Clay?.
Terebratula obsoleta? t. 83, f. 7. | Trigonia clavellata? t. 87.

Haldon Hills, SW of Exeter, Devons., in Green Sand, below the Chalk.
Chama plicata, t. 26, f. 4.
——— recurvata, t. 26, f. 2.
Pecten quadricostata, t. 56, f. 1, 2.
Trigonia dædalea, t. 88.

Halifax, N, (Cathrine-slack, vol. xxxix. p. 352), Yorks., in 3rd Coal-shale.
Ammonites Listeri (Brit. Min. t. 455).
Orthocera Steinhaueri, t. 60.

Hamsey, 1½ m. N of Lewes, Sussex, in Chalk Marl, below the Chalk.
Ammonites Mantelli, t. 55.
Arca subacuta, t. 44, u.
Hamites intermedius, t. 62, f. 2, 3, 4.
Scaphites obliquus, t. 18, f. 4 to 7.
Turrilites costatus, t. 36.
——— undulata, t. 75, f. 1, 2, 3.
Vermicularia umbonata, t. 57, f. 6, 7.

Harwich, near, (Essex Cliffs) Essex, in Crag-marl, on or in the London Clay?
Cardium Parkinsoni, t. 49. | Murex rugosus, t. 34.

Heddington, 3 m. S of Calne, Wilts, in the Aylesbury Limestone.
Melania Heddingtonensis, t. 39, r, le.

Heytsbury, NE, 4 m. SE of Warminster, Wilts., in hard, flintless Chalk.
Plagiostoma spinosa? t. 78.

Highgate Archway, E of the Town, Middles., in the London upper Clay, with Septaria, rotten Wood, Resin, Fish, &c.
Avicula media, t. 2.
Cardium nitens, t. 14, lo r, le, r, lo le.
Cassis carinata, t. 6, u.
——— striata, t. 6, lo.
Cypræa oviformis, t. 4.

Dentalium

Dentalium incrassatum, t. 79, f. 3, 4.
——— nitens, t. 70, f. 1, 2.
Modiola depressa, t. 8, u.
——— elegans, t. 9, le u, m, lo.
Murex trilineatus, t. 35, f. 4, 5.
Natica glaucinoides, t. 5, u.
——— similis, t. 5, m.
Nautilus imperialis, t. 1, u.
——— ziczac, t. 1, lo.
Pectunculus decussatus, t. 27, f. 1.
Rostellaria lucida, t. 91, f. 1, 2, 3.
Serpula crassa, t. 30.
Solarium patulum, t. 11, lo, r. le.
Solen affinis, t. 3, & t. 8, u.
Strombus amplus, t. 30.
Teredo antenautæ, t. 102.
Turritella conoidea, t. 15, f. 1 & 4.

Holywell, of Ipswich Town, Suffolk, in Crag-marl, in or on the London Clay?.

Balanus crassus, t. 84, f. 2.
Dentalium costatum, t. 70, f. 8.
Emarginula crassa, t. 33, u.
——— reticulata, t. 33, l.
Infundibulum rectum, t. 97, f. 3.
Murex contrarius, t. 23.
Murex corneus, t. 35, u.
——— rugosus, t. 34.
——— striatus, t. 22.
Scalaria similis, t. 16, u.
Turritella conoidea? t. 51, f. 5.
——— incrassata, t. 51, f. 6.
Venus equalis, t. 21.
Vivipara suboperta, t. 31, f. 6.

Hordle-Cliff, 4 m. SW of Lymington, Hamps., in the London upper Clay.

Dentalium entalis, t. 70, f. 3.
——— striatum, t. 70, f. 4.
Fusus longævus, t. 63.
Ostrea gigantea, t. 64.
——— .. .. ? t. 63.
Pectunculus costatus, t. 27, f. 2.
Vivipara lenta, t. 31, f. 3.

Horningsham, SE of, 4 m. SW of Warminster, Wilts., in hard, lower Chalk.

Terebratula subrotunda, t. 15, f. 1, 2.

Ditto, in Green Sand, below the Chalk.

Turrilites costatus? t. 36.

Hornton Quarry, 14 m. SSE of Warwick, Oxfords. (Dr. Plot, p. 64), in Blue Lias Limestone.

Terebratula punctata, t. 15, f. 4.
——— subrotunda? t. 15, f. 1, 2.

Hopton-Wafers Court, or Hall, 3 m. W of Cloebury-Mortimer, Shrops., in Derbyshire-peak Limestone.

Pentamerus lævis, t. 28, r.

Hunstanton Cliff, 14 m. NNE of Lynn, Norf., in Chalk Marl, hard and bright red.

Terebratula biplicata? t. 90.

Husband-Bosworth Tunnel, 1½ m. N of the Town, Leicesters., in Blue Marl, above the Lias.

Orthocera .. .. .. p. 128.

Ilminster,

Ilminster, E of, 12 m. W of Yeovil, Somers., in the great Oolite Limestone.

Ammonites concavus, t. 94, l. | Ammonites elegans, t. 94, u.

Isle of Dogs (in West India Dock), Middx., in the Lond. up. Clay.

Lingula tenuis, t. 19, f. 3.

Islington Tunnel, under the S end of the Town, Middx., in the London upper Clay.

Rostellaria lucida, t. 91, f. 3.

Kellaway-Bridge, 2 m. NE of Chippenham, Wilts., in Kellaway or Calloway Limestone.

Ammonites sublævis, t. 54.

Kendal (near), Westmoreland, in Derbyshire-peak Limestone.

Nautilus discors, t. 13. | Orthocera striata, t. 58.

Kilbride, East, (W of), 7 m. SSE of Glasgow, Lanarks., in Derbyshire-peak Limestone.

Productus longispinus, t. 68, f. 1.

Leicestershire, in Blue Lias Limestone.

Helecina compressa, t. 10, m.

Lewes, Sussex, in lower, hard Chalk.

Dianchora lata, t. 8, f. 2.
Pecten quinquecostata ? t. 56, f. 3 to 8.
Plagiostoma spinosa? t. 78.

Limington, 1½ m. E of Ilchester, Somers., in Blue Lias Limestone.

Melania striata, t. 47, u.

Linlithgow County (Livingstone ?), Scotland, in Derbyshire-peak Limestone.

Productus Flemingii, t. 68, f. 2.
——— longispinus, t. 68, f. 1.
Productus Scoticus, t. 69, f. 3.
——— spinosus, t. 69, f. 2.
——— spinulosum, t. 68, f. 3.

Little Sodbury, 3 m. NE of Sodbury, Chipping, Glouc., in the under Oolite.

Trigonia clavellata ? t. 87. | Trigonia costata, t. 85.

Longleat (near), 5 m. WSW of Warminster, Wilts, in Green Sand, below the Chalk.

Terebratula biplicata, t. 90.
——— intermedia ? t. 15, f. 8.

Lyme-Regis, Dorset, in Blue Marl and Marston Marble, above the Lias.

Ammonites stellaris, t. 93. | Terebratula lampas, t. 101, f. 3.

Maidstone, near, Kent, in the Aylesb. Limestone, or upper Oolite.

Modiola parallela, t. 9, u, r.

Marston-magna, 5 m. NE of Yeovil, Somers., in Marston or Melbury Marble.

Ammonites planicosta, t. 73.

Middleham,

Middleham, 3 m. NE of Lewes, Sussex, in Chalk-Marl, below the Chalk.

Turrilites tuberculata, t. 74.

Midford (or Mitford), 2½ m. S of Bath, Somers., in the Great Oolite.

Mytilus amplus, t. 7.

Minster Cliff, ¾ m. NE of the Village, in Sheppey Isle, Kent, in the London upper Clay.

Ammonites acutus, t. 17, f. 1. | Teredo antenautæ, t. 102.
Nautilus imperialis, t. 1, u.

Muddyford, 1 m. ESE of Christchurch, Hamps., in the London upper Clay.

Fusus longævus, t. 63.

Norfolk County, in Crag-marl, in or on the London Clay.

Cardium Parkinsoni, t. 49.

Northfleet, 2 m. W of Gravesend, Kent, in soft, flinty Chalk.

Plagiostoma spinosa, t. 78, f. 1, 2.

Norton-Bavant (NE), 2 m. SE of Warminster, Wilts., in lower, hard Chalk.

Plagiostoma spinosa? t. 78.

Nutfield, 4 m. E of Reigate, Surry, in Green (brown) Sand, above the Fullers-Earth.

Nautilus undulatus, t. 40.

Osmington, 4 m. NE of Weymouth, Dorset., in Blue Marl, on he Aylesbury Limestone.

Perna aviculoides, t. 66, f. 1, 2.

Oxford, E of, in the Aylesbury Limestone, or upper Oolite.

Trigonia clavellata, t. 87, u.

Ditto, NW of, in the under Oolite.

Trigonia costata, t. 85.

Pakefield, 2 m. S of Lowestoft, Suff., in Crag-marl, in or on the London Clay.

Lingula ovalis, t. 19, f. 4.

Pickeridge Hill, South Wales, in Blue Marl, on the Lias.

Terebratula ornithocephala?, t. 101, f. 4.

Ditto, in Blue Lias Limestone.

Plagiostoma gigantea, t. 77.
Terebratula crumena? t. 83, f. 2, 3.

Pickwick, 4 m. SW of Chippenham, Wilts, in Clay, under Cornbrash Limestone.

Terebratula digona? t. 96, f. 1 2, 3.

Portland Island, Dorset., in the Aylesb. Limest., or upper Oolite.

Ammonites triplicatus, t. 92, f. 2. | Trigonia clavellata, t. 87, u.
Solarium conoideum, t. 11, m.

Plumstead, 2 m. ESE of Woolwich, Kent, in the Woolwich Loam, with Chert Nodules.

Cardium

Cardium Plumstediense, t. 14, r, l.
Infundibulum echinulatum, t. 97, f. 2.
Murex latus, t. 35, le, lo.
Mya plana, t. 76, f. 2.
Pectunculus Plumstediense, t. 27, f. 3.

Radipole, 2 m. NNW of Weymouth, Dorset, in the Aylesbury Limestone.
Trigonia clavellata, t. 87, u.

Richmond-Park Well, SE of the Town, Surry, in the London upper Clay.
Dentalium incrassatum, t. 79, f. 3, 4.
Modiola elegans, t. 9, le u, m, lo.
Nautilus centralis, t. 1, le.

Rickmansworth, Hertf., in soft, upper Chalk.
Plagiostoma spinosa, t. 78, f. 1, 2.

Ringmer, 2 m. NE of Lewes, Sussex, in Chalk-marl, below the Chalk.
Ammonites Mantelli, t. 55. | Turrilites tuberculata, t. 74.

Scalebar, 6½ m. NNW of Settle, Yorks., in 1st Limestone.
Euomphalus catillus, t. 45, f. 3, 4. | Orthocera undulata, t. 59.

Scarborough, S of, Yorkshire, in Blue Clay, under the upper Oolite of Aylesbury.
Ammonites nodosus, t. 92, f. 5.

Settle, N of, 13 m. SE of Skipton, Yorks., in 1st Limestone.
Helix carinatus, t. 10, u, lo.

Sherborne-Park Well, 6 m. E of Yeovil, Dorsets., in Marston Marble, above the Lias
Ammonites planicosta, t. 73.

Shotover Hill, 3 m. E of Oxford, in Blue Marl, on the Aylesbury Limestone.
Perna aviculoides, t. 66.

Ditto, in Aylesbury Limestone or upper Oolite.
Ammonites cordatus, t. 17, f. 2, 4.
Melania Heddingtonensis, t. 39, r, le.
Vermicularia ovata, t. 57, f. 8.

Sidmouth, E. of, 8½ m. SSW of Honiton, Devons., in hard, lower Chalk.
Plagiostoma spinosa? t. 78.

South-end, 3½ m. S of Rochford, Essex, in the Lond. upper Clay.
Teredo antenautæ, t. 102.

South Horton, 3 m. NE of Devizes, Wilts., in Chalk-marl, below the Chalk.
Hamites intermedius, t. 62, f. 2, 3, 4.

Staveley, 4 m. NE of Chesterfield, Derbys., in Ironstone, in 9th Coal-shale.
Unio uniformis, t. 33, f. 4.

Stoneham,

Stoneham, $1\frac{1}{2}$ m. N of Lewes, Sussex, in Chalk-Marl.
Turrilites tuberculata, t. 74.
Stourhead, $3\frac{1}{2}$ m. NW of Mere, Wilts., in Green Sand, below the Chalk.
Chama haliotoidea, t. 25.
Pecten quadricostata, t. 56, f. 1, 2.
Stubbington-Cliff, 4 m. WNW of Gosport, Hamps., in the London upper Clay.
Melania sulcata, t. 39, m. | Turritella conoidea, t. 51, f. 1 & 4.
Suffolk County (NW part of), in hard, lower Chalk.
Terebratula subrotunda, t. 15, f. 1, 2.
Sussex County, in Chalk Marl.
Dentalium decussatum, t. 70, f. 5.
Swanswick, 2 m. NE of Bath, Somers., in the Great Oolite Limestone.
Cardita abrupta, t. 89, f. 2. | Cardita striata, t. 89, f. 1.
Sydenham, NW of, 4 m. NNE of Croydon, Surry, in Septaria, in Crag ?, in or on the London Clay.
Venus equalis, t. 21.
Tees River, upper part, Durham and Yorks., in 1st Coal-shale?
Terebratula crumena? t. 82, f. 2, 3.
Teignmouth (Little), NW of, 5 m. E of Newton-Abbot, Dev., in the Green Sand.
Venus equalis? t. 31.
Tideswell, NE of, 6 m. SW of Bakewell, Derbys., in 1st Limestone.
Euomphalus catillus, t. 45, f. 3, 4.
Ditto, W of, in 3rd Limestone.
Productus scabriculus, t. 69, f. 1.
Tog-hill, S of, 5 m. N of Bath, Somers., in the under Oolite.
Terebratula digona? t. 96.
Trowse, 2 m. SSE of Norwich, in soft, upper Chalk.
Terebratula carnea, t. 15, f. 5, 6.
Tupton, $3\frac{1}{2}$ m. S of Chesterfield, Derbys., in Ironstone, in 9th Coal-shale.
Unio uniformis, t. 33, f. 4.
Walton-Nase, 6 m. S of Harwich, Essex, in Crag-marl, in or on the London Clay.
Murex corneus, t. 35, u. | Murex rugosus, t. 34.
Wapping (in London Dock), Middx., in alluvial Silt and Peat, on the London Clay.
Vivipara fluviorum? t. 31, f. 1.
Warminster (near, NE), Wilts, in soft upper Chalk.
Terebratula carnea, t. 15, f. 5, 6.
Terebratula subundata, t. 15, f. 1, 2.
Ditto, (near, E), Wilts, in hard, lower Chalk.

Scaphites

Scaphites obliquus ?, t. 18, f. 4 to 7.
Terebratula semiglobosa, t. 15, f. 9.

Warminster, in Green Sand, below the Chalk.
Terebratula biplicata, t. 90.
Terebratula intermedia ?, t. 15, f. 8.

Whitby (Cliffs), Yorkshire, in Alum-shale, or lower part of the Clunch Clay.
Ammonites armata, t. 95. | Orthocera conica, t. 60, f. 1, 2.
Modiola depressa ? t. 8, m.

White-Lackington Park, 1½ m. NE of Ilminster, Dorsets. in the great Oolite.
Ammonites jugosus, t. 92, f. 1.

White-nab, 3 m. SE of Scarborough, Yorks., in Blue Marl, on the upper Oolite.
Perna aviculoides, t. 66.

Wigan, Lancas., on Cannel Coal, in the 9th Coal-shale ?
Mytilus crassus, p. 84, and Brit. Min. t. 386.

Wiltshire, in Cornbrash of the Bedford Limestone.
Terebratula obsoleta, t. 83, f. 7.

Winster, 5 m. SSE of Bakewell, Derbys., in 1st Limestone.
Euomphalus catillus, t. 45, f. 3, 4.
Terebratula crumena, t. 83, f. 2, 3.

Wisbeach, (in a Well,) 19 m. NE of Peterborough, Camb., in Kelloway Rock ?.
Ammonites sublævis, t. 54.

Wollaton, 3 m. W of Notts., in Ironstone, in the 12th Coal-shale ?.
Unio subconstrictus, t. 33, f. 1, 2, 3.

Wolsingham, Durham, in Ironstone, in the 9th Coal-shale ?.
Lingula mytiloides, t. 19, f. 1 & 2.

Woodbridge, 8 m. NE of Ipswich, in Crag-marl, in or on the London Clay.
Natica depressa, t. 5. | Venus equalis, t. 21.

Worlingham, 1½ m. SE of Beccles, Suff., in Crag-marl.
Ammonites serratus, t. 24.

Yeo-Edge, 7 m. NW of Ludlow, Shrops., in Derbyshire-peak Limestone.
Pentamerus Aylesfordii, t. 29.

Yeovil, (NE of,) 4½ m. SSE of Ilchester, Somers., in Marston or Melbury Marble.
Ammonites planicosta, t. 73.
Scaphites equalis, t. 18, f. 1, 2, 3.

At my earliest leisure, I intend to send you some Geological remarks, suggested by the above species and localities of fossil Shells, and am

Your obedient servant,

12, Upper Crown Street, Westminster, Sept. 10, 1815. JOHN FAREY, Sen.

XL. *On* Jameson's *Preface to* Cuvier's *Theory of the Earth.*

Being something of a Geologist, and possessing a high veneration for the Bible, it was with no inconsiderable pleasure that I read in the preface written by Professor Jameson to the translation of Cuvier's admirable "Essay on the Theory of the Earth," some observations asserting that "the structure of the Earth, and the mode of distribution of extraneous fossils or petrifactions, are so many direct evidences of the Scripture account of the formation of the Earth;" and that "even the periods of time, the six days of Mosaic description, are not inconsistent with our theories of the Earth." If however, instead of leaving the reader to find the precise points of agreement, the Professor had condescended to explain them, he would, I conceive, have done service to the community. I have read the performance of Cuvier with much interest, but without being able to discover the promised agreement.

The researches of Geologists have within the last few years caused many wonderful discoveries. They have found that a certain class of rocks contain no animal or vegetable remains; which therefore they term *primitive*, as having, in their estimation, been first created. These are found beneath all other rocks, and upon them rest another class, by Werner called *transition* rocks, which contain only the remains of zoophytes; the animals which formed the first link in the chain of animated beings. Upon the transition, lies another and more numerous class, called *floetz* rocks; the older of which inclose the remains of animals, which not being now found, are of course extinct; and the newer of them, the remains of animals which approach, but are not actually the same as those inhabiting the present seas; and the newest of them, as well as the *alluvial* formation, which lies above them, contain the remains of animals not to be distinguished from those now existing. The latter also inclose the bones of large land animals.

But "it is quite undeniable that no human remains have hitherto been discovered among the extraneous fossils" (page 117) in any solid rock, which is not decidedly, both in respect of its situation and composition, of very late date.

We have the authority of eminent Geologists for asserting that the remains of animals are found in mountains ten and even fourteen thousand feet above the level of the sea. What amazing revolutions must have befallen the earth, to produce such catastrophes as have buried in mountains so elevated, the shells which once inhabited the ocean! Geologists are not, I believe generally inclined to consider this as the consequence of the universal

versal deluge; nor does any thing in the Mosaic account of that wonderful event, induce the necessary belief that any important change took place on the surface of the earth in consequence of it. Indeed, had this been its effect, we should in all probability have discovered the bones of men mingled with extraneous fossils; and since the bones of men have never been thus found, or even in alluvial soils in any considerable quantity, may we not reasonably infer that they were carried off by the retiring water, and that they lie buried in ocean?

It forms a question that continues greatly to puzzle some Geologists, Why, if man inhabited the earth at the same time with those animals whose remains are found in such vast numbers, are not human bones found with them? since there is nothing in their composition that should make them less enduring than those of other animals, as is amply testified by such as are found beneath the surface of the soil in ancient fields of battle, both in this and in other countries.

But other Geologists are of opinion that some of those catastrophes, by which animal life suffered so greatly, and of which the remains actually constitute almost entire strata in certain calcareous mountains, actually took place before the earth was inhabited by man;—that other animals were created long before him;—and that the earth existed a long time previously to the creation of animals. The following extracts will, as it seems to me, show that these are the opinions of Cuvier.

He speaks, p. 3, of the revolutions which took place "*previously to the existence of all nations.*"

At p. 17, he says pointedly,—"*there have not been always living creatures on the earth.*"

At p. 21, he says, "It is impossible to deny that the waters of the sea have formerly, and for a long time, covered those masses of matter which now constitute our highest mountains; and further, that *these waters did not support any living bodies.*"

The concluding sentence of his Essay is as follows: "And man, to whom only a short space is allotted upon earth, would have the glory of restoring the history of *thousands of ages which preceded the history of his race, and of thousands of animals that never were contemporaneous with his species.*"

These quotations seem decisive of the opinions of Cuvier; and it remains for Professor Jameson to show the agreement of such opinions with the sense in which the words of Moses are *usually accepted:* they appear to me exactly the reverse.

There now lies beside me an edition of the Bible printed by Baskett in 1767, in which, at the head of the first chapter of Genesis, are these words, "Year before the common year of Christ, 4004," which I take it for granted means that we should consider

sider that as the period of the Creation of the earth in computing its age; whence it follows that it is now 5819 years old; which is the common opinion.

It follows, therefore, that the five first verses of the first chapter of Genesis are usually considered as including the work of the "first day"; in which is comprehended the conception of an almost simultaneous creation of the earth, supposing the day spoken of to be equal only to our natural day.

If this be true, where were the "thousands of ages" alluded to by Cuvier, as "preceding the history of the race of man?" who, according to Moses, was created on the sixth day (in the usual acceptation of his words) from the creation of the earth.

Jameson explains the word "day" by a resort to the hypothesis of Bishop Horsley, that the motion of the earth may have been slower at the time of its formation, than after it was formed. This hypothesis is plausible, and may be true; but we can by no means verify it.

Let us attend to the words of Moses.

"1. In the beginning God created the heaven and the earth.—2. And the earth was without form, and void; and darkness was upon the face of the deep. And the spirit of God moved upon the face of the waters."

On perusing and re-perusing the above, as well as the context, often and with the utmost attention, I cannot persuade myself that it is incumbent on us to include the whole of the facts recorded in the five first verses, as necessarily comprehended in the work of the first day; and though it is not for us to attempt to unravel the great and sublime mysteries of creation, it seems to me that we shall not be doing despite to the meaning of the inspired writer, by declaring that "in the beginning God created the heaven and the earth;" and by making a pause, where he has concluded a sentence, by recording a fact.

In this record there is no limit implied, except that the earth was created "in the beginning,"—nor is there in the succeeding verse any limit implied, as to the length of time in which it continued "without form, and void;" nothing by which we can possibly deduce the conclusion that, in order to make up the exact term of 5819 years, the earth has existed only the precise number and duration of so many natural days as make up that number of years.

If we accept the proffered hypothesis of slower revolution, we shall have a term totally undefined; it may comprehend the "thousands of ages" alluded to by Cuvier, greater in number than human thought can compass.

But it seems probable that, however the hypothesis of slow revolution might apply to the time which I conceive to have been

been only included in the Mosaic account of the six days of creation; it cannot be philosophically applied to that term of time, *after* "the beginning," in which the earth continued to be "without form and void," and during which, "darkness was upon the face of the deep;" because that time was before the great scheme of creation had been completed,—before the universe was framed—before the "two great lights" were "made."—Is it not probable that, while the earth continued in this state, it suffered no revolution at all?—there was no day, nothing by which to measure the length of time during which it so existed.—How indeed shall the finite comprehension of man soar so high as to "the beginning?" by what means shall he ascertain its limit?

Surely it is far more reasonable, and even less presumptuous, to conclude that no limit was intended to be implied by Moses, in that term; and simply to believe, that, the earth being without form, and void, and in darkness, (without presuming to say how long it thus existed,) it pleased the Creator at a certain period, and in his own good pleasure, that "his spirit should move upon the face of the waters,"—that the "two great lights and the stars" should be "made,"—and, in a word, that the universe should receive its impulse and its laws!

And as the account of Moses declares that previously to the completion of the wonderful scheme of the universe, by the creation of the sun, the moon, and the stars, the earth existed, how is it possible for us to assign a term for its duration in what may thence be termed its isolated state? What reason is there that we should not adopt the belief of its having thus existed "thousands of years that preceded the history of man?"

The sum of what is intended to be conveyed in the preceding observations amounts to this; that, instead of assuming that the whole of the first five verses of the first chapter of Genesis were intended to be included in the Mosaic account of the first day's work, we ought rather to conclude that the first verse records an antecedent fact, that the earth was created "in the beginning."—The first part of the second verse relates, that the earth was without form, and void, and in darkness, (without relation to the time it had so continued,) when, according to the latter part of the same verse, the first movement of Omnipotence took place towards reducing to its present state of order and beauty the earth, which was the first created portion of the universe; the creation of the other parts of which was comprehended in the six days work. In a word, that we ought to consider the moving of the spirit of the Creator upon the surface of the waters, related in the latter part of the second verse, as the first movement of the six days work; the commencement of which took place at an uncertain period after the creation of the earth.

This

This mode of interpreting the words of the inspired writer may have heretofore been proposed, of which I have no knowledge; but it is now offered in the hope that there is nothing in it that can offend the most scrupulous zeal for the cause of religion, or those who are most tenacious for the modes of thinking of their predecessors. I have privately submitted it to many thinking men, without giving offence to any. If it should be asked, why an innovation should now be broached, and why an attempt should now be made to disturb the tranquil belief of generations? I reply, that it will be found, as I trust, in no respect to injure the character of Moses, but rather to confirm it, since this interpretation will best agree with our knowledge of the structure of the earth. And as Truth alone is my object, I shall most readily and dispassionately attend to any arguments that may temperately be brought against it, and cheerfully yield to and acknowledge such conviction as they may produce.

HOMO*.

* The idea suggested by HOMO, modified in various ways, is a very old one, and not only has had, but still has, advocates. PHILO held that the world was formed out of pre-existing matter: that is, he conceived the word ברא [he created] not to include the *creation* but the *arrangement* of matter. The author of the Book of *Wisdom* believed that GOD fashioned the world out of *unfashioned matter*, εξ αμορφȣ ὑλης. JUSTIN MARTYR (Apol. 1.) says it was the Christian doctrine of his time, Παντα, την αρχην, αγαθον οντα δημιȣργησαι αυτον [Θεον] εξ αμορφȣ ὑλης δεδαγμεθα: and elsewhere he says that PLATO [who held that the world was created out of pre-existing matter] borrowed his doctrine from MOSES.

MICHAELIS conceives ברא to have the same sense as the Latin PARIO. GEDDES probably comes still nearer the sense when he makes it the same as PARO. Certain it is that ברא is used in Scripture in a sense very different from that of *creating* in the common acceptation of the word; as in *Joshua* xvii. 15. where the word is rendered *cut down*, but should rather be *cut out*, or *make for thy self* (the portion of land which they wanted); and the same word is used in the same sense in the 18th verse.—HOMO should see Mosheim's Dissertation *De creatione mundi ex nihilo*.

The Hebrew may certainly be rendered in such a manner as to carry the sense contended for by HOMO; for it is not necessary that the VAU should always be translated as a copulative conjunction. The words may be rendered thus:—*In the beginning God created the heaven and the earth*, BUT *the earth was without form*, *yea*, *void* (of form), *and darkness was upon the face of the deep: and the spirit of God moved upon the face of the waters. Then God said, Let there be light*" &c. But some for "*the spirit of God*" translate "*a wind of God*," and contend that this is a mere Hebrew form of speech for "a mighty wind." If this be taken as the meaning, then the *mighty* or *tempestuous wind* may even be referred to the chaotic state of the matter subsequent to its creation out of nothing, but prior to the six days work. But the general opinion of those who so render the words, and who hold that the matter was created at a period not specified, is, that *the mighty wind* refers not to the turbulent state of the matter before the six days, but to the first kind of *agency* employed to bring the chaos into order.

XLI. *Notices respecting New Books.*

Mr. Charles Sylvester is preparing for publication An Account of the Improvements in Domestic Œconomy adopted at the Derbyshire Infirmary, describing the Means of warming and ventilating the Apartments, the Kitchen, Laundry, &c. illustrated with plates. The arrangements at this Infirmary having long excited the attention of social œconomists, Mr. Sylvester's work we doubt not will prove highly interesting.

Mr. Hanson of Manchester will shortly publish a folio chart, entitled The Meteorologist's Assistant; accompanied with a card explanatory of the mode of notation. The chart will serve for any year and place required; but the principal object of it is to bring into one view a year's observations of the weather by means of curves and characters; of course it will facilitate a comparison of cotemporary notations of remote places.

For some interesting particulars respecting Mr. Smith's Geological Map, and Mr. Arrowsmith's Great Map of England and Wales, which have just been published, our readers are referred to Mr Farey's Communication published in the present number.

A new edition of Dr. Wells's Essay on Dew is in the press, and will appear in October.

XLII. *Intelligence and Miscellaneous Articles.*

We continue M. Van Mons's valuable communication to the Editor of this work.

[Continued from p. 153.]

"The analysis of azote which your Journal has given, throws considerable light on the nature of the acidifiable combustibles. A peculiar inflammable gas, strongly oxidated, forms this combustible; and this inflammable gas united to hydrogen constitutes a metal, since the azote united to this principle constitutes a metallic oxide, which is ammonia. Azote may, according to the analysis of M. Berzelius, consist of a peculiar inflammable gas and of oxygen, since in a dry acid there ought to be found double the quantity of oxygen of that in water; but its elements cannot be 46 hydrogen and 54 oxygen; such a compound, as having only four of caloric, being no longer either a burning or a combustible body, equal parts forming complete saturation with exclusion of all caloric, and being inorganized

organized matter. Besides, in such a kind of composition, ammonia, instead of being a metallic oxide, will be a reduced metal; for primitive matter, rather than hydrogen, forms the metals. I have written to Berzelius, who has not yet informed me of this discovery for the details of his processes.

[To be continued.]

Extract of a Letter from M. J. B. Van Mons *to Mr.* Singer.

"You will find interspersed through the first and second volumes of my Translation of Davy, which I have now the honour to present to you, some fragments of my System of Meteorology. I consider atmospheric air as a fourth kingdom, having organized azote for its basis; as the vegetable kingdom has for its basis organized carbon; and as the animal kingdom has both organizations. Organization consists in being hydrogenated by water, instead of being so by reduced hydrogen; and the atmospherical organized body can hydrate itself and suroxygenate itself, in the same manner as the organized vegetable body hydrates and surhydrogenates itself. Animal life consists in causing a cessation of this suroxygenation and surhydrogenation of the organized matter of the other two kingdoms, by the formation of water, which at the same time causes animal heat. The changes of weather which take place from the varying influence of the planets*, and from the electrifying and unelectrifying influence of the sun, consist in the oxygenation and hydrogenation, sometimes alternate, but more frequently concomitant, of organized atmospherical matter. As the oxygen in separating itself from this matter is not quite in the state of gas, it is obliged to supply its deficiency of caloric by the vapour of water. Air perfectly hydrated, and in which the oxygen is so also, may likewise be dissolved by water; it is when the barometer ascends, and when at the same time the thermometer descends, and when the contrary march or progress of these two instruments takes place, that hydrated air dedissolves† itself. Azote, hydrogenated by water, is the oxide of the metal ammonium, of which ammonia is the oxidule, and organized azote is consequently a sort of gaseous alkali, which like the other alkalies can oxygenate itself, hydrate itself, hydrato-oxygenate itself, dissolve, and then take up hygrometric water. There is no meteorological phænomenon, however complex, which may not be clearly explained by these principles. Temperature depends on changes in the combinations that take place in air; and winds are the effects, not the causes, of these changes. And this explains how all the variations are announced beforehand by the indications of our ap-

* *L'influence tendant et distendant des astres.*

† *Se dedissout*

paratus. Electricity forms itself in the air by the combination of light with heat, and changes itself into heat when there is a tendency to change for rain*. The moon may by its reflected light electrify the heat of the atmosphere, and thus excite cold; but it also lowers the temperature by promoting the dissolution of clouds, without contributing to that effect by heat.

"Will you not make some observations on conducting-rods (*paratonnerres*) which subtract from the atmosphere such enormous quantities of electric fluid, which is lost to the globe by being conducted into water, and converted into heat? It is only since the introduction of these rods, which have been established in general use about 35 years, that the seasons have become irregular, and that the weather has scarcely been otherwise than unfavourable to a regular vegetation. They occasion the abortion of the small storms so necessary to afford rain in summer, and they derange the great tempests which should mark the transition of the seasons. Under the Austrian regime, in consequence of long droughts and continued rains, the conducting rods were ordered to be taken down, and the weather changed immediately. Since their introduction, storms have also been much more destructive than they were before. Perhaps the Harlem Society will offer a prize on this subject.

"J. B. Van Mons."

Annotation by Mr. Singer.

This synopsis of the meteorological views of M. J. B. Van Mons was obligingly communicated to me by the author, in consequence of the announcement of my intended publication on the phænomena of the atmosphere. Researches of a different nature have of late so closely occupied my attention, as to render it uncertain at what time I may be able to complete the proposed work, which will have for its leading object a practical and popular introduction to the means of observing meteorological phænomena: I have therefore thought it better to give publicity to the opinions of my learned correspondent without delay, more especially as my present experiments have as yet no prospect of a speedy termination.

On the question relating to lightning-rods, and their supposed influence in modifying the electric state of the atmosphere; I must observe that I know of no facts that authorize such an opinion, but I do know of many that militate against it. We have indeed heard of proposals for procuring or averting storms at pleasure, by the action of pointed conductors! And more recently it has been suggested, that accidents from thunder

* *Lors des detentes aux changemens en pluie.*

storms might be prevented over a whole kingdom by an extensive series of conductors: but it will, I presume, appear unquestionable, that neither of these proposals can have proceeded from a sufficiently attentive consideration of the subject, and there is reason to think they would never have been advanced by any one practically conversant with the details of such phænomena.

There are two modes in which a conducting rod may be supposed to influence the electricity of the atmosphere. 1st, It may be conceived to diminish the usual positive electricity of the air. And 2ndly, it may be regarded as a source of constant diffusion, a kind of silent discharger, calculated to prevent the formation of regular thunder clouds, by transmitting the electricity of their elementary modifications to the earth. If the first mode of operation were admitted as probable, it would teach us nothing; for at present we know not either the origin or purposes of this regular electricity, nor the absolute relation it bears to those changes in the state of atmospherical moisture and dryness by which its indications are so obviously and materially affected: still less can we pretend to assert that it is, or is not, essential to vegetation. Besides, if a conducting-rod had the power here supposed, how materially would the usual electricity of the atmosphere be diminished by natural causes! for the surface of the earth abounds with points and prominences, of equal efficacy as silent dischargers; and every tree, every spire, and each mountain and promontory, must be then regarded as subtracting constantly from the electricity of the atmosphere: it is indeed probable they do so, at least occasionally; but the quantity thus distributed bears so small a proportion to the whole, that the usual signs exhibited by our instruments are nearly as powerful in the neighbourhood of an extensive conductor, or a lofty tree, as in the open air; and the horizontal extension of a mile and a quarter of wire uninsulated, and at the elevation of 100 feet from the ground, has been found to have but a very trifling effect on the atmospherical electricity in its vicinity*. It may also be observed, that the modification of cloud called stratus remains positively electrified, though its lower surface is in contact with the earth.

These objections apply with still greater force to the second idea; for thunder clouds are usually formed at such an elevation from the earth's surface as to be entirely out of the influence of the highest projections thereon; and when they descend within the striking distance, we know from the frequency of damage in large towns, where there are numerous conductors, besides

* This experiment was made by that very active electrician, A. Crosse, Esq. of Broomfield.

steeples and lofty edifices, that even myriads of points and prominences are not sufficient in all cases to exhaust a thunder cloud of its electricity. Neither are thunder-storms observed to be unfrequent in situations where these causes are operating, comparatively, to the greatest possible extent.

Many other objections might be stated: but as the question is proposed merely as a subject for inquiry, it appears unnecessary to pursue it further. The statement of M. Van Mons appears to advance a circumstance in itsfavour ; but it does not appear to be given as a fact he has observed himself, and is certainly not supported by any correct observations whith which I am acquainted.

London, Sept. 14th, 1815. G. J. SINGER.

P.S. In the last number of this Magazine it is stated, that Professor Dobereiner obtained an amalgam of mercury and hydrogen, by decomposing water in contact with negative mercury, and that no gaseous hydrogen was liberated. I have made such experiments with every attention I could devise, but have not been able to detect any apparent change in the mercury; and in every case hydrogen has been given off at its points of contact with the water. The experiment is not perhaps accurately described.

GUNNERY :—EXPERIMENTS AT WOOLWICH.

We are glad to learn that experiments on this interesting subject, analogous to those which were conducted some years ago by Dr. Hutton with so much ability and success, are now carrying on at Woolwich—Dr. Gregory of the royal military academy, having been associated with a committee of general and other officers of artillery, for that purpose.

In the determination of the initial velocities of projected balls, the *ballistic pendulum* is the instrument which has been hitherto most advantageously employed. Mr. Robins, its original inventor, in the experiments described in his Gunnery, made use of a ballistic pendulum which weighed very little more than half a hundred weight. In the experiments made by Dr. Hutton, the ballistic pendulums were of various sizes and weights, increasing gradually from 560 pounds to 2420 pounds in weight, according to the size of the projected ball, of which the largest was *six pounds*. The pendulum recently constructed in the royal arsenal at Woolwich considerably exceeds in weight and dimensions the largest that was employed in Dr. Hutton's experiments. Its weight, at the commencement of the experiments, was about 7400 pounds, or *more than three tons and a quarter*. The face of the block of timber which forms the core of the pendulum is 3 feet broad and 4 feet high, its horizontal length or thickness being

being 6 feet. The timber rests upon an iron plate of an inch and a quarter thick, and the whole is held together by strong vertical cheeks of gun-metal and the requisite screws and bolts. The materials of the pendulum are so disposed as to bring the centre of *gravity* about 11 feet, and the centre of *oscillation* about 12 feet, below the axis of rotation; and to allow the point of impact of the ball upon the face of the pendulum to occupy any assigned place between those two centres. This pendulum, which was constructed under the direction of Colonel Millar of the royal artillery, has its frame-work mortised into a permanent edifice; and is so admirably framed and so exquisitely suspended, as to produce an extraordinary union of freedom and steadiness of motion with stability of structure. Such is the firmness of the construction, that the shock not merely of 6-pound balls, but even of 24-pound balls, moving with a velocity of 1300 feet per second, caused no prejudicial strain in the framing, nor even produced the slightest perceptible sinuosity in the groove described by the stylette on the broad indicatory arch below the pendulum. And at the same time, such was the freedom of suspension, that the slightest breeze moved this immense mass; and that in every case when the experiment was tried, it required more than 60 oscillations to reduce the semi-arc of vibration from 7° to 6°.

By the construction of a ballistic pendulum of sufficient magnitude to sustain without derangement the shock of large balls moving with great rapidity, an important point is gained; since the velocities with which balls move when propelled from the heavier artillery will no longer remain a matter of mere induction, but a fair result of actual experiment. The velocities of balls fired from 12-, 18-, and even 24-pounders can now be readily and satisfactorily deduced from the motion communicated to the oscillating block by the several balls. In the experiments already made by means of this new pendulum, only 6-pounders and 24-pounders have been employed; the intermediate sizes being left for subsequent experience. What has been done, however, fully confirms the experiments of Dr. Hutton as to the law between the charges and the velocities, and as to the increased initial velocity occasioned by an additional length of bore. In a variety of rounds, both with different charges and with guns of different lengths, there was not a single deviation from either of these laws; nor was there a single case in which stronger powder, as indicated by an accurate eprouvette, did not furnish the predicted result.

Other apparatus, we are told, is constructing for the purpose of confirming or modifying the old results, and of extending the inquiry to many points hitherto untouched.

STEAM

STEAM ENGINES IN CORNWALL.

The following is an abstract from Messrs. Leans' Report for July 1815, of the coals consumed by steam engines in Cornwall.

Thirty-four engines consumed 79233 bushels of coals, and lifted 676,496,000 pounds of water one foot high. Average work of the thirty-four engines 19,897,000 pounds lifted one foot high with each bushel of coals.

Two of Woolf's engines, one at Wheal Vor*, the other at Wheal Abraham, consumed 2674 bushels of coals, and lifted 92,510,500 pounds one foot high; being an average of 46,255,250 pounds lifted one foot high with each bushel of coals.

NEW VOYAGE OF DISCOVERY.

Count Nicolas Petrowitsch Romanzow, a most enlightened and public-spirited Russian nobleman, has at his own expense built and equipped a ship for a new voyage of discovery. This vessel sailed from Cronstadt on the 31st of July last, and has since touched at Plymouth on her way out. She is called the Rurik, carries the Russian military flag, and is commanded by Captain Kotzebue (son to the celebrated author of that name), a lieutenant in the Russian navy, and who has already sailed round the world in the Nadeshda, Captain Krusenstern. There are on board two other lieutenants in the Russian navy, Messrs. Schichmarew and Sacharün, the former of whom, although longer in the service than M. Kotzebue, has cheerfully consented to serve under him. Dr. Eschholz of the university of Dorpat, M. Chamisso the naturalist from Berlin, the Danish naturalist Wormskild, and the Russian painter Choris, also accompany the expedition. The Rurik, it is said, will double Cape Horn in the month of November next, and the expedition will employ the whole of 1816 and the beginning of 1817 in visiting in the South Seas those places which have not been as yet sufficiently examined. During the summer of 1817 they will coast along the inner shores of America to Behring's Straits, and return by the Straits of Torres to the Cape of Good Hope; so that they will probably return to Cronstadt in August 1818. It is left, however, to M. Kotzebue to prolong his voyage beyond this period if he think it necessary for the purposes in view. The whole plan of the voyage has been drawn up by Captain Krusenstern.

" * This engine has not had a fair average of coal this month, as brought from the wharf, on account of some of the stones [round coal] having been gathered for the use of a steam stamping-mill adjacent, which could not otherwise produce a supply of steam. T. L. J. L."

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To William Madeley in the parish of Yardley, in the county of Worcester, farmer, for his improved drilling machine for drilling beans, turnips, pease, pulse, corn, and seeds of every description. —27th July, 1815.—6 months.

To David Mushett, of Coleford, in the county of Gloucester, iron-master, for his improvement or improvements in the process or processes of making or manufacturing iron.—27th July, 1815. —6 months.

To John Lewis, of Brinscomb, in the county of Gloucester, clothier, for his improved shearing machine.—27th July.—2 months.

To Pierre Pelletan, for his new method of making sulphuric acid, commonly called oil of vitriol.—3d August.—6 months.

To Joseph Harvey, of Long-Lane, in the parish of Saint Mary Magdalen, Bermondsey, in the county of Surrey, turner, for his machine for the better striking and finishing leather.—4th August. 2 months.

To William Edridge, of Rotherhithe, in the county of Surrey, brass-founder, for his pump or fire engine.—4th August.—6 months.

To John Street, esq., of Clifton, in the county of Gloucester, for further improvements in his mode of making and working bellows.—11th August.—2 months.

To Richard Dixon, of High Holborn, in the county of Middlesex, trunkmaker, for improvements in the construction of trunks and portmanteaus, and the application of materials hitherto unused in the construction thereof.—11 Aug —6 months.

To John Edwards, of Canterbury Buildings, Lambeth, in the county of Surrey, for his method or means of preventing leakage in ships' boats and other vessels.—15th Aug.—6 months.

To Stephen Price, esq., of Stroud, in the county of Gloucester, engineer, for his invented improved machine for shearing or cropping woollen and other cloths that may require such process. —12th Aug.—2 months.

To John Chesholms, of Edinburgh, for his method of constructing register and other stoves.—21st Aug.—2 months.

To Thomas Field Savory, of New Bond-Street, in the county of Middlesex, chemist, for his combined neutral salt or Serdletz Powder, which possesses all the properties of the medicinal spring at Serdletz in Germany.—23d Aug.—2 months.

To Robert William Bemman, of Eldersfield, in the county of Worcester, farmer, for improvements in ploughs.—23d Aug.— 2 months.

To

To James Carpenter, of Wellenhall, in the county of Stafford, currycomb-maker, for his improvement to a curry-comb by inverting the handle over the back of the comb, and thus rendering the pressure when in use more equal.—23d Aug.—2 months.

LECTURES.

A Course of Lectures on Chemistry will be commenced at the Chemical Theatre, 42, Great Windmill Street, on Tuesday the 3d of October, at Nine in the Morning, by W. T. Brande, F.R.S. L. & E. Prof. Chem. R. I.,&c.

The Lectures on Midwifery at the Middlesex Hospital, by Dr. Merriman, Physician, Man-Midwife to that Hospital, and Consulting Physician, Man-Midwife to the Westminster General Dispensary, will recommence on Monday, October the 9th.

School of Medicine in Ireland.—The Winter Courses of Lectures on Anatomy, Physiology, Pathology, Surgery, Chemistry, Materia Medica, Institutes and Practice of Medicine, will commence on the 6th of November at their respective hours.

Anatomical Demonstrations will commence the 1st of December.

TO CORRESPONDENTS.

However just the remarks sent us by "A SHIP-BUILDER" may be, they would have the appearance of personality on the part of the Editor, who never, but by inadvertency, suffers any thing that can be considered acrimonious to appear in his pages, and certainly not without the author's real signature. When gentlemen subscribe their names, the Editor considers them the best judges of the language it may be proper for them to employ; but the case is otherwise when the Correspondent is anonymous.

Mr. CARTER'S Communication respecting Machinery applicable to Steam-Boats should have been sooner noticed. The arrangement which he has proposed is not new; but as it is not generally known, we hoped to have been able, before this time, to have spared a corner of one of our plates to illustrate it.

E. S. On Meteoric Stones;—Mr. Spencer On the comparative Specific Gravity of the Human Body and Sea Water;—and H. on Metallic Salts, are deferred for want of room. They shall appear in our next Number.

Meteorological Observations made at Tunbridge Wells, from August 30 *to Sept.* 17, 1815.

August 30.—Fair, *cumuli*, &c. (at Tunbridge Wells.)

August 31.—*Cirrus* and *cumulostratus; cirri* and *cirrostrati* richly coloured in the evening.

Sept. 1.—Hot bright day, with clear distances, and fine ephemeral *cumuli* in one plane.

Sept. 2.—Fair day, with much *cumulostratus.* Beautiful evening with *cirrocumulus, cirrus,* &c.

Sept. 3.—Warmer even than yesterday, as is usual after *cirrocumulus. Cumuli, cirri,* &c. A breeze with *cirrostratus:* at night small meteors falling through clear sky into *cirrostratus.*

Sept.

Sept. 4.—Clouded early, then clear with *cumuli* below *cirri*, and windy from WSW.

Sept. 5.—Fair day, somewhat cooler, and various clouds. Calm evening, and very fine indeed: features of the different modifications not strongly marked. The sky exhibited in places a fire.

Sept. 6.—Cooler air still. Clear morning with faint features of *cirrostratus*; *cumuli* in the day. The white and partial *strati* which I saw creeping up the valley at sunrise, as I rode to Berwash, gave place afterwards to greater mist, which made the horizon obscure. Fine evening, and some red in the sunset.

Sept. 7.—Fine day; *cumuli* prevail. N. wind. A fair, still evening, with *cirri*, and a fine deep red refraction at sunset and for some time afterwards.

Sept. 8.—Fine morning when *stratus* cleared off. Clear sunset and red-coloured horizon. At night small meteors.

Sept. 9.—Fair dry day, with northerly wind, and some *cumuli*. In the evening, features of the other modifications. At sunset, a fine red blush marked by diverging red and blue bars.

Sept. 10.—Fair and almost cloudless, save a few *cumuli*. Fine clear sunset; the horizon about the west a beautiful red, fading into yellow afterwards.

Sept. 11.—Fair but misty horizon, as of late; in the morning *cirrocumulus* appeared early. I saw features of *cirrostratus* in large bars below it: *cumulostratus* and indication of thunder, with increased warmth. It cleared off, and we had a fine calm evening, with mixed and coloured clouds.

Sept. 12.—Warm clear day, with *cirrocumulus*, &c.; red sunset.

Sept. 13.—Fine clear dry day again, and quite without a cloud all day. The heat increased, but there were light gales from SE.

Sept. 14.—Fine clear hot day with few clouds, and a beautiful sunset; some fleecy *cumuli* by moonlight.

Sept. 15.—Very hot day and clear, except a few *cirri*, &c. and some small *cumuli*; dry SE wind.

Sept. 16.—Hot morning with a light shower early; through the day *cumulostratus*, &c. increased, and we had gentle showers at night.

Sept. 17.—Hot fair morning, *cirri* and *cumuli* ill-defined.

THOMAS FORSTER.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND.

For September 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Aug.	27	66	75	60	29·90	57	Fair
	28	60	69	66	·91	30	Showery
	29	55	66	54	·98	52	Fair
	30	56	68	57	30·10	56	Fair
	31	54	70	55	·12	57	Fair
Sept.	1	55	69	54	·10	60	Fair
	2	54	68	53	·02	55	Fair
	3	58	67	56	·03	50	Fair
	4	54	66	63	·03	56	Fair
	5	53	66	50	·04	58	Fair
	6	49	62	54	·10	59	Fair
	7	47	63	50	·19	46	Fair
	8	46	64	54	·18	45	Fair
	9	52	69	57	·20	52	Fair
	10	58	67	59	·21	50	Fair
	11	59	69	58	·21	56	Fair
	12	60	71	60	·06	60	Fair
	13	60	77	62	29·97	62	Fair
	14	55	73	66	·86	60	Fair
	15	62	78	64	·82	63	Fair
	16	61	74	57	·83	60	Fair
	17	56	68	55	·92	56	Fair
	18	55	70	63	30·01	58	Fair
	19	60	71	65	·12	57	Fair
	20	55	62	54	29·99	51	Fair
	21	54	69	56	·94	62	Fair
	22	56	69	52	·70	54	Fair
	23	56	66	54	·72	56	Fair
	24	57	60	54	·70	0	Rain
	25	56	59	54	·84	33	Fair
	26	57	64	52	·72	30	Stormy

N.B. The Barometer's height is taken at one o'clock.

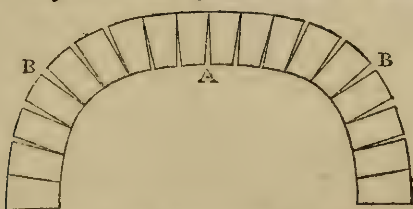
XLIII. *On the Principles of Arches.* *By* A Correspondent.

To Mr. Tilloch.

Sir,—At a time when so many large bridges are building, a few objections to the principles of arches, as exhibited by the writers on that subject, may perhaps be of some service.

Dr. Hutton has stated, in his Tracts, vol. i, that there are three theories; and that they are all the same in effect, and yield the same conclusions: but we will suppose there are only two; that is, the wedge theory, and the theory of Emerson, for the Catenarian theory is the same in fact with Emerson's theory, and is liable to the same objections.

We think the most likely method of ascertaining the manner in which the forces act, is to examine an arch that has changed its form in consequence of being overloaded either at the crown or haunches. If the wedge theory is correct, the arch-stones would slide on each other, and the stability of an equilibrated arch would wholly depend on the friction of the surfaces in contact and the cohesion of the cement. But when an arch is so loaded as to press it out of its original form, the arch-stones do not slide on each other, neither is there any thing like friction takes place*: for instance; suppose the weight to press on the crown, then the joints would open, as shown at A, at the same



time the joints at the back of the arch would open as shown at BB: this is what every one who has paid any attention to the subject must have noticed.

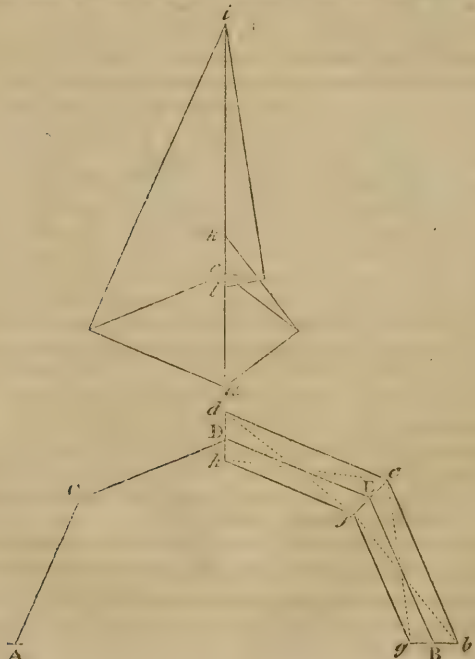
If the arch-stones were perfectly smooth, they might perhaps act as wedges; but this is a mere hypothetical case, and has nothing to do with the present inquiry: but suppose the arch-stones were connected with a cement which would unite them as firmly at the joints as the solid part of the stone; it is evident the arch would be stronger, but we are at a loss to conceive how the wedge theory could be applied to such an arch: yet, if loaded in a similar manner, the fractures would take place at the

* A late writer on bridges thinks that an arch may be balanced by the friction of the joints alone, by giving them different degrees of inclination.

same points as they would if the arch-stones had been joined with common mortar.

The Emerson theory supposes the arch-stones to be inflexible lines, which are balanced by vertical weights pressing on the angles of contact; or, what amounts to the same thing, the depth of the abutting joints of the arch-stones is not supposed to be of sensible magnitude. Of this we may say, what Dr. Hutton has said of the wedge theory, that "it is founded on a supposition which is neither in nature nor art, and which can never take place in any real construction of an arch." The Emerson theory certainly furnishes no practical rules for construction, neither for the depth of the arch-stones nor for connecting them together so that they may be less liable to change their positions: but this is not the only defect it has, for it does not determine the form of an arch which would be equally strong throughout.

We will take a simple case: let four equal and inflexible bars, in the same vertical plane, be kept in equilibrio by weights laid upon the angles, the depth of the abutting joints being very small, and the points A and B fixed. Let the weight at D be



represented by ac; then, lines being drawn parallel to the directions of the bars, ci will represent the weight on the angle C or E.

Now, suppose the depth of the joints to be equal dh, and let the weight at D be the same as before; then, if the load at D was sufficient to disturb the equilibrium; the bars would turn on the points d, f, and b; therefore, we find the force at E, that would balance the pressure at D, is measured by ck, and the stability of the system will be as the difference between ck and ci equal ki.

If the load at E, equal ci, was sufficient to disturb the equilibrium, it is evident the bars would turn on the points h, e, and g; then the stress at E being ci, the weight that would balance it at D, is cl; and the difference between cl and ca, equal la, will express the stability: but had the system been uniformly stable, the differences should have been equal: Therefore, a system which would be in equilibrio were the joints without sensible depth, is not of uniform stability when the depth of the joints is of some magnitude.

We conceive that a bridge ought to be so constructed that it may resist the action of a weight moving along it, with an equal force at any part of its length; and the greatest stress that could possibly come upon it ought not to produce a sensible effect on the arrangement of the arch-stones. Equilibrium, if we understand the term, signifies a balance of parts which would be deranged by the slightest pressure partially applied; therefore, the form that gives equilibrium to a bridge, a roof, or a dome, is the worst that can be adopted.

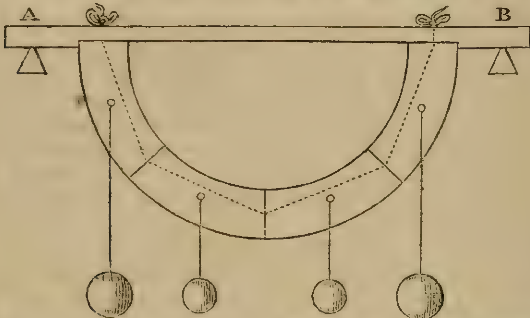
There is yet another objection we have to make, and that is to the method of finding the thickness of the piers. The piers ought to be considered as a part of the arch, and the stability of the whole should be investigated at the same time; for the strength of the piers must be computed from the forces acting at the places of fracture, which is not always at the springing of the arch.

It would be easy from what has been said, to determine the stability of any given bridge, for the place of fracture might be nearly ascertained by inspection; and find the centre of gravity of each of the segments into which the joint of fracture divides the semi-bridge; then the stability both at the crown and the haunches might be determined. For finding the directions of the forces, see Mr. Southern's paper, Philosophical Magazine, vol. xli. p. 321.

A bridge formed on the principles of uniform stability would certainly be the most scientific; but considerable deviations from that form may be made, without any other bad effects than those

those of using a greater quantity of materials, and rendering it more difficult to secure the foundations.

The following method may be used for determining the stability of an arch experimentally; and also to illustrate the principles of arches. Let a piece of wood be cut to the shape of the ring of arch-stones, if it be a semi-circle, let it be divided into four equal parts, and make holes through them to meet each other at the joints, connect them to each other, and to the bar AB, with an extensible thread passing through the holes;



invert the arch, and let weights, proportional to the pressure of materials on each part, be suspended from its centre of gravity*, then the additional weight required at any part to cause the joints to open will be proportional to the stability. The whole might be cut into the shape of arch-stones, and weights hung to each piece; but this causes the experiment to become too complicated.

It is not necessary that the arch should be inverted; for if it was prevented from giving way at the ends A and B, it might be placed over an opening, and the weights suspended by lines from the centres of gravity as before. This last method might be employed with advantage to explain the principles of arches to young students.

We must not omit to observe, that the centre of gravity of the superincumbent load will not be in the same vertical line with that of the segment of the arch; but the weights ought to be suspended to a point that would be the common centre of gravity of both the arch and the load above it.

I am, sir,

Your most obedient servant,

London, Sept. 25, 1815. T. T.

* The centre of gravity of the pieces may easily be found by the mechanical methods described in Gregory's Mechanics, vol, i. p. 50.

XLIV. *On*

XLIV. *On the Origin of Meteoric Stones.*

To Mr. Tilloch.

Sir,—M. La Place with many other philosophers have been of opinion that meteoric stones were masses ejected from the moon; and the chief objections urged against this theory are, that their appearance is invariably attended with a swift horizontal motion and violent explosion. But if it can be proved that under certain circumstances these effects must necessarily follow, the theory will receive an interesting and material corroboration. These stones, it is well known, are composed almost entirely of silicia, magnesia, oxide of iron and nickel, partly metallic and partly oxidized. Now modern discoveries have proved silex to be an oxide formed by an unknown inflammable base which by its extreme avidity for oxygen becomes instantaneously saturated with it. Sir H. Davy has demonstrated that magnesia is also a metallic oxide, and not without great difficulty separable from its base. Respecting nickel and iron, we know that the former can only be oxidized, and then but slowly, by exposure to atmospheric air at an intense heat, and that the latter becomes more or less oxidized according to its quality and degree of exposure to air.

Here then we have a solid body composed of materials all susceptible of oxygenation, and in that state in which we might naturally expect to find them if, previously existing in their original purity, they had been suddenly exposed to the action of oxygen. In addition to these, let us suppose the admission of any of the alkaline metals. What would be the result if a mass thus composed were to be thrown from the moon with a force sufficient to come within the range of the earth's attraction?—It would be precisely that which is experienced in the case of a fall of aërolites. The body would pursue its rapid course, and by the laws of motion be partly impelled in an horizontal direction. On approaching our atmosphere the effect upon the component molecule would be immediate. The unknown inflammable base of silex would absorb its oxygen, and appear in the state in which it now exists. A similar effect would be experienced by the magnesium, which would become magnesia. The nickel would from its partial affinity for oxygen be only partly oxidized by the heat evolved during the detonation of the alkaline metals, and the iron be so in rather a stronger degree, and chiefly on its external surface where it is most exposed, which we find to be the case. The alkaline metals would rapidly combine with oxygen; and as heat would naturally be increased by the velocity of motion, a violent explosion attended with brilliant flame

flame would be the consequence*: and the heated fragment would be precipitated in all directions.—We have only to conceive then that oxygen forms no part in the œconomy of lunar formations; and such would undoubtedly be the effects if a body thus composed once fell within the limits of our atmosphere. With respect to the alkaline metals, it may be observed that they can only be known here in a state of high oxidation; but we may easily conceive their existence in planets, as abundantly as iron, nickel, and other metals, provided there is no oxygen to form its combinations;—and however important this subtile substance appears to be in the operations carried on in our globe, we are not justified in concluding it to be requisite in other systems:—on the contrary, there are arguments for concluding that in the moon oxygen does not exist. It has for instance been generally admitted that she is destitute of an atmosphere, or possessed of one infinitely rarer, and therefore probably differently composed from ours; and many accurate observers have on good grounds doubted the existence of water. It is almost unnecessary to remind our readers that air and water depend upon oxygen for their formation. It is true that the semblance of fire has been observed in active and incessant volcanoes; but as caloric, heat, and light are all independent properties or substances, it by no means follows that oxygen should be the only supporter which Omnipotence is capable of furnishing for the act of combustion.

E. S.

XLV. *On Metallic Salts. By* A Correspondent (H.) *in Reply to the Answer of* G. S.

To Mr. Tilloch.

Sir,—I flatter myself you perceived my paper of June last was merely an opinion founded on a few experiments; and that my ambition and endeavour was to promote, if possible, and simplify the science, by forming that which, if correct, cannot fail to afford chemists, in a general rule, an acquisition not undeserving their attention.

Your correspondent, in his attempt to confute me, leads you so far from the purport of my paper, by changing the word *excess* for *free*, that were I for an instant to conceive it to be intentional, I should put an end to a controversy so unfairly carried on.

That some of G. S.'s assertions [see the July number] are in-

* See the various experiments upon potassium on exposure to oxygen.

correct,

correct, and that he had never tried that which he gives as a fact, will appear by experiment. The greater proportion of free acid, so far from rendering a metallic salt more soluble, will in most instances precipitate it;—this also appears to prove the watery solution to be a super-salt; for were it held in solution merely by free acid, acid in any proportion could not precipitate it,—otherwise each acid would become both the solvent and precipitant of its corresponding salts. To me the solution appears to depend entirely on the water. The nitrates of lead and silver are precipitated by nitric acid, the nitrates of iron, zinc, and copper by sulphuric acid, in the state of sulphate; whereas if nitric acid is dropped into a solution of a sulphuric salt, no precipitate is formed, as the water required to dissolve it is sufficient even after the addition of nitric acid to hold it in solution; but the nitrates of these metals being more soluble than the sulphates, do not afford, when sulphuric acid is added, sufficient water to keep the sulphate in solution; and these precipitates may be alternately taken up by water and precipitated by acid—nevertheless G. S. insists the acid is the solvent.

"Metallic salts are generally, if not universally, more soluble," says your correspondent, "according to the greater proportion of free acid." By this it will plainly appear that his experiments must have been extremely confined, or that the inaccuracy of them hurried him into error; as it is the deliquescent metallic salts which will not precipitate on the addition of acid; as on the contrary, permanent salts are precipitated by it. Therefore G. S. either made his experiments on deliquescent salts alone, or used dilute acid, or dilute solutions. He cannot then with propriety assert the solution depends on free acid, where water is present, and that in a greater proportion than in acids of the usual specific gravity.

Your correspondent says, He cannot conceive any period during the formation of a salt, that the action towards chemical union would cease, whilst a portion of both its constituents remain in contact and uncombined. This I believe is the general opinion: let me then draw your attention to the process of forming sulphate of iron, &c. We find in this, that after the action ceases, the solution by test is acid, although a quantity of the iron (if sufficient were introduced) still remains undissolved: therefore, as he asserts, the acid in the solution is free, that is uncombined—the uncombined acid is consequently by his rule in contact with the iron, &c. without acting on it; and this he protests cannot be the case.—Now, were it allowed that the excess of acid, instead of being free, was combined with the metal to form a salt, then it would be natural to conclude the metal would remain untouched, on account of its being subject to the influence of a super-salt, and not to an acid.

I have always conceived compact and regular formed crystals, far from containing any extraneous matter, do in that form exist in the purest state: but this your correspondent contradicts, when he observes, The excess of acid is free, and merely essential to solution. Let him then break a large crystal of sulphate of iron, &c. and apply his test for acid; and if it answers, I hope he will allow me to class it among the super-salts, and not tell me that this acid is also free. Most of the earthy and alkaline salts* are neutral, and are capable of being separated from acidulated water by crystallization. This, as may be seen above, is not the case with metallic salts: but many of their properties agree so far that I shall at some future period trouble you with an account of them.

According to G. S. I should have to apologize for troubling you with a subject already treated of by so able a chemist as Fourcroy. My aim was, to prove the state in which the acid exists in metallic salts. Fourcroy, as I have since found, observes there is always an excess of acid in metallic solutions, and does not conclude this to be essential to them. I therefore conceive your correspondent has equally failed in his endeavour to detract from the originality of my opinion, and in his attempt to condemn my conclusions as fallacious and contradictory.

Your obedient servant,

Sept. 16, 1815. H.

XLVI. *Comparative Gravity of the Human Body and Sea-Water.* *By* Knight Spencer, *Esq.*

To Mr. Tilloch.

Sir,—The fact that the human body is, bulk for bulk, lighter than sea-water has been long known, and may be easily proved by any one who has confidence to throw himself upon his back in the sea, and there lie at full length with arms extended, but without the least motion. I do not, however, remember to have seen it any where recorded, *how much* the human body is lighter than sea-water.

With a view, therefore, to ascertain this point, I have lately taken the opportunity of a smooth sea to throw myself on the surface with flint stones in each hand, my arms extended, and I

* As my opinion has been called in question, I shall not presume to term them *super-salts* till the fact is determined.

found

found myself still on the surface and quite at my ease, with six pounds avoirdupois in all. I had myself weighed immediately, and found I weighed 130lbs. avoirdupois.

The knowledge of this fact may perhaps give confidence to some of your readers, who may be thrown into that situation where confidence alone will preserve life.

Should you deem this experiment worth recording, you will oblige me by giving it a place in your Magazine.

I am, dear sir,

Your obedient servant,

Surrey Institution, Sept. 14, 1815. KNIGHT SPENCER.

*** Will thank our correspondent if he will have the goodness to inform us, in a brief note for our next Number, whether the flint stones were held under or above the surface of the water.

XLVII. *Observations upon Wheel-Carriage Experiments, submitted to the Consideration of the Dublin Society*. By* TOWNLEY RICHARDSON, *Esq.*

Experiments, &c.

Exp. I. COMMON car against a Scotch dray.

Exp. II. To determine whether the application of springs is advantageous to the draught of carriages.

Two carriages exactly similar, and both of them upon springs, will be tried against each other, one having the springs prevented from acting—the other having the springs at liberty.

Exp. III. That carriage, which appeared to have the advantage in Experiment the 2d, will be loaded till it nearly keeps pace with the other carriage.—The difference of weight thus carried will show the relative advantage of each carriage.

Exp. IV. The 3d Experiment will be repeated, changing the situation of the carriages, so that the different draughts of the carriages may not appear to have been occasioned by the different roughness or smoothness of the track on which they ran.

Exp. V. The relative advantages of short and long perches will be compared.—Two carriages, similar in all respects, will be compared; and afterwards the same carriages, but with the perch of one of them lengthened, will be compared.

Exp. VI. The relative advantages of high and low carriages will be compared.

Exp. VII. Two one-horse drays, similar in all respects, ex-

* Printed by Order of the Dublin Society.

cepting that one is supported upon wooden springs, and the other without springs, shall be compared, and the relative advantage of their draught shall be ascertained.

Observations upon Wheel-Carriage Experiments, conducted by RICHARD LOVELL EDGEWORTH, *Esq. at Leinster House. On the* 22*d April,* 1815. *Agreeably to the Arrangement of the annexed Syllabus printed for that occasion.*

Experiment the First; see the preceding Syllabus.—The practical experience of all counties of Ireland, for many years, has happily proved the superiority of the dray or cart, excepting where bye-roads are too narrow for carts, or too uneven for any carriage but such as have their wheels so fixed, that the axle and wheels must turn together in the manner of the older Irish car, a construction very well suited to the state of the roads in the age in which it was invented:—this experiment was neglected, I suppose, as unnecessary.

Experiment II. III. and IV.—These three experiments I shall consider as one between two carriages to determine the effects of springs.

That advantage has been derived from the use of springs to carriages, with respect to lessening their draught, has been practically proved by many years experience in almost all countries. Carriages for burden on two wheels with springs are commonly used in the populous parts of England; in London hundreds of such carts are daily to be met with. It would however be important to ascertain the precise weight of additional loading that can be carried in this manner; *and if the subject were to be considered more immediately in an agricultural point of view, it would be judicious to determine what sort of spring is best calculated to answer this purpose, and best able to bear the rough treatment of careless and ignorant labourers—but here I am exceeding the bounds of the syllabus before me.*

Prior to drawing any conclusion, from the occurrences of the 22d of April, with respect to the use of springs, it is incumbent upon me to endeavour to calculate the effects of the unobserved circumstances, that appear to have baffled the exertions of the gentleman conducting those experiments, to whose patriotic energy for many years the public is much indebted. It is reasonable to begin with a scrutiny of the nature of the unnecessary weight of the unwieldy bodies of the carriages chosen in this case, and of their other weights, in my opinion, very erroneously called equalizing weights.

It appears from the subsequent statement of the weights of the several parts of the carriages, that these unwieldy bodies and

and their cumbrous appurtenances are more than ten times heavier than they should be, in proportion to their wheels and axles; an inconsistence sufficient to destroy the useful effect of any experiment. The body of the common cart or dray, used in the last of these experiments, is not the one-third of the weight of its wheels and axle; whereas these deceptive bodies, as prepared, are above three times heavier than their wheels and axles—Here is a disparity in proportion of above ten to one. For the sake of illustration, and to enable me to draw reasonably any conclusion from this experiment, I shall consider all weight beyond that of the common dray as so much loading on both of these peculiar carriages, unsuitably chosen, where the advantage by use of springs was to be estimated in proportion to the weight drawn. It will appear agreeably to my subsequent calculation, that at the time these carriages were supposed by Mr. Edgeworth to be empty carriages, and perfectly similar, they were in reality loaded carriages, of different weights, of different burden, and differing essentially in construction. It will appear, that the previous loading, consisting of the unnecessary weight of body and weights called equalizing weights, of the one was 5 cwt. 1 qr. 23 lbs. to which five hundred weight was added, and called its loading, together making 10 cwt. 1 qr. 23 lbs. and the previous loading of the other was 5 cwt. 0 qrs. 13 lbs. to which was subsequently added, under the denomination of loading, 6 cwt. 1 qr. 0 lb. making together 11 cwt. 1 qr. 13 lbs. exhibiting a difference of 3 qrs. 18 lbs.

These carriages being supported in front by men, during the experiment, renders the result very uncertain.

It was observable that their metal loading was not confined; there might therefore be much weight, to be supported by the men in front of one, and much force of a contrary nature, to be overcome by the men, in front of the other, as in the instance of the shafts of one of these carts raising two men many feet from the ground, during its progress; weights for such a purpose should be so fixed as to be unalterable by the motion of the carriages.

The axles of the carts in question were conical; their apexes or extremities were set much below a line horizontal with their bases respectively.

Mr. Edgeworth in his work upon Roads and Wheel Carriages, page 74, describing the nature of a conical axle bended downwards, says, "Whoever has observed the hinder wheels of a carriage, that has the axle much bent downwards, will perceive a tremulous motion of the whole wheel, which arises from its truly circular motion being counteracted by the conical motion, to which it is disposed by the bended axletree."—I am not aware of what is meant here by the conical motion of the wheel; the

proper

proper ties of the cone do not appear to me to be in any manner concerned in the cause of the motion alluded to.

The centre of the box is supposed to be in the centre of the wheel, and to be fitted to the axle, whether it be a cone or a cylinder; the wheel is confined between the shoulder on one side, and the nut on the other, on the axle in both cases, against either of which the wheel will work with increased friction, according to the directions of the forces applied. This tremulous motion can at any time be produced upon a paved or hard and uneven surface, by the wheels not being parallel, or by being obliged to go forward, with the carriage, in directions not in the respective plains of their circumferences, whether the axle be a cone or a cylinder.

In either of these cases there are lateral, and not conical motions imposed upon them, which upon pavement become injurious and particularly conspicuous—upon a smooth road less so, but increase much the difficulty of draught upon a soft road, in proportion to the depth to which the wheels may happen to sink. The position of such axles as used in this experiment, and necessarily the position of the wheels, is ever governed by the elevation or depression of the shafts in a two-wheeled carriage,—had their axles been cylindrical, and placed horizontally, these difficulties would not have existed. The wheels of the car, that drew the greater load, were several inches higher than the other, and the flexibility of its springs was very great, while those of the other car were strong enough for the largest mail-coach. The experiment would probably have been less rough, had the carriages been previously weighed, an indispensable requisite upon such an occasion, and the springs of both cars rendered inflexible during the operation, that was supposed to equalize them; but their equalization, as attempted, was quite impossible to be adequately accomplished, while they differed so materially in the essential points, viz. flexibility of springs, height of wheels, and difference of weight, independent of any irregularities caused by the use of men in front.—The superiority, that would have appeared in one carriage by pliability of springs and height of wheels, was countervailed by the embarrassing weight of its body, and weights called equalizing weights, together weighing eighty-eight pounds more than the yellow car with its embarrassments. The equalization of the cars in the manner, made use of for this experiment, is a case somewhat similar to the deceitful balance. See Helsham's Lectures.

I am indebted for a knowledge of the weights of these carriages to the kind accommodation given me at the weighing machine of the Farming Society of Ireland, a few days subsequent to these experiments, since which time, I understand the carriages

riages have been weighed by Mr. Hutton at his yard, a difference appears between the two weighing machines of about one quarter of a hundred in seven hundred weight; as this may have arisen from difference of weather, or from street dirt being upon the carriages at the time of weighing, or from Mr. Hutton's machine being intended for weighing greater weights, I have in consequence taken the trouble to prove, by stamped weights, the correctness of the Farming Society machine; and, on again weighing the yellow car, I find it seven pounds lighter than before,—a few hours dry weather may have produced this change. I shall however make use of the weights taken in the first instance, as those nearest to the weights at the time of the experiment.

List of Weights.

	Cwt.	qr.	lb.
Weight of the yellow car without equalizing weights	7	1	7
The wheels of this car	1	1	0
Axle	0	1	22
Springs and shackles	1	1	0
Weights called equalizing weights fastened to this car	1	0	0
Weight of green car without equalizing weights ..	7	2	11
Wheels	1	1	14
Axle	0	1	22
Springs and shackles	1	0	0
Weights called equalizing weights fastened to this car	1	2	0
A body of common Scotch dray of Society yard ..	1	0	18

I shall now endeavour further to unravel and calculate the effects of the misleading circumstances, that escaped general observation, and to simplify them so as to bring them fairly into view, and under such denominations as they are fairly entitled to. The weight of the wheels and axle of the yellow car appears to have been 1 cwt. 2 qrs. 22 lbs. which being deducted from its total weight leaves the weight of its embarrassing body, with its weights called equalizing weights, 6 cwt. 2 qrs. 13 lbs. The weight of a body of a common Scotch dray of the Farming Society yard is 1 cwt. 0 qr. 18 lb. The embarrassing body, &c. is therefore 5 cwt. 1 qr. 23 lbs. more burden than the body of the common Scotch dray, which for the purpose of elucidation I will now suppose with that weight upon it, and placed upon the wheels and axles of this car; to this weight was subsequently added five hundred weight, called loading, making together 10 cwt. 1 qr. 23 lbs. as before mentioned. The weight of wheels and axle of the green car is 1 cwt. 3 qrs. 8 lbs. which, deducted from its total weight, leaves the weight of its embarrassing body, and weights called equalizing weights, 7 cwt. 1 qr. 3 lbs. the weight of body of a Scotch dray of Society yard, and springs and shackles

shackles of this green car, is 2 cwt. 0 qr. 18 lbs. the embarrassing body, &c. is therefore 2 cwt. 0 qr. 13 lbs. heavier than the body of Scotch dray, with springs and shackles of this car; to this weight was added six and a quarter hundred weight, called loading, making together 11 cwt. 1 qr. 13 lbs. The real nature therefore of this experiment is fairly stated, when described as one carriage without springs, having

Cwt.	qr.	lb.	
10	1	23	upon it; another with springs, carrying
11	1	13	or additional loading, by use of springs,
	3	18	about the eleventh part of the load, *pro-*

vided the inaccuracies with respect to the use of men, the difference in size of wheels, and the impossibility of equalization by the mode resorted to, had not rendered the experiment indefinite. The weight of loads as here calculated, when considered proportionally to the weights of the carriages drawing them, is pretty similar to the common cart or dray of six or seven hundred, drawing a load of twenty or thirty. Had such an experiment as this been tried upon a very bad and rutted road, I am inclined to think that much useful information would have been received by the public.

Experiment, No. 5, see annexed syllabus.

Here, as before, I have to point out unobserved dissimilarities. I have not had any opportunity of weighing these carriages—they were said to be of equal weights, and exactly similar in construction, *but for the purpose of equalization, as mentioned in Report, it was necessary to make the one heavier than the other;* this would appear paradoxical; it has however arisen from a mistake—they did not exactly or reasonably correspond in their construction—the one had the advantage of flexibility by having fewer and weaker plates in its springs; this dissimilarity caused the necessity of increasing the weights upon it; they therefore differed in weight, and in springs, and also in various minor though necessary considerations externally visible. By the pliability of the slender perch when lengthened, too slight to be used in ordinary, a further advantage was given to this carriage, but without much apparent operation; it may, therefore, not very unreasonably have been suspected, during the experiment, that the length of the perch was the countervailing disadvantage. I will not however draw this or any precise conclusion with respect to the facts singly or collectively—they are all too indefinite, the previous arrangements, particularly with respect to the surface used in this case, do not appear to me to have been such as to show the nature of a long perch comparatively with that of a short one. I am of opinion that a long perch

perch has several advantages, and a short perch many serious disadvantages, from causes which have not yet come before the public, either by experiment or in print; one cause will however be considered, when observing upon experiment No. 6 in syllabus. It will there appear, I presume, that similar disadvantages with respect to draught may be produced by shortening the perch, or by the elevation of the centre of gravity.

Experiment No. 6, see annexed syllabus.

The question of advantage or disadvantage, by the difference of height of the centre of gravity of a loaded carriage, involves the necessity of considering the effect of the increased velocity and distance of its motion upon a laterally inclined road, a road of many ruts, or of short ascents or descents: this motion I would presume to be seriously diadvantageous, and it is increased proportionately with the height of it. The experiment, as I understood, was supposed to show that variation with respect to height made no difference as to facility of draught or otherwise. The surface used in this case was a tolerably smooth pavement; had it been a very bad road, with various lateral inclinations, I am inclined to think, for the reasons following, that the elevation of the weight to the height, which is very common practically, would have appeared to be seriously injurious.

By the first effect of a sudden lateral inclination all the wheels at the same moment have a force acting upon them in a direction nearly at a right angle with the direction of the forces applied by the horses; a worse effect may be produced by ruts alternately at either side, the velocity of the centre of gravity, and the distance passed through by it, must in these lateral motions increase in proportion to its height, and thereby it will swing with additional violence from side to side. Again, when the front wheels of a carriage fall together into a hollow or rut, the weight upon that axle becomes increased proportionally to the diminution of the length of the perch, and the height of the centre of gravity; in this case of the short perch, the angle formed by it with a horizontal line will be nearly double in a rut of a given depth what it would be, if the perch were of double the length. The same increase of weight acts upon the hinder axle when in the rut or hollow; some proportion of these disadvantages may be countervailed by the axle, that is out of the rut, having less weight upon it than in ordinary; notwithstanding which, I have no hesitation in thinking that it would be judicious, by lowering the centre of gravity or by lengthening the perch, to avoid increasing the weight upon the axle in the rut, because in the one case the weight is to be lifted upwards, and in the other to be drawn horizontally. The general impression of increased power, by shortening the distance between the horse and

and the axle, is perfectly correct with respect to either a four- or a two-wheeled carriage; because a horse can apply more of his strength, when his traces make an angle, than when they approach more to the horizontal,—but it has not any bearing upon the question of the length of the perch or means of bringing forward the hind wheels, because the direction of the force applied to them is at all times governed by the parallel of the road and position of the carriage. There are other more important considerations, particularly with respect to mail-coaches, that would be too tedious to enter into upon the present occasion. Advantage would be derived, if it were possible to have the weight so low as to act with less power upon the axle in the rut: and, although this fact can be shown mathematically or experimentally by an apparatus for the purpose, yet from the impracticability of it, with respect to roads, I shall not dwell upon it.

Experiment, No. 7, see annexed syllabus.

I am not aware of any difference in the object of this experiment from that of number 2, unless it be expressed by the use of the word *wooden;* but as no comparison was made between wooden and steel springs, which in an agricultural point of view I very much lament, I shall not enter into that question, but merely observe upon the facts in the manner they took place. I am sorry that here, as in the former cases, I have to begin by pointing out dissimilarities in the essential points.

The equalization of these carriages, that was mentioned to have previously taken place in private, must, as in the case of experiment No. 2, have been deceptive for reasons somewhat similar, as follows;—

The quantity of weight acting by the shafts upon the wheels, that supported them in front to supply the place of horses, was unascertained. Lifting by hand the points of the shafts, as attentively done by Mr. Edgeworth, jun. was deceptive, because in so doing he was obliged unavoidably to lift the metal machinery and front wheels. The shafts of Mr. Edgeworth's cart, being supported by a larger wheel, were seven inches higher than those of the other cart, which in front was lower than in rear: this circumstance was a serious disparity, because their axles were bended—two metal weights were previously fixed by cords at the fore sloat of this cart, under the denomination, I suppose, of equalizing weights; placing these weights so far forward had simply the effect of increasing the disadvantages this cart already laboured under, particularly by the inferiority in size of its front wheel.

It is further to be observed, that the carts started from situations pretty level with each other: but at the termination or end of the track, a point twenty yards from the western wall of the yard,

yard, and ten from the south side, the cart to the southward was very much lower than the other, a fact that is particularly well worth attention. They should therefore have been permitted to return upon their respective tracks, as this would have shown the disparity in point of surface; or had they been brought back to the east side and again started, having changed places, the difference with respect to road would likewise have been ascertained. The carts, with the view of counteracting the effect of any disparity of road, were erroneously made to change places at the west end of the track; but this was as deceptive as before, because the cart upon the south side, that had the advantage of fall of ground in its first progress, was again given the advantage by being placed to go back upon the north side horizontally, while the other, now put to the south side, was obliged to go back, ascending the rising ground.

These circumstances with their various dissimilarities, and the unsuitability of an experiment, where the weight of the loading was inferior to the weight of the cart, and that a heavy cart supported upon light springs unfit for a weighty load, render it impossible, in my humble opinion, to collect any accurate or useful information from this experiment. Carts of the weight and strength, in this case used, commonly draw a ton or thirty hundred weight.

I hope I may not be thought presumptuous, when I say that the construction of wheel carriages, particularly with regard to the difference of the effect of weight of body and weight of wheels, questions distinct that have hitherto been confounded, and the application of the force of horses to carriages, are all of them subjects more unconnected than is commonly supposed, and very much in their infancy with respect to public knowledge. Many practical errors have of late years been discovered, and much useful instruction is no doubt yet to be received.

I cannot help believing, although with very much deference to the high authorities upon this subject, that there is an error in considering wheels in their ordinary application to carriages as levers. Mr. Edgeworth, in his work upon Roads and Wheel Carriages, page 75, considers them as such. He mentions two questions having taken up much time of a committee of the House of Commons of England, that sat for ten years upon the subjects of roads and wheel-carriages. The one was, "Do the spokes of a wheel on a level road act as levers?" The other, "What has been the cause, and what is the use of dishing wheels?" To this latter question every one can give the answer. I cannot, however, with respect to the former question, agree with Mr. Edgeworth when he says, "Whoever looks at a wheel going over an obstacle, must see that the spoke and part of the fellow

fellow form a bended lever." This does not appear to me to be the case. A third point of force I conceive to be essentially necessary to form a wheel or any other body into a lever; this is sometimes accomplished by a man applying his force to lift the back part of a wheel, while the horse is perhaps endeavouring to draw the carriage out of a rut. It does appear to me, that a wheel as in ordinary to a carriage, does not resemble a lever of any class, for the reasons following. All forces acting upon the axle, either by gravity of the carriage or by any variety of propelling forces, are resolvable into one force acting upon a point within the wheel. All forces acting against the circumference by obstructions or unevenness of the surface, upon which the wheel moves, are also resolvable into one force acting upon one point. We have now two forces acting against each other in an inflexible line. When these forces are in direct opposition to each other, the wheel stands still; when they become so indirect as to overcome the friction, the wheel moves forward or backward according to the direction of the impelling force of the axle. There are, therefore, in my humble opinion, but two forces and two points of force, mathematically speaking, in the operation, whether a wheel be formed of spokes or of a solid mass. Wheels in other machinery are often unquestionably levers, as axis in peritrochio, or where two points of force happen to be at different distances from the common centre in the same wheel, or in inflexibly attached wheels; or where a wheel moveable upon its centre be used to transfer a force from one point of its circumference to another, it unquestionably becomes a lever*.

If the foregoing observations should in any degree tend to induce a further course of experiments upon this subject of the highest national importance, I cannot help entertaining the warmest expectation, that the Report of the Committee given in the Minutes of the Dublin Society of the thirtieth of March last, (in consequence of which the experiments were instituted,) recommending that "the relative advantages of the different carriages be *accurately and unequivocally* ascertained," may be more reasonably complied with, and that the scientific knowledge of that valuable body may be suitably assisted by the experience of practical agriculturists, so as to insure the exhibition of satisfactory and decisive evidence of the valuable truths

* It would perhaps be reasonable for any one interested in this question, to obtain in writing the nature of the analogy, that may, by any mathematician, be supposed to exist between the lever and the operation of a wheel with respect to a carriage. When on paper, such analogy may fairly be scrutinized; but, if given verbally, it may be plausibly deceptive, or counter to the doctrine of the resolution of forces on the definition of the lever.

relating

relating to wheel carriages, of which the United Kingdom is at present in so much want.

I have not the honour of being a member of either the Dublin Society, or the Farming Society of Ireland; but I feel the strongest assurance, from the large portion of scientific and of agricultural experience in both these bodies, that a very little caution will be sufficient to guard against such injudicious arrangements as render experiments unsatisfactory.

However praiseworthy it is to offer instruction to the public, yet, if the boon be garbed in mystery, the advantage may be lost—nay perhaps worse, by inducing prejudiced and ignorant persons to exclaim against innovations upon old practice. 'Tis therefore insufficient to give instruction, unless it be offered in such a manner as to insure its reception in the minds of the public, who will then no doubt gratefully feel the extent of their obligations to the gentleman, whose recent exertions have been so conspicuous in calling forth pointed and general attention to a subject of the highest national importance.

June 8, 1815. TOWNLEY RICHARDSON.
43, Hardwicke-street, Dublin.

XLVIII. *Some Account of the Electrical Experiments of M.* DE NELIS, *of Malines in the Netherlands. Communicated by Mr.* SINGER.

THE experiments on the singular effects of the electric charge on metallic cylinders which have been already described, were continued by M. De Nelis for many years, and have been varied by him with much ingenuity, and with a degree of patience and perseverance rarely equalled; several of the cylinders employed in his experiments are now in my possession, and evince the action of very considerable power, but the number of explosions employed is only stated for two of them; and I have therefore preferred to detail the more systematic series of experiments in which I have been so ably assisted by Mr. Crosse; especially as these latter appear to exhibit the phænomena more obviously than any of the results transmitted by M. De Nelis: yet it may be proper to notice, that amongst those received from this active experimentalist there is a part of the breach of a pistol barrel, and also an iron cylinder near an inch thick and of small bore, each of them very distinctly cracked by repeated explosions.

It has been already mentioned that M. De Nelis did not find in his experiments any relation between the resistance of the different cylinders, and their comparative tenacity as expressed

 in

in Mechanical Tables, and that he has consequently ascribed these effects to molecular attraction, rather than to the action of an expansive force: but the tenacity of a metal as usually considered, must not be taken as a measure of its power to resist a sudden impulse; and in my experiments it may be observed, that the results appear to correspond sufficiently with the known mechanical properties of the metals. Possibly very different results may have been obtained by M. De Nelis, because all his first experiments were made with water, and mine have been made almost exclusively with oil, which is more certain in its action, not only as a more expansible fluid, but as it presents less facility to the dissipation of the charge in a lateral direction.

The power which has been most frequently employed in these experiments is very considerable; but the principal facts may be exhibited with more facility by employing very thin cylinders filled with oil, and having but a small bore, for these may be distinctly affected by a small battery, or even a few jars.

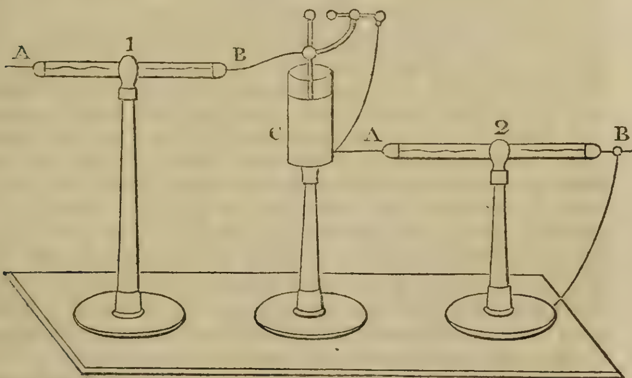
M. De Nelis has attempted to trace the course of the electric fluid, and has instituted some curious experiments for that purpose. A square pane of window glass, perfectly clean and dry, is slightly powdered over on each side with lycopodium or with powdered resin (the former adheres best). In this state it is placed vertically between the points of Henly's discharger, which are made to touch the opposite surfaces of the pane at its centre; the charge of a small jar is then passed from one wire to the other, and two radiated figures of different appearance are produced. Fig. 1, Plate IV, represents that on the positive side, and fig. 2 that on the negative side.

I find that the results of this experiment are not very regular, and are much influenced by the height of the charge, and the continuance of the contact of the charged jar with the discharger. Each figure is exhibited most distinctly when the opposite surface of the glass is clean. I have therefore sometimes employed two panes of glass placed together, and with their outer surfaces only powdered: the effect is usually best obtained by keeping the jar in contact with the wires for a few seconds, and even in that case the jar will be but partially discharged: the effects are therefore analogous to those obtained in the Experiments of Lichtenberg and Bennett, and do not appear to me to offer any satisfactory evidence of the course of the fluid in the discharge.

The same observation applies in some degree to another ingenious experiment of M. De Nelis, with is interesting from other considerations. The figure on the opposite page represents the apparatus. 1 and 2 are glass tubes, nearly filled with water, and each containing two thick silver wires A and B, which are disposed in

the

the axis of the tube, and with a small interval between them at its centre, so as to form an interrupted metallic circuit in water. The tube 1, is placed between the conductor of an electrical machine and the knob of an insulated jar C, and the tube 2, between the outside of the same jar and the ground. The jar is provided with a Lane's electrometer, which occasions a discharge regularly at a certain number of turns of the machine. Now, according to the Franklinian theory, the jar can only be charged under these circumstances by electricity passing through the tube 1 from A to B, to the knob of the jar; and through



the tube 2, also from A to B in its course from the outside of the jar to the earth. M. De Nelis found that by repeated charges of the small jar for four or five hours, the wire A in each tube became oxidated, and the oxide produced was also in each tube attracted by the wire B. By reversing the tubes at the end of the experiment, and proceeding with the charges, these effects were precisely reversed, as is the case when the same manipulation is employed with the Voltaic apparatus. From this he concludes, that the phænomena of electricity are produced by a single current, but at the same time objects to Franklin's theory in explanation of them. The indications of this experiment are very analogous to many of those usually employed in illustration of the Leyden jar; but it exhibits more decisively the identity of the signs evinced by the outside of an insulated jar during the process of charging, with those of the conductor by which the jar is charged; and it shows at the same time that the electrical machine,as well as the Voltaic apparatus, produces very opposite chemical effects when its influence is transmitted from a metal to water, to those which are invariably attendant on its transmission from water to metals.

In pursuing these experiments, M. De Nelis fastened two gold wires on a plate of glass by means of sealing-wax; the wires were disposed in a line, with their proximate ends in close contact with the surface of the glass, and at the distance of half a line from each other. A drop of oil being placed over the interval, and a current of sparks passed from one wire to the other, it was found that after fifty turns of the machine, a small hole similar to the impression of the angle of a diamond, was made in the glass beneath the negative wire, but no effect of a similar kind was to be observed about the positive wire.

I have repeated this experiment with a slight variation; the gold wires were attached to a plate of glass, cut in the form of the letter V. The wires were fastened to the glass near its edges with a separation between them of an eighth of an inch, which was placed very near the lower apex of the plate; the glass plate being immersed in a wine glass filled with olive oil, the effect is a little assisted by the pressure of that fluid; the sparks were received from the conductor by a large insulated ball, placed at an inch distance from it, and connected with one of the gold wires; the other being in communication with the ground. With one of my cylinder machines $57\frac{1}{2}$ inches in circumference, the effect was very conspicuously obtained in a few turns; and by continuing the sparks a few minutes, a deep excavation was made beneath the negative wire.

An experiment very similar to this was made some years ago by my friend Mr. Crosse, who has repeatedly shown it to me and others; he employed water instead of oil; and having in view only the decomposition of the water, and the perforation of the glass, made the separation of the wires so small, that the constant occurrence of the perforation under the negative wire escaped his observation.

It will be obvious to electricians, that the effect produced in this experiment is very analogous to the perforated card in that contrived by Mr. Lullin of Geneva, and which I have employed with slight alteration as an illustration in the Elements of Electricity, page 165, Experiment 66. And it will probably be generally admitted by unprejudiced observers, that the experiment of M. De Nelis, and that of Mr. Lullin, evince very distinctly the action of a material agent, moving from the positive to the negative wire: at least it is not at present easy to account for their results in any other way.

London, Oct. 4, 1815. G. J. Singer.

P. S.

P. S.—Since the preceding account was drawn up, a letter has been received from M. De Nelis, in which some other particulars concerning his experiments are detailed. Speaking of the radiated figures produced on a plate of glass covered with powdered lycopodium; he says, that if a large pane is employed coated on one side, and covered with lycopodium on the other, the full charge of a jar of a square foot coated surface, conveyed to the powdered side of the plate by a pointed wire, will produce a beautiful radiated figure of considerable size, in which it may be distinctly observed, that when a positive charge is employed, the action by which the figure is produced proceeds from the centre (that is the positive wire) to the circumference; and that when a negative charge is employed the appearance is directly contrary, evidently proceeding from each pore of the vitreous surface towards the negative point at its centre. This, M. De Nelis observes, "appears to him to furnish much light in explanation of the charge of idio-electric substances by the theory of molecular attraction."—It may indeed be observed, that the plumose and star-like figures produced by positive electricity, when contrasted with the smooth lines, dots, and balls, which result from negative electricity, evince an action which corresponds very accurately with the opinion that the opposite electricities are respectively but the condensed and rarified states of a single fluid: but the same evidence is given in a very similar manner by the different appearance of positive and negative light; and all the varieties of Lichtenberg's figures have been familiar to English electricians since the extensive experiments of Mr. Bennett on that subject.

Another experiment which M. De Nelis has lately made is thus described: "Take a small pane of flint glass, and two thin strips of silver, each about two or three lines wide, and so thin that it may easily bend. These strips are to be fixed on the centre of the plate of glass by means of wax, their ends being at the distance of a line from each other; around this interval fix a circle of wax one inch diameter, and a third of an inch high, and fill it with olive oil, over which place a plate of glass to prevent the oil from being thrown about by bubbles of gas which are always disengaged from it by the action of electricity. Place this apparatus so that a current of sparks from a powerful machine may traverse the interval between the silver slips; immediately inflamed bubbles of gas will appear; and the attraction of the fluid for the bases of the glass is so great, that these little balls of fire pass alternately over the whole surface of the silver slips. If the action of a powerful current of electricity is continued in this way for an hour, the glass between the slips will be entirely destroyed*; and what has surprised M. De Nelis most, the silver bands will be bent back from the glass in proportion as it is destroyed."

XLIX. *An Analysis of the Mineral Waters of Dunblane and Pitcaithly.* *By* JOHN MURRAY, *M.D. F.R.S. Ed.*

I. *Analysis of the Dunblane Water.*

THIS water was discovered last summer, and was first taken notice of, from the circumstance of the frequent resort of flocks of pigeons to the ground where it breaks out. It appears in two springs, at the distance of nearly half a mile from each other, in a field about two miles to the north of Dunblane, the property of the Earl of Kinnoul. This district is at no great distance from the range of the Grampians, to which it ascends; masses of the primitive rocks are spread over the surface, and are found in the beds of the streams; among which the conglomerate rock that seems to skirt the Grampians is abundant. The prevailing rock of the district itself is the red sandstone, and it is generally covered by a bed of gravel, in many places of considerable depth. It is from this sandstone that the water appears to issue. The spring, however, in both the places where it breaks out, has been laid open only to the depth of two or three feet from the surface, and has not been traced to any extent. Its proper source is therefore unknown, and it also remains uncertain, how far it may be diluted with water from the surface, or from other springs. The water from the lower, or what for distinction may be named the South Spring, is weaker in taste than the water of the North Spring, and, from the subsequent experiments, is proved to contain rather less foreign matter. The ingredients, however, are the same, and the difference therefore probably arises from the water of the lower spring being further diluted in its course. This difference led to the analysis of the water of both springs. It is proper to remark, that both have been submitted to examination after a season unusually dry.

Analysis of the Water of the North Spring.

The taste of this water is saline, with some degree of bitter-

* Much less time will be required to produce this effect, when the interrupted circuit is assisted by receiving the spark from the conductor on a large insulated ball, as I have described for the other experiments.

G. J. S.

ness,

ness. As procured from the principal pool at which it issues, it is free from smell; procured, however, from some other pools, at the distance only of a few feet, its smell is slightly sulphureous, probably owing to impregnation from matter at or immediately under the soil. Its sensible operation on the system is that of a diuretic and purgative. The former effect is usually obtained, when a quantity is taken by an adult, from an English pint to a quart; the latter, when more than a quart is taken. The specific gravity of the water is 1·00475. It suffers no change in its sensible qualities from exposure to the air.

The state of the spring is at present such, that any gaseous impregnation of the water cannot be determined with precision. Bubbles of air frequently rise from the bottom of the pool, but this is merely atmospheric air: transmitted through lime-water, it produced no sensible milkiness; nor does the water appear to contain any free carbonic acid.

The usual reagents present with the water the following appearances;

1. The colours of litmus, violet, and turmeric, are not sensibly affected.
2. Muriate of barytes produces an immediate turbidness, and rather copious precipitation, which is very slightly, if at all, removed by nitric acid.
3. Nitrate of silver gives a very dense and abundant precipitate.
4. Water of potash produces a turbid appearance, not very considerable.
5. Carbonate of potash throws down an abundant precipitate, which disappears with effervescence on adding nitric acid.
6. Lime-water causes no change.
7. Ammonia does not cause any precipitation, nor does it even impair the transparency of the water.
8. Oxalate of potash, or of ammonia, occasions a copious precipitation.
9. Tincture of galls has no immediate sensible effect; but after an hour or two a purplish tint is exhibited, which deepens from exposure to the air, and inclines to olive-green.

These results establish the following conclusions:

Exper. 1. proves that no free acid or alkaline matter is present, nor any alkaline carbonate.

Exper. 2. denotes the presence of sulphuric acid.

Exper. 3. indicates the presence of muriatic acid.

From Exper. 4. and 5. may be inferred the presence either of lime, or magnesia, or both.

Exper. 6. and 7. prove that magnesia is not present, nor argil.

Exper. 8. proves the presence of lime.

Exper. 9. indicates a minute portion of iron.

The

The saline taste of the water, and the precipitation so abundant by nitrate of silver, render probable the presence of muriate of soda, and it is accordingly obtained when the water is evaporated nearly to dryness, cubical crystals of it forming in the saline liquid.

From the whole, therefore, the principal ingredients of this water may be inferred to be muriates of soda and lime, with a smaller portion of a sulphate, and a minute quantity of iron. These conclusions suggested the following method of analysis.

An English pint of the water was evaporated to dryness; and the solid residuum was exposed to a heat approaching to redness, until it became perfectly dry. It weighed while warm 47 grains. It quickly attracted moisture from the air, so that its surface soon became humid; and on leaving it exposed for twenty-four hours, a considerable portion was dissolved, forming a dense liquor, while a portion remained undissolved.

The whole solid matter being rendered dry, was submitted to the action of alcohol, with the view of separating by solution the muriates of soda and lime, of which it was supposed to be principally composed. It is well known that this method is liable, in some degree, to two sources of error; the one, that a little muriate of soda is dissolved by the alcohol with the muriate of lime; the other, that even when a large quantity of alcohol is employed, the undissolved muriate of soda retains a small portion of muriate of lime. In estimating the quantities from the results, these errors, indeed, in some measure, counterbalance each other; but still they may exist in different degrees, according to the quantity and strength of the alcohol, and it is necessary therefore to obtain perfect precision, to obviate them as far as possible.

With this view, the entire matter was digested with repeated portions of alcohol, of the specific gravity of 836, until about six times its weight had been employed; the solvent action being aided by frequent agitation, and an occasional heat of about 100°. It was then lixiviated with a small portion of distilled water, to remove more effectually from the muriate of soda any adhering muriate of lime. The different liquors being mixed, were evaporated to dryness; and this dry mass was again submitted to the action of alcohol, more highly rectified, (being of the specific gravity of 825,) and in smaller quantity, so as to dissolve only that part of it which was muriate of lime. A small portion of muriate of soda, which had been dissolved in the first digestion, was thus obtained, and was added to the residue of that operation. The whole undissolved matter being dried at a low red heat, weighed while warm 28·5 grains: it was in small grains, having a taste purely saline. The alcoholic solution afforded,

forded, by evaporation, a matter which entered into fusion, and which, after being dried at a heat approaching to redness, weighed while warm 18·2 grains. It was highly deliquescent, so as to increase quickly in weight, and in a short time became humid on the surface.

These two products were evidently principally muriate of soda, and muriate of lime. But it was necessary to ascertain if they were entirely so, as both of them might contain small portions of other ingredients.

The matter dissolved by the alcohol, supposing it to be muriate of lime, would require for its conversion into sulphate of lime about sixteen grains of sulphuric acid, of the usual strength. Eighteen grains were added with a small portion of distilled water, and heat was applied; vapours of muriatic acid were discharged: To render the mutual action more complete, small portions of water were successively added, the soft mass being frequently stirred, and when the vapours had ceased to exhale, the heat was raised to redness, to expel any excess of acid. The dry matter weighed 22 grains, precisely the quantity that ought to be obtained from 18 grains of muriate of lime.

It was diffused in a quantity of water, which it at first absorbed with a hissing noise. The water, after having been added in successive quantities, with frequent agitation, being poured off, the undissolved matter was dried at a low red heat: it weighed 18·5 grains, and formed a soft white powder free from taste. The water poured off was very slightly acidulous. This was neutralised by ammonia; it was then evaporated to dryness, and the solid matter was heated to redness. On again submitting it to the action of a small quantity of water, a portion remained undissolved, which weighed when dried two grains.

There were thus obtained 20·5 grains of sulphate of lime, a quantity equivalent to 16·7 of dry muriate of lime. The small portion of liquor which remained in the last operation had a bitterish taste: by spontaneous evaporation it formed acicular crystals; diluted with distilled water, it became slightly turbid on adding oxalate of ammonia, and more so on the addition of alcohol; but in the latter case the transparency was restored on adding water. With a minute portion, therefore, of sulphate of lime, it appeared to be principally sulphate of soda, derived from a little muriate of soda, which, notwithstanding the precautions that were employed, had adhered to the muriate of lime.

The matter which remained undissolved by the alcohol, weighed, it has been stated, 28·5 grains. It remained to ascertain if it were entirely muriate of soda.

Being

Being agitated with about half an ounce of distilled water, the greater part was dissolved. The portion which remained undissolved, after being washed with small quantities of distilled water, and dried, weighed 2·4 grains. To this matter a little diluted nitric acid being added, a slight effervescence was excited: a thin crust, too, adhered to the sides of the small glass globe in which the last stage of the evaporation had been performed, which was dissolved with effervescence by a weak acid. The quantity of *carbonate of lime* thus indicated, may be estimated at 0·5 grain. The remainder of the undissolved residue being washed and dried, was heated with two or three drops of sulphuric acid, and was thus rendered soluble in water. When neutralized by ammonia, the solution became milky; but its transparency was restored by adding more water; it became quite turbid on adding oxalate of potash, and a precipitate was thrown down by alcohol. It was therefore *sulphate of lime.* Its quantity may be stated at two grains.

The solution had a taste purely saline. The test of oxalate of ammonia, however, showed the presence in it of a small quantity of lime; the addition of the oxalate was therefore continued as long as any precipitation took place, and the precipitate was collected and dried. It weighed 1·3 grain. This production of oxalate of lime evidently arose from the presence of a small portion of muriate of lime, which, notwithstanding the precautions that had been employed, had adhered to the muriate of soda. Supposing that this had not escaped the action of the alcohol, but had been dissolved by it, and in the subsequent stage of the experiment, been converted into sulphate of lime, it would have increased the quantity of this sulphate about 1·2 grain, making it therefore 21·7, equivalent to 17·6 grains of dry *muriate of lime* which the pint of water contains.

The solution contained also a minute quantity of sulphuric acid; for after removing any slight excess of oxalic acid that might have been present, it still gave a precipitate on the addition of muriate of barytes. Supposing this, as well as the rest of the sulphuric acid, to have existed in the water in the state of sulphate of lime, it will increase the quantity of that ingredient (calculating from the weight of the precipitate of sulphate of barytes obtained) from the two grains formerly noticed to 2·9.

There appeared now to remain nothing but pure muriate of soda. The solution by slow evaporation afforded that salt in cubical crystals, which, dried at a low red heat, weighed 24·5 grains. Allowing 0·8 of this as the portion of product formed by the action of the muriate of barytes, it leaves 23·7 grains. And if to this be added one grain, as the equivalent of the small portion

portion of sulphate of soda, already noticed as formed by the action of the sulphuric acid on the muriate of soda adhering to the muriate of lime after the operation of the alcohol, it gives the quantity of muriate of soda at 24·7 grains.

From these results, the solid ingredients in a pint of this water appear to be

Muriate of soda	24·7	grains.
Muriate of lime	17·6	
Sulphate of lime	2·9	
Carbonate of lime	0·5	
	45·7	

With a trace of iron.

Having completed the analysis in this manner, I wished to confirm it by a different method. A very simple one presented itself,—to reduce by evaporation to dryness,—obtain the sulphate of lime as before,—then, dissolving the mixed mass of muriate of lime and muriate of soda in water, decompose the muriate of lime by oxalate of ammonia, so as to find the quantity of it present, and after evaporation to volatilize the muriate of ammonia by heat, and thus obtain the muriate of soda. The results in this mode ought to correspond with those in the former; and the one, therefore, afford a confirmation of the other, or lead to the discovery of any fallacy if it exist.

A pint of the water was evaporated to dryness, and afforded, as before, 47 grains of solid matter. This being submitted to the action of a small quantity of distilled water, was dissolved, with the exception of a residue of sulphate of lime, which weighed 2·6 grains, and a little carbonate of lime, which may be estimated, as before, at 0·5 grain.

To the clear solution, a solution of oxalate of ammonia was added as long as any turbid appearance was produced; and after the precipitate had subsided, the liquor was heated nearly to boiling, to render the mutual action and the precipitation more perfect. The precipitate being repeatedly washed with distilled water, was dried by the heat of a sand-bath raised gradually, and kept lower than a red heat. It weighed 21 grains. The quantity of muriate of lime which would be equivalent to this, cannot be inferred with certainty, from any previous analysis of oxalate of lime; for as the oxalate cannot be exposed to a red heat without decomposition, it cannot easily be subjected to a precise degree of heat, by which we can be certain of obtaining it in a uniform state of dryness*. It is necessary, therefore,

* Referring to those analyses which may be supposed to be most accurate, 21 grains of oxalate of lime will be found equivalent to various proportions, from 17·5 to 19·9 of muriate of lime.

that

that in every case the quantity of lime should be found in the oxalate that is operated on. The above quantity of 21 grains was converted by calcination into carbonate of lime, and this being decomposed by muriatic acid, the quantity of muriate of lime obtained, dried at a low red heat, and weighed while warm, amounted to 18·3 grains.

The liquor poured off from the precipitate was evaporated to dryness; and to expel the muriate of ammonia formed by the action of the oxalate of ammonia on the muriate of lime, the heat was continued while any vapours were disengaged, and at the end was raised nearly to redness. The dry mass weighed, while warm, 25 grains. Being dissolved in water, its solution was rendered very slightly turbid by the addition of muriate of barytes, showing the presence of a minute portion of sulphuric acid. A quantity of precipitate was collected, which, when dried, weighed 0·8 grain. Supposing the sulphuric acid of this to have originally existed in the water, along with the other portion of this acid, in the state of sulphate of lime, it gives a proportion of that sulphate of 0·5 grain, and of course increases the quantity of it from the 2·6 grains obtained by evaporation to 3·1 grains. An equivalent quantity must at the same time be subtracted from the proportion of muriate of lime, which may therefore be reduced to 18 grains. By evaporation of the liquor, muriate of soda was obtained, weighing, when it had been dried at a low red heat, 24·3 grains. Of this a small portion (0·4) would be formed by the muriate of barytes, which requires to be deducted; but then the sulphuric acid which existed in the mass, could, after the action of the oxalate of ammonia, and the exposure to a red heat, exist in it only in the state of sulphate of soda, in the production of which an equivalent portion of muriate of soda would be decomposed. The quantity of muriate of soda obtained, therefore, by the evaporation, may be regarded as the just proportion indicated by the analysis.

The results, then, by this method, agree very nearly with those by the other; being of solid ingredients in a pint of the water,

Muriate of soda	24·3 grains.
Muriate of lime	18
Sulphate of lime	3·1
Carbonate of lime	0·5
	45·9

With a trace of iron.

With regard to both analyses, a small correction is to be made in the proportion of sulphate of lime. The mode of ascertaining it, by evaporation, being rather imperfect, I afterwards determined it by the more accurate method of precipitation by muriate

muriate of barytes; applying this reagent with a slight excess of acid, so as to prevent any precipitation of carbonate. The quantity of precipitate thrown down from a pint of the water, amounted, after drying at a low red heat, to 6·1 grains, equivalent to 3·5 grains of sulphate of lime. As the portion of sulphate of lime thus obtained, above that obtained by the evaporation, would remain principally mixed with the muriate of soda, the quantity of that ingredient falls to be reduced a little, and may therefore be stated at 24 grains.

It remained to ascertain the proportion of iron. The quantity, however, was evidently so small as to present a difficulty. Succinate of ammonia, and benzoate of soda, produced little or no effect on the water in its natural state. Infusion of galls produced, after some hours, a dark colour, and a precipitate very slowly subsided. This method has been employed to ascertain minute quantities of iron, and I endeavoured to avail myself of it,—adding to the water infusion of galls, in small successive portions, at the interval of a day or two, as long as the colour appeared to be rendered deeper; leaving it exposed to the air for a longer time, that the whole matter rendered insoluble might subside; and lastly, washing the precipitate, drying and calcining it, to consume the vegetable matter, and obtain the oxide of iron. The difficulty, however, attending this method, is that of precipitating entirely the iron, the liquor never becoming colourless. In one experiment, conducted with much care, the quantity of the calcined product from two pints amounted to 0·4 grain; but it consisted partly of carbonate of lime. To remove this, pure muriatic acid diluted was added in excess, and a moderate heat was applied; the precipitate was entirely dissolved, and the liquor acquired a deep yellow colour. Being further diluted, a little pure ammonia was added to it, in a close phial, to precipitate the oxide of iron, while the lime should remain dissolved. The quantity thus obtained, when dried, amounted to 0·27 grain.

This method being liable to the above objection, I employed another: two pints of the water were evaporated: when reduced to about two ounces, a brownish coloured sediment was deposited, which was removed; the evaporation was carried to dryness, and the dry mass was redissolved in distilled water. The insoluble residue was of a grayish colour, and to this the deposite formed during the evaporation was added. It was known by previous experiments, that the greater part of the iron was separated in this way; the insoluble matter, when digested with muriatic acid, affording a liquor, when diluted with water, which gave, after neutralization with ammonia, a deep colour with tincture of galls. To ensure, however, the more perfect separation

separation of the iron, ammonia was added to the solution of the solid matter which had been procured by evaporation; and care being taken that the ammonia was free from carbonic acid, little or no precipitation could take place but of oxide of iron. A yellowish flocculent precipitate subsided slowly, which, after being washed, was added to the insoluble residue.

The insoluble matter thus collected consisted, as the preceding steps of the analysis establish, chiefly of sulphate, with a smaller portion of carbonate of lime, with which was mixed the oxide of iron. A drop or two of sulphuric acid was added, to convert the carbonate into sulphate of lime; and heat was applied to expel any excess of acid. A little pure muriatic acid was then added to dissolve the oxide of iron, and to form with more certainty the red muriate, soluble in alcohol, a drop of nitric acid was added along with it. On applying heat, with the addition of a little water, to favour the action, a yellow colour was acquired. When the excess of acid was nearly dissipated, the mass was repeatedly lixiviated with alcohol, in which sulphate of lime being insoluble, the muriate of iron only would be dissolved. The alcohol acquired accordingly a yellow colour. Being evaporated by a gentle heat, it gave a residuum, which, on a drop of nitrous acid being added, became of a deep reddish-brown colour, and after being heated strongly, weighed 0·34 grain. Redissolved in muriatic acid, it formed a rich yellow coloured solution, and gave a deep colour with tincture of galls.

Even in this way, the whole iron is not obtained; for the solution of the saline matter, though ammonia had been added to it, to precipitate the iron, still gave a weak colour with galls. The quantity therefore is rather under-rated. Taking the above however as the proportion, the whole composition will be in a pint of the water of the North Spring,

Muriate of soda	24 grains.
Muriate of lime	18
Sulphate of lime	3·5
Carbonate of lime	0·5
Oxide of iron	0·17
	46·17

Analysis of the Water of the South Spring.

The water of this spring has a taste similar to that of the other, but rather weaker: it produces similar medicinal effects. In the present state of the spring, its strength is more variable, according to the state of the weather. From this circumstance, and from its being rather weaker, it has probably a greater intermixture

termixture of surface-water, or of the water of other springs. When taken up after continued dry weather, it afforded, by evaporation, 42 grains of solid matter from a pint; the other affording, at the same time, 47 grains. Its specific gravity was found to be 1·00419. It was in this state, the strongest in which it was found, that it was submitted to the following examination.

The application of reagents produced the same appearances as with the water of the North Spring, indicating, therefore, the presence of the same ingredients. To determine this with more precision, and to ascertain the proportions, the same methods of analysis were employed which had been used with regard to the other. It will be sufficient to state the results by one method,—the second of those before described.

A pint of the water was submitted to evaporation, and afforded of dry matter, weighed while warm, 42 grains. This was redissolved in distilled water. There remained undissolved a portion, which, when thoroughly dried, weighed 2·5 grains. This suffered a very slight effervescence with muriatic acid, similar to that excited in the insoluble matter of the water of the North Spring; a similar thin crust, too, had formed on the sides of the glass capsule, which was removed with effervescence by a drop of muriatic acid. The relative proportions, therefore, of sulphate and carbonate of lime may be regarded as the same: and the insoluble residue will thus consist of 0·3 of carbonate, and 2·3 of sulphate, of lime. By precipitation by muriate of barytes, from another pint of the water similar results were obtained.

To the clear liquor, oxalate of ammonia was added as long as it produced any turbid appearance. The precipitate collected and dried, being converted by calcination into carbonate of lime, afforded, when acted on by muriatic acid, 16 grains of dry muriate of lime.

The solution poured off from the precipitate was evaporated to dryness, and the dry mass was exposed to a heat gradually raised to redness, until it ceased to exhale any vapour. The muriate of ammonia formed by the action of the oxalate of ammonia on the muriate of lime, was thus expelled, and the muriate of soda of the water remained. It weighed 22·5 grains.

The results, then, by this method, are from a pint of the water,

Muriate of soda	22·5 grains.
Muriate of lime	16
Sulphate of lime	2·3
Carbonate of lime	0·3
Oxide of iron	0·15
	41·25

The

The proportion of iron I have stated as similar to that of the North Spring, from the colour produced by the tincture of galls being nearly the same.

From the almost perfect similarity in the composition of the two waters, with regard to the proportions of their ingredients, there is every reason to conclude that they are from the same spring; the weaker being either mixed with surface-water at the pool, or being diluted in its course.

The determination of the composition of this water suggests the question, whether this is such as to account for the medicinal effects it produces. It acts, as has been stated, as a diuretic, and in a larger dose, as a cathartic. This water, and the mineral water of Pitcaithly, present, in some respects, a peculiarity. The greater number of saline waters which have a purgative quality, contain magnesian salts, to which, as they are known to act as cathartics, the effect is obviously to be ascribed. Of the ingredients of the Dunblane and Pitcaithly springs, muriate of lime is scarcely known to have any purgative power in its pure form, and if muriate of soda possess it, it is only in an inconsiderable degree. Still, there can be no doubt, that it is to this impregnation that their operation is owing, and they afford a proof, therefore, of what is indeed sufficiently established, that the powers of mineral waters are often much greater than could be expected from the nature and quantity of their ingredients, and that the action of saline substances is increased, and considerably modified, when they are in a state of great dilution.

Independent of its purgative operation, and its adaptation to the treatment of diseases in which this is advantageous, its composition may probably render it a remedy of efficacy in some constitutional affections, particularly in scrofula. Muriate of lime has attained some celebrity in the treatment of this disease; it is a substance of considerable activity in its effects on the living system; and it will probably operate with more effect, and more advantage, in the state of dilution in which it is presented in a mineral spring, than when given in a more concentrated form. The muriate of soda may coincide with it in efficacy, and will be of advantage from its grateful taste, and its stimulant action on the stomach. And the chalybeate impregnation will communicate some degree of tonic power. When employed in such cases, it probably ought to be given in smaller doses than when the advantage to be derived from it depends on its purgative operation; and it may even prove more advantageous, if given in a state of greater dilution. I shall in the sequel have to state a view of its composition, which connects it with some mineral springs of great celebrity, and particularly with the Bath waters.

Dunblane, as a watering-place, would be possessed of considerable

derable advantages. Situated between the range of the Ochil Hills and the Grampians, it is well sheltered, and hence enjoys a mild atmosphere; and the soil, from being a bed of gravel for a number of miles around, is extremely dry,—an advantage inestimable in a moist climate.

II. *Analysis of Pitcaithly Water.*

The water of Pitcaithly may be regarded as the principal mineral water of the saline class in this country. Dr. Donald Monro showed, that, along with a little mild calcareous earth, it contained muriate of soda, with a deliquescent salt, which he inferred to be chiefly "a calcareous marine," that is, muriate of lime*. An analysis of it was published a number of years ago, executed by Messrs. Stoddart and Mitchell of Perth. There are different springs, the waters of which they found to be somewhat different in strength. The nature of the impregnation is in all of them, however, the same.—Selecting the strongest, it contains, according to their analysis, the following ingredients in an English pint:

Atmospheric air	..	0·5 cubic inch.
Carbonic acid gas	..	1 —— ——
Muriate of soda	..	12·5 grains.
———— lime	..	22·5
Sulphate of lime	..	0·7
Carbonate of lime	..	0·6†

The composition of this water, according to this analysis, is very similar to that of the Dunblane water. No account is given, however, so far as I have been able to discover, of the manner in which it had been executed, and it is therefore uncertain to what state of dryness the ingredients had been brought to which their proportions are referred. Hence no comparative estimate can be made of it with any other mineral water; and this led me to undertake its analysis, in the same manner as that of the Dunblane water.

Pitcaithly is situated in the valley of Strathern; and though at rather a greater distance from the front range of the Grampians than Dunblane, it is not improbable that the spring may have a similar origin with the Dunblane one, and may rise from the red sandstone which appears to form the first rock on descending from the primitive rocks, and to extend over all this district.

The taste of this water is saline, and somewhat bitter. Comparing it with the Dunblane water, both being tasted at the same time, the taste of the Dunblane water is stronger, and in particular more saline than that of the other. The medicinal

* Philosophical Transactions, vol. lxii.
† Statistical Account of Scotland, vol. viii.

operation of the Pitcaithly water, in the sensible effects it produces, is diuretic and purgative.

The gaseous impregnation of the water could be examined properly only at the spring, which I had not the opportunity of doing. But having procured a quantity of the water, I submitted it to the same examination as in the preceding analysis, to ascertain its solid contents. The usual reagents produced the following appearances:

1. The colours of litmus, violet, and turmeric, were scarcely affected. If there were any change, it was that of the litmus becoming more blue, and that of the violet green; but this was so slight as to be rather doubtful. The turmeric underwent no change.
2. Muriate of barytes produced a turbid appearance and precipitation; but this was much less considerable than in the Dunblane water. The transparency was not restored by nitric acid.
3. Nitrate of silver produced a very dense and copious precipitate.
4. Water of potash gave a milkiness not very considerable.
5. Carbonate of potash threw down a copious precipitate, which disappeared with effervescence on adding nitric acid.
6. Lime water had no sensible effect.
7. Ammonia, when perfectly free from carbonic acid, caused no turbid appearance.
8. Oxalate of ammonia produced an abundant precipitation.
9. Tincture of galls, added in a very minute quantity, did not immediately produce any effect; but after a few hours, a dark colour appeared, which gradually deepened, inclining to an olive-green.

With all these tests, the general results are the same as those from the operation of the same tests on the Dunblane water. In experiment 7th, the ammonia, if not perfectly free from carbonic acid, produced a slight turbid appearance; and even when in its purest state, a very slight opalescent hue was perhaps apparent: but this obviously depended on the presence of a little carbonic acid; for when a drop or two of nitric acid was previously added, and the water heated, no such appearance was produced; or, if boiled strongly, without any addition of acid, on restoring the original quantity of liquid, by adding distilled water, the transparency was not in the slightest degree altered on adding pure ammonia. The slight precipitate, too, which did occur in any case, was dissolved by the most minute quantity of muriatic acid with effervescence; and this solution became turbid on adding oxalate of ammonia, proving the precipitate to have been carbonate of lime.

The

The same general conclusions, then, with regard to the nature of the ingredients, are to be drawn from the preceding results as from the application of the same tests to the Dunblane water. They suggest of course a similar mode of analysis. I preferred the second of the methods above described, as being the most simple, and easy of execution.

An English pint of the water was submitted to evaporation. Before the matter became dry, numerous cubical crystals were formed, indicating the presence of muriate of soda; when dry, the solid matter entered readily into fusion with effervescence, denoting the predominance of muriate of lime. The dry matter was highly deliquescent. After exposure to a heat inferior rather to redness, it weighed while warm 35 grains.

This dry matter was redissolved in about ten times its weight of distilled water. A small portion remained undissolved, which being washed and dried, weighed 1·2 grain. A little diluted muriatic acid dropt upon this, excited slight effervescence; but the greater part remained undissolved, and weighed, after washing and exsiccation, 0·9 grain. It was *sulphate of lime.* A very thin crust adhered to the sides of the glass globe in which the last stage of the evaporation had been performed. This was dissolved with effervescence by diluted muriatic acid, and the solution became quite turbid on adding oxalate of ammonia. The quantity of *carbonate of lime* thus indicated, adding the portion abstracted, as above, from the sulphate, cannot be estimated at more than 0·5 grain. These results were confirmed by precipitation from another portion of the water by muriate of barytes, the proportions indicated being nearly the same.

The liquor poured off from the insoluble residue, being diluted with distilled water, oxalate of ammonia was added to it, as long as any turbid appearance was produced; and after the subsidence of the precipitate, the liquor was boiled a little, to render the decomposition and precipitation complete. The clear liquor was then evaporated to dryness, and the dry mass was exposed to heat, to volatilize the muriate of ammonia, the product of the action of the oxalate of ammonia on the muriate of lime; the heat being continued as long as any vapours exhaled, and at the end being raised to redness. The muriate of soda thus obtained, weighed 13·4 grains. By solution and crystallization it was obtained in cubes.

The precipitate of oxalate of lime having been thoroughly washed, was exposed in a sand-bath to a heat short of redness, until it had ceased to exhale any vapours, and appeared perfectly dry; it weighed 23·8 grains. The portion of muriate of lime equivalent to any quantity of oxalate of lime, cannot, as has been already remarked, be exactly assigned, from the difficulty of bringing the oxalate to one uniform state of dryness. But, according to the most accurate analyses, 23·8 grains of dry oxalate are equivalent to 20 grains of dry muriate. To avoid any error, however, the oxalate was converted into carbonate of lime by calcination; and this decomposed by muriatic acid, afforded 19·5 grains of dry muriate of lime.

The proportions, then, of the saline ingredients in an English pint of the Pitcaithly water, are, according to this analysis,

Muriate of soda	13·4	grains.
Muriate of lime	19·5	
Sulphate ———	0·9	
Carbonate ———	0·5	
	34·3	

To which are to be added of aërial ingredients,

Atmospheric air ..	0·5	cubic inch.
Carbonic acid gas ..	1	cubic inch.

It also gives slight indications of the presence of iron; but as far as can be judged from the shade of colour produced by tincture of galls, the quantity is much smaller than in the Dunblane water. It does not admit, therefore, of being determined with much accuracy by actual experiment.

L. *A Reply to Dr.* W. H. Gilby; *with some additional Facts regarding the Stratification of Britain, in proof of the Futility and Absurdity of the Attempts, at applying to the same, the Terms and Distinctions of the Wernerian Geognosy. By Mr.* John Farey, *Sen. Mineral Surveyor.*

To Mr. Tilloch.

Sir,—It should not be forgotten, that Dr. Gilby, in p. 300 of your last volume, *untruly* charged me, with having "alluded to no *writer* who had noticed the fact," of the unconformableness of the Red Marl on the Coal-measures of a part of Somersetshire, and, quoting *Professor Jameson's authority*, for as unfoundedly appropriating *to himself* this discovery (of near a century old), took occasion, thereupon, to commence an unprovoked attack upon me; for which he has not apologized in his Letter at p. 182, as candour would have dictated: but seems to think it sufficient, to charge on the garbled account of a brother Geognost*, the

* Another of these garblings of the papers in the Philosophical Transactions, is to be found in p. 296, of the 2d Edition of Williams's "Mineral Kingdom," where the mention, of the Faults *not being visible* in the form of the surface of the South Wales Coal-basin, is wholly omitted!: I first learned this, from p. 129 of Dr. Kidd's Geology.

"ignorance,"

"ignorançe," which led him into those improprieties: and so far from appearing to think it necessary, to apologize for the use he had thus made of Professor Jameson's Name, again speaks on the same subject (p. 183) by his authority and *permission!.*

In page 188, Dr. G. unhandsomely charges me, with wishing to introduce *confusion*, as to the Geology of England: — If having been the first to bring forward more *facts* than any one else, regarding the stratification of our Island, and having occasionally drawn the Inferences that these seemed to me to warrant, be manifestations of the design with which the Doctor charges me, I must plead guilty thereto (although I may now further thus offend):—but fortunately for me, the Doctor's Letter sufficiently shows the source of all the confusion of which he complains, viz. the rashness and pertinacity of himself and brother *Geognosts*, in their attempts to garble and bend the facts of the British Stratification, to suit their favourite German Theory:—a vain attempt, I venture once more to assure them.

The Doctor in his last communication, p. 185, affects to complain, of "perplexing language and confused description," in my Account of the Strata of Derbyshire: and this apparently, because I have not written in the fashionable (Anglo-Wernerian) jargon, although the same would have been quite unintelligible to the people of the County, *for whose use, principally, the Report was intended* by the Board of Agriculture, who printed it: the Doctor might, however, have learned from p. 305, of your xlist volume, that the very learned Geognosts who sat in judgement upon, and ultimately rejected a very detailed account of the Strata, regarding which he seems yet in such perplexity, (see vol. xlii. p. 55 Note), so far from alleging the *unintelligibleness* of my descriptions, of the Coal-measures in particular, have pronounced them to be, "the *independent* Coal-formation"! and surely, to this authority, all good Geognosts ought to bow with reverence: I will however take the liberty of dissenting from their decision, and of maintaining, that the name itself, with every effort at defining this *independent* formation, which I have seen, are as truly ridiculous, as the attempts have been, in England or in the South-west parts of Scotland, at applying it to particular Coal-fields: — the *independent* Coal-formation, belongs to "the ideal world of Werner," and not to any part of Britain, I believe,

It seems rather extraordinary, after the facts which I have mentioned in p. 166, of your last volume, that Dr. G. should assert p. 183, "the measures accompanying the Coal," in Derbyshire, to be "*quite different* from those usually associated with the Coal in England:"—the identity of all of our British

Coal-fields with each other, except those belonging to the Clunch Clay (mentioned in the note p. 100 of vol. xxxix., and p. 256, and 257 of vol. xliii.), is to me now sufficiently made out; and the same with respect to the Derbyshire-peak Limestone, underlieing each of these several Coal-fields; and the fossil Shells in this Limestone, which Mr. Sowerby has described in the 1st volume of his "Mineral Conchology," go far I think towards proving this identity.

The Authority with Geognosts, against whom they seem to admit no appeal, M. Werner, has given contradictory accounts of the place, in his system, or of the formation, to which the Derbyshire-peak or mountain Limestone is to be referred, as observed in the note onp. 2·8, of my Derbyshire Report: so also, the next greatest Authority with these Gentlemen, Mr. Jameson, after stating the Toadstone interlaying these Limestones to be *transition* (Geog. iii. p. 149), now, if we may credit Dr. Gilby (p. 183), considers this Limestone to be *first Flœtz!*, see also Dr. Kidd's Geology, p. 115. The Geognostic Committee on my Description of these Rocks, seem to have been awed by the contradictions above mentioned, from venturing to pronounce, on the formation to which they should belong: but Mr. Bakewell, Dr. Kidd, Dr. Prichard, and others I believe, have since given their Opinions; and it may I think justly be said, that respecting this, and every other particular Rock in England which has been claimed by the Geognosts, as belonging to one or other of their Formations, such a mass of *complete contradiction* now appears, as to render the subject quite ridiculous, and soon will render it contemptible, if they persevere.

Dr. G. seems to take it for granted (p. 184), that what he chuses to denominate the "old red sandstone," occurs *universally*, between the Derbyshire-peak Limestone and the coarse slate: yet, I believe, that a sandstone rarely occurs in this situation: in all the very long course of the junction of these two Rocks in North Wales, which I have particularly mentioned in pp. 163 to 165 of your last volume, I believe, *no instance occurs of sandstone* in this situation: in like manner, in examining lately, the entire circuit of the Limestone* Rock, which underlies the shallow basin of Coal-measures around Thornhill in Dumfries-shire, a circuit of near 30 miles, a thick *whin* or basaltic rock, and not sandstone, is everywhere found, interposed, between the Limestone and the coarse Slate. I cannot find that

* In the greater part of its range, this Rock proves unfit for the lime-burner's purpose, and is called Dogger: as happens also in Anglesea, to the SW of Ceint, and more locally, in the quarries S of the great Ponteyssyllte aqueduct, and other places.

Mr.

Mr. Jameson has noticed this remarkable Rock, in his Survey of this County, unless we admit him to have classed it as *newest flœtz* (although underlieing what he calls the *Independent Coal-formation!*): it is true, in p. 79, he tells us, what members Werner says this latter formation consists of, and this Rock having no place there, it may from this circumstance have been overlooked:—an *ideal* and a real thing, will not always correspond exactly: and theory, we have been told, sometimes blinds observers.

I do not pretend to say, that Sandstone is *never* found in the situation mentioned by Dr. G. because he will find in pp. 126 and 326 of vol. xliii. that I observed, very variably coarse and conglomerate Gritstone upon the coarse Slate, on most of the western side of the Anglesea Coal-basin; but it is proper I should add, that in the southern part of its eastern side, no such rock was visible, but in its place, a basaltic kind of rock, very similar to that of Dumfries-shire which I have mentioned, but nearer perhaps approaching to slate. I saw also a great deal of variably coarse Gritstone, basseting from under the Limestone in the great Forest of Brecon, p. 125, of vol. xliii.

The very probable errors of Mr. W. Forster and of Mr. Bakewell's Sections, in placing red sandstone *under* the metalliferous Limestone of Cross Fell, have been pointed out, in the note on p. 172 of your last volume; and in p. 458 of the Appendix to Mr. Bakewell's Geology, 2d Edition (subsequently printed) the justice of this remark seems admitted: and Mr. B. here and pages 369, 397, &c. concurs with me in saying, that *sometimes only*, sandstone is found in this situation, and not *generally*, as Dr. G. wishes to persuade.

It seems, however, extraordinary, that Dr. G., after *generally* assuming, that red sandstone is to be found under the English mountain Limestone, should in pages 185 and 187, as peremptorily infer, that it *is not to be found* under that of the Low Peak Hundred of Derbyshire and the adjacent parts of Staffordshire, in *the absence of all proof*, for or against his latter assumption; as he might have learned, from my Report i. 280, 297, and 478, Phil. Trans. 1811, or P. M. vol. xxxix. p. 29, &c. In the detailed account of the Derbyshire Strata, which underwent in 1813, the Geognostic ordeal and suppression, already mentioned, I suggested, and endeavoured to enforce the use and propriety, of *a boreing* (with Ryan's trepan Auger) being made, in the most deeply excavated part of the 4th Limestone Rock, for correctly ascertaining the facts of its under strata. *Quære*, would not a great majority of the subscribers to the Society alluded to, better approve the appropriation of a part of its Funds to such a purpose, than to the unnecessary (not to say the piratical) copying and *engraving* of other persons' Maps?.

In

In p. 187 Dr. G. mentions North Wales, as one, of the only two situations in the main land of *England* (South Britain) where *primitive* Rocks occur: Mr. Arthur Aikin, in pages 42, 65, 191, 199, 202, 217, and 219 of his "Tour," certainly mentions these, and in pages 99, 130, 209, 211, 214 and 215 speaks also of *Granite*, among the minerals of the Principality. I believe however, from all I have seen, or could learn from others on the subject, that in the latter case, Mr. Bakewell (p. 398, 2d Edit. of his Geology) is right, in referring the granite mentioned, to loose or *alluvial Blocks:* and in considering the former theoretic deduction, to be quite unfounded:—if there be anything of consistency or truth in the boasted Geognosy, the Copeland or Lake Hills of Cumberland, must surely be better entitled to the class of *primitive*, than any part of Wales?: unless by the terms, "unassociated by the *older* Rocks," Dr. G. means, wanting something *older* than Granite? — but, "Who shall decide, where Doctors disagree?."

In charging me, towards the bottom of p. 187, with the "inaccuracy and confusion," of not knowing the relative age and situation of the Red Marl, to be more recent, or above the Coal-measures, Dr. G. forgets, that it was, for re-asserting this very truth (which I had long before published), but which Professor J. (as he says) had fondly persuaded him, he might appropriate to himself, that he commenced his attacks upon me: "I clearly see (says he) that it (the observation I had first *generalized*) is of considerable importance," &c.

With whatever truth Dr. G. may charge Dr. Kidd, and others of his Geognostic brethren, with confounding the different Red Marls, or the red-ground and red Sandstone (as he chuses to call them, pages 184 and 188), he cannot fix this charge on my friend Mr. David Mushett, p. 50 of your xlist volume, much less on myself, who in the Phil. Trans. for 1811, or p. 29 of your xxxixth vol. p. 105, vol. xxxixth p. 54, vol. xlii. and on many subsequent occasions, have spoken of *different* red Marls; all of them, alike, imbedding or passing locally, into sandstone, and not therefore distinguishable from each other *by this circumstance.*

Dr. G. might here, have been aware, that something more was wanting, than a reference, to "the truly admirable system of Werner," or to any of its classes of formations or their members, to overturn what I have observed and written, regarding the Red Marl, and its locally imbedded masses of Sienite, coarse Slate, &c. in Leicestershire (not *Derbyshire*, as the Doctor writes):—when I may have examined the Malvern hills, I shall be able, I trust, to speak of them less diffidently than in vol. i. p. 152, and, if necessary, it shall be with more truth:—often as descriptions

descriptions have been given or quoted, concerning these Hills, I could not find these applying, to more than *one* side of them (the western), and which I consider, rather too much like hearing one side of a story. If ever I should see the very highly improbable sight (p. 188), of a granite mountain 3000 feet high, rising out of a gravelly plain: the Doctor may rest assured, that I am too well aware of, that grossest of absurdities, in his favourite Geognosy (Rep. i. p. 134 Note), to confound the *alluvia* of the plain, with *strata* of any kind:—certainly I do, and scores of others of my acquaintance do the same, most heartily despise every such "systematic arrangement," should they even issue from the university of Edinburgh (Dumfries-shire, p. 111), or the British Museum, (Dr. Rees's art. Flœtz).

I beg to solicit the Rev. Mr. Townsend's attention, and reply, to what Dr. G. says in p. 188, concerning Brandon Hill, and as to *micaceous* sandstone, being found in the upper Red Marl, in Gloucestershire:—I can assure him of its frequent occurrence in and near Derbyshire (Rep. i. 148, 466), and in very numerous other situations (see also Dr. Kidd's Geo. pp. 10, 107), however the same may startle this stickler for "systematic arrangement."

In enumerating the beds of the Red Marl (p. 190), Dr. G. mentions, a *coarse* breccia or conglomerate, as the lowest of them: I beg to state, that I have never seen this to be the case: in all the long range in which the yellow Limestone (answering to the Doctor's limestone, breccia, or conglomerate, I believe, as mentioned p. 342 Note, vol. xlv.) lieing unconformablly over the Coal-measures of Nottingham, Derby, York, Durham and Northumberland Counties, a *fine-grained* sand, or soft sandstone, invariably lays under the Limestone, or next to the Coal-measures, as I have particularly mentioned in the note on p. 410 of the 2d volume of my Derby Report, concerning that County and its vicinity: and a Letter now before me, from Mr. Thomas Fenwick, an eminent Coal-viewer in the Tyne and Wear district, who is also the Director of a large Colliery at Garforth* in Yorkshire, and who is therefore well acquainted with these distant coal districts, mentions, that a similar *sand* of considerable thickness, *is always found* under the edge of the buff or yellow Limestone, after it passes out of Yorkshire, and traverses Durham,

* From whence the first information came, of the *unconformableness* of the yellow Limestone and this Sand, as mentioned p. 167 and 174 of vol. xlv.: of which curious facts, I have since had the opportunity of fully satisfying myself, by an examination and mapping of part of the district; and my kind Friend Mr. James Porter, has undertaken to soon complete this Map, and send it up for publication: it will establish the facts mentioned, beyond all controversy.

ham, by Brustleton, Ferry-hill, Coxhoe, Pittington, Painshar, Hilton, West Bowden and South Shields, and thence by North Shields, to Whiteburn in Northumberland.

I have observed, that upon the buff or lower magnesian Limestone Rock, a middle Red Marl, of considerable thickness, occurs through this extensive line of Country. On this, the upper or light straw-coloured free Limestōne occurs: and then the upper Red Marl *, perhaps imbedding towards its lower part, the very irregularly coarse and irregularly stratified sandstone of Sherwood Forest?, &c.:—in short, much remains yet to be known, before Dr. G. can safely proceed in his favourite course, of systemizing; before which, he will I doubt not see the folly, of dubbing our Strata with geognostic names and distinctions:—every Country must work out its own Geology, and not *import* it, read made.

Where is *Ness?*, mentioned p. 188, and where has Dr. Kidd spoken of this place?.

In the note at foot of p. 183, Dr. Kidd is rather severely chid by Dr. G., for not ascribing to Mr. Jameson the whole merit, of correcting the former absurd notions and theories (they made part of the Geognosy, see Wern. Trans. i. 290), which ascribed all the *grains* of silex composing sandstones, to the *mechanical* abrasion of *former Rocks*, and the same, with respect to the separate masses composing *conglomerate* Rocks: if he will turn to p. 179 of my Report, and p. 253 of your xlivth volume, he may perhaps perceive, that something more than justice has been contended for, in this case, in behalf of his friend Mr. Jameson. When I was in Sutherland, in August 1812, and had perfectly satisfied myself from very repeated and minute inspection of the conglomerate Rock there, of its true nature, I had opportunities of conversing with Sir Humphry Davy on the subject, and finding him most decided in the prevailing notions, as to the origin or mode of formation of these Rocks, and seeing that he daily passed by them, in his angling Excursions, I pressingly invited him to devote an hour, to examining with me, the *newly exposed faces*, which the blocks that had been recently cleaved, and quarries which had been opened, by the parliamentary Road-makers, in this conglomerate Rock, so readily and numerously presented for our inspection: but this he declined, and even ridiculed the idea and proposal, before the company we were in!: this however did not discourage me, from pretty fully stating my observations and conclusions on this head, to Mr. Jameson, on my return through Edinburgh. Mr. J. with great candour heard and replied to my statements; and whence I conclude, that at

* See my recent enumeration of the chief British Strata, at the end of Mr. Sowerby's 1st volume of "Mineral Conchology," No. 19.

that

that time he had not committed to writing, or even fully embraced the opinions, on these subjects, which have since done him so much credit, in the opinion of all impartial Observers:—it is by still retaining in Geology, so many notions and opinions, which were worthy only of *the nurseries of our grandmothers*, that it remains in its present degraded state.

I had intended to have this month fulfilled my promise in p. 224, as to some remarks and inferences regarding Fossil Shells, but the above has too much encroached on my time, to allow it: the delay may also, I hope, give me the advantage, of some observations from yours or Mr. Sowerby's correspondents, on what has already been attempted, in the way of arrangement, by . . . . . . Your obedient servant,

12, Upper Crown Street, Westminster, JOHN FAREY, Sen.
Oct. 10, 1815.

P. S.—Your Correspondent "HOMO," (p. 229), has only to turn to p. 339 of vol. xliii. and p. 276 of vol. xlv. of your work, to find, that the interpretation of the *Mosaic account* of the Creation, which he mentions*, has long been entertained and enforced by me, as also in p. 515 of vol. xxxiii. of The Monthly Magazine, and in various other places: my observations on fossil Shells, for a future number, will further enforce this doctrine, I trust.

LI. *On the Cosmogony of Moses; with some preliminary Observations on Dr.* GILBY'S *Communication in Number* 209. *By* J. C. PRICHARD, *M.D.*

To Mr. Tilloch.

SIR,—I OBSERVE in the last number of the Philosophical Magazine and Journal, a communication by my friend Dr. W. H. Gilby, containing some ingenious geological observations, in the course of which he animadverts on a paper of mine which appeared some time since in the Annals of Philosophy, and imputes to me an opinion which is very different from that which I there maintained. I was so far from considering the mountain limestones as belonging to the transition formation, that the professed object of my paper was to ascertain the position, which they

* I don't mean to include that of his Commentator, where he seems to revive the absurd notion, of a *chaotic* state of the Earth. F. †.

† The Editor, to whom Mr. F. has assigned the profession of "Commentator," in the Notes which he subjoined to HOMO, thought he had so expressed himself that he could not have been misunderstood. He had no other aim than to state the opinions of *others*, tending to confirm the leading idea of HOMO. Mr. F. may for the offensive word *chaos*, read *matter*, and expunge the word *chaotic*, without injuring the sense intended to be conveyed by T.

and

and the independent coal-formation with which they are connected, occupy in the flœtz series; and the conclusion at which I arrived was, that they succeed the old red sandstone.

Having been induced to take up the pen, in order to remove a misconstruction of my meaning, I am unwilling to lay it down again without adding a few observations on a subject alluded to in your last number, by a correspondent who subscribes himself "Homo," viz. the comparison of the epochs of creation as unfolded by geological researches, with that series of events which is recorded in the Genesis. In the preface which Professor Jameson has condescended to prefix to Cuvier's Preliminary Essay, he has mentioned that the inquiries of naturalists have established many coincidences between these successions; and this has always appeared to me so *manifest* and *striking*, that I wonder at the misconception which appears to prevail on the subject. The desire of removing this, and setting in a clear point of view the relations in question, is the motive which induces me to trouble you with the following remarks, which, if you think fit, I beg you will insert in your excellent Journal.

The whole of these relations depend upon the sense affixed to the word *day*, as used in the Mosaic Cosmogony. It has often been conjectured, that in this place it designates an indefinite portion of time, and is synonymous with our expression *period*. But it appears to me that something more than conjecture may be offered in favour of this interpretation.

The Hebrew being the language of a nomadic people, who possessed few arts and still less of science, cannot be supposed to contain many abstract or general terms, nor does it in point of fact: consequently, when the Hebrew writers are in want of general expressions, they always adopt particular terms in a tropical sense. And this is the way in which general expressions were originally formed in all languages. The word *period* itself, in its first application to time, signified a single circuit of the sun. Therefore, if the author of the Cosmogony had intended to describe a succession of periods of indefinite length, he would necessarily have proceeded in this manner, and would have used some word which properly indicated a limited duration in a tropical acceptation. The question which remains is, What particular expression the Hebrews (and especially Moses himself) were in the habit of applying in this indefinite way; and the fact is, that the word יום indicating *day*, is the very one of which they made choice. It is used indisputably in that sense in chap. ii. ver. 4. "These are the generations of the heaven and earth when they were created, *in the day* that the Lord God made the earth and the heavens." This passage has already been quoted by Mr. Parkinson.

It

It is therefore clear that the term *day* will bear the sense of an indefinite period, according to the genius of the Hebrew language. That it was actually intended to be so received in this particular place, appears to me fully evident from the context. Those who do not admit the supernatural intelligence of the author of the Genesis will yet allow him a great portion of natural sagacity and good sense, and will certainly acquit him of so palpable an absurdity as speaking of days in the literal meaning of the word before the creation of the sun.

If this interpretation be allowed, the following series of facts is found to be detailed in the Genesis:

1. That the waters of the ocean for a long time covered the whole earth.

2. That no organized being existed for a long time in this universal ocean.

3. That the water had subsided before the creation of organized beings.

4. That an indefinite period followed, during which the vegetable creation was formed. It is to be remarked, that this part of the creation was effected before the existence of fishes in the sea.

5. That in the next period the sea produced locomotive animals. This is the precise meaning of the Hebrew word used in this place, as any of your correspondents will find by consulting a lexicon, and it was so understood by the LXX. Zoophytes and testacea are of course excluded from this class, and, not being enumerated, must find their place in the æra of the creation which belongs to beings lower in the scale than locomotive animals, viz. that appropriated to vegetables, to which, in fact, zoophytes especially have a strong analogy. The creation of birds is referred to the same epoch with that of aquatic *locomotive animals*.

6. The creation of quadrupeds follows. No reason can be assigned for placing this event later in the series than the formation of aquatic animals; and if the statement be confirmed by positive proof, the correctness of the history is so much the more striking.

7. The creation of man was later than all the above-mentioned events.

Let us now try how far these facts can be proved by geological phænomena.

1. That the ocean covered the whole earth, cannot be questioned on any reasonable ground, because many of the highest mountains are stratified, and strata are allowed by all to be deposits from a state of chemical solution or mechanical suspension

in

in water. Besides, the rocks of which they consist often have a crystalline composition, and crystallization can only have been effected in water. The Huttonians, indeed, pretend that fire was the solvent: but then they find it necessary to assert that the fusion was performed in some hot Tartarus or Pyriphlegethon beneath the weight of the whole ocean.

2. That no organized being existed in the universal ocean, is evident from the total want of organic remains in the oldest class of rocks.

3. That the water had subsided before the creation of organized beings, is evident from the primitive rocks occupying the highest situations. Had it been otherwise, they would be found to be enveloped everywhere in a covering of flœtz rocks on mountainous tracts equally as in valleys and plains.

4. That in the next period organized beings of the simplest kinds were created, is evident from the series of formations containing these remains and those of no other creatures. This series begins with the transition rocks, and includes the mountain limestones and the rocks belonging to the coal-formation: in fact, all those strata which in South Britain are found in an inclined position. In the coal-formation we find impressions of vegetable bodies in great abundance, and in the limestones zoophytes and testacea.

5. That in the next period the sea produced locomotive animals, is proved by our finding their remains in the rocks which succeed them above mentioned, viz. in the first horizontal formations which in England lie over the inclined. Thus the lyas limestone contains abundance of the remains of fishes and those large marine animals which were erroneously supposed to be crocodiles. The remains of birds are so perishable that we could not expect to find many of them; but Blumenbach and Faujas St. Fond mention some specimens of them found in marle slates, together with numerous impressions of fishes, which seem to prove that they began to exist at this æra.

6. The remains of quadrupeds are found only in strata which are much more recent than all those above mentioned.

7. That man was created at a later æra than all the above-mentioned beings, is proved by a similar method. The reason why no human bones are found even in the newest rocks, is, probably, that all the rock-formations were deposited before the creation of the human species.

I may observe, that modern discoveries in physiology confirm this order of events. Animals only feed on animal and vegetable matter, while vegetable bodies, and probably zoophytes, derive nutriment from mineral substances. It follows that vegetables must

must have existed long before the animal creation, in order to prepare a store for the sustenance of the latter. Physiology and geology were equally unknown at the time when the Genesis was written; and it is certainly a most remarkable circumstance, that we find a detail of facts set down there, which accords so exactly with the results of recent discoveries.

But if this coincidence is surprising in itself, it appears the more so when we compare the Cosmogony of the Hebrews with the notions on this subject that prevailed among other nations of antiquity. We find invariably that all other speculations on this subject are founded on some fanciful analogy with natural processes that are daily observed. Thus the Egyptians pretended that the mud of rivers acted upon by the solar beams had generated all animals, including men, as they assured Diodorus that the mud of the Nile continued to generate rats even in his time. Many of the Greeks imagined that the world and all things in it grew from seeds; and the celebrated story of the mundane egg, or the egg produced spontaneously in the womb of Erebus, was another childish attempt to explain the origin of the universe by a loose and fanciful comparison with natural processes. Just of the same character is that of Virgil:

> "Cum Pater omnipotens fœcundis imbribus æther
> Conjugis in lætæ gremium descendit, et omnes
> Magnus alit vasto diffusus corpore fœtus."

Nothing of this kind can be found in the Mosaic Cosmogony; there is not the remotest attempt to explain the manner in which any thing was produced. For the sense attributed to "the Spirit of God moving on the face of the waters" by Milton and some modern paraphrasts, is altogether forced, and a pollution of the simple and sublime sense of the texts.

I will conclude by observing that one single fact seems to me of more importance than all the other inferences that can be collected from geology, and that is, the proof it affords that the animal creation really had a beginning. All men naturally feel a great difficulty in believing that any miracle, that is any event contrary to the course of their experience and to the usual tenour of nature, has ever taken place. In recognising, however, the proof that there was a time when man had no existence, and that at some particular time he began to exist, or was created, we receive evidence of so great a miracle that all those related in the Old and New Testaments appear quite trifles in comparison with it; and it being once granted that so wonderful an event as the former ever took place, the latter must be admitted as capable of satisfactory proof, due testimony being afforded in their favour. Now this great point has been, as I apprehend, incontestably established by geological researches; and I know of no other method by which it is possible to establish it: for all the metaphysical reasonings which have been essayed on this subject, not excepting the vaunted demonstration of Dr. Samuel Clarke, appear to me to be as visionary as any piece of chimerical nonsense from the Cabbala, and to afford not the smallest presumptive proof that the world has not existed in its present state from all eternity.

I have the honour to be, sir,

Your obedient servant,

Bristol, Oct. 10, 1815. J. C. Prichard, M.D.

LII. *Means of preventing the Development and Propagation of contagious Typhus. By M.* Le Roux, *Dean of the Faculty of Medicine at Paris: published with the Approbation of the French Minister of the Interior*.*

It is ascertained that every place in which any number of patients affected with contagious typhus have been immured, retains for a very long time in its whole extent, and still more in all the furniture which has been used by the patients, deleterious and contagious miasmata calculated to communicate typhus to those who are afterwards doomed to inhabit the place.

The method of disinfecting such a spot is very simple. It consists in decomposing and destroying contagious miasmata; and this method is certain. Chemistry has procured us these means, and the healing art teaches us their application and precepts which secure its success.

To the use of the medicaments which ought to be employed in different periods of the disease, acid fumigations ought to be added, which, by diffusing themselves through the air, seize upon the putrid and contagious miasmata with which it is loaded, combine with them, and consequently destroy their deleterious properties.

1. The fumigations of oxygenated muriatic acid according to the process of Guyton Morveau, the efficacy of which has been demonstrated by experience, ought to be managed in the following manner, in rooms not inhabited. Muriate of soda (common salt) 90 grammes (three ounces), black oxide of manganese seven grammes (two drachms).

This powder is put into a capsule of earthenware, which is

* *Annales de Chimie,* tome xciv. p. 327.

placed

placed on a stove*: sulphuric acid is then to be poured over.

Sulphuric acid (oil of vitriol) 60 grammes, or two ounces.

When the rooms are inhabited, these fumigations must be made very slowly, and by small portions. Into a capsule we must put two or three pinches of salt, and pour in, successively, some drops of sulphuric acid. This must be repeated at least four or five times in the course of the day.

Disinfecting flasks are prepared by putting into a large flask 120 grammes (four ounces) of muriatic acid, seven grammes (two drachms) of black oxide of manganese, two grammes (half a drachm) of nitric acid (common aqua-fortis). The flask must then be closed with its ground stopper, in order to open it occasionally, when it is necessary to destroy any putrid miasmata diffused through the atmosphere.

2. For sulphurous fumigations, a mixture is to be made of equal parts of sulphur or flowers of sulphur and nitrate of potash in powder; and we divide this mixture into small packets of half a gramme (nine grains) each, which are to be thrown, when wanted, on a charcoal stove.

3. The nitric fumigations are made in a large glass vessel or a shallow crucible, in which we put from 15 to 20 grammes (about four drachms) of concentrated sulphuric acid. It is to be placed on a sand-bath, which is to be slightly heated, and a little nitrate of potash, in coarse powder, is to be thrown into it from time to time. This salt is slowly decomposed: there is an acid gas extricated, which slowly diffuses itself through the atmosphere; and we may multiply those small apparatus in different parts of a sick-room without incommoding it.

4. With respect to the purification of bedding, bed-clothes, &c. &c. sulphurous fumigations are best. The evaporation of vinegar, its projection upon live coals, as well as the combustion of various aromatic substances so commonly employed, cannot be considered as a means of disinfection. All these substances may mask the smell which is exhaled from the secretions of a patient, but will not destroy the contagious and deleterious principle which supports and propagates the disease.

When an individual is suspected of carrying about with him the germ of contagious typhus, he ought to submit in a naked state to the fumigation No. 2, bathe, wash and rub himself, for the deleterious miasmata may adhere to the surface of his body. He ought to change his clothes, and not to use those which he

* The application of heat having no other effect than that of producing a more complete decomposition of the sea salt in the same time, the very slender object of œconomy which results has not appeared in general to compensate for the embarrassment of a heating apparatus.

has worn, until they have been disinfected. The only method is the fumigation No. 4, for clothes, utensils, &c.

For wards filled with patients, the fumigation No. 1, with the modifications alluded to in No. 2.

For empty wards which have been occupied by typhus patients, the fumigation No. 1; afterwards the walls and floors ought to be scraped, white-washed with lime and water, the furniture washed, &c.

In the article of the precautions for preventing the origination of typhus and arresting the contagion in its very outset, the instructions recommend the scrupulous employment, and several times a-day, of the same methods used in disinfecting wards. And finally, all the physicians, surgeons, nurses, &c. are to undergo fumigations every time they quit the ward. This precaution ought to be strictly attended to, in order to preserve them from infection, and that they may not spread it when they leave the hospital.

LIII. *On a Substance to which the Name of* Inuline *has been given. By M.* Gualtier de Claubry*.

M. Rose a few years since† made known a substance which he extracted from the root of the elecampane (*inula helenium*), and which he regarded as a peculiar matter, which might hold a middle place between starch and sugar.

No person, so far as I know, has repeated the experiments of M. Rose; but chemists have ventured various opinions as to the nature of this substance. M. Funcke, in a note on the analysis of the elecampane, speaks of the *inuline*, but does not say a word as to its properties‡.

Dr. Thomson regards it as a particular matter, and classes it among the number of the immediate materials of vegetables. He proposed to give to it the name of *inuline*. M. Tromsdorf also, regarding it as a peculiar substance, has called it *alantine*.

Dr. Henry, considering it on the contrary as a substance of doubtful existence, places it among those bodies the nature of which is not well known. He designates it by the name of *elecampane*. M. Thenard in his work on chemistry assigns the same rank to it.

Having had occasion to make some experiments on the substance discovered by M. Rose, I shall now detail them.

The name of *inuline*, given by Dr. Thomson to the substance in

* *Annales de Chimie*, tome xciv. p. 200.

† Gehlen's Journal, tome iii. p. 217.

‡ *Annales de Chimie*, tome lxxvi. p. 98.

in question, seems well adapted to it: I shall therefore use that term.

M. Rose assigns the following properties to inuline:

It is in the form of white powder, insoluble in cold water; it is suspended in it by agitation, and is deposited upon cooling; boiling water dissolves it easily. A solution of four parts of inuline in one part of water is slightly mucilaginous, but passes through a filter easily: the greater part of the powder is extricated upon cooling.

Alcohol precipitates inuline from its solution in a short time.

When placed upon burning coal the inuline flows almost like sugar: a thick white pungent but not disagreeable vapour arises: the smell resembles that of sugar when burning.

When we heat the inuline in an iron spoon, it melts, gives out a white smoke; and when the spoon becomes red hot, the inuline burns with a brisk and lively flame, and leaves but a very small carbonaceous residue.

On distillation, the inuline gives out a brown acid without any trace of oil.

The nitric acid converts inuline into malic, oxalic, and acetic acids.

On repeating the experiments of M. Rose, I proved most of the properties which I have mentioned as having been discovered by him; but I constantly saw the inuline burn on the simple hot charcoal without making it red hot in an iron spoon: it then presents a blue flame, and gives out a very strong smell of caramel.

The following are some properties which I observed:

Water at 60° dissolves four or five times its weight of inuline: we cannot bring the solution to the consistence of jelly, as with starch, but the liquor is viscous when it is highly concentrated.

The inuline which is deposited upon cooling retains a great quantity of water, in which it may be melted by raising the temperature. On continuing a gentle heat, we obtain scales which appear grayish, but which give an inuline of a fine white, when well pulverized.

When we wish to obtain inuline in a perfectly dry state, we must not attempt to dry it upon filters, for it adheres to them so strongly that it cannot be detached.

Iode forms with inuline a greenish-yellow compound, which is easily decomposed, partly at least, in a short time. The inuline remains slightly coloured yellow, and retains a small portion of iode.

The same thing takes place when we treat this compound with boiling water.

Potash dissolves inuline, but it does not form with it a magma as it does with starch: when we add water, the solution takes place completely, but the liquor is never perfectly clear.

Concentrated sulphuric acid precipitates inuline from this solution.

The inuline is dissolved in concentrated sulphuric acid, which becomes of a brown colour; ammonia precipitates the inuline from it; water and alcohol occasion no precipitate.

The nitric acid dissolves inuline in a cold state, and assumes a slight yellow colour. When heated we obtain, as M. Rose has observed, malic, oxalic, and acetic acids.

The water of barytes precipitates abundantly the solution of inuline; the precipitate is white; it is easily dissolved in nitric acid, and with difficulty in hydro-chloric acid.

The solutions of lime and of strontian form no precipitate in the solution of inuline.

The hydro-chloric acid is not sensibly dissolved in inuline, and does not form a jelly with it, as with starch.

When we boil 100 parts of inuline with four parts of sulphuric acid and a sufficient quantity of water, for twelve or fifteen hours, we obtain a small quantity of sweet matter, but which retains a bitter taste.

The infusion of gall-nuts precipitates the inuline from its solution in the state of a grayish matter, which is collected very speedily at the bottom of the vessels, and which appear slightly glutinous and elastic.

The solution of inuline does not precipitate any metallic solution; nor does it form any precipitate in silicated or aluminated potash.

In order to obtain inuline in a state of purity, we must boil elecampane roots in a great quantity of water; filter the liquor, evaporate it to the consistence of an extract, and treat this extract with cold water. A great quantity of inuline is precipitated, which we ought to wash several times, and always by decantation; we collect it afterwards and dry it slowly, but taking care not to place it on filters, as I have mentioned above.

Prepared in this way, the inuline has several peculiar properties. It resembles in a particular manner nothing but starch, and yet it is easy to distinguish it by the following properties:

The principal character of starch is to form a jelly with warm water, and not to be dissolved but in a very great quantity of water. Inuline on the contrary is easily dissolved in a small quantity of water, without giving any jelly, and it is deposited in a white powder upon cooling.

Distilled

Distilled starch gives pyro-mucous acid and oil.

Inuline does not afford the slightest trace of oil in this operation.

Iode forms with starch a compound of a very fine blue colour.

Inuline gives with iode a greenish-yellow compound.

The hydro-chloric acid as well as the alkaline solutions render starch gelatinous.

The inuline is dissolved without giving jelly.

Concentrated sulphuric acid carbonizes starch with extrication of sulphurous acid.

Inuline is dissolved in concentrated sulphuric acid without any smell of sulphurous acid, and ammonia can precipitate it from this solution.

As other substances, like starch, are susceptible of being converted into sugar by means of sulphuric acid, we cannot assign this as a character inherent in starch.

As to the property which Dr. Thomson regards as a distinctive character of starch,—that of forming with gall-nuts an insoluble compound,—it does not appear that it ought to be admitted, since Dr. Bostock has made experiments which contradict those of Dr. Thomson.

There remains only the property of forming with barytes an insoluble compound which inuline shares with starch; but this property does not seem of a nature to decide the identity of those substances.

We therefore think we may conclude, from what has been said, that inuline is a peculiar substance, that it cannot be confounded with any other known vegetable substance, and that it ought to be classed among the immediate materials of vegetables.

LIV. *Some further Particulars respecting Mr.* Woolf's *Steam-Engine.*

In our last two numbers we laid before our readers regular Reports of the work performed by certain steam-engines employed on the mines in Cornwall, and which we have continued in our present number, showing the comparative advantage in point of œconomy of fuel possessed by Mr. Woolf's engine over any other in use. Some of our readers having expressed a wish to be made acquainted with the arrangement of the valves for the two cylinders in his engines, we have in fig. 1. (Plate V.) given a complete section of the nozles and valves of the engines of his construction now working on the Wheal Abraham and Wheal Var mines,—those mentioned in the Reports above alluded to.

 A, the

A, the large cylinder, B the little cylinder, each in its steam-case. The steam is admitted from the boiler into the steam-case of the large cylinder by a communication at C, and there is a communication between this steam-case and that of the small cylinder, so that both of the steam-cases become part of the communication between the boiler and the little cylinder. D furnishes a communication for carrying back to the boiler any water that may be produced by condensation in the steam-case, before the engine is got up to her proper temperature.

E is the pipe from the steam-case to the engine, with its regulating valve. F the valve-box; the spindle of the one valve working through that of the other. The passage for the steam from the case into the small cylinder is between the two valves. G is the valve that opens the communication between the bottom of the small cylinder B and the top of the large cylinder A.

H is the valve that returns the steam from above to below the large piston; and I is the exhaustion valve.

When the engine makes its stroke, the upper valve at F suffers the steam to pass from the boiler (through the steam-case) upon the small piston; the valve G at the same time allows the steam to pass from the underside of the small to the upper side of the large piston, and the valve I opens to the condenser. These three valves open at the same instant of time.

When both pistons arrive at the bottom these valves shut, and the lower valve at F opens to return the steam from above to below the small piston; the valve H doing the same to the large one, and the engine returns in equilibrium: but the upper valve at F can be shut off at any part of the stroke, according to the load of the engine.

Those who are conversant with steam-engines will perceive, from the passing of the steam as above described from the upper to the lower side of each of the pistons respectively, that the engines at Wheal Var and Wheal Abraham are at present working *single*. Were these engines working *double*, the steam would on the down stroke be made to pass from the under side of the small to the upper side of the large piston, steam in the mean time coming in upon the small piston and the under side of the large being open to the condenser; and on the up-stroke steam would pass from the top of the small under the large piston, while steam was received from the boiler under the small piston, and the top of the large cylinder open to the condenser.

Fig. 2 represents another method of arranging and applying the power of Woolf's engine; for the arrangement admits of great variety of modification. The valves will be understood from the foregoing description of those of fig. 1; but the mode of working the engine fig. 2 is different. In this the steam is made

made to act first on the upper side of the small piston, the under side of both being then open to the condenser. (N. B. The upper side of the large piston is in this arrangement always kept in open communication with the condenser): of course the large piston descends in equilibrium, both sides of the large being open to the condenser. When both pistons reach the bottom, the exhausting valve is shut, and the steam from above the small piston is sent under both pistons, and both ascend;—the upper side of the large, as before observed, being open to the condenser, and the small piston ascending in equilibrium. When they reach the top, the under side is again opened to the condenser, and a fresh supply of steam admitted on the piston in the small cylinder, and the stroke is again repeated. In the engraving the pipe A *a*, which is the communication between the top of the large cylinder and the eduction pipe, is broken off at *a*, for the purpose of showing the lower valves, but is actually continued to and joins the eduction pipe at B.

Fig. 3 is the same in arrangement as fig. 2, only the action of the power is reversed—the valves which in fig. 2 are at the top of the small cylinder being here placed at its lower end.

LV. *Determination of the Laws according to which Light is polarized at the Surface of the Metals.* By M. Biot. *Extracted from a Memoir read to the Institute on the* 15*th of May* 1815*.

When Malus had discovered the polarization which light undergoes on being reflected at the surface of diaphanous bodies, he also ascertained that this phænomenon is not produced, at least in the same way, on the surface of the metals. Since that memorable epoch in science, he returned twice to this singular exception; but without doubt, if time had not been wanting, he would have seen the propriety of modifying the first ideas which he published, and the true laws of the metallic polarization would not now be a desideratum.

Several facts have since been added to the results of Malus. I have shown in my work upon Light, p. 137, that in general two kinds of reflections take place at the surface of coloured bodies: one, which seems to take place outside of the bodies, acts indiscriminately on all the luminous molecules, and produces a white ray, if the incident light be white; the other, deeper seated,

* *Annales de Chimie,* tome xciv. p. 209.

acts

acts solely on the luminous molecules which compose the peculiar tint of the body. The first, under a certain incidence, polarizes in a great measure the light in the direction of the plane of reflection, after the manner of diaphanous bodies; the second, on the contrary, does not produce this effect, or at least produces it with a much less intensity. Hence it is easy to conclude, that if we place a glass so that it may transmit or absorb the first species of light, it will reflect the other; and we shall see the body in its own colour, without any mixture of foreign whiteness. Such, in fact, is the process which I have employed in the work cited, in order to bring out the peculiar colours of gold, iron, and copper. But I thought then that the portion of light of which these colours are composed issued from the bodies with a polarization completely confused. M. Arago has informed me that a very considerable portion issued from all sides, polarized parallel to the surface of the body and perpendicular to the plane of emergence, which, in fact, I have since had an opportunity of verifying. We knew nothing more as to the mode of polarization which the metals exercise, when Mr. Brewster wrote me, that by reflecting several times on laminæ of silver or of gold, a streak of light already polarized, this light was so modified, that on analysing it with a prism of Iceland spar it was divided into two fasciculi differently coloured. I exerted myself to verify this remarkable observation; and the better to distinguish the nature of the tints, I made to fall upon the laminæ a streak of white day light previously polarized in black glass; then by varying the incidences of the rays on the laminæ, it was easy to ascertain that the tints, into which the fasciculus reflected was divided, were precisely those of the coloured rings reflected and transmitted which were observed by Newton; and that in this respect, as well as from the direction of the polarization, these phænomena followed absolutely the laws of moveable polarization observed in the thin crystallized laminæ. I communicated this analogy to the Institute on the 27th of March last, in giving an account of the new discovery of Mr. Brewster, and I instantly communicated it to that gentleman also. At the present moment multiplied observations put me in a condition of establishing it with greater certainty, and of proving that silver as well as the other metals modify the light which they reflect, precisely as crystals endowed with double refraction modify those which they refract; the number of successive reflections answering to the greater or less thickness of the crystal.—Such is the object of the paper I now present.

But by an accident which ought not to surprise in a field of inquiry so new, it happened that I at first observed different phænomena, at least in appearance, from those seen by Mr. Brewster.

Brewster. In fact, he described the tints of the reflected images as succeeding each other by simple alternations from the greatest to the smallest refrangibility, whereas I therein ascertained evidently the whole series of the rings reflected and transmitted. He indicated these tints as polarized, one in the plane of refraction, and the other in the perpendicular direction; whereas I found them polarized at equal distances from this plane, the one in the direction of the primitive polarization, and the other on the opposite side, conformably to the theory of oscillations: hence it followed also that a single reflection upon silver could not impress on the natural light any determinate polarization. Having spoken to M. Arago of this apparent contradiction, he assured me that he had observed that the light reflected by silver, like the other metallic bodies, always experiences a very sensible partial polarization, according to the plane of incidence; and he gave me a piece of polished silver on which in fact I could ascertain this property. This seemed to agree with the indications of Mr. Brewster, and to be contrary to mine. But as facts can never be really contrary to facts, I inquired what could be the difference in the elements of the two observations; and I at length thought that the different nature of the polish might have an influence on the mode of polarization exercised by the metallic laminæ. This was in short perfectly confirmed by experience.

We may give a polish to a metal in two ways, viz. by the hammer or by friction. The first mode consists in beating the metallic plate on a polished anvil with a polished hammer; after which a lustre is given to its surface by rubbing it with glove-leather impregnated with a very fine polishing red. This process when applied to silver gives it a very fine whiteness; but the images are always a little undulated, and as if frothy at the edges. In the abundant reflection of light which takes place, we do not recognise the lively and brilliant polish of looking-glasses.

The polish by friction is that which is given to telescope-mirrors: they are first rubbed on a stone of a very soft grain (hone-stone), and a brilliancy is given to their surface by rubbing them on pitch coated with putty of tin. Then, if the operation has been well conducted, the images are well defined, lively, and the reflection has all the specular appearance.

Now, by a very remarkable circumstance those two kinds of polish do not act in the same way upon incident light. I do not speak here of the quantity more or less considerable of light which the surfaces reflect, but of the mode itself in which they act upon the luminous molecules, and of the direction in which they polarize them. When the surface of silver or of any other metal has received the specular polish, it produces by regular

reflection

reflection two distinct effects. It impresses in the first place on a part of the incident light the polarization moveable around the plane of incidence; that is to say, it causes to oscillate the luminous molecules on both sides of this plane, in the same way as a thin crystallized plate or one of weak polarizing power makes them oscillate on both sides of its principal section; and in both cases equally the tints pass through the whole series of reflected and transmitted rings of Newton. But besides, the metallic surface imprints on a white portion of the incident light, the fixed polarization in the plane of incidence, in the same way as a thick crystallized lamina, or one of energetic polarizing force, gives to the light which passes through it the fixed polarization, in two rectangular directions; and in the same way as in all crystallized bodies I have shown that the luminous molecules pass progressively from the moveable to the fixed polarization, when they have penetrated to a certain depth, in the same way in every reflection between metallic laminæ we observe that a part of the light which had undergone the moveable polarization in the preceding reflections, takes the fixed polarization, which it can never afterwards quit if the reflecting laminæ are parallel; so that in this case, after a number of reflections, more or less considerable, according to the nature of the metal and that of the polish which was given to it, we ought to find, and in fact we do find, almost the whole light polarized fixed according to the plane of reflection. In the reflection upon steel, and probably on the other metals which take a very lively specular polish, the portion of white light which is thus taken off from the moveable polarization is incomparably the strongest; so that the phænomenon of the colours which the moveable polarization can alone produce becomes insensible, or cannot be perceived but in certain peculiar positions which theory alone can point out. Thus Mr. Brewster has announced that this phænomenon had not taken place upon steel and upon the alloy which is used for mirrors; but by guiding myself by the indications of theory, I succeeded in observing it in a manner not to be doubted, even upon the best polished steel. When we employ laminæ of silver which have received the specular polish, the portion of the light which takes the fixed polarization at each reflection is still more considerable; but it is nevertheless much less than upon the two metals which I have cited; by a necessary compensation, the portion which takes the moveable polarization is greater, and the phænomenon of the tints then becomes finer and more easy to observe. But the direction of polarization of the white fasciculus being precisely intermediary between those of the coloured fasciculi, it results that it also mixes with them in the refraction operated by the rhomboid; and

and it is only by refracting them in peculiar directions that theory indicates, that we may show plenty of evidence for the law of their tints. In short, this difficulty almost entirely disappeared in the laminæ of silver polished by the hammer. Then the portion of light, which takes the fixed polarization at each reflection, becomes extremely feeble comparatively to that which preserves the moveable polarization, at least when we do not present the laminæ to the incident rays under an extreme obliquity; for we know that in this case all the plane surfaces, even those which have been purposely unpolished, assume the specular polish. Thus by avoiding great inclinations, and confining ourselves to few reflections, the laws of moveable polarization are alone perceived, and the tints of the fasciculi which nothing alters are developed with the greatest regularity according to the series of Newton's rings. This case is fortunately that which first offered itself to my observations, and it served me as a guide to pass on to the more complicated case, in which the moveable polarization becomes less sensible, and the fixed polarization more considerable. Now, since the only difference of a polish more or less smooth determines more abundantly the passage of the light reflected from one of those states to the other; ought we not to conclude, that here, as in the crystals endowed with double refraction, the moveable polarization is also the first which is exercised, when the luminous molecules are sufficiently removed from the reflecting surface in order that the asperities of the latter should be insensible at the distance at which they are? But the distance always diminishing, and the effect of the inequalities of the surface becoming more sensible, it happens, if they are very little, that the reflecting force becomes energetic enough to give polarization to a great part of the luminous molecules; whereas, if the asperities are stronger, and consequently the reflecting power weaker, a greater number of these particles continue their oscillations without being fixed. We have here, therefore, in the action of the bodies on light, the example of an effect analogous to those of capillarity; for if, as M. Laplace has demonstrated, the latter are produced by the attraction more or less strong which a body exercises at its surface, according as that surface is plane, or concave, or convex, in the same way in the new phænomena which I announce, the different configuration of the reflecting surfaces exercises on the luminous molecules a different mode of polarization. But the phænomena of capillarity are produced by differences of curvature appreciable to our senses, and even to our measures, instead of our being obliged, in order to change the mode of action of bodies upon light, to produce undulations almost imperceptible, such as the unequal nature of the polish gives them. Further, we we might probably never have been able to obtain similar effects in the ordinary phænomena of refraction, because they take place at distances too short; instead of their becoming possible in the phænomena of polarization, which, depending on the reflecting powers, are exerted at distances much more considerable, as I have proved in several memoirs.

LVI. *On the Gold of the Coast of Guinea, and Methods of obtaining it in the Country.* *By* DENYS DE MONTFORT, *Member of, and formerly Secretary to, the French Society for exploring the Interior of Africa**.

THE mountains in the interior of Africa contain in their sides great numbers of gold mines: they are very seldom wrought, however, the natives confining themselves almost entirely to collecting the gold-dust which is found upon washing certain earths which may be termed auriferous. In many countries of this vast continent the earth is as it were impregnated with gold; and not only do we meet with it in powder, but in considerable masses. This gold has formed and still forms the object of a very extensive and lucrative commerce: the natives of the interior bring it down to the inhabitants of the coast, and the latter sell it in their turn to the Europeans, who have given it the name of the Gold Coast, where it most abounds. Sometimes the gold-merchants, who are also slave-dealers, treat directly with strangers, but the latter most frequently purchase gold which has already passed from nation to nation and through several hands. In spite of all the attempts which have been made, and particularly in latter times by the English, to penetrate into the interior of Africa, this interior is still very little known to us, and the city of Tombuctoo,—that city which is said to contain an immense population,—is still problematical, for we have nothing on the subject but the vague and lying assertions of some Moorish and African merchants. Some of the latter undertake long voyages, which frequently last upwards of a month. Being situated at two or three hundred leagues from the coast, they penetrate as much further into the interior in order to procure gold, slaves, and elephants' teeth, which they deliver to the European vessels or establishments. In short, these people are very mysterious in all their operations, and it is very difficult to obtain from them the slightest intelligence: not only their taciturnity, their reserve and jealousy, are obstacles, but their various languages furnish others, for it requires an interpreter always to make one-

* *Bibliothèque Physico-Economique*, tome ii. for 1814, p. 45.

self

self understood. Africa is so divided among tribes without number, that we presume it would not be difficult to reckon more than a thousand different languages, without including the numerous dialects which are derived from them. It is thus that we see arrive from the source of the river of Volta, the mouth of which is situated in 5° 55′ north latitude, people who from tribe to tribe, and from interpreter to interpreter, at length fall in with the great island of Malfi, a kind of religious capital, which, placed in the midst of the river, is still upwards of 60 leagues from the coast, and the inhabitants of which, almost all brokers, and of course linguists, end by accompanying them to the sea-shore.

Whether it is in small grains or in dust, the gold of Guinea is extremely pale in colour, although very pure; and it greatly resembles the filings of yellow copper, with which Negroes or other cheats mix it fraudulently. When a Negro plays this trick, if he is discovered (and this is easily done by aqua-fortis), he is instantly made a slave: a White man comes off a little better. But there is still another fraud which a buyer must be upon his guard against: this is when the gold has not been thoroughly cleaned; and as the sand mixed with it is quartzous, the nitric acid has no effect on it: in this case it requires a keen eye, a glass, or even the crucible if it be at hand. The gold-dust is the only part of this precious metal which the Blacks sell to the Europeans. The lumps, of which there are some so large that the king of Assianti possesses one requiring four men to lift it, (the Negroes call these pieces "image gold,") are held sacred, and when they do not exceed an ounce in weight are bored to make necklaces and bracelets for the arms or legs. They know also how to work and melt them. The principal image or grand deity of Akra is a man's head of solid gold, or perhaps even a naturally formed mass which has assumed that form.

The Black merchant is always extremely skilful in this commerce: he knows the price of what he sells with the utmost precision; and that there may be no fraud, he weighs it himself with scales which he always carries with him. Formerly this trade was much more considerable than it is now:—we shall see the reason presently.

The Negroes have in common with Europeans two ways of procuring gold, digging and washing. The Negroes of the coast are washers only, while those who live among the mountains are essentially miners.

The mountains of Guinea, at least those which we are acquainted with, are in general granitic and schistous; thin masses of granite, as their summits prove, have formed by the lapse of time,

time, and by their *detritus* the gneiss which forms broad beds on their lower flanks. In the rainy seasons, torrents descend from these mountains, carrying with them stones and gravel, which being torn from the higher rocks present the same elements. These mountains are filled with mines of gold and iron. The first of these metals seems to have been sought for by Negroes from time immemorial: as to the latter, they do not know how to use it, and it is not the interest of Europeans to teach them: gold is found in them in a primitive state in narrow stripes, and it is found as usual between two layers of a granite, finer, more compact, and more highly coloured than the rest of the rock: the Negroes have not yet thought of working the latter, but it is probable that avarice will compel them to do so, now that the slave-trade is abolished, and that the excess of population is forced to provide for itself: for, notwithstanding the enormous exportation of human beings being stopped, they have still their helots: these are Negroes who are slaves either from being taken in war, from being insolvent debtors, from having lost their personal liberty at play, or from being sold by their parents*. As to malefactors and rebels, they are uniformly sold to Europeans.

The Negroes, therefore, work only the auriferous sands and the gneiss or schistous beds and banks of granite, which constitute the base of their mountains, and which being friable are easily dug into. If they attack the sides, they dig a fosse in the first place from twenty to thirty feet in depth, on an indeterminate breadth, until they begin to be alarmed for the crumbling down of the earth; the gold, as being heavier than quartz, schorl, and feldspar, the constituent principles of primitive granite, has been deeper seated in their common fall: they begin to find it, however, at the depth of three feet: they had no idea of using props of wood until they were taught by Europeans, and nothing in the world could induce them to make a regular pit, or bury themselves under ground. In proportion as they advance in the work, the lumps are put into pouches fixed round their waists, and some miners get very rich, as they only pay the king a fixed and daily allowance. In 1790, the king of Assianti had six hundred slaves at work for him, each of whom engaged to supply him with half an ounce *per diem*, and some of them had so much good sense as to form a sort of company, and throw into a joint stock the fruits of their labours. The earth thrown up during the digging is laid in heaps on the edges of the fosse, where other miners, their wives and children, receive it in bags and carry it to the nearest river on their heads, for the Negro

* This case is extremely rare.

never

never carries any thing on his back. They wade into the river up to the middle, and then dexterously dipping in their bags, they wash and shake its contents, so as to make the gold fall to the bottom: they then pour off the sand and earth, and the gold-dust remains.

As to the gold-finders on the banks of rivers and the sea-shore, they are less fortunate in their researches, and it is generally women who are thus employed. They conduct themselves precisely like the mountaineers, who in their turn are more fortunate than those on the sea-shore: the latter collect in bags the sand thrown up by a tempest, and act precisely like the former by washing, &c. In general the price of gold is fixed in Africa, and never fluctuates: in Europe it is supposed to yield 25 per cent. profit.

But it is not so considerable now as it has been; for several African princes more powerful than others, and anxious to secure a monopoly, have compelled the weaker to renounce all searching for gold. Thus the sovereign of Akim, who has been conquered by the king of Assianti, dares not any longer work his rich mines: they used to furnish upwards of 80 ounces of gold per week to the coast, *i. e.* nearly 5000 ounces of gold per annum.

From what has been said, it is not to be wondered that the English have attached much importance to exploring the interior of Africa; and without admitting all the reports on the subject to be true, it cannot be doubted that the precious metal is very abundant, and that the mines may still be considered as virgin mines, never having been visited by Europeans.

In the year 1800 a Society was formed in France for exploring Africa, and it soon consisted of 300 persons: but it received no encouragement from the Government, and fell to pieces. For my part, I had quitted it previously, on being appointed mineralogist to the voyage round the world under Capt. Baudin.

Certainly, if France will consent to abandon for ever the odious slave trade, our august sovereign will have it in his power not only to promote greatly the welfare of his own country, but the peace and tranquillity of Africa. There exists no country in the world so susceptible of general cultivation: we know that certain districts in Africa are fertile in corn, and grain of every kind grows there intermixed with sugar canes lately introduced, and which protect the grain from hail. The plants of India, Europe, America and Australasia, or the fifth portion of the globe, will flourish there in perpetual spring, and the animals of all climates can be easily naturalized. The Negroes, whose respect for the Whites is extreme, notwithstanding what they have suffered from them, will cheerfully give up their fields to be cultivated by us. Hands, servants, and even slaves will not be wanting;

and

and this will be a true method of preventing these nations from massacring their prisoners of war, as the king of Dahomet does at the present moment. May our feeble voice on this subject reach the ear of royalty!

LVII. *Statistical Account of the Quicksilver Mines of Idria in Illyria. By M.* PAYSSE, *Superintendant of the Manufacture of Mercurial Productions*.*

THE authors who have written upon Idria, and among whom we may reckon Walter Pope, Edward Brown, Walvasor, Frederick Stampfer, Scopoli, Ferber and Hacquet, are generally agreed as to the æra of the discovery of this mine, which they date in 1497.

It is said that towards the end of the thirteenth century the valley and basin of Idria were covered with wood; that the situation had a savage appearance, two peasants only having huts there, and which were even far distant from each other.

One of these peasants, who we are told was a kind of cooper, having one day placed a tub under a spring of water which issued from the hillock on which the church of St. Trinity has subsequently been erected, found at the bottom of the vessel, when he emptied it, some globules of a white metallic substance, which struck him with astonishment. On returning home he collected this substance, and carried it to a jeweller in the little town of Bischofflaach, about four leagues off. The jeweller, having ascertained that the substance was mercury, employed every method of knowing from the peasant the place where he found it: but neither promises nor rewards could induce him to reveal his secret.

What the jeweller could not effect was obtained by a peasant named Cazian Auderlin, who promised to assist the discoverer as a partner in working the mine. In fact, these two peasants began their researches; and being contented with some very slight excavations in the schistous soil which contained the metal, they soon ceased to find the mercury in a native state, and they abandoned their labours.

Already, however, some peasants in the neighbourhood had become acquainted with the labours of our two miners; and scarcely had the latter desisted, when the former, guided by views of interest, united, and undertook digging in their turn: but being as ignorant as those who preceded them as to the nature of the bodies with which the mercury was united in the bowels

* *Annales de Chimie,* tome xci. p. 161.

of

of the earth, they ceased their researches when they no longer found this metal in the liquid state, or in the form of cinnabar.

A company of mining adventurers succeeded, guided by the hopes of better success, and instructed by scientific men, among whom the names of Witig Tholhauser and Florian Thater occur. This company commenced a series of operations suited to the state of knowledge at the time: they opened a vertical well for extracting the produce (which is still shown by the name of St. Agatha) on the side opposite to the first experiments.

In 1510 the Venetians, in making war upon the people who inhabited the Frioul, in which Idria is situated, seized the works by force of arms. This conquest, however, was not of long duration; for soon afterwards Maximilian, emperor of Germany, who was then at war with the Venetians, retook the country, and restored the mines of Idria to the original proprietors.

In 1525 a catastrophe destroyed the greater part of the result of fifteen years labours. A violent earthquake threw down almost the whole of the galleries; mountains which skirted along the valley were thrown down and precipitated into the torrent called the Idrixa, the course of which was changed in some places: and this unexpected accident ruined the company of miners.

The next adventurers were from Saltzburg; and about the year 1572 the labours were resumed with new activity, and in a regular manner.

In 1575 the Archduke Charles of Austria took possession of the mines as a conqueror, and he was the first who set about creating a regular administration for the benefit of the state.

Francis Toichel, who was sent as deputy from the Government to establish it, dug the vertical pit known by the name of Santa Barba: he contrived furnaces for separating the mercury from the substances which mineralize it in the bowels of the earth, and suggested the mixing of the sulphuret of mercury with lime, in order to isolate the metal from the sulphur. It was he also who gave the first ideas of the fabrication of saline mercurial productions.

The administration of Idria was then composed of a president who was chief director, five counsellors, and a secretary.

In 1803 a fire broke out in the inside of the mines, and occasioned great damage; but by the sage counsels of M. Fihold, who then directed the subterranean works, its ravages were stopped by introducing water into the interior. Several persons were suffocated in the act of assisting their comrades. M. Vorauer, sub-engineer, remained a long time insensible, but was

 at

at length restored to life; and M. Scherowitz the chief accompt-ant was dreadfully burnt.

It was in 1809, as a consequence of the treaty of Znaim, that the mines of Idria as part of the Illyrian provinces came into the possession of France; and the French administration lasted until 1813, when the Austrians occupied these provinces by force of arms.

The town and quicksilver mines of Idria are situated in 31° 33′ longitude, and 46° 16′ latitude, about twelve leagues north from Trieste, and twelve leagues north-east from Gorice.

Under the Austrian regime, Idria was a seignory dependent on the circle of Adlerberg.

Under the French regime, this seignory was a canton, and a mayoralty composed as follows:

1. Town of Idria. 2. Village of Lower Idria. 3. Hamlets of High Canomla. 4. Middle Canomla. 5. Lower Canomla. 6. Carnitza. 7. Iellitsch Scheuwerk. 8. Tschekounig. 9. Woiska. 10. Magdalenaberg.

The extent of this domain is about two leagues and two-thirds of a league square, two-thirds of which are forests.

When I was mayor in 1812, the population was 7060—of whom 4095 lived in the town of Idria alone.

The town of Idria is situated in an almost conical basin. It is partly built of the rubbish of the mine.

A small valley which rises at the well of Sagoda on the Idrixa, and which terminates at the mouth of the Canomla in Lower Idria, gives a course to the torrent of Idrixa. This torrent takes its source in the Julian Alps, about three leagues from the town. After passing through about nineteen leagues, and operating its junction with the torrent of the Canomla near Lower Idria, it throws itself into the Isonzo near Canale, not far from Gorice.

The small torrents of the Trebuschanka, the Zalla, the Nicova, and some subterranean springs which flow near the mill of Sagoda, at least double the volume of the waters of Idrixa. Without these kinds of springs, which are very abundant in all seasons, and which alone are sufficient for putting in motion the various wheels of the above mill, it is certain that the workmen of the Idrian establishment would want a moving power during summer, in order to put in motion the hydraulic wheels of the machinery for the mercurial preparations.

The torrent of Canomla (already mentioned) is greatly increased by the waters of the torrent of the Woitschiack, and by those of some subterranean springs which issue near the mouth of this last torrent.

The little valley of Canomla is more open and more extensive than

than that of the Idrixa; there are more cultivable grounds, and particularly meadows, in it.

Sluices for retaining the water destined for floating the timber necessary for the service of the mines have been made on several of those torrents; and the French during their short administration erected one on that of the Woitschiack, which is distinguished from the rest by its solidity and elegant construction. It was just finished under the care of General Andreossy when the French evacuated Idria in 1813.

Among the torrents which swell the Idrixa, there are none truly permanent but those of the Canomla and the Zalla, and even the latter is often dry in summer.

The narrow valley through which the Idrixa flows is bounded by two chains of mountains, the mean height of which measured barometrically is about 288 fathoms. They are in general calcareous, of the kind called transition. All these calcareous masses are frequently intersected by very much inclined strata of argillaceous schistus, particularly in the environs of Bas Idria. These schists sometimes contain very thin layers of beautifully white laminar quartz, and frequently also carbonated lime.

In other parts there are schists strongly depressed, reticular and curvilinear, in thicker strata, mixed alternately with quartz and carbonated lime of a decided whiteness. Every thing shows in this singular arrangement, that these various strata have undergone a slow or instantaneous pressure at the moment of their formation, and at epochs when these masses still possessed a certain degree of softness. This formation is particularly remarkable at the back of the mountain, the interior of which is mined, and which overhangs the canal the water of which feeds the pumping engines of the wells of St. Barba and St. Agatha. We find it also in the section of the chain against which the furnaces are situated, and in the ravines made by the waters, as well as in many other parts in ascending or descending the Idrixa.

Banks of a coarse *gres*, and a kind of breche almost entirely quartzous, are seen on the road which leads from the church of St. Trinity to that of Calvary; parts are decomposed by their exposure to the air; others resist its influence, and are not changed by the action of the most violent heat produced by large bellows. Thick strata of an argilo-calcareous schist frequently cover these siliceous masses.

The manganesiated argil, in which we meet with masses or detached pieces of sulphuretted iron, sometimes crystallized, are often found exposed in certain ravines of these chains, which the water produced by the melting of the snows seems to have laid bare.

The mineralogy of the mayoralty, although not very rich or various, nevertheless exhibits carbonated lime in great varieties. We find also black and gray marble very compact and of very close texture, as well as breches susceptible of a very fine polish.

The valley of Canomla furnishes white marble; saccharine carbonated lime, similar to the statuary marble of Carrara, the grain of which is not quite so compact; a carbonated lime containing alumine or stalactite in masses, a species of cavernous tuf. very useful for constructing arches. We there also find marls and argils of various colours, adapted for the potter and brick-maker; amorphous quartz in banks, very considerable in the vicinity of Trebuschanka, and crystallized in the confines of Tolmino and Walmistrate. Jaspers of various colours in thick layers are found in the country of Woiska: they appeared to be inclined strongly on the side of the torrent of Trebuschanka, where we observe their superficial decomposition in a very marked manner. There is also a kind of freestone (both fine- and coarse-grained) in the environs of the forest called Razore; coal freestone (*gres houilliers*) in the little valley of Tschekourig, as well as in the vicinity, where there have been excavations, and where there are traces of fossil coal.

Calcareous *thumacheles* susceptible of polish, shells agglomerated by an argillaceous paste containing sulphuretted iron, which is decomposed in the air, accompany the substances which have been extracted from the excavations practised at the time of the search for coal.

Amphibolic rocks and micaceous schists are found in several places.

Sulphurated iron inclosed in lumps in argil, oxide of the same metal, in this earthy substance, forming the luminous manganese-formed iron, is disseminated in the district of Pouschenick, in strata, the thickness of which has not been examined.

Finally, native mercury combined with various matters forms the chief mineral riches of this country.

I have already observed that the town of Idria was built at the bottom of the conical basin and over the subterraneous works. Its height above the level of the sea is, according to the various barometrical measurements which I have made, 169 metres.

Although the town of Idria is not regularly built, it is nevertheless pleasing to the eye. The houses in which the workmen live, far from being distinguished by a black and dismal look as is usual in the vicinity of mines, seem on the contrary, from their external neatness, to indicate that the inhabitants enjoy ease and comforts not usual among this class of workmen in other countries.

Two

Two or three principal buildings are here remarkable:—a fine parish church, and a castle for accommodating the chief director of the mines, and for containing the treasure; the magazine for the products of the mine; the hall of the council of administration; and all the public offices; a very fine granary or store-house for the inhabitants as well as for the workmen; a very fine forge; some houses for those employed by Government, for the curé and his vicars, &c.

There is also a public school with six professors, and a female to superintend the education of the young girls; a very fine hospital, to which two physicians and a surgeon are attached, and a dispensary for giving medicines to the sick. These were among the improvements executed under the inspection of the French.

The situation of the town of Idria does not expose it to the tempestuous winds which are common on the shores of the Adriatic. The *Borra* and *Sirocco*, which are the most violent of the season, pass above the chain which overhangs Idria, follow the course of the torrent, and are consequently but very little felt by the inhabitants: the air, however, is not stagnant; for it is constantly renewed in consequence of the vicinity of forests, an active vegetation, and a rapid torrent. It is remarked that a current exists constantly in the atmosphere, from the direction taken by the vapours which rise from the furnaces for the distillation of mercury, and those for the fabrication of mercurial productions.

The variations of the barometer are neither very frequent nor very considerable at Idria. The mean height of the mercury, according to my observations for four years, on a barometer of the diameter of eight lines, was 27 inches 5 lines*.

The seasons are less inconstant at Idria than in many other countries of the globe: they are generally rainy in autumn, very cold and snowy in winter. Thunder storms are very frequent during summer, and there are sometimes two or three in a day; or rather the great atmospherical currents carry them there.

The snow covers the mountains of Idria as well as the valley nearly six months in the year, and is often five feet deep: the great road from Layback to Idria is then intercepted; and the administration is forced to employ three or four hundred workmen to clear it, in order to convey provisions to the population.

* It may be of importance to remark that an instrument of a large diameter is requisite for observations which require great precision. The oscillations of the mercury in large diameters are constantly more sensible and more regular: attraction and thermometrical effects may also be regarded as insensible in these large instruments.

The cold weather although of long duration is not severe; but the thermometer rarely descends to 15 degrees below zero of Reaumur. The heat of summer is frequently oppressive. The mountains which form this basin being generally calcareous, and almost entirely devoid of wood; these masses are easily heated, and reflect the calorific rays in the town with great intenseness: the rocks, therefore, particularly those with a south exposure, contain a great number of serpents, vipers, lizards, and insects in great variety. The red viper is very common there.

[To be continued.]

LVIII. *Notices respecting New Books.*

Mr. Thomas Forster has it in contemplation to publish, in the course of a few months, and to continue periodically, a work entitled "*Journal de Météorologie, avec d'Observations sur les Phénomènes de l'Atmosphère,*" &c. &c. Mr. Luke Howard and other meteorologists have promised their occasional assistance. The object of the work will be to publish journals of meteorology kept in various parts of Europe; together with occasional essays on subjects immediately connected with this science. The publication and the period of the appearance of the first number of the work will depend on the success the gentlemen concerned in the work meet with, in receiving journals and other communications from abroad. Letters on meteorology, notices of meteors and other occasional phænomena, besides regular journals, are within the plan of the work. The principal object of this work is to collect accounts from abroad, and to communicate to the continent the discoveries made in this country.

Mr. Sowerby, the indefatigable naturalist, of No. 2, Mead Place, Lambeth, has published a most valuable Catalogue of coloured Drawings of English Medicinal Plants, as a Companion to the Materia Medica of the College of Physicians. The intention of Mr. Sowerby in his present undertaking, is to make medical practitioners (whose knowledge of botany may not be extensive) acquainted with all the English plants which they may have occasion to use. We presume we shall gratify our medical and scientific readers highly by selecting the catalogue of mushrooms, as divided into eatable and poisonous, which Mr. Sowerby has faithfully depicted in elegant coloured prints. The medicinal plants, strictly so called, are fifty-two in number; the mushrooms are seventeen in number, fourteen of which are eatable, and only three poisonous. Many fatal accidents have occurred,

occurred, however, precisely from the want of that kind of information which it is Mr. Sowerby's object to diffuse.

EATABLE MUSHROOMS.

Agaricus campestris,	Boletus hepaticus,
cantharellus,	Clavaria muscoides,
cinnamomeus,	Helvella esculenta,
deliciosus,	Hydnum imbricatum,
pratensis,	repandum,
violaceus,	Tuber album,
Boletus edulis,	cibarium.

POISONOUS.

Agaricus Georgii,	Agaricus muscarius.
virosus,	

Dr. Spurzheim has just published a new edition of his Physiognomical System. It contains a great deal of new matter, as the Doctor has made very considerable improvements in the science since his arrival in England. Among the many discoveries which Gall and Spurzheim have made, none are perhaps more curious than those relating to hydrocephalus, and its effects on the structure of the brain. In pages 157 and 158 of the above work, are some very curious observations relating to the paper originally presented to the National Institute of France in 1808, and on a paper read at the Royal Society in 1814, the substance of which will be further discussed in a future number of our Journal.

LIX. *Intelligence and Miscellaneous Articles.*

Correspondence of M. Van Mons.

[Continued from p. 232.]

"Why do men of science persist in saying that meteoric stones fall from the heavens, as if planets could contain within their bodies a force of projection superior to their force of gravity, and capable of pushing their matter beyond the limits of their attraction, or as if masses so enormous could be formed in the air; where, beside, there exists no base of bodies as in azote, without an extrication of air, felt over the whole globe, being the effect of it. We no longer hear of buildings, men, or animals, which the fall of such kind of stone has injured*: the

* Here we beg leave to remind our valued correspondent, that in the very last shower of meteoric stones, recorded as having fallen in the South of France, the roof of a house was injured. Vide Phil. Mag. vol. xliv. p. 100.

phænomenon

phænomenon always takes place in open grounds, and where, without finding much resistance, the stone may sink into the ground: therefore, they do not fall, but are formed of the substance of the soil which the lightning puts in a state of fusion. If this substance be pure silex, the stone forms rock crystal rounded like flint. If it be a mixed soil, the flux is also mixed, and some of its oxides may by the force of the fire be reduced, nay it is even compounded into metal. Let the stone be weighed, and let us measure the metrical capacity of the hole which it has formed; the difference will not be great.

"I saw when a young man a meteoric stone *fall*, as it was called. A globe of fire, flat in front and lengthening behind into a tail, advanced towards me, gradually coming nearer the ground until it was at the distance of thirty paces: an explosion took place on the ground, with a repeated cracking noise, projecting a whirlwind of dust into the air, and giving out a sulphurous smell which affected me. After the dust was allayed, I approached the place where the explosion took place; and I there found in a hole broader than it was deep, a stone partly gray, partly brown, and which exhibited rather a conglomeration than a flux; and there was no portion of reduced metal found. I formed at that instant the same idea of its origin which I now entertain of all the other stones of that nature. I regarded it as proceeding from the flux of the soil: supposing it to be of celestial extraction, it must have travelled in the centre of the inflamed globe, which would have been too grand an escort for a stone so coarse. Subsequently I analysed this stone, which might be a foot in circumference, and I therein found plenty of sand, argil, lime, carbon, and iron; I sent the remainder to M. Delametherie. It was on a road made across a heath that I found this stone: it preserved a very strong heat for a long time.

The sub-oxygenated chlorine of Davy, &c. which Gay-Lussac had already obtained, may be regarded as an oxygenate of this body, and which has elements as if chlorine and euchlorine were united together. The chlorine is a salt, in which the oxygen performs the function of an acid, and the dry muriatic acid, the function of an oxide: an addition of oxygen renders it oxygenule, and still more renders it oxygenously dissolved: then it is in the state of euchlorine. I say that in the chlorine the acid performs the office of an oxide, because it is that which displaces caloric from the oxygen; the second oxygen also undergoes this displacement; but the third oxygen must bring caloric, because it ought to replace that which the muriatic acid has displaced from the two oxygens, salification and oxygenulation: of this the solution consists.

"Berthollet has recognised a state of chlorine in which the muriatic

muriatic acid is in hyper-combination to the oxygen; it is an oxygenate oxidinulated by the acid, and in which the excess of acid is saturated by the water performing the functions of oxide. The muriatic acid gas is muriate of *hydrose:* by adding water until the acid undergoes no more displacement in its caloric, there are formed a muriate with excess of water and a true hydrinulated salt; with still more water, caloric is fixed, and the acid becomes dissolved. Chlorine ought to have its complement of oxygen, or be oxgyenously dissolved in its muriatic acid, in order to give physical marks of acidity: it is thus that, in sulphurated hydrogen gas, the sulphuric acid of the sulphur ought to be hydrogenously dissolved in order to give the same marks; in the sulphur it forms sulphate of hydrogen; in hydrogenated sulphur it is sulphate with excess of hydrogen, or hydrogenulated sulphate. The concentrated sulphuric acid is sub-hydrinulated sulphate of *hydrose*; with enough of water to prevent it from heating it is hydrinulated sulphate of *hydrose;* and with still enough of water not to cool any more, it is acid, or sulphate of hydrose dissolved; and so on with respect to the other combustibles and acidifiable burning bodies (*comburans*) and their acids. I am impatient to be made acquainted with the new work of Sir H. Davy, when completed.

"M. Kastner informs me that he is about to publish a Journal adapted to the use of the friends of manufactures in Germany; and that he is drawing up for publication at Easter, A Report on the Progress of various Nations in Chemistry, which Report will be annual. M. Kastner has made a great number of experiments with iodine, which will appear in the next number of Schweiger's Journal. Among other things he has found that iodine is a reagent for sugar as it is for starch; for while water, a solution of gum, spirit of wine, vinegar, &c. turn brown with iodine, the solution of sugar remains colourless; some cheese-like flakes merely being deposited. It results that with those two substances the oxygenated iodic acid forms oxygenated iodates fully saturated, for it is only the super-combination which gives the colour.

"When recently attending to the production of fulminating silver by a nitric acid obtained from the acid of the residues of sulphuric ether, I found out a favourable process for procuring muriatic ether: Take a residue as from the manufacture of such an ether; add to it the half of the first quantity of alcohol, and heat without boiling for an hour in a curved retort well luted. Afterwards mix with the liquid which from acidinulated sulfat of ether has become neutral sulphate, as much sea-salt as the primitive sulphuric acid can decompose. This acid is substituted

substituted for the muriatic acid, and the soda is substituted for the ether, and the two displaced bodies unite with each other. The ether obtained is liquid, and therefore hydrated; but with the help of calcined muriate of lime, we may constitute it in part in the state of gas, which nevertheless is not entirely exempt from water, since its decomposition in a red-hot tube gives muriatic acid gas: it also gives carbon allied to a combustible which is probably the acidifiable radical of the muriatic acid, since, being burnt in dry oxygen gas, it is converted into carbonic acid gas and into muriatic acid gas."

[To be continued.]

To Mr. Tilloch.

Sir,—As I have no doubt but that most of your scientific readers understand the principles of a common gun-lock, I should feel much indebted to any of them, if they would favour me with a general expression of the curve necessary for the construction of the hammer, so that the flint may meet with an equal resistance in every point of the curve; likewise allowing the cock and hammer to be urged by constant forces (in every position of them) by their respective springs: the rest of the data, I believe, will be readily perceived from the construction of the lock.

I am, sir,

Your obliged and humble servant,

Oct. 12, 1815. Rusticus.

HYDROPHOBIA.

Professor Hufeland of Berlin has announced in his Journal, that bleeding in cases of hydrophobia (first brought before the European practitioners by an article from the East Indies published in The Philosophical Magazine) has met with equal success in Germany as in India. He intends to publish some of the cases forthwith.

We lament to have to announce the death of Gehlen, many years the Editor of an excellent Journal on Chemistry and other sciences, and a profound chemist. He fell a victim to his ardent desire to promote the advancement of chemical knowledge. He was preparing, in company with his colleague Mr. Rehland, some arsenated hydrogenic gas; and whilst watching for the full development of this air from its acid solution, trying at every moment to judge, from its particular smell, when that operation would be completed, he inhaled the fatal poison which has robbed science of his valuable services.

GAS

GAS LIGHTS.

This valuable improvement in science is about to be extended to the British Settlements in North America: the following communication on the subject appears in a recent Halifax paper:

"Fort Ellis, Nova Scotia, August 22, 1815.

"Mr. Editor,—From a sense of the great benefits which society may derive from the following, we solicit a place in your valuable paper, so as to communicate our discoveries, for the general benefit of mankind, and of this province in particular. We think its utility will be universal throughout the whole continent of America, as it may be carried into effect so as to supersede in a great measure the consumption of candles and oil, whether in streets, warehouses, stores, workshops, or dwelling-houses; it will be found beneficial to every rank and class of inhabitants. The simplicity and easy expense of materials, and in constructing an apparatus to put it into practice, will, we trust, render it an object worthy of public attention. We took about *six* ounces of birch bark, and about *two* ounces of pitch pine knots, which we put into a metal tea-kettle holding about a gallon; we secured the top with clay, to prevent the gas evaporating therefrom: then placing the kettle on the fire, in about *five* to seven minutes the current of smoke began to play out of the nose of the kettle, to which we applied a lighted candle: it communicated with the gas as quick as gunpowder, and continued burning with a clear and bright light, equal to *three* candles, for the space of *one hour and thirty minutes*. At the next trial, after cleaning the kettle of all the substances, so as to ascertain the effects of the bark alone; we put *ten* ounces of birch bark, and lengthened the conductor with reeds, or rush-pipes, and applied a lighted candle as formerly to the current issuing from the pipes, and it continued to give a most beautiful light for the space of *three hours* without ever burning the reeds. We observed that there was about one or one and a half inch of current between the reed and the flame. The longer the conductor the more pleasant the light, so that from one conductor there may be a variety of lights, and that too may be carried to the distance of miles by means of a conductor. Though the assertion may seem strange to some, to those who are any way acquainted with the nature of gas we are assured it will meet their approbation.

"Thus far, sir, we have detailed, and trust that future experiments will give demonstrative proof of its utility.

(Signed) "James Harris.
"James Harper."

Messrs.

Messrs. Salisbury and Co. of the Old Buffery's Iron Works near Dudley, have discovered a mode of preparing cast iron, which gives it toughness, flexibility and elasticity, promising most valuable results to the arts, and to architecture both civil and naval, particularly in the construction of bridges. Mr. Brande, of the Royal Institution, is engaged on a series of experiments to ascertain the comparative strength of common cast iron, wrought iron, and the prepared cast iron of Messrs. Salisbury and Co., the results of which we expect to be able to lay before our readers in our next number. In the mean time the trials which have been made by others of the prepared cast iron, have answered every expectation.

NATIVE IRON.

Mr. Konig, on the authority of a letter from Baron Moll, of Munich, has announced (in Dr. Thomson's Annals of Philosophy for August last) that in October 1814 a mass of native iron weighing about 200 pounds was discovered by a shepherd at Lenarto, in the comitate of Sarosh, on the declivity of a small range subordinate to the Carpathian mountains. Its colour internally is light steel gray approaching to silver. It is covered by a thin coat of rust: its surface is uneven, rough, and marked with impressions. It presents three cellular cavities, but they are without the olivine-like substance found in those of the Siberian native iron. The mass is irregular and flat, as if compressed: its fracture is hackly, it takes a high polish, is perfectly malleable cold, and its solution in nitric acid is of a light emerald-green colour.

NEW PETRIFYING SPRING.

The following paragraph appeared in the Cambridge Chronicle of 11th July:—"During the last week two persons belonging to the University printing-office called upon the Professor of Mineralogy (Dr. Clarke) with specimens of what is vulgarly termed 'petrified moss,' said to have been found 'in a petrifying well near Coton.' The Professor has been to the spot; and having no other means of communicating to the University, at this season, what he conceives to be the greatest natural curiosity in the county, he has desired the Editor of this paper to inform his readers, that the said well is in all respects similar to the celebrated petrifying spring of Matlock in Derbyshire; incrusting moss, rushes, &c. with a deposit of carbonate of lime, so as to exhibit a beautiful reticular stalactite. Persons going from Cambridge are recommended, for a guide, to the cottage of William Paulett, the first on the right upon entering the village of Coton. The well is situated in a field belonging to Mr. Angier, who has given permission to this man to conduct strangers to the spot."

STEAM-

STEAM-ENGINES IN CORNWALL.

From Messrs. Leans' printed Report, it appears that in the month of August 33 engines consumed 78,421 bushels of coals, and lifted 659,171,000 pounds of water one foot high, being an average of 19,975,000 for each engine.

Woolf's engine at Wheal Var during the same month consumed 830 bushels, and lifted with each bushel 48,152,000 pounds one foot high; his engine at Wheal Abraham during the same period consumed 1314 bushels of coals, and by each bushel lifted 42,482,000 one foot high.

During the month of September, 32 engines consumed 87,792 bushels of coals, and lifted 589,912,000 pounds one foot high = 18,372,000 for each engine with one bushel of coals.

At Wheal Var in September, Woolf's engine with each of 180 bushels raised 47,690,000; and for part of the month with each of 594 bushels consumed, lifted 44,377,000 one foot high.

Woolf's engine at Wheal Abraham consumed in September 1278 bushels, and each bushel lifted 49,284,000 pounds one foot high.

By a letter from Cornwall of the 18th of October we are informed, that up to that date Woolf's engine had been performing from the 1st of the month so high as 56,000,000 with each bushel of coals consumed.

LECTURES ON ELECTRICITY.

Mr. Singer will commence a Course of Lectures on Electricity and Electro-Chemistry on Monday the 6th of November.

Particulars may be had of Mr. Banks, 441, Strand; and at the Lecture Room, 3, Prince's Street, Cavendish Square.

SURRY INSTITUTION.

The following Lectures have been announced for the approaching season:

1. On the Philosophy of Art, or Principles of Connexion between Nature and the Arts of Painting, Poetry, Music, &c. &c. by John Landseer, Esq. F.S.A. and Engraver to His Majesty; to commence on Tuesday the 21st of November, and to be continued on each succeeding Tuesday.

2. On Chemistry, by James Lowe Wheeler, Esq., to commence on Friday the 17th of November, and to be continued on each succeeding Friday.

3. On Music, by W. Crotch, Mus. D. Professor of Music in the University of Oxford; to commence in February 1816.

New Patent.—To Thomas Ashmore, now resident at Portland Hotel, Portland Street, in the county of Middlesex, esq. for his new mode of making leather.—9th Sept. 1815.—6 months.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For October 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Sept.	27	51	62	50	29·92	29	Stormy
	28	47	61	52	30·08	42	Showery
	29	55	62	55	29·45	36	Showery
	30	49	61	50	·52	44	Showery
Oct.	1	54	60	47	·52	31	Showery
	2	47	59	50	·62	30	Showery
	3	47	58	55	30·10	42	Fair
	4	55	61	55	·01	32	Fair
	5	55	60	56	·01	46	Fair
	6	55	60	50	·02	0	Rain
	7	47	60	49	·10	26	Fair
	8	46	54	49	·29	29	Cloudy
	9	46	53	48	·28	34	Fair
	10	47	54	47	·08	35	Fair
	11	48	52	50	29·85	0	Rain
	12	50	54	19	·69	19	Cloudy
	13	52	56	54	·74	36	Fair
	14	51	55	50	·81	40	Fair
	15	48	57	54	·90	36	Fair
	16	57	63	54	·90	36	Fair
	17	56	58	55	·84	40	Fair
	18	56	60	54	·52	0	Rain
	19	57	61	55	·46	16	Stormy
	20	60	61	56	·43	18	Stormy
	21	53	58	47	·68	29	Fair
	22	47	56	50	·80	39	Fair
	23	50	55	50	.50	0	Rain
	24	51	57	51	·44	36	Stormy
	25	47	57	49	·44	39	Fair
	26	44	54	45	·50	37	Fair

N.B. The Barometer's height is taken at one o'clock.

LX. *Experiments tending to prove the Possibility of causing Balloons to deviate considerably from the general Direction of the Current of Wind which carries them along in the Atmosphere.* *By Mr.* JOHN EVANS, *Jun.*

To Mr. Tilloch.

SIR,—THE numerous methods hitherto proposed for guiding balloons have so totally failed, that philosophers of the present day seem much inclined to regard the solution of the problem as an impossibility. The writer of the present paper is fully aware of this circumstance; and, in offering to your notice a plan which he has had the satisfaction to see attended with success, cannot but express his wish that no position here advanced should be received, unless it be evidently founded on the laws of mechanics, or directly proved by experiment.

The first object ought certainly to be the discovery of a moving power sufficiently great to overcome the vast resistance which a balloon experiences from the air. Indeed, the failure of all the experiments which have been made with oars, is evidently to be attributed to the very trifling size of the machinery and the smallness of the force employed on it. In the present plan, the ascending and descending forces, or in other words its levity and gravity, are alternately the moving powers. It is evident that these forces are sufficiently powerful. It will, however probably be urged that the ascending and descending forces always act perpendicularly, and hence cannot be of any service in the present inquiry. That these forces always act perpendicularly is readily granted, but it by no means follows that the *motion* which they cause the balloon to assume should be in a perpendicular direction. The direction of the motion is regulated by another cause, viz. the form of the resisting surface; and hence, spherical balloons having their form perfectly uniform on all sides must necessarily ascend perpendicularly. Now the principle of the present invention consists in applying a resisting surface in such a manner as to cause the ascending or descending motion of the balloon to deviate by a considerable angle from a perpendicular direction. This was attempted and accomplished by a method which I now proceed to describe.

A square plane ABCD (fig. 1, Pl. VI.) is formed by extending paper or linen on the cross constructed of the slight pieces of wood AC and BD. This plane is attached to the balloon by strings adjusted so that the diagonal BD shall constantly lie parallel to the horizon, whilst the other diagonal AC may be placed at any angle whatever. A small triangular sail E is also applied, which can be moved to either extremity of the diagonal BD.

Having completed this apparatus, let the plane be placed so that the diagonal AC shall form with the vertical line AF an angle FAC, equal to 70°. Before I relate the experiments, I will attempt to ascertain the direction in which such a balloon might be *expected* to move, assuming the proportions used in the first experiment, when the ratio of the surface of the attached plane to that of a circular plane of the same diameter as the balloon was 1·4 : 1.

The first object is to find the relative resistances of the balloon and plane, which may be effected thus: Supposing the resistance of the balloon to be 1, the resistance of a circular plane of the same diameter, and moving in a direction at right angles to its surface, would be 2. Again: The square plane attached to the balloon opposing 1.4 times as much surface as such a circular plane, would have a resistance equal to 2·8, if it were to lie in a horizontal position. But the breadth of the column of air which it meets is evidently as the sine of the angle of incidence, as is also the force with which it will strike that column: hence the resistance, being in the compound ratio of the surface and the force, will be as the square of the sine of the angle of incidence. In the present case the angle of incidence being 70°, the resistance of the plane is expressed by $2{\cdot}8 \times \sin(70^\circ)^2 = 2{\cdot}472$, and the resistance of the balloon and plane united is equal to $1 + 2{\cdot}472 = 3{\cdot}472$. Now it is evident that the velocity of ascent or descent will be accelerated until the resistance becomes equal to the power or weight of the balloon, which may be therefore expressed by 3·472 also. This being ascertained, let the straight line AB (fig. 2 and 3) be drawn equal to 3·472, which may represent the ascending power (fig. 2) or the weight (fig. 3) of the balloon: draw another indefinite straight line AC, making the angle BAC=70°, which will mark the position of the plane. Then, since the angle of reflection is always equal to the angle of incidence, and the angle at which the plane strikes the air is 70°, the angle of reflection would also be 70°. Therefore a straight line AD, equal to 2·472, and making the angle DAC equal to 70°, will represent the resistance of the air to the plane. The balloon and plane being acted upon by the two forces AB and AD, will consequently describe the diagonal AE of the parallelogram ABED, varying from a perpendicular direction by the angle BAE, which is found by trigonometry to be 45° 12′. From the figures it is also evident, that the balloon when descending would move in a direction (from A to E, fig. 2) directly contrary to that (from A to E, fig. 3) in which it would move when ascending. The preceding rough calculation cannot

not be deemed very accurate, on account of the uncertainty of the present theories of resistance, but will serve to give us a general idea of the effect likely to be produced.

The first experiment was performed on April 18, 1814. The diameter of the balloon was 4·5 feet, and the plane was constructed in the form of an ellipse of which the diameters were 4·5 and 6·36. The plane being elliptical instead of square could not possibly make any perceptible difference in the effect, which wholly depends on the surface.

The balloon was inflated in a building of considerable height, and there suffered to ascend. The experiment was attended with the utmost success: for the balloon, instead of ascending perpendicularly, *actually moved at an angle of* 45° till it reached the ceiling. The power of the balloon being soon exhausted, it descended along the very track it had described whilst ascending, and returned to the spot whence it had set out. Here the result presents a striking coincidence with the preceding calculation.

This experiment was afterwards frequently repeated with a square plane instead of an elliptical one, as I found the former shape better adapted for furling. There was also added the triangular sail E (fig. 1) before mentioned, which, by increasing the resistance on the side to which it may be moved, is calculated to answer every purpose of a rudder. The experiment was now equally successful, the balloon turning to one side or the other, according to the position of the small sail.

The same experiment was performed in the open air for the first time, October 12, 1814. The balloon's diameter was 3·5 feet, and the length of the side of the square plane 4·25 feet. The triangular sail was fixed to one side, for the following reason: The balloon rising in the open air is carried by the wind at the same time that it is acted upon by the attached plane, and it would, therefore, be difficult for an observer to determine the effect produced by the apparatus. The triangular sail serves as a test for this purpose; since, if the great plane *did not* act, the former would feel no resistance, and no particular effect be produced on the balloon. But if the great plane *did* act, the small sail would constantly experience resistance, and have the effect of making the balloon ascend in a spiral direction. The balloon with its appendages was launched, and after a few seconds began to describe the expected spiral, which it continued to do until lost in the clouds.

On October 2, 1815, another experiment of the same nature was made with a balloon six feet diameter, having a square plane whose side was 7·5 feet, and a triangular rudder in proportion. The effect produced during the ascent was perfectly similar to

 that

that mentioned in the last experiment. With the aid of a good telescope, I had also the additional satisfaction of being able to observe the descent. Soon after the balloon became in equilibrio, the spiral motion ceased; but as soon as ever the balloon began to fall, it was resumed, but *in a contrary direction.*

These experiments appear to me to prove in a satisfactory manner the possibility of causing balloons to deviate by a very considerable angle from a perpendicular ascent or descent. This conclusion might be confirmed, if necessary, by analogy from several other contrivances of art founded on the same principle; such as kites, ships sailing with a side wind, &c. but I consider it would be superfluous.

As I have now described these experiments at full length, it will be but right to mention an inconvenience with which this method of aërial navigation is attended. Perhaps the reader may have anticipated the observation that, to make a balloon proceed to any considerable distance in a given direction, it is necessary it should ascend and descend alternately with great rapidity. In a Montgolfier balloon this may be easily accomplished by the mere increase or diminution of the fire, and, perhaps, by the addition of a large valve. A hydrogen-gas balloon, on the contrary, could not sustain for any length of time the continual losses of gas and ballast necessary for such repeated ascents and descents. Hence, until some means are discovered of ascending and descending in a hydrogen-gas balloon without loss of gas or of ballast, the present plan is applicable only to Montgolfier balloons. These balloons are generally regarded in England as extremely dangerous, although I am inclined to think the idea arises chiefly from the circumstance that in this country no person has ever yet ascended in one of them.

The mode of using the plane will perhaps be better understood by supposing that we have a large Montgolfier balloon furnished with the necessary apparatus, and that it is required to direct it to a particular spot. It would be requisite that the plane should be furled during the inflation, and the commencement of the ascent. When the balloon has risen to a sufficient height, the plane is unfurled, and set at an angle of 70° from the perpendicular. The triangular sail is next to be expanded and managed as a rudder until it brings the machine round to such a position that the elevated end of the oblique diagonal points towards that part of the horizon to which it is required to direct the balloon. The greatest possible power must then be communicated to the machine, which will *ascend describing the diagonal of a square;* and consequently move one mile horizontally for every mile of perpendicular ascent. After continuing a rapid ascent as long as convenient, the balloon must

be

be made to descend, at the same time adjusting the plane so that it shall slant in the contrary direction to what it did during the ascent. The balloon will now *descend along the diagonal of another square*, and in the same plane as before. These alternate ascents and descents are continued until the machine reaches the required spot.

The rate of the travelling of such a balloon may be estimated by the application of the common laws for the resistance of the air. On the supposition that the diameter of the balloon is 80 feet, the inclosed air will have a power of 8377 lbs., two-thirds of which may be allowed for the weight of the balloon, apparatus, men, &c. The remaining 2792 lbs. would give the balloon a velocity of 28 feet per second, or 19 miles an hour, which would also be the horizontal velocity. If this should not be sufficient for the object in view, it may be increased by using a balloon and apparatus, the velocity being in the sub-duplicate ratio of the diameter*. If, however, the principle could be applied to a hydrogen-gas balloon, the diameter necessary to produce a velocity of 28 feet per second would be only 40 feet.

It is obvious that the *absolute* motion *gained by this plane* will only be equal to the excess of the balloon's *relative* velocity in the air above the *absolute* velocity of the wind. The most serious objection, therefore, to all schemes which have for their object the direction of balloons, is this—that the velocity of the wind is generally so very great as to destroy in a considerable degree the relative motion of the balloon. This objection, however, derives its chief weight from an opinion entertained by many persons, that a balloon always moves with the very same velocity as the wind. This they attempt to prove by saying that a balloon being in perfect equilibrio with the surrounding air, becomes in effect a part of that fluid, and consequently moves entirely with it. This reasoning I do not pretend to doubt; but let it be observed that it does not at all apply to a balloon which is *not in equilibrio*. I am inclined to think that the further a balloon is removed from a state of equilibrium, the less is its motion with the wind, which opinion appears to be confirmed by the following easy experiment, which I have repeatedly

* When the diameter of a balloon is increased or diminished, the velocity is affected by the alteration of the *power* and the *surface*.

Let d be the diameter, p the power, s the surface, r the resistance, and v the velocity.

Now, since p is as d^3, and v is as $\sqrt{p}$, it follows that v is as $\sqrt{d^3}$, when the alteration of *power* only is considered.

Again, r is as s or d^2, and v is *inversely* as $\sqrt{r}$: hence v is *inversely* as $\sqrt{d^2}$ or d, considering only the alteration of *surface*.

The actual velocity is therefore as $\frac{\sqrt{d^3}}{\sqrt{d^2}}$ or $\sqrt{\frac{d^3}{d^2}}$ or $\sqrt{d}$.

 performed.

performed. When there is a very strong wind blowing, let a stone, a clod of earth, and a piece of paper, all having the same form and equal surfaces, be dropped together from a small height. It may then be observed, that *in the same time*, the paper will be carried to a considerable distance by the wind, the clod of earth to a less distance, while the stone is scarcely moved at all. This certainly tends to establish the fact, that a diminution of velocity does take place, which I am led by analogy to believe is in the sub-duplicate ratio of the difference between the specific gravity of the body and that of the air Hence, as the plan I have described requires the balloon never to be in equilibrio, it may reasonably be expected to move with a velocity considerably less than the wind.

I shall just observe, that the plane may with very slight alterations be made to serve as a parachute, which may be directed on the same principle as the balloon; and from the experiments I have made with parachutes thus formed, it appears to be greatly superior to that used by Mons. Garnerin, both as to security and uniformity of motion, being entirely free from oscillation.

It may perhaps appear unnecessary to some persons that I have had recourse to an oblique plane, independent of the balloon, instead of constructing the balloon itself of such a form as to present to the air the required oblique surface. I have lately tried several experiments with balloons of different shapes, and which have convinced me of the great difficulty in attaining the object by such a method. But it would extend this paper to an excessive length, if I were to detail the various experiments, and explain the causes of their failure.

I am, sir,

Yours respectfully,

Islington, Oct. 19, 1815. JOHN EVANS, Jun.

P.S.—The method of forming the gores of a balloon, which is given by Cavallo, and after him in most of the Encyclopædias, is so very tedious, that any abridgement of this essential process must be considered an advantage. A brief description of the construction which I have employed for some time may, perhaps, prove acceptable, on account of its great simplicity and accuracy. It is derived from the obvious property, that the breadth of the gore in any particular part is proportional to the chord drawn through that part parallel to the equatorial diameter.

By the usual methods find the middle breadth AB (fig. 4) and the length CD of the gore. On AB as a diameter describe the required shape AEBF of the balloon. Divide the curve line FAE and the straight line DC into the same number of equal parts, and through the points of division draw the straight lines *gh*, *ik*, GH

GH and IK parallel to AB. Make GH equal to *gh*, and IK to *ik*, and through the points I,A,G and K,B,H, draw by eye the curved lines CIAGD and CKBHD, which will be the shape of the required gore. In the diagram I have divided FAE and DC into only four parts, for the sake of clearness: in practice, however, it is necessary to make use of eighteen or twenty divisions. This method is equally applicable to any shape which may be adopted, an advantage of which no other construction is possessed.

LXI. *New Outlines of Chemical Philosophy.* *By* Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from p. 211.]

On the Leyden Phial.

The various hypotheses which have been invented to account for the phænomena of the Leyden phial plainly evince that the subject still remains in some obscurity. Nor is this to be wondered at, when we reflect that the properties of the electrical elements are so little understood, that philosophers are divided in their opinions respecting them. Franklin, Æpinus, Robison, and some others, have supposed that all electrical effects are produced by one fluid only; whilst Du Faye, Symmer, Cigna, Eeles, &c. maintain that there are two fluids, which exist together in all bodies, that they counteract each other when united, and can be made evident to the senses, only by their separation.

Men are too apt to adopt the opinions of others without strict examination. But it is an axiom in philosophy, which ought always to be kept in mind, that "no position should be assumed as the basis of any reasoning whatever, except what has been proved by incontestable facts."

General Properties of Electricity.

1. It has been demonstrated by an instrument, which may be called the pendulum electrometer, described in a former paper*, that the mechanical forces of those elements which produce a spark are equal, moving through each other; but the spark so produced is not an element, but the effect of those invisible elements which are brought into action by the motion of the machine. But no spark can be produced unless those elements pass through some conducting medium.

2. The two electrical elements exist together in all bodies; which may be called their *natural state.*

* Phil. Mag. vol. xlii. p. 163, and vol. xliii. p. 284.

3. Two bodies electrified with contrary elements attract each other strongly, although an electric plate be interposed between them.

4. Two bodies electrified with the same element repel each other, although a thin electric plate be interposed between them.

Application of these Properties to the Explanation of the Leyden Phial.

When a Leyden phial is receiving a charge, a succession of sparks passes between the ball of the prime conductor and the ball upon the top of the phial; whence it is evident, from what has been previously stated, that when one of the elements (thermogen for instance) is thrown into the phial from the prime conductor, the other element (photogen) is attracted out of the phial by the former element, and by passing through the air in opposite directions they generate combustion. And as soon as the thermogen on the inside of the phial becomes sufficiently powerful to act through the two coatings and the glass contained between them, it repels the element of its own kind into the earth from the outside of the phial, and attracts the contrary element at the same time out of the earth; and as they pass through each other, they generate a spark at the bottom of the phial.

When all the photogen is drawn out of the phial, and an equal portion of thermogen is thrown in, the phial is fully charged; for then the outside of the phial is deprived of all its thermogen, and its place supplied with an equal portion of the other element from the earth.

This is the theory of Mr. Eeles, which appears to be well founded. Some objections, however, have lately been advanced against it by Mr. Donovan*, which ought not to be passed over in silence; for, if these objections cannot be answered by incontrovertible facts, the theory of electricity is still involved in obscurity?

Objection 1. "If the contrary powers are situate on each surface of the charged phial, and if they strongly attract each other through its substance, why do they not pass through it," since it has been proved that glass is permeable to each kind of electricity?

Answer. This objection will vanish when it is understood that the coating prevents the two elements from passing through the glass and combining. This conclusion must be admitted as a just one, since an uncoated phial cannot be charged; for these elements pass through glass as freely as light. More light will pass through thin glass than thick, and the same law seems to obtain in electricity.

* Phil. Mag. vol. xliv. p. 406.

Objection 2. It has been supposed that when one of the elements is passing from the inside of the phial towards the conductor, and the other from the conductor into the phial, "these two powers necessarily meet, and condense each other into the form of flame: they return to the natural state, and therefore do not exert any sensible action."

Answer. It has been proved by the pendulum electrometer that the two elements, after having passed through the card in opposite directions, still have power to throw off the two pendulums in a direction perpendicular to the card, so as to form an angle with each other of 70 degrees. Consequently the two elements exert a very sensible action on matter, *after* they have generated combustion, and *before* the return to their natural state.

The 3d Objection is founded on the supposition that the two elements in the natural state have a strong attraction for each other.

"If the two electricities have so great an attraction, why did the power thrown in on one side overcome the attraction of the similar to the contrary power naturally co-existing on the other side? Why should not the natural combination remain unaltered, when it was maintained by a force equal to that which endeavoured to effect a separation of its component electricities?"

Answer. If the two elements of electricity have an attraction for each other in their natural state, it must be strongest where their quantities are greatest. Now the earth is the great repository of these elements, from whence all the electrical energy which is constantly acting in every part of the œconomy of nature is derived: consequently, according to this hypothesis, the attraction of these elements for each other must be inconceivably greater in the earth than in small quantities of matter.

How is it possible, then, that all the various compositions and decompositions in meteorology should take place, and that all the living functions of animals and vegetables should be carried on, if a strong attraction existed in the natural state of these two elements?

From a great number of experiments which I have made upon this subject, it evidently appears to be an invariable law of nature, that the slightest excitement of an electric is sufficient to attract the contrary element out of the earth.

The following experiment proves to a demonstration that the two elements of electricity, in their natural state, have no attraction for each other.

A conductor AB, consisting of a brass rod 12 inches long, having a ball of the same metal fixed to each end of it, was placed upon an insulating stand, and a jar electrometer placed

at

at the end B. A glass tube 3-10ths of an inch in diameter being drawn *once* through a piece of silk held in the hand and placed at the distance of 12 inches from the end A of the conductor AB, the leaves of the electrometer diverged with positive electricity to an angle of 40 degrees. And the electrometer, being removed from the conductor before the tube began to lose its energy, remained permanently electrified. Hence the 3d Objection to the theory of the Leyden phial appears to be founded on a supposition which is erroneous.

Lynn, Nov. 3, 1815. Ez. Walker.

[To be continued.]

LXII. *Statistical Account of the Quicksilver Mines of Idria in Illyria. By M.* Paysse, *Superintendant of the Manufacture of Mercurial Productions.*

[Concluded from p. 312.]

Vegetation is neither so active nor so early at Idria as at Laybach, and even Upper Laybach. The months of August and September are those in which vegetables seem to shoot out most luxuriantly.

A great number of rare and curious plants grow on the alps which surround Idria. The celebrated Scopoli has described them very minutely, and very few have escaped him. Rye, barley and maize, succeed very badly around Idria.

The trees of which the forests are composed are various kinds of pines, yews, ash, &c.

The leguminous plants are reared at great difficulty; besides, they are not juicy.

Fruits with kernels rarely ripen, and those gathered are tasteless and watery.

Apples and pears thrive a little better, and are fit to be eaten or used for cyder and perry.

The vine, which shoots up here vigorously, furnishes a grape which ripens but very rarely.

Medical Constitution.

Considered in a medical point of view, the climate of Idria is not insalubrious: those thick and frequent fogs, which are the sources of many morbific affections to those who reside in the marshy plains of Laybach, do not prevail here.

The workmen are subject to certain fevers; phthisis is frequent: but the diseases which most generally prevail among the working class arise from a checked perspiration. It will be easily

easily conceived that this kind of affection is the more common, as the workmen when they come out of the mine, the temperature of which is very high, perspire abundantly, and take no precaution to avert the bad effects of the cold air, even during winter: they walk for half an hour at the interval of meals, &c. with the breast uncovered*. Colds and catarrhs are therefore very common during winter, but contagious diseases are exceedingly rare.

The disease which most affects the inhabitants of this country, and which is also common in Carniola, is the tooth-ache; persons of both sexes are subject to it, and even the young. Every person loses the teeth very early in this climate, and it is rare to find a person with good teeth. It is true that they are all careless with respect to their teeth. Pedicular diseases are almost unknown among them, but scrophulous cases are very common.

The common people of the Illyrian provinces, and even those around them, have imbibed a prejudice that it is sufficient to live in Idria to lose one's health: a thousand stories are told, to prove that the mercurial vapours there inhaled render the air injurious to health.

This prejudice is unfounded: no atmosphere is more steady; its pressure is nearly always the same, and better health is there enjoyed than in the other part of the provinces. I have seen many sick persons from Laybach, both Frenchmen and natives, come to Idria, and there get well more promptly than they could anywhere else. Among the French who resided there four years, and who were consequently incessantly exposed to the combined action of the air and mercurial substances, with which one would suppose it impregnated, not one ever experienced any of the symptoms supposed to result from inhaling this metal. The nature of my duty compelled me to be continually exposed to emanations from the preparations of this metal, and the metal itself; and I never felt the slightest inconvenience, nor the slightest tooth-ache, whilst the inhabitants were most severely affected by it.

It is true, however, that the workmen who are employed in the very painful labours of sweeping the chambers of condensation of the mercury in the furnaces, are those who really experience all the deleterious effects of the metal while they are executing this operation. In fact, these workmen, being for two hours in an atmosphere of mercury in the state of pulverulent oxide in extremely minute division, inspire a considerable quan-

* The clothing of the miners when at work consists of a jacket of brown or black cloth, breeches, stockings, shirt, and hat or bonnet, with shoes or rather boots; all of the worst kind.

tity

tity of it while they are detaching it from the sides of the condensers, into which they are obliged to go: thus they undergo a copious salivation; and after working in this way several years in the furnaces, shiverings come on, which do not permit them to stand erect any longer. Several workmen are in this melancholy condition at Idria.

Much fewer *goitres* are met with in Idria than in the mountainous countries of Carinthia and Styria. *Cretins* are also very rare.

The fair sex in Idria, without being either beautiful or fresh-coloured as in other mountainous countries, nevertheless enjoy a robust and vigorous health. The sedentary life of the women, who are occupied at Idria in making a common sort of lace; the little care which they take of their persons; the food which they eat, being always farinaceous, with lard, and cabbages soured by fermentation; the extremely high temperature in which they live, the small windows of their apartments being but rarely opened;—all these circumstances concur in taking away their colour, diminishing their vivacity, and giving their countenances at the same time a sickly hue.

Many of the male children are affected with rickets. Indeed, the boys seem in general to have a less robust constitution than the girls, with the exception of those whose parents are employed in the woods. It may be easily conceived, that the son of a miner who is introduced to his father's work at the early age of twelve, exerts his bodily powers much more than the son of a wood-cutter, who does not handle the axe until he is of mature age. The former, therefore, become old and decrepid much sooner than the latter, who of course breathe a purer air.

The water drunk by the working classes at Idria is very pure, although it filters through limestone. Chemical analysis exhibits but trifling quantities of foreign matters held in solution. The sub-carbonates of lime and of magnesia are manifested by the reagents.

It has been also remarked, that the good races of animals, and particularly those of horned cattle, do not prosper here: they are in fact always poor, and miserably fed. The inhabitants of these countries ascribe this defect to the mercurial and sulphurous vapours which escape from the furnaces while they are at work, which is not the case generally, but only once a week.

This observation, which I am authorized to consider as very ill-founded, would deserve a particular examination in a separate work. I shall content myself for the present with detailing, in a summary way, what has been taught me by an experience of four years in concert with some skilful physicians.

Influence

Influence of the Vapours of the Furnaces on Animals and Vegetables.

All the vapours which issue from the furnaces while they are at work, pursue, as I have already observed, the course of the torrent of the Idrixa, and proceed almost always to Lower Idria, or into the valley of the Canomla: it ought therefore to be in the places where these gases seem to be accumulated, that their influence should be exercised in all its fury on the animals there reared; but nothing of this kind seems to act upon them: there cannot be finer animals than are to be seen in the opposite districts of Zala, Nicova, &c. The general diseases are, in fact, much less frequent among the inhabitants of the Lower Idrixa and the Canomla than at Idria.

Epizooties, or cattle epidemies, are extremely rare in this village, while on the other side of the Alps, and at the distance of three or four leagues, they exercise frequent ravages: hence it appears natural to conclude, that if the finer kinds of animals do not prosper in the district of Idria, this may be owing to other causes than to the deleterious influence of the vapours which are extricated from the minerals while they are in the furnaces. I am greatly inclined to think, that if cultivators gave themselves the trouble to renew the species, and to rear them with more care than they have generally done, they would soon meliorate the breed.

Situation of the Mine.

It is at the lower end of the valley or basin of that name, and the town, as I have already said, is built with the materials of the subterranean excavation.

This metallic stratum has been recognised to be 400 toises (fathoms) or 800 metres in length, 500 toises or 1000 metres in breadth, and 120 toises or 240 metres in depth.

There are six apertures or chief borings in this mine, four of which are vertical. Three of the latter are intended for the extraction of the ores, rubbish, or for pumping out the water. The fourth is solely used for conveying into the mine all that is necessary for the wood or mason work, to make the interior operations safe. The names of those vertical wells are Saint Barba, Saint Theresa, Saint Francis, and the Emperor Joseph.

The machines for drawing off the water are common pumps; fifteen cylinders are adapted to each other, with fifteen pistons with suckers. Every pump has its reservoir, which serves at the same time for a feeder to the upper cylinder. The hydraulic wheel which sets these various pumps in motion is about 36 feet diameter.

The machines for drawing up the ore, &c. are boxes of a

paral-

parallelogram form, well mounted with iron, capable of containing seven or eight hundred weight of ore: a hydraulic wheel with double power, which a workman stops or puts in motion when ordered, gives the impulse to a capstern on which a cable rolls and unrolls. The water which moves those two wheels comes from a canal which takes its rise in the Idrixa, and which feeds two wells, as well as the workshops and washing-houses.

The entrance of the mine by which the workmen descend, as well as the travellers from various parts of Europe to visit these immense caves, is inclosed within a building in the town, which also contains the room in which the workmen are collected previously to going upon duty. It is in this room (named in German *geselstube*) that the workmen are called over and paid their wages, &c. by the controllers.

There is also an inclined opening or stair-case of stone and wood steps, by which persons may descend into the mine for 150 yards. But in order to get lower there are wooden ladders placed, and some of these are riveted perpendicularly against the wall; which makes the descent very inconvenient to those unaccustomed to such expeditions.

Nine stages of galleries or horizontal borings (including intermediate ones, with their numerous ramifications,) constitute the whole of these vast subterraneous works. Most of these galleries are walled up with much art and skill, in order to prevent the falling in of the earth so frequent in these kinds of works. The arches and walls are very simple and œconomical, and yet very solid: they are of brick.

The galleries are generally about seven feet high, and as many broad. In general they are all well aired, currents being established between them and the main pits. Some of them, however, particularly towards the centre, are not so well aired.

The temperature is variable in the interior of the mine. In fact, it may be easily conceived that it ought to be higher in regions where it is not possible to establish currents easily. I have made a great number of thermometrical observations; and I remarked that in countries where there are great beds of rich ores, or in their vicinity, the heat raised Reaumur's thermometer from 24° to 28° above zero. I have also observed the temperature at 24°, where the strata were blackish schists, very poor in mercury.

Compelled to work in an atmosphere of which the heat is very considerable, the air rarefied, and the atmosphere often loaded with carbonic acid gas coming from the combustion of the oil used by the workmen in their lamps, the health suffers greatly.

The working day at Idria consists of eight hours: but when digging

digging in galleries, the temperature of which is very high, the workmen relieve each other every two hours; and yet they are in a continual state of perspiration, although they work almost naked. This perspiration weakens them much.

The workmen are prohibited from drinking spirituous liquors: water is their only beverage, and it is brought them by boys who are constantly going about the pits to supply them. This is a wise regulation.

When the workmen are employed on a part of the mines which contain native mercury, a small portion of this metal is mixed with the dust of the matrix, and is thus inhaled: persons thus employed for any length of time lose their teeth very soon*.

The rocks of the mine are in general calcareous, and belong to the formation of the Julian alps. This calcareous mass is, however, rarely pure: it is composed of a major proportion of lime, some alumine, and a small portion of magnesia and iron.

The geological constitution of this mine seems, in short, to be very extraordinary: it astonishes all the mineralogists who visit it, and it is extremely difficult to account for its formation.

In general it has been considered as very ancient: this seems to be proved by our meeting at very great depths with the same *brêches* with those at the surface: these are again covered with banks of limestone of a very regular inclination over a very great extent in length, as well as schists which alternate frequently with the latter. The limits which I have prescribed to myself in this notice, do not admit of my considering this important question at present, but I shall recur to it in a work I am preparing on this establishment.

The mine of Idria on account of its strange formation resembles no other of the kind. It is far from presenting that facility of working which we generally meet with in other metallic mines with regular seams.

The miner has no certain guide for finding good ore in any particular direction: thus he must dig out every thing before him: the work is therefore equally arduous in the galleries where they pick out the metal, as in those for common digging. Nature, in short, does not seem to have acted at Idria, as she has done in some other parts of the globe where she has been pleased to conceal metallic treasures.

Every thing seems to announce disorder, confusion, and chaos at Idria. Some dreadful catastrophe seems to have presided at this extraordinary formation, and the whole mineral kingdom

* It is somewhat singular, that since the fire which broke out in the mine of Idria on the 15th of March 1803, it has been remarked that the air was vitiated less easily in certain galleries, and that the temperature was much lower.

has

has been confounded, to constitute the mineral riches which are contained within this vast cavern.

Enormous heaps of shells are mixed with mercury, bitumen, and sulphur. The ores are therein disseminated in the most unequal and most original manner, so that the indications which were favourable at one time are not met with again for a great length of time, and even never. What we discover one moment is lost the next, and almost always it is chance only which leads to rich beds: thus, when discoveries have been made, great care is taken to œconomize them.

In general the mercury obtained at Idria is combined with sulphur, bitumen, and iron. The substances which serve for its matrix are often very complex, as will be seen on becoming acquainted with the principal varieties of these ores.

1. We have native mercury disseminated through a gray, leafy, argillaceous schist, in considerable beds in the part which adjoins the pit of St. Francis. *Graue schiefer.*

2. Native mercury in masses of black schists, disposed in laminæ, joined together by small layers of hydrogenated red sulphuretted mercury, called hepatic mercury. *Schwartze schiefer.*

3. Native mercury united to sulphuretted iron, in flat buttons or nipples, enveloped with a blackish argillaceous schist. *Krezel schiefer.*

4. Mercury combined with sulphur or sulphurated mercury, in very close compact masses, steel grained, of a more or less intense red, mixed with sulphurated iron. *Staal-erz.*

5. Compact sulphurated mercury of a deeper red than the foregoing, mixed with fine freestone and sulphurated iron of a brick colour, called *siegel-erz.*

6. Compact sulphurated mercury, in masses or tables which are separated in polygons, without any very decided form, of a fine liver red, with polished surfaces often striated. *Leber-erz.*

7. Sulphurated mercury united with a variety of bituminiferous mercury or *brand-erz,* lighter than the foregoing, or mixed *leber-erz.*

8. Oxide of mercury combined with bitumen in a slight brittle mass, very variable in colour from the dark brown to the gray, more inflammable than the foregoing, burning with flames, and emitting white mercurial vapours of an agreeable bituminous smell, without being sensibly mixed with sulphur, called *brand-erz**.

9. Sulphurated mercury mixed with abundance of grayish argil,

* This species of combination, extremely curious, is almost always found filling the intervals which separate two layers of *leber-erz,* of which it takes the impression. It is remarkable for its lightness, its combustibility, and particularly for the yellow bituminous matter which it furnishes, when we treat

argil, in which are catseyed small roundish nipples of carbonated lime, quartz, sulphurated iron, &c. of a brown colour. *Bohner-erz.*

10. Compact mercury, in brittle, shining masses, very often irisated with the finest colours, easy to be cut, and susceptible of a very fine polish. Irisated *Staal-erz.*

11. Sulphurated mercury in brown masses, dividing itself by curvilinear laminæ, an inch and half thick, imitating the testaceous form, containing plenty of iron and argil. *Braun-erz.*

12. Sulphurated mercury of a coarse and close grain, of a blackish brown, strewed with lenticular sulphurated iron, containing bitumen; very rare. A variety of the above.

13. Compact sulphurated mercury, mixed with shells which have partly passed to the state of cinnabar, of a red colour. *Corallen-erz.*

14. Sulphurated mercury, less compact, the shells of which have a black colour owing to the bitumen mixed with it, not so rich as the foregoing.

15. Sulphurated mercury, less shelly than the foregoing, holding little bitumen, formed partly of freestone and of sulphurated iron called *Corallenwande.*

16. The same as the foregoing, not so rich in mercury, almost black bituminous, having for its base carbonated lime, and the shells remaining in this state after calcination without losing their forms.

17. Crystallized sulphurated mercury in octohedra or in pyramidated tetrahedral prisms of a lively red, sometimes irisated, in a species of argillaceous *geodes* in which native mercury is often found. *Crystallisirte zinober.*

18. Globular schistous sulphurated mercury, in round or oval masses weighing from two ounces to four or five pounds, the nucleus of which is often sulphurated iron. *Kiesel kougel.*

19. Sulphurated mercury, prismated or octohedral, or rather amorphous cubes, of an extremely bright red, united with hydrogen, giving out a smell of hydro-sulphuret when it is pounded, and causing slight detonations when we triturate the transparent crystals in a glass mortar. This variety has for its matrix a quartzo-argillaceous black schist, emitting fire with steel. *Hepatische zinober**.

20. The

treat it in close vessels by the action of fire. This substance is in brilliant, micaceous, light laminæ, liquefying like wax at a gentle heat, when burning exhaling a balsamic smell, and giving as a residue after sublimation a very hard charcoal, light and irisated, but very difficult to incinerate. This species has been confounded with *leber-erz*: it contains from 6 to 33 pounds of mercury in the hundred weight.

* Several mineralogists have described this mine as the most abundant at

20. The foregoing variety is an argillaceous schist mixed with sulphurated iron, which gives rise to sulphated iron by the spontaneous decomposition which it undergoes.

21. Sulphurated, not hydrogenated mercury of a bright red, in thin layers, pulverulent, accompanying the crystallized cinnabar, called native vermilion.

22. Sulphurated mercury, not very perceptible to the eye, disseminated throughout a black earthy schistus, mixed with sulphated iron which crystallizes in annular prisms several inches in length, mixed with sulphates of lime, alumine, and magnesia; a mixed salt affecting the silky form, reticulated or straight, several inches in length. This phænomenon is visible in such specimens as collections afford.

23. Black schistus, very poor in mercury, in masses shining at the surface, composed of curvilinear laminæ. *Schwartze schiefer.*

24. The same as the foregoing, in masses not so leafy, divided into angular fragments, of the appearance at first of compact lignite, but more friable, very poor in mercury.

25. Black schistus, mixed with bitumen, fœtid, in tables a foot and a half broad, with soft and shining surfaces like a mirror, *Spiegel schiefer.*

26. Black or brown schistus, in tables more or less thick, with smooth surfaces strewed with carbonated lime and pulverulent sulphuret of mercury. *Flamberg schiefer.*

27. Calcareous breccia strewed with abundance of sulphurated mercury, crystallized confusedly, or in powder, of a very lively carmine red.

28. Argilo-calcareous rock, mixed with sulphurated mercury and sulphated iron in decomposition, proper for the stampers and the washings. *Wasche gaand.*

29. Compact rock, a kind of hard marl, veined with brown schist: treated as the above.

I could describe many more rocks, schists, &c. which are met with in the mines of Idria; but the above I presume will give a correct idea of the substances with which the mercury is in general combined.

The interior of the mine is very dry, most of the borings are shored up with wood or walled with stone. Where the solid

at Idria; but this is not the case, for it is neither abundant nor rich: and as it is decomposed even in the interior of the works, it produces the silky sulphates which are found in the galleries. It is a kind of sulphurous hydrate of mercury, and is put through the washers' hands before it can be used.

rock

rock has been cut through, there is of course no occasion for these precautions.

The workmen are classed according to their rates of pay. They are divided into companies, and their classification with respect to the distribution of the corn which is furnished them, is in proportion to the number of children belonging to each family, and to the rank the workmen respectively hold.

Every month the labours for the interior of the mine are regulated. With this view the chiefs of the establishment call a general meeting of all the workmen, who are bound to attend. A person called the *Schickten Schreiber* reads with an audible voice the arrangements which have been made for all the works. The workmen are formed into companies of four men for the galleries, and each designated by his name for the spot which is intended for him.

The hours in which the labourers work are from four in the morning to noon,—from noon to eight in the evening,—and from eight in the evening to four o'clock in the morning: this gives each man eight hours labour daily. The masons who are employed in building up the mine walls, as also some other persons who are not exactly miners, work ten hours.

At three o'clock every morning a bell rings to summon the workmen to the hall or *geselstube* of the mine, where their names are called over previous to descending.

Every person then proceeds to the spot allotted to him, furnished with a common oil lamp. The masters and other subaltern officers also descend into the mine to superintend the various gangs of miners. At mid-day these sets of workmen are removed, and again at eight o'clock in the evening.

The chief director of the mines visits them on the 1st and 15th of each month, accompanied by all the engineers, &c. of the establishment, when he inspects all the works and regulates the price of the ulterior labours. There are two ways of settling with the workmen; either by the day or by the piece, but the latter way is preferred. They are paid a certain sum for each c ubicfathom.

The price of the cubic fathom of ore, or of cutting into the rock, is regulated by the hardness, the difficulty of working, the distance of the workman from the mouth of the pit, and the temperature of the region in which he works; and sometimes the deleterious effects of a particular spot are taken into the account.

Besides sweeping clean the gallery in which he works, the workman is also obliged to pick out the richest ores, to put them aside in a corner, and to mark them with a cross of wood, that they may not be confounded with those which are poor.

The ores when drawn up from the pits are conveyed by workmen to the washing-rooms, where the richest are set apart. The middling sorts are picked also, and arranged according to their value. Men and boys are employed in this business.

The wirework or sieves which serve for washing the ores are of seven numbers, and each has its character of fineness; for the ores are of various sizes, and contain a very great number of particles very rich in mercury.

Afterwards an expert set of workmen put the same ores into other sieves: these are suspended over a cistern filled with water, into which the workmen immerge them constantly, taking care to shake the sieve, in order that the heaviest molecules may fall to the bottom. The lighter portions at the surface are decanted, and great care is taken to collect those which appear red, as they contain cinnabar.

The ores after thus passing through the various sieves are called *kerns*, No. 1, 2, 3, 4, 5, 6, 7.

The ores which have passed through the sieves, and which are separated and rejected, are the coarsest. They are placed under stampers to be reduced into powder, and afterwards undergo with the rest a new concentration in washing-houses with tables forming inclined planes.

There are two kinds of inclined tables: some are moveable, and the mineral dust which comes from them is called *stösherd schlick:* others are fixed, and what comes from them is known by the name of *schliecks*, No. 1, 2, 3, 4, and 5.

The regulations of the mines of Idria require that the ores in powder, called *schliecks*, should never be less than 7 per cent. of mercury. When, after the analysis which the assayer makes every week, it is ascertained that these metals do not contain the requisite quantity of metal, the workmen are not paid their wages, and they are obliged to recommence the labour of concentration gratuitously.

In the washing-houses the ores in powder are weighed and put into the depôt on Friday every week; from thence they are carried to the furnaces.

Before, however, submitting the ores to the action of the fire they are previously analysed by the assayer, and the quantity which ought to compose a charge, both coarse and fine, is subjected by an officer appointed for the purpose to a rigorous admeasurement by weight; so that the quantity of mercury to be expected from the operation may be ascertained beforehand with tolerable precision.

As the ores are of two kinds, in respect of size, there are also two sets of furnaces for treating them.

These furnaces are forty fathoms long by eight broad, including the external walls. The

The first, which is destined to roast the ores in large pieces, has in the first place a trapezion fire-place, surmounted by a sort of open vault or chimney, which communicates with a kind of furnace into which the charge of the ore is put. Care is taken to supply this first vault with poor ores in large pieces, in order afterwards to be able to charge it with all the fragments which are richer. This furnace once well filled, the second furnace, which is superposed on the first, is charged; its vault is also open at top, and its hollow part is formed by the vault or chimney of the latter. Instead of charging this second vault with ores in pieces of a certain size, it is charged with lumps of earth into which small ores have been put called *grüben klein*, or the varieties which we have called *kernno*. This last furnace being charged, the doors of all the chambers are luted.

This first furnace has two fronts, *i. e.* two fire-places, and four chambers, which communicate by lateral pipes with two rows of reservoirs of mason work, or very high chambers separated by intermediate walls, but communicating with each other by apertures of a square foot, placed alternately. The number of these reservoirs or condensers is twenty-eight. The area is inclined, in order to facilitate the flowing of the mercury into reservoirs destined to receive it. The last of these chambers of condensation is surmounted by a second chamber above the first, which is terminated by a chimney to give vent to the vapours that cannot be condensed in this large apparatus. A funnel common to all the reservoirs facilitates the arrival of the mercury in another large reservoir in the apartment where the mercury is collected in order to pack it.

The second furnace destined to burn all the ores in powder is constructed on the same principles, with this difference only, that there are six furnaces instead of four, and twenty-four chambers of condensation instead of twenty-eight; and the furnaces are charged with lumps of earth filled with *schliecks*, or ore in powder.

In the first-mentioned furnace only 300 quintals (cwt.) of ore are burnt, and in the second the charge may be 600 quintals and upwards.

The charge of the ores when once effected, all the apertures both of the furnaces and of the chambers of condensation are carefully luted with clay and slacked lime; afterwards the whole is heated for about nine hours with a progressive fire, until the earthen clods are red hot, and the ores in pieces burn vehemently. This time is generally sufficient. The apparatus is then allowed to cool for six days, and the mercury is afterwards collected. It would be difficult to obtain the metal sooner, as the furnace retains its heat a long time.

 One

One operation of the furnace produces about 80 quintals of mercury, more or less, according to the quality of the ores.

The quicksilver, after being poured into the common reservoir, is taken out and packed in sheepskins in portions of 100, 50, and 25 pounds: they are tied with cords previously rubbed with soap. The skins are then conveyed to another department, where each is inclosed within a second skin.

But the Austrian government has not confined its attention to the production of pure quicksilver alone: all the preparations of mercury known in commerce are also made at Idria.

Corrosive sublimate (muriate of mercury oxygenated to the *maximum*), mild sublimate (or submuriate of mercury at the *minimum*), red precipitate, cinnabar or red sulphuret, vermilion or washed mercurial sulphuret, &c. are also daily manufactured at Idria.

These articles are so well manufactured under the Austrian regime, that nothing is left to desire. *Chinese vermilion*, as it is called, forms the only exception; and even in this branch of the manufacture I conceive that in the four years of my superintendance I made several valuable improvements.

The buildings destined for the preparation of mercurial products contain three very large laboratories. The first is intended for the amalgamation of the mercury with the sulphur by means of casks which a hydraulic machine turns on its axis. The same wheel acts as a stamper for pulverizing the sulphur.

The mixture of sulphur and mercury, when once effected, is converted into cinnabar. For this purpose furnaces are placed beside the apartment for the amalgamation; they contain each six vessels, into which eighty pounds of matter may be introduced. Nine hours of a graduated and steady heat are sufficient to finish one operation.

The second laboratory is destined for the preparation of vermilion; and for this purpose eight mills are arranged for the grinding of the cinnabar, as well as a certain number of large wooden tubs to wash the products. The washing being finished, the vermilion is dried in a stove which is warmed by the waste heat of the furnaces in which the cinnabar is sublimed.

In the third laboratory there are twenty furnaces, each furnished with six large iron capsules. In this laboratory are prepared all the mercurial salts, the red oxides at the *maximum*, the sulphates of mercury, the nitric acid, &c. &c.

The accessory apparatus consists of stampers, a reverberating furnace for the calcination of such substances as require that operation before they enter into saline compositions; sieves for cleansing the ores, and casks fitted with measurements for ascertaining precisely the matters which enter into the compounds.

In

In addition to the above establishments connected with the preparations of mercury, there are the following:

A fine glass-work provides the bottles and window glass necessary for the various establishments, besides exporting a considerable quantity.

A pottery also supplies all the earthenware required. A tannery prepares the sheepskins for the packages: and a rope-work furnishes the ropes for the machinery, and the smaller cords for packing the quicksilver.

There are also mechanists, engineers, blacksmiths, carpenters, coopers, cartmakers, metal-founders, &c.

The forests around Idria are under the management of an inspector, sub-inspector, six guards, and three hundred wood-cutters. From this source all the wood necessary for firing, and the various uses of the mines, &c. is derived.

The whole administration of the mines is intrusted to a council which is composed of the heads of the various departments. The president of this council is the director in chief. It meets every Thursday, and takes cognisance of all the great interests of the establishment. Every Saturday there is a meeting of the chiefs and sub-chiefs. At this last meeting, every chief or his deputy reads an account of what has been done during the week, and states the wants of the ensuing week. The complaints of the workmen, petitions from widows, &c. are also heard at this meeting.

The receipts and expenditure are under the direction of a treasurer; but he cannot make any payment without the consent of the chief director and council. The daily payments are made out of a smaller treasury chest. There is also a board of accounts, which declares once a year the profits of the establishment.

The receipts from the mines may vary considerably, as may be easily supposed. For instance, if the richest ores only were wrought and brought to the furnaces, it is evident that in two or three years an immense quantity of quicksilver might be produced; but the poorer ores, which require much more trouble and expense to make them productive, would remain, and several unprofitable years would ensue, and the proper equilibrium of the establishment would be lost.

The mines of Idria were managed by the French government for the behoof of the order of the Golden Fleece (*toison d'or*) from the 1st of January 1810 to the month of September 1813.

During these four years (wanting four months) there were sublimated in the furnaces 198,176 quintals and 54 pounds of various ores, which produced 17,076 quintals and 54 pounds of quicksilver. Hence the medium will be 8,616 of quicksilver for the quintal; which, at the regulated price of 130 Austrian florins per quintal, gives a sum of 2,219,945 florins, or 5,740,463 francs.

Of this quantity of metal there was actually delivered into the magazines the following quantities of quicksilver or of mercurial preparations: viz.

	14,194	quintals	25	pounds	quicksilver
	702	——	25	——	cinnabar
	2,700	——	29	——	vermilion
	24	——	50	——	corrosive sublimate
	64	——	..	——	calomel
	28	——	67	——	red precipitate
Total	17,713		96		

All the expenses of the establishment amount to 800,000 francs per annum; and by balancing the receipts with the expenditure the average annual profits will be 713,409 francs.

There are about 700 miners, 300 wood-cutters, and nearly 600 pensioners, including widows and children. The pensions alone amount to 50,000 francs per annum.

The payment of the workmen of every description is made partly in kind and partly in money. By reflecting for an instant on this mode of paying the working classes, it will be seen how wise the views were of those who founded the establishment. This method unites the double advantage of attaching the workmen and keeping at all times the price of labour at a low price, and always the same.

The administration which furnishes them with corn, obtains it at moderate prices in consequence of the great consumption; and as it is dealt out to the people employed, at a price which has not varied since it has formed part of their wages, the workmen have had at no time any pretext for an increase of salary.

This beneficent administration is extended to the children of both sexes, and at the same price as to the workmen. In the case of the male infants, the distribution takes place until they are fourteen, and the girls are served until they are twelve; because at these respective ages both sexes can be admitted to various occupations connected with the mines. To those who from age or infirmity are unable to work, the distribution of corn is continued throughout life.

All the persons employed, and workmen after a certain number of years of service, or such as become infirm, enjoy pensions, which are continued to their widows, and to their children until they are able to work for themselves.

From

From all that has been said on this subject, I have no doubt that the general conclusion of my readers will be—that the establishment which I have attempted to describe is a model of liberality, which reflects the greatest honour on the government under whose enlightened policy the foundations of the œconomy of the mines were laid.

LXIII. *Some Experiments on a solid Compound of Iodine and Oxygen, and on its chemical Agencies. By Sir* HUMPHRY DAVY, *LL.D. F.R.S.**

In the two papers containing researches on iodine which the Royal Society has done me the honour of publishing in the Transactions, I have described a class of bodies consisting of iodine, oxygen, and different bases analogous to the hyper-oxymuriates. In the last of these papers I mentioned that I had not been able to procure any binary combination of iodine and oxygen from these compounds, either by the method proposed by M. Gay-Lussac, namely, the action of sulphuric acid on the oxyiode of barium, or by other methods of my own institution; and that in experiments on the effects of the acids on the oxyiodes, new combinations only were formed. I have lately resumed this inquiry; and by pursuing a new and entirely different plan of operation, I have at last succeeded in combining oxygen and iodine. In the following pages I shall describe the circumstances which led me to ascertain the existence of this compound, and I shall detail some experiments on its analysis and its chemical agencies.

In the course of my researches, I observed, that when a solution of the compound of iodine, and chlorine was poured into alkaline solutions, or even into certain muriatic solutions, the precipitate was an oxyiode; and this fact seemed to indicate that iodine had a stronger attraction for oxygen than chlorine; iodine, likewise, has an attraction for chlorine: it appeared, therefore, extremely probable, that euchlorine, or the gaseous combination of oxygen and chlorine, would be decomposed by heat, and two compounds formed, one of oxygen and iodine, and the other of iodine and chlorine, or that a triple compound would be produced from which chlorine could be easily separated; and on submitting the idea to the test of experiment, I found that I had not been deceived.

To produce the compound of oxygen and iodine, it is necessary merely to bring the euchlorine and iodine together at the ordinary

* From the Philosophical Transactions for 1815, part ii.

ordinary temperature of the atmosphere. As soon as the euchlorine comes in contact with the iodine, there is an immediate action, its colour changes to bright orange, and a liquid is formed. When the euchlorine is in sufficient quantity, a white substance likewise appears. By the application of a gentle heat, the orange compound of chlorine and iodine may be made to rise in vapour: and the compound of oxygen and iodine remains.

When this compound is required to be dry, the euchlorine should be passed through dry muriate of lime (calcane) before it is admitted to the iodine. The apparatus that I have employed for producing the substance is a curved bent tube, in the form of an inverted L (ꓶ), closed at one end, the closed leg of the tube being longest, and which serves as a retort for generating the gas; a thin long-necked glass receiver for containing the iodine, and a curved tube of smaller diameter than the first, and cemented or ground into it for conveying the gas into the receiver. The muriate of lime is placed in some dry paper in the upper part of the large curved tube; and to produce the substance from 40 grains of iodine, 100 grains of the hyperoxymuriate should be used, and four times the quantity of solution of muriatic acid of specific gravity about 1·005; a very small spirit lamp should be employed to generate the gas; and to prevent explosions, the heat should be applied with great care, and only to the bottom of the tube.

The compound of oxygen and iodine when entirely freed by heat from the compound of oxygen and chlorine, appears as a white semi-transparent solid; it has no smell, but a strong astringent sour taste. Its specific gravity is considerable, for it rapidly sinks in sulphuric acid. When heated strongly, it decomposes, undergoing fusion at the moment, and is entirely converted into gaseous matter and iodine, leaving no residuum whatever.

It requires for its entire decomposition a heat which is rather below the boiling point of olive oil, and there seems to be little or no increase of temperature in the process.

Its nature is proved both by analysis and synthesis; for when euchlorine acts upon iodine, the volatile substance produced has all the characters of the body produced by the immediate action of chlorine on iodine; and when the compound I am describing is decomposed in a pneumatic apparatus, the gas formed is found to be pure oxygen, and the solid sublimate produced is pure iodine.

I endeavoured to determine the proportions of the elements in the compound, by decomposing it in glass tubes carefully weighed, and ascertaining the loss of weight of the tube, and the

the volume of oxygen evolved. I have used very small quantities of the substance; but as my balance is delicate, I do not think there can be any considerable error in the results. I give those which I consider as the most accurate.

In one experiment, three grains of the substance afforded a quantity of oxygen equal to 517·3 grain measures of water, and lost in weight ·68. In a second experiment, two grains afforded 348·3 grain measures of oxygen. In a third experiment, one grain yielded 191 grain measures of oxygen.

Many experiments that I have lately made, have convinced me, that in my first paper I rated the number representing the proportion in which iodine combines too low; indeed at the time, I stated that my results afforded rude approximations, they demonstrated merely that iodine was represented by a very high number. In an experiment recently made with care, fifty grains of the iode of potassium decomposed by nitric acid, afforded 32·8 grains of nitre. According to this result, the number representing the proportion in which iodine combines is 227·3; but I do not venture to state this number as exact, as I am not secure of the purity of the hydrate of potassa from which the iode was made.

The compound of iodine and oxygen is very soluble in water; it slowly deliquesces in a moist atmosphere, but remains unaltered when the air is dry; its solution first reddens, and then destroys vegetable blues, and reduces other vegetable colours to a dull yellow. When its aqueous solution is heated, as the water rises in vapour, it gradually thickens, gains the consistence of a syrup, becomes pasty, and at length by a stronger heat yields the solid substance unaltered; unless a sufficient heat is applied to decompose a portion of it, when it gains a purplish tint apparently from some iodine set free. The pasty substance that it forms is evidently an hydrate, for it yields moisture during its decomposition.

Its action upon inflammable bodies is such as might be expected from its composition. When it is heated, mixed with charcoal, sulphur, resin, sugar, or the combustible metals in a finely divided state, detonations are produced; and its solution rapidly corrodes all the metals to which I have exposed it, and it acts both upon gold and platinum, but much more intensely on the first of these metals.

When a solution of it is poured into solutions of the alkalies or alkaline earths, or when made to act on their carbonates, oxyiodes or triple compounds of oxygen, iodine, and the metallic bases, are the results. By its action on solution of ammonia, a substance is produced apparently the same as that which is formed

formed by the action of the compound of iodine and chlorine, saturated with chlorine on the same solution, and which I have mentioned in a former paper, and which, consequently, must be regarded as an oxyiode of ammonia.

When an aqueous solution of the compound is poured into a solution of the soluble salts of baryta and strontia, a copious precipitate of their respective oxyiodes is produced. The oxyiode of barium, as I have mentioned in my last communication on iodine, is a compound very slightly soluble in water; that of strontium is rather more soluble; and those of calcium, magnesium, glucinum, ittrium, aluminum, zirconium, are more so, and I believe in the order in which they have been named.

It forms combinations with all the metallic oxides I have tried its agency upon, and precipitates lead and mercury from their nitrous solutions.

The action of the compound upon acids is much more singular than that upon alkalies, earths, or metallic oxides. It appears to form combinations with all the fluid or solid acids to which I have been able to expose it, that it does not decompose. When sulphuric acid is dropped into a concentrated solution of it in hot water, a solid substance is precipitated, which consists of the acid and the compound; for, on evaporating the solution by a gentle heat, nothing rises but water. On increasing the heat in an experiment of this kind, the solid substance formed, fused; and on cooling the mixture, rhomboidal crystals formed of a pale yellow colour, which were very fusible, and which did not change at the heat at which the compound of oxygen and iodine decomposes, but sublimed unaltered. When urged by a much stronger heat, it partially sublimed, and partially decomposed, affording oxygen, iodine, and sulphuric acid.

With hydro-phosphoric acid, the compound presents phænomena precisely similar, and they form together a solid, yellow, crystalline combination.

It dissolves in solution of hydro-phosphorous acid; but on heating the combination, iodine is immediately produced by a decomposition of part of the compound, and the remaining part unites to the phosphoric acid formed.

When hydro-nitric acid is poured into a concentrated solution of it, white crystals form in plates of a rhomboidal figure, and which when dried partially decompose, and partially sublime at a much lower heat than the sulphuric or phosphoric compounds, and afford hydro-nitric acid, oxygen, and iodine.

It dissolves in solution of oxalic acid; but by a very gentle heat, the oxygen of the compound acts on the inflammable bases of the acids, and iodine and carbonic acid are disengaged in great quantities. By

By liquid muriatic acid the substance is immediately decomposed, and the compound of chlorine and iodine is formed.

When boracic acid was added to a solution of the new compound, it dissolved in it by heat, and did not crystallize on cooling. By evaporation a solid white substance was procured, not so easily decomposed by heat as the compound itself.

The taste of all these acid compounds is very sour, though in different degrees of intensity: they redden vegetable blues, and they dissolve gold and platinum. When they are made to act on the alkalies or earths, or on saline solutions which they are capable of decomposing, common neutral salts and oxyiodes are formed at the same time.

The facts of the combination of the new compound with acids, serve to explain the phænomena of the action of these substances on the oxyiodes, which I have described in my last paper on iodine, and they confirm the opinions there stated on the nature of this action. The substance procured by M. Gay Lussac, by the action of sulphuric acid on the solution of the oxyiode of barium, and which he has supposed to be a pure combination of oxygen and iodine mixed with a little sulphuric acid, has evidently for its base the combination just now described of sulphuric acid and the new compound, and, as I have shown, it likewise contains baryta. However minute the quantity of sulphuric acid made to act on oxyiode of barium, a part of it is always employed to form the compound acid; and the residual fluid contains both the compound acid, and a certain quantity of the original salt.

That this compound acid is a true chemical combination, is evident from the observations already detailed, and from its crystalline form. There is every reason to believe that the proportions of its elements are definite. In one experiment I found, that a small quantity of the new compound in being converted into the rhomboidal crystals, gained rather less than half its original weight from the addition of the acid, *i. e.* two grains became 2·8 grains.

In experiments in which the products of the decomposition of the compounds from phosphoric and sulphuric acids were collected, the acids disengaged were found in their state of hydrates, from which it is probable that the crystalline compounds are hydrates, and that the common acids carry their definite proportion of water into the combination. It is not indeed unlikely that the presence of water is connected with the phænomenon of combination, and there is an instance of this kind which I long ago pointed out. Sulphurous acid gas, and nitrous acid gas, appear to have no action on each other, unless water be be present; but with the vapour of water they form a solid crystalline hydrate.

Reasoning from analogy, it is probable that a compound of oxygen may be formed, containing less oxygen than the new compound. I have made many experiments with the hope of discovering a body of this kind; but without any decided success. When the solution of the new compound is made to act on the double compound of iodine and the alkaline metals, iodine is produced which during its sublimation yields no gaseous product. Iodine heated in a solution of the new compound slightly colours it, but this appears to be merely in consequence of its combining with the water; and the iodine rises in vapour with the water without decomposing the compound. In some experiments on the action of euchlorine on iodine, in which the iodine was in great excess, the solid substance formed had a chocolate tint; but this may possibly have depended upon a small quantity of free iodine, and when dissolved in water, it afforded by the evaporation of the water, the white compound only.

I detailed in my last paper on iodine, some unsuccessful attempts to procure a compound of oxygen and iodine from the chlorionic acid, the substance produced by the agency of the combination of iodine and chlorine in water, on the idea that water was decomposed in this experiment. I have made some further researches, on the supposition that it might contain a compound of iodine containing less oxygen than this new substance; but without any success; neither by distillation at very low temperatures, nor by the action of small quantities of oxide of silver, nor by any other means, have I been able to separate any compound of oxygen from it: and when it forms triple compounds, the oxyiodes, by its action upon alkalies, or earths, or metallic solutions, it appears that the oxygen of the alkalies or earths is only newly combined at the moment of its operation upon them, an effect assisted by the attraction of the bases of the earths for chlorine. The conclusion which I formed, that the chlorionic acid is a simple combination of the chlorionic sublimate in water, is still further proved by the circumstances of the action of muriatic acid on the new solid compound of oxygen and iodine*.— Page 349.

As I have called the compounds of oxygen, iodine, and bases,

* The chlorionic acid offers an easy method of procuring pure baryta. By dropping a solution of it into solution of muriate of barium, as I have shown in my last paper on iodine, a precipitate of oxyiode of barium is produced, which when decomposed by a strong heat yields pure baryta, the attraction of oxygen for barium being, as I have stated, stronger at this temperature than that of iodine.

oxyiodes,

oxyiodes, I venture to propose a name in conformity, that of *oxyiodine* for the new solid compound, and *oxyiodic* acid, for the acid compound it forms with water. M. Gay Lussac, as I am informed, has proposed in a paper which I have not yet been so fortunate as to procure, but which is said to contain many new and important facts, the name of iodic acid for the compound of oxygen and iodine, the existence of which he conceived he had proved by his experiment on the action of sulphuric acid on the oxyiode of barium, and the terms iodats for the substances consisting of oxygen, iodine, and bases. I am willing to pay every compliment to the sagacity of this ingenious chemist, in anticipating the knowledge of the nature of a body the separate existence of which he had not demonstrated by experiment; but the term *iodic* acid does not appear to me sufficiently definite. For the hydroionic acid, and the chlorionic acid, as well as the oxyiodic acid, may be all called as a class iodic acids, or acids from iodine, and the termination in *at* would place the oxyiodes in the common class of neutral salts, from which they differ in many respects. When they become binary compounds in consequence of their decomposition by heat, though they lose all their oxygen, their neutral and saline character remains unaltered, which is not the case with any other known class of bodies, except the hyper-oxymuriates; and the terms iodes and oxyiodes which I proposed in the first paper, in which the distinction between these bodies was pointed out, sufficiently express the nature of the double and triple compound, and the difference between them.

I am desirous of marking the *acid* character of oxyiodine combined with water, without applying the name acid to the anhydrous solid. It is not at all improbable that the action of the hydrogen in the combined water is connected with the acid properties of the compound; for this acid may be regarded as a triple combination of iodine, hydrogen, and oxygen, an oxyiode of hydrogen, and it is possible that the hydrogen may act the same part in giving character, that potassium, sodium, or the metallic bases perform in the oxyiodes; and as hydrogen combined with iodine forms a very strong acid, and as this acid would remain, supposing all the oxygen to be taken away from the oxyiodic acid, it is a fair supposition that its elements must have an influence in producing the acidity of the substance.

Rome, February 10, 1815.

LXIV. *Ob-*

LXIV. *Observations on Dr.* Thomson's *Critique on Mr.* Seppings's *new Method of Ship-building. By* Alexander Stuart, *Esq.*

To Mr. Tilloch.

Sir,—I observe in Dr. Thomson's Annals for last month, among the analyses of books, an account of Mr. Seppings's method of constructing ships. As that gentleman has thought fit to erect himself into the situation of critic as well as analyst of the books described in his journal, it would be but rendering the public that justice to which they are entitled, if he would make himself acquainted with the sciences concerning which he has the assurance to speak,—if indeed he has either the capacity and acquirements of a philosopher, or the honesty of an editor. His account of Mr. Seppings's construction is such a mixture of ignorance and blundering as would disgrace a common boat-builder: but it is of a piece with the confusion of his conceptions on the Trigonometrical Survey, on mathematical subjects in general, and on geology, a science in which his total and ridiculous ignorance is only equalled by his ill-breeding and insolent dogmatism. In the review now under notice he talks of "substituting triangular or oblique beams for the parallel ribs which have hitherto constituted a ship's frame."

To pass over the total want of correspondence between the terms here made equivalent, *triangular* and *oblique;* who could comprehend from such a description, that Dr. Thomson's "triangular or oblique beam" was a connected frame-work added to the ordinary rectangular framing of a ship, and so connected with it as to form diagonal ties or supports, similar in their effects to those which constitute the very existence of common carpentry?—a branch of mechanics of which this self-erected judge has probably never heard, or he could not have so deplorably blundered in the account he has attempted to give of this improvement. But to proceed: He talks of "substituting the oblique for the parallel ribs, which adds *prodigiously* to the stiffness as well as the strength." Really, a ship built of oblique triangular beams seems very well fitted for the voyage of such a critic to the land of Gotham, since to his other acquirements he probably has not added that of witchcraft—the power of keeping himself afloat in a sieve. If Dr. Thomson's conceit and insolence would permit his ignorance to derive any benefit from our friendly advice, we would tell him that Mr. Seppings's very ingenious improvement consisted in substituting a series of diagonal trusses in lieu of those parallel to the framing called riders, and that the ship's ceiling was omitted as superfluous; while

while smoothness, compactness, and solidity were given to her walls, by filling the intervals of all the timbers with short wood—an expedient attended with many advantages, which I shall not dwell on, as he could not comprehend them. He goes on to say that the "decks are not loose as in the old system." Now, really, when a Doctor, who is besides F.R.S. L. & E., has sailed to the Baltic in a ship, for the purpose of giving colour to a quarto volume which might as well have been written in Grub-street, we cannot help either suspecting his capacity for observation, or doubting his assertion respecting the voyage, as the Leith traders (of which I have built two or three) are not in the slovenly practice of carrying their decks about them "loose."

But I have dwelt longer on his review than it merits, and shall conclude by advising him to pursue a Pythagorean system—Seven years of silence and seclusion to his chamber, with a diet of oatmeal, may perhaps increase his knowledge, and neutralize his superacetic disposition.

I am, &c.

Glasgow, Sept. 14, 1815. A. STEWART.

LXV. *On a new Species of coloured Fringes produced by the Reflection of Light between two Plates of parallel Glass of equal Thickness.* By DAVID BREWSTER, *LL.D. F.R.S. Edin. & F.A.S. E.**

DURING a series of experiments in which I was lately engaged, for the purpose of determining the law of the polarization of light, by successive reflections from plates of parallel glass, I observed that all the images of the luminous body which were formed by more than one reflection, were crossed by parallel fringes of coloured light, when the two plates had a small inclination to each other; and that these fringes suffered considerable changes, by varying the position of the plate with regard to the incident ray.

These coloured fringes seemed at first to have the same origin as those of thick plates, which were discovered by Newton, and afterwards examined by the Duke de Chaulnes, Mr. Brougham, and Mr. Jordan; and I considered the second plate of glass as performing the part of the quicksilver in Newton's glass mirror, or of the metallic speculum in the experiments of the Duke de Chaulnes and Mr. Brougham. A more attentive examination, however, convinced me that this was a mistake, and that the coloured fringes constituted a new class of phænomena, having

* From the Transactions of the Royal Society of Edinburgh for 1815.

a different origin from those of thick plates, though explicable by the beautiful theory of fits of easy reflection and transmission by which Newton was enabled to explain all the phænomena of the colours of thick and thin plates.

In order to observe the phænomenon to the greatest advantage, let the light of a circular image subtending an angle of 1° or 2° be incident perpendicularly, or nearly so, upon two plates of parallel glass placed at the distance of one-tenth of an inch, and let one of the plates be gently inclined to the other, till one or more of the reflected images be distinctly separated from the bright image formed by transmitted light, and received upon the eye placed behind the plates. Under these circumstances the reflected image will be crossed with about fifteen or sixteen beautiful parallel fringes: the three central fringes consist of blackish and whitish stripes, and the exterior ones of brilliant stripes of red and green light; and the central fringes have the same appearance in relation to the external fringes, as the internal have to the external rings, formed either by thin plates, or by the action of topaz upon polarized light. If the two plates of glass are turned round in a plane at right angles to the incident ray, the reflected images will move round the bright image, and the parallel fringes will always preserve a direction at right angles to a line joining the centres of the bright and reflected images. Hence it follows, *that the direction of the fringes is always parallel to the common section of the four reflecting surfaces, which exercise an action upon the incident light.*

The position of the plates remaining as before, let the inclination of the plates, or, what is the same thing, the distance of the bright and the reflected image, be varied by a gentle motion of one of the plates, the coloured fringes will be found to increase in breadth as the inclination of the plates is diminished, and to diminish as the inclination of the plates is increased.

In order to determine the law according to which the magnitude of the fringes varies, I employed two plates of parallel glass $\frac{121}{1000}$dths of an inch thick, and obtained the following measures for the fringes which crossed the image that had suffered two reflections between the plates. The pencil of light was incident nearly in a vertical direction upon the first plate.

Inclination of the Plates.	Angular breadth of each Fringe.
1° 11′	26′ 50″
2 20	13 3
5 36	5 41

Now since 5° 36′ : 26′ 50″ :: 1° 11′ : 5′ 40″, and since 5 36 : 13 3 :: 2 20 : 5 27, it follows, that *the breadth of the fringes is inversely as the inclination of the plates.*

Owing

Owing to the rapid diminution of the fringes, by increasing the angle formed by the plates, I could not with any degree of accuracy determine their breadth at greater angles of inclination; and therefore it still remains to be ascertained whether it varies with the sine, tangent, or secant of the angles.

If the light of the circular object, instead of falling perpendicularly upon the plates, is incident at different obliquities, so that the plane of incidence is *at right angles to the common section of the plates*, no fringes are visible across any of the images. But if the plane of incidence is *parallel to the common section of the plates*, the reflected images increase in brightness with the obliquity of incidence, and the coloured fringes become more vivid. When the angle of incidence increases from 0° to 90°, the images that have suffered the greatest number of reflections are crossed by other fringes inclined to them at a small angle. At an angle of about 44°, the image formed by four reflections is covered with interfering fringes; but it is not till the angle of incidence is greater, that this irregularity is distinctly seen on the image formed by two reflections.

Hitherto I had observed no fringes upon the first or bright image, which is obviously composed of light that has not suffered reflection from the second plate of glass. By concealing, however, the bright light of the first image, so as to perceive the image formed by a second reflection, within the first plate, and by viewing this image through a small aperture, which I found of the greatest service in giving distinctness to all the phænomena, I observed fringes across the first image, far surpassing in precision of outline, and in richness of colouring, every analogous phænomenon which I had seen. When these fringes were concealed, I also observed other fringes on the image immediately behind them, and formed by a third reflection, from the interior of the first plate. I now concealed the second image, upon which the fringes were extremely bright, and very faint stripes were seen upon the one immediately behind it.

On examining these phænomena a little more attentively, I observed that the size of the fringes in the first image varied with the distance of the eye from the plates, while those on the second and fourth image diminished with that distance.

The magnitude of this change will be understood from the following experiments:

	Number of Fringes across the first Image.		Number of Fringes across second Image.	
Angles of Incidence.	Eye near.	Eye a few Inches distant.	Eye near.	Eye a few Inches distant.
0°			6	$6\frac{1}{3}$
47	$4\frac{1}{3}$	$3\frac{1}{2}$	5	$5\frac{1}{3}$
61	$3\frac{1}{2}$	3	4	$4\frac{1}{3}$
73	$2\frac{2}{3}$	$2\frac{1}{2}$	3	$3\frac{1}{2}$

When the fringes on the second image were inclined to the right, those on the first image were inclined to the left; so that, both in point of position and magnitude, the two sets of fringes follow a different law.

The preceding measures of the magnitude of the fringes at different obliquities, were not taken with that accuracy which is necessary for determining the law of their variation. I have made numerous experiments for this purpose; but when the angle of incidence is considerable, there is always such a degree of distortion in the fringes, and such a perceptible variation in their magnitude, from the slightest change in the position of the eye, that I found it quite impracticable to take measures in which any confidence could be placed. This difficulty no doubt arises from the imperfect flatness of the surfaces of the plates of glass; and I fear that even our best artists are not capable of producing better plates than those which I used in the preceding experiments. The following measures may be considered as tolerably correct.

The inclination of the plates was not the same as in the preceding experiments:

Angles of Incidence.	Number of Fringes across the first Image.	Number of Fringes across the second Image.
0° 0′		$6\frac{1}{3}$
36 56	5	$5\frac{1}{4}$
58 48	$3\frac{1}{3}$	$4\frac{1}{6}$
62 52	$4\frac{1}{6}$	$3\frac{2}{3}$
71 30	$2\frac{1}{4}$	$2\frac{1}{2}$

If the two parallel plates are placed at *any distance whatever*, and the preceding experiments repeated, the fringes will be found to suffer no change either in their magnitude or direction.

I now took three plates of parallel glass, that gave the coloured fringes when any two of them were put into the proper position. When the third plate was placed either before the other two, or between them, or behind them, it did not in the least degree affect the fringes which they produced. When it was placed, however, in such a position as to form a new reflected image, this image was also crossed by the coloured fringes.

Whe the *third* piece of parallel glass was cemented with Canada balsam upon the face of the *first* plate, or upon the back of the *second* plate, the fringes disappeared. When the interval between the two plates was filled with water, or with Canada balsam, the fringes were very faint, though distinctly perceptible. Hence it follows, that *the production of the fringes depends*

depends upon the action of all the four surfaces of the two plates of parallel glass.

All the preceding experiments were made with plates which were cut out of the same piece of glass, and had therefore the same thickness. I now tried plates of different thicknesses, both when ground parallel, and when cut from common plate glass; but I could never render the coloured fringes visible, unless when the glass was parallel, and exactly of the same thickness in both plates. I also tried plates of topaz, of equal thicknesses, and plates of sulphate of lime; but though I used pieces of various thicknesses, I have never succeeded in making them exhibit the coloured fringes, owing, perhaps, to the imperfect flatness of their surfaces.

In order to ascertain if the magnitude of the fringes depended on the thickness of the glass plates, I procured a piece of parallel crown glass $\frac{168}{1000}$dths of an inch thick, and compared the fringes which it produced, at an inclination of 2° 20′, and at a vertical incidence, with those produced by another piece of glass $\frac{121}{1000}$dths of an inch thick. In the first case, the circular image was crossed by *five* fringes, and in the second case with *seven* fringes: but

$$\frac{121}{1000} : \frac{168}{1000} :: 5 : 7 \text{ nearly.}$$

In another experiment, I found, from a mean of five measurements, that the thickest of these pairs of plates produced fringes each of which had a breadth of 11′ 10″, when the inclination of the plates was 1° 58′. Now the other pair of plates gave fringes 13′ 3″ broad, at an inclination of 2° 20″, which gives 15′ 29″ for their breadth at an angle of 1° 58′, and

$$\frac{121}{1000} : \frac{168}{1000} :: 11' \, 10'' : 15' \, 30''.$$

Hence *the magnitudes of the fringes are inversely as the thicknesses of the plates which produce them, at a given inclination;* and in general *the magnitudes of the fringes are in the compound inverse ratio of the thickness of the plates, and of their angle of inclination.*

Hitherto we have supposed the glass plates to be placed between the eye and the luminous object, so that only the 2d, 4th, and 6th reflected images were seen. When the eye is placed between the plates and the luminous object, so as to perceive the 1st, 3d, and 5th reflected images, the coloured fringes are also seen, having the same characters as those already noticed.

The phænomena which have been described are equally produced when the fringes are formed by polarized light, and they do not suffer the least change when examined by doubly refracting or doubly polarizing crystals.

When the eye is placed at a considerable distance, either before or behind the glass plates, all the fringes have a very distorted appearance, arising probably from the imperfect figure of the reflecting surfaces.

In order to explain the changes which the light undergoes in its passage through the plates of glass, let AB, CD, Plate VII. fig. 1. be a section of two plates at right angles to the common section of their surfaces, and let RS be a ray of light incident nearly in a vertical direction. This ray, after passing through the first plate AB, will suffer a small refraction at P and Q, and emerge in the direction QV parallel to RS. At the point P, in the second plate CD, the ray TP will be reflected to a, again reflected to b, and after suffering a refraction at b and c, will emerge in the direction cd, forming with RV an angle equal to twice the inclination of the plates. A portion of the reflected ray Pa will enter the first plate at a, and having suffered reflection and refraction at β, the reflected portion $\beta\gamma$ will reach the eye at θ. The ray Pabc will likewise suffer a reflection at c and at e, and will reach the eye at g. In like manner, a part of the ray PQ will be reflected at Q, and move in the direction Q$rstuv$, and another part of it in the direction $swxyz$, and these rays will suffer several other reflections; but the images which they form will be so faint, that the eye will not be capable of perceiving them. When the observer, therefore, looks at a luminous body, in the direction SR, through the glass plates, he will perceive two images, one of which is a bright image, seen by the transmitted light QV, and the other is a faint image, seen principally by the reflected light P$abcd$, and composed of several images formed by the pencils cd, uv, $\varepsilon\theta$, z&, and eg. The bright image is not crossed by coloured fringes, but the fringes appear distinctly upon the other image; and the light by which these fringes are formed, has suffered two reflections from the exterior surfaces, and two refractions at the interior surfaces of the plates.

When the ray RS is incident obliquely, so as to produce the coloured fringes, the plane of incidence is parallel to the common section of the plates. In this case, it is difficult to represent in a diagram the progress of the rays, as they are reflected in a plane at right angles to that in which they are refracted. The changes, however, which the light must undergo in the production of the fringes, may be understood from figs. 2, 3, 4, 5, 6, 7, and 8, where AB and CD are the two plates of glass inclined at a small angle, and RS a ray of light incident obliquely, in a plane at right angles to the common section of the plate.

In fig. 2. the plates are so arranged, that the incident ray RS

RS does not pass through the first plate AB. In this case, the *fringes are produced* in the same manner as if the ray had passed through AB.

In fig. 3. the rays reflected from the plate AD do not pass through the second plate CD. In this case, the *fringes are produced* as formerly.

In fig. 4. the reflection from the external surface *mn* of the plate AB is destroyed by a layer of indurated Canada balsam. In this case *no fringes are produced.*

In fig. 5. the refraction and reflection at the interior surface *op* of the plate AB are destroyed by a layer of Canada balsam. In this case *no fringes are produced.*

In fig. 6. the refraction of the interior surface of the plate CD is destroyed by a layer of Canada balsam, and in this case *no fringes are produced.*

In fig. 7. the reflection from the external surfaces *mn*, *op*, of the two plates is destroyed, and *no fringes are produced.*

In all these cases, the fringes are obviously produced by a refraction and a reflection in each of the two plates, and the interfering fringes are produced by the secondary reflections within the glass plates.

The fringes, however, which appear upon the first or bright images, are produced in a different manner from those formed by the light that has been reflected from the plate CD; for the light of which they are composed has suffered two or more reflections within the plate AB, as shown in fig. 8. and two refractions by the plate CD. These refractions are absolutely necessary to the production of the fringes; for they disappear when the light reaches the eye, without passing through the second plate. Any variation in the distance of the plates, when their inclination and thickness remain the same, ought obviously to produce no change in the appearance of the fringes, as the fits will return in the same manner as before.

In order to compare the preceding phænomena with the Newtonian Theory of Fits, I propose to resume the investigation with plates of parallel glass, that differ very considerably in thickness, and that have their surfaces ground as flat and polished as highly as possible; and I have no doubt but that all the results may be calculated by means of that beautiful theory.

The fundamental experiment by which I ascertained the production of coloured fringes by two plates of glass of equal thickness, has been repeated and verified by my friend M. Biot of the Institute of France, and was exhibited at a public meeting of that distinguished body.

 LXVI. *Ob-*

LXVI. *Observations and Experiments on the new Species of coloured Fringes discovered by Dr.* Brewster, *in the Light reflected between two Plates of parallel Glass of equal Thickness. By M.* Biot, *Member of the National Institute, &c. &c. &c.**

The discovery of a new species of coloured fringes by M. Brewster being extremely remarkable, I was anxious to verify it by experiment. For this purpose, I first made use of two plates of glass cut from the sides of a plane mirror with parallel surfaces which had been ground and polished by M. Cauchoix. Each of these plates had a thickness of about three millimetres (a little more than 1-8th of an inch). I then placed them one upon another, keeping them separate, at their extremities, by small pieces of card of the same thickness, which I could multiply at pleasure when I wished to increase the distance or the inclination of the plates. In this way I obtained the coloured fringes which M. Brewster has announced. I obtained them even when the distance of the plates was at least two millimetres (nearly 1-13th of an English inch): but it ought to be remarked, that the experiment is very delicate when the thickness of the glasses is great, and their distance considerable; for it is then very easy to go beyond the limits of inclination within which the phænomenon is produced. The fringes were more easily obtained with thin plates of glass about half a millimetre in thickness (1-50th of an inch). It then appeared to me, that at a given distance the phænomenon began to be produced by these plates when the incident ray formed a much more considerable angle with their surface. It appeared also, that in proportion to the thinness of the plates the parallelism of their surfaces was no longer a condition rigorously necessary; for those which I used had not been wrought for this purpose, and were a little prismatic. It is, besides, easy to convince oneself that the light which produces the fringes has been reflected several times from one plate to the other; which explains why the fringes cease to be produced when one of the exterior surfaces is wetted, as M. Brewster has remarked.

These fine experiments have obviously a great connexion with those which Newton has described at the end of his Optics, relatively to the coloured rings formed with thick plates of glass whose two surfaces were spherical and nearly concentric. In the experiments of Newton the incident ray falls at first perpendicularly upon the plate, and traverses it once. A great part

* From the *Bulletin des Sciences par la Société Philomatique de Paris,* Mars 1815, p. 44.

of

of the light is, besides, reflected perpendicularly by the second surface, and emerges at the place where it entered; but the portion of light reflected irregularly at this second surface radiates in all directions from the point of reflection. The luminous molecules, therefore, of which it consists, traverse the glass a second time, but in a different direction; and thus the length of their fits changes both from the different extent of their paths, and from the obliquity of their directions to the reflecting surfaces. Hence it follows that, in returning to the first surface of the mirror, some of these particles are in fits of transmission, and others in fits of reflection. Knowing therefore the length of their primitive paths, and also that which they describe in their return, and the ratio in which the fits of the particles are enveloped by obliquity, it is easy to calculate in what points each colour ought to emerge, and in what points the other colours ought to be reflected anew. By following the light emerging into the air according to the ordinary law of refraction, one can calculate the diameter of the rings which ought to be formed upon a white surface at any distance from the mirror. This is what Newton has done, for plates which had a thickness of nearly a quarter of an inch; and the results were exactly conformable to the calculations even when the particles in first traversing the plate had suffered more than 34386 fits.

In the experiments of M. Brewster, however, the equal thickness of the two plates and the small inclination of their surfaces appear to me to act the part of the equal curvature of the two reflecting surfaces in Newton's experiments, the inclination of the plates having the same influence as the sphericity in changing the length of their paths. It therefore appears to me probable that the two results may be calculated by the same formulæ, and I propose in a short time to verify this supposition. But, whatever be the result, I have thought that natural philosophers would peruse with pleasure these details relative to experiments which will probably lead to a thorough knowledge of the manner in which the coloured rings are produced.

LXVII. *Dr.* Gilby *in Reply to Mr.* Farey *on the Stratification of Great Britain.*

To Mr. Tilloch.

Sir,—Your readers will (I doubt not) be heartily tired of a controversy which is neither productive of interest or instruction, and which consists of little else than recrimination bandied from one side to the other. I foresaw that a few expressions in in my last paper could not fail to excite the irritability of so pugnacious a polemic as Mr. Farey, and in that case I resolved to forbear from any reply; and, to use a childish phrase, to allow him the last blow. But upon reading this reply, I find it so full of misrepresentation, and to contain so many perversions of my meaning, that I cannot forbear noticing them.

First, then, I have not made any use in my *last* paper of Professor Jameson's name, in order to appropriate to myself the discovery of the unconformable position of the red ground, as Mr. F. has falsely asserted top of p. 279.—My words are, "But any thing I can say will be of far less consequence than the opinion of Professor Jameson, who allows me to state, that as far as he can judge from the description I have given, and the specimens I have shown him, he considers the red sandstone and mountain limestone as members of the first flœtz formation."

I do not unhandsomely charge Mr. Farey with wishing to introduce confusion as to the geology of England; for I say nothing more than is warrantable from the flagrant geological errors which Mr. F. has committed. One of the most preposterous is that of referring the sienite of Leicestershire to that *omnigenous* formation, the red ground,—contrary, I venture to say, to the opinion of every sensible geologist in Britain; and contrary to the facts so beautifully displayed by the stratification on the western side of the Malvern range. This range, I need not repeat, consists in great measure of a sienite, which even Mr. Farey, as he wishes to imbed it in the red ground, cannot but allow to have been formed at the same time with the sienite of Leicestershire. The fact to which I allude I have mentioned p. 186 and 188 of my paper, but I beg leave once more to press them upon Mr. Farey's notice. Upon the west side of the range there rests a formation of limestone, which Dr. Prichard in the Annals of Philosophy assures us is seen dipping under the red sandstone of Herefordshire. This red sandstone, as I have myself ascertained in company with Dr. Prichard, lies below the mountain limestone; so that we have these rocks formed, besides a world of coal beds and coal measures, before the red ground, which is supposed to be a twin production with the sienite, was deposited. All these facts, however, will, I dare say, be of little avail in altering Mr. Farey's opinion; and I unfortunate

"Non profecturis littora bobus aro."

Another of these extravagancies, by which Mr. Farey outrages every thing like system, is to be found at p. 280 of his Report, where he gives it as his opinion, without the shadow of a proof in support of that opinion, "that the *4th limestone of Derbyshire* is the *lowest* which is any where seen in England, not excepting

cepting the rocks of Devonshire and Cornwall, which probably (says he) will appear to rank with the *red marl* in the British series; and so, perhaps, will those of the great part of Wales."

This is Mr. Farey's English Geology: and it is quite of a piece with his English Mineralogy; for, upon finding some rock resting upon the sienite of Leicestershire, he with the utmost simplicity tells us that he had "rather call it coarse slate, than *risk* any of the German names greenstone, hornstone, schistus, trap formation,"—substances as different from one another, and from coarse slate, which I suppose should be read clay slate, as can well be imagined, and which it is hard to conceive how one who pretends to be a mineralogist, and who even styles himself a *mineral surveyor, should not know* how to distinguish. After having thus strangely perverted the order of succession as determined by the united labours of the ablest men in every country, will it be believed that Mr. Farey should think himself of sufficient authority to venture to assure us, that it is a vain attempt to reconcile the facts of British stratification to the Wernerian system? This however is the *ipse dixit* of our English Werner, and it becomes us to bow in humble submission to this oracle of geological knowledge.

I have not anywhere said that Mr. Jameson considers the limestone of Derbyshire to be flœtz, and nothing but a determination to misrepresent my meaning could have induced Mr. Farey so far to misquote me:—for, after mentioning Professor Jameson's opinion that the mountain limestone, and the old red sandstone usually associated with it, belong to the first flœtz formation; I immediately add, "it is a point yet to be ascertained, whether the limestone associated with the coal-field of Derbyshire is to be considered as belonging to the flœtz or transition series"—thereby implying that Mr. Jameson's opinion does not extend to the limestone of Derbyshire, which is altogether different in its geological relations from the limestone associated with the coal-fields in other parts of England.

But of all the misrepresentations which Mr. F. has made, the most vexatious is that where he says that I seem to have taken it for granted that the old red sandstone occurs universally between the Derbyshire-peak limestone and the coarse slate. I have nowhere mentioned during the course of my paper either of these rocks, and hardly know what they mean. But if Mr. F. means by the Derbyshire-peak limestone the 4th limestone; I have been so far from having taken it for granted that the red sandstone is found under this rock, that in a separate paragraph I have mentioned the absence of the red sandstone as one of the circumstances which distinguish the Derbyshire limestone from the mountain limestone in other parts of England. This paragraph,

paragraph, p. 185, Mr. F. has read and quoted from it: and although I wrote it for the express purpose of stating that the stratification of Derbyshire forms a striking exception to every thing I had been saying respecting the mountain limestone in other parts of England; yet Mr. Farey has several times in the most unhandsome manner applied *to the limestone of Derbyshire, what I had said of the mountain limestone so commonly associated with the red sandstone.* But the strangest thing of all is, that after Mr. F. had understood me in the way above mentioned, he should cite the passage where I had asserted the *absence* of the red sandstone, and upbraid me for having spoken in too unqualified a manner on the subject, as the under strata of the 4th limestone are still unknown, so that the red sandstone may still be found under the limestone.

After having quoted authorities to show that a red sandstone occurs under the mountain limestone in Somersetshire, Gloucestershire, Herefordshire, Monmouthshire, Brecknockshire, Shropshire, in the north of England, and in Anglesea (as Mr. Farey now mentions), are not these facts sufficient to show that it is not merely an accidental association, but that it will be found still more general, particularly if it be true that the red sandstone commonly occupies the low part of the country?

I have not charged Mr. Farey (towards the bottom of p. 187) with the inaccuracy and confusion of not knowing the relative age and situation of the red marl to be more recent or above the coal-measures, as he has most falsely asserted. The latter part of the sentence, "that not only members of the flœtz but even of the transition series have been referred to this formation," is plainly the inaccuracy with which in part only I have charged Mr. Farey.

I have little doubt that this letter will be followed by another reply from Mr. Farey: but as I have sufficiently attained my purpose by correcting the mis-statements which Mr. Farey has made, and as I can employ my time much more to my inclination than by carrying on a controversy of this nature, I beg leave to say that I shall on no account take any further part in it.

I am, sir,

Very respectfully yours,

W. H. Gilby, M.D.

York Crescent, Clifton, Bristol,
Nov. 11, 1815.

P.S.—Is it not probable that the 4th limestone of Derbyshire may hereafter be found to rest on the sienite?

The word *Ness* has been misprinted for *Ross* in Herefordshire.

LXVIII. *Further*

LXVIII. *Further analytical Experiments relative to the Constitution of the prussic, of the ferruretted chyazic, and of the sulphuretted chyazic Acids; and to that of their Salts; together with the Application of the Atomic Theory to the Analyses of those Bodies.* *By* Robert Porrett, *jun. Esq. Communicated by* W. H. Wollaston, *M.D. Sec. R.S.**

The Royal Society did me the honour of printing in the volume of their Transactions for last year, a paper of mine on the nature of the salts termed triple prussiates, and on acids formed by the union of certain bodies with the elements of the prussic acid.

In that paper I endeavoured to prove that the elements of the prussic acid would combine with a certain proportion of black oxide of iron, and form a peculiar and hitherto unknown acid, for which I proposed the name of the ferruretted chyazic acid. I showed that this was the real acid portion of the salts which had received the erroneous appellation of triple prussiates, and that the property of combining with the prussic acid, so as to change its nature, and increase its acid properties, was not confined to the black oxide of iron, but was possessed probably by many other bodies, but certainly by sulphur, which formed with it another acid, for which I proposed the name of the sulphuretted chyazic acid. The paper also contained some analytical researches into the proportions in which the elements of these new acids are combined in them, and also into the proportions in which they unite with different saline bases.

My object in this paper is to add to the analyses contained in the former, two analyses which I have since made; and then to apply to the whole the admirable theory of Dalton, by which the proportions in which bodies can combine are conceived to be governed by the relative weights of their chemical atoms, and also Berzelius's addition to this theory, by which the combinations of oxides with one another are conceived to take place in such a manner, that the oxygen contained in one of these bodies is either equal to or is a multiple by a whole number of the oxygen contained in the others.

I begin with describing the two analyses to which I have just alluded.

Analysis of Prussiate of Mercury.

A. Fifty grains of this salt finely pulverized were kept at the temperature of 212° for six hours, at the end of which time they weighed exactly the same as before.

* From the Philosophical Transactions for 1815, part ii.

B. Forty

B. Forty grains of this salt were dissolved in water and decomposed by hydro-sulphuret of potash: the products of this decomposition were prussiate of potash and black sulphuret of mercury; the quantity of the former could not be ascertained with accuracy, owing to the escape of much of the prussic acid, but that of the sulphuret amounted to 37·2 grains.

C. Disappointed in my attempt to estimate the quantity of prussic acid by the last experiment, owing to its very volatile nature, I availed myself of the property I had discovered in the hydroguretted sulphurets, of converting the prussic acid at the moment they detach it from prussiate of mercury, into sulphuretted chyazic acid; which being much less volatile, and having a stronger attraction for alkaline bases than the prussic, could not escape from the liquid, and would give me the quantity of prussic acid it represented, by deducting from its weight that of the sulphur which I knew to exist in it. I therefore dissolved ten grains of prussiate of mercury in hot water, and poured hydroguretted sulphuret of soda into the solution until it no longer occasioned a black precipitate. This black precipitate when dry weighed 9·3 grains: to the liquid from which it was separated I added a few drops of diluted sulphuric acid: these caused a separation of a minute quantity of sulphur, which was got rid of by subsidence, after which I poured into it an aqueous solution of the two sulphates of copper, and of black oxide of iron, in which the former salt was to the latter by weight as 2 is to 3, until no further effect was produced. By these means I threw down the whole of the sulphuretted chyazic acid contained in the liquid, and collected it combined with protoxide of copper in the form of an insoluble white salt, which weighed 9·7 grains.

But as 100 grains of this salt contain 40·62 grains of sulphuretted chyazic acid, composed of 26·39 sulphur and 14·23 prussic acid, according to my analysis, Phil. Trans. for 1814, page 549, Exp. C. (corrected by calculations in the table facing page 370 of the present paper), therefore the before-mentioned 9·7 grains represent 1·38 of prussic acid, which according to this experiment is the quantity existing in 10 grains of prussiate of mercury.

D. I had next to ascertain how much red oxide of mercury was represented by the 37·2 grains of black sulphuret obtained in Experiment B, and by the 9·3 grains of the same substance obtained in Experiment C. In order to effect this, I made the following experiment: 25 grains of corrosive sublimate were dissolved in water, and decomposed by hydro-sulphuret of potash; the black sulphuret thus formed weighed 21·5 grains, which therefore represents 19·94 grains of red oxide of mercury, that being

being the quantity contained in 25 grains of corrosive sublimate.

Then as 21·5 : 19·94 : : 37·2 : 34·48 the quantity of red oxide in 40 prussiate of mercury,

And as 21·5 : 19·94 : : 9·3 : 3·62 the quantity of ditto in 10 of ditto,

100 grains of prussiate of mercury are therefore composed of

Prussic acid, Experiment C.	13·8
Red oxide of mercury, Experiment B. C. and D.	86·2
	100·0

Analysis of Prussic Acid.

Being very desirous of accomplishing the analysis of this acid if possible, I considered very attentively the nature of the difficulties to be surmounted in order to effect it. The principal ones appeared to me to be the following.

1st. That of always ascertaining with precision the quantity which is the subject of analysis.

2d. That of effecting its combustion with oxygen in such a manner, that while, on the one hand, the whole of its carbon and hydrogen should be oxygenated; so, on the other, that none of its azote should undergo this process.

3d. That of determining with great accuracy the quantity of oxygen which combines with the elements of the prussic acid during its combustion, so as, after allowing for what has been expended in the formation of carbonic acid, to be able to infer with confidence, from the disappearance of the rest, the quantity of hydrogen which was contained in the acid.

The property which the prussic acid possesses of assuming the liquid form at a low temperature, and that of a gas or vapour at common temperatures, the volume of which is materially influenced by mixture with other gases, and by slight alterations of temperature and pressure, did not appear to me to be favourable to the employment of it in an uncombined form for the purpose of its analysis.

I therefore determined upon employing it in the state of condensation in which it exists in prussiate of mercury, and this determination made me undertake the analysis I have just described of that salt; of the correctness of which having satisfied myself, I conceived that I had overcome the first difficulty.

The second and third difficulties I thought would be best surmounted by employing, for the combustion of the prussic acid, the same oxide with which it is united in the prussiate of mercury, namely, the red oxide of that metal; increasing the quantity of it by multiples of that which the salt contains, until I found that the whole of the prussic acid was decomposed.

I made

I made a number of experiments upon this plan, the results of which proved to me that the quantities of carbonic acid and of azote gases produced, did not arrive at the maximum until five times the quantity of red oxide of mercury contained in the prussiate had been added to it, making together six of that oxide to one of prussic acid; and that, whenever a less quantity of the oxide than this had been employed, there always existed in the gaseous products a portion of undecomposed prussic acid. I further observed, that in all cases the volume of azote gas obtained was exactly equal to that of the prussic acid decomposed, that the volume of carbonic acid gas produced was invariably twice that of the azote gas liberated in the same experiment, and that the carbonic acid produced accounted for only one-third of the oxygen consumed. The observance of these laws by which the decomposition was regulated, enabled me in constructing the following table (facing page 370,) to correct the minute and unavoidable inaccuracies of experiment, by the superior accuracy to be acquired by applying to the results so obtained, the corrections necessary to make them correspond with the above-mentioned laws. It enabled me also to represent in the column denoting the measures of prussic acid gas, equal quantities by equal bulks; which, for the reasons before stated, experiment does not exactly show, and thus to render evident the true progress of its decomposition.

It may be proper, before proceeding further, to describe my mode of operating in conducting the experiments from which the table was compiled. This mode is similar in principle to that invented by Gay Lussac and Thenard in their Analysis of Animal and Vegetable Substances, and improved by Berzelius. I am greatly indebted to those two French chemists for the valuable information respecting this kind of analysis, which I have obtained from their *Recherches Physico-Chymiques*, and to Dr. Berzelius for that which I have received from his paper on the definite proportions in which the elements of organic nature are combined, published in Dr. Thomson's Annals of Philosophy for December last. It is to this information that I principally attribute the success which has attended the experiments of a similar nature which I have made.

The method pursued by me, however, differs in several respects from that of either of the chemists just mentioned.

1st. In the apparatus employed, which is much more simple in my process.

2dly. In the nature of the oxygenised body employed to effect the combustion.

3dly. In the method to which I had recourse for proportioning the oxygenised to the combustible body, by making the former

former a multiple of that which enters into chemical union with the latter.

4thly. In decomposing a much less quantity of the combustible body at a time, than either of the above chemists.

In the present case, each of these alterations appeared to me to possess very decided advantages over the other methods. How far they may be applicable to other cases, I do not pretend to determine.

Having thus generally stated in what my process differed from former ones, I proceed to rather a more particular description of it.

I prepare the peroxide of mercury which I employ, by decomposing with pure soda, a solution of corrosive sublimate. Having weighed out the proportions of prussiate of mercury, and of the peroxide which I intend to decompose, I triturate them together in a small polished mortar of porphyry or agate for several minutes, then collect into the centre of the mortar what adheres to its sides, and repeat this alternate trituration and collection at least six times.

I then take a tube of glass $4\frac{1}{2}$ inches long, about the size of a common writing quill and tolerably stout, I close one end of it, and bend the other round, so that nearly an inch of that end forms a right angle with the rest. I call this the retort tube. I make a second tube similar to the first, except that, instead of being closed at one end, it is open at both. I call this the adapting tube.

The retort tube is then charged with the mixed materials, by means of a small paper funnel fixed with sealing-wax to the top of the tube; the charge is introduced in about three equal portions, each of which is separated from the others by the introduction of a little coarsely powdered green glass: the charge generally occupies about $2\frac{1}{2}$ inches of the tube. After its introduction, the wax which fastened the funnel is softened by heat, and the funnel detached.

A graduated glass tube, capable of containing about $2\frac{1}{2}$ cubic inches, was next filled with mercury, and placed in the mercurial pneumatic trough, not in the usual perpendicular position, but with its upper end raised, but very little, from the horizontal situation, being about an inch above the surface of the mercury, while its lower or open end just dipped below that surface. In this position, the long leg of the adapting tube was passed up into it, which being open at both ends became filled with mercury: the short end of this tube was then connected with the short end of the retort tube, by means of a caoutchouc tube firmly tied to both. The long end of the retort tube, when thus disposed, hung over the outside of the end of the mercurial trough, in a position declining a little from the horizontal one towards the table. The decomposition was then commenced, by applying the flame of a spirit-lamp to the empty part of the tube, and bringing it down gradually, so as to explode in succession the three strata of the mixture. The arrangement of the apparatus at the commencement of the process will be instantly seen by an inspection of the annexed sketch. (Plate VI. fig. 5.)

When the retort tube was cold, it was separated, under the surface of the mercury, from the adapting tube, in such a manner, that any gas remaining in the latter might pass up into the graduated tube: the volume of gas collected was then ascertained, making the necessary corrections for temperature, pressure, and the capacity of the retort tube; after which a solution of pure potash was passed up into it, and the diminution of volume which it occasioned was noticed: from the gas which remained, a deduction was made, for the quantity of atmospheric air in the upper part of the retort tube before the combustion, and which seldom exceeded $\frac{1}{30}$th of a cubic inch: the residual gas was considered as azote, and found to be so by all the tests to which I subjected it. The small quantity of solution of potash employed to effect the absorption was then examined; and if, besides carbonic acid, it was found to contain prussic acid, I concluded that I had not employed enough of the red oxide of mercury in the combustion, and repeated the experiment with an increased proportion of it.

Such was my method of effecting the analysis of the prussic acid, and by which, as will be seen in the last line of the table, I succeeded in discovering that 0·3442 gr. of it were composed as follows:

Carbon	= to that in 0·4389 gr. of carbonic acid, or	0·1198
Azote	= to the weight of the azote gas collected	0·1401
Hydrogen	= to that in 0·7230 gr. of water	0·0843
		0·3442

consequently that 100 grains contain

Carbon	34·8
Azote	40·7
Hydrogen	24·5
	100·0

Having finished my analytical investigations, I pass on to the last division of my subject, which is the following comparative view of the composition of the prussic, ferruretted chyazic, and sulphuretted chyazic acids, and of their salts, as deduced from my analytical experiments, and as inferred from the atomic theory.

TABLE

TABLE showing the Results of the Decomposition by Heat of Prussiate of Mercury, by itself, and also when mixed with Multiples by whole Numbers of its Base.

Materials before Decomposition.					Products after Decomposition.														
Prussiate of Mercury and red Oxide of Mercury.			Weight of Oxygen in Oxide of Mercury.	Total Weight of Materials.	Gases.								Water.	Mercury.	Weight of Oxygen in Products.			Total Weight of Products.
					Prussic Acid.		Carbonic Acid.		Azote.		Total.				In the Carbonic Acid.	In the Water.	Total.	
Grs.	Prus. acid.	Red ox. mer.	Gr.	Grs.	C. In.	Gr.	C. I.	Gr.	C. I.	Gr.	C. I.	Gr.	Gr.	Grs.	Gr.	Gr.	Gr.	Grs.
2·5 of prussiate of mercury or red oxi. merc.	0·344 +	2·155	0·159	2·5	0·395	0·2366	0·158	0·0732 *	0·079	0·0234 †	0·632	0·3832	0·1205	1·995	0·053	0·106	0·159	2·4987
Ditto, with 2·155 or	0·344 +	4·31	0·319	4·655	0·316	0·2293	0·316	0·1465	0·158	0·0467	0·790	0·4223	0·2410	3·991	0·106	0·213	0·319	4·6549
Ditto, with 4·31 or	0·344 +	6·465	0·479	6·81	0·237	0·1719	0·474	0·2195	0·237	0·0701	0·948	0·4615	0·3615	5·986	0·160	0·319	0·479	6·809
Ditto, with 6·465 or	0·344 +	8·62	0·638	8·965	0·158	0·1146	0·632	0·2926	0·316	0·0934	1·106	0·5006	0·4820	7·982	0·213	0·425	0·638	8·9646
Ditto, with 8·62 or	0·344 +	10·775	0·798	11·12	0·079	0·0573	0·790	0·3658	0·395	0·1168	1·264	0·5399	0·6025	9·977	0·266	0·532	0·798	11·1194
Ditto, with 10·775 or	0·344 +	12·93	0·957	13·275	0·000	0·0000	0·948	0·4389	0·474	0·1401	1·422	0·5790	0·7230	11·972	0·319	0·638	0·957	13·274

* Weight of Carbonic Acid taken as 0·463 Grain for a Cubic Inch.

† Weight of Azote taken as 0·2956 Grain for ditto.

I was very well aware of the probability of my placing some of my analyses in a very unfavourable light, by contrasting the results obtained by the application of a theory capable of giving the composition of bodies with absolute certainty, with those results which I have obtained by practical experiments on a class of bodies hitherto little examined or understood, and the analyses of which were very difficult: but I would not allow this consideration to have any influence in deterring me from making such a contrast; for, as I had not the vanity to give these analyses as perfect, so I feel no mortification in now proving that they were not so; and being confident that I had not spared either time or trouble in making them, I expose their imperfections without hesitation, confiding in the candid judgement of those who, having undertaken similar investigations, are aware of the numerous difficulties and sources of error attendant upon them.

I have arranged and collected these comparisons into the form of a Table, which I beg leave now to introduce.

I infer from the Table, that the acids and salts included in it, are so composed as to harmonize perfectly with the doctrines of Dalton and Berzelius, and to be very compatible with the opinion respecting the compound nature of azote.

I shall be happy if this attempt to elucidate the nature and composition of these bodies adds in any degree to the daily and rapid progress now making in chemical science.

Tower, Feb. 22, 1815. ROBERT PORRETT, Jun.

LXIX. *Dreadful Accident occasioned by the Explosion of a Boiler at Mr.* CONSTANT'S *Sugar-House in Well-Street on the 15th of November.*

It has lately been ascertained that when the boiling of sugar, in the process of refining, is carried on without any fire being allowed to come directly in contact with the pan, a waste of sugar is prevented, and a better article obtained. On the process of refining we do not mean to offer any remarks, but merely to speak of the accident which has occurred in consequence of steam being employed, in an injudicious manner, to boil the sugar pans in place of fire as hitherto. The arrangement was simply this:—A large close boiler was constructed for the purpose of generating steam, to be conveyed through tubes, under the sugar pans, to bring them to the required temperature for boiling the syrup. These pans, made of copper, were each put into an exterior pan made of cast iron, and closely joined at their brim to prevent the escape of steam. Only one pan we believe had

had been got ready to be worked in this manner; and on the 15th of November a trial was made of the boiler.

The fire was lighted between three and four o'clock in the morning. At nine o'clock Mr. Hague the engineer came to the premises, and it was proposed to prove the boiler by applying a large fire. Mr. Constant the proprietor objected to the large fire; but through the obstinacy of some of the engineer's men, as is believed, the fire was urged unnecessarily, and even the safety-valve, provided for the escape of steam when the internal pressure should reach a certain point, was overloaded to prevent the steam from escaping. The consequence which might have been anticipated followed. About half past ten o'clock the boiler exploded, and with such a force as to bring down the whole building, burying a number of people in the ruins. The house was about 70 feet high, and of proportional depth and width. The effect of such an explosion may be more easily conceived than described. Most miraculously, several of the people who were buried in the ruins escaped without personal injury, the lower part of one of the walls keeping up one end of the joists of a part of the lower floor, which was thus thrown over them as a shed. In the course of the day ten other people were dug out of the ruins, seven of them less or more burnt, lacerated or bruised, who were sent to the hospital, and three of them dead. Among the latter was Mr. Spear jun. aged 15, son of Mr. H. A. Spear of Broad-street, who was there at the time on business, his father having sent in a great quantity of sugar to be manufactured.

The effects of this accident did not however end here. After the ruins were partly removed, air getting to the wood which had come in contact with the scattered fire of the furnace, the whole, at night, burst out in a violent flame, which communicated to two contiguous sugar-houses, also belonging to Mr. Constant, which were entirely consumed.

Great blame attaches somewhere; and the accident is the less excusable, as this is not the first arising from ignorance or inattention in the application of steam of high temperature to different purposes. Only a few months ago a loco-motive engine was exploded in the country, and several people lost their lives, from the folly of a man (calling himself an engineer, a name now given to every person who is employed to throw coals under a boiler,) locking down the safety valve, that his machine might go off in style! And latterly, a salt-pan heated by steam was blown up by a similar imprudence. Such madness cannot be sufficiently reprobated. Nothing is more manageable than steam, in the hands of men of common prudence; but nothing more dangerous when fools and pretenders are suffered to play with

with it. The very idea of *proving the boiler with steam* savoured of insanity; for, if too weak, it could do nothing but explode. What would be said of the man who should seek to ascertain the lowest heat at which gun-powder would explode, by thrusting in succession into a barrel of that article, bars of iron heated to different degrees of temperature?

When a large boiler is to be employed to generate steam of high temperature, it should be proved, not with water and a fire applied to it, but with cold water forced in by a pump or syringe till the boiler has been subjected to more than twice the pressure to which it is intended it shall ever be exposed with steam. The most accurate way to ascertain the pressure is by a tube of sufficient length connected with the boiler, and containing mercury. Should a boiler subjected to this test prove too weak, it only rends at the weak part—no explosion takes place, and no one can receive any personal injury.

We went to the ruins on the 20th of November, and ascertained several facts, indicating in our opinion either great ignorance or great carelessness on the part of those who had the care of constructing the boiler and pans. In the first place, as to the form of the boiler itself,—it was somewhat globular, with a concave bottom, and of no less than *eight feet diameter**. For generating strong steam, boilers composed of tubes of comparatively small diameter should always be employed; for the strength *ceteris paribus* is inversely as the squares of the diameters of the vessels compared. The best, indeed the only boiler with which we are acquainted, that can with safety be employed for such a purpose, is Woolf's, composed of tubes. The substance of the boiler which we saw in Well-street, was in no part two inches—in some parts not more than one inch. In other words—a boiler made for the purpose of generating steam of a pressure of 40 or 50 pounds per inch†, and not exceeding an inch in thickness, (for the greater thickness in some places goes for nothing in such a case as this,) and eight feet diameter, was set to work without even its substance (so far as we could learn) having

* The fragment which we saw (perhaps about a fourth of the whole boiler) had been removed about twenty feet from its original position into another apartment, from which it had before been separated by a brick wall.

† It cannot be ascertained to what pressure the boiler had attained when the explosion took place. Mr. Constant saw the gauge a few minutes before, and it was then under 40 pounds per inch: one of the workmen saw it at 46. From the weakness of the boiler, it is not probable that the pressure was much beyond the latter point when the boiler exploded; but from the great accumulation of heat in the substance of the boiler, in the furnace and in the surrounding materials an instant generation of a new quantity of steam would be produced by the liberated water.

been

been ascertained either by drilling it in different places, or by calculating from its weight its general thickness, supposing that to have been uniform.

We need not stop to say much on the construction of the sugar pans and the steam-case under them: suffice it to say, that on inquiry (for these had not been cleared from the ruins when we were on the spot*) we learnt that these also were eight feet in diameter — and, if we understood rightly, the bottom of each pan was nearly a plane, or had scarcely any discernible rotundity.—Pans that are to be heated by strong steam should be narrow, to gain strength; and the surface wanted for evaporation should be obtained by giving them sufficient length.

Such accidents are much to be lamented, not only on account of the individual and family sufferings which they occasion, but also for the impediments they serve to throw in the way of improvements in the manufactures of the country: for, though it is certainly true that steam, as we have observed, is perfectly manageable and safe in proper hands, yet when people witness such calamities, and that under the management of people *supposed* to be competent, nothing can be more natural than that, instead of encouraging, they should be led to oppose improvements which threaten such dreadful consequences.

LXX. *Notices respecting New Books.*

The Philosophical Transactions for 1815, Part II, contain the following papers:

10. On some Phænomena of Colours exhibited by thin Plates. By John Knox, Esq. Communicated by the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—11. Some further Observations on the Current that often prevails to the westward of the Scilly Islands. By James Rennell, Esq. F.R.S.—12. Some Experiments on a solid Compound of Iodine and Oxygen, and on its chemical Agencies. By Sir Humphry Davy, LL.D. F.R.S.—13. On the Action of Acids on the Salts usually called Hyperoxymuriates, and on the Gases produced from them. By Sir Humphry Davy, LL.D. F.R.S.—14. Further analytical Experiments relative to the Constitution of the prussic, of the ferruretted chyazic, and of the sulphuretted chyazic Acids; and to that of their Salts; together with the Application of the atomic Theory to the Analyses of those Bodies. By Robert Porrett, jun. Esq. Communicated by W. H. Wollaston, M.D.

* One of them has been found since, not broken, but turned upside down—a proof that the explosion happened in the boiler.

 Sec.

Sec. R.S.—15. On the Nature and Combinations of a newly-discovered vegetable Acid; with Observations on the malic Acid, and Suggestions on the State in which Acids may have previously existed in Vegetables. By M. Donovan, Esq. Communicated by W. H. Wollaston, M.D. Sec. R.S.—16. On the Structure of the Organs of Respiration in Animals which appear to hold an intermediate Place between those of the Class Pisces and the Class Vermes, and in two Genera of the last mentioned Class. By Sir Everard Home, Bart. V.P.R.S.—17. On the Mode of Generation of the Lamprey and Myxine. By Sir Everard Home, Bart. V.P.R.S.—18. On the Multiplication of Images, and the Colours which accompany them in some Specimens of calcareous Spar. By David Brewster, LL.D. F.R.S. Lond. and Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. G.C.B. P.R.S.—19. A Series of Observations of the Satellites of the Georgian Planet, including a Passage through the Node of their Orbits; with an introductory Account of the telescopic Apparatus that has been used on this Occasion; and a final Exposition of some calculated Particulars deduced from the Observations. By William Herschel, LL.D. F.R.S.—20. An Account of some Experiments with a large Voltaic Battery. By J. G. Children, Esq. F.R.S.—21. On the dispersive Power of the Atmosphere, and its Effect on astronomical Observations. By Stephen Lee, Clerk and Librarian to the Royal Society. Communicated by W. H. Wollaston, M.D. Sec. R.S. —22. Determination of the North Polar Distances and proper Motion of Thirty fixed Stars. By John Pond, Esq. Astronomer Royal, F.R.S.—23. An Essay towards the Calculus of Functions. By C. Babbage, Esq. Communicated by W. H. Wollaston, M.D. Sec. R.S.—Some additional Experiments and Observations on the Relation which subsists between the nervous and sanguiferous Systems. By A. P. Wilson Philip, Physician in Worcester. Communicated by T. Andrew Knight, Esq. F.R.S.

An Account of the most important recent Discoveries and Improvements in Chemistry and Mineralogy to the present Time; being an Appendix to their Dictionary of Chemistry and Mineralogy. By A. and C. R. Aikin. 4to.

The title of this work sufficiently explains its object; and it is but justice to say that the authors have not merely "endeavoured to give," but have succeeded in exhibiting "a perspicuous and sufficiently detailed account of the various and highly important discoveries by which the science has been enriched during the few years that have elapsed since the publication of their former volumes."

As

As a specimen of the style of the work, and the happy manner in which the new facts are brought together and classed, so as to lay before the reader, in a condensed yet perspicuous detail, their application by their respective authors, and others who have illustrated them, we select a part of the article "*Affinity*."

After noticing Berthollet's now exploded opinion, that it is the tendency of Chemical Affinity to combine bodies in all proportions; and also the opinion of Richter, Proust, and other eminent chemists, supported by a daily increasing body of experiments, that substances unite in proportions which are rendered definite by the sole operation of their mutual affinity, they notice Mr. Dalton's most important rule on this subject, that "where two bodies combine in different proportions, if the quantity of one of them be assumed as a fixed number, the proportions of the other body that unite to it are in the simplest possible ratio to each other, being produced by multiplying the lowest proportion by a simple integral number, as 2, 3, 4, &c."—and then give the following illustration and application of the doctrine and the rule:—

"Thus, for example, if a metal can combine chemically with different proportions of oxygen, if 100 of the metal take 9 of oxygen for the lowest degree of oxygenation, all the other degrees will be in the proportion of 100 of metal to twice 9 (18) of oxygen; or 100 of metal to three times 9 (27) of oxygen; or 100 of metal to four times 9 (36) of oxygen, &c. &c. A reason for this simplicity in the ratio of binary compounds may be found in the general principle assumed by Mr. Dalton, which is, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or if either is in excess it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms.

"Hence, from the relative weights of the constituent parts of a compound, Mr. Dalton infers the relative weights of the ultimate particle or *atom* of each of these parts; and, this being found, the number of atoms of each constituent which enters into the formation of the compound particle is also deduced.

"Thus (taking a compound of two constituent parts A and B, as the simplest case) if its elements are found by experiment to unite in the proportion of 5 of A to 7 of B, it is inferred by Mr. Dalton that the numbers 5 and 7 express the comparative weight of an atom of A and B respectively. And these elements, though uniting in several proportions, will yet be found by experiment to be confined to either 5 A to 14, 21, 28, &c. of B, which is, one atom of A to 2, 3, 4, &c. atoms of B; or conversely it will be 7 B to 10, 15, 20, &c. of A, which is one atom of B to 2, 3, 4, &c. atoms of A. It is essential to the consistency of this system, therefore,

therefore, that there should be no other proportions of combination between these two elements, unless indeed it be one that is expressed by an even sub-division of one of these proportions, as for example, 5 A to 7, 10½, 14, &c. of B; in which case the 10½ being resolvable into three portions of 3½ each, the number expressing the relative weight of an atom of B must be reduced to 3½ instead of 7, and consequently the several proportions of 7, 10½, 14, and 21 of B will be resolved respectively into 2, 3, 4, and 6 atoms of B.

"To verify the numbers expressing the relative weights of an atom of A and B, (supposing that of A to be assumed as 5, and that of B as 7,) let them each be examined in their separate compounds with a third body C. Then, suppose that in the simplest binary compound of A and C, analysis discovers 3 parts by weight of C, to 5 of A, it is assumed, that, as 5 is taken as the numerical expression of an atom of A, the number expressing an atom of C must be 3; and *consequently*, if this mode of estimation be just, it will also be found by experiment that in the simplest combination of C with B, 7 parts of B will unite exactly with 3 parts of C. This supposes indeed that this simple combination of one atom of each body is known by experiment; but even if this should not be the case, the general principle will not be contravened, if, instead of a single portion of C being found, there should be a double, triple, or quadruple portion, provided the radical number or common divisor is 3, that of B being 7.

"To illustrate this by an example from Mr. Dalton (in which however the numbers assumed are not perfectly accurate though sufficient for the present purpose). The substance of which as far as we yet know the smallest relative weight enters into chemical combination is hydrogen, and on this account the weight of its atom is assumed as unity, and is the standard of comparison for the relative weight of the atom of all other bodies. The only compound of hydrogen and oxygen that we know is water, in which the oxygen is to the hydrogen as 7 to 1. The number 7 therefore is assumed as the relative weight of the atom of oxygen, and water is a binary compound containing an atom of hydrogen with an atom of oxygen in every atom of water. Sulphuretted hydrogen is composed, according to Mr. Dalton, of 13 parts by weight of sulphur, and 1 of hydrogen. If it be assumed that an atom of sulphuretted hydrogen contains an atom of sulphur united to an atom of hydrogen, the relative weight of an atom of sulphur must be 13. To prove that this number 13 may be assumed as the weight of an atom of sulphur, let it be examined in its compounds with oxygen, and, if correct, all the compounds of these two elements will contain for every 13 parts by weight of sulphur, either 7, or 14, or 21, &c. parts of oxygen,

gen, according as the compound contains to every atom of sulphur one, or two, or three atoms of oxygen.

"Now, according to Mr. Dalton, *Sulphureous* acid actually contains 13 parts by weight of sulphur to 14 of oxygen, and therefore its atom consists of one atom of sulphur to two atoms of oxygen; and *Sulphuric* acid consists of 13 parts by weight of sulphur to 21 of oxygen, or one atom of the former to three atoms of the latter.

"This hypothesis therefore is perfectly consistent in the above examples.

"The comparative *weight* of each atom being thus ascertained, the relative *diameter* of the atom is found by comparing its relative weight with the specific gravity of the substance of which the atom is an integrant particle. But as this part of Mr. Dalton's system is not essential to our present purpose, we shall not pursue it.

"Not only do elementary atoms unite in definite proportions, but also compound particles unite in the same manner. Thus both sulphuric acid and potash are compound particles, being each oxyds, the one of sulphur and the other of potassium. But as potash combines with two different proportions of sulphuric acid, one being just double the quantity of the other, the compound, *sulphat of potash*, may with propriety be considered as composed of an *atom* of potash with an *atom* of sulphuric acid, and the compound, *super-sulphat of potash*, may be considered as consisting of one atom of potash to two atoms of sulphuric acid.

"Mr. Dalton gives the term *Binary* atom to any compound of two elements in which one atom of each is combined, and hence there can be but one species of binary compound of any two elements.

"A *Ternary Atom* is composed of two atoms of one of the elements with one atom of the other, and hence there may be two species of ternary atoms of the same element, according as one or other is in the greater proportion. Thus an atom of nitrous oxyd is a ternary compound of two atoms of azote and one atom of oxygen; and on the other hand, nitrous acid is also a ternary compound of the same elements, but consisting of one atom of azote with two atoms of oxygen.

"A *Quaternary Atom* is composed of three atoms of one element with one atom of the other, and hence also there may be two species of quaternary atoms as either element predominates; and so on of the other numbers.

"We shall now proceed to mention a number of facts that illustrate in a striking manner the chemical union of substances in *definite proportions*.

"If

"If one measure of pure oxygen and two measures of hydrogen be mixed in a jar over mercury, and ignited by the electric spark, both the gasses will disappear, and water will be produced. If two measures of each gas be used, water will be produced as before, but one measure of oxygen will remain. Hydrogen therefore unites with water in one exact proportion, and in no other.

"If a piece of well burnt charcoal be confined in oxygen gas, and inflamed by a burning glass, the volume of gas is not altered when again cooled, but the whole is converted into carbonic acid gas. If more oxygen be present than is necessary for the consumption of the charcoal, the products will be carbonic acid gas and an excess of oxygen; if there is less oxygen than will consume the charcoal, carbonic acid alone will be produced, and part of the charcoal will remain unconsumed.

"The combination of two elements in several definite proportions is very happily shown by the various compounds of oxygen and azote. These are nitrous oxyd, nitrous gas, and nitrous acid gas.

"If two measures of *Nitrous Oxyd* and two measures of hydrogen are ignited by the electric spark, the product is water, and two measures of azote remain. Now, as water is produced by two measures of hydrogen and one of oxygen, the nitrous oxyd here employed must have consisted of two measures of azote with one of oxygen condensed into the space of one measure.

"If charcoal is ignited in two measures of *Nitrous gas*, the products are one measure of carbonic acid gas and one measure of azote. Hence, as carbonic acid gas always occupies the same volume as the oxygen of which it is formed, nitrous gas consists of equal volumes of oxygen and of azote not condensed by their union.

"If two measures of nitrous gas be mixed over water with one measure of oxygen gas, both of them totally disappear, and a solution of *Nitrous acid gas* in water is the result.

"In all the above examples the proportions of azote and oxygen increase or diminish by equal quantities, and no intermediate states of combination are known.

"Dr. Wollaston has given some striking experiments in illustration of this theory of definite proportions in the composition of some super-acid and sub-acid salts*, which may be here mentioned, as they are easily performed. Let two grains of crystallized carbonat of potash, recently prepared, be wrapped in thin paper, and passed up into an inverted tube filled with mercury, and let the gas be extricated from it by muriatic acid, and the space it occupies be marked on the tube. Then let four grains

* Phil. Trans. for 1808.

of

of the same carbonat be exposed for a short time to a red heat, and afterwards let the gas be expelled from it in the same apparatus, and it will be found to occupy exactly the same space as that obtained from the two grains of crystallized salt in the former experiment.

"The same results are obtained from the super-carbonat and the sub-carbonat of soda.

"Super-sulphat of potash, in like manner, contains twice the quantity of acid as the neutral sulphat. Let twenty grains of carbonat of potash be mixed with about twenty-five grains of sulphuric acid in a covered platina crucible, or in a glass tube, and this mixture gradually heated till it ceases to boil, and becomes slightly red-hot. This will produce the super-sulphat of potash, which will be very nearly neutralized by an addition of 20 grains of the same carbonat of potash.

"The common super-oxalat of potash is shown by Dr. Thomson to consist of potash united to twice the quantity of oxalic acid necessary to saturate it. If two equal portions of this super-oxalat be taken, and one portion calcined, so as to destroy the acid, the alkali that remains will be just sufficient to saturate the other portion.

"When nitric or muriatic acid is added to the common super-oxalat of potash, the latter salt is only partially decomposed, and crystals form in the mixture, which are found on examination to be a *Quadroxalat of Potash,* or potash with *four* times as much acid as will saturate it.

"The formation of these various salts with a definite excess of acid, which is expressed by a simple multiple of the least definite quantity, is particularly important, as it affords an answer to the powerful objection urged by M. Berthollet against the common opinion of chemical affinity. This eminent chemist shows that a considerable excess of a weaker acid will decompose a compound of a base and a stronger acid; for example, that a large quantity of nitric acid added to sulphat of potash will occasion some crystals of nitre to form, though the nitric acid has a weaker affinity to potash than the sulphuric. But though there is an undoubted decomposition of some portion of the sulphat of potash in this case, it is highly probable that for every particle of nitre formed there is an equivalent quantity of a super-sulphat of potash produced, in equally definite proportions with the neutral sulphat, and in which, probably, the acid is in the ratio of a simple multiple of that which exists in the neutral salt.

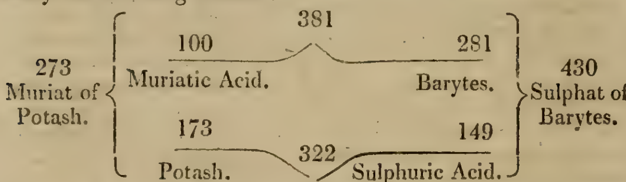
"Among the philosophers who within these last few years have laboured with the greatest zeal and success in discovering the laws of chemical affinity, a distinguished place is due to Professor

fessor Berzelius*, whose experiments we shall now briefly notice. He states that he was led to this train of inquiry from the two following most important theorems laid down by the learned Richter:

"1. When two bodies, A and B, have each an affinity for two others, C and D, the C which saturates a given quantity of A is to the D which saturates the same quantity of A, in the same proportion as the C saturating any given quantity of B is to the D saturating the same quantity of B. Hence the saturating proportions of A C, A D, and B C being known, those of B D may be found by simple calculation. For example, let A, B, C, and D be respectively sulphuric acid, muriatic acid, barytes, and potash, and let 100 parts of sulphuric acid be saturated by 190 of barytes, and by 116 of potash, and let 100 of muriatic acid be saturated by 281 of barytes, then the quantity of potash saturating 100 of muriatic acid will be 173;

	Bar.		Pot.		Bar.		Pot.
for	190	:	116	::	281	:	173 nearly.

This law of chemical combination is indeed a direct inference from the fact, that, when two neutral salts are mixed together, and decompose each other, the mixture remains neutral. Thus, supposing 381 parts of muriat of barytes be exactly decomposed by 322 parts of sulphat of potash, and the mixture remain neutral, the respective proportions of the new compounds formed will be 273 of muriat of potash, and 430 of sulphat of barytes, as by the following scheme:



"The *respective* proportions of potash and barytes that saturate any acid are always as 173 : 281, and in the decomposition here assumed the muriatic acid that saturates these bases is 100 parts. Therefore the sulphuric acid must be 149 parts, as calculated both from the elements of sulphat of potash and sulphat of barytes. In the former case 116 of potash saturate 100 parts of sulphuric acid, and 116 : 100 :: 173 : 149. In the latter case 190 of barytes saturate 100 of sulphuric acid, and 190 : 100 :: 281 : 149.

"The importance of this law of chemical combination is so great that the composition of all the binary compounds might be

* An. Chim. tom. 77 to 83 inclusive.

found

found with absolute certainty, provided the data founded on actual experiment could be brought to perfect accuracy. Hitherto however this has not been the case, as no series of numbers representing the neutral compounds has yet been given which will apply throughout. It is a chief part of the present labours of Prof. Berzelius to correct the elements of these calculations by varied experiments conducted with great care and intelligence.

"Another equally important law laid down by Richter is, that when the metal of a neutral metallic solution is precipitated by another metal, it is the metallic base alone which is changed, the oxygen and the acid remaining united with the last added metal. Hence it follows that all the different metallic oxyds which saturate a definite portion of acid contain the same quantity of oxygen. Or in other words it may be expressed, that a given quantity of any acid combines to saturation with only a definite proportion of oxygen united to so much of any base as contains this portion of oxygen. Thus for example, if 100 parts of sulphuric acid saturate 20 parts of oxygen and the base united with it, the proportion of every oxyd that combines with 100 of sulphuric acid, and the composition of every neutral sulphat is known as soon as the oxyd itself is analyzed. And, conversely, the composition of any unknown oxyd may be discovered by finding the quantity of this oxyd which neutralizes a given weight of any acid whose saturating quantity of oxygen in any other oxyd is previously known. Thus, for example, if 100 parts of sulphuric acid saturate any oxyd that contains 20 of oxygen, it may be inferred that 116 parts of potash are composed of 20 of oxygen and 96 of potassium, since this quantity of potash saturates 100 of sulphuric acid. The like quantity of oxygen is therefore contained in 78 of soda, in 190 of barytes, &c. &c. each of which saturate 100 of sulphuric acid.

"The late discovery of the compound nature of the alkalies and earths, therefore, gives an additional importance to this law of chemical affinity.

"Another law of chemical combination, which is laid down by Berzelius, and illustrated by numerous examples, is the following: viz. In any compound of two oxydated substances, that substance which is attracted to the positive pole of the electric circuit (the acid, for example,) contains as much oxygen as is produced by multiplying the oxygen of the substance attracted to the negative pole (such as alkali, earth, metallic oxyd,) by some of the integral numbers 2, 3, 4, 5, &c. For example, as 100 parts of sulphuric acid saturate as much of any oxyd as contains about 20 parts of oxygen, the quantity of oxygen in 100 parts of the acid itself must be equal to twice 20, or three times 20, &c. In this example it will be shown presently that

that sulphuric acid contains almost exactly three times 20, or 60 per cent. of oxygen. Most of the other acids, however, contain only twice the oxygen of their saturating base: the carbonic and sulphureous acids are of this kind.

"In all the compounds in which water forms an element (which are all the crystallized salts and liquid acids) this substance also seems to be subjected to some similar law of definite proportion, as will be soon explained.

"We shall now give a short abstract of a few of the important series of Prof. Berzelius' experiments, to show to what degree they illustrate and confirm the above laws of chemical combination, together with some others which will be stated in their place.

"The composition of sulphuric acid which has so often been attempted was ascertained in several methods; and first through the medium of the oxyd and sulphuret of lead.

"*Lead and Oxygen.* Lead has three degrees of oxygenation, viz. the yellow, the red, and the brown.

"For the yellow oxyd, some pure lead (reduced from the nitrat of lead) was dissolved in nitric acid evaporated and ignited; a hundred parts of metal thus gained 7.8 of oxygen.

"The red oxyd, or purified minium, contains to 100 of metal 11.07 oxygen.

"The brown oxyd formed by digesting minium in nitric acid, contains to 100 of metal 15.6 of oxygen.

"Therefore these portions of oxygen, viz. 7.8; 11.07; and 15.6, are respectively in the proportions of 1, $1\frac{1}{2}$, and 2.

"The yellow oxyd is the only one which enters into the salts of lead.

"*Lead and Sulphur.* 100 parts of lead mixed with as much pure sulphur, and heated in a close vessel, as long as any sulphur was sublimed, produced 115.6 of the sulphuret; so that 100 parts of lead, when thus united with sulphur, absorb 15.6, which is exactly twice the weight of oxygen united with the same quantity of lead in the yellow oxyd. On this coincidence a law of combination is deduced, which will be presently mentioned.

"This sulphuret of lead is therefore thus composed:

Lead......	100	86.51
Sulphur	15.6	13.49
	115.6	100.00

"A hundred parts of the sulphuret of lead last described were digested in nitro-muriatic acid till the whole was converted into sulphat of lead, the sulphur and lead both acquiring oxygen from the nitro-muriatic acid. No product whatever was yielded from the

the sulphuret but the sulphat of lead, which was perfectly neutral, and weighed 126.5 parts. The respective changes that took place therefore, supposing the base of this salt to be the yellow oxyd, are as follows:

			Oxygen.		
Lead . . .	86.51	with	6.748	produce 93.258	oxyd of lead
Sulphur	13.49	with	19.752	produce 33.242	sulphuric acid
Sulphuret of lead	100.00		26.500	126.5	sulphat of lead.

"Two things are to be particularly noticed here:

"One of them is: that the sulphur of the sulphuret of lead was exactly sufficient, when converted to sulphuric acid, to saturate the lead of the same sulphuret when converted to the suboxyd of lead.

"The other thing to be noticed is: that the quantity of the sulphur in the sulphuret was almost exactly double the quantity of oxygen taken up by the lead of the sulphuret, being as 13.49 to 6.748.

"From the former of these facts the author infers as a general rule, that a metal combines with sulphur in such a proportion, that when the sulphur is converted to sulphuric acid, and the metal to an oxyd, the sole product will be a neutral sulphated oxyd of the same metal.

"From the latter of these two facts the author infers, that in every neutral sulphat the oxygen of the base equals half the weight of the sulphur of the acid with which it is saturated.

"The composition of sulphuric acid is inferred from the above oxydation of sulphuret of lead, to be in the proportion of 13.49 of sulphur to 19.752 of oxygen, the whole addition of oxygen to the sulphuret being 26.5, and 6.748 of this being estimated as the portion belonging to the oxyd of lead.

"Sulphuric acid therefore consists of

Sulphur	40.58	—	100.000
Oxygen	59.42	—	146.426
	100.00		246.426

"According to the second of the two propositions just mentioned, 100 parts of sulphuric acid therefore will saturate so much of any oxydated base as contains $\frac{40.58}{2} = 20{\cdot}29$ of oxygen.

"As an exact determination of the elements of sulphuric acid is of great importance in analysis, the author proceeds to compare the above-mentioned result with those produced by other modes of operating.

"Both Bucholz and Klaproth have sought to fix the elements of of this acid by acidifying a given quantity of sulphur, and then combining it with the barytes of a known quantity of some soluble barytic salt. The analysis of sulphat of barytes depends also on that of carbonat of barytes, out of which all the barytic salts are prepared. By multiplied experiments the author fixes the elements of carbonat of barytes at

Carbonic acid	21·6 —	100
Barytes	78·4 —	363
	100·0	463

"Of this carbonat, 100 parts (containing 78·4 of base) gave from 118·6 to 119 parts of sulphat of barytes, which will give for the elements of sulphat of barytes within $\frac{5}{10000}$ of the following numbers:

Sulphuric acid	34 —	100
Barytes	66 —	194
	100	294

"Bucholz acidified 100 parts of fused sulphur with nitro-muriatic acid, and by combining with barytes the sulphuric acid thus produced obtained 724 parts of sulphat of barytes. If this sulphat is estimated according to Berzelius's result above given, its acid part will be 246·16, whence 100 parts of sulphur will have combined with 146·16 of oxygen, and this estimation of sulphuric acid will be found to correspond almost exactly with that already given as found by the acidification of sulphuret of lead.

"*Sulphureous Acid.* The production of this acid by the direct combustion of sulphur being attended with nearly insuperable difficulties, Pr. Berzelius attempted its analysis by means of the sulphite of ammonia. This salt was decomposed by muriat of barytes, and the resulting sulphite of barytes was changed to the sulphat of barytes, by means of nitric acid, none of the sulphureous acid being expelled in the process. By these and other experiments the author determines the elements of sulphureous acid to be

Sulphur	50·57 —	100·00
Oxygen	49·43 —	97·83
	100·00	197·83

"The oxygen in the sulphureous acid is therefore to that in the sulphuric acid very nearly as $1 : 1\frac{1}{2}$; as 100 parts of sulphur unite with 97·83 of oxygen in the sulphureous, and with 146·426 in the sulphuric, and $79{\cdot}83 \times 1\frac{1}{2} = 146{\cdot}785$.

"*Copper with Sulphur and Oxygen.* The author proceeds to examine the compounds of copper with sulphur and oxygen, to ascertain whether they agree with those of lead in the three laws of of combination which he had laid down. These laws (to repeat them) are the following:

"1. That the sulphuret of any metal, when oxydated, is totally changed to a neutral sulphat.

"2. That in every sulphat the oxygen belonging to the base equals half the weight of the sulphur in the acid.

"3. That in every compound of an acid and a base, the oxygen of the acid equals that of the base multiplied by some integral number. To this may be added the other law of all chemical combination (which is entirely adopted by the author) namely,

"4. That where two bodies unite in different proportions, if the quantity of one of them be assumed as a fixed number, the proportions of the other body are in the simplest ratio to each other, being produced by multiplying the lowest proportion by some integral number."

[To be continued.]

LXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 9. This Society again assembled after the long vacation; the Right Hon. Sir Joseph Banks, Bart. President, in the chair.

A very important paper by Sir Humphry Davy was read, On the Nature of the Fire-damp in Coal-mines, and the Means of preventing its Explosion.

In this paper Sir H. Davy describes a very simple lantern, which he calls the Safe Lantern, for preserving the lives of colliers in mines liable to explosions from the fire-damp.

By various chemical experiments Sir H. Davy ascertained that the fire-damp is the least combustible, and least expansive in combustion, of all the inflammable gases; and that explosive mixtures of fire-damp have their explosive power destroyed by small admixtures of azote and carbonic acid: and likewise that explosions from the fire-damp are incapable of passing through small glass or metallic tubes.

On these principles his lantern is founded, which is an air-tight apparatus having small air tubes below, through which an explosion cannot pass; and a chimney above, in which the admixture of azote and carbonic acid always prevents the possibility of explosion.

The candle or wick in burning forms the quantity of azote and carbonic acid necessary to lower explosive mixtures, should they

 exist

exist in the atmosphere, below the explosive point: and when the atmosphere becomes dangerous the flame enlarges, and is then extinguished, and the miners are warned that it is proper to leave this part of the mine till it is properly ventilated.

With a common candle, when the air becomes explosive, life is extinguished; with the Safe Lantern, the only inconvenience is a temporary loss of light.

It appears that Sir Humphry found the utmost facility in pursuing his researches, when travelling in the coal-mining districts, and that all persons seemed equally eager and confident that he should discover some means of alleviating, if not entirely preventing, the dreadful calamities which so often occur in coal-mines. This fact is perfectly sufficient to refute the silly calumnies which have been so liberally heaped on the heads and hearts of a number of the most respectable and humane characters in the kingdom. It requires neither talents nor knowledge to charge any man or class of men with inhumanity; but it is an act which no truly humane or benevolent man can do without the plainest, most unequivocal and often repeated evidence. It is now, indeed, sufficiently evident that they must be very bad observers of natural phænomena, who could so grossly confound the rational dictates of prudence arising from long experience and baffled philanthropy, with apathy and vulgar inhumanity! We cannot be surprised that such persons have not discovered an adequate mode of modulating or extinguishing the fire-damp of coal-mines; neither can we be at a loss to account for their accusations. Let us hope, however, that as this last discovery of Sir Humphry must be eminently serviceable to humanity, it may also contribute to teach sciolists, in future, the necessity of observing and thinking before writing and accusing.

Nov. 16. A Supplement to Sir Humphry Davy's paper was read. It contained some additional evidence of the perfect adequacy of his "Safe Lantern" to preserve the miners from the dangers of explosion.

Sir H. began his researches on the fire-damp in a manner very different from that of preceding chemists: his first inquiry was, to ascertain what is the peculiar composition of this gas? what are its explosive powers? and in what combinations it is most dangerous to miners, or with what admixtures it is rendered inert? He found that it is tolerably pure carburetted hydrogen gas; that it contains neither olefiant nor sulphuretted hydrogen gas, like the common product of coals by distillation. He then proceeded to ascertain its combustible powers: burning charcoal does not inflame it, if there be no blaze, and iron must be at a white heat to fire it. One part oxygen to one of fire-damp produced a mixture slightly combustible; but the most combustible

combustible mixture consisted of six or seven parts oxygen to one of fire-damp. With mixtures of atmospheric air it is also combustible; and it appears that even one part fire-damp to 30 parts of the atmosphere increased the flame of a candle, and gave it a blueish tinge; but that with one part fire-damp to 15 parts atmospheric air it is perfectly harmless. It required two parts oxygen to one of fire-damp to produce one part of carbonic acid. Lastly, one part of azote or of carbonic acid renders it wholly incombustible; and as the chimney of Sir Humphry's "Safe Lantern" must necessarily be always supplied with this gaseous matter, it is scarcely possible to conceive any thing more effectual, or more likely to answer all the expectations of the most sanguine philanthropist. It seems, reasoning theoretically, to combine in an unprecedented degree all the requisites of simplicity, portability, œconomy and efficacy, which are necessary to the common purpose of manual labour. We cannot therefore doubt that it may be the means of preserving many useful lives, and that it will prove to the coal-miners a "perfectly safe lantern."

The reading of a paper by F. Daniel, Esq. F.R.S. was begun, and continued on

Nov. 23. It was An experimental Inquiry into the Process of Crystallization, and the Effects of Solution on the Structure of Crystals. The author's object was to verify, if possible, the hypothesis of the sphericity of molecules. He began by making solutions of alum, nitrates of copper and of potash, &c. These solutions took two or three weeks to yield regular crystals; they began by depositing a sediment which gradually assumed octohedral, rhomboidal, &c. forms. Experiments of this nature he varied with great industry and ingenuity, and detailed minutely the results. His next object was to observe the changes which took place in crystals during solution. He weighed certain crystals, immersed them in a given portion of water, left them to remain in the fluid so many hours, then withdrew them, wiped them dry with blotting paper, ascertained what weight they had lost, and what were the modifications produced on their crystalline structure: he again immersed them in water for a limited time, and repeated his observations. This process he continued some days, always carefully noting the changes evinced on the exterior form of the crystals, the modulations or changes of their angles, the operations of the solvent on their faces, sides or summits, the parts which disappeared entirely or became truncated, &c. These results were illustrated by figures, without which it would not have been possible to render them intelligible. A considerable diversity occurred in the changes which the different crystals experienced; but all were more or less truncated, at

 least

least one side or other. Mr. D. then entered into a somewhat elaborate inquiry into the differences which should have appeared had the primitive molecules of crystals been cubes and not spheres; and some of his facts he admitted were favourable to the former hypothesis, but more to the latter. In observing that the crystalline masses formed at the bottom of the vessels by spontaneous evaporation often presented very different geometric figures, and totally dissimilar to the forms usually ascribed to such salts by mineralogists, he was induced to consider the effects of mechanical agency in the product of such anomalies. Notwithstanding these and some other discrepancies, he is however inclined to consider Dr. Wollaston's theory of molecular sphericity as perfectly satisfactory, and the best adapted to explain the phænomena of nature. Mr. D. considers it an additional proof of the infinite wisdom of the great Author of nature, that the molecules of our globe should have the same form as those almost incomprehensibly great orbs which appear in the heavens.

LXXII. *Intelligence and Miscellaneous Articles.*

CRYSTALLIZATION.

Dr. J. R. Coxe has made some experiments on the crystallization of salts, which in some respects contradict the generally-received opinions respecting that process. He found, contrary to the notion of air being necessary to the consolidation of crystals, that solutions of nitrate of potash, muriate of ammonia, and Glauber's salt, put into a phial at the boiling temperature, and either corked or closely covered with a piece of bladder, frequently deposited regular crystals as the mass cooled without any exposure to the atmospheric pressure. On the other hand, saturated solutions of these salts remained uncorked and exposed to the atmospheric pressure during three days without any consolidation. These experiments have been varied and repeated in different sized vessels, and always with the same anomalous results. Nor did any material difference appear when the water was fully saturated, or only charged with the salts. Solutions of these salts even on shaking sometimes did not immediately crystallize, while at others they yielded crystals without any apparent cause. Sometimes he had regular transparent crystals two inches long; in other instances, only irregular masses at the bottom of the vessel. All the rapidly-formed crystals, as might be expected, were spongy, soft, striated, and had a satin-like aspect; and the hasty crystallization always began at the surface. While in some cases regular crystals were formed at the bottom of both corked

corked and uncorked phials, afterwards a solid crust has formed in the upper part of the phials, without any obvious cause. Dr. C. considers it the most singular and inexplicable fact, that when regular, firm, transparent crystals are deposited (under all the above circumstances), as soon as the remaining saturated solution above them consolidates, the first formed crystals almost instantly lose their transparency, and become of a porcelain white colour. He ascribes this change to the abstraction of water of crystallization, as he found that the whitish mass, when dissolved in water and again crystallized, afforded a quantity of transparent crystals *heavier* than the original white ones. In this and in all the other experiments Dr. Coxe has taken no notice of the action of light, nor does he appear to be aware that light has a potent influence on crystallization. Dr. Brewster's ingenious and successful experiments on the effects of saline solutions on rays of light, prove the necessity of attending to their influence in all experiments of this kind. Dr. C. has tried similar experiments with solutions of alum, sulphates of magnesia, iron, copper, and zinc, muriate of lime and subcarbonate of soda, without any very notable dissimilarity. But so uncertain and irregular were the results, that in no case could he anticipate them. He thinks, indeed, that his experiments fully disprove the theory of latent heat, which has itself become latent long ago, as water saturated at 212° remained fluid and transparent even at the freezing point, although the theory ascribes its solvent power to the temperature.

A QUESTION RESPECTING THE COMPARATIVE STRENGTH OF OAK AND FIR.

To Mr. Tilloch.

Sir,—In most of the authors who have written on the strength of materials, we meet with the following assertion, viz. "Oak will *suspend* much more than fir, but fir will *support* twice as much as oak." As it is of some importance to the practical mechanic to know how far this statement may be depended upon, I would thank any of your correspondents to inform me, if the above assertion rests on any better authority than that of Muschenbröck's experiments with pieces four feet long and 7-10ths of an inch square? For it would be easy to show that the repulsive force of oak, or of any other material, could not be ascertained by these experiments.

I have tried several experiments on both kinds of timber, and find that oak will both *suspend* and *support* more than fir, the specimens being equally good of their kinds.

In making the new experiments, at Woolwich, an attempt

 might

might be made to discover the laws of penetration. Blocks might be penetrated with balls of different dimensions, and in different directions of the fibres: the blocks being split or cut open, the nature of the fracture might be examined. One of the principal points wanting to form a theory, is, to know whether there is a cylinder of wood, whose base is equal to the area of the middle section of the ball, driven before the ball, or only part of the area, and the rest turned to the sides and crushed into the surrounding wood as the ball passes.

I am, sir,

Your most obedient servant,

Bentinck-street, Nov. 6. T. T.

ANSWER TO A NOTE IN OUR LAST.

DEAR SIR,—In answer to the question in your last number (p. 249), Whether the flints held in my hands when floating on my back were immersed in the sea or not? I have to observe that *they were under the surface.* I have since weighed them in water, and found they lost 2 lbs. 5 oz. avoirdupois; consequently the actual weight sustained was only 3 lbs. 11 oz. I am, however, of opinion I could have carried much more: and it is my intention, should I live to the autumn of next year, to repeat the experiment *with my clothes on,* and make use of lead weights. I am, &c.

Surry Institution, Nov. 11, 1815. KNIGHT SPENCER.

MILK A REMEDY FOR THE POISON OF FALSE MUSHROOMS.

On the 13th of October last, a family of six persons residing in the commune of Samans, in the department of the High Garonne, were poisoned by partaking plentifully, by mistake, of a poisonous species of mushrooms. They were all speedily seized with drowsiness, and remained several hours without any signs of life. At day-break they awoke, but in a state of perfect imbecility: the pulse was very low, and betokened speedy dissolution. The surgeon first called wished them to swallow oil, but none was to be procured. An ecclesiastic who was called in, made them drink copiously of milk. Long and violent vomitings came on, and the remedy succeeded perfectly; for a few hours after the whole family were declared free from danger.

It may not be improper on this occasion to remind our readers, that the juice of lemons, and other vegetable acids, has been found to be a remedy for the effects of opium; and to suggest that, possibly, in some cases of poison by mushrooms, they might be used with advantage.

DEATH

DEATH OF THE CELEBRATED ORIENTIAL TRAVELLER DR. SEETZEN.

The readers of The Philosophical Magazine have from time to time been made acquainted with the progress of this most indefatigable and truly enlightened traveller. We lament to state, that like our estimable countrymen Major Houghton and Mungo Park, the earthly career of Dr. Seetzen has been prematurely terminated; and, alas! not without ground for the strongest suspicion that he has fallen a victim to the barbarous policy or cupidity of an Arab chief. We select from a Bombay paper of May 25, the following elegant tribute to his memory, and succinct detail of his short but useful public life.

"Among the few travellers whose thirst of knowledge and intrepidity of mind have directed their views towards exploring the interior regions of Africa, and whose labours have so uniformly been interrupted by a lamented and untimely fate,—Dr. Seetzen, a German gentleman of considerable talent, and distinguished qualifications for the arduous task of discovery, seems to have been less known to the world than most of his predecessors in that path, although his want of notoriety arose rather from a retiring modesty of character than from any deficiency of claim on the gratitude and admiration of the friends and patrons of civilization and knowledge.—Of the early part of this traveller's history nothing further is known to the writer than that he was a native of Saxony, had passed through a regular course of studies as Doctor of Medicine, and had added to his professional acquirements an intimate knowledge of botany, mineralogy, and natural history in general, besides an acquaintance with oriental languages, and particularly a proficiency in the Arabic.

"So rare an union of science and learning, with a robust constitution, unshaken courage, and enthusiasm in the pursuit of knowledge, had recommended him to the notice of the Duke of Saxe Gotha, by whom he was introduced also to the Emperor of Austria, and the learned Orientalists of Vienna, who were then engaged in their first labours on *Les Mines d'Orient*.

"Under the joint patronage of these distinguished characters, he left Europe about the year 1807, on a tour through Syria and Palestine, chiefly with a view to improve his qualifications for the great work of exploring Africa, by gradually bringing himself to suffer the greatest privations, by familiarising his habits to those of savage life, and by acquiring a facility in the colloquial dialects of Arabic, which would enable him to pass for a native of that country.

"During his stay in Syria, he visited Palmyra, Balbec, Lebanon, Jerusalem, and the intermediate scenes of sacred or profane history;

tory; but his most interesting tour was through the plains of the Hauran, east of the river Jordan, and the Dead Sea; a wide and now desert tract, chiefly peopled by wandering Bedouin tribes, though abounding with ruined cities, and traces of former opulence and cultivation, in canals, bridges, and other public works of utility. As it was a district, too, entirely unknown to Europeans, his researches as a naturalist were abundantly successful; and the scientific world had much to hope from his discoveries even here, particularly in his mineralogical acquisitions, and his examination of the shores and waters of the lake Asphaltes, concerning which so many pious falsehoods had been circulated, to gratify the taste of those who still believed the hissing bubbles of the burning brimstone that swallowed Sodom and Gomorrah to be visible, and who devoutly inferred that no animal could sustain without suffocation the fumes sent up by this sink of patriarchal abomination.

"In Egypt, the labours of Dr. Seetzen were chiefly directed to researches of science, particularly in correcting the errors of Sonnini as a naturalist, and exploring the vast field which that interesting country offers to the geologist and the agricultural observer.

"At Cairo, where he remained some months occupied in the arrangement of his materials, specimens and observations, he was personally known to most of the European inhabitants, who universally speak of him as a man of extensive general information, profound scientific knowledge, extraordinary qualities of disposition, in the union of patience, fortitude enterprise, and gentleness of manners, and possessed of every qualification likely to insure success.

"From Egypt he appears to have sent to Vienna all the valuable specimens which he had been enabled to collect in the different departments of science, as well as copious observations on the sacred and literary antiquities of Palestine, and the manners of the Bedouin tribes who range the extensive plains of the eastern deserts: on both of which subjects his inquiring turn of mind, and invincible emancipation from the shackles of vulgar prejudice, enabled him to exercise a clear unbiassed judgement, and to give the best and most impartial account of them that has ever been written, since the intelligent remarks of the philosophic Volney.

"Horeb and Sinai, as the venerable mount of early miracle, from amid the lightning of whose terrific brow the thunders of the Decalogue were first rolled forth, next attracted Dr. Seetzen's attention; but although he was not successful in discovering the rock from whose cleft the murmuring Israelites were watered, the blackened soil of the burning bush, and the many other scenes

scenes of early wonders shown to travellers by the pious Greeks of Santa Catherina; yet his travels through that route, and their continuation through the unfrequented paths of Arabia Petræa, brought to light a mass of information.

"After his tour through the northern part of Arabia, and from thence into the Hedjaz, Dr. Seetzen reposed a few months at Mecca, where he mingled with the crowd of pilgrims that annually assemble there from every quarter of the eastern world; and by the disguise of a long black beard, an Arab dress, an intimate acquaintance with all the doctrines and ceremonies of Islamism, and a proficient fluency of expression in the various dialects of the Arabic tongue, he acquired the title of Hadjee Moosa, after performing a pilgrim's duties; when he quitted the Holy City, without a suspicion having once been excited of his want of veneration for the Caaba and the Prophet.

"From Mecca, he journeyed through the southern territories of the Hedjaz with a caravan, and reached Saana, the capital of the Yemen, through a route before untravelled by any European; from whence he continued his tour through the most fertile parts of Arabia Felix, and arrived at length at Mocha, bringing with him the spoils which his intrepidity alone had gained him, in botanical, mineralogical, and other specimens of natural history. Like the collection of the industrious Niebuhr, it had scarcely entered the gates of Mocha before it was seized by the rapacious Dola, under the hope of its containing immense treasures; but finding himself miserably disappointed in his estimation of their value, as if to avenge himself for it, by securing at least their loss to the proprietor, he caused them all to be conveyed to the Imaum of Saana, under a pretence of their being intended for the exercise of magic and incantation. They were accordingly confiscated, and for ever lost to the proprietor of them, who seemed only to be stimulated thereby to greater undertakings in the way of arduous research.

"During Dr. Seetzen's stay at Mocha, he invariably bore the character of a Mussulman dervish, under the name of Hadjee Moosa; nor does it appear that he was known to any of the natives even there, as an European, his disguise being (as was before observed) complete; and his prudence was carried so far, that he never visited any of the factories there, although Captain Rudland, who was at that moment the East India Company's Agent in the Gulph, gave him repeated invitations, and paid him every indirect attention which he could be prevailed on to receive; his constant residence being in the common caravanserah of Mohammedan travellers.—It had been the general opinion of the best informed people at Cairo, who at all interested themselves in the question, and consequently of his patrons in Europe, who

who depended solely on their consuls in Egypt for information, that Dr. Seetzen had met his death in Africa, in some wars among the Samaulies in Berbera; but it is confidently asserted at Mocha that he did not once cross over even to the opposite coast. After some stay there, wasted in ineffectual endeavours to obtain a restoration of his seized specimens, he formed the determination of revisiting Saana in person, and of journeying from thence to the eastern extremity of Arabia, to cross from Muscat into Persia, and return again to Syria, from whence he would at length enter on his great undertaking.

"For this purpose he left Mocha in the month of October 1811, having with him a number of camels laden with baggage, provisions, scientific apparatus, &c.; when three days only after his departure information was received of his sudden death near Tais, and the consequent dispersion of all his property. No doubt seems to have been entertained, even by the Arabs themselves who were of his party, that he was poisoned by some agents of the Dola among them, with the connivance, or perhaps at the express orders of the Imaum, to wrench from him the little mite he possessed, and to remove him effectually beyond the power of remonstrance.

"Beyond this, nothing further is positively known as to the detail of his sufferings; but the fact of his having died a sudden and violent death is unfortunately too well established. Two evenings before he quitted Mocha, he passed some few minutes with Mr. Aitkins, the surgeon of the Company's Establishment there, and at the same time confided to the care of Mr. Benzoni, an Italian, in their service also, the whole of his valuable papers and journals, which he had congratulated himself on securing from the grasp of rapacious ignorance, to be forwarded by that gentleman through Egypt to his distinguished patron the Duke of Saxe Gotha in Europe. It fell, however, to the lot of poor Benzoni himself to close a chequered existence on that inhospitable shore: so that the only manner in which he could acquit himself of his trust, was to transfer the charge of his murdered friend's memoranda to the chief of the Banians there, who was then the commercial broker of the East India Company. From him these papers were soon afterwards seized by the Dola, and are now, it is to be feared, irretrievably lost to the friends of science and the patrons of discovery, who would doubtless otherwise, have found among their details, information of the most valuable and interesting nature.—Such has been the melancholy termination of the labours of one of the most enterprising and promising of modern travellers, who, like Houghton, Park, Hornemann, and Roütgen, has fallen a victim to an ardent thirst for information, and sacrificed not only all that could render

der life agreeable, but even that life itself, by a devotion to one of the noblest, if not the most esteemed, of all pursuits, the acquisition and extension of rational knowledge, with a view to the improvement and further civilization of mankind."

Correspondence of M. Van Mons.

[Continued from p. 316.]

We continue with much pleasure the valuable communications of M. Van Mons.

"M. Proust, in a recent letter to M. Delametherie, demonstrates the inexactitude of several other degrees of oxidation given by Thenard. Once for all, therefore, let it be understood, that there are only two degrees, that of oxidule and that of oxide; that the ashes of the metals are combinations, by adherence, when fire is employed. With respect to mercury, which is liquid, trituration effects the same; and with regard to all of them, the acids resolve them into an oxidule or oxide, and into a reduced metal; and finally, that amalgamation is evident in those two bodies, oxidule and oxide. The true oxidule has always the half of the oxygen of the oxide, and the other relations are combinations between those two degrees, of which the former then performs the functions of oxide, and the latter the functions of acid. The oxide may be hyperoxidated, and it is so almost always; and as the hyperoxygen takes much acid from it, it cannot in this state be combined with acids without depositing it; which shows that it is then insoluble in the acids of combustibles, and soluble only in the acids of the burning bodies (*comburans*), under the formation of chlorine, or of iodine: this character is very striking. It is a species of salt formed by oxygen, like that which is formed by water, by the acids, by the oxides, &c.: the oxides unite it also to their own oxidules, and to oxides foreign to them. We cannot analyse these combinations, but by an acid aided by heat, which forms salts of them, partly in oxides and partly in oxidules. When we analyse these compounds by means of reduction, we obtain, naturally, particular appearances of oxygen, which we then take for proper degrees of oxidation; then the hydrates a little intense, the hydrato-oxygenates so common in the oxides of the metals, and the combinations between oxides and oxidules, with excess of the one or the other. In order that two bodies should unite, it is sufficient that one should be more burnt than the other, and that caloric should be displaced. Thus there is a sub-oxidule, or oxidation by adherence. These are the ashes of the metals; next comes oxidule, or first degree of oxidation, and of which some metals only are susceptible; afterwards oxide, or second degree; and the only degree for such as are susceptible of one degree only; and finally hyper-

hyperoxide or combination, from which we may by fire and the oxygenable acids take off the hyperoxygen. All the other degrees are salts; and all the proportions of oxygen, which are not in the ratio of 1 to 2, are so likewise. Every thing which has a crystalline form is a salt; and the red precipitates, such as litharge, which have a lamellous appearance, are already suboxygenates of the oxides of their metals."

[To be continued.]

STEAM ENGINES IN CORNWALL.

By Messrs. Leans' Report of work done in October 1815, it appears that 34 engines consumed 75,009 bushels of coals; that these engines collectively lifted 618,902,532 pounds one foot high by a bushel of coals each; being an average duty of 18,203,016 pounds lifted one foot with every bushel of coals. During the same month, according to their Report, Woolf's engine at Wheal Var consumed 984 bushels of coals, and lifted 41,572,070 pounds one foot high with each bushel; and his engine at Wheal Abraham consumed 1062 bushels, and lifted with each bushel 51,443,639 pounds of water one foot high.

*** The title at the top of the fourth column of the table of work done by the engines in Cornwall, given in page 118 of the present volume, is incorrectly expressed. It should be read thus: "*Pounds of water lifted one foot high, by every number of bushels of coals equal to the number of the engines.*"

CRANIOLOGY.

Mr. Forster has of late been making some observations on the heads of insane persons in some public madhouses, with a view to determine how far the particular organization of the head appears to modify the character of the mania. He has found in the majority of cases a very striking connexion, particularly with regard to the melancholic insane, who have been found to have the parts of the brain which, according to the new system, produce the sentiment of fear, much developed. It is to be hoped that repeated observations of this kind may lead to important discoveries in the history of this interesting, but at present obscure, class of disorders.

Dr. Spurzheim has given an introductory lecture on the new anatomy to a scientific class of above 300 persons at Dublin.

Meteorological Observations made at Cambridge, and other Places, from October 31 *to November* 17, 1815.

Oct. 31.—Fair day, with *cumuli*, &c.; warm for the time of year.

Nov. 1.—Chiefly clouded over; a mild air and hazy distances.

Nov.

Nov. 2.—Fair day, with common ephemeral *cumuli.* Fine starlight night, and rather cooler again. The Barometer 30·10. Thermometer 44° at midnight.

Nov. 3.—Cold northerly wind this morning; elevated masses of heavy *cirrostratus; cumuli* formed under them; there were also fibrous detachments descending: the whole appearance threatened rain about two of the afternoon, but the fair weather returned. Barometer at midnight 30·38. Thermometer 42°.

Nov. 4.—Clear, except features of the lighter modifications.

Nov. 5.—Clear, with light features of *cirrus,* &c. and small *cumuli;* the night clouded, and became warmer.

Nov. 6.—Fine clear white frost, with light features of *cirrus,* &c. In the evening clouded, and small rain.

Nov. 7.—Clouded morning; fine clear day; cold frosty night.

Nov. 8.—Gentle nimbification in the morning; fine afterwards, when I noticed *cirrostratus* of nimbiform kind spread in various stations aloft, and *cumuli* under. In the evening (Thermometer 50°) *cirrocumulus* followed by rain. I heard the hoarse screams of migrating wild geese about 11 P.M.

Nov. 9.—Fair cool day, with much cloud.

Nov. 10.—Cloudy, but without rain.

Nov. 11.—(At Bury St. Edmunds.)—Fine clear warm day, with *cumuli, cirrostratus,* &c. Evening clouded with small rain.

Nov. 12.—(At Norwich.)—Cloudy morning; the afternoon became fine and cooler.

Nov. 13.—Cold wind, with a hard shower of hail and rain about daybreak, afterwards chiefly cloudy and raw.

Nov. 14.—(Ixworth.)—Raw cold day, and strong wind; but the nightwas calm and warmer.

Nov. 15.—White frost followed by showers of snow*.

Nov. 16.—Showers of snow and sleet, with wind.

Nov. 17.—(At Cambridge.)—Clear morning; *cirrostratus* condensing and falling in snow showers. At midnight the Barometer was 30·68, and the Thermometer within doors in a room without a fire 33° of Fahrenheit. The night was clear, and the moon very brilliant.

Corp. Chr. Coll. Cantab. THOMAS FORSTER.
Nov. 18. 1815.

P. S.—As I am collecting matter for a future publication on Meteorology, I shall be obliged to any of your correspondents who can communicate to me, through your Magazine, or by other means, any journals or other meteorological observations kept in the distant parts of the country or abroad.

* They have a proverbial saying in Norfolk, "Three rimes and then a rain," in allusion to white frost; which is often the result of a *cirrostratus.* But the fall often happens before the third rime.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For November 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct.	27	51	51	50	29·52	0	Rain
	28	45	46	47	·60	0	Rain
	29	50	54	44	·93	26	Fair
	30	43	50	46	·94	29	Fair
	31	42	50	44	30·01	34	Fair
Nov.	1	40	46	45	·02	24	Cloudy
	2	45	49	40	·10	29	Fair
	3	34	37	39	·30	36	Fair
	4	33	42	40	.35	20	Foggy
	5	42	48	45	·30	27	Fair
	6	44	51	49	·26	36	Fair
	7	50	52	46	·10	16	Foggy
	8	49	54	52	29·99	10	Foggy
	9	54	55	50	·99	12	Foggy
	10	50	56	49	30·10	16	Cloudy
	11	50	55	48	·08	26	Fair
	12	52	56	50	29·75	27	Fair
	13	52	48	44	·15	16	Stormy
	14	42	48	40	·06	36	Stormy
	15	36	46	37	·08	33	Fair
	16	33	37	28	·40	25	Sleet
	17	28	38	30	·82	27	Fair
	18	27	37	29	30·01	16	Foggy
	19	25	29	29	·10	10	Foggy
	20	35	46	38	29·61	12	Cloudy
	21	37	43	37	·78	14	Cloudy
	22	28	37	34	·86	13	Cloudy
	23	28	39	34	30·15	15	Fair
	24	35	40	36	·36	22	Fair
	25	35	46	35	·53	24	Fair
	26	34	43	35	·63	20	Fair

N.B. The Barometer's height is taken at one o'clock.

LXXIII. *On the Franklinian Theory of the Leyden Jar; with Remarks on Mr.* Donovan's *Experiments. By Mr.* Thomas Howldy.

To Mr. Tilloch.

Sir,—I have sent you for insertion in the next number of your Magazine, a demonstration of some principles of the Franklinian theory of the Leyden jar; with an exposition of the fallacious experiments which Mr. Donovan has opposed to them in his "Reflections on the Inadequacy of the principal Hypotheses to account for the Phænomena of Electricity."

The following extract from the "Reflections" contains an account of two experiments made by Mr. Donovan, the results of which he considers as opposing Franklin's doctrine of accumulation and deficiency. "During the process of charging, both surfaces (of a jar) ought to manifest their characteristic properties in proportion as the charging increases. Whether or not this is the case, will be seen by the following experiments. The apparatus employed was an insulated jar, from the stem of which projected a wire at right angles, and from the outside coating another, two pith-balls being attached to each by means of gilt thread. The jar, perfectly dry, is to be placed on the insulating stand, and a chain is to be connected with the outside, so as to form a communication with the ground. The electric machine being in strong action, is made to pour in a stream of sparks until the jar be fully charged. The chain being removed, the balls connected with the inner surface will be repelled by excited sulphur, and those with the outer surface will be attracted. If the inside be now positive,—why are the balls repelled by sulphur? And if the outer surface be negative,—why are its balls attracted? If the jar be weakly charged, the results are as described by Franklin; and it was no doubt on such that this philosopher made his observations. But were the doctrine of accumulation and deficiency well founded, the results should be the more decided as the jar is more strongly charged. Yet when the hypothesis supposes the jar to contain the greatest quantity of electricity, it is then that it evinces symptoms of containing the least."—Phil. Magagine, vol. xliv. p. 347.

The apparently contradictory results of the above-described experiments very much excited my attention on perusing Mr. Donovan's "Reflections;" and I strongly suspected that he had been deceived by mere appearances.

In consequence of this suspicion, I determined to investigate the

the electrical states of the surfaces of a jar under the conditions mentioned by him as necessary to obtain the specified results; and at the same time, if I found that they occurred, to ascertain the cause or circumstances which produced them. Accordingly, besides a jar with its pith-balls and insulating stand, the following additional apparatus was selected for the purpose. A smooth glass tube, two sticks of sealing-wax, a pair of pith-balls suspended from an insulating stand, a gold-leaf electrometer, and a small Leyden jar.

Before I proceed to describe the experiments, it is proper to give notice, first, that as the investigation occupied the space of at least five hours (all the experiments having been several times repeated), whenever it appeared that the charge of the jar was sensibly diminished, the machine was put in action till the charge was restored to its original intensity;—secondly, that the pith-ball and gold-leaf electrometers were placed in such situations as to be out of the influence of the charged jar;—thirdly, that the balls of the former were kept diverging with *negative*, and the leaves of the latter with *positive* electricity;—and lastly, that one of the sticks of sealing-wax was reserved for examining the balls connected with the inside of the jar (except where it is otherwise mentioned), while the other was exclusively used for electrifying the pith-ball electrometer, and examining the balls connected with the outside of the jar.

Exp. 1. The inner surface of the jar being connected with the prime conductor, and the outer with the ground, a very strong charge was communicated to it, and the connexion with the ground removed. A stick of sealing-wax about an inch in diameter being strongly excited, was brought *suddenly* towards the balls connected with the inner surface, and they were instantly *attracted* by it; but the wax being kept for a moment at the same distance, the attraction ceased, and the balls returned to their original position.

Exp. 2. The wax being excited again as before, was brought slowly towards the pith-balls: the attraction between the wax and them was perceptible, though it was weak and momentary; but when the wax was brought nearer, repulsion took place between it and the balls, which then kept briskly receding from the wax. The wax being now withdrawn, was presented to each of the electrometers in succession; and by these tests the wax was found to be in *a positive state* of electricity.

Exp. 3. The positive electricity on the wax being taken off, it was again excited negatively, and presented to the balls as in Experiment 2, with the same results ensuing. After the balls had been repelled several times by the wax, it was withdrawn and brought *suddenly* towards the balls connected with the outside,

outside, and these were immediately *attracted* by it. On withdrawing the wax, its electrical state was ascertained by presenting it to the electrometers as before, and found to be *positive*.

Exp. 4. The preceding experiment was repeated with this difference only, that after the wax had repelled the balls connected with the inner surface, it was brought, instead of once, two or three times in succession, towards those connected with the outer; and the attraction between the wax and the balls decreased every time the wax was presented to them till it disappeared; and then, on bringing the wax somewhat nearer, repulsion took place between it and the balls. While the wax attracted the balls connected with the outer surface, the electrical state of the wax, on examination, always proved to be *positive*; and when it repelled them, its state was always found to be *negative*.

Exp. 5. The glass tube was next excited and presented to the balls communicating with the inner surface, and they were repelled by the tube; and continued to be so in whatever manner it was presented to them, and at every distance at which it could influence them.

Exp. 6. The second stick of sealing-wax being strongly excited, was brought towards the balls connected with the outer surface of the jar, and they were vigorously *repelled* by the wax; and continued to be so in whatever manner it was presented to them, and at every distance at which it could influence them.

Exp. 7. The glass tube was excited strongly and brought rapidly towards the same balls, and they were instantly attracted by it; the tube was withdrawn, and again brought towards them as before; they were again attracted, but not quite so strongly as in the first instance.

Exp. 8. The tube was well excited again, and brought slowly towards the balls: at first they were slightly attracted; but when it was advanced, they were slightly repelled; and when it was brought still nearer, the balls kept briskly receding from the tube. The tube being withdrawn, was presented to each of the electrometers in succession, and its effects on them proved it to be in a *negative* state of electricity.

Exp. 9. The negative electrical state of the tube being destroyed, it was again excited positively, and presented *suddenly* two or three times in succession to the balls connected with the outer surface; the attraction between the tube and balls was diminished every time the tube was presented to them, till it disappeared; and then, on bringing the tube something nearer, repulsion took place between it and the balls. The tube, after

 repelling

repelling the balls of the outer surface, was presented to those of the inner, which were attracted by it. Whenever the balls of the outer surface were attracted by the tube, its electrical state was *positive*; and whenever they were repelled by it, its state was found to be *negative*.

Exp. 10. At the conclusion of the preceding experiments the jar which had been the subject of them was discharged, and its outside connected with the ground. I then took the small jar before mentioned, and brought its ball in contact with the prime conductor, its outside being held in the hand; the machine was put in action, and a strong charge was communicated to both the jars. The communication between the ground and the jar whose electrical states were under examination being removed, the ball of the small one was presented to the balls connected with the inner surface of the former, and they were repelled by that of the small one in whatever manner it was presented, and at every distance at which it could influence them.

Exp. 11. The ball of the small jar was next brought towards the balls connected with the outer surface; and these were attracted in whatever manner it was presented, and at every distance at which it could influence them.

Exp. 12. Both the jars having been discharged, that whose electrical states were to be examined was charged again as in Experiment 10; but the small one being held by its ball while its outside coating was kept in contact with the prime conductor, its inner surface acquired a charge which was the reverse of that acquired by the inner surface of the other jar, but of equal intensity with it. After the jars had been strongly charged, the ball of the small one was presented to the balls connected with the inner surface of the other; and they were attracted by it in whatever manner it was presented to them, and at every distance at which it could influence them.

Exp. 13. The ball of the small jar was next brought towards the balls connected with the outer surface; and they were repelled by it in whatever manner it was presented, and at every distance at which it could influence them.—After both the jars had been discharged, the larger with its pith-balls, and the insulating stand were removed from the *positive*, and placed at the *negative* conductor; the inner surface of the jar being connected with it, while the outer communicated with the ground. And as the same mode of proceeding was followed in making the subsequent experiments as had been in the foregoing, it will be necessary to state their results only, which will be found perfectly accordant with those of the former.

Exp. 14. The jar being strongly charged, the balls connected

nected with its inner surface were examined. An excited glass tube first attracted, and then repelled them; when the latter effect was produced, the tube was ascertained to be *negative.*

Exp. 15. An excited stick of sealing-wax, and the small jar charged negatively, each being separately applied, constantly repelled them.

Exp. 16. The small jar charged positively always attracted them.

Exp. 17. The balls connected with the outer surface were next examined. The excited tube, and the small jar charged positively, constantly repelled them.

Exp. 18. The excited wax first attracted, and then repelled them; when the latter effect was produced, the wax was found to be *positive.*

Exp. 19. The small jar charged negatively, always attracted them.

On reviewing the foregoing series of experiments, it was evident that the object for which they had been undertaken was fully accomplished; but it seemed requisite to notice and account for the rapid changes which took place in the excited electrical states of the stick of sealing-wax and the glass tube when presented to the surfaces of the jar possessing an electrical state contrary to their own. For in performing the experiments care was always taken to prevent their coming into contact with the pith-balls, or even approaching nearer to them than was necessary to produce the intended effect. Those changes were clearly occasioned by these two circumstances:—first, the great intensity of the charge upon the surfaces to which the excited bodies were presented; and secondly, the small electrical capacity of those bodies in relation to that of the surfaces to which they were opposed. For when the small jar, whose electrical capacity is so greatly superior to that of either of the excited bodies, was charged, and presented as they had been to the surface possessing an opposite electrical state; the electrical state of the jar underwent no change of kind, the only effect produced on it being a very small diminution of intensity in the charge. Those changes therefore did not arise from any peculiar property of the jar in modifying or changing the electricity communicated to its opposite surfaces; for any simply electrified surfaces whose capacity and charge are much superior to those of the excited bodies will produce similar effects, as the following experiments show.—One extremity of a wire which had a brass ball at its opposite end, was inserted in the hole at the extremity of the prime conductor, so as to project horizontally from it; and two pith-balls were suspended from the end of the wire that was furthest from the conductor. The machine was put in action,

 and

and kept so while the experiments were performed. An excited glass tube always repelled the pith-balls; but excited wax *first attracted* and then repelled them; the wax when it repelled them was found to be *positive.* The prime conductor was now connected with the ground, and the experiments performed at the negative conductor. The excited wax always repelled the balls, but the excited tube *first attracted* and then repelled them; the tube when it repelled the balls was found to be *negative.* These experiments beautifully illustrate and confirm those made with the same bodies on the jar; and at the same time demonstrate that the causes assigned for the electrical changes which occurred in those bodies are the true ones.—And as the investigation is now completed, it will be proper to state the conclusions or propositions which appear to be established by it.

1st. That the surface of a Leyden jar which communicates with the conductor of an electrical machine in action, acquires a charge (however intense that charge may be) of the same kind of electricity as that of the conductor with which it communicates.

This proposition is proved by Experiments 1, 5, 10, 12, 14, 15, and 16.

2d. That the surface of the same jar which communicates with the ground acquires at the same time a charge (however intense it may be) of the contrary kind of electricity to that of the conductor with which the other surface communicates.

This proposition is proved by Experiments 6, 7, 11, 13, 17, 18, and 19.

3d. That an excited electric by being opposed to a surface highly charged with the contrary electricity and of greater electrical capacity, will rapidly lose its excited electrical state, and acquire a state similar to that of the electrified surface to which it is opposed.

This proposition is proved by Experiments 2, 3, 4, 8, 9, 14, 18, and by two of those made at the conductors of the machine.

Now the two former of the above propositions may be reduced into one, which may be thus expressed: That the opposite surfaces of a charged jar, however intense its charge may be, are always in opposite states of electricity. And as these opposite states are the immediate results of those laws of electrical action which the Franklinian theory of the jar contemplates; that theory, so far as it accounts for these opposite states, is by the above-cited experiments fully demonstrated. It is now time to advert to Mr. Donovan's experiments, in order to point out the source of his errors, the negligence with which he examined the electrical states of the jar on which he operated, and the futility

futility of his objections to Franklin's theory of charged electrics. His first two experiments are simply these: He presented excited sulphur to the pith-balls connected with the inner surface of his jar, and observed that they were repelled by the sulphur; he then presented the *same sulphur* to the balls connected with the outer surface, and observed that they were attracted by it. Now Experiments 2 and 3 of my foregoing series prove that his sulphur, when it repelled the balls of the inner surface, was in a *positive state* of electricity; and consequently, when he presented it to the balls of the outer surface, *attraction* was the necessary consequence. And Experiment 4 proves that had he presented the sulphur to the latter balls more than *twice*, he must have discovered that the sulphur *after attracting* would have *repelled* them. Again, Experiments 1 and 2 prove that the balls of the inner surface were *attracted* by the sulphur *before* they were repelled. Hence it is proved that in making his first two experiments, Mr. Donovan observed only one half of what he ought to have observed; and, unfortunately for him, this was the wrong half:—such was his attention in making them! Moreover he neglected to employ any additional means to ascertain the electrical state of either the inner or outer surface of his jar! He also neglected to examine the electrical state of his sulphur after it had repelled the balls of the inner surface, and likewise after it had attracted those of the outer! And not in the least suspecting the electrical change that the sulphur had undergone, but resting perfectly satisfied with his two fallacious observations, he *supposed* first that the inside of his jar was in a *negative*, when in reality it was in a positive state of electricity! secondly, that the outside was in a *positive* state, when in reality it was in a negative one! and lastly, that his sulphur was in a negative when it really was in a positive state! The results of his two experiments being, as he imagined, so discordant with Franklin's theory, he proceeds thus: "The ERROR does not relate merely to the surface occupied by the accumulation. Let the jar with its wires, balls, &c. be set on the stand as before, but not connected with the ground. Let the jar be charged, and while the cylinder is still turning let the surfaces be examined: it will be found that excited sulphur repels the balls of both the inside and outside."—Here again, neglecting all other assistance, Mr. Donovan employs his favourite but ungrateful and treacherous sulphur, and is, in the same manner as before, deceived by it. He however goes on: "This *cannot be explained* while it is supposed that the outside is then giving out electricity, urged by the repulsion of the fluid thrown in: for in this case if any body with a diminished quantity, as sulphur, be presented, an attraction ought to follow." True: and attraction *did* follow; but Mr. Donovan's attention was not sufficiently alive to observe it. "When the charging," says he, "is discontinued, another militating phænomenon occurs: the balls collapse; and after touching, again separate, but with a different power, for they are now attracted by excited sulphur." The last clause in the preceding sentence shows that Mr. Donovan presented the *same sulphur* to the balls of the outside after they had collapsed, that he had the moment before presented to them while they were evolving positive electricity in consequence of the action of the machine: the sulphur therefore which had become positive in that experiment attracted the balls now, because, the action of the machine having ceased, they were actually in a negative state in consequence of the electricity which had been evolved, and had departed from the outside during the previous experiment. But Mr. Donovan, still unconscious of the errors into which he had been seduced by placing implicit confidence in the mischievous sulphur, thus proceeds:

"Here then are strong instances which oppose the Franklinian hypothesis: it appears that the supposed excess may evince the symptoms of a state of diminution without any abstraction having taken place; and the diminution may appear to be an excess without any addition." These truly are singular discoveries, all made by Mr. Donovan and his sulphur! And as he terminates with these discoveries his inquiry concerning the electrical states of a charged jar, so likewise with them shall terminate my examination of that inquiry. And I shall add only that if, at first view, it should seem that I have entered more into the detail of experiments than was necessary to demonstrate Mr. Donovan's errors, the circumstances under which his Reflections have been ushered into public notice must be considered. Now it appears that the Memoir or Essay, of which the Reflections are an abridgement, was read to a learned and respectable Society, of which Mr. Donovan is himself the Secretary*; that the above Essay was honoured with the prize by the Royal Irish Academy†; and that the author communicated the Reflections to you, sir, for insertion in your valuable Magazine*. He must therefore have considered his Reflections entitled to attention; and a superficial examination of his experiments and opinions would not have been either satisfactory to the author, or of any use to the inexperienced electrician, who would still have been in danger of being misled by them.

I am, sir, with great respect,

Your obliged servant,

Hereford, Nov. 13, 1815. THOMAS HOWLDY.

* Phil. Mag. vol. xliv. p. 334. † Ib. vol. xlv. p. 222.

LXXIV. *An*

LXXIV. *An Account of some Experiments with a large Voltaic Battery.* By J. G. Children, *Esq. F.R.S.**

In 1809 I presented to the Society a short account of some experiments performed with a Voltaic battery of unusually large plates, which has been honoured by publication in the Philosophical Transactions for that year. Since that period I have constructed another of still larger dimensions, the effects of which form the subject of the present communication. The copper and zinc plates of this apparatus are connected together, in the usual order, by leaden straps; they are six feet long, by two feet eight inches broad, each plate presenting 32 square feet of surface. All the plates are attached to a strong wooden frame suspended by ropes and pulleys, which being balanced by counterpoises, is easily lowered and elevated, so as to immerse the plates in the acid, or raise them out of it, at pleasure. The first trials of the power of this instrument were made in July 1813, in the presence of several philosophical friends; but the effects then fell very short of my expectations, arising, as I afterwards found, from a defect in the construction, which has been since remedied, and another copper plate added to each member of the series, so that every cell now contains one zinc and two copper plates, and each surface of zinc is opposed to a surface of copper. This was done at the suggestion of Dr. Wollaston, and has very considerably increased the power of the battery. From some comparative experiments, which I made with a small apparatus, the increase in quantity of electricity, thus effected, is at least one half. The cells of the battery are 21 in number, and their united capacities amount to 945 gallons. To each pole of the battery a leaden pipe, about 3-4ths of an inch in diameter, is attached by solder, and the opposite end of each pipe immersed in a basin of mercury, (a separate basin for each pipe,) by means of which the circuit is completed, and a perfect contact ensured. The first experiments I shall mention were made on the comparative facility with which different metals are ignited when placed in the electrical circuit. For this purpose, in each experiment, two wires of dissimilar metals were taken, of equal diameter and length; one end of each was in contact with one of the basins of mercury communicating with the poles of the battery, and the other end bent to an angle, and the wires connected continuously by hooking them together. The length of each wire was eight inches, and the diameter $\frac{1}{30}$th of an inch. The battery was moderately excited by a charge of one part acid diluted with 40 parts of water.

* From the Philosophical Transactions for 1815, part ii.

Exp.

Exp. 1. A platina and a gold wire being thus connected, and introduced into the electrical circuit, the platina was instantly ignited, the gold remained unaffected.

Exp. 2. A similar arrangement of gold and silver wires. The gold was ignited, the silver not.

Exp. 3. The same with gold and copper. No perceptible difference in the state of ignition; both metals were heated red.

Exp. 4. Gold and iron. The iron was ignited; the gold unchanged.

Exp. 5. Platina and iron. The iron ignited instantly at the point of contact next the pole of the battery. Then the platina became ignited through its whole extent. After this the iron became more intensely heated than the platina, and the ignition of the latter decreased.

Exp. 6. Platina and zinc. The platina was ignited; the zinc was not; but melted at the point of contact. In a subsequent experiment, the zinc did not melt; but the platina ignited as before.

Exp. 7. Zinc and iron. The iron was ignited; the zinc bore the heat without fusing.

Exp. 8. Lead and platina. The lead fused at the point of contact.

Exp. 9. Tin and platina. The tin fused at the point of contact. No ignition of either wire took place in the two last experiments.

Exp. 10. Zinc and silver. The zinc was ignited before it melted: the silver was not ignited.

The results in every case were the same to whichever pole of the battery either wire was presented. I varied these experiments by introducing several alternations of different wires continuously connected into the circuit, and obtained in every instance analogous results. Thus

Exp. 11. Alternations of platina and silver, three times repeated: all the platina wires were ignited, and none of the silver.

Exp. 12. One zinc wire between two platina: both the platina wires were ignited, the zinc not.

Exp. 13. One iron between two platina. Both the latter first ignited; then the iron, which soon became most heated, and fused.

It is unnecessary to enter into a further detail of these experiments; it will be sufficient to say generally, that when wires of several different metals were introduced at once into the circuit, the order of their ignition was precisely that of the former experiments. In one experiment with copper and gold, the copper was decidedly most heated.

I feel some difficulty in attempting an explanation of the preceding

ceding phænomena, and offer the following conjecture with diffidence. When a perfect communication is established between the poles of the battery, the electricity circulates without producing any visible effect; but if it meet with resistance in its passage, it manifests itself by chemical action, by the evolution of heat, or both. Thus, if a bar of metal be connected with one pole of the battery, and its extremity immersed in a basin of mercury connected with the other pole, at the instant the surfaces come in contact, heat and light are evolved, which cease as soon as the bar, if it be of sufficient size, is fairly plunged beneath the surface of the quicksilver. If the circuit be completed by two pieces of charcoal, the evolution of heat and light is permanent, as long as their surfaces remain in contact, because that contact can never be so perfect as to oppose no resistance to the electricity; whereas, in the case of the bar of metal and the mercury, it soon becomes complete, and the current is then uninterrupted. Resistance, therefore, appears to occasion the development of heat (whatever be the ultimate cause of the phænomenon); and as this must be inversely as the conducting power, when any two of the wires connected continuously are placed in the circuit, that which is the worst conductor must be most heated; and thus platina, having the lowest conducting power, is ignited before all the rest; and silver, which conducts best, is not heated red when connected with any of the other metals. Should it be objected, that if the electricity meet with greater resistance in one body than in the other, equal quantities cannot be transmitted in equal times by the two substances, (a circumstance essential to electrical action,) I answer, that a body may be propelled through two media of different densities, with equal velocity, if the propelling forces be proportionate to the resistances; and it is a necessary consequence that, whatever effect the passage of the body be capable of producing in the least resisting medium, it will produce it in a still greater degree in the most resisting; and if that effect be heat, the greatest portion will be developed in the latter instance. In the case in question, indeed, there is but one propelling force; but as it is sufficient to overcome the greater resistance, the analogy is unshaken. That the ignition of the wire is generally first perceptible at the point of contact next the pole of the battery (to whichever pole it be presented) is in favour of the hypothesis. I once thought the phænomena might be owing to the joint effect of difference of conducting power, and inequality of the capacity of different metals for heat; but the experiments of Crawford, Leslie, Dalton, Irvine, and others, militate against that idea; for, according to them, the capacities of iron and platina exceed those of all the other metals, whereas, on the supposition

supposition alluded to, they ought to be inferior. From the foregoing results, the order of the conducting powers of the metals tried is silver, zinc, gold, copper, iron, and platina. Tin and lead fuse so immediately at the point of contact, that they cannot be placed. Between gold and copper the difference is trifling; and with regard to platina and iron, their relations to each other, in this circumstance, seem to be affected by elevation of temperature. It may be observed, that the order of the above metals, as conductors of electricity, nearly follows that of their powers to conduct heat.

In an experiment in which equal lengths of two platina wires, of unequal diameter, (the larger being $\frac{1}{30}$th, the smaller $\frac{1}{50}$th of an inch,) were placed together in the circuit *parallel* to each other, the thicker wire was ignited, because it conveyed more electricity without proportional increase of cooling surface. When connected continuously, the order of ignition was reversed. These two results were foreseen by Dr. Wollaston, who suggested the experiments.

The experiments which I now proceed to mention, were made with the battery in a high state of excitation; and I consider them as representing nearly the maximum of effect which it is capable of producing. As the quantity of acid was increased from time to time, and that previously added often almost spent before fresh was put in, it is not easy to say exactly what proportion it bore to the water; perhaps the largest may be stated at about $\frac{1}{20}$th. On this, as on former occasions, I found a mixture of nitrous and sulphuric acids, to produce the most powerful and permanent effects.

Exp. 1. 5 ft. 6 in. of platina wire, $\frac{11}{100}$ of an inch in diameter, were heated red throughout, visible in full day-light.

Exp. 2. 8 ft. 6 in. of platina wire, $\frac{44}{100}$ of an inch in diameter, were heated red.

Exp. 3. A bar of platina 1-6th of an inch square, and $2\frac{1}{4}$ inches long, was also heated red, and fused at the end; and,

Exp. 4, a round bar of the same metal, $\frac{276}{1000}$ of an inch in diameter, and $2\frac{1}{2}$ inches in length, was heated bright red throughout.

Exp. 5. Fine points of boxwood charcoal intensely ignited in chlorine, neither suffered any change, nor produced any in the gas. The result was similar when heated in azote.

I next tried the power of the battery to fuse several refractory substances. The subject of experiment was placed in a small cavity, made in a piece of well burnt boxwood charcoal, floating on the surface of the mercury in one of the basins before mentioned, and the circuit completed by another piece of charcoal, communicating by stout copper wire, with the other basin.

Exp.

Exp. 1. Oxide of tungsten, which (as well as all the other metallic oxides operated on) had been previously intensely ignited in a charcoal crucible, in a powerful furnace, fused, and was partially reduced. The metal grayish white, heavy, brilliant, and very brittle.

Exp. 2. Oxide of tantalum. A very small portion fused. The grains have a reddish-yellow colour, and are extremely brittle.

Exp. 3. Oxide of uranium; fused, but not reduced.

Exp. 4. Oxide of titanium; fused, not reduced. When intensely heated it burnt, throwing off brilliant sparks like iron.

Exp. 5. Oxide of cerium; fused, and when intensely heated it burnt with a large, vivid, white flame, and was partly volatilized, but not reduced. The fused oxide, on exposure for a few hours to the air, fell into a light brown powder, containing numerous shining particles of a silvery lustre, interspersed amongst it, and exhaled an odour similar to that of phosphuretted hydrogen.

Exp. 6. Oxide of molybdena; readily fused and reduced. The metal is very brittle, of a steel-gray colour, and soon becomes covered with a thin coat of purple oxide.

Exp. 7. Compound ore of iridium and osmium; fused into a globule.

Exp. 8. Pure iridium; fused into an imperfect globule, not quite free from small cavities, and weighing 7·1 grains. The metal is white, very brilliant, and in its present state its specific gravity is 18·68, which must be much too low, on account of the porous state of the globule. In the minutes of the experiments, in July 1813, mention is made of the fusion of a small portion of pure iridium into a globule weighing $\frac{62}{100}$ of a grain, which had been previously submitted to the action of a battery of 2000 plates, of four inches square, without melting.

Exp. 9. Ruby and sapphire, were not fused.

Exp. 10. Blue spinel, ran into a slag.

Exp. 11. Gadolinite, fused into a globule.

Exp. 12. Magnesia, was agglutinated.

Exp. 13. Zircon from Norway, was imperfectly fused.

Exp. 14. Quartz, silex, and plumbago, were not affected.

In the year 1796, M. Clouet converted iron into steel, by cementation with the diamond, with the view of confirming the nature of that substance, and of ascertaining the exact state in which carbon exists in steel. Clouet had also previously formed steel by cementation with carbonate of lime. Mr. Mushet repeated this experiment, using instead of the carbonate, caustic lime, and obtained also what he considered to be cast steel; whence

whence he concluded that the carbon necessary to convert the iron into steel had not been furnished, as Clouet supposed, by decomposition of the carbonic acid, but that it had found its way from the ignited gas of the furnace to the iron. This result occasioned suspicions of the accuracy of the deductions from the experiment with the diamond; and Mr. Mushet accordingly, at the suggestion of the editor of The Philosophical Magazine, repeated the experiment made at the Polytechnic School, only *keeping out the diamond.* The results (for he made several experiments) uniformly gave him good cast steel, whence he concludes that we are still without any satisfactory or conclusive proof of the steelification of iron solely by means of the diamond; and adds that he doubts whether the diamond afforded *even one particle of carbon to the iron.* The details of both Clouet's and Mushet's experiments may be found in the fifth volume of The Philosophical Magazine. Sir George M'Kenzie repeated both Clouet's experiments, and those of Mr. Mushet, and obtained results confirming the conclusions of the French chemist. The labours of this gentleman indeed seem sufficiently conclusive; but, if a doubt should remain, it occurred to Mr. Pepys, that the battery would afford an *experimentum crucis* on the subject; and his ingenuity readily suggested a mode of making it, every way unobjectionable. He bent a wire of pure soft iron, so as to form an angle in the middle, in which part he divided it longitudinally by a fine saw. In the opening so formed, he placed diamond powder, securing it in its situation by two finer wires, laid above and below it, and kept from shifting, by another small wire, bound firmly and closely round them. All the wires were of pure soft iron, and the part containing the diamond powder was enveloped by thin leaves of talc. Thus arranged, the apparatus was placed in the electrical circuit, when it soon became red hot, and was kept so for six minutes. The ignition was so far from intense, that few who witnessed the experiment, expected, I believe, any decisive result. On opening the wire, however, Mr. Pepys found that the whole of the diamond had disappeared; the interior surface of the iron had fused into numerous cavities, notwithstanding the very moderate heat to which it had been exposed; and all that part which had been in contact with the diamond was converted into perfect blistered steel. A portion of it being heated red and plunged into water, became so hard as to resist the file, and to scratch glass. This result is conclusive; for as the contact of any carbonaceous substance, except the included diamond, was effectually guarded against, to that alone can the change produced in the iron be referred. This experiment will also probably

bably be deemed fatal to the opinion of those mineralogists (if any do still maintain that opinion) who class the diamond with substances of the siliceous genus.

When dry caustic potash was exposed to the intense heat between the two pieces of charcoal, it fused, and appeared to decompose, throwing off a large flame of the peculiar purple-red colour that attends the combustion of potassium. When moist caustic potash was placed in the circuit, the water only was decomposed.

I endeavoured to ascertain if there be any difference in the degree of heat produced at either pole of the battery, by placing two small earthen-ware cups, each containing an equal weight of mercury, in the circuit, and connected together by a platina wire of such size and length as to be kept constantly ignited. The mercury in the cup connected with the zinc end of the battery, attained in 20 minutes the temperature of 121°; that in the other cup 112°.

The battery, even in its most active state, communicated no charge to the Leyden phial.

I give the following experiment, the last with which I shall occupy the time of the Society, without comment. I separated all the zinc from the copper plates, by dividing the leaden straps that united them; and then by means of other leaden straps, I connected all the zinc plates together as one plate, and all the copper plates in the same manner; thus reducing the whole battery to only two plates, each presenting a surface of 1344 square feet, reckoning the copper surface as only equal to the zinc. When the plates, thus arranged, were suspended, quite out of contact with the acid, a communication was made between the two metallic surfaces by means of a platina wire $\frac{1}{2000}$ndth of an inch diameter, and about $\frac{1}{30}$th of an inch long, with every possible attention to ensure a perfect contact; but, although the experiment was made in the dark, not the slightest appearance of ignition was perceptible in the minute wire by which these extensive surfaces were connected. It is known, I believe, to almost every member of this Society, that Dr. Wollaston has shown, with the delicate apparatus invented by him, that a platina wire, of the same dimensions as that just mentioned, is instantly ignited by a single pair of plates one inch square, on being immersed in a diluted acid. The ratio of the areas of the plates of the respective batteries is as 1 to 48384. When the plates of the large battery, in the usual order of arrangement, were immersed in mere pump water, previous to any acid having been put into the cells, they ignited $\frac{1}{4}$th of an inch of platina wire $\frac{1}{400}$th of an inch diameter, and fused the end of it into a perfect globule.

LXXV. *On*

LXXV. *On the Elasticity of Fluids.*

To Mr. Tilloch.

Sir,—The particles of elastic fluids are generally supposed to repel each other: and from the manner in which the first principles of pneumatics are investigated, it would appear that this property is necessary to the existence of an elastic fluid. If we admit that it is, we must also admit that the same particles may at one time attract, and at another time repel each other. For the particles of water attract each other, and when water is converted into steam its properties are not altered; but steam is an elastic fluid, therefore the particles must have been separated beyond their sphere of attraction, and must have got within a sphere of repulsion. How distance alone can change the properties of matter we are at a loss to conceive.

Let us endeavour to explain the phænomena on other principles. Heat and water combine with each other in a constant proportion under the same atmospheric pressure; but in this state it can hardly be called an elastic fluid, because on being pressed the heat is expelled, and part of it becomes water; but if it can instantly regain the quantity of heat expelled, it is elastic; and this elasticity is owing to the strong tendency of heat and water to combine, when a certain proportion of heat is present: this proportion varies as the pressure. When steam is compressed, heat is given out; and it is not compressed into a less space, as the advocates for the repulsive system suppose it to be.

In your paper on Elasticity (Phil. Mag. vol. xxii.) you say that attraction and heat are the causes of elasticity: but all the phænomena may be exhibited without the assistance of heat: for instance, immerse a sponge in water, press it and remove the pressure, it is elastic, and attraction and form are the causes.

I have not, hitherto, seen any experiments to prove the existence of repulsion, that do not admit of a satisfactory explanation from attraction only, and we never ought to admit of more causes than are sufficient to account for the effects.

I was led to make the above observations, from seeing a paper On Chemical Composition (Thomson's Annals for November, p. 376), where the doctrine of repulsion is brought forward to explain a part of that theory.

I am, &c.

Bentinck-street, Dec. 7, 1815. T. T.

LXXVI. *On the supposed Diminution of Volume which Water at 32° undergoes by Additions of Temperature till it reaches 41°.* By A Correspondent.

To Mr. Tilloch.

Sir,—Previous to the admission of a fact which is a remarkable exception to a known law of nature, it ought to be established by the most direct and decisive experiments; and it ought to serve some important purpose in the œconomy of nature which could not be accomplished by any other means; otherwise, such a deviation from a general law would not be consistent with the dignity of an infinitely wise Creator.

It appears to be a general law of nature, that all bodies are expanded by heat: water, at low temperatures, is said to be an exception: the following are supposed to be the phænomena.

When heat is added to water ice-cold or at 32°, the water contracts, and continues to contract as heat is added till its temperature is increased to 41°: then, without any perceptible alteration in its properties, the water begins to expand, and increases in volume with every subsequent addition of heat.

Mr. Dalton, after a long train of experiments, was led to believe that he, and his predecessors in the same field of investigation, had fallen into a mistake with regard to the contraction of water by heat, in consequence of under-rating the effect which the change in the capacity of the apparatus employed must occasion in the apparent volume of the fluid.

To remove this objection, to which all the preceding experiments were liable, Dr. Hope made a new set of experiments; in which, we think, there is more confidence placed than the nature of the experiments entitles them to. The principle on which these experiments were made, is, that when a fluid is partially dilated or condensed the equilibrium is destroyed, and the fluid put in motion: consequently, if the surface of ice-cold water when heated descends, it shows that heat condenses water, and proves the existence of the anomaly, and the reverse if it does not descend.

It must be obvious that the manner in which the heat is communicated will make a very great difference in the results. In the first of Dr. Hope's experiments (Phil. Mag. vol. xxiii. p. 153), a glass jar was used: it was placed upon a table, with a considerable thickness of matter of little conducting power between it and the table. Now the worst solid conductor will communicate heat much better than a fluid; and glass, interposed between two fluids, transmits heat very slowly till it acquires the temperature of the fluid giving out heat: hence the greater part of of the heat would be communicated from the bottom, for the top of the fluid would condense the stratum of air in contact with it: but we do not suppose it would continue to obtain heat from the air, at least it would be at a very slow rate. Also it is evident, that the lower part of the water must require a considerable degree of expansion to enable it to overcome the cohesion and inertia of the part above; and the expansion at low temperatures being very small, it is probable that it would require several degrees of heat to put it in motion. The motion once begun, a less quantity of heat would continue it; also the expansion would be more as the temperature increased. In this manner the results of the first experiment may be accounted for, without supposing the water to contract with the addition of heat.

In the second experiment we are not certain that any motion took place till the upper thermometer sunk to 36°; and whatever care might be taken to keep the surrounding fluid at an equal temperature, near the surface, that part of it under the jar would acquire a temperature sufficient to cause the lower thermometer to become stationary, till the upper part of the water became dense enough to overcome the resistance and descend.

In the third experiment, motion would speedily take place: but it appears that when the difference between the top and bottom temperatures was only one degree, the motion ceased; and here we meet with a fact which in some measure proves the correctness of our explanation; that is, the fluid in the axis at the top was at 34°, at the same time that part of the fluid, we suppose at about half the height of the jar, was freezing. Why did it not ascend, and let the warmer portion take its place?

Without some difference in specific gravity, we must say that motion could not take place. Now it appears clearly that there was no motion in the water during the fourth experiment; for, when both the thermometers were at 36°, a part of the water must have been at 32°: had there been any motion, either the upper or lower thermometer must have been at 32°, or nearly so. The jar was placed in a pan, we suppose without any thing between it and the bottom of the pan:—the pan would be supported by something that would conduct heat; and in consequence of this arrangement, that part of the water round the lower thermometer would be receiving heat from the bottom of the jar; which will account for its not being cooled at a quicker rate than the top one.

The fifth experiment was made with a taller jar, and the heat applied at the middle: it would have been interesting to have known the temperature in the axis of the jar, at the height where the heat was applied.

This experiment, in common with the others, tends to prove that,

that, at low temperatures, a difference of several degrees is necessary to produce motion in the fluid. The evaporation of the warm water, and of the surface of that in the jar, would tend to cool the top of the fluid at a quicker rate than it could be supplied from the middle, till the motion had become accelerated; when the supply from the middle would exceed the expenditure, and consequently the temperature at the top would increase.

The sixth experiment only proves that, the thermometers being 20 inches distant from each other, and the upper part of the water at 33°, the lower part might be heated to 39° without causing motion in the fluid; for that there was no motion is evident, because when the upper thermometer was at 36·5° the water was freezing at the middle of the height of the jar.

Dr. Hope seems to have been aware that part of the facts might be accounted for by the effects of the cohesion and inertia of the water. The fourth experiment led him to believe that these forces had little effect. But that an imperceptible change of density should overcome the effects of cohesion and inertia, appears as much an anomaly as the expansion of water by cooling; for the direct cohesion of a square inch of water is not less than 40 grains troy: add to this the effect of inertia, and it will not appear surprising that the small increase of bulk which water under 40° acquires by increasing its temperature 6°, should be necessary to put in motion.

Had Dr. Hope used three thermometers in the two last experiments, it would have thrown some additional light on the subject: at present we have many reasons for doubting the fact, that water is condensed by the addition of heat.

I am, sir, your most obedient servant,

T.

LXXVII. *On the Solubility of alkaline and earthy Salts, as affected by an Excess of the Acid.* *By* A Correspondent.

To Mr. Tilloch.

Sir,—In prosecuting a set of experiments on the alkaline and earthy salts, the anomaly which takes place on their solution, some in an increased and others in a decreased ratio, by an excess of acid, as are observable in the tartrate and super tartrate of potass and the sulphate and super sulphate of potass, could not escape my observation. Potass with one dose of tartaric acid forms a salt easily soluble; an extra dose being added, instead of precipitating in the neutral state, it combines with it and falls down: but a saturated solution of this less soluble and super salt, when treated with the crystalline acid, affords a pre-

 cipitate

cipitate of super tartrate of potass. Sulphate of potass, on account of its sparing solubility, would be precipitated on a slight addition of acid, were it not capable of combining with an excess of acid and being rendered by it a super and more soluble salt: this then requires a much larger proportion of acid to deliquidate it than a salt of the same solubility as the neutral sulphate, and which is not capable of combining with an extra dose of acid. But other and more important facts presented themselves to my notice, which do not appear to have received that attention by chemists which their importance in the science requires. Students naturally conceive that the bases of soluble salts are soluble in the acids which form these combinations—this very extensive error may include the whole class of salts; but as in this I allude to acids of the usual specific gravity, it is alone the permanent and efflorescent salts which are precipitated by them.

Some observations in your valuable Magazine, on the subject of barytes and strontian, by Mr. Hume, somewhat lessened the difficulties to which the student is subject: but whether that gentleman knew that these properties were not peculiar to those earths, I am entirely ignorant. Berthollet hinted in a more general way that such properties do exist, when he stated, that by increasing the points of contact of the salt precipitated from the solution of sulphate of potass by sulphuric acid, it may be redissolved: this solution however cannot be obtained without exposure of the mixture to the atmosphere (from which it absorbs moisture) by an increase of temperature, or perhaps by some other circumstance, which, if it be the case, ought to have been explained.

Deliquescent Salts.	*Muriates.* Lime. Magnesia. Alumina. — *Nitrates.* Magnesia. Lime. Ammonia. Alumina. — *Sulphate.* Alumina.	Are not precipitated either on the addition of their acids of the usual specific gravity to a saturated solution, or on the addition of the dry base to its acid.
Permanent and efflorescent Salts.	*Muriates.* Potass. Soda. Ammonia. Barytes. Strontian. — *Nitrates.* Potass. Soda. Barytes. Strontian. — *Sulphates.* Soda. Magnesia. Ammonia.	Are precipitated either on the addition of their acids to a saturated solution, or by throwing the dry base on its acid.

Metallic

Metallic salts are also subject to similar changes. We see then, that salts in general, alkaline, earthy, and metallic, partake with barytes of the property of being precipitated under similar circumstances by their respective acids;—and generally, according to the greater or less solubility of salts in water, they are precipitated by their acids more or less concentrated.

Had the facts here contained been generally known, the authors of chemical rudiments must necessarily incur great censure, for plunging deeper into mystery, that which is already intricate: it must have involved many novices in the science into the greatest difficulties; for their first attempts are generally to form the salts, and in performing which, if instead of solution they obtain a precipitate, they conclude that the experiment by some unknown accident has failed,—they refer to the instructions and repeat the experiments in vain; and this they are subject to, not only in the formation of alkaline and earthy salts, for matallic salts, as we have seen, are included in the common error. What can be a greater discouragement?—But long after, when least expected, they by some fortunate accident obtain by means of a dilute acid the earnestly sought-for solution; this event steals on them, not as a discovery, for they charge it to their own ignorance rather than the instructor's neglect. Water then, when required to the effect, should be expressed, as it cannot be supposed that one who is ignorant of its properties can understand that it should in this case be present, when in many experiments its absence to a certain degree is absolutely necessary.

Rudiments should be such, that the inexperienced learner should obtain by them a competent knowledge of those laws by which the more abstruse parts of a science should become less difficult, as he has been more arduous in imbibing the means of success.

Under these circumstances, I cannot forbear to advise the authors of *chemical* rudiments, to explain more clearly that it is the acid which unites with the base, and the water alone which dissolves this combination.

Your obedient servant,

Nov. 15, 1815. H.

LXXVIII. *Experiments in Voltaic Electricity.* By Andrew Crosse, Esq. *In a Letter from the Author.*

To Mr. Tilloch.

Sir,—Having seen a statement in your Magazine for August, that M. Dobereiner of Jena subjected mercury in contact with water to the Voltaic action, and obtained at the negative pole, where the mercury was placed, an amalgam of that metal and hydrogen, which was not given off in the form of gas; I thought it worth while to repeat the experiment, which I have varied in every possible way, and of which the following is an accurate account,—if you think it worth whil to make room for it among the more important communications you are in the habit of receiving. Having let fall a globule of pure mercury of about twice the size of a pea into a watch-glass of common spring water, I plunged two platina wires, proceeding from each end of a Voltaic trough, into the water, neither of them touching the globule, but removed to the opposite extremities of the glass.—Oxygen and hydrogen were given out by each as usual, the mercury remaining unaltered; the wires being brought nearer, the mercury lost its globular form, extending itself in an oval shape, much flattened, appearing equally attracted by each wire. The wires being both made to touch the globule and drawn back again, the mercury adhered to the negative. In this situation, the positive wire not touching the metal, no hydrogen gas was given out by the negative, or so small a quantity that it required very minute inspection to detect it: the mercury appeared of less specific gravity, more fluid and brilliant than before; whilst the positive wire separated a copious stream of oxygen. In two or three minutes some bubbles of hydrogen began to arise, but in small quantity.

If the positive wire is brought within a small distance of the mercury when connected with the negative, a great portion of the oxygen passes over the surface of the globule and of the water in a rapid current towards the negative wire; and on bringing it almost in contact, the mercury is attracted by it, touches it, and immediately shrinks from it, assuming the form of a cone, and again expanding so as to touch the wire a second time, continuing this motion or rather pulsation for some time. When the trough is first charged, this motion is inconceivably rapid, and has continued with a single two-inch trough of 50 plates for two hours or more. The negative wire will be found coated with mercury on being removed. If the positive wire is plunged into the globule (the negative not touching it), and even drawn out of it immediately, it becomes fixed in a moment, losing its brilliancy and fluidity, depositing a black oxide at first, and after some hours a beautiful lemon-coloured oxide; but it does not become less fluid after remaining in this situation a week than it does after a few seconds. No oxygen is given out by the positive wire at first; but after some seconds, when the globule appears a good deal oxided, a few bubbles begin to appear, while the negative wire gives out a copious

copious stream of hydrogen. Examined by the microscope, a metallic crystallization appears to shoot over the surface of the metal accompanied with prismatic colour. Heat tends to make the globule less fluid, by evaporating the mercury and leaving the oxide. Having evaporated the water from a globule exposed to the Voltaic action for 16 hours, the glass being much heated, a series of small explosions accompanied with coruscations of fire appeared to take place from the residuum surrounding the mercury; but as I have failed in reproducing this effect by the same means, it is not impossible to have been an ocular deception. On touching with the negative wire the globule after it has been oxided by the positive, the oxide falls off in scales, and the metal quickly reappears in its pristine brilliancy and fluidity, a portion of oxide passing to the negative pole, and no hydrogen gas given off, being absorbed by the globule—so that it is easy to fix or make fluid the metal, which is best done by separate watch-glasses connected with each other by a conducting substance—the positive globule will be fixed, the negative fluid, and by reversing the glasses, both will be changed. If the mercury has remained in contact with the negative wire some time, and the positive is withdrawn so as to break the connexion with the poles, the metal contracts and expands for some seconds, as if to get rid of the hydrogen gas. The mercury cannot be set in motion after having been connected with the negative for several minutes, until, having been first touched by the positive, it is put in contact with the negative. These effects take place when the globule is subjected to the action of a small trough charged with common water, but in a much less degree. If the globule is very small, gas is given out by the wire in contact with it, as well as by the other. If the water is warmed by a lamp during the action of the wires on the mercury, gas is more copiously liberated from both, and the oxide instead of being dark is of a lemon colour; but when the water boils, the oxide is dissolved, the mercury is rendered clear, and *cannot be fixed;* nor has the negative any attraction for it, nor can it be coated with metal. In vacuo gas is given out by both wires at the same time, and in a much greater quantity; when the air is admitted, the wires cease to give out more than before it was exhausted. The watch-glass with its globule under common water being subjected to a stream of common electricity, passing from one platina wire to the other, and *not intercepted;* precisely the same effects took place as with the Voltaic trough, but it required longer time. An intercepted stream put the globule in most violent motion between the wires. No gas was visible from either wire when the current of electricity was un-

 interrupted;

interrupted; but when broken by Lane's electrometer, gas was given out by both wires, accompanied with a torrent of small sparks, and a cloud of oxide rising between the positive wire and the globule. This current of electricity, when not intercepted, had power to tinge litmus paper red and turmeric paper brown at the respective positive and negative wires—a few turns of the winch made this visible. The above-mentioned effects took place when the globule was subjected to the Voltaic action under double distilled rain-water, but in a much less degree, as the pure water was with *difficulty decomposable* with a hundred four-inch plates highly charged. This appeared to arise from its being so imperfect a conductor in consequence of being freed from acid: indeed, a successive minute stream of sparks attended with an electrical snapping, took place from one wire to the other, and not much gas was discernible from either; but the moment a drop of acid was let fall into the water, the snapping ceased, and gas was most copiously disengaged from both wires. No motion could be given to the mercury, until the water was slightly acidified. If the sulphuric, nitric, or solution of citric acid are added to the distilled water in any quantity, the gas is abundantly increased; but the mercury is not easily fixed nor set in motion as with common water, and the surface when in contact with the negative wire is covered with bubbles of hydrogen rising from every part of the globule. Under water impregnated with carbonic acid gas or acetic acid much the same effects take place as under common water, except that more gas is liberated; but with the acetic acid less gas is liberated than with the carbonic acid, and the globule is not so easily fixable. When the pure water was acidified with either the muriatic or oxalic acids, a most singular effect took place on connecting the globule with the positive wire—it lost all fluidity in a moment, and could be drawn out in very long fibres as small as the finest hair—with the muriatic acid the mercury in contact with the positive became partially coated with a very beautiful deep-blue coat, occasionally mixed with a fine orange-coloured oxide; when touched by the negative the coloured scales fell off in a moment, and the metal appeared perfectly brilliant—the oxalic acid produced no colour. This is a beautiful experiment. Under solution of boracic acid in alcohol, the mercury was not so friable nor capable of being set in motion as under water. A singular smell was emitted when the trough was highly charged. With all these acids gas was liberated by both wires, whether in contact with the globule of mercury or not. When the platina wires are plunged in liquid ammonia, and held even an inch asunder, the most violent motion is communicated to the globule,

bule, and a great quantity of gas arises from both wires—in contact with the positive, it is fixed and oxided in a moment—in contact with the negative the metal first resumes its former brilliancy and fluidity, then appears more fluid and brilliant; lastly, it begins to swell and become dull, gradually increasing in size till it becomes *more than ten times* its original bulk. In this state it appears like a spongy light metallic mass, covered with protuberances, of very irregular shape, being a complete amalgam of mercury and ammonia. When the connexion is broken by removing the positive wire from the ammonia, a vast quantity of hydrogen gas is liberated by the amalgam, and the mercury gradually returns to its former state. When solution of pure soda or potash is made use of, nearly the same appearances occur as with the ammonia, excepting the swelling of the mercury. Under lime-water it is fixed and oxided slowly, when in contact with the positive, but appeared evidently inclined to form an amalgam; when touching the negative, a quantity of gas arose from both wires.

Under alcohol or ether the same appearances were visible as under distilled water; the quantity of gas was increased if the spirit was heated or fired; no change takes place in the globule under naphtha unless the mercury is ignited by the Voltaic stream, in which case a great deposit of charcoal is visible on the negative side. Under ether holding phosphorus in solution, the positive wire in contact with the mercury causes it to lose its fluidity and be oxided rapidly; and it is so much flattened that it covers four times the surface it occupied before, extending itself in a singular manner in every direction from the positive wire, till it ceases to touch it, leaving around it a circular space; gas is given out by the wire not touching the metal as under water. No effect takes place under phosphorized olive oil, and very little under olive oil holding sulphur in solution, the mercury being slowly fixed by the positive wire, but no perceptible gas given out by either wire.

From the foregoing experiments I venture to draw the following conclusions: That the change in the mercury from a fluid to a more solid substance when in contact with the positive pole is simply occasioned by the metal being oxided, which oxygen unites with the hydrogen from the negative wire when touching it, and restores it to its fluidity. That this is proved by its being difficult or impossible to fix the mercury under a fluid which has power rapidly to dissolve the oxide formed. That phosphorus and sulphur are still simple substances, and have no metallic base; that carbon has never been metallized, as in that case it would in all probability form an amalgam with the mercury under naphtha, instead of being deposited in its

state

state of powder. That consequently ammonia, the fixed alkalies and lime, are the only substances mentioned in this course of experiments which contain a metallic base.

I am, sir,

Your obedient humble servant,

Broomfield, near Taunton, Nov. 25, 1815. ANDREW CROSSE.

LXXIX. *On the Action of Acids on the Salts usually called Hyperoxymuriates, and on the Gases produced from them. By Sir* HUMPHRY DAVY, *LL.D. F.R.S.**

THE effects produced when concentrated hydro-sulphuric acid (oil of vitriol) is poured upon hyperoxymuriate of potassa, have been often objects of chemical discussion; the acid and the salt, it is well known, become deep orange, and if any moisture is present, or if heat is applied to the mixture, a detonation occurs. In a paper read before the Royal Society, I have ventured to suppose that these phænomena depend upon the development and sudden decomposition of the compound of chlorine and oxygen, which I have named euchlorine.

A statement, which I understand has been made by M. Gay Lussac, namely, that a peculiar acid, which he has called chloric acid, may be procured from the hyperoxymuriate of baryta by sulphuric acid, led me to examine the action of acids on the hyperoxymuriates under new circumstances, and I have made some observations which appear to me not unworthy of being communicated to the Royal Society.

If 30 or 40 parts of sulphuric acid be poured upon one part of dry hyperoxymuriate of potassa in a wine-glass, and the salt be agitated in the acid, there is a very slight effervescence only, the acid becomes of a deep orange tint, and white fumes, mixed with orange fumes, fill the upper part of the glass, which have a very peculiar and not a disagreeable smell.

The *slight* effervescence taking place in this process, induced me to suppose that the substance which coloured the acid must contain a larger proportion of oxygen than euchlorine; for I have shown, in a work published in 1812†, that hyperoxymuriate of potassa contains six proportions of oxygen; and by its decomposition 2·5 volumes of oxygen ought to be evolved for every volume of chlorine; and euchlorine procured from the hyperoxymuriate of potassa by solution of muriatic acid, yields only one volume of oxygen, and two volumes of chlorine.

* From the Philosophical Transactions for 1815, part ii.

† Elem. of Chem. Phil.

I en-

I endeavoured to procure the substance which coloured the sulphuric acid during its action upon hyperoxymuriate of potassa, and after several failures, in which explosions took place, I at length succeeded in the following manner. Dry oxymuriate of potassa in powder was mixed with a small quantity of sulphuric acid, and they were rubbed together with a spatula of platinum till they had incorporated, and formed a solid mass of a bright orange colour. This mass was introduced into a small retort of glass, and exposed to the heat of water which was gradually warmed; a bright yellowish green, elastic fluid arose from the mixture, which was rapidly absorbed by water, giving to it its own tint, but which had no sensible action on mercury.

To make this experiment without danger, not more than 50 or 60 grains of the hyperoxymuriate should be employed, great care should be taken to prevent any combustible matter from being present, and the water should not be permitted to attain a temperature equal to 212°, which may be easily managed by mixing it with alcohol. There are dense white fumes when the mixture is first made, but there seems to be no heat produced; a small quantity of the orange gas is disengaged at this time; but the greater part of it remains attached to the sulphuric acid in the solid mass, and is expelled from it by the heat.

The gas procured by this process over mercury, when compared with the gas procured from the hyperoxymuriate, by liquid muriatic acid, is found to have a much more brilliant colour, is much more rapidly absorbed by water, has a peculiar and much more aromatic smell, unmixed with any smell of chlorine. It destroys moist vegetable blues without previously reddening them. When it is heated to a temperature about that of boiling water, it explodes with more violence than euchlorine, and greater expansion of volume, producing much light. After the explosion over mercury, rather less than three (from 2·7 to 2·9) volumes appear for two of the gas decomposed, and of these, two are oxygen, and the remainder chlorine.

A little chlorine is always absorbed by the mercury during the explosion of the gas; and it appears reasonable to conclude, that the deep yellow gas is in reality composed of two in volume of oxygen, and one of chlorine, condensed into the space of two volumes, and that it consists in weight, of one proportion of chlorine 67, and four of oxygen 60.

None of the combustible bodies which I have tried, decompose this gas at common temperatures, except phosphorus; this when introduced into it occasions an explosion, and burns in the liberated gases with great brilliancy.

Its saturated solution in water is of a deep yellow colour, it does not taste sour, but is extremely astringent and corroding; when

when applied to the tongue, it leaves for a long while a very disagreeable sensation.

It occurred to me that the gas procured from the hyperoxymuriate by the action of liquid muriatic acid, might be a mere mixture of this gas and chlorine; and two in volume of this gas and three in volume of chlorine, would produce by explosion the same products as euchlorine. The only fact which I am acquainted with, opposed to the idea, is the circumstance of Dutch foil not burning spontaneously in the gas from muriatic acid, which might be expected if it contained as much as 3-5ths of uncombined chlorine; though the force of this argument is suspended, till it is supported by an experiment showing that Dutch foil inflames in a mixture of two of the deep-yellow gas, and three of chlorine. I have not yet been able to procure at Rome, metallic foil fitted for this experiment.

I have ascertained that the gas from hyper-oxymuriate and muriatic acid, though it acts much more slowly upon water than the other gas, yet in the end gives it the same tint and properties; and when much of it is exposed to a small quantity of water, it always leaves a residuum of chlorine, so that if it be not a mixture, but a compound, the new gas is formed from it by the action of water.

The action of hydro-nitric acid on the hyper-oxymuriate, affords the same gas as that produced by the action of sulphuric acid, and a much larger quantity of nitric acid may be safely made to act on the salt; but as the gas must be procured by solution of the salt, it is always mixed with about 1-5th of oxygen.

From the solid mixture made with sulphuric acid, I have obtained a gas containing only $\frac{1}{20}$ of oxygen; the fifth proportion obtained in the experiments with nitric acid, being evolved during the time the mixtures were made.

The saturated solution of the gas affords white fumes, similar to those produced at the moment the hydro-sulphuric mixture is made, from which it is probable, that these fumes consist of a hydrate of the gas.

The saturated solution, when mixed with solution of fixed alkalies, or of ammonia, does not immediately lose its colour, nor neutralise the alkalies; but after some time the effect is produced, and hyper-oxymuriates are obtained (probably mixed with a minute quantity of muriates). The solution exposed to air, or suffered to remain in close vessels, becomes soon colourless; and I am inclined to believe that this depends upon a decomposition of water, for some of it exposed to a small quantity of air rather increased its volume.

I shall not propose to give any name to this substance, till it is determined whether euchlorine is a mixture or a definite compound,

compound, and I hope soon to have the means of making a decisive experiment on this subject.

It appears that this new substance, though it contains four proportions of oxygen, is not an acid; and hence it is probable, that the acid fluid compound of oxygen, chlorine, and water, which M. Gay Lussac calls chloric acid, owes its acid powers to combined hydrogen, and that it is analogous to the other hyperoxymuriates, which are triple compounds of inflammable bases, chlorine, and oxygen, in which the base and the chlorine determine the character of the compound. Muriate of potassa (potassane) is a perfectly neutral body; and when six proportions of oxygen are added to it, it still remains neutral. Muriatic acid (chlorine and hydrogen) is a strong acid; and according to the relation above stated, it ought not to lose its acid powers by the addition of six proportions of oxygen. Till a pure combination of chlorine and oxygen is obtained, possessed of acid properties, we have no right to say that chlorine is capable of being acidified by oxygen, and that an acid compound exists in the hyperoxymuriates. We know that chlorine is capable of being converted into an acid by hydrogen, and, as I mentioned in my last paper, where this principle exists its energies ought not to be overlooked; and all the new facts confirm an opinion which I have more than once before submitted to the consideration of the Society, namely, that acidity does not depend upon any *peculiar* elementary substance, but upon *peculiar combinations* of various substances.

Rome, Feb. 15, 1815.

NOTE.

Since my return to England, I have made some further investigations on oxyiodine, on the oxyiodes, and on the deep yellow gas. The portable apparatus which I employed in Italy, enabled me to operate only on very minute quantities of oxyiodine; I have lately made my experiments on a larger scale.

Thirteen grains of oxyiodine decomposed by heat, afforded 9 25 cubical inches of oxygen: and 48 grains of oxypotassame or oxyiode of potassium yielded when decomposed by heat 31 cubical inches of oxygen gas: and 30 grains of potassame or iode of potassium (a portion of the salt so decomposed) afforded by treatment with nitric acid 17·8 grains of dry nitre. These results give the number 246 as the number representing iodine, and prove that oxyiodine consists of one proportion of iodine and five of oxygen; and that the oxyiodes contain six proportions of oxygen.

The deep-yellow gas when mixed with chlorine in the proportion of 2 to 3, or even of 2 to 2, deprives it of the power of acting upon Dutch foil, though one of chlorine when mixed with two of common air still burns this substance. Hence it appears probable, that the deep-coloured gas and chlorine have a chemical action on each other, and that euchlorine is not a simple mixture of them. I hope soon to be able to present to the Society some new results on this subject.

London, June 12, 1815.

LXXX. *New*

LXXX. *New Outlines of Chemical Philosophy.* *By* Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from p. 331.]

From that œconomy which we observe in every part of the creation, it is reasonable to suppose that some general law obtains, in most of the phænomena which appear upon the surface of our globe, and in the atmosphere that surrounds it. The rising vapour, the falling rain, the vivid lightning, and the rolling thunder, are governed by the same laws that rule all the living functions of plants and animals.

Now the earth contains two *elements*, whose joint action on matter causes all those various effects which engage the attention of the chemical philosopher.

An element is an active power which is invisible, imponderable, and untangible, and of which we know nothing, but from the effects which it produces on matter; gravity, magnetism, and electricity, are not the effects of any known cause, and therefore they may be called elements.

The imponderable element of hydrogen gas of Lavoisier, the phlogiston of Scheele and Priestley, and the negative electricity of Franklin, are only different names for the same element.

And the fire-air of Scheele, the dephlogisticated air of Priestley, and the oxygen gas of Lavoisier, are only different names for the same gas, the elementary part of which is the same as the positive electricity of Franklin.

When a Leyden phial is discharged through the air, a spark is elicited producing both light and heat. This well-known experiment seems to establish beyond the reach of doubt, that this effect is produced by the two elements of combustion; positive electricity being the element of heat, and negative electricity that of light.

Whence it must evidently appear, that heat and light are not elements, but the effects of the two elements of combustion, which exist together in the earth and in all other bodies upon its surface. These elements, when thus united in matter, counteract each other, but can be made evident to the senses by several well-known methods. Now fire being produced by friction, collision, percussion, &c. it was supposed that fire existed in bodies in a fixed state, and was, therefore, called *caloric* or *fixed fire*. This hypothesis seems to have arisen from the true theory of combustion being then unknown.

Light and heat are two separate and distinct bodies; for we have light without heat, and heat without light. Animal heat produces

produces no light; and the moon produces abundance of light without any sensible heat.

On the Electricity of the Earth.

It is well known to every electrician, that the electrical machine acts much more powerfully at one time than at another, but the cause of these variations does not seem to be clearly understood. It has been generally supposed, that this is owing to the state of the atmosphere; that the air may be too moist or too dry for electrical experiments; or that the machine may be out of order. But as the Leyden phial cannot be charged unless it have communication with the ground, it may therefore be supposed that the action of the machine is influenced by the electrical state of the earth. Many ingenious instruments have been invented for determining the electricity of the atmosphere, but the electricity of the earth has been much less attended to, and therefore much less understood: this may have been owing to the want of appropriate instruments.

By an extensive series of experiments, I find that the electricity of the earth is as variable as that of the atmosphere; but my experiments are too few (amounting only to about a thousand) to determine the exact quantity of it. I may, however, venture to state, that when the earth is very dry it contains very little electricity, but after heavy showers of rain it is generally found very strongly electrified.

"In Lima, where there is no rain, they never have any lightning or thunder; and, as M. Tournefort was assured, it never rains in the Levant but in winter, and that is the only season in which any thunder is heard."—Phil. Trans. abr. vol. xii. p. 661.

"The mean annual quantity of rain at Granada, Antilles, 12° N. is 126 inches*," (= 10 ft. 6 in.) and if the greater portion of this water rises again into the atmosphere, after the rainy season is over, immense quantities of electricity must be carried up with it. Hence the thunder and lightning, tornadoes, hurricanes, and tempests, are much more dreadful and frequent in the torrid zone than in colder climates, where the quantity of water and electricity which rises into the atmosphere by evaporation, and falls again to the earth in rain, is much less.

On the Electricity of the Atmosphere.

That the atmosphere contains much more moisture and electricity at one time than at another, is well known; and that electricity descends to the earth from the clouds in hail, rain, or snow, has been ascertained by various experiments. But

* Phil. Mag. vol. xliv. p. 350.

how water and electricity ascend into the atmosphere is a meteorological question on which philosophers entertain different opinions.

The electric spark being passed through atmospheric air, light and heat are produced, which shows us the nature of combustion; but when it is passed through water, two gases are formed. The element of heat and water produce oxygen gas, the element of light and water produce hydrogen gas; water being their common basis. Consequently by the attraction between the water upon the surface of the ground and the electricity in the earth, oxygen and hydrogen gases are formed, and become atmospheric air. Large quantities of water and electricity are thus carried up into the atmosphere from the surface of the ground in a short time.

In this case the atmosphere becomes heavier, and the barometer rises; but when a decomposition of the air takes place, it is resolved into its component parts, water and electricity, which descend together to the earth, as mentioned above, the atmosphere becomes lighter and the barometer falls.

But as the quantities of rain, which have been collected at different elevations, differ so widely from one another, a few remarks upon that subject may not be unacceptable to those who are engaged in the study of meteorology.

Of the Rain Gage.

"If a rain gage be placed on the ground," says Dr. Thomson, "and another at some height perpendicularly above it, more rain will be collected into the lower than the higher; a *proof* that the quantity of rain increases as it descends, owing perhaps to the drops attracting vapour during their passage through the lower strata of the atmosphere, where the greatest quantity resides*."

The first account that I have seen of observations made on two rain gages, is that published in the Philosophical Transactions, by Dr. Heberden in 1769.

But from what is stated above by Dr. Thomson, it does not appear that any satisfactory explanation has yet been given of this curious experiment. It does not seem, however, to depend upon any operation of nature but what is very common. When the tops of the rain-gages are placed in an horizontal position, the higher cannot collect so much rain as the lower, when the wind is high, because, it may be presumed, the drops of rain fall more obliquely upon the higher than the lower.

Suppose the wind were so high that the direction of the drops forms an angle of 45 degrees with the top of a rain-gage, it

* Dr. Thomson's Chemistry, vol. iv. p. 84.

would

would then only collect half the quantity of rain it would have done, had the drops fallen perpendicularly.

From this explanation it follows, that the best situation for a rain-gage is near the ground, and where it may be well defended from the wind.

Lynn, Dec. 11, 1815. EZ. WALKER.

[To be continued.]

LXXXI. *On the Nature and Combinations of a newly-discovered vegetable Acid; with Observations on the malic Acid, and Suggestions on the State in which Acids may have previously existed in Vegetables. By M.* DONOVAN, *Esq. Communicated by* W. H. WOLLASTON, *M.D. Sec. R.S.**

HAVING often observed the sour disagreeable taste of the berries of the *Sorbus* (or *Pyrus*) *aucuparia*, it occurred to me that the juice might contain an acid of a peculiar nature, and I resolved to submit it to a strict examination. I was not at that time acquainted with the fact, that these berries had already occupied the attention of Scheele, and that this philosopher had pronounced their acid to be the malic.

Some preliminary experiments showed me that the juice occasioned a precipitation in acetate of lead and nitrate of silver; but it produced no change in lime-water, barytes-water, sulphate of copper, nor, although it might be expected from its very astringent taste, did it affect sulphate of iron.

A quantity of the juice was poured into a solution of sugar of lead; a curdy and somewhat heavy precipitate appeared, which was separated by filtration: this, from its solubility in acetic and dilute nitric acid, I judged to be malate of lead. The filtered liquor was red, and perfectly transparent; but after a few hours I observed a powder deposited at the bottom, and as I saw no sufficient reason for its appearance, it attracted my attention. To the naked eye it looked like a coarse powder; but, when examined by the microscope, proved to be composed of amorphous crystalline grains. With this small quantity of powder I made some trials, which ended in exciting further curiosity. It was probable, that the precipitate which remained on the filter might be made to furnish more grains, and accordingly, after edulcorating the mass, I caused boiling water to pass through it. After two hours I examined the different washings; those made with cold water remained as at first, but those with boiling

* From the Philosophical Transactions for 1815, part ii.

water had become white and turbid, from the suspension of a subtile powder. In several hours after, the cloud had disappeared, and displayed an abundant and beautiful formation of slender prismatic crystals, which glistened with silvery splendour at the bottom of the vessel. The mass which remained on the filter had become hard, was much diminished, and was very dense.

I accounted for the production of crystals by supposing that the precipitate, whatever might be its composition, was insoluble in cold water, but soluble in very large portions of boiling water, and hence the crystalline deposition on cooling. Experiment, however, showed that the residual hard mass, even when levigated, boiled with water, and filtered, would no longer afford crystals on cooling: and this was a sufficient objection to my supposition; for, if one portion of the compound be soluble, why not the whole?

I observed, that even when the precipitate newly obtained was washed with portions of cold water until it no longer altered vegetable colours; yet the first, second, and sometimes the third washing with boiling water would, after the deposition of the crystals, immediately redden litmus. An opinion now suggested itself, which the following trials greatly strengthened.

In order to obtain the acid of the saturnine precipitate, the latter was boiled with dilute sulphuric acid: the liquor became red and transparent, while the sulphate of lead subsided to the bottom. Imagining that I had now obtained the acid in a state of sufficient purity, although coloured with adhering vegetable matter, I thought to separate the sediment by filtration, but was disappointed: for the liquor came through very turbid, notwithstanding that the supernatant liquor had been transparent. It was apparent, therefore, that the sulphuric acid being in too small a quantity, had displaced but a part of the vegetable acid, that the latter dissolved the remaining part of its own combination with lead, and deposited it when the solution touched any thing cold, thus producing the turbidness. After some hours, the turbidness in the filtered liquor subsided, forming a stratum on the bottom of the vessel, over which lay a number of crystals.

The theory of the crystalline formation in the washings of the precipitate now became obvious. It appeared that when the saturnine compound was washed with cold water, no other effect than edulcoration was produced: but that hot water partially decomposed the mass into a super- and a sub-salt, the former of which being soluble in boiling water, filtered through, but on cooling, deposited crystals of the neutral salt, while free acid

acid was left dissolved in the liquor. The first washing contained most free acid, and therefore suffered least of the neutral salt to crystallize.

The red acid liquor, as has been stated, contained much lead, and this it was necessary to separate. The most unexceptionable method appeared to be the transmission of sulphuretted hydrogen through the clear liquor. This was accordingly done, after having heated the acid liquor so as to redissolve the crystals and sediment. The resulting sulphuret of lead was filtered off, and the clear fluid was boiled for a length of time, to expel the superfluous gas.

Supposing now that I had obtained the pure acid, I began to form various conjectures as to its nature; in the midst of which I discovered that the berries of the *Sorbus aucuparia* had already occupied the attention of Scheele, and that he had pronounced their acid to be the malic. There was indeed a great coincidence of properties between the two acids: malic acid is red, when evaporated to dryness it deliquesces, its combinations with potash, soda, and ammonia are deliquescent; such were also the properties of the acid under consideration. Yet I had never understood that malate of lead could be made to afford crystals: an experiment on this head, therefore, became necessary.

The juice of nearly ripe apples was saturated with potash, and the solution when filtered was mixed with solution of acetate of lead: the precipitate was collected on a filter. This after being edulcorated was washed with boiling water, as before. In sixteen hours, crystals, precisely the same as the berries had afforded, were deposited, although less in quantity.

The production of crystals in both these cases seemed to show that the acid of both fruits was the same: yet there was one difference. The precipitate remaining on the filter, after the action of boiling water, was, in that produced from apples, soft and pasty; but that obtained from the *Sorbus* berries was hard and dense. It became therefore necessary, to discover what would be the habitudes of malate of lead obtained by other means.

Scheele showed that the primary action of nitrous acid on sugar is to form malic acid. I therefore heated together equal weights of these substances, until the effervescence ceased. The brown residue was diluted with lime-water, and when the oxalic acid that might have been formed was in this manner separated, the remaining acid substance was saturated with potash. Acetate of lead was added, and the malate of lead thus formed was collected and edulcorated.

It now remained to ascertain whether this malate possessed

 the

the property of crystallizing, like that prepared from apple juice. I accordingly poured on it different portions of boiling water, which were received in different vessels: the washings were all of a brownish-yellow colour, from a small quantity of malate of lead held dissolved. At the end of 48 hours this salt was all deposited in the state of a brown subtile powder, but there was no formation whatever of crystals. On the surface of each washing was an iridescent pellicle of some lustre, which I found to be characteristic of the malic acid. This experiment, as Scheele directs, was made with weak nitric acid; I repeated it with an acid of considerable strength, but after sixty hours there was not one crystal.

It deserves attention, that the matter which remained on the filter in these two experiments, after washing with boiling water, were as soft and pasty as when first collected; whereas the salt of lead obtained from the berries of the *Sorbus* had grown dense, hard, and was much diminished.

The saturnine compound that had been formerly obtained from the berries, when partially decomposed by sulphuric acid, afforded crystals on cooling. In order to ascertain if the malate of lead obtained from sugar would do the same, I repeated the process on it, but obtained no crystals.—These experiments were made under almost every possible circumstance with the same results.

I next precipitated all the lead from the remaining malate: the malic acid thus obtained was again mixed with acetate of lead, and the precipitate treated with boiling water as before: but the results were the same. From this malate of lead the acid was again separated, and the same process as before was four times repeated: but notwithstanding nearly all the foreign matter was thus separated, no crystals formed.

Scheele found that the gooseberry contains nearly equal parts of citric and malic acids. I thought it necessary to examine if this fruit would afford crystals. Scheele's process for separating the acids is as follows: The juice is to be evaporated to the consistence of honey, the pure acid to be separated by alcohol, and the alcohol to be finally evaporated. The residuum is to be dissolved in water, saturated with carbonate of lime; the solution is to be filtered, and boiled so as to separate the citrate of lime. The remaining brown solution is malate of lime: the salt is to be precipitated by alcohol, redissolved after edulcoration, and the solution is to be precipitated with acetate of lead*. All this I followed: but after treating the malate of lead with boiling water, I obtained no crystals.

* Crell's *Chemische Annal.* 1785, vol. ii. 292.

The

The raspberry was found by Scheele to contain the same acids. I examined this fruit in a similar manner, but no crystals were produced.

Vauquelin detected malic acid in a variety of plants, but in none so abundantly as the *Sempervivum Tectorum*. In the juice of this plant it is united to lime in the state of a supersalt*.

Malate of lead was formed by pouring solution of acetate of lead into the filtered juice of this plant. The precipitate, when washed with boiling water, deposited on cooling the same subtile powder as usual, but no crystals. Nor were any obtained when the malate was partially decomposed by sulphuric acid, in the manner already described.

Scheele found that the berries of the *Sambucus nigra* contain the malic acid unmixed with any other. I therefore examined their very mucilaginous juice, but could not produce crystals.

As in my experiments on apples I had obtained crystals, I wished to try what effect the vinous fermentation might have on their juice. The process was repeated with cyder; but I was surprised to find, that although a malate of lead was obtained, the property of forming crystals had been destroyed.

From various conjectures it appeared possible that the berries of the *Sorbus*, while very young, might perhaps contain a malic acid that would furnish few or no crystals. In the beginning of August (1812), the berries being still green, I made an infusion of them, which after filtration afforded a precipitate with acetate of lead. Boiling water produced no crystals, nor did the mass become hard as on former occasions.

Seven varieties of malic acid had now been examined, which when united to lead would not afford crystals. In the two latter cases it was surprising that those acids which in other trials afforded abundance, would now afford none. That the juice of the green berries was real malic acid, was proved by the following properties:

1. The acid, when separated from the juice, was of a crimson colour.
2. When evaporated to dryness, it soon deliquesced.
3. When rendered solid, and acted on by nitrous acid, some crystals of oxalic acid appeared.
4. The acid formed deliquescent salts with potash, soda, and ammonia.
5. Its combinations with lime and lead possessed their proper characteristics.

The acid of very young sour apples was combined with lead, and the compound treated with boiling water: the washings

* *Annales de Chimie*, tome xxxiv. p. 127.

deposited much more crystals than mature apples would have done.

The truth was now manifest. Beside the malic, there is another acid formation, which in different fruits is formed at different periods, and which has hitherto escaped observation: and I consider the preceding details by no means redundant, inasmuch as they estabish one decided difference between the two acids. Many others will shortly be stated.

The first step towards confirming the difference, was to obtain the new acid in a state of purity: and after numerous attempts, I found that the only way to obtain it pure, was to separate it from the crystals. The process is indeed complex and difficult.

The berries of the *Sorbus aucuparia* are to be collected when first they have arrived at maturity. After sufficient bruising in a Wedgwood mortar, they are to be subjected to strong pressure in a linen bag. If collected at the most favourable time, they will afford nearly one half their weight of juice, s. G. 1077. This after due subsidence is to be strained, and mixed with filtered solution of acetate of lead. The precipitate is to be collected on a filter, and in order to separate any uncombined colouring matter, it should be washed with cold water. A very large quantity of boiling water is to be poured on the filter, and allowed to pass through the precipitate into different glass jars. After some hours, the washings become opaque, and at length deposit crystals of singular lustre and beauty. Those which have been formed in the colourless washings are to be alone retained; they are to be separated by the filter, dried in the air, and preserved for a further process.

The original mass remaining on the filter, from which the crystals have been obtained, being now hard and brittle, is incapable of affording any more, without undergoing a new operation. It is to be boiled for half an hour with a little more dilute sulphuric acid than is sufficient to decompose the salt; when cold it is to be filtered. The filtered liquor is to be mixed a second time with acetate of lead; the precipitate washed, as before, with boiling water, and the crystals selected from the colourless washings only. The remaining mass again grown hard, is to undergo the process of decomposition with sulphuric acid, combination with lead, and the formation of crystals: and after all, it will be found that the crystals of all the processes will be inconsiderable when collected.

The whole of the crystalline product being dried, is to be boiled for half an hour with 2·3 times its weight of sulphuric acid, s. G. 1090, supplying water as fast as it evaporates, and taking care to keep the mixture constantly stirred with a glass rod. The clear liquor is to be filtered off, and poured into a tall

tall glass jar of small diameter. While still hot, a stream of sulphuretted hydrogen is to be transmitted through it; and when all the lead has been precipitated, the fluid is to be filtered off, and boiled in an open basin, until the discharged vapour no longer blacken paper impregnated with acetate of lead.

The theory of the process is obvious. When acetate of lead is added to the juice, malate of lead and the combination of the new acid with lead precipitate; the latter is decomposed by boiling water into a super- and a sub-salt: the super-salt is held in solution; but as the liquor cools, the neutral compound deposits itself in crystals, and the first washings contain most free acid. When boiling water is no longer able to overcome the attraction of the latter portions of acid to oxide of lead, no more crystals can be formed. We then apply the stronger power of sulphuric acid, we obtain the free vegetable acid, and proceed as at first. When all the crystals are collected, such a quantity of sulphuric acid is added as will be *nearly* sufficient to decompose them: this is so done in order completely to exclude the sulphuric acid, which without this precaution would be exceedingly difficult to effect. The undecomposed portion of the crystals dissolves in the vegetable acid newly extricated: but if in the boiling the fluid were not continually stirred, a mass would be formed in the bottom so hard as to resist decomposition. If the liquor after filtering were allowed to cool, the neutral salt would crystallize; it must therefore be used hot. The stream of sulphuretted hydrogen passing through so high a column of fluid soon separates the lead, while the pure vegetable acid is liberated, contaminated indeed with a little sulphuretted hydrogen. This gas does not disappear completely by boiling, for the acid retains the odour, be it ever so long boiled; exposure for a few days in an open vessel dissipates it completely.

In preparing this acid, it is not necessary, as it is in the process for malic acid, to saturate the juice of the berries with potash, at the commencement of the process; for, of the two compounds formed after the mixture with acetate of lead, the malate dissolves in the evolved acetic acid in preference to the other. The colouring matter, which adheres obstinately to the malate of lead, is very apt, when extricated during the washing with boiling water, to tinge the otherwise perfectly colourless crystals, which form as the liquor cools. This is a great inconvenience, for the colouring matter cannot be washed away, even by cold water, without decomposing a quantity of the salt: hence the only remedy is, to reject all the crystals formed in the coloured washings, and to reserve those only that are of a pure whiteness. The crystals are of so delicate a fabric that they must be separated by the filter. When dried on paper, by exposure to air, they form a

white

white brilliant flake of a silver lustre, resembling well-prepared acetate of mercury, but still more beautiful.

As to the amorphous crystalline grains which first attracted my attention, the following experiment elucidates the theory of their formation. A quantity of the pure acid obtained by the above process, was boiled for some time on an excess of carbonate of magnesia. The liquor after filtration was found to restore the original colour of reddened litmus, and to render green the tincture of cabbage. An acetate of lead was formed by boiling solution of super-acetate on carbonate of lead. This solution was mixed with the former, and the precipitate was collected by the filter.

Notwithstanding the evident excess of magnesia existing in one solution, and the necessary neutrality of the other, yet the filtered liquor was found strongly to redden litmus paper. We are not to suppose that the two salts evolved a free acid during their mutual decomposition. As much oxide of lead was liberated from the acetate, as was necessary to the neutrality of the acid eliminated from the compound with magnesia; the solution would therefore have retained all its ingredients in a state of neutrality, except that which originally contained an excess of magnesia. But the new salt of lead, at the moment of its formation, was decomposed by the water present into a super- and a sub-salt; the excess of acid being not only sufficient to saturate the redundant magnesia, but also to leave a portion free in the solution. This liquor, after an hour, deposited a quantity of crystalline grains, and after that, the acidity increased. Hence appears the reason of a crystallization in the original liquor: a super-salt is formed, which after a while deposits the neutral salt in a crystalline form.

This acid appearing, from what has been already stated as well as from what will be hereafter detailed, to be of a peculiar nature, it became necessary to give it a name. After some consideration I bestowed on it one, which, although not unexceptionable, is sufficiently accordant with the general analogy of chemical nomenclature, and which has received the approbation of some competent judges. Until a better name be devised, I have called it the Sorbic acid.

To establish its peculiar nature, I have examined its combinations with certain bases, but have confined myself to those of which the analogous combinations amongst the malates had been already examined by Scheele. The sorbic and malic acids not having been distinguished by that philosopher, it seemed that here the distinction ought particularly to be established: and the standard of comparison must necessarily be whatever had been ascertained of the malates by their discoverer.

Sorbic

Sorbic acid, when perfectly pure, is a transparent, colourless, and inodorous fluid, soluble in alcohol, and in any proportion of water. When evaporated, it forms an uncrystallizable solid mass which deliquesces: when subjected to distillation, the liquor which passes over shows no traces of acidity. Its acidity is such that it causes even a painful sensation on the organs of taste. It is not much altered by being kept in an uncombined state. I have had it for more than a year in a corked phial, and at the end of that time no other change was produced than the separation of a tenuous coagulum, small in quantity, as the acid was very pure, but it is more abundant when the acid is impure. When mixed with malic acid, as in fruits, this acid is the first to disappear, while the other retains its properties long after the commencement of decay in the plant.

A quantity of malate of lead obtained from *Sempervivum Tectorum* was boiled with sorbic acid and a little water; the whole from being colourless became somewhat brown. The liquor was then filtered, and the turbid liquor which came through was heated until it became clear; it was then suffered to rest. As it cooled, it let fall a powder; but when this was filtered off the liquor remained clear, and in an hour a great profusion of crystals was let fall. The mass which remained on the filter contained some gritty particles.

Thus it is evident that malate of lead was decomposed by sorbic acid, which could not happen unless the latter were a distinct substance. The malate was only partly decomposed, the oxide of lead united to the excess of sorbic acid forming super-sorbate of lead, while the disengaged malic acid dissolved as much as it could of the remaining undecomposed malate, forming super-malate of lead. The brown colour was produced by disengaged malic acid. The super-malate, as it cooled, deposited its malate in the state of powder, and the super-sorbate soon after deposited sorbate of lead in the state of crystals; and the original mass was found to contain gritty particles of sub-sorbate.

In the same manner, when a precipitate obtained by acetate of lead from the juice of the *Sorbus* berries is washed with boiling water, scarcely any malate of lead is deposited; and if the fluid contain much free sorbic acid, the iridescent pellicle, which is a characteristic of malate of lead, does not appear.

I shall now proceed to the combinations of this acid, so as to distinguish it from malic acid: and first the salts which it forms with lead should be briefly recapitulated.

The sub-sorbate is insoluble in water; if in a mass, it is dense and hard: if in powder, it is gritty.

The neutral sorbate, if obtained by precipitation, is a white powder,

powder, but if obtained from solution in its own acid, it is in beautiful silvery crystals. Neither of these salts is soluble in 5000 times its weight of water. The sorbate when heated to redness undergoes a somewhat brilliant combustion.

The super-sorbate never assumes the solid form; its taste is sweet. Thus the sorbic acid forms three combinations with lead: malic forms but two; the neutral malate, which is an uncrystallizable soft powder, and the super-malate. Not less distinguishable are the two acids by their combinations with the alkalies.

Sorbate of potash, when there is an excess of acid, forms permanent crystals soluble in water, but insoluble in alcohol.

Sorbate of soda, when there is an excess of acid, forms permanent crystals, which agree in characters with the former.

Sorbate of ammonia, when there is an excess of acid, also forms permanent crystals of similar characters with the preceding.

These three salts will not crystallize unless there be a tolerable excess of acid; they are to be considered as super-salts. That of soda even requires the aid of cold to render it solid. The malates of potash, soda, and ammonia are known to be uncrystallizable and deliquescent.

In the combinations of these acids with earths, there are also striking differences. Scheele found, that when he added carbonate of lime to malic acid, a great quantity was dissolved, but the solution gave with litmus indications of an abundant acid which it was impossible to neutralize with further additions of chalk*. I obtained the same result with malic acid from the *Sempervivum;* I even found that the solution might be boiled to dryness, on a fresh portion of carbonate of lime; yet when lixiviated, the filtered solution would still redden litmus, and the salt finally afforded was readily soluble. These results often obtained, prove that it is not possible to form neutral malate from carbonate of lime.

But with sorbic acid the case was quite different. When it was diluted, and agitated for a little while with carbonate of lime, the solution, before it could be filtered, deposited the principal part of the sorbate in the form of a discrete, gritty powder. The liquor when filtered produced no redness in tincture of litmus, and every thing proved that the fluid by mere agitation over the carbonate had been completely neutralized.

The same results which Scheele obtained from lime were afforded by carbonate of barytes: but with sorbic acid I produced a liquor which showed no signs of acidity. The best test for

* Scheele, *Chem. Annal.* 1785, ii. 292.

ascertaining this fact, seemed to be infusion of brazil wood altered by distilled vinegar; and with this it even appeared that the solution contained an excess of base.

Thus it appears, that malic acid never forms with carbonate of lime any other than *acidulous salts;* and, as Scheele observes, these solutions in *some days* deposit the neutral salt in *crystals*. But with these carbonates the sorbic acid forms *neutral salts*, which, *as soon as formed*, precipitate.

Scheele ascertained that malate of magnesia is a deliquescent salt*, and in my trials I could not obtain it in a crystalline form. When evaporated, it became thick, and dried into a semitransparent substance, which softened with the smallest quantity of water, and formed matter of a syrupy consistence. The same earth, heated in sorbic acid, afforded a liquor which, after filtration, deposited permanent crystals in abundance: they required for solution no less than 28 parts of water at 60.†

The malate of alumina was found by Scheele to be a salt very difficult of solution. I wished to discover the properties of the sorbate. I therefore boiled some very pure alumina that had been just prepared, and was therefore still soft, with sorbic acid: the boiling was continued for almost an hour, and after filtration, I discovered with no small surprise that the alumina had not been acted upon. The acid was tried by every means, and nothing but the most minute vestiges of the earth could be obtained. Thus there is no sorbate of alumina.

I consider that from this property the sorbic acid may become a valuable instrument of analysis. The process for separating alumina from other earths has been complicated and uncertain: may it not be rendered simple by the use of this acid, employed in excess?

Thus, I think there can be no doubt that the sorbic acid is an acid *sui generis*, and probably intermediate between malic and oxalic. With regard to the other acids, with which the sorbic coexists in fruits, it is to be observed, that it is never found in mature fruits that contain any other than the malic; that the latter is never found alone in any mature fruit, but always accompanied by the sorbic; and that these two acids, when together, exclude all others. To this, however, there is an apparent exception, namely, the berry of the *Sambucus nigra*, which (probably from the immense quantity of mucilage and colouring matter present) afforded me no sorbic acid. The fruits that

* Crell's *Chem. Annal.* 1785. ii. 297.

† It deserves remark, that in Scheele's experiments there could have been no sorbic acid present, as might have been expected, had he prepared his acid from apples: he obtained it from gooseberries, and thereby avoided this source of fallacy.

contain

contain the sorbic and malic acids together are apples, plums, berries of the *Sorbus*, barberries, and sloes. Of these, the berries of the *Sorbus* contain the greatest quantity of sorbic acid, unripe apples less, ripe apples and sloes still less, barberries very little, and plums least of all. The green berries of the *Sorbus*, (perhaps,) those of the *Sambucus*, and the plant *Sempervivum Tectorum*, contain no other than the malic; and agreeing with the foregoing statements, raspberries and gooseberries, as they contain citric and malic acids, contain no sorbic whatever.

[To be continued.]

LXXXII. *On the Fire-damp of Coal-mines, and on Methods of lighting the Mines so as to prevent its Explosion.* By *Sir* H. Davy, *LL.D. F.R.S. V.P.R.I.**

The accidents arising from the explosion of the fire-damp or inflammable gas of coal mines, mixed with atmospherical air, are annually becoming more frequent and more destructive in the collieries in the North of England.

A committee has been for some time formed at Sunderland for the benevolent purpose of investigating the causes of these accidents, and of searching for means of preventing them. In consequence of an invitation from the Rev. Dr. Gray, one of the most active members of this committee, I was induced to turn my attention to the subject. I went to the North of England, and visited some of the principal collieries in the neighbourhood of Newcastle, for the purpose of ascertaining the condition of the workings, and the state of their ventilation. I found the greatest desire to assist my inquiries in the gentlemen acquainted with the northern collieries, as well as in the inspectors or viewers of the mines: and I have particular obligations on this point to the Rev. Dr. Gray, Cuthbert Ellison, Esq. M.P., the Rev. John Hodgson, Mr. Buddle, and Mr. Dunn. Dr. Fenwick, Dr. Clanny, and Mr. Fenwick, likewise kindly offered me their assistance.

From the information which I collected on the spot, increased by the perusal of a Report of Mr. Buddle on the state of the mines, I was convinced that, as far as ventilation was concerned,

* From Phil. Trans Part II. for 1815, now in the press.—We have not deviated from our usual practice, without permission, in giving this interesting paper before the usual period. In fact, the Royal Society, with its usual liberality, waved its restriction in regard to Sir H. Davy's communication, that the public might the sooner be made generally acquainted with and enjoy the advantages afforded by this most valuable contrivance.

the

the resources of modern science had been fully employed; and that a mode of preventing accidents was only to be sought for in a method of lighting the mines free from danger, and which, by indicating the state of the air in the part of the mine where inflammable air was disengaged, so as to render the atmosphere explosive, should oblige the miners to retire till the workings were properly cleared.

An account of an ingenious apparatus for burning a candle supplied with atmospherical air by a bellows through water, has been published in the Philosophical Transactions by Dr. Clanny; but I believe this apparatus has not yet been used in any of the collieries.

The common means employed for lighting those parts of the mine where danger is apprehended from the fire-damp, is by a steel wheel, which being made to revolve in contact with flint, affords a succession of sparks: but this apparatus always requires a person to work it; and, though much less liable to explode the fire-damp than a common candle, yet it is said to be not entirely free from danger.

Mr. Buddle having obligingly shown to me the degree of light required for working the collieries, I made several experiments, with the hope of producing such a degree of light, without active inflammation; I tried Kunckel's, Canton's, and Baldwin's phosphorus, and likewise the electrical light in close vessels, but without success; and even had these degrees of light been sufficient, the processes for obtaining them, I found, would be too complicated and difficult for the miners.

The fire-damp has been shown by Dr. Henry, in a very ingenious paper published in the nineteenth volume of Nicholson's Journal, to be light carburetted hydrogen gas, and Dr. Thomson has made some experiments upon it; but the degree of its combustibilty, as compared with that of other inflammable gases, has not, I believe, been examined, nor have many different specimens of it been analysed; and it appeared to me, that some minute chemical experiments on its properties ought to be the preliminary steps to inquiries respecting methods of preventing its explosion. I therefore procured various specimens of the fire-damp in its purest state, and made a number of experiments upon it. And in examining its relations to combustion I was so fortunate as to discover some properties belonging to it, which appear to lead to very simple methods of lighting the mines, without danger to the miners, and which, I hope, will supply the desideratum so long anxiously required by humanity. I shall in the following pages have the honour of describing these properties, and the methods founded upon them, to the Royal Society, and I shall conclude with some general observations.

The

The fire-damp is produced in small quantities in coal mines, during the common process of working.

The Rev. Mr. Hodgson informed me, that on pounding some common Newcastle coal fresh from the mine in a cask furnished with a small aperture, the gas from the aperture was inflammable. And on breaking some large lumps of coal under water, I ascertained that they gave off inflammable gas*. Gas is likewise disengaged from bituminous schist, when it is worked.

The great sources of the fire-damp in mines are, however, what are called blowers, or fissures in the broken strata, near dykes, from which currents of fire-damp issue in considerable quantity, and sometimes for a long course of years†. When old workings are broken into, likewise, they are often found filled with fire-damp; and the deeper the mine the more common in general is this substance.

I have analysed several specimens of the fire-damp in the laboratory of the Royal Institution; the pure inflammable part was the same in all of them, but it was sometimes mixed with small quantities of atmospherical air, and in some instances with azote and carbonic acid.

Of six specimens collected by Mr. Dunn from a blower in the Hepburn Colliery, by emptying bottles of water close to it, the purest contained $\frac{1}{15}$ only of atmospherical air, with no other contamination, and the most impure contained $\frac{5}{12}$ of atmospherical air; so that this air was probably derived from the circumambient air of the mine. The weight of the purest speci men was for 100 cubical inches 19·5 grains.

* This is probably owing to the coal strata having been formed under a pressure greater than that of the atmosphere, so that they give off elastic fluid when they are exposed to the free atmosphere: and probably coals containing animal remains evolve not only the fire-damp, but likewise azote and carbonic acid, as in the instance of the gas sent by Dr. Clanny.

In the Apennines, near Pietra Mala, I examined a fire produced by gaseous matter, constantly disengaged from a schist stratum: and from the results of the combustion, I have no doubt but that it was pure fire-damp. Mr. M. Faraday, who accompanied me, and assisted me in my chemical experiments, in my journey, collected some gas from a cavity in the earth about a mile from Pietra Mala, then filled with water, and which, from the quantity of gas disengaged, is called Aqua Buja. I analysed it in the Grand Duke's laboratory at Florence, and found that it was pure light hydro-carbonate, requiring two volumes of oxygen for its combustion, and producing a volume of carbonic acid gas.

It it very probable that these gases are disengaged from coal strata beneath the surface, or from bituminous schist above coal; and at some future period new sources of riches may be opened to Tuscany from this invaluable mineral treasure, the use of which in this country has supplied such extraordinary resources to industry.

† Sir James Lowther found a uniform current produced in one of his mines for two years and nine months. Phil. Trans. vol. xxxviii. p. 112.

One

One measure of it required for its complete combustion by the electric spark nearly two measures of oxygen, and they formed nearly one measure of carbonic acid.

Sulphur heated strongly, and repeatedly sublimed in a portion of it freed from oxygen by phosphorus, produced a considerable enlargement of its volume, sulphuretted hydrogen was formed, and charcoal precipitated; and it was found that the volume of the sulphuretted hydrogen produced, when it was absorbed by solution of potassa, was exactly double that of the fire-damp decomposed.

It did not act upon chlorine in the cold; but, when an electric spark was passed through a mixture of one part of it with two of chlorine, there was an explosion, with a diminution to less than 1-4th, and much charcoal was deposited.

The analysis of specimens of gas sent to my friend John George Children, Esq. by Dr. Clanny, afforded me similar results; but they contained variable quantities of carbonic acid gas and azote.

Different specimens of these gases were tried by the test of exposure to chlorine both in darkness and light: they exhibited no marks of the presence of olefiant gas or hydrogen; and the residuum produced by detonation with chlorine showed them to be free from carbonic oxide.

It is evident, then, that the opinion formed by other chemists respecting the fire-damp is perfectly correct; and that it is the same substance as the inflammable gas of marshes, the exact chemical nature of which was first demonstrated by Mr. Dalton; and that it consists, according to my view of definite proportions, of 4 proportions of hydrogen in weight 4, and one proportion of charcoal in weight 11·5.

I made several experiments on the combustibility and explosive nature of the fire-damp. When 1 part of fire-damp was mixed with 1 of air, they burnt by the approach of a lighted taper, but did not explode; 2 of air and 3 of air to 1 of gas produced similar results. When 4 of air and 1 of gas were exposed to a lighted candle, the mixture being in the quantity of 6 or 7 cubical inches in a narrow-necked bottle, a flame descended through the mixture, but there was no noise: 1 part of gas inflamed with 6 parts of air in a similar bottle, produced a slight whistling sound: 1 part of gas with 8 parts of air, rather a louder sound: 1 part with 10, 11, 12, 13 and 14 parts, still inflamed, but the violence of combustion diminished. In 1 part of gas and 15 parts of air, the candle burnt without explosion with a greatly enlarged flame; and the effect of enlarging the flame, but in a gradually diminishing ratio, was produced as far as 30 parts of air to 1 of gas.

The

The mixture which seemed to possess the greatest explosive power, was that of 7 or 8 parts of air to 1 of gas; but the report produced by 50 cubical inches of this mixture was less than that produced by $\frac{1}{10}$ of the quantity of a mixture of 2 parts of atmospherical air and 1 of hydrogen.

It was very important to ascertain the degree of heat required to explode the fire-damp mixed with its proper proportion of air.

I found that a common electrical spark would not explode 5 parts of air and 1 of fire-damp, though it exploded 6 parts of air and 1 of damp: but very strong sparks from the discharge of a Leyden jar seemed to have the same power of exploding different mixtures of the gas as the flame of the taper. Well-burned charcoal, ignited to the strongest red heat, did not explode any mixture of air and of the fire-damp; and a fire made of well-burned charcoal, *i.e.* charcoal that burned without flame, was blown up to whiteness by an explosive mixture containing the fire-damp, without producing its inflammation. An iron rod at the highest degree of red heat, and at the common degree of white heat, did not inflame explosive mixtures of the fire-damp; but, when in brilliant combustion, it produced the effect.

The flame of gaseous oxide of carbon as well as of olefiant gas exploded the mixtures of the fire-damp.

In respect of combustibility, then, the fire-damp differs most materially from the other common inflammable gases. Olefiant gas, which I have found explodes mixed in the same proportion with air, is fired by both charcoal and iron heated to dull redness. Gaseous oxide of carbon, which explodes when mixed with 2 parts of air, is likewise inflammable by red-hot iron and charcoal. And hydrogen, which explodes when mixed with 3-7ths of its volume of air, takes fire at the lowest visible heat of iron and charcoal; and the case is the same with sulphuretted hydrogen.

I endeavoured to ascertain the degree of expansion of mixtures of fire-damp and air during their explosion, and likewise their power of communicating flame through apertures to other explosive mixtures.

I found that when 6 of air and 1 of fire-damp were exploded over water by a strong electrical spark, the explosion was not very strong, and, at the moment of the greatest expansion, the volume of the gas did not appear to be increased more than $\frac{1}{2}$.

In exploding a mixture of 1 part of gas from the distillation of coal, and 8 parts of air in a tube of a quarter of an inch in diameter and a foot long, more than a second was required before the flame reached from one end of the tube to the other; and

and I could not make any mixture explode in a glass tube 1-7th of an inch in diameter: and this gas was more inflammable than the fire-damp, as it consisted of carburetted hydrogen gas mixed with some olefiant gas.

In exploding mixtures of fire-damp and air in a jar connected with the atmosphere by an aperture of half an inch, and connected with a bladder by a stopcock, having an aperture of about 1-6th of an inch*, I found that the flame passed into the atmosphere, but did not communicate through the stopcock, so as to inflame the mixture in the bladder: and in comparing the power of tubes of metal and those of glass, it appeared that the flame passed more readily through glass tubes of the same diameter; and that explosions were stopped by metallic tubes of 1-5th of an inch†, when they were $1\frac{1}{2}$ inch long; and this phænomenon probably depends upon the heat lost during the explosion in contact with so great a cooling surface, which brings the temperature of the first portions exploded below that required for the firing of the other portions. Metal is a better conductor of heat than glass: and it has been already shown that the fire-damp requires a very strong heat for its inflammation.

Mixture of the gas with air I found, likewise, would not explode in metallic canals or troughs, when their diameter was less than the 1-7th of an inch, and their depth considerable in proportion to their diameter; nor could explosions be made to pass through such canals.

Explosions likewise I found would not pass through very fine wire sieves or wire gauze.

I mixed azote and carbonic acid in different quantities with explosive mixtures of fire-damp, and I found that even in very small proportions they diminished the velocity of the inflammation. Azote, when mixed in the proportion of 1 to 6 of an explosive mixture, containing 12 of air and 1 of fire-damp, deprived it of its power of explosion; when 1 part of azote was mixed with 7 of an explosive mixture, only a feeble blue flame slowly passed through the mixture.

1 part of carbonic acid to 7 of an explosive mixture deprived it of the power of exploding; so that its effects are more remarkable than those of azote; probably, in consequence of its

* Since these experiments were made, Dr. Wollaston has informed me, that he and Mr. Tennant had observed some time ago, that mixtures of the gas from the distillation of coal and air would not explode in very small tubes.

† I do not give this result as perfectly exact, as the bore of the metallic tube had not the same polish as that of the tube of glass.

greater capacity for heat, and probably, likewise, of a higher conducting power connected with its greater density.

The consideration of these various facts led me to adopt a form of a lamp, in which the flame by being supplied with only a limited quantity of air, should produce such a quantity of azote and carbonic acid as to prevent the explosion of the fire damp, and which, by the nature of its apertures for giving admittance and exit to the air, should be rendered incapable of communicating any explosion to the external air.

If in a close lantern, supplied with a small aperture below and another above, a lighted lamp having a very small wick be placed, the natural flame gradually diminishes, till it arrives at a point at which the supply of air is sufficient for the combustion of a certain small quantity of oil; if a lighted taper be introduced into the lantern through a small door in the side, which is instantly closed, both lights will burn for a few seconds, and be extinguished together.

A similar phænomenon occurs, if, in a close lantern, supplied with a quantity of air merely sufficient to support a certain flame, a mixture of fire-damp and air is gradually admitted: the first effect of the fire-damp is to produce a larger flame round that of the lamp, and this flame, consuming the oxygen which ought to be supplied to the flame of the lamp, and the standard of the power of the air to support flame being lowered by the admixture of fire-damp and by its rarefaction, both the flame of the fire-damp and that of the taper are extinguished together; and as the air contained a certain quantity of azote and carbonic acid before the admission of the fire-damp, their effect, by mixing with it, is such as to prevent an explosion in any part of the lantern.

I tried several experiments on the burning of a flame in atmospheres containing fire-damp. I inclosed a taper in a little close lantern, having a small aperture below and a larger one above, of such size that the taper burnt with a flame a little below its natural size. I placed this lantern, the taper being lighted, on a stand under a large glass receiver standing in water, having a curved tube containing a little water, adapted to its top to confine the air, and which was of such a capacity as to enable the candle to burn for some minutes; I then rapidly threw a quantity of fire-damp into the receiver from a bladder, so as to make the atmosphere in it explosive. As the fire-damp mixed with the air, the flame of the taper gradually enlarged, till it half filled the lantern; it then rapidly diminished, and was suddenly extinguished without the slightest explosion. I examined the air of the receiver after the experiment, and found it highly explosive.

I tried

I tried similar experiments, throwing in mixtures of air and fire-damp, some containing the maximum and others the minimum of fire-damp necessary for explosion, and always with the same satisfactory results. The flame considerably increased, and was soon extinguished.

I introduced a lighted lantern to which air was supplied by two glass tubes $\frac{1}{10}$ of an inch in diameter and half an inch long, into a large jar containing an explosive mixture of 1 part of fire-damp and 10 parts of air; the taper burnt at first with a feeble light, the flame soon became enlarged, and was then extinguished. I repeated these experiments several times, and with a perfect constancy of result.

It is evident, then, that to prevent explosions in coal mines, it is only necessary to use air-tight lanterns, supplied with air from tubes or canals of small diameter, or from apertures covered with wire gauze placed below the flame, through which explosions cannot be communicated, and having a chimney at the upper part, on a similar system for carrying off the foul air; and common lanterns may be easily adapted to the purpose, by being made air-tight in the door and sides, by being furnished with the chimney, and the system of safety apertures below and above.

The principle being known, it is easy to adopt and multiply practical applications of it.

The first safe-lantern that I had constructed was made of tin-plate, and the light emitted through four glass plates in the sides. The air was admitted round the bottom of the flame from a number of metallic tubes of $\frac{1}{8}$ of an inch in diameter, and an inch and $\frac{1}{2}$ long. The chimney was composed of two open cones, having a common base perforated with many small apertures, and fastened to the top of the lantern, which was made tight in a pneumatic rim containing a little oil; the upper and lower apertures in the chimney were about $\frac{1}{3}$ of an inch: the lamp, which was fed with oil, gave a steady flame of about an inch high and half an inch in diameter. When the lantern was slowly moved, the lamp continued to burn, but more feebly; and when it was rapidly moved, it went out. To obviate this circumstance, I surrounded the bottom of the lantern with a perforated rim; and this arrangement perfectly answered the end proposed.

I had another chimney fitted to this lantern, furnished with a number of safety tin plate tubes of the sixth of an inch in diameter and two inches long: but they diminished considerably the size of the flame, and rendered it more liable to go out by motion; and the following experiments appear to show, that if the diameter of the upper orifice of the chimney be not very

 large,

large, it is scarcely possible that any explosion produced by the flame can reach it.

I threw into the safe-lantern with the common chimney, a mixture of 15 parts of air and 1 of fire-damp: the flame was immediately greatly enlarged, and the flame of the wick seemed to be lost in the larger flame of the fire-damp. I placed a lighted taper above the orifice of the chimney: it was immediately extinguished, but without the slightest previous increase of its flame, and even the wick instantly lost its fire by being plunged into the chimney.

I introduced a lighted taper into a close vessel containing 15 parts of air and 1 of gas from the distillation of coal, suffered it to burn out in the vessel, and then analysed the gas. After the carbonic acid was separated, it appeared by the test of nitrous gas to contain nearly $\frac{1}{5}$ of its original quantity of oxygen; but detonation with a mixture of equal parts of hydrogen and oxygen proved that it contained no sensible quantity of carburetted hydrogen gas.

It is evident, then, that when in the safe-lantern the air gradually becomes contaminated with fire-damp, this fire-damp will be consumed in the body of the lantern; and that the air passing through the chimney cannot contain any inflammable mixture.

I made a direct experiment on this point. I gradually threw an explosive mixture of fire-damp and air into the safe-lantern from a bladder furnished with a tube which opened by a large aperture above the flame; the flame became enlarged, and by a rapid jet of gas I produced an explosion in the body of the lantern; there was not even a current of air through the safety tubes at the moment, and the flame did not appear to reach above the lower aperture of the chimney; and the explosion merely threw out from it a gust of foul air.

The second safety-lantern that I have had made is upon the same principle as the first, except that instead of tubes, *safety canals* are used, which consist of close concentric hollow metallic cylinders of different diameters, and placed together so as to form circular canals of the diameter of from $\frac{1}{25}$ to $\frac{1}{40}$ of an inch, and an inch and $\frac{7}{10}$ long, by which air is admitted in much larger quantities than by the small tubes. In this arrangement there is so free a circulation of air, that the chimney likewise may be furnished with safety canals.

I have had lamps made for this kind of lantern which stand on the outside, and which may be supplied with oil and cotton without any necessity of opening the lantern; and in this case the chimney is soldered to the top, and the lamp is screwed into the bottom, and the wick rises above the air canals.

I have likewise had glass lamps with a single wick, and Argand

gand lamps made on the same principle, the chimney being of glass covered with a metallic top containing the safety canals, and the air entering close to the flame through the circular canals.

The third kind of safe lamp or lantern, and which is by far the most simple, is a close lamp or lantern into which the air is admitted, and from which it passes, through apertures covered with *brass wire gauze* of $\frac{1}{200}$ of an inch in thickness, the apertures of which should not be more than $\frac{1}{120}$ of an inch; this stops explosions as well as long tubes or canals, and yet admits of a free draught of air.

Having succeeded in the construction of safe-lanterns and lamps, equally portable with common lanterns and lamps, which afforded sufficient light, and which bore motion perfectly well, I submitted them individually to practical tests, by throwing into them explosive atmospheres of fire-damp and air. By the natural action of the flame drawing air through the air canals, from the explosive atmosphere, the light was uniformly extinguished; and when an explosive mixture was forcibly pressed into the body of the lamp, the explosion was always stopped by the safety apertures, which may be said figuratively to act as a sort of *chemical fire sieves* in separating flame from air. But I was not contented with these trials, and I submitted the safety-canals, tubes, and wire gauze fire-sieves, to much more severe tests: I made them the medium of communication between a large glass vessel filled with the strongest explosive mixture of carburetted hydrogen and air, and a bladder $\frac{2}{3}$ or $\frac{1}{2}$ full of the same mixture, both insulated from the atmosphere. By means of wires passing near the stop-cock of the glass vessel, I fired the explosive mixture in it by the discharge of a Leyden jar. The bladder always expanded at the moment the explosion was made; a contraction as rapidly took place; and a lambent flame played round the mouths of the safety apertures, open in the glass vessel; but the mixture in the bladder did not explode: and by pressing some of it into the glass vessel, so as to make it replace the foul air, and subjecting it to the electric spark, repeated explosions were produced, proving the perfect security of the safety apertures; even when acted on by a much more powerful explosion than could possibly occur from the introduction of air from the mines.

These experiments held good, whatever were the proportions of the explosive mixture and whatever was the size of the glass vessel, (no one was ever used containing more than a quart,) provided as many as 12 metallic tubes were used of $\frac{1}{7}$ of an inch in diameter, and $2\frac{1}{2}$ inches long; or provided the circular metallic canals were $\frac{1}{25}$ of an inch in diameter, $1\cdot\frac{1}{7}$ of an inch

 deep,

deep, and at least two inches in circumference; or provided the wire gauze had apertures of only $\frac{1}{120}$ of an inch. When twelve metallic tubes were employed as the medium of communication, $\frac{1}{7}$ of an inch in diameter and an inch long, the explosion was communicated by them into the bladder. Four glass tubes of the $\frac{1}{16}$ of an inch in diameter and 2 inches long, did not communicate the explosion; but *one* of this diameter and length produced the effect. The explosion was stopped by a single tube $\frac{1}{26}$* of an inch in diameter, when it was three inches long, but not when it was two inches long.

The explosion was stopped by the metallic gauze of $\frac{1}{120}$ when it was placed between the exploding vessel and the bladder, though it did not present a surface of more than half a square inch, and the explosive mixture in the bladder in passing through it to supply the vacuum produced in the glass vessel, burnt on the surface exposed to the glass vessel for some seconds, producing a murmuring noise.

A circular canal $\frac{1}{25}$ of an inch in diameter, and an inch and a half in circumference, and $1\frac{7}{10}$ of an inch deep, communicated explosion; but four concentric canals, of the same depth and diameter, and of which the smallest was two inches in diameter, and separated from each other only by their sides, which were of brass, and about $\frac{1}{40}$ of an inch in thickness, did not suffer the explosion to act through them.

It would appear then, that the smaller the circumference of the canal, that is, the nearer it approaches to a tube, the greater must be its depth, or the less its diameter to render it safe.

I did not perceive any difference in these experiments, when the metals of the apertures were warmed by repeated explosions: it is probable, however, that considerable elevation of temperature would increase the power of the aperture to pass the explosion; but the difference between the temperature of flame, and that marked on our common mercurial scale, is so great, that the addition of a few degrees of heat probably does not diminish perceptibly the cooling power of a metallic surface, with regard to flame.

By diminishing the diameter of the air canals, their power of passing the explosion is so much diminished that their depth and circumference may be brought extremely low. I found that flame would not pass through a canal of the $\frac{1}{70}$ of an inch in diameter, when it was $\frac{1}{4}$ of an inch deep, and forming a cylinder

* These results appear at first view contradictory to those mentioned in page 449. But it must be kept in view, that the first set of experiments were made in tubes open in the air, and the last in tubes exposed to the whole force of air explosion, and connected only with close vessels filled with explosive mixtures.

of

of only $\frac{1}{4}$ of an inch in circumference; and a number of apertures of $\frac{1}{100}$ of an inch are safe when their depth is equal to their diameter. It is evident from these facts, that metallic doors, or joinings in lamps, may be easily made safe by causing them to project upon and fit closely to parallel metallic surfaces.

Longitudiual air canals of metal may, I find, be employed with the same security as circular canals; and a few pieces of tin-plate soldered together with wires to regulate the diameter of the canal, answer the purpose of the feeder or safe chimney as well as drawn cylinders of brass.

A candle will burn in a lantern or glass tube made safe with metallic gauze, as well as in the open air: I conceive, however, that oil lamps, in which the wick will always stand at the same height, will be preferred.

But the principle applies to every kind of light, and its entire safety is demonstrated.

When the fire-damp is so mixed with the external atmosphere as to render it explosive, the light in the safe lantern or lamp will be extinguished, and warning will be given to the miners to withdraw from, and to ventilate that part of the mine.

If it be necessary to be in a part of the mine where the fire-damp is explosive, for the purpose of clearing the workings, taking away pillars of coal, or other objects, the workmen may be lighted by a fire made of charcoal, which burns without flame, or by the steel-mill, though this does not afford such entire security from danger as the charcoal fire.

It is probable, that when explosions occur from the sparks from the steel mill, the mixture of the fire-damp is in the proportion required to consume all the oxygen of the air, for it is only in about this proportion that explosive mixtures can be fired by electrical sparks from a common machine.

As the wick may be moved without communication between the air in the safe-lantern or lamp and the atmosphere, there is no danger in trimming or feeding them; but they should be lighted in a part of the mine where there is no fire-damp, and by a person charged with the care of the lights; and by these inventions, used with such simple precautions, there is every reason to believe a number of lives will be saved, and much misery prevented. Where candles are employed in the open air in the mines, life is extinguished by the explosion; with the safe lantern or safe lamp, the light is only put out, and no other inconvenience will occur.

Amongst various plans for preventing accidents from the fire-damp, it has been proposed to burn the fire-damp in the mine; but this will only render the ventilation more difficult; for there will be less respirable air in the residuum of the combustion than in the mixed gas, and the ventilation must be greater to free the mine from the choke-damp so generated, than from the original fire-damp.

It does not appear, by what I have learnt from the miners, that breathing an atmosphere containing a certain mixture of fire-damp near or even at the explosive point, is attended with any bad consequence. I ascertained that a bird lived in a mixture of equal parts of fire-damp and air; but he soon began to show symptoms of suffering. I found a slight head-ache produced by breathing for a few minutes an explosive mixture of fire-damp and air; and if merely the health of the miners be considered, the fire-damp ought always to be kept far below the point of its explosive mixture.

Miners sometimes are found alive in a mine after an explosion has taken place: this is easily explained, when it is considered that the inflammation is almost always limited to a particular spot, and that it mixes the residual air with much common air; and supposing 1 of fire-damp to 13 of air to be exploded, there will still remain nearly $\frac{1}{7}$ of the original quantity of oxygen in the residual gas; and in some experiments, made sixteen years ago, I found that an animal lived, though with suffering, for a short time, in a gas containing 100 parts of azote, fourteen parts of carbonic acid, and seven parts of oxygen.

Explanation of the Plate. (Plate VIII.)

Fig. 1. Represents the safe lantern, with its air-feeder and chimney furnished with safety metallic canals. It contains about a quart of air. The sides are of horn or glass, made air-tight by putty or cement. A. is the lamp through which the circular air-feeding canals pass: they are 3 concentric-hollow cylinders, distant from each other $\frac{1}{50}$ of an inch: the smallest is $2\frac{1}{2}$ inches in circumference; their depth is 2 inches. B. is the chimney, containing 4 such canals, the smallest 2 inches in circumference: above it is a hollow cylinder, with a cap to prevent dust from passing into the chimney. C. is the hole for admitting oil. D. is a long canal containing a wire by which the wick is moved or trimmed. F. is the tube forming a connexion between the reservoir of oil and the chamber that supplies the wick with oil. F. is the rim round the bottom of the lantern to enable it to bear motion.

Fig. 2. is the lamp of fig. 1., of its natural size, the references to the letters are the same.

Fig. 3. is a common chimney which may be used in the lantern; but the safety chimney doubles security.

Fig.

Fig. 4. exhibits the safety concentric canals or fire sieves, which, if $\frac{1}{25}$ of an inch in diameter, must not be less than 2 inches in exterior circumference, and 1.7 inches high.

Fig. 5. exhibits the longitudinal safe canals or fire sieves.

Fig. 6. exhibits a safe lamp, having a glass chimney covered with tin-plate, and the safety apertures in a cylinder with a covering above : the lower part is the same as in the lantern.

Fig. 7. An Argand lamp of similar construction, with safe air canals without the flame, and metallic gauze apertures within.

Fig. 8. A tin-plate chimney top for the lamp, made safe by metallic gauze.

Fig. 9. A metallic gauze safe lamp. AAA. Screens of metallic gauze or *flame sieves*. BB. Wires for trimming the wick.

Fig. 10. A glass tube furnished with *flame sieves*, in which a common candle may be burnt. AA. The flame sieves. B. A little plate of metal to prevent the upper flame sieve from being acted on by the current of hot air.

The lamps burn brighter the higher the chimney.

From my experiments it appears, that a mere narrow throat and opening to the metallic part of the chimney, is sufficient to prevent explosions from passing through the lamp, supposing them possible ; but with the safety canals or metallic gauze in the chimney the security is absolute.

The circular canals and the apertures covered with metallic gauze, are so much superior to tubes in practical application, that I have no doubt of their being generally used ; I have therefore given no sketch of the first safe lantern I had constructed with tubes ; but substituting tubes for canals it is exactly the same as that represented fig. 1.

Appendix.

1. In the beginning of my inquiries I had another close lantern made, which may be called the fire-valve lantern. In this, the candle or lamp burns with its full quantity of air, admitted from an aperture below, till the air begins to be mixed with fire-damp ; when, as the fire-damp increases the flame, a thermometrical spring at the top of the lantern, made of brass and steel, riveted together, and in a curved form, expands, moves a valve in the chimney, diminishes the circulation of air, and extinguishes the flame. But I did not pursue this invention after I had discovered the properties of the fire-damp, on which the safe lantern is founded.

2. The safety of close lamps or lanterns may probably be likewise secured by sieves made of asbestus, or possibly even hair or

or silk, placed over the air apertues: but metallic gauze will be necessary above in the chimney. I have little doubt but that windows of fine metallic gauze may be used for giving light in lanterns, with perfect security; perhaps for the chimney it may be worth while to have fine silver plated wire gauze made.

3. The expansive powers of the fire-damp during its explosion are so small as to render no precautions, with respect to the thickness of the glass or horn in the lamps or lanterns, necessary.

LXXXIII. *On Safe-Lamps for Coal Mines; with a Description of the one invented by Mr.* Stephenson, *of Killingworth Colliery.*

To Mr. Tilloch.

Sir,—On Tuesday, the 5th of December, a very numerous meeting of the members of the Literary and Philosophical Society at Newcastle-upon-Tyne took place, in consequence of an intimation that some *safe-lamps*, intended to warn miners of the presence of inflammable air, were to be exhibited; and a memoir read on the same interesting subject, which had been kindly transmitted by its author, Dr. Murray of Edinburgh. On this occasion several viewers and other gentlemen connected with the coal trade attended. The first lamp produced was that contrived by Dr. Clanny, brought by the inventor in person. Of this machine, with its bellows, water-cisterns, jack and chain, little need be said, the Philosophical Transactions having introduced it to public notice; but though the Doctor's lamp is unfit for the use of the coal-miner while at work, and equally so when traversing the narrow and frequently obstructed galleries of a colliery, yet much praise is certainly due to the individual who was the first to dedicate his time, besides spending some money, on a project intended solely for the benefit and protection of a class of men with which he was in no way particularly concerned.

Mr. William Brandling's lamp next occupied the attention of the meeting: it resembles a common lantern in size and shape, with a pair of double bellows fixed on the top, to free it from the air exhausted of oxygen by combustion, and a flexible tube to reach to the floor of the mine, where carburetted hydrogen is less likely to be met with than nearer the roof. To this contrivance the objection was, that without great care in managing the bellows, the light might be easily extinguished; the tube also appeared to be an unnecessary incumbrance.

Of Sir H. Davy's and and Dr. Murray's lamps nothing need be said, descriptions of them having been printed in some of the

the philosophical journals of last month; but I shall speak in detail of one produced by a Mr. Stephenson, engine-wright at Killingworth Colliery, which met with universal approbation. This is a small copper lamp, of a semi-globular form, with an outer case of the same shape: round the lower part of this case are numerous small holes, by which the air is admitted, and after passing between the two cases, escapes through a circle of similar orifices surrounding the wick, and feeds the flame: this simple apparatus is covered with a glass chimney, which fixes in a rim so as to be rendered air-tight, and the chimney protected by a tin covering pierced full of holes, at once to emit light and save the glass from external injury. In this manner combustion is carried on while the air is sufficiently pure for its support; but no sooner is it contaminated by carburetted hydrogen up to the firing point than the flame gradually dies out without explosion; and should the inflammable gas be precipitated down the chimney by any unforeseen accident, the azotic gas accumulated above the flame is found equal to extinguish it. The height of this lamp, of which the inclosed correct though rough drawing (Plate VIII. fig. 11) will convey a just idea, is twelve inches, and cost under five shillings. Of its great utility in places of danger, the following experiment seemed to carry the strongest conviction to every practical miner present. A quantity of carburetted hydrogen, obtained from a blower in Killingworth Colliery, was pressed from a bladder so as to enter through the small holes near the bottom of the lamp, the contents of another bladder being discharged into a gasometer inverted at the same time over t e chimney: the flame in the lamp gradually died away, as was the case in a previous trial, and without the gas above it being inflamed; but no sooner was a candle introduced into the gasometer than an explosion took place.

From what has been already said, together with the inclosed section, an invention nearly similar to Sir H. Davy's will be immediately recognised; but that it has not been pirated from that gentleman is a fact known to most of the mine owners here; for on the 3d of November the Reverend Mr. Hodgen of Haworth, and Dr. Grey of Wearmouth, communicated to a very full meeting of the coal trade held at the Assembly-rooms, the contents of letters just received by them from Sir Humphry, reciting the particulars of his newly-invented safe-lamps; and to the same company Mr. R. Lambert, chief agent for Killingworth and other mines on this river, mentioned the discovery made by Stephenson, who is employed by him as an engine-wright, observing, at the same time, that his lamp had been taken into the pits, and found to answer the intended purpose. Sir Humphry

phry Davy's discovery flowed from science judiciously applied. Stephenson's discovery appears to have resulted from trials made below ground; for, though an excellent mechanic and acute man, he is unacquainted with the science of chemistry. Thus a great object has been obtained in rendering the occupation of the coal miner in this part of the kingdom less hazardous from the dreadful effects of inflammable air; and I hope you will not think the pages of your valuable journal misapplied in giving publicity to a contrivance, by which the lives of hundreds may be saved in other coal-mining districts, as well as on the banks of Tyne and Wear.

Newcastle, Dec. 10, 1815. N.

Explanation of Mr. STEPHENSON'S *Lamp for Miners.*
(Fig. 11.—Plate VIII.)

A. The lamp made of copper.
B. The glass chimney.
C. The perforated tin cover.
d d d d. Air holes.
E. Wick tube.
F. Oil tube.

LXXXIV. *On* WOOLF'S *Steam Engine*; *with Certificates of their Performance, from the Managers of the Mines in Cornwall where they are at work.*

To Mr. Tilloch.

SIR,—HAVING observed with pleasure for several months past, in the Philosophical Magazine, a statement of the duty performed by the different sorts of engines at work pumping water out of the mines in Cornwall; and presuming that the effect must have been to excite a more general interest in favour of Woolf's engines; we transmit for insertion copies of two certificates from the agents of the mines where they are at work; to which it seems needful to add, that the *very great* quantity of coal represented to be saved by the use of Woolf's engine at Wheal Vor, is in some measure to be attributed to the indifferent state of the one pumping water on Boulton and Watt's plan. Woolf's engine is, however, doing the *same duty* as is usually done by good engines on Boulton and Watt's plan, *with less than one half the quantity of coal.*

From the facts exhibited practically by the engines we have made on Woolf's plan, we do not hesitate to give it as our opinion, that these engines are by far the most eligible where a saving in the consumption of coal is an object.

Foxes and Neath-Abbey Iron Company,

Neath-Abbey Iron Works, Glamorganshire, 12th Month 20th, 1815. Per Joseph T. Price.

Copy.

We, the undersigned, being the principal agents of Crenver, Oatfield, and Abraham Mines, in the county of Cornwall, do hereby certify, that an engine erected on that part of the aforesaid mine called Wheal Abraham, on Mr. Woolf's patent principle, the large cylinder of which being forty-five inches diameter has been working for the last fourteen months drawing water from a depth of 190 fathoms, with a load for the first ten months of upwards of sixteen pounds per square inch, and for the last four months about fifteen pounds per square inch; that the said engine has executed her work *far beyond* our most sanguine expectations; and we can with pleasure state, that the engine during the first ten months exceeded the average effect of Boulton and Watt's engines in this county as thirty-four to twenty; and during the last four months considerably above two to one, or as forty-seven to twenty. We also consider an engine on Mr. Woolf's principle less liable to hinderment than an engine on any other principle we have yet seen.

N. B. The load is lifted by the steam acting on both pistons, but is equal to what is above stated, calculated on the superficies of the large piston.

Andrew Vivian,
Robert Bennett,
Philip Richards,

Thomas Lean,
Wm. Davy.—I occasionally attend at Wheal Abraham, and believe the above statement to be true.

November 1815.

Copy.

Wheal Vor, in Breage, Dec. 4, 1815.

We the undersigned, being the agents, do certify that the duty done by Woolf's engine in this mine exceeds that of the other engine which is on Boulton and Watt's plan of erection, on an average, as 116 to 34; that is to say, that Woolf's engine performs the same duty by consuming thirty-four bushels of coal, as the engine on Boulton and Watt's plan does on the consumption of 116.

Witness our hands,

John Gundry,
William Thomas,
Samuel Adams,

John Perry,
John Chynoweth,
Thomas Powning.

LXXXV. *On*

LXXXV. *On the Work performed by Steam-Engines in Cornwall with each Bushel of Coals.*

We find that the Table given in our number for August last, p. 118, of work performed by the steam-engines in Cornwall, is still misunderstood by some of our readers, as are also the notices on the same subject given in the numbers since published. The first column gives the months, viz. August, September, &c.; the second contains the number of engines reported in each month; the third gives the whole quantity of coals consumed during the month by all the engines reported; the fourth is the sum of the work performed by the engines reported, with *one bushel of coals each:* for example, in August 1811, the first month reported, *eight engines, each using one bushel of coals,* lifted 126,126,000 one foot high, one-eighth of which quantity is the average work of these eight engines. If the whole quantity of water raised one foot high by the whole quantity of coals consumed by the eight engines be required, then the number of pounds in the fifth column, which is the work of every bushel (on an average of the eight engines reported), must be multiplied by the number of bushels expressed in the 3d column. Thus in August 1811, the average work of eight engines was 15,760,000 pounds lifted one foot high by one bushel of coals: and these pounds multiplied by 23,661 bushels would give for the whole quantity of water lifted one foot high by all the coals consumed during the month 372,897,360,000 pounds—and so of the rest of the months in the Table: but this would be only an approximation to truth; for some engines work more days in a month than others, and those as well as the engines consuming the greatest or least quantity of coals may chance to be sometimes the better and sometimes the worse engines of the number reported. The third column in the Table was no way necessary to its construction; it was inserted merely as a piece of curious information respecting the general consumption of coals in Cornwall. But, in fact, the title of the fourth column was inaccurately expressed. To save trouble in future references, we again insert the Table, but now brought down to November inclusive.

		Number of Engines reported.	Bushels of Coals consumed by all the Engines reported.	Pounds of Water lifted one Foot high by all the Engines, each Engine using one Bushel of Coals.	Average Pounds lifted one Foot high with each Bushel of Coals.
1811.	August - -	8	25,661	126,126,000	15,760,000
	September -	9	23,237	125,164,000	13,900,000
	October -	9	24,487	121,910,000	13,540,000
	November -	12	30,998	189,340,000	15,770,000
	December -	12	39,545	204,907,000	17,075,000
1812.	January -	14	50,089	237,661,409	16,972,000
	February -	15	54,349	260,514,000	17,900,000
	March - -	16	59,140	274,222,000	17,138,000
	April - -	16	62,384	276,233,000	17,260,000
	May - -	16	51,903	273,546,000	17,096,000
	June - -	17	50,410	288,076,000	16,940,000
	July - -	17	51,574	300,441,000	17,677,000
	August -	17	44,256	314,753,000	18,5[illegible]0,000
	September -	18	46,536	348,396,000	19,355,000
	October	18	53,941	321,900,000	17,883,000
	November -	21	57,176	381,460,000	18,160,000
	December -	19	55,734	341,803,000	18,200,000
1813.	January -	19	60,400	363,906,000	19,153,000
	February -	22	58,044	438,737,000	19,940,000
	March - -	23	73,862	440,642,000	19,157,000
	April - -	23	61,739	431,032,000	18,700,000
	May - -	24	58,890	463,346,000	19,300,000
	June - -	24	53,110	470,157,000	19,590,000
	July - -	23	56,709	443,462,000	19,281,000
	August - -	21	50,110	416,898,000	19,852,000
	September -	22	58,008	427,143,000	19,415,000
	October -	26	74,796	488,671,000	18,795,000
	November -	28	77,135	537,958,000	19,212,000
	December -	29	86,273	581,721,000	20,162,000
1814.	January -	28	91,753	550,751,000	19,670,000
	February -	26	78,986	536,677,000	20,641,000
	March - -	28	109,904	565,406,000	20,193,000
	April - -	29	91,697	576,617,000	20,325,000
	May - -	28	79,437	569,319,000	20,305,000
	June - -	30	75,343	626,669,000	20,888,000
	July - -	27	85,224	573,208,000	21,229,000
	August - -	26	70,143	545,019,000	20,961,000
	September -	27	73,167	560,603,000	20,763,000
	October -	32	75,030	630,704,000	19,709,000
	November -	32	82,000	637,322,000	19,916,000
	December -	29	81,669	573,744,006	19,784,276
1815.	January -	32	110,824	637,320,990	19,916,250
	February -	33	101,667	710,271,250	21,523,370
	March - -	34	117,342	706,071,990	20,766,820
	April - -	35	105,701	695,212,340	19,863,210
	May - -	34	107,530	669,299,140	20,479,350
	June - -	*			
	July - - -	34	79,233	676,496,000	19,897,000
	August - -	33	78,421	659,171,000	19,975,000
	September -	32	87,792	589,912,000	18,372,000
	October -	34	75,009	618,902,532	18,203,016
	November -	33	91,472	598,683,845	18,141,934

* This month's report has been mislaid.

During the last month (November), according to Messrs. Leans' Report, Woolf's engine at Wheal Vor consumed 1154 bushels of coals, and with each bushel lifted 50,445,150 pounds of water one foot high; and his engine at Wheal Abraham during the same month consumed 1044 bushels of coals, and lifted, with each bushel, 52,327,4000 pounds, one foot high.

LXXXVI. *On Boilers employed to produce Steam of High Pressure. By* T. Gill, *Esq. one of the Chairmen of the Society for the Encouragement of Arts, Manufactures and Commerce.*

To Mr. Tilloch.

Sir,—I was much pleased with the judicious remarks contained in your last number, on the lamentable accident in Well-street, Goodman's Fields, occasioned by exposing a close boiler to so high a temperature as to cause an explosion attended with and followed by such fatal consequences. If the makers of boilers that are to be employed in producing steam of high pressure do not adopt those precautions which shall preclude the possibility of such accidents, they will deter all prudent people from the use of this invaluable agent. Already this has been the case with respect to the use of loco-motive engines on some of the rail-ways, in consequence of the explosion of that at New-bottle Colliery, alluded to in your last number, and which was occasioned by a culpable obstinacy which caused the death of the engineer and several other people. Some years ago, when a high pressure engine near Woolwich was burst by the steam, the mischief was owing to similar madness—the *wilful* obstruction of the safety valve to prevent the possibility of steam escaping! After the last-mentioned accident, several effectual means to prevent such accidents were contrived by Mr. Trevithick (who first brought the high-pressure engines into use). In the first place, he proposed *inclosing the safety-valve* in such a manner that no one could get access to it to increase the load beyond that intended; and secondly, in addition to the recurved tube with mercury (which can be adjusted with precision to any pressure intended to be employed, and will blow out before the steam, when the pressure rises even a single pound higher) he drilled a hole in the boiler, which he plugged up with lead, at such a height from the bottom, that the boiler could never boil dry without exposing the lead to be melted; and consequently, making an opening for the steam, which by its escape into the fire extinguishes it. This contrivance is calculated to prevent the

the bursting of the boiler by forcing cold water into it when allowed by carelessness to become dry, and to get red hot. Metal plugs should always be riveted into such boilers. They should be made of a fusible mixture, that will melt whenever the contents of the boiler attain a certain degree of heat. This should never be omitted, even when the boiler is of the best form, which certainly is that contrived by Mr. Woolf. Indeed, I think it impossible to conceive any other arrangement for a boiler possessing equal strength and durability, and consequently, safety. As it would appear that some of these safeguards are not so generally known as they ought to be, you may, perhaps, render some service to the community by giving them a place in your useful miscellany.

I am, sir,

Your most obedient servant,

THOMAS GILL.

LXXXVII. *Hints on the Localities of Coal.*

To Mr. Tilloch.

SIR,—As a spirit of inquiry has lately been excited on the subject of the various localities of coal, the following brief notices of the different formations or sets of rocky strata among which it has been detected in the north-east of England, may afford amusement to such of your readers as pay attention to geological facts, and some useful information to those who suspect that this valuable fossil may lie concealed under beds of stone found on their own estates or royalties.

1st. Tolerably good seams of coal are spread over that part of Northumberland situated north of the Coquet and west of Bywell on the Tyne, with the exception of the porphyritic and grau wacke mountains forming the Cheviots:—here it alternates with sandstones, shales, and blue encrinal limestone, and overlying masses or mountain caps of basalt are scattered over the whole country. As the limestone stretches from the sea-shore towards the south-west, and rises into the chain of mountains which reach from Northumberland to Derbyshire, the seams diminish in thickness, and the coal consists chiefly of carbon and sulphur with a very small proportion of bitumen. Such is the crow coal of Alston Moor.

2d. The Newcastle coal formation measures 58 miles from Acklington in Northumberland to Cockfield in Durham, and 24 miles from the sea-coast at Tynemouth to the neighbourhood of Bywell. Its strata comprise sandstones and shales, with occasional

casional dykes of basalt, but no regular beds, nor mountain caps of that rock, are here to be found. This formation is evidently of fresh-water origin, for marine exuviæ do not occur in it; but the impressions of ferns, reeds, and fresh-water muscles are met with in every colliery. The magnesian or Sunderland limestone next succeeds, containing casts of fish, encrinites, flustræ, and other productions of the ocean, but no bed of coal or coal measures alternate with it.

3d. The red calcareous sandstone with masses of gypsum, over which the Tees flows from the vicinity of Croftbridge, was bored for coal at Dinsdale to the depth of 54 fathoms, but no *seam worth working* detected.

4th. The alum shale of the Yorkshire moorlands is covered by a thin coal formation, and some of the schistose rocks accompanying the coal envelop casts of ammonites, pectinites, &c. Jet coating mineralized wood in the manner of bark is also found in the alum shale itself.

5th. On the Lincolnshire coast, fragments of a black coal, but resembling that of Bovey in texture, are washed on shore, together with an ash-gray bituminous shale containing numerous sea-shells; and several beds of the same shale have been bored through near Louth, in search of a seam worth working, but without success. The hard chalk forming the Wolds rests immediately on this set of strata.

In hopes that these short memoranda may induce some of your numerous correspondents to communicate additional observations on a subject so nearly connected with the prosperity of this kingdom,

I remain, &c.

Newcastle, Dec. 17, 1815. N. J. W.

LXXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 30. Anniversary. — On the Copleyan medal being adjudged to Dr. Brewster by the President and Council of the Royal Society, it was delivered to Mr. Troughton, his proxy, with a suitable address from the President. This discourse proved that, however time and disease may have affected his corporeal powers, they have not been able to assail his mental faculties, and his capacious mind still retains that native vigour which once carried it around the globe. Sir Joseph, in his usual perspicuous manner, took a concise retrospect of the recent discoveries in physical optics; he described with equal simplicity and

and accuracy the modifications of light discovered by Malus, and the altered direction of rays, in consequence of passing through certain crystals, which, having much analogy to magnetic phænomena, has been called polarization. He next took a review of the numerous experiments and discoveries of Dr. Brewster, observing that few of the other fellows of the Society had contributed an equal number of curious papers in so short a period; and in brief but very comprehensive terms unfolded the singular results and important consequences of this indefatigable philosopher's labours, which have appeared in the Philosophical Transactions. Such of the noblemen and prelates who are fellows of this society and omitted attending on this occasion, lost an opportunity not to be regained, of easily acquiring a distinct knowledge of a very curious and interesting branch of science, which has lately engaged the attention of philosophers in all countries, and which may ultimately be brought to administer to the comforts, amusements, or conveniences of persons in opulent life. The Right Hon. President, in conclusion, addressed himself to Mr. Troughton, charging him to convey to their absent fellow, Dr. B. the sentiments of the Council, their high sense of the value and importance of his labours, and in their name and in that of the Society to urge him to persevere in his experiments, to continue his researches in that branch of physical optics which he had chosen, and in which he had been so signally successful. Lastly, he observed, "how gratifying it must be to be praised by those who are themselves so worthy of praise!" and that no higher stimulus could be offered to a philosopher, than that now given by the decision of the Council in adjudging this medal, and an expression of the sentiments which influenced that decision.—Afterwards the Society proceeded to the choice of a Council and Officers for the ensuing year, when on examining the lists, it appeared that the following gentlemen were elected:—

Of the Old Council.—The Right Hon. Sir Joseph Banks, Bart. K.G. C.B., Sir Charles Blagden, Knight, Samuel Goodenough, Lord Bishop of Carlisle, Taylor Combe, Esq., Davies Giddy, Esq. M.P., Sir Everard Home, Bart., Samuel Lysons, Esq., George, Earl of Morton, K.T., John Pond, Esq., William Hyde Wollaston, M.D., Thomas Young, M.D.

Of the New Council.—John Barrow, Esq., Mark Beaufoy, Esq., Henry Browne, Esq., Sir Humphry Davy, Knt., Philip, Earl of Hardwicke, K.G., Edward Howard, Esq., John Latham, M.D. Pres. Coll. Phys., Thomas James Mathias, Esq., Sir John Nichol, Knt. M.P., George, Earl of Winchilsea, K.G.

Officers.—President, the Right Hon. Sir Joseph Banks, Bart.

Bart. K.G.C.B.—Treasurer, Samuel Lysons, Esq.—Secretaries, William Hyde Wollaston, M.D., and Taylor Combe, Esq.

After the election the members of the Society dined together, as usual, at the Crown and Anchor Tavern in the Strand.

Dec. 7.—The President in the chair. A short paper by Dr. Reid Clanny was read, containing an account of some recent experiments made with his lamp in coal-mines, when their atmosphere was in a highly combustible state, being saturated with carburetted hydrogen gas. In this communication, which Dr. C. calls a Supplement to his paper in the Phil. Trans. 1813, the author narrates the dreadful events which have occurred in the different coal-mines since that period, when he first proposed his lamp as a protection to miners. He notices the increased number of such calamities in consequence of the increasing extent of the mines, and adds that, nevertheless, his lamp had not been tried! One alleged that ventilation was the best protection; but this is impossible where the mine, as often happens, has only one shaft. Another, that the lamp was liable to be broken by the fall of pieces or stones from the roof. To remove this objection, he had a lamp constructed of glass so thick and strong that a ton weight falling on it could not break it. He likewise made it so portable that it may be carried in a great coat pocket. Still he has been unable to get it introduced into practice, or even fairly tried, although it has received the approbation of all the scientific men who have seen it. Even till very lately (in the month of October only) he could not obtain a favourable opportunity of making himself a fair experiment with his lamp, in a coal-mine saturated with fire-damp. Dr. C. gives an instance of persons either evading or declining to make an experiment with his lamp, although he lives in the vicinity of coal-mines. Ultimately, he has succeeded, through the beneficent exertions of Mr. Holmes, in making two different experiments in mines which were in a highly combustible state, and where the presence of a common lamp must have been attended with instant ruin. One of these experiments was made in rather a perilous situation, in the chamber of a mine 100 fathoms below the surface of the earth, and where the air was so inflammable that a common lamp must have blown up the mine: but with Dr. C.'s lamp no danger whatever occurred, and all persons remained in perfect safety. In these experiments, atmospheric air was first thrown into the lamp by means of the bellows: carburetted hydrogen was then added till a slight explosion took place within the lamp, which was then extinguished, without affecting the circumambient air, which was highly inflammable. These satisfactory and successful results are attested

tested by the signatures of —— Holmes, Esq. the superintendants and experienced miners present. In a P.S. Dr. C. assigns a cause for the occasional extinction of his lamp, from the irregularity of throwing in the necessary supply of air; but he has also improved the machinery for blowing the bellows, which can now be wound up to act above thirty hours.

Dec. 9.—A mathematical paper by Mr. Herschel was laid before the Society, but it was of a nature not to be read. On this evening, and on

Dec. 21,—Part of a paper by Dr. Brewster was read, detailing a series of experiments on the effects of heat on transparent bodies, and their consequent production of pencils of colours. These experiments are deemed interesting to the chemist, mineralogist, and persons who amuse themselves with experiments in natural philosophy. The results of these experiments were detailed in propositions. The first proposition stated, that by heating plates of glass in water, they yielded, according to their thickness, all the series of colours described by Newton, one falling short of another so as to give the regular series. Glass was crystallized into parallelopipedons by heating on iron bars, and these crystals gave coloured fringes. The reading was postponed till a future meeting, and the Society adjourned till Jan. 11, 1816.

LXXXIX. *Intelligence and Miscellaneous Articles.*

FIRE-DAMP.

THE friends of humanity will rejoice that means have at length been devised, which promise effectual security against the recurrence of those dreadful explosions of fire-damp in coal-mines, which have occurred so often within these few years.

In a recent volume of the Philosophical Transactions an account is given of Dr. Clanny's lamp contrived for this purpose. We need not here repeat the nature of the construction. It is but justice to say, that it was very ingenious, and with proper care and management seemed calculated to answer the desired end: nor could we allow the objections that have been urged against it, on the score of complexity, to have such force as to prevent its adoption—if more simple and equally efficacious means had not since been devised.

At a late meeting of the Literary and Philosophical Society at Newcastle, a paper was read, describing a new lamp invented by Dr. Murray of Edinburgh, intended to prevent explosions. It is described as a close lamp furnished with a

flexible

flexible tube reaching to the floor of the mine—on the principle that the inflammable gas, being lighter than the atmospheric air, will be found to occupy the higher station: but in fact, the fire-damp is often disengaged from below; which is a sufficient objection (were there no other) to the use of this lamp.

Another lamp invented by W. Brandling, Esq. was also examined by the Society, which, if there were now any necessity, might be used for exploring dangerous places with safety. The azote disengaged by the combustion is drawn out by double bellows connected with the top; and fresh air in consequence rushes in through a flexible tube attached to the bottom of the lamp, and of sufficient length to reach a current of pure air.

In our last we gave a brief account of Sir H. Davy's safe-lamp, and in our present number we have been enabled to lay his communication to the Royal Society, in an entire state, before our readers—a circumstance that will prove gratifying to several correspondents, who had expressed great anxiety to have such information on the subject as might enable them to apply it to practice.

We have been favoured with a description of another safe-lamp, also inserted in the present number, invented by a Mr. Stephenson of Killingworth Colliery. In principle it is somewhat similar to Sir H. Davy's, and, considered as the invention of a man of humble pretensions, has considerable merit, though much inferior in construction to Sir Humphry's. It is rather a curious coincidence, that two lamps at all approximating each other in the means resorted to for obtaining the desired object, should have been brought forward by different inventors; for we have every reason to believe that Mr. Stephenson's as well as Sir H. Davy's invention was perfectly original. The experiments with Mr. Stephenson's lamp are, to a certain extent, satisfactory; but, if we have rightly understood the communication of our correspondent, the absolute safety of the lamp was not proved by them; for *pure fire-damp* appears to have been thrown into it, which we know does extinguish flame; whereas *an explosive mixture* should have been thrown in.

We have seen Sir Humphry's lamp. He has succeeded in bringing it to a state of *absolute security*. This we saw proved by a variety of experiments, in which the most explosive mixtures that could be formed were thrown into it, and the effect invariably was, the extinction of the flame. It is impossible to conceive any lamp more easy to be kept in order, while at the same time it is as simple as any common portable lamp.

Thus have the public been put in possession of a contrivance which was long a desideratum, and which, not many months ago, was ranked among those objects which, however desirable, could

hardly

hardly be hoped to be attained. We have seen, and with regret, a most illiberal communication in the Tyne Mercury, charging Sir H. Davy with having pirated Dr. Clanny's lamp (described in the Philosophical Transactions, and with keeping the Doctor's name out of view, in his communication made to the Royal Society. Now the fact is, that Sir Humphry made very handsome mention of the Doctor's name in the communication alluded to. As to the charge of piracy:—excepting the circumstance of both having a wick (in common with all oil lamps) the two contrivances are not nearly so similar as a blast-furnace and an air-furnace. Indeed they are much more remote from each other.—The Doctor rests the safety of his lamp on the intervention of water to prevent the transmission of ignition, and supplies the air required to maintain the flame by means of bellows. Sir Humphry rests the safety of his lamp on quite a different principle—the non-conducting power of tubes, or of apertures below a given size, to transmit ignition, even when filled with the inflammable gas and in contact with flame; or, in other words,—on the power which small apertures possess to extinguish inflamed gas; and the air required to maintain combustion is supplied, as in a common Argand's lamp, by passing through (properly adapted) apertures at the bottom, without the intervention of any liquid.

As to those pseudo-philosophers who take upon them to assert (probably because the fact, simple as it is, is beyond their comprehension) that Sir H. Davy's lamp will not anwser, we can offer them no consolation. For ourselves, we seek no better evidence that the sun will rise to-morrow morning, than its having regularly done so all the yesterdays that are past.

GEOLOGY.

We have been enabled, by the favour of a correspondent, to give, in the present number, a brief sketch of the different formations of rocky strata in which coal has been detected in the North-East of England.—A geological sketch of the Eastern part of Yorkshire from the Tees to the Humber, offered by one of our correspondents, will be received with thanks, and receive instant attention.

Mr. Bracy Clarke, Veterinary Surgeon, of Giltspur-street, has in the press, intended for speedy publication, A Treatise on the Bots of Horses and other domestic Animals; being a reprint of his Treatise on that subject, formerly published in the Linnean Society's Transactions, with numerous and interesting additions. He has introduced an account of a newly-discovered race of flies bred in the living bodies of animals in America.

LECTURES.

LECTURES.

Royal Institution.—The Lectures will commence on Saturday the 3d of February next, and the following Arrangements have been made for the ensuing Season.

On the Chemistry of Vegetable and Animal Substances, by W. T. Brande, Esq. F.R.S L. & E. Prof. Chem. R.I.—On Geology, by the same.—On Practical Mechanics, with their Applications to the Arts and to Manufactures, by John Millington, Esq. Civil Engineer.—On Astronomy, by the same.—On the Principles and Practice of Drawing and Painting, by W. M. Craig, Esq. *delivered gratuitously.*

Russell Institution.—A Course of Lectures on the Elements of Electrical Science will be commenced about the end of January at the Russell Institution, by Mr. Singer.—These Lectures will include Galvanism and Electro-Chemistry. They may be attended by the Public as well as by the Members of the Institution. A Prospectus may be obtained at the Russell Institution, Great Coram-Street, Russell-Square.

Dr. Clutterbuck will begin his Spring Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, about the middle of January, at Ten o'Clock in the Morning, at his House No. 1, in the Crescent, New-Bridge-Street; where further particulars may be had.

Theatre of Anatomy.—Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S. of Hatton Garden.

The Winter Course will commence on Saturday, January 27th, 1816, at Eight o'Clock in the Evening precisely, and be continued every Tuesday, Thursday, and Saturday, at the same hour.—Particulars may be had on applying to Mr. Taunton, 87, Hatton Garden.

Theatre of Anatomy, Medicine, &c. Blenheim Street, Great Marlborough Street. The Spring Courses of Lectures at this School will begin on the following days.

Anatomy, Physiology, and Surgery, by Mr. Brookes, daily at Two, on Monday, January 22, 1816. Dissections as usual.

Chemistry, Materia Medica, &c., daily at Eight in the Morning; Theory and Practice of Physic, at Nine; with Examinations, by Dr. Ager, on Monday, February 5th.

Three Courses are given every year, each occupying nearly Four Months. Further particulars may be known from Mr. Brookes at the Theatre, or Dr. Ager, 69 Margaret Street, Cavendish Square.—Mr. Brookes's Museum will be open to all Gentlemen (gratuitously) on the first Monday of every Month from Eleven till Two.

Mr. Clarke will commence his next Course of Lectures on Midwifery and the Diseases of Women and Children, on Wednesday, January 24th. The Lectures are read every Morning from a Quarter past Ten to a Quarter past Eleven, for the convenience of Students attending the Hospitals. For particulars apply to Mr. Clarke, at the Lecture-Room, 10 Saville Row, Burlington Gardens.

Chemical Lectures.— Mr. Cooper will commence his Spring Course of Lectures on Experimental Chemistry, on Monday, 22d January, 1816, at his Laboratory, 76 Drury Lane, and continue them every Monday and Thursday Evenings, at 7 o'Clock, till the completion of the Course.

A short Syllabus of the Course may be had upon application as above.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For December 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Nov.	27	36	42	32	30·25	12	Fair
	28	32	37	28	29·96	14	Fair
	29	26	36	32	·97	10	Fair
	30	37	51	51	·80	0	Rain
Dec.	1	49	53	48	·85	17	Showery
	2	47	52	46	30·14	18	Fair
	3	47	45	39	29·98	22	Showery
	4	42	46	40	·78	20	Showery
	5	39	45	45	·50	0	Rain
	6	40	40	38	·38	10	Stormy
	7	37	40	28	·98	21	Fair
	8	26	29	25	·99	16	Fair
	9	24	29	32	30·02	10	Fair
	10	32	34	33	·38	10	Fair
	11	35	39	35	.38	9	Cloudy
	12	35	42	32	·43	10	Fair
	13	38	44	32	·22	8	Cloudy
	14	32	38	38	·26	12	Fair
	15	38	48	38	29·70	12	Fair
	16	48	45	33	·10	10	Cloudy
	17	34	35	29	28·91	15	Fair
	18	26	36	26	29·38	10	Fair
	19	25	35	32	·50	0	Snow
	20	45	47	46	·20	0	Rain
	21	37	39	30	·40	9	Fair
	22	29	34	36	·65	10	Fair
	23	32	37	42	·60	7	Fair
	24	42	40	32	·32	9	Fair
	25	32	37	27	·62	10	Fair
	26	37	42	42	·30	0	Rain

N. B. The Barometer's height is taken at one o'clock.

Errata.---We are desired to correct the following errata, which in the hurry of transcribing for publication crept into Mr. Evans's article on balloons in our last number. In page 325, line 12, for "a velocity of 28 feet per second," &c. read "of 15½ feet per second, or 10½ miles per hour." In line 19, for "28 feet," read "15½ feet."

INDEX TO VOL. XLVI.

END OF THE FORTY-SIXTH VOLUME.

PRINTED BY RICHARD AND ARTHUR TAYLOR,
SHOE-LANE, LONDON.

ALERE FLAMMAM.

Woolf's Boiler.

Fig. 3.

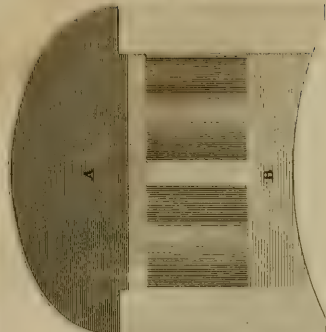


Fig. 2.

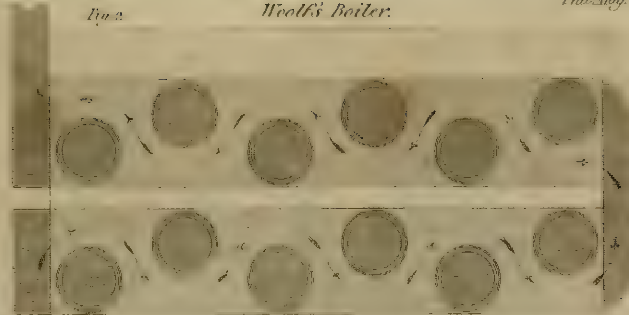
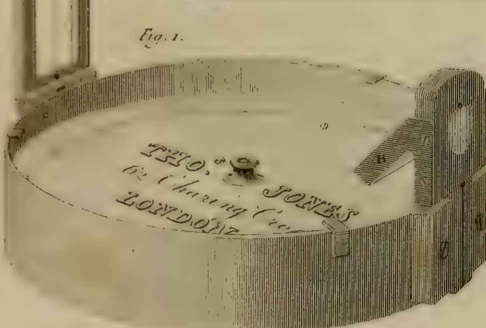


Fig. 1.



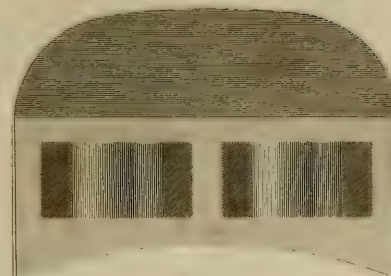


Fig. 3.

J Porter sc.

Fig. 1.

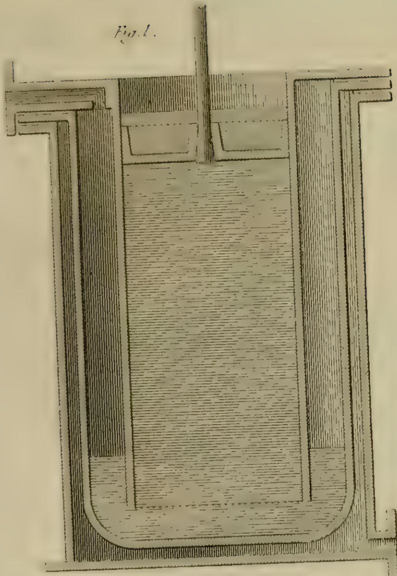
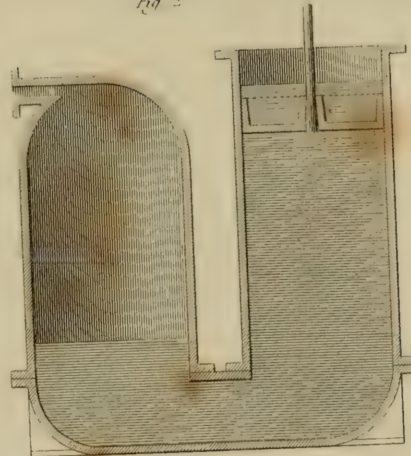


Fig. 2.



S. Porter sc.

Fig. 1.

Fig. 2.

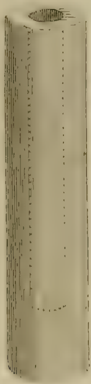


Fig. 3.

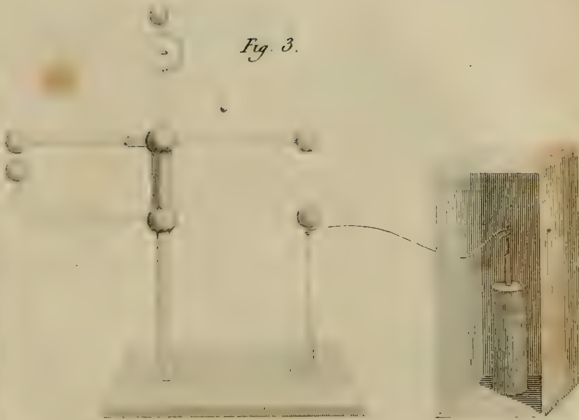


Fig. 4.

Fig. 5.

Fig. 6.

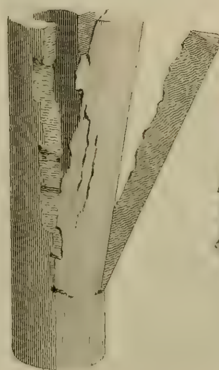


Fig. 7.

Fig. 8.

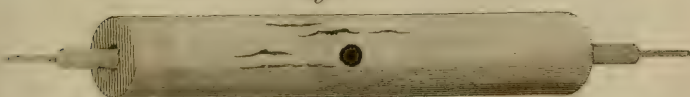


Fig. 9.

Entrance of the Fluid

Fig. 1.

Exit of the Fluid

Fig. 2.

210.

S Porter

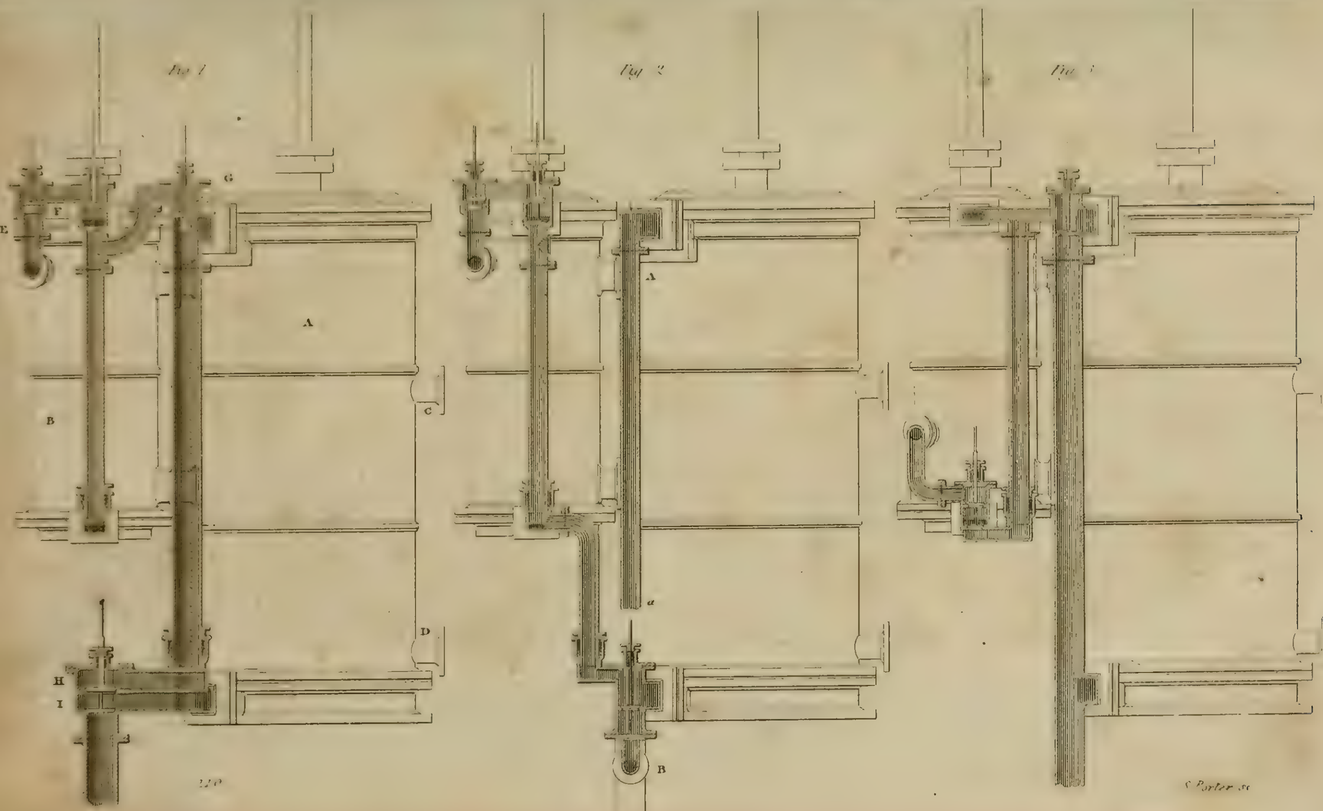


Fig 1
Fig 2
Fig 3
A
B
C
D
E
F
G
H
I
A
a
B
C Porter sc

Fig 1.

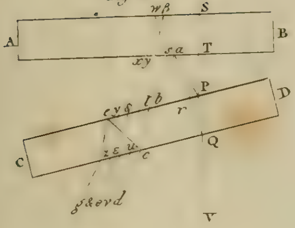


Fig 8.

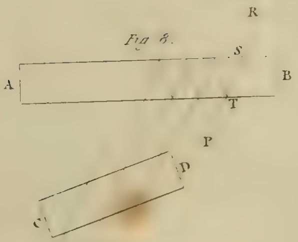


Fig 2

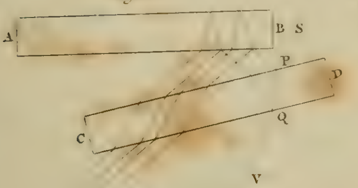


Fig. 4

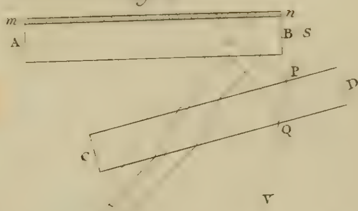


Fig 3

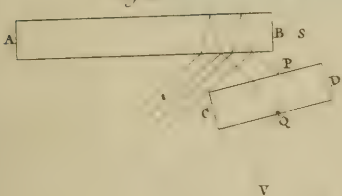


Fig 5

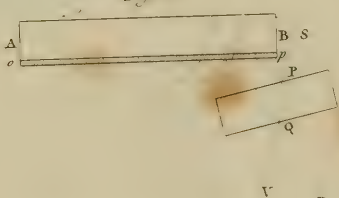


Fig 6

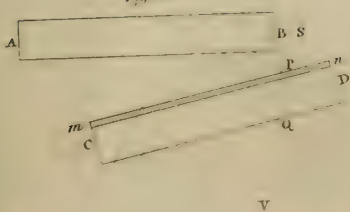
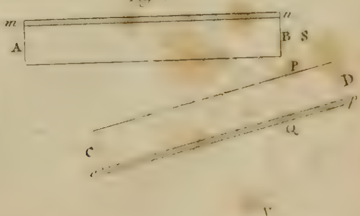


Fig 7



S. Porter sc.

Fig. 1.

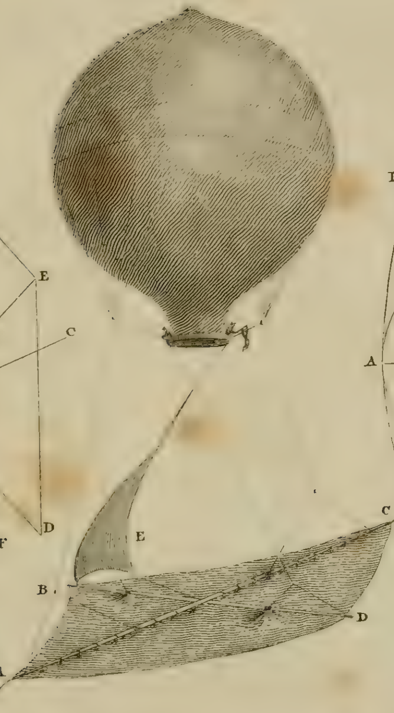


Fig. 2

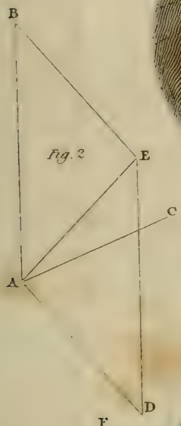


Fig. 4.

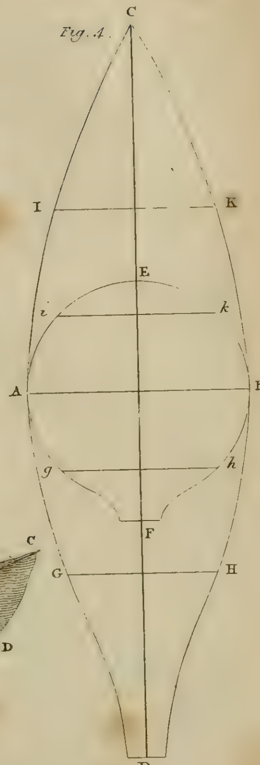


Fig. 3.

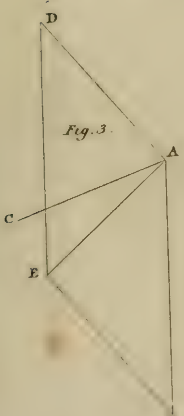
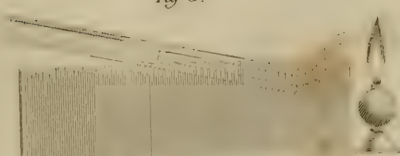


Fig. 5.



Mr. Perret's Apparatus.

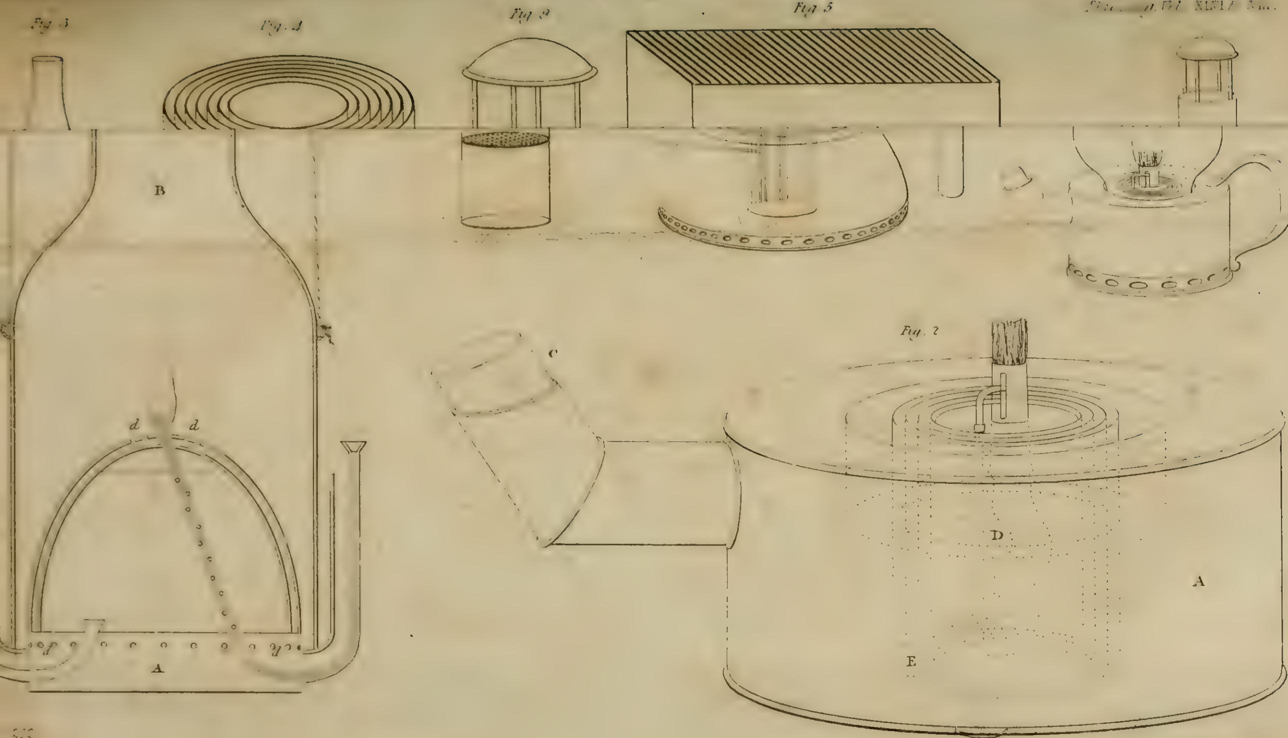
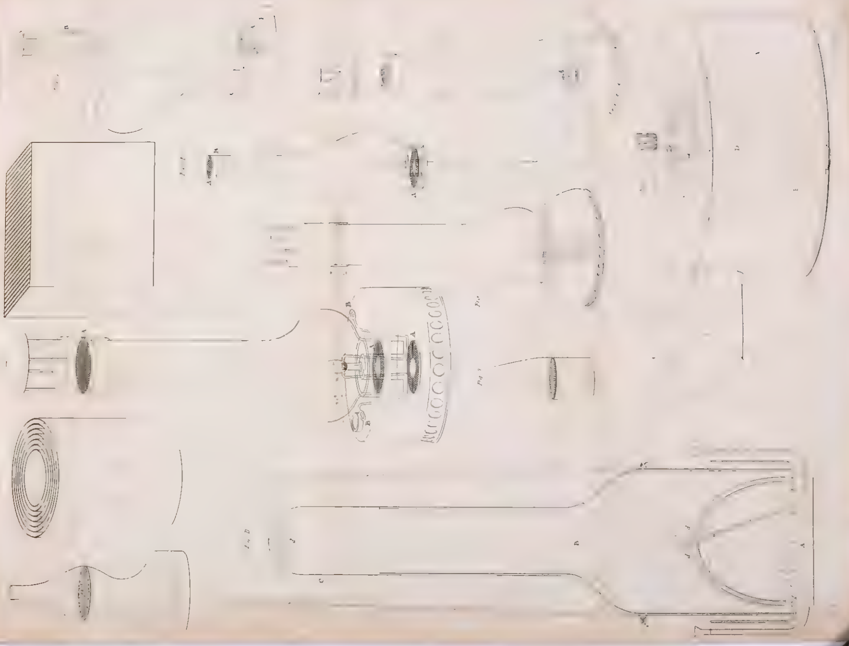


Fig. 3
Fig. 4
Fig. 2
Fig. 5
B
d
d
A
C
Fig. 2
D
A
E



ENGRAVINGS.

Vol. XLII. A Plate containing Professor LESLIE's Atmometer, and Figures to illustrate Dr. WOLLASTON's Paper on the elementary Particles of certain Crystals.—A Sketch of that Part of the Island of Java which contains the natural Lake of Sulphuric Acid.—Interior of Volcano in the Island of Java, and Figures to illustrate Mr. WALKER's Paper on the Electric Fluid.—A Plate to illustrate M. LINK's Memoir on the Anatomy of Plants, and Dr. WOLLASTON's Cryophorus.—A third Plate to illustrate M. LINK's Memoir on the Anatomy of Plants.—Mr. T. JONES's Sectograph,—a new Instrument for dividing right Lines into equal Parts, measuring Angles, and inscribing Polygons in the Circle, &c.

Vol. XLIII. A Plate to illustrate a New Transit Instrument invented by Sir H. C. ENGLEFIELD, Bart.—A Plate to illustrate the Use of Air-Vessels in Plants. By Mrs. IBBETSON.—A Plate to illustrate M. SEMENTINI's new Apparatus for producing Oxygen Gas to restore suspended Animation—Mr. READE's Paper on the Refraction of the Solar Rays—and Mr. HARGREAVES's Observations on Colours.—A Plate to illustrate Mr. J. WHITFORD's mechanical Substitute for Leeches; and Mr. J. TILLEY's Hydro-pneumatic Blow-pipe.—A Plate to describe Mr. R. HUGHES's Gudgeon for the Shaft of a Water-wheel; and Mr. PADBURY's Guard for a Carriage-wheel.—A Plate to describe Captain PEAT's Temporary Ship's Rudder.—A Quarto Plate of Mr. SAMUEL JONES's Sofa Bed.

Vol. XLIV. A Plate to illustrate Mr. HUME's Gazometer and Blowpipe; a Proposal for an Improvement of the Galvanic Trough; and a new Apparatus for preparing pure Muriatic Acid.—A practical Diagram for obtaining the Lunar Distances observed by a Sextant.—Electrical Apparatus to illustrate Mr. BRANDE's Paper on some new Electrical Phænomena.—A Quarto Plate to illustrate Mrs. IBBETSON's Paper on the Cuticle of Leaves.—BRUNTON's Patent Chain Cable.—Figures relative to Dr. BREWSTER's Paper on the Affections of Light transmitted through crystallized Bodies.—A Plate to illustrate Dr. BREWSTER's Paper given in our last Number.

Vol. XLV. A Plate to illustrate the Nourishment produced to the Plant by its Leaves. By Mrs. IBBETSON.—A Plate to illustrate Mr. BAKEWELL's Sketch of the Arrangement of the Rocks and Strata in the Northern Counties of England.—Plates to illustrate Mr. ROBERTSON BUCHANAN's Description of the Steam-Boats on the Clyde; and Mrs. IBBETSON's Paper proving that the Embryos of the Seeds are formed in the Root alone.—A Plate to illustrate Mr. JOHN WALTERS's Improvements in Naval Architecture.—A Quarto Plate on the Roots of Plants.—An Octavo Plate of Mr. SINGER's Electric Columns; Mr. WALKER's Electrometer; ROCHON's Apparatus for ascertaining the Heat of coloured Rays.

Vol. XLVI. Mr. T. JONES's New Reflecting Compass.—Mr. WOOLF's Patent Boiler for Steam Engines and other Purposes.—A Plate to illustrate Mr. WOOLF's Improvement on the Steam-Engine, by which the possibility of Steam escaping past the side of the Piston is effectually prevented.—A Plate to illustrate some Electrical Experiments by M. DE NELIS of Malines; with an Extension of them by Mr. SINGER and Mr. CROSSE.—A Plate to illustrate the Electrical Experiments of M. DE NELIS of Malines.—WOOLF's improved Steam-Engine.—A Plate to illustrate Mr. EVANS's Paper on guiding Balloons through the Atmosphere.—A Plate illustrative of Dr. BREWSTER's Paper on Light.

Vol. 46. Philosophical Magazine. November 181

Contents of Number 211.

*** Communications for this Work, addressed to the Editor, Pickett-Place, Temple Bar, will meet with every attention.

Richard and Arthur Taylor, Printers, Shoe Lane, London.

ENGRAVINGS.

Vol. XLII. A Plate containing Professor LESLIE's Atmometer, and Figures to illustrate Dr. WOLLASTON's Paper on the elementary Particles of certain Crystals.—A Sketch of that Part of the Island of Java which contains the natural Lake of Sulphuric Acid.—Interior of Volcano in the Island of Java, and Figures to illustrate Mr. WALKER's Paper on the Electric Fluid.—A Plate to illustrate M. LINK's Memoir on the Anatomy of Plants, and Dr. WOLLASTON's Cryophorus.—A third Plate to illustrate M. LINK's Memoir on the Anatomy of Plants.—Mr. T. JONES's Sectograph,—a new Instrument for dividing right Lines into equal Parts, measuring Angles, and inscribing Polygons in the Circle, &c.

Vol. XLIII. A Plate to illustrate a New Transit Instrument invented by Sir H. C. ENGLEFIELD, Bart.—A Plate to illustrate the Use of Air-Vessels in Plants. By Mrs. IBBETSON.—A Plate to illustrate M. SEMENTINI's new Apparatus for producing Oxygen Gas to restore suspended Animation.—Mr. READE's Paper on the Refraction of the Solar Rays—and Mr. HARGREAVES's Observations on Colours.—A Plate to illustrate Mr. J. WHITFORD's mechanical Substitute for Leeches; and Mr. J. TILLEY's Hydro-pneumatic Blow-pipe.—A Plate to describe Mr. R. HUGHES's Gudgeon for the Shaft of a Water-wheel; and Mr. PADBURY's Guard for a Carriage-wheel.—A Plate to describe Captain PEAT's Temporary Ship's Rudder.—A Quarto Plate of Mr. SAMUEL JONES's Sofa Bed.

Vol. XLIV. A Plate to illustrate Mr. HUME's Gazometer and Blowpipe; a Proposal for an Improvement of the Galvanic Trough; and a new Apparatus for preparing pure Muriatic Acid.—A practical Diagram for obtaining the Lunar Distances observed by a Sextant.—Electrical Apparatus to illustrate Mr. BRANDE's Paper on some new Electrical Phænomena.—A Quarto Plate to illustrate Mrs. IBBETSON's Paper on the Cuticle of Leaves.—BRUNTON's Patent Chain Cable.—Figures relative to Dr. BREWSTER's Paper on the Affections of Light transmitted through Crystallized Bodies.—A Plate to illustrate Dr. BREWSTER's Paper given in our last Number.

Vol. XLV. A Plate to illustrate the Nourishment produced to the Plant by its Leaves. By Mrs. IBBETSON.—A Plate to illustrate Mr. BAKEWELL's Sketch of the Arrangement of the Rocks and Strata in the Northern Counties of England.—Plates to illustrate Mr. ROBERTSON BUCHANAN's Description of the Steam-Boats on the Clyde; and Mrs. IBBETSON's Paper proving that the Embryos of the Seeds are formed in the Root alone.—A Plate to illustrate Mr. JOHN WALTERS's Improvements in Naval Architecture.—A Quarto Plate on the Roots of Plants.—An Octavo Plate of Mr. SINGER's Electric Columns; Mr. WALKER's Electrometer; ROCHON's Apparatus for ascertaining the Heat of coloured Rays.

Vol. XLVI. Mr. T. JONES's New Reflecting Compass.—Mr. WOOLF's Patent Boiler for Steam Engines and other Purposes.—A Plate to illustrate Mr. WOOLF's Improvement on the Steam-Engine, by which the possibility of Steam escaping past the side of the Piston is effectually prevented.—A Plate to illustrate some Electrical Experiments by M. DE NELIS of Malines; with an Extension of them by Mr. SINGER and Mr. CROSSE.

Vol. 46. Philosophical Magazine. October 1815

Contents of Number 210.

*** Communications for this Work, addressed to the Editor, Pickett-Place, Temple Bar, will meet with every attention.

RICHARD AND ARTHUR TAYLOR, PRINTERS, SHOE LANE, LONDON.

ENGRAVINGS.

Vol. XLI. PEPYS's Mercurial Voltaic Conductor.—LESLIE's Differential Thermometer. Diagrams to illustrate Dr. WOLLASTON's Periscopic Camera Obscura and Microscope.—Count RUMFORD's improved Coffee-pots.—Mr. GOSS's Instrument to work Addition of Numbers with Accuracy and Dispatch.—Mr. DAVIS's hanging Scaffold; and Mr. MARTIN's Method of relieving a Horse from a Cart when fallen down in its Shafts.— A Quarto Plate to illustrate Mr. J. HICKS's Account of the Situation and Means of raising the Royal George.—A Plate to illustrate Mr. SOUTHERN's Essay on the Equilibrium of a Combination of Beams, Blocks, &c. and on the Polygon of Forces.—A Plate to illustrate Mr. STEEVENS's Account of a Meteor seen at London and other Places in March last.—A Plate to illustrate Mr. MACHELL's Annular Saw.

Vol. XLII. A Plate containing Professor LESLIE's Atmometer, and Figures to illustrate Dr. WOLLASTON's Paper on the elementary Particles of certain Crystals.—A Sketch of that Part of the Island of Java which contains the natural Lake of Sulphuric Acid.—Interior of Volcano in the Island of Java, and Figures to illustrate Mr. WALKER's Paper on the Electric Fluid.—A Plate to illustrate M. LINK's Memoir on the Anatomy of Plants, and Dr. WOLLASTON's Cryophorus.—A third Plate to illustrate M. LINK's Memoir on the Anatomy of Plants.—Mr. T. JONES's Sectograph,—a new Instrument for dividing right Lines into equal Parts, measuring Angles, and inscribing Polygons in the Circle, &c.

Vol. XLIII. A Plate to illustrate a New Transit Instrument invented by Sir H. C. ENGLEFIELD, Bart.—A Plate to illustrate the Use of Air-Vessels in Plants. By Mrs. IBBETSON.—A Plate to illustrate M. SEMENTINI's new Apparatus for producing Oxygen Gas to restore suspended Animation—Mr. READE's Paper on the Refraction of the Solar Rays—and Mr. HARGREAVES's Observations on Colours.—A Plate to illustrate Mr. J. WHITFORD's mechanical Substitute for Leeches; and Mr. J. TILLEY's Hydro-pneumatic Blow-pipe.—A Plate to describe Mr. R. HUGHES's Gudgeon for the Shaft of a Water-wheel; and Mr. PADBURY's Guard for a Carriage-wheel.—A Plate to describe Captain PEAT's Temporary Ship's Rudder.—A Quarto Plate of Mr. SAMUEL JONES's Sofa Bed.

Vol. XLIV. A Plate to illustrate Mr. HUME's Gazometer and Blow-pipe; a Proposal for an Improvement of the Galvanic Trough; and a new Apparatus for preparing pure Muriatic Acid.—A practical Diagram for obtaining the Lunar Distances observed by a Sextant.—Electrical Apparatus to illustrate Mr. BRANDE's Paper on some new Electrical Phænomena.---A Quarto Plate to illustrate Mrs. IBBETSON's Paper on the Cuticle of Leaves.—BRUNTON's Patent Chain Cable.—Figures relative to Dr. BREWSTER's Paper on the Affections of Light transmitted through crystallized Bodies.—A Plate to illustrate Dr. BREWSTER's Paper given in our last Number.

Vol. XLV. A Plate to illustrate the Nourishment produced to the Plant by its Leaves. By Mrs. IBBETSON.—A Plate to illustrate Mr. BAKEWELL's Sketch of the Arrangement of the Rocks and Strata in the Northern Counties of England.—Plates to illustrate Mr. ROBERTSON BUCHANAN's Description of the Steam-Boats on the Clyde; and Mrs. IBBETSON's Paper proving that the Embryos of the Seeds are formed in the Root alone.—A Plate to illustrate Mr. JOHN WALTERS's Improvements in Naval Architecture.—A Quarto Plate on the Roots of Plants.—An Octavo Plate of Mr. SINGER's Electric Columns; Mr. WALKER's Electrometer; ROCHON's Apparatus for ascertaining the Heat of coloured Rays.

Vol. 46. Philosophical Magazine. September 1815.

Contents of Number 209.

*** Communications for this Work, addressed to the Editor, Pickett-Place, Temple Bar, will meet with every attention.

RICHARD AND ARTHUR TAYLOR, PRINTERS, SHOE LANE, LONDON.

... Pepys's Mercurial Voltaic Conductor.—Leslie's Differential Thermometer. Diagrams to illustrate Dr. Wollaston's Periscopic Camera Obscura and Microscope.—Count Rumford's improved Coffee-pots.—Mr. Goss's Instrument to work Addition of Numbers with Accuracy and Dispatch.—Mr. Davis's hanging Scaffold; and Mr. Martin's Method of relieving a Horse from a Cart when fallen down in its Shafts.— A Quarto Plate to illustrate Mr. J. Hicks's Account of the Situation and Means of raising the Royal George.—A Plate to illustrate Mr. Southern's Essay on the Equilibrium of a Combination of Beams, Blocks, &c. and on the Polygon of Forces.—A Plate to illustrate Mr. Stevens's Account of a Meteor seen at London and other Places in March last.—A Plate to illustrate Mr. Machell's Annular Saw.

Vol. XLII. A Plate containing Professor Leslie's Atmometer, and Figures to illustrate Dr. Wollaston's Paper on the elementary Particles of certain Crystals.—A Sketch of that Part of the Island of Java which contains the natural Lake of Sulphuric Acid.—Interior of Volcano in the Island of Java, and Figures to illustrate Mr. Walker's Paper on the Electric Fluid.—A Plate to illustrate M. Link's Memoir on the Anatomy of Plants, and Dr. Wollaston's Cryophorus.—A third Plate to illustrate M. Link's Memoir on the Anatomy of Plants.—Mr. T. Jones's Sectograph,—a new Instrument for dividing right Lines into equal Parts, measuring Angles, and inscribing Polygons in the Circle, &c.

Vol. XLIII. A Plate to illustrate a New Transit Instrument invented by Sir H. C. Englefield, Bart.—A Plate to illustrate the Use of Air-Vessels in Plants. By Mrs. Ibbetson.—A Plate to illustrate M. Sementini's new Apparatus for producing Oxygen Gas to restore suspended Animation.—Mr. Reade's Paper on the Refraction of the Solar Rays—and Mr. Hargreaves's Observations on Colours.—A Plate to illustrate Mr. J. Whitford's mechanical Substitute for Leeches; and Mr. J. Tilley's Hydro-pneumatic Blow-pipe.—A Plate to describe Mr. R. Hughes's Gudgeon for the Shaft of a Water-wheel; and Mr. Padbury's Guard for a Carriage-wheel.—A Plate to describe Captain Peat's Temporary Ship's Rudder.—A Quarto Plate of Mr. Samuel Jones's Sofa Bed.

Vol. XLIV. A Plate to illustrate Mr. Hume's Gazometer and Blow-pipe; a Proposal for an Improvement of the Galvanic Trough; and a new Apparatus for preparing pure Muriatic Acid.—A practical Diagram for obtaining the Lunar Distances observed by a Sextant.—Electrical Apparatus to illustrate Mr. Brandl's Paper on some new Electrical Phaenomena.—A Quarto Plate to illustrate Mrs. Ibbetson's Paper on the Cuticle of Leaves.—Brunton's Patent Chain Cable.—Figures relative to Dr. Brewster's Paper on the Affections of Light transmitted through crystallized Bodies.—A Plate to illustrate Dr. Brewster's Paper given in our last Number.

Vol. XLV. A Plate to illustrate the Nourishment produced to the Plant by its Leaves. By Mrs. Ibbetson.—A Plate to illustrate Mr. Bakewell's Sketch of the Arrangement of the Rocks and Strata in the Northern Counties of England.—Plates to illustrate Mr. Robertson Buchanan's Description of the Steam-Boats on the Clyde; and Mrs. Ibbetson's Paper proving that the Embryos of the Seeds are formed in the Root alone.—A Plate to illustrate Mr. John Walters's Improvements in Naval Architecture.—A Quarto Plate on the Roots of Plants.—An Octavo Plate of Mr. Singer's Electric Columns; Mr. Walker's Electrometer; Rochon's Apparatus for ascertaining the Heat of coloured Rays.

Vol. XLVI. Mr. T. Jones's New Reflecting Compass.—Mr. Woolf's Patent Boiler for Steam Engines and other Purposes.

CONTENTS OF NUMBER 208.

*** Communications for this Work, addressed to the Editor, Pickett-Place, Temple Bar, will meet with every attention.

RICHARD AND ARTHUR TAYLOR, PRINTERS, SHOE LANE, LONDON.

ENGRAVINGS.

Vol. XLI. Pepys's Mercurial Voltaic Conductor.—Leslie's Differential Thermometer. Diagrams to illustrate Dr. Wollaston's Periscopic Camera Obscura and Microscope.—Count Rumford's improved Coffee-pots.—Mr. Goss's Instrument to work Addition of Numbers with Accuracy and Dispatch.—Mr. Davis's hanging Scaffold; and Mr. Martin's Method of relieving a Horse from a Cart when fallen down in its Shafts.— A Quarto Plate to illustrate Mr. J. Hicks's Account of the Situation and Means of raising the Royal George.—A Plate to illustrate Mr. Southern's Essay on the Equilibrium of a Combination of Beams, Blocks, &c. and on the Polygon of Forces.—A Plate to illustrate Mr. Steevens's Account of a Meteor seen at London and other Places in March last.—A Plate to illustrate Mr. Machell's Annular Saw.

Vol. XLII. A Plate containing Professor Leslie's Atmometer, and Figures to illustrate Dr. Wollaston's Paper on the elementary Particles of certain Crystals.—A Sketch of that Part of the Island of Java which contains the natural Lake of Sulphuric Acid.—Interior of Volcano in the Island of Java, and Figures to illustrate Mr. Walker's Paper on the Electric Fluid.—A Plate to illustrate M. Link's Memoir on the Anatomy of Plants, and Dr. Wollaston's Cryophorus.—A third Plate to illustrate M. Link's Memoir on the Anatomy of Plants.—Mr. T. Jones's Sectograph,—a new Instrument for dividing right Lines into equal Parts, measuring Angles, and inscribing Polygons in the Circle, &c.

Vol. XLIII. A Plate to illustrate a New Transit Instrument invented by Sir H. C. Englefield, Bart.—A Plate to illustrate the Use of Air-Vessels in Plants. By Mrs. Ibbetson.—A Plate to illustrate M. Sementini's new Apparatus for producing Oxygen Gas to restore suspended Animation.—Mr. Reade's Paper on the Refraction of the Solar Rays—and Mr. Hargreaves's Observations on Colours.—A Plate to illustrate Mr. J. Whitford's mechanical Substitute for Leeches; and Mr. J. Tilley's Hydro-pneumatic Blow-pipe.—A Plate to describe Mr. R. Hughes's Gudgeon for the Shaft of a Water-wheel; and Mr. Padbury's Guard for a Carriage-wheel.—A Plate to describe Captain Peat's Temporary Ship's Rudder.—A Quarto Plate of Mr. Samuel Jones's Sofa Bed.

Vol. XLIV. A Plate to illustrate Mr. Hume's Gazometer and Blow-pipe; a Proposal for an Improvement of the Galvanic Trough; and a new Apparatus for preparing pure Muriatic Acid.—A practical Diagram for obtaining the Lunar Distances observed by a Sextant.—Electrical Apparatus to illustrate Mr. Brande's Paper on some new Electrical Phænomena.—A Quarto Plate to illustrate Mrs. Ibbetson's Paper on the Cuticle of Leaves.—Brunton's Patent Chain Cable.—Figures relative to Dr. Brewster's Paper on the Affections of Light transmitted through crystallized Bodies.—A Plate to illustrate Dr. Brewster's Paper given in our last Number.

Vol. XLV. A Plate to illustrate the Nourishment produced to the Plant by its Leaves. By Mrs. Ibbetson.—A Plate to illustrate Mr. Bakewell's Sketch of the Arrangement of the Rocks and Strata in the Northern Counties of England.—Plates to illustrate Mr. Robertson Buchanan's Description of the Steam-Boats on the Clyde; and Mrs. Ibbetson's Paper proving that the Embryos of the Seeds are formed in the Root alone.—A Plate to illustrate Mr. John Walters's Improvements in Naval Architecture.—A Quarto Plate on the Roots of Plants.—An Octavo Plate of Mr. Singer's Electric Columns; Mr. Walker's Electrometer; Rochon's Apparatus for ascertaining the Heat of coloured Rays.

Contents of Number 207.

*** Communications for this Work, addressed to the Edit[or] Pickett-Place, Temple Bar, will meet with every attention.